

## Economics and normativity in four sections

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### By way of context: the separation, subordination and sublimation of normativity

In articulating what it is that they do, mainstream economists tend to emphasise that they build models and use these to convey information regarding datasets. The grounds of this are supposed to be clear because economic models are mathematical in expression. Clarity is putatively reinforced because the empirical processing is based on statistical methods and publically available tests. The combination becomes the available material offered to, or constructed for, policymakers. Policymakers use the constructed models as *they* see fit, typically to envisage likely consequences of policies. The material also becomes part of public discourse directly or indirectly. At least tacitly the material involves the claim that these models adequately describe the world to its inhabitants, albeit synthesised, simplified or reduced in ways whose seminal expression often includes caveats regarding relevance and realism.

Concomitantly, two highly general understandings form the background to how an economist thinks of herself as a social scientist. Policymakers use the models as they see fit and *they* are responsible for making policy-decisions. There is heavy emphasis on a concept of the *scientist* in the economists' self-understanding as a "social scientist". According to the dominant view, her field of study is *particularly* suited to measurement and quantification (see Fourcade et al. 2015, but for geographical-cultural difference, Fourcade, 2009). When an economist replies to a critic that they are "objective" it is this that she typically has in mind. From within this mindset, much of the critique of economics to a mainstream economist seems either ill-founded, misplaced or patronising, creating oppositional problems of response.<sup>1</sup> Few will for instance feel the need to respond to accusations or claims that they are neoliberal ideologues or that they are positivists in some pejorative sense or unreflective technocratic number crunchers.

It remains the case, of course, that one can be a positivist even if one does not know what that is (in the sense of aspire to or unwitting adoption or conformity to its tenets). Although the term "positivism" is often used pejoratively, it can nonetheless be an accurate categorisation of the worldview of the person. And, one can be a positivist with variant characteristics, and being a positivist can be problematic even if one has a received position that accords with or approves of the term under some understanding.<sup>2</sup>

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<sup>1</sup> By mindset I do not mean to imply a free-floating set of ideas or beliefs reducible to psychology of the person; the whole occurs within structures of social relations that shape how knowledge is produced and reproduced.

<sup>2</sup> It is important to stress that not all economists are likely positivists in terms of any given account of that term. Caldwell (1994; 2013) focusses narrowly on logical positivism and logical empiricism – involving the strong requirement that science should only make reference to observables and the central quest for ever more precise predictions – and comes to the conclusion that the rhetoric of positivism has waned. "Economists were not following the strictures of positivism, because no science can" (2013, 757). However, in the broader sense (as used for instance by the critical theorists of the German *Positivismusstreit* and, later, by critical realists) also contemporary practices and rhetoric of economists do emanate from positivism. Characteristically positivism in this broad sense involve belief in the one

To be clear, if, based on philosophy of science or social science, one labels an economist as a positivist they will not necessarily know what is meant. This is because their familiarity with the term is unlikely to derive from philosophy of science or social science. The most likely places an economist will come across the term is in a general textbook on research methods (as part of research training where it reduces to use of quantified methods that presuppose or seek regular law-like states of affairs) or from Friedman's well known essay (irrelevance of assumption, so long as predictive success) or its many imitators. Positivism in a general research methods textbook is presented as one approach among many that different disciplines might think best "fits" what they do. If selecting from a research methods textbook, especially based on a mainstream economists' mindset, one is acquiring a language of confirmation rather than engaging with a source that encourages questioning. These kinds of textbooks are not detailed arguments in philosophy of science or social science that place approaches in purposive critical context to address whether each is fundamentally defensible or appropriate (as matters of epistemology and ontology etc; see Lawson, 2015; Mäki, 2001).

This is not to suggest that there is no philosophy or methodology of economics that develops the discourse of philosophy of science or social science (see Blaug, 1992; Caldwell, 1994; Boumans and Davis, 2016). Nor is it to suggest that nothing has been written by philosophers and methodologists of economics about Friedman and the nature of the argument posed (see, for example, Mäki, 2009). Questions raised include: is positive science the same as positivism, and which kind? Is the positive-normative divide necessarily associated with these? For instance, if science should only make reference to observables, it would appear that values cannot be scientific (testable).<sup>3</sup> More generally, if economics is a science but not necessarily a positivist science, what kind of science is it? For example, what do prominent originators later say regarding the consequences of what they have wrought.<sup>4</sup> What I want to emphasise is that little of these philosophical discussions has actually filtered into the discipline at large (see also Colander, 2013).

