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The counter-revolution of criminological science: a study on the abuse of reasoned punishment

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Abstract: Trends in the history of social science dedicated to the study of crime and punishment are presented as a case study supporting F.A. Hayek’s theory of social change. Designing effective social institutions and public policies first requires an accurate vision of how society operates. An accurate model of society further requires scientific methods uniquely suited for the study of human beings as purposeful agents and the study of human institutions as complex social phenomena. If guided by faulty methods, theories are inaccurate and policy outcomes veer from their intentions. Hayek termed such outcomes “abuses of reason”. Aiming to replicate the objectivity of physical sciences via formal modeling and statistical measurement, economists throughout the 20th century imposed an excessively technical vision of human decision-making. Policy failures and social problems resulted. This paper argues that the historical trends of applied social science dedicated to crime and punishment can be understood similarly. Formal modeling and statistical measurement continuously displaced methods more attuned to human intentionality and social complexity. In result, amidst a long-run history of intellectual and political change, US law enforcement and criminal punishment policies became technocratic, and outcomes became disjointed from their stated intentions to promote social order and welfare.

Keywords: criminology, punishment, scientism, methodology, Hayek

JEL Classification: B15, K42

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Discipline in general, like its most rational offspring, bureaucracy, is impersonal. Unfailingly neutral, it places itself at the disposal of every power that claims its service and knows how to promote it. —Weber (1946, 254)

I. INTRODUCTION

In recent years, US criminal justice policies and outcomes have arisen deep concern. Extreme growth, fiscal unsustainability, and racial disparity are widely recognized features of the US prison industrial complex. Overcriminalization, heightened sentencing, police militarization, and the excessive use of force are all additional areas of concern. While it is well understood that these contemporary trends have deep seated political, cultural, economic and historical roots, such trends cannot be fully explained with references to real crime rates or other features of US exceptionalism. The need for reform is popular amongst experts and citizens alike. But, the question remains: Why are such outcomes so entrenched and difficult to reshape? What particular reforms will move towards more socially desirable results? And how can such transitions be effectively implemented?

I argue that this tension between the perceived needs for reform on the one hand, with the inability to substantially reshape outcomes on the other, can be at least partially attributed to methodological patterns in the social sciences dedicated to understanding crime and punishment. There is a lack of conclusive explanations for the causes and consequences of crime, as well as an incomplete understanding regarding the potentials, and limitations of different law enforcement and punishment strategies. This theoretical lacuna is not merely for lack of good science, but rather stems from certain shortcomings in the professional, academic, and political environments within which social science is conducted. Methodological orthodoxies therein elevate and insist upon quantitative analyses at the expense of qualitative, descriptive, and comparative alternatives. When in fact, such latter techniques are needed to recognize and cope with those features of our current malaise that specifically stem from the unintended consequences of policy failures. Hence, amidst such methodological trends, error becomes a self-fulfilling prophecy and the potentials for effective reforms narrow.

This analysis draws heavily from F.A. Hayek’s (1952) comments surrounding the similar methodological fashions of the economics
profession amidst the latter part of the 20th century and fits compatibly with Feeley and Simon’s (1992) thesis that actuarial standards reshaped the aspirations of prison managers away from rehabilitation and towards more narrow goals of internal behavioral compliance. When scientism insists upon formal modeling and quantification, political incentives promote strategies and policies that conveniently attend to such standards, thus displacing and, at times, suppressing alternative methods that better appreciate and accommodate processes of institutional innovation and experimentation.

In short, criminal justice failures persist, and reform efforts continually fall short, because the conceptual frameworks that are used to understand crime, punishment, and their surrounding public policies and social outcomes are not fully attuned to the factors that shape such problems. Nor are they well geared to recognize the potentials of certain social processes that may be needed for the discovery and implementation of effective reforms. To some degree, criminal justice failures require genuinely innovative thinking and actions to discover and implement preferable outcomes. Not only do political structures suppress incentives for social entrepreneurship, but the professional, academic, and political establishments also reinforce status quo frameworks and techniques against alternative conceptual approaches. As evidence of this claim, I survey methodological trends across a long swath of criminological history. The criminological sciences endured similar methodological trajectories as Hayek (1952) described amongst the economics profession of the 20th century.

It is difficult if not impossible to fully link the detailed global trends of criminal justice policies and outcomes onto any individual theory of social change. Full and accurate data sets regarding the internal operations of criminal justice systems and outcomes, be they quantitative or qualitative, are simply not available for a large sampling of under-developed nations around the world. Stylized empirics of criminological phenomena are perhaps inherently more difficult to obtain relative to those related to other social processes. I choose to focus on how long-run methodological trends of social science can be seen to relate to similarly long-run criminal justice policies and outcomes in the American experience. Hence, the analysis herein should not be seen as fully explanatory of the US experience, nor is it necessarily applicable beyond the US context. My claim is merely that the long-run trajectory of US policy history preceding the morass of
contemporary criminal justice failures and the persistent incapacity of effective reforms were at least partially shaped by and related to methodological trends within the social sciences during that same long-run history.

Early in America’s founding, criminological perspectives drew heavily from the classical school of social science wherein methodological standards focused upon the behavioral conditions of rational decision-making, the coordinative potentials and limitations of inter-individual and group behaviors, and comparative institutional analysis. Hence effective law enforcement and punitive strategies were thought to be a function of matching the unique social conditions and resource constraints of a community with legal procedures reflective of local norms and values. Such was ultimately a challenge of social epistemology. By what institutional processes and mechanisms could communities discover and convey their preferences and knowledge sets regarding criminal justice conditions and resource allocations? The early strength of Federalist checks and balances thus promoted criminal justice institutions that conveniently preserved a large degree of local autonomy. Hence, the early dispersed townships of the American colonies and frontier spaces existed within relatively competitive institutional contexts. The technological and authoritative gap between local law enforcement authorities and individual citizens was small, and the potentials for citizen to exit local jurisdictions in favor of alternative communities were relatively high. Such conditions certainly had their limitations and imperfections but they tended to conveniently avoid many of the types of failures endemic to modern systems.¹

Later in US history, methodological trends in social science took a more empiricist turn and thus complemented political demands for interventionism. Criminal justice transformed from a largely dispersed and informal institutional network of different local townships and independent administrative units into a more formally hierarchical system unified by overt intentions of social control and improvement. Law enforcement and correctional institutions were newly financed, operated, and aimed towards deterrence, incapacitation, and rehabilitation. Such in turn, provided new opportunities and demands

¹ An obvious caveat should be made at this point regarding slavery. Though a full analysis regarding the inter-relationship of criminal law across recognized citizens and subjugated populations is needed, it is far beyond the scope or aims of this paper. For present purposes this paper reserves its focus to the scope of criminal law presiding over only classes of recognized citizens.
for a more quantitative approach to the social science of crime and punishment.

As quantitative data became more reliable and available, theories and techniques followed in stride. Formalistic approaches further complemented political demands for a more professionalized law enforcement system. Larger and more empowered governmental bureaucracies and an increased role of federal—relative to state, or local authority—supposedly provided unique vantages and opportunities for implementing more formalized and quantitative approaches of social science. Law enforcement agencies and officers became less directly assessed by local citizens with regard to their promotion of social order or their effective promotion of community norms. Instead, technical approaches narrowed in upon tangible and measurable performance proxies like reported crime rates, operating expenses, and recidivism rates. Decision-making within criminal justice systems became more bureaucratized and governed by political processes. Overt commitments to the tracking and targeting of such variables thus displaced potentials for alternative institutional innovations through social experimentation in so far as new proposals carried uncertainty regarding their relationships to standard proxies. Thus, professional academic and political outlets for applied social science placed low value on qualitative and comparative methods. Such methodological and practical legacies have narrowed the realm of considered strategies for crime control to typically expansionist and centrally managed proposals. Increased budgets, more technologically sophisticated police weaponry and equipment, tougher laws, bigger prisons, and longer sentences are all obviously incentivized under an ever growing bureaucratized system, but they are all also direct implications of the most dominantly entrenched mental models of social science dedicated to crime and punishment.

The remainder of this paper is organized as follows. Section II explains Hayek’s theory of social change foundationally driven by methodological trends in the social sciences. I argue that similar trends can be seen throughout the dedicated social science treatments of crime and punishment. Sections III to VI survey various schools of applied social science on crime and punishment popular and influential during US history: section III surveys the classical school, section IV the positivist school, section V the Chicago school, and section VI the most recent approaches to researching crime and punishment. Each
framework's methodological and topical traits can be seen as counter-revolutions from previous perspectives. Stemming from methodological dilemmas, criminal justice institutions historically grew unreasoned from their stated social functions. Section VII concludes.

II. HAYEK'S THEORY OF SCIENTISM

Beneath the research heading, “on the abuse & decline of reason”, Nobel economist Hayek wrote, “it is human ideas which govern the development of human affairs” (1952, 3). But, for Hayek, social change was more complex than the cliché adage ‘ideas matter’. Rather than positing social trends as the direct result of populist ideologies, Hayek emphasized the role that scientific methodology plays in shaping social outcomes. If ideas drive society, then the procedural origins of ideas matter foundationally. How scientists formulate theories will shape their accuracy and in result, shape real-world consequences. Bad methods lead to bad theories, bad theories lead to bad policies, and bad policies lead to bad outcomes.

Concerned about the challenges of subjective bias, 20th century social scientists—especially economists—adopted formal modeling, hypothesis testing according to statistical measurement, and predictive forecasting. Hayek argued that this trend lost sight of the purposeful nature of human decision-making and thus truncated the treatment of volitional human actions and the formations of complex social institutions into a series of programmable mechanic operations. Machined visions of economic exchange overly simplify the otherwise subtle and complex processes of real human decision-making and social cooperation. Hence, legislating public policies informed by such models carried unintended consequences for social welfare.

Hayek thought many of the social problems of the 20th century stemmed foundationally from a methodological bias within the economics profession. Social scientists writ large suffered the ill of ‘scientism’—a general obsession with imposing the techniques of the physical and natural sciences onto the subject matters of human actions.

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2 The counter-revolution of science was originally published in Economica in 1941 and subsequently with additional material in a book-length treatment under the fuller title, The counter-revolution of science: studies on the abuse of reason (1952). Individualism true and false was a lecture delivered in 1945 and published shortly thereafter within the text Individualism and economic order (Hayek 1948). These materials have been combined, as originally intended, in Hayek (2010).

3 Hayek (1949) also attempts to explain sociological issues of professional intellectuals and their influence upon ideological and social change.
and social operations. Lightning does not result from the anger of the gods. But, when social scientists purged normativity by embracing formalism, they also inadvertently discarded some necessary techniques for comprehending the distinctive features of their subject matter. Deduction and natural language are perhaps crucial techniques for appreciating and conveying the unique purposefulness of human choices and the socially coordinative operations of institutions. In contrast, formal mathematical models must simplify broad swaths of diversely motivated choices into definable and measurable units. However, economic policy cannot be understood anathema to the preferences and choices of the human beings living amidst their operation without consequence. Designing economic policy to blindly chase statistical proxies can result in production schemes that are, in fact, out of sync with social welfare. As development economists often quip, ‘you can’t feed a family with GDP growth rates’.

There is a meaningful difference between the challenges of scientism and merely bad science. Bad science can be recognized as under-specified models, imprecise or inaccurate data, and/or flawed experimental designs. The conclusions of bad science cannot be trusted, but bad science can be corrected and improved upon. Furthermore, the scientific process is, in fact, aimed at the rooting out and correcting of bad science, but it is perhaps less well-alert to the needs of rooting out scientism. Better data and better models allow for better science. Only when such empirical methods appropriately capture and convey the essential features of the subject matters that they aim to explain, does good science inform good public policies.

In contrast, addressing the challenges associated with scientism is more complex, as scientism is a sort of cultural trend within the professional academic and political arenas. Scientism occurs when the formalistic and quantitative methods of the physical sciences are demanded and insisted upon within the social sciences irrespective of their inabilities to fully explain or predict human and social phenomena. No individually designed or constructed model, no matter how precisely

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4 This is not to say formalism has not contributed to social science; merely that trends in data, technology, political and professional interests all influence the popularity and dominance of methods among researchers through history. In so far as policies were shaped by misapplied theory, social problems result and persist. Nor is it the case that comparative and procedural analyses are impossible amidst professional formalism. Boettke, Coyne, and Leeson (2003) point out the rise of analytic narrative (Bates et al. 1998) and new institutional economics (North 1990) as hopeful trends returning natural language methods and universal topics to the economics profession.
defined or informed by granulated empirics, is capable of fully reflecting the uniquely complicated and subjectively varying elements of human psychology and purposive behavior. Proxy variables are just that, proxies. They are not direct windows into the community’s collective conscience or consciousness. Hence, formal modeling of society within an atmosphere of scientism creates orthodoxies hostile to processes of institutional experimentation and structural change. In so far as genuine social entrepreneurship is needed to discover and implement preferable institutional arrangements and policies, social problems foment and persist amidst scientism.

The problems of scientism most often occur at the intersection of science and public policy. If we accept the basic premise that individuals behave rationally in at least a bounded sense (Kahneman 2003), then it is also reasonable to extend this assumption onto policy makers and political authorities. Hence, political authorities are likely to prefer those methods and conclusions that most insulate and promote their own interests, unless otherwise constrained through constitutional checks and balances (Brennan and Buchanan 2000). Hayek argued such was the relationship between economic science and public policy, as formalism and quantitative analysis promoted political desires to expand centralized control over the economy (Hayek 1935).

Akin to Hayek’s model, Boettke, Coyne, and Leeson (2003, 2) offer a visual outline to map trends in economic thinking. Methods ranged from formal modeling to natural language; topics from particular case studies to universal theories. Figure 1 graphs these dimensions and places schools of thought within their respective quadrants.

“[T]he movement in economic thinking is composed of four competing visions” (Boettke, Coyne, and Leeson 2003, 1). The first (bottom right quadrant) “verbal economic analysis [...] maintains the universal nature of economic propositions” (2). Economic decision-making is seen as a consistent sub-set of a broader and universal vision of human action and such theorizing is conducted in natural language methods of deduction and comparative analytics rather than formal modeling and statistical measurement.
Trends in Economic Thinking

The second vision (bottom left) shares natural language methods, but “it is believed that economic truths revealed through study are merely particular” (2). There is little to no effort to fit economic analysis within a consistent or universal vision of human decision-making or social operations. Each social context may or may not exhibit its own unique set of causal properties and explanatory principles. In the third (upper right), “the mode of exposition is purely a formal one of mathematical modeling and statistical testing” (2), but it retains a universal element wherein all human actions can be modeled in mathematical terms and such propositions can be tested against quantitative data. The fourth (upper left) uses formal modeling to investigate multiple equilibria rather than universals. Unique contexts of behavioral patterns accord to unique arrangements of mathematical formula, but typically no unifying principles necessarily align these diverse observations and applications.

Boettke, Coyne, and Leeson (2003), like Hayek, see the greatest potential for theoretical accuracy in the social sciences from the first vision. Social processes must be understood within a broader, consistent, and accurate theory of society. In turn, this social theory must rest upon an accurate understanding of human decision-making. A sound theory of human behavior requires a methodology appreciative of the uniquely human features of human decision-making: purposeful
choice, subjective evaluation, and strategic interaction. Unfortunately, such facets are difficult to capture or convey with statistical aggregates. For example, public policy can too narrowly focus on minimizing unemployment at the expense of other individual or societal aspirations. A bridge built primarily for the sake of job creation risks being a bridge to nowhere (Hazlitt 1946, 32). Similarly, if law enforcement resources are arranged to merely chase arrest rates, clear cases, promote the special interests of police unions, or create jobs in a local economy, they too can work against the more complex needs for peace, security, and social order. Natural language methods are argued to be better equipped at capturing the procedural and adaptive operations of individual and social processes, as formal techniques—especially those deployed amidst the 20th century—imposed a relatively static vision of equilibrium conditions.

In this view, correcting the failed outcomes of policy trends foundationally depends upon correcting the methodological trajectory of the social sciences. We cannot resolve social problems, if we do not first correctly understand their causes. Furthermore, we cannot correctly understand how society operates if we do not have the right scientific methods and analytical tools to investigate the unique and distinctive features of human actions and human society. Hence, correcting the trajectory of methodology within the social sciences depends in part upon relaxing professional and political orthodoxies surrounding scientism. New techniques must be allowed to enter the debate and attempt to arrange the accumulated research and findings into coherent theories that challenge and contest the policy inferences drawn from more mechanical approaches.

I argue that a swath of social problems surrounding the US criminal justice system can be seen as a partial by-product of a process of abused reason akin to Hayek’s descriptions of scientism within economics. In so far as the intersections of social science and public policy writ large suffered from the challenges of scientism, so too did focused applications of social science on the topics of crime and punishment.5

5 These patterns have been noticed in other applied topics as well. For one example, Boettke and Aligica (2009) explain that Ostrom’s (Blomquist 1992; Ostrom and Cole 2011) research in urban planning paralleled the theoretical structure of the socialist calculation debate. Scholars equated centralization with efficiency by presumption. Taubes (2007; 2010) argues similar issues plague discussions surrounding public health and nutrition. Easterly (2002; 2007) suggests that foreign aid programs endure systemic failure from models lacking variety and complexity in development.
Until the early 19th century, the ‘classical school’ of criminology was largely comprised of political economists who sought to understand crime and punishment within a broader and consistent model of society (akin to the first vision in the bottom right quadrant of figure 1). By the early 1800s, criminology was shifting away from natural language methods towards empirical formalism. Akin to the economic historicists (the second vision within the bottom left quadrant of figure 1), positivist criminology developed theories of biological and social determination. In reaction to ineffectual rehabilitation efforts (Martinson 1974; Lipton, Martinson, and Wilks 1975), the 1960’s Chicago school re-introduced rational models of criminal behavior and also eventually embraced mathematical formalism and quantitative analysis (akin to the third vision in the upper right quadrant of figure 1). In the late 20th and early 21st century, the applied studies of crime and punishment have taken form as quantitative case studies of particular times and places (akin to the fourth vision in the upper left quadrant of figure 1).

### III. THE CLASSICAL SCHOOL

The classical school of political economy possessed unique methodological features relative to later theoretical traditions. Methodological individualism, rational choice, and comparative institutional analysis provided a unique vantage from which classical authors could recognize processes of social order as stemming from spontaneous, decentralized, and unregulated processes. Furthermore, the classical model was keenly equipped to identify the scope of social problems that stemmed from the unintended consequences of failed constructivist policy efforts. This section demonstrates the methodological similarities between the classical school of political economy and the classical school of criminology. Such is a relatively straightforward task as much of classical criminology is in effect a retroactive composition of commentaries provided by classical political economists that happened to touch upon the subjects of crime and punishment.

Hayek (1948; 1973) distinguished two threads of individualism within the classical school of political economy.6 "True individualists"

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6 The classical school emerged amidst the Scottish enlightenment more generally, dating to the latter 17th and 18th centuries.

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6 Economics. Coyne (2008; 2013) tracks failures of imposed plans in post-war reconstruction and international policy.
viewed society as an evolutionary process, "the result of human action but not the result of human design" (Ferguson 1996, 187). Hayek (1948, 4) lists Locke, Mandeville, Hume, Tucker, Ferguson, Smith, De Tocqueville, and Acton—who all sought to understand how interacting individual choices spontaneously contribute to social order. What are the universal laws of human action, how do they operate, and by what methods can they be identified and understood (Barry 1982)?

In contrast, Hayek (1948, 4) termed constructivist rationality, a form of “false individualism”. Descartes and Rousseau spawned the trend and their influence upon Mill and Spencer caused later confusions. To constructivists, individual behavior is universally rational—mechanically so. Such mechanization is arguably the avenue through which methodological formalism eventually took hold amongst the constructivists. Conscious human intention is the predominant drive behind social order, but in this view, mankind has a responsibility to actively promote and impose rational social conditions through institutional designs and control. Hence, formal modeling and measurement become presumed techniques for improving the accuracy and effectiveness of public policy. Bentham’s (1907) utilitarianism expressed in formal policies of panopticism is an obvious example. Better technologies of observation and isolation presumably lead to better rates of behavioral compliance.7

Perhaps no subject captures the tension between these competing individualisms as well as their respective members’ comments surrounding crime, punishment, and incarceration. Comparative historical research using natural language methods led ‘true’ individualists to recognize structural relationships between enforcement regimes on the one hand, and crime and punishment outcomes on the other. These individualists supported the adaptive and evolutionary features of the common law, favored decentralized authority through constitutional checks and balances, and opposed torture and corporal punishments.8 In contrast, constructivists sought to impose quantitative

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7 Hayek (1973, 22, fn. 28; 1948, 4) explicitly mentions Tocqueville as preferable to Bentham.
8 D’Amico (2010, 465) surveys, “John Stuart Mill, though concerned about protection against force and fraud, did not conclude an absolute role of government authority, ‘people might be required to protect themselves by their skill and courage even against force, or to beg or buy protection against it, as they actually do where the government is not capable of protecting them’ (Mill 1982, 800). Perhaps Mill’s assessment of prisons resulted from his own observations. ‘Nothing is done to make the prisoner
and predictable influence upon crime and punishment for the supposed betterment of society. Bentham’s panopticon designs popularized incarceration as the now standard form of criminal punishment worldwide. Constructivist proposals were more compatible with and appealing to political desires and thus dominated policy and research trends thereafter.

Smith, Beccaria, and several other individualist economists⁹ are also recognized as founding members of the classical school of criminology (Cullen, Agnew, and Wilcox 2002), which situated the study of crime and punishment within a broader effort, “to understand the forces which determine the social life of man, and only in the second instance a set of political maxims derived from this view of society” (Hayek 1948, 6). In other words, support for laissez-faire economic management was not motivated by normative claims to natural rights per se. It was instead a positive inference stemming from the recognized consequences of interventionism. A similar humility regarding the potentials of public policy and a similar focus upon procedural checks and balances characterized true individualist commentaries on crime and punishment.

Individualists viewed the functional aspects of society as the bi-product of unconscious trial, error, and effective endurance. “Man has achieved what he has in spite of the fact that he is only partly guided by reason, and that his individual reason is very limited and imperfect” (Hayek 1948, 8). Those institutions reacting to crime via formally designed law enforcement regimes and punishments are obviously shaped by direct rationality and conscious effort. Thus, any individual program is likely shortsighted, narrowly focused, and prone to errors and imperfections. However, classical thinkers saw the degree to which one law enforcement regime succeeded relative to alternative strategies, more as a consistent bi-product of spontaneous social operations. Preferable criminal laws and enforcement techniques were discovered, implemented, and selected over alternatives through time and across different regimes as part of a long historical process entailing social experimentations and institutional evolutions.

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⁹ Francesco Carrara and Enrico Pessina are lesser-known affiliates. Montesquieu (1989) and Chadwick (1829) are also included.
Beccaria (1995) writing in *Of crimes and punishments* explains the limited influence of constructivist rationality in criminal legislation and jurisprudence:

Our knowledge is in proportion to the number of our ideas. The more complex these are, the greater is the variety of positions in which they may be considered. Every man hath his own particular point of view, and, at different times, sees the same objects in very different lights. The spirit of the laws will then be the result of the good or bad logic of the judge; and this will depend on his good or bad digestion, on the violence of his passions, on the rank or condition of the accused, or on his connections with the judge [...].

Each individual, who will always endeavour to take away from the mass, not only his own portion, but to encroach on that of others. Some motives therefore, that strike the senses were necessary to prevent the despotism of each individual from plunging society into its former chaos. Such motives are the punishments established, against the infractors of the laws (8-9).

Smith (2009) similarly saw the functional aspects of criminal law and enforcement as evolved from imperfectly motivated inter-individual behaviors.

The revenge of the injured which prompts him to retaliate the injury on the offender is the real source of the punishment of crimes. That which [...] other writers commonly allege as the original measure of punishments, viz. the consideration of the public good, will not sufficiently account for the constitution of punishments (104).

In this classical view, crime and punishment, like all human behaviors, are seen as generally purposeful and responsive to incentives. Though individual choices are reasoned, their intentions are determined subjectively. Individuals may be ignorant, or strive for goals not understood by others. But, in so far as punishments are perceived harmful and costly to criminal actors, increasing the severity of punishment and/or the reliability of its application—like raising consumer prices—should, ceteris paribus, deter criminal behaviors. The ceteris paribus presumption carries a hefty weight of the analytical traction.

This purposeful view of crime does not provide a practical blueprint for punishment strategies within any particular social context, let alone across different environments. Exogenous factors are rarely ever held...
constant in the real world. What types and magnitudes of punishment prove efficient in a particular time and place, requires knowing the varied costs and benefits of criminal behaviors therein, compared to their relevant enforcement strategies and opportunity costs.

Individualists possessed a relatively limited sample of social observations and theories to inform their causal models of crime and punishment, but they were nonetheless early to notice a meaningful relationship between varied enforcement regimes with crime and punishment outcomes. For Smith (1982; 2009) pervasive criminal acts such as “raping, pillaging and plundering” (1904, 25) impede economic development and social order. He thus identified well-functioning legal systems by their abilities to harmonize human intentions through “trucking, bartering and exchanging” (1904, 25).

Specifically on the topic of penal policies, Alexis de Tocqueville's research took a similar comparative approach to Smith and other classical authors. Bauemont and Tocqueville (1833) were initially sent to America, among many other state officials (Johnston 2000), to investigate prison facilities as a potential source to early American exceptionalism. Hence, the structure of their empirical report took form as a survey of criminal legal policies, enforcement patterns, and internal management styles viewed across prison facilities in various townships throughout the early United States. Such methods were clearly mimicked in Tocqueville's more prominently known Democracy in America (1990a; 1990b)—also drafted from research and observational accounts garnered during his initial nine-month prison voyage.

