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The riddle of the use of impossible examples in microeconomics textbooks

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Abstract

This paper examines the use in microeconomics textbooks of what we call 'impossible examples', looking specifically at certain examples employed in chapters dealing with the production process or markets for factors of production. After showing that such examples are mostly to be found in textbooks for beginners, it asks what beginner students can possibly learn through them, taking the case of a concept (the marginal product), a law (the law of diminishing marginal product), and a theory (the income theory of distribution). The use of impossible examples in such textbooks appears to be a riddle, since, by using them to introduce, present, or explain parts of neoclassical theory, authors are in effect teaching a fake and inconsistent version of that theory.

Key words

Economics education, microeconomics textbooks, examples, marginal product, theory of distribution.

1. Introduction

Since Kuhn's work, we have been well aware of the paradigmatic character of examples found in scientific textbooks, for "[i]t is [...] the sense of 'paradigm' as standard example that led originally to [his] choice of that term" (1977 [1974], 307).¹ According to Kuhn, the standard examples students read in scientific textbooks are an essential part of the process by which they not only absorb the significance and scope of theories, but also tacitly assimilate a "group-licensed way of seeing" (1970, 189) the world, so that differences in the nature and use of examples reveal the structure of the subgroups within the scientific community (1970, 186-7, 189, 191, 1977 [1974], 298, 306-8, 1977, xix):

"these books [textbooks in physics] exhibit concrete problem solutions that the profession has come to accept as paradigms, and they ask the student [...] to solve for himself problems very closely related in both method and substance to those through which the textbook or the accompanying lecture has led him. Nothing could be better calculated to produce "mental sets" or *Einstellungen*. Only in their most elementary courses do other academic fields offer as much as a partial parallel." (1977 [1959], 229)

Within this framework, the use in textbooks of 'impossible examples' thus seems somewhat puzzling, and stands in need of further examination.

¹ 'Paradeigma' is a Greek word for 'example'.

What do we mean by ‘impossible examples’? In a previous paper dealing with examples in microeconomics textbooks concerning the competitive firm, we distinguished examples by reference to the four types of world they describe: real, abstract, imaginary or impossible (Jallais 2018).

First, an example can describe the *real world*, as in this passage of Mankiw's microeconomics textbook:

“We begin our discussion of costs at Caroline’s Cookie Factory. Caroline, the owner of the firm, buys flour, sugar, chocolate chips, and other cookie ingredients. She also buys the mixers and ovens and hires workers to run this equipment. She then sells the cookies to consumers. By examining some of the issues that Caroline faces in her business, we can learn some lessons about costs that apply to all firms in an economy.” (Mankiw 2015, 260)

or as in this one of Pindyck and Rubinfeld (2018):

“An example is the reconstruction of concrete sidewalks using jackhammers. It takes one person to use a jackhammer — neither two people and one jackhammer nor one person and two jackhammers will increase production. As another example, suppose that a cereal company offers a new breakfast cereal, Nutty Oat Crunch, whose two inputs, not surprisingly, are oats and nuts. The secret formula for the cereal requires exactly one ounce of nuts for every four ounces of oats in every serving. If the company were to purchase additional nuts but not additional oats, the output of cereal would remain unchanged, since the nuts must be combined with the oats in a fixed proportion. Similarly, purchasing additional oats without additional nuts would also be unproductive.” (229)

Note that what we call ‘real-world examples’ can be invented (as is Caroline’s Cookie Factory in the above-quoted passage): they need only to describe a plausible fact, even if the world posited thereby is highly simplified.

Second, examples can also describe *abstract worlds*. This is the case where the output is not specified at all, as in this passage of Kreps (2013):

“A *netput* vector is a vector from $\mathbb{R}^k$, where a negative component connotes a net *input* into the firm’s production process and a positive component connotes a net *output*. So, for instance, if $k = 5$, the netput vector $(-2, 0, 3, -1, 2)$ is interpreted as the firm transforming two units of the first commodity and one unit of the fourth into three units of the third and two units of the fifth.” (319)

In the same vein, but far less frequently, examples can describe *imaginary worlds*, where the firm produces an imaginary output such as the snarks in Begg, Fisher and Dornbush (1989) or the pfillips in Kreps (1990):

“In a particular economy, a product called *pfillip*, which is a nonnarcotic stimulant, is produced by a competitive industry. Each firm in this competitive industry has the same production technology, given by the production function

$$y = k^{1/6}p^{1/3},$$

where y is the amount of pfillip produced, k is the amount of *kapitose* (a specialty chemical) used in production, and l is the amount of *legume* (a common vegetable) used in production.” (Kreps 1990, 274)

Finally, we also find examples in microeconomics textbooks which describe *impossible worlds*: in impossible worlds, firms don’t produce pfillips or snarks, but real outputs such as clothes or cookies, but the production technology they use does not exist (yet). For example, Mankiw introduces the notion of marginal product of labor through Caroline’s Cookie Factory. In order to “simplify” the presentation, Mankiw has previously assumed “that the size of Caroline’s factory is fixed”, a “realistic” assumption “in the short run” (2015, 263). The equipment is then fixed, as well as somewhat oversized because otherwise it would be impossible to produce more cookies. So far so good: simplification is a good way to teach difficult or abstract concepts. But then Mankiw introduces a table (table 1) containing the following columns:

Number of workers	Output (quantity of cookies produced per hour)	Marginal Product of Labor
0	0	
		50
1	50	
		40
2	90	
		30
3	120	
		20
4	140	
		10
5	150	
		5
6	155	

As one might guess from column 3, Mankiw uses this table to explain marginal product of labor without speaking about (among other things) all the ingredients needed to make cookies (flour, sugar, chocolate chips, and so on):

“When the number of workers goes from 1 to 2, cookie production increases from 50 to 90, so the marginal product of the second worker is 40 cookies. And when the number of

workers goes from 2 to 3, cookie production increases from 90 to 120, so the marginal product of the third worker is 30 cookies.” (Mankiw 2015, 264)

If Caroline’s factory were real, inputs would not be substitutes but complements: Caroline could not replace flour by labor, labor by ovens, nor ovens by chocolate chips. Furthermore, there would be no need to reduce the number of inputs for the sake of simplicity, for any student will understand that, unless something like the miracle of the loaves and fishes was to be repeated, no one could produce more cookies without, say, more flour, even by working more: the marginal product of labor would therefore be zero (not 50, 40 or even 5 per hour). Indeed, the concept of the marginal product of an input requires that this input be increased by one unit while all other inputs are held constant. This consequence is obvious wherever the production process needs material inputs, as Moseley (2015) clearly puts it;² but this is also exactly what happens in the two real-world examples of Pindyck and Rubinfeld’s textbook quoted above, including the example of ‘the reconstruction of concrete sidewalks using jackhammers’ which does not mention any other input except labor. For, as has been known for a long time, if the production function is fixed proportions, “then an extra unit of one factor, unaccompanied by an increase in the other, will yield precisely no addition to the total product” (Hicks, 1932, 81): the marginal product of each input is then zero.³

Hence *either* Mankiw’s table 1 is appropriate to present the concept of the marginal product of labor, and this example describes an impossible world where “the second worker”, whose name might be Jesus, can produce 40 cookies without any flour, sugar, or chocolate chips’;⁴ *or* this example describes our world and therefore can’t be used to explain what marginal product of labor means, since the figure 40 cannot indicate the increase in the quantity of output obtained from one additional unit of labor *holding the used quantity of all other inputs fixed*.

We use the terminology “impossible examples” to designate these kinds of example that either describe an impossible world (where real outputs are produced through a technique which does not exist) or are inconsistent with the very concept or hypothesis they are supposed to exemplify (production techniques are plausible but don’t fit the concept or hypothesis). In short, impossible examples are either *impossible-world examples* or *real-world-but-inconsistent examples*.

While examples describing real, abstract, or imaginary worlds are, in many ways, very useful from a pedagogical point of view (see Jallais 2018), this is not so obvious for impossible examples. Unless they are unambiguously presented as being impossible, the use of this sort of example seems to be an

² “[I]t is not possible to incorporate materials inputs (and intermediate goods in general) into the theory in a reasonably coherent way, because materials inputs *cannot be held constant as output increases*. In order to increase output in goods-producing industries, the quantity of materials used to produce these goods must also be increased (e.g. more wood to produce another chair, more cloth to produce another shirt, more tires to produce another car). [...] the existence of materials inputs in the production process renders the concept of the marginal productivity of capital impossible in reality (this fundamental problem also applies to the marginal product of labor).” (Moseley 2015, 294-5)

³ This phenomenon was well known, for example, by Pareto at the end of the 19th century. Pareto notes, in fact, that “when some of the production coefficients are constant, considering the marginal product cannot be done without correction. Indeed, if, for example, the quantity of iron ore is to be proportional to the quantity of metallic iron produced, one cannot assume that the product will rise by by increasing only one of the other production factors.” (Pareto 1897, §717, 85).

⁴ In Moseley 2012b, examining the example of a bakery given in Mankiw’s “best-selling intermediate Macroeconomics textbook (Chapter 3)”, Moseley speaks about ‘the additional bakery worker’ as ‘a miracle worker’. ‘Jesus would be jealous’ he adds (Moseley 2012b, 118).

aberration: *a priori* they cannot enable students to understand a concept or a hypothesis, and even less so its scope. We may ask, then: why do microeconomists use such examples? and what can students learn from them? Before these questions can be answered, we will (in Sect. 2) present the textbooks we have examined and explain how we chose them. We will then (Sect. 3) propose a typology of these textbooks according to their intended audience. This will allow us to reveal the kind of students who are reading impossible examples. The presentation of the uses of impossible examples and the way they are — or aren't — justified (Sect. 4) will then allow us to infer what these students can learn through this type of examples, addressing the cases of a concept (Sect. 5), a law (Sect. 6), and the related theory (Sect. 7).

2. Corpus

The material we chose to investigate for this research is composed of “popular” microeconomics textbooks, meaning those that are used by a wide range of students. To make our selection, we examined the rankings of the “top” or “best” microeconomics books provided by three websites. First, we used two rankings provided by Goodreads, based on the recommendations of the readers:⁵ the top ‘microeconomics’ books and the ‘popular microeconomics books’ ranking. We also examined BookAuthority’s ranking, based on multiple criteria, including the recommendations of “domain experts”. BookAuthority claims to be “the world’s leading site for nonfiction book recommendations”. It uses various data to “identify and rate the best nonfiction books, using dozens of different signals, including public mentions, recommendations, ratings, sentiment, popularity and sales history. This includes maintaining the most comprehensive collection of book recommendations from domain experts such as Elon Musk, Warren Buffett, Prof. Daniel Kahneman, Sheryl Sandberg, and David Allen” (<https://bookauthority.org/about>). Last, we used ‘Amazon Best Sellers Rank’, as a representation of general sales of textbooks throughout the world.⁶ These rankings were then narrowed down to academic textbooks (the result may be found in the Annex).

We then chose an initial set of textbooks comprising the first two textbooks of each of the four rankings. This set contained six different textbooks, and includes the five best-sellers (according to Amazon’s ranking). We also looked into other textbooks that were further down in the rankings if the author was a major figure in the academic field. For instance, we chose to examine Varian’s *Intermediate Microeconomics* and *Microeconomic Analysis*, which appear among the first ten or eleven textbooks of two rankings; and although David M. Kreps’s *Microeconomic Foundations* was ranked 9th in ‘popular microeconomics books’ but only 22nd in the global BookAuthority list (and 15th if we exclude double rankings), we chose to examine this textbook because its author, like Krugman, was also awarded the John Bates medal in 1989 by the American Economic Association.

Hence, we studied the ten following textbooks:

- [Frank, *Microeconomics and Behavior*, 9th ed., 2014 \[1991\].](#)⁷
- Kreps, *Microeconomic Foundations I*, 2013.

⁵ “Goodreads is the world’s largest site for readers and book recommendations. Our mission is to help people find and share books they love. Goodreads launched in January 2007”. It claims to have 90 million members, over 2.6 billion books, and 90 million reviews (<https://www.goodreads.com/about/us>).

⁶ “The Amazon Best Sellers calculation is based on Amazon.com sales and is updated hourly to reflect recent and historical sales of every item sold on Amazon.com.” (<https://www.amazon.com/gp/help/customer/display.html?nodeId=GGGMZK378RQPATDJ>)

⁷ The year in brackets is that of the first edition, when the edition used is more recent.

- Krugman & Wells, *Microeconomics*, 4th ed., 2015 [2006].
- Mankiw, *Principles of Microeconomics*, 7th ed., 2015 [2000]
- Mas-Colell, Whinston & Green, *Microeconomic Theory*, 1995.
- Mateer & Coppock, *Principles in Microeconomics*, 2nd ed., 2018 [2014].
- McConnell, Brue & Flynn, *Microeconomics: Principles, Problems and Policies*, 21st ed., 2018 [1975]
- Pindyck & Rubinfeld, *Microeconomics*, 9th ed., 2018 [1988]
- Varian, *Intermediate Microeconomics: A Modern Approach*, 9th ed., 2014 [1987]
- Varian, *Microeconomic Analysis*, 3rd ed., 1992 [1978]

Within this corpus, we decided to focus our analysis on the presentation of the “competitive firm” and on the examples given in chapters dealing with the production process or markets for factors of productions. This choice — of the firm over the consumer, for example — was motivated by the fact that output is real, whereas utility (‘satisfaction’) is a notion more difficult to grasp. As Varian puts it: “the output of a production process is generally observable, whereas the ‘output’ of consumption (utility) is not directly observable” (2014, 350). Furthermore, impossible examples are unlikely to be found in chapters on consumers, since any kind of preference seems to be *a priori* possible: hence if microeconomics textbooks were dealing only with consumer theory, our question would have been of no interest.

3. A typology of textbooks

Our focus on the use of impossible examples in the chapters we were interested in led us to categorize textbooks into three distinct groups: beginner, intermediate and advanced textbooks. Krugman and Wells 2015, Mankiw 2015, Mateer and Coppock 2018, McConnell, Brue and Flynn 2018, and Pindyck and Rubinfeld 2018 are beginner textbooks: they aim at first-year college students who will not necessarily major in economics. This characteristic makes them ‘bestsellers’ because of the number of students taking Econ 101.⁸ On the other end of the spectrum, Kreps 2013, Mas-Colell, Whinston and Green 1995 and Varian 1992 are what we called “advanced” textbooks, addressed at first-year graduate students taking a course in microeconomic theory (Mas-Colell, Whinston & Green 1995, xiii, Kreps 2013, 16). Falling between these, in a way we explain below, we labeled Varian 2014 and Frank 2014 “intermediate” textbooks.

The purpose of beginners’ textbooks is primarily to ‘help’ students to “think like economists” (Mateer & Coppock 2018, xxvi; Mankiw 2015, 19),⁹ so that they can understand economic issues and policies (McConnell, Brue & Flynn 2018, x), and grasp how the economy — or even the world — works (Pindyck & Rubinfeld 2018, 15), or more specifically “what people do and how they interact” (Krugman & Wells 2015, xvii). But they aim also to show students how they can “apply economic principles to their decision-making” (Mateer & Coppock 2018, xxvi), whether they are a manager, a public policy maker, or something else entirely (Pindyck & Rubinfeld 2018, 15).

⁸ “We have been extremely gratified by the success of the first three editions of *Economics*, which has made it one of the best-selling economics textbooks” say Krugman & Wells (2015, xvii), while McConnell, Brue & Flynn begin with this welcoming phrase: “Welcome to the 21st edition of *Economics*, the best-selling economics textbook in the world. An estimated 15 million students have used *Economics* or its companion editions, *Macroeconomics* and *Microeconomics*” (2018, x). For his part, speaking about all his textbooks, Mankiw guesses that, “including translations, about four million copies of [his] books are in print” (2020, 215).

⁹ More precisely, Mankiw invites his readers to use the economists’ language to think about the world in which they live. His purpose is to “help” students “learn the economist’s way of thinking” (Mankiw 2015, 19).

On the contrary, the avowed objective of advanced textbooks is technical and theoretical, as is apparent from their titles: ‘Microeconomic *Theory*’, ‘Microeconomic *Analysis*’ and ‘Microeconomic *Foundations*’ (emphasis added). For example, Varian 1992 is aimed at helping students “[master] the standard tools of economic analysis” (xiv). These textbooks aim first at rigor and precision (Mas-Colell, Whinston & Green 1995, xiv). Kreps describes the main differences between beginner and advanced textbooks as follows:

“The standard textbooks are not written with command or mastery of the material as their primary objective. Because they are written to serve very broad audiences, breadth of coverage is stressed over depth, and the authors sometimes omit technical details, to avoid panicking less well-prepared readers. This book sacrifices breadth for depth, avoids compromises about details (with a few exceptions), and tries to explain to the reader both why economic foundations are done the way they are done and what are some of the limitations in how things are done.” (Kreps 2013, 14)

Hence beginner textbooks present microeconomic concepts and assumptions through stories, make extensive references to the real world, and seldom contain mathematical equations or demonstrations.¹⁰ On the contrary, advanced textbooks are very technical. Their authors concentrate on mathematical hypotheses and demonstrations of neoclassical producer theory, and seem to have little interest in linking their presentation to real-world issues.¹¹

Finally, like beginner ones, intermediate textbooks are aimed at undergraduate students, but with a more thorough treatment of microeconomic theory. They are therefore hereafter categorized as intermediate since they both try to refer to real-world stories while also proposing a formal presentation of microeconomic theory.¹² As Varian puts it:

“My aim in writing the original text was to present a treatment of the methods of microeconomics that would allow students to apply these tools on their own and not just passively absorb the predigested cases described in the text. I have found that the best way to do this is to emphasize the fundamental conceptual foundations of microeconomics and to provide concrete examples of their application rather than to attempt to provide an encyclopedia of terminology and anecdote.” (Varian 2014, xix)

In the same vein, Frank writes: “My goal in writing *Microeconomics and Behavior* was to produce an intellectually and challenging text that would also be accessible and engaging to students. [...] Some texts have done well by sacrificing rigor in the name of user-friendliness. [...] Other texts have succeeded in sacrificing accessibility in the name of rigor, where rigor all too often means little more than mathematical density” (Frank 2015, p. vii).

¹⁰ “We believed that students’ understanding of and appreciation for models would be greatly enhanced if they were presented, as much as possible, in the context of stories about the real world, stories that both illustrate economic concepts and touch on the concerns we all face as individuals living in a world shaped by economic forces.” (Krugman & Wells 2015, xvii)

¹¹ As Kreps says: “Some first-year graduate students are utterly turned off by their first-year theory courses. They have come to the study of economics to understand real-world phenomena and, perhaps, to make a difference in the real world, not to study mathematics. To those students, my response is that if you plan to use economic techniques to understand the real world and to see how to make a difference, your effectiveness will depend in part on how well you understand those techniques; doing all this math builds your understanding of the techniques.” (Kreps 2013, 17)

¹² We named this category after Varian (2014).

All these textbooks use examples, and most of them stress their usefulness, albeit for very different reasons. Examples in advanced textbooks are designed to illustrate the mathematical issues underlying microeconomic producer theory, and to help students acquire the technical tools of neoclassical modeling. Examples in beginner textbooks are used to present the main concepts, assumptions, and properties, seeking to attract the students' interest to the topics, motivate their learning, and increase their understanding of the models as well as their relevance and application to the real world.¹³ For example, McConnell, Brue and Flynn's 'boxes' aim to provide "analogies, examples, or stories that help drive home central economic ideas in a student-oriented, real-world manner" (2018, xi). Krugman and Wells seem to be the most fervent defenders of the use of examples:

"We believed that students' understanding of and appreciation for models would be greatly enhanced if they were presented, as much as possible, in the context of stories about the real world, stories that both illustrate economic concepts and touch on the concerns we all face as individuals living in a world shaped by economic forces. [...] Because Robin and I both feel that the teaching of economics is at its best when it engages students with real-life issues and problems, we have done a major updating of examples, stories, and cases to incorporate many of the most current economics topics. No other economics textbook updates examples as extensively with each new edition as ours does. This thorough refreshing of examples was one major focus of the revision." (Krugman & Wells 2015, xvii)

Between these cases, Varian (2014, xx) says he "put in a lot of examples of how to use the theory described here" to teach students how "to translate an economic story into an equation or a numerical example".¹⁴

All this at least partly explains the kind of examples these different textbooks use. Advanced textbooks refer exclusively to abstract examples in the chapters we examined.¹⁵ In the intermediate textbooks, a few real-world or impossible examples are given to illustrate the concepts presented in chapters relating to production. For instance, in Varian's (2014) chapters relating to technology, competitive firm behavior, or markets for factors of production, he refers to: holes made by a man and a shovel (352); "the case of farming [with] one man on one acre of land" (357); the pumping capacity of an oil pipeline (360); a data center (360); and Intel's "'fab plants' that fabricate, assemble, sort, and test advanced computer chips" (361). Similarly, in his chapter on production, Frank speaks of: meals produced with equipment-hours/week and person-hours/week (250); the FM radio broadcast of classical music (251); the owner of a fishing fleet (258); "trips" produced by "two types of gasoline inputs, Texaco and Amoco" (263); and typing letters using typewriters (263). Yet, in both of these textbooks, the rest of the concepts

¹³ Pindyck & Rubinfeld insist on this last use of examples: "This applied emphasis is accomplished by including examples." Indeed, students "want and need a good understanding of how microeconomics can actually be used outside the classroom" (Pindyck & Rubinfeld 2018, 16).

¹⁴ Similarly, Frank argues that "applications and examples are integrated fully into the text" (vii), and that "more than any other text currently available in the marketplace, *Microeconomics and Behavior* prepares students for its end of chapter problems by taking them through a sequence of carefully crafted examples and concept checks within each chapter" (Frank 2015, viii).

¹⁵ Note that they may contain other kinds of examples in other chapters. Mas-Colell, Whinston & Green, for example, use a few real-world examples in the chapters dealing with preference or consumer choice, among others: "For example, imagine that a poor consumer initially is fulfilling much of his dietary requirements with potatoes because they are a low cost way to avoid hunger. Etc." (1995, 26).

are either illustrated by abstract examples or are introduced without any example at all. On the contrary, beginner textbooks contain mostly real-world and impossible examples.¹⁶

It is in this latter category of textbooks that most impossible examples can be found: the authors refer to numerous producers (most of whom are even given surnames) and real products to introduce, present, explain, or even define the theoretical concepts. For example, Krugman and Wells appeal to George and Martha's farm (2015, 330 et sq.), Marty's Frozen Yogurt "a small shop that sells cups of frozen yogurt" (2015, 353), Magnificent Blooms "a florist specializing in floral arrangements" (2015, 353), or Noelle's Christmas tree farm (2015, 361 et sq.). Mankiw makes extensive use of "Caroline's Cookie Factory" (2015, 260 et sq.) or "Conrad's Coffee Shop" (2015, 266 et sq.) when dealing with firm behavior (part V of his "Principles of microeconomics") while Mateer and Coppock do the same thing with a "McDonald's restaurant" (2018, 251-4). Pindyck and Rubinfeld evoke, among others, a "bakery" producing "such outputs as bread, cakes" (2018, 212), "the number of personal computers that can be produced each year with a 10,000-squarefoot plant and a specific amount of assembly-line labor" (2018, 213), or a "clothing factory" (2018, 214). Finally, McConnell, Brue and Flynn refer to a "farmer who is deciding on how much corn to plant" (2018, 53), "Leah, an apple grower" (2018, 79), "Carlos, Courtney, Chuck, Cindy, Craig and Chad, orange growers" (80), "a table-making firm" (2018, 180), a "small t-shirt manufacturer" (2018, 182), or "a wood-shop manufacturing furniture frames" (2018, 184). These are only a few of the very numerous examples given by these authors when presenting producer theory.

Beginner textbooks also sometimes include boxes with real data on issues pertaining to the sector they have presented. For instance, when Krugman and Wells introduce the production process with George and Martha's farm, they also include a Box on "wheat yields around the world" (with FAO data and analysis of the differences of wheat yields in various countries) (Krugman & Wells 2015, 332).

4. What justification for the use of impossible examples?

As mentioned in the introduction to this paper, the pedagogical usefulness of impossible examples is far from obvious. We therefore looked carefully for a justification of their inclusion in beginner textbooks; we found none.

Beginner textbooks systematically refer to examples when presenting concepts, and it appears that they generally use impossible examples to illustrate, introduce, present, or explain part of the core concepts, assumptions, or theorems of microeconomic neoclassical theory — such as the marginal product, the law of diminishing marginal product, or the distribution of revenue.

In fact, the textbook authors never mention that the example they may be using while presenting these particular aspects happens to be impossible. To add to the confusion, the same basic storyline — e.g. Caroline's Cookie Factory or Conrad's Coffee Shop (Mankiw 2015), George and Martha's farm (Krugman & Wells 2015) or even McDonald's (Mateer & Coppock 2018) — can switch from real to impossible without any warning. As a matter of fact, impossible examples are often introduced as real-world examples, and the reader is never alerted when the latter turn into the former (or when these impossible examples turn once again into real-world examples, when the concept to which they pertain is not inconsistent with reality).

¹⁶ We did not find any example describing an imaginary world in our corpus.

But if students don't guess this metamorphosis — and as beginners how could they? — what can they possibly learn through these examples? To illustrate our point we will focus on the concept of the marginal product (Sect. 5), the law of diminishing marginal product (Sect. 6), and the income theory of distribution (Sect. 7).

5. What can students possibly learn through impossible examples? The case of the concept of the marginal product

Let us recall that the marginal product of labor is the increase in the quantity of output obtained from one additional unit of labor, holding the quantity of all other inputs fixed. In our beginner textbooks, marginal product (most frequently — but not always — of labor) is *always* presented through at least one impossible example. Yet, in our view, undergraduate students cannot possibly understand the concept of the marginal product when it is introduced, explained, and finally defined through impossible examples, for throughout these examples they are never told that '*holding the quantity of all other inputs fixed*' means '*holding the used quantity of all other inputs fixed*', if indeed they are ever informed about the existence of such a condition.

Before introducing the concept of the marginal product, authors generally reduce the number of inputs and only consider two: generally speaking 'labor' and 'equipment', 'capital', 'machines', 'factory', or even 'land'. The question is then: where have all the other inputs gone?

Let's take the case of Caroline's Cookie Factory. For precision, let us cite Mankiw's exact phrasing:

"In the analysis that follows, we make an important simplifying assumption: we assume that the size of Caroline's factory is fixed *and that Caroline can vary the number of cookies produced only by changing the number of workers she employs*. This assumption is realistic in the short run but not in the long run." (2015, 263, our emphasis)

Since the assumption "Caroline can vary the number of cookies only by changing the number of workers she employs" is not presented as an impossible but as a "realistic" one "in the short run", students are probably led to understand that the factory is oversized — like the fixed amount of 'capital' or 'equipment' in Pindyck and Rubinfeld's clothing factory (2018, 214 et sqq.), for example — and that the term 'factory' (or 'equipment' or 'capital') includes all other inputs, as in Mateer and Coppock's McDonald's example in which capital "includes the building itself, the equipment used, the parking lot, the signs, and all the hamburger patties, buns, fries, ketchup, and other foodstuffs" (2018, 251), or in McConnell, Brue and Flynn's woodshop:

"Assume a woodshop is manufacturing furniture frames. It has a specific amount of equipment such as lathes, planes, saws, and sanders. If this shop hired just one or two workers, total output and productivity (output per worker) would be very low. The workers would have to perform many different jobs, and the advantages of specialization would not be realized. Time would be lost in switching from one job to another, and machines would stand idle much of the time. In short, the plant would be understaffed, and production would be inefficient because there would be too much capital relative to the amount of labor. The shop could eliminate those difficulties by hiring more workers. Then the equipment would be more fully used, and workers could specialize in doing a single job." (2018, 184, our underlining)

In Mankiw's example, students would then understand the term 'factory' to include, among other things, enough 'flour, sugar, chocolate chips, and other cookie ingredients' to produce at least 155 cookies (per hour). Hence, they would misunderstand what 'marginal product' means: *the increase in the number of cookies that arises from one additional unit of labor* would not be then obtained *holding the used quantity of all other inputs (flour, sugar, chocolate chips, ...) fixed*. This condition is, by the way, not specified by Mankiw when he defines the marginal product:

"The marginal product of any input in the production process is the increase in the quantity of output obtained from one additional unit of that input." (2015, 264)¹⁷

Most of the time, the composition of the equipment, machines, capital, factory, or even land is not sufficiently precisely specified for students to know whether it includes all other inputs. Students can therefore reasonably ask themselves where all those other inputs are. Where are the seeds, fertilizer, tools, or agricultural machines in George and Martha's farm (Krugman & Wells, 2015, 330 et sq.), for example?

On the contrary, sometimes it is very clear that the oversized equipment, machines, capital, or land does not include all other material inputs. Yet this doesn't seem to matter, since the quantity of those inputs nevertheless increases with the quantity of labor, as in the second problem proposed by Krugman and Wells (2015) at the end of their chapter 11, 'Behind the supply curve: inputs and costs':

"Marty's Frozen Yogurt is a small shop that sells cups of frozen yogurt in a university town. Marty owns three frozen-yogurt machines. His other inputs are refrigerators, frozen-yogurt mix, cups, sprinkle toppings, and, of course, workers. He estimates that his daily production function when he varies the number of workers employed (and at the same time, of course, yogurt mix, cups, and so on) is as shown in the accompanying table. [similar to Mankiw's table 1 quoted above]

Quantity of labor (workers)	Quantity of frozen yogurt (cups)
0	0
1	110
2	200
3	270
4	300
5	320
6	330

¹⁷ He is not the only one. Krugman & Wells, for example, don't mention this condition in the definition of marginal product, but do so later, when explaining diminishing returns to an input:

"In general, the **marginal product** of an input is the additional quantity of output that is produced by using one more unit of that input." (2015, 331)

"In general, there are diminishing returns to an input when an increase in the quantity of that input, holding the quantity of all other inputs fixed, reduces that input's marginal product. Due to diminishing returns to labor, the marginal product of labor curve is negatively sloped." (2015, 332)

The condition "holding the quantity of all other inputs fixed" cannot be understood by an undergraduate student since it is indicated using an impossible example.

[...] c. What is the marginal product of the first worker? The second worker? The third worker? Why does marginal product decline as the number of workers increases?" (Krugman & Wells 2015, 353, our underlining.)

In this case, as in the previous one (where equipment-capital-land included all other inputs), "marginal product of labor" seems to designate the increase in the quantity of output (frozen yogurt, here) that arises from one additional unit of labor, *and "of course" from additional used quantities of all other inputs* (yogurt mix, cups, and so on). And when reading Mankiw's textbook, for example, if students do not guess that Caroline lives in an impossible world — and how could it be otherwise? — they cannot understand anything but this ill-defined concept of the marginal product of labor. Whether or not they believe that capital-factory-equipment-land includes all inputs but labor, they cannot understand that, in all those examples, the output increases after the use of an additional unit of labor *holding the used quantity of all other inputs fixed*, because, in all those examples, it is clearly impossible.

This confusion is the reason why beginner students can end up believing that the marginal product of labor can be positive in the case of a fixed-proportions production function (such as that of a cookie or furniture frames factory, a farm, or a Frozen Yogurt shop). Hence, for these students, when the marginal product of labor is null — as in Pindyck and Rubinfeld's example of "reconstruction of concrete sidewalks using jackhammers" (2018, 229) quoted above — it can't be because of a fixed-proportions production function, for it is positive in Caroline's Cookie Factory (Mankiw 2015) or in Pindyck and Rubinfeld's clothing factory (Pindyck & Rubinfeld 2018, 214 et sqq.); they probably think that this only occurs when the quantity of other inputs is not excessive.

6. What can students possibly learn through impossible examples? The case of the law of diminishing marginal product

Yet, at this stage of their reading, the beginner students will certainly think that the marginal product of an input is the increase in the quantity of output that arises from one additional unit of this input, and "of course" from additional used quantities of all other inputs, unless the quantity of (at least) one of these other inputs cannot be increased, in which case the marginal product of the input is null. What else could they possibly be thinking?

And yet, it is not that clear: as a matter of fact, such a comprehension does not explain Mankiw's table 1 (Mankiw 2015, 263)¹⁸ for it does not allow the marginal product of any input to be diminishing. This ill-defined concept of the marginal product looks more like 'returns to scale' than like 'marginal product', since the used quantities of all inputs increase at the same time. Yet, as it appears in intermediate or advanced textbooks, the diminishing returns to scale is a knotty point, and cannot happen in models in which the production function reflects nothing but technology. As Mas-Colell, Whinston and Green put it:

"It is important not to lose sight of the fact that the production set describes technology, not limit on resources. It can be argued that if all inputs [...] are explicitly accounted for, then it should always be possible to replicate production. After all, we are not saying that

¹⁸ Nor does it explain, among many others, Pindyck & Rubinfeld's table 6.1 (2018, 215), Mateer & Coppock's figure 8.1 (2018, 253), or tables in Krugman & Wells (2015, 353).

doubling output is actually feasible, only that in principle it would be possible if all inputs [...] were doubled.” (1995, 134)¹⁹

Hence, according to them, the usual explanation of diminishing returns to scale is the scarcity of (at least) one forgotten (Varian 2014, 360)²⁰ or ‘unlisted’ input:

“In this view, which originated with Marshall and has been much emphasized by McKenzie (1959), decreasing returns must reflect the scarcity of an underlying, unlisted input of production.” (MasColell, Whinston & Green 1995, 134)

For the same reason, Varian regards diminishing returns to scale as “a short-run phenomenon, with something being held fixed” (Varian 2014, 360).²¹

Nevertheless, if we look back at Mankiw’s table 1, this would only explain why the second worker added 40 cookies to the total product, while one worker could produce 50: the reason could be an insufficient quantity of flour or a congestion of the oven or whatever. But, in this case, the third worker could not, however, produce any additional cookies, so that his marginal product must be zero, not 30 as in Mankiw’s table 1.²²

There is, however, an exception in the textbooks we examined: the case where the fixed, underlying, unlisted input is land. Krugman and Wells, for example, explain the diminishing returns through the example of George and Martha’s farm:

“To grasp why diminishing returns can occur, think about what happens as George and Martha add more and more workers without increasing the number of acres of land. As the number of workers increases, the land is farmed more intensively and the number of bushels produced increases. But each additional worker is working with a smaller share of the 10 acres — the fixed input — than the previous worker. As a result, the additional worker cannot produce as much output as the previous worker. So it’s not surprising that the marginal product of the additional worker falls.

The crucial point to emphasize about diminishing returns is that, like many propositions in economics, it is an “other things equal” proposition: each successive unit of an input will raise production by less than the last *if the quantity of all other inputs is held fixed.*” (Krugman & Wells 2015, 333)²³

¹⁹ Varian says almost the same thing in his intermediate textbook: “This case [diminishing returns to scale] is somewhat peculiar. If we get less than twice as much output from having twice as much of each input, we must be doing something wrong. After all, we could just replicate what we were doing before!” (Varian 2014, 360). “If the firm has twice as much of each input, it can just set up two plants side by side and thereby get twice as much output. With three times as much of each input, it can set up three plants, and so on” (Varian 2014, 359).

²⁰ “The usual way in which diminishing returns to scale arises is because we forgot to account for some input.” (Varian 2014, 360)

²¹ In his advanced textbook, Varian claims almost the same thing: “It turns out that it can always be assumed that decreasing returns to scale is due to the presence of some fixed input” (Varian 1992, 16).

²² And Mateer & Coppock’s figure 8.1 (2018, 253) or Pindyck & Rubinfeld’s table 6.1 (2018, 215), for example, are problematic for the very same reasons.

²³ We find almost the same example in Varian’s intermediate textbook (Varian 2014, 357).

It is then obvious that “all other inputs” means not only “the fixed input” land, but also seeds, fertilizer, tools, or agricultural machines, etc., the used quantities of which must necessarily increase unless, again, the marginal product of labor is soon to be negligible.²⁴ “Fixed” here means two different things: the fixed quantity of land which is farmed more and more intensively, and the fixed, but sufficient, quantities of all other inputs for the number of bushels produced to increase. Here beginner students who were taught the Ricardian theory of rent surely, and unfortunately, will not see the difference between the law of diminishing marginal product of labor in neoclassical theory and the diminishing return on land which generates rent in Ricardo’s *Principles* (2001 [1821]).²⁵

But beyond the fact that land is a somewhat particular input, all this takes us very far indeed from what diminishing marginal product of labor means in neoclassical theory.

7. What can students possibly learn through impossible examples? The case of distribution of revenue

Marginal product is a key concept of neo-classical economics, for, in the perfect competition model, it determines inputs’ demands (the quantities chosen by a competitive profit-maximizing firm are such that the value of the input’s marginal product equals the input’s real price). It is then a core concept of neoclassical distribution theory, the so-called ‘marginal productivity theory of income distribution’ (McConnel, Brue & Flynn 2018, chapter 16, 6th section’s title, 325), in which the price of each input equals its marginal productivity. This is generally what appears in the chapter of beginners’ textbooks which deals with “the markets for the factors of production”,²⁶ which students read thinking that marginal productivity of an input is the increase in the quantity of output that arises from one additional unit of that input, *and “of course” from additional used quantities of all other inputs.*

Yet, concerning the presentation of distribution theory, using impossible examples to explain marginal product has two unfortunate pedagogical consequences, consequences that are all the more unfortunate that they happen to be conflicting. On the one hand, (i) it leads to an absurd, and sometimes inconsistent, income distribution theory; on the other hand, (ii) it (more or less explicitly) encourages students to interpret our world through this dubious theory without further questioning.

- i. Our beginner students, to whom the concept of the marginal product (most of the time, labor) has been presented through examples with a fixed-proportion production function and excess quantities of all inputs (except labor), must surely be thinking that an additional unit of labor leads to something similar to the consequences of the withdrawal of one unit of factor, to which Hicks draws attention in the following passage:

²⁴ This is the one issue on which we would disagree with Moseley (2015).

²⁵ For it seems to be what is described in this passage (among others): “It often, and, indeed, commonly happens, that before No. 2, 3, 4, or 5, or the inferior lands are cultivated, capital can be employed more productively on those lands which are already in cultivation. It may perhaps be found, that by doubling the original capital employed on No. 1, though the produce will not be doubled, will not be increased by 100 quarters, it may be increased by eighty-five quarters, and that this quantity exceeds what could be obtained by employing the same capital, on land No. 3. In such a case, capital will be preferably employed on the old land, and will equally create a rent; for rent is always the difference between the produced obtained by employment of two equal quantities of capital and labour.” (Ricardo 2001 [1821], 42)

²⁶ This is the title of Mankiw’s (2015, 373) chapter 18; the title of chapter 19 in Krugman & Wells (2015, 543) is “Factor Markets and the Distribution of Income”; the title of chapter 14 in Pindyck & Rubinfeld (2018, 543) is “Markets for Factor Inputs”.

“If the proportions are fixed, then [...] the withdrawal of one unit will lead to a far greater diminution in the product that can fairly be attributed to that unit alone, since its removal put corresponding units of other factors out of action. If all the factors were paid according to their marginal products calculated in this second manner, their total pay would undoubtedly be far in excess of the value of the goods they produced. Which is absurd.” (Hicks 1932, 81)

For example, if returns to scale are constant, and if we choose units of measure so that the quantity of each of the n inputs needed to produce one unit of output equals 1, if the quantities of $n - 1$ inputs are sufficiently excessive, then an additional unit of the n^{th} input will lead to the production of an additional unit of output. If we calculate the marginal product of each input in this manner, for each unit of output produced, the total pay for inputs will be n times greater than the total product!^{27 28}

This ill-defined concept is also clearly inconsistent with the normative content of the marginal productivity theory of distribution, be it implicit — as when Mankiw speaks about the marginal product of a factor as “its marginal *contribution* to the production of goods and services” (Mankiw 2015, 390, emphasis added) — or explicit — as in McConnel, Brue and Flynn 2018, which claims: “In this **marginal productivity theory of income distribution**, income is distributed according to contribution to society’s output”, and “To each according to the value of what he or she creates” (McConnel, Brue & Flynn 2018, 325). For, when inputs are complements, their physical productive contributions are impossible to disentangle: it is obviously impossible to separate out the productive contribution of each input.²⁹

- ii. Explaining marginal product and factor prices through impossible examples presented as if they were real-world examples make students believe that the theory by and large reflects the world in which they live, whereas it is in fact almost impossible to find real-world examples of positive marginal (physical) product. This illusion is implicitly reinforced given that the marginal product theory of distribution generally appears at the end of a presentation where the invented impossible examples — “Siam Soups” (McConnel, Brue & Flynn 2018, 321 et sq.), an apple producer (Mankiw 2015, 374 et sq.), and so on — through which it is explained are entangled with boxes full of empirical data. But authors can also make it absolutely clear, as do Krugman and Wells in the following passage:

“It leads us to the **marginal productivity theory of income distribution**, which says that each factor is paid the value of the output generated by the last unit of that factor

²⁷ Of course, the problem remains even if only a few factors are considered. For example, Krugman & Wells (2015, 544) sometimes consider four of them (land, labor, physical capital or capital, human capital), and sometimes only two (labor and capital), which is still (at least) one too many.

²⁸ In the first decade of the 20th century, Hobson formulated this kind of argument — of excessive total factor payments — to criticize the marginal productivity theory of distribution, as for example in Hobson 1900; this is stressed by Mandler (1999, 21), who supports Hobson’s criticism using the example of a Leontiev production function.

²⁹ According to Pullen (2010), this theoretical problem, called the ‘disentanglement problem’, is well known, at least since Hobson’s criticism of the marginal product theory of distribution (for example in Hobson 1972 [1900] or Hobson 1969 [1910]), and persists — even if it is less obvious — when inputs are substitutes, since the increase in the total product that occurs after the employment of one additional unit of an input is seldom a monocausal phenomenon.

employed in the factor market as a whole — its equilibrium value of the marginal product.

To understand why the marginal productivity theory of income distribution is important, look back at Figure 19-1 [2015, 546], which shows the factor distribution of income in the United States [in 2013], and ask yourself this question: who or what decided that labor would get 66% of total U.S. income? Why not 90% or 50%?

The answer, according to the marginal productivity theory of income distribution, is that the division of income among the economy's factors of production isn't arbitrary: it is determined by each factor's marginal productivity at the economy's equilibrium. The wage rate earned by *all* workers in the economy is equal to the increase in the value of output generated by the last worker employed in the economy-wide labor market." (Krugman & Wells 2015, 555)³⁰

8. Conclusion

"[I]n economics nothing is more important than teaching, because, as Galbraith senior once observed, economics is primarily a teaching profession. This makes economics pedagogy a natural starting point for an analysis both of how economics went so horribly wrong and of how it might be made less a facilitator of human disaster in the future." (Fullbrook 2009, 17)

In presenting and explaining the marginal product, authors of beginners' textbooks generally use what we call impossible examples. In doing so, as we have shown, they instil misconceptions about neoclassical theory: since beginner students are never told and cannot possibly guess the nature of these examples, they can only acquire an ill-defined conception of marginal product, e.g. that the marginal product of an input designates the increase in the quantity of output that arises from one additional unit of this input, and "of course" from additional used quantities of all other inputs. Furthermore, not only is this ill-defined concept inconsistent with the diminution of what is alleged to be the marginal product of an input, but it leads to an absurd income distribution theory where, as Hicks put it, the sum of the incomes "would undoubtedly be far in excess of the value of the goods they produced" (Hicks 1932, 81). Consequently, the most motivated students — those who try hard to understand — must at least end up confused, if they don't indeed conclude that the theory is absurd or logically inconsistent: which would be a pity given the way this theory was originally built. Anyhow, there is no way for the beginner students to understand the theory that these examples are allegedly designed to present. In the chapters of beginners' textbooks dealing with the production process and the market for factors of production, they are taught nothing but a fake and inconsistent version of neoclassical theory.

Thus, our examination of beginners' textbooks shows that, unlike the use of other types of examples (Jallais 2018), the use of impossible ones is an aberration, at least from a pedagogical point of view.

Let us recall what Krugman and Wells (2015) claim:

³⁰ At the end of the chapter, they conclude that "the marginal productivity theory of income distribution is not a perfect description of how factor incomes are determined but that it works pretty well" (Krugman & Wells 2015, 562).

“Chapters build intuition through *realistic examples*. In every chapter, we use real-world examples, stories, applications, and case studies to teach the core concepts and motivate student learning. The best way to introduce concepts and reinforce them is through real-world examples; students simply relate more easily to them. [...] We use a fluid and friendly writing style to make concepts accessible and, whenever possible, we use examples that are familiar to students.” (Krugman & Wells, 2015, xix, our emphasis)

But instead of using realistic examples to explain why the marginal product of labor (or of any other input) is null whenever inputs are complements — which even a beginner student can easily understand — they choose examples with inputs which present a fake concept of the marginal product, hiding the fact that the marginal productivity theory of the demand for factors does not apply in the case of fixed proportion production functions.

Why? Why do authors of beginner textbooks choose to teach a fake and inconsistent version of neoclassical theory? What pedagogical or epistemological principle can support such a practice? Do they do this because they can't find any real-world examples that fit the concept or the law? If so, why not tell the students? Or why not at least replace these impossible examples by abstract or imaginary ones? The mystery remains.

Almost 70 years since Joan Robinson pointed this out, it seems that “the production function” is still “a powerful instrument of miseducation” (Robinson, 1953-4, 81). For this way of teaching economics is the exact opposite of what is generally expected from a university education, e.g. “the training of the student in critical thinking”³¹ as French economics professors put it when answering the students’ petition which initiated the “post-autistic” economics movement in June 2000 (Fullbrook, 2003, 1-9).

This particular way of excluding critical thinking — by teaching a fake and inconsistent version of the neoclassical theory of the competitive firm and of distribution through impossible examples — is quite different from the many other ways of excluding critical thinking that have been identified by these movements.³² Hence it seems to us that we have found a new “clue as to how both the economics profession and the public which it educates became so ignorant, misinformed, and unobservant of how economies work in the real world” — a clue which Fullbrook (2009, 17) invited us to seek³³ — since, on the one hand, beginner textbooks are the most widely used and, on the other, this pedagogical practice leads people to interpret our world by means of concepts and theories they are not aware of misusing.

Hence, until the relevant authors explain themselves, or until they change their way of teaching economic concepts, laws, and theories, we must continue to warn beginner students, following Hill and Myatt, that it “is not enough” to “study a conventional textbook (and to solve the multiple choice questions that supposedly test understanding of the subject)”; that they “need to read with a critical eye, and to note what is omitted [...] what is unsupported” (Hill and Myatt, 2010, 254); and, of course, that they must recognize what is inconsistent, whether this be in the explanation of a law, between a concept and an example, between two examples, or between two alleged pieces of the same theory.

³¹ “Two fundamental features of university education should be the diversity of the student’s degree course and the training of the student in critical thinking” (“The French Professor Petition”, in Fullbrook Edward (ed.), 2003. *The Crisis in Economics. The post-autistic economics movement: the first 600 days*. Routledge. London, p. 17)

³² See for example Fullbrook 2003 & 2009, Collectif Peps Économie 2013, <http://www.isipe.net/>, <http://www.rethinkeconomics.org/>, Reardon 2009 & 2015.

³³ Although we did not find this in the textbook by Mankiw that Fullbrook invited us to explore, it was present in all the best-ranked microeconomics textbooks for beginner students (including Mankiw’s).

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Annex

I. Goodreads' rankings

<https://www.goodreads.com/> (accessed 22 March 2019)

The top microeconomics books

1. Robert S. Pindyck & Daniel L. Rubinfeld, *Microeconomics*, 1989
2. Paul Krugman & Robin Wells, *Microeconomics*, 2004
3. Gregory Mankiw, *Principles of Microeconomics*, 1997
4. Jeffrey M. Perloff, *Microeconomics*, 1998
5. Andreu Mas-Colell, Michael D. Whinston, Jerry R. Green, *Microeconomic Theory*, 1995
6. Hal R. Varian, *Intermediate Microeconomics : A modern Approach*, 1987
7. A.Koutsoyiannis, *Modern Microeconomics*, 1975
8. Paul Samuelson & William Nordhaus, *Microeconomics*, 1989
9. Michael Parkin, *Microeconomics*, 1990
10. Robert Frank, *Microeconomics and Behavior*, 2006
11. Hal R. Varian, *Microeconomic analysis*, 1980

Popular microeconomics books ranking

This ranking is slightly different from the previous list. If we choose to narrow this list to academic authors and textbooks (with microeconomics in the title), the ranking goes:

1. Pindyck (Microeconomics)
2. Mas-Colell (Microeconomic theory)
3. Mankiw (Principles)
4. Varian (Intermediate)
5. Varian (Microeconomic analysis)
6. Nicholson (Microeconomic theory)
7. Koutsoyiannis (Modern Microeconomics)
8. Ichiishi (Microeconomic theory)
9. Kreps (Microeconomic foundations) : notes on the theory of choice is prior in the general ranking, and a course in microeconomic theory is 3 ranks below)
10. Jehle (Advanced microeconomic theory)

II. Bookauthority

<https://bookauthority.org/> (accessed 22 March 2019)

The best microeconomics books

BookAuthority ranking was narrowed (excluding business books or macroeconomic books which figured in the ranking)

1. Lee Coppock & Dirk Mateer, *Principles of Microeconomics* (ranked 16th again)
2. Campbell R. McConnell, Stanley L. Brue, Sean Masaki Flynn, *Microeconomics with Connect*
3. Felix Munoz-Garcia, *Advanced Microeconomic Theory : an intuitive approach with examples*
4. Paul Krugman & Robin Wells, *Microeconomics* (Canadian edition, with Iris Au & Jack Parkinson, ranked 13th ; ranked 17th)
5. R. Glenn Hubbard & Anthony Patrick O'Brien, *Microeconomics* (ranked again 14th)
6. Daron Acemoglu, David Laibson & John List, *Microeconomics*

7. Gregory Mankiw, *Principles of Microeconomics*, 7th edition (6th edition « with coursemate » is ranked 8th, and the 7th is ranked again 9th)
8. Astan Goolsbee, Steven Levitt, Chad Syverson, *Microeconomics*
9. Eric Chiang, *Microeconomics*
10. Neva Goodwin, Jonathan Harris, Julie Nelson, Biran Roach, *Microeconomics in context*

III. Amazon bestsellers

<https://www.amazon.com/> (accessed 22 March 2019)

The best sellers in microeconomics list (narrowed down to microeconomic textbooks):

1. McConnell, Brue & Flynn, *Microeconomics*
2. Mankiw, *Principles of Microeconomics*
3. Krugman & Wells, *Microeconomics*
4. Pindyck & Rubinfeld, *Microeconomics*
5. Coppock & Mateer, *Principles of microeconomics*
6. Cowen & Tabarrock, *Modern Principles of Microeconomics*
7. Frank, *Principles of microeconomics*
8. Chiang, *Microeconomics : Principles for a Changing World*
9. Varian, *Workouts in Intermediate Microeconomics*
10. Baumol, *Microeconomics : Principles and Policy*
11. Mas-Colell, Whinston & Green, *Microeconomic Theory*
12. Goolsbee, *Microeconomics*

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The 1-2-3 toolbox of mainstream economics: Promising everything, delivering nothing

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Abstract

We write this essay for both lay readers and scientists, though mainstream economists are welcome to enjoy it too. Our subject is the basic toolbox of mainstream economics. The most important tools in this box are demand, supply and equilibrium. All mainstream economists – as well as many heterodox ones – use these tools, pretty much all the time. They are essential. Without them, the entire discipline collapses. But in our view, these are not scientific tools. Economists manipulate them on paper with impeccable success (at least in their own opinion). But the manipulations are entirely imaginary. Contrary to what economists tell us, demand, supply and equilibrium do not carry over to the actual world: they cannot be empirically identified; they cannot be observed, directly or indirectly; and they certainly cannot be objectively measured. And this is a problem because science without objective empirical tools is hardly science at all.

1. Introduction

Our purpose in this paper is not to criticize demand, supply and equilibrium as such, but to show that, right or wrong, *these tools do not translate into actual science*.¹

We begin in Section 2 with what we call the 1-2-3 toolbox of mainstream economics. Mainstream economists claim that the key tools in this box – namely, demand, supply and equilibrium – explain virtually any and every market. We argue they do not. In Section 3, we illustrate how in practice these tools produce baffling if not contradictory results and suggest they merit closer inspection. In Sections 4, 5 and 6 we offer a clean-slate outline of demand, supply and equilibrium analysis, show the price and quantity history of the U.S. shoe market, and illustrate how mainstream economists would use their 1-2-3 toolbox to explain it. Their explanation, though, is deeply problematic, and for the simplest of reasons: nobody, including economists, has any idea what demand, supply and equilibrium look like!

As we explain in Section 7 and 8, the demand and supply curves express the *intentions* of buyers and sellers, and these intentions are unknowable to outsiders (and sometimes even to those who supposedly possess them). In practice, the best economists can do is estimate demand and supply *indirectly* – and that does not work either. The first method, which we examine in Section 8, is to interview buyers and sellers. On the face of it, this method might seem sensible, but a deeper look shows its results are impossible to assess and often nonsensical. The second method is to estimate demand and supply curves econometrically, based on actual price and quantity data. In Section 9 we show that this method too runs into the wall. Demand and supply regressions, no matter how fancy, are

¹ Perhaps the most accessible yet rigorous critique of mainstream economic theory is Keen (2011). For our own bit, see Nitzan and Bichler (2009: especially Chs. 5 and 8).

tautological: they assume what they seek to prove. And that is hardly the end of it. Econometric estimates are only as good as the econometric models they are based on, and, as we show in Sections 10 to 12, these models – and therefore the estimates they generate – are virtually all bad (though nobody can say how bad, because the ‘true’ demand and supply curves, assuming they exist, are unknowable).

So, all in all, the 1-2-3 toolbox, even if perfectly effective on paper, is virtually useless in practice. It spews out tons of estimated coefficients, but these estimates have no demonstrable relation to the demand, supply and equilibrium they presumably represent. For all we know, these estimates are no more than ghosts in the minds of the estimators. And, as Section 13 illustrates, these ghost-like estimates often end up spread all over the place. On these counts, mainstream economics is not even close to being a science.

2. Demand, Supply and Equilibrium: The 1-2-3 Toolbox

The best place to start is at the beginning. Let’s look at how Gregory Mankiw, author of the best-selling introductory textbook, *Principles of Economics* (2018), introduces the subject to his first-year students. First, there is little pep talk:

“When a cold snap hits Florida, the price of orange juice rises in supermarkets throughout the country. When the weather turns warm in New England every summer, the price of hotel rooms in the Caribbean plummets. When a war breaks out in the Middle East, the price of gasoline in the United States rises and the price of a used Cadillac falls. What do these events have in common? They all show the workings of supply and demand.” (65)

‘Supply and demand’, says Mankiw, ‘are the two words economists use most often – and for good reason’:

“Supply and demand are the forces that make market economies work. They determine the quantity of each good produced and the price at which it is sold. If you want to know how any event or policy will affect the economy, you must think first about how it will affect supply and demand.” (65-66)

Next, he introduces the equilibrium of these two forces:

“At the equilibrium price, the quantity of the good that buyers are willing and able to buy exactly balances the quantity that sellers are willing and able to sell. The equilibrium price is sometimes called the market-clearing price because, at this price, everyone in the market has been satisfied: Buyers have bought all they want to buy, and sellers have sold all they want to sell.” (76-77)

And then, when all is said and done, he delivers the ‘natural’ punchline:

“The actions of buyers and sellers naturally move markets toward the equilibrium of supply and demand.” (77)

Most economists will recognize this outline in their sleep: it is the neoclassical doctrine they teach and make their students rehearse. The uninitiated reader, though, may need a little explication, so here is a short outline.

The ideal neoclassical economy is a natural construct made up of numerous utility-maximizing agents. These agents are independent of each other, autonomous and rational. They have unlimited desires for hedonic pleasure, which they derive from consuming goods and services, but they have only limited means – or resources – to satisfy these desires. The difference between what they crave for and what they can afford generates ‘scarcity’.² Scarcity is inherent and permanent, so agents are compelled to produce, sell and buy more and more commodities without end. Their individual buying and selling desires-turned-intentions can be aggregated into market demand and supply curves. These curves interact in the market. Their interaction, mediated by the ‘invisible hand’ of pure competition, causes them to equilibrate. Equilibrium is a natural miracle that kills two birds with one stone: it generates the maximum attainable pleasure for market participants; and it answers the key question that economists never tire of asking: how much gets bought and sold, and at what price?

This is the pristine setup. The actual world, of course, is never as pure as mainstream economists would like it to be. So, when necessary, they tack onto their ideal economy a whole slew of real-life distortions – from evil monopolies and perk-hungry labour unions, to arbitrary government regulations and unanticipated global shocks, to partial or wrong information, irrational calculations and capricious habits, among other ills. And yet – and this point is key – whatever the distorting menace, demand, supply and equilibrium can handle it. Or so we are told.

Mankiw’s 866-page doorstopper articulates this confidence with fervour. The book mentions the words demand, supply and equilibrium virtually on every leaf, and more than once. The most popular of the three is demand: it appears no less than 2,558 times – roughly 3 times per page. Supply is less frequent; it is mentioned only 1,985 times, or 2.3 times per page. The least common is equilibrium, which the book uses only 669 times, or 0.8 times per page.

These repetitions are typical. Table 1 compares the per-page occurrence of each term across five leading introductory textbooks: Bade and Parkin’s *Foundations of Economics* (2018), Case, Fair and Oster’s *Principles of Economics* (2012), Krugman and Wells’ *Economics* (2015), Mankiw’s *Principles of Economics* (2018) and Samuelson and Nordhaus’ *Economics* (2010).

Remarkably, the per-page frequency of each word is almost identical across the five textbooks (note the tiny standard deviations in the bottom row).

² We use scare quotes often in the paper, and for a reason: we find many concepts economists take for granted deeply problematic.

Table 1: Occurrences per Page of the Words ‘Demand’, ‘Supply’ and ‘Equilibrium’ In Five Introduction-to-Economics Textbooks

Textbook	Demand	Supply	Equilibrium
Bade and Parkin	2.8	1.6	0.9
Case, Fair and Oster	2.9	2.0	1.0
Krugman and Wells	2.7	2.0	1.0
Mankiw	3.0	2.3	0.8
Samuelson and Nordhaus	2.9	1.9	0.9
Average of the Five Textbooks	2.9	2.0	0.9
<i>Standard Deviation (% of the Average)</i>	<i>3%</i>	<i>11%</i>	<i>8%</i>

Note: Occurrences are computed from the full PDF versions of the following books:

1. Bade, Robin, and Michael Parkin. 2018. *Foundations of Economics*. 8th ed. New York, NY: Pearson. (996 pages)
2. Case, Karl E., Ray C. Fair, and Sharon M. Oster. 2012. *Principles of Economics*. 10th ed. Boston: Prentice Hall. (819 pages)
3. Krugman, Paul R., and Robin Wells. 2015. *Economics*. 4th ed. New York, NY: Worth Publishers, a Macmillan Education imprint. (1,172 pages)
4. Mankiw, N. Gregory. 2018. *Principles of Economics*. 8th ed. Australia and Boston, MA, USA: Cengage Learning. (866 pages)
5. Samuelson, Paul A., and William D. Nordhaus. 2010. *Economics*. 19th ed. Boston: McGraw-Hill Irwin. (745 pages)

If these frequencies are common to other textbooks, we can refer to demand, supply and equilibrium as the economists’ 1-2-3 *toolbox*. Equilibrium is the numeraire, with roughly one appearance per textbook page; supply is second in line with two; and demand leads the pack with nearly three.

Some may see these repetitions as a sign of confidence, but to us they look rather suspect. Unlike organized religion, science does not require reiterations. Instead, it lets logical argument and empirical evidence speak for themselves. So why don’t mainstream economists do the same? Why do they constantly repeat and praise the power of their 1-2-3 toolbox? Is there something wrong with this toolbox? Do they have something to hide?

3. Because and Despite in the Oil Market

In everyday discourse, the 1-2-3 toolbox is usually taken for granted. For most people, the ‘laws of demand and supply’ seem self-evident, even if they cannot clearly articulate them. They find it sensible that ‘scarce’ things rise in price while ‘abundant’ ones fall. And yet, in practice, few can pin down these seemingly self-evident principles. Table 2 illustrates this difficulty in the global oil market.