To reiterate, because of their training and background assumptions, few mainstream economists will feel the need to respond to accusations or claims that they are positivists or unreflective technocratic number crunchers or neoliberal ideologues. Moreover, not all mainstream economists are positivists and well-known economists have also included critics of positivism; and to reduce all of economics to a unidirectional ideology will likely seem to mainstream economists a grotesque parody that impugns the professional basis of a hard earned and valued (in high demand and relatively well compensated) skillset. So, a further problem is that the sociological framing provokes personal affront and "it is" becomes "you

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single standard scientific method, often mimicking the Newtonian paradigm in physics (for Smolin 2013, the Newtonian paradigm includes also the theory of relativity and quantum mechanics); reliance on quantitative data and precisely formulated scientific theories and mathematical models; and belief in cumulatively progressive science that discards errors over time. Thus there is no point in doing philosophy because 'we' already know what the standard scientific method is; and there is also no point in studying the history of ideas, because the most recent papers contain all there is to know. Lawson (1997) identifies positivism as monism (the knowledge process is viewed as the accumulation of incorrigible facts) and deductivism (science is viewed as deduction events from sets of individual conditions and constant-conjunction 'laws'). Lawson argues that while criticism and experiences may have shattered monism, formal deductivism is very much alive in economics, with its atomistic assumptions about closed systems and causality understood as law-like regularities.

<sup>3</sup> Even if one allows for non-observables (e.g. realist interpretations of preferences) or realist accounts of what models are, values can still be seen as untestable, non-scientific, non-rational or subjective.

<sup>4</sup> Compare Lionel Robbins (1932) and (1953), or early and later John Hicks, or most mainstream Nobel memorial prize winners when asked to reflect on rather than engage in economics. Even Milton Friedman later bemoaned the unthinking use of mathematics in economics based on easy access to computer packages that reduced understanding of what was calculated.

are” (it is difficult to balance a sociological account with a genuine dialogue about both substance and philosophy of science). That critics are actually concerned with fundamental issues of economics as knowledge, its realism, social construction, social significance and normative consequence is readily lost in translation.

In the main, mainstream economists will self-identify if pressed, as scientists offering objective, since mathematically expressed and quantified, technically skilled insight for others to make use of. Within the mindset, the positive-normative divide has become a self-understanding where normative issues are the aspects *separated out*. The understanding is that the economist is offering something technical and responsibility for what is done with that is left to others, *even if* the concepts, findings or claims lead to advocacy. The positive-normative divide has come to mean in practice, and upon reflection, an “it’s up to you what you do with this”, and a view that economics is “about how you (best) make choices” not what you choose. To be clear, this is its practical understanding, not its formal statement, and, of course, it tends to fall back on the means-end distinction built into Robbins’ originating definition. Today the means-end distinction is linked to a looser reference to the “objective” status of that which is offered, creating a degree of circularity and a whole host of possible tensions if one questions what objectivity means. For example, if objectivity is understood in terms of rational self-interest, behavioural economics has shown that choices can be manipulated, often in subliminal ways. Public relations, marketers, economists, politicians etc may have an interest in manipulating the choices presented to the decision-makers.

What I am suggesting is that economists have a particular socialised sense of a normative separation, which is also about where responsibly is *placed* for knowledge produced. Responsibility for knowledge produced is ultimately *placed* with the recipient (including the subject that is objectified as a modelled entity – the locus of utility, preference or profit). Still, there is a presumption that economists have a privileged *right* to be heard, and they have a *duty* to offer themselves to policymakers to be heard. This is unquestioned, and yet, from a sociological point of view, quite odd. Perhaps it is a facet of the status of the skillset, its “objectivity”, which adds to the “it’s up to you” a “but given the data any rational person would...” (the problem of “observational equivalence” notwithstanding)<sup>5</sup>.