Tocqueville exemplified the individualist perspective regarding his usage and reliance on core assumptions concerning human rationality and purposeful motivations. Criminal agents were seen as motivated by strategic plans, responsive to incentives, and shaped by the complex processes of social cooperation and coordination. Tocqueville also shared the classical concern against excessive punitive authority wielded by state governments. Prisons were only effective in so far as they were small and infrequently relied upon (Whitman 2007). To Tocqueville, society’s level of orderliness, functionality, and prosperity did not primarily depend upon the omnipresent threat of observation or incarceration. Social order was instead more dependent upon a “delicate art of civil association” (Tocqueville 1990b, 895). Though subtle, this

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10 Smith's (2009, 544-590) early reliance on comparative analytics can be seen in his continual juxtapositions relating England to France with regard to policing.
core insight of Tocqueville’s broader social theory is fully intact, albeit less developed, within the earlier and more hastily drafted prison report. Verbal communication and social interaction are the key factors separating the different outcomes across the three core prison models surveyed therein.\(^{11}\)

Individualists, as philosophers of political economy, were active in policy debates,\(^{12}\) but their main focus within such discussions was usually the institutional differences across social contexts. Given the universal features of human decision-making, public policies need to be framed in general terms in order to be effective across varied environments (Hayek 1960, 149). As Hobbes (1996) correctly noticed, when societies cannot mitigate violent conflict they cannot develop. When they cannot enforce property rights and contracts they cannot prosper. Some communities succeed at these challenges better than others. Hobbes believed centralizing state authority was a necessary precondition for social order. Classical individualists did not emphasize the superior enforcement capacities of a particularly centralized regime, but instead highlighted how certain institutions incentivize the social processes of production, innovation, and adaptation therein better relative to others.\(^{14}\) True, violent conflict was a significant barrier to initial social development, but punitive variance simply cannot account for the patterns of wealth observed in the industrial and post-industrial

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\(^{11}\) “[w]hy are these nine hundred collected malefactors less strong than the thirty individuals who command them? Because the keepers communicate freely with each other, act in concert, and have all the power of association; whilst the convicts separated from each other, by silence, have, in spite of their numerical force, all the weakness of isolation” (Bauemont and Tocqueville 1968, 26).

\(^{12}\) Mill (1869) was an early advocate for women's suffrage and opposed capital punishment. Beccaria (1995) expressed strong opposition to torture as a means of interrogation, and Smith (2009) supported habeas corpus as a constraint against excessive monarchical power.

\(^{13}\) Boettke, Coyne, and Leeson (2003) would imply natural language methods are explicitly needed to recognize these forms of generalizable social processes. Weber (1946) referred to the analytic role of human intentionality as verstehen (literally translated 'meaning'), a useful parallel to Hayek's 'reason'. Mises (1957) distinguishes pure historicist descriptions of human behavior from praxeological accounts. The pure historian, akin to pure economist, does not assess the normativity of her subject, nor does she presume phenomena to be governed by singular or external control. Praxeological accounts, on the other hand, insist upon the presumption that agents define their various purposes subjectively.

\(^{14}\) Here again Smith's (1982, 544-590) comments on policing are most revealing as the vast majority of the text is dedicated to describing the functions of the division of labor rather than detailing formal enforcement efforts per se. In Smith's view, a peaceful social environment is most dependent upon a general condition of prosperity and legal equality across citizens to engage in trade—not necessarily a superior strategy of law enforcement.
economies (Boettke, Caceres, and Martin 2012). Political authorities are purposeful and adaptive as well. When afforded the choice, political leaders will select those enforcement policies and technologies that they perceive to best promote their interests, which are not necessarily identical to the public welfare.

When township populations first grew into formal political city-states, ruling authorities found public and corporal punishments useful tools for deterring crime and promoting social order. Prior decentralized and customary legal systems were largely self-enforcing.\(^\text{15}\) Theft and fraud were suppressed without state-sponsored punishments, because potential criminals feared being denied access to market spaces and legitimate courts.\(^\text{16}\) As populations grew, the probability of any individual being detected and brought to trial waned. The severity, frequency, and/or publicity of penalties grew in part to maintain deterrence from crime, but also to promote compliance, retain authority, and expand political power. Goebel (1937) and Benson (1992) suggest the historical prohibition of customary feuds and self-serving violent enforcements complemented state interests for greater abilities to tax, regulate, conscript, and avoid insurrection. Without the enforced threat of exile, outlawry, or legitimate retaliation, the incentives for crime grew.\(^\text{17}\) The transition from isolated American colonies to formal states under the later federalist system was a similar transition from decentralized autonomous units, to more formalized state powers (Rothman 1971).

State sponsored, physically corporal, and publically visible punishments such as scaffolding and flogging signaled severe costs to criminal behavior across later growing populations, hence they simultaneously increased deterrence while supporting authority power, but only to a point (Foucault 1975; Moskos 2011). Tolerance for violent


\(^{16}\) Olson (1971, 1982) argues costs of isolated anarchic living were an impetus behind serfdom organizational forms in early city-states. Sources listed in footnote 15 all describe the effects of customary norms such outlawry, familial feuding, exile and death penalties as sufficient mechanisms for inducing compliance to person and property rights norms and court procedures within earlier legal systems.

\(^{17}\) Benson (1992) surveys a wide swath of legal historians whom all confirm that criminal law developed later than civil law.
spectacles diminished as they were applied to relatively arbitrary sanctions amidst growing proprietary crime rates (Tobias 1979; Beattie 2001; Berman 2006, 606-629). Publicly financed and managed police and prison systems renewed deterrence, appeased puritanical preferences for humane alternatives to scaffolding, and served authority interests in both the British and US context. They also represented a newly concentrated interest in the development and application for dedicated, formalized, and quantitative social science of crime and punishment.

Momentum towards centrally planned criminal justice was further buttressed by quantitative methods of criminal sentencing and applied punishments. Constructivists thought that social order and progress were impossible without the guiding influence imposed by designed legal systems. Hayek (1948) quotes Descartes (1999), explicitly referencing criminal legislation:

[…] nations which, starting from a semi-barbarous state and advancing to civilization by slow degrees, have had their laws successively determined [...] by experience of the hurtfulness of particular crimes and disputes, would by this process come to be possessed of less perfect institutions than those which [...] have followed the appointment of some wise legislator (10-11).

Bentham (2010) similarly writes:

[...] a man may pretend to abjure their empire: but in reality he will remain subject to it all the while. The principle of utility recognizes this subjection, and assumes it for the foundation of that system, the object of which is to rear the fabric of felicity by the hands of reason and of law (14, italics in original).

Constructivists early noticed a tension between the subjective nature of crime and the practical desires for regimented punishments.

18 Elias (1969), Gatrel (1980), and Pinker (2011) have all tracked a substantial decline in inter-personal and privately motivated violence in the modern era and amidst the rise of more formally defined and empowered nation states.

19 Caldwell (2010) notes “[Wesley Claire] Mitchell concluded that the Philosophical Radicals [such as Bentham] were successful in pushing through certain [penal] reforms not because of their theories of human nature [...] but because their ideas matched up well with the sorts of changes that powerful, interested parties already favored” (21).

Bentham (2010) described the philosophical standard of proportionate punishment as “oracular” more than “instructive” because “[t]he same nominal punishment is not, for different individuals, the same real punishment” (32). Penalties could encourage rather than deter criminal behavior if not gauged optimally. If more severe crimes do not earn more severe penalties, to the extent that criminals gain utility from their crimes, they are, in effect, inclined to commit a more severe offense (140-146).21

The panopticon (Bozovic 1995) was designed not only as a punitive tool for gauging criminal sentences but also as an elaborate scheme of social engineering.

Morals reformed—health preserved—industry invigorated—instruction diffused—public burthens lightened—Economy seated, as it were, upon a rock—the Gordian knot of the Poor-Laws are not cut, but untied—all by a simple idea in Architecture (31)!

Little regard was paid to the individualists’ observations that enforcement regimes varied according to local conditions.

“No matter how different, or even opposite the purpose: whether it be that of punishing the incorrigible [...] curing the sick [...] or workhouses [...] It is obvious that, in all these instances, the more constantly the persons to be inspected are under the eyes [...] the more perfectly will the purpose of the establishment have been attained” (34).

Johnston (2000) notes almost every developed nation of the time sent investigators to report the logistics and effects of newly designed, built, and managed American penitentiaries. Many such facilities were direct recreations of Bentham’s panopticon design with central observational towers and surrounding circular amphitheaters of inmate cells, others merely shared the general observational principles of panopticism but leveraged larger floor plans via central observational hubs with spokes of hallways laid out therefrom (Rubin 2013). Facilities were mimicked and constructed throughout the developed world within decades. Incarceration has been the standard form of criminal penalty since.22

21 Such is a particularly early recognition of the third law of demand (Alchian and Allen 1964).
Foucault (1975) argued prisons, though couched in humane rhetoric, secluded punishment from the effective constraint of public oversight. Hence, incarceration galvanized state power and opportunities for corruption. Staff and wardens faced logistic challenges to maintain the conditions of panopticism: isolation and continual observation. Early reports describe harrowing facilities and dehumanizing practices (Spierenburg 1991; Canuel 2007; Geltner 2008). While prison architecture aimed for rehabilitation and social improvement, real prison management required hoods, shackles, physical deprivations, violence, and intimidation to continuously suppress social cooperation and communication amongst inmates. Prisons supposedly incapacitated criminals from harming society, but facilities were designed and managed to suppress any semblance of social community within their walls. Such sites were dystopian settings, antithetical to the artful forms of civil association explained by Tocqueville (1990). Similarly, Rothman (1971) documents how New Deal spending drove prison constructions throughout America thereafter. In effect, displacing civil institutions previously evolved to serve the social needs of security, rehabilitation, and monitored living.23

According to Putnam (2001), the latter 20th century endured significant declines of social capital, via fewer civic organizations, and weaker informal institutions. Many of these institutions were dedicated to social functions such as poverty assistance and healthcare (Cornuelle 1965; 1983; Beito 2000). Rothman (1971) explains similar displacements reshaped the social provision of public safety and criminal justice as early civic groups and organizations played major roles in securing persons and property, and facilitated reputational norms for criminal deterrence and rehabilitation prior to the New Deal. Inversely, during the latter half of the 20th century federally managed and financed criminal justice bureaucracies experienced multiplicative growth (Stephan 2004; Kyckelhahn 2012).

Some early members of a structural functionalist tradition, writing on American investigations at the beginning of the 20th century, shared the natural language methods and universalizing features of human behaviors as the individualist classical school (Reimer 1937; Schrag 1944; Clemmer 1958; Sykes 1958; Cloward et al. 1960; Irwin and Cressey

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23 See also Sellin (1931), Friedman (1994), and Meskell (1999).
Inmate populations were a unique opportunity to study human actions and community dynamics in a closed environment over long periods of time. Inmates were seen as purposeful agents and their social bonds and inter-personal relationships served important functions in maintaining order and promoting collective goals, but such required an intimate and theoretically informed vantage to recognize and understand. Sykes (1958) writes, “we must see the prison as a society within a society [...] prisons appear to form a group of social systems differing in detail but alike in their fundamental processes, a genus or family of sociological phenomena” (xxx-xxxi).

For Sykes, the unique societies within prisons functioned and maintained order in so far as the policies of prison managers effectively harmonized with the schemes of informal norms that organically developed amongst inmates. If policies controlled inmate conditions sufficiently to suppress escapes while also coordinating the incentive patterns of inmates, peace prevailed. If policies were instead too tyrannical, inmates were likely to revolt. He explains features of the prison riot in terms similar to Smith’s (2009, 212) “man of systems”, Sykes (1958) writes, “many individuals bound together for long intervals. Such aggregates enduring through time must inevitably give rise to a social system—not simply the social order decreed by the custodians” (xxx).

Over time, more prison facilities were built and operated throughout American cities and states, many larger and less amenable to relatively laissez faire models of inmate compliance as Sykes proscribed. Wardens in larger facilities sought uniformity and standardization as criminal justice standards homogenized across states and jurisdictional boundaries. In time, the structural functionalist tradition declined in prominence, as professional and political standards preferred more technical and quantitative methods. As Sykes (1958, 138) explains such “was closely linked to the growth of federal funding for the scientific research”.

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24 Goffman (1957) was an exception to the individualist aspect of the functionalist tradition, as he tended to emphasize the pervasive and/or permeating influence of prisons and asylums as ‘total institutions’. He shares common ground in our model with other functionalists given his use of natural language over quantifiable empirics.
IV. THE POSITIVIST SCHOOL

As a product of the 19th century, the positivist school maintained a presentation of natural language methods compatible to and generally similar to the analytical presentations of classical thinkers. Where positivism differed substantially, was its commitment to universalist understandings of human behavior—or lack thereof. Whereas classicals sought to fit their understanding of crime and punishment within a consistent paradigm of human agency, positivists held no such commitments. Positivists were in effect early adherents to scientism in so far as they prioritized the measurement and testability of social propositions over a consistent understanding of purposive agency or human intentionality. Criminals and criminal behavior were first recognized as an object of inquiry for their obvious social consequences, but viewed as totally distinctive from normal and/or legitimate individuals and behaviors. Positivist research was motivated by questions such as: What are criminals? What measurable factors contribute to crime, and by what empirical proxies can we gauge and improve the effects of punishments?

Prison construction and operation expanded throughout the developed world amidst the 19th century. As inmate populations grew in size and frequency, they drew increased attention from social scientists and thus the perspectives of the classical political economists and individualist wing of the functionalists held less sway. Prison managers sought to better manage their facilities, control the populations therein, compel inmate labor, and suppress rebellions. Wardens developed theories of inmate behavior and sought out useful frameworks for prison management and population control. Prison authorities accommodated social scientists’ requests to observe and investigate the newly contained samples of social organization. The positivist school found prisons ideal for quantitative data collection. Inmates were confined, easily observed, required to answer surveys, and could be tracked consistently over time. The question remained, what precise datum should investigators measure and track?

Convinced that criminal behavior had biogenetic causes, Lombroso (1911) measured and tracked inmate's physical traits and characteristics such as sloping foreheads, hair color, eye and lip shape, and facial symmetries. He quickly noticed that, relative to the general population, inmates more often possessed scars, asymmetric features, and a variety of so-called abnormal or deformed traits.
The distinction between the criminaloid, the occasional criminal, the criminal by passion, and the born criminal, as well as the study of the more important causes of crime, enables us to determine with precision the individuals to whom we can apply our curative processes, and the method appropriate to each case (xxxiii).

Given the inherent challenge to social science posed by subjective bias, social theorists long sought consistent and quantifiable forms of behavioral measurement. The scarcity and rarity of data shaped the focus and direction of research through time. Homing in on tangible physical factors complemented public policy attitudes desiring proven and targeted responses to the social problems that came with criminal behavior. When in fact, studies informed and motivated by abundant data may be biased representations of real history. An observed commonality across physical characteristics of inmates is not necessarily support for a genetic causation of criminal behavior. For one counterpoint, early violent routines of arrest and punishment contributed to physical abnormalities observed in prisons (Foucault 1975). Bad science has a greater opportunity to arise and an easier time sustaining itself amidst a professional and political culture of scientism.

In hindsight, positivist criminology demonstrates an obvious flaw to blind insistence for quantification. Such scientism directly spawned eugenic proposals for sterilizations, insulated authoritative political theories of ruling birthrights, and drew out complicity regarding psycho-physical forms of rehabilitation such as genital mutilation (Hamowy 1977) and electroshock therapy. Peart and Levy (2005) explain that eugenics, like all central-planning schemes, was subject to unintended consequences and corruption by ruling interests. Hence there is more at play than just bad science fostered through hateful prejudice. Such bad science can instead be seen as a direct consequence of methodological biases. Criminological positivism and its eugenic policy implications conveniently serviced the professional and political standards of formal modeling and quantifiable measurement, but at the cost of obviously more foundational social demands for fairness and justice.

A later, second branch of positivism (Ferri 1996; 2004; Lacassagne 1891) saw the dominant causes of crime explained by measurable social, rather than biophysical, factors. Inmates shared similar family histories such as being orphaned and/or violently abused. More attune to human intentionality and purposefulness than Lombroso, the social wing of the positivist school still obsessed for quantification and statistical
predictability. Influenced by Durkheim (1997; 1982), later positivists perceived crime and social processes as direct results of social structures. Ferri (1996) wrote, “[e]very physical phenomenon is the necessary effect of the causes that determined it beforehand” (35).

In so far as positivists believed social structures were the ultimate cause of criminal behaviors, the organization of social institutions bore the moral responsibility for the harms caused by crime in society.

The social environment is the breeding ground of criminality; the germ is the criminal, an element which has no importance until the day where it finds the broth which makes it ferment [...] Justice shrivels up, prison corrupts and society has the criminals it deserves (Lacassagne 1891, 364).

Puritanical motivations for corporal rehabilitations such as isolation and flogging, and spectacular deterrence techniques such as scaffold hangings were displaced as rehabilitation’s psychotherapeutic intentions to form as a wide swath of experimental conditions imposed on inmates. Several commentators note the coeval institutional histories of prison facilities and insane asylums, especially in America during the 19th and 20th centuries (Foucault 1964; 1975; Rothman 1971; 1980; Szasz 1973).

Neither biological nor sociological positivists could explain patterned changes in crime and punishment as time passed. Why do periods experience more or less crime systematically without significant biological or sociological changes?

The honest historicist would have to say [...] All we believe to know is how similar policies worked in the past. Provided all relevant conditions remain unchanged, we may expect that the future effects will not widely differ from those of the past (Mises 1957, 203).

By the mid 1970s many studies had attempted to track the effectiveness of rehabilitation strategies ranging a wide gamut including spectacular corporal, psycho-punitive, psycho therapeutic, pharmacological, and behavioral counseling. Martinson's (1974) survey concluded bluntly: nothing works! Not only were criminals apparently no less likely to commit crime upon release, but recidivism rates—the frequency that released criminals re-offend—continuously rose despite growing support and funding for criminal justice institutions (Lipton, Martinson, and Wilks 1975).
V. THE CHICAGO SCHOOL

Shaw and McKay (1942) arguably provided the foundations for the Chicago school approach to criminology on compatible methodological grounds to the classical approach. Their work utilized comparative analytics to recognize how socio-conditional factors related to local community performance. Educational and employment opportunities were critical to shaping criminal trends amongst juveniles.

Much like how functionalists across ideological camps (Grossman and Sykes alike) gave way to more empirical academic and political standards, demands for formal modeling and quantitative measurement quickly rebranded the Chicago school into more econometric forms. These latter approaches proved more appealing to ‘tough on crime’ policy efforts.

In the midst of rising crime and shaken support for the rehabilitation paradigm, Becker (1968; 1974) and Wilson (1975; Wilson and Herrnstein 1985) re-introduced a formalistic rational-choice approach to crime and punishment. Parallel to the classical view, rational criminals commit less crime when the costs of criminal behavior are high and the expected returns low. By increasing the severity and/or the probability of punishment, the quantity of criminal behavior should—other things constant—decline. Formal police budgets represented an obvious and tangible proxy for adjusting the probabilities of catching criminals, while prison sentences similarly served as a tangible means to gauge the severity of punishments and fine-tune social incentives away from crime.

Unlike the rational choice approach of the classical school, Chicago scholars replaced natural language methods with formal mathematics. Combined with newly gathered and already available data, the rational model of crime and punishment formulated hypotheses testable against real evidence. Ehrlich (1972) measured deterrence from punishment ratcheting. The data agreed with the logic and struck hard against earlier sociological and psychological perspectives. Whereas the rehabilitation paradigm posited social responsibilities for crime; the Chicago approach modeled criminal agents as volitional and calculative.

25 See also Ehrlich (1975; 1977; 1981; 1982).
in their decision-making, thus complementing the rise of retributive philosophies and tough on crime policies.26

Significant debates followed Becker’s and Ehrlich’s works.27 Beyleveld (1982) accused Ehrlich of imposing a formalized version of rationality upon all criminal behaviors equally. Cameron explains, similarly, “all individuals obey the same calculus and even have the same utility function, which is unchanging over the life cycle” (1989, 32). Such fit Hayek’s (1952) descriptions of scientism, “before it has considered its subject, claims to know what is the most appropriate way of investigating it” (Hayek 1952, 24). Hayek further quotes Bridgeman (1928) explaining scientisisms tendencies “to postulate that all possible experience conforms to the same type as that with which we are familiar” (46). Ehrlich’s (1982) response concedes to the minimal standards of policy relevance when he writes, “for the theory to be useful in explaining aggregate behavior, it is sufficient that a significant number of the ‘marginal offenders’ conform with this hypothesis” (126-127).

Formal hypotheses require specification and often simplification to be testable. Which deters more: longer prison stays or capital punishment? Ehrlich’s empirics show a significant and consistent correlation between applied capital punishments and lower violent crime rates. Such demonstrates that under past institutional conditions capital punishment deterred more strongly against violent crime than incarceration. While execution offers obvious financial savings over incarceration (Friedman 1999), Ehrlich’s research does not inherently imply policy support for more executions. It does not follow from Ehrlich’s approach that capital punishment is universally efficient or socially optimal. More narrowly, capital punishment seems to provide cost savings given current institutions. There may be better solutions, perhaps infinite possibilities, but they are unknown and often require forms of social experimentation and institutional evolution akin to classical descriptions.28 Furthermore, despite such research not carrying

26 Mundle (1969), Davis (1972), and Kleining (1973) represent the key moral arguments in favor of desert inspired retributive justice.
27 Critics include but are not limited to Baldus and Cole (1975); Bowers and Pierce (1975); Passell (1975); Friedman (1979); Pasell and Taylor (1976; 1977); Blumstein, Cohen, and Nagin (1978); and Barnett (1978).
28 One example is Friedman (1989), who argued that the profit motives of private enterprise if applied to the provision of law and order could increase the real term efficiency of crime prevention and detection.
the direct policy implications that capital punishments represent an efficient norm over alternatives or that tougher punishments ought to be imposed relative to viable alternatives, policy makers and strategic campaigns effectively capitalize on such findings as support for tough on crime proposals nonetheless.

One must presume incarceration is an efficient form of punishment if one is to try and economize on prison space via mathematical modeling (Avio 2003). Policy makers treat status quo police, courts, and prisons as components of a presumably effective institutional black box. When it is accepted that increasing the costs of crime deters criminals, punishment acts as a cost, and incarceration is the assumed technique, it follows that in times of high crime, the policy response is more and tougher police, longer and more frequent punishments, and the building of more prisons. Though a variety of early theory and empirics consistently demonstrated the larger impact from changing the probabilities rather than the potencies of punishments, political authorities tended to view the new Chicago research stream as a mandate for tougher sentencing laws and increased criminal justice budgets nonetheless (Harcourt 2011).

Forms of non-pecuniary utility do not fit well within the formal calculus of mathematical models. Real people consider concepts such as fairness, liberty, and equality before the law seriously when assessing the qualitative features of criminal justice systems and other social institutions. Benson (2003) explains technological efficiency is not synonymous with economic efficiency. Ehrlich admits “there would be a range of alternative enforcement instruments with a deterrent potential that could be brought to bear in an effort to achieve a desirable degree of crime prevention” (1982, 137). But the incentives for discovering those alternatives are altered by public subsidy whereas police enhancements and prison expansions conveniently appease and service status quo and ever growing interest groups. In result, those frameworks, with quantifiable data and precise statistical predictions were most preferred and subsidized by increasingly centralized and federally managed criminal justice bureaucracies amidst the American 20th century experience.

VI. MODERN RESEARCH ON CRIME AND PUNISHMENT
Social science research on the topics of crime and punishment in the latter 20th and early 21st centuries has been dominated by formal
methodologies. Again, in contrast to the classical school, modern projects were not seeking to provide a universal theory of crime nor a consistent rationale for punishment. By nature of such formalism and the advanced standards of better science, contemporary research is more often constrained in application to particular times and places. This methodological trend seems less subject to pernicious forms of political capture compared to previous instances of scientism and perhaps possesses more opportunities for comparative analyses. Contemporary topics cover more narrowly framed questions such as why did crime rates decline so unexpectedly (DiIulio 1996) in the US during the 1990s? Or, what accounts for the exceptional increase in US incarceration rates?

The explanations are varied, sometimes contradictory, debated, and remain unsettled. Blumstein and Beck (1999) repeat the insights of the Ehrlich debates when they explain that the probability of being incarcerated once arrested has doubled since the 1980s, while arrests, sentence lengths, and guilty verdicts have all remained relatively constant. Donohue and Levitt (2001) hold that increased incarceration partly explains the recent crime decline, but Levitt (1996) also shows prison overcrowding can have a hardening effect on inmates, driving crime up instead of down. Again, universal implications or generalizable insights are less discernable from these current streams of research as they are framed in comparably narrow terms to the earlier historicists.

Data availability and empirical techniques have radically improved since the days of the positivist school and should rightly be distinguished therefrom. For example, a vibrant field of research exists at the intersections of neuroscience and criminology (Glenn and Raine 2014). One might inappropriately rush to infer similar policy intentions as the positivists of old. If crime has genetic origins, apart from genetic selection, what can be done? Thankfully, broader social conditions simply do not tolerate anything likening to eugenics. Contemporary neuroscientists should be given the charitable interpretation that they pursue knowledge within some minimally presumed political commitment to contemporary standards of justice and fairness (Broer and Pickersgill 2015). Scientism is not all-powerful. But, while bad science has been displaced by much better techniques, scientism’s expression in the criminal justice arena is in part mitigated by its expression in other policy arenas. Given surrounding social standards of equality and justice, neurological origins of criminality are perceived
and translated into political opportunities for expanding educational and mental health resources. Only time will tell the impacts and efficacies of such reactions.

Furthermore, newer studies of crime and punishment often do not directly imply any particular policies, to a significant degree because there is now a widespread recognition that real crime rates are largely separable from public policies. Research into the linkages between various empirical factors with criminal behavior must more narrowly define crime types than in the past, as criminal prohibitions have widely expanded. Asking what contributes to criminal behavior today is very different from asking the same question one hundred or two hundred years ago, if only because the criminal code is much more expansive today. Hence, today’s empiricism tends to focus on more narrowly individual crime types such as violent behavior.

General trends in violent and/or property crimes are beginning to be re-recognized as outcomes of a complex set of interacting variables and incentives. Kleiman (2009) and Gladwell (2002) for example, explain well that the contributors to declined crime are multi-faceted, subtle, and often seemingly unrelated to formally designed criminal justice institutions. 29 Arguably, crime is more complex than traditional institutions can hope to perfectly regulate or control, and hence more complicated than static modeling techniques can hope to fully capture.

Boettke, Coyne, and Leeson (2003) argue great potentials exist to bridge the methodological gap between the formalistic and particular (fourth vision—upper left quadrant of figure 1) contemporary economics and the universal natural language (first vision—bottom right quadrant of figure 1) of the classical approach. Similar potentials can be seen across such methodological approaches to crime and punishment. Amidst the new appreciation of crime and punishment as complex social phenomena have arisen a number of obviously fascinating topics for social science. What are the industrial dynamics of organized crime? What explains the successful cooperation and collective actions observed in historic examples of large-scale criminal endeavors? How do we explain the rise, internal operations, and industry wide dynamics of drug trafficking organizations and prison gangs? Such questions simply do not lend themselves to formal modeling and quantitative analytics.

29 For example, Lott (1998) tracks increased gun ownership, Benson (1998) and Benson and Mast (2001) show census data on new private security investment, and Rizzo (1980) explains that even minor decisions like taking a taxi can be conceptualized as anti-crime investments.
Data on illicit trades and behaviors is inherently more difficult to gather and assess, though these questions are socially prescient and command scholarly attention nonetheless. Hence, the culture of scientism, may sow the seeds of its own destruction because the greater marginal returns associated with resolving challenging and complex subjects attracts methodological innovators.