Table 2: Because and Despite in the Oil Market

Saudis expect oil price drop because of glut (<i>The Globe and Mail</i> , 8/1/1980) Iran raises oil price, despite signs of a glut (<i>The Washington Post</i> , 28/3/1980)
Oil price rises continue despite governments release of strategic petroleum reserves (<i>Independent</i> , 28/9/1990) Oil price falls again as Gulf hopes rise (<i>The Times</i> , 3/10/1990)
Oil price rally continues despite bearish signals (<i>Reuters</i> , 19/5/1994) Oil price up as Korean tensions rise (<i>The Hamilton Spectator</i> , 16/6/1994)
Oil price rally stalls as U.S. crude stocks rise (<i>Reuters</i> , 11/10/1995) Oil price firm despite news of Iraqi sales (<i>South China Morning Post</i> , 9/8/ 1996)
World oil price rise as Iraq rattles the market (<i>Reuters</i> , 6/11/1997) Oil price slides despite Iraq confrontation (<i>Reuters</i> , 10/11/1997)
Oil price on the rise as demand resumes (<i>The Scotsman</i> , 15/10/1999) Oil prices stabilise despite continued production uncertainty (<i>AFP</i> , 13/3/2000)
Oil price falls as expectations grow of OPEC production increase (<i>AFP</i> , 20/3/2000) Oil price rises despite OPEC output increase (<i>AFP</i> , 22/5/2000)
Oil price hits 10-year high as traders bank on OPEC failure (<i>National Post</i> , 5/9/2000) Oil price edges back despite production lift (<i>The Evening Standard</i> , 12/9/2000)
US oil price soars as stocks fall (<i>Irish Times</i> , 22/8/2001) US heating oil price off despite stock fall (<i>Platts Oilgram News</i> , 19/9/2001)
Oil price close to record high as output rise has little effect (<i>Independent</i> , 20/5/2008) Oil price fall continues despite escalation of Georgia-Russia conflict (<i>Global Insight Daily Analysis</i> , 12/8/2008)
Oil price closes in on \$100 a barrel as global demand continues to rise (<i>The Independent</i> , 13/1/2011) Oil price rises despite inventory increase (<i>Xinhua News Agency</i> , 26/1/2011)
Oil price to hit seven-year high as global supplies fall (<i>Independent</i> , 30/5/2016) Oil price decline despite no oversupply (<i>Dow Jones Institutional News</i> , 11/8/2016)
Oil price continues to rise as Saudis see output curbs into 2018 (<i>ArabianBusiness.com</i> , 22/5/2017) Oil price extends drop to 7-month lows, despite OPEC cuts (<i>The Canadian Press</i> , 21/5/2017)
Oil price decline as Iran sanctions fears ease (<i>Xinhua News Agency</i> , 16/7/2018) Oil prices rise despite US-China trade war fears (<i>express.co.uk</i> , 21/7/2018)
Oil price fall amid fears over China trade war slowdown (<i>Investing.com</i> , 9/12/2019) Oil price rise muted in 2019 despite sanctions, supply cuts, attack in Saudi Arabia (<i>Reuters</i> , 30/12/2019)
WTI oil price tumbles as US inventories rise (<i>AFP</i> , 24/5/2020) Price of crude oil stabilizes despite unexpected rise in U.S. oil inventories (<i>CE NoticiasFinancieras</i> , 23/12/2020)

Source: Dow Jones Factiva

Oil is a highly standardized commodity whose price, many believe, moves up and down with its scarcity: when sellers want to sell less than buyers want to buy, scarcity increases and so does the price; and conversely, when sellers want to sell more than buyers wish to acquire, scarcity and price decline. But as Table 2 demonstrates, this common sense fails often.

The table pairs headlines that are less than a year apart. In each pair, one headline notes that the price of oil obeys the laws of demand and supply, while the other indicates it disobeys them. For example, the first headline might note that the price increased *because* of a glut, while the second, posted a couple of months later, might state that it fell *despite* the glut. Or one headline will note that the price fell because OPEC increased output, while the other will observe that it rose despite the higher output.

An economist might dismiss this table as a low-brow critique. After all, journalists seek drama, so they often cut corners, neglecting to mention other possible explanations. Nonetheless, the ease at which newspapers can use and abuse the same explanation suggests that maybe the 1-2-3 toolbox is not as omnipotent as economists want us to believe.

4. Demand, Supply and Equilibrium: The Essentials

To examine this possibility, let's begin with the clean slate shown in Figure 1. This is probably the most famous graph in economics, and all economists know it by heart. But for our non-economist readers, we describe it briefly in the remainder of this section, just to make sure everyone is on the same page.

The graph pertains to a given commodity (such as shoes, carrots, or microchips), a particular market boundary (for instance, Esquerra de l'Eixample in Barcelona, the city of Beijing, or the United States as a whole) and a specific period (say as a day, a month, or a year).

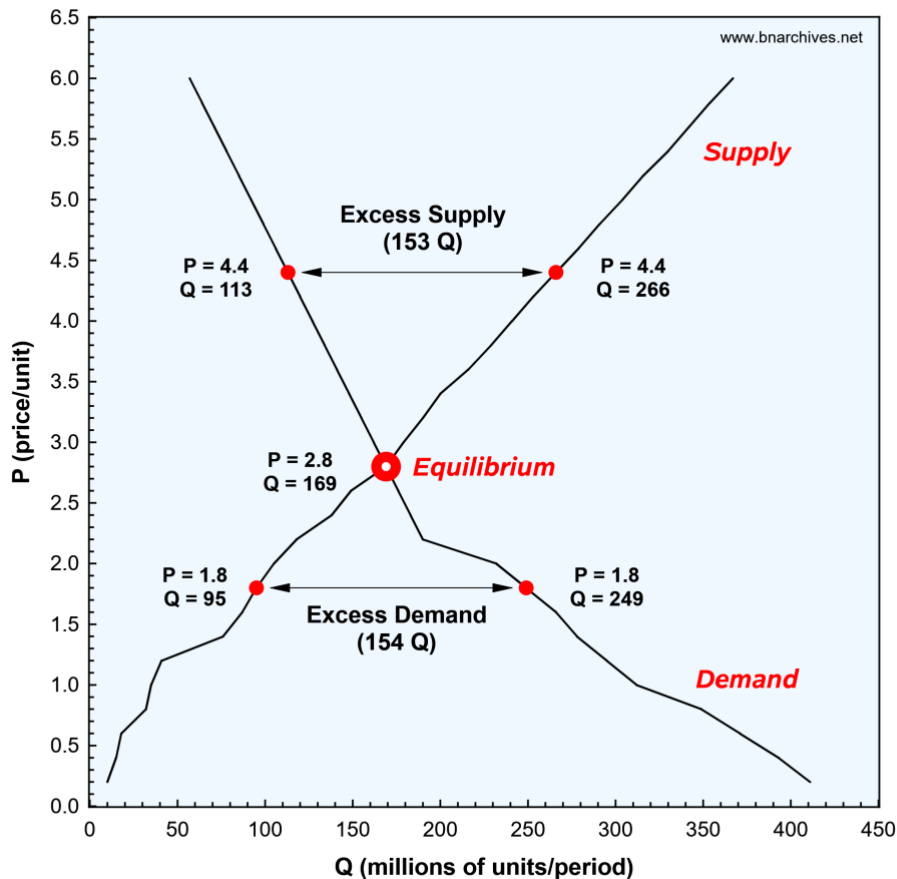
The vertical axis shows the price of the commodity, P . This price is expressed in 'real terms', meaning it is measured not in nominal \$, but relative to the average \$ price of other commodities. To compute it, the economist will divide the commodity's \$ price by a \$ price index of other commodities, so the result P is a pure number.³ The horizontal axis shows the physical quantity of the commodity, Q – in this case, in millions of units – demanded and supplied during the period.

To illustrate with a concrete example, pretend that this chart shows the U.S. shoe market in 1995. The vertical axis will measure the 'real' price of U.S. shoes in 1995 – i.e., their \$ price relative to the average \$ price of all other U.S. consumer goods and services sold and bought in that year. The horizontal axis will show the overall number of shoes that were demanded and supplied in the United States in that same year.

Inside the chart we see two curves. The one going down from the top left to the bottom right is the demand curve. This line expresses the total desires of U.S. consumers to buy shoes at alternative prices, *all else remaining the same*.

³ For those in the know: the result is a pure number only if we can express all commodities in one universal unit, such as the 'utils' they generate or the 'SNALT' (socially necessary abstract labour) they take to produce. There is no such universal unit, of course, but economists pretend it exists anyway.

Figure 1: Supply, Demand and Equilibrium



The expression 'all else remaining the same' is here to remind us that the desires of consumers to buy the commodity in question – in this case, shoes – are determined not only by its relative price, but also by other factors. These factors include, among others, the number of consumers in the shoe market, their average income and the way in which this income is distributed among them, the relative prices of substitutes (like cloth to wrap your feet with?) and of complementary commodities (such as socks, shoelaces and shoe polish) and, of course, the very tastes of consumers (an umbrella term denoting the general structure of the demand curve for shoes as well as for all other commodities).

Now, just like the commodity's price, these factors too tend to change. But here is the thing: according to economists, to isolate the *specific* effect of price on quantity demanded, we must assume – at least analytically – that these other factors are all unchanged. And since everything other than prices and quantities (and the odd parameter in a more complex inquiry) is assumed unaltered, this type of analysis can never be applied to actual time. It works only in hypothetical time. In actual time, price changes are always accompanied by changes in numerous other things. In hypothetical time, we can simply freeze all those other things. Economists call this synthetic type of analysis 'comparative statics'.

The line rising from the bottom left to the top right is the supply curve. This line describes the desires of market sellers to sell the commodity at alternative prices – again, all else remaining the same. Here too, the desires to sell are affected by price as well as other determinants. These determinants include factors such as the number of potential shoe suppliers, the cost of the different factors of production (the machinery used up in production, labour, electricity, leather, nails, rubber, glue, etc.), the level of knowhow, the available fixed equipment, the relative prices of other commodities that shoe sellers can produce and/or sell (for example, leather coats, upholstery, plastic and rubber products) – and, finally,

the way in which sellers mix these factors with profit considerations to generate their final selling intentions. And here too economists insist on comparative statics – in other words, on assuming, at least analytically, that all factors other than price remain fixed.

The intersection of demand and supply is the point of equilibrium. Economists like to think of this point as *both desired and stable*. It is desired because, when the price is 2.8 per unit, buyers can buy and sellers can sell exactly what they can afford and want to – namely 169 million units. And it is stable, first, because at this price there is no reason for either buyers or sellers to change their actions; and second, because if for some odd (read exogenous or irrational) reason the price deviates from equilibrium, market forces will immediately kick in, forcing it to converge back to that position.

For example, if the price happens to be 4.4 instead of 2.8, there will be an ‘excess supply’ of 153 million units. The excess occurs because, at that price, sellers will want to sell 266 million units – which is more than the 113 million units buyers will want to buy. Unlike equilibrium, excess supply is inherently unstable, or so we are told. Sellers, goes the argument, find themselves stuck with undesired inventories of shoes, so they start underbidding each other by lowering their price. As the price declines – assuming all else remains the same – buyers buy more and more. This process of shoe sellers lowering the price and shoe buyers increasing their quantity demanded will continue till the price reaches 2.8 per unit. No one knows how long this comparative-static process might take to complete. It can be instantaneous, stretch over ten years, or take any other period to terminate (all counted in units of hypothetical time). But once done, sellers and buyers will exchange exactly the quantities they wish to. And given that everyone is again satisfied, stability is happily restored.

The opposite situation is triggered by ‘excess demand’. For example, if the price happens to be only 1.8 per unit, buyers will want to buy 154 million shoes over and above what sellers would like to sell. Naturally, frustrated buyers who cannot buy the number of shoes they want at that price will compete by bidding it up. And as the price rises – all else remaining the same – sellers will supply more. The updrift will continue – again, nobody knows for how long – till the price of shoes reaches 2.8 per unit. At that point agents will again be as happy as they can be given the circumstances, and equilibrium will be safely reinstated.

Now, so far, we looked at how demand, supply and equilibrium determine a specific combination of price and quantity. But what causes prices and quantities to *change*? Here too the answer comes right out of the 1-2-3 toolbox – though this time the explanation is a bit different. Previously, we assumed that ‘all other conditions’ – from tastes, to the prices of other commodities, to incomes, distribution, cost and technology, etc. – remain fixed so that demand, supply and equilibrium stay put. But when one or more of these ‘other conditions’ varies, the result is that demand, supply or both shift and the point of equilibrium changes.

According to mainstream economists, the general principles here – which they label the ‘laws of supply and demand’ – are simple and elegant. When the demand curve shifts up or down along a fixed upward-sloping supply curve, quantity and price will move in the *same* direction – either up or down. Conversely, when the supply curve shifts either up and to the left or down and to the right along a fixed downward-sloping demand curve, quantity and price will change in *opposite* directions – with one going up and the other down. And when both supply and demand shift, the movements of price and quantity will depend on the *relative extent* to which the two curves change. And that is pretty much all there is to it.

Neoclassicists celebrate this theoretical parsimony. Markets, they point out, are incredibly complex, yet much of this complexity can be sorted out with their simple 1-2-3 toolbox. No wonder this box is the Holy Grail of economics. With just three tools – demand, supply and equilibrium – it determines the

quantities of sales, purchases and prices, along with plenty in-between. It can explain – with variations, modifications and distortions if necessary – the prices and quantities of every commodity, in every period, in every society. There is virtually no market it cannot account for.

There is only one little caveat. The 1-2-3 toolbox works only on paper. In practice, it accounts for virtually nothing.

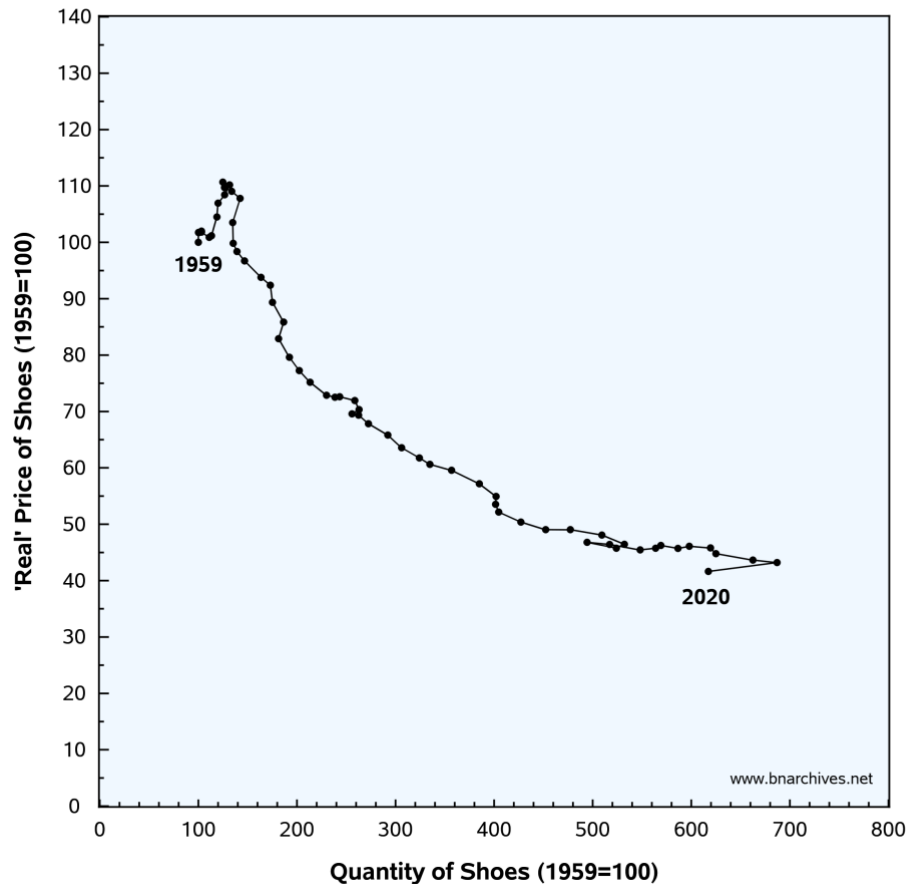
5. The U.S. Market for Shoes

To see why, consider Figure 2. The chart shows the average annual ‘real’ prices and quantities of shoes in the United States from 1959 to 2020. The horizontal, quantity axis depicts ‘real’ consumer expenditures on shoes – i.e., dollar spending corrected for changes in shoe prices. This is how economists approximate aggregate quantities. The vertical axis shows the ‘real’ (read relative) price of shoes – namely, the price index for shoes divided by the price index for all consumer expenditures. Both magnitudes are normalized with 1959=100 (meaning that the price and quantity series are each divided by their respective magnitudes in 1959 and multiplied by 100).

Now, the shoe market is not exactly the incarnation of ‘perfect competition’. The sell side, particularly the sport-shoe segment, is dominated by giants such as Nike, Adidas, New Balance, Asics and Kering (Puma). On the buy side there are many millions of buyers – but then, advertisement and other forms of conditioning serve to distort their ‘information’ and mess up their ‘sovereignty’ and ‘rationality’. There is also ‘government intervention’ in the form of regulation, taxation, tariffs, quotas and other chronic ills. But, for argument’s sake, let us ignore these minor glitches and assume that this market is purely competitive to the letter.

With this assumption in mind, what should we make of the time series shown in the chart? We can see that the quantities of shoes sold and bought in the United States have grown over time (indicated by the annual observations drifting to the right). We can also see that their ‘real’ prices relative to other consumer commodities have declined (shown by the observations edging downward). But what caused these long-term movements?

Figure 2: Prices and Quantities of Personal Consumption Expenditures on Shoes in the United States



Note: The price of shoes is expressed relative to the price of all personal consumption expenditures (both measured as indices). The quantity of shoes shows 'real personal consumption expenditures on shoes and other footwear'.

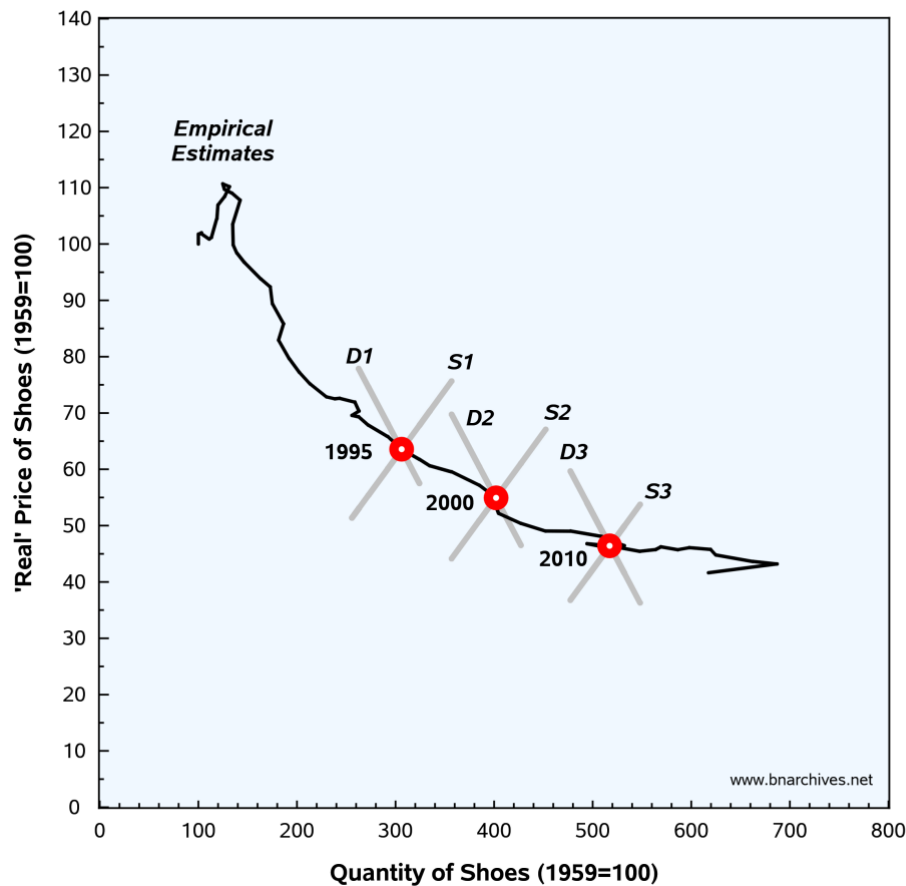
Source: U.S. Bureau of Economic Analysis through IHS Markit (series codes: JQCNCOSOSFR for the quantity of shoes and other footwear, JPCNCOSOSF for the price index of shoes and other footwear, and JPC for the price index of all personal consumption expenditures).

6. The 1-2-3 Toolbox in Action

The standard economic explanation is offered in Figure 3. As Figure 2, this chart too shows the prices and quantities in the U.S. shoe market for 1959-2020. But here we do more: we use the 1-2-3 toolbox to explain why these prices and quantities have changed.⁴

⁴ Incidentally, the U.S. shoe market was one of the first markets to which economists 'fitted' a statistical demand curve – sort of. See von Szeliski and Paradiso (1936).

Figure 3: The 1-2-3 Toolbox Applied to the U.S. Shoe Market



Note: The price of shoes is expressed relative to the price of all personal consumption expenditures (both measured as indices). The quantity of shoes shows 'real personal consumption expenditures on shoes and other footwear'.

Source: U.S. Bureau of Economic Analysis through IHS Markit (series codes: JQCNCSSOSFR for the quantity of shoes and other footwear, JPCNCSSOSF for the price index of shoes and other footwear, and JPC for the price index of all personal consumption expenditures).

According to this explanation, each annual observation is a point of equilibrium between demand and supply. We illustrate this claim for three different years: 1995, 2000 and 2010. In 1995, the equilibrium was determined by the intersection between the demand curve $D1$ and the supply curve $S1$. During that year, 'all else' supposedly remained the same, so equilibrium stood still. In time, though, the 'all else' factors changed, and by 2000 these changes caused demand and supply to shift to new positions, indicated by $D2$ and $S2$, respectively. The empirical observation for 2000 emerged from the equilibrium of these new curves. And the same happened in subsequent years. Conditions continued to change, and the curves shifted with them. In 2010, demand and supply moved to $D3$ and $S3$, intersecting in yet another equilibrium – which, as you correctly guessed, produced the observed price and quantity for that year.

Since the explanation is general, it applies to every observation in the 1959-2020 series: as conditions and tastes changed, demand and supply shifted; with these shifts, each new equilibrium gave rise to a new empirical observation; and when we connect the dots, we get the entire time series. And this simple logic is by no means restricted to the U.S. shoe market of 1959-2020. It can explain the market for any commodity, anywhere, anytime. It is universal. The only question is whether there is any science to it.

7. Where Do Demand and Supply Come From?

The title of this section may sound ironic, but the question is genuine. Note that, in simulating the neoclassical explanation, we drew each pair of demand and supply curves so it intersected exactly at the designated observation. But how did we know what these curves looked like and that they indeed equilibrated at those designated points?

The answer is we didn't. Just like the neoclassicists, we have no idea what actual demand and supply curves look like. Just like the neoclassicists, we simply plotted them so that they intersected in the observations for 1995, 2000 and 2010. And like the neoclassicists, we did so because these intersections are consistent with the neoclassical doctrine.

But since our curves are imaginary, our explanation is irrefutable. We could have achieved the exact same accuracy – and irrefutability – by drawing magic rays through these observations, or by attributing their specific locations to the will of God.

When neoclassicists say that the time series in Figure 3 is the consequence of a rolling equilibrium between shifting demand and supply curves, they make a *theoretical proposition*. To assess this proposition scientifically, though, they must contrast it with *actual data*. Specifically, they must plot the actual demand and supply curves, show the way in which these actual curves shifted over time, demonstrate the exact points where they intersected, and compare these intersections with the time series shown in the figure. The problem is that none of these things can be done.

The five introductory textbooks listed in Table 1 offer no help in this matter. In fact, they skip it altogether. Their combined 4,600 pages mention demand, supply and equilibrium roughly 26,000 times, and they embellish these repetitions with thousands of multicolour pictures, tables and graphs. Yet none of these tomes provides a single picture, table or graph of an *actual* demand curve, supply curve, or equilibrium. Not even one! They also make no mention of how actual demand and supply curves can be estimated to start with. Lastly and most importantly, they fail to indicate that, for demand, supply and equilibrium to be scientifically useful, they must *exist* in the actual world and be *known* to economists. Perhaps the authors of these textbooks feel that these issues are better left to second-tier courses in 'managerial economics', where practical training and endless drill crush the remaining critical faculties of their fall-in-line students.⁵

⁵ Managerial economics textbooks like Thomas and Maurice (2016), Samuel, Marks and Zagorsky (2021) and Baye and Prince (2021) teach 'as-is' practical techniques. They use the word 'theory' often, but mostly as an uncontested template of 'how the world works'. Words such as 'debate', 'critique' and 'disagreement' are rarely if ever mentioned, and when they are mentioned, it is always in passing.

8. What Can Economists Learn from Interviewing Agents?

So how can we find out what actual demand, supply and equilibrium look like? To reiterate, what we look for are *desires*. In their pristine incarnation, supply and demand curves represent the resource-constrained desires of fully autonomous, well-informed and totally rational utility-maximizing agents to buy and sell the commodity at alternative prices in a given market during a particular period of time, all else being the same.

Strictly speaking, the only way to know these desires – and the market demand and supply curves they give rise to – is to have direct access to the mind of each actor, find out their preferences and sum them up.⁶ In the late nineteenth century, some economists fantasized having a ‘hedonimeter’ – a contraption that would measure these preferences directly from the agent’s mind or body.⁷ So far, these fantasies have not materialized, and until they do, the only alternative is to ask the agents and hope they’ll answer honestly and accurately.

This alternative, though, is fraught with difficulties. To start with, markets can be very big. In the case of the U.S. shoe market, for example, we would have to ask hundreds of millions of buyers as well as hundreds or thousands of producers and retailers – and we would have to do so again and again every time preferences or market conditions change (which can be every month, every week, or even every day). Obviously, this is impractical even for a single commodity like shoes and inconceivable if we need to do the same for every commodity.

So how about interviewing only a small sample? Companies and economists conduct such interviews regularly, and assuming their samples represent their respective populations correctly, these interviews can be used to specify broader market demand and supply curves.

Or can they?

We admit having no idea how many such interviews have been conducted. But let us err on the upside and guess that, over the past century, an average of one million have been made annually around the world. Our guess implies that, in the last one hundred years, economists accumulated raw material for specifying as many as 10^8 actual demand and supply curves.

This number may look very large but compared to the total number of demand and supply curves out there it is rather miniscule. To see how small, consider the following equation for computing the total number of demand and supply curves:

$$1. \text{ number of demand and supply curves} = \text{number of commodities} \times \text{number of markets} \times \text{number of time periods}$$

⁶ Although this paper does not criticize neoclassical theory as such, it is worth noting that, contrary to what economists tell their first-year students, all-else-remaining-the-same market demand curves *cannot* be aggregated from individual demand curves. The reason is that a change in the price of the commodity – i.e., a movement *along* the individual demand curves – will alter the absolute and relative incomes of consumers. And since income and its distribution are two of those ‘all other things’ that supposedly remain the same, this movement on the demand curve will cause the curves themselves to *shift*, thus violating the assumption of comparative statics. For a clear explanation, see Keen (2011: Ch. 3).

⁷ Francis Edgeworth (1881). See also David Colander (2007).

The overall number of commodities can be guesstimated by using the North American Industrial Classification System (NAICS) and the North American Product Classification System (NAPCS). These systems use a 10-digit coding for commodity types that can be further extended to account for specific items. This coding can accommodate billions of different commodities, so its order of magnitude is equal to or greater than 10^9 . Next, consider that these commodities are bought and sold in hundreds of thousands of different markets – from world and national markets, to regional markets, to city and special markets – and that each of these markets has its own curves. The total number of these markets might be in the order of 10^5 or more. Finally, because tastes and conditions change continuously, each commodity in any given market will have as many demand and supply curves as the number of tastes-conditions sets. And if tastes and conditions change daily, over a period of a century we will need to estimate each curve tens of thousands of times – so the order of magnitude in this rubric is 10^4 . Multiplying these three orders of magnitudes gives us 10^{18} actual demand and supply curves (of course, this calculation assumes that demand and supply are ontological entities to start with).

So even if we were able to derive perfectly accurate demand and supply curves from every interview ever conducted during the past century, the proportion of these derived curves out of the total would be only $1/10^{10}$ – or 0.00000001%. And this proportion would remain very small even if we overestimated the number of existing demand and supply curve several orders of magnitudes. Empirically speaking, then, the 1-2-3 toolbox is *almost entirely empty*.

Of course, the fact that interview-based estimates account for only a tiny proportion of existing demand and supply curves does not disqualify the underlying method as such. The important question is whether the method itself is reliable. And the answer to this question is that nobody can tell.

The main reason for this inherent limbo is that interview-based demand and supply curves can never be assessed, let alone verified. Recall that demand and supply estimates denote intended purchases and sales at alternative prices, and yet only *one* of those intentions – namely, the intention to buy and sell at the prevailing market price – can ever be contrasted with actual market data. The remaining intentions – i.e., the entire demand and supply curves less one point – never get expressed in the market, so there is no way to contrast them with actual price and quantity observations.⁸ In this sense, economists can never know whether to believe what buyers and sellers tell them. The demand and supply curves they construct – whether based on the entire population or only a small sample – can be spot on, pure rubbish, or something in between, yet there is no way to tell which one it is. And this inability to judge is only amplified when using samples, since the extent to which these samples represent the underlying population is also impossible to assess!⁹

And that is just for starters. Whether interviewing all agents or just a sample, economists need to formulate their questions accurately and clearly so that interviewees understand exactly what they are being asked, and this requirement too is rather taxing. In the case of the U.S. shoe market, for example, interviewers of buyers would need to specify all available shoe types, sizes and attributes (so agents know what is on offer) and remind their interviewed subjects that, in answering their questions, they

⁸ In some markets, such as electricity, institutional sellers and buyers submit offers and bids, and when these offers and bids are ranked by price, they resemble upward-sloping supply and downward-sloping demand curves (see for example, Shah and Lisi 2019: Figure 2, p. 245). But this is an optical illusion. The observations on these curves are not the *sum totals* of the quantities supplied by all sellers and demanded by all buyers at each given price. Instead, each observation is a single offer or bid made by a *different* seller or buyer – only that when these offers/bids are ranked by price, they give the false impression of upward-sloping supply and downward-sloping demand curves.

⁹ Election pollsters can gauge how well their samples represent the population by comparing their prediction with the election outcome. Demand and supply curve pollsters, dealing with intentions that never get expressed in the market, do not have that luxury.

must keep 'all things other than price unchanged'. These specifications can easily fill hundreds of pages, making everyone's head spin.

And that is just for shoes, which is a rather simple commodity. Imagine what it takes to specify the full attributes of commodities such as electronic gadgets, cars, houses and complex financial services. Note that the specification of all these other commodities – which could number in the billions – is relevant, indeed essential, for the shoe market. According to the neoclassicists, utility-maximizing agents determine their buying preferences *jointly for all commodities*, so not knowing the nature of all those other commodities will make utility-maximizing demand curves – including for shoes – impossible to specify.

Finally, economists need to assume – without ever knowing whether their assumption is right or wrong – that the interviewees answer honestly; that they indeed mean to follow their stated intention; and, most importantly, that they have clear buying intentions in the first place.

This last requirement may seem bizarre: is it not obvious that consumers have buying intentions? Well, as it turns out, generally they do *not*! To see why, try to recall the last time you answered – or even asked yourself – the question 'how many shoes do I intend to buy or sell at alternative prices, all else remaining the same'? Probably never. And how often have you asked and answered this question with respect to other commodities? Moreover, when you buy or sell commodities, how often do you stick to your earlier intentions, assuming you had any? The simple fact is that most people never spend time conceiving all-else-being-the-same price lists, let alone think of how they will adjust their quantity demanded and supplied as prices go up and down those lists. In this sense, most people – sorry, most 'agents' – *have little or no conception of their individual supply and demand*. If asked, they will simply answer off the top of their head.

In fact, even die-hard, fully informed utility maximizing wannabees will find it difficult to form consistent buying and selling intentions. A carefully designed experiment by Reinhard Sipple showed that, once the number of commodities exceeds a handful, consumers who otherwise seem perfectly 'rational' violate the most basic assumptions of neoclassical utility theory. The reason: they simply lose track of the rising number of commodities and the exponential increase in possible consumption bundles.¹⁰

In sum, the neoclassicists' claim that their 1-2-3 toolbox can explain any and every market in the world is somewhat of an empty boast. In practice, demand, supply and equilibrium do not – and cannot – explain any, let alone all, markets simply because they are *unknown*. They can be guessed through interviews – but interview-based curves are logically dubious, statistically inadequate and, most importantly, empirically unverifiable. In this sense, the 1-2-3 toolbox is not almost empty, it is *entirely empty*.

¹⁰ Reinhard Sipple (1997). For a highly readable explanation, see Keen (2011: 67-72).

9. Can Demand and Supply Curves be Derived from and Explain Market Data?

Informed readers – particularly economists – are likely to deem our last claim both pompous and ignorant. Economists, statisticians and businesspeople, they will point out, have been busy estimating actual demand curves, supply curves and equilibrium for over a century now, and these estimates, they will add, have been based not on flimsy interviews, but on hard market data and state-of-the-art econometrics. Bichler and Nitzan, they will opine, are spreading fake news. The economist's 1-2-3 toolbox is not empty at all. In fact, it is overflowing.

And this counterclaim is partly true. The literature that derives empirical demand and supply curves from actual market data is indeed voluminous. However – and here is the key point – there is no objective way to show that the estimates produced by this large literature, no matter how data-grounded and econometrically sophisticated, have anything to do with the *actual* market demand and supply curves (again, assuming these curves exist).

To explain this last claim, we need to distinguish *intentions* from *actions*. Market demand and supply curves express intentions. They quantify the desires of autonomous buyers to buy and sellers to sell the relevant commodity at alternative prices, all else remaining the same. By contrast, empirical demand and supply curves estimated from market data reflect action. They mirror actual buying and selling.

And here is the problem. Mainstream economists assume that market data represent equilibrated demand and supply curves. They then use market data to estimate these demand and supply curves. And finally, they use the estimated curves to explain the market data they come from. In short, they go in a circle.

To flesh out the circularity, recall that, in their textbooks, mainstream economists argue that the intentions-read-desires of buyers and sellers (demand and supply) explain their actions (buying and selling), and that these actions in turn drive market outcomes (prices and quantities). This process is shown in Sequence 1:

SEQUENCE 1:

intentions (demand, supply) → actions (buying, selling) → market outcomes (prices, quantities)

Yet, in deriving their market-based estimates they go in reverse. Sequence 2 below is identical to Sequence 1, with one exception: its arrows, instead of pointing right-ward, point left-ward. When following Sequence 2, economists begin from market outcomes, argue that these outcomes are created by the actions of agents, and conclude that these actions therefore represent the agents' intentions – i.e., their market demand and supply curves:

SEQUENCE 2:

intentions (demand, supply) ← actions (buying, selling) ← market outcomes (prices, quantities)

And here is where things fall apart. Economists are free to believe that both sequences are valid. But, because the second sequence *assumes* the first, it cannot be used to *prove it*. And yet that is precisely what mainstream economists do. They estimate demand and supply curves from market outcomes (Sequence 2) – and then use these derived demand and supply curves to explain the very market

outcomes their estimates come from (Sequence 1). In other words, they substitute a tautology for proof.¹¹

10. Regression in Action: Running on Empty

Since tautologies are always true, we expect them to be neat. But using market-based demand and supply curves to explain market data is anything but neat. In fact, it is rather messy.

The main reason for the mess is that econometrics is ill-suited for estimating processes that are unknowable, unstable and out of equilibrium. And since demand and supply curves denote desires that cannot be known, that change constantly, and that might not be acted upon, it is no wonder their estimation is fraught with difficulties.¹²

The basic model involves two regressions, one for the quantity demanded Q_d , the other for the quantity supplied Q_s :

$$2. \quad Q_d = f_d(P, x_1, x_2, x_3, \dots, x_n, u)$$

$$3. \quad Q_s = f_s(P, y_1, y_2, y_3, \dots, y_m, v)$$

For those unschooled in econometrics, the glossary for the equations is as follows. In both equations, the elements on the right-hand side are said to determine those on the left. P is the observed market price of the commodity; the x_i elements denote the n different conditions that determine the position of the demand curve as we saw in Section 4, and the y_i elements are the m conditions that set the corresponding position of the supply curve; u and v are the equations' error terms, representing the patternless white noise created by all excluded determinants (i.e., factors other than P and the x_i and y_i elements that do not appear in the equation but are impactful nonetheless); finally, f_d and f_s are the respective functional forms of the two regressions.

These regressions are supposed to help economists bypass the problem of comparative statics. As we saw in Section 4, comparative statics stipulates that in order to identify the relationships between price and the quantities demanded and supplied, we must assume that 'all else remains the same'. This assumption is necessary because, unlike movements *along* the demand and supply curves which occur because of changing prices, *shifts* of the curves themselves are affected by changes in factors other than price (namely, the x_i and y_i elements in the two equations, as well as tastes for demand and profit considerations for supply). If these other factors are fixed, the curves stay put. If they change, the curves shift. Now, when economists engage in pure theory, they pretend that these other conditions are frozen and concentrate solely on how price changes affect the quantity demanded and supplied. But the actual world is dynamic, and since its conditions vary constantly, it is difficult to disentangle their effects from those of price proper.

¹¹ Of course, economists might succeed in predicting out-of-sample prices and quantities. But even if they do, their success will not imply, let alone prove, that the estimated equations represent the underlying desires of buyers and sellers. It will simply mean that they found a good 'fit', and that the fit remained stable. The factors driving this fit could be many things *other* than the utility and profit-maximizing desires of autonomous agents.

¹² Some of these difficulties were pointed out, including by leading neoclassicists, almost a century ago – but generally to no avail. See for example Schultz (1923), Working (1927), Frisch (1933) and Stigler (1939).

This is where regression analysis comes to the rescue: it helps identify the *unique* impact of each of the right-hand factors on the left-hand variable of interest – in this case, the quantity demanded or supplied. And it does so by *simulating* the assumption of ‘all else being the same’ for every one of the right-hand side variables.

For example, applying Equation 2 to U.S. shoe-market data, the regression will estimate the unique effect on quantity demanded Q_d of changes in price P , as *well* as of changes in each of the x_i elements – including changes in the number of consumers, their average income, the distribution of their income, the prices of other commodities, etc.

But what appears neat on paper quickly gets scrambled in practice. The problem is that econometrics in general and regressions in particular are only as good as the assumptions on which they are based. If the assumptions are correct, so are the results. If they are wrong, the results are incorrect. And in the study of society – and of the economy generally and demand and supply particularly – the assumptions, and therefore the results, are virtually *always* wrong. Worse still, nobody can say exactly in what way they are wrong and by how much.¹³

Let us unzip these issues, beginning with the market’s demand and supply functions. As we saw in Section 8, economists cannot know these functions. Nonetheless, they specify them, and with great precision. Equations 2 and 3, for example, show very specific lists of determinants along with concrete functional forms. Do economists know that their specified equations are correct? Not in the least. They simply construct them as they see fit and estimate their coefficients.

Typically, they begin by choosing a functional form that suits their theoretical or practical fancy, populate it with a list of variables they like, input the necessary data, and, when everything is ready, hit the computer *Enter* key and examine the results.¹⁴ The initial estimates tend to be all over the place and are seldom consistent with their specifier’s hypothesis and preferences. But not to worry. The next step is to add new variables and perhaps delete some of the existing ones, massage and transform the data, tweak the period of the study and rerun the regression. If the new results still fall short, the process begins anew. These cycles can be long and tedious, but with enough permutations they tend to improve the results, often markedly.

Occasionally, though, these re-adjustments don’t do the trick. And when that happens, the next step is to alter the equation’s functional form. For the lay reader, the functional form is the concrete mathematical structure of the equation. It determines whether the equation is linear or nonlinear, its particular shape, whether it contains lagged or multiplicative variables, the nature of the coefficient associated with each variable, whether these coefficients are fixed or varying (i.e., a function of other variables), the relationships between the different variables, the properties of the error term, and so on. In the case of the demand curve, this functional form represents the desires of consumers – i.e., the way in which they translate the values of the right-hand-side variables into buying intentions. In the case of the supply curve, it denotes how sellers weigh costs and profits in calculating their desires to sell.

Of course, since economists know virtually nothing about the actual desires of buyers and sellers, they cannot know the actual functional forms of their demand and supply curves. So, they specify these

¹³ The debate over the adequacy of econometrics started with a famous exchange between John Maynard Keynes (1939) and Jan Tinbergen (1940) whose opposite arguments are yet to be reconciled. For an accessible assessment of the history, promises and travesties of econometrics, see Imad Moosa (2017, 2019).

¹⁴ For fancy and largely inaccessible tracts on how to specify empirical demand and supply functions, see for example Pollak and Wales (1992), Fisher, Fleissig and Seletis (2001) and MacKay and Miller (2019).

forms as they see fit – that is, in line with their theoretical inclinations, pragmatic dictates, or sheer fancy. And the fact that they can do so once, means that they can do it again and again until the resulting estimates are deemed adequate.

Now, there is nothing inherently wrong in specifying and changing the variables and functional forms of these two equations, and in doing so repeatedly. After all, science involves plenty of trial and error by sleepwalking thinkers. But since the actual market demand and supply reflect desires that can be anything, and since the functional forms and determinants of these desires are forever unknowable, the econometric estimates – whether generated by a single computer run or after a thousand re-specifications – will be correct only by an extremely improbable fluke. In other words, they will be wrong. Worse still, nobody can ever tell in what way they are wrong, in which direction, and by how much. The estimators, of course, will vehemently deny it, but these considerations suggest that their search for the elusive demand and supply curves is mostly running on empty.

11. Nonstationarity: When the Rules Change and Nobody Knows How

The final and perhaps most disturbing difficulties arise because the estimated processes are probably nonstationary and out of equilibrium. We look at these two difficulties in this and the next section.

Start from this fancy term, nonstationarity. Regressions, no matter how adaptive and dynamic, always end up imposing a *fixed* structure on reality. This imposition seems reasonable in the natural sciences, where it is common to assume that the ‘laws of nature’ do not change – or, in technical lingo, that they are stationary. Applying stationarity to the ‘laws of society’, though, seems rather presumptuous, if not silly. Society – including the economy – is not only in constant flux; it also alters its own governing principles. In other words, it is *non-stationary*.¹⁵

In fact, this nonstationarity lies at the very basis of neoclassical demand and supply. Mainstream economics glorify its autonomous agents. Agents are the basic building blocks of the economy, the sovereign masters whose rational choices generate the sacred ‘micro foundations’ of the entire discipline. But autonomy means freedom to change one’s own mind, alter one’s own desires, modify one’s own actions in relation to the broader world. And since neoclassicists insist that their agents are autonomous, it follows that demand and supply curves are doubly dynamic: not only do they *shift* because of external circumstances, they also *self-transform* their shape as agents alter their tastes and preferences.

In practical terms, this self-transformation means that the two curves can accommodate new variables and purge old ones, alter their coefficients and modify their overall mathematical structure, and that these changes will happen anytime agents change their mind. In short, demand and supply are inherently nonstationary – and to an unknowable degree to boot. And if, as the neoclassicists’ own theory implies, demand and supply can self-transform at any time, estimating them with fixed regressions – and arbitrary ones at that – is bound to generate even more meaningless results, if that were at all possible.

Neoclassical economists have addressed this nonstationarity mostly by assuming it away. On the demand side, economics Nobelist Milton Friedman conceded that wants ‘cannot be evaluated

¹⁵ For a neat visual comparison between coefficient estimates in stationary physics and nonstationary economics, see Moosa (2017: Figure 4.3, p. 82).

objectively' but recommended that economists 'take wants as fixed' anyway.¹⁶ George Stigler and Gary Becker, two other Nobelists, took a stronger, ontological stand, stipulating that tastes simply do not change (and that they are similar across agents). In their words: 'tastes neither change capriciously nor differ importantly between people'. Just like the Rocky Mountains, they posit, actual tastes 'are there, will be there next year, too, and are the same to all men'.¹⁷

On the supply side, the main solution is to enslave sellers to the fixed dictates of profit maximization. In this setup, autonomous sellers are reduced to mere algorithms, and since these algorithms simply translate objective costs to selling orders, nonstationarity disappears. The fact that, in practice, nobody knows what 'maximum profit' is, let alone how to achieve it, is usually neglected, or considered a non-issue.

To an outsider, insisting that demand and supply curves are stationary must sound feather-brained – if not deceitful, since it contradicts the agents' presumed autonomy. But neoclassicists have no choice here. They must assume stationarity. Otherwise, they cannot even pretend to be doing science.

12. Out of Equilibrium: All Bets are Off

And then there is the possibility that the world is out of equilibrium. So far, we followed mainstream economists in assuming that buying and selling take place at equilibrium prices, and that actual prices and quantities trace the temporal shifting of demand, supply and their intersection (Figure 3).

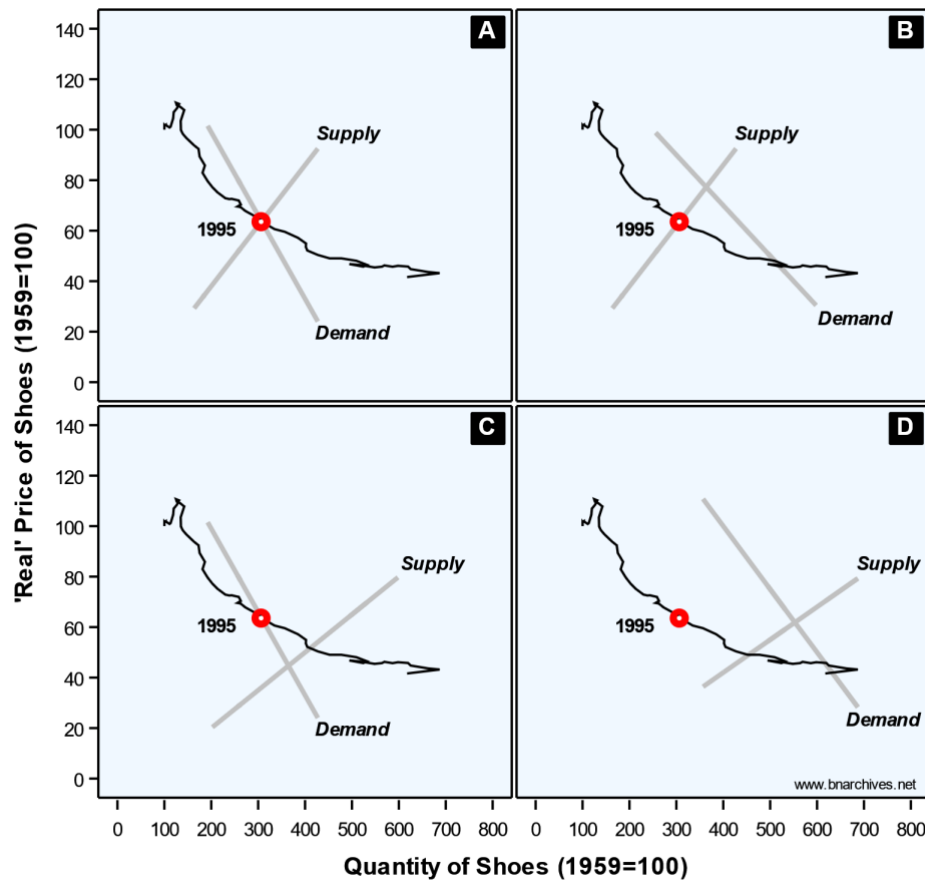
But this assumption is invalid. In fact, it is invalid according to the neoclassicists' own view. To see why, consider the four panels in Figure 4. Each panel reproduces the U.S. shoe market data from Figure 3. The panels also show different sets of hypothetical demand and supply curves for 1995. Panel 4-A represents the preferred neoclassical view, with the demand and supply curves intersecting at the market price and quantity for that year. But neoclassicists admit there are other possibilities.

One such possibility is shown in Panel 4-B, where the 1995 price and quantity sit on the supply curve but not on the demand curve, creating excess demand. An opposite configuration is illustrated in Panel 4-C, where the 1995 observation sits on the demand curve but not on the supply curve, leading to excess supply. And then there is Panel 4-D, which is doubly disturbing, because, here, the 1995 observation sits on neither curve. It just floats there, totally indifferent to the 1-2-3 toolbox.

¹⁶ Friedman (1962: 13).

¹⁷ Stigler and Becker (1977: 76).

Figure 4: Equilibrium?



Note: The 'real' price of shoes is expressed relative to the price of all personal consumption expenditures (both measured as indices). The quantity of shoes shows 'real personal consumption expenditures on shoes and other footwear'.

Source: U.S. Bureau of Economic Analysis through IHS Markit (series codes: JQCNCSSOSFR for the quantity of shoes and other footwear, JPCNCSSOSF for the price index of shoes and other footwear, and JPC for the price index of all personal consumption expenditures).

Now, we have already seen that even if markets are always in demand-and-supply equilibrium, there is no way to empirically identify the actual demand and supply curves underlying this bliss. But what are economists to do when they face *dis*-equilibrium as illustrated in Panels 4B and 4C, or even *non*-equilibrium as shown in 4D?

Well, one popular solution is to assume that demand and supply discrepancies are eliminated instantaneously, so that markets are practically always in equilibrium. But this assumption is just as good as the opposite one, namely that markets never equilibrate. And the thing is that, in practice, no one can tell, simply because, as of 2021, no one – including none of the profession's 89 Nobelists – ever managed to objectively identify an actual market equilibrium.

This inability means that, while econometricians think they trace actual demand and supply curves, or at least segments of these curves, in practice they are chasing ghosts. Without an objective yardstick, the likelihood that any specific market observation will sit on both the demand and supply curves (Panel 4A), or even on one of them (4B or 4C), is infinitely small. They most likely sit on neither (4D). And until

we can tell which is which, all bets are off. Demand and supply cannot be estimated, even approximately.

13. An Example

These cumulative difficulties have consequences. Economists prefer to ignore these difficulties, so usually the consequences remain buried. But occasionally they bubble up to the surface, and in this section, we illustrate what happens when they do.

Our focus is the fish market. Fish is an important source of food with global annual sales in excess \$160 billion, so there is plenty of interest in its demand curve. Also, according to many economists, the fish market is one of the few perfectly competitive markets left, so its demand curve should be relatively easy to estimate. Or so it seems.

Individual researchers are usually careful to report only consistent estimates and hide those that deviate too much from received convention (particularly if they yield the wrong slope). But they cannot control the estimates made by others, and when we compare various results, sometimes the differences are too large to ignore.

Table 3 illustrates this variance with estimates from 38 studies of the demand curve for fish conducted in different countries over more than half a century. The first column shows the type of fish, while the second gives the estimated price elasticity of demand.

For our lay reader, elasticity here measures the *relative* responsiveness of quantity demanded to a change in price (all else remaining the same), such that:

$$4. \text{ price elasticity of demand} = \frac{\% \text{ change in quantity demanded}}{\% \text{ change in price}}$$

If the demand curve slopes downward, a given change in price will cause the quantity demanded to move in the opposite direction. And since the numerator and denominator in Equation 4 will have opposite signs, the elasticity will be negative.

The magnitude of the elasticity measures responsiveness. If it is smaller than -1 , the relative change in quantity demanded is greater than the relative change in price, meaning the demand is elastic (i.e., responsive). For example, if a $+5\%$ change in price, all else remaining the same, leads to a -10% change in quantity demanded, the price elasticity of demand is -2 . By contrast, if the elasticity is greater than -1 , the relative change in quantity demanded is smaller than the relative change in price, so the demand is inelastic (irresponsive). For instance, a $+5\%$ change in price, all else remaining the same, leading to a -2.5% change in quantity demanded yields a price elasticity of demand of only -0.5 .

Now, it would have been nice for economists if estimated demand elasticities for fish were broadly similar, but as Table 3 shows, their range is huge: from -0.006 for canned salmon (almost totally inelastic), to -22.7 for whiting (highly elastic).

Table 3: Representative Price Elasticities for Fish Products

Product	Price Elasticity	Study	Type of Price Data	Region ¹	Dep Var ²
all fish products	-0.65	Brandow	Retail index	US	q
all fish products	-0.793	Hassan	Retail index	CA	q
blocks	-0.05	Newton	Import	US	q
blocks	-0.60	Gillen	Wholesale	US	q
blocks	-0.796	Crutchfield	Import	US	q
blocks	-2.0	Pacz	Import	US	q
blocks	-2.89	Tsoa	Wholesale	US	q
bluefish	-4.19	Waugh	Wholesale	NY	p
clams	-5.10	Waugh	Wholesale	NY	p
cod	-0.46	Tsoa	Wholesale	US	q
cod	-0.9	Gillen	Retail	US	q
cod	-2.9	Waugh	Wholesale	NY	p
cod	-3.300	Bell	Ex Vessel	NE	p
crabs	-3.60	Waugh	Wholesale	NY	p
fillets and portions	-0.88	Holmes	Retail index	US	q
fillets and portions	-2.5	Holmes	Retail index	US	p
flatfish	-0.53	Gillen	Retail	US	q
flatfish	-1.04	Tsoa	Wholesale	US	q
groundfish	-0.385	Crutchfield	Import	US	q
haddock	-2.609	Bell	Ex Vessel	NE	p
haddock	-3.22	Waugh	Wholesale	NY	p
lobster	-0.83	Waugh	Ex Vessel	US	p
mackerel	-16.00	Waugh	Wholesale	NY	p
oysters	-0.103	Suttor	Ex Vessel	US	q
redfish	-0.702	Tsoa	Wholesale	US	q
redfish	-2.5	Gillen	Retail	US	q
salmon (canned)	-0.006	Suttor		US	q
scallops	-1.529	Bell	Ex Vessel	NB	p
scallops	-1.602	Bell	Ex Vessel	NE	p
shrimp	-0.290	Suttor	Ex Vessel	US	q
shrimp	-0.38	Elkin	Wholesale	CG	q
shrimp	-0.41	Doll	Wholesale	US	q
shrimp	-0.78	Waugh	Ex Vessel	SAG	p
tuna (canned)	-0.572	Suttor		US	q
whitefish	-1.09	Nash	Wholesale	LK	q
whiting	-22.727	Bell	Ex Vessel	NE	p
yellow perch	-0.560	Nash	Wholesale	LK	q
yellowtail	-2.283	Bell	Ex Vessel	NE	p

¹ NE - New England; NY - New York; US - United States; NB - New Bedford; CG - Chicago; LK - Great Lakes States; SAG - South Atlantic and Gulf; CA - Canada

² Dependent variable is price (p) or quantity (q).

Source: Roy, Tsoa and Schrank (1991: Table 1, p. 15).

The enormity of this range, including for the same type of fish, has not been lost on the article's authors. This is their opening sentence: 'What do statistical demand curves show?' And their reply is brutal: 'usually not as much as we would like, and frequently very little' (Roy, Tsoa, and Schrank 1991: 13). You can almost sense the embarrassment behind the measured language:

"The impetus for this study originated out of our concern with the inconclusive evidence on price elasticities of demand in a number of markets for primary commodities. A representative example, with which we are most familiar, is the market for fish products in the United States – a market described by one participant as 'one of the last industries

where demand and supply really plays a direct role'. Despite a number of demand studies on various segments of this market – to which we have made our own contribution – there is no consensus on the value of the price elasticity representative of this market.” (14)

And this is from experts who have devoted a lifetime to the subject.

14. Summary

In this paper, we reviewed the scientific usefulness of the 1-2-3 toolbox of mainstream economics. The main tools in the box are demand, supply and equilibrium. Economists claim that these tools answer the two key questions of economics: how much gets exchanged and at what price. We argued they answer neither. Here is why:

1. According to mainstream economics, demand and supply represent the desires-read-intentions of buyers and sellers to buy and sell the commodity at alternative prices, all else remaining the same.
2. Nobody has direct access to these desires-read-intentions of buyers and sellers. In fact, most people never articulate their buying and selling desires in the first place, and the few who do quickly lose track of their preferences once the number of commodities exceeds a few dozens. In this sense, demand and supply curves might not even exist to start with.
3. Mainstream economists ignore these difficulties. They assume that demand and supply exist and insist they can be estimated, albeit indirectly, in two ways: (i) by interviewing agents about their buying and selling intentions; and (ii) by fitting econometric regression models to actual market data. Both methods have serious limitations.
4. Interview-based demand and supply curves are impossible to assess. Even if mainstream economics is correct, only a single point on the demand and supply curves ever gets expressed in the market. The rest are forever invisible. Consequently, there is no way to know – including after the fact – that the interviewees expressed their true buying and selling intentions (assuming they have such intentions to begin with).
5. Econometric-based estimates of demand and supply curves are tautological. Since these estimates *assume* that market data represent the intentions of buyers and sellers, they cannot be used to *explain* those intentions.
6. Econometric estimates of demand and supply are always wrong to an unknown degree. Valid regression estimates can be generated only by valid models, but in the case of demand and supply, the models are always invalid: (i) they are arbitrary and therefore almost certainly misspecified; (ii) they impose a stationary structure on a self-transforming world; and (iii) they assume, without a shred of evidence, that the world is in equilibrium. This triple mismatch means that even if demand and supply curves do exist, their estimates are nonetheless wrong – though nobody can ever say in what way and by how much.

These issues are yet to be addressed and resolved. In the meantime, mainstream economics remains a theology dressed as science. Like a well-organized religion, it promises everything and delivers nothing.

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Capitalists Are Dispensable, Laborers Are Not

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Abstract

This essay works out the implications of a neglected asymmetry in a capitalist economy between the capitalist's *separability* from capital and the laborer's *inseparability* from his working capacities. Capitalists organize production because they own capital; if they lack the skills or inclination to organize production, they can hire managers to do so. On the other hand, workers—lacking capital—cannot rent or borrow capital to organize production; this forces them to hire themselves out to capital. At the same time, since workers are inseparable from their working capacities, the capitalists eager to extract a maximum of labor services from the workers, gain control over the worker's person and working capacities in the workplace. As a result, capitalists in a capitalist economy gain control over society per se and over the workplace. The asymmetry we have identified also offers an escape from capitalist-centered production. The separability of capitalists from capital creates the possibility of transferring capital to workers, thus enabling them to establish worker cooperatives (WCs) to organize production. An economy consisting of WCs will dramatically alter the dynamics of the society and the workplace. Inside the enterprise, workers will put in place a democratic governance; they will also establish democratic governance at the national level that serves the needs of society as a whole, not the interests of capitalists and accumulation.

*"If you set to work to believe everything,
you will tire out the believing-muscles of your mind,
and then you'll be so weak you won't be able
to believe the simplest true things."
Lewis Carroll*

Capitalists—*qua* owners of capital—are dispensable, but laborers are not.¹

This thesis flows from a neglected asymmetry between capitalists and laborers. The capitalist does not stand in the same relation to capital and the services of capital as a laborer does with respect to his laboring capacities and the services of these capacities. This distinction goes unacknowledged by neoclassical economists as well as economists of other persuasions.² If this is because they have concluded that this is of no consequence, we offer some arguments to the contrary.

There is irony in this thesis even as capitalism threatens to render laborers 'useless,' that is, replace them with intelligent machines faster than it creates new jobs. But this is not the place to address this irony.

¹ For the purposes of this essay, laborers (or workers) in a capitalist economy include all persons who make a living from selling their labor; this includes all employees.

² David Schweickart, a philosopher, makes this distinction in his book, *Against capitalism* (Boulder, CO: Westview Press, 1996): 12-13.

1. Why are capitalists dispensable?

Consider a series of relations moving from a capitalist to his capital, and from capital to the services of capital. Next examine another series moving from a laborer to his laboring capacities, and from these capacities to the diverse services of these capacities, also known as the services of labor. There exist important differences between the two series.

There exists no integral—or, if you prefer, visceral—connection between the capitalist and his capital or between the capitalist and the services of capital. Capital and the services of capital are wholly external to the capitalist *qua* owner of capital; their relationship to the capitalist is *merely* legal so that a capitalist can be physically separated from his capital and the services of his capital. If the capitalist prefers leisure to work or lacks the requisite managerial skills, he can delegate the task of running his enterprise to hired managers.³ Once the capitalist appoints a manager to run his enterprise, he could be soaking the sun inside one of the many craters on the moon while his enterprise continues to produce goods or services somewhere in Agawam, Massachusetts.⁴ All this notwithstanding, at the end of every year or every quarter, this capitalist will receive the profits or carry the losses of his enterprise. This is his return for risking his funds to organize production.