This also helps to explain how economists can make sense of the interminable disagreement that also characterises the profession. By this I mean there are many disagreements regarding what models are best, and what is to be done because of what is modelled, amongst those with common commitments to economics as a *science*. Even this restricted world involves norms and normative concerns. However, normative concerns are rarely discussed as such. Rather dispute regards differences of assumption and technical construction: what to loosen, what to emphasise, what to weight, omit or order, how to test, what to infer, what degree of failure (imprecision, proxy, error or variation) constitutes current progress based on best practice. The responsibility for normative choices is externalized to decision-makers, business-leaders and consumers.

Little, if any, thought is given to *the problem of normativity* that prefigures the issue of any given norms or normative principles built into economic knowledge, and the normative commitments that flow from how the world is “described” or policy possibilities are reduced and framed. This has been clearest when and where formalism and axiomatization dominate.

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<sup>5</sup> Observational equivalence refers to indeterminacy between models in relation to findings based on the data. Any number of models can be made to fit with given set of data.

As economics becomes more axiomatic it loses both the means and the will to deliberate regarding value judgements as value judgements; these become propositions, postulates and predicates. However, the practice of exteriorisation and lack of comfort with values as values is not restricted to the highly formalistic varieties of economics (pure theory) or to the era of its dominance. It has remained relevant through the “empirical turn” of the late 1990s and 2000s and into the contemporary “credibility revolution”. The main reason for this is the gradual elimination of history, philosophy and methodology from an economics education and their replacement by mathematical and statistical methods (this elimination seems to follow logically from positivism, cf. note 2).

The language and concepts of normativity have been lost, and with them the simple sense of ease that this is a legitimate area of concern *as economics*. Just as responsibility is *placed*, the problem of normativity is *displaced*, in so far as it is delegated, either to other social sciences or to sub-disciplines of economics (philosophy, methodology or history) that are no longer part of the majority’s economics education (a point well expressed by Cartwright and Davis, 2016; Lawson, 2017). All of which is to suggest that an economists’ approach to the positive-normative divide is not quite, in its contemporary form, an unambiguous ought-is divide.<sup>6</sup> It is separation, subordination and sublimation of normativity in the actual practices of the economics profession.

### **Neutralization is not neutrality**

Focusing on placing responsibility does not make the knowledge produced by economists neutral. The kind of objectivity that most economists have in mind cannot make it neutral. The construction has consequences, and emphasising who decides does not negate that there are consequences that flow from the forms that can be chosen from. Though economics’ capacity to address normative concerns is in a sense neutralized (externalized) it does not follow that economics is normatively neutral.

If one falls back on the clarity made possible by, first, mathematics as a means to express models and, second, statistical analysis as a means to use datasets (where datasets are supposedly used to test models and models are supposedly used to extract information from datasets), then one has restricted the scope for contestation but has not created neutrality. This is the framing within which failure (imprecision, proxy, error or variation) is expressed as progress. The restricted scope bends criticism inwards. What counts is primarily what can be counted and thus economics burgeons where datasets are available (even if the focus is trivial or distracting – as, for example, many have noted of the effects of experiment on development economics and funding priorities at the World Bank)<sup>7</sup>. When models are criticised, innovations take the form of different constructions of similar modelling formats. Models proliferate through technical modifications in various (sub) branches. This is one important reason why change diffuses so slowly through economics and why change rarely involves fundamentals.

Consider how long it took behavioural economics to gain traction within economics and consider how marginal many of the changes in practice are. Contextualizing concepts, shared

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<sup>6</sup> It, for example, owes little to Hume’s guillotine, just as subsequent social science discussion of the positive-normative divide owes little to what Hume actually meant.

<sup>7</sup> Specifically, the problem of “randomized control trials” (RCTs).

with much of information-theoretic economics, such as disequilibrium, sub-optimality, limited rationality, inefficiency and so forth take as their point of departure concepts and meanings that are otherwise unrealistic or impossible, and this typically hampers the degree of genuine difference in the diversity offered by behavioural and other new branches of economics. Behavioural economics is in the end not about the messiness of human conduct, but the well-behaved variation in modelled agents. The real world is neutralized in what otherwise seems like a *more* realistic approach to modelling. The agent is never addressed as a moral centre of ultimate concerns within a field of economic problems that she is invited to deliberate in regard of. Instead she becomes a test subject to isolate a trait that can then be expressed and/or manipulated (loss aversion, status quo, disposition and injustice effects). Normativity is neutralized in so far as economics, which otherwise involves intrinsically ethical subject matter, becomes a billiard ball decision-making enterprise (what does an agent do if treated this way or that?). This infantilizes the human in both conceptual and policy terms, whose mirror is a paternalised corporation or state.