Various scholars leverage heterodox techniques to peel back the professional and political orthodoxies that previously obstructed compelling research. Retaining the basics of formal modeling, Buchanan (1972) argued the socially efficient level of organized crime was conceptually greater than zero, as mafias akin to traditional monopolies increased prices and restricted output. Leveraging more intensive and ethnographic case study approaches, Reuter (1983) noticed competitive equilibriums when prohibitions impede illicit operations from exploiting economies of scale. Most recently, Gambetta (1996; 2011), Leeson (2009), and Skarbek (2014) have provided detailed accounts of how organized criminal outfits innovate constitutions to provide governance within their ranks, maximize profits, and evade punitive enforcements. Such work is in a way re-discovering the comparative institutional techniques and insights of the classical school by beginning from a broad swath of quantitative and qualitative case studies and applying universal concepts of rationality and profit-driven actions. Again, time will tell if such methodological innovations can re-direct scientism's past influences upon criminal justice policies and outcomes.

VII. CONCLUSIONS

Prison architectures and the practices of incarceration were designed to accomplish philosophically abstract intentions such as rehabilitation and the promotion of social order. Those aims proved difficult and at times impossible given practical limitations and resource scarcities (Feeley and Simon 1992). Though prison facilities and incarceration did prove convenient for the enforcement of certain social engineering regimes, prohibition efforts, and regulatory controls. The promises of Bentham’s (1907) panoptic architectures, to resolve the calculative challenges of punishments and promote social order via precise incentives, were quickly benchmarked and replicated around the world. Positivism allowed for both the eugenics movement and the various forms of corporal rehabilitation deployed throughout the 20th century. Chicago-style modeling well-served the political platforms to ‘get tough
on crime. Lastly, individual criminological case studies accumulated while enforceable regulations and prohibition resources grew in size and complexity by orders of magnitude.

Micro-sociological accounts of the prison experience reveal practices and conditions commonly perceived as inhumane (Rhodes 2004). Facilities are designed to suppress both the individual and social features of the human condition. The technologies of confinement, doors, fences, lights, food service, healthcare, and recreation are subjected to automation, isolation, and perpetual observation. Amidst fiscal constraints, even if dirigiste proposals for rehabilitation were effective, they are simply not optimal from managerial perspectives. Contemporary prisons cannot meaningfully be considered penitentiaries, reformatories, or houses of correction. Today's incarceration is a process of warehousing human beings amidst bureaucratic incentives. In result, such processes are not substantially different from the warehousing of standard commodities or livestock.30 In so far as such practices affect the relative balances of power in society, criminological sciences are amidst a process of abused reason.

Hayek's methodological theory of social change would suggest these a-rationalities of America's criminal justice stem in part from the methodological trends in the fields of social science attended to crime and punishment. The production of scientific research endures processes of creative destruction. Changes in the relative availabilities of data and research technologies reflective of contextual political and professional interests inspire changes in dominant theory. Through history, bureaucratic incentives favored formal methods in so far as they accommodated central planning efforts, desires for quantifiable analysis, and preferences for statistical forecasting. In particular, criminal punishment by means of penal incarceration was complemented by specific economic and criminological social theories. Significant and effective penal reform today may require changes in the dominant methods used in criminological social science.

30 Garland (2001) writes, "[i]mprisonment has emerged in its revived reinvented form because it is able to serve a newly necessary function in the workings of late modern, neo-liberal societies: the need for a 'civilized' and 'constitutional' means of segregating the problem populations created by today's economic and social arrangements" (199). Wacquant (2009) similarly notes "incarceration serves to physically neutralize and warehouse the supernumerary fractions of the working class and in particular the dispossessed members of stigmatized groups" (xvi). Alexander (2010) parallels the structural influence of mass incarceration upon American blacks as akin to a new Jim Crow era.
In the decades since the late 1970s America’s criminal justice resources arguably became unreasoned. Pecuniary costs (Bauer and Owens 2004), enforcement techniques, and the demographic make up of incarcerated populations (Aladjen 2008; Loury 2008; Brown 2009; Wacquant 2009; Alexander 2010), did not correspond to the structural magnitudes of crime rates, nor democratic measures of public opinion (Walker and Hough 1988; Flanagan and Longmire 1996). American criminal justice was disproportionate relative to other times and comparable nations (Pease 1992; Sutton 2004; Cavadino and Dignan 2006; Tonry 2007; Lacey 2008; Walmsley 2003). Several described these trends as inefficiently severe and unsustainable (Becket 1997; Garland 2001; Roberts et al. 2003; Whitman 2003; Moskos 2011). Higgs (2004) bluntly explained at the height of the trend,

[…] if the total incarcerated population were to continue to grow by 7.3% annually, it would double approximately every ten years […]. Hence, in the decade of the 2080s, within the lifetime of many people already born, the prison population would overtake the total population (96).

In summary, a strong inter relationship can be discerned between the methodological trends of social science dedicated to crime and punishment on the one hand, with real policy trends and social outcomes on the other. As luck would perhaps have it, our processes of supposed systemic failure have presently stagnated amidst a similar proliferation of newly diversified research streams rekindling an attempt to situate crime and punishment studies into a consistent model and understanding of human social order.

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31 See Garland (2000, 349-350) for an explanation of the broad concept of contemporary increased punitive severity.


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Mainstream economics and the Austrian school: toward reunification

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Abstract: In this paper, I compare the methodology of the Austrian school to two alternative methodologies from the economic mainstream: the ‘orthodox’ and revealed preference methodologies. I argue that Austrian school theorists should stop describing themselves as ‘extreme apriorists’ (or writing suggestively to that effect), and should start giving greater acknowledgement to the importance of empirical work within their research program. The motivation for this dialectical shift is threefold: the approach is more faithful to their actual practices, it better illustrates the underlying similarities between the mainstream and Austrian research paradigms, and it provides a philosophical foundation that is much more plausible in itself.

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Among mainstream economists, there is a basic orthodoxy about the philosophical foundations of economic theory. They agree that idealizations about people's psychological preferences serve in some way to ground economic theory, that economic theory can be represented using mathematical functions, and that those functions can guide and be guided by empirical research into a large range of social phenomena. Theorists associated with the Austrian school reject this approach. They think that speaking of a person's 'preferences' is just summarizing that person's past observed behavior in a misleading way,

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that formal rationality is empty, and that econometric analysis is of little empirical value.

These differences have led Austrians and mainstream theorists to develop different bodies of theory, with different practical implications. But it is the philosophical disagreements between the two camps that explain why they currently have almost no productive engagement with each other, despite the Austrian school remaining one of the most institutionally established heterodox research programs. Neither side appears to have much interest in reconciliation, but, given the degree of overlap in both approach and ambition, this schism is due for a correction.

The schism has been driven in part by the fact that the standard comparison between those in the mainstream and those in the Austrian school is too abstract to be meaningful. So, I will argue that a serious comparative evaluation of research paradigms requires a discussion cast at a lower level of abstraction. From this perspective, the problem can be more clearly seen to revolve around ‘extreme apriorism’. The disappearance of that idea, I argue, is a Pareto improvement for economics.

II. PRELIMINARIES
Before comparing the mainstream and Austrian paradigms, it is necessary to explain at least roughly what they are and how they differ. Some of the ways in which differences between these paradigms arise are visible to everyone. The two, for instance, differ in terms of their respective working practices. For mainstream theorists, economics is the enterprise of formal model-building. And sometimes, for the sake of simplicity or tractability, economists will need to include idealizations in their models. Robert Solow acknowledges as much:

[I]f you ask a mainstream economist a question about almost any aspect of economic life, the response will be: suppose we model that situation and see what happens [...]. The idea is to focus on one or two causal or conditioning factors, exclude everything else, and hope to understand how just these aspects of reality work and interact. There are thousands of examples; the point is that modern mainstream economics consists of little else but examples of this process (1997, 43).
Austrian theorists don’t view the discipline of economics in this way. They do not see it in terms of building formal models, and they are usually unwilling to accept idealizations or the implications underwritten by them. The more fundamental disagreements between the two camps result from two different understandings of the basic categories in play.

One disagreement among mainstream and Austrian theorists, for instance, concerns the theory of preferences: what preferences are, how we can learn about them, and how we should theorize about them. Ludwig von Mises writes of preference rankings (referring to them as “scales of values”):

[O]ne must not forget that the scale of values or wants manifests itself only in the reality of action. These scales have no independent existence apart from the actual behavior of individuals. The only source from which our knowledge concerning these scales is derived is the observation of a man’s actions. Every action is always in perfect agreement with the scale of values or wants because these scales are nothing but an instrument for the interpretation of a man’s acting (1998, 95).

There are two claims in need of discussion here. One claim is that the only empirical source of information concerning people’s preferences is their choice behavior. I will call this claim epistemological choice exclusivity, or ECE. The other suggests that theorizing about preferences should not make formal reference to anything other than choices. This claim I will call methodological choice exclusivity, or MCE.

Resistance to these two ideas is not universal among mainstream economists. In fact, I believe that nearly everyone accepts ECE. However, a subset of mainstream theorists, revealed preference theorists, claims to also accept MCE. The ambition of the revealed preference approach is to justify the key theoretical insights of the mainstream approach

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1 Mises also makes a claim about the metaphysical status of preferences, in describing them as having no “independent existence” (1998, 95) apart from behavior, but I think this claim is better left aside here without deeper commentary. It is difficult to characterize the contemporary Austrian school’s metaphysical view as either realist or anti-realist. Mäki describes it as “a combination of ontic subjectivism and ontological objectivism” (1990, 336). He argues that “[i]t is ontic subjectivism in that many fundamental objects of economic theory are claimed to be subjective in nature” but “ontological objectivism or realism in the sense that those subjective entities are maintained to have an objective existence, they exist independently of economists’ theories of them” (336).
without reference or appeal to anything metaphysically exotic, such as a psychological disposition or mental state (Samuelson 1948). The revealed preference theorist’s characteristic objection to the mainstream approach is, in short, grounded in behaviorism.

As I suggested at the beginning of the paper, the approach of the orthodox methodology is to take idealized preferences as primitive, and then to develop a theoretical framework from deductive reasoning about them. But, to whatever extent we have behaviorist sympathies, we may be unsettled by preferences as some sort of “primitive characteristic of the individual” (Mas-Colell, Whinston, and Green 1995). That assumption may amount to nothing more than a fancy way of begging the question against MCE. With this in mind, revealed preference theorists try to build the same basic framework from the other “direction”; they take idealized choice behavior as primitive, and then develop of model of rational preferences from deductive reasoning about that choice behavior.

There is a well-known criticism of the revealed preference approach, stemming from its commitment to MCE. As has been discussed by many (Camerer, Loewenstein, and Prelec 2005; Caplin and Schotter 2008; Hausman 2011), the correspondence between a person’s preferences and her choices appears to fail in contexts in which she has false beliefs. Daniel Hausman (2011, 27-28) offers the following example, drawn from Shakespeare’s Romeo and Juliet: since Romeo’s alternatives were either to die or to elope with Juliet, and since the choice expressed by his behavior was to die, it appears as if revealed preference theorists are committed to the claim that Romeo preferred to die rather than elope with Juliet. But, on the intuitive interpretation of preference, Romeo had no such preference. Shakespeare intends the viewer of the play to understand that Romeo took his own life because he believed falsely that Juliet had died, and so, was unaware that eloping with her was a live option.

As Hausman (2011, 28-29) points out, there is a natural reply to this problem. If we abandon the intuitive understanding we have of preference in favor of simply stipulating that people’s preferences are exhaustively defined by their actual choices, the problem disappears. According to that interpretation of preference, it follows from Romeo’s choice that he did have a preference, so understood, to die rather than to elope with Juliet. Problem solved.
This apparent solution loses track of why we are interested in preferences in the first place. As Hausman argues (2011, 29), if our interest in preferences were merely terminological, this kind of interpretative shift might be justified. But most theorists—and certainly revealed preference theorists—are interested in preferences only insofar as they believe that learning about them is instrumental in learning how to make reliable predictions about people’s behavior. This ambition requires facing up to the methodological problems that ECE presents. If we cannot come to know what governs people’s behavior solely from information about their choices, then assimilating the concepts of preference and choice cannot carry us very far. To an extent, the dispute between the ‘orthodox’ and revealed preference methodologies just is a dispute about whether ECE entails MCE.

Like all behaviorists, revealed preference theorists avoid explicitly realist commitments at all costs. However, I do not think they manage to actually abandon those commitments. Ken Binmore, for instance, writes:

The theory of revealed preference therefore makes a virtue of assuming nothing whatever about the psychological causes of our choice behavior. [...] [It] succeeds in accommodating the infinite variety of the human race within a single theory simply by denying itself the luxury of speculating about what is going on inside someone's head. Instead, it pays attention only to what people do. It assumes that we already know what people choose in some situations, and uses this data to deduce what they will choose in other situations (2011, 8-9, emphasis in original).

This description of the revealed preference methodology carries substantive commitments, at least by implication. According to Binmore, the revealed preference methodology relies on an inference from what people choose in some particular contexts to what they will choose in other contexts. But what could possibly justify an inference of this sort, if not an implicit auxiliary assumption about people's (relatively stable) psychological dispositions? As Binmore argues, revealed preference commitments do not “mean that economists believe that our choice behavior isn't caused by what goes on in our heads” (8). His grievance with a realist interpretation of preferences is not that preferences actually lack such grounding. Rather, his grievance is that a methodology that helps itself to robust realism about whatever it wants, runs the risk of unsound practice. It is hard to disagree with that.
At bottom, revealed preference theorists don’t accept MCE, at least not in the strongest sense. They simply can’t. If people’s preferences don’t have any special metaphysical dependency on their choices, and if theorizing about people’s preferences is limited to analysis of their actual choices, then theorizing about people’s preferences will be impossible (at least in many contexts of interest). We are left with what might be called predictive nihilism.

Revealed preference theorists may think that the preceding discussion has presented a caricature of their view, for one reason or another. I hope this is not true, but my purpose here is not to enter the debate between orthodox and revealed preference theorists. I only want to point out a striking convergence in what Austrians and revealed preference theorists say. As Murray Rothbard—a student of Mises’s—describes it, this convergence belies a more fundamental contrast to be drawn between them:

‘Revealed preference’—preference revealed through choice—would have been an apt term for our concept. It has, however, been preempted by Samuelson for a seemingly similar but actually quite different concept of his own. The critical difference is this: Samuelson assumes the existence of an underlying preference scale that forms the basis of a man’s actions and that remains constant in the course of his actions over time. Samuelson then uses complex mathematical procedures in an attempt to “map” the individual’s preference scale on the basis of his numerous actions. The prime error here is the assumption that the preference scale remains constant over time. There is no reason whatever for making any such assumption. All we can say is that an action, at a specific point of time, reveals part of a man’s preference scale at that time. There is no warrant for assuming that it remains constant from one point of time to another (2011, 294, emphasis added).

Here, we find the outline of a second sort of problem for revealed preference theorists (and mainstream theorists more generally). What Rothbard is denying is that there’s enough stability in a person’s values over time to license understanding her choices in terms of some stable set of psychological dispositions. This objection can be traced back to Mises (1998, 103), but it continues to enjoy attention from contemporary Austrians (Block and Barnett 2012), and also hasn’t gone unnoticed by the mainstream (Grüne-Yanoff and Hansson 2009).

To be sure, it is very difficult to determine people’s ‘real’ preferences. In some cases, this difficulty arises because of the
instability of people's preferences; what a person wants can change over time or across contexts. This is Rothbard's complaint. In other cases, people's preferences are difficult to assess because their factual ignorance opens a chasm between what they want and what they choose. This is the moral of the Romeo and Juliet case. There is a third sort of theoretical problem, also involving a chasm between people's dispositions and their behavior. This problem, however, results from people's irrationality rather than their ignorance.

Austrians take the concerns about stability and ignorance so seriously that their theory of preferences is structured around them. In contrast, Austrians seem to regard the irrationality problem as a pseudo-problem. Human action can't be irrational. Of rationality, Mises writes:

Human action is necessarily rational. [...] When applied to the ultimate ends of action, the terms rational and irrational are inappropriate and meaningless. The ultimate end of action is always the satisfaction of some desires of the acting man. Since nobody is in a position to substitute his own value judgments for those of the acting individual, it is vain to pass judgment on other people's aims and volitions. [...] When applied to the means chosen for the attainment of ends, the terms rational and irrational imply a judgment about the expediency and adequacy of the procedure employed. The critic approves or disapproves of the method from the point of view of whether or not it is best suited to attain the end in question. It is a fact that human reason is not infallible and that man very often errs in selecting and applying means. An action unsuited to the end sought falls short of expectation. It is contrary to purpose, but it is rational, i.e. the outcome of a reasonable—although faulty—deliberation and an attempt—although an ineffectual attempt—to attain a definite goal (1998, 19-20).

This way of understanding rationality is actually quite friendly to the mainstream research program. Mainstream theorists, like Austrians, aim for 'thin' theories of rationality, ones that cast the content of preferences as beyond the evaluative reach of economics. Most espouse value subjectivism at least to some extent, because they appear to respect a certain kind of neutrality.

It seems plausible at first to say that, since an act simply is intentional, purposeful behavior, there cannot be action without rationality. But this theory of rationality is so thin that it is practically invisible. It cannot explain why, by all appearances, some intentional
and purposeful actions are *much* more intentional and purposeful than others, while other actions appear to have been done without much intention or purpose at all. Mises considers these implications:

The assertion that there is irrational action is always rooted in an evaluation of a scale of values different from our own. Whoever says that irrationality plays a role in human action is merely saying that his fellow men behave in a way that he does not consider correct [...] When the expressions ‘rational’ and ‘irrational’ are applied to the means employed for the attainment of an end, such a usage has significance only from the standpoint of a definite technology. However, the use of means other than those prescribed as ‘rational’ by this technology can be accounted for in only two possible ways: either the ‘rational’ means were not known to the actor, or he did not employ them because he wished to attain still other ends—perhaps very foolish ones from the point of view of the observer (2010, 35).

In other words, there is only one way an agent may genuinely err in “selecting and applying means” (1998, 20): if the agent does her best but is incompetent. One can, in other words, commit a ‘competence’ error. As Mises points out, we are not generally inclined to describe such errors as failures of rationality. But the reason the objection seems to arise at all is that everyone knows the feeling of making a ‘performance’ error—a one-off error against a backdrop of general competence—and these errors are the canonical failures of rationality. For Austrians, this is never how we are to think about the world. So, rationality requires not just subjectivism about a person’s *ends*, but subjectivism about her *means* as well.

Roderick Long motivates this ultrathin theory of rationality against the problem of intuitively irrational preferences, writing:

What [those who appear to have irrational preferences] are doing seems crazy only because we assume their preferences are like ours, and that their beliefs about how to satisfy those preferences are also like ours. But the very fact that they are behaving so oddly should give us reason to doubt those assumptions. Of course they might assure us verbally, ‘Yes, yes, our beliefs and preferences are just like yours’. But talk is cheap. They might be lying, or confused. For that matter, they might not even be speaking our language. After all, the best evidence we have that their word ‘money’ means the same thing as our word ‘money’ is what they do with what they call money. Meaning cannot be separated from use. Something is money only if
it plays the role in people's actions that constitutes its status as money (2004, 354-355).

This passage may appear to be just an affirmation of Mises's claim that assessments of irrationality always bottom out in differences of value, but Long's claim is stronger than that. Consider a person with an intransitive set of preferences. How do Austrians escape trouble with respect to their evaluation of that person's behavior? At first glance, they do this by denying that we could know the person's preferences are wrongheaded by her lights. After all, she can adopt whatever ends she likes, and it is no criticism of her ability to act purposefully that those ends are wrongheaded by our lights, even if they leave her bankrupt. Perhaps she sincerely desires to be Dutch-booked, or her tastes change quickly. One never knows for sure.

But the Austrian theory of rationality reaches further. Long also claims:

[Those who appear to have irrational preferences] are not a counterexample to praxeological principles, even if we assume that their coins really are money. And of course the latter assumption too may be questioned. [...] Nothing counts as buying or selling unless it is in accord with the laws of economics. Hence we are in no danger of encountering irrational prices, for the same reason that we are in no danger of encountering a chess game that consists of tossing a ball back and forth across a net. That wouldn't be chess. Those wouldn't be prices (2004, 353-355, emphasis in original).

So the Austrian theory of rationality doesn't just deny that the person is being irrational. It denies that the person is making an economic transaction at all.

For Austrians, the theory of preference and the theory of rationality are not foundational. Both are implications of a broad skepticism about empirical social science (Mises 1998, 55-56), which is itself an implication of an anti-reductionist thesis about psychological phenomena called “methodological dualism”. In Mises’s words:

Methodological dualism refrains from any proposition concerning essences and metaphysical constructs. It merely takes into account the fact that we do not know how external events—physical, chemical, and physiological—affect human thoughts, ideas, and judgments of value. This ignorance splits the realm of knowledge into two separate fields, the realm of external events, commonly

One of Rothbard's criticisms of the revealed preference methodology is that it commits what he calls “the fallacy of psychologizing”, which he understands to be “the treatment of preference scales as if they existed as separate entities apart from real action” (2011, 296). From one perspective, this criticism is surprising: revealed preference theorists see their mission precisely as that of dodging metaphysical commitments, yet this is the accusation of the Austrians. But, as I argued earlier, the revealed preference approach ultimately inherits the metaphysics of the orthodox methodology.

In contrast, Austrians are even more methodologically nihilistic about preferences than revealed preference theorists. There are disputes among Austrians about both how metaphysically loaded Mises understood methodological dualism to be, and about how loaded it is in its most plausible formulation.2 But in any case, it is what has led them to adopt a different theoretical framework (Wiśniewski 2014). This alternative framework is at once both wider and narrower in ambition. On paper, the framework has two aspects. One is praxeology, which relates to the deductive implications of the idea that people act, with intention and purpose; the other is thymology, the study of the causes that underlie the acts.

The relative importance of the praxeology and thymology within the Austrian theoretical framework is the source of the most basic controversy within the Austrian school (Block 2012). On one side lies a sort of praxeological fundamentalism, which claims to uphold extreme apriorism (Rothbard 2011, 103-111) and which de-emphasizes thymology. On the other end lie those who, although broadly sympathetic to the Austrian paradigm, favor a more thymologically-informed research agenda. The motivation for this sort of moderatism is the hope that the use of empirical data can somehow be reconciled with the rest of the Misesian theory (Lavoie and Storr 2011). Some Austrians regard the disagreement as internal to the Austrian school; others regard it as the essential difference between two fundamentally different schools of thought (Boettke 2012, xii).

2What underlies Mises’s dualism is not settled among Austrians. See Kirzner (1982), Lewis (2010) and Hauwe (2011).
Nothing substantive turns on who ‘counts’ as what, but the schism has been the source of confusion. Without some measure of agreement about what the essential commitments of the Austrian school are, it is meaningless to have a critical discussion about what ‘the Austrian school’ has the conceptual resources to defend. The Austrian school is its essential commitments. So, productive engagement with it requires simultaneously exploring both positions in the context of a single argument. If we understand the Austrian school as methodologically defined by extreme apriorism, then the resultant theoretical framework will wind up being obviously inadequate for the purposes of serving as a free-standing research paradigm. So, we should understand the Austrian school as more open to moderatism. This turns out to have other benefits: it is more consistent with some of the most important contemporary ideas produced by the Austrian school, and also more consistent with the mainstream research program.

II. THE INADEQUACY OF EXTREME APRIORISM
Rothbard offers a characterization of extreme apriorism that emphasizes its connection to praxeology. Extreme apriorists hold characteristically that:

(a) the fundamental axioms and premises of economics are absolutely true; (b) that the theorems and conclusions deduced by the laws of logic from these postulates are therefore absolutely true; (c) that there is consequently no need for empirical ‘testing’, either of the premises or the conclusions; and (d) that the deduced theorems could not be tested even if it were desirable (2011, 103-104).

At bottom, the disagreement between extreme apriorists and everyone else lies in their differing interpretations of the explanatory burden. Extreme apriorists claim that their critics are trying in vain to accomplish the impossible, while their critics argue that extreme apriorists cannot accomplish enough. So, while mainstream economists aspire for economic theory to apply to all interactions, even at the cost of making false assumptions about some of the features of those interactions, extreme apriorists are willing to accept praxeology’s incompleteness in exchange for its deductive soundness. It is on account of this soundness that they often describe praxeology as the more “realistic” approach to evaluating economic interactions (Mises 1998, 34).
But extreme apriorists don’t get this soundness for free. The restrictions they place on the domain of discourse impose a high explanatory cost. By itself, praxeology lacks the resources to explain, predict, or even characterize many interactions of interest. To bring this out, I will examine two somewhat familiar games. First, one (Rubinstein and Salant 2008, 19; Hausman 2011, 29-31) which admits of representation within the standard formalisms of game theory:

**Figure 1: Recess**

**Recess**: Timmy plays first, and has two choices, *Face His Fear* or *Stay Inside*. If he plays *Stay Inside*, he loses 1, Bully receives 1, and the game ends. If Timmy plays *Face His Fear*, Bully can respond either *Back Down* or *Fight*. If Bully plays *Back Down*, then Timmy receives 2 and Bully loses 2. If Bully plays *Fight*, Timmy loses 2 and Bully receives 2.

The payoffs in this game are defined so that Timmy prefers the outcome of (*Face His Fear*, *Back Down*) to (*Stay Inside*) and the outcome of (*Stay Inside*) to (*Face His Fear*, *Fight*), and so that Bully prefers the outcome of (*Face His Fear*, *Fight*) to (*Stay Inside*), and the outcome of (*Stay Inside*) to (*Face His Fear*, *Back Down*). Under the standard interpretation, Bully never has a chance to play at all. This is highly intuitive: if Bully were to get the opportunity to play (i.e., if Timmy were to play *Face His Fear*), Bully would play *Fight*. Knowing this, Timmy always plays *Stay Inside* and the game ends immediately.
To examine this topic using the apparatus of game theory may appear question-begging, but the underlying insights don't really depend on the formalisms. The formal game *Recess* is nothing more than an abstract way of representing a familiar set of collective social dynamics, and all the formalisms do is lend those implications a deductive flavor. The important thing to notice is that in order to even define the situation as one of a certain type, one must ascribe to Bully a preference between fighting and backing down, even though he's never given the opportunity to reveal this preference. So how can that preference be understood?