Now consider an economy whose capital has been transferred to the workers in each enterprise. The workers in each enterprise form a worker-cooperative; they *collectively* own its capital, vote to elect a workers' council who then appoint managers, or elect the managers directly. In addition, each WC is free to borrow funds from deposit banks or tax-funded investment banks. Each WC is also free to sell some fraction of its fixed capital should this not be needed; the proceeds from this sale may be used to pay off loans, deposited in a bank as a reserve fund against rainy days, or allocated to research and development.⁵

Each worker cooperative may choose its *modus vivendi* and its goals. At first, therefore, a variety of worker cooperatives may emerge, with different goals and structures; they may be more or less egalitarian; some may work primarily with fixed wages, others with a combination of fixed wages and shares in the coop's residual. Some may give greater weight to steady employment, others to higher wages. It is difficult to say, which types of cooperatives may emerge as winners. Collectively, the cooperatives may set some limits to the goals, and the governance and the reward structures of the coops to ensure the viability of the system as a whole.

In one stroke then, we have created a market economy *without* any capitalists as the term is understood in a capitalist economy. Production in this economy is organized by worker cooperatives, whose policies are formulated by a management periodically elected by workers. The workers in each enterprise are free to set the rules under which they work; they are also free to leave one WC and join another.

³ Should the capitalist manage the factory or business he owns, he will compensate himself for this managerial service. The capitalist may also invest his capital—*qua* funds—in an established or new enterprise by buying bonds issued by this enterprise.

⁴ Alternatively, nothing will change in the enterprises they own, if all the capitalists in United States were placed in a spaceship, equipped with all the comforts that this class has become accustomed to, and sent to another planet in a distant galaxy.

⁵ We draw upon David Schweickart's (1996: 60-77) 'economic democracy' for defining the basic features of our WC.

However, the capital of any enterprise is not traded, nor does it move when any worker leaves the enterprise.

In a splendid piece of circular reasoning, neoclassical economists justify private ownership of capital on the ground that it is the capitalist who organizes production. This is a *non sequitur*. The capitalist organizes production precisely because he is a capitalist: he commands his own or borrowed funds that allow him to do so. In an economy consisting of worker cooperatives, teams of workers could organize production with funds borrowed from deposit banks or tax-funded investment banks.

It may be argued that the co-ops cannot match the performance of corporations because they have no skin in the game. This is not true. Unlike workers who receive fixed wages, the members of WCs may receive all or some fraction of their income that is tied to their co-op's residual. Since they share in the coop's decision-making, co-op members are more likely than wage-workers to identify with their enterprise. In themselves, these factors may help to reduce shirking. In addition, shirkers in co-ops are likely to come under moral censure from non-shirkers. If free-riding is seen as a problem, the WCs can empower the management to monitor the performance of members and work out rules for dismissing habitual shirkers. Collectively, co-ops may discourage shirking by discounting the earnings of new members who have a record of shirking. Shirkers may also be given the chance to expunge their record by improving their performance.

Since the capitalist is physically separable from his capital, this creates the possibility of separating *ownership* of capital in an enterprise from control over it. In medieval times, we observe trade partnerships in the Middle East—known as *qirad* in Arabic—where some partners advanced capital to traders who managed the task of conducting the trade. In 1932, Adolph Berle and Gardiner Means showed that ownership and control of most large-scale enterprises in the United States had ceased to reside in the same hands.⁶

In activities without significant economies of scale and high costs of supervision, the capitalist may lend and/or rent his capital to workers and let them organize production. In agriculture, we find landlords who rent their lands to peasants instead of hiring wage-workers to organize production. During the late eighteenth and nineteenth centuries, capitalists in Britain set up textile factories with work benches that workers could rent to produce yarn and cloth on their own account.⁷ It is also common for laborers lacking capital to rent cars, trucks and rickshaws to produce transport services. Others rent space in commercial buildings to organize retail businesses.

⁶ Adolf Berle and Gardiner Means, *Modern corporation and private property* (New York, The Macmillan, 1932).

⁷ Gregory Clark, "Factory discipline," *Quarterly Journal of Economics* 54, 1 (March 1994): 128-163.

2. A laborer cannot be physically separated from his working capacities or the services of his working capacities.

A laborer's working capacities include his body, the metabolism that converts food into energy, his cognitive powers, memory, will, emotions, skills, intuition, and his powers of reasoning, sight, speech, hearing, taste and touch. The execution of any task by a worker in real time—whether this entails digging a trench, a sitar recital, performing surgery, crafting a violin, or time spent solving a yet unsolved mathematical conundrum—is the joint product of various manifestations or services of his working capacities.

Under a wage-contract, however, a laborer may sell the services of his working capacities to an employer. In addition, a self-employed person with access to some capital may employ his working capacities to produce services (such as haircuts) that he sells directly to buyers. Alternatively, as a carpenter, he may employ his working capacities to saw, bend, carve, plane, sand, screw together, and polish timber into chairs, and then sell the chairs for a profit. In both cases, the laborer, will likely receive the market wage for his labor and a similar return on the capital he employs in his enterprise.

Some exceptions to the inseparability of a laborer from his laboring capacities may also be noted. If medical technology permits, he may sell or donate his organs or tissues for transplantation to another person. However, a person is unlikely to consent to such transplantations, except when this promises to save the life of a loved one. Of course, without the living person's consent, or after he dies or is killed, the possibilities become endless.

3. The first asymmetry we have just described leads to another.

The separability of the capitalist from his capital nudges us to explore egalitarian re-arrangements of the ownership of capital. Should such rearrangements occur, the capitalists will only lose their claim to the surplus of capitalist enterprises, but they will retain all their rights as citizens and as members of WCs in the new economy. Should they also possess some valuable skills, the WCs will recognize and reward these skills without any prejudice arising from their past history as capitalists.

On the other hand, a laborer cannot be separated from his laboring capacities without also losing his freedom. The capitalist can take ownership of the laboring capacities of legally free laborers only *after* he takes ownership of their persons for some part of a day, that is, by turning them into wage-slaves.

Importantly, the absence of any integral or visceral connection between the capitalist and his capital points to the potential for reorganizing the ownership of capital on an egalitarian basis. It would be disingenuous to think that the capitalists themselves are not aware of this potential for egalitarian rearrangements of the ownership of capital. In addition, we may surmise that the capitalists know that the workers too—perhaps, with help from their protagonists in the intellectual classes—are aware of this vulnerability.

Given all of the above, we may surmise that capitalists have always been at work—no doubt with help from self-serving economists—to obfuscate this vulnerability and, simultaneously, to harness the coercive powers of the state to protect their pivotal position in the capitalist system. Since the actual use of coercion is costly, the capitalists employ all the forces at their command to convince the workers of the superiority of an economic system based on the private ownership of the means of production. However, should the workers start questioning this 'natural order,' the capitalists are prepared to call

on the machinery of the state—especially the courts, police and prisons—to knock the workers until they back the capitalist narrative.

4. Which of the two classes—capitalists or workers—is likely to organize production in a capitalist economy?

In the 1950s, after nearly a century of mathematical ‘modling,’ neoclassical economists concluded that they had worked out the contours of an imaginary economy that would serve as the ideological fortress of capitalism.⁸ Markets in this economy instantaneously reach an equilibrium that is unique, stable and efficient in the sense that no person can be made better off without making at least one person worse off. Capital and labor in this economy receive their just deserts: that is to say, there is no exploitation of labor. In the words of Paul Samuelson, in this economy “it really did not matter who hires whom; so have labor hire ‘capital.’”⁹

The message of the neoclassicals to the capitalists is: Don’t let your hard work as bosses give you a bad conscience. It gives you no advantage. The message to the workers is: Why bother with organizing production; let the capitalists worry about this. Enjoy your hassle-free life as workers; you have nothing to gain from losing your chains.¹⁰ These charming results hold only in perfectly competitive markets in which capitalists can costlessly write and enforce complete employment contracts.

In the real world, however, production is nearly always organized by capitalist bosses. Hurling what he thinks is a challenge to Karl Marx, David Landes asks “if [capitalist] bosses make (cream off) so much money, why shouldn’t boss-free enterprises (cooperatives, collectives, small self-employed workers, and the like) be able to outdo those capitalistic units that pay so heavy a toll to owners and managers?”¹¹ Landes imagines that he is setting up a test of the feasibility of workers organizing production. He is challenging boss-free enterprises to organize production in capitalist economies under property rights, institutions and rules established by capitalists. Now, since worker co-ops do not—with rare exceptions—organize production in capitalist economies, this supposedly ‘proves’ that they cannot compete with capitalists in organizing production.

Landes forgets that workers cannot organize production because they have been stripped of the means of production. Since they cannot offer any collateral, they also cannot borrow money or rent capital. Moreover, financing the establishment of WCs is not only a financial issue. The economic and political

⁸ For the etymology of ‘modl,’ see Lex Leijonhufvud, “Life among the econ,” *Western Economic Journal* (Sep. 1973): 327-337. Arrow and Debreu are the two economists who completed the mathematical edifice of neoclassical economics. See Arrow, Kenneth and Gérard Debreu, “Existence of an equilibrium for a competitive economy,” *Econometrica* 22 (1954): 265-90; Arrow, Kenneth, “An extension of the basic theorems of classical welfare economics,” in *Proceedings of the Second Berkeley Symposium on Mathematical Statistics and Probability* (Berkeley: University of California Press, 1951).

⁹ “...in a perfectly competitive market it really doesn’t matter who hires whom: so have labor hire capital...” Paul Samuelson, “Wages and Interest: A Modern Dissection of Marxian Economic Models,” *American Economic Review* (1957): 894; quoted in Michael Perelman, *The perverse economy* (London: Palgrave Macmillan, 2005): 153. What this means is that the core model of neoclassical economics cannot explain why capital hires labor.

¹⁰ Pierre Fleckinger and David Martimort, “Contract Theory in the Spotlight: Oliver Hart and Bengt Holmström, 2016 Nobel Prize Winners,” *Revue D’économie Politique*, 2018, Vol.128 (4): 493-533.

¹¹ David Landes, “What do bosses really do?” *The Journal of Economic History* 46, 3 (Sep. 1986): 585.

power of capitalists is built around their *class monopoly* over production. Why would they dilute this class monopoly by lending capital to workers? It is unlikely that capitalist guilt over exploitation of workers ever reaches the point at which they become willing to commit hara-kiri.

5. Is the separation of capitalists from capital unjust?

Can any economic system be considered just that has a tendency to place—and often ends up placing—the means of production in the hands of a tiny minority of capitalists, thereby endowing them with the power to organize production not in the interests of the workers or society but exclusively in the interests of the capitalists.¹² Depending on conditions in the labor market, and driven by the profit imperative, the capitalist may hire and fire workers at will. In addition, he may demand that his hired hands put in long hours of work, six days a week, quicken their pace of work to keep up with the machines, and take no breaks in the workplace, even if they have to piss in a bottle or wear diapers.

In a capitalist economy, moreover, workers are disposable, unless they have acquired skills specific to the enterprise in which they work; they may be laid off or fired whenever they are not needed. Can an economic system be just that nearly always fails to provide some workers with employment, and during a depression may fail to provide employment to as much as a third of the workers; when it does provide employment, many of these jobs may not offer the workers a living wage. Can an economic system be humane whose commitment to profit-making demands that workers be fired when they fall sick or are injured even when the sickness and injury are caused by hazards of the work and the workplace? It is ironic that while wage-workers in capitalist economies are legally free and wage-work is considered to be superior to slavery, the capitalists do not have any intrinsic interest in the livelihood and health of their workers that slave-owners have in their slaves and their families.

Similarly, while wage-workers are legally free, they lose their freedom the moment they enter the workplace. The neoclassical economist will predictably protest that a wage-worker remains free even at work since he is free to leave his present job whenever he wishes; if he does not quit that is because he *chooses* to stay. Sorry: this is a flawed inference. The neoclassical economist is always liberal in making assumptions; when he needs a can opener he just *assumes* that he has one. Hence, neoclassical economics assumes that workers face no costs in moving to another job that he prefers over his current job.

However, the ability to switch jobs at will does not overcome the lack of freedom at the workplace. A worker cannot escape the lack of freedom at the workplace because this is an unavoidable condition of capitalist employment, unless the nature of a job makes monitoring of work very costly. The lack of freedom at work cannot be blamed on technology; the capitalists have been choosing technology, layout of factories and offices, and work organization that truncate worker autonomy.¹³ Bosses can monitor their employees' pace of work, even when they work remotely.

¹² This tendency proceeds from economies of scale and scope. In turn these economies arise from large initial investments in fixed capital—such as building, machinery, and software—combined with low marginal costs, so that average fixed costs decline over a wide range of output.

¹³ Stephen Marglin, "[What Do Bosses Do? The origins and functions of hierarchy in capitalist production, Part I.](#)" *The Review of Radical Political Economics* 6, 2 (1974): 60-112.

6. Conservatives and liberals are likely to view with alarm any talk about the rearrangement of capitalist property rights in the means of production.

Although such alarm is to be expected from the historical beneficiaries of capital accumulation over the last half a millennium, this shows societal amnesia about the horrendous crimes against humanity that attended, and still attend, the expropriation of great masses of humans—men, women, children and the unborn—to finance and support the creation of capitalists and capitalist power in Western Europe and North America. Karl Marx applied the moniker of ‘primitive accumulation’ to the “historical process of divorcing the producer from the means of production.”¹⁴

The capitalist organization of British agriculture, which began in the late fifteenth century, was accomplished over the next three centuries through successive expropriations of the peasantry either by force or through laws passed by the largest landowners—formerly feudal lords—in the British parliament. The free peasantry that had emerged in England by the late fourteenth century earned a living from cultivating strips of arable land, but they also depended crucially on access to commons for firewood, pasturing their cattle, gathering berries and mushrooms, and catching game and fish. Once the growth of woolen manufactures in Flanders raised the price of wool, English landowners began to privatize the commons, either by force or passing enclosure acts that denied peasants access to the commons, and converted them into sheep pastures; this forced some peasants to seek wage-work in agriculture, while others abandoned their lands to seek wage-work in the towns. In addition, the large landowners also began to enforce gaming laws to ride roughshod over the farms of peasants resulting in losses to their crops.¹⁵

Next consider a historical rearrangement of property rights during the nineteenth century that is more germane to the subject of this essay. I am referring to the large-scale separation—in the Americas during the nineteenth century—of a very important class of capitalists from their capital invested in slaves. In August 1791, the black slaves in Saint-Domingue—a French colony that was a source of immense profits to France—began a successful rebellion against slave-owning capitalists—who after defeating two massive invasions ordered by Napoleon Bonaparte in 1801 and 1802, declared the establishment in 1804 of the Republic of Haiti ruled by former black slaves. Do we object to this transfer of property rights in slaves from plantations owners to the slaves themselves?

In the nineteenth century, we encounter several more examples of the separation of capitalists from their capital invested in slaves. In 1833, Britain transferred the ownership rights of capitalists in humans to the humans themselves in nearly all its colonial possessions. Some of the northern states in the USA, following the War of Independence in 1776, began passing laws that effected similar transfers of ownership rights in slaves. In 1861, in the midst of the American Civil War, slavery was also abolished in the Southern States. Brazil, perhaps the largest slave-based economy in history, abolished slavery in 1888. In general, where slavery was abolished under law, the cash compensation was less than the market value of the slaves.

¹⁴ Drawing on the literary resources available in the library housed in the British Museum, Karl Marx has documented eloquently and in graphic details the history of primitive accumulation in British agriculture and globally down to the mid-nineteenth century. It is worth noting that primitive accumulation does not end once ‘capital accumulation’ has created a society in its image. It continually invents new mechanisms for the “dispossess of capital” in the centers of capitalism and around the world. Karl Marx, *Capital: A critique of political economy, volume one*, translated by Ben Fowkes (London: Penguin Classics, 1990/1867): 875.

¹⁵ Marx (1990): chapter 27; Perelman (1990): xx.

7. The principal results of this essay are easily summarized.

The capitalist is physically separable from the capital that he owns, but the laborer cannot be parted from his laboring capacities.

The first part of the previous statement establishes the feasibility of separating the capitalist from his capital. Indeed, the capitalist (*qua* capitalist) is a rentier since he does not contribute the services of any of his *present* working capacities to the enterprise in which he owns capital. The capitalist's claim to profits is rooted in a *legal* relationship, not an economic relationship.

Au contraire, the services of a laborer are inseparable from his person. This means that in order to maximize his profits, the capitalist who hires labor must gain control over the laborer's person and his working capacities in the workplace and—if necessary and feasible—off the workplace. In other words, the capitalist logic demands that the laborer be stripped of his autonomy once he enters the workplace. Although the worker is free in theory to choose his employer, this freedom does not restore the autonomy that he enjoyed in his work and workplace as a self-employed peasant, artisan, peddler or shopkeeper.

A clear-eyed focus on the asymmetry in the two binaries—the separability of the capitalist from capital and inseparability of the laborer from his working capacities—suggests significant gains that are likely to flow from an alternative organization of production that transfers ownership and control over capital from capitalist enterprises to worker cooperatives.¹⁶

A detailed discussion of these gains is a subject for another essay. However, broadly speaking, these gains are likely to flow from two forms of democratization that will attend the transfer of capital from the capitalists to the workers. First, there is the political democratization that will flow from the transformation of the capitalist enterprises to worker cooperatives. A political system that grows out of the interests and actions of workers and worker co-ops is unlikely to be hijacked by sectional interests.

Secondly, there is the democratization in the workplace. A variety of benefits are likely to flow to workers from the establishment of co-ops. These may include sharing by members in policy-making, creation of a culture of egalitarianism, improvements in working conditions, equal access of all members and their families to education, health and social services, less hierarchy in the organization of work, sharing and phasing out of tedious work, and greater income inequality. An economy consisting of worker co-ops will nurture cooperation, in the workplace and outside it. Once profits are dethroned as the only or chief objective driving production and technological change, the economy will have a chance to redirect its focus from endless capital accumulation towards a thousand improvements in the quality of life for everyone.

¹⁶ Moreover, the transfer of capital from the capitalist to the workers may occur without any prejudice to the capitalist's rights as a citizen or a laborer in an economy where production is organized by workers.

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Redistributing Income Through Hierarchy

Blair Fix [York University, Toronto, Canada]

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Abstract

Although the determinants of income are complex, the results are surprisingly uniform. To a first approximation, top incomes follow a power-law distribution, and the redistribution of income corresponds to a change in the power-law exponent. Given the messiness of the struggle for resources, why is the outcome so simple?

This paper explores the idea that the (re)distribution of top incomes is uniform because it is shaped by a ubiquitous feature of social life, namely *hierarchy*. Using a model first developed by Herbert Simon and Harold Lydall, I show that hierarchy can explain the power-law distribution of top incomes, including how income gets redistributed as the rich get richer.

To study income is to be perplexed

In a famous 1933 speech, John Maynard Keynes lamented his discontent with capitalism:

“It is not intelligent, it is not beautiful, it is not just, it is not virtuous — and it doesn’t deliver the goods. In short, we dislike it, and we are beginning to despise it. But when we wonder what to put in its place, we are extremely perplexed.” (Keynes, 1933)

Today, we might attribute a similar sentiment to researchers who study the distribution of income. Heterodox economists agree that the current distribution of income is ‘not virtuous’, and that the dominant approach to understanding income (marginal productivity theory) ‘doesn’t deliver the goods’. But when we look for a better approach to understanding inequality, we are ‘extremely perplexed’.

Like so many aspects of human society, the distribution of income is frustratingly complex — the joint result of ideology, politics, class struggle, and everything in between. Reviewing these complexities, Sandy Hager argues that it may be best to study inequality using a ‘plurality of methodological approaches’ (2020). I largely agree, but with one caveat. While the causes of inequality are surely complex, the outcome is not. Regardless of where we look, we find that top incomes follow a simple pattern: they are distributed according to a *power law*. That is, the probability of finding someone with income I is roughly proportional to $I^{-\alpha}$.

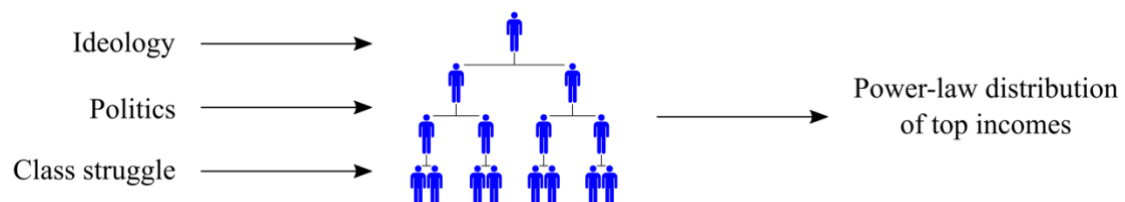
If the causes of income are complex, why can we model the result with a single parameter — the power-law exponent α ? Moreover, why can we model income redistribution by shifting this parameter, and this parameter alone? Given the complexity of human society, the success of such a simple model seems unreasonable. How do the myriad of different forces driving inequality ‘conspire’ to create such a simple outcome?

One possibility is that the ultimate causes of inequality are indeed complex, but that they are mediated by a ‘proximate’ cause that is far simpler. If this mediator was ubiquitous, it could lead to the simple outcome that we observe (the power-law distribution of top incomes). So, what might this mediator be?

I propose that it is *hierarchy*. Although largely ignored by mainstream economics, hierarchy is a common feature of human life. It seems to be the default mode for organizing large groups. And its use appears to have spread with industrialization (Fix, 2021a).

The distinguishing feature of hierarchy is the chain of command, which concentrates power at the top. It is this feature, I propose, that mediates the distribution of top incomes. For a power-law to emerge, all we need is for income to increase (roughly) exponentially with hierarchical rank. Varying this rate of increase then causes a redistribution of top incomes. The result is a proximate explanation of inequality that locates the source of power-law distributions in the chain-of-command structure of hierarchies (Figure 1).

Figure 1: Hierarchy as a proximate cause of inequality.



Although this focus on hierarchy does not explain the ‘ultimate’ cause of inequality, it dramatically changes the way we think about the problem. It is one thing to look at top incomes and wonder what is causing them to increase. It is quite another thing to understand that top incomes can be directly linked to the hierarchical pay structure of individual firms.

In the latter case, we realize that each firm is a microcosm of the distribution of income at large. Moreover, when we link top incomes to hierarchy, we are implicitly connecting the distribution of income to the power structure of society. The consequence is rather incendiary. When top incomes increase, it suggests that firm hierarchies are becoming more despotic.

The shape of top incomes

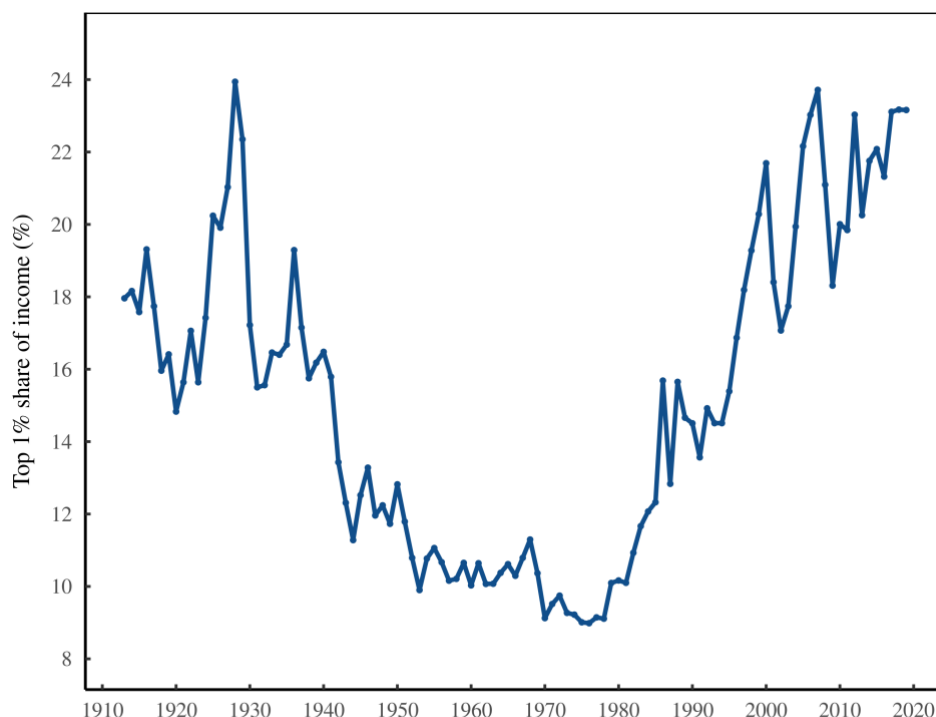
Before discussing how hierarchy relates to top incomes, we must cover some requisite knowledge about income and its (re)distribution. In the introduction to his 2014 treatise on inequality, Thomas Piketty observed:

“Intellectual and political debate about the distribution of wealth has long been based on an abundance of prejudice and a paucity of fact.” (Piketty, 2014)

Today, thanks in large part to Piketty’s work, the ‘paucity of facts’ is no longer a problem (at least among people who are concerned with facts).¹ Many people know that income inequality has risen dramatically in recent decades. Matters came to a head during the Occupy movement when the term ‘one-percenter’ became a well-known put down (Di Muzio, 2015). The term alludes to the growing divide between the income of the majority (the bottom 99%) and the income of the elite (the top 1%).

Figure 2 shows this divide — the income share of the US top 1%. The U-shaped trend is now well known. After World War II, US inequality declined rapidly and then remained low for 30 years. But from the 1980s onward, inequality rose dramatically.

Figure 2: The fall and rise of US inequality.



The timing of this rising inequality has eluded few observers. It corresponded with a seismic shift in US politics — a turn from the post-War expansion of the welfare state to the ‘trickle down’ policies of the

¹ Although Piketty popularized the study of top income shares, he built on the work of many researchers, including Atkinson & Harrison (1978), Atkinson & Bourguignon (2001), Atkinson & Piketty (2010), and Alvaredo, Atkinson, Piketty, & Saez (2013).

Reagan era. Given this conspicuous political shift, many researchers leap straight from the inequality evidence to a list of possible ‘causes’.

I sympathize with this move but think that it is partially premature. Yes, we should look for correlates of inequality, of which there are many. (See, for instance, the work of Huber, Huo, & Stephens, 2017.) But we should also realize that looking only at the income share of a specific group (like the top 1%) gives a rather narrow window into the wider distribution of income.

Unfortunately, looking at the whole distribution of income takes some technical skills, which is likely why doing so is less popular than studying top income shares alone. Still, if we want to study growing inequality, we need to understand how *all* income is distributed.

Viewing the distribution of income in its entirety

In the interest of accessibility, I offer here a brief tutorial of how to visualize income distributions from top to bottom using log histograms. Readers familiar with this technique can skip to the next section.

The most basic way to visualize a distribution of income is to use a histogram. To construct a histogram, we put the data into size ‘bins’ and count how many observations occur within each bin. Then we plot the results.

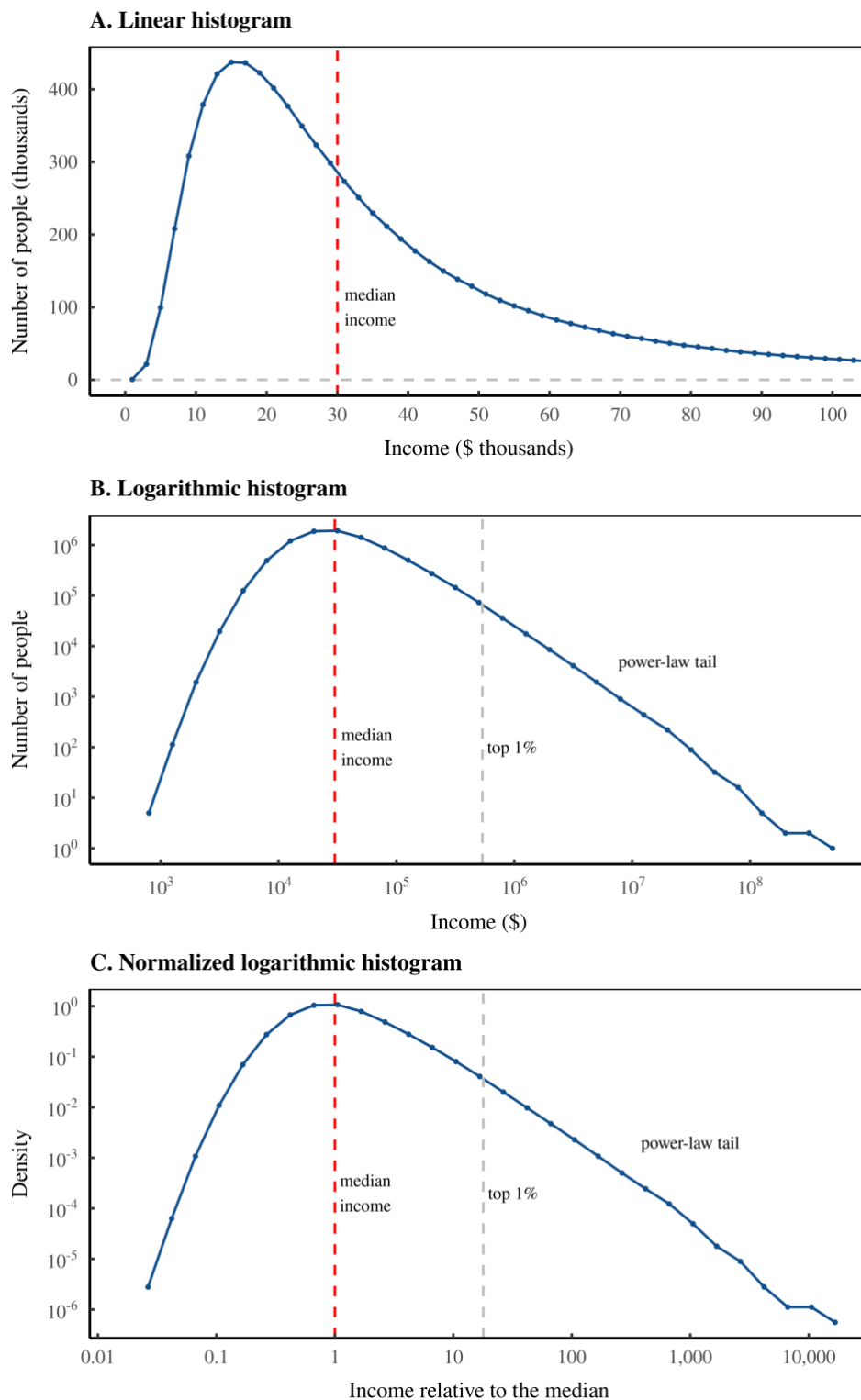
Figure 3A shows a histogram of a hypothetical distribution of income. (For reference, this simulated society has about 10 million people, a median income of \$30,000, and a top 1% income share of about 20%. It’s intended as a scaled-down version of the modern United States.)

I have put individual incomes into bins that are \$2000 wide. On the vertical axis, I have plotted the number of people within each bin. Each point represents the person count, plotted at the midpoint of the income bin. This representation of a histogram, which connects bin counts with a line, is sometimes called a ‘frequency polygon’. But for ease of reference, I will simply call it a ‘histogram’.

Our Figure 3A histogram does not look like the familiar ‘bell curve’. Rather, it has a ‘fat’ right tail that continues far past the chart’s income cutoff of \$100,000. This fat tail is a ubiquitous feature of distributions of income and is the face of inequality in histogram form. It tells us that some individuals earn far more than the average person.

Figure 3: Three ways to visualize a distribution of income.

Using a simulated distribution of income, this figure shows three ways of visualizing the distribution with a histogram. Panel A shows the standard form with income bins of constant size. The problem here is that the rich are 'off the chart'. Panel B uses log-spaced bins, with both the bins and counts plotted on log scales. We see the power-law tail of top incomes on the right side. Panel C normalizes the histogram so that it is comparable to different samples of income.



The problem with our standard histogram is that we cannot see the rich — they are literally off the chart. To visualize the distribution of top incomes, we need a different approach. The best option is to move to a logarithmic histogram.

A log histogram uses income bins that are logarithmically spaced. For instance, the first bin might go from \$1 to \$10, the second from \$10 to \$100, the third from \$100 to \$1000, and so on.² By using log spacing, we can reach enormous incomes with relatively few bins. The key is that we then plot both the bins and the corresponding counts on logarithmic scales. In the resulting logarithmic histogram, shown in Figure 3B, we can see the rich and the poor alike. The poor are on the left, with incomes that are far smaller than the median. And the rich are on the right, with incomes that are far larger than the median.

In our log histogram, we can also see a key feature of top incomes: they tend to be distributed according to a power law.³ A power law is a type of distribution in which the probability of finding a person with income I is proportional to that income, raised to some exponent α :

$$P(I) = c \cdot I^{-\alpha} \quad (1)$$

Power law distributions have the interesting feature that if we plot their logarithmic histogram (as we have in Figure 3B), we get a straight line. The reason is beautifully simple. When we take the logarithm of both sides of Equation [1](#), we get a linear relation whose slope is $-\alpha$:

$$\log P(I) = \log c - \alpha \cdot \log I \quad (2)$$

So, the fact that the right tail of our log histogram looks like a straight line means that top incomes roughly follow a power law.

If we wish to compare the distribution of income at different points in time (or between different countries) there is one last step: we must ‘normalize’ the histogram. To do that we convert incomes from dollar values to relative values. In Figure 3C, I compare all incomes to the median. Next, we normalize the histogram counts so that they are unaffected by sample size. I do that in Figure 3C by converting bin counts to a ‘probability density’. This transformation defines the vertical scale so that the area under the histogram sums to 1.

Although our normalized histogram looks identical to the un-normalized version, it now has standardized axes. That means we can compare different distributions of income.

² Instead of using log-spaced bins, another option is to use linear bins but count the frequency of $\log(\text{income})$. The results will be the same.

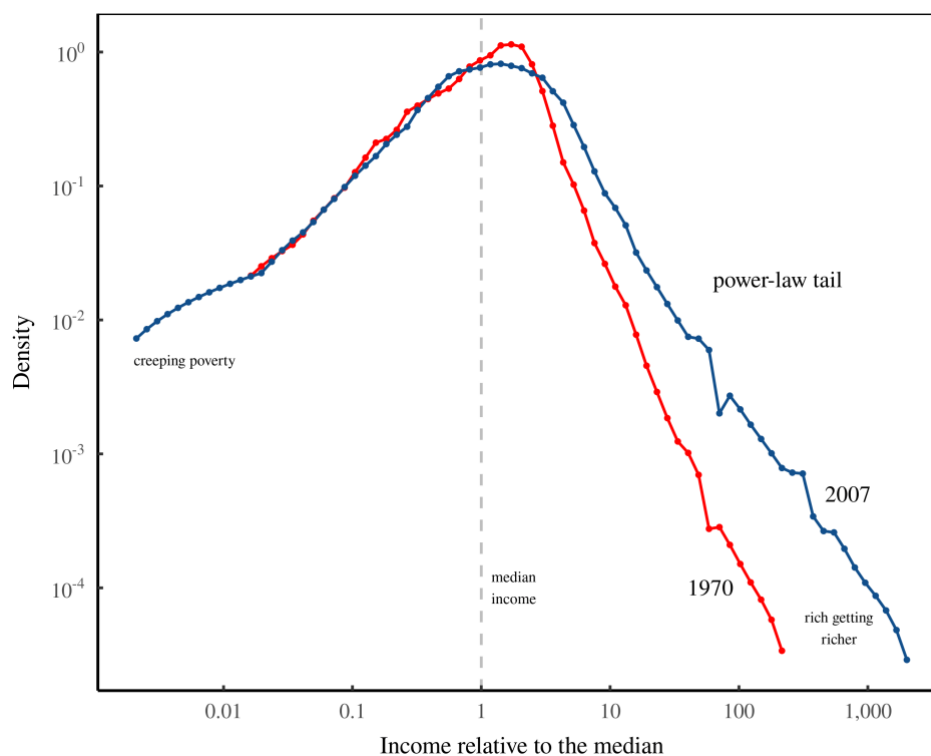
³ The power-law distribution of top incomes (and wealth) was discovered at the turn of the 20th century by Vilfredo Pareto (1897). For a sample of subsequent confirmations of Pareto’s discovery, see Di Guilmi, Gaffeo, & Gallegati (2003), Clementi & Gallegati (2005), Coelho, Richmond, Barry, & Hutzler (2008), Toda (2012), and Atkinson (2017).

Income redistribution in the United States

Now that the reader has the requisite knowledge, we are ready to look at the distribution of US income in its entirety. Figure 4 shows the US distribution of income in 1970 and 2007. I have chosen these years because they are the dates of minimum (1970) and maximum (2007) inequality in recent US history. The change in the distribution of income is easy to spot.

Figure 4: The US distribution of income, 1970 and 2007.

Using the log-histogram technique outlined in Figure 3, I plot here the distribution of US income when inequality was at a minimum (1970) and a maximum (2007). For sources and methods, see the Appendix.



Let us start, however, with what did *not* change between 1970 and 2007. To spot a lack of change, look for locations where the two histograms overlap. In Figure 4, we can see that this overlap occurs below the median income, where the two histograms are nearly identical. This similarity tells us that for the bottom half of Americans, little has changed (in terms of relative income) over the last 4 decades.

Among the American poor, though, there is one conspicuous difference between 1970 and 2007: in the latter year, the social safety net had been removed. This removal appears in Figure 4 as a leftward extension of the blue histogram into ever-more diminutive incomes. This is creeping poverty in histogram form. Today, many Americans earn less than 1% of the median income — something that was not true in 1970.

While creeping US poverty is worth studying, it is not the subject of this paper. Instead, I am concerned with the right-side of the histogram. Here we can see the egregious redistribution of top incomes. Between 1970 and 2007, the American rich got richer ... much richer. Whereas in 1970, no one earned more than a few hundred times the median income, by 2007, a handful of Americans earned more than 1000 times the median.

It is easy to marvel at the absurd size of top US incomes. But here I am more concerned with the *uniformity* of income redistribution. As expected, top US incomes (roughly) follow a power-law distribution, evident as the straight right tail in both distributions. What is fascinating is that despite the complex reasons for growing US inequality, to a first approximation, all that changed between 1970 and 2007 is the *slope* of the distribution tail.

This simple result deserves an explanation. Why can we model the messy business of the rich getting richer by turning a single dial — the power-law exponent of top incomes?

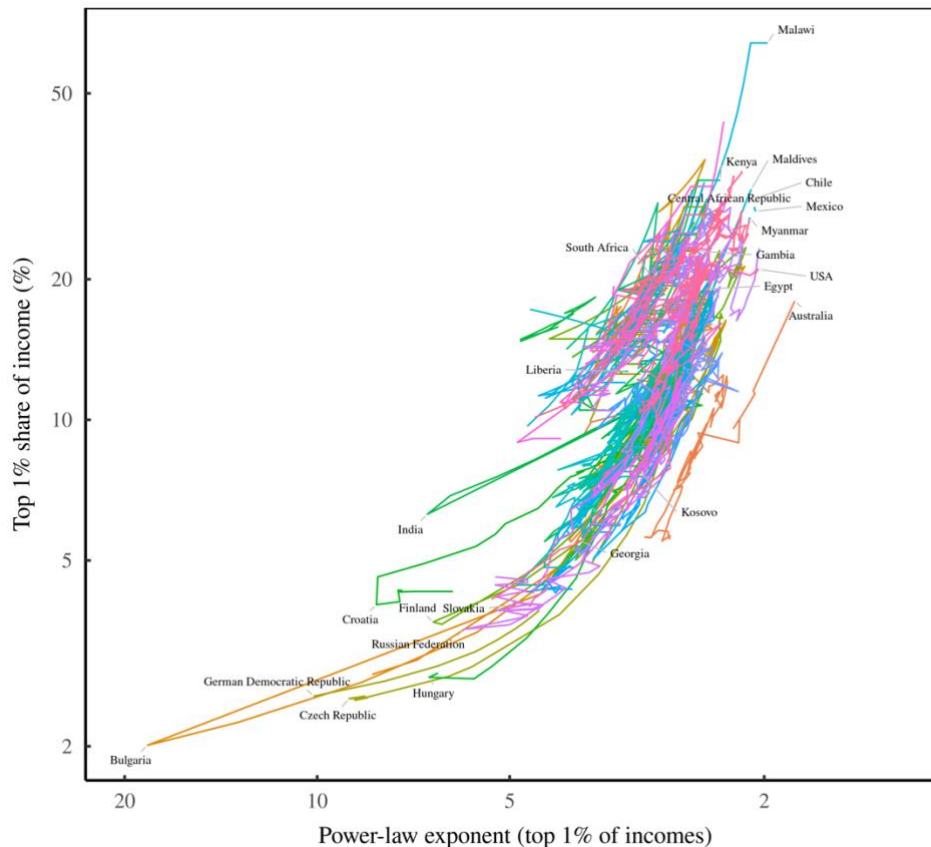
Income redistribution among all countries

Before we conclude that the rich getting richer is a simple process, we ought to look at more data. It could be, for instance, that the United States is a uniquely simple case, and that elsewhere, the redistribution of income is more complicated.

To test this possibility, let's look at income redistribution in every country for which there is suitable data. Using data from the World Inequality Database, Figure 5 plots the income-redistribution trends for 176 different countries covering the years 1900 to 2019.

Figure 5: As top income shares grow, the income-distribution tail gets fatter.

This figure visualizes income redistribution among countries. Each line indicates the path through time of a particular country. The vertical axis shows the country's top 1% share of income. The horizontal axis shows estimates for the power-law exponent of top incomes (fitted to the top 1% of incomes). As top income shares increase, the power-law exponent tends to decline, indicating that the distribution tail gets fatter. For sources and methods, see the Appendix.



Rather than show the complete distribution for each country (in each year), I have plotted the top 1% income share against the power-law exponent of top incomes. To reiterate, this exponent measures the slope of the income distribution tail. A smaller exponent indicates a fatter tail. (For power-law fitting methods, see the Appendix.)

If income redistribution was a messy, heterogeneous process, we would expect no clear relation between top income shares and the power-law exponent of top incomes. But that is not what we find. Instead, we see in Figure 5 a very clear relation. Growing top income shares are associated with a decline in the power-law exponent of top incomes. In other words, there is startling uniformity in the way that societies redistribute income.

Generating power laws

To understand the distribution of top incomes, we need to understand more about power laws. Where do they come from? How are they generated?

Although the causal mechanisms may appear complex, the mathematical mechanisms for generating power laws are surprisingly simple. I will discuss two main routes. (For a review of mechanisms for generating power laws, see Mitzenmacher, 2004.)

The first route to a power law is through income *dynamics*. Suppose an individual starts out with annual income I . Over time, their income grows and shrinks for reasons that we do not understand. But what we do know is that this income change can be modelled as a random number. After t years, the person's new income is the product of successive random growth rates, g :

$$I_t = I_1 \cdot g_1 \cdot g_2 \cdot \dots \cdot g_t \quad (3)$$

Now suppose that everyone's income behaves the same way: it is the product of a series of random growth rates. After many growth iterations, the resulting distribution of income will follow a lognormal distribution — a fact discovered by Robert Gibrat (1931).

To get a power-law distribution, we introduce one more requirement: a lower 'wall' that limits the smallness of incomes. If anyone's income gets below this lower threshold, it gets 'reflected' in the opposite direction. After many growth iterations, income will be distributed according to a power law.

This 'stochastic' model of income was first articulated by David Champernowne (1953). While the model's mathematics are beyond dispute, many political economists find its appeal to 'randomness' troubling. After all, incomes have definite causes (or so we believe). But to be fair to the Champernowne model, it does not claim that income dynamics are *actually* random, only that we can model them as such.

The Champernowne model tells us that we can understand the power-law distribution of top incomes without knowing anything about the complexities of human behavior. All that we need are general assumptions about the dynamics of income. I find this result fascinating because it is counter-intuitive. Yet it is also underwhelming because it does not tell us why people earn what they do. For that reason, I will focus on a second route to power laws — a route that can be tied to social structure.

The second route to a power law comes from merging two different exponential functions. Suppose two variables, x and y , are both exponential functions of a third variable, t :

$$x = e^{a \cdot t} \quad (4)$$

$$y = e^{b \cdot t} \quad (5)$$

If we combine these two functions and eliminate t , we find that x and y are related by a power law:⁴

$$y = x^{b/a} \quad (6)$$

So, we can create a power law by merging two exponential functions. The question is, why would such functions apply to income? The answer, I propose, is simple. These are the equations that describe income in a hierarchy.

Power-laws via hierarchy

Hierarchies are perhaps the dominant feature of our working lives. Yet paradoxically, they rarely enter into mainstream theories of income distribution. Fortunately, a handful of researchers have explored the distributional consequences of hierarchy. I build on their work here.

To my knowledge, the first person to explicitly model income within a hierarchy was the polymath Herbert Simon (1957). Simon noted that hierarchies are government by a chain of command in which each superior controls multiple subordinates. The consequence is that the number subordinates one controls increases exponentially with rank. At the same time, income within a hierarchy tends to increase exponentially with rank. Combining these two exponential functions gives a power law.

Simon, though, was not interested in the power-law distribution of top incomes. Instead, he was interested in another power law — the fact that CEO pay scales with the power of firm size:

$$\text{CEO pay} \propto (\text{Firm size})^D \quad (7)$$

Simon argued that this scaling (which was discovered by David Roberts in 1956), stemmed from hierarchy. It was caused by merging the exponential growth of subordinates (with hierarchical rank) and the exponential growth of pay (with hierarchical rank).

⁴ Here are the algebraic steps. First, take the logarithm of both functions and solve for t :

$$t = \frac{1}{a} \log x$$

$$t = \frac{1}{b} \log y$$

Next, combine the two equations to eliminate t :

$$\log(y) = \frac{b}{a} \log(x)$$

Note that $\frac{b}{a} \log(x)$ is equivalent to $\log x^{b/a}$. Therefore,

$$y = x^{b/a}$$

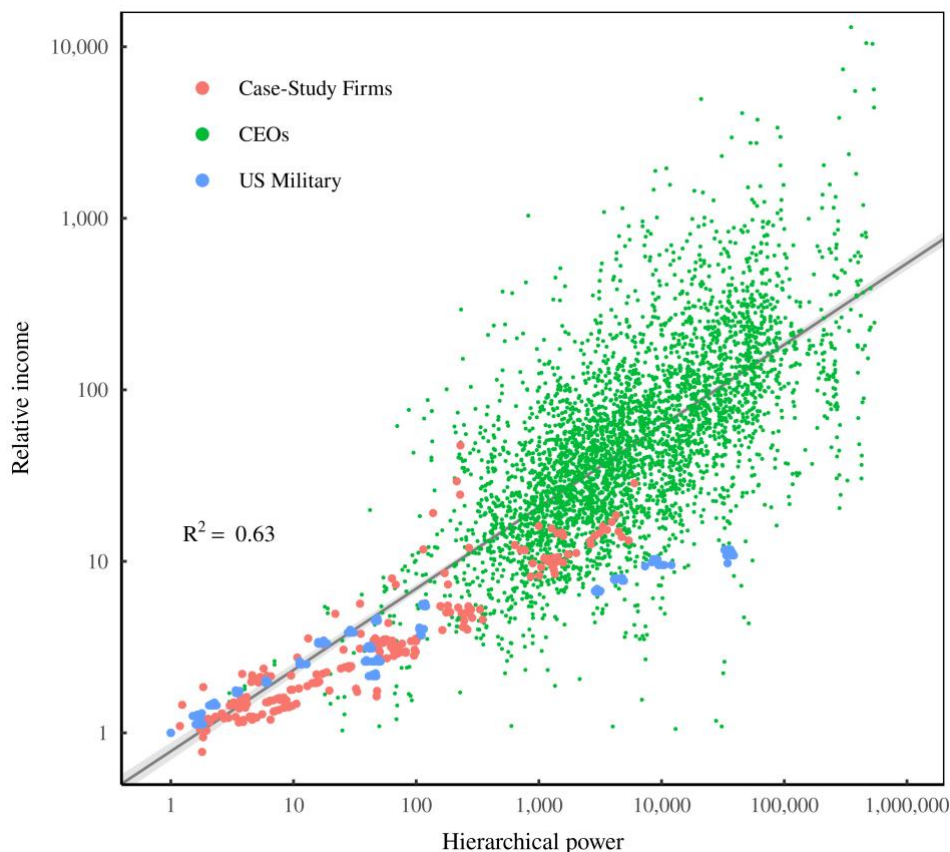
Although largely ignored by mainstream economists, Simon's reasoning remains sound. In fact, we can extend it to every member of the hierarchy (not just CEOs). As Figure 6 indicates, relative income within hierarchies scales with the number of subordinates one controls. For ease of reference, I give 'the total number of subordinates' a shorthand name. I call it 'hierarchical power', defined as:

$$\text{hierarchical power} = 1 + \text{number of subordinates} \quad (8)$$

Across a wide variety of institutions, relative income appears to scale with hierarchical power.

Figure 6: Within hierarchies, income grows with hierarchical power.

This figure shows evidence from a variety of institutions indicating that relative income within hierarchies scales with 'hierarchical power'. In the case-study firms and the US military, income is measured relative to the average in the bottom hierarchical rank. Each point indicates the average hierarchical power within a rank. For CEOs, income is measured relative to the average pay within the firm. I assume the CEO commands the firm, meaning their hierarchical power is equivalent to the firm's total employment. For sources and methods, see the Appendix.



Two years after Herbert Simon published his results, Harold Lydall (1959) realized that the same model of hierarchy could explain the power-law distribution of top incomes. The mechanism was exactly the same — the merger of two exponential functions. (Interestingly, Lydall appears to have been unaware of Simon's work.)

Like Simon, Lydall assumed that income grows exponentially with hierarchical rank. That gives exponential function number one. The second function comes from the number of people within each rank. As we move up the hierarchy, the number of people within each rank declines exponentially — a consequence of the nested chain of command. By merging these two exponential functions, Lydall showed that hierarchy could create a power-law distribution of income.

Because Simon and Lydall's pioneering research was completed a half century ago, one would think that today there would be a burgeoning literature on the distributional consequences of hierarchy. Sadly, this is not the case. Instead, shortly after Simon and Lydall published their work, the study of income distribution became dominated by human capital theory, which focused on personal traits and neglected 'structural' explanations of income (Fix, 2021b). And so today, we know little about how hierarchy affects the distribution of income.

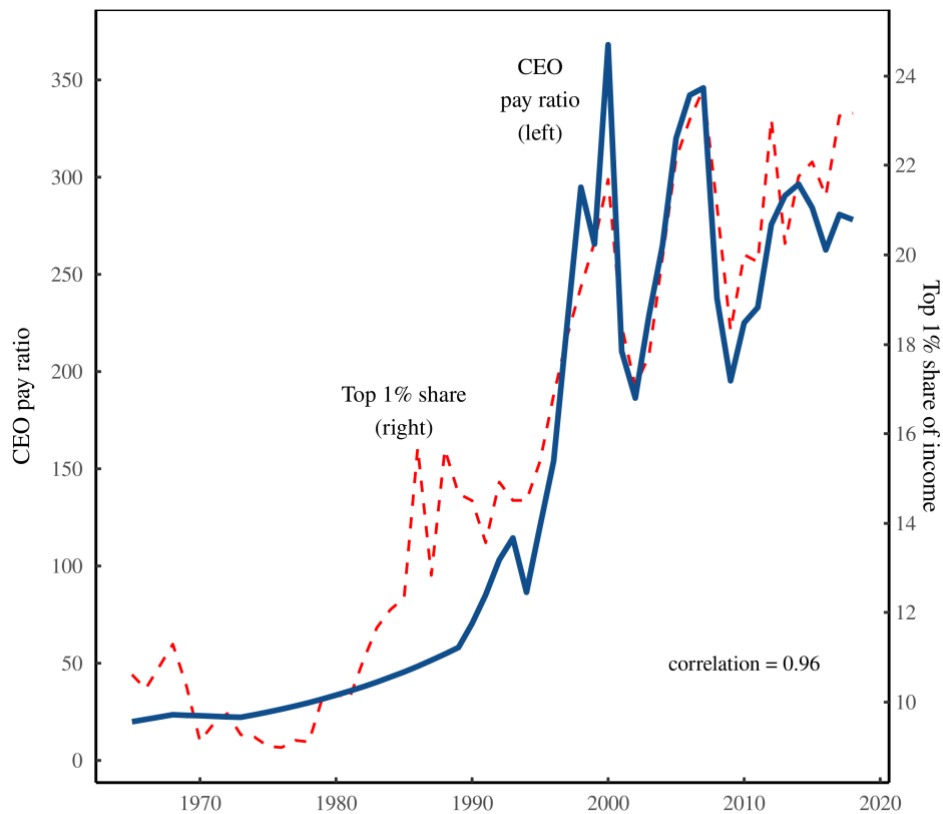
Despite the historical neglect, I think focusing on hierarchy is a promising way to understand income (Fix, 2018, 2019b, 2020). And as I discuss below, I think it is also a promising way understand income *redistribution*.

A sign from CEOs

To understand how income redistribution relates to hierarchy, I propose that we return to where Herbert Simon started: with CEOs. Over the last 40 years, the relative pay of US CEOs has increased dramatically. The timing of this pay explosion aligns tightly with rising US inequality. Figure 7 shows the trend.

Figure 7: Increasing US inequality corresponds with a growing CEO pay ratio.

The CEO pay ratio is calculated by dividing the pay of CEOs in the 350 top US firms (ranked by sales) by the average income of workers in the corresponding industry. For sources and methods, see the Appendix.



The obvious conclusion, reached by many observers, is that runaway CEO pay is related to runaway inequality. Interestingly, however, there have been few attempts to generalize this finding into a model of income distribution.

The way to do this, I believe, is by treating CEOs as canaries in the coal mine. I propose that the exploding pay of CEOs is part of a wider redistribution of income within hierarchies. It is evidence that US firms are becoming more *despotic*.

I use the word 'despotic' in both a general sense (as in the abuse of power) and in a more technical sense, as follows. A key feature of hierarchies is that they concentrate power at the top — a feature that inevitably creates problems. Yes, rulers *can* use their power to benefit the group. But they can also use their power to enrich themselves. The more they do so, the more 'despotic' the hierarchy.

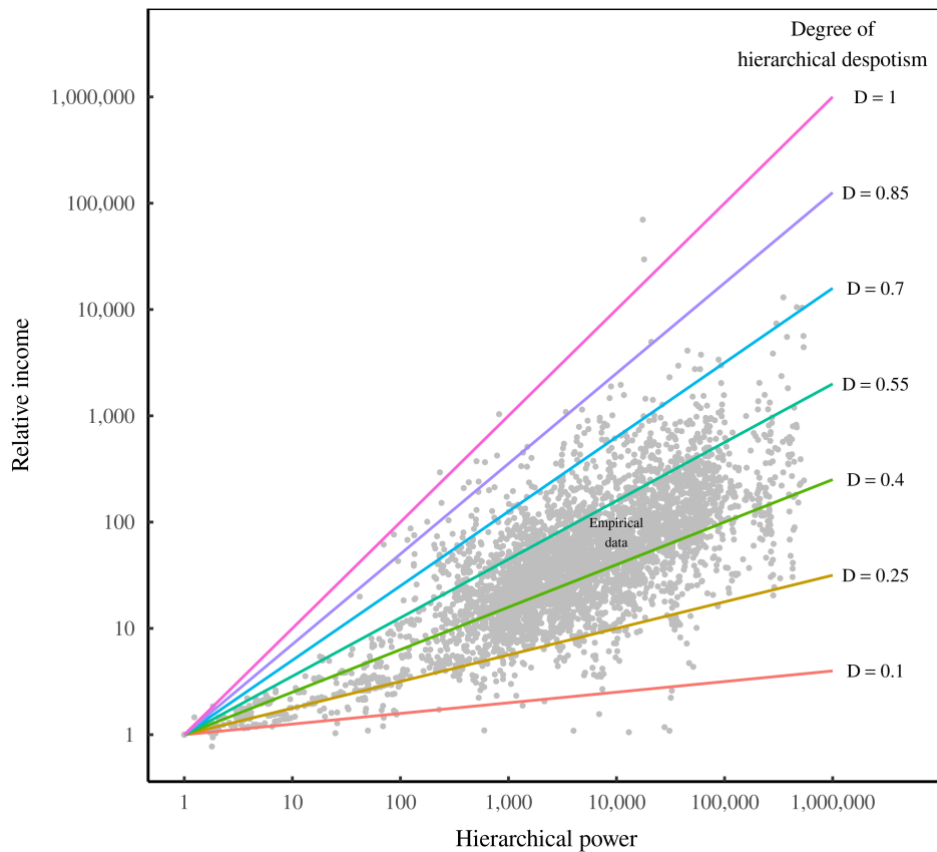
Importantly, despotism is not just a game for rulers. It is a game played by everyone in the hierarchy. The result, I propose, is that the more despotic the hierarchy becomes, the more rapidly income will increase with hierarchical power. It makes sense, then, to use the scaling of income with hierarchical power, D , as a measure of the 'degree of hierarchical despotism'. The greater the value of D , the more despotic the hierarchy.

$$\text{relative income} \propto (\text{hierarchical power})^D \quad (9)$$

To frame this idea, let's return to the empirical evidence. In Figure 8, I have replotted (as grey points) the empirical trend between relative income and hierarchical power (the trend originally shown in Fig. 6). Over top of this data, I show scaling relations for different values of D .

Figure 8: How the degree of hierarchical despotism, D , affects income.

Grey points replot empirical data from Fig. 6. Colored lines indicate the (hypothetical) scaling of income with hierarchical power for different values for D — the degree of hierarchical despotism. For sources and methods, see the [Appendix](#).



In large hierarchies, the value of D affects top incomes dramatically. For instance, when $D = 0.1$, a CEO with one million subordinates will earn only about 4 times more than a bottom-ranked worker. But when $D = 1$, the same CEO will earn a *million* times more than an entry-level employee.

US CEOs as canaries of hierarchical despotism

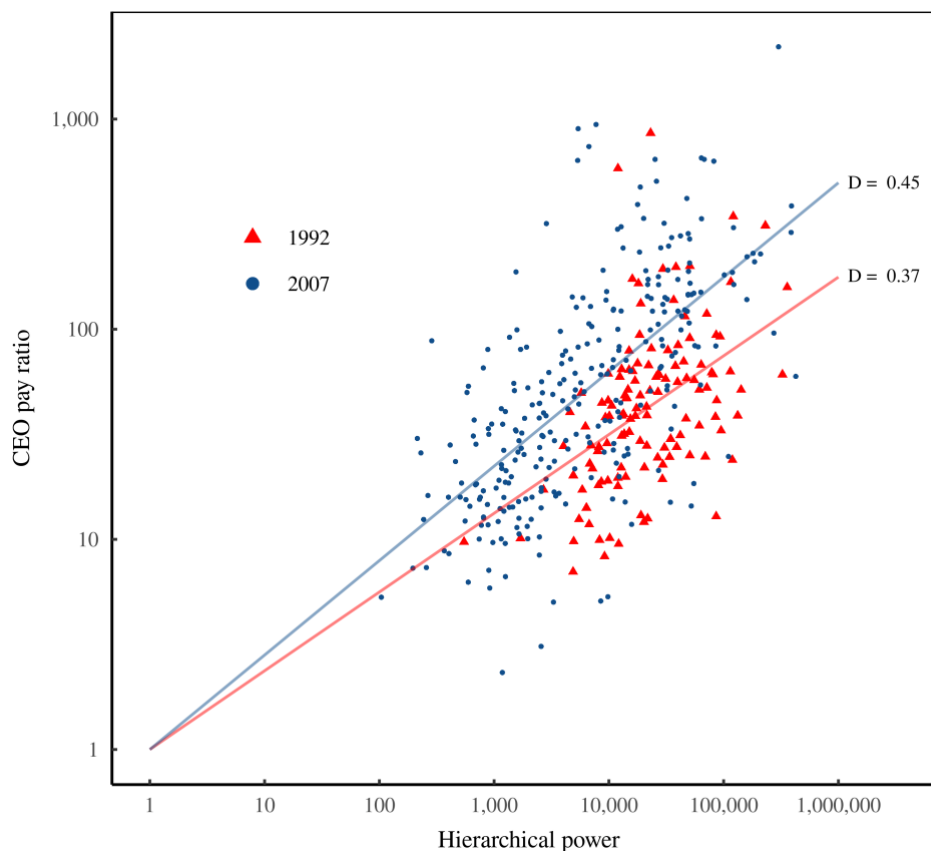
Based on the scatter in the empirical data (in Fig. 8), it seems clear that the 'degree of despotism' can vary between hierarchies. The question is, can the average degree of despotism also vary over time?

To answer this question definitively, we would need time-series data for the hierarchical pay structure of many different firms. Since such data does not exist, I propose a rougher approach: we use CEOs as despotism ‘canaries’. Among US CEOs, we know that income scales with hierarchical power (where the CEO’s hierarchical power is measured by firm size). What we do not know, though, is how this relation has changed with time.

To investigate this question, Figure 9 plots data for US CEO pay in two years: 1992 and 2007. In both years, the CEO pay ratio tends to increase with hierarchical power. Yet the rate of this increase differs. In 2007, CEO pay scaled more steeply with hierarchical power than it did in 1992. If CEOs are ‘canaries’ for a larger trend within firms, this result hints that US firms have become more despotic.