Fundamental problems can and are recognized. However, where statistical analysis is involved this typically leads to permanent deferral of the otherwise clear consequence of the criticism. Ed Leamer, for example, has been a consistent critic (and practitioner) of econometrics for twenty five years. Yet if you compare Leamer 1983 and 2010 he is essentially making the same point: economics has not solved its so called “con” problem (the data potential cannot reproduce conditions that allow for practice equivalent to ideal test conditions, and method includes inherent problems of accounting for cause via variation in possible models and findings). Rather it has changed the context in which the con is produced, the new randomization literature of the twenty first century does not genuinely address the problem of randomisation he actually had in mind. It does not make the new methods any more reliable in their findings. Leamer’s substantive critique is just one version of a set of fundamental concerns (see Velupillai, 2007; Freedman, 2010 or Lars Syll).

Model adequacy requires the basic assumption that the model is “well-specified”. That is, all relevant variables are accounted for (completeness), all are measurable, separable and independent, and are properly ordered, consistent and transitive, whilst conditions assumed in the treatment of the error term remain unchanged; and, of course, that the findings are transferable (have external validity). This technical language may not mean much to you, but it is a highly restrictive set of requirements. It immediately invokes two kinds of possible response: seek to solve the problems of technique and use only where appropriate. The former assumes there are in fact technical solutions that are also relevant, and the latter requires one to first ask about the nature of the world to which the techniques are applied. Technical solutions are the overwhelming focus. Most econometrics textbooks or courses begin with an introduction that tries to articulate scope, but this quickly becomes an introverted focus on what is appropriate *to* statistical analysis (what kind should one do, what do tests indicate, what makes for “well-behaved” data etc.). This is subtly different than is the statistical analysis appropriate *for* its point of reference (the actual problem under investigation).

You may be thinking the argument I am making is peripheral to problems of normativity. It is not, however, this argument but the actual subject of argument (economics) that has become peripheral to normative concerns; demonstrating or stating this requires deviation. The nature of contestation in economics has important relevant effects. It does not lead to a questioning of appropriateness, but rather to continuity of practice and focus, a “this is what we do” (rather than a “what should we do?”). The skillset dictates what is done. This is important because

mainstream economists are not wrong when they suggest their skillset is valued (in high demand and relatively well compensated). The limitations are often exposed: explanatory and predictive failure; non-transferability of claims; and continual need to justify re-estimations. At the same time, specific studies are usually not replicated or confirmed independently, although that would be required to fulfil the status stated for findings in terms required by the prevailing methods.

The *social* value in clarity of mathematical and statistical expression is problematic once it becomes clear that they are technically unclear because of differences that cannot be reconciled, fixes that are required, and limits that are unaddressed. Social value speaks as much to social influence and social usage-as-power as it does to success in its own terms, and response to exterior critique of those terms. This is self-reinforcing. Quantification provides authority, and it is this that no other social science can match. But this involves sacrifice. The focus on measurement elides the social significance of quantification. It also represents a huge transfer of resources from problems that cannot be posed, developed or explored without models or datasets.<sup>8</sup> Normative neutralization is thus also non-neutral in terms of what becomes absent by virtue of focus.

### **Invisibility, deformation and ideational preference: inequality**

What mainstream economics has become creates limits on what society can be because mainstream economics is an extremely powerful source of ideational content. This is not just a matter of what cannot be explored without models or datasets, since many things that *can* be explored in this way are *not* actually explored and some that *are*, are deformed. One well-known current example is inequality. This problematic was conceptually invisible within the mainstream prior to the popularity of Piketty's *Capital in the Twenty First Century* (2014) despite more than two decades of growing (if variable) wealth and income inequality (within states rather than necessarily between them), and despite longstanding work by James Galbraith and others. As Piketty notes, this was not accidental, it was a consequence of the dominant conceptual constructs, mindsets and lack of empirical curiosity amongst economists.