The orthodox methodology has a straightforward answer to this question. According to orthodox theorists, Bully has a real but unrevealed preference for fighting rather than backing down. Bully's preference ranking is just a formal representation of that preference. To explain these results, orthodox theorists need not to say anything evaluative about his dispositions. Naturally enough, revealed preference theorists have more trouble with the case. They cannot define Bully's preference in terms of his choice, because he doesn't act. But they can ease their trouble by relaxing their commitment to MCE, a strategy that does not automatically involve their taking on metaphysical commitments any more substantive than those Binmore accepts when he connects our choice behavior to “what goes on in our heads” (2011, 8).

Extreme apriorists cannot give this response, because Bully's unrevealed dispositions are invisible to praxeology. They cannot give any account of *Recess*' important counterfactuals, and so, cannot examine how things may be expected to change if Bully were absent from school one day, or if Timmy were a little stronger. To extreme apriorists, this sort of counterfactual is categorically off-limits. In fact, they cannot define the game described above at all, let alone offer a nontrivial explanation of its outcome. There is only an inexplicable and unpredictable surface phenomenon: each day, Timmy decides not to go outside for recess.

As far as extreme apriorists are concerned, there is no Bully. Perhaps Timmy is, as Long puts it, “lying, or confused” (2004, 354). If this is the only possibility from the perspective of extreme apriorism, we should hope those who actually monitor schoolchildren playing at recess aren't extreme apriorists!
Games can also be used to draw out problems with the Austrian theory of rationality. Chess is a useful instrument for this particular task, because the errors that chess players make cannot be attributed to their factual ignorance—chess is a *perfect information* game. So, errors in chess *must* be understood in terms of the other sort of mistake, in terms of irrationality. Given our present technology, the game tree of chess is too large for exhaustive computational analysis. However, we can consider particular (relatively simple) chess positions, for which exhaustive game trees (called ‘endgame tablebases’) actually can be given.

Consider the following position, one for which there exists a tablebase:

![Chess board](image)

**Figure 2: White to play**

Suppose that White were to advance her only pawn forward one space (from the space whose coordinates are f3, to f4), or to slide her rook on e3 one space to the left, to d3. (The names of these moves are, respectively, ‘f4’ and ‘Rd3’). These are the moves that strongest human players would play, were they thinking as clearly as they could. However, both of these moves are actually suboptimal: White's moving her king one square to the right (that is, playing 'Kg2') is the first move of a 99-move forced checkmate, and any other move leads to a forced draw. So, playing Kg2 is White’s best move irrespective of how Black responds; it is a *strictly dominant* strategy. Unfortunately, no heuristic
that could be interpreted and understood by human beings would recommend it.\footnote{See Zarsky (2011) for an explanation of the distinction between ‘interpretable’ and ‘non-interpretable’ algorithms.}

Mainstream, idealized theories of rationality have trouble with this sort of case. They diagnose playing the suboptimal f4 or Rd3 as ‘irrational’, which seems inappropriately stringent, given that knowing to play the optimal Kg2 requires near-omniscience about the implications of one’s play. But these idealized theories have the resources to offer some kind of indictment of the suboptimal moves (even if the indictment is too heavy-handed). Perhaps playing f4 or Rd3 is forgivable, but there’s an important sense in which anyone who plays f4 or Rd3 in that position could have done better.

Extreme apriorists, meanwhile, have the opposite problem. They cannot offer any indictments of those two moves (or indeed, of any moves). Against any move other than Kg2, they can only insist that that player was either incompetent, or abandoned perfect play in service of some other objective. But true incompetence is better exemplified by a move like ‘Rc3’, in which White slides her rook on e3 two spaces to the left, to c3. Here, the Rook is ready to be taken by Black’s queen without compensation. Anyone who plays this particular move is either incompetent or pretending to be.

This case provides a nice illustration of Long’s paraphrase of Kant: “Praxeology without thymology is empty; thymology without praxeology is blind” (2004, 359). The theories of rationality that perform best with respect to this sort of case are theories of bounded rationality (Simon 1957), which relax the requirements of rationality, for instance, by letting go of the requirement for logical omniscience. And indeed, in this case, a theory of boundedly rational chess play is able to offer the correct general diagnosis: playing Rc3 reflects incompetence, playing f4 or Rd3 reflects rationality at the boundary of human capacity, and playing Kg2 (probably) reflects access to the tablebase. Austrians would recognize such a theory as employing a synthesis of praxeological and thymological theorizing. So, the idea of bounded rationality is clearly useful in this case, but it is also inconsistent with extreme apriorism.

The bounded conception of rationality lends itself to much wider application than do the idealized (mainstream) or empty (extreme apriorist) conceptions of rationality. Consider a casino whose
management team wishes to structure their house rules in a profit-maximizing way. Given these motives, which set of rules the casino should adopt depends on certain kinds of facts that are, by their nature, invisible to praxeology. If, for instance, the management team discovers empirically that their customers tend to play dominated strategies to a greater extent than normal when the stakes reach a particular amount or the clock strikes a particular hour, these facts can be incorporated into a model whose agents are boundedly rational in some particular way, which will in turn inform the design of a better rule set. Whatever the details, however, extreme apriorism is inadequate here. At least for their purposes, the team will want, need, and be able to fruitfully infer much more about the players' ends and means than praxeology alone warrants.

Now, praxeology might seem inadequate in this sort of case only because the case implicitly contains substantive thymological features. Within this limited domain of action, it is safe for the casino to assume that most will adopt maximizing their winnings as an end, and will use this end to shape their choices of means to that end. But human action in general is much more teleologically open-ended. So, as those sympathetic to extreme apriorism may observe, no examples of the above sort (indeed, no examples at all) could falsify extreme apriorism.

However, the examples do illustrate the general nature of the conflict between extreme apriorists and everyone else. In general, the greater the significance of empirical information and mathematical modeling in generating the results we want, the more limited in scope extreme apriorism appears by comparison. The best strategy for extreme apriorists, then, is to argue that empirical information and the predictive models borne out of that information have little to contribute in the contexts of greatest interest. And on the context of greatest interest—competitive markets—Mises argues:

Within the frame of a market economy competition does not involve antagonism in the sense in which this term is applied to the hostile clash of incompatible interests. [...] Competitors aim at excellence and preeminence in accomplishments within a system of mutual cooperation. The function of competition is to assign to every member of a social system that position in which he can best serve the whole of society and its members. It is a method of selecting the most able man for each performance. Where there is social cooperation, there some variety of selection must be applied. Only where the assignment of various individuals to various tasks is
effected by the dictator's decisions alone and the individuals concerned do not aid the dictator by endeavors to represent their own virtues and abilities in the most favorable light, is there no competition (1998, 116-117).

On what basis would an extreme apriorist be entitled to make the kinds of claims Mises does about the nature of markets or of their participants? Without recourse to thymology, extreme apriorists aren't entitled to make claims about the concepts of cooperation or competitiveness. Praxeology lacks the resources to even characterize those concepts so as to be able to distinguish between them, to say nothing of explaining their general functions in large-scale social settings. Extreme apriorists also aren't entitled to claim that all participants ‘aim at excellence' in any nontrivial way, because the Austrian theory of rationality prevents them from giving content to such judgments. Extreme apriorists are not entitled to make claims about the kinds of values, dispositions or attitudes actual participants have in actual markets. In short, they are committed to remaining silent about almost everything actual market participants do. Nothing about the nature of the aims of market participants or the nature of the large-scale social phenomena that emerge from the interaction of those aims follows from the axioms of praxeology. What extreme apriorism grounds is predictive nihilism.

But when praxeology is supplemented by thymology, the prospects for the Austrian school research program improve considerably. Allowing for use of thymology opens up a large buffet of useful tools for Austrian theorists, the most significant of which is game theory itself. Game theorists now work on many topics that are traditionally of interest to Austrians, such as spontaneous order (Axelrod 1984), but for the most part, Austrians haven't internalized game theory’s insights, on account of their supposed allegiance to extreme apriorism. Any loss of working efficiency on these grounds is unnecessary.

Thymology also provides the resources to make sense of the chess position from fig. 2. Long explains:

[I]f I am praxeologically mighty but thymologically weak, I might be able to write hefty tomes on, say, monetary theory, and yet be woefully unable to recognise monetary exchanges in real life—in which case I would be helpless in trying to explain historical events like depressions and hyperinflations. It may thus appear that praxeology is useless in explaining anything unless it is
supplemented by thymology, which in turn seems to require some special knack of intuition whose presence or absence seems more a matter of luck than of scientific insight. [But] we don’t count as possessing a concept unless we are—not perfectly reliable, but—reasonably reliable at applying it. It follows that the just-imagined scenario of praxeological proficiency combined with thymological ineptitude is not a real possibility; we don’t count as possessing praxeological concepts except insofar as we are generally able to apply them accurately (2006, 42).

So, praxeology by itself lacks the resources to even recognize figure 2 as a chess position. One may, in other words, run across two persons seated across the table from each other, a chessboard with that position prepared, and praxeology is not only silent about what either person should do, it is silent about what the two are doing. It is only thymology that allow us to categorize this state of affairs as that of two people playing chess, and thus, only thymology that allows us to apply to that situation a theory of boundedly rational chess play.

I don’t mean to suggest that all of the various challenges to the Austrian research program can be met by jettisoning a few incidental commitments, or by reinterpreting a few inopportune passages of Mises’s writing. To accept the explanatory benefits of empiricism is at the same time to accept its problems.

How, for instance, does methodological dualism square with this newfound empirical optimism? If methodological dualism truly prevents using the methods of natural science to examine human action, how can thymology ever get off the ground? And what exactly are the standards of Long’s “reasonable reliability” (2006), either in the particular case of chess or in a systematically more general sense? And however we understand it, how could we ever know that we have it? And even if we set aside those ontological and epistemological questions, how in practice could we distinguish, say, a chess game from some other kind of structured activity that is played by chess players at a chessboard but with different payoffs? The introduction of thymological considerations isn’t in itself a panacea which lacks a need for further consideration. It immediately raises several standard philosophical issues. But this isn’t

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4 Scott Scheall (forthcoming) argues that the problems associated with extreme apriorism aren’t somehow undercut or explained away by the fact that the scope of praxeology within economic theory is narrow.
always an indication of a methodological misstep. And here, I think it helps reorient Austrians in a more productive direction.

In fact, at a lofty enough level of abstraction, the practical difference between the moderate Austrian and mainstream research programs seems to run thin. Peter Boettke (2012) offers a set of three commitments he regards as characteristic of the contemporary Austrian school. This set includes (1) methodological individualism, the idea that social phenomena are explained in terms of how they result from the behavior of individuals; (2) the methodological priority of exchange over allocation, the idea that economics is primarily about “exchange behavior and the institutions within which exchanges take place” (xii) rather than mere allocative concerns; and (3) methodological dualism.

Each of these three commitments has defenders inside the mainstream, at least outside the Austrian school. Methodological individualism, for instance, is an axiomatic assumption of non-evolutionary game-theoretic analysis, and there has been consistent interest in the idea itself since Weber, including a renaissance brought about by Jon Elster (1982). The ‘institutional’ and ‘new institutional’ approaches to economic theory likewise suggest understanding the study of economics in terms of exchange rather than optimal allocation. Even methodological dualism, the least mainstream of the three commitments, continues to enjoy serious discussion (Chomsky and Smith 2000) as an idea. Overall, mainstream and Austrian theorists seem to share a similar broad ambition: to understand the nature and mechanics of intentional action, according to a methodology constructed from the subjective preferences of individuals.

It is important not to undercut the importance differences between the two camps. Both, for instance, claim to accept value subjectivism, but the sort of subjectivism Austrians accept (Lachmann 1986) is often much more comprehensive in nature. But the broad point stands apart from these issues. As more and more Austrians conduct empirical work, and as more and more mainstream theorists work on topics that were traditionally of interest to Austrians, the value of methodological reconciliation between the two camps comes into view.

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5 See Stringham (2003), Chamlee-Wright and Storr (2010), and Selgin, Lastrapes, and White (2012).
6 For work on subjectivism, coordination, institutions, and entrepreneurship, see Foss (1999). For work on complexity, see Rosser (2012), and for work on spontaneous order, see Leeson (2014).
So, one might wonder, who are the extreme apriorists? Was Mises? He’s not especially clear on this issue (1998, 858; 2010, 13-18), but I think the answer is no. He writes: “For lack of any better tool, we must take recourse to thymology if we want to anticipate other people’s future attitudes and actions. Out of our general thymological experience [...] we try to form an opinion about their future conduct” (2007, 313). Mises does not regard this sort of ‘opining’ as the practicing of economics proper. But insofar such opining as a superior substitute for what mainstream economists just call ‘economics’, this is a terminological quibble.

I think that the idea of free-standing extreme apriorism is best understood as a relic of Mises’s hyperbolic way of speaking. Even Rothbard, the author of a paper entitled In defense of extreme apriorism, describes the basic assumptions of praxeology as “derived from the experience of reality and [...] therefore in the broadest sense empirical” (2011, 65). And almost all contemporary Austrians have retreated even further. Boettke, for instance, writes:

The epistemological issue Mises sought to address with his insistence on apriorism, while more exotic in its philosophic treatment than his predecessors, boils down to the claim that theory comes prior to observation. We use theory to make sense of the economic world around us. The choice for the analyst is never theory or no theory, but instead always theory that has been articulated and defended or theory that remains inarticulate and hidden from critical examination. The analyst does not confront the ‘data’ pure and simple. [...] It is a mistake to believe that these arguments either claimed that the entire field of economics was a priori or that economics is completely insulated from criticism of an empirical nature (2012, 161).

I think this passage is most charitably understood methodologically: as a way of distilling Mises’s project into a more palatable Popperian insight about the appropriate relationship between theory and data in the social sciences (Iorio 2015). It is not that there is no such thing as economic data, or that such data cannot make contact with economic theory in virtue of the kind of thing it is, as Mises himself sometimes suggests. Rather, economic theory should be regarded as prior to data,

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7 For more on this way of defending Mises, see Long (2004), Boettke and Leeson (2006), Long (2006), and Lavoie and Storr (2011).
and that this priority is worth understanding and taking seriously. I agree.

I do not want to be misread as admitting to having been attacking a triviality. Even if extreme apriorism doesn't endure in content—and so, no one is to blame for its persistence—it still endures in spirit. It should be eliminated. Once it is gone, the mainstream will be free to share its data with the Austrian school, and the Austrians will be free to share their theory in return.

III. CONCLUSION

I think there are important lessons to be learned here, both for those broadly sympathetic to the Austrian agenda and for those not.

As Solow noted, the interests of the Austrian school—to examine the nature and dynamics of human action—are far more comprehensive and metatheoretical than nearly all of what goes by ‘economics’ in mainstream circles. But why let methodological allegiances interfere with collaboration, ‘interdisciplinary’ or not? The Austrians are good at (and have always been good at) shedding light on what the economic mainstream could do better. Mainstream theorists would be well-served to develop a greater appreciation for coherent metatheory, the theoretical complexity of the topics of interest, and the limits of empiricism. And taking note of what the Austrians have done poorly—for instance, embracing the synthetic a priori and their own heterodoxic status to a self-destructive extent—is also instructive. It offers a good explanation of why certain philosophical ideas are better left aside.

For those with Austrian sympathies, the lesson is less cautionary. Austrian theorists should allow their theoretical frameworks to further cozy up to empiricism. It is time, in other words, for a ‘thymological turn’. Contemporary Austrians are increasingly receptive to this movement, and any further developments along those lines will serve to move economics even further toward reunification between Austrian theorists and their critics. In the end, the most important lesson for everyone is that, upon careful examination, the two camps have less to disagree about than either one thinks.

REFERENCES


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The survival of Aristotelianism in early English mercantilism: an illustration from the debate between Malynes and Misselden

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Abstract: Handbooks of the history of economic thought typically assume a strict fault line between scholastic economics and mercantilism. Historically, the distinction between the two streams of thought was less evident—especially when it came to the style of argumentation, in which there is much continuity between the scholastic doctors and early mercantilists. However, although the latter did not employ the scholastic method, both traditions frequently called upon classical authorities to strengthen their arguments. What is striking is the high regard for Aristotle among the late-sixteenth and early-seventeenth century English mercantilists. By way of illustration, this article reviews the surprising role of Aristotelian ideas, primarily from the Metaphysics and Physics, within the debate between Gerard Malynes and Edward Misselden on England's economics crisis.

Keywords: Aristotle, Aristotelianism, mercantilism, Malynes, Misselden

JEL Classification: B11, B31

I. INTRODUCTION

Of the handbooks on the history of economic thought that do pay attention to the 'prehistory' of economics, most assume a strict fault line between scholastic economics and mercantilism. Late medieval and early-modern economic thought literally comprise different chapters in the subject's history.

Of course, there are many good reasons for regarding these traditions separately. Anyone who has glanced at the economic writings from the successive periods is struck by manifest differences, which is

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revealed both in their content and their modes of presentation. As parts of voluminous theological and legal works written by university graduates, medieval economic discussions are full of erudite questions, objections, and metaphysical distinctions, and often refer to authorities from a distant past. Rather than being concerned with economic expediency, scholastic economic analyses were meant to offer moral guidance in microeconomic affairs. Mercantilist reasoning, by contrast, has come in the form of self-contained tracts and pamphlets, produced by lay writers to influence economic policy. Frequently prompted by threatened private interests, the focus is on particular economic problems at a national or international level. The descriptions of the economy put forward in these writings are mechanical and impersonal; they are much less concerned with morality than with material wealth. Finally, the scholastic method is absent, with the ideas contained therein presented in a more or less modern writing style. The products of scholastic economics and mercantilism thus are easily distinguishable.

Be this as it may, the distinction between the two streams of thought was historically speaking less evident. First, several early mercantilists of the sixteenth and seventeenth centuries were clearly influenced by medieval economic thought (De Roover 1955). Having consulted scholastic treatises, some mercantilist writings simply echoed the schoolmen’s ideas about money and trade—albeit, they did so using a modernized vocabulary and applied these ideas to new contexts. The scholastic influence is best evinced by the works of Gerard Malynes, a transitional figure who, despite his frequent allusions to the teachings of the scholastic doctors, within the secondary literature is invariably counted among the mercantilists (cf. De Roover 1974). Second, the scholastic economic tradition survived far beyond what is traditionally regarded as the terminal point of the Middle Ages (Grice-Hutchinson 1952; 1978; 1993). After the fifteenth century, scholastic treatises dealing with economic questions continued to be published for at least another century and a half; in Spain and Italy ‘economic scholasticism’ continued to flourish in the hands of theologians and jurists. The teachings of the scholastics on subjects such as money, banking, and foreign exchange were disseminated with little modification and were used to solve the problems of the modern economic world.

Interestingly, also when it came to argumentation style there was a greater continuity than is often acknowledged. For instance, both doctors of the church and the early mercantilists made frequent appeals
to classical authorities in support of their arguments. Instead of completely breaking with the past, many early modern pamphleteers “continued to venerate old saints”, as expressed by Raymond de Roover (1949, 286). Influenced by the humanist spirit of the times, they even broadened the economic intellectual horizon by adding a whole range of Platonist and Stoic authorities to the traditional Aristotelian arsenal. Among the early mercantilists, a writer like Tobias Gentleman stands out as an exemplar for his self-proclaimed lack of erudition. The son of a fisherman, he admits in his *England way to win wealth* that “I am more skilfull in nets, lines, and hookes, then in rethoricke, logick, or learned bookes” (1614, 3). Save for a mention of the ancient king Artaxerxes, the text indeed lacks any references or allusions to pre-modern wisdom. The writings of many of his mercantilist contemporaries, in contrast, abound with scholarship and learnedness.

Philosophers such as Aristotle, Plato (the “diuine Philosopher, and most Christian writer”), and Seneca, poets like Virgil and Horace, and orators like Cicero are a few examples of the authorities that were frequently cited to decorate or strengthen the main line of argument. For instance, the author of *A discourse vppon usurye*, Dr. Thomas Wilson (whom has been called a genuine schoolman), is known to have referred to the largest number of authorities in a single book. In a letter from the Bishop of Salisbury to Wilson, which was included in the book as advertisement, it is remarked that “suche weygte of reasons, suche examples of antiquitie, suche authority of doctours both Greekes and latines [...] suche learninge, suche eloquence, and so evident witnesse of gods holye wyl, can neuer possibly passe in vayne” (Wilson 1572, ‘A letter founde’). In addition to countless references to Scripture, Wilson’s dialogue is illuminated by a great number of opinions and quotations from ancient philosophers, Church Fathers, popes, scholastic doctors, and first-generation reformed theologians. To be clear, Wilson himself was not a theologian but a lawyer and government official.

A similar tendency to appeal to (classical) authorities can be observed in the writings of Malynes and Edward Misselden, together with Thomas Mun the key figures in early mercantilism. Judging from their writings, both merchants were well-versed in the classics, the schoolmen, and contemporary Renaissance thinkers alike (Finkelstein 2000a). Upon reading their pamphlets, one regularly encounters great

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1 Identical qualifications of Plato like this one from Wilson (1572, fol. 147a) can be found in W.S. (1929, 28, 109) and Misselden (1623, 73).
names like Herodotus, Plato, Aristotle, Seneca, Pliny, Plutarch, Virgil, Horace, and Aquinas, as well as modern writers like More, Bodin, and Grotius. Malynes, who is believed to be self-taught, drew from common and civil law, English histories, and modern scientific insights with the same ease. In one lengthy digression in his magnum opus—on the legendary philosopher’s stone—he apologizes to his readers for the reason that the subject “which being farre from merchants profession, I hope shall not giue offence to the reader of this booke” (1622a, 258). A similar apology can be found in the second edition of one of the pamphlets of Misselden, whose education is unknown to us (though he sent his son to Emmanuel College in Cambridge). “Some men aske me”, he writes in the introduction, “quorsum haec iactura? Wherefore all this cost and wast of learning & languages, in the trodden way of trade? […] as if it were not contingent to a merchant, to be acquainted with the muses” (1622, ‘To the reader’). For the sake of illustration, he insists, “learning and languages are an appendix not unnecessary to the facultie of a merchant” (ibid.).

What is striking amidst all the displays of learnedness by Malynes and Misselden is their high regard for Aristotle, “the Philosopher” for the scholastics. Though they were far from the only writers to lean about the “sharpest philosopher of witt that there ever was”—as one sixteenth-century writer on economics had put it (W.S. 1929, 109)—the Greek philosopher plays a more significant role in their works than elsewhere. Malynes’s observation that the Stagirite lived in the “infancy of traffique” (1622b, 38; 1622a, 316, 486) did not prevent either of them from presenting several Aristotelian ideas as truisms suitable for the modern commercial age. For example, Malynes time and again repeated Aristotle’s distinction between natural and artificial riches (Politics 1256a1 ff.), and attached great importance to Aristotle’s idea of money as mensura publica rather than a source of gain (Nicomachean ethics, 1133a7-b28; Politics, 1257a7-b17). He moreover stressed the necessity of distributive justice among members of a commonwealth and commutative justice in the commerce and traffic between nations (Nicomachean Ethics, V), two Aristotelian measures “ordained by God amongst men, to defend the feeble from the mightie” (1603, 2). Misselden, in turn, quoted the Philosopher in order to demonstrate that trade arose from the natural order of things (Politics, 1257a7-41) and is

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2 Various Aristotelian notions in Malynes and Misselden, including the idea of a “balance” of trade, are discussed in Finkelstein (2000a, chaps. 2, 3, 5).
therefore pleasing to the creator. In searching for a definition of monopoly he mentions some of Aristotle’s examples of this kind of restrain of the liberty of commerce (*Politics*, 1258b41-59a36).

In this article, the controversy between Malynes and Misselden and the role therein of Aristotelian ideas will be further reviewed. This specific case is meant to illustrate that, contrary to what is often suggested, not only the scholastics (to whom Aristotle was the economic authority par excellence) but also the early English mercantilists frequently reasoned from authorities like Aristotle. What is overlooked in the otherwise well-documented³ debate between the two is that Malynes and Misselden went beyond the Aristotelian commonplaces about money and trade just mentioned. Their theories were based not only on the *Politics* and *Nicomachean ethics*—two texts that formed the foundation of scholastic economics and remained influential well into the mercantilist age—but also on Aristotle’s *Metaphysics* and *Physics*. Despite their fundamentally different economic outlooks, both writers adhered to an Aristotelian theory of causality and used Aristotle’s doctrine of the four causes to analyse the nature of trade. Before discussing the role of these lesser-known Aristotelian ideas in the writings of Malynes and Misselden, I will first make some introductory remarks on the debate and the debaters more generally.

II. THE OLD WORLDVIEW VERSUS THE NEW

The debate between Malynes and Misselden took place in 1622 and 1623. Gerrard (or Gerard de) Malynes (fl. 1585-1626) was an Antwerp-born assay master at the mint and commissioner of trade, whereas Misselden (fl. 1615-1654) was a descendant of a family of Hackney merchants and prominent member of both the Merchant Adventurers’ Company and East India Company. The debate resulted in four lengthy pamphlets.⁴ Though, in response to the acute economic crisis experienced

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³ Detailed accounts include Johnson (1937, chap. 4), Gould (1955), Supple (1959, chap. 9) and Muchmore (1969). See Elmslie (2015) for a recent study that discusses the influence of Malynes and Misselden among others.

⁴ Incidentally, Mun also contributed to the debate on England’s economic crisis. His *A discourse of trade, from England vnto the East-Indies* (1621) in defence of the East India Company does not explicitly refer to the writings of Malynes or any other author, and therefore will not be taken into account in this analysis. As one commentator rightly observed, Mun was a “merchant pure and simple, with no claim to scholarship” (Beer 1938, 147). It is only in his *England’s treasure by forraign trade*, published posthumously, that Mun discussed the books of Malynes. “I find him skilful in many things which he hath both written and collected concerning th’ affairs of merchants”, he writes, “but where he hath disguised his own knowledge with sophistry to further
by the English in the early 1620s, Misselden started the public quarrel by criticizing one of Malynes' treatises, which had been published as a report twenty years earlier for the government commission on foreign exchange. It is, Misselden claimed, as if Malynes tried to cure one economic disease with another, stating that, “contrary to our Saviours argument, that Satan cannot cast out Satan” (1622, 105-106). The disease with which Malynes was concerned in his early publications, and which remained central to the later debate, was England's chronic shortage of money. This problem was thought to be caused by an outflow of coin and specie, and had previously been attributed to either the low domestic prices, the decline of foreign trade, or the rise of unemployment.