Figure 9: Changing hierarchical despotism among US CEOs.

This figure plots the relation between the CEO pay ratio and hierarchical power for US CEOs. I assume that CEOs command their respective firms, meaning their hierarchical power is equivalent to the firm’s employment. Data for 1992 is shown as red triangles. Data for 2007 is shown as blue circles. Lines indicate the trend line, which indicates the ‘degree of hierarchical despotism’, D . The evidence suggests that US firms have grown more despotic over the period shown. For sources and methods, see the Appendix. Note 1: By 1992, the pay ratio of US CEOs had already increased significantly from its low point in the 1970s. Unfortunately, the data used here (from Execucomp) begins in 1992, so we cannot observe ‘hierarchical despotism’ in earlier years. Note 2: I estimate hierarchical despotism, D , using a regression that is fixed through the point (1, 1). Although it is usually inadvisable to force a regression through a fixed point, this is a special circumstance. By definition, when a firm has 1 member, that person has a hierarchical power of 1. And since there is only one member, the ‘CEO pay ratio’ is by definition 1. It follows that the relation between the CEO pay ratio and hierarchical power must go through the point (1, 1).

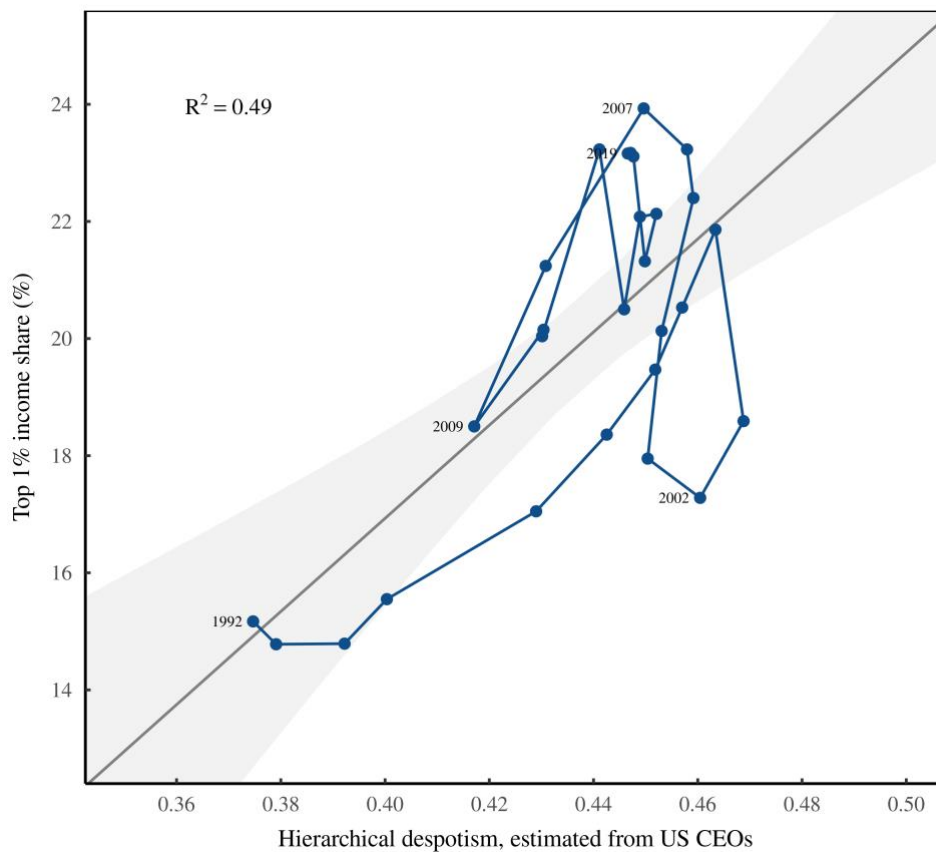


The next question is — does changing hierarchical despotism correspond with growing inequality? To test this possibility, we can generalize the method shown in Figure 9. In each year between 1992 and 2019, we regress the relative pay of US CEOs onto their hierarchical power. The result is a time-series estimate of the average degree of hierarchical despotism among US firms.

We want to know whether this changing despotism relates to rising inequality. The evidence, shown in Figure 10, suggests that it does. As my estimates for hierarchical despotism rise, so does the income share of the US top 1%.

Figure 10: Increasing despotism among US CEOs correlates with growing US inequality.

This figure generalizes the regression shown in Fig. 9. In each year between 1992 and 2019, I regress the pay ratio of US CEOs onto their hierarchical power. The slope of this regression is D , the estimated ‘degree of hierarchical despotism’ within these firms. Here, I show that this degree of despotism correlates with growing US inequality, as measured by the income share of the top 1%. For sources and methods, see the Appendix.



If US CEOs are indeed ‘canaries’ in the hierarchy, this evidence suggests that rising US inequality has been driven by growing despotism within firms. Ultimately, I would like to test this incendiary idea directly by peering into corporate hierarchies. But since big corporations are unlikely to open up their payroll structure anytime soon, we are forced to further test this idea using a more indirect route. On that note, let us return to the modelling work of Herbert Simon and Harold Lydall.

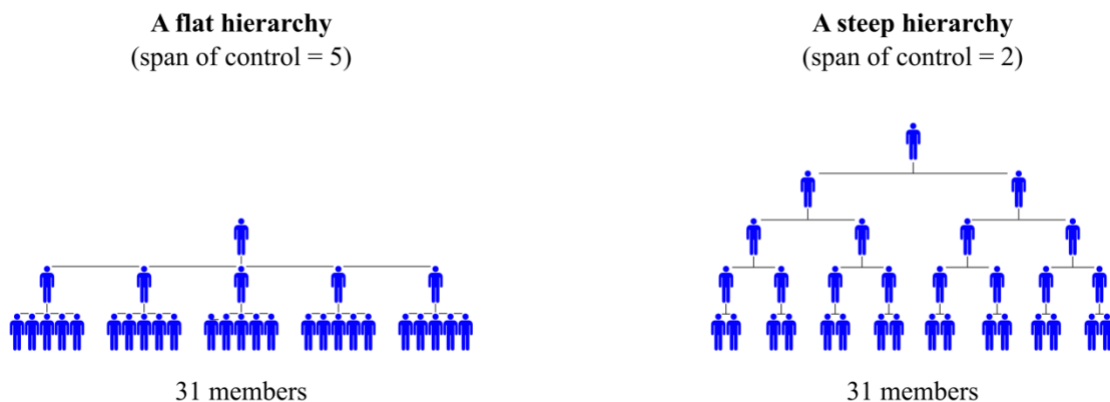
Returning to the Simon-Lydall model

In the 1950s, Simon and Lydall both used a simple model of hierarchy to explain the power-law behavior of top incomes. Simon showed how hierarchy could explain why CEO pay scales with firm size. And Lydall demonstrated that hierarchy could create a power-law distribution of income.

The key feature of the Simon-Lydall model is the ‘span of control’, which is assumed to be constant. The ‘span’ determines how many direct subordinates each superior controls. If the span is constant throughout the group, we get hierarchies that look like the ones shown in Figure 11. A large span of control creates a ‘flat’ hierarchy. A small span of control creates a ‘steep’ hierarchy.

Figure 11: The Simon-Lydall model of hierarchy.

In the Simon-Lydall model, hierarchies are assumed to have a constant span of control. A large span creates a ‘flat’ hierarchy (left). A small span creates a ‘steep’ hierarchy. For visualization purposes, I show here the actual chain of command within each hierarchy. However, the Simon-Lydall model only simulates aggregate membership within each rank. For model equations, see the Appendix.



The second key element of the Simon-Lydall model is that income increases exponentially with hierarchical rank. Merge this exponential function with the exponential behavior of the chain of command, and out pop power laws. In what follows, I generalize the Simon-Lydall model to understand how hierarchy affects the distribution of top incomes.

Unlike Simon and Lydall (who used analytic methods), I will build a numerical model. The model starts not with hierarchies, but with the size distribution of firms. Empirical evidence suggests that firm sizes are distributed according to a power law (Axtell, 2001). Based on this observation, I simulate a size distribution of firms by drawing random numbers from a discrete power-law distribution. The simulation is designed to roughly match the size distribution of firms in the United States.

The next step is to use the Simon-Lydall model to give each firm a hierarchical structure. Each individual in the firm is assigned a hierarchical rank, and from this rank we calculate their hierarchical power. (For the model equations, see the Appendix.)

I then model individual income as a function of hierarchical power. To make the model realistic, I introduce stochastic ‘noise’ into the power-income relation:

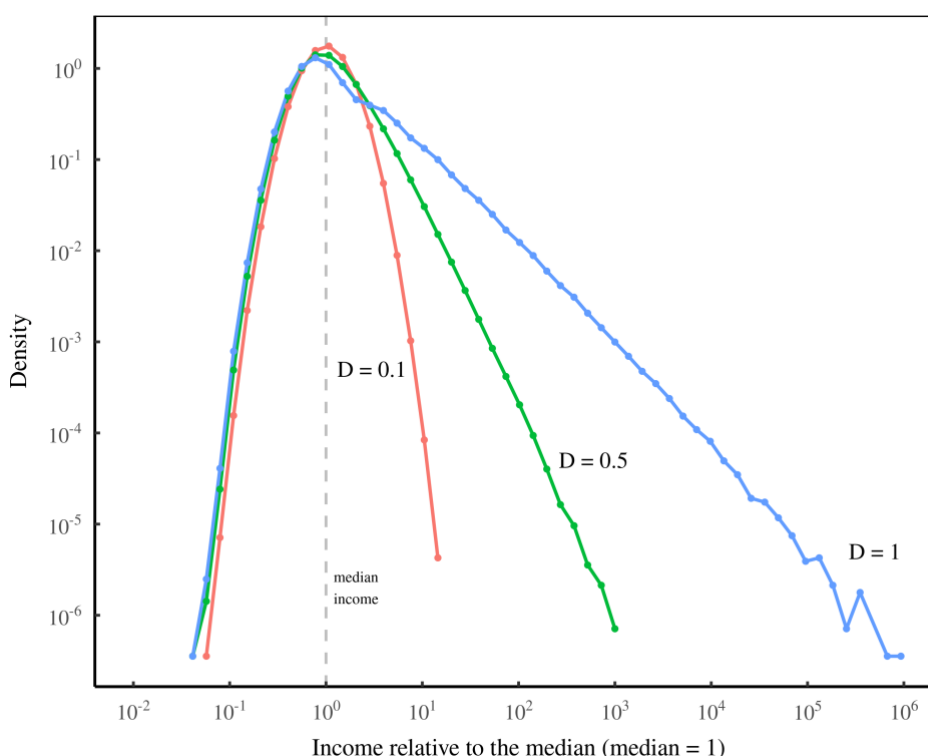
$$\text{income} = \text{noise} \cdot (\text{hierarchical power})^D$$

The output of the model is a simulated distribution of income. What we want to understand, from the model, is how the degree of hierarchical despotism, D , affects the distribution of top incomes.

Figure 12 shows my results. I have plotted here the distribution of income (using a log histogram) for three iterations of the hierarchy model. Each iteration uses a different value for D . As expected, the model produces a power-law distribution of top incomes, evident as the straight line in the right tail. (Note that when D is small, the income ‘noise’ dominates the distribution of income, so we do not get a power law.)

Figure 12: In a model of hierarchy, increasing hierarchical despotism fattens the income-distribution tail.

This figure shows results from my implementation of the Simon-Lydall model of hierarchy. In the model, income is assumed to scale with hierarchical power, where the scaling rate is D (a rate which I call the ‘degree of hierarchical despotism’). Varying D changes the distribution of top incomes. A larger value of D causes the distribution tail to get ‘fatter’. For sources and methods, see the Appendix.



What we are interested in is how the distribution of top incomes is affected by hierarchical despotism. On that front, the results are clear. Increasing hierarchical despotism ‘fattens’ the distribution tail. In short, it makes the rich get richer in a highly uniform way.

To summarize the evidence thus far, we know the following:

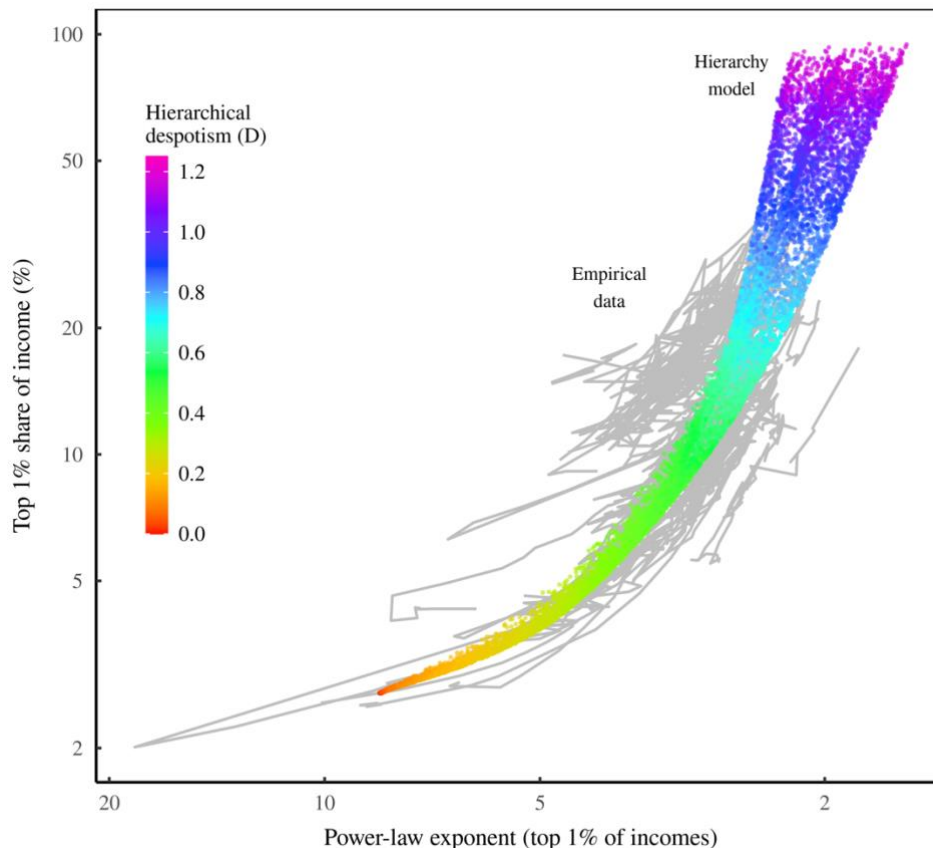
1. The United States has grown more unequal over the last 4 decades (Fig. 2);
2. This growing inequality occurred via a ‘fattening’ of the income distribution tail (Fig. 4);
3. Growing inequality is associated with a dramatic increase in US CEO pay (Fig. 7);
4. Like the redistribution of top incomes, the pay increases of US CEOs have an underlying uniformity: the rate at which income scales with hierarchical power seems to have increased (Fig. 9);
5. This increasing ‘hierarchical despotism’ among US CEOs correlates with rising US inequality (Fig. 10), suggesting that US hierarchies have become more despotic.
6. When we put changing hierarchical despotism into a model of hierarchy, we find that it produces a ‘fattening’ of the income distribution tail (Fig. 12).

All in all, this evidence strongly hints that hierarchy lies at the root of US income redistribution. But perhaps the US is a unique case. To test this possibility, the last step of the puzzle is to see if the hierarchy model can explain the redistribution of income observed across countries.

Recall from Figure 5 that across a wide swath of countries, greater inequality is associated with a smaller power-law exponent among top incomes. Figure 13 replots this data in grey. On top of the empirical data, I plot the trend produced by the hierarchy model. Each colored point represents a model iteration, with color indicating the degree of hierarchical despotism. As we ramp up despotism, the hierarchy model cuts through the middle of the path tracked by real-world countries.

Figure 13: Changing the degree of despotism within modelled hierarchies reproduces international trends in income redistribution.

Grey lines show the empirical trend within countries — the top 1% share of income plotted against the power-law exponent of top incomes. (The empirical data is replotted from Figure 5.) Colored points show iterations of the hierarchy model. By varying the degree of hierarchical despotism within hierarchies, the model reproduces the trend observed across countries. This result suggests that the redistribution of income consists largely of a change in hierarchical despotism. For sources and methods, see the Appendix.



Having noted the model's success, there are a few caveats. First, the model cannot reproduce the low levels of inequality observed in countries like Soviet-era Bulgaria (bottom left of Figure 13). That is because even when we remove all returns to hierarchical rank, there is still income 'noise', which generates inequality. We could change this noise if we desired. But to keep the model as simple as possible, I leave the noise function constant.

Second, the hierarchy model assumes a constant size distribution of firms, similar to the distribution found in the United States. In the real world, the firm size distribution varies both across countries and across time within countries. (See Fix, 2017 for details.) A more complex model could incorporate this firm-size variation.

Finally, in the Simon-Lyda model, the span of control is a free parameter. In the model used here, I let the span vary randomly between 1.2 and 13 — a range consistent with what we know from case studies of hierarchy. (See the appendix in Fix, 2019b for a review.) In the real world, we expect the span of

control to vary between firms and possibly between societies. Such patterns could be incorporated into a more complex model. That said, the span of control has a weak effect on inequality — far weaker than the effect of hierarchical despotism. (See Figure 14.)

To summarize, my model of hierarchy is highly stylized, neglecting many elements of the real world. But its purpose is not to be ultra-realistic, but instead, to isolate the effects of hierarchical despotism. And these effects are clear — increasing hierarchical despotism makes the rich get richer in much the same way as they do in the real world.

Conclusions

Despite the complexities of human life, the distribution of top incomes follows a remarkably uniform pattern. To a first approximation, top incomes are distributed according to a power law. And when income gets redistributed, this power law changes. In short, it seems that we can model the rich getting richer with a single parameter — the power-law exponent α . Such simplicity deserves an explanation.

The reason top incomes follow a uniform pattern, I have argued, is not because income has an ultimately simple cause. Instead, it is because the complex forces that shape income pass through a ubiquitous feature of human organization: *hierarchy*. Thus, I propose that hierarchy is a proximate cause of both the distribution of top incomes, and the uniformity with these incomes get redistributed when the rich get richer.

We have known since Lydall's work in the 1950s that hierarchy can produce a power-law distribution of top incomes. The more complex model used here confirms Lydall's result. I also find that by varying the rate that income increases with hierarchical rank, we vary the distribution of top incomes in much the same way as we observe in the real world. This result suggests that growing inequality is caused by a redistribution of income within hierarchies. Importantly, evidence from CEOs points at the same trend — namely, that growing inequality is associated with hierarchies becoming more 'despotic'.

Appealing to hierarchy, I have admitted, does not explain the root cause of inequality. To do that, we would need to explain why income within hierarchies scales the way it does (something that I do not attempt here). So, in a sense, the hierarchy model of income merely kicks the causal can: it explains one parameter (the power-law exponent of top incomes) in terms of another parameter (the degree of despotism within hierarchies).

Still, I consider that progress. It suggests that we can better understand the causes of inequality by studying the command structure of firms.

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Appendix

Source data and code for this paper are available at the Open Science Framework: <https://osf.io/h98gn/>.

Top income shares

Data for top income shares comes from the World Inequality Database (WID). For the long-term trend in US inequality (Fig. 2), I use the average of series `sfiinc992t` and `sfiinc999t`. These series are the closest to the measurements presented in Piketty (2014). International data (Fig. 5) is from WID series `sptinc992j`.

US income density

To estimate the density function for the US distribution of income (Fig 4), I use income threshold data from series WID `tfiinc999t`. This series reports the income thresholds for various income percentiles. From these thresholds, I first construct the cumulative distribution of US income. Then I take the derivative of this function to estimate the density curve.

Estimating power-law exponents

To estimate the power-law exponent of the top 1% of incomes, I use the method outlined in Virkar & Clauset (2014). They describe a maximum-likelihood function for fitting power-laws to binned data. The required data is:

1. bin thresholds;
2. counts within each bin.

The WID series `tptinc992j` provides the needed data. It reports income thresholds for various income percentiles. I use the various percentiles as the 'bins'. The percentile income thresholds are therefore the bin thresholds. And the bin count is simply the income percentile itself (i.e., the portion of the population it represents).

The caveat is that any data can be 'fitted' with a power-law exponent. But this does not mean that the data itself is distributed according to a power law.

US CEO pay ratio

Data for the US CEO pay ratio (Fig. 7) is from the Economic Policy Institute (Mishel & Wolf, 2019). I have plotted data in which stock options are measured using 'realized gains'. For why this is the most appropriate way to measure stock-option income see Hopkins & Lazonick (2016).

Relative income vs. hierarchical power

Data for the relative income within hierarchies (Fig. 6) is from a variety of sources:

- Case-Study Firms: Data is from Audas, Barmby, & Treble (2004); Baker, Gibbs, & Holmstrom (1993); Dohmen, Kriechel, & Pfann (2004); Lima (2000); Morais & Kakabadse (2014); Treble, Van Gasteren, Bridges, & Barmby (2001). For details about these studies, see the appendix in Fix (2019b).
- CEOs: The data covers the years 2006–2019 and includes CEOs across many countries (but mostly within the US). CEO pay data is from Execucomp, series TOTAL_ALT2. I estimate the CEO's hierarchical power from firm size — Compustat series EMP. I plot, in Fig. 6, the CEO's income relative to the average employee. I estimate average income in the firm by dividing employment expenses (Compustat series XLR) by firm employment. (Compustat series EMP). For more details, see Fix (2020).
- Note that the CEO data is not strictly comparable to the other series in Fig. 6 because it measures pay relative to the firm *average*. All other series, however, measure pay relative to the average in the *bottom rank* of the hierarchy.
- US military: Data is from annual demographics reports (*Demographics: Profile of the Military Community*) between 2010 and 2019. I exclude warrant officers from the data. I calculate the pay within each rank as the average of the minimum and maximum pay by years of experience. For details, see Fix (2019a).

Hierarchical despotism of US CEOs

The CEO data used in Figures 9 and 10 is slightly different than the CEO data used in Fig. 6. For one thing the Fig. 9-10 includes only US CEOs. But more importantly, the Fig. 9-10 data measures CEO pay using Execucomp series TDC1, rather than series TOTAL_ALT2. The latter series offers a better accounting of stock-option income (using realized gains). But it begins in 2006. In contrast, series TDC1 uses the (more dubious) Black-Scholes method to estimate stock option income. However, data for TDC1 extends back to 1992.

Hierarchy model

The hierarchy model used in this paper is based on equations derived independently by Herbert Simon (1957) and Harold Lydall (1959). In this model, hierarchies have a constant span of control. We assume that there is one person in the top rank. The total membership in the hierarchy is then given by the following geometric series:

$$N_T = 1 + s + s^2 + \dots + s^{n-1} \quad (10)$$

Here n is the number of ranks, s is the span of control, and N_T is the total membership. Summing this geometric series gives:

$$N_T = \frac{1 - s^n}{1 - s} \quad (11)$$

In my model of hierarchy, the input is the hierarchy size N_T and the span of control s . To model the hierarchy, we must first estimate the number of hierarchical ranks n . To do this, we solve the equation above for n , giving:

$$n = \left\lfloor \frac{\log[1 + N_T(s - 1)]}{\log(s)} \right\rfloor \quad (12)$$

Here $\lfloor \cdot \rfloor$ denotes rounding down to the nearest integer. Next we calculate N_1 — the employment in the bottom hierarchical rank. To do this, we first note that the firm's total membership N_T is given by the following geometric series:

$$N_T = N_1 \left(1 + \frac{1}{s} + \frac{1}{s^2} + \dots + \frac{1}{s^{n-1}} \right) \quad (13)$$

Summing this series gives:

$$N_T = N_1 \left(\frac{1 - 1/s^n}{1 - 1/s} \right) \quad (14)$$

Solving for N_1 gives:

$$N_1 = N_T \left(\frac{1 - 1/s}{1 - 1/s^n} \right) \quad (15)$$

Given N_1 , membership in each hierarchical rank h is:

$$N_h = \left\lfloor \frac{N_1}{s^{h-1}} \right\rfloor \quad (16)$$

Sometimes rounding errors cause the total employment of the modeled hierarchy to depart slightly from the size of the original input value. When this happens, I add/subtract members from the bottom rank to correct the error.

Once the hierarchy has been constructed, income (I) is a function of hierarchical power:

$$I = N(\bar{P}_h)^D \quad (17)$$

Here D is the ‘degree of hierarchical despotism’ — a free parameter that determines how rapidly income grows with hierarchical power. N is statistical noise generated by drawing random numbers from a lognormal distribution. (The noise function generates inequality equivalent to a Gini index of about 0.2.) $\bar{P}_h$ is the average hierarchical power (per person) associated with rank h . It is defined as:

$$\bar{P}_h = 1 + \bar{S}_h \quad (18)$$

where $\bar{S}_h$ is the average number of subordinates per member of rank h :

$$\bar{S}_h = \sum_{i=1}^{h-1} \frac{N_i}{N_h} \quad (19)$$

The model is implemented numerically in C++, using the Armadillo linear algebra library (Sanderson & Curtin, 2016). For R users, I have created R functions implementing the model, available at Github:

- github.com/blairfix/hierarchy
- github.com/blairfix/hierarchical_power

Size distribution of firms

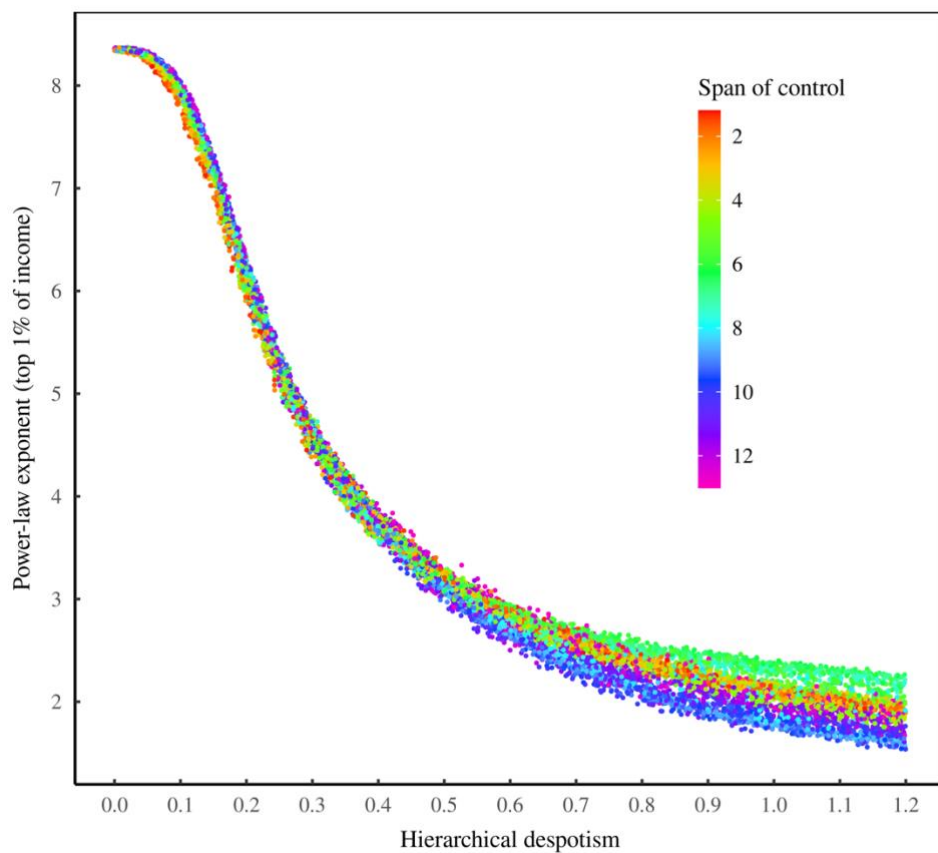
The input into the hierarchy algorithm is a size distribution of firms generated from a discrete power law distribution with $\alpha = 2$. The resulting distribution is similar to that found in the modern United States. See Fix (2020) for details.

The span of control

In the hierarchy model, the span of control is a free parameter. I let it vary between a low of 1.2 and a high of 13. As Figure 14 shows, this variation has a small effect on the power-law distribution of top incomes. Instead, the effect is dominated by the degree of hierarchical despotism.

Figure 14: In the hierarchy model, the span of control weakly affects the power-law distribution of top incomes.

Points represent different iterations of the hierarchy model, with the degree of hierarchical despotism shown on the horizontal axis. The vertical axis shows the resulting power-law exponent of top incomes. Color indicates the span of control, which has a weak effect on top incomes.



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The Becker model of discrimination is anachronistic and should no longer be taught

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Becker's model as well as the statistical model of discrimination should no longer be taught. They were conceived in the twilight of the Jim Crow era, an era in which future Nobelist George Stigler could write about the "Negro in America", that they were "inferior workers and the "problem is that on average he lacks a desire to improve himself, and lacks a willingness to discipline himself to this end" (Stigler, 1965). The models prevalent in the most popular textbooks continue to trivialize that the injustices associated with discrimination are worse than anachronistic. In the era of the BLM movement, they are in bad taste and should be seen as providing scholarly support for systemic racism.

Ever since Gary Becker's 1955 dissertation, the economic theories of discrimination fail woefully to appreciate the deep ethical nature of the problem and skirt its devastating impact on minorities. Becker's coldblooded reference to discrimination as a "non-pecuniary element" in transactions or as a "disutility caused by contact with some individuals" are typical of the pretense at objectivity of this literature (Becker, 1971, p. 13). His framing of the issue nonchalantly as a "taste for discrimination" makes it appear legitimate: essentially equating it with our taste for a consumption good (Charles and Guryan, 2009). The "taste for discrimination" thereby became a component of the benign theory of free choice and part of the democratic liberal tradition of market exchange between equals (Peart and Levy, 2021).¹ The theory also supposes that firms that discriminate will pay higher wages to whites which will lower their profits. Moreover, the blacks will be hired by non-discriminating firms which can, therefore, provide the product or service at a lower price. Supposedly, the higher profits of non-discriminating firms will attract other non-discriminating firms. Hence, the discriminating firm will be at a further disadvantage so that the internal logic of Becker's analysis suggests that the discriminating firm will be outcompeted, and discrimination will be mitigated (Lang and Spitzer, 2020). This theory should have been discarded decades ago, as it has been obviously falsified by an overwhelming amount of evidence, including experimental data (Arrow, 1998; Lang and Lehmann, 2012; Neumark, 2018).

Statistics was invoked to complement Becker's theory (Moro, 2018). In this theory discrimination became a rational response to the "scarcity of information about the... characteristics of workers.... If the cost of gaining information about the individual applicants is excessive, skin color or sex is taken as a proxy for relevant data not sampled. The a priori belief in the probable preferability of a white or a male over a black or female candidate... might stem from the employer's previous statistical experience..." (Phelps, 1972, 659).² Kenneth Arrow also proposed this analysis simultaneously and independently (Arrow, 1971).³

¹ It is also misleading, because it assumes that those who discriminate are making conscious decisions to do so based on a cost-benefit analysis, whereas the discrimination often occurs at the unconscious level (Bertrand, Chugh, and Mullainathan, 2005).

² To be sure, the author does add that "Discrimination is no less damaging to its victims for being statistical."

³ However, he did not refer to its statistical nature. "Skin color is a cheap source of information and therefore may be used by an employer in discriminating against what he believes to be inferior workers." At least Arrow did express "the greatest moral outrage" and "moral indignation" at his "dispassionate" analysis (Arrow, 1971, p. 27).

This line of overtly racist reasoning has been critiqued extensively (Darity, 1995; Darity, and Hamilton, 2012; Darity, and Mason, 1998; Hamilton, 2017; Mason, Myers and Darity, 2005; Shulman and Darity, 1989). Nonetheless, these theories have not only survived but still dominate, in the main, the discussion of discrimination in almost all popular textbooks without caveats and not only at the introductory or intermediate levels but also at the more advance level including in labor economics (Borjas, 2005, Chapter 10).⁴ Graduate lecture notes also focus on the mathematical elegance of these models without any caveats whatsoever (Autor, 2003).⁵ A discussion of the pernicious nature and injustices of discrimination and the social ills (such as widespread imprisonment) that stem from it are lacking. None emphasizes its illegal character, the urgency of ending it, or that laissez-faire market processes failed to end it.

Instead, economists frame the issue in such a way that “the market is exonerated” (Koechlin, 2019, p. 563). For instance, even liberal economists such as Samuelson and Nordhaus reiterate without caveats Becker’s argument that discrimination is self-correcting, because “Nondiscriminating firms could enter the market, undercut the costs and prices of the discriminating firms by hiring mainly brown-eyed workers, and drive the discriminating firms out of business.”⁶ Thus, even if some employers are biased against a group of workers, their bias should not be sufficient to reduce that group’s income”⁷ (Samuelson and Nordhaus, 2009, p. 261).

Subsequently, Samuelson and Nordhaus restate the concept of statistical discrimination by asserting that “One of the most interesting variants of discrimination occurs because of the interplay between incomplete information and perverse incentives.” Yet, there is nothing at all interesting about discrimination, and it is illegal to boot. However, they do at least add that “Statistical discrimination is particularly pernicious when it involves race, gender, or ethnic groups” (Samuelson and Nordhaus, 2009, p. 262). That is good to know, but what other kinds of discrimination are there? Age or gender discrimination? Are those less pernicious?

Similarly, Mankiw concludes that “at least some of the difference between the wages of whites and the wages of blacks can be traced to differences in educational attainment... In the end, the study of wage differences among groups does not establish any clear conclusion about the prevalence of discrimination in U.S. labor markets. Most economists believe that some of the observed wage

⁴ “Statistical Discrimination” brings up 21,000 hits on google scholar and “taste for discrimination” brings up 3,500 hits.

⁵ Kevin Murphy’s laudation of Becker’s work has a similar tone: “Becker’s analysis would extend the reach of economics, and completely reshape the field—and social-science research in general.” And the discrimination also hurts those who discriminate: “the discriminating employer incurs greater expense to obtain the same productivity” (Murphy, 2015).

⁶ In addition, they support Becker’s theory by framing the question of discrimination in terms of “blue-eyed” versus “brown-eyed workers”, which is, of course, ridiculous, and belittles its deeply corrosive nature, thereby avoiding the emotionally charged issue of real-world racial discrimination on the basis of skin color and not on eye color, especially as it pertains to the descendants of American slaves and all the social injustices that stem from that (Samuelson and Nordhaus, 2009, p. 261).

⁷ Becker’s theory has many hidden assumptions including that productivity is easily ascertainable prior to hiring someone. However, if that is not the case, then the mechanism he invokes may not work because the non-discriminating manager could assume that people are willing to work for less because they are less productive. Furthermore, it also assumes that non-discriminating firms which have enough capital to enter the market actually exist and that there are sufficient number of entrepreneurs who can withstand the social pressure of going against the cultural norm of discrimination and the violence of the klu-klux-klan. So, there are many reasons for refuting the theory rather than reproducing it.

differentials are attributable to discrimination, but there is no consensus about how much” (Mankiw, 2018, pp. 392, 393). Yet, of the circa 21% differences in wages about half is due to education (11%) and half to outright discrimination (10%) (Altonji and Blank, 1999, Table 5). Of course, the difference in educational attainment is also due to (pre-market) discrimination (MacLean, 2021). Mankiw continues with Becker’s argument that, “the profit motive is a strong force acting to eliminate discriminatory wage differentials, but there are limits to its corrective abilities. Two important limiting factors are customer preferences and government policies” (Mankiw, 2018, p. 395). Note that in this framing of the issue, the government is part of the problem that limits the market’s ability to shed itself of discrimination. Such perspectives are repeated in other contexts as well: “employers who discriminate pay an economic penalty” (Hubbard et al., 2013, p. 388).⁸ That, in the main, is the tenor of most of the canon on discrimination.

Another overlooked factor in the above assertions is the use of violence in suppressing upward mobility of minorities. It does not need to be practiced on a daily basis to be effective. One lynching can stifle ambitions for generations. For instance, the destruction of “Black Wall Street” in Tulsa in 1921 sent a signal that still resonates (Darity and Mullen, 2020). The unmistakable message, that it is useless for blacks to attempt to accumulate wealth, does not fit well into the above narratives.

In contrast, some progressive economists do strike a different tone (Bruegel, 2018; Schneider, 2019, p. 519).⁹ They point out that discrimination “was based on racist beliefs that certain groups were innately inferior” and that it has been against the law since 1964. They also refer to a case study of FedEx which was fined \$3 million for violating that law (Goodwin et al., 2015, pp. 238-240). Nonetheless the dominance of orthodox theory means that “a student is likely to leave ECON 101... with a sense that ‘economic science’ has ‘shown’ that discrimination is not that big a deal...” (Koechlin, 2019, p. 563).

To be sure the recognition is growing that “economics has a diversity problem” (Bayer, Hoover and Washington, 2020, p. 217; Kvangraven, Harvold, and Kesar, 2020), but they fail to acknowledge that a discipline that trivializes discrimination and dubs prejudice a “taste” will be naturally shunned by minority students. It should also be obvious that market mechanisms were incapable of reducing, let alone eradicating the evils of discrimination. Therefore, a canon that adulates unfettered markets will likely appear objectionable to the descendants of slaves. Thus, to continue to teach the Beckerian or the statistical models of discrimination, conceived in the twilight of Jim Crow era, that trivializes the injustices associated with discrimination is worse than anachronistic. In the era of the BLM movement, it is in bad taste and should be seen as providing scholarly support for systemic racism (Komlos, 2020, 2021). These unsubstantiated and covertly racist theories should be expunged from the canon of the 21st century and relegated to the dustbin of history.

⁸ “Do not underestimate the power of markets to offer at least a degree of freedom to oppressed groups. In many countries, cohesive minority groups like Jews and emigrant Chinese have managed to carve out a space for themselves through their economic activities, despite legal and social discrimination against them” (Taylor, Greenlaw, and Shapiro, 2018, p. 341).

⁹ Even liberal economists confirm the conventional reasoning that markets are beneficial and government is not: “market forces tend to work against discrimination.... Discrimination has sometimes been institutionalized in government policy. This institutionalization of discrimination has made it easier to maintain it against market pressure.... Companies that engage in workplace discrimination but whose competitors do not are likely to have lower profits as a result of their actions” (Krugman, Wells, Olney, 2007, pp. 229-230). Never mind that this institutionalization ended in 1964 in the U.S. so why has the market not ameliorated the problem in the intervening half century?

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“Susan Strange saw the financial crisis coming, Your Majesty”: The Case for the LSE’s Great Global Political Economist

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Abstract

This paper makes three arguments. First, that Susan Strange (1923-1998), who founded the field of international political economy in the UK, is one of the principal thinkers who foresaw multiple aspects of the global financial crisis of 2008 and its aftermath. She deserves credit for her prescience but has been overlooked. Second, that Strange’s insight was not a series of lucky guesses but emerged from a rich and consistent theoretical and philosophical understanding of “the political economy of the planet”.² Third, that Strange should interest economists today and be thought of as a ‘worldly philosopher’ – after Heilbroner’s term for the first political economists – rather than a figure in a sub-discipline of international relations. With the centenary of her birth in 2023, the time is ripe to reassess this great global political economist of the London School of Economics. It is time to put Strange back into the story we tell about financial globalisation and its discontents.

A Strange omission

Some thought history had stopped at the end of the Cold War. The global financial crisis of 2008 is one of the pivotal events which showed, if anything, it was accelerating. In the months after, economists and other policy elites scrambled to understand how the impossible had happened. The financial crisis had not erupted in a relatively under-developed region (think of Mexico in 1995 or Thailand in 1997), but inside the advanced and apparently sophisticated Western financial system.

One of the best early books to dissect what went wrong was *Fool’s Gold* by the *Financial Times*’ Gillian Tett. Published less than a year after the bankruptcy of Lehman Brothers, it offers both a brilliant exposé of the bankers who dreamt up the financial ‘weapons of mass destruction’ and an analysis of the intellectual failures behind the worst economic freeze since the Great Depression. Tett points to a “social silence” around credit derivatives which allowed the global banking system to spin almost unnoticed out of control. She despairs at the fragmentation of

¹ Nat Dyer is an independent researcher and writer and a Fellow of the Schumacher Institute. He is writing a book on ‘theory-induced blindness’ in economics going back to Ricardo and helping to organise a conference in London for the centenary of Susan Strange in 2023. Find out more on Twitter [@natidyer](#) and at www.earthriseblog.org.

² John Pinder’s introductory remarks for Susan Strange’s talk on ‘The limits of politics’ at the LSE talk on 1 June 1995: “You could say that now her subject was the political economy of the planet”. <https://digital.library.lse.ac.uk/objects/lse:qaq423fok>

knowledge, the absence of “a more *holistic* vision of finance” and a lack of generalists and “cultural watchdogs”.³ The analysis is spot on, but Tett gets one big thing wrong.

She writes that the chain of links between the credit derivatives boom and real people was “so convoluted it was almost impossible for anybody to fit that into a single cognitive map.”⁴ *Impossible? No one saw the crisis coming?* It’s a story often repeated by politicians and central bankers. It’s wrong, in part, because it overlooks a remarkable woman, a former journalist and researcher, who became one of the most influential academics in Britain in the late twentieth century: Susan Strange.⁵

Tett is not alone. Famously, Queen Elizabeth II asked professors at the London School of Economics (LSE) in 2008 why they did not notice the crisis coming. The moment has become a founding story of ‘new economics’. In the aftermath, the net was cast wide to catch those who did anticipate parts of the crisis. Many names have been put forward including Dean Baker, Steve Keen, Nouriel Roubini, Ann Pettifor, Raghuram Rajan, and Hyman Minsky.⁶ But, a name that’s missing from virtually every list was much closer to home, hiding, like the credit boom, in plain sight. Strange arguably foresaw more aspects of the crisis than anyone else. Ironically, in light of the Queen’s question, she taught global economics and politics for almost thirty years at the LSE.⁷

Of all the answers to the Queen’s question from economists, commentators, and academics, one of the simplest and most truthful would have been: “The LSE’s Susan Strange saw the financial crisis coming, Your Majesty”.⁸ If the Queen had got that answer, we can imagine she may have asked: “Really? How? What else did she say?” This paper offers some answers through a close reading of Strange’s writing, and of those inspired by her, ending with a call for renewed engagement with a unique and undervalued thinker.

³ Gillian Tett (2010) *Fool’s Gold*, Abacus.

⁴ *Ibid.*

⁵ When writing *Fool’s Gold*, Tett says that she “didn’t know about Strange and wished I had since it looks incredibly relevant!” She added that Strange “looks very prescient though! And deserves credit!” (personal communication with author, August 2021).

⁶ See for example: Fullbrook, E. (2010), ‘Keen, Roubini and Baker win Revere Award for Economics’, in Fullbrook, E. (ed), *Real World Economics Review Blog*. Real World Economics Review, New York;

<https://rwer.wordpress.com/2010/05/13/keen-roubini-and-baker-win-revere-award-for-economics-2/> and Bezemer, D.J. (2009), “No One Saw This Coming”: Understanding Financial Crisis through Accounting Models. Faculty of Economics University of Groningen, Groningen, The Netherlands.

⁷ Contrary to an often-repeated biographical myth, Strange did not come to academia after her journalism career. She began teaching at UCL at the age of 26 in 1949 – while working at *The Observer* in London – and continued in academia until her death in 1998. She left *The Observer* in 1957. She first taught part time at the LSE from 1959, and then more regularly every year from 1964 (after she left UCL) until her forced retirement due to age from LSE in 1988. She taught at the LSE for 29 years and in academia for 49 years.

⁸ For other responses to the Queen see: Besley, T and Hennessy, P (2009), “The Global Financial Crisis – Why Didn’t Anyone Notice?”, *British Academy Review*, Issue 14 (November) and Neate, R (2012), “Queen finally finds out why no one saw the financial crisis coming”, *The Guardian*, 13 December 2012 - <https://www.theguardian.com/uk/2012/dec/13/queen-financial-crisis-question>

Did Strange really see the global financial crisis coming?

The 2008 crisis crashed not only stock prices, banking institutions, and the prospects of millions, but also economic theories - especially, the neoclassical macroeconomic theories which dominated policy-making and leading universities in the US and UK. They were bankrupted by awkward and undeniable facts. Global financial markets revealed themselves to be far more anarchic, less rational, and difficult to manage than believed. Sophisticated financial products such as derivatives made the financial system more not less risky, especially over-the-counter (OTC) trades. Senior bank managers and regulators did not understand the complex transactions. Banks – which had fought for decades for a hands-off approach from governments – had to be bailed out. The chief financial danger came from America.

What's remarkable about these unwelcome truths – ignored or denied for decades before the crash – is that Susan Strange saw them all, and repeatedly, clearly and loudly warned of the risks they posed to the global system. Strange also foresaw aspects of the aftermath: that the US Federal Reserve would be the only organisation able to provide enough dollars to keep the global economy from freezing solid. And that the rise and fall of global finance would alienate ordinary people and help fuel a right-wing political backlash.

I will take each of these claims in turn, providing supporting quotations from some of Strange's six books and over 120 articles.⁹ The case is not that Strange predicted X event on Y date. Precise predictions in human global affairs are not possible, she argued, in what is an irrational, emotional and organic world, not a mechanical one. Like Hyman Minsky, who is held up as a prophet of the crash, Strange died a decade before it hit. She did, however, with stunning prescience accurately identify time and again the major trends.

In 1986, Strange, then the Montague Burton Professor of International Relations at the LSE, published a book called *Casino Capitalism*. It was the year of the 'Big Bang' deregulation of London financial markets. The book argued that the past fifteen years had seen a global revolution in credit creation which impacted almost every corner of world affairs. Strange picked out the dangers of derivatives and other innovative financial products which could lead to a contagious, global financial panic: "far from stabilizing the system by damping its ups and downs, the devices such as futures markets – developed to deal with uncertainty – have actually served to exaggerate and perpetuate it."¹⁰ She warned about increased market volatility as "new credit instruments are thought up every month, and new assets 'securitized' – which means that the banks find new ways of passing on to a speculative market the loans they have made to homeowners or businesses".¹¹ Although she wrote little about the US real estate market, she had her finger on the right spot.

Contemporary reviewers understood her message well enough but not all believed it. *The Economist* – where Strange started her career in the 1940s after a First in Economics at the LSE – summarised *Casino Capitalism's* message neatly: "financial innovation, she argues, has

⁹ May, C, (2014) "An Annotated Bibliography of Susan Strange's Academic Publications 1949-1999", Fourth Edition.

¹⁰ Strange, S (1986), "Casino Capitalism", Manchester University Press, p. 119.

¹¹ *Ibid.*, p. 174.

put the world economy at the mercy of Wall Street gamblers who are “playing Snakes and Ladders with people’s lives”. The book was “well written” “enjoyable” and “original”, *The Economist* said, but it criticised Strange’s “misjudgements” chiefly the idea that “financial innovation is itself destabilising” and that the Federal Reserve may have to play a greater role in calming global markets.¹² In hindsight, it’s an embarrassing mistake for the magazine. After the crash, *The Economist* would hold up its hands for only mentioning Hyman Minsky once during his lifetime. It has not offered a similar apology to Strange. She was only mentioned twice. The second reference was for her 1998 book *Mad Money*, in which she argued that the “erratic, unpredictable, irrational behaviour” of financial markets were best understood as a form of insanity.¹³ *The Economist* dismissed the book in a few lines under the headline, “Beware of the Bears”.¹⁴

Strange warned of the risk of sophisticated financial products in other venues too. Her address as President of the US-based International Studies Association (ISA) in 1995 is renowned for her dismissive remarks about feminist scholars. (Strange spoke her mind bluntly and was almost incapable of holding her tongue). She also told delegates, according to the published version of her speech: “The financial market dealers insist that futures, options, and other derivatives are efficient devices to protect their clients from risk and uncertainty. But it is not so certain that the system as a whole is protected – as taxpayers in Orange County are now aware.”¹⁵ Although forgotten today, the reference is to Orange County, California which went bankrupt in 1994 after losing \$2 billion on a bad bet on interest-rate derivatives.¹⁶ Her insight into systemic risk was far ahead of her time.

In *Mad Money*, Strange devoted a whole chapter to financial market innovations and the “truly phenomenal... explosion of dealing in derivatives” which allowed banks to make huge profits betting ‘on margin’ for themselves.¹⁷ She also – remarkably – singled out for concern the credit default swaps (CDS), at the heart of the later global financial crisis. The story of CDSs would only become widely known after 2008 in press reports and books like Gillian Tett’s *Fool’s Gold*. Strange wrote: “in May 1997, credit derivatives were one of the most talked about new products in financial markets. These new contracts allowed a bank to sell off to someone else the risk that one of its borrowers might default. First thought up in New York five years before as default swaps or default options, they allowed a bank to pay an investor another kind of insurance premium for taking on the risk and becoming liable to the original lender if the borrower did not pay up – or even if its estimated ability to pay – its credit rating with one of the bond rating

¹² The Economist (1986), “Russian roulette.” 28 June 1986, tinyurl.galegroup.com/tinyurl/5BRHv6. Accessed 11 Sept. 2017.

¹³ Strange, S (1998) “Mad Money”, Manchester University Press, p. 1.

¹⁴ The Economist, (1998) “Beware of the Bears”, October 17th 1998: <https://www.economist.com/review/1998/10/15/beware-of-bears>. Accessed 17 August 2021.

¹⁵ Strange, S (1995) “1995 Presidential Address: ISA as a Microcosm”, *International Studies Quarterly*, Vol 39, No. 3, (Sept.), pp. 289-295.

¹⁶ Chicago Tribune (1994) “The \$2 Billion Gamble”, 19 December 1994: <https://www.chicagotribune.com/news/ct-xpm-1994-12-19-9412190147-story.html>

¹⁷ Strange (1998), *op cit*, p. 30.

agencies – were to fall.”¹⁸ Perhaps the pre-crash problem was selective listening and not ‘social silence’ as Tett suggests.¹⁹

Presciently, Strange warned that derivatives trades were so opaque and the risks so interconnected, that it would be almost impossible for a bank to know its exposure. It is the “OTC [over-the-counter] trading which is least transparent, in which the area of significant ignorance of what is really going on is most extensive... every deal is made in ignorance of other OTC deals. Neither of the parties knows how exposed the other is.”²⁰ Most other commentators would only catch up when the Bear Stearns’ disastrous balance sheet was revealed ten years later. Strange’s *Mad Money* was published just two weeks before she died, and on its concluding page she suggested the US make the contracts for “the most bizarre and sophisticated kinds of derivatives... not illegal but legally unenforceable”. Making this change would “shift the risk concerned back from the market to the risk taker – possibly giving rise to second and more prudent thoughts.”²¹ Characteristically, Strange was realistic enough to see that this was unlikely – that personal experience moved people more than intellectual argument. The final line of her final book reads: “Perhaps, therefore, money has to become really very much more mad and bad before the experience changes preferences and policies.”²² Although Strange did not write in great detail about the mechanics and players in the derivatives trade – as Matthew Watson writes “she never really takes the reader inside the casino”²³ – her assessment of its importance and danger are on point. In her honour, fellow LSE academic Nigel Dodd calls the toxic assets which blew up in 2008 ‘Strange Money’.²⁴

Strange was aware too of how the internal and external regulators of the derivatives trade had dropped the ball. She saw it as “wildly foolish” to “let the financial markets run so far ahead, so far beyond the control of state and international authorities”.²⁵ She doubted whether internal

¹⁸ *Ibid*, p. 31.

¹⁹ Gillian Tett says that she “would take issue” with the idea that CDS’s were widely talked about: “It may have been most talked about among a small group of financiers but not more widely!” (personal communication with author, August 2021).

²⁰ Strange (1998), *op cit.*, p. 32.

²¹ The full quote is: “This is a weapon that leading governments like that of the United States could use if they wished against the most bizarre and sophisticated kinds of derivatives. As mentioned in chapter 2, it is the fancy new ones that generate most profit. To make such contracts not illegal but legally unenforceable would shift the risk concerned back from the market to the risk taker – possibly giving rise to second and more prudent thoughts” from Strange (1998), *op cit.*, p. 190.

²² *Ibid.*, p. 190.

²³ Watson, M. (2016) “Introduction to Casino Capitalism”, in *Casino Capitalism* published by Manchester University Press.

²⁴ Dodd, N. (2011), ‘Risk, Finance, and socialized debt’, *The British Journal of Sociology*, Vol 62, Issue 1, March, pp. 175-194. <https://onlinelibrary.wiley.com/doi/abs/10.1111/j.1468-4446.2010.01349.x>

²⁵ Strange (1998), *op cit.*, p. 1.

supervisors²⁶ or bank executives²⁷ understood these complex trades. The lack of understanding, she argued, was widespread leading to what she termed “significant ignorance”.²⁸ International regulators were no better. Strange saw that the Bank of International Settlements (BIS) had, by 1996, shifted its position to allow banks to regulate themselves and “virtually thrown in the towel on capital adequacy rules [and] abandoned, in effect, the whole idea of agreed common standards of banking supervision.”²⁹ At the time, this was written off as her tendency to exaggerate.³⁰ Who would argue with her analysis today?

One of the defining images of the 2008 crisis, at least for those in the UK, were the queues of Northern Rock customers snaking along high streets in the first run on a British bank for 150 years. It wasn't customers withdrawing money that sunk the Rock, but its reliance on short-term loans from the interbank money market. Although she couldn't have foreseen the details, Strange was well aware of the risk of this banking practice. In the mid-1980s she wrote, “Nor does the major threat to the system come from small depositors who might take their money away from a particular bank, but rather from the other banks who, when rumours begin to fly, refuse to extend interbank credit lines to it”.³¹ The dependence of banks on the interbank market was “a dark secret”, she wrote, and “perhaps the most important weak point in the whole information picture”.³² It added up to a “game of Blind Man's Bluff” where creditors and governments were “blindfolded”.³³ Again, what may have seemed an exaggeration of the time, now appears as prescient common sense. Strange was also aware of the danger from the shadow banking system and non-banks such as insurance companies, which was revealed in 2008.³⁴

The mega bailouts of banks, firms, and insurance companies in the wake of the 2008 crash would have fascinated but not surprised Strange. She would likely have seen the bailouts as a continuation of a long trend. She regularly pointed out the US government bailouts in the

²⁶ “Doubt arises first over the ability of internal bank supervisors to keep up with accelerating technological change in finance” from Strange (1998), *op cit.*, p. 168.

²⁷ “And even in the most prudent banks these days, the complexities of derivative trading are often beyond the comprehension of elderly managers.” From Strange, S. (1999), “The Westfailure System”, *Review of International Studies*, Vol 25, Issue 3.

²⁸ “The appreciation in financial circles of the power of those forces [financial innovation] and the analysis of their significance for regulatory policy makers are lagging far behind...” from Strange (1998), *op cit.*, p. 28.

²⁹ See *Ibid*, p. 168 and p. 171. Strange did not live to see, and comment on, the work of Claudio Borio and others at the BIS on macroprudential regulation prior to 2007/8.

³⁰ See Watson, M (2016) *op cit.*, who defends Strange against critics of her “alleged tendency towards hyperbole”.

³¹ Strange (1986), *op cit.*, p. 160.

³² “Perhaps the most important weak point in the whole information picture is the absence of information about interbank lending... how much a particular bank is depending at any point on the interbank market, either for deposits or from the profit from loans, is a dark secret... And the importance of this is easily seen when any bank begins to get into trouble... the extent of the ignorance on this point is important... to the whole tangled question of the global money supply.” From *Ibid.*, p. 141.

³³ *Ibid.*, p. 143.

³⁴ “The other way in which technology has got ahead of regulation is in the development of banking by non-banks.” In Strange (1986) *op. cit.*, p. 54.

Savings and Loans crisis in the early 1980s, in the collapse of Continental Illinois in 1984 (the largest bank failure in US history before Lehman Brothers), and of the US government rescue of its banks in the Mexican peso crisis of 1995 among others. All this contributed, she said, to a “belief that in the last resort the government would never let the big banks fail for fear of the effects on the US economy”.³⁵ It was a toxic belief, she wrote, because large bank’s “too-big-to-fail status actually encourages them to take more risk than they might have done without the status”.³⁶ After 2008, this is a standard analysis, but in the 1980s and 1990s, it put Strange on the fringes of a debate.

As we saw, Gillian Tett laments the absence before the crisis of someone with a holistic vision of finance, who could bring together the credit derivatives boom and real people in “a single cognitive map”. Strange, I would argue, from the early 1980s onwards comes closest. Much is in this passage from *Casino Capitalism*: “Because of... the ignorance among the controllers and regulators of what the bankers and speculators are up to, it must be expected that we have not seen the last financial scandal or the biggest banking collapse. Another Continental Illinois cannot be ruled out. And the outcome will be the same, that the US government has to take over liabilities and debts because the consequences for the economy at large of doing otherwise would be too great. The nationalization of banking will be taken a step further. But it is the political repercussions of this depressing economic scenario which are far more serious. When a whole generation becomes disillusioned with the economic system... there are bound to be political reactions.”³⁷

Strange had a holistic vision: a powerful synthesis of economics, politics, and international relations which she helped pioneer called international political economy (IPE). In her field, she’s known as the person who argued correctly – and again presciently – that the USA was still the dominant global power in the 1980s. Her understanding of the USA’s ‘structural power’ allowed her to appreciate dynamics that baffled many economists. “The chief danger to the system has come, and still comes, from the American banks”, she wrote plainly in 1986.³⁸ And apparently paradoxically, in a global crisis, “only the United States can convincingly offer”, she wrote, “support against a liquidity shortage in dollars”.³⁹

In the immediate aftermath of the 2008 crisis, it was the US government – the Federal Reserve – which helped to steady the global economy, more than the International Monetary Fund. Days after Lehman collapsed, the Fed struck deals with the UK, Canada, and Japan to loan them billions of dollars. To do so, the Fed revived a technique rarely used since the 1960s called a

³⁵ “The US government came to the aid of US banks over-extended in Mexico and Brazil in the 1980s. The rescue of Continental Illinois in 1984 only confirmed their belief that in the last resort the government would never let the big banks fail for fear of the effects on the US economy.” Strange, S. (1994), “States and Markets”, Second Edition, Continuum, p. 100.

³⁶ Strange (1998), *op cit.*, p. 189.

³⁷ Continental Illinois was the largest ever bank failure in the US until Lehman Brothers in 2008. It was bailed out by the US government in 1984 which led to the popularisation of the phrase ‘too big to fail’ from Strange (1986), *op cit.*, p. 192.

³⁸ Strange (1986), *op. cit.*, p. 161. See also p. 175.

³⁹ *Ibid*, p. 166.

'credit swap line'.⁴⁰ Similar agreements had been made the year before with the European Central Bank and Switzerland. The sums were enormous. In a single week in late October 2008 the Fed lent \$850 billion through swap lines and total swaps surpassed \$10 trillion.⁴¹ The credit swap network was extended to more countries and, in 2013, the Fed made it permanent. The Fed effectively bailed out the world: it became the lender of last resort to the entire global banking system. Historian Adam Tooze calls it "historically unprecedented, spectacular in scale and almost entirely unheralded".⁴² These developments over ten years after her death, are another feather in Strange's cap. Strange knew about credit swap lines as she had studied and written about them in the 1960s and 1970s while working for Chatham House (the Royal Institute of International Affairs). Eric Helleiner has argued that the USA acted in "the manner precisely predicted by Strange in a prescient analysis 25 years before the crisis."⁴³ At the time, however, few people listened. *The Economist* in 1986 branded Strange's insight that the power of the Federal Reserve was likely to increase "a fantasy".⁴⁴

Finally, few will fail to see echoes of recent tumultuous, strongman politics in Strange's insight that without corrective action, the public will either switch off from politics or follow a populist leader "who will wrap a hodge-podge of social prejudices in a package of phoney history and phoney science that arouses strong and violent nationalist emotions".⁴⁵ Or in her warning that the "increasing concentration of wealth" excluding so many could make "the desperation of the 1930s" she knew from her childhood "just a small foretaste of social reactions in the twenty-first century". Just last year, Robert Skidelsky echoed this warning: "Future historians," he wrote, might see "financial-led globalisation as the root cause of the tribulations of the twenty-first century".⁴⁶ Have we not yet seen money at its most mad and bad?

Strange has only received scattered praise for her stellar record. Her LSE colleague Fred Halliday praised her "prophetic insight" in 2008 and Susan Sell has written of how Strange "warned us about this [the 2008 crisis] years ago".⁴⁷ Mostly she has been ignored. James K. Galbraith has complained that economists who were right on the financial crisis have been overlooked. "They are not named. Their work is not cited. Their story remains untold. Despite having been right on the greatest economic question of a generation—they are *unpersons* in

⁴⁰ Tooze, A. (2018) "Crashed: How a Decade of Financial Crises Changed the World", Allen Lane/Penguin.