The idea that marginal productivity in competitive markets equals to its price means, from a normative point of view, that labour is paid what it is worth (naturalising social division). Trickle down assumptions lead economists to anticipate incomes are all growing and wealth diffuses (rather than power allows wealth to be captured, concentrated and protected). Given the prevalence of these kinds of ideas and assumptions, little attempt was actually made to establish or critique the relations that were assumed, and this in turn, was reinforced by a reluctance to go beyond standardised tests of readily available datasets. This highlights how the economist's skillset can be an impediment to empirical work, since testing data is not the same as seeking out all available *evidence*, some of which only comes into view if one is prepared to think in terms of a range of methods and sources – for Piketty that was many different types of tax record (see Pressman, 2015). Mainstream economics has subsequently been required by circumstance to address inequality. However, its prior invisibility – and focus

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<sup>8</sup> I would note here that there is limited scope in the general argument that all attempts to explore the world are in some way “models”. This does not properly engage with what is specific to the way economics models, and so is either disingenuous, trivial or misleading. The way Mäki argues sometimes seems to beg such issues, partly due to the way metaphysics and ontology are treated (a point made by Dennis Badeen).



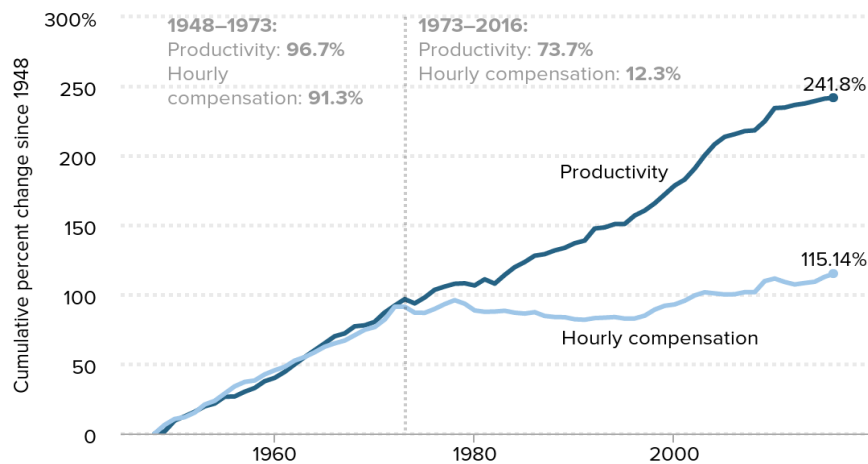
on incentives to individuals and firms – meant in effect that the most powerful social science discourse acted to reinforce growing inequality, since it was to economics one would look for argument and evidence regarding it as a possible problem.

And to be clear, despite new focus on it, mainstream economics has still not fully reconciled to inequality as a problem. For example, mainstream theory does not usually differentiate between different social groups in terms of the impacts of distribution, so it provides no theorisation of the real effects of inequality, including the benefits of raising wages for targeted multiplier effects or their retardation through “hysteresis” effects. Mainstream theory still tends to assume that it is education, skills and competition that determine wages, thereby presupposing a particular account of competitive markets and the market mechanism. This is despite that the value of education and skills, and the nature of competition, are dependent on the rules of bargaining domestically, and the rules of trade internationally. Changes in these provide the actual explanatory grounds for the institutional effects that create the scope for inequality to rise (Morgan, 2015).<sup>9</sup>

The same problem holds for the other main facet of theory, which is that the fundamental engine of income growth is productivity growth. This may well be its context, but it is one subject to distribution. In most wealthy countries productivity and wage growth have been divergent for decades, and nowhere is this more evident than in the US:

### The gap between productivity and a typical worker’s compensation has increased dramatically since 1973

Productivity growth and hourly compensation growth, 1948–2016



**Note:** Data are for compensation (wages and benefits) of production/nonsupervisory workers in the private sector and net productivity of the total economy. "Net productivity" is the growth of output of goods and services less depreciation per hour worked.