Obviously of concern to both English writers, Malynes and Misselden profoundly disagreed about the underlying causes of the kingdom's "want of money". Given their different ideological premises and mutual accusations of ignorance and plagiarism, the polemic involved more than pure economic differences of opinion (Appleby 1978, chap. 2; Seligman 2000, 668-671). In possibly siding with Mun's verdict that Malynes was wrong, most commentators agree that Malynes held an "old" view concerning international economic affairs, while his opponent embodied a "new" one (Johnson 1933, 442). While these differences may have been exaggerated, the writers clearly had different economic outlooks: Malynes harkening back to the medieval world and Misselden anticipating a modern milieu. However, both men still agreed on the status of the king. As a representative of the almighty God, his duty was to watch over the welfare of the "microcosme" of the "great body politique" of the "weale publike" (Misselden 1622, 4). Next to increasing his own revenue, it was the duty of the king to promote the Christian religion and the material wealth of his subjects. Misselden defines the public good in terms of a flourishing trade, an improvement in navigation, and employment of the poor. Malynes, however, emphatically holds the monarch responsible for establishing economic justice and equality, consistent with the "lawe of God and Nature" (1603, 4). These typically medieval standards, which were based on book 5 of Aristotle's *Nicomachean ethics*, formed the basis of all Malynes' writings and marked a clear contrast with Misselden's more 'worldly' concerns.

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some private ends by hurting the publick good; there ought he to be discovered and prevented" (1664, 109-110).
The diverging worldviews of the two writers translated into what may be seen as technical disagreements about England's economic crisis. In summary, Malynes traced the source of all evil to the usurious behaviour of “bankers”, i.e., people involved in foreign exchange. Instead of using bills of exchange (in the Aristotelian sense of public measure) between different countries, he argued that bankers abuse foreign exchange for their own gain. By the incorporation of usury, manipulation of the rate of exchange, and all kinds of speculative constructs, foreign exchange had become a merchandize itself. By controlling the exchange rate and systematically undervaluing the English coin (placing it below its par value as set by the mint), bankers were able to secure their own gains. It is this undervaluation, Malynes maintained, that caused the exportation and outflow of coin and specie. The only remedy was to restore the Royal Exchange in London, and to forbid all sales under the true value of exchange (par pro pari). Contrary to Malynes, Misselden claimed that the shortage of money was not caused by “merchandizing exchange”, as his opponent called it, but by England's negative balance of trade. It was the consumption of luxury goods, importation of East-India stock, and exportation of low-value cloth, among others things, that restricted the inflow of money and induced its outflow. Whereas Malynes believed that the international course of commodities and money was overruled by foreign exchange, Misselden conceived of exchanges as something passive, the price of which was determined by laws of supply and demand.

The question of foreign exchange was one of the central concerns of early mercantilism as a whole (De Roover 1949). The malicious behaviour of bankers and exchange-dealers had already triggered some medieval commentators. Also in the second half of the sixteenth century several texts dealing with the issue were published. Building on these earlier writers, Malynes and Misselden proceeded from a similar strategy to resolve the nation's economic problems. Consistent with the intellectual fashion of their days, they conceived of the English commonwealth as a diseased body that could only be cured by administration of the right medicine. The medical terminology was first introduced by Malynes in his Treatise of the canker of Englands common wealth (1601b), where “canker” referred to an overbalance of foreign commodities with home commodities resulting in a decrease of wealth and exportation of money. According to the treatise’s subtitle, “the author imitating the rule of good phisitions, first declareth the disease.
Secondarily, sheweth the efficient cause thereof. Lastly, a remedy for the same”. Before any remedy can be applied, the unknown disease of the “body politic” and its efficient cause must be diagnosed. Misselden in his first contribution similarly speaks of the “sicknesse of the trade”, which if the causes are mistaken or remedies ill-applied, “may be brought from a disease in fieri to an habituated and in facto as the phisitians schoole hath it” (1622, 6).

III. ARISTOTELIAN (META)PHYSICS

Underlying Malynes’s and Misselden’s (then popular) medical approach is a highly mechanical theory of causation, one which was typical for this phase of mercantilism (Heckscher 1955, 308-316; Spiegel 1991, 96). Instead of being concerned with formulating ethical standards, as was the case with their predecessors, the economic pamphleteers explained the economy in terms of impersonal causes and effects. Often the belief in the existence of socio-economic causality was imbued with an emphasis on government intervention, by which the causes of economic ills could be identified and controlled. Sticking to this idea, the writings of Malynes and Misselden form a restless search for the true causes of England’s economic crisis. It was thought to be effected, in the literal sense of the word, by prior flaws in the economic chain of events. In order to prevent these causes from exercising their harmful effects, they needed to be suppressed and removed through government intervention. Sublata causa, tollitur effectus, as can be read on the title page of Treatise of the canker: remove the cause and the effect will cease. The Latin phrase and its English rendering occur no less than six times in Malynes’s writings and are explicitly presented as saying of “the Philosophe, […] graffed in euery mans iudgement” (1601b, 3). The same saying is mentioned in Misselden’s Free trade. It is of vital importance to inquire into the causes of the decay of trade, the author states, “for the causes being remoued, the effects must needs cease, according to the common maxime in philosophy, sublata causa tollitur effectus” (1622, 102).

5 Cf. “the cause of any thing being taken away, the effect is taken away withall” (Malynes 1601b, 3; cf. 16); “take away the cause, and then the effect will cease” (1603, 93 [mismatched]; cf. 156); “This cause being preuented, maketh the effect to cease; and this is engraffed in euery mans iudgement, according to the maxime often noted heretofore, sublata causa, tollitur effectus” (1622b, 14); “sublata causa, tollitur effectus” (1623, 50).
Unfortunately, Malynes—the first to postulate it in the debate—did not provide a source for this maxim. It is likely, though, that he borrowed it from *A compendious or briefe examination* (1581), an early mercantilist dialogue that discusses enclosures, high prices, and international trade among many other economic subjects. In the dialogue, which is better known under the title of *A discourse of the common weal of this realm of England*, the maxim appears three times and is likewise presented as a saying of the Philosopher. With regard to the increasing dearness of things, it is remarked that, “for knowinge the occasion of the griefe, a man may soune avoyde the same occasion; and that beinge avoided, the greife is also taken awaye; for as the Philosopher saithe: Sublata causa tollitur effectus” (W.S. 1929, 97, cf. 99, 100, 121). However interesting this earlier occurrence may be, Misselden’s reference to a “common maxime” suggests a wider dissemination. A quick search through sixteenth-century sources indeed reveals many other applications, mostly in legal and theological contexts. An unexpected place where the same idea was voiced was sixteenth-century English drama (Dent 1984, 241). Sometimes the maxim was expressed in different terms, for example as *ablata causa tollitur effectus, remota causa removetur effectus* or, mainly in legal texts, as *cessante causa cessat effectus*. Focusing on the last-mentioned “celebrated proverb”, the French jurist André Tiraqueau (Tiraquellus) in 1551 even published a book on civil law entitled *Tractatvs cessante cavsæ cessianc effeçtvsvs*. Hence, the use of the maxim was not limited to economic texts.

Even though in scholastic and humanist discourses “the Philosopher” almost invariably referred to Aristotle, it may of course be that the mercantilist writers used the phrase in a looser sense. The maxim would then be a common saying of philosophers in general. This impression is reinforced by the fact that the maxim, or any equivalent, cannot be traced in Aristotle’s works. The closest to a credible source is an observation from Aristotle’s *Rhetoric* (1400a25), that “if the cause is present, the effect is present, and if absent, absent. For cause and effect go together, and nothing can exist without a cause” (Aristotle 1984, 2231). But there is no mention here that in order to remove an effect, one must seek to remove the cause. Yet not only Malynes but also the anonymous author of the *Discourse* and another writer by the name of Richard Eburne (1624, 12) deliberately capitalized the term “Philosopher”, thereby suggesting a connection to the Greek
philosopher. And rightly so, because even if the phrase sublata causa tollitur effectus did not come from the pen of Aristotle himself, it is not farfetched to call it an Aristotelian maxim. The idea that an effect will cease when the cause is removed was one of the scholastic axioms of causality, which were based on and largely consistent with Aristotle’s philosophy (Aveling 1909, 463; Söllner 1960, 187). The maxim is frequently mentioned, be it in alternative wordings, in commentaries on various works of the Philosopher, including Thomas Aquinas’ Sententiae metaphysicae (V, l. 3). In Aquinas’ immensely influential Summa theologicae, the maxim occurs many times in one of the formulas mentioned before, for example, as one of the premises in his cosmological argument for the existence of a first cause (Summa Ia, q. 2, a. 3).

When the economic crisis is conceived of as an effect of one or more causes, Malynes observes, “the remedy is easie” (1601b, 99). Once the true causes have been successfully identified, it is up to the king to adopt the appropriate measures to take them away. The difficulty, however, is to discriminate between efficient and secondary causes. The “first and principall cause of putting forward all the rest afore him”, Malynes argues, consistent with Aristotle’s Metaphysics (1013a24-1014a25) and Physics (194b16-195b30), “[is] called causa efficiens, which not being rightly discerned from the meane causes, made that many men were neuer the neare to remedy the thing they went about” (1601b, 96). Alternatively, Misselden establishes that, although over time many causes of the decay of trade have been discussed and discoursed, the problem still awaits proper analysis. “To find out the causes of things”, he believes, is no less than “a worke of philosophy” (1622, 6). The trade of the commonwealth can only be reformed if the crisis is first analyzed through “deformation” or decomposition into its constituent parts.

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6 There have been several attempts to pinpoint the origin of the maxim. Ultimately it can be traced back to Roman law (Krause 1960) and, as a basic philosophical principle, further to Aristotle’s Physics and Posterior analytics (Gouron 1999). According to Krause, although during the Middle Ages the maxim was converted to a common rule, the “geistigen Hintergrund bildete Aristoteles” (1960, 86; cf. Nederman 1987, 33). Tiraqueau, a Renaissance writer sensitive to history, similarly believed that it had once been taken from book 2 of the Physics: “Estque ex lib. 2 Physicorum Aristotelis (ni fallor) deprompta” (1559, 8). A different, more recent suggestion that sublata causa is one of the medical aphorisms of Hippocrates (Forget 1854) is widespread but lacks an original source.

7 The anonymous Policies to reduce this realme of Englande vnto a prosperus wealthe and estate (1549) speaks of “the moste aunciente and trewe principle in phisike: cessant causa, cesset effectus [sic]” (Tawney & Power 1924, 341).
Whereas Malynes maintains that there are several accidental secondary causes and only one efficient cause, namely the “canker” of merchandizing exchange, Misselden's philosophical method yields a variety of causes believed to play a role. In addition to distinguishing between causes in matter and in form, he discusses respectively “immediate” and “mediate” (or remote) causes for the want of money and “deficient” and “efficient” causes of the decay of trade.

The Aristotelian method of decomposition employed by both authors was not new. Half a century before, the same strategy to find appropriate remedies for economic diseases had been proposed by the author of the Discourse of the common weal. In the opening pages of the third dialogue, which presented a remarkably detailed theory of causation for the time, it is explained that there are different sorts of causes. The causa sine quibus non, material and formal cause have to be distinguished from the efficient and principal cause “with oute removeinge of which cause the thinge can not be remedied” (W.S. 1929, 99). The point is that multiple effects may have one principal cause in common. Grievances like the general dearth, impoverishment, and the process of enclosure may be brought about by different secondary causes—which explains the existence of a great “diversitie of mens myndes and opinions” (W.S. 1929, 98) about the matter—but, in fact, have a shared first and original cause. The nature of reality, according to the author of the Discourse, works like a clock in which the first wheel drives the second cog, the second cog the third, and so on until the last that drives the instrument that strikes the clock. In order to find out the efficient cause of one or more effects, all “meane” causes that are propelled by it need be left out of consideration. For only by identifying the efficient cause and stopping it from operating, the negative effects are definitively removed.

IV. ARISTOTELIAN PHYSICS

Malynes' response to Misselden's Free trade, published several months later, was again framed in terms of Aristotelian causality (see 1622a, 1-10). The “moderne merchant of Hackney” (1622a, 9), by which he now denotes his opponent, failed to truly distinguish between efficient and secondary causes. Even though Misselden was right that the want for money is one of the secondary causes of the decay of trade, in actuality...
this want was a direct consequence of the abuse of exchange. Furthermore, although the bartering, buying, and selling of commodities overseas was certainly part of the problem, what was ignored was the “mystery of exchange” (1622a, ‘The epistle dedicatory’). Interestingly, from this point Malynes’ arguments draw on book 3 of Aristotle's *Physics*. Commodities and money as such, we are told, are merely “things passive”, the course of which is determined by the course of exchange, which is the efficient cause and the “thing active” (1622a, 6). Misselden, Malynes argues, is like a novice who, when conversing with another novice about the active causes of sailing, suggested that either the winds, the sails, or the compass is the most decisive. In reality, the efficient cause that makes a ship perform well is its rudder, the other causes such as the winds and sails being merely secondary. 

In the chapters that follow, Malynes returns twice to the concepts of “activity” and “passivity” as developed in Aristotle's *Physics*. Somewhat confusingly, he now cites money, and not exchange, as the thing active. Further, he maintains that commodities are passive and that exchange determines both the flow of money and commodities (1622a, 15). According to Malynes, Misselden's theory became entangled since he failed to distinguish between the thing active and passive. Making his argument even more complicated, Malynes paraphrases Aristotle's idea from the *Physics* (202b11-14) that “action and passion” are merely relatives: each differing no more than the way from Thebes to Athens and from Athens to Thebes (1622a, 38). Does Malynes believe that commodities and money are so related that they are essentially the same? This precisely is how Misselden interprets Malynes's train of thought in a counterattack that he published the year following. He accuses the “poore man [...] that hath neither wit nor art” of having misunderstood and abused Aristotle. Quoting from both the Greek philosopher and the Italian Julius Pacius (1550-1635), a contemporary Aristotle scholar and translator, Misselden explains that only “grosse ignorance” could lead one to adopt the view that Aristotle's philosophy permits seeing money and commodities as relatives or even the same thing (1623, 40-43). 

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9 Note that the same obscure section can be found in Malynes’s *Consuetudo* (1622a, 486), but in a different context in which it is more sensible. It is possible that the author wrote it for his handbook (“the great whale”) first, on which he was working while publishing his treatise (“the little fish”) *The maintenance of free trade* (see 1622b, ‘The epistle dedicatory’).
Earlier in his The circle of commerce, Misselden already had expressed doubts about the intellectual capacities of his opponent. The first chapter opens with the following rhetorical question: “What hope can we haue of this mans treatise, when hee failes in his title?”. In Misselden’s view, Malynes erroneously referred to commodities, money, and the exchange of money by means of bills of exchange as the three essential parts of trade. As a matter of fact, of the four Aristotelian causes in nature, only the material and formal causes constitute an object’s essence, leaving no room for a third factor. By treating commodities and money as the matter of trade and buying and selling as its form (cf. Misselden 1622, 6-7, 53), the exchange of money can only be an essential part of trade if it is understood as a merchandise (matter) or kind of buying and selling (form) itself. Misselden goes on to quote a section from Malynes’ handbook Consvetvdo, vel lex mercatoria in which the author suggests that trade consists of three beings or “simples”, since the essence of objects is not only determined by materia and forma but also deprivation, i.e., “an imperfection so conioyned to the matter, that without her, if she were separated, nothing would be ingendered” (1622a, 500; see Physics I, §7-9). Again, drawing from the Physics and the commentary by Pacius, Misselden (1623, 11) accuses Malynes of having confused the principles of natural things with their essence. It is true that Aristotle reduces the principles of natural things into matter, form, and privation, but he explicitly excludes privation from their being.

In Misselden’s opinion, Malynes was not familiar with Aristotle’s works anyway. In an ultimate attempt to prove the ignorance of his opponent, Misselden accuses him of not having read the primary texts of the Greek philosopher. Speaking about Malynes’ interpretation of Aristotle, he sneers:

[…] as ill a sophister is Malynes, not to discerne privation from the essence of naturall or artificial things. Which he might haue better vnderstood, if he had beene able to consult with Aristotle, or any of his interpreters. But alas, how should hee vnderstand him or them, when hee cannot so much as translate a sentence of him out of Latin, much lesse out of the originall, into proper or significant words (1623, 12)?

Malynes, in short, has “more skill in philomythy”—i.e., the love of legends and fables—“then philosophy” (1623, 22), which is necessary to distinguish between the different kinds of causes. According to
Misselden, even his illustration of such an easy and familiar thing as navigation was mistaken: it is not the rudder of the ship, as Malynes had argued, which is the efficient cause—or *causa sine qua non*—of sailing, but the winds that fill the ship’s sails.

To my knowledge, Malynes’ and Misselden’s analyses of economic phenomena in terms of Aristotle’s four causes were quite uncommon. Yet there were forerunners, both among the early mercantilists and the scholastic doctors. For example, Jean Buridan, a pupil of William of Ockham and teacher of Nicole Oresme, in an exercise of Aristotelian (meta)physics had analysed the nature of money in terms the four causes:

The material cause is what money is made of. [...] The final cause is that man, with money, can have these things which are necessary for life. The formal cause is the figure of money, and the sign of the weight of money of such value. The efficient cause is the Prince, who has the government of the city, or the community of the citizens (quoted in Lapidus 1997, 28).

While most scholastic writers on economics based their theories on Aristotle’s *Politics* and *Nicomachean ethics*, Buridan in the fourteenth century was among the few who drew from his *Physics*.

A fine sixteenth-century illustration can be found in the *Cystvmers apoloby* of Thomas Milles (1599). In this pamphlet that primarily attacks the Merchant Adventurers Company for undermining the English customs system, the author (himself a customs-officer) investigates “The causse or ground, whence such duties growe and haue their first being. The matter what, and where vpon such duties growing are to be paide and taken. The persons, whome such duties either immediatly or by consequence touch and concerne. The forme how to collect such duties, fit and peculiar to the cause, matter, and persons” (1599, *The state of the cystymes*, unpaginated). The cause of duties, he explains in quite some detail, is traffic. The matter, that which duties are taken upon, is merchandise transported over sea and imported in the country. The form, finally, is the manner of collection fit to the matter and persons (the prince, merchants, and customs-officers) involved. Since “cystymes follow traffick as the effect doth the causes” and costumes enrich the prince and the commonwealth, to promote a just international trade based on the rules of reciprocity and equity is crucial. “Al effects”, Milles claims with a rule reminiscent of *sublata*
causa tollitur effectus, “work only by & liue or dy with their proper cayses”.

V. CONCLUDING REMARKS
This article exposed the role of Aristotelian ideas, directly and indirectly derived from Aristotle's *Metaphysics* and *Physics*, with regard to the controversy between two of the best-known mercantilist writers, Malynes and Misselden. Commentators may be right that of the two, Malynes was most attracted to the “old” philosophy. As Andrea Finkelstein remarks, despite being a “voracious reader who sampled every school of thought his age had to offer”, Malynes “remained at heart an Aristotelian” (2000b, 26). Like scholastic economics, Malynes’s work was still very much concerned with economic justice as an indispensable means to preserve the harmony of the commonwealth. In addition to his disgust for profit-driven foreign exchange, he also wrote extensively on the evils of usury. His early work *Saint George for England, allegorically described* quite characteristically features a dragon, “called *foenus politicum*, [whose] two wings are *usura palliata* and *usura explicata*, and his taile inconstant *cambium*” (1601a, ‘To the reader’), which brings inequality and deprives the prince of his wealth.

Misselden, by contrast, altogether ignores the issue of justice. Well aware of the new economic reality of the modern age, he wanted to leave foreign exchanges alone and advocated (limited) freedom of international trade. Nevertheless, his theory of causality and discussions of matter, form, and essence are unmistakably Aristotelian. Apparently, Misselden’s encounter with radical opposition to Aristotelianism by the “famous logician of France” Petrus Ramus (1623, 72) did not stop him from employing an Aristotelian framework (Magnusson 1994, 76).

The case of Malynes and Misselden, and the examples from other writers provided in this article, is evidence for the more general claim that Aristotelianism survived in early English mercantilism. The term ‘Aristotelianism’ should not be taken too literally, however. Whatever its exact definition, Malynes and Misselden were neither members of an Aristotelian school, nor followers of Aristotle, nor commentators on his oeuvre. They at most subscribed to his philosophy and drew inspiration

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10 Max Beer went even further by observing in Malynes’ writings a “self-imposed mission to uphold and spread the principles laid down by Aristotle, [and] the Schoolmen” (1938, 146).

11 On the literary background of this book, see Sandison (1943).
from his works. In discussing the views of the mercantilists on the nature of money, Eli Heckscher (the author of the standard work on mercantilism) has rightly observed that “[a]lmost everything that they stated on the matter had age-old roots reaching back to Aristotle and the schoolmen, but what is important is that they held fast to it” (1955, 260). This article showed that the same was true for other subjects. Early mercantilists such as ‘W.S.’ (i.e., the author of the Discourse of the common weal), Milles, Malynes, and Misselden attached importance to what Aristotle had declared about causality and implemented his ideas in their discussions of trade and commerce. The Greek philosopher in post-medieval economic thought still figured as a viable authority.

In a sense this conclusion is not a surprising one. The early English mercantilists wrote in a period when the Aristotelian worldview still dominated knowledge and science. The works of Francis Bacon and René Descartes, which helped to pave the way for a revolution in natural philosophy and economic thought alike (Letwin 1963; Webster 1975; cf. Leng 2014), were either not yet published or still had yet to gain momentum. All the same, the Aristotelian preoccupation of some early pamphleteers calls into question an all-too strict demarcation between mercantilism and scholastic economics as presented in some handbooks on the history of economic thought. Particularly with respect to argumentation style, both the scholastics and early mercantilists wrote in a vocabulary that gathered inspiration from Aristotle. To suggest, therefore, that only the former reasoned from authorities and the latter were empirically-minded, or to assume that scholastic influences were only present in the School of Salamanca, is historically inaccurate. The debate between Malynes and Misselden exemplifies that Aristotle’s philosophy, which was once so fundamental to scholastic reasoning, was far from played out in the early years of mercantilism. An interesting thing is that they not only borrowed from Politics and Ethics, but also from Metaphysics and Physics.

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Philosophy without borders, naturally: an interview with Harold Kincaid

Harold Kincaid (Indiana, United States, 1952) is a philosopher of the social sciences in the School of Economics at the University of Cape Town (UCT). Before South Africa, he was in the departments of Philosophy, Sociology, and Epidemiology at the University of Alabama at Birmingham. He was chair of the International Network for Economic Methodology between 2008 and 2010. He obtained a PhD in philosophy (1983), with a minor in economics, from Indiana University and University of Heidelberg.

Harold Kincaid’s research interests are wide-ranging. While he started his career tackling more general philosophy of science issues, his work addresses topics in the philosophy of the social sciences, philosophy of economics, naturalism, philosophy of medicine, and he also conducts empirical work. He is the author of Philosophical foundations of the social sciences: analyzing controversies in social research (1996) and of Individualism and the unity of science: essays on reduction, explanation, and the special sciences (1997). He also edited or co-edited a large number of books: Toward a sociological imagination: bridging specialized fields (Phillips, Kincaid, and Scheff 2002), Value-free science?: ideals and illusions (Kincaid, Dupré, and Wylie 2007), Establishing medical reality: essays in the metaphysics and epistemology of biomedical science (Kincaid and McKitrick 2007), Distributed cognition and the will: individual volition and social context (Ross et al. 2007), The Oxford handbook of philosophy of economics (Kincaid and Ross 2009), What is addiction? (Ross et al. 2010), The Oxford handbook of philosophy of social science (2012), Scientific metaphysics (Ross, Ladyman, and Kincaid 2013), Classifying psychopathology: mental kinds and natural Kinds (Kincaid and Sullivan 2014), Routledge companion to philosophy of medicine (Solomon, Simon, and Kincaid 2017). His work has been published in Philosophy of Science, Journal of Economic Methodology, Philosophy of the Social Sciences, Synthese, The Monist, and Analyse & Kritik, among others. He currently carries out a research funded by the South African National Research Foundation titled Understanding

EJPE’s Note: this interview was conducted by Philippe Verreault-Julien. He is a PhD candidate at the Erasmus Institute for Philosophy and Economics and co-editor of the Erasmus Journal for Philosophy and Economics. His research concerns the epistemology of economic modelling.
addiction: using economic experiments to understand the dynamic of tobacco smoking.

The Erasmus Journal for Philosophy and Economics (EJPE) interviewed Harold Kincaid about, amongst others, what led him to have this diverse set of interests, his views on laws and causality, the relationships between philosophy and science, between economics and the social sciences, and conducting empirical work.

EJPE: Professor Kincaid, most of your formal education is in philosophy. What brought you to be interested in the social sciences in general and to economics in particular? Were you interested in them from the beginning or is it something that you developed during your studies?

HAROLD KINCAID: I started out as a political science major as an undergrad and was involved in various left wing social causes at the same time (this was in the early 1970s when the civil rights, anti-war, and women’s movements were very present), so I had a deep interest in the social sciences from early on. I found the US political science of the time however not that interesting or rigorous and had some really terrific philosophy mentors so switched majors. Then in grad school I was required to have a minor and I did economics. So I have always had a deep interest in the social sciences and my philosophy interests were primarily in philosophy of science, thus the two combined naturally.

And were you attracted to economics because of some perceived shortcomings of the discipline, perhaps similar to those you identified about US political science, or because you thought it was by and large successful? And what did you think back then was the role of philosophical reflection with respect to the social sciences?

I found the models very rigorous compared to political science at the time (political science has much improved in this respect, in no small part by borrowing from economics). But, yes, I also went into my economics education as a critic, because I thought economics left out much—what sociology provided such as class and social structural analysis—and because there was little concern about dealing with the problems of highly unrealistic models. I had a heavy dose of Quine from age 18 on, so I always thought philosophy of science was continuous with and constrained by the science. I don’t think I thought philosophy
was going to fix the problems of economics, but then it was clear philosophy of science issues were there and needed to be addressed.

**Do you consider that the criticisms you had towards economics have been dealt with satisfactorily during the past thirty years? For instance, do you see economics as having incorporated relevant insights from sociology? And do you believe that philosophy was instrumental in bringing about (positive) changes to economics, or these were mostly internal to the discipline?**

Well, no. I think the insights incorporated from sociology are pretty thin and uninfluential. Basically it is one insight—that institutions matter. Yet the notion of institution is basically the rules of the game. I am no historian of economics, but my guess is that the development of game theory was more important here—the rules of the game, of course, have a natural game theory interpretation—rather than borrowing from sociology. I do not think philosophy had any influence on this.