⁴¹ *Ibid.* pp. 213 and 214.

⁴² *Ibid.*, p. 203.

⁴³ Quote from Germain, R (ed.), (2016) "Susan Strange and the Future of the Global Political Economy", Routledge. See Helleiner, E, "Still an extraordinary power after all these years: the US and the global financial crisis of 2008" in Germain (2016).

⁴⁴ *The Economist* (1986), *op. cit.*

⁴⁵ Strange (1986), p. 192

⁴⁶ Skidelsky, R. (2021) "What's Wrong With Economics?", Yale University Press. Strange does not get a mention in the 200-page book.

⁴⁷ Halliday, F (2008), "The revenge of ideas: Karl Polanyi and Susan Strange", *Open Democracy*, 24 September; Sell, S (2014), "Ahead of her time? Reflections on Susan Strange's contributions to contemporary political economy", for a workshop on Susan Strange at Princeton

the tale”.⁴⁸ He names twenty-two economic thinkers who got it right, but Strange is not one of them.⁴⁹ She is still an unperson.

Strange did not get everything right, of course, and many aspects of her work have dated. Reading *Casino Capitalism* takes you back to a world where the USSR exists, and *Mad Money* to when Japan, not China, was the biggest threat to US dominance. Strange deserves to be honoured as one of the principal thinkers who foresaw the global financial crisis of 2008 and its immediate aftermath. But, did this prescience emerge from genuine insight or a series of lucky guesses? If genuine, what was the framework that knit her thinking together?

How did she see it coming? Strange's 3D glasses

There is a paradox at the heart of Susan Strange's reputation. Despite being acknowledged as a forceful and influential intellectual who “was almost single-handedly responsible for creating ‘international political economy’... in Britain”,⁵⁰ she's often dismissed as a journalist rather than an academic: a “naïve empiricist” who had no real theory.⁵¹ Benjamin Cohen, in his history of IPE, largely dismisses her ideas.⁵² Matthew Watson writes: “If Strange is to be viewed as a trailblazer, it is often said, then it is for a style of IPE for which the trail has gone cold.”⁵³ Her legacy in economics – orthodox or heterodox – is virtually non-existent. The portrayal of Strange as a shallow thinker is a misreading of, and an injustice to, her work. Strange's prescient, and uncelebrated, insights into the global financial crisis emerged from a coherent cognitive map: they were no fluke.

Strange's way of looking at the world helped her foresee the crisis in a number of ways. Foremost, because she gave no credence to the idealised model-world of neoclassical and neoliberal economics. In an article called ‘The Bondage of Liberal Economics’ published in 1986, she wrote: “I do not take too seriously the pretensions – or most of the conclusions – of the liberal economic doctrine. On the contrary, I am beginning to think that some of these concepts and ideas actually impede understanding and mislead people in their perception of the main problems confronting the world today”. She advises students “to devote more of their time to reading economic history and less to learning how to decipher the algebraic equations

⁴⁸ Galbraith, James K. (2009) “Who are these economists, anyway?”, *The NEA Higher Education Journal*, Fall. Italics as per the original.

⁴⁹ Galbraith says that his list is “far from comprehensive. *Ibid.*”

⁵⁰ Brown, C (1999), “Susan Strange – a critical appreciation”, *Review of International Studies*, 25, 531-535.

⁵¹ “As a result Susan can easily be mistaken for a naïve empiricist driven by strong moral convictions.” from Palan, R. (1999), “Susan Strange 1923-1998: A Great International Relations Theorist”, *Review of International Political Economy*, Vol 6, No 2, (Summer), p. 122.

⁵² Cohen, B.J. (2008), “International Political Economy: An Intellectual History”, Princeton University Press.

⁵³ Watson, M (2016) *op cit.*, p. viii.

so beloved by the economists.”⁵⁴ Similar quotations can be found throughout her work.⁵⁵ She saw more clearly, she suggested, because she took off the glasses of mainstream economics, which distorted the vision of so many of her contemporaries inside and outside academia. In her final analysis, she saw neoliberal economics “specifically, monetarism and supply-side economic logic” as a “concealing ideology” providing a rationale for “national policies serving special interests”.⁵⁶ The interests, we would say today, of the 1%.

What makes Strange relevant, though, is that she didn’t just critique the mainstream macroeconomics of the day, she analysed how it erred and put forward a new, alternative framework. Time and again in Strange’s writing, she criticises international economics – and other theories of global affairs – as “far too narrow”. Mainstream economics, she argued, was too narrow as it artificially excluded politics, institutions and power from its analysis. It took the social structure of the economy for granted. Also, even in the late twentieth-century, most economic thinking was national, ending at the state’s boundaries, and not global. Strange’s response to these shortcomings, her attempt to construct a new way of looking at the world, can be seen most clearly in her theory of structural power, which she compared to 3D vision.

Strange’s 1988 book *States and Markets* is the most complete exposition of her theory. As Chris May, who compiled her bibliography writes, “this is Strange’s defining book, one about which the rest of her work revolves.”⁵⁷ Conventional international thinking focused on relational power: the ability of a state to force another to do its will. Structural power is different. It is the ability of a powerful state to define the rules of the game that everyone plays. Strange explained structural power with an analogy of men controlling women through “social status, legal rights and control over the family money”.⁵⁸ Strange didn’t invent the idea of structural power, but she did popularise her novel take. Structural power, she argues, is how insights from economics, politics, international relations, and other disciplines can be synthesised and the interconnections seen. Her theory identifies four forms of power – which she calls structures – in the global system. These are security, production, financial, and knowledge. These four structures underpinned her analysis ahead of the 2008 crisis.

The security structure was traditional international relations (IR). Strange thought the field of IR, like economics, was far too narrow. She explained this, as usual, through history, looking at the context of the field’s development. IR had emerged in the first half of the twentieth century, she explained, in response to two horrific world wars and the threat of a third. Its primary area of study, therefore, had been state power, conflict and war. It often overlooked the dynamic role of markets, technology and the growing power of non-state actors such as multinational corporations. Strange fought this “totally unreal separation” between politics and economics

⁵⁴ Strange, S. (1986b), “The Bondage of Liberal Economics”, SAIS Review, 6, 1, (Winter/Spring).

⁵⁵ For example, in Strange (1994), p. 116, she writes, “There is a bankruptcy of ideas and theories in the profession of economics...”.

⁵⁶ “In the period covered by *Mad Money*, the concealing ideology has been that of liberal economics and specifically, monetarism and supply-side economic logic.” From Strange, S (1998b) “What Theory? The Theory in *Mad Money*”, CSGR Working Paper, No 18/98, December.

⁵⁷ May, C, (2014), p. 27.

⁵⁸ Strange (1994), *op. cit.*, p. 37.

from both sides, on and off the page her entire adult life.⁵⁹ Security and order were powerful forces but anyone who fixated on them alone was left with a distorted, one-dimensional view.

You cannot understand global affairs without paying attention to economics, Strange argued. Economics was so important it merited two separate but interlinked forms of power: the production and financial structures. The production structure she defined as “what is produced, by whom and for whom, by what method and on what terms”.⁶⁰ It is the traditional focus of Marxist and radical economic thinkers. Yet again, Strange argued that this school of thought was too narrow and often neglected the other structures, especially finance.⁶¹ The financial structure focuses on money. More specifically on how the power to create credit is used or misused “feeding on itself rather than nourishing the real economy”.⁶² The financial structure was often overlooked by most observers, Strange argued, including thinkers inspired by Marx and Gramsci and neoliberal economists who had a rose-tinted view of banks as “intermediaries”. For her, finance, not trade, was the key to understanding the global political economy.

The fourth and final piece of Strange’s theory of structural power is the knowledge structure: the broadest and most philosophical of her forms of power including everything from belief in religious salvation, the scientific method, and advances in high-tech industries. The knowledge structure includes “what is believed (and the moral conclusions and principles derived from those beliefs); what is known and perceived as understood; and the channels by which beliefs, ideas and knowledge are communicated – including some people and excluding others.”⁶³ It was also the structure that pushed up to and beyond the limitations of Strange’s reading in philosophical debates, and perhaps where there is most scope for further work.⁶⁴

⁵⁹ From Strange, S (1983), “Structures, Values, and Risk in the Study of the International Political Economy”, p. 210, in Barry Jones, R.J. (1983), “Perspectives on Political Economy”, Continuum.

⁶⁰ Strange (1994), *op. cit.*, p. 64.

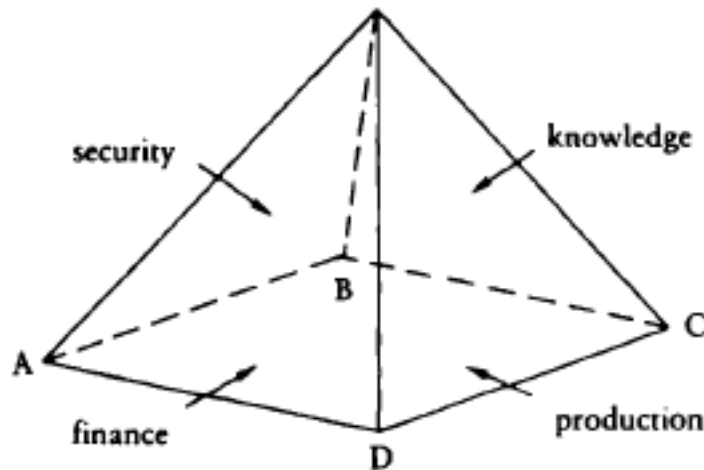
⁶¹ Strange writes that she wants to “build on the fundamental point of Marxist structuralism” by adding three “new – or at least, hitherto neglected – structures” in Strange (1983), *op. cit.*, p. 209.

⁶² Quote from Cox, R.W. (1996) “Take six eggs: theory, finance, and the real economy in the work of Susan Strange” in Cox, R.W. with Sinclair, T. (2012) “Approaches to World Order”, Cambridge University Press.

⁶³ Strange (1994), *op. cit.*, p. 119.

⁶⁴ *Ibid.*, p. 136: “Well aware of my own limitations, I have made no reference in the course of this brief survey of the knowledge structures of the international political economy to the active debates conducted by philosophers... They are debates with roots going back at least to Nietzsche, Hegel and Weber, and some would say to Plato and Aristotle.”

Figure 1: A diagram of Strange's four power structures model as a pyramid.



Source: States and Markets (1998)

Which of these four structures is dominant? Followers of Marx would likely answer the production structure, followers of Foucault the knowledge structure, traditional IR scholars the security structure. But Strange visualised the structures not stacked on top of each other like a layer cake, but a “four-faceted triangular pyramid” – where each structure is “supported, joined to and held up by the other three.”⁶⁵ (see Figure 1) Neither security, production, finance, nor knowledge is dominant. If only the political economy of the planet was so simple! She argued that you can see these structures in families and small human groups as well as in global affairs.⁶⁶ Her theory was like a pair of 3D glasses, she claimed. By looking at a problem from the perspective of all of the structures and their interactions, she said, you could see a “three-dimensional picture”.⁶⁷ It allowed her to analyse Strange’s see-saw – the swinging back-and-forth of power between states and markets which she identified as the key dynamic of the twentieth century.

Strange’s 3D glasses were crucial in helping her identify the risks and trends which would burst into popular consciousness with the crisis of 2008. Economist Dirk Bezemer surveyed twelve economists and commentators who did see the 2008 crash coming and identified several shared elements of their “common underlying analytical framework”. These include: “the distinction between financial wealth and real assets”, a concern with debt and the power of credit, and systemic risk.⁶⁸ Strange shared all these. She was not mesmerised by the prevalent macroeconomic and financial models such as the efficient markets hypothesis or the Black-Scholes derivatives model. Her vision was holistic and global, not partial and national. She knew that finance and the creation of credit were deeply political not just technical. Strange saw that finance was locked in a dance with the security structure: reliant on state power while being able to reinforce or undermine that power (just think of bailouts, bond markets, and global tax evasion). She knew that finance was linked to, but not bound by, the production structure. She

⁶⁵ *Ibid.*, p. 26.

⁶⁶ *Ibid.*, p. 26.

⁶⁷ Strange (1983), *op. cit.*, p. 210.

⁶⁸ Bezemer (2009), *op. cit.*

was well aware that credit creation could be revolutionised by technological advances in the knowledge structure or permissive regulation leading to ‘overbanking’. She understood that financial markets were held up by societal beliefs and trust – crucially in the value of money – not just laws and statutes. And, finally, from her deep historical knowledge, she was aware of the importance of constraints to the creation of credit and of the role of a lender of last resort, as well as the global power the United States wielded in all four structures. Strange saw the financial crisis coming because she was studying the real world in all its complexity, not the neat and predictable world imagined by economic modellers working in the tradition of Paul Samuelson.

For many, however, Strange’s theory is unsatisfactory. It has been criticised as imprecise, too descriptive, and lacking in analytical rigour.⁶⁹ The broad focus on power and politics means that much of the technical, mathematical model-building work – which gained prominence in economics and politics during her working life – was impossible in the framework. Strange saw the structures as “organic” developing and decaying over time and did not believe that global change “can be reduced to a single set of factors ranked in predictable order of importance”.⁷⁰ Strange never provided a timed prediction of a financial crash. This would have excluded her under Bezemer’s selection criteria. Strange would have responded that precise predictions of the global system are impossible with any theory. The future does not yet exist, she thought, and is made by our actions in the present. She was sceptical, wrote Robert Cox, of “the grander notions of theory... whose adepts conceive it as the revelation of the inner essence of the universe, the deep and abstract explanation of everything.”⁷¹

Strange argued that embracing humbler theoretical aims would save much confusion, effort, and money. Her touchstone was empiricism and the real world. She berated colleagues for distorting “the facts of life” to fit them into theories rather than adjusting the theories to the facts.⁷² She tried to impress on both her students and her six children “to distrust ideologies – but to respect the evidence”.⁷³ A lack of specific predictions about global affairs, then, was a feature not a bug of Strange’s way of seeing the world. The desire for it, she argued, was a “wild goose chase” based on muddled thinking that failed to see the difference between physics and society.⁷⁴ The social scientist’s search for truth “is not best served by aspiring to the unattainable or promising that which cannot in the nature of things be delivered.”⁷⁵

⁶⁹ Palan, R. (2015), “Foreword” in Strange, S. (2015), “States and Markets”, Second Edition, Bloomsbury Revelations.

⁷⁰ Strange, S. (1996), “A Reply to Chris May”, *Global Society*, Vol 10, No 3.

⁷¹ Cox (1996) *op. cit.*

⁷² See for example, Strange (1994b), ‘Wake up, Krazner! The world *has* changed’, in *Review of International Political Economy*, 1:2, Summer 1994.

⁷³ Strange, S. (1989), “I Never Meant to be an Academic”, in Joseph Kruzel and James N. Rosenau (eds), “*Journeys Through World Politics*”, Lexington Books, pp. 429 – 436.

⁷⁴ Strange (1994), *op. cit.*, p. 16.

⁷⁵ *Ibid.*, p. 16.

Where does Strange belong today? Back on the open range

Strange's work defies neat categorisation. It was a difficult to know where it fit even during her lifetime. "Uniquely," Ronen Palan wrote shortly after her death, Strange's reputation was equally high in "IPE, political economy, geography, business studies and international law".⁷⁶ Why, then, should economists, and others outside IPE, read her? For one, as she had a remarkably consistent focus: Randall Germain estimates that from the 1960s to the 1980s, 75% of her published work was on money and finance.⁷⁷ Summing up Strange's career, Robert Cox wrote, "the focal point of her interest lies in the relationship of states to global finance."⁷⁸ Clearly, economics played a central role in her thinking, even if her eclectic historical approach places her outside mainstream or perhaps even heterodox macroeconomics.

Strange worked with many economic thinkers from across the spectrum throughout her career. From their time at *The Observer*, she knew *Small is Beautiful* author E.F. Schumacher.⁷⁹ While Strange was at Chatham House, she met and exchanged letters and papers with the economist Robert Triffin, an expert on the international monetary system.⁸⁰ Here she also worked alongside Italian economist Marcello de Cecco. Around the same time, she met economic historian Charles Kindleberger – author of the celebrated *Manias, Panics, and Crashes* – whose 'hegemonic stability theory' helped inform how she saw the USA's global economic role. She worked with former journalist and economist Fred Hirsch, whose work on the social limits to growth has been revived by political philosopher Michael Sandel.⁸¹ She knew also Kenneth Boulding, an early evolutionary economist, who provided one of her references for the LSE Chair. That's not to mention all the academics within the IPE tradition and its neighbouring fields which she corralled, and inspired, with impressive energy across Europe, North America, and Japan.

Her work engaged with a wide range of economic ideas from Friedman to Keynes. Keynes she credits with developing "the only coherent, rigorous and influential theory concerning the conduct of financial markets". His *General Theory* she says approvingly is more a "sociological theory than a purely economic one".⁸² But the problem with Keynes – and his later followers such as Hyman Minsky – and Friedman and the monetarists is that both theories were "myopic", she said, as they "stop short at the frontiers of the state". Economists need to think "globally instead of just nationally."⁸³ Strange was so far ahead of her time that her insights fit neatly with

⁷⁶ Palan, (1999), *op. cit.*, p. 122.

⁷⁷ Germain, R. (forthcoming).

⁷⁸ Cox, R.W. (1996), *op. cit.*

⁷⁹ "I knew Fritz quite well through our shared involvement with *The Observer*." from Strange, S. (1996), *op. cit.*

⁸⁰ Letter from Strange to Triffin on 3 August 1966 in the Chatham House Archives: "I need hardly add how grateful I am personally for your comments and criticism of my paper and on the problem as a whole; they have made me seriously re-examine a good deal of what I had in mind to write and have been most stimulating."

⁸¹ Sandel, M.J. (2012), "What Money Can't Buy", Penguin, pp. 120-121.

⁸² Strange, S (1998b), *op. cit.*, p. 10.

⁸³ Full quote: "The fact is that the vision of both Keynesian economists and monetary economists appears to be so myopic that theories stop short at the frontiers of the state, or the water's edge. Neither can see

those in Adam Tooze's *Crashed*, a 2018 history of the financial crisis. The global financial crisis blindsided experts steeped in a "familiar cognitive frame of [national] macroeconomics" inherited from Keynes which Tooze calls an "island model". What was missing was an "interlocking", "macrofinance" view.⁸⁴ Strange provides an interlocking view.

Strange's reading was broad. She admired economic thinkers who recognised "the impossibility of divorcing politics from economics" such as Joseph Schumpeter and Karl Polanyi.⁸⁵ She frequently referenced George Soros's 'reflexive principle'. She also dug back into the work of Chicago School economist Frank Knight to build her understanding of risk in the money system. She integrated the insights of right-leaning business historians such as Peter Drucker and left-wing radicals like Immanuel Wallerstein. The list could go on. The point is Strange was open to and deeply engaged with economic thinkers from across the ideological spectrum. Palan notes the "great injustice" of ignoring her theoretical engagement: "Susan was widely read and perfectly aware of a great diversity of theoretical traditions."⁸⁶

Strange wanted IPE to be an "open range," accessible to people of all national and disciplinary backgrounds. Her life's work, Robert Cox says, challenged "the dividing and enclosing practice that is characteristic of academic disciplines".⁸⁷ She did not see herself belonging to the field of international relations. It was her bold ambition that IPE would replace both international economics and international relations. That never happened. It is ironic, then, that she is seen by some as a historical figure in a sub-discipline of international relations. Even in her lifetime, the field she created, IPE, was re-enclosed. It was taken over, Cox writes, by "new orthodoxies" and "new hierarchies" which have tended to exclude her. "Susan Strange never gave them her official blessing. They never succeeded in taming her iconoclasm".⁸⁸ It is time to let Strange loose on the open range.

Despite working in universities for fifty years, Strange was always an insider-outsider academic. Even in the 1970 paper that made her name, she skewered colleagues for "building intellectual card-houses and playing academic word-games".⁸⁹ Twenty-five years later, when she returned to the LSE to give a speech, her message was similar. The problem was not just in the world of global affairs and finance but also inside university walls. The problem was the walls. As she said: just as companies have to restructure in the face of globalisation so do universities: "we have to start thinking about how we break down the old walls between the social sciences and do things differently." She went on "most social science hasn't yet caught up with the times nor with the truly revolutionary nature of social and economic globalisation."⁹⁰ Another twenty-five years have passed and the revolution has yet to come.

beyond. If either doctrine could be applied globally instead of just nationally, it might make some sense and produce some results." From Strange, (1994), *op. cit.*, p. 117.

⁸⁴ Tooze (2018), *op. cit.*, pp. 8-12.

⁸⁵ *Ibid.*, p. 15.

⁸⁶ Palan, (1999), *op. cit.*, p. 123.

⁸⁷ Cox, (1996), *op. cit.*

⁸⁸ *Ibid.*

⁸⁹ Strange, S. (1970), "International Economics and International Relations: A Case of Mutual Neglect", *International Affairs*, Vol 46, No. 2, p. 315.

⁹⁰ Susan Strange's talk on 'The limits of politics' at the LSE talk on 1 June 1995.

Her mistake was, perhaps, to think that it could happen inside the gated communities of academia. Conceiving of Strange outside IPE, even outside academia, might be a liberation. She could then be judged not as a wayward IPE scholar, but a global thinker with a deep historical, even philosophical, approach to understanding “the political economy of the planet”.⁹¹ Her clear writing style, dislike of jargon, and distrust of grand theory and algebra as a way of understanding global affairs could be seen as strengths. It harks back to the first modern political economists. Indeed, Randall Germain considers Strange a “critical classical liberal” working in the tradition of Adam Smith.⁹² Inspired by Robert Heilbroner’s term for the original political economists, we should see her as a ‘worldly philosopher’.

What can we do with this prescient, spiky late twentieth-century worldly philosopher? Strange’s work offers abundant research opportunities in two directions. First, bringing her writings into dialogue with thinkers outside of IPE. There has yet to be a detailed study comparing Strange’s work with that of Hyman Minsky.⁹³ Or a paper examining how Strange’s theory of structural power compares with a similar four-pronged framework of power put forward by her contemporary at LSE, the sociologist Michael Mann.⁹⁴ Strange could also be put into dialogue with contemporary economists such as Paul Krugman, who have written on similar topics but taken decades to wake up to the influence of politics and power in economic affairs.⁹⁵ Another line could explore to what extent Strange’s work can be understood as a continuation of the historical or institutional school of economics which flourished in Germany, America, and Britain in the late nineteenth.⁹⁶ Her structural power framework could be taken in new directions by exploring how the presence or absence of patriarchal and/or structural racist beliefs in a society’s knowledge structure influences the other structures and, therefore, global affairs. Strange’s focus on the human condition as central to global affairs suggests her work could be examined and broadened out in dialogue with philosophers such as Mary Midgley, another insider-outsider academic.

Second, there is much work to apply Strange’s thinking to contemporary developments in the global political economy. What insight does Strange’s work have on claims that reforms have made OTC derivatives safer? How do her core concepts of structural power, casino capitalism, top and negotiated currencies, trilateral diplomacy, significant ignorance, and the state-market see-saw relate to global affairs today? Some of this has been started with the global financial

⁹¹ John Pinder, in *Ibid.*

⁹² Personal communication with author.

⁹³ See my brief article Dyer, N. (2019), “The Financial Crisis Was a Minsky Moment but We Live in Strange Times”, *Naked Capitalism & Economic Questions*, February.

⁹⁴ Michael Mann’s model of power is known as the “IEMP model” where the four prongs are: ideological, economic, military, and political.

⁹⁵ Adam Tooze (2021), “The Gatekeeper”, *London Review of Books*, Vol 43, No, 8, 22 April. <https://www.lrb.co.uk/the-paper/v43/n08/adam-tooze/the-gatekeeper>

⁹⁶ Strange often quoted historical school thinkers such as Georg Simmel, Max Weber, and Joseph Schumpeter. Like them, she has been falsely caricatured as not having a theory. Many of Strange’s orientations fit with Richard T. Ely’s account of the main elements of the historical school: a rejection of the method and premises of classical economics, a concern for historical development and context, an organic conception of economic life and society, concern about corporate power, acceptance of the role of the state etc. See Ely (1884) “The Past and the Present of Political Economy”, John Hopkins, Baltimore, pp. 43-51.

crisis and its aftermath.⁹⁷ Other work has begun mapping Strange's structural power onto the rise of China.⁹⁸ This is just a start: both could be carried much further. Other areas where Strange's 3D glasses might help clear the fog are free trade and protectionism, the reform of economics, the role of the US Federal Reserve, and the regulation of multinational corporations. Her framework could also be used, in dialogue with ecological economists, to illuminate our response to the climate crisis. The year 2023 will be the centenary of her birth: a fitting time for Strange to be read afresh.

Several societal and economic trends demonstrate a need to return to Strange. Younger generations – who came of age since the global financial crisis – are questioning the basic assumptions of economics and the global economy. They are facing up to global problems, such as ecological breakdown, and paying greater attention to power and the need for structural change. Strange's approach to power in the global political economy could appeal. In economics, leading figures such as Nobel laureate Paul Romer have spoken against the misuse of mathematical models to conceal truth and intellectual stagnation stemming from deference to senior figures.⁹⁹ Strange's more radical hostility to algebra that is blind to power and her willingness to call out the academic "barons" seems again essential and urgent.

Strange's fight against conventional thinking in global finance and her fight to reform academia are similar. Both rested on a diagnosis of the problem which was holistic, ranged widely and swam against the tide of opinion. Both were "uncomfortable for vested interests" in business and in academia who benefited from the status quo.¹⁰⁰ Both were passed over. Nevertheless, she showed remarkable tenacity and energy to push on when she was almost the only senior female in the field. She could cause a stink, but after her death in 1998, it was easier for both the financial and academic worlds to ignore or downplay her ideas. Can we still afford to? Most commentators on global affairs admit the West is lost in new territory without a reliable map. We could do worse than finding again the lost trail blazed by Susan Strange and seeing where it leads.

⁹⁷ See Germain (2016).

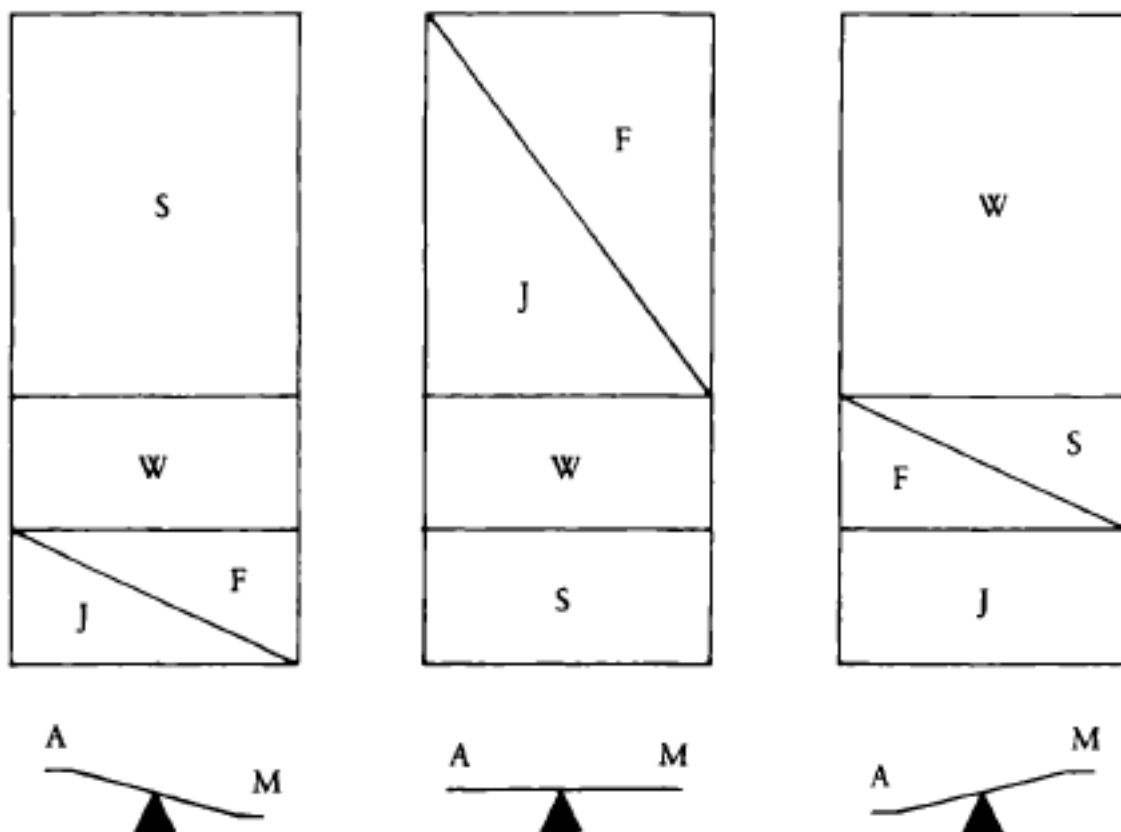
⁹⁸ See for example Malkin, A. (2020), "The made in China challenge to US structural power", *Review of International Political Economy*, <https://doi.org/10.1080/09692290.2020.1824930> and Dian, M. (2016), "Does China Have Structural Power?", *The Journal of Northeast Asian History*, Vol 13, No 2.

⁹⁹ Romer, P. (2016) "The Trouble With Macroeconomics", the Commons Memorial Lecture of the Omicron Delta Epsilon Society.

¹⁰⁰ Susan Strange 'The Limits of Politics', talk at the LSE in 1995: "The implications of redefining politics in this way, of enlarging the limits of what we understand as politics may be uncomfortable for vested interests both in government, in business, and perhaps even in the universities. As I said in the beginning, I think universities, like firms, are going to have to think about restructuring in the way they operate and the way they are organised."

Figure 2:

Strange's diagram representing different value preferences of three societies where S=security, W=wealth, J=justice, and F=freedom. These lead to the different outcomes in Strange's see-saw, below, where A=authority and M=markets.



Credit: States and Markets (1998)

Ultimately for Strange, the political economy of the planet comes down not just to structures, but also to values. Long before social psychologist Jonathan Haidt popularised the idea, Strange argued that neither the political left nor right had a monopoly on values. Strange admitted that her structural power approach was both politically to the left and right of her mainstream colleagues. To the left, as it grew from a structural perception of the world system inspired by Marxism, and to the right as she was sceptical of international organisations and saw nation-states as “final determinants of outcomes”.¹⁰¹ The great advantage of her theoretical openness is that it can accommodate, she wrote, “men and women of all political persuasions and predilections, whether realist and traditional or radical and neo-Marxist.”¹⁰² It still offers the possibility of a framework for a realistic, non-utopian dialogue in angry times.

¹⁰¹ Strange, S. (1982) “Cave! hic dragones: a critique of regime analysis”, *International Organisation*, 36, 2, Spring. Strange scepticism of the power and importance of international organisations continued throughout, but her belief in the centrality of states was challenged by the rise in power of multinational corporations and global finance which she explored in ‘Rival States, Rival Firms’ (1991) and ‘The Retreat of the State’ (1995).

¹⁰² Susan Strange, 1983, *op. cit.*, p. 209.

She focuses on four basic values that individuals, groups, and societies seek: wealth, security or order, justice, and freedom (see Figure 2). Just as a cook can mix the same ingredients in varying proportions to create cakes, pancakes, or cookies, she says, human groupings combine values in different proportions to produce a range of societies. It is not hard to think of societies that prioritise wealth and ignore justice (or even vice-versa) and others that venerate order and quash freedom and so on. Similar to an argument made by Deirdre McCloskey, Strange criticised mainstream economics for being primarily built around the value of maximising wealth, and often ignoring or denying the other values. The explosive growth in the size and power of global financial markets, she argued, was due partly to an erroneous (in her view) prioritisation of wealth above other values. Strange often pointed out that there were always trade-offs: the four basic values interact and can conflict with each other. The prioritisation of values in society is not fixed: there is no one, universal, magic formula.¹⁰³

Pay attention to your own preferences and leanings, your own values, Strange taught with her “desert island stories”.¹⁰⁴ Observe how values are embedded, sometimes hidden, in different schools of thought, and understand that they are not universal nor uncontroversial. Strange’s admirable openness to which values should be prioritised feeds her theoretical, epistemic humility. So many economic and political theories are built on a fixed conception of human nature, but Strange does not dictate one. It was the only honest response, she thought, to the diversity of opinion on the nature of the human condition. Strange was humble enough to set before others not “a set menu, not even an *à la carte* menu, but the ingredients for you to make your own choice of dish and recipe. This is partly because I believe profoundly that the function of higher education is to open minds, not to close them.”¹⁰⁵

Let’s open our minds to Susan Strange, a worldly philosopher with remarkable insight into financial globalisation, and put her back into the story. Her real-world, historical approach contains some of the ingredients for a recipe for better economics.

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¹⁰³ A similar view is known as ‘value pluralism’ by the philosopher and historian Isaiah Berlin.

¹⁰⁴ See Strange (1994), *op. cit.*, p. 1.

¹⁰⁵ *Ibid.*, p. 9.

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Measuring economic transformation – what to make of constant price sectoral GDP – evidence from Vietnam

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Abstract

The paper discusses the analysis of economic growth and transformation and the concept of constant price sectoral GDP, usually understood to measure real factor rewards, linked to actual factor inputs. It reviews criticisms of such statistics and statistical conventions underlying GDP data, their focus upon current price factor incomes and implications of the practice of constructing constant price sectoral GDP from revalued net output (gross output less non-factor inputs). Innovatively, it shows how recalculations at constant prices of actual sectoral factor inputs at a year away from the base-year will **not** necessarily equal revalued gross output less non-factor inputs, the usual basis for such data. The accounting identity that requires their equality only holds for current prices. Therefore, constant price sectoral GDP data does not measure actual factor inputs. Despite this, the analytical frameworks of economists analysing structural transformation often assumes that they have, in constant price sectoral GDP, a measure of actual factor inputs (when they do not). This inhibits analyses from engaging properly with incentives, often disregarding the possibility of disequilibria by adopting a production function approach that, encouraged by the belief that constant price sectoral data measures changes in actual factor inputs, expects technical conditions to determine incentives (factor rewards). The paper shows this risk of confirmation bias by examining work on Vietnam.¹

Key words

Structural change, national income accounting, economic growth, economic development, confirmation bias.

1. Introduction

1.1. GDP

In this paper I discuss constant price sectoral GDP. By this I mean statistics reporting GDP generated in sectors (such as services), or sub-sectors (such as, within services, retail trade) that are revalued to create measures in some way independent of price changes. This can be done (as for example by the Vietnamese General Statistical Office (GSO)) by revaluing to the prices of a base year (currently 2010 [GSO 2020]), or through a ‘chain’ procedure that gives

¹ I thank David Dapice for collegial informal discussions, and comments from an anonymous commentator introduced to me by Frank Stilwell, and other anonymous reviewers. This paper draws upon ‘*Services and development – structural change in the Vietnamese economy during the 1992-2016 ‘economic miracle’: some questions, a few answers and several suggestions*’ presented to the National Center for Economic Forecast and Information of the Ministry of Planning and Investment Hanoi August 2018. I thank Tran Toan Thang, Nguyen Ngoc Anh, Tran Kim Chung and others for their comments.

estimates of real changes year to year based upon prices for each year. My central point is that these statistics are conceptually confusing, and that this has unfortunate consequences for analysis: they appear to create data on actual – or ‘real’² - factor rewards, and so inputs (given the removal of price changes), for these are thought equal to the difference between gross output and non-factor inputs, as indeed they are at current prices. But, as I show, this equality, based on an accounting identity, only holds at current – observed – value, but does not hold at constant prices. Reported constant price sectoral GDP almost always is calculated as real gross output less real non-factor inputs (as these are in practice calculable, but real factor inputs, especially of capital, are not). Therefore, they do not create data on real factor rewards, and so inputs (given the removal of price changes). The common assumption, though, is that they do.

One common consequence of this is that some economists, and I show this, therefore tend to think that these constant price statistics offer them an empirical door into a physical world where actual factor inputs and changes in them (rewards to factors with price changes taken out) are known. In fact, they do not, which removes the empirical foundation of analyses and views of the knowable economy that assume this. This I argue facilitates confirmation bias, helping to explain why important trends, such as the shift from industrialisation to servicisation in economic development in poor countries, are ignored for surprisingly long periods (see below) [Fforde 2016b].

Further, this bias then encourages a de-emphasis of research into actual factor incentives and development of suitable models to use that data, and an over-emphasis on the use of production function methodologies. These methods appear to embody actual factor inputs, linking production functions to incentives, such as through marginal product pricing³ (either actually or as a reference point to which markets will seek to move), and assumptions that relative factor prices are determined by technical conditions embodied in the production function. This bias assumes what must be shown, and, for example, tends to dismiss possible alternatives, which may or may not pertain in different sub-sectors, such as the possibility of various forms of rents (for example, with fast structural change, labour and / or capital receiving rewards largely independent of the costs workers or employers face in supplying work or capital).

What we find in the literature, too often, is inadequate attention to experienced incentives that drive shifts in labour and capital as core elements of structural transformation. Part of this bias, but of course not all, is explained by my argument here, pointing to the false belief that statistics on constant price sectoral output, whether chained or at base year prices, and derived from revalued net output, should equal constant price factor inputs **because at current prices they must**. Away from the base year, at imposed ‘constant’ prices, whether using an index or a chain basis, there is no reason for this equality to hold, so revalued changes in net output (which is what the data almost always is) do not record changes in actual factor inputs. Further, this tends to shift attention away from the valuable GDP data we do have, which is the current price factor

² By ‘real’ some mean that changes in these statistics might be interpreted as changes in isolation from price changes, and so mappable to physical quantities. This can itself be confusing in a NIA context, given the NIA framework’s internal definitions, which I discuss below. Essentially, there is a major conceptual gap between a ‘deflated’ current price measure and notions of physical quantities. See on this for example Rymes 1971 as a extensive discussion. This is not the meaning of ‘real’ used by Kuznets, a founder of NIA methods – see below – for whom ‘real’ meant current price data.

³ See McLeod 2019 for a thought-through discussion of links between this and GDP data.

income data for sectors and sub-sectors: focussing on this implies a refocussing of what we mean by economic growth upon the changing pattern and value of factor incomes generation, which, as a measure of economic activity, is the conceptual heart of National Income Accounting. In this framing, the central issue to understanding economic growth is the capacity of an economy to generate increases in factor rewards without excessive inflation or external instability, so that workers' incomes and profits have increasing purchasing power. This shifts the analytical focus away from real GDP and towards current price incomes and final demand. Revaluation of the latter to manage price changes is conceptually straightforward.

1.2. Motivations

So, in this paper I examine the construction and definition of constant price sectoral and sub-sectoral GDP data or changes in it, if a chain basis is used - that is, the associated ontological issues - and evidence for how this has influenced research on economic growth, and I illustrate the associated issues in applied research by using Vietnam as a case study. The motivation for this is, first, my own attempts at analysing contemporary Vietnamese economic history, specifically the 'Economic Miracle' that started in 1992, and the need to produce a plausible and coherent analysis of structural change: movements of economic activity and payments to factors of production (rewards to labour and capital) between sectors.⁴ If 'water flows downhill', in the Vietnamese expression ('Nước chảy vào chỗ trũng'),⁵ or heeding Bloomberg's advice to 'follow the money', we clearly need to know the analogous issues of gravity, the height of the hill, etc. This research quickly ran up against the question of what is meant by constant price sectoral GDP, and my issues with much of the extant literature. Thus, my second motivation is methodological - how economists may avoid pitfalls through a clearer understanding of the data they are using.

1.3. Vietnam

The question of the actual incentives operating upon factors of production in Vietnam, and how they are reflected in data, is given added importance by important aspects of the Vietnamese political economy. Recent research by Vietnamese scholars has reported that payments of bribes by businesses are about the same as their retained profits [Nguyen & van Dijk 2012 and Nguyen et al 2016]. Based on these and other data I think that bribes probably "amount to at least ¼ of GDP and are likely far higher" [Fforde 2021a:5]. Further, research also shows that the value of relations with officials, whilst high in the early stages of the Vietnamese 'Economic Miracle', have evolved significantly. By the late 2010s, bribe payments whilst clearly high were not seen as influencing the commercial success or failure of individual businesses [Nguyen et al 2016]. Further, qualitative research shows how the ruling Vietnam Communist Party is involved in procedures that prevent state bodies investigating Party members' possibly corrupt

⁴ It is the case that structural transformation was mainly not industrialisation, but an expansion of services. I claim credit for first publishing on this in Fforde 2016. Vietnamese economists are starting to publish on this [Nguyen 2018; Nguyen & Ta 2019]. See also Helbe & Shepherd Eds 2019 from the ADB that is, so far as I know, the first major work from a major donor to look seriously at the issue.

⁵ The saying derives I think from rice farming, for the phrase literally means 'water flows to low-lying land', which, in the Red River delta, being somewhat waterlogged, tended to require quick-growing rice varieties to take a crop. Such areas were called 'Chiêm trũng', as the strains came originally (it is said) from Champa (Chiêm) in central Vietnam.

activities without the formal approval of the requisite Party body [Vu Anh Dao 2017].⁶ This means that we need to be careful in how we explain incentives and especially careful in how we treat GDP data reporting value-added. There is evidence that ‘rents’ created by relationships nourished by bribes have fallen sharply over time, and no longer seem greatly to influence business success or failure [Nguyen et al 2016 and Nguyen thi Xuan Huong 2018].⁷ Whilst this is clearly a topic that requires deep research, not least into case studies that explore business’, bribe-receiving officials’ and workers’ perceived incentives, such research is made harder by uncritical deployment of analyses based upon production functions that assumes that constant price sectoral GDP data measures actual factor rewards and that this buttresses assumptions that factor rewards are either determined by technical conditions (such as their marginal products obtained from the production function) or strongly influenced by them.

For example, in the Vietnamese GDP data four sectors stand out as having sub-sectoral Gross Value Added (GVA) well above the average: power, mining, real estate and financial services.⁸ Personally, I am not hostile to assuming that this reflects, for power and mining, the low levels of labour costs compared to capital in generating electricity and extracting crude oil, but I am not happy assuming that there are high levels of actual inputs of highly skilled and so costly labour that explain adequately the situation in real estate and financial services. One reason for my judgement here is that the year-year variation in this current price data is large for the latter, but not the former.

The evidence from Vietnam is that GVA/worker in manufacturing, although relatively high when fast growth started, likely due to favourable resourcing by the state as part of the central-planning system, fell back relative to national averages rather quickly, so that employment generation there created rather low additions to current price GDP compared with services sectors – so the economy serviced [Fforde 2021b].⁹ This basic arithmetic can be obscured by deploying research methods that rely on misinterpretations of data to generate confirmation

⁶ This refers to Directive # 15 (2007) of the VCP Politburo ordering that state organs could not investigate Party members for possibly corrupt activities without Party permission [Vu Anh Dao 2017], in effect making corruption legal, if judgements of legality are determined by how local sovereign power determines what the law actually means in practice.

⁷ This implies that officials may often treat their ‘portfolio’ of bribe-generating businesses as best managed by a ‘hands-off’ approach that minimises transactions costs. The widely cited McMillan & Woodruff 1999 reports on the vigour and economic power of Vietnam’s informal business institutions. De Vylder & Fforde 1996 analyses the transition ‘from plan to market’ of the 1980s largely in terms of informal ‘bottom-up’, not policy-driven, change processes, linking them to processes in the north before 1975 and stressing the importance of macroeconomic shocks in the late 1970s. Fforde 2018c examines the subsequent literature, arguing that despite peer review and the availability (‘down the corridor’) of country expertise, many major scholars, in accepting arguments that change was driven by the Party, have made serious empirical mistakes.

⁸ These sub-sectors often contain sub-sub-sectors (e.g., power usually includes water supply) but I ignore these for convenience. I use GVA rather than GDP for clarity – they are statistically the same.

⁹ Data in Fforde 2012b reports that the ratio between manufacturing GVA/worker and the national average - *fell*, according to official data, from a 1999 peak of 206.1% of average GVA/worker to 125.4% in 2013. According to the latest data, in 2010 this was 95.5%, *lower* than the national average (in 2018 the ratio was 89.3%). Thus, just after 1992 in the first years of the Miracle, despite relatively high GVA/worker, because of weak job creation the share of manufacturing in total current price GDP, 15.4% in 1995, peaked at only 21.2% in 2007 before *falling* to 16.0% in 2018. Manufacturing was not acting arithmetically as a leading sector driving rapid GDP growth.

bias, such as supporting the view that industrialisation is both necessary and the actual average structural transformation of contemporary developing countries.

The focus on production conditions and use of production functions as the core metaphor of economic activity is influential. For example, the standard division of GDP in NIA is 'physicalist' - to do with production conditions - under three broad headings: primary (agriculture, forestry and fishing), secondary – industry (power, mining, manufacturing and construction) and then a group of activities grouped as services. The latter is in many ways a residual; in the Vietnamese data, which now follows international practice, there are 13 sub-sectors - Trade +, Science +, Arts +, Transport and Information, Administration +, Other services, Accommodation +, Party +, Household Employment, Financial Services +, Education, Real Estate + and Health.¹⁰ Whilst the basic argument made is that each of the three broad headings includes activities that share similar production methods, there is clearly much to discuss about this view. Clearly, this way of categorising economic activity itself encourages thinking of activities in a 'physicalist' way, by focussing upon the importance of similar production conditions. This then seems to encourage production function analysis, with the associated tendency to assume that in constant price sectoral and sub-sectoral GDP data analysts possess measures of actual factor inputs, when they do not.

Personally, treating tourism as sharing similar production conditions and technologies to financial services seems to me a big ask. For example, tourism, with its substantial investments in fixed capital such as hotels, is likely to enjoy economies of scale as occupancy rates rise, with no obvious equivalent in financial services.¹¹ This issue is becoming of greater importance as data suggests (see Table 1) that structural transformation is no longer centrally industrialisation. Rather, transformation appears as servicisation, often in situations with fast-growing GDP and implying relatively high GVA/worker in services sectors. Table 1 suggests the faster the growth the greater the servicisation. But whilst Table 1 covers a period since the end of the Cold War, this view is not widely accepted [Fforde 2018a], I think in part due to the belief 'in production', so that it is shared production conditions that are central to the sectoral categorisation of economic activity in GDP data. Services indeed can also be seen as simply a residual – not primary, nor industry. The content of servicisation is therefore obscured: just what do services sub-sectors have in common other than being neither primary nor industrial? And also confirmation bias is encouraged.

1.4. Servicisation vs. industrialisation, and 'confirmation bias'

There is widespread assertion of the necessity of industrialisation for economic development, and so deviation from this is seen as a mistake and research into servicisation strikingly limited [Fforde 2018a]. Given the statistical position of 'services', deviation from industrialisation is by definition, if primary sectors are shrinking, 'servicisation'. This evidence also suggests that faster-growing countries (excepting China) tended to increase the share of services in their

¹⁰ As already mentioned, these sub-sectors often contain sub-sub-sectors, marked here by the '+' sign.

¹¹ Sheehan 2008 channels the analytical framework of Murphy et al 1989 to point to the possibility that their 'drivers' of industrialisation – economies of scale, pecuniary externalities (industrial workers' tendency to spend on industrial goods) and structural issues helping to preserve profit levels ('dual economy') – could apply to servicisation. Domestic tourism would seem to be a good example in the case of Vietnam. Sheehan unfortunately did not test his conjecture empirically.

GDP more than slower-growing countries, on average (see Table 1). Fforde 2018a offers empirical evidence that shows the general ignorance of this at global level in terms of both academic work (by examining article abstracts) and that of donors, taking the World Bank as a main indicator. These facts notwithstanding, and consistent with established belief rather than what the data shows, industrialisation retains considerable normative power. As one example we find that Rodrik, a globally influential scholar, has recently published work fearing ‘premature servicisation’ [Rodrik 2015], which he tellingly calls ‘premature de-industrialisation’. Again, whilst the Vietnamese data shows rapid growth with servicisation [Fforde 2016, Nguyen 2018 and Nguyen & Ta 2019], study of what happened during the Economic Miracle that started in 1992 must cope with studies that support Rodrik’s position and deploy analytical techniques based upon production functions that assume, for empirical work, that constant price sectoral GDP data measures actual factor rewards.

There are two issues here: first, the one I confront in trying to understand structural change in Vietnam, which is - what happened? Second, how does the assessments that we have depend upon a specific interpretation of our available data, specifically constant price sectoral and sub-sectoral GDP (or changes in it if a chained series is used)? It seems that reliance upon constant price sectoral GDP (or constant price changes in it) is common but, as I argue, built upon a basic misunderstanding of the data. It is possibly that this misunderstanding is in some way linked to the confident assertions that industrialisation is necessary and sufficient for fast growth, when the data clearly shows that it is not. This may be because the ‘economic homunculus’ – the ‘physicalist’ model many economists use to think about and debate economic growth – relies upon a production function view of the economy, whose links to reality are thought strong (because of the mistaken believe that the data they have - constant price GDP - measures actual factor inputs) with implicit associated parameters derived from belief in industrialisation, leading to confirmation bias in applied research.¹²

Table 1: Structural change in developing countries’ economies, 1991-2013

	Percentages of countries	Change in the measured share of GDP in the whole economy	
		A. Services	B. Industry
Group 1 (less than 100% growth)	25%	+2.1%	+0.6%
Group 2 (100%-200% growth)	48%	+6.5%	+1.4%
Group 3 (200%-300% growth)	15%	+7.5%	-0.7%
Group 4 (more than 300% growth)	10%	+10.8%	+0.1%
All countries		+6.2%	+0.5%

¹² Statisticians and econometricians are aware of the extent to which extant statistical techniques face, at root because ‘so much is in the null’, difficulties in managing situations where, for example, the empirical analysis is founded upon a production function approach that cannot itself easily be denied by the statistical results. And, of course, choice of functional form and proxies, usually not well-guided by theory, can lead to good statistical results (which may be used to attack opponents) that are in fact spurious in technical terms (that is, meaningless). In such ways confirmation bias can occur.

Note: Averages are unweighted: data is for individual countries. Growth defined as change in current PPP US dollars. <http://data.worldbank.org/indicator/NV.IND.TOTL.ZS/countries> (24 November 2015). Reproduced from Fforde 2018a.

Both issues can be seen in Rodrik 2015. In a ‘physicalist’ view, productivity for him conceptually means constant price GDP/worker which is assumed to be data measuring actual factor inputs.

Typically, manufacturing experiences more rapid productivity growth than the rest of the economy. [3]

Premature deindustrialization is not good news for developing nations. It blocks off the main avenue of rapid economic convergence in low-income settings, the shift of workers from the countryside to urban factories where their productivity tends to be much higher. [23]

The argument focusses upon the assertion that manufacturing – a sub-sector of industry in the NIA – “experiences more rapid productivity growth”. My questions are therefore, first, what happened to manufacturing, and second, what does the available data on constant price output – manufacturing GDP – really mean? To repeat: the accounting identity that means that net output equals value-added **only holds at current prices**: away from the base period and revalued at base year prices using a chain method or fixed base year prices there is no reason conceptually that price-adjusted sectoral or sub-sectoral GVA viewed as actual factor inputs equals the difference between the constant price values of gross output and non-factor inputs. I argue that we are on far firmer ground when we ask whether factor incomes in manufacturing are growing relatively fast or slowly **in current price terms**. Table 1 suggests that similar arithmetic to that we see in Vietnam must also be common given the global averages: GVA/worker at current prices in manufacturing has been relatively low compared with national average GVA/worker.¹³

1.5. The GDP arithmetic of structural transformation

It needs I think to be stressed that simple arithmetic means that if there is structural change (understood as movements of factors of production from low to high GVA/worker sectors) rapid growth in current price GDP can only take place *if* current price GVA per worker in leading sectors is well above the average. Servicisation therefore, if GDP is growing fast, means movements to sectors – services - where GVA/worker there must be relatively high.

Since this arithmetic is fundamental, for NIA in the first instance measures current price factor incomes which are in accounting terms identical to final demand (net of indirect taxes and subsidies – the so-called ‘factor cost adjustment’), then we must see constant price statistics (whether chain based or not) as derivative. On the expenditure side, such measures are derived from estimates of price changes; but derived from what on the output side? Sub-sectoral and sectoral statistics derived by deflating non-factor inputs and subtracting them from deflated gross output exist, but though they are called sectoral and sub-sectoral constant price GDP,

¹³ This echoes research by Wood 1997 arguing that technology has often evolved to prevent factor rewards shifting from rich countries as off-shoring takes place – reducing GVA/worker in offshore manufacturing below what it might have been.

what do they mean, and, crucially, what do the textbooks on NIA method say they are, and are not?

1.6. The paper

These arguments are developed and supported in the rest of the paper. In the next section (2) I explore how, and with what assumptions, constant price measures labelled sectoral and sub-sectoral GDP are derived and used, and what meanings are given to them. In the section after that (3) I look at what the relevant manuals on NIA methodology say. I then (section 4) discuss how these general issues appear to have impacted upon the Vietnam literature and the country's economic strategy. I then (5) conclude.

2. Use of volume measures of sectoral and sub-sectoral GDP – literature

In the next section (3) I will review and clarify the statistical assumptions behind National Income Accounting (NIA) to support my argument, which so far has not been in sufficient depth. Prior to that, in this section, I look at the uses made of volume measures of sectoral and sub-sectoral GDP, to show the major meanings attributed to it.

2.1. Global patterns of research

A search for the most widely-cited relevant studies of structural change gives us the following most-cited papers (with Google Scholar Citations, as of August 27th, 2018, in brackets) – Arrow 1962 (14232), Levine 1999 (8547), Miller & Blair 2009 (6778) and Woolcock 1998 (6489).¹⁴ These are very high citations levels.

The common metric of structural change in these papers is, as expected, constant price sectoral and sub-sectoral GDP. This, as I discuss in the next section, constructs statistics by revaluing gross output and non-factor inputs at base year prices.

Arrow models and conceptualises in a 'physicalist' world where there are, conceptually, clear relationships between observed factor rewards and an underlying production function and where "both capital and labor are paid their marginal products" [159]. For Arrow productivity is conceptually a real 'physical' measure, and he is assuming that sectoral and sub-sectoral actual output per worker has coherent meaning as an observable, through the relationship between the production function's form and factor rewards – their marginal products.

Levine 1999 works within a similar conceptual framework, within which it is assumed that it makes sense to use "existing theory to organize an analytical framework of the finance-growth nexus and then assesses the quantitative importance of the financial system in economic growth." [1]. This world view sees economic growth as inextricably linkable to production function analysis, and his empirical work takes this 'physical' metaphor as its core:

¹⁴ I used Harzing's *Publish or Perish* which platforms on Google Scholar and searched under 'structural change, economic, productivity'. Some findings were excluded as not suitable or relevant.

The three growth indicators are as follows: (1) the average rate of real per capita GDP growth, (2) the average rate of growth in the capital stock per person, and (3) total productivity growth, which is a "Solow residual" defined as real per capita GDP growth minus (0.3) times the growth rate of the capital stock per person. [33]

Miller & Blair 2009 reveal a similar approach:

A key source of growth and health in many economies is the rate of growth in its economic productivity, broadly defined as the level of output of an industry or of the economy as a whole per unit of input. Exploring different methods of measuring this economic productivity has been an active area of analysis for the last two decades (Jorgenson and Griliches, 1967). [670]

Again, this clearly requires and deploys a physical concept of real sectoral or sub-sectoral output (GDP).

Jorgenson & Griliches 1967 share this conceptual framing:¹⁵

... the theory consists of a production function with constant returns to scale together with the necessary conditions for producer equilibrium. Quantities of output and input entering the production function are identified with real product and real factor input as measured for social accounting purposes. Marginal rates of substitution are identified with the corresponding price ratios. [249]

For Woolcock 1998 productivity is, also, ontologically, 'real' and a necessary part of a valid explanation of economic growth.

This rapid survey shows that the most cited works in this field assert 'physicalist' views of the nature of economic growth, structural change, and the underlying reality upon which observables, such as constant price sectoral GDP, sit.

2.2. Core aspects of the dominant global literature

From this discussion it is clearly the case that these (the most cited and generally highly cited) authors treat constant price (volume) sectoral or sub-sectoral GDP uncritically as a suitable tool for the analysis of structural change and assume it to refer to something 'real' in ways that make sense. But in fact, this data as provided by NIA statisticians is conceptually (and usually in practice) the difference between output and non-factor inputs revalued at constant prices. Sectoral and sub-sectoral volume GDP is in these and other texts assumed to mean 'a measure of actual output', and this is linked to the deployment of production function analyses.

In passing, I think that, apart from these core issues, a strong factor supporting these practices is the availability of proxies for labour input, such as employment (and perhaps hours worked). If it is thought that the available sectoral or sub-sectoral constant price GDP data measures actual factor inputs (or, if using a chain-based method, changes in the sum of actual factor

¹⁵ I discuss below their understanding of what they mean by 'real', which contrasts with that of Kuznets.

inputs), then the availability of labour input proxies surely suggests that, in some way, (changes in) actual capital inputs can be obtained. These can be augmented by devices such as creating estimates for changes in the sectoral or sub-sectoral capital stock by constructing lagged functions of investments (which can be deflated without too much worry as they are part of final demand). This then reinforces beliefs that the production function approach is empirically supported by the available price adjusted sectoral GDP data, which is why the point I make here, that the accounting identity that equates net output to GVA only holds at current prices, is important. I illustrate this below with a simple arithmetic exercise, and, once realised, I think this is obvious.

3. Economic growth and its analysis: some core conceptual issues

3.1. NIA – a review

Economic growth viewed in terms of commonplace measurement, economic analysis and public discussion is no more than changes in constant price GDP over time. Economists know that GDP is not in the first instance a measure of physical output. It starts from the desire to measure economic activity and the thought-through view that total economic activity is best viewed conceptually, and measured, as the rewards paid to factors of production – labour and capital. How these might be related to actual inputs of factors of production and physical productivity is a secondary question.¹⁶ This is why it is called National Income.

The conceptual ‘soul’ of this way of measuring levels of economic activity is, then, *factor incomes*. In its essence, the primary insight of the conceptualisation is not about physical inputs and outputs, but about flows of income to factors of production – labour and capital. Only secondarily, and only if these factor inputs can be measured and priced, to get at what they actually are, can factor rewards be linked to actual factor inputs. The discipline of double-entry accounting means that at the level of the production unit, as well as sectorally, sub-sectorally and the national economy, value-added, which equals factor rewards, also equals the value of gross sales (gross output) less non-factor inputs. This discipline of course does not apply for any set of prices other than those in the base period. Some expositions argue that GDP, as a statistic, is mainly aimed at giving a correct measure of output ‘by netting out double-counting’.

But what does it net out, and why? Thus:

The main output from the national accounts is a measure of the overall value of economic production in Australia in a given period, but without any double counting of the goods and services being produced. Many goods and services are bought by businesses for use in their own productive activities (e.g., steel is bought by car manufacturers). If the value of all goods and services produced were simply added together, there would be serious duplication because some goods and services would be added in several times at various stages of production. The overall measure of production, excluding double counting, is

¹⁶ I base my exposition on three official texts covering nearly 50 years: Maurice Ed, 1968; United Nations 2009; and Statistics Canada 2002; also, ABS 2012. There has been a trend to shift towards use of chain-based methods, using period-period price changes, but many still use the base year prices method (e.g., the Vietnamese GSO) and the core issue I stress here is unaffected by this. Statistics Canada have invested heavily in attempting to get at actual capital and labour inputs – see below.

called 'gross domestic product', which is commonly referred to as GDP. [ABS 2012: n/p]

This definition moves to what happens at the level of the whole economy from what happens at the level of the economic unit. This I think hides the important issue that whilst accounting identities hold at current prices at the level of the economic unit, and so sectorally and sub-sectorally, at fixed prices, whether chained or using a single year as the base, there is no reason for them to hold.

Activity in an economic unit, such as a business, can be measured as its gross value-added (GVA), being payments by it to factors of production (labour and capital) which as an accounting identity is also the value of the unit's other purchases (called in the quote above intermediate inputs) subtracted from its total sales. What is meant here by an economic unit's production is clearly just the GVA generated. It has always seemed to me personally that this is an odd use of the term productivity. It means that if actual levels of work and capital use do not change, but factor rewards rise, then so does productivity (on the surface a physical concept), which seems absurd. Anybody who has had to explain how a value-added tax works will have had to go through this.

For the purposes of argument, think of a one-man business that uses two inputs and labour to make a single output, using a tool, and pays something to the owner of the tool. We can look at the accounts and say that what he pays himself and the tool owner equals the gross value of sales less what he pays for his two inputs. We can do that again for another period and see what the change is in GVA – the business's contribution to current price GDP. But to say anything about his actual physical productivity is highly problematic, and simply saying that this equals the changes in his gross output less his non-factor inputs with all revalued at fixed prices is incoherent.