**Source:** EPI analysis of Bureau of Labor Statistics and Bureau of Economic Analysis data

Updated from Figure A in *Raising America's Pay: Why It's Our Central Economic Policy Challenge*

Economic Policy Institute

<sup>9</sup> Which arguably is why much of the effect is observed – the statistical differences – within states rather than between states. Every country where common policy tenets apply may experience wealth and income concentration even as some benefit more than others from globalized relations – uneven accumulation and its wage effects may also serve to equalize the distribution of income and wealth among states.

So, there is something partial in the blithe statement that if labour wants higher wages in the near future it must *first* deliver higher productivity in order for this to be affordable. This assumes a fixed relation between wages and productivity and a fixed line of causation for any wage effect (based on a definite relation to, for example, “human capital”). This simply ignores the role of mechanisms that affect distribution. Or perhaps more accurately, this makes it a problem to be modelled in ways restricted by mathematical expression – which has been the case in contemporary “matching” models. The existence of divergence establishes that fixed relations have not applied. Moreover, the way in which it has not been the case has had political consequences. CEO pay is nominally performance based, but has grown in ways that grossly exceed any fixed relation to metrics of corporate activity (which has not prevented attempts to justify top-layer pay, for example, Gabaix and Landier, 2008, which re-specifies Rosen’s 1981 work on “superstars”; and contrast this with Bolchover, 2010).

Populist anger has many sources, but one of them is likely the sense of injustice that is provoked by changes in the world that economics has facilitated and then failed to address. To reiterate, the invisibility of inequality has subsequently become a problem field mainstream economics has not fully reconciled to. Things that *can* be explored are *not* and in so far as they *are*, they are deformed. This, as should be clear from the above, has ideational content and consequence that exceeds the formal statement of theory. It involves implicit principles of what is just (paid what one is worth). Moreover, it involves theory-driven assumptions about foci for research based on what variables are important mechanisms for progress in income and wealth effects, which are manifestly misleading (the dominant picture of relevant mechanisms and processes seems thus inadequate).

Early on I noted that few mainstream economists would feel the need to respond to accusations or claims that they are neoliberal ideologues relying on positivist philosophy of science. In fact, research tends to indicate many economists (in Europe and the US) would identify as politically liberal in the US sense of the term, meaning they consider themselves socially progressive, centrist or left leaning. This is not irrelevant, but this is not the most relevant way to position mainstream economists. They may not be ideologues (some are, but that is true of any field), but economics has form and function, it has ideational content, absence and consequence.<sup>10</sup> The focus and practice of the profession occludes this and decentres it, since economists are not encouraged as economists to be concerned with this *normativity*. Its lack affects the empirical field.

### **Thought closure as normative consequence: money creation and the ideational positioning of finance**

In *The Narrative Fixation in Economics* (2016) Edward Fullbrook argues that knowledge does not just become ideological by virtue of content and framing, but also by the absence of alternatives. If there is more than one way for a problem, issue or phenomena to be positioned, conceptualised and explored, but only one is conveyed then the capacity for any given position to be constructively critiqued or questioned is reduced, and this is to the detriment of potential progress over and above any given position. This too is a normative issue with normative consequences. It begins with pedagogy. Presenting one position when

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<sup>10</sup> Note: this point is not intended to under-emphasize the importance of those who have had the specific intent of transforming the ideational landscape. The point addresses the vast majority of the many 1,000s of economists, not the role of Mont Pelerin, the willed construction of a Washington Consensus etc. The two are, in many ways related, but this does not speak to the self-understanding of the majority.



several exist conveys the impression that the norm is one position. The aim is to achieve one position (unity), and the existence of or seeking for alternatives lacks legitimacy or relevance. This is different than mere disagreement within a broad position on fundamentals. It is a kind of meta-norm of what is the status of knowledge based on the nature of the world to which it is supposedly directed. It is based, arguably, on a misrepresentation of natural science, specifically physics.

As Fullbrook notes, physics has not made progress by closure, but rather by a series of open framework developments, and where physicists have been prepared to maintain an open mind and adopt or consider different frameworks. Shifting between fundamental theoretical perspectives does not cause physicists to collapse in a state of existential angst or confusion, and nor should an equivalent in the social sciences. Though some in physics seek unity they accept that the possibility of this presupposes a prior and for-all-intents-and-purposes permanent *openness to* alternatives. Economics' physics envy combined with paradigm policing would thus seem to be a misapprehension of form and practice.