**Especially in your earlier work (Kincaid 1988; 1990; but also 2004), you defended the existence of social laws. Do you still think they exist? How have your views evolved on this topic?**

I think my views have “evolved”. Maybe that is just a weasel word for “completely changed”, but I do not think so. From the beginning of my little career I have thought that the social sciences can pick out causes but in a piecemeal, stepwise process. So in the 1996 book I argued that claims qualified ‘ceteris paribus’ could still be confirmed if we could show that unknown factors had minimal influence, that claims held up whatever the complicating factors were, etc. I did not think and still do not think that laws are the main issue. Instead, to me the big issue is when and where we can have reasonable evidence about causes. Knowing that A causes B probably entails some kind of law, but I do not think we need first to confirm the law to have evidence for the causal claim.

**So you would agree with Hausman (2009), for instance, who says that ‘causation’ is a more useful concept for philosophy of economics than the one of ‘law’? And what are the approaches to causation you find promising for economics in particular and for the social sciences in general?**
Yes, I certainly do think causation is more useful and what gets at the law notion in practice is “well established association”. I do not think causal notions and inferences have gotten much of a foothold in economics. Economists have randomized control trials (RCTs) and natural experiments, but outside of that, they still talk of “determinants” and other ambiguous terms, and cite the ‘correlation is not causation’ mantra, all the while making policy recommendations—which is inconsistent, in my view.

There are two things to note about causation in economics: First, the Pearl (2009) et al. framework is a revolution and we now know how to formulate and test causal claims with a rigor we did not have 20 years ago. Unfortunately, economics has been very slow to take advantage of these advances. Economists will talk about “endogeneity” and use instrumental variables to deal with it, but the instrumental variables framework was and still is largely motivated by concerns about consistent estimation, not causation (why we care about whether inferences converge in infinite samples escapes me by the way). Explicit causal models are still fairly rare. It is still standard to assume that the right hand side variables are each independent causes when that is obviously false. The cross-country growth regressions are an egregious example—there is no way that education, rule of law, and so on do not causally interact. Explicit causal models can try to deal with this but they are still rare. And an enormous simple fallacy shows up across the field. If cause A influences outcome E only, or partly by influencing intermediate M, then standard multiple regression techniques will conclude that A has no effect or much less effect than in fact is the case. Controlling for an intermediate cause removes the correlation between the distal cause and the effect. This is a huge problem in much applied work.

Then as much as I like Pearl-type models, it also seems that they assume a picture of causation that sometimes/often may not hold in the economy. The causal reality may involve moderating causes (causes that do not cause the effect, but influence how much influence various causal factors have) as well as necessary causes (factors that have no independent influence but are part of the causal constellation as it were), thresholds (nonlinear relations), and a host of other complexities. The Pearl framework does not easily handle these, though maybe useful extensions are possible—to me, the jury is still out on these issues.
Are you then mostly skeptical of inductive approaches such as “mostly harmless econometrics” (Angrist and Pischke 2008)?

Well these are complicated issues and if I had this all figured out I would have published a block buster article. Maybe that will happen soon... I would look at articles by Deaton (2010), Cartwright (2011), and Harrison (2011) among others. A first point: the economics literature on instrumental variables is a mess. Instrumental variables (IVs) were first introduced to handle consistency issues in the statistical technical sense of converging in infinite samples (I ask again in my Bayesian mode while I should care about samples I will never have?) and then slowly and ambiguously morphed into solutions for dealing with confounding by variables economists do not include, because they do not have measurements or even know what they are. The literature still is not clear on this. If I can find an IV that I am sure does not cause the dependent variable and does cause the effect variable, then great. But that is hard. Many empirical studies, e.g., in the growth literature (Barro [1998] is a prime example), never show that and are not even aware of the problem. It seems to me that “structural models”—ones that do not claim to mimic randomization but try to test a full causal model against the data—are just as likely to be convincing. I am pretty sure that a claim that inflation rates over three hundred percent reduces growth has more evidence going for it than any economics result from an RCT. But, again, these are complicated issues, and the best I can do is to put some bugs in peoples’ ears, as we would say in the US Midwest where I grew up.

You have been an active participant in debates over methodological individualism and reductionism (e.g., Kincaid 1986; 1990; 1993; 1997; 2014). How would you qualify the progress we have made in our understanding of these issues in the past decades?

I think four things have been conclusively demonstrated. This does not mean everybody has gotten the message, especially social scientists. The first result that has been established conclusively is that even if society is composed of individuals (and the story is really more complex than that), nothing inevitably follows about our ability to explain in terms of individuals. This is an instance of general moral about parts, wholes, levels, and explanation that was first shown in debates over physicalism in the 1970s—e.g., Putnam’s peg example—and has been elaborated on in various ways since.
Secondly, reduction in the Nagel (1961) bridge law sense in the social sciences from social explanation to purely individualist explanation is quite implausible. It is not clear there are any successful cases and a variety of reasons it is unlikely to be done, chief among them simply that good social explanations are almost always of individuals and institutions, organizations, and other social entities in mutual interaction.

A third result is that individualism as a claim about mechanisms is really a bunch of independent claims, some of which are very plausible and others that are not. For example, in the social sciences, as in science in general, there can be well-confirmed explanations entirely in macro terms, but supplying detail at the micro-level is usually evidentially and explanatorily useful. I think we have learned that quick, blanket pronouncements about mechanisms and individualism are not helpful.

Fourthly, there are many current empirical debates in the social sciences about how far we can go in explaining in terms of individuals and how much social organization, social structure, etc. do we need. I have argued in a couple of recent pieces (e.g., Kincaid 2014; 2015) that many social science controversies turn on this question and that the evidence has to be assessed case by case and in a modest way that says ‘here is how things seem at the current time’. Sometimes we can be relatively individualist, and other times not.

Are there issues in particular where you wish social scientists, especially economists, would have received the message? Or are there areas where you think a commitment to methodological individualism impedes rather than foster scientific progress?

I have a long list that I keep. Two important and obvious ones that interest me now are macroeconomics and studies of race. Dynamic stochastic general equilibrium (DSGE) models dominate macroeconomics so far as I can see and have had pretty unfortunate consequences—they are strongly motivated by the idea of individualism, though what they do is a bit of a farce if you want to have individualist foundations, since the ‘individuals’ are representative agents. In studies of racial disadvantage, individualism shows up as an emphasis on individual attitudes leading to discrimination and individual characteristics such lack of human capital leading to differential outcomes for blacks and whites in the US. These factors are not irrelevant, but they miss really important structural, macro forces—
spatial segregation and character of neighborhoods, quality of schools, government policies on sentencing, etc. in the present, and then a host of past institutional/social forces—slavery, legalized discrimination in education, housing, employment, wages, and so on, that have systemic effects now and will indefinitely.

**And to what extent do you consider that behavioral economics deviates, or is consistent with methodological individualism? Does it sufficiently take into account the social embeddedness of individuals?**

Nice question which I have not thought much about and do not know that others have either, but I may be missing some literature. In very general terms, to the extent that behavioral economic models have a richer preference structure, then they implicitly presuppose a more robust social influence to explain them. Framing effects would be a prime example. Yet major criticisms of behavioral economics along the lines that in real market environments laboratory results supporting behavioral economic models do not hold up because the social environment channels behavior to a more traditional rational choice pole might seem to argue in the opposite direction.

**You have in various occasions (e.g., Kincaid 1993; 1996; 1997; 2014; 2015) warned against philosophical blanket claims in the individualism-holism debate. Yet, you also seem to believe philosophers have a genuine contribution to make. Could you elaborate on how you see the specific contribution of philosophy to this debate?**

If philosophers want to provide an all-purpose answer on the individualism-holism debate by conceptual analysis or whatever, they will not add much. Yet if they get into the nitty gritty details of social explanations, they certainly can help with much more localized claims. Philosophers I still think are well trained to sort out theses at issue and the kind of arguments made for them. For example, Don Ross has done some nice and interesting work on versions of individualism in economics (e.g., Ross 2005). So has Kevin Hoover (2001; 2008)—who is a closet philosopher—on microfoundations. Economists and social scientists in general are still inclined to give weak meta-empirical arguments for or against—for example, the argument mentioned before that society is composed of individuals and therefore everything can be explained in terms of individual or the holist argument that society
causes or constrains individuals and therefore explanations in terms of
individuals must always fail. Philosophers have helped to keep social
scientists honest when they invoke bad philosophical arguments of this
sort.

This seems to be related to the more explicit naturalist position you
have advocated recently (e.g., Kincaid 2012; Ross, Ladyman, and
Kincaid 2013). Could you tell us what sparked this interest in
naturalism (Quine perhaps?) and what is it, for you, to be a naturalist?
It is hard to give a sufficiently careful formulation within the context of
an interview, but naturalism for me as a general thesis—not one
specifically about the social sciences—is just the claim that there is no
specifically philosophical method or knowledge independent of
scientific evidence. Science and philosophy (where ethics and political
philosophy fit in is a big question I will not pronounce on) are
continuous. There are lots of avowals of naturalism out there among
philosophers where all the doctrine comes to is that you should
consider scientific results “relevant” to philosophical concerns. Well, on
a total evidence or Bayesian perspective, that is trivial and no real
constraint. So, on my view, attempts by scientific realists or antirealists
to provide general arguments showing that science is or is not believable
are misguided. Philosophers are in no position to show such things
(illusions of grandeur sparked by being in a profession without data?).
Philosophers can make good arguments about what the data show or do
not show in specific instances, I think, and to me that is the way to go.
The general view I am advocating here gets a very nice presentation in
Penelope Maddy’s Second philosophy (2007). Yes, Quine is behind much
of this, but as Maddy argues, Quine can be improved on in these
regards.

Your own work has often been about very general concepts like ‘laws’,
‘individualism’, ‘explanation’, ‘realism’, etc. How do you conceive the
prima facie tension between naturalism and the philosophical
investigation of these concepts?
Off and on I get lulled into doing general conceptual analysis (that was
my training in large part, so I cannot help but look for necessary and
sufficient conditions if I am not careful). Yet on my more consistent
days—which I hope are the majority—I have argued that all these issues
are local, empirical ones that have no general answers. I certainly have
argued repeatedly that the individualism question is really many different questions, ultimately empirical questions, that have to be assessed case by case. My piece on inference to the best explanation (IBE) in the early 1990s (Day and Kincaid 1994) had that as its main point as have other more recent pieces on realism (Kincaid 2000; 2008), including two forthcoming pieces on realism in economics and pieces on race and class (Kincaid 2016). Also, I have several articles on causation with a similar theme (e.g., Kincaid 2009; 2011; 2012). Hopefully most of my work is about what the science shows us and how to understand (social) scientific controversies, not to give a general theory of laws, for example, which I suspect is a dead end.

*I guess we can fairly say that philosophers of economics and of the social sciences now pay more attention to how science is actually conducted. In what areas do you think this ‘naturalist turn’ has been successfully applied and the ones where it has not?*

Again, a hard question and not one I think I can pronounce on with any confidence (and there is a philosophy of social science/economics community that I value over making partly thought out judgements). The problem is telling who really is trying to be naturalist and when the label is only used rhetorically. If I am going to throw stones, the social ontology literature comes to mind immediately. Much of that has no clear connection to real social science. But I do not know how naturalist its advocates intend to be. Searle (e.g., 2009) is quite explicit in saying we need to first get clear on the social ontology before we do the social science, a very un-naturalist approach, though I assume he would describe himself as in the naturalist tradition. Certainly, there is still plenty of general philosophy of social science that is armchair conceptual analysis very distant from real social research. But on the whole philosophy of economics seems to me to have become over the last 20 years much more attuned to real economic research in good naturalist fashion.

*You have been an active participant in philosophy of the social sciences and of economics and have edited handbooks (Kincaid and Ross 2009; Kincaid 2012) on the fields. What is, according to you, the relationship between philosophy of the social sciences and philosophy of economics? How do you see the connection between the two?*
I only see difference where there is justified difference in problems, methods, etc. of the fields themselves. How much difference there is and ought to be is a great question. As a factual matter I think the differences in the social sciences has narrowed as game theory, other formal models, and experiments are much more common across fields than they used to be. A related big issue is the place of formal models that are highly idealized. That is still a huge issue for philosophy of economics, but less so for general philosophy of social science, though it is increasing important there as well. Economics has also been realizing that institutional details, social networks, etc. matter, though on my view there is still a long way to go. I have been putting this issue in terms of the slogan “How individualist can we be?” and I think it gives a way of framing lots of debates that are up in the air in economics.

You say that the differences in the social sciences have narrowed. What do you think of the discussion over ‘economics imperialism’ (e.g., Fine and Milonakis 2009; Mäki 2009; Vromen 2009)? Are the narrowing differences justified?
I wish I had a fully worked out answer but I do not—its on the to do list for my research. There are lots of complicated questions here; see for example Don Ross’s work (e.g., 2005; 2014). But here are some general considerations.

First, there is no right answer about what economics is about. Instead you have to look at various modeling approaches and ways of getting evidence and see how far they get you in explaining social phenomena.

Second, if you think of economics as providing a perfectly general theory of choice, then of course it should generalize across multiple social domains. Yet there are large caveats here even if you buy this idea. First, there is the individualism issue: many social science explanations are about macro, aggregate phenomena where the role of individual behavior and choice in explanation is up for grabs. Second of course is the behaviorist critique—if you want a perfectly general theory of choice, expected utility theory may be too narrow, though I think there is a fair amount of hype and bandwagon effects around behavioral economics. And, of course, many applications of economic rational choice models outside economics are only as if-just so stories with questionable empirical warrant I would guess.
Third, if you think of constrained maximization in markets as the key to economic analysis, as I would tend to do, then to what extent can you think of most social phenomena as market like? And more basically, to echo the work of Phil Mirowski (1994), what is a market? It is not so clear there is a defensible generic conception of a 'market'.

Fourth, a certain pragmatism however seems to be called for. If I can get interesting successful predictions out treating aggregate social entities as maximizing a utility function, then all the better. There is plenty of work like this in political science for example.

Fifth, then, a really big question to me is how far we can get in explaining markets without doing sociology. My bet is there is no general answer—it depends on the market. “New institution economics”, for example, suggests ways to bring in the social, though I think there is much more potentially to say.

How is your work in philosophy of medicine (e.g., Kincaid and Sullivan 2014; Ross, Kincaid, and Spurrett 2010; Solomon, Simon, and Kincaid 2017) related to what you have done in the philosophy of social sciences? And what have you learned about medicine that could be of interest for philosophers of the social sciences?

I think several things are relevant. The first is that explanation goes on without elaborate formalized theories; instead progress comes from piecemeal, localized causal explanations.

The second is that classification and the development of defensible categorizations is a complex and absolutely fundamental endeavor, but the basic process is one Mill or Hempel would recognize: do you have multiple ways of measuring a given concept and do the concepts allow for successful explanations. The psychometric tradition in the study of psychopathology works hard at this, though they are overly enamored of purely formal methods and do not take causality sufficiently into account—I would prefer an explicit structural equation causal approach.

The third is that classification can be quite local, pragmatic, and plural if that helps provide successful explanation; cancer is a clear case where there is not one “thing” and local context matters and that is certainly true for psychopathology.

The fourth is that classification and categories can be messy in that they may pick out well defined core groups but with lots of individuals with a fuzzy, ill-defined relation to the concept. Depression and addiction are like this. I would bet “underclass” for example is like this.
A last one is that social constructionist skeptical doubts have to be taken seriously even in the best of biomedical sciences—how the science proceeds can be a mix of social processes, some of which are truth conducive and some of which are not.

The morals for philosophy of social science and economics are just that since these are traits of biomedicine, we should not be surprised to find them to hold of the social sciences. I have a recent paper on social class (Kincaid 2016) and hopefully one coming out on race, caste and ethnicity that directly embody these ideas. But lots of other people have pursued approaches to the social sciences like these.

*It does seem then that conversely the relationship is not a one-way street; philosophy of social science can also contribute to our understanding and the conduct of the biomedical sciences. Could you expand on what you think are some key areas for contribution?*

There are multiple possible ways philosophy of social science might contribute, but to what extent or whether it has, is an open question, and haven’t looked at this carefully. Reductionism and mechanisms is one area. Lots of thought has gone into these issues in the philosophy of social science that might help deepen these discussions in biomedical science. Discussions of causation and laws is another. Both the social sciences and areas in biomedicine seem to me have various kinds of complex causes, e.g., necessary but not sufficient causes, path dependence, etc., and both are probably short on general laws, so philosophy of social science work on these ought to be relevant to philosophy of science questions in biomedical science. The role of the social in knowledge production is another and as are fact/value questions. I am sure there are more possibilities.

*Don Ross has been a close collaborator of yours for now over ten years. How did Don and you start working together?*

“Collaborating” is not quite the right word. We have only coauthored a couple of papers together. But you are right in that we have coedited a number of volumes and have been conducting empirical studies together (surveys and experiments on behavioral economics issues) for a decade and that is still ongoing. I was department chair at the University of Alabama in Birmingham, and Don applied for an opening we had; we were very lucky to get him to come. It turned out our interests and approaches overlap enormously—we only vaguely knew each other’s
work before he joined the department. But once he did, it was a natural to work on joint projects and as importantly to have regular intellectual back and forth.

The fact that you are involved in empirical studies is rather unusual for a philosopher, especially for someone who worked on very general and foundational philosophical issues. It seems natural—no pun intended—considering your naturalist position, yet few philosophers actually conduct this sort of work. How did you start doing empirical research? What relationship do you have with the other properly social scientists you are working with?

Well, yes it is no pun that my naturalist tendencies naturally lead to this, but then it is also a matter of circumstance and opportunity. My general mantra that you cannot evaluate social science on general conceptual grounds but have to look at the empirical details obviously drove me to better understand empirical methods. In the 1996 *Philosophical foundations* book I proposed some (no doubt simplistic) possible regression models for assessing functional explanations in defense of their scientific status. I have written multiple papers on growth theory and there the main source of evidence—cross-country regressions—just begs for a critical analysis of the evidence. I was teaching a graduate epidemiology course on causal inference at the same time I wrote these and it was natural to actually learn and apply those methods to actually analyzing real data.

Then, as usual, Don Ross was an influence. He got grant money for a national prevalence study of addictive gambling in South Africa and included me. That led me to learn techniques for deciding whether symptoms that we were recording should be seen as continuous or dichotomous. There are obviously more general philosophical issues about concepts, classification, etc. lurking here, and these issues tied into my interests in the status of psychiatric diagnoses. Then I moved into the School of Economics at UCT where my teaching was graduate behavioral economics and supervising empirical master’s dissertations, so again incentives to learn more empirical techniques was obvious. Decisive recently is that I have been part of an international team (the experimental economist Glenn Harrison at Georgia State is the intellectual heavy weight here) that has been successful in getting grant funding for empirical work—on experimental eliciting risk and time preferences and other behavioral economic variables among farmers,
urban poor, and addicts, with more projects waiting on grant success. In addition, I am now involved in a large German funded, four country empirical field study on clientelism with another set of social scientists.

My relation with these collaborators? Probably best to ask them! It is in part philosopher of science embedding with the natives, in part my research ranking in South Africa helps with grants, in part my collaborators are especially philosophically astute and see some value in having a philosopher of science on board, and then I think because there are inevitably philosophy of science issues lurking in these studies and so then I may have something to add. A philosopher of science who knows the empirical literature and has the analytic skills typical of the profession can make a contribution I would hope.

And has doing empirical work influenced your own thinking, or has your philosophical beliefs remained rather stable while carrying out this kind of research? What are the most valuable things that you have learned?

A major thesis I have defended for some time is that social science can be good science by the broad standards of the natural sciences. I have not changed my mind on this as a result of the empirical work, but now have a much greater appreciation of the complexities and nuances involved. Our survey work on the prevalence of pathological gambling was encouraging in this regard. We used a relatively simple nine-question screen about problem gambling and I was able using relatively novel taxometric methods to provide evidence that it picked out a distinct group of individuals with addictive gambling behaviors; that result has been confirmed in a different sample by different investigators. So, encouraging.

Ironically, maybe, our experimental work is actually less reassuring (reversing the usual experimental equals reliable and observational is unreliable trope that is common). I am impressed by the number of factors needed to draw conclusions in experiments that are important but that you cannot have much confidence in. We have lots of subjects doing risk and time attitude lotteries for real money who prefer, for example, $100 now over $500 now. Do they understand the task? Are they hiding their true preferences for some reason? In general, there are many decisions that have to be made in setting up the experiment and interpreting the data that are just not clear cut. The people I work with have many sophisticated ways of dealing with these issues. Still the
Do you have any advice for young aspiring philosophers of economics/the social sciences? What skills should they develop? What are promising or underrated areas for research?

The no brainer is to learn lots of social science and especially social science research methods. Take stats courses, qualitative methods, etc. It is hard to do this once you graduate and have lots of responsibilities. It will pay off later.

Archeology, anthropology, demography, behavioral ecology, much political science, growth theory/development economics, RCTs in development economics, macroeconomics, network theory, social psychology, and on and on. There is an enormous range of social science that has barely been looked at by philosophers of social science. My general strategy is always to look for controversies in such areas and see what philosophical tools can contribute. No doubt there are other ways to proceed, but this has worked for me.

Of all the books (fiction and non-fiction), what are three that you would recommend or had a major influence on you?
I am not sure what the dependent variable is... my beliefs? How do we measure that and is it a single continuous variable! Probably not. So completely obvious books are Kuhn’s *The structure of scientific revolutions* (1962), Marx’s *The German ideology* (1998), and Mannheim’s *Ideology and utopia* (1936). Less obvious ones are Quine’s *Ontological relativity* (1969), Michael Williams’s *Groundless belief* (1977), Penelope
Maddy’s *Second philosophy* (2007), Jeffrey Paige’s *Agrarian revolutions* (1975), and Brown and Harris’s (1978) *Social origins of depression: a study of psychiatric disorder in women*—the last two because both are very successful pieces of social science research. Pearl’s *Causality* (2009) and Abbott’s *Time matters* (2001) I would also recommend.

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In her 2012 Descartes Lectures at Tilburg University, Cristina Bicchieri addressed three questions that have been central in her research for the last twenty years: What are social norms? How can they be measured? And how can we change them? The monograph under review is a descendant of those lectures. Conceived originally as a standard academic volume—with commentaries and replies—the nature of the book changed significantly during its gestation. Since 2010 Bicchieri had been involved in a project aimed at training social workers. The project, led by UNICEF in collaboration with various NGOs, aimed at tackling practices such as breastfeeding prohibitions, female genital mutilation, child marriage, and open defecation—which all had proven to be remarkably resistant to traditional policy interventions. Thanks to this project, Bicchieri had the chance to do what philosophers rarely do, namely, to make her theory practically relevant.

Rather than a classic academic book of philosophy and social science, Norms in the wild is a partly theoretical, partly practical manual for policy-makers and social workers interested in changing people's behaviour through the manipulation of norms. The book is organised in five chapters: “Diagnosing norms”, “Measuring norms”, “Norm change”, “Tools for change”, and “Trendsetters”. The style is direct and simple, with frequent repetitions and summaries (‘take-home messages’) at the end of sections, to facilitate comprehension and recollection. Footnotes are kept to a minimum, the bibliography is essential, and importantly—Bicchieri never engages in conceptual or empirical disputes with alternative theoretical frameworks or interpretations of the evidence. Her task here is not to convince researchers, but practitioners and, presumably, politicians.

If you are unfamiliar with Bicchieri's earlier work and with related research on social norms, this is a good point to start. The monograph is well-organized, clear, delivers complex ideas in the simplest manner,
and offers an overview of how these ideas can be applied to concrete problems. *Norms in the wild* is not theoretically innocent, to be sure. But new ideas are not highlighted as such, nor are they discussed in the context of recent research. As a consequence, a sophisticated reader must do most of the work of reconstructing the existing debate herself, as well as placing the development of Bicchieri’s theory in its context.

**I. WHAT ARE NORMS?**

Bicchieri has tried to give an answer to this question for more than two decades. The key elements can be found already in *Rationality and coordination* (1993), and have been developed in later work with slight modifications. So, a curious reader will want to know whether *Norms in the wild* offers anything new from this respect.

Bicchieri’s interest in norms was originally prompted by the puzzle of cooperation. How do we explain the fact that many people—in experiments as well as, arguably, in real life—engage in cooperation in situations in which it is a dominated strategy? A standard way to tackle this question, both in the philosophical and in the scientific literature, has been to tinker with individual payoffs. The idea is that people may be motivated by factors that go beyond the narrow selfish preferences of standard economic models. Bicchieri’s original twist has been to ignore people’s moral concerns—their taste for equality, for example, their altruism, or their desire to reciprocate ‘nice’ and ‘nasty’ actions—and to focus instead on social norms. A norm is essentially a shared preference to follow a behavioural rule, conditional on the fact that other people follow that rule and expect others to do it. Her 1993 definition goes like this:

Let $R$ be a behavioural regularity in population $P$. Then more generally, $R$ is a social norm iff $R$ depends on the beliefs and preferences of the members of $P$ in the following way:

1. Almost every member of $P$ prefers to conform to $R$ on the condition (and only on the condition) that almost everyone else conforms, too.
2. Almost every member of $P$ believes that almost every other member of $P$ conforms to $R$ (p. 232).

Bicchieri’s idea was developed further in her second, very successful monograph, *The grammar of society* (2006). The *Grammar* not only contained a richer theoretical account of social norms, but also backed
it up with a wealth of evidence drawn from social psychology, experimental and behavioural economics (see Guala 2007).

In the Grammar we find the following, more sophisticated account:

\[ R \text{ is a social norm in a population } P \text{ if there exists a sufficiently large subset of } P \text{ such that, for each individual belonging to this subset:} \]

1. \( i \) knows that a rule \( R \) exists and applies to situations of type \( S \);
2. \( i \) prefers to conform to \( R \) in situations of type \( S \) on the condition that:
   a. \( i \) believes that a sufficiently large subset of \( P \) conforms to \( R \) in situations of type \( S \), and either
   b. \( i \) believes that a sufficiently large subset of \( P \) expects \( i \) to conform to \( R \) in situations of type \( S \), or
   b’. \( i \) believes that a sufficiently large subset of \( P \) expects \( i \) to conform to \( R \) in situations of type \( S \), prefers \( i \) to conform, and may sanction behaviour (from Bicchieri 2006, p. 11, with a few slight modifications to simplify the notation).

Condition 2(a) is called “Empirical expectations”, while 2(b) is “Normative expectations” and 2(b’) is “Normative expectations with sanctions”. These requirements distinguish Bicchieri’s theory of norms from every significant account proposed earlier, making individual preferences conditional on a set of connected beliefs or expectations.\(^1\) A distinctive feature of the 1993 formulation is that the beliefs in 2 are second-order empirical expectations. At first sight, the formal definition proposed in the Grammar seems to replicate this approach, except that in 2(b’) the beliefs are backed up by sanctions. A couple of pages later, however, Bicchieri asks:

What sort of belief is this? On the one hand, it might just be an empirical belief. If I have consistently followed \( R \) in situations of type \( S \) in the past, people may reasonably infer that, \textit{ceteris paribus}, I will do the same in the future, and that is what I believe. On the other hand, it might be a normative belief: I believe a sufficiently

\(^1\) Regarding 2(a), I still think it is odd to say that John’s preference to conform is conditional on his belief that others conform: unless John is an idealist philosopher, surely, he is more concerned about what Jane will do, than about his own belief about what Jane will do. Of course, John’s action—his decision to conform or not—will depend on his beliefs about Jane’s behaviour. But his preference, rationally, should not (see Guala 2007).
large number of people think that I have an obligation to conform to R in the appropriate circumstances (2006, p. 15).