This incoherence is clarified if we think through whether the accounting identity which holds definitionally in the base period will hold at any other set of values revalued at constant prices: clearly, there is no reason why it should. If we revalue actual inputs and outputs, factor and non-factor, away from the base period at the prices of the base period there is no reason why constant price GVA should equal the difference between constant price gross output and constant price non-factor input costs (or changes in them if we use a chain method). The accounting identity that makes the NIA system work only applies at the base period, and to the aggregate data, where it works because final demand is definitionally equal to GVA adjusted for indirect taxes and subsidies (the 'factor cost adjustment').

The components of final demand (consumption both private and public, investment, export and imports ...) clearly are flows of actual goods and services, and so with some choice of price index can be adjusted in a conceptually consistent manner to give constant price measures (or changes in them if we use a chain method).

So, whilst sectoral or sub-sectoral factor incomes are the difference between the value of gross outputs and non-factor inputs for a given sector or sub-sector, it is not clear what we are looking at if we deflate these two collections of physical quantities – that is, measure them at a different set of prices. We may also assert, to complicate the issue further, that the 'actual' value of factor incomes rather reflects to those who receive them the costs of what they may spend them on, and **variations in such costs are not measured by changes in either non-factor sectoral input or output prices**. To quote a UK government manual [Maurice et al 1968]:

*Direct estimates of gross domestic product at constant prices cannot be made from income data. Although wages and salaries may be deflated by an index of wage and salary rates, this provides a measure of only part of net output which does not take full account of changes in labour productivity ... The various types of factor income might be deflated by changes in the prices of the goods and services on which the income is spent; but not all income is immediately spent, and until income is spent it cannot be identified with actual goods and services. However, since by definition total factor incomes equals total expenditure on the gross domestic product at current prices, the price index derived by dividing the expenditure-based estimate at current prices by the corresponding estimate at constant prices can be treated as a currently-weighted price index applicable to total factor income [The GDP deflator – AF]. The estimate of gross domestic product at constant prices obtained in this way ... provides no information on changes in the deflated values of **separate components of factor incomes** [Maurice et al 1968:45, stress added].*

Another manual [United Nations 2009] reaches similar conclusions:

Para 15.178. The limit to a set of integrated price and volume measures within the accounting framework of the SNA is effectively reached with net operating surplus. It is conceptually impossible to factor all the flows in the income accounts of the SNA, including current transfers, into their own price and volume components into unequivocal price and volume components. However, any income flow can be deflated by a price index for a numeraire set of goods and services to measure the increase or decrease of the purchasing power of the income over the numeraire but this is quite different from decomposing a flow into its own price and volume components... [314]

What Maurice et al and United Nations 2009 are saying is that one can calculate data and call it constant price sectoral or sub-sectoral GDP, but though it is clear how the data is constructed it is not at all clear conceptually just what it measures. What is clear from my analysis is that this data does not measure actual factor inputs or changes in them.

These sceptical views contrast with those we find in Statistics Canada 2002, which echo those assumptions in the texts reviewed in Section 2 and the suggestion from ABS 2012 that GDP, a net measure, is usefully thought of as an output measure *from which double-counting has been netted out*. Statistics Canada have sought to develop measures of actual capital and labour inputs. This is interesting but must confront severe measurement difficulties. Durand 1994 and 1995 grapples with the issue of ‘double deflation’ for measuring “real industry value-added” [1994: 303] in the Canadian NIA data. In this he is following up on criticism of ‘double deflation’ measures by Bruno 1978 and Denny & May 1978. The former reports that the relevant literature is “remarkably small” [4] and concludes that the requirements that such measures correctly measure marginal factor productivities are unlikely to be met. This literature tends to show that, when the situation is explored with reference to possible formulations of production functions, it becomes clear that Kuznets was likely correct – see below - to consider that the ‘real’ data is the current price data.

An analytical problem that arises when constant price sectoral or sub-sectoral GDP is used is, I think, often a feeling that the implied underlying distributional assumptions are not risky, and

so the physicalist metaphor with its implications for what determines factor rewards that we see in the quotations above will robustly generate and explain actual observables. This is conceptually linked to thinking in terms of production functions that, as we have seen, are conventionally linked by the economists I cite through the standard partial differential analysis to marginally priced inputs (including factors of production). Further, it may be the case (I have not checked) that the specific algebra of the chosen production functions **imposes** the requirement that constant price GVA equals the difference between constant price gross output and non-factor inputs, further clouding judgement (for this is not necessary ‘in reality’, as I have already discussed – see below for a simple arithmetic example). There is, rather, no strong analytical reason for applied research to make such assumptions: rewards to capital and work, and other prices, may reflect disequilibria, rent creation or other factors and the point of research is to establish what. The approach of Jorgenson & Griliches 2016 encourages belief that variations in GVA/worker are, *in any empirical context*, caused by technological and demand conditions (such as risk, human capital etc), and *not* by anything else (or that disequilibria reflect an orderly departure from equilibria so understood). This assumes away, though, what should be researched empirically.

3.2. Current and constant price sectoral GDP

What do different scholars mean by ‘real’? Whilst the view of those cited above is that it is actual factor inputs and their relationships that are real, this is not the view of the founder of NIA methods, Kuznets. He argued that “real investment” was made up, in the NIAs, of “the part of the current national product that is diverted from immediate consumption into additions to the capital of business and public enterprises” [Kuznets 1942: 3]. Here for him it is **current** price statistics that are real, and this implies that for him the central conceptual essence of NIAs, what was for him real, are the current price statistics, not those revalued in some way to give constant price information.¹⁷ His subsequent discussion treats constant price investment on the demand side – that is, like consumption, a deflated element of final demand. Kuznets 1941 Chapter 1 discusses the “Concept of National Income” and focusses upon current price aggregates: the duality that means that it “may be defined as the net value of all economic goods produced by the nation” [3] and the “total of all net receipts of individuals and business agencies” [4].¹⁸

¹⁷ There is of course a considerable literature in many cultures about such issues that I do not refer to here - apart from flagging Gillespie 2008 (and in summary 1999): “[T]he process of secularisation or disenchantment that has come to be seen as identical with modernity was in fact something different than it seemed . . . the gradual transference of divine attributes to human beings (an infinite human will), the natural world (universal mechanical causality), social forces (the general will, the hidden hand), and history (the idea of progress) [272–273]. And Nisbet 1969: “... what we have brought into conceptual existence, we are prone to believe has actual existence.” [241]. Nisbet’s comment seems applicable to the ontological positioning of the widely-cited scholars in Section 2.1 above.

¹⁸ Note also “For those not intimately acquainted with this type of work it is difficult to realise the degree to which estimates of national income have been and must be affected by implicit or explicit value judgements” [5]. See also pp.17-18 showing the author’s awareness of the variety of possible definitions of productivity – suggesting the value of focusing upon current price data, for all its dependency upon value judgements, such as the definition of activities deemed productive of factor incomes. See also pp. 29-31 discussing inherent issues with deciding upon how to measure prices. It is striking how the discussion of this Chapter largely excludes reference to constant price measures: for Kuznets, as the quote in the text shows, ‘real’ national income is a current price measure.

But the ability to generate constant price statistics called sectoral or sub-sectoral real GDP poses the issue of what actual economic activity this data measures. Rymes 1971, in a thorough discussion, surveys the debates about value and meaning in this area.

That, in contrast with Kuznets, Jorgenson & Griliches 1967 use the word ‘real’ in a physicalist sense is clear from:

*Quantities of output and input entering the production function are identified with real product and real factor input **as measured for social accounting purposes**. Marginal rates of substitution are identified with the corresponding price ratios ... if quantities of output and input are measured accurately, growth in total output is largely explained by growth in total input. Associated with the theory of production is a system of social accounts for real product and real factor input. [249 – stress added]*

Clearly, they disagree with Kuznets in their understanding of what the ‘social accounting’ (NIA) framework is, and what it measures.¹⁹

What this contrast between the views of Kuznets and others I think shows is that current price measures pose fewer problems when facing critiques that come down to the question (in a more popular register) of ‘well are these measures measuring something real’. Physical conceptualisations, whether of actual labour inputs (proxied by employment or hours work), or of capital, perhaps proxied by capital stock estimates, or through a production function with associated links to remuneration, are far less robust in the face of annoying questions. Workers may turn up, they may be paid, but ‘how hard they work’ is far less easy to measure; a factory may exist and be used to produce goods that can be sold, but ‘what it contributes to production’ is a far harder question to answer. Answers to neither of these questions in terms that rely upon constant price sectoral GDP measures are robust if criticised.

3.3. Viewing the situation ‘epistemologically’

The previous section (2) argued that a common view in the most-cited parts of the literature, based upon a production function conceptualisation of the economy, is usefully seen as deploying physical metaphors. We saw this in the common use of language that refers to GDP in physical terms, as ‘output’, close conceptually to the RHS of a production function: physical inputs are transformed into physical outputs. That is, the notion is that constant price sectoral or sub-sectoral GDP ‘measures something real’ in these, rather than in Kuznets’, terms. This perhaps seems apparent because it is the difference between two constant price measures – intermediate inputs and gross outputs. Yet this metric ‘just happens’ because the current price measures are constructed by statisticians and used to generate the data. Subtracting revalued constant price intermediate inputs from gross output does not create a measure of output, in any coherent sense of the word output, not least as there is no reason to suppose that a revalued set of inputs and outputs will be one where GVA equals gross output less non-factor inputs.

¹⁹ From another perspective, which stresses the view that labour inputs are ‘real’ in the sense that they are measurable, see De Juan & Febrero n/d drawing upon Sraffian and Ricardian traditions. Here, by contrast, the focus is not upon concepts of physical capital inputs, but labour, as the ‘real’ source of all outputs, and so capital. Again, see Rymes 1971.

Further, as Maurice et al pointed out, there are two quite different senses to what is meant conceptually by constant price factor incomes. Do they measure, in a physical conceptualisation, real inputs? Or the actual value of them to their recipients? There is no reason, outside of some formal model that assumes it, that these are identical. In the latter sense, sectoral 'constant price wages', for example, if we are interested in them because of any effects of changes in them upon workers' incentives, are clearly not derivable from current incomes by deflating with a price index that is sector specific. The actual value to workers of their incomes in the tourist sector are not determined by price shifts within the tourist sector. They might be in a formal model, but that is another matter. Rewards to capital suffer from similar arguments – for example, perceptions by capitalists in the tourist sector of the effects of price changes on the inflation-adjusted value of their profits are not likely to be determined by what is going on in the tourist sector – for example, if construction costs fall then this arguably may influence whether they want to build more hotels. And so on.

3.4. How constant price GDP is measured – double deflation is not real factor rewards – a simple arithmetic illustration

As I have stated, away from the base year (whether using a chain basis or a fixed set of prices), the accounting identity that equates net output with factor rewards does not apply. I now illustrate this with a simple arithmetic example.

Consider a sub-sector of services, perhaps finance or real estate, that is measured through NIA to produce GVA/worker. Conceptually, we can think of the actual inputs in terms of factors and non-factors, and the real gross outputs. Perhaps to 'fix the mind' we can think of the factor inputs as workers and owners' expertise and actual assets such as the office infrastructure, non-factor inputs as purchases of services from outside the sector as well as goods (electricity), and gross outputs as services to capital market participants, management of IPOs etc. In the base year, these various actual flows are priced in some way and GVA definitionally and practically has to equal factor rewards, which equal gross outputs less non-factor inputs. This follows from the accounting identity faced by all producers.

Now let us do a 'thought experiment' away from the base year. Conceptually, the actual flows shift, and are revalued at base year prices. This gives us two estimates of constant price GDP – one based on revalued factor inputs, the other based on revalued gross outputs less revalued non-factor inputs. By comparing the base year with another year, we include both the chained and non-chained methods we find in the data.

Say there is a sharp fall in services provided, and we think of this (to concretise our conceptualisation a little) as largely caused by a sharp change in the work done on IPOs – in the base year, this was mainly done by highly paid senior executives structuring the deals; in the new year, the work shifted to administrative preparation, and much of this was outsourced, away from the financial services sub-sector towards professional and technical.

In the base year, factor inputs were, say, 100 units x 1 (price), with gross outputs (sales to IPO companies) of 200 units x 1, and non-factor inputs 100 units x 1. Factor incomes equal gross sales less non-factor inputs, as they must at current prices.

In the new year, say, factor inputs fall to 80, now priced at 1.1, so 88 at current prices (GVA on one measure) and 80 at constant prices; gross output, now priced at 1.1 (for whatever reason

– disequilibrium, increased demand ...) at constant prices rise to 120, so 132 at current prices; non-factor inputs rise to 110, say, which, however, for the accounting identity to hold at current prices must be valued (at say 'p') so that the two measures of GVA are equal. Gross inputs less non-factor inputs = $132 - 110.p$, which must equal 88. Therefore 'p' (non-factor input average prices) must equal $(132 - 88)/110 = 0.40$. At that price non-factor current price inputs are 44, so that GVA equals $132 - 44 = 88$. So, at constant prices GVA equals 80 under the first measure and 20 under the second measure. What many economists want is to know one, but the statisticians report on the other, as they in practice cannot get at estimates of actual capital or labour inputs, nor the prices of the former, but they can revalue gross outputs and non-factor inputs, and they calculate the difference between the two and publish it.

This means that whilst there is no reason to assume that the accounting identity between GVA as factor rewards and GVA as gross output less non-factor inputs will hold away from the base year, the results of assuming that it does may be large, and we have no way of knowing in practice whether the error is large or not. One might be estimated, but such estimates are not to be found in the standard data collections.

This conclusion is not altered by the fact that sometimes measures of constant price sectoral GDP are not derived by statisticians from 'double deflated' non-factor inputs and gross outputs, but estimates based upon non-factor inputs. This is common when, as with much services output, outputs may not be standardised and/or marketed so it is hard and/or expensive to reliably estimate gross output. Both services and agriculture sectoral constant price GDP are thus often derived from deflated inputs, with an adjustment to estimate constant price GDP based upon a 'double deflation' conception (such as the ratio between current inputs and current GVA) [United Nations 2009: 294, 310-1].

4. The Vietnam literature

The discussion so far has two implications: first, that Kuznets is likely right in suggesting to us that the 'real' data is current price GDP, but that there is a powerful strand, expressed by Griliches & Jorgenson and others, that asserts that reality is rather captured through constant price data; second, that this view confronts problems in interpreting constant price sectoral GDP measured through 'double deflation' as measuring real factor inputs (or changes in them). This then readily opens doors to confirmation bias, which in the case of Vietnam is pushed by the tensions between VCP and donor advocacy of industrialisation and the experienced reality of servicisation. Examination of the Vietnam literature may therefore show this. It is worth here remarking that the ruling Vietnamese Communist Party, whilst taking propaganda and its own ideology seriously, has shown itself quite capable of managing processes leading to major shifts in strategic direction [Fforde 2018c].

I focus upon the most cited work, and the two studies already mentioned [Nguyen 2018 and Nguyen & Ta 2019]. I have already mentioned the aspects of the Vietnamese political economy that, I think, make it important to have an empirically well-founded basis for assessing incentives operating on capital and labour. I have also pointed out that the year-year changes in some current price sub-sectoral GVA/worker have often been high.

Examination of literature on Vietnamese growth tends to lead to two conclusions. The first is related to the relative unimportance of policy change and the second shows the influence of 'physicalist' views of what 'real' changes are that are implied by conceptualisations that rest

upon production functions are important and lead to confirmation bias, in part as they rely upon constant price sectoral GDP data.

4.1. Endogenous forces vs policy

As would be expected from the reality of servicisation, in contrast to VCP and donor beliefs, some analysts point to powerful endogenous forces in play independent of policy. For example: Pham et al. 2008 conclude:

The result from three national IO tables {give} strong support for the evolutionary movement of Vietnamese economy, or in our terminology, a bottom-up process ... [33]

Giesecke and Tran 2008 study the period 1996-2003 and conclude:

*In our story, we find rapid growth in GDP to be due to productivity and labour force growth. ... **Our results downplay policy reforms** ... [26-27 – emphasis added].*

4.2. 'Physicalist' views, production function-based analysis, and reliance upon constant price GDT data

Older studies other than those already mentioned that specifically focus on structural change (Bui et al 2012, Agola et al 2015, Abbott et al 2017, Tisdell 2011, McCaig & Pavcnik 2013) tend to avoid current price data in favour of constant price data and show problems with confirmation bias. More recent studies, likely aware of the servicisation issue [Fforde 2016a], avoid this [Nguyen 2018 and Nguyen & Ta 2019].

The earlier studies tend to conclude that manufacturing has been a central growth driver, **without placing this in an overall analysis of structural transformation that would reveal the greater importance of services**. They are strongly influenced by the production function approach, but do not succumb always to confirmation bias and support for industrialisation.

Bui et al 2012 report that “When analyzing long - term economic growth, most countries around the world follow the Solow growth model which was developed based on production functions.” [7]. But they conclude by asking the “reasonable question {as} to if the economic structure with the following priority order of industry, service and agriculture is an appropriate structure?” [9].

Agola et al 2015 explicitly contrast Vietnamese and Japanese experiences.²⁰ Their analysis, like that of Sheehan 2008 and Murphy, Sheifler & Vishny, 1989, is interested in dual economy issues, more generally the balance of welfare gains between the rural and urban sectors. As they put it:

²⁰ Current price sectoral GDP data shows that in Japan, for the years 1953, 1960 and 1970 respectively, GVA/worker in manufacturing was 116.5%, 138.6% and 120.0% of average GVA/worker [Fforde 2021b]. This contrasts sharply with the Vietnamese relative sectoral GVA/worker – see above Section 3.1.

The main finding is that raising productivity uniformly across sectors and regions was central to Japanese structural economic transformation, and that such productivity rise can be facilitated by uniform infrastructure development and judicious governmental policies directly impacting the right combinations of factors of production, namely increase in capital and technology in all productive sectors. [168]

Their analysis relies upon positing the existence of a production function and distributional relations that determine factor rewards [169] and so thinks in terms of the ‘real products’ of workers. Agola et al 2015 focus also upon sectoral real GVA/worker, and its average growth rate, and, by focussing upon real sectoral GDP, they can conclude that the manufacturing sector has ‘grown rapidly’ with high increases in gross exports [173].²¹ They therefore ‘miss’ what the arithmetic shows - the GVA/worker story of the sector’s rather unimportant contribution to economic growth (see above Section 1.3).

Abbott et al 2017 place upfront the concern on the part of Vietnamese policy makers that “ongoing structural transformation is creating too few jobs” [54]. They use production functions of various types [61] and assume (without empirical justification) that observed prices and factor rewards reflect technical conditions.

The data on factor rewards is fudged, with an observed interest rate less inflation treated as the same as the reward to capital manifest in non-labour GVA. Their analysis of “the extent to which higher wages would induce factor substitution towards more capital-intensive techniques {leads to} estimates of elasticities of substitution {that} yielded quite low ... values” [65]. This suggests, of course, that we are not in an equilibrium world at all, so their empirical results are technically spurious (that is, based upon assumptions that do not hold, and so technically meaningless). Be that as it may, they conclude that variations in labour productivity are caused by “biased technical progress” [66].

McCaig & Pavcnik 2013 (a later version is in Ed McMillan et al 2016) was used by Rodrik (see Ed. McMillan et al 2016) to support his thesis of ‘premature deindustrialisation’ [Rodrik 2015]. They are ignorant of the global reality of servicisation (above Table 1; Fforde 2018a). They stress the role of manufacturing in structural change and note the data that shows the rising share of the services sector [2013:2]. In contrast to the results cited above that de-emphasise the role of policy, they stress policy’s causative power. They rely upon the constant price sectoral GDP data to give them accurate measures of real factor inputs, following the Griliches & Jorgenson rather than the Kuznets view of what is ‘real’. Recall that the current price sectoral data shows that manufacturing GVA/worker was by 2010 below the national GVA/worker – at 95.5%. In 2018 the ratio was 89.3% [see above Section 1.3 and Fforde 2021b].

One of their core conclusions is that:

²¹ This is misleading as the domestic value added in manufacturing exports is likely below 15% of the value of exports. Total GVA in manufacturing for 2018 (using 25,000 VN Dong to the US\$ as an exchange rate) was around US\$35 bn. This compares with total manufacturing exports in that year of US\$ 208 bn on an SITC basis [GSO 2019 - NGTK 2018: 612], or US\$ 227 bn using the GSO’s definition [611]. Clearly, these export values included rather little Vietnamese value-added (below 15%, as some of the sector’s output was domestically consumed).

*Manufacturing **stands out** as a sector that experienced a large increase in productivity during this period, averaging an annual growth rate in labor productivity of 5.1 percent, and a large expansion of its employment base at an annual growth rate of 7 percent. ... In general, relative employment tended to expand in manufacturing industries ... [27 – stressed added]*

The logic here is flawed, equivalent to 'X won the race because they ran fast'. Given Vietnamese rates of GDP growth, and the facts that manufacturing's current price GVA/worker fell sharply, it is misleading to argue that fast growth meant that the sector 'stands out', given the **declining** share of that sector in GDP and economic activity as measured by the NIA. A race is won, not by running fast, but by running the fastest. Further, value added in manufacturing exports is now very low, confirming the point made far earlier by Wood and mentioned above. This is a stark example of confirmation bias, facilitated by the combination of the particular use of data – constant price sectoral GDP – and theory – production functions [2016: 107 et seq].

By contrast, Nguyen 2018 and Nguyen & Ta 2019 take positions closer to the observed current price GVA data. These show - like Bui et al 2012 - that confirmation bias is a flexible tool. Nguyen 2018 uses constant price sectoral GDP but avoids production functions. However, by avoiding current price data the author can remark that "the contribution of service industries to productivity improvement was solely driven by structural change effect, i.e., absorbing more labour" [246]. This helps perpetuate the view of McCaig & Pavcnik 2013 and 2016 that services are 'low productivity' sectors with low GVA/worker, which is not the case, as the data – and the simple arithmetic of growth - shows. However, note their view that "One of the most prominent observations over the 2000–13 period is the diminished role of the manufacturing sector in driving productivity growth." [247]. By contrast, Nguyen & Ta 2019 sub-title their article "The Shift to Services". Their analysis is based upon input-output data and constant price sectoral data. Most importantly, they look at the whole economy and avoid the logical flaws we saw in McCaig & Pavcnik 2013. Their use of input-output data and a case study of eCommerce sees them end by deploying standard arguments for policy – "To enhance the competitiveness of the services sector, it is important to focus on privatization, eliminating restrictions on supply, and creating a favourable business environment." [272]. But they have little analysis of just why structural change took the path that it has done, so far.

I conclude from the literature that it is relatively clear that the confirmation bias encouraged by the issues I have discussed is present, but that it is not determinant. The shifting position of Vietnamese scholars suggests that we may be seeing a strategic realignment in the Party's strategic policy, likely away from a particular pattern of structural transformation, not least as the reasons for the servicisation remain somewhat unclear, part because one effect of the dominance of industrialisation as a policy prescription has been extensive confirmation bias and a severe lack of research into alternatives, such as servicisation [Fforde 2018a]. This implies that Kuznets' view of the real meaning of current price GDP is sufficiently powerful to lead rather quickly to local analysts like Nguyen & Ta (the former is Director of a senior Institute at the Vietnamese Academy of Social Sciences) to generate research that contradicts Rodrik's position as well as those long-established by donors.

4.3. Policy implications

VCP and donor policy stressed, from the early 1990s, continuity with the structural transformation of the Soviet development strategy of industrialisation. Table 1 above shows that this was not the global pattern, and Vietnamese data shows that Vietnam servicised, with manufacturing's share of GDP failing to rise as agriculture's share fell [Fforde 2016a; Nguyen & Ta 2019].

There are therefore two policy implications of the literature just discussed. First, related to the analytical frameworks widely adopted that focus upon production functions as models of economic 'reality' viewed essentially in physicalist terms, and rely unrelially on misunderstandings of the meaning of sectoral and sub-sectoral constant price GDP data, policy makers should be cautious of the results of such analyses, and their high degree of confirmation bias. Second, causality that explains the observed pattern of servicisation and rapid GDP growth both deserves further research and should pay particular attention to the pattern of incentives and the clear acceptance and power of Vietnam's factor markets as they emerged and developed.

5. Conclusions

My argument against use analytically of constant price sectoral GDP leads to my conclusion that analysis of structural change should assert that the main and conceptually clear effect of economic change is upon **current** price factor incomes. Constant price measures are conceptually troubled. This permits confirmation bias. However, the Vietnam case study suggests that the story told by current price measures has a certain power.

What is needed is a serious re-examination of the pattern of economic growth and structural transformations in growth experiences, for me personally the post-1992 Vietnamese 'economic miracle'. We need to know far more about the roles played by structural change - the shift in factors of production – labour and capital - from areas where GVA paid to factors were relatively low to areas where they were higher. That this approach poses analytical questions that we need to solve does not imply that we should assume that they are already solved, as I hope I have shown. Fforde 2018b showed that servicisation was not mentioned once in the World Bank's flagship *World Development Reports* and had been largely ignored in an academic literature producing a host of studies of industrialisation.

The paper has argued that constant price sectoral GDP, whilst it exists as a clearly defined statistical measure, widely published and used analytically, has in fact no coherent analytical meaning. Within the NAI framework, to repeat, there is no reason to suppose that if all actual inputs and outputs were known, away from the base period, and were revalued at base year prices, constant price GVA (revalued factor inputs) would equal the difference between constant price gross output and non-factor inputs. The formal algebra used for convenience by modellers may assume this, but that is no reason to assume it true.

We can use current price sectoral GVA as a conceptually coherent measure of structural change, and this approach then focusses us analytically on the correct question, which is what explains, in real historical time, variations in sectoral GVA between sectors and over time. Case studies are a powerful research tool to explain this behaviour.

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Booming wealth alongside fiscal concerns about ageing populations

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Introduction

Billionaires are treating space as a new playground while the rest of us are being warned that population ageing means there will not be enough to go around when the present working generation expects to retire. The IMF warns that “the G-20 countries are in the midst of stark demographic change....[p]opulation aging can have significant macroeconomic implications... Aging would ... exert pressure on public finances as outlays for pensions and health care increase” (IMF Staff 2019).

The OECD conveys similar messages when it says:

“Populations are ageing rapidly across advanced economies... because of rising life expectancy and declining fertility... The number of people over 65 for each working-age person will at least double in most G20 countries by 2060... Rising old-age dependency ratios will put unprecedented stress on the financing of public pensions, health and long-term care, especially in a slow growth environment... ageing pressures could increase the public debt burden by an average of 180% of GDP in G20 advanced economies ...Alternatively, tax revenue would need to increase by between 4½ and 11½ percentage points of GDP by 2060 ... Pension reform should address the triple challenge of improving fiscal sustainability and reducing old-age poverty risk, while ensuring a fair sharing of the burden across generations.” (OECD 2019)

The Congressional Budget Office has projected spending to 2050 and finds federal spending will grow from an average of 21.3 per cent of GDP over the period 2010 to 2019, to 29.3 percent, average over the years 2041 to 2050 (CBO 2020).

The European Commission has said:

“The long-term budgetary projections show that population ageing poses a challenge for the public finances in the EU. The fiscal impact of ageing is projected to be high in most Member States, with effects becoming apparent already during the next decade. The projected change in strictly public age-related expenditure ... is almost 2 pp. of GDP in the period to 2060... the increase between 2013 and 2060 is mostly driven by health care and long term care

spending, which combined is projected to rise by about 2 pp. of GDP.” (European Commission 2015).

The concern in all this is what government spending means as a share of GDP. Common to all these expressions of concern about fiscal “viability” are the comparisons to GDP.

On the issue of health care, the OECD observes that over the 20 years to 2015, public health spending exceeded GDP growth in all OECD countries and was on course to reach almost 9 per cent by 2030 and 14 per cent by 2060. It put the pressures down to “new technologies which extend the scope, range and quality of medical services; rising incomes, which engender higher expectations on the quality and scope of care; and population ageing.” (OECD, 2015, 20). Officials in charge of government budgets complained that because:

“...health care is perceived by citizens as a very high priority, and government policies in this area are closely watched. In addition there are many stakeholders who intervene between the beneficiary of health care (the citizen/patient) and the public resources that finance it. These include purchasers (Ministries of Health, social security institutions, social insurance funds or sub-national governments); a wide ranges of service providers (clinicians with different specialities, operating within hospitals and other health facilities); providers of medicines, tests and equipment (pharmaceutical companies and laboratories); and other bureaucratic and administrative intermediaries.” (OECD, 2015, 20).

While these issues are being debated throughout OECD countries and elsewhere, the present paper will attempt to highlight the general problem but with reference to Australian experience to illustrate the issues involved. The Australian government has also warned of the looming fiscal “problems” with the publication of an Intergenerational Report (IGR) roughly every five years and was clearly designed to scare us into accepting a neoliberal/austerity agenda. Indeed the 2015 IGR was a political exercise to the extent that it gave three scenarios, first the government’s austerity plan which would “solve” the fiscal challenge, second, the government plan modified by not including measures it was still lobbying to get passed by the Australian Senate and third was the continuation of the policies from the last Labor Government (Australian Government, 2015). They showed fiscal projections that were respectively good, better and worst in terms of debt and deficit outcomes. The last report released this year (2021) is more moderate but still raises concerns about the ageing population and the fiscal implications and invites us to wonder if Australia will be able to “afford” the government expenditures associated with the aging population (Australian Government 2021).

What is missing in these exercises?

Whatever the subject of fiscal burdens and the like are discussed there is a reference to income or GDP in the international and country reports that cover these issues.¹ The “tax burdens” are expressed as a share of income or GDP for example. Ability to pay, intergenerational issues and so on are also typically expressed in terms of proportions of GDP or are sometimes so obvious the comparison with GDP does not have to be spelt out. Hence the emphasis is based on GDP as if other measures of income and wealth were irrelevant. Capital gains for example are only rarely mentioned and then it is in the context of realised capital gains that might be included in personal income.

This is an important limitation of the meaning of income and the analysis can look dramatically different if capital gains are taken into account in a wider definition of income. When considering such things as living standards, wellbeing and so on it is also important to add back some of the things missing in traditional macroeconomic outlooks. Income as measured in the national accounts is a very inadequate measure of wellbeing. Wealth should be considered and, of course, if properly measured income itself would be so much higher if capital gains are included.

Net worth is another measure of affluence. Household net worth in Australia in March 2021 was \$12.7 trillion (ABS 2021b), over six times national income. The increase in wealth has taken us by surprise to some extent. In March 1990 household net worth was a modest 3.58 and by March 2021 it stood at 6.37 times GDP. Australia is not unique in this respect. In the US wealth to income (net national income) increased from approximately 3.8 times in 1990 to 5.5 times in 2017 (Robins, 2019). Likewise in the UK the ratio of wealth to national income rose from 3.7 in 1995 to 5.7 in 2018 (Advani et al, 2020).

“In 1970, private wealth-national income ratios ranged from around 200%–350% in most developed countries ... The past four decades saw a sharp rise in these ratios in all countries. By 2007, the year in which the global financial crisis began, private wealth-national income ratios in the countries observed averaged 550%, peaking at 800% in the extreme case of Spain.” (Alvaredo et al, 2018).

The evidence would seem to suggest there has been a very substantial shift in the structure of the economy. Wealth is extremely important as a resource at the disposal of households and now looms so much larger than it did three decades ago. Wealth also generates capital gains, the increments in the value of wealth as a result of price changes. These are too often ignored in macroeconomic commentary and are not taken into account at all in the IGRs. The distribution of income and wealth is almost entirely neglected in these exercises but will become increasingly serious as the distribution of income and wealth worsens into the future. With increasing inequality there are also worsening political power imbalances.

We now take a step back and consider the treatment of capital gains in public finance discussions about revenue.

¹ In principle income and production are equal in a national accounting sense (United Nations 2003). However, national income needs to adjust GDP for income accruing to foreigners, and residents' earnings from overseas.

Haig-Simons income concept

Public finance courses have stressed for generations that fiscal policy discussions should be using something along the lines of a comprehensive definition of income. Those definitions say that one's income over a time period must be equal to consumption plus any increase/decrease in one's financial assets. Most discussions refer to the Haig-Simons (H-S) income concept which is simply defined as "consumption plus changes in net worth" (Staff of the joint Committee on Taxation, 2012). The references are to economists, Robert Haig (1921) and Henry Simons (1938). While tax systems tend to include only realised capital gains for practical reasons, the H-S definition definitely includes capital gains on an accrual basis (Armour et al, 2013).

Given capital gains are an essential part of the ideal measure of capacity to pay, attention should have been given to the behaviour of capital gains and their implications for macroeconomics generally and especially fiscal/budgetary policy and related topics. As a measure of capacity to pay tax measures of comprehensive income should also be relevant also for distributional analysis. Indeed, it might even be suggested that the onus should be put on analysts who do not include capital gains to justify that omission.

For most of the present discussion GDP *plus* capital gains as measured by the change in household net worth from year to year is used. Arguably household income should be used instead of GDP but GDP may still be appropriate given a large part of the tax revenue is taxes on production. The reader may keep in mind that there is a significant difference and there may be valid arguments in favour of either. The other consideration is whether the analysis should include any capital gains accruing to other sectors, the government, corporations, and the rest of the world. The view here is that capital gains in especially the corporate sector are likely to be reflected in higher valuations of corporate equity held by the household sector so that including the latter should be sufficient. Otherwise there would be an element of double counting. The paper now turns to examine the empirical importance of net worth and increases in net worth among households.

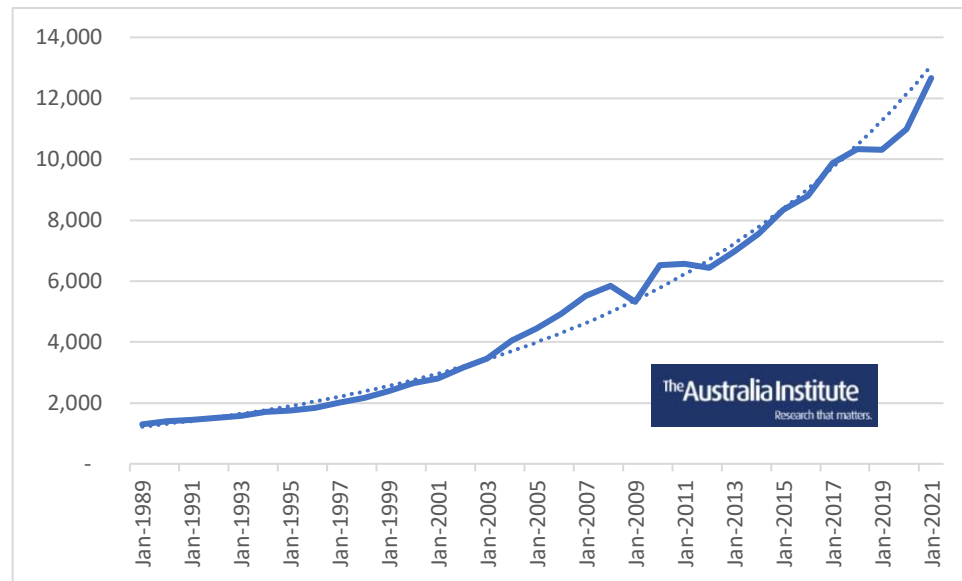
Wealth

Figures for Australia show that since September 1988 net worth has been growing at 7.5 per cent per annum and, as mentioned above, is now (March 2021) \$12,665 billion.² At that rate in 40 years it would be \$228,521 billion or roughly \$89,160 billion in 2021 prices. Figure 1 plots household net worth over that period. These figures are shown in the solid line in Figure 1 with a trend line included as the dotted line in Figure 1.³

² September 1988 is the first data point in the ABS series for household net worth.

³ To cope with the volatility of capital gains other researchers have used cumulative measures of income. Here the preference is to use trend annual data. Experimenting with alternatives in excel it appeared the exponential functional form was the best fitting equation for the data with a high correlation coefficient of 0.9859.

Figure 1: Household Net Worth, \$billions



Source: Author's calculations and ABS (2021b).

Figure 1 shows a very steady upward trend in the movement of household net worth over the period since 1989. There is a hint in the graph that the upward trend might have been even higher were it not for the global financial crisis on the one hand and the weak economy just prior to pandemic and then the pandemic itself on the other hand. The strength of the trend suggests it is likely to continue well into the future.

Of the total \$12,665 billion in household net worth, household savings contributed just \$1,215 billion in the period since March 1989 (ABS, 2021c), the rest was capital gains worth some \$10,116 billion over that period together with the opening balance of \$1,298 billion in March 1989. At the moment (2021) household net wealth is 6.37 times GDP and on the projections net household wealth will increase to 14.0 times GDP in 2060–61. It is not clear what has driven the large increases in wealth either in Australia or elsewhere. Monetary policy has been implicated. Reductions in interest rates can increase the value of many assets since they increase the discounted value of any stream of income that may be associated with the ownership of particular assets or businesses. However, just looking at the graph in Figure 1 it is hard to discern the influence of the low interest rate policy. The trend before the global financial crisis was at least as strong under higher interest rates.

Capital gains

The consequences of leaving capital gains and wealth out of the analysis is very important. The hidden income will have a lot of consequences for notions of capacity to pay tax and general wellbeing and how those might be estimated. Moreover, given the relative stability of the estimated equation in Figure 1 it can be used to make rough projections over the next 40 years which show household net wealth grows at a very fast rate. One of the implications already mentioned is that capital gains will swamp income and/or GDP as measured in the national accounts. Using the equation in Figure 1, trend capital gains are 44 per cent of GDP in the year to March 2021. This means that a conventional analysis misses almost half the actual income

in Australia.⁴ When it is assumed that household net wealth continues to grow at 7.5 per cent over the next 40 years and using the average growth in nominal GDP assumed by the IGR (5.0 per cent) then in the year 2060–61 capital gains will increase to \$18.2 trillion and will equate to some 132 per cent of GDP at that time. Some of that \$18.2 trillion will be household savings (\$0.42 trillion) implying capital gains of \$17.8 trillion which will be 127 per cent of GDP. This is a large increase in the wealth to GDP ratio driven by just the small gap in the assumed growth in wealth, 7.5 per cent, compared with the 5.0 per cent increase in GDP. The gap of just 2.5 per cent per annum is sufficient to generate the rapid rise in wealth to income ratio that concerns Piketty (2014) for example.

Conventional thinking about income needs to be thoroughly re-examined because conventionally measured GDP will only be 43 per cent of H-S income in 40 years' time on these trends. This type of finding suggests Australia and many other OECD countries are on their way to some new Belle Époque as foreshadowed by Piketty (2014). While Belle Époque applied to the lifestyle of the elite, the importance of the period for Piketty was the extreme inequality in which the ranks of the wealthy could live in a style that was unattainable to those with even the best of wage and salary jobs.

Note too, income itself may be incomprehensibly bigger. Unlike conventionally measured income, there does not appear to be any mechanism that would stabilise a net worth to GDP ratio nor a capital gains to GDP ratio. The productive capital stock in an economy might also asymptote towards an equilibrium share of GDP.⁵ However, wealth not only includes the industrial capital stock but also a lot of other assets, such as art works, jewellery, private dwellings, commercial real estate and so on.

Further reflections on wide conceptions of income

In considering why macroeconomic discussions traditionally ignore capital gains it might be noted that the traditional conception of income as equal to production has a lot of appeal. Most of the concepts in the national accounts are based on transactions between different players in the economic system. Capital gains, however, are not transactions. There are exceptions but for the most part the national accounting income concepts derive from transactions and subsequently on transfers made out of the original income. H-S income is not constrained at all by GDP or any other magnitude. The capital gains component of H-S income as a valuation phenomenon can in principle lead a life completely independent of other GDP magnitudes as seems to be the case.

The use of GDP has been criticised in other contexts. As is well known, national accounting aggregates were never intended as a measure of wellbeing. Instead, they merely set out to measure what was being bought and sold in the market. Those who designed the national accounts in the 1930s and 1940s knew they were not designing measures of human welfare but merely aimed “to provide a gauge of the size and health of the economy” (Syrquin, 2011).

⁴ GDP is much larger than household income with capital gains being 54 per cent of measured household income.

⁵ If net investment is say 25 per cent of GDP and GDP is growing at 5 per cent, then the capital stock to GDP ratio will stabilise at 5 (= 25/5). But there is no mechanism that ties capital gains to any national accounting magnitude. It is very important to distinguish wealth from the capital stock which is used in the production of goods and services.

Critics point to examples such as the contraction in GDP if someone marries their housekeeper or that cleaning up pollution can increase GDP. Hence GDP can be a perverse measure of wellbeing. These and other limitations of GDP are discussed in a report by Stiglitz and others commissioned by the French Government which kicked off a renewed worldwide discussion on the limitations of GDP (Stiglitz et al, 2009). It is interesting that to date there has been little analogous discussion about whether GDP is the appropriate yardstick for evaluating fiscal issues such as the fiscal consequences of ageing discussed here.

During the Second World War Keynes was interested in measures of GDP and related magnitudes with the aim of measuring how much output might be increased or reoriented for the war effort (See Tilly 2009). For present purposes the productive capacity is less important than the questions of how much tax to raise and how to distribute tax liabilities. Hence, the present paper is more interested in the capacity to finance spending in which case the “burden” can be shared with income sources that do not derive from GDP, including capital gains which is our present concern.

Fairness

It is already inequitable that wealth and capital gains are taxed so lightly in many jurisdictions. OECD statistics for example show that the OECD average tax on property was 1.9 per cent in GDP in both 1965 and 2018 (OECD, 2020). The average theoretical tax rate applying to capital gains on real property was just 15.7 per cent in 2016 with capital gains on shares subject to various concessions relative to taxes on other earned income (OECD, 2018). The idea of a fair tax is that two people with the same income, the same deductions and so on should be taxed the same no matter the source of that income. Capital gains or personal exertion or any other source of income should be taxed equally. Paul Krugman says, “the low tax rate on capital gains is bad economics, even ignoring who it benefits” (Krugman, 2012). The failure to adequately tax capital gains is an enormous advantage to the rich.

Here the Australian example is used but while the actual numbers will be different in different economies, the qualitative message should be much the same. In Australia the average top 20 per cent of wealth holders enjoyed capital gains of 108 times the capital gains of the lowest 20 per cent of wealth holders. That suggests capital gains will continue to inflate the incomes of the wealthy and so worsen the distribution of income in Australia. The following table examines the incomes of the five quintiles by wealth ownership. Row 1 in Table 1 presents total gross income as defined by the ABS and which excludes capital gains. In the next row capital gains themselves are given. Capital gains for each quintile are calculated by deducting the previous years’ net worth from net worth in 2017-18 (the latest figures available)⁶ That may mean incorrectly attributing some acquisition of assets to capital gains instead of savings, however, that should not change the orders of magnitude calculated here. The final row in Table 1 is our estimate of the H-S income.

⁶ In fact, data limitations meant having to use the difference between 2017-18 and 2015-16 and dividing by two.

Table 1: Household Wealth Quintiles, Incomes received, 2017-18, \$.

	Lowest	Second	Third	Fourth	Highest	Ratio highest to lowest
Total gross income	80,712	138,893	152,217	164,185	270,696	3.4
Capital Gains	3,591.50	22,068	71,704	129,367	389,396	108.4
Total (H-S income)	84,304	160,961	223,921	293,552	660,092	7.8

Source: Author's calculations and ABS (2021a) *Australian National Accounts: Distribution of Household Income, Consumption and Wealth, 2003-04 to 2019-20*, 18 June.

Before discussing the implications of Table 1 it is important to note that wealth and income are poorly correlated. While it may strike the reader as odd in that, for example, the bottom quintile has an apparently reasonably high gross income. Younger households may well enjoy reasonably high incomes. That means when considering the least wealthy we are not necessarily looking at the lowest income earners. Households with new entrants into the labour force are likely to have few assets but may have high incomes. At the other end of the wealth distribution, high wealth households may use companies and other vehicles to hold assets and receive income and may not necessarily pay themselves much of that income.

The important thing about Table 1 is that it shows rather dramatically how capital gains are critical to understanding income and wealth in Australia. Standard measures of income suggest the wealthiest households receive 3.4 times the income of the least wealthy. That seems a rather small degree of inequality. However, as might be expected, there is a large inequality in the distribution of capital gains with the ratio of top to bottom at 108.4 to 1 meaning the top 20 per cent of households receive 108.4 times the capital gains received by the bottom wealth quintile. The effect of capital gains then is to increase the top to bottom comprehensive income ratio which becomes 7.8 to 1, much higher than simple income measures might suggest.

Looking ahead, as net worth outpaces conventional income measures so too will capital gains increase relative to other incomes. That implies that ever higher capital gains will pull the income distribution further apart. For example, if capital gains were twice as high as shown in Table 1 then the top to bottom ratio would blow out from 7.8:1 to 11.9:1. So long as wealth to income ratios are rising then this sort of trend is inevitable.

Of course, as wealth increases it is also possible and perhaps likely that its distribution will be biased even further in favour of the rich. The OECD has noted that the “growth in net wealth levels has ... been very uneven across the distribution. On average, wealth levels for the top 10% have grown by 13% in real terms over the past decade, and wealth for the next 50% has increased by 6%. Meanwhile, the bottom 40% saw their average wealth shrink by more than 12%. This resulted in widening wealth gaps, with the wealth shares of the wealthiest 10% increasing at the expense of the remaining 90% of households. This development affected most countries...” (OECD 2021).

In its latest *Annual Economic Report* the Bank for International Settlements (BIS, 2021) has examined the effect of monetary policy on the distributions of income and wealth in the major high-income countries. To the extent that lower interest rates encourage economic activity the BIS argues that should be associated with lower inequality that tends to follow higher employment. In relation to wealth, the BIS suggests that lower interest rates may have increased wealth but not worsened the inequality in wealth ownership. Indeed, if housing is a higher proportion of wealth at the bottom of the distribution and if lower interest rates disproportionately affect house prices, then the distribution of wealth may even be improved somewhat.⁷ But Table 1 illustrates the problem in thinking that way. Suppose wealth increased across the board but without changing the ratio of the top to bottom wealth holders or any of the other distributional measures. That is likely to mean that the annual capital gains are also doubled. The very unequal distribution of capital gains can greatly worsen the distribution of H-S income compared with conventionally measured income and it all gets so much worse as both wealth and capital gains increase.

One thing is clear; higher levels of wealth generate higher capital gains as wealth gets bigger and higher wealth is likely to be associated with even higher future capital gains. And this greatly worsens the equity of the distribution of comprehensive income. This shows how the BIS fell into a trap by separately considering income and wealth distributions. It is important to appreciate how they interact through the capital gains mechanism.

The exact numbers are uncertain, but these exercises drive home the point that the increase in wealth and associated capital gains will be associated with worsening inequality. Wealth tends to be held by the older people so that higher wealth levels will increase the disparities between young and older households. But even within the older households there will still be large numbers with low incomes and low wealth. Hence there will be in the future, as there is now, a problem associated with a good deal of inequality among older people. Those inequalities will be an issue for future generations to address. But it is important to stress that this is not a young versus old problem as it is often portrayed; it is more correctly a problem of rich v poor or even old rich v old poor. There need only be a higher burden on the working age population if governments chose not to fairly tax wealth and capital gains.

Without wanting to stray too far off topic, it is interesting to also reflect on the fact that while there has been a trend towards lower company tax and lower personal taxes on capital income, it is not national governments but organisations such as the IMF that have led the charge to change course. The IMF has also long opposed the trend towards international tax competition and former IMF director Christine Lagarde (2014) stated that “there would be more revenue for all if countries resisted the temptation to compete with each other on taxes to attract business. By definition, a race to the bottom leaves everybody at the bottom”. The IMF pointed out that taxes on capital income are important in “shaping the progressivity of a tax system” (IMF, 2017). It stressed that capital income is distributed more unequally and has been rising relative to other incomes and is now a large share of personal incomes among the rich. It cited American figures that show the highest 400 taxpayers received 60 per cent of their income in capital gains which we take to be realised capital gains.

⁷ As it happens, housing for the bottom and second bottom quintiles is 28 and 35 per cent of net worth compared with just 16 per cent for the top quintile. However, it is not clear that price increases for housing have outstripped the prices of other assets.

Revenue implications

Any discussion of capital gains and expanded definitions of income raise the question of taxation and there are important implications for the revenue effort from our discussion to this point. In the US it is more widely recognised that it is the top income earners who escape their fair share of tax through the low or concessional rates on capital gains. As Krugman (2012) says:

“The main reason the rich pay so little is that most of their income takes the form of capital gains, which are taxed at a maximum rate of 15 percent, far below the maximum on wages and salaries. So the question is whether capital gains — three-quarters of which go to the top 1 percent of the income distribution — warrant such special treatment.”

Incidentally, for comparison, the top one per cent of taxpayers in Australia declare 56 per cent of all the capital gains declared to the Australian tax office but declare a much smaller 9.6 per cent of all taxable income. For the top ten per cent the figures are respectively 71 per cent and 21 per cent.⁸ This confirms that capital gains overwhelmingly go to the rich and are taxed more lightly than regular income. Anthony Atkinson, Thomas Piketty, and Emmanuel Saez have pointed to the world-wide erosion of the tax base with the gradual removal of capital gains.

“The erosion of capital income from the progressive income tax base. Early progressive income tax systems included a much larger fraction of capital income than most present progressive income tax systems. Indeed, over time, many sources of capital income, such as interest income or returns on pension funds, have been either taxed separately at flat rates or fully exempted and, hence, have disappeared from the tax base. In all cases, only realized capital gains are included, if at all, in tax statistics and no information on accruing capital gains is available.” (cited in Roth 2021)

Of course, in the US and other tax jurisdictions, capital gains attract tax only when they are realised. Part of the reason is obviously practical; measuring capital gains is difficult and many unique assets do not have deep and liquid markets in which market valuations can be used to assess the value of assets and how they change over time. However, the failure to tax capital gains until they are realised gives a large benefit to their beneficiaries. Capital gains are allowed to grow at compound growth rates until they are eventually realised, if at all.⁹ It remains very unfair that capital gains are tax free for the most part.

A topic not discussed so far is the implication for tax avoidance. While an important topic in its own right it also reminds us that ordinary definitions of income will miss a good deal because people are deliberately disguising other income as capital gains. That makes the exclusion of

⁸ Author's calculations with data from Australian Tax Office (2021).

⁹ To appreciate the benefit of taxing capital gains only on realisation, compare two tax regimes, one with an annual 35% tax on capital gains as they accrue and second, taxing them on realisation. An asset worth 100 held for 40 years appreciating at 10 per cent per annum has an after-tax value 2.5 times higher if taxed on realisation rather than on an accrual basis.

capital gains from much macroeconomic analysis so much worse and incidentally hides a lot of the income at the top end of the income distribution (see Advani and Summers, 2020).

An alternative to taxing capital gains on an accrual basis is to tax wealth on a regular basis, a topic taken up in the next section. Before doing that note that there are other policies that may also be worth examining but will not be pursued further in this paper. To begin with economic power and the associated monopoly profits are increasing throughout the world. Those feed into non-labour incomes and often through low or lightly taxed capital gains. Attempts to limit that power through such things as competition policies and price controls should slow down the accumulation of wealth on the part of the rich even if there is little evidence of that to date.

Wealth tax

To the extent they are taxed at all, capital gains are only taxed on realisation. In that way avoidance avenues can take place by simply not realising capital gains. The arguments against taxing gains as they accrue are pragmatic. It would be difficult to measure the value of assets each year to calculate tax liabilities.

If capital gains are difficult to tax because they are difficult to value, then perhaps wealth should be taxed instead. Wealth could be deemed to have a provisional value equal to historic cost plus the general level of inflation since purchase. The existing capital gains tax could then be used to impose additional tax or give credit for any gains on realisation above or below the amounts previously assessed. Atkinson points out that while the increasing inequality due to capital gains suggests that tighter capital gains taxes may be warranted, in fact a good deal of the capital gains have already taken place and dramatically increased the wealth of the wealthy (Atkinson, 2015). Past capital gains can only be captured by a wealth tax of some sort.

A capital tax or wealth tax is the major policy Piketty calls for to address the fundamental problem identified in his book—the tendency for wealth to grow more quickly than the economy generally and so for wealth to be more and more unequally distributed among the population.¹⁰ Piketty suggests that those with “fortunes” worth less than €1 million might pay nothing, while a tax of 1 per cent would apply to “fortunes” between €1 million and €5 million, and 2 per cent to those greater than €5 million. Piketty thought the tax would need to involve international cooperation with respect to rates, definitions and similar so as to avoid countries being played off against each other. A wealth tax as a tax on capital is not related to the rate of return on capital or the way it is invested and so wealth owners will be undeterred from seeking out the best pre-tax return options. Such a tax design would indeed seem to leave the investor with a fixed tax irrespective of the return they might earn.

Stiglitz (2020) has argued that Europe should pursue wealth taxes more aggressively both for the revenue potential but also to address concentrations of family wealth built up through inheritance. That would include annual wealth taxes but also death duties. Stiglitz cites studies that show inherited wealth discourages work on the part of the beneficiaries. That makes sense; people who can live in comfort without working are unlikely to take it up.

¹⁰ See Piketty T (2014). Piketty’s work leads him to the conclusion that everything is dominated by the rule; $r > g$ where r is the rate of return on capital and g is the growth in the economy. If r exceeds g , then the stock of wealth is growing more quickly than the size of the economy.

Mike Truman (2006), the then editor of the journal *Taxation*, put it very well when he said:

“...the problem with inheritance tax is that we’re not paying enough of it...For all its faults in practice, it is in principle a perfect tax. ... the tax liability comes at a point where those who did have the money no longer need it, and those who are about to get the money have managed quite well so far without it. Except in a very few cases, there is no problem with liquidating assets in order to get the funds to pay the tax.”

Conclusions

This paper has tried to show that the existing analyses of intergenerational issues fail to take account of some of the major trends that will determine the wellbeing of a nation, how that wellbeing is distributed between the rich and poor and the extent to which a nation can “afford” government services in the context of an ageing population. As the introductory public finance textbooks point out, broad definitions of income should be used. That includes the present case when looking at questions such as the future wellbeing people and how that might be affected by the revenue raising efforts of future governments along with trends in government spending. Wealth and increases in wealth, or capital gains, are typically missing in such analysis but are very important in considering whether or not people can “afford” government services.

By including capital gains in a comprehensive measures of income such as the Haig-Simons income measure the distribution of income looks so much worse than when capital gains are ignored. This is likely to be the case in most OECD countries and maybe much worse in some of them. Observers who focus on just the distribution of income or wealth in isolation miss an extremely important joint dynamic that ever increases the income disparities. Given the likely huge increase in income (widely defined) over the next four decades it is ludicrous to suggest there are any social needs national governments cannot “afford” if they want to. Eventually countries are going to have to face the need to tax capital gains more seriously and/or bring accumulated wealth into the tax system. That is not to suggest that taxes must finance all spending but as the distribution of comprehensive measures of income is driven further apart it must become more apparent that many people are waking up with ever-increasing wealth each morning having done nothing to earn it.

The booming wealth and capital gains in the system will profoundly affect the social and economic structures emerging over future decades. But it is incorrect to put the intergenerational problem in terms of the burden an ageing population will impose on future generations. The future generations are going to have to ensure that the rich and very rich old people assist the poor old people in their communities. We have misallocated our worries towards thinking that the problem is one of entitlements to government services when it is not. We can only wonder at the political power behind tax systems that impose most of the burden on ordinary people as they work or consume but very light taxes on wealth and capital income, especially capital gains. It is hard to even imagine a better con on the part of the rich who:

- Take most of their income in a form that is hidden from even the national accounts and does not appear in most distributional analysis.
- Lobby hard to ensure that even when it is taxed on realisation, it is taxed lightly, while

- Traditional definitions mean that half or more again of the income going to the rich is barely noticed at all.
- If capital gains are noticed we are told they are not real income, just market fluctuations, so don't bother your pretty heads about it all.

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Price Indices Suitable for the Monetary Policy: A Measurement in Economics Perspective

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Abstract

Although the consumer price index (CPI) has constituted the first choice as the monetary policy target, currently central banks might examine a “second-generation approach” based on other indicators. Founded on an extensive literature review into the subdiscipline called “measurement in economics”, we will outline some suggestions to upgrade the monetary policy framework applied by central banks. Firstly, we elaborate on a methodological guideline. Next, we review the theoretical foundation of the CPI, that is, the notion of the purchasing power of consumers. Later on, taking as a starting point some theoretical insufficiencies and practical concerns relative to the CPI compilation, we offer some considerations about the unsuitability of using CPI growth rate as the monetary policy target. We then submit our proposals.

Introduction

The consumer price index (CPI) has constituted the first choice as the monetary policy target, to the extent is widely known by the general public and is available on a timely basis. Without ruling out the above, currently, central banks might examine a “second-generation approach” based on other indicators, such as the producer price index (PPI) or the gross domestic product (GDP) deflator (Bloem, Armknecht, and Zieschang, 2002, p. 173).

The aforementioned authors, officials of the International Monetary Fund, argued in the following terms (2002, p. 180):

“The conclusion of the foregoing discussion of the scope and valuation dimensions of inflation indices is that such an index should satisfy several criteria. An inflation index should include all types of monetary transactions; exclude nonmonetary transactions; exclude taxes and subsidies on products in the valuation of market transactions; and permit transparent analysis of the contributions to change in the inflation index arising from the prices of exported and imported goods and services.”

Founded on an extensive literature review into the subdiscipline called “measurement in economics” (among others, Haavelmo, 1944; Solow, 1957; Arrow, 1958; Nordhaus, 1996, 1998; and Deaton, 1998), we will outline some suggestions to upgrade the monetary policy framework applied by central banks in nowadays.

Firstly, we elaborate on a methodological guideline. Next, we review the theoretical foundation of the CPI, that is, the notion of the purchasing power of consumers. Later on, taking as a starting point some theoretical insufficiencies and practical concerns relative to the CPI

compilation, we offer some considerations about the unsuitability of using CPI growth rate as the monetary policy target. We then submit our proposals, among others the following:

- 1) We recommend that monetary authorities ask the statistical agencies to compile *additional* consumer price indices, to dispose of better information on the dynamics of the purchasing power of citizens throughout the expenditure distribution.
- 2) When conducting their monetary policy, including the determination of the inflation target, central banks have to take into account the measurement biases of the CPI and not to overlook that measured inflation, and its volatility, are the result of the application of an instrument that numerically *resizes* the available price information.
- 3) It would be convenient that central banks add, as a monetary policy target, at least one of the other two major price indices of an economy, the GDP deflator –which would require the change of its frequency from quarterly to monthly– or the PPI. Under this scenario, monetary authorities: 3.1) would have as a target a broader price index growth rate, with its matching instrument, the interest rate; and 3.2) would have an intermediate target in terms of the CPI growth rate –which is a subset of the prices of an economy–, with the intention of taking care of the purchasing power of consumers. To the extent that the link between the PPI and CPI growth rates is, firstly, a theoretical matter –for example in terms of its scope–, and secondly, an empirical one, central banks would have to analyze it carefully in order to determine the target –in terms of the PPI growth rate–, and as its byproduct, the *intermediate* target –in terms of the CPI growth rate. Once again, the PPI growth rate would be the policy target –with the aim of maintaining the current transparency and accountability of central banks– and the CPI growth rate would be merely an intermediate target.

Methodological guideline

Scientists should make a distinction between the theoretical definition of a variable, its measured value, and its true value (Haavelmo, 1944). An applied economist, for example, should be aware of the weak or strong correspondence between a theoretical definition and its observed value, and of the difference between the true value and its measured one –known as the measurement bias. With sharpness, Morgan (2001, pp. 236-7) illustrated Haavelmo's concerns:

“Whereas some might suppose that economic measurement is merely counting ‘what is there’, a similarly naïve view would have us think that x-ray machines merely look through our flesh to reveal bones. We don’t see a macroeconomy, nor a consumer price index... hence fashioning measuring instruments in economics has been, in part, a matter of developing ways of observing the economy... Economic observations must not only be registered but also converted into measurements, and converted in ways which serve particular theoretical or empirical or bureaucratic purposes.”

Extending Haavelmo (1944), Koopmans (1947) labeled Burns and Mitchell's efforts to analyze the business cycle empirically as “measurement without theory”. Although Mitchell was regarded, initially, as one of the main exponents of quantitative economics in the early twentieth century, for Hendry and Morgan (1996, p. 8) “he cannot be said to have adopted statistical ways

of thinking... Instead, he is an empiricist who adopted quantification as a natural tool for gleaning evidence in economics.”