Conversely, a lack of openness, is an invitation to indoctrination, not an encouragement of critical thinking for problem solving. Disciplines, of course, require standards, but this is different than disciplining a discipline to impose a single position. This is the closure of minds. It leads to highly conservative approaches to development within a discipline, and thus to exactly the kind of slow diffusion of change that I have already noted characterises mainstream economics. The vulnerability of economics to this kind of problem is exacerbated by the elimination of philosophy, methodology and history, and by the focus on methods within economics peculiar "objective" framing. If the field heavily emphasises technical skillsets then there is a tendency to teach through didactic demonstration followed by confirmation through practice. The mindset is "accept the basis of this way of doing things and then everything else follows", "concentrate on mastering the skills". Arguably it is due to the conformist mindset cultivated in economics that there is often little resistance to the (from any other point of view) weird substance of many core assumptions and concepts in economics.

Consider the problem of money. Money is of central importance to any modern capitalist market economy. Yet it is mainly sociologists, philosophers and dissenters that have maintained an interest in what money "is" with a view to continued critique and development (see Ingham, 2004, 2018; Lawson, 2016; Searle, 2017; Peacock, 2017). One might think this is because economics has already provided an agreed clear concept of money. But this is not the case. Contemporary economics defines money in terms of function (unit of account, store of value, medium of exchange), but puts aside both the actual history of money (after an origin *story*) and the conceptual problem of money, both of which likely affect the functionality of money in the broader sense of its role and consequence in real systems. Mainstream economics has tended to treat money in a highly problematic way, which, arguably illustrates Fullbrook's key point.

What appears weird to those outside of the mainstream is that in economic theory in general money is typically *absent*. It is usually assumed that in a properly functioning market system prices express the value of output such that all prices effectively become representative of ratios between goods and services (and inputs), and this ultimately means a market system operates as though it were barter. Money simply becomes the convenient symbol (in its medium of exchange guise) that expresses these ratios. As such, it has no independent significance, and one ought to look through money to the operation of "real" economic factors, and can in effect ignore money as a contributory, contextualising or significant component in a

system. This is except in so far as monetary policy leads to “monetary disorders” (well-meaning governments and central banks push too much money into an economy, and this has no long term effect on “real” factors but does result in inflation, so the role of monetary authorities is primarily to prevent excess).

The point, however, is that the role of money in real systems has generally been peripheralised because of an arbitrary limitation created by the assumption that money is separate from and then circumspectly significant to “real” factors. This statement may seem odd to a non-economist, since we live in a world where monetary policy is high profile, and a great deal of attention is paid to central bank policy (inflation targeting for price stability), and to the existence and activity of banks. This is different than whether money itself is a well-conceived concept, in economic theory in general, and then in central bank and financial theory in particular.

Within the economics of the finance system little attention is paid to how money is *created* over and above the role of the state in facilitating the printing/minting of money and central banks in terms of reserve system management. It is simply assumed that money exists and it is used. For decades undergraduate economics textbooks have taught what is termed the money multiplier, where banks can collectively but not individually create money (subject to a mathematical limit expressed as the reciprocal of the reserve ratio). However, this position is not taught to postgraduates and is not generally endorsed by economists who work on banking and finance.

At this level, the general position is referred to as intermediation of loanable funds (ILF). ILF does not actually address how money is created. Instead, it assumes that banks are intermediaries who gather the savings (surplus) of citizens and corporations, pool them, and then lend the pool to borrowers. An interest rate spread exists based on the difference between what they must pay savers to use their savings and what they charge borrowers in order to provide their service (which is essentially a risk management and maturity transformation function). ILF implicitly represents banks as a beneficial social utility providing a vital service that underpins productive investment in the economy; one, moreover, where banks, based on a profit motive and risk assessment, are efficiency enhancing; risk-focused credit management leads to best use of available capital. Any deviation from this is a matter of information asymmetries creating distortions in credit channels, but the norm to which the system tends is credit and capital efficiency, and the positioning of the role and consequences of banks is positive by default.