This annotation changes the nature of the theory significantly: while her earlier account attempted to analyse social norms in terms of concepts that did not include any normative element, Bicchieri's 2006 account allows norms or normative terms to appear on both sides of the equation. Normative expectations are not second-order empirical beliefs anymore. There may be an explicit ‘ought’ in condition 2(b), which is not further analysed.

The normative character of expectations is further emphasised in *Norms in the wild*, and made more explicit. Here is Bicchieri's new definition of social norm:

A social norm is a rule of behaviour such that individuals prefer to conform to it on condition that they believe that (a) most people in their reference network conform to it (empirical expectation) and (b) that most people in their reference network believe they ought to conform to it (normative expectation) (p. 35, italics in original).

Another way to put it is to say that *Rationality and coordination* offered a reductive account of social norms, while *The grammar of society* and (more explicitly) *Norms in the wild* do not. The non-reductive account is not circular—norms are not analysed in terms of norms, but rather in terms of individual normative beliefs. Nonetheless, the notion of normative belief is not explained further. One possibility is to say that a normative expectation is simply an empirical expectation augmented by a propensity to punish deviant behaviour (along the lines of condition 2(b’) in the Grammar). Another solution is to say that normative expectations are constituted by various propensities that may include, say, mental representations (the propensity to represent future behaviours), verbal reactions (the propensity to praise or reproach), emotions (the propensity to be surprised or annoyed), and so on. In other words, the sort of ‘reactive attitudes’, à la Strawson, that we use on a daily basis to regulate each other's behaviour.

Following the latter approach would make Bicchieri’s theory rather similar to its main current rival, the account of norms as clusters of normative attitudes proposed by Geoffrey Brennan et al. (2010). Brennan and co-authors explicitly offer a non-reductive account, and defend it
using various ingenious arguments (aimed in particular at Bicchieri’s theory). Although I am not convinced by some of these arguments, I wonder whether Bicchieri has anything to say about them. It is a pity that alternative accounts of norms are not discussed in *Norms in the wild*—or, as far as I know, anywhere else in Bicchieri’s writings.

Another issue that remains unexplored is the motivational basis of norm compliance. When beliefs are backed up by the threat of sanctions (condition 2(b’) in the *Grammar*), the instrumental motive to comply is pretty obvious. But what about condition 2(b), or condition (b) in *Norms in the wild*? Why should I be motivated to do something, merely because someone believes that I ought to do it?

One suggestion is that we might have an innate propensity to care about other people’s expectations.² Our failure to comply with norms and conventions is a potential source of unpredictability, and being unpredictable hinders one’s capacity to engage in cooperative tasks. This, in turn, may reduce one’s fitness. So, it would not be surprising, from an evolutionary psychology perspective, if we were endowed with a propensity to fulfil others’ expectations. But then we should also expect people to use a variety of ways to make such expectations salient by means of reproach, punishment, and the like. Bicchieri seems to follow this line of reasoning:

> the social pressure to conform, expressed in the social expectation that one ought to conform, is a powerful motivator. [...] If others believe one *ought* to conform, the reaction to non-conformity may range from slight displeasure to active or even extreme punishment (2017, pp. 34-35, italics in original).

But again, we are spared the details, and the causes of our motivation to fulfil others’ expectations remain largely opaque.

## II. Measuring norms

A good deal of chapter two (“Measuring norms”) is devoted to illustrating experimental and non-experimental techniques to detect and quantify the impact of social norms. Bicchieri has been actively engaged in this research, designing and running laboratory experiments with various collaborators during the last decade. Laboratory experiments, however, are very useful to test causal hypotheses, less so for policy-

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making. Because *Norms in the wild* is particularly focused on field intervention, Bicchieri spends a great deal of time illustrating alternative techniques for the measurement of social norms—like surveys, questionnaires, and vignettes.

The limitations of these methods are well-known: they are not incentivized, they do not involve the direct manipulation of key variables, and they do not allow the observation of people’s real choices. Still, verbal reports help researchers to elicit peoples’ conscious beliefs about others’ expectations.

Many techniques illustrated by Bicchieri are based on direct questions about ‘what would happen if’ someone in the community did this or that, or on the visual presentation of little stories in which fictional but realistic characters engage in behaviours that may be considered problematic from a normative point of view. The fact that such stories are couched in familiar contexts and scenarios is clearly an advantage compared to the anodyne choice situations of many laboratory experiments. Among other things, they facilitate the emergence of plausible responses from our largely subconscious behavioural repertoire. Counterfactual scenarios help individual responders to make explicit the content of rules, and the attitudes that others may have with respect to such rules. An important point is that observation and manipulation are mingled here, because the elicitation of beliefs often constitutes the first step of the policy intervention: *thinking* about norms is one of the conditions that make social change possible.

### III. A Dual Account

In the *Grammar* and in *Norms in the wild*, Bicchieri is careful to say that her expectation-dependent model of norms is not supposed to give a literal description of the psychological processes that govern norm compliance in ordinary situations. If you see people queuing in front of a desk, you usually do not begin to ponder: ‘What do these people expect me to do?’, or ‘What would their reaction be if I cut in front of the line?’. In most ordinary cases, you simply apply automatically the rules that apply in the circumstances. Your actions are governed by ‘scripts’:

> Scripts are essentially prescriptive sequences of actions of varying levels of specificity that people automatically engage in (and are expected to engage in) while in particular situations (p. 132).
This account of rule-following is intuitively correct. It is consistent not only with the phenomenology of ordinary action, but also with the bounded rationality, heuristics, and dual-system theories that have come to dominate the cognitive and behavioural sciences over the last thirty years. The interesting issue is what relationship there may be between automatic script-guided behaviour, on the one hand, and the expectation-dependent preferences that are at the core of Bicchieri’s definition of norms, on the other.

Bicchieri’s writing tends to become metaphorical at this point:

Norms are embedded into scripts because scripts contain empirical and normative expectations, and violations of scripts typically elicit negative emotions and remedial actions (p. 132).

We have beliefs and can refer to them when we want to, but the majority of the time they stay dormant—exerting unconscious influence on our behaviour up until the point when their validity is challenged (p. 128).

How does a script “contain” expectations, exactly? What does it mean that a belief remains “dormant”? How can it “exert unconscious influence on our behaviour”? These metaphors unfortunately do not contribute much to our understanding of norm-abiding behaviour.

In light of the evidence surveyed by Bicchieri, it seems more appropriate to admit that expectations play a minor role in ordinary cases of compliance with social norms. Scripts, heuristics, cue-driven automatic behaviour probably do all of the job that needs to be done. But this then means that there are two accounts of norm-following in Bicchieri’s theory—one for ‘ordinary’ and one for ‘extraordinary’ situations, so to speak. This is an important point that Bicchieri does not emphasise enough, in my view. It is important because it brings her theory in line with recent developments in the philosophy of mind, and because it shows in which sense it may provide a rational-choice explanation of norm-driven behaviour.

What picture of the mind is implicit in Bicchieri’s theory? Her definition of norms is essentially game-theoretic, and standard game theory moves from the assumption that behaviour is determined by individual beliefs and desires (or preferences). Moreover, it presupposes that people are able to form beliefs about other people’s beliefs and desires. People are belief-desire mind readers.
A substantial body of research, however, has recently questioned the belief-desire model of mind reading that economists and philosophers cherish. This body of research points out that belief-desire attributions are used only rarely to predict and explain other people’s behaviour. In fact, they seem to be used mainly in anomalous circumstances—that is, when other techniques of interpretation fail.³

Suppose you are sitting in the waiting room of a public office, with a ‘no-smoking’ sign hanging on the wall in front of you. The main reason why you don’t pull out a cigarette, probably, is that the sign says so. You don’t need to think about what other people in the room might possibly think if you smoked. But if a lady is lighting a cigarette, you begin to speculate about her beliefs, and about the expectations of by-standers (you try to meet their gaze, trying to detect cues of their normative attitudes). As Bicchieri points out, “we often become consciously aware of our beliefs the moment something unexpected happens and those beliefs are challenged” (p. 128).

The story Bicchieri is offering therefore is a dual account of the psychology of social norms. Norm-following on the one hand, and norm-enforcing and norm-transgressing on the other, are likely to be governed by distinct mechanisms.⁴ But her rational choice definition of norms is able to capture only half of the story.

One may argue that rational choice models are explanatory even when the causal factors they mention (normative expectations, in this case) are not activated. Philip Pettit (1995), for example, has argued that rational choice models can explain the resilience, as opposed to the continuation of behaviour. Explanations of resilience tell us why a certain pattern does not change, or would not change, in spite of changes in the circumstances. Thus, given a certain structure of incentives and beliefs, people may follow a norm without thinking about those incentives and beliefs at all (continuation). But the important point is that, were the situation to become anomalous, the mechanisms mentioned by Bicchieri (expectation-dependent preferences) would bring the behaviour back in line with the content of the norm (resilience).

³ The origins of this idea can be found in Bruner (1990) and have been developed in various directions in the following literature. See, for example, McGeer (2007) and Hutto (2008).

⁴ To put it concisely, using the standard two-systems terminology (e.g., Kahneman 2011): norm-following is mostly system 1, while norm change, enforcement, and perhaps also transgression engage (mostly) system 2. Pluralist accounts of mind-reading are defended by Andrews (2012) and Fiebich and Coltheart (2015), for example.
would reconcile Bicchieri’s formal definition of norms with the dual account that she outlines informally.

IV. REPRESENTING AND INTERVENING
At this point, it is important to recall what the ultimate goal of Bicchieri’s theorizing is. Because her fundamental interest is intervention, norm-enforcement and norm-evasion are salient in her discussion. And because expectations are crucial both to enforce and to change existing norms, the theory places expectations at centre stage. But if the question is what norms are, and how they influence behaviour, the complete answer must go along these lines: they are rules mapping behavioural responses onto stereotypical situations, that are automatically triggered and followed in ordinary circumstances (for example, when everyone complies); that activate the reactive attitudes of others when they are violated occasionally, and a thorough examination of other people’s empirical and normative beliefs when they are systematically or collectively breached by several people. In the latter two cases, our preference for compliance is truly conditional on expectations, that is, we deliberately modulate our behaviour according to others’ psychological and behavioural dispositions. Otherwise, expectations do not play a direct causal role in norm-compliance.

There is a sense in which Norms in the wild may be read as a guidebook for the creation of anomalous situations, that is, situations in which people stop following rules in an automatic manner, and begin instead to think critically about the attitudes that other people have—or ought to have—about those rules. Social workers, health practitioners, political activists and leaders can play a crucial role in this process that through exposure, recognition, questioning, and collective discussion may lead eventually to the replacement of harmful norms with beneficial ones. Bicchieri’s definition of norms, the measurement techniques that she illustrates, and the policy interventions they prepare the ground for, are different parts of one and the same package. To paraphrase Ian Hacking (1983), Bicchieri’s theory is not only (perhaps not mainly) for representing, but also (perhaps especially) for intervening.

This is what makes Norms in the wild interesting and different from most books in the philosophy of social science. While reading it, you will learn about the importance of convincing mothers-in-law that feeding colostrum does not harm babies; you will learn how to use flies to
demonstrate that open defecation leads to food poisoning; you will learn why soap operas emancipate women; you will learn what characteristics an individual must have to become a trendsetter; and many other fascinating tricks to manipulate the norms that govern our behaviour.

Bicchieri must be commended for trying to turn a theoretical insight into a toolbox for policy making. If you are tired of philosophers’ unlikely thought experiments, if you are fed up with the pursuit of conceptual analysis for its own sake, or if you are worried that social ontology and the philosophy of social science may have no practical implications, then there is no better reading than this.

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Michel De Vroey’s *A history of macroeconomics from Keynes to Lucas and beyond* is an important contribution to the history of economic thought. Standing upon the shoulders of two giants of the twentieth century, John Maynard Keynes and Robert Lucas, De Vroey sets out a clear-cut narrative. The modern history of macroeconomics is the result of a two-step process. The first step corresponds to the transition from Keynes’s actual writing in his *General theory* (1936) to Keynesian macroeconomics, which roughly occurred between the 1940s to the 1970s (sections 1 to 8, or part I). The second step corresponds to the era of “DSGE macro”, i.e., dynamic stochastic general equilibrium, which started in the mid-1970s with the “Lucasian revolution” and culminated in the 2000s with the new neoclassical synthesis (sections 9 to 18, or part II).

With the primary ambition of helping economists and teachers “ponder the origin of the kind of modeling with which they are familiar” (p. xiii), De Vroey endorses the canonical textbook reading of the history of macroeconomics—a reading that he himself argued for in a long series of papers—which highlights models developed by the main protagonists of both Keynesian and classical approaches. This list includes models of Nobel Prize-winning economists (e.g., John Hicks, Franco Modigliani, Laurence Klein, Edmund Phelps, Milton Friedman) as well as new classical economists (e.g., Robert Lucas, Finn Kydland, and Edward Prescott) and new Keynesians (e.g., Joseph Stiglitz and Georges Akerlof), among many others. Undoubtedly, any economist who wants to learn more about the theoretical connections between these models will be enthralled with the book.

However, by sticking to an “official” account of the development of macroeconomics, De Vroey limits himself. Because his account pays attention to logical transitions from one model to another, it neglects authors and groups of scholars whose impact has been retroactively
marginalized. Yet, revisiting these authors may prove to be fruitful. While they may be considered marginal when compared to canonical authors, such authors left undeniable marks in the development of mainstream macroeconomics. I do not mean that De Vroey’s analysis is questionable because of subject matter, which—for instance—chooses not to discuss developments in applied economics or heterodox economics. Rather, by paying more attention to historical developments, the book would have provided a more complete picture of macroeconomic development.

Three examples based on recent works on the history of macroeconomics can help make that point clear. I do not aim to be exhaustive here, but only to draw attention to elements that, in my opinion, would have dramatically helped complement the narrative. The first example relates to the emergence of macroeconomics in the context of the Cowles Commission in the United States, the second brings us back to the notion of neoclassical synthesis in the 1960s, and the third has to do with the development of the new neoclassical synthesis. In these three cases, I show that a broader historical account sheds a new light on the development of macroeconomics.

De Vroey’s reading of the “first era” of macroeconomics is defined by Keynesians who challenged the core concepts of Keynes with several modeling strategies. Keynes’s attempt to prove the existence of an equilibrium with involuntary unemployment, under the assumption that wage-rigidity was not responsible for it, touched off a long theoretical controversy. The first generation of Keynesian economists—led by Hicks, Modigliani, and Klein—admitted that Keynes had failed in his enterprise and argued that involuntary unemployment was, in fact, due to wage rigidity. This recognition became the cornerstone of Keynesian macroeconomics embedded in the IS-LM model. A second generation, including Patinkin, Leijonhufvud, and Clower, launched new lines of research by questioning the wage rigidity hypothesis which eventually led to the development of disequilibrium explanation of involuntary unemployment.

An historical account shows that these two lines of research originate in debates that took place at the Cowles Commission in the early 1940s under the guidance of Oskar Lange, who aimed at clarifying the consistency of Keynes’s analysis with the help of Walrasian theory, a theoretical corpus he came to master when debating market socialism. More precisely, the central issue was whether one could prove, in this
framework, the existence of an equilibrium that permits involuntary unemployment. As required by ‘Walras’s law’, a term coined by Lange, when considering any particular market, if all other markets in an economy are in equilibrium, then that specific market must also be in equilibrium. Any macroeconomic equilibrium thus logically implies that all markets clear. Keynesian equilibrium is no exception. It is by definition that an equilibrium entails that all markets clear. What Keynes called ‘involuntary unemployment’ thus refers to a situation in which the employment level, for any given level of money wage, is not maximum; or, put differently, an equilibrium in the labor market occurs in the horizontal part of infinitely elastic labor supply curves. Only then Keynesian macroeconomic equilibrium is compatible with the clearing of all markets represented at the intersection between supply and demand curves and the existence of involuntary unemployment or what Lange labels underemployment.

It is precisely that interpretation elaborated at the Cowles Commission in collaboration with economists like Jacok Marshak, Jacob Mosak, and Leonid Hurwicz, that Modigliani, in his famous 1944 *Econometrica* paper, suggested to compare to Keynesian and classical analyses. The division between Keynes and the classics would hence be based on two conceptions of the functioning of the labor market. The field of Keynesian economics would pertain to the short-term; featured by rigid money wages and infinitely elastic supply curves, while the field of classical economics would pertain to the long-term, featured by flexible money wages. Involuntary unemployment would thereby result from high rates of interest, which are due to a high ratio of nominal quantity of money to fixed nominal wage.

Though Lange was the one who suggested that interpretation, he did not forcefully argue for it. In his 1944 Cowles monograph, in reference to Hicks (1939) and Samuelson (1941), Lange argued that Keynes chose the wrong battleground, a point quickly taken up by Patinkin during his stay at Cowles. Equilibrium analyses and comparative statics are not the best tools for addressing macroeconomic issues. Based on this observation, Lange made two arguments. The first asserted that stationary states are full employment equilibria. The second that the excess supply of labor must cause money wages to decline, but also, that, under depression, employment may not increase. Full employment equilibria may thus be unstable. In this context, Lange concluded that one can demonstrate Keynes’s claim that a “trap” might exist from
which the economy could not be rescued; even though, low wage and price levels are relevant. As to that last point, he was aware that declining money wage rates are (potentially) unfavorable to aggregate demand. Even if this did not succeed in eliminating unemployment, one might not be justified in declaring a situation in which money wages and prices are persistently falling an equilibrium. But perhaps he did not insist upon it strongly enough, for the subsequent theoretical argument in the 1950s mostly focused on the statics of alternative stable wage levels (Modigliani) and later on the stability of full employment (Patinkin). The issue was by no means dead in the late 1960s when ‘full employment’ became the ‘natural rate’ of unemployment. Once again, with the works of Friedman, it is alleged that the private market economy can and will, without aid from government policy, steer itself to full employment equilibrium.

Lange’s stance, though marginalized, never completely vanished. Leijonhuvud (1973) and Tobin (1975) precisely strived to revive it with the notion of the “corridor of stability”, with which they argued that the economy may be locally stable but globally unstable. Currently, it is the basis for a policy of letting the recession run its course when the economy is close to the stationary state in confidence that in a relatively short time, equilibrium will be restored at full employment. Conversely, when the economy is pushed out of the corridor, deflation may be dangerous and destabilizing.

Besides the roads taken by Keynesian macroeconomists that are explored by De Vroey, one can see that Lange’s route, though much less followed, also influenced Keynesian debates from the start. Doubtless, it would have greatly helped De Vroey to shed new light on developments in Keynesian economics and in complementing his summary displayed in the form of a decision tree given in Box O.1 (p. xvii).

The second lacking historical account refers to the neoclassical synthesis. De Vroey’s History attempts to give a clear account of the content of the neoclassical synthesis introduced by Samuelson in the third edition of Economics (1955) in order to describe a consensus in the American economic community whereby 90% of its members adhered to the views of “[m]odern theories of income determination” [Keynesian] and “older economics” [classical] (Samuelson 1955, p. 212). As De Vroey explains, “in this edition of his textbook, Samuelson used the expression time and again, yet in a loose, almost metaphorical way, without clarifying its content” (p. 46). According to De Vroey, only Klein (1950)
and Patinkin (1956)—although neither of them used that terminology—gave it a clear content. In particular, Patinkin is credited with providing a synthesis between short period (i.e., in disequilibrium) Keynesian theory and long period Walrasian theory, with Walrasian equilibrium supposedly acting as the center of gravity for Keynesian disequilibrium states.

Because the book has no ambition of providing any detailed historical account of such synthesis, the analysis ends there. Historically, however, it was understood in radical different way (Assous, Dal-pont, and Manseri 2016). If De Vroey is right to say that Samuelson was unclear about the content of the neoclassical synthesis in 1955, it does not mean that he failed to clarify its content later. Returning to it in the 1960s, he argued that the neoclassical synthesis refers to the idea that through judicious government intervention and planning, economists agreed that the economy would behave like a neoclassical growth model. Along these lines, a forthright exposition of the neoclassical synthesis was given in the sixth edition of *Economics* in which a chapter on new growth theories was introduced for the first time.

Solow, Tobin, and Samuelson crafted growth models in which constant full employment of factors of production (Solow’s seventh assumption of his 1956 paper) was assumed (Halsmayer and Hoover, 2016). If proponents of the neoclassical synthesis considered that their growth model was complementary to and not competing with Keynesian analysis, it is because they thought they were relevant in a “managed economy”.

Furthermore, the neoclassical synthesis presented an opportunity to address long-run issues without assuming that the economy adjusts ‘automatically’ towards a state of full employment. If that were the case, assuming government intervention would no more be essential. As Solow made it clear in a letter addressed to Sen October 26th 1964: “To the extent that “neo-classical” describes the belief that a capitalistic economy tends automatically to full employment, I am no neo-classical and neither is James Meade” (Solow 1951-2011 and undated).

Much evidence shows that the neoclassical synthesis supported by most Keynesians in the 1960s was not based on any implicit idea that the economy could eventually adjust to full employment. So, by not addressing what the neoclassical synthesis meant historically, De Vroey is led to ignore it. It is true that this would have meant devoting some
pages to growth, a topic he deliberately chose to set aside. But without extending the presentation too much, the narrative would have greatly gained insight into the development of macroeconomics.

Let us now turn to the last historical example. De Vroey’s canonical reading of the developments in macroeconomics since the 1970s is that new Keynesians challenged Lucas’s (1972) policy ineffectiveness result by moving from a flexible price to a sticky price environment. This was what in fact helped spread the rational expectations hypothesis, according to Alan Blinder (1989, 104). In the early 1980s, real business-cycles macroeconomists advanced Lucas’s flexible price approach with a dynamic general equilibrium model where technological shocks drove the business cycle. The opposition in this period was between this group and some new Keynesians with their static models of price stickiness. When this latter group went to dynamic models, they contributed to the development of DSGE macroeconomics.

This reading illuminates some developments in mainstream macroeconomics in the 1980s, but there is a potential pitfall in not questioning it. One very important reaction to Lucas was achieved with multiple equilibria models with flexible prices (Assous and Duarte 2017). This started, after Gale’s (1973) seminal contribution, with the very model used by Lucas (1972), an overlapping generations model (OLG), and it developed into two strands: first, the deterministic cycles, and, second, sunspots. The OLG model was really a workhorse model in the 1980s that brought together communities of economists with different backgrounds and interests: those general equilibrium theorists wanting to extend their analysis to intertemporal models, monetary economists searching for microfoundations of money demand, and other more eclectic economists not really committed to a single macroeconomic model such as Lucas.

However, there was a clear effort by such endogenous fluctuations economists to analyze the generality of their results. Deterministic cycles, or sunspots, can occur in more general models, and macroeconomists such as Jess Benhabib, Michael Woodford, Roger Farmer, and Timothy Kehoe moved from OLG to infinitely-lived agent models with market imperfections to establish this. Price stickiness was not a major concern of this literature well into the 1990s. An important concern of the Keynesian view on fluctuations of this literature was with the design of policy regimes that could ameliorate inefficiencies, that was also key to Woodford’s 2003 book on an economy modeled with
infinitely-lived agents, price stickiness, and an interest rate rule. And Woodford is not the only important player in the DSGE literature who was involved with the endogenous fluctuations literature—Galí is another. This eventual connection between the endogenous fluctuation literature and DSGE macroeconomics does not receive much attention in De Vroey’s book.

Hence, a historical analysis—in addition to shedding light on important modeling strategies and on important historical developments in macro in the 1980s—brings to the fore an important issue related to the emergence of the new neoclassical synthesis. If the readers grant us the case for a richer understanding of the DSGE literature, it is hard not to see the endogenous fluctuations literature as having been very influential on the issue of policy stabilization, and on contributing in particular ways to the spread of the infinitely-lived agent models in mainstream macroeconomics, which do not conflict with the view that other issues that were important to the sunspot literature were marginalized. Unfortunately, these lines, and the consequences of that research, are not really part of the narrative, even if one may say that works by Farmer are mentioned in final chapters.

To conclude, I would like to strongly emphasize that favoring a historical perspective does not mean giving up assessing different modeling strategies or coping with various technicalities. Both approaches complement each other. My point is that it is by combining these two approaches into a single investigation, one may eventually end up with a much deeper understanding. De Vroey’s book is certainly a useful book, but more than half the work still remains to be done.

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*Ethics in the real world* is a collection of short essays, columns, and opinion pieces written by philosopher Peter Singer between 2001 and 2016. Each of the pieces constitutes a short chapter, in which Singer reflects on pressing moral and social issues. Many of these issues are familiar from Singer's previous work. They include animal suffering and the ethics of eating meat (do we have a moral duty to become vegans?), the sanctity of life (under what conditions are abortion and euthanasia morally permissible?), public healthcare (should we attempt to prolong a person's life at all costs?), sex and gender (should incest be criminalized?), doing good and effective altruism (can there be such a thing as evidence-based charity?), politics and global governance (will polluters pay for climate change?), and science and technology (is resistance against genetically modified organisms warranted?).

Apart from a short introduction and some added postscripts, all of the book’s pieces have been published before. To read a book with this format—an ensemble of earlier-published opinion pieces, on a diversity of topics and aimed at a general audience—might seem like browsing over a page-a-day calendar. It is to Singer's merit that reading his book is a worthwhile endeavor, in spite of its lack of novelty and the format's inherent limitations. Singer is a provocative, well-informed and hands-on philosopher, with a lucid and engaging writing style. The collection provides a comprehensive and accessible overview of themes that are central to Singer's ethics. It will specifically be of interest to those not yet acquainted with his work, but may appeal to anyone with an interest in applied ethics and social policy.

Rather than summarizing each of its 82 chapters, in what follows I make three general observations about the book. The first observation concerns Singer's engagement with public philosophy, the second his use of methods in applied ethics and the third the distinctively
cosmopolitan character of Singer's utilitarianism. I conclude with a remark about the societal relevance of applied philosophy.