And Mitchell participated in the decision to rename the Cost of Living Index (COLI) as a CPI, which implied, as will become clear later, narrowing its microeconomic foundations. Although it seemed that it was a theoretical discussion, strictly speaking, the debate at stake was about the *precision* of the measurement. On that Persky (1998, p. 204) wrote the following: “However, the Mitchell committee did recommend that the name of the ‘Cost of Living Index’ be changed, arguing that an index of prices couldn’t fully reflect changes in welfare (Davis, 1944, p. 23), and as a result, the Bureau of Labor Statistics renamed its series as a ‘Consumer Price Index’.”

So data are theoretical constructions, or in other words, scientific objects, in the sense that they constitute the measured values of theoretical definitions and because of the manipulation of measuring instruments for its compilation. In particular, the subdiscipline known as measurement in economics strongly advises not to lose sight of the precision of the observed values. Lequiller and Blades (2014, p. 44) addressed the case of the emblematic variable of the System of National Accounts:

“National accounts could better be called ‘national accounts statistics’ because without this qualifier users may think they are as reliable as the business accounts of a company. This is not true. In particular, while GDP for technical reasons is often expressed in millions of units of the National currency, users should be aware that they are very, very far from being accurate at the level of millions... It is not even possible to give a summary figure of the accuracy of the GDP. Indeed, national accounts, and in particular GDP, are not the result of a single big survey for which one might compile a confidence interval. They are the result of combining a complex mix of data from many sources, many of which require adjustment to put them into a national accounts database and which are further adjusted to improve coherence, often using non-scientific methods.”

A recent point of reference is a paper written by Moulton (2018, p. 31 and p. 33), a former official who worked for 32 years at the US statistical system. He estimated a pair of mismeasurements linked each other, that is, the upward bias of the inflation rate of 0.85 percentage points per year, and the underestimation of the economic growth around -0.65 percentage points per year during the post-Boskin Commission years. To size the mismeasurements it is worth to remember that, according to the Bureau of Labor Statistics (BLS) and to the Bureau of Economic Analysis (BEA), the US annual average inflation rate was 2.14% between 1997 and 2017, and its economic growth was 2.27% in the same period. According to Feldstein (2017, p. 2), mismeasurement in economics not only distort public and private decisions but may erode social cohesion, among other collaterals:

“The resulting widespread references to slow economic growth reduce the public’s faith in the political and economic system. The low measured growth of incomes exacerbates concerns about mobility with people worrying that they and their children are ‘stuck’ at low income levels. I think it creates a pessimism that contributes to political attitudes that are against free trade and critical of our market economy more generally... The underestimation of growth also distorts Federal Reserve policy. The perception of slow real growth now supports a Federal Reserve policy of exceptionally low interest rates that is contributing to potential financial instability. Back in 1996, Fed chairman Alan Greenspan persuaded members of the Federal Open Market Committee in 1996 that the

official data underestimated productivity growth so that maintaining strong demand would not cause a rise in inflation.”

With great insight, the guru of measurement in economics warned us that (Boumans, 2005, p. 121), a “relevant problem of instruments used to make unobservables visible is how to distinguish between the facts about the phenomenon and the artifacts created by the instrument.” The heart of the matter is that, in economics, measuring instruments functioned initially as “artifacts of measurement” but rapidly turned into “analytical devices” (Klein, 2001; and Morgan, 2001). Therefore, it is always necessary to keep in mind that measuring instruments shape our subject matter, and we would better use them with extreme caution all the time.

In short, measurement in general, and measurement in economics in particular, is full of risks and obstacles. Muller (2018, pp. 3-4) summarized them as follows:

“Used properly, measurement, as we’ll see, can be a good thing... (But measurement) can also distort, divert, displace, distract, and discourage. While we are bound to live in an age of measurement, we live in an age of mismeasurement, over-measurement, misleading measurement, and counter-productive measurement... There are things that can be measured. There are things that are worth measuring. But what can be measured is not always what is worth measuring; what gets measured may have no relationship to what we really want to know. The costs of measuring may be greater than the benefits. The things that get measured may draw effort away from the things we really care about. And measurement may provide us with distorted knowledge –knowledge that seems solid but is actually deceptive.”

The main measuring instruments used in economics are index numbers. This paper deals with a relevant one, the well-known CPI.¹ The implementation of this methodological guideline will allow us to put forward an upgrade of the current monetary policy framework.

On the theoretical foundations of the Consumer Price Index

The purchasing power, as a theoretical concern, is one of the oldest and persistent in what we call economics nowadays. Fleetwood (1707, 1745, [1969], p. 49), the “real father of index numbers” (Balk, 2008, p. 6), compared the purchasing power of an Oxford student from 1460 to 1707 using a basket with the following content, “5 quarters of wheat, 4 hogsheads of beer, and 6 yards of cloth”. In the same direction, according to Edgeworth (1925, p. 381) “the earliest treatise on index numbers and one of the best” is Fleetwood’s *Chronicon Preciosum*.

¹ Long time ago Foster (1922, p. vii) wrote: “All sciences are characterized by a close approach to exact measurement. How many of them could have made much progress without units of measurement, generally understood and accepted, it is difficult to image... In economics, however, as in education, though the need for quantitative measurement is as great as in physics or in medicine, we have been guided in the past by opinions and guesses... In the future, we must substitute measurement for guesswork. Toward this end, we must first agree upon instruments of measurement. To the Pollak Foundation for Economic Research, it seems fitting, therefore, that its first publication should be *The Making of Index Numbers*.”

The other two founders are C. F. Dutot and G. R. Carli. About the methods proposed by Dudot (1738) and by Carli (1764), Walsh (1901, p. 553 and p. 554) wrote the following:

“(Dudot)... compares the total sums made up, at two periods (times of Louis XII and Louis XIV), of the prices of the customary mass-units of various articles (including wages). [Uses the arithmetic average of prices with haphazard weighting. See Appendix C, I.]... (Carli)... averaging the prices of grain, wine and oil, in the periods about 1500 and 1750, he compares them by taking the earlier as units and reducing the later to the proper figures in proportion, thereby representing the variations, and draws the arithmetic average”.

Fleetwood's *un-weighted* price index constitutes the first registered attempt “to solve” what is currently named “the problem of index numbers”. Afriat (2005, pp. 21-2), “the guru of the price index” (Deaton, 2005, p. xvii), defined the problem we are dealing with as follows:

“In the other extreme, the only authority for the present is the past. In early economics, the correct price or wage is simply the price or wage that has been settled by custom and has the value which, so far as anybody can remember, it has always had... Price indices express respect for that authority. They offer a kind of exchange rate between £s in different periods... A force acting on what a wage should be this year is what it was in some former year converted into this year's £s. Sums today are measured by a yardstick which has reference to the past. The ‘index number problem’ can be understood as the problem of fashioning such a yardstick.”

The *Consumer Price Index Manual: Theory and Practice* (ILO et al., 2004, Chapter 15, corrected, p. 1) transformed our theoretical problem into a practical one: “How exactly should the microeconomic information involving possibly millions of prices and quantities be aggregated into a smaller number of price and quantity variables? This is the basic problem of index numbers.” Various “calibrating instruments” have been proposed in order to establish a common metric (Afriat, 2005, p. 22), among others, the fixed basket approach; the statistical approach; the test approach, also known as axiomatic or instrumental; the Divisia approach; the economic approach or constant utility; and the factorial approach.

Nevertheless, in reality, there are only two competing approaches to support the compilation of the CPI. The first one, known as the COGI approach, is based on a sort of a pragmatic point of view. From this perspective, the CPI (Schultze and Mackie, 2002, pp. 1-2) “measures the change in expenditures required by a household to purchase a fixed weight basket of goods and services when prices change between some initial reference period and a subsequent comparison period.” One example is the HICP, which is compiled by the European Statistical System and the National Statistical Institutes with harmonized methods, and is the index used in the European Central Bank's definition of price stability (ECB, 2014).

The second one, the so-called (Konüs) conditional COLI approach, provides a foundation of the fact that, when prices change, consumers do not continue to purchase the same fixed basket, but shift their purchases toward goods whose relative prices have fallen. The COLI quantifies the effect of this *substitution behavior* in reducing the expenditure required by a consumer to maintain a given standard of living when prices change. In this regard, from the trenches, Groshen et al. (2017, p. 188) highlighted that the BLS uses the COLI approach “as a unifying framework and is the standard by which BLS defines any bias.” Triplett (2001, p. F315),

chief economist at the BEA from 1985 to 1997, explained the full meaning of the adoption of the economic approach to base a CPI program:

“Constructing a CPI is not just a matter of choosing a formula that combines the detailed component indexes-price indexes for coats and carrots and computers and cars, indexes that are sometimes called ‘elementary aggregates’. Hundreds and perhaps thousands of decisions must be made in measuring those detailed component indexes. Those decisions are not solely statistical or sampling or collection and processing decisions. Many of them involve economic questions – they are ‘what do we want to measure?’ questions. Applying the theory of the COL index to the CPI means that those ‘What do we want to measure?’ decisions are guided by a consistent, overall decision-making framework, which is the economic theory of consumption.”

From our perspective, to conceptualize the CPI as a COLI means recuperate its original purpose, that is, the measurement of the purchasing power of a target population. As a consequence, we would be able to highlight the closeness between the theoretical object and its empirical counterpart, and we could call the compilation of prices and its conversion into a CPI as an exercise of “measurement with theory”.

The foundation of the CPI as a COGI or as a COLI has subtle implications. For example, US authorities not only acknowledge that their statistical measures have potential errors and biases, but for a long time, they have been trying to estimate them and reduce them. Ulmer (1946), an official at the Department of Commerce, written the first guesstimate of the substitution bias. He estimated a maximum bias of 1.5 percent per year over the period 1929-1940 in the CPI. On the opposite side, ILO et al. (2004, chapter 11, point 11.66) proposed a mixed balance, which may be useful to understand the generalized adoption of a COGI instead of a COLI approach: “Statistical agencies have been reluctant to provide their own estimates of CPI bias. In some cases, they have accepted the existence of substitution bias... Statistical agencies have, however, been reluctant to draw even qualitative conclusions from fragmentary and speculative evidence on quality change, new products and new outlet bias.” To the extent measurement errors in the CPI might lead to inadequate monetary policy actions (Smets, 2006, p. 9), it is central bank officials who have researched on biases typically (among others, Lebow and Rudd, 2003 and 2006; Rossiter, 2005; Shiratsuka, 2006; and Sabourin, 2012).²

On the notion of purchasing power

The primary objective of a central bank is to procure the stability of the purchasing power of the national currency. It sounds correct and substantial. Nonetheless, we think that here lays a subtle misunderstanding. The purchasing power is not a characteristic of a coin. Only to the extent that a consumer’s spending pattern is known, it is plausible to define her purchasing power, or making an abusive use of the language, the purchasing power of her earnings.³

² In the annex we present a summary table about the measurements of the CPI biases.

³ Retrieving the analysis of money’s functions, Afrait (2005, p. 59) endorses our point of view: “The distinction made between exchange value and use-value for goods produces the idea that money, or–what is more specific and understandable–any income, has a purchasing power which is variable depending on prices. The distinction is recognized, but at the same time there is need to avoid the

As a scientific community, we believe, we all share this subtle misunderstanding, and possibly it has its root in a literal reading of Fisher's *The Purchasing Power of Money, its Determination and Relation to Credit, Interest, and Crises*. Perhaps some quotations, necessarily long, may be useful to improve our understanding of Fisher's ideas. At the beginning of his book, Fisher (1922, p. 25) clarified that we are dealing with a microeconomics issue related to consumer's decisions: "The equation of exchange may therefore be written: $MV = \sum pQ$. That is, the magnitudes M , V , the p 's and the Q 's relate to the entire community and an entire year; but they are based on and related to corresponding magnitudes for the individual persons of which the community is composed and for the individual moments of time of which the year is composed."

In page 106, Fisher proposed shortlists of Q 's and P 's –and reviewing them, we found the description of a model of the entire economy and its *intrinsic price heterogeneity*:

"The chief subclasses under these three groups, which occur in actual sales, may be indicated as follows: wealth (real estate and commodities), property (bonds, mortgages, private notes, and time bills of exchange), and services (of rented real state, of rented commodities, of hired workers, and of some or all these agencies combined)... The prices of these various classes of goods cannot all move up and down in perfect unison. Some are far more easily adjustable than others. Only by extremely violent hypotheses could we imagine perfect adjustability in all. The order of adjustability from the least to the most adjustable may be roughly indicated as follows: 1. Contract prices of properties and services, especially where the contracts, are for a long time; these include bonds, mortgage notes, use of real estate by leases. 2. Contract prices of properties and services, where the contracts are for a shorter time; these include bills of exchange, use of rented real estate and commodities, services of workmen, etc. 3. Prices of commodities made of the money metal. 4. Prices of substitutes for said commodities. 5. Prices fixed by law, as court fees, postage, tolls, use of public utilities, salaries, etc. 6. Prices fixed by custom, as medical fees, teachers' salaries, etc., and to some extent wages. 7. Prices of real estate. 8. Prices of most commodities at retail. 9. Prices of most commodities at wholesale. 10. Prices of stocks."

Finally, in page 105 Fisher summarized the condition, based on an "extremely violent hypotheses", that enabled him to integrate the notion of a single price level in the context of the quantity theory of money: "We have found that the general level of prices is determined by the other magnitudes in the equation of exchange. But we have not hitherto defined exactly what a 'general level' may mean. There was no need for such a definition so long as we assumed, as we have usually done hitherto, that all prices move in perfect unison. But practically prices never do move in perfect unison."

Our interpretation is that Fisher was perfectly aware that, in his analysis, he juxtaposed the spheres of production and consumption, that is, on the one hand, the monetary label for Q 's, on the other hand, the purchasing power of individual persons. Fisher chose as the title of his

awkwardness of maintaining it fully. The need must be understood, because otherwise there can be bewilderment at consequences of it."

book what he thought was a correct mixture of words in the context of the *equation* of exchange founded on the quantity theory of money.

Unsuitability of using the CPI growth rate as the monetary policy target

According to Schumpeter (1954, p. 14): “We need statistics not only for explaining things, but also in order to know precisely what there is to explain... It is impossible to understand statistical figures without understanding how they have been compiled.” His perspective is relevant to the following discussion.

Founded on our measuring in economics approach, we want to propose four considerations about the unsuitability of using the CPI growth rate as the monetary policy target. Firstly, we have a pair of theoretical definitions –which obey two distinct purposes– and its measured values. In this sense, it is a confusion to identify the CPI –an observed value– with the price level of an economy –a theoretical variable. We have, on the one hand, the purchasing power of a target population, typically the urban consumers, and its measurement, or from a COGI approach, without more ado, a CPI –that serves to “measure the change in expenditure necessary to maintain a certain consumption pattern.” On the other hand, the price level of an economy, which, to say it briefly, synthesizes the supply and demand forces of markets, and its observed values, the GDP (Paasche index) deflator or the Producer Price (Laspeyres) Index (PPI). On this first consideration, we propose a couple of examples. To show empirical evidence on money, prices, and output, Walsh (2003, pp. 13-4) utilized the detrended GDP deflator. In his seminal paper Taylor (1993, pp. 201-1) highlighted the following: “The Council of Economic Advisers published a consensus estimate that a one-year temporary increase in oil prices of 50 percent could temporarily raise the overall price level (GDP deflator) by about 1 percent and with a longer lag, cause real output to fall by about the same amount.” Therefore, it should be clear that the scope, or domain –as it is called in the specialized literature (Diewert, 2002)– of the CPI and the GDP deflator, or the PPI, does not match, implying that these measurements correspond to different theoretical definitions.

Secondly, the aggregate index numbers are not “neutral” statistical indicators (Muellbauer, 1976, p. 32). By construction, the CPI depends on the weights structure; specifically speaking, in the plutocratic method, the CPI relies on the spending pattern of the highest expenditure deciles (Ley, 2005). In this regard, according to Deaton (1998, p. 43), the household for which the American CPI weights are correct lies at the 75th percentile of the expenditure distribution. In the case of Spain, the applicable percentile is the 61st (Izquierdo, Ley, and Ruiz-Castillo, 2003, p. 149), and for the Mexican CPI, the percentile in question is the 86th (Guerrero, 2010, p. 2). To the extent the measured value of the CPI depends on the selected aggregation method, Arrow (1958), Prais (1959), and Pollak (1998), proposed different aggregation methods, that is to say, alternative weights to approximate the purchasing power of a population and its groups.⁴ For its part, in the case of the GDP deflator, its weights are the value aggregated shares of each one of the activities that structure the economy. For example, in the case of Mexico, the CPI aggregates price indices of 299 items strata, and according to the North American Industry Classification System (NAICS), the GDP deflator aggregates price indexes of 796 economic activities.

⁴ Ball (2019) has just solved the “puzzle” regarding the loss of validity of the Phillips curve after the Great Recession, using the *median* as a weight to determine the core inflation. Taking account that weighting information is not available at the elementary level, an extreme proposal would be the use of un-weighted formulae, such as the Dutot, the Carli or the Jevons indices (Ralph, O'Neill, and Winton, 2015, p. 125).

Thirdly, the numerical difference between the inflations measured using the CPI or the GDP deflator, the CPI or the PPI, or the differences between the inflation in each one of the items contained in a basket of goods and services and the one measured using the entirely CPI, may seem “negligible”, and to some extent they are, but solely by construction! Such a reduction –or “compromise” that is, the adjustment, as it was repeatedly written by Fisher (1922)– is the result of the selected measuring instrument, that is, a weighted average index number.⁵

As an illustration of the *resizing effect* of the Laspeyres formula, or said metaphorically the “funnel” effect, we prepare three examples. The selected period was due to the availability of information after the Great Recession. The US CPI-U (for all urban consumers) divides the population into 38 geographic areas called index areas and divides all goods and services purchased by consumers into 211 categories called item strata. In its hierarchical approach, we find major groups, expenditure classes, item strata, and entry-level items, which are the ultimate sampling units for items and represent the level of item definition from which data collectors begin item sampling within each sample outlet. The first example displays the growth rate of the first item strata –other bakery products (FB04)– that shows three entry-level items –fresh sweet rolls, coffeecakes, and doughnuts (S02063), crackers, bread, and cracker products (S0206A), and frozen and refrigerated bakery products, pies, tarts, and turnovers (S0206B)– of the CPI.

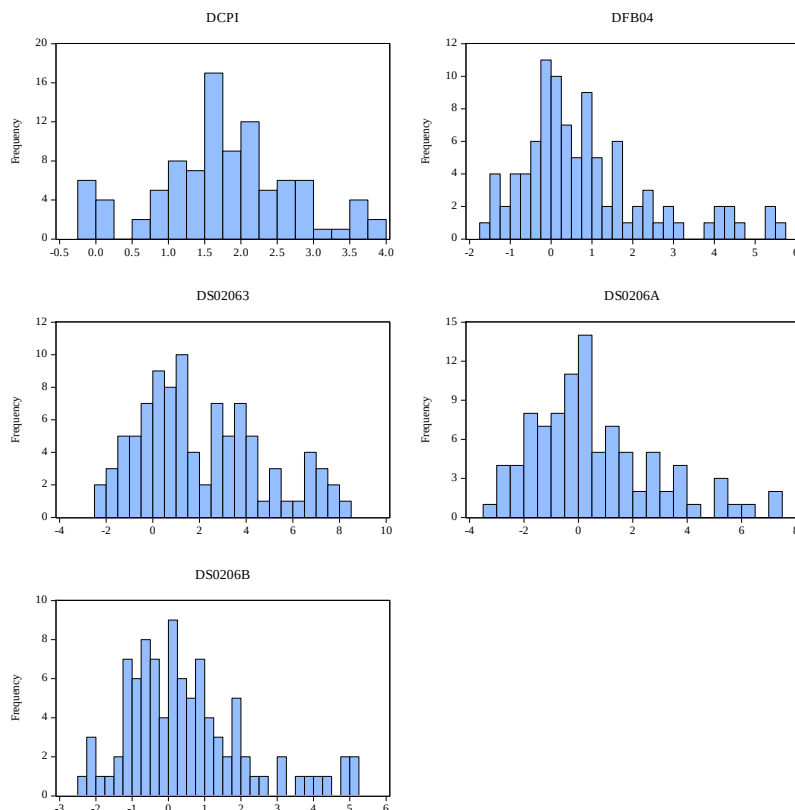
Table 1. Descriptives of (item strata and its entry-level items) price indexes growth rates, in percentage, from 2010.10 to 2018.10.

	Mean	Median	Maximum	Minimum	Standard deviation
Consumer price index	1.78	1.73	3.87	-0.20	0.95
Other bakery products	0.84	0.44	5.58	-1.60	1.63
Fresh sweet rolls, coffeecakes, and doughnuts	2.11	1.48	8.34	-2.40	2.64
Crackers, bread, and cracker products	0.60	0.14	7.31	-3.15	2.32
Frozen and refrigerated bakery products, etc.	0.51	0.22	5.23	-2.47	1.71

Source: Author’s calculations based on BLS monthly data.

⁵ With the intention of not being repetitive, we selected the following quotation (Fisher, 1922, p. 2): “There would be no difficulty in such measurement and hence no need of index numbers, if all prices moved up in perfect unison or down in perfect unison. But since, in actual fact, the prices of different articles move very differently, we must employ some sort of compromise or average of their divergent movements.”

Figure 1. Histogram of (item strata and its entry-level items) price indexes growth rates, in percentage, from 2010.10 to 2018.10, based on BLS monthly data.



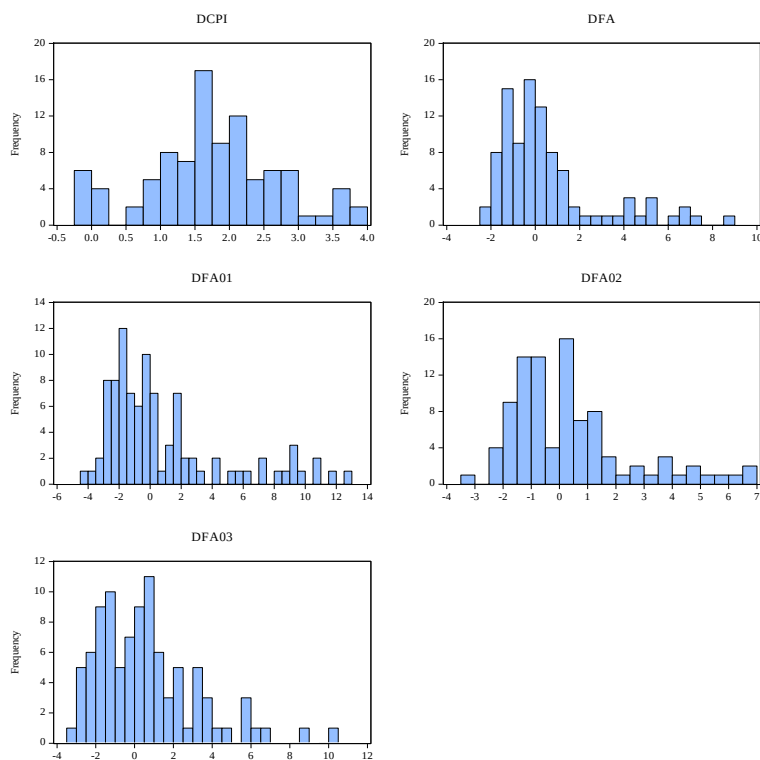
It is sufficient to compare the axes of the x in the panels of Figure 1 to find out how the dispersion of prices for the entry-level items was resized in the price index of the item strata (FB04) that groups them. It should be noted that this exercise was conducted at the highest available level of disaggregation. If we compare the scale of the changes of the item strata (FB04) and the CPI, the resizing effect generated by the measuring instrument is confirmed. The second example displays the growth rate of the first three item strata –flour and prepared flour mixes (FA01), breakfast cereal (FA02), and rice, pasta, and cornmeal (FA03)– of the CPI that compound the “cereals and cereals products” (FA) expenditure class.

Table 2. Descriptives of (expenditure class and its items strata) price indexes growth rates, in percentage, from 2010.10 to 2018.10.

	Mean	Median	Maximum	Minimum	Standard deviation
Consumer price index	1.78	1.73	3.87	-0.20	0.95
Cereals and cereals products	0.53	-0.07	8.55	-2.19	2.30
Flour and prepared flour mixes	0.84	-0.46	12.57	-4.08	3.97
Breakfast cereal	0.36	0.03	6.60	-3.08	2.12
Rice, pasta, and cornmeal	0.65	0.22	10.48	-3.01	2.63

Source: Author's calculations based on BLS monthly data.

Figure 2. Histogram of (expenditure class and its items strata) price indexes growth rates, in percentage, from 2010.10 to 2018.10, based on BLS monthly data.



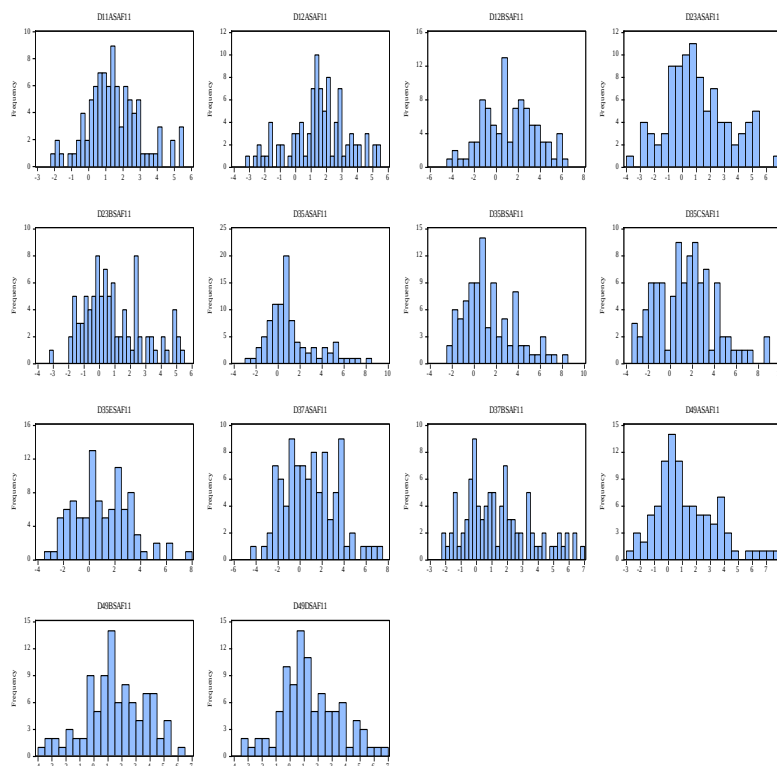
In Table 2, statistics of the price indexes that compose the “cereals and cereal products” are different, but the histograms evidence of how the price of the expenditure class compromised the prices of its item strata. The third example displays the growth rate of food at home price indexes by metropolitan areas. For the selected period and frequency, the urban regions available data were Boston-Cambridge-Newton, MA-NH (D11ASAF11); New York-Newark-Jersey City, NY-NJ-PA (D12ASAF11); Philadelphia-Camden-Wilmington, PA-NJ-DE-MD (D12BSAF11); Chicago-Naperville-Elgin, IL-IN-WI (D23ASAF11); Detroit-Warren-Dearborn, MI (D23BSAF11); Washington-Arlington-Alexandria, DC-VA-MD-WV (D35ASAF11); Miami-Fort Lauderdale-West Palm Beach, FL (D35BSAF11); Atlanta-Sandy Springs-Roswell, GA (D35CSAF11); Baltimore-Columbia-Towson, MD (D35ESAF11); Dallas-Fort Worth-Arlington, TX (D37ASAF11); Houston-The Woodlands-Sugar Land, TX (D37BSAF11); Los Angeles-Long Beach-Anaheim, CA (D49ASAF11); San Francisco-Oakland-Hayward, CA (D49BSAF11); and Seattle-Tacoma-Bellevue, WA (D49DSAF11).

Table 3. Descriptives of food at home price indexes growth rates by metropolitan area, in percentage, from 2010.10 to 2018. 10.

	Mean	Median	Maximum	Minimum	Standard deviation
Boston-Cambridge-Newton	1.44	1.36	5.40	-2.17	1.59
New York-Newark-Jersey City	1.50	1.55	5.49	-3.02	1.90
Philadelphia-Camden-Wilmington	1.18	0.98	6.33	-4.27	2.41
Chicago-Naperville-Elgin	1.08	0.84	6.76	-3.70	2.20
Detroit-Warren-Dearborn	0.89	0.45	5.30	-3.01	1.92
Washington-Arlington-Alexandria	1.20	0.68	8.47	-2.52	2.21
Miami-Fort Lauderdale-West Palm Beach	1.39	0.87	8.16	-2.25	2.33
Atlanta-Sandy Springs-Roswell	1.42	1.41	8.66	-3.40	2.65
Baltimore-Columbia-Towson	1.05	0.88	7.99	-3.28	2.19
Dallas-Fort Worth-Arlington	0.94	0.77	7.05	-4.36	2.38
Houston-The Woodlands-Sugar Land	1.42	1.02	6.96	-2.17	2.16
Los Angeles-Long Beach-Anaheim	1.26	0.75	7.53	-2.87	2.15
San Francisco-Oakland-Hayward	1.54	1.37	6.09	-3.78	2.17
Seattle-Tacoma-Bellevue	1.41	1.09	6.76	-3.49	2.12

Source: Author's calculations based on BLS monthly data.

Figure 3. Histogram of food at home price indexes growth rates by metropolitan area, in percentage, from 2010.10 to 2018.10, based on BLS monthly data.



The resizing effect of weights on prices is also observed when we use spatial information, that is to say, the above information evidences the *idiosyncratic character of prices*, at an aggregated level and by metropolitan areas, whether it is measured by its mean, its maximum or minimum values, or its dispersion.

Before moving on to the next section, we stress that the equalization of the price level of an economy with the purchasing power of a group of consumers, would imply that the list of biases of the GDP deflator, of the PPI, and the CPI, are similar, or at least we would expect some degree of correspondence between them. But it is not like that because its theoretical senses and practical purposes are distinct, regarding, for example, the outlet substitution bias –and its statistical framework, the Point-of-Purchase Survey– which it may be defined exclusively for the CPI.

The problem of quality changes: theoretical insufficiencies and practical concerns

We borrowed the first part of the title above from Arrow (1958, p. 84). Although the original version has a few paragraphs, the messages are significant. To the extent he acknowledged that the perspective used was theoretical, we want to highlight the following three fundamental insights. The first one is that “at this level of abstraction”, there is no “logical difference” between an improvement in the quality of a product and the introduction of a new one (Arrow, 1958, p. 85). The second one, Arrow (1958, p. 86) underlined that to compile a CPI based on a COLI approach is necessary to assume “an unchanged want structure between one period and the next”. In this sense, no more no less, “We have gone so far as to impute tastes for commodities not available in one time period or the other in order to preserve the theoretical foundation.” The third fundamental insight, Arrow (1958, p. 86) emphatically pointed out that “the only true measure of quality is the satisfaction yielded to the consumer.” In short, even from a COLI perspective, we face theoretical difficulties hard to overcome. Having said all of the above, Arrow warned us all that the compilation of the CPI requires the excellent judgment of its makers.

From the theoretical ground, we would like to add a consideration to those proposed by Arrow (1958). To put it briefly, the analytical breakdown caused by the transfer from an individual-true cost of living index to a social one, that is, the difficulty of jumping from Konüs (1939) to Pollak (1981). Konüs (1939) defined the individual-true cost of living index as the ratio of the minimum costs of achieving a certain reference utility level in a base period, given the prices prevailing at that time, and at a later current period, given whatever changes in prices had occurred in the interval. And Pollak (1981) defined the social-true cost of living index as the ratio of the expenditure required to enable each of the households present in the two periods to attain their reference utility levels in both periods. Here we face a theoretical insufficiency regarding the comparison and aggregation of utility levels, which requires further analysis beyond this paper and may provoke hard-to-answer questions, such as those implied by Deaton (1998, p. 44):

“Not everyone will benefit from advances in heart or cataract surgery, which means that the reduction in the cost of living associated with such advances is different for different people, so that once again it makes no sense to talk about ‘the’ cost-of-living index (Griliches and Cockburn, 1994). It is all very well to suggest that the CPI should move closer to a true cost-of-living index, but it would be wise to discuss whose cost of living we are talking about.”

Similarly, Nordhaus (1996, 1998) made clear that the linking method used by statistical agencies misses the impact of “radical new products” on living standards; in other words, the “tectonic shifts in technologies” are not captured in the official figures. For Nordhaus (1996, p. 30), the task faced by statistical makers is a science-fiction mission to the extent it is necessary to compare “the services of horse with automobile, of Pony Express with facsimile machine, of carbon paper with photocopier, of dark and lonely nights with nights spent watching television, and of brain surgery with magnetic resonance imaging.” From his point of view, statistical agencies might require numerous teams of economists and plenty of time to account for changes in the quality and range of goods and services that we consume. We would add adequate financing because statistical institutions are under-funded all around the world.⁶ Although his suggestion may not be attractive for some professionals, Nordhaus (1998, p. 68) is an enthusiast of this measurement in economics perspective to the extent that, for him, “it is hard to think of a more exciting and worthwhile topic in applied economics.”

Proposals for an upgrade of the monetary policy framework

We listed an inventory regarding the unsuitability of using the CPI growth rate as the monetary policy target. Afriat (2005, p. xxiv) wrote: “It should no longer be plausible, even to convinced ‘price level’ believers, that the whole situation can be effectively summarized by a single number.” Afriat is right –the price level exists only in the imaginary of models builders. Keeping in mind the information requirements to design the monetary policy, the selection of index numbers as measuring instruments –in both senses, as “artifacts” and as “analytical devices”– falls short. Thus, possibly, as a scientific community, we erred in selecting index numbers as measuring instruments. Suffice it to say that it was a decision made a century ago. How quickly time passes!

Albeit we are not able to thoroughly analyze its consequences for the theory and practice of monetary policy and, in general, for the making of macroeconomics, we are in a position to submit the following:

- The use of an index number –that is, of a weighted average–, resizes the available price information. Fisher (1922, p. 291) wrote: “These weights are, of course, merely matters of opinion, but, as is well known, wide differences in systems of weighting make only slight differences in the final averages.” We recommend that central banks ask the statistical authorities to compile the following *additional* indices: a democratic one in which each household’s index is averaged to obtain the CPI (Prais, 1959); a second one base on the median of the expenditure distribution (Pollak, 1998); and ten other price indexes corresponding to the ten deciles of the income distribution (Arrow, 1958).⁷

⁶ Griliches elaborated the following diagnosis a long time ago which, we believe, continues in force until our days (1994, p. 14): “Why are the data not better? I don’t really have good answers to this question, but at least three observations come to mind: (i) The measurement problems are really hard. (ii) Economists have little clout in Washington, especially as far as data-collection activities are concerned. Moreover, the governmental agencies in these areas are balkanized and underfund. (iii) We ourselves do not put enough emphasis on the value of data and data collection in our training of graduate students and in the reward structure of our profession.”

⁷ Here an extract of a pair of resolutions of *The Second International Conference of Labour Statisticians* held in April 1925 (ILO, pp. 69-70): “(1) In each country statistics should be published showing changes in the cost of living... Separate index numbers should be published for each of the groups... (2) A series

All the above with the intention that monetary authorities dispose of better information on the dynamics of the purchasing power of citizens throughout the expenditure distribution. These new measurement exercises should be based on the well-known expenditure surveys and less-known point of purchase surveys by expenditure group levels (Guerrero, 2014, p. 151).

- When conducting their monetary policy, including the determination of the inflation target, central banks have to take into account the measurement biases of the CPI, and not to overlook that measured inflation, and its volatility, are the result of the application of an instrument that numerically resizes the available price information. All the above requires strong efforts by both the statistical agencies and the central banks to research on these topics.
- The objectives of central banks are complex. Noticeably, monetary authorities look out for the inflation of an economy. In this regard, it would be convenient that central banks add, as a monetary policy target, at least one of the other two major price indexes of an economy, the GDP deflator –without losing sight of its drawbacks–, or the PPI –suggested by Bloem, Armknecht, and Zieschang (2002) from the International Monetary Fund. Under this scenario, monetary authorities: 1) would have as a target a broader price index growth rate, with its matching instrument, the interest rate; and 2) would have an intermediate target in terms of the CPI growth rate –which is a subset of the prices of an economy–, with the intention of taking care of the purchasing power of consumers. To the extent that the link between the PPI and CPI growth rates is, firstly, a theoretical matter –for example in terms of its scope–, and secondly, an empirical one, central banks would have to analyze it carefully in order to determine the target –in terms of the PPI growth rate–, and as its byproduct, the intermediate target –in terms of the CPI growth rate. Once again, the PPI growth rate would be the policy target –with the aim of maintaining the current transparency and accountability of central banks– and the CPI growth rate would be merely an intermediate target.⁸

Last but not least, we firmly believe that the feasibility of adopting our suggestions depends on the following prerequisites. 1) Robust coordination among central banks, founded on common points of view. 2) The construction of a joint research agenda between monetary authorities and statistical institutes. And 3) The implementation of a successful educational crusade that allows citizens an adequate understanding of the measurement decisions suggested here.

of national index numbers, calculated by using weights based on the consumption of industrial workers generally, should be published...”.

⁸ According to Lombardozzi and Kelley (2013, p. 2), “PPI data most often are used as a general indicator of inflation, for price trend analysis and forecasting, for contract escalation, and for price comparisons of a company’s products with those of its competitors. According to the user survey results, the value of the average contract escalated by the PPI is \$115 million dollars. This outcome illustrates the importance of the proper use of the PPI as a contract escalator, because a 1-percent error in the escalation of a \$115 million contract would result in more than a \$1-million adjustment.” In its web, the Bureau highlights the following: “BLS estimates that agreements with a lifetime worth in the trillions of dollars are currently adjusted using the Producer Price Index (PPI) family of indexes, either alone or in conjunction with other sources of economic data”; and also makes available the *Escalation Guide for Contracting Parties* (<http://www.bls.gov/ppi/ppiescalation.htm>).

Final thoughts

Morgan (2007) has already shown that it is unsuitable to measure a variable using an equation –in her case, the income velocity of money using the equation of exchange. One obvious problem is the lack of independence between the velocity and the money stock. We want to propose the residuals of the aggregate production function as the emblematic example of the use of an equation as a measuring instrument.

There is an acknowledged continuity between Solow (1956) and Solow (1957). The first one founded the neoclassical theory of economic growth, and the second one constitutes its practical extension (1957, p. 312): “The new wrinkle I want to describe is an elementary way of segregating variations in output per head due to technical change from those due to changes in the availability of capital per head.” With his second effort, Solow met the modern most demanding requirements for the complete exposition of a theory proposed by Boumans (1999, p. 410), that is to say, “a measurement formula” and its “standardization”.

A quick re-reading of the masterly document reveals that Solow (1957) himself was aware of the complexity of his methodological proposal. Among other examples, he mentioned, as theoretical challenges, the doubts about the precise meaning of the “quantity of capital” and of the “technical change”; and the need for a problematic assumption –the factors are paid according to their marginal products. As empirical challenges, the author analyzed, among other issues, the weak correspondence between the theoretical definition of “output” and its empirical counterpart, the GNP; the problem of measure aggregates; the poor quality of data; and the difficulty to approximate a non-observable variable, that is, the idle capacity of the capital stock, and its implausible solution. In his own words (p. 314): “Lacking any reliable year-by-year measure of the utilization of capital I have simply reduced the Goldsmith figures by the fraction of the labor force unemployed in each year, thus assuming that labor and capital always suffer unemployment to the same percentage.”

In the case we are dealing with, the weighted average Laspeyres price index, the problem is not that an equation was used as a measuring instrument but that the equation required a single representation of the prices of an economy. In other words, the equation of exchange played the role of “straitjacket”. Therefore, we believe that it is time for us to recognize that a single measurement of prices, originated in an equation, does not constitute an adequate representation of the real economic phenomena that we have to understand to design the monetary policy.

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Annex

The following table summarizes some recent empirical findings on the biases of the CPI.

Table 1A. Estimates of biases in the CPI-based measure of the cost of living (percentage points per annum)

Sources of bias	United States	Japan	Canada	Mexico
Commodity substitution	0.35	0.10	0.22	0.198
New products + Quality change	0.37	0.70	0.19	0.215
New outlets	0.05	0.10	0.04	0.100
Total	0.77	0.90	0.45	0.513

Source: Lebow and Rudd (2003), Shiratsuka (2006), Sabourin (2012), and Guerrero (2017).

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INTERVIEW:

From the Political Economy of Financial Regulation and Economic Governance to Climate Change: An interview with Andrew P. Baker

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Jamie Morgan [Leeds Beckett University Business School, UK]

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Andrew P. Baker is Professor of Political Economy, University of Sheffield, UK. While his name may not be familiar to those who look outwards from the economics discipline, his work on the politics of economics, in the form of knowledge with influence in the world, is well known in the social sciences – especially for those working in international/global political economy circles. The politics of knowledge is a subject many economists pay insufficient attention to, despite that the standard critiques of mainstream economics both highlight the general failure of the mainstream to adequately conceptualise power and institutions and point to the role of power *in* institutions and organizations as a key facet of the way mainstream economics reproduces itself (its sociology of knowledge).¹ Baker's work focuses in particular on the role of frameworks, theory and expertise in relation to financial processes, governance and regulation – issues such as the meaning, significance and limits placed on systemic risk and the rise of macroprudential approaches in the wake of the global financial crisis.² As such, disciplinary economics is just one of his concerns, which he places in broader context to explore various aspects of financialization. His work resonates with and should be of interest to post Keynesians, institutionalists and anyone with an interest in finance and policy and its many consequences – not least contemporary inequality.

Professor Baker is a former editor of *British Journal of Politics and International Relations* and current co-editor of the journal *New Political Economy*, which along with *Review of International Political Economy* has played a leading role in the renaissance of political economy in the social sciences in the UK in the twenty-first century, as has the Sheffield Political Economy Research Institute (SPERI).³ SPERI, for example, published *The UK's Finance Curse? Costs and Processes* which Baker co-authored with Juan Montecino and Gerald Epstein from the Political Economy Research Institute at University of Massachusetts, Amherst, (Epstein is one of the

¹ For a range of papers on related subjects see: D'Ippoliti, C. (2021); Ylönen et al., (2021); Javdani and Chang (2019); Ioannidis, et al., (2017); Çalışkan and Callon (2009); Callon (2007).

² See, for example, Baker (2018, 2017, 2015, 2013a, 2013b, 2013c); Baker and Widmaier (2015, 2014).

³ Visit: <http://speri.dept.shef.ac.uk> Notable work on financialization was also done at University of Leeds as part of the Financialization, Economy, Society and Sustainable Development (FESSUD) project and at the Centre for Research on Socio-Cultural Change (CRESC) at Manchester University. For example, Erturk et al. (2008). Visit: <http://fessud.eu/the-project/> and <https://www.cresc.ac.uk>

most prominent scholars to work on financialization over the last twenty years).⁴ The report quantifies the lost GDP attributable to financialization in the UK from 1995 to 2015 (approximately £5.2 trillion or about 3 years of GDP).⁵ Baker has been visiting professor at Copenhagen Business School and visiting scholar at the Bank for International Settlements (BIS) (see Baker, 2020). Amongst other projects, he has worked with the well-known MMT proponent and expert activist critic of financialization and tax regimes, Richard Murphy, provided consultancy for the Tax Justice Network (TJN) and, along with Leonard Seabrooke and Eleni Tsingou (both at Copenhagen), has been funded by the Institute for New Economic Thinking (INET) to explore the influence of the Annual Jackson Hole Central Banking Symposium.⁶

Professor Baker graduated BSc Hons Politics and History from the University of Bradford, in 1993, received an MA. in Democratic Theory and Process, University of Leeds, in 1995, before moving on to the University of Ulster, where he was awarded a D.Phil in 2000 for the thesis “The Politics of G7 Co-operation in the 1990s: Global Finance, Macroeconomic Policy and Multi-Dimensional Diplomacy.” This provided the platform for his book *The Group of Seven: Finance Ministries, Central Banks and Global Financial Governance* (Baker, 2006). He has since published more than forty articles in academic journals as well as written for and edited collected essays and contributed various shorter pieces to *The Conversation* and similar outlets.⁷ Most recently he co-authored the e-book *Making Tax Work: A framework for enhancing tax transparency* (Murphy and Baker, 2021).

His work can be accessed at:

<https://www.sheffield.ac.uk/politics/people/academic-staff/andrew-baker>

He is interviewed by Jamie Morgan for *RWER*...

Jamie: Let’s start by creating some context and setting out some themes or foci for discussion – while introducing some terminology which we can explain and elaborate as we proceed and as we explore how some of your work fits into these themes and foci. Historically, there have been quite different ways to frame, theorise and explain the activity of banking and finance and the differences make a difference to how regulation is formulated and what the role of a regulator is deemed to be. This is an incredibly important issue when we consider the significance of banking and finance for economies and the grave consequences that can result from their activities. The twentieth century witnessed a transition from a more structural-

⁴ Visit: <https://peri.umass.edu/economists/gerald-epstein> See Epstein (2005).

⁵ See Baker, Epstein and Montecino (2018) and note that while undertaken through SPERI the report was an initiative of several universities. See also Christensen et al. (2016); Baker and Underhill (2015).

⁶ See, for example, Baker and Murphy (2021, 2020, 2019).

⁷ For example, Baker (2020): Bank for International Settlements podcast “Tower of Contrarian Thinking.” <https://bispodcast.libsyn.com/andrew-baker-contrarian-thinking-at-the-bis> and

Baker (2013), <http://theconversation.com/powerful-and-plural-q20-is-worth-listening-to-17903> and Baker et al. (2005).

interventionist conceptualisation of regulation (epitomised by the host of legislative changes to banking and finance that occurred in the US from the 1930s onwards) to one with more of a focus on enabling market processes as sources of efficiency, which gained momentum in the 1970s (see Harnay and Scialom, 2016). Readers will likely be most familiar with the repeal of Glass-Steagall (repealed in the US by the Gramm-Leach-Bliley Act 1999) as one important event commonly identified as a contributing factor (for the range see Lo, 2012) on the road to the Global Financial Crisis (GFC). As readers are likely also aware, the GFC has led to various reform initiatives and discussion of issue-areas over the last ten years or so:

1. There has been more focus on systemic causes of financial problems, and this has resulted in “macroprudential” policy initiatives, albeit the concept predates the GFC.
2. There has been development of “resilience” mechanisms, notably improvements to bank capital levels and leverage ratio levels, drawing on or complying with changes to the “Basel Committee” rules (Basel III (IV)).
3. There has been development of “resolution” strategies i.e. what to do with a failing bank.
4. There has been particular attention paid to “too big to fail” banks (TBTF) and more generally to global “systemically important financial institutions” (G-SIFIs), and these are the subject of greater scrutiny and are held to different standards.
5. There has been growing attention paid to researching and understanding the more complex functions and interconnections of contemporary banking within the “universal bank” model (which combines retail and investment banking).
6. In conjunction with #5 there has been growing attention paid to the evolving nature of the finance system and its parts. For example, how “repos” are used, the functioning and efficacy of derivatives, the growing role of “shadow banking” entities etc.

Overall, there has been growing acknowledgement of the need for a more broad-based understanding of the finance system and its vulnerabilities, both globally and nationally. In this regard the Financial Stability Board (FSB) and many other organisations, such as the Bank for International Settlements (BIS) and Basel Committee, provide analysis and advice on international standards and these are also adopted and modified by different countries. Arguably, though, there has been more rhetoric than actual fundamental reform (even if there has been a lot of regulatory change of one kind or another), though this, of course, depends on what one expects from a regulatory system and this, in turn, depends on how one frames and theorises banking and finance. One strand of your work has been on systemic risk and perhaps we might begin by discussing how this term is used and how you have approached the subject.

Andrew: I would define systemic risk as the prospect of a systemic synchronised downward movement in asset prices, causing a contraction in financial activity and wealth, that in turn leads to a sustained recession and reduced activity in the wider economy, caused by excessively optimistic assessments becoming highly pessimistic sentiments in a system wide reappraisal. The key thing is that systemic risk refers to a collective systemic wide outcome, produced by collective interactions and connections in a market. This in turn leads to arguments that blame for systemic risk can never be attributed to any one single actor, or group of actors, but is a collective property and dynamic, that results from complex systems being more than simply the sum of their parts. So just because something might be true of any given unit in a system, it won't mean it is necessarily true of the system as whole, or that you can understand

system dynamics or outcomes, simply by aggregating the actions and decisions of units within a system.

Jamie: And just to be clear systemic risk inheres in a system and its properties and these are not reducible to the individual entities who are members of a system but rather inhere in the organization of that system. So, one cannot look merely to the individual balance sheets of banks and other financial actors but rather the combination, their relations and collective tendencies and possible vulnerabilities on this basis. Perhaps you might illustrate...

Andrew: Yes. One way to think about this is that systemic risk is comprised of two related dimensions: a time dimension, where individual perceptions can change from one point in time to another, so prospects come to be assessed collectively in a much less rosy fashion, than they were at an earlier point in time, and of course collective behaviours can lead to manias and panics. A second dimension is a cross sectional one, which accounts for the interactions between units in a system. As these become more numerous and complex, systems can become more risky and there is more potential for destabilizing synchronised movements in the system as a whole. In this regard, it is under appreciated that systemic risk is a phenomenon that can take hold and impact beyond banks and the financial sector.

What is sometimes call financialization reaches beyond the conventional financial sector. In our recent work at Sheffield led by Adam Leaver, we think that a potential big systemic risk is currently posed by the large capital (non-financial) sector. That research found that around 20% of the world's leading companies have regularly been paying out over 100% of their annual income in the form of dividends and management share buybacks, over the last decade, – creating what we term “hollow firms” (Baker et al., 2020). This has been made possible by companies taking on increasing amounts of corporate debt, an increasing share of which is low grade, and by manipulating time through accounting techniques that push back costs and liabilities and pull forward optimistic revenue projections. That makes a significant chunk of the modern corporate world very vulnerable to economic shutdowns.

Jamie: And this process of hollowing out firms (arguably a kind of legalised looting through changing both the capital structure and relationship between current reporting and uncertain future outcomes) requires an accommodating regulatory environment, the cooperation of consultancy and accountancy firms, governance-influencing non-executive directors on corporate Boards etc. – and perhaps most notoriously recently, the case of Carillion in the UK, which used strategies like “negative accrual” and, over the years, various manipulations of its Private Finance Initiative contracts. And the form this vulnerability to economic shutdowns takes is?

Andrew: The form this vulnerability potentially takes in the large cap sector, is something accountants call an “impairment shock”, a dramatic reappraisal of valuations, when an optimistic projected future fails to materialise due to reduced economic activity. That can lead to all kinds of procyclical dynamics and downward asset price spirals, that are incredibly difficult to predict in advance and are highly contingent, so may not fully materialise. The broad point is you don't want to be in a position where such a large number of the world's major companies are vulnerable to those kinds of processes, in a world characterised by pandemics and climate emergencies, which will reduce economic activity temporarily, for periods of time.

In the research, we argued for regulation to stop the practice of paying out more than annual income (a “dividend cover limit”), in any given year and a raft of other measures to make leading

companies more resilient. This is not directly related to financial regulation, but it does illustrate that systemic risk is a broader phenomenon than just developments in the formal financial sector.

Jamie: Still, a higher profile concern with “systemic risk” and at least some attempt to address it came to the fore in and after the Global Financial Crisis (GFC), and focuses primarily on the financial system generally and banks in particular. You have had a lot to say about this over the last decade or so. How did you become interested in this subject?

Andrew: I became interested in systemic risk because I am a political economist, concerned with how ideas and images of markets and finance in particular are used to construct processes of macroeconomic and financial governance by leading governments and through their interactions with major market players. As you suggest in your introduction, two years before the 2008 crisis I wrote a book on the G7 as an apex policy forum, that looked at what contribution the forum had been making to global financial governance over the prior decade.

A lot of the book detailed how the G7 had created and authored a very specific approach to understanding and dealing with financial crises. There were several beliefs present in the prevailing G7 approach/consensus. The main cause of financial crisis was believed to be a lack of transparency and information, primarily caused by governments concealing information about their intentions in macroeconomic policy and the true state of the economy.

So straight away this dominant approach said most financial crises are caused by governments, either intervening too much, or concealing data. There was a general acceptance that markets accurately priced risks and were broadly efficient most of the time. Distortionary interventions and forces outside of markets – exogenous factors and shocks – were seen to be the primary cause of crises. Endogenous causes of financial instability (those internal to markets) were barely considered at all. Looking back that is an incredible form of myopia, and I was essentially arguing that at the time. The emphasis was on enhanced transparency, public authorities making more data available, so markets could make better and more informed decisions, including where needed, disciplining and calling for adjustments in national Macro policies. This was a very constraining and limiting set of beliefs that left a lot off the table, but also immediately put the onus onto any government affected by a financial crisis to adjust and be more “transparent”. It meant there was relatively little attention on the functioning of markets themselves and the instability, market decisions and interactions could produce.

Jamie: Yes and for anyone working in or with an interest in economics this evokes a whole set of broader concerns regarding how economics conceives an economy and how it makes sense of the role of money, monetary policy, financial stability and a whole concatenation of related concepts and commitments regarding information, efficiency, rationality, self-interest etc. all of which feeds into policy via the education of economists – albeit there has always been a gap between the practice of policy and the contents of textbooks (see Dow, 2017). Perhaps we might come back to this when we discuss your take on Keynes, Minsky, Fisher and others (whose work takes us some distance from a standard mainstream textbook framework). In any case, a “money economy” in the heterodox sense is quite different than the quasi-barter economy of neutral money or money as a “veil”. How did your research proceed?

Andrew: Shortly after the financial crisis of 2008, I found myself being invited to a series of brainstorming/policy advice sessions in elite transatlantic circles: The Atlantic Council, Chatham House etc. These events were convened to discuss the issues of the moment and

reform trajectories, and invitees included national policymakers from central banks and finance ministries, some international organization people, public intellectuals from a range of disciplines, some of the big investment houses, journalists from big national and global titles. What stood out was the actual paucity of ideas, but I have a powerful recollection of John Eatwell (Professor of Financial Policy, Judge Business School, Cambridge – now Emeritus) at one event forcefully arguing that the old regulatory consensus had been a complete failure and had rested on a failed trinity of increased transparency, better risk management and enhanced disclosure.

Jamie: And that trinity rested on – “government failure” notwithstanding – the idea that the primary problem in markets (and thus for policy approaches to markets) was information asymmetries, and that better information, combined with individual rational actor self-interest, would correct “market failure” and return banking and finance to some kind of “normal”; implying a background theoretical framework in which “normal” for financial agents is as disciplined intermediaries, providing capital in and for efficient processes, subject to those information asymmetries? A quite different perspective than one that orients on structure and institutions where financial agents are powerful actors and that begins from the premise that those actors are opportunistic financiers able to, and interested in, influencing their own institutional-organizational environment (which, of course, in turn operates as an emergent system).

Andrew: Yes, though I am deliberately making the contrast a little starker than it probably was to illustrate the point; but there was an ensconced mind-set that filtered out and rejected alternative ways of thinking. If you look at Bank of England speeches on financial stability in the early years shortly after they were granted operational independence in 1997, you will find the odd nod at the need to keep an eye on the overall systemic risk profile, as a collective concern that went beyond any single institution, but these were quite vague and, of course, most national authorities had little in the way of mandates, or instruments to take action. There was the odd exception like the system of dynamic provisioning in Spain. But the other strong pattern in the Bank’s speeches in 97-98 was of politics being an irrational process that upset otherwise efficient rational markets. This is evident in how monetary union was talked about, as something driven by politics, that risked upsetting and destabilising market operations and in relation to the Asian financial crisis, where government relations with corporate entities cultivated for political reasons, were seen to cause the market instabilities in that region. Crises as something caused by exogenous irrationalities were a very clear strong image in how the Bank talked about financial crises that was not untypical of the time.

There were those who were broadly part of central banking as a field who were trying to challenge that and change thinking. A good example is that in 2003, two Bank for International Settlements (BIS) economists, William White and Claudio Borio, presented their thinking at the Jackson Hole conference organised by the Kansas City Fed.⁸ They argued that the big future threat was less about inflation, but rather relative price stability *plus* financial liberalisation/globalisation, combining to produce longer run financial and credit cycles – making financial instability the policy challenge of the future. They suggested that markets had an inherent procyclicality, and underestimated risks in the upswing of a cycle and overestimated risks in the downswing. The reaction to the paper from the US delegates present was hostile (interestingly Japanese and European attendees were more positive). But crucially the

⁸ Note from Jamie: see https://www.bis.org/author/claudio_borio.htm and

<https://www.bis.org/about/biowrw.htm>. Note from Jamie: see <https://www.bis.org/publ/work147.pdf>

American participants did not engage with the central concept in the paper – market procyclicality. Instead they talked about monetary policy. This pattern is really stark if you look at the full transcripts on the Kansas City Fed site. It is as if the BIS staff are speaking a different language to the US economists, who neither hear, nor comprehend the central message coming from the BIS.

Jamie: Though as the general hostility indicates that was because there was a pre-crisis consensus...

Andrew: That's right, as my book on the G7 had identified, the emphasis was on non-market sources of financial distress and greater transparency to aid market decision making; so I instantly recognised what John Eatwell was saying later about the failure of the regulatory consensus. And this was the first time I had heard someone publicly confront people from the Treasury and the Bank of England in this direct way. Eatwell stood out because he was publicly stepping outside the dominant approach and saying regulators must base their approach on the system as a whole, based on a macro risk assessment of the system as a whole.

As we noted earlier, this brings to the fore something mainly neglected in the previous years – a system has properties and these are not reducible to those of its individual participants, so that requires tools that are adjusted based on a system-wide reading of emerging risks, rather than those profiled by individual institutions. As I have written elsewhere:

“Macroprudential policy involves pre-emptive interventions to minimise the threat of financial instability and moderate cyclical risk-taking across financial systems as a whole. It promised a greater role for public authorities in overseeing and framing private decision-making, after two decades of light-touch oversight based on faith in private risk management techniques.” (Baker, 2018: 294)

This has involved development of “stress tests” and a “toolkit”, for example (see also Baker, 2013: 117):⁹

- Leverage limits/ratios;
- Time-varying capital requirements (e.g. the countercyclical capital buffer in Basel III);
- Loan-to-value requirements (LTV) and
- Loan-to-income requirements (LTI) on mortgages;
- Margins and haircuts;
- Management of foreign exchange liabilities as well as the possible levying of “macroprudential” taxes.

Jamie: And you argue that there have been various constraints and tensions in the development of financial stability policy in general and macroprudential policy in particular, much of which is indicative of insufficient attention to “macroprudential ontology” and also to “social purpose”, but we can come back to that. Your initial point was that there was growing

⁹ Note from Jamie: for an introduction and explanation of the mechanics of stress testing see Dent and Westwood (2016). The approach is also modified year to year; for latest see: Bank of England (2021).

focus on the need for a macroprudential response to systemic risk and Eatwell was “stepping outside the dominant approach”.

Andrew: Eatwell, was also calling for countercyclical provisioning by institutions, raising reserves in good times, so they could be used in bad times. These were different from conventional capital requirements which were effectively a charge to operate in the market, and which could not be used as a “buffer”. They were to be set in relation to patterns in the economy as a whole. Regulation he said also had to target highly leveraged institutions. In this diagnosis the problem was excessive complexity, not a lack of transparency. This might not sound like very much, but in 2009 it was a very different mindset and a call for a much more activist approach to regulation that would have been summarily dismissed and rejected a few years earlier in settings such as Jackson Hole and completely ignored by the G7. At the time, I didn’t think these kinds of ideas had much chance of getting onto the G20 agenda, but I remember the language of “macroprudential” and the need to counter the procyclicality of markets did feature in the “Horsham communique” issued by finance ministers and central bank governors, a few weeks before the London summit in 2009.¹⁰ At the time, it felt like a significant sea change moment. In reality, I think it has been less significant than I thought it might be at the time.

Jamie: But this set in train an interest in why some ideas resonate and others do not?

Andrew: That’s right, I got very interested in where these ideas and arguments came from. John Eatwell was doing a very good job of articulating them in that particular setting, but as I looked into it, I realised they had a richer and deeper heritage. I mentioned Borio and White earlier, but these ideas had been part of the Bank for International Settlements work programme for some time and had been in circulation from the end of the 1970s, but the BIS intensified its work post East Asian Financial Crisis (1997), and following two public speeches from its General Manager, Andrew Crockett. London School of Economics (LSE) financial markets group also had a go at promoting similar ideas and wrote an academic’s response to “Basel II”, saying it risked making market procyclicality worse.