Clearly, this has significant ideational content in terms of how banks are positioned. One might consider this positioning ideological, since the positive representation of banking conveys an impression, and the essential implication is that there is a “normal” of efficiency that banks can be returned to. It might also be categorised as ideological because alternatives exist that are not taught and are not part of mainstream economics. That is, varieties of credit creation theory (promoted by post-Keynesians, modern money theorists etc). This may partly be because the problem of what money “is” is not reconciled to the basic characteristic that money involves credit-debt relations. Moreover, the dominance of ILF continues despite that empirical evidence tends to suggest that banks create money *ex nihilo* each time they extend a loan (though this has limits and is not equivalent to infinite scope), and despite that central bank researchers increasingly recognise this (even if central bank governors etc often do not; see Kumhof and Zoltan, 2015; Mcleay, et al, 2014).

The difference between ILF and credit creation is important, and the failure to teach or discuss the latter is of equal importance. Consider the difference it makes. *If* banks create money each time they extend a loan and banks are *not* dependent on savings in order to do this, then money creation is instantaneous and discontinuous. It is thus difficult to forecast and control for central banks, and banks themselves are more important socio-economic actors than they appear (and they are already manifestly important). If banks are not primarily intermediaries they are instead financiers, and given they are, under this description, the main source of money creation in an economy, what money is created *for* is highly significant. Money gives banks transformative capacity; that is, power.

There is no reason to assume that money is created and lent for productive purposes (since banks are interested in profit, not social welfare, and an evolving finance system creates scope for profit to be made from lending for and against financial assets). Given also that banks are able to influence the environment in which they operate, there is no reason to assume that their consequences will be efficiency enhancing, even as an emergent unintended consequence, since expanding lending may mean following bubbles in financial assets, and banks can operate on the basis of shifting risk through a system of ever greater complexity that banks are helping to evolve (for example, via originate and distribute lending strategies, securitisation and shadow banking). These are significant concerns for regulators, even with a dominant ILF position, but the whole looks quite different if there is no “normal” to return banks to.<sup>11</sup> The alternative opens up very different ways of exploring banking and finance, such as financialization, something which sits awkwardly with ILF, where the term financial deepening is more common.

So, what have I suggested? Norms inhere in knowledge, and normativity in debate is promoted or suppressed by the plurality or lack in what is and is not taught.

### **Conclusion: non-conformity and disagreement**

Mainstream economics has been the subject of widespread criticism in recent years. The focus of that criticism is relatively new (failure to forecast and respond adequately to the global financial crisis etc.), but the themes are longstanding. Economics has become less concerned with realism and with real world problems.<sup>12</sup> As a stand-alone statement this seems absurd, and yet if one considers how economics develops, it has become oddly introverted, and this has affected the way economics engages with the world. Economics has normative content and consequence, but in a misplaced and misunderstood manner.

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<sup>11</sup> According to Rethinking Economics recent ‘33 Theses’: “24. The majority of new money circulating in the economy is created by commercial banks, every time they make a new loan. 25. The way in which money is created affects the distribution of wealth within society. Consequently, the method of money creation should be understood to be a political issue, not merely a technical one. 26. Since banks create money and debt, they are important actors in the economy, and should be included within macroeconomic models. Economic models that do not include banks will not be able to predict banking crises. 27. Economics needs a better understanding of how instability and crises can be created internally within markets, rather than treating them as ‘shocks’ that affect markets from the outside. 28. Financialisation has two dimensions: short-termist and speculative finance, and a financialised real economy. The two problems must be studied together” (2017: p. 4).

<sup>12</sup> In some discourses in order to avoid conflation it can also be important to distinguish realism (the ontological approach within philosophy) from “realisticness” as a facet of specific theory and claims (a point that Mäki is concerned by and Dennis Badeen explores).

The claims I have made here are not ones that many mainstream economists are likely to accept. But as I have noted, there are entirely understandable reasons for this. At the same time, there are *good* reasons not to be bound by this. Economics is too important to be left in its current state. It is in need of genuine plurality, rather than conformist (sub) diversity. It is in need of philosophy, methodology and history as much as mathematics and statistics. It is in need of frameworks that discuss normativity, rather than stratagems that suppress, repress, deform, condone or circumvent norms. It is in need of deliberative engagement *with* the world rather than an objectification of that world.

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