Jamie: And just to be clear, the Basel rules refers to sets of recommended capital adequacy requirements and associated mechanisms and measures intended to maintain the “resilience” of bank balance sheets. These are developed in cycles by the Basel Committee on Banking Supervision (BCBS), which is hosted at the Bank for International Settlements (BIS) and the BIS has numerous functions, including, as essentially, the central bank to central banks. The rules in each cycle develop over time through consultation, but have no formal legal power, though they are widely adopted and adapted. Basel I was agreed 1988 and became active 1992, preparation for Basel II began in the late 1990s and was formalised 2004, but was still not fully implemented at the time of the GFC and Basel III was initiated 2010 but delays continue (with a current focus on 2023). So, both BIS economists and the LSE group raised concern during Basel II? Perhaps you might just briefly explain what procyclicality is and so why countercyclicality is important in a regulatory sense.

Andrew: That’s broadly right – the BIS employs many research economists that act as a source of advice and those economists have more intellectual independence than most people appreciate – so there was internal concern at the BIS, and the LSE group were saying similar

¹⁰ Note from Jamie: see point 6 bullet point 2 of the communique:

<http://www.g20.utoronto.ca/2009/2009communique0314.pdf>

things at this time. The pro- and countercyclical point they were making was that Basel II would require institutions to raise more capital at times of markets distress, that could in turn make market-induced credit contractions worse. The countercyclical argument was, get institutions to hold *more* capital when asset prices are rising, when risks appear to be low to most market participants, but are in fact increasing as balance sheets become extended in good times.

Jamie: Which if mandatory could slow or curb any transition to levels of risk that might threaten financial stability and if discretionary (in response to the regulator identifying specific sources of systemic risk) could also act as a signal to the relevant financial actors, while providing greater loss-absorbing capacity?

Andrew: If we refer to your original list of issue-areas that have come to the fore since 2008, these combine macroprudential and resilience measures with a view to highlighting the more complex interconnections of banks – albeit there is a lot more to say about that. But the point is there were voices making critical arguments and questioning the direction in which regulation under Basel II was headed.

The LSE group included Charles Goodhart, Hyun Shin, Markus Brunnermeier. (Readers of *RWER* are likely aware of who Charles Goodhart is – given “Goodhart’s Law” is named after him and given he is a former member of the Bank of England Monetary Policy Committee (MPC), which sets the Bank Rate in the UK, intended to put a floor under commercial interest rates).¹¹ The LSE group later went on to write an important report in 2009 – “The Geneva Report” (see Brunnemeier et al., 2009). The Geneva Report laid out a macroprudential approach in more detail. Many people conflate the BCBS as the national regulators, with the BIS, but the politics were more complex than this. When the BCBS was preparing Basel II, the General Manager of the BIS at the time (who I have already mentioned), Andrew Crockett, wrote to the chair of the Committee, William McDonagh of the New York Fed in 1999, and said he feared the proposals were procyclical and risked making downturns and balance sheet distress worse. Crockett called for Basel II to build a new system of countercyclical provisioning to counter such systemic risks of system wide downward spirals in asset prices.

The Basel Committee wrote back rejecting the suggestion that their proposals were procyclical and claimed the proposed countercyclical approach would encourage “moral hazard” by providing a cushion in bad times.¹² I was fortunate enough to be invited into the BIS archives and shown these letters, and they really are an insight into the degree of intellectual dissent that was present at the time Basel II was being prepared. The BIS decided to do more analytical work to bolster the arguments in Crockett’s letter, and Crockett effectively gave two public speeches on a macroprudential system focused approach, which were intended to give his staff cover, to enable them to continue to develop the work. The Basel Committee and its US chair

¹¹ Note from Jamie: a policy approach that draws a lot of criticism from structural and post Keynesian economists regarding its roots in a natural rate of interest and natural rate of unemployment, modelled using a new-classical-new-Keynesian synthesis and tending to use a price stability target that cuts off the possibility of (genuine) full employment, while making further use of dubious mechanisms, such as the Taylor Rule in its structural equations.

¹² Note from Jamie: as most people know, moral hazard argument was commonplace, and for example, later led to numerous delays in the run up to the GFC and just after under Mervyn King at the Bank of England.

were not happy at what the BIS were doing, and even some BIS staff were uneasy at taking a divergent stance to its main stakeholders.

Jamie: Though, as you say, they persisted, and you've already mentioned Claudio Borio and William White in this regard. Borio has been at BIS since 1987 and William White joined in 1994. Both have written extensively on financial instability and preventative policy and William White has written for *RWER* in the past.¹³ The joint paper given at Jackson Hole in 2003 is another in a long line of works that highlight that there was in fact concern (if not simple prediction) prior to the events of September 2008 – Steve Keen, Nouriel Roubini and numerous others.¹⁴

Andrew: It was genuinely hard to make those type of arguments publicly in the early 2000s, because it meant challenging the consensus and moving against the status quo. The Jackson Hole example is a good one. The US critics of Borio and White rounded on their suggestion that monetary policy should become part of the armoury for stemming the financial cycle, even when there was little in the way of an inflationary outlook, which was only small part of their argument (but easy to hone in on). But crucially all these critical voices completely ignored the main conceptual point Borio and White were making, which was about an inherent financial instability caused by market procyclicality. Nobody responding to the paper even mentioned procyclicality. The concept got completely overlooked – the respondents didn't even acknowledge financial instability, they started talking about monetary policy! So while these arguments about the propensity of financial markets to produce widespread instability were not entirely absent in elite policy circles at the time, they went unheard, or were ignored, especially by most US based policy economists.

Actually, in that same year 2003, senior people at the Bank of England told me they began thinking more in terms of systemic risk and began to more actively research the properties of non-equilibrium scenarios. In large part this was because they had hired Hyun Shin from the LSE as a consultant and he and the LSE financial markets group, were doing this kind of work and speaking to Borio and White at the BIS, as well as raising their concerns about the procyclicality of Basel II. But at the Bank, they kept this work in-house in the financial stability division because they felt there was no audience for it in other parts of the Bank, in the political arena, and of course they knew that was little traction for this kind of thinking in the United States.

Jamie: So, what you are suggesting is that there was considerably more discussion than one might imagine, if one's only insight came from a standard economics textbook or one was of the opinion that ideas of efficiency and rational actors etc. reigned supreme (without *any* reservations)? The politics of financial regulation reveal a situation where there was opportunity to recognise underlying problems of the kind that would ultimately facilitate the global financial crisis, even if the scale and form of that crisis were not necessarily anticipated? The system could have been more resilient prior to the GFC had things lined up differently? As I understand it your work highlights several conceptual components that were already in discussion prior to the GFC.

¹³ Note from Jamie: see <http://www.paecon.net/PAERReview/issue63/White63.pdf>

¹⁴ Note from Jamie: for frequency of financial and banking crises see Laeven and Valencia (2012). For some context and discussion of financial stability issues see Keen (2017, 2016).

Andrew: That's right, the argument and thinking were in circulation, so when the crisis struck that body of work was ready to go, but the wider institutional, intellectual and political environment had not been particularly receptive, prior to the crisis. The dominant position was that if markets are rising and inflation is low, credit is helping the economy, and crises are something that mainly take place in the developing world and emerging markets.

Jamie: And if they do occur in core financial centres, such as the crisis induced by the collapse of the hedge fund Long-Term Capital Management in 1998 (due to failure, in the wake of the East Asian and Russian crises, of its heavily leveraged strategy for arbitrage trading of fixed income securities) then the Fed can call on market participants to organise a bail-out allowing markets to return to their putative role of efficient and disciplined allocators of capital ("credit-rationers") without any need for a broader macroprudential perspective of the kind BIS and the Geneva Group were starting to suggest was advisable.¹⁵ But your point is that this work has some fundamental conceptual planks at odds with the consensus of the time.

Andrew: Yes, I think the dominant view was when these things occurred in core financial centres, it was the result of bad individual risk decisions, rather than being due to the inherent instability of market processes, or collective behaviours and incentives, and even things like the Asian contagion owed more to the politics of corporate structures and the role of government in that part of the world. For as long as you are viewing market activity, behaviours and dynamics in largely benign terms, which assumes rationality and efficiency, and with instability primarily caused by exogenous factors outside of the market, why do you need to think in terms of systemic malfunction and systemic risk? Even when that prospect was raised, as by Borio and White, it got ignored and people started talking about something else – monetary policy. Now whatever we think post crisis regulation has gone on to achieve, that earlier mode of thinking was challenged following the crisis. My characterisation is that the work on macroprudentialism from BIS and the Geneva Group rested on four main conceptual planks:

1. A recognition of "fallacies of composition" that arise from thinking solely in terms of individual actors, rather than systems as whole. Even if something is true for each individual unit, it does not mean it will be true for the system as a whole. In other words, the system, and with it systemic risk, are more than the sum of their parts. What is good for each individual actor, and make sense for that actor's behaviour, can create a dynamic that is bad for the system and for society as whole. That insight, both the BIS and the Geneva group claimed, was something regulators had lost sight of.
2. The inherent "procyclicality" of market risk taking, which could go to extremes in both parts of the cycle – the optimism, pessimism cycle, which can change quite suddenly as time progresses – a time dimension of risk. As I have already suggested, this can result in herding and other amplification mechanisms, making both upward and downward spirals in prices much worse, not least because price sensitive risk management systems can push upward and downward cycles to extremes.
3. A more "endogenous" understanding of financial instability that views it as a market balance sheet problem, as much as something caused by exogenous shocks and looks closely at the asset side of the balance sheet in upswings, as well as the liability side, because it is the expansion in the asset side that sows risks in good times (bearing mind a bank's assets are mainly the loans it extends). The BIS refer to the "paradox of financial

¹⁵ Note from Jamie: see Lowenstein (2002); Edwards (1999).

instability”, namely that risks are actually highest from a systemic perspective, when they appear lowest to many participants, as this is when the expansions in debt, credit and investment take place, and when systemic risks are being sewn.

4. An acknowledgement that complex financial products and debt instruments create novel amplification mechanisms and interconnectivity of the financial structure, that can induce and exacerbate financial instability further – the cross-sectional dimension.

I think all four of these run through the work of the BIS and the Geneva Group and they were talking to one another throughout the 2000s in developing their ideas.

Jamie: “Developing their ideas” is an interesting phrase here, since it does not necessarily imply original thinking, but rather also receptivity to thinking that has fallen out of favour or has never been in favour. For me, your work and various others raise extremely important issues that decompose into two parts:

- The nature of regulation depends on how the finance system and banking within that system are conceptualized or theorised – since we regulate based on how we understand the system, its goals and weaknesses (subject, of course, to the real-world influence of the powerful).
- The efficacy of regulation depends on how adequate or successful we are in theorising the system. Key questions are, have we understood the system’s mechanisms and weaknesses, and have we regulated to address them?

Put another way, what there is to regulate depends on how we view the finance system, and this evolves in complex ways as time passes and little of this reduces to simply “the decisive evidence” or “the best theory”.

Andrew: Yes, that’s exactly right. Regulation will depend on the images you have of the system and your understanding of its constituent processes. What’s more, dominant thinking will begin to shape what financial systems look like and how they function (to some degree).

Jamie: More specifically though, you highlight the antecedents and inspirations of the “new” macroprudentialism and also its limits, but let’s start with the former – since most readers are likely to immediately recognise that these four conceptual planks are not new.¹⁶

Andrew: What struck me as remarkable is the extent to which two such notionally conservative, “establishment” groups were clearly influenced in their thinking by Hyman Minsky. I don’t mean actively citing him, though they did do that a bit, but that their arguments were essentially a Minskyan view of the world, at least in terms of a diagnosis of financial instability. Right at the beginning of our discussion you laid out the main areas in which financial reform has occurred and suggested the reality of reform has been less than the rhetoric. The problem and the relatively conservative trajectory that you refer to, I think is because although the diagnosis from the BIS is similar and in its own way quite radical, given the dominant mindsets at the time, the prescriptions and way of thinking arising from that, have been rather timid.

¹⁶ Note from Jamie: for a range of works on macroprudential policy and systemic risk that illustrate the foci and its limits see Kranke and Yarrow (2019); Saporta (2018); Lombardi and Moschella (2017); Casey (2015) Tarullo (2013); G30 (2010); Morgan (2022).

Jamie: You do more than suggest it is timid though and this bring us to limits and to matters of framing. In your 2018 paper you argue that the four conceptual planks have roots in not just Minsky, but also Fisher, Keynes and Kindleberger and these are suggestive of a “macroprudential ontology” and I made reference to this earlier. This seems an appropriate point for you to elaborate on what you mean by that term.

Andrew: Well, in BIS accounts the roots are especially in Minsky I think – BIS accounts of balance sheet cyclical extension posing systemic threats and being the primary source of financial instability are by my reading strikingly similar to the arguments Minsky was making two or three decades earlier. Note that Minsky went largely unheard too, rather like the BIS economists. The key difference, however, is that virtually nobody reads or takes seriously the second half of Minsky’s book *Stabilizing an Unstable Economy* (Minsky, 1986; and 2008, new edition). This is where his prescriptions are (notably Part Five on policy), and I think this part of his work is not read enough. That’s a pity because they contain some important arguments about financial governance.

Jamie: Ontology typically refers to “theory of being” or (using the Kantian question form) “what must be the case” for given observable phenomena to be as they are, though the claim should not be confused with the assertion that reality is either definitively encapsulated by a given “ontology” or that any such ontology is of a fixed world – scientific method entails an ontology even though it is built around fallible inquiry as the basis of progress, and social construction is still an “ontology”. But your point seems to be that there are tensions between the inherent features of the underlying ontology of macroprudential policy (what its concepts imply about financial systems) and the subsequent development of that policy...

Andrew: So by ontology I was referring to the account of the properties and processes that characterise modern financial systems and markets – procyclicality and leverage cycles, with systemically destabilizing consequences. The BIS view which has travelled around the central banking world to some extent, acknowledges the above as inherent features of financial markets, as things shaped by collective social interactions and expectations, meaning it is the collective systemic outcomes that matter most. Since the crisis we now effectively have a new field of financial systemic risk governance, but it has largely been handed off to technocrats and central banks. That is understandable because it is hugely complex, it requires some very technical calculations and a lot of technical knowledge. But this process I would suggest comes with a conspicuous downside. It has resulted in, fed into and been accompanied by a form of political abdication on how we should be seeking to manage systemic risk, given the costs it imposes on society as a whole, particularly low-income groups, who’s contribution to the build-up of systemic risk is often minimal. In short, technocrats were effectively left to pick their own objectives for systemic risk management, and that was at least in part due to a lack of political leadership, insight, or understanding, as well as a search for neat convenient solutions and delegations.

Jamie: And what has that meant in practical terms?

Andrew: For any central banker, their first instinct is to protect the status of the institution and its vaunted independence. We’ve just written a paper on how the Bank of England has been performing its financial stability role post-crisis, and it is a very clear pattern. We analysed 900 financial stability speeches over twenty years. I can’t cite it directly here, because it is about to enter peer review. Since the crisis, Bank staff repeatedly emphasise, when speaking on

financial stability, that the most important consideration is to maintain and observe the Bank's independence, and respect the constraints associated with that. One of the consequences of that is that central banks confine themselves to analyses of market risk and their role in enhancing market resilience or facilitating smooth market adjustments. System wide resilience has become the *de facto* objective of systemic risk governance, but the question of the form the system can take, or the role of regulators in changing the system, can never be broached.

Jamie: And this too has context? In response to the GFC, the Financial Services Act 2012 and various other pieces of legislation were enacted in the UK to establish a new division of labour for financial services and create an organizational focus and set of institutions and mechanism to identify and manage financial instability and prevent crises – a macroprudential focus. The Financial Policy Committee (FPC) was created at the Bank of England. However, its powers are circumscribed and its use of those powers questionable. The FPC can issue “communiqués” warning financial entities of areas of rising risk and it can issue “directives” which are implemented via the Prudential Regulation Authority, which has micro-prudential responsibility. So far the FPC's activity has been low key and it seems extremely reluctant to use directives. Moreover, its existence does not seem to address a basic tension in contemporary political economy – in making decisions the FPC is required to take into account government policy for economic growth and a (non-voting) Treasury representative sits on the FPC; but in a financialized debt-dependent economy, this seems to create a conflict of interest between allowing financial innovations and looser credit conditions and curbing financialized growth (with the inevitable pushback this entails if no crisis occurs – given that the point is to prevent crisis and absence can be either a signal of success or an indication of undue intervention). Independence is not necessarily neutral, in this sense, and, in any case, the personnel of the FPC are drawn from banking and financial economics.¹⁷ They have a perspective (for a related problem of “revolving doors” see Seabrooke and Tsingou, 2021)...

Andrew: A really good example of what I mean comes in Mark Carney's famous 2015 speech on climate change (Carney, 2015). Carney was pretty much the first central banker to speak on climate change and he was roundly criticised by the *Daily Telegraph* and the *Times*, for even daring to mention it as a central banker. But the speech was not making a case for activist central banks. It actually explains that an independent central bank cannot do much about climate change and it is not the job of the central bank to green the economy. Its job is to encourage and prompt forward looking rational agents in markets to adjust their investment strategies, in a measured incremental fashion, that avoids big destabilising disinvestments by making better information available on risks. Carney goes on to say you can't use capital requirements to shape the economy, or incentivise activity and that sort of thing, should not be done, or attempted. I was actually struck by reading that speech of the similarity of the kinds of argument being made and the thinking on display, in Gary Becker's arguments on market rationality, where the only barrier to rational optimum efficient allocations is information, – so

¹⁷ Note from Jamie: see also Hartwell (2019); Howells (2013); Downward and Mearman (2008). For a useful set of learning/teaching aids, hosted at Manchester Metropolitan University, setting out aspects of the Bank and other institutions see:

<https://www.mmu.ac.uk/research/research-centres/future-economies/projects/teaching-economic-policy-institutions>.

See also:

<https://www.mmu.ac.uk/sites/default/files/2021-11/Understanding%20the%20Bank%20of%20England.pdf>

that everything can be solved by markets, if you just give the participants access to the right information. It's a remarkably optimistic reading of how markets are going to solve the biggest existential crisis facing the human race and it seems to me to draw directly on the kind of thinking that Becker would advocate. Whether that is the product of training that produces a kind of reflex, or instinctive thinking I don't know, but the central banker daring to speak about climate change offers this incredibly optimistic, conservative and dare I say it complacent reading, that is more concerned with protecting and preserving central bank independence than it is in setting out a systematic policy response to the threat of climate change.

In any case, it is perhaps not surprising that central banks have defaulted towards a narrow, technical objective for macroprudential policy, of enhancing financial system resilience, by adjusting instruments that reduce the prospect of an institution being hurt by a system wide downturn. None of that tells us much about the system as a whole, or how it could be designed, or for what purpose. Central banks understandably don't want to move on to that territory at all, but the result is the questions never even get asked, essentially because institutional arrangements do not allow them to.

Jamie: Given the context... But this aspect of our discussion does raise a broader issue of limits imposed by the lack of attention to, as you put it in your work, "social purpose".

Andrew: Social purpose in my reading is a question of systemic vision. In this case what do you want the financial system to do and how are you going to design it and seek to govern it, based on that? My point has been that accounts of financial systems, such as that sketched by the BIS and the Geneva Group, do provide accounts about the inherently destabilising nature of much financial activity and that should provide a foundation for sketching a systemic vision. The issue is that if you are essentially engaged in those kinds of technical analyses, you don't want to make the leap to that next stage and translate that into a systemic vision. Professionally, there are disincentives to doing that kind of thing if you are a "serious economist". By comparison Keynes and later Minsky were much more willing to make that move. As a political economist I find that interesting, and I attribute it mainly to modern professional norms and identities that have evolved and been created over many years. I would argue it's become an inhibiting force that is detrimental to society as a whole, as is the other side of this equation, which is politicians' reluctance to pick up technical arguments relating to finance and run with them to articulate a systemic vision. They have abdicated on these questions. Leave it to the experts.

The problem with all of this is once you start thinking in terms of systemic risk you are moving into deeply normative territory around questions of justice, because those who have not contributed to risk, often suffer some of the most severe consequences of the materialisation of systemic risk, including threats to life chances, that can become chronic. Then there is the question not just of how much systemic risk a society is prepared to tolerate and how it should be distributed, but also what the system as whole should look like and what its purpose is (social purpose). If we have moved to governance of the financial system as a whole since the financial crisis, we have left these deeply normative questions largely unspoken. There is now an attempt to govern systemic risk, but most of the world has abstained on what kind of system you are trying to produce through this new systemic governance. How can you govern systemic risk, if you are not prepared to state what kind of system you are trying to produce and what the purpose of the financial system is? This is a huge vacuum and its sources as I indicate above are complex mix of politics and professional cultures and incentives.

Jamie: This is a fascinating subject and is despite first appearances not of merely academic interest. Responses to the GFC have resulted in a slew of legislation, regulatory and oversight change and I set out some of the main areas right at the beginning. The international Financial Stability Board (FSB), chaired by Mark Carney 2011-2018, and most of the major organizations responsible for financial stability suggest that the system is “safer”.¹⁸ It is clearly “different” in some respects but is it really “safer”? “Different” invites the question, according to what criteria, while “safer” invites the response, for who and under what terms, doesn’t it?

Andrew: We recently ran a UK Economic and Social Research Council (ESRC) funded project, where we tried to come up with some key normative principles to inform debate – what we would call a normative theory of systemic risk. It’s a complex argument but we came to view the main ethical issue with systemic risk as its potential to have dominating effects on economically vulnerable individuals and groups. We looked at this using neo-republican political theory (not to be confused with the US political party).

In neo-republican political theory “domination” is the one condition to be avoided if you are to have a free and minimally just society. Domination arises when individuals have their life chances arbitrarily reduced or interfered with, through no fault of their own, as a result of decisions which they did not take, but through situations and conditions which are imposed on them. Obviously systemic financial forces and prolonged system wide recession can have that effect, as individuals engaged in only minimal and mainly subsistence risk taking, have the rug pulled from under their feet, by systemic contraction, leading to a downward spiral in their life chances. Even when risks don’t materialise largely due to good luck, you still have a situation of risk imposition on sections of the population by society as whole, of which they largely have no awareness. The answer for neo-republicans is to make the principle of non-domination, avoiding domination, central to governing institutions. That led us to a discussion of how you make a “precautionary principle” operational in financial regulation (Baker, Schuppert and Cullen 2020).

There is no easy answer to how you implement a precautionary principle, but given the tendency for some rather gung-ho behaviours in finance, this is the kind of thinking you require and it can inform how you use and design loan to income ratios (LTIs) and loan to value ratios (LTVs), for example, which are designed to constrain the mortgage market. If a crisis effectively makes things substantially worse for low-income groups, and then you use LTIs and LTVs to largely target them, to make the system as a whole more resilient, that may benefit society as whole, but it can shut these groups out of credit and mortgage markets for a prolonged period. That is effectively a form of double punishment, as these actors experience the greatest relative losses in life chances, or dominating effects caused by the immediate impact of crisis, then find steps to build system resilience disproportionately penalise them. It raises serious issues of compounding injustices, while simultaneously raising questions about a society being even minimally just, according to the neo-republican position.

Jamie: It strikes me there are multiple strands here, and your comment highlights just how important it is to ask what finance is for, and this too requires deliberation of a kind that, as you say, is largely absent. But just as central bank independence is not neutral, this absence is not without significance. Metaphorically one might say it operates in much the same manner as

¹⁸ Note from Jamie: the FSB was created by the G20 in 2009 as a follow on from the previous Financial Stability Forum: <https://www.fsb.org>

water taking the shape of whatever vessel it is poured into – in this case omission serves to reinforce existing perspectives or underlying claims. For example, the banking and finance sector makes much of its putative “intermediate” role – pooling savings of households and corporations and lending these out – and through this activity banks claim to occupy a key role (which we briefly alluded to previously) as efficient allocators of capital: facilitating transformations from the short-term to the long-term, smoothing out a lifecycle of credit needs for households and providing disciplined – via profit motive self-interest – investment, all of which is core to the vitality of a growing and developing capitalist economy. The ideological emphasis is on the role of banking as a social utility (as vital as and equivalent to water or power) servicing the “real economy”.

It is the hold this image has on how banking and finance are presented to society that confounds the need for a more basic debate regarding what finance is for – that debate requires a more realistic assessment of what banking and finance really do, so readers should be aware the normative issues you mention are also practical and evidential. The problem though is that there are few who take an interest in this – general antipathy to “banksters” and “masters of the universe” and periodic movements like Occupy Wall Street notwithstanding – it is alien to academic economics, though it is a major source of interest to post Keynesians (via a “money economy”) and political economists such as yourself who have produced fascinating work on the issues.

In any case, there are obvious tensions today within mainstream economic theory and banking practice – many central banks and notably the Bank of England acknowledge that banks individually create money when they extend a loan, they are not mere intermediates, and while creation has its limits these are not those set out in standard undergraduate textbooks (the money-multiplier). Lending is not limited by existent reserves (since reserves follow lending) and investment is not a product of savings (savings are mainly deposits resulting from payments induced by investment), much of bank money creation activity is for the production and trading of financial assets with only a peripheral relationship to productive investment. Banks are (stating the obvious which mainstream theory has tended to occlude) tremendously powerful agents *not* passive recipients of savings who conform to timeless axiomatic economic principles of efficiency that are necessarily beneficial to society, merely through the activity of market processes.

Andrew: I think a lot of this goes back to technocracy. I am not anti-technocracy. We need it. But as societies we have put so much faith in technocracy it is arguably becoming a barrier to efforts to create better sustainable societies, that are able to produce the scale of transformation required and avoid the societal collapse that will ensue if we don't get the material transformation in economies that we need. So quantitative easing (QE) is a good example. It was targeted at relatively narrow range of financial assets, after the financial crisis, in an effort to repair not just the financial system, but also patch up a model of private credit sustained demand. From my perspective that was a choice made by central banks because it was the easiest route technically and the least political stance they could take, with the added bonus it might get them back on mandate of targeting inflation. In other words, once again their primary consideration was how do you do QE and preserve “independence”. I do think that central banks by and large underestimated the extent to which QE would amplify inequality and they were simply doing their best to execute technical operations largely in good faith. This also raises the spectre of Green QE, – how you could use money creation to lever the kind of material transformation required. If you listen to the main criticism of proposals like Green QE it is that you can't do it because it would politicise the central bank and undermine

independence. That rather underplays the acute situation facing the human race as a whole as an existential crisis for the entire species, where the potential avenues open are being restricted because the priority is to protect central bank independence.

The same position is adopted on financial regulation – you shouldn't use it to try to shape economic activity, or the shape of the economy more generally. Carney in the climate change speech essentially asserted that kind of thing doesn't work, but moreover it wasn't the kind of thing a central bank should attempt. So technocracy has been elevated to a level where it starts to become an obstacle to, rather than enabling, human progress. I don't want to get into a big debate on the rights and wrongs of Modern Monetary Theory (MMT), which I know divides post Keynesians, but we do live in a world of state backed money creation albeit with a major role for commercial/retail banks, but we are not allowed to talk about the purpose we put that money creation to, because we shouldn't be doing that sort of thing.¹⁹ Given the situation confronting the entire human race that seems an absurdity.

Jamie: A subject that many contributors to *RWER* over the years have highlighted and increasingly so as we have entered a period of acknowledged “climate emergency” (see special issue *RWER* 87). Still, there seems great scope here to rethink the finance system and its role. It is also worth considering how the main central banks define financial stability since this also relates to how they continue to treat banks and other organizations as financial actors. The US Federal Reserve, for example, publishes a periodic guide titled “The Federal Reserve System: Purposes and Functions,” and this is currently in its 10th edition (Federal Reserve, 2016). This provides a whole chapter on financial stability (Federal Reserve, 2016: 54-71). However, while there is a strong emphasis on a combination of macroprudential and microprudential issues, the approach to these essentially defines financial stability as the capacity of banking and finance to maintain the payments system and continue to offer lending services under situations of stress, which when one thinks about it is a low bar for a viable financial system. It presupposes a system prone to crisis, acknowledges that it will at times experience collapses of one kind or another, and does so without ever having to legitimate its existence or the way it distributes the fallout of crisis – existence is a given, distribution is simply a residual of the technical exercise (resilience and resolution).

Moreover, the Fed still uses a language of monitoring, transparency and more and better information as the bedrock of banking organizations and of effective regulation. Clearly, these are not irrelevant, but they are not sufficient and what they are suggestive of is closely associated with how banks and other financial actors are framed and treated by regulatory bodies – here the background still seems to be that they are potentially efficient providers of capital and fulfil a vital role in supporting the “real” economy. I by no means want to suggest the functions are unimportant but what is more significant is the reality which tends to be occluded by the language and the actuality that is misrepresented by the assumptions built into theory (which condition attitudes to the value we should place on what banks are actually doing rather than what they could be doing). This harks back to a point we just made – it matters that we understand how money is created and what it is created for (i.e. what the scale of lending and the priorities of banks are). These are not merely arid issues of relative leverage levels and resilience to “shocks”, but rather a matter of *responsibility* within endogenous processes.

¹⁹ Note from Jamie: Andrew is not implying here that MMT is QE.

Equally it matters that we live in societies where debt is a necessity for many (creating a greater role for banking) because of longstanding trends in welfare spending, wage levels and income inequality. In the UK for example, according to the Institute for Fiscal Studies July 2021, for 2019-20, 22% of households in the UK (more than one fifth) lived in relative poverty (60% or less of median income) and only 33% of that 22% were “workless” and this excludes pensioners, 31% of children in the UK lived in families in relative poverty. General trends in home ownership and house prices also matter for renting and mortgage purposes. In 1966 total household debt in the UK was 57% of disposable income, by 1997 it was 95% and it peaked at 160% in 2008, but over recent years has started to climb back towards and around that peak (see also Bezemer et al., 2021).

In any case, it also strikes me that many of the changes made in the last decade are about protecting the state and its finances from the collapse of banks and finance rather than preventing credit cycle behaviour liable to lead to losses or to lending practices that are detrimental to the public, as well as societies, built around debt dependency and (finance induced) asset inflation – a point that speaks to your previous comments, but what is your take on the general direction of travel of policy?

Andrew: I think that last point on the state is exactly right. I would say the main thing here is there is a lot of opposition to attaching any broader public policy objectives to financial regulation. So I have used the example of Carney in his climate change speech. He says you can't do it and you shouldn't do it, but offers scant evidence for either claim. Carney is also explicit. Central Banks cannot produce the adjustment needed and they cannot guide the financial system or promote “Green finance”.

Read the second half of *Stabilizing an Unstable Economy*, and you will see Minsky is making exactly the opposite argument to Carney. He essentially argues that financial capitalism necessitates central banks guiding the system towards what he calls stability inducing activities. We've talked about money creation above. I would also highlight the design of the tax system too. There are big macro levers available. The Green New Deal proposals in the United States acknowledge these, but the biggest obstacle this kind of thinking faces is that if you try a big macro co-ordinated strategy like that, it will be the end of central bank independence as we know it, so don't go there.

It seems to me we are now in a pressing situation where we do need that kind of Green New Deal thinking, but its primary criticism is the problems it would pose for central bank independence. Of course, there is also an argument that it wouldn't work, but that tends to get asserted, rather than demonstrated. As you noted in your introduction I and some colleagues argue the UK has been suffering from a “finance curse”, a financial version of the resource curse, where a dominant financial sector crowds out and erodes other sectors, by extracting and attracting resources from them, both financial and human. My view is that lifting and avoiding the finance curse, should become an objective of financial regulation, but we are of course miles away from that kind of thinking, as Carney's speech illustrated.

Jamie: Odd then that Carney is often represented as a progressive figure in the media and as his recent book, *Values* (Carney, 2021) indicates he clearly wants to be seen that way. Carney has been replaced as Governor of the Bank of England by Andrew Bailey, who originally joined the Bank in 1985 and is considered a conservative or “safe” pair of hands in the tradition of Governors – selected, perhaps, over the more innovative choice of Andrew Haldane who as chief economist was critical of the current state of the finance system (Haldane has since

departed).²⁰ As your work with Duncan Wigan illustrates (Baker and Wigan, 2017) banking and finance spend a great deal of time and expend a great deal of money in persuading politicians and the public that they are indispensable “as is”, and this invites scepticism regarding the general set of claims and this contrasts with the issues raised by work on financialization. Perhaps the simplest way to shed light on this is with a “naïve” question along the lines of John Rawls’ “veil of ignorance” (though I expect any political philosopher would find the analogy as a thought experiment awkward). The UK and US are two of the main financial centres in the world, given this, if one knew nothing about the realities of banking and finance, but did pay attention to the way both are presented, what would one expect the infrastructure and non-financial business environment to be like in these countries?

While both offer financial services to the world and so one would not expect financing to reduce to domestic initiatives only, one would surely expect that each country would exhibit low-cost, long-term, widespread and continuous investment in state-of-the-art infrastructure as well as a continual flow of investment to fund research, development, innovation and growth in non-financial businesses of all sizes and maturities. This is what one *would* expect if finance primarily focused on supporting the “real” economy and if major financial centres operated according to local knowledge and advantages, spillover effects and economies of scale. Insofar as this is *not* the case, the onus is placed on banking and finance to justify and thus legitimise itself based on the fundamental question we stated earlier, “what is finance for?” And this surely provides grounds for democratic deliberation regarding the norms that inform the design and regulation of the finance system. There are, of course, many statistics regarding the poor state of infrastructure in the UK and US as well as the general paucity of long-term investment compared to other OECD countries – to say nothing of the long-term debt peonage many suffer in contemporary society and the fallout from any financial crisis, and this speaks to the work you did with Juan Montecino and Gerald Epstein.²¹

Andrew: Yes, this is the finance curse work, I just alluded to. But first a word on the work with Duncan you also mentioned. If we go back to 2009-10 and the response to the crisis, I was mainly focused on looking at what was happening on the macroprudential front as we have covered, but looking back I think the most significant and remarkable thing that happened in the UK on the regulatory front, was the way HM Treasury responded to a financial and banking crisis, that raised serious questions about the City of London, its role in the world economy and its impact on the British economy. The Treasury worked to improve the City’s lobbying capacity. I missed it at the time, but still find it remarkable. I shouldn’t be surprised, but the audaciousness of it still makes me shake my head in wonderment. The Treasury proposed creating a new umbrella institution CityUK to strengthen the City’s capacity to articulate its needs and interests to the British government and to international bodies. Given that many people were saying the crisis was in part a function of the City and finance having too much power, both in policy terms and wider structural terms, it seems surprising the UK state responded by seeking to deepen and further consolidate that. The Treasury gave the CityUK a seat on its own high-level board

²⁰ Fellow travelers of Baker at the Centre for Research on Socio-Cultural Change (CRESC) have produced a great deal of related work on aspects of financialization (e.g., Erturk et al., 2008). About a decade ago they published “Haldane’s Gambit”, (Erturk et al., 2011) which explores the form and limits of Haldane’s strategic critique of finance at the Bank and as a response to the activity of a distributed coalition around finance (DCAF), which just after the GFC engaged in public image and “knowledge repair” on behalf of the banking industry.

²¹ Note from Jamie: see also Montgomerie (2019); Pettifor (2018); Lavery (2018); Hudson (2015); Soederberg (2014).

and made sure Treasury views were represented at the heart of the CityUK research machinery and decision making, by putting senior Treasury officials on key committees. So after the crisis, the British state and financial interests effectively became more structurally entwined than ever before and all of this was orchestrated and led by the Treasury at a time when we might have expected the opposite to happen.

Jamie: And the work on the finance curse?²²

Andrew: The work with Jerry and Juan tried to put a price on the costs for the UK of having an oversized financial sector and contrasted that with a similar figure for the United States. The headline figure of £4500 Billion from 1995-2015, was comprised of the costs of the financial crisis and a category drawn from econometric literature on “Too Much Finance” (Arcand et al, 2013, Cecchetti and Kharroubi, 2012, 2015). Misallocation – this is a bit of a black box – involves the crowding out dynamics referred to earlier, it includes a brain drain effect, rent extraction, a kind of structural gravitational pull effect of having a large financial sector in close proximity, with lots of intermediaries and advisors, arranging for capital to be invested in financial assets, and a form of Dutch disease, when countries with a big financial sector often have an overvalued exchange rate due to capital inflows (though this is now less of an issue in the UK post-Brexit).

For the US we also looked at over inflated salaries and profits. We left this out of the UK figure, on the basis of a rather generous assumption that this was the UK extracting from the rest of the world, rather than the domestic economy. The main point about that work is less the headline number, and more that over sixty per cent of the UK figure comes from the misallocation category. For the US it was only 20% of the figure. This suggests the UK has a major problem with misallocation and financial crowding out (for a general reader’s account of the finance curse see Nick Shaxson’s excellent book, 2018).

Jamie: Yes, and relatedly it has been noted before that while both the US and UK house the world’s major financial centres they constitute different proportions of the overall economies, though smaller relative size in terms of domestic economy in the US case does not necessarily reduce power, influence and consequences domestically or globally (one need only look at both the US and UK’s dominance of derivatives markets or financing for private equity leveraged buyout activity).²³ Your interest though is in how to address this curse effect and relative size does matter for issues of regulation...

Andrew: If we want to do something about it, then we need a much better understanding of how the figure breaks down and what the constituent processes are. That could then feed into a more focused reform and regulatory agenda, aimed at lifting, or mitigating finance curse effects. But the difference does strongly suggest that the UK suffers from a major financial crowding out effect. It also means the so-called “levelling up” agenda in the UK needs to be aware of and respond to that major structural dynamic. Needless to say I am very sceptical there will be any consideration of that, but it is a major factor in producing UK regional disparities and societal wide inequality. As we saw with the earlier example, the process also spills over into politics and the very architecture and purpose of the UK state.

²² Note from Jamie: see also Christensen et al., (2016).

²³ Note from Jamie: see, for example, Batt and Morgan (2020); Morgan and Nasir (2021).

Jamie: This reference to the contemporary situation tends to imply banking and finance remain extremely powerful and this invites a final comment-question here before we turn to other areas of your recent research. As your work and that of others indicates (keeping in mind, again, the list of issue-areas we set out at the beginning), there has been no major rethink of the finance system and the role of banking – no major or consequential deliberation regarding its purpose or norms – instead there has been greater recognition of systemic risk, some new organizations, institutions and focus on macroprudential policy and then a series of data and technical developments (stress tests, measurement and toolkit issues etc.). This has mainly been wrapped up in enhanced “resilience measures” under Basel III, which requires relevant banks to hold higher percentage levels of loss-absorbing capital against their balance sheet.²⁴ This capital is an additional last resort store of liquid assets, since banks, in any case, build a default rate into their lending practises and business model and can draw on reserves held at the central bank. Basel III also introduces a (non-risk-weighted to limit gaming) leverage level measure and a liquidity measure and “net-stable funding” measure whose purpose is to ensure the bank is not over-leveraged and that it can survive an extended period of collapse in interbank markets (which would make it difficult for a bank to meet its regulatory and conventional funding requirements that signal its solvency). To the layperson the detail here can seem tedious, but from a critical point of view it is worth highlighting what this direction in regulation presupposes and what it is not.

As various post Keynesians have noted, such as Gary Dymski, reform and regulation have not entailed a prohibition on scale and concentration in the finance system – so, Too Big to Fail or Globally Systemically-Important Financial Institutions still exist and, if anything, consolidation in the banking system has reduced the number of universal banks and contributed to growing concentration.²⁵ Moreover, the regulatory focus does nothing to alter the basic ethos of the finance system – which is that banks continually seek to innovate financial instruments and expand and extend markets – which tacitly assumes “financial deepening” is a social good rather than a potential adverse form of debt-dependent financialization... In this sense, the system remains normatively permissive and regulation is mainly reactive despite its technical focus on macroprudential policy. Also, while there has been some tightening up of “off-balance sheet” strategies used by banks to reduce their apparent risk (rather than real risk based on actual connections), “securitisation” and “originate and distribute” approaches to lending still enable assets to be created out of debt and then flow from originating banks out and around the system, acting as both connective assets and sources of collateral. And though there has been some reform of the derivatives markets, there has been no major regulatory reform of shadow banking, which remains a major and relatively opaque component of the finance system and one which regulators regularly express concern about. Overall the system remains complex, highly interconnected and little understood in its entirety according to any theory and with no overarching body of organizational monitoring, control or intervention – regulation and

²⁴ Note from Jamie: under Pillar I of Basel III capital requirements (with some modification by country) has three components: 1) an increase of minimum Tier 1 risk weighted capital from 4% to 6% against the balance sheet (decomposed into 4.5% common equity and 1.5% additional liquid assets) 2) An additional capital conservation buffer of 2.5% and 3) a discretionary countercyclical buffer set by the regulator at between 0% and 2.5%. So, Basel III greatly increased the level of loss-absorbing capital and tightens up the “quality” criteria for the kind of assets held, in addition to retained earnings under common equity, which a bank can buy and hold to fulfil the loss-absorption function; and, of course, banks are free to hold more than these minimum levels.

²⁵ Note from Jamie: for example, for recent work on TBTF and G-SIFIs as continual problems see Ioannou et al., (2019).

oversight remains fragmented and piecemeal even if organizations like the Financial Stability Board assert progress has been made to make it less so.

Your work, though, nicely summarises the implicit conceptual planks of macroprudential policy, which requires that the whole be both adequately theorised and monitored, but, of course, your work also highlights the limited nature of developments from these planks and the tensions involved. Resilience via loss-absorption in this context is like applying a break without knowing you (or someone else somewhere else) have taken your foot of an accelerator (while financial deepening as a direction of travel allows for that acceleration, though countercyclical mechanisms *might* place a drag on this). As the split between the worlds of Fama and Schiller (and behavioural finance) illustrates, financial economics has still not come to terms with what asset prices indicate – do they express objectively true states of the economy at large or are they more snapshot psychological indexes (few it seems read Keynes on this)?

There seems then, still to be scope for Minsky's question, "Can it Happen again?", since a complex system with endogenous processes can shift into adverse positive feedback loops because of sudden changes to aspects (some of which might result from "innovations" that regulators do not yet understand even if they are aware of them) that might otherwise seem to be only small proportions of the whole system and of limited interest from the point of view of individual balance sheets. Interestingly, this is something regulators are concerned by, despite their otherwise limited approach to regulation and reform over the last decade. For example, in a recent speech, Jon Cunliffe (deputy governor of the Bank of England responsible for financial stability and member of the FPC) noted the rapid growth in cryptocurrency markets in recent years and that regulation was urgently needed, since in their guise as speculative assets they were becoming a part of standard investment portfolios and were increasingly being traded by hedge funds. As Cunliffe notes, while their total value in October 2021 may have only been \$2.3 trillion and that was a small proportion of the total of \$250 trillion of outstanding financial assets in 2021, sub-prime was only \$1.2 trillion in 2008 – a similarly small proportion. It is what can be triggered in a complex system that matters... The question I am posing here in a rather elaborate way is, what vulnerabilities do you see emerging?

Andrew: Well, as I noted right at the beginning of our discussion, we (colleagues and I) think there is an under-appreciated systemic vulnerability in the large cap sector produced by excessive distributions and dividend payments, mainly funded by debt and creative accounting. To coin a phrase – the danger is "good will" evaporates. There has been some interesting work done at the BIS by Hyun Shin. This concerns whether there are enough revenue profits to meet long term debt obligations of certain investment funds. Pension funds seek long term returns but also have large daily bond redemptions. The worry is some funds might need to sell assets to meet their redemptions and the danger is that a procyclical downward price spiral takes hold, creating a sharp increase in bond funding. Asset managers often engage in cash hoarding to protect themselves against these bond redemption claims, but that in turn can amplify procyclical dynamics and create fire sales. I haven't thought too much about cryptocurrencies, so will refrain from commenting.

One of the biggest challenges that post crisis regulation has faced relates to the difficulties of extending the regulatory perimeter so that it goes beyond conventional banking and allows certain macroprudential techniques to be used in regulating shadow banking activities. The issue here is that proposals for things like margin setting in central counterparties would have to be taken forward by securities regulators. Legal and accounting backgrounds feature more prominently there, so the potential for the kinds of systemic discontinuities, that are identified

by macroeconomic analysis, have had less traction and are much less of a concern in that regulatory setting. Securities regulators tend to adopt a “buyer beware” attitude and see their role solely in terms of providing the necessary transparency in information standards to inform individual transactions. The Financial Stability Board (FSB) has written several reports on extending macroprudential policies to asset management, but runs into this very institutional obstacle, where the securities regulators shrug and say that is not our concern and it is not how we see the issue. So even when the political and analytical will is there to do more on the regulatory front at the FSB, there are institutional obstacles, relating to turf, which can be very constraining and mean parts of the financial system are effectively not subject to macroprudential policies intended to limit the build-up of systemic risks.

Jamie: And of course, “turf” invokes the further point that the world does not reduce to the major recognized financial centres. One might, for example, point to “financialization with Chinese characteristics” – China’s response to the GFC through a massive turn to domestic infrastructure investment, combined with a domestic economy consumption and housing boom, and with both connected through state banks, provincial and local government control of land and manipulation of special purpose vehicles, which has created a huge problem of leverage and non-performing loans; and while it is debatable how far this allows for financial contagion outside of China (opinions differ), it does make one of the production powerhouses of the world vulnerable.

As for the previous issues, again, opinions differ among influential commentators, Daniel Tarullo (former member of the Fed Board of Governors) argues that unless oversight and regulation extend from banking further into the finance system, covering shadow banking etc., then it is not realistic to suggest systemic risk can be anticipated or controlled:

“Macroprudential policy refers to financial regulation formulated with a view to the health of the financial system as a whole, rather than to the health of individual firms, no matter how large. By definition, macroprudential measures should cover both bank and nonbank actors; thus, the limits of shadow banking regulation handicap development of macroprudential policies... For a number of reasons, macroprudential measures are relatively underdeveloped. While the broad conceptual case for macroprudential measures is strong, substantive analytic work is needed to translate intuitions on system-wide feedback and second-order effects into well-considered and manageable regulatory practice.” (Tarullo, 2019: 73-74).

Darrel Duffie, meanwhile argues that while there are still problems with derivatives markets, bank balance sheets are in the main safer.²⁶ For example, major banks (due to Fed intervention) in the US are deterred from offering “intra-day” financing for large depositors (a practice where in between repos – a Tri-Party banking function - the risk of continual flows of large deposits from money market funds etc. were carried by the bank within a short-term funding cycle) and this was a major vulnerability (Duffie, 2019).²⁷

²⁶ Note from Jamie, visit: <https://www.darrellduffie.com>

²⁷ Note from Jamie: to be clear repos are a standard aspect of balance sheet management and financial practice but do carry inherent problems (see, for example, Gabor, 2016) and have at times been a source of egregious conduct – facilitating the kind of intertemporal accounting sleights of hand Baker alludes to early in the interview. The classic example is Lehman’s use of “Repo 105”, exploiting an accounting loophole in the categorization of Repos to reduce the level of debt on the balance sheet at key reporting

Still, as your colleague Adam Leaver points out (among others) debt instruments along the lines of those at the heart of the GFC have not gone away and in some respects have made a return, even though their use and thus nodal role in contagions may be different (Leaver and Tischer, 2019). Collateralised Loan Obligations (CLOs), built and tranced according to similar mathematical-statistical techniques as CDOs and including increasing bundles of “covenant-lite” loans equivalent to sub-prime, have increased in recent years – by 2018 CLO volumes were equivalent to CDO volumes in 2006 – and various organizations (such as the Bank of England) continue to flag this as a source of systemic risk (see Chan et al., 2019). In any case, avoiding worst case outcomes is *not* automatically the same as promoting a useful productive banking sector...

Finally then, let’s turn to your recent work, which as I understand it focuses on the various connections between finance systems and taxation and some of which involves collaboration with Richard Murphy.

Andrew: That’s right. One of the conceptual underpinnings of that tax work is that Modern Monetary Theory (MMT), or more exactly a world in which government creation of money is a prominent reality, either directly through the central bank, or by private banks operating under license from the government, has implications for how we think about the role of tax (e.g. Murphy, 2019). This is less about making an original contribution to monetary theory. We are not in that business. We are in the business of encouraging a rethink and reframing of the role of tax in society. We argue it is more useful to think in terms of a spend-tax cycle, rather than the more common image of a tax-spend cycle. Under this reading, tax is recovering or reclaiming money already created, on behalf of the government and performing a withdrawal function (for related work see Baker and Murphy, 2019, 2020, 2021). We have called this Modern Taxation Theory.

Jamie: And most of our readers are aware that MMT theorists hold that money is spent into the economy so tax is not the source of government spending, but rather within a version of Abba Lerner’s “functional finance”, a way of draining it out of the economy and of addressing issues of distribution, social coherence, inequality etc. So, revenue has a different meaning frame in MMT. And proponents like Randall Wray have taken an interest in Richard’s work (see Fullbrook and Morgan, 2020).²⁸

points in the year (Vitan, 2010): “In a standard repo transaction, a firm like Lehman sells assets to another firm, agreeing to buy them back at a slightly higher price after a short period, sometimes just overnight. Essentially, this is a short-term loan using the assets as collateral. Because the term is so brief, there is little risk the collateral will lose value. The lender – the firm purchasing the assets – therefore demands a very low interest rate. With a sequence of repo transactions, a firm can borrow more cheaply than it could with one long-term agreement that would put the lender at greater risk. Under standard accounting rules, ordinary repo transactions are considered loans, and the assets remain on the firm’s books... But Lehman found a way around the negotiations so it could count the transaction as a sale that removed the assets from its books, often just before the end of the quarterly financial reporting period... The move temporarily made the firm’s debt levels appear lower than they really were. About \$39 billion was removed from the balance sheet at the end of the fourth quarter of 2007, \$49 billion at the end of the first quarter of 2008 and \$50 billion at the end of the next quarter.”

²⁸ Note from Jamie: see comments at <https://neweconomicperspectives.org/2019/10/mmt-report-from-the-front-part2.html>

Andrew: Obviously revenue flows remain important, but so too is the repricing, redistributive, and reshaping function of tax under this reading. We see taxation as the most extraordinary legal, administrative system societies possess to impact their economies and societies – to reiterate, repricing, redistributing and reshaping economic activity. This conceptual underpinning in our academic work has spilled over into policy related work, we have been doing for the Global Initiative for Fiscal Transparency (GIFT) based in Washington DC and funded by the World Bank and IMF. GIFT commissioned us to write a primer, framework document on the concept of tax transparency and a series of practical tools for achieving greater tax transparency in national systems. We essentially argue that the starting point for greater national tax transparency, is for governments to be explicit and clear about the public policy objectives they wish to attach to their tax systems. We then call for tax systems to be evaluated in their entirety in relation to the extent to which these objectives are being met.

Jamie: This seems quite different than the current dysfunctional state of domestic tax systems regarding the right to tax (and its progressive structure) and of international tax opportunity – especially corporation tax and matters of avoidance etc. Much of which suffers from analogous problems of framing to banking – issues of legitimization etc. as various of your colleagues have also argued – Len Seabrooke and so on.²⁹

Andrew: Richard and I argue that the problem is that many tax systems around the world have seen their redistributive integrity systematically undermined over the last thirty years or so. That is they are structured in ways that often increase and amplify, rather than reduce inequality in those places. If you want to repair and restore the redistributive integrity of tax systems, you need much better readings of what they are doing now and how they are performing, including the impacts of extensive systems of often hidden reliefs and allowances, on which there is generally very little data worldwide. There are £400 billion reliefs provided each year in the UK, which is twice the annual budget of the NHS. We estimate that around 25-30% of that goes to incentivising certain financial products and assets, in others words a subsidy to existing wealth. We argue there is an assessment imperative connected to this and we have designed a series of tools for assessing tax system performance: tax gap analysis and tax spillover assessment, are two such tools. In July GIFT published the finished version of our report for them *Making Tax Work*, that was peer reviewed by GIFT staff, IMF and World Bank Staff, national officials from South Africa, and Mexico and a variety of civil society voices (see Murphy and Baker, 2021).

That work led us to identify ten high level principles of tax transparency, which GIFT have now translated into 13 principles. Those principles are now part of a wider public consultation process. The World Bank is interested in using these in future conditionality, the IMF is interested in their data generating potential and the OECD wants to work more actively on national level tax transparency in the future. We see tax transparency – as a public good that creates information to enrich national level multi-stakeholder policy dialogue and to better enable the identification and targeting of tax reform agendas – which is how we define it. This is beginning to emerge as a new policy subfield for the international organizations. This is something we think has a lot of potential and is a major part of the potential policy toolkit for aligning national tax systems, with a sustainable, inclusive growth agenda. Hopefully, we have made a contribution here and set down the basis for a useful policy trajectory through that work.

²⁹ Note from Jamie: see reference lists in Morgan (2021, 2016).

Jamie: Allowing, of course, that transparency implies more than just better information leading to efficiency in the economics sense. But this is one of the things I like about your work and many of your colleagues, it brings together critique of the power dynamics of a status quo, substantive theory and also practical suggestions and interventions. This it seems to me has become a hallmark of academic political economy in recent years – Johnna Montgomery, Daniela Gabor, Adam Leaver, Jan Fichtner, Dean Baker, Eileen Appelbaum, Rose Batt, Özlem Onaran and so on. So, by way of conclusion perhaps you might comment on how you see the role of contemporary political economy and academic expertise – some of your editorial colleagues I note, have called for more attention to “blind spots” (Best et al., 2021; LeBaron et al., 2021).

Andrew: I would call for a more forward-looking political economy. 95% of the political economy scholarship I have seen in ten years as a journal editor has been backward looking – basically explaining and theorising things that have already happened. A lot of it is good robust high-quality work, but we also need to do more to talk about the future. But doing that and retaining academic rigour, or credibility is very hard to do. In the work with Richard on tax, what we’ve tried to do, is go back to the distinction drawn by Andrew Gamble back in 1995, between scientific political economy (though I prefer the term “diagnostic political economy”) – an account of what is actually happening in the economy, normative political economy and practical political economy (policy oriented). We have tried to combine all three of those elements in one package in the work on tax.

The practical aspect has involved us trying to invent actual policy tools and frameworks that could be used by other actors. Richard wrote one of the first versions of what became Country by Country Reporting, which has now been developed by the OECD, so he has a track record in that regard. But if you do that you also need a good understanding of the political appeal of certain tools and how they can build coalitions of support, and that is where the political economy analysis comes in. We also started by picking a normative objective for our tax work (though that was informed by our diagnostic analytical work on reliefs and spillovers), that was to reduce the harm done to the integrity of tax systems by tax competition (harm by states to their own tax system, but also to other countries’ tax systems) and to try to repair the redistributive integrity of tax systems, bit by bit on a systematic basis. The policy tools we created were informed by that normative objective. I hope we have shown how it is possible for academics to do forward looking political economy in that work on tax, and that others are able to learn from our experience.

Jamie: A lot of this I guess, these days has yet another limiting framework – one we briefly alluded to earlier and that is climate emergency and your own work on tax and MMT as well as finance seems to have applications here – though degrowth, postgrowth and social ecological economics adherents are sceptical regarding what can be achieved from new financing if Green New Deals are not coordinated within realistic approaches to scale and intensity of economy; and as the CORPNET project illustrates one cannot neglect the role here of massive behind-the-scenes owners of corporate assets (such as the Big 3 passive investment funds).³⁰ The integral role of consumption in an unstable endogenous debt-dependent system of finance is perhaps another matter worth highlighting. As an editor of *New Political Economy* I expect you are starting to see a body of work coalescing around climate emergency? It is certainly

³⁰ Visit: <https://theconversation.com/three-financial-firms-could-change-the-direction-of-the-climate-crisis-and-few-people-have-any-idea-131869>

something Edward Fullbrook has been keen to encourage at *RWER* (Fullbrook and Morgan, 2019).

Andrew: On climate change some, but probably not nearly enough. The key is political economy has as its stock in trade analysis of crises and dynamics of transformation, so as a field it is well placed, as Matt Paterson notes (Paterson, 2021), to steer us away from a collapse dynamic towards a transformation dynamic, but we need to see much more scholarship investigating that, and which also has an appreciation of the urgency of the situation and the scale of the transformation required.

Jamie: I guess your own sense of how a responsible academic might respond has evolved too.

Andrew: Well I am not sure I had a clear plan to become an academic. I did become interested in global governance when I was doing my PhD and its potential to do good, which was clearly not being realised, but had been achieved to some degree in the past. When I started in academia, most forms of global economic and financial governance were pushing in directions, often quite aggressively, which seemed to have potentially harmful long-term consequences for social cohesion and humanity as a whole. So I guess if I had to pick one thing that has motivated me, it has been how do you build coalitions, ideas, narratives and institutional mechanisms and devices to change that, as well as what the obstacles to that are and how you overcome them. So if we take climate change, I have heard scientists at Sheffield connected to the Grantham Institute say we have the technologies and science to abate climate change, but it is a combination of the politics and the economics that stops that. That is telling us we need political economy analysis more than ever.

Jamie: And a more assertive critique of the tensions in “technofixes”. But drawing this discussion together and to a close, where do you see the world going over the next “pivotal” decade or so? In observing the slow car crash of current events, such as COP26, it seems hard to avoid a “dialectics of despair”. Civilisation is unlikely to end in one big event, there will be many small endings over the next decade, equally it cannot be saved by one big gesture, there will be many small acts of salvation over the next decade... How this will turn out though... (for possible constraints imposed by a “Wall Street Consensus” see Dafermos et al., 2021).

Andrew: I am afraid I don’t have clear answers to the messy post-crisis politics that we have seen, or a clear sense of how that will play out. Under the pandemic I would point to two trends, a gradual undermining and softening of mechanisms of democratic accountability, in a range of ways and in numerous domains through a variety of mechanisms in the UK. That is multi-faceted, but it covers parliamentary process, electoral law, the role of the electoral commission, the Information Commission Office, media and Supreme Court reform. At the same time, outsourcing has continued apace and new contracts, without competitive tender, have built the centralised data capacity of the state, often involving relationships with some quite dubious partners. An open question, to which I don’t have the answer, but which needs to be posed, is whether the combination of these two things is producing a qualitatively different form of state in the UK?

Jamie: A question (the nature and future of the state – the central question, for example, of Bob Jessop’s work) being asked in many places – not least the US.

Andrew: Broadly speaking I would make the point that we have been in an era of stealth politics from the right. That's that curious mix of libertarian and authoritarian nationalist thinking, that has been playing out through Trump and Brexit, – but also in other forms of so-called populism around the world. My interpretation of this is that nationalist discourses have in part been about politically concealing libertarian intent in relation to all sorts of regulatory matters, healthcare and even taxation. Nationalistic messaging has been used as a kind of shield in that context, - sometimes called the age of post-truth politics, to conceal or obscure the intent around a lot of economic agendas. There is a lot of potential around the Green New Deal, but the hard reality is the growth elements of that will occasionally need to be curbed. I will leave the final word on this to my Sheffield SPERI colleagues Matt Bishop and Tony Payne (Bishop and Payne, 2021, Baker and Murphy, 2021).

Bishop and Payne call for a form of re-globalisation. A reforging and repurposing of global governance mechanisms for what they call a post-neoliberal era, and indeed the creation of new forms and mechanisms to tackle and reverse enduring inequality.

Jamie: Heikki Patomäki and Barry Gills are others arguing along these global lines and a previous *RWER* has discussed post-neoliberalism (Fullbrook and Morgan, 2021).

Andrew: It is very clear that whatever we do, especially on climate change and inclusive green agendas, it must involve a vastly different looking economy. Net-zero means decarbonizing electricity, electrifying ground transport, radical improvements in energy efficiency, eliminating ruminant agriculture (dairy as well as beef or lamb), creating radical breakthroughs for production of cement, steel and plastics, possibly eliminating flying entirely and radically new “negative emissions technologies” (Paterson, 2021). This needs to have a strong global co-operative element, combined with more localist agendas around new municipalism and community wealth building.³¹ That's a transformed political economy and one that requires transformational thinking and probably a very different role for the state and the public authority in the economy. There are some interesting grassroots things stirring all around the UK on the latter. Beacon in Liverpool, my home city is a very interesting development and agenda.³² On the former, I hope that the work Richard and I have been doing plays a small practical role in the kinds of re-globalisation Bishop and Payne envisage. Progress on the scale of transformation required, will also demand a financial system imbued with a much clearer sense of social purpose, which means that financial regulation has to be seen as more than just a technocratic enterprise, but something in need of political and normative leadership. That is what my scholarship has been trying to illuminate by creating a better sense of the political economy obstacles that exist to that process, and how to overcome them.

³¹ Note from Jamie: as well as some practical and ideational resistance to the continual use of claims about future technologies as reasons to do less now... which is a problem that has undermined claims regarding net-zero and negative emission technologies such as Bioenergy and Carbon Capture and Storage (BECCS) since at least the 5th synthesis assessment report of the IPCC.

³² Note from Andrew: <https://beaconliverpool.co.uk/>

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