Singer's essay collection belongs to the tradition of public philosophy—that is, the tradition of doing philosophy in public, non-academic settings. The topics he addresses and the terms in which he frames them are meant to appeal to a general audience. He notes, sarcastically, that

[t]here is a view in some philosophical circles that anything that can be understood by people who have not studied philosophy is not profound enough to be worth saying. To the contrary, I suspect that whatever cannot be said clearly is probably not being thought clearly either (p. x).

Indeed, insofar as popularizing is a matter of style, Singer succeeds admirably: his essays are well-structured, engaging, and exemplarily clear. Moreover, his arguments tend to be nuanced and non-dogmatic, in spite of his well-known ethical agenda: here is an ethicist not looking for arguments to support a preconceived conclusion, but sincerely pondering the implications of his utilitarian stance. A disadvantage of the book's short chapters is that the lack of scholarly detail does, occasionally, preclude the level of discussion that a topic calls for. This struck me, for instance, in Singer's defense of the late Derek Parfit's metaethical objectivism: a thousand words treatment simply does not suffice to touch upon the intricacies of the metaethical debate.

A second observation about Singer's approach concerns his extensive engagement with numbers, statistics, and matters of fact. Much of our ethical behavior takes place in a face of uncertainty, and weighing the moral importance of any given issue can be a difficult task; numbers, however, often provide us with a rough indication of moral weight, or so Singer suggests. “I would rather be vaguely right than precisely wrong”, John Maynard Keynes once noted. Singer cites him approvingly: the big picture is what matters, and numbers and statistics are invaluable to get a hold on it. For example, in the chapter If fish could scream, Singer highlights the staggering number of fish on which humans inflict death: somewhere in between 1 and 2.7 trillion every year. In the chapter The real abortion tragedy, Singer notes that 86 percent of all abortions occur in the developing world. In A case for veganism, Singer emphasizes the wastefulness of modern industrial animal production: pig farms use six pounds of grain for every pound of
boneless meat they produce. Numbers such as these help us, *inter alia*, to set our ethical priorities.

“We must make policies for the real world, not an ideal one”, Singer (p. 112) argues. In the real world facts matter and numbers count. Psychologically, we tend to be more easily moved to help one specific individual rather than an anonymous multitude. Helping the multitude, however, has greater moral importance, or so Singer maintains. This type of argument—related to so-called “evolutionary debunking arguments”—comes up on several occasions: Singer picks out the morally salient features of a situation, and emphasizes that our intuitions are misled with regard to them. Another instrument central to his applied ethics toolkit is reasoning by analogy. Oftentimes, Singer’s baseline for judging whether a morally controversial policy is justified is to take a less controversial case, and to argue that both cases are analogous.

A final observation about Singer’s approach is his thorough cosmopolitanism. The fact that we are globally connected holds implications for many of Singer’s ethical views. For instance, he criticizes the enormous expenditures on works of art, both with private and public money, by stressing that meanwhile people in Africa are dying from malaria. In and by itself there is nothing wrong with spending money on art, but the fact that this money could also be spent for different purposes changes the ethical playing field. Singer’s utilitarianism leads to a holistic outlook, in which all actions are morally laden. To take a transatlantic flight is to contribute substantially to \( \text{CO}_2 \) emissions; to invest money in high-price art is to refrain from investing the same money to help the global poor. According to Singer (p. 161), “those who have enough to spend on luxuries, yet fail to share even a tiny fraction of their income with the poor, must bear some responsibility for the deaths they could have prevented”. Note that a “tiny fraction” of one’s income is much less than what Singer has previously argued that should be given away to charity. A sense of realism about what people, in general, can be expected to contribute, guides Singer’s proposal here; small donations constitute the bare minimum of what one *must* do to lead a morally decent life, he submits. To lead a morally exemplary life, however, more work is required.

For readers familiar with Singer’s work these ideas are not new. For those who are not, on the other hand, they are likely to offer an original perspective that may leave a lasting imprint. Myself, I feel better
informed about a variety of social issues, and have slightly shifted my perspective about some of them, after having read this book. Precisely this power to shift people’s opinion about urgent ethical matters is what gives philosophy its social relevance, Singer (p. 37) submits. “I know from my own experience that taking a course in philosophy can lead students to turn vegan, pursue careers that enable them to give half their income to effective charities, and even donate a kidney to a stranger”, he recounts. “How many other disciplines can say that?”

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Peter Spiegler examines the current state of theoretical and empirical modelling in economics. According to him, this involves answering two questions: First, how can one determine what causal factors are essential for a phenomenon and, second, how does one ensure that the methodological tools employed represent these features faithfully (p. 9)? Spiegler puts forward new answers to these questions. He argues that a look *behind* mathematical models in economics is necessary: these methods are themselves not capable of showing that the economic phenomena and their representations within formal models are compatible, in the sense that the formal models conceptualise their domain of applicability accurately for a given epistemic purpose. He suggests that this compatibility between formal methods and economic phenomena needs to be checked with the help of an interpretative-hermeneutic method, akin to techniques used in anthropology and sociology, and that this should give rise to a new subfield of economics: interpretative economics.

Economists and philosophers of economics alike might be puzzled by this suggestion of giving qualitative methods such a key role within economics. I think both should be stimulated by Spiegler’s proposal. Throughout the book, it becomes evident that he is a philosophically highly informed economist who identifies relevant issues in a precise manner and skilfully navigates through the nitty gritty details of particular episodes of economic modelling. Even if one leaves aside his call for a substantial reform of economics, this book contains a lot of food for thought. For example, his discussion of the New, New Institutional Economics and Dynamic Stochastic General Equilibrium (DSGE) modelling provides rich case studies that put on the radar of philosophers of economics subfields of the discipline that have so far not received enough attention.
Spiegler's two specific claims are the following: Blind spots are a problem for all formal methods in economics and there needs to be an interpretative-hermeneutic method to assess the aptness of these formal techniques. I am intrigued by these two claims but not convinced by them. My worry is that Spiegler does not do enough to support them. Before spelling this out, let me briefly summarise the three parts of the book.

In part I, Spiegler introduces a pragmatic account of formal modelling and puts forward a criticism of theoretical and empirical formal modelling that sets the stage for the rest of the book. He suggests the following framework that is inspired by Mäki (2009): An epistemic agent S uses a model M to represent X for purpose P. The success of S in accomplishing P is judged against disciplinary norms N (p. 25). He further differentiates between four stages of formal theoretical modelling: In the delimitation phase, a social phenomenon is delimited and a research question is formulated in ordinary language (e.g., why is there involuntary unemployment?). In the denotation phase, the delimited social phenomenon is connected to a mathematical model in two steps: first, the formal structure of the model is described informally—with the help of ordinary language names for the phenomenon (yielding what he calls a proto-model); second, the model is presented in purely formal terms. In the solution phase, purely mathematical operations are performed to arrive at a result. In the interpretation phase, the solution stated in mathematical terms gets re-translated into ordinary language using the correspondence established in the denotation phase (pp. 46-52). According to Spiegler, the same four phases can be used to describe econometric modelling. In this case, however, two additional relations need to be accounted for: 1) econometric models are (sometimes) models of an economic theory and 2) econometric data is data about economic phenomena (p. 73).

Crucial for understanding Spiegler's criticism of formal economic modelling is his view of models as metaphors. Metaphors invite us “to project the attributes of mathematical objects onto […] social entities: to ‘see’ social phenomena through the overlay of mathematical relations” (p. 53). Following Hesse (1963), he suggests that the illumination provided by a metaphor requires that there is enough relevant similarity (positive analogy) and sufficiently little relevant dissimilarity (negative analogy), such that one can see the neutral analogies (i.e., the ways in which the two entities related in a metaphor may possibly be similar) as
relevant similarities (p. 54). He helpfully clarifies this requirement further by introducing the “no essential negative analogies” (NENA) condition (p. 58). What the NENA condition requires is that the dissimilarities between a formal model and its target do not pertain to the essential properties of the target. Essential properties are defined with respect to the purpose of the modelling exercise (p. 56). Put differently, the model should not “distort or obscure [...] the target subject matter” but rather should be apt for the target system in light of an epistemic purpose (p. 58). This prompts the crucial question for Spiegler: What properties does a given social phenomenon need to have to be compatible with a formal construct introduced in the denotation phase of formal modelling? According to Spiegler, formal modelling presupposes that “(1) the objects under study are plausibly stable, modular and quantitative, with no qualitative differences among instantiations of each type; and that (2) the relations between these objects are plausibly fixed and law-like throughout the context of the study in the modelling exercise” (p. 63). He illustrates these two conditions with the help of Shapiro and Stiglitz's (1984) efficiency wage theory. According to this theory, it can be beneficial for managers to pay more than the market clearing wage since it undermines the incentive for workers to shirk because there is a credible threat of being unemployed. For Spiegler, the concept of effort as represented in this theory (i.e., a continuous variable that does not allow for qualitative distinctions among its instantiations) violates condition 1) as it does not, for example, account for relevant qualitative distinctions between effort types such as effort in an assembly line vs. effort in an advertising firm (p. 64). The theory also violates condition 2), since the proposed stable relation between wage level and effort of employees is depending on the connotation that a wage regime has in a company and this effect cannot be a priori known and, crucially, not be assumed to establish a fixed relation between wage and effort level (p. 65). Hence, Shapiro and Stiglitz's efficiency wage theory lacks essential compatibility with its intended target domain and, hence, should be viewed as a problematic formal modelling exercise. Spiegler closes part I by arguing that an exactly parallel condition to the NENA condition holds for econometric modelling: successful econometric modelling presupposes that there is a homorphism between data and social phenomena (p. 77).
In part II, Spiegler applies the essential compatibility requirement between models and their target domain to two case studies: New, New Institutional Economics and DSGE modelling. In the interest of brevity, I focus on the latter. He argues DSGE models had failed in the run-up to the recent financial crisis since their formal structures precluded seeing the crisis’ relevant dynamics. In other words, DSGE models have significant blind spots since they do not meet the essential compatibility requirement. In particular, the assumptions that aggregate macroeconomic behaviour can be represented as optimizing behaviour of a representative agent, that financial markets are efficient, and that the macroeconomy is a log linear system represent essential features of macroeconomic phenomena incoherently as they rule out chaotic dynamics (pp. 120-125).

In part III, Spiegler puts forward his constructive proposal against the background of the criticisms in the first two parts of the book. He suggests founding a new discipline within economics: interpretative economics. The role of this discipline is to provide the relevant information for checking whether the methodological tools of economists are essentially compatible with their intended domain of application. This new field should establish the meaning of economic concepts (such as effort or wage) and assess whether these concepts display the stability requirements set out in the NENA condition, and, hence are susceptible to formal modelling (p. 166). He suggests an interpretative-hermeneutic method to accomplish these tasks. This method involves three related steps: 1) choosing a fore-understanding of the phenomenon to be analysed, 2) refining this fore-understanding through a contact with the phenomenon that is open-ended enough to “allow the phenomena to speak for themselves” (p. 172), and 3) interpreting the phenomena of interest utilising the information in 1) and 2) (p. 172).

Spiegler discusses Bewley’s (1999) work on wage rigidity during recessions as an exemplar of this new field of economics. Bewley, frustrated by empirical and theoretical limitations of available accounts of wage rigidity (e.g., Shapiro-Stiglitz’s efficiency wage theory), engaged in a set of unstructured interviews, inspired by interpretative survey techniques in sociology and anthropology, with businesspeople responsible for hiring and compensation decisions. He found, among other things, that these businesspeople are focussing on the morale of the work force and that the mechanism of the efficiency wage theory
qua punishing mechanism undercuts this morale significantly. This is evidence for Spiegler's claim that Shapiro and Stiglitz' theory violates the NENA condition (pp. 176-181). Importantly, Spiegler believes that a hermeneutic-interpretive method, and hence a non-formal method, is necessary to retrieve the information about the essential compatibility between models and target systems (pp. 146-148).

I think Spiegler's emphasis of potential blind spots of formal modelling techniques and his attempt to come up with a constructive proposal to determine the extent of the respective blind spots of various formal techniques should be highly welcomed. However, in my view, there is a central ambiguity running through this book that makes it hard to pin down exactly what Spiegler's main line of criticism against formal economic modelling is. Because of this ambiguity and the dialectical space it opens up, I do not see why the interpretative-hermeneutic method should be the way forward to address the shortcomings of formal modelling techniques.

Spiegler's comments about the essential compatibility of formal models with economic phenomena can be read in two ways. One can read it in the strong sense that formal models in economics are bound to fail since economic phenomena do not exhibit the required modularity and stability described in the NENA condition in virtue of some fact about economic reality. This reading is supported by Spiegler's claim that the “potential hazards of model-target mismatch are endemic to all mathematical economic modelling” (p. 191), his characterisation of the meaning of social phenomena as “fluid, evolving, and imprecise” which are “formed dialectically—i.e., by agents acting within norms and conventions which are in turn shaped by these individual actions” (p. 146), and his remarks about how formal models of New, New Institutional Economics “render institutions susceptible to economic analysis by converting them into something else” (p. 96). Alternatively, one can read Spiegler's claim in the weak sense that formal models in economics might fail since economic phenomena might not exhibit the required modularity and stability described in the NENA condition. The clearest indication that Spiegler might have this interpretation in mind is contained in a footnote on Lawson's realism (see, e.g., Lawson 1997). Spiegler claims in this passage that there is a central difference between the NENA condition and Lawson's conditions for the aptness of formal modelling of economic phenomena: “A central difference between [Lawson’s] conditions and [my conditions] is that Lawson's conditions...
are pitched at the level of ontology—whereas [my conditions] deal with the plausibility of claims and therefore are essentially pragmatic conditions” (p. 63, fn. 17).

I think that both readings of the claim face some argumentative challenges. Consider the strong reading to start. To make a case for a general inaptness of formal methods to model the economic realm requires taking a stance regarding the ontology of the economic realm. In fact, one needs to defend a substantial (social) ontology which rules out characterisations of economic entities and relations that could be—in principle or in relation to the epistemic purposes of modelling—formally modelled. Coming up with such an ontology and some sort of epistemic access conditions to establish that the ontology is in fact an accurate description of the economic realm is a daunting task. Taking Spiegler’s remarks about Lawson’s realism seriously, a charitable interpretation suggests that he never actually had the strong, more radical claim in mind, while discussing the limits of formal modelling. However, I do think there are some gaps in the defence of the weak claim as well.

Spiegler (p. 121) states explicitly that the NENA condition should be read as not only involving the dyadic relationship between models and targets but also the purposes and norms of the modelling exercise. So, showing that a formal economic technique is inapt for modelling a domain requires showing that the technique is inapt in relation to a particular epistemic purpose. The case studies that Spiegler provides reveal that some modelling techniques are inapt in relation to some epistemic purposes such as the explanation of the phenomenon of wage rigidity. However, I do think that his discussion overlooks two important aspects of the model-purpose link.

First, a formal modelling technique might be apt given one changes the epistemic purpose. To put it differently, a domain of investigation might be satisfying the NENA condition given one changes the epistemic purpose. Let me illustrate this with one of Spiegler’s own papers. Together with Stephen Marglin, he analysed the effectiveness of the fiscal stimulus in the 2009 American Recovery and Reinvestment Act (ARRA) (Marglin and Spiegler 2014). With the help of surveys—an interpretative technique—they established that the counterfactual claim that states would have been able to spend at the observed levels in the absence of ARRA was false. Spiegler (pp. 187-188) rightly concludes that this piece of information invalidates the use of an econometric model by
Cogan and Taylor (2012) aiming at measuring the stimulus impact which is built on this false assumption. However, what it fails to show is that for a different epistemic aim, Cogan and Taylor's (2012) formal model would be inapt. It might be apt, for example, for revealing *one relevant causal factor* in the causal-nexus of the stimulus impact, i.e., consumption smoothing considerations by states. Importantly, as Spiegler (p. 187) himself notes, the surveys of Marglin and Spiegler (2014) do support assumptions regarding consumption smoothing on state level. Moving away from Marglin and Spiegler (2014), some theoretical models might not be apt for the analysis of policy-intervention, however, they can be fruitfully used to study the working of isolated causal mechanisms in the economy (see, for example, the Caballero et al. 2015 model that introduces a mechanism for rebalancing asset markets in an economy that is at the zero-lower bound of the interest rate). I do think that shifts of epistemic aims can be pursued across different theoretical and empirical modelling exercises. The underlying reason for this is that for different epistemic aims, distinct aspects of a model could be representationally relevant. Now, if for a given epistemic purpose some aspects of a model are not representationally relevant, then these aspects cannot ground a violation of the essential compatibility requirement between a model and a target. For example, if one claims that a formal model provides a causal-mechanistic explanation of an economic phenomena, more elements of the model must be viewed as representationally relevant than in the situation where a model is used for short-term forecasting with the option for daily feedbacks of prediction errors into the model (for example a vector-auto-regression model). If one disagrees with this possibility of formal models being apt (or inapt) depending on the epistemic purpose pursued, I think, one is committed to the strong reading of Spiegler's claim and, hence, faces the challenge that was mentioned above.

Second, some of the methods that Spiegler discusses might be apt for the epistemic purposes that he evaluates them on, despite his claim to the contrary. His discussion of state of the art DSGE models is a case in point. Spiegler claims that this modelling framework is inapt for assessing real world macroeconomic phenomena since it cannot display non-linear dynamics (pp. 120-125). However, it should be noted that the standard DSGE framework with rational expectations does not preclude markets from collapsing and allows for multiple equilibria outcomes.
(see, e.g., Den Haan 2003; 2007). Again, the key question seems to be what epistemic aim should be realised with a particular model.

If these aspects are relevant for state of the art economic modelling, then I do not see the urge to have a special discipline within economics assessing the aptness of formal modelling techniques. An alternative upshot of Spiegler’s analysis—following from the weak claim—could be to demand from economists to state more precisely what the epistemic purpose of their formal modelling endeavours are. Note that this point is independent of additional reservations one might have against the reliability of interpretative-hermeneutic methods (and in particular against their empirical tools of interpretative interviews and participant studies).

Spiegler’s book is an economically extremely well-informed engagement with the foundations of formal modelling. Even if one shares my reservations regarding his reform proposal for the discipline, this book provides plenty of fruitful case studies and frameworks that certainly advance our understanding of the economic practice.

REFERENCES

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PHD THESIS SUMMARY:
Economics as a “tooled” discipline: Lawrence R. Klein and the making of macroeconometric modeling, 1939-1959

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Large-scale macroeconometric models have played a paramount role in the transformation of US-American macroeconomics in the political, academic, and intellectual spheres since the 1940s. In an era of progressive liberalism that pursued economic stability through the advocacy of government intervention, macroeconometric models provided powerful tools for economic planning and forecasting. In addition, these models changed the way macroeconomics was done, emphasizing empirical orientation and technical sophistication in the context of “big science”, growing computerization, and fundamental transformation of scientific practices, which increasingly relied on teamwork effort and a new kind of expertise.

Taking Lawrence R. Klein as a focal point, I travel across the economics discipline of the 1940s and 1950s to study the intersection between the history of macroeconomics and the history of econometrics. I thus provide a new understanding of 20th century economics as a “tooled” discipline, in which theory, application, and policy become embedded within a scientific tool: a macroeconometric model. This new understanding presents the history of macroeconomics not as the product of ideological and purely theoretical issues, but rather of divergent epistemological views and modeling strategies that go back to the debates between US-Walrasian and US-Marshallian approaches to empirical macroeconomics.

My dissertation is divided in two parts, each composed of three chapters. In the first part (chapters 2-4), I explore Klein’s intellectual trajectory, and present how he constructed his identity as a macroeconometrician, contributing to the development of macroeconometric modeling. Indeed, in chapter 2, I revisit the intellectual situation of the economics discipline at the time, and the relations that Klein established with different institutions and personae,
which marked the formation of his identity as a macroeconometrician. In particular, I study Klein’s passage through the University of California, Berkeley, the Massachusetts Institute of Technology (MIT), the Cowles Commission, the National Bureau of Economic Research (NBER), and the University of Michigan, as well as his encounters with people like Jerzy Neyman, Griffith C. Evans, Edwin B. Wilson, Paul A. Samuelson, Jacob Marschak, and Trygve Haavelmo, among others.

In chapter 3, I consider Klein’s project to “redo” Jan Tinbergen’s macroeconometric work as a well-informed reaction to John Maynard Keynes’s criticism of Tinbergen, and as one that decisively contributed to the development of macroeconometric modeling. As an expert in Keynesian thought and a leading figure of Cowles’s macroeconometric program, Klein surmounted the difficult task of reconciling Tinbergen’s world, which strove for the implementation of technical and rigorous devices from which to draw inferences, with Keynes’s world, which showed a clear aversion to this kind of technicality, although not necessarily to empirical work.

Chapter 4 provides an account of Klein’s distinctive way of doing econometrics. Focusing on his time at the Cowles Commission (1944-47), I discuss a series of publications and events that were decisive in shaping his image of econometrics. In particular, I argue that Klein’s adoption of a flexible and practice-oriented methodology, and his endorsement of pluralistic economic theories, resulted from his participation in empirical model-building. Furthermore, I show that Klein’s flexible approach contrasts with the prescriptive methodology used in the abstract and theoretical work led by his colleagues at Cowles. I conclude that Klein’s distinctive image of econometrics allowed him to enrich the process of model specification, to pursue the macroeconometric program beyond the 1940s, and to remain optimistic about what he thought was the political objective of econometrics: economic planning and social reform.

The second part of my dissertation (chapters 5-7) revisits the longstanding opposition between the econometrics program à la Klein, and the statistical economics program à la Milton Friedman. Following Ted Porter’s (1994, p. 128) idea that “the modern history of American economics” is fundamentally a history of “rival ideals of quantification” rather than a history of rival theories or ideas of economic analysis, I study the opposition between the Cowles’s and the NBER’s approaches.
to empirical macroeconomics, and in particular between Klein and Friedman.

Indeed, in chapter 5, I consider the Marshall-Walras divide as the point of departure and center of the methodological debate between these two empirical approaches to macroeconomics. I argue that the transformation of economics into a “tooled” discipline changed the relations between economic theory, applied economics, and the policy sphere, and insist on the fact that rather than bridging the gap between theory and data, macroeconometrics radically transformed the preeminence of theory over application, data, and political issues in economics. I conclude that independently from the economist himself, the macroeconomic practice of the 20th century (which implies adherence to the econometric tool) does not allow for a dissociation of theory, application, and policy, but instead combines and fuses these elements into a single system of reasoning: macroeconometric modeling.

Chapter 6 clarifies the differences between the Cowles’s Walrasian and the NBER’s Marshallian modeling strategies. These differences, I argue, consist not only in the use of diverse statistical methods, economic theories, or political ideas, but also in deeply rooted methodological principles and modeling strategies that raise questions on both the way macroeconometricians represent and understand the world, and on how they deal with problems of operationality and concrete problem-solving. While Cowles’ Walrasian approach necessarily considers the economy as a whole, despite the economist’s inability to observe or understand the system in all its complexity, the Bureau’s Marshallian approach takes into account this inability and considers that economic models should be perceived as a way to construct systems of thought based on the observation of specific and smaller parts of the economy.

Focusing on the 1957-1958 controversy between Gary Becker, Friedman, and Klein, chapter 7 provides an account of the discussions on how to evaluate the performance of macroeconometric models. At this occasion, Friedman and Becker questioned Keynesian macroeconometric models for their inappropriate treatment of the consumption function, and for their inability to yield accurate predictions of income, resulting from the adoption of the “misleading” (Becker and Friedman, 1957, p. 64) criterion to judge models’ performance. While macroeconometricians adopted reduced form
extrapolations to evaluate their models, Friedman and Becker insisted on the necessity of carrying out full model simulations to conduct sound model selection. Independently of Friedman and Becker’s critical tone, I conclude that their argument can be interpreted as a constructive critique and as a precursor of a criterion to evaluate models’ performance that became common ground among macroeconometricians in the subsequent decades.

In a nutshell, my thesis is that Klein was the most important figure in the creation of a new way to produce scientific knowledge that consisted in the construction and use of complex tools (macroeconometric models) within specific institutional configurations (econometric laboratories) for explicit policy and scientific objectives, in which well-defined roles of experts (scientific teams) were embodied within a new scientific practice (macroeconometric modeling).

**REFERENCES**


**Erich Pinzón-Fuchs** received his PhD in economics under the supervision of Prof. Annie L. Cot from the University Paris 1 Panthéon-Sorbonne in March 2017. His areas of expertise span the fields of history and methodology of recent economics, macroeconomics, and econometrics. Specifically, his research focuses on the history and methodology of large-scale macroeconometric modeling. He is currently a research fellow at the Center for the History of Political Economy at Duke University, and a postdoctoral researcher at the Economics Department of Universidad de los Andes in Bogotá, Colombia.

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PHD THESIS SUMMARY:
Rational choice theory: its merits and limits in explaining and predicting cultural behaviour

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The questions this dissertation addresses are: (1) What principles must govern the decision-making process in order for persons to be called instrumentally rational? (2) Are these principles satisfactory for human rationality in all domains? To answer these questions, I focus on rational choice theory (RCT) and public choice theory (PCT), which are extensively studied as examples of instrumental rationality in contemporary debates in philosophy and economics. To see their merits and to determine their limits, I have applied RCT and PCT to cultural behavior, given that it is mainly motivated and determined by the norms of a given culture, and that it can be contrasted with behavior that is initiated and chosen by the individual for reasons other than norms.

For example, eating is seen as an individual act, but table manners are accepted as the products of a specific culture. Furthermore, identity, class membership, group-belonging, cultural rituals and traditional practices are, among others, generally considered as imposed upon individuals by culture. This gives the impression that culture primarily shapes and determines behavior. Cultural behavior in this sense is not subject to rational assessment and is formed through habits, customs, and traditions that essentially remain within the domain of senses, attitudes, and emotions other than choice and rationality. Contrary to this approach, I apply choice theories to cultural behavior in three chapters to discuss whether the choice theories have the potential to explain different forms of human behavior in general.

The application of the models shows that cultural behavior can be subjected to the criteria of rationality as opposed to previous approaches. However, the application also shows that RCT and PCT have arguable success in explaining the complexity and subtlety of cultural behavior. Their success is limited because, for example, they make unrealistic assumptions about human cognitive capacities, they
disregard the content of preferences, and they dismiss the role of emotions in decision making, among other shortcomings.

In the second chapter of the dissertation, I introduce four criticisms—these are: (1) individuals are not atomic and unconnected entities; (2) individuals are not perfectly rational; (3) instrumental rationality cannot explain fully human behavior; and (4) institutions and structures cannot be reduced to individual choices. These criticisms have three goals.

The first goal is to reformulate the choice theories according to the general features of human behavior. Choice theories tend to ignore the relation of individuals to each other in their environments, treating social groups as secondary and reducing public decisions to the choices of individuals. Social life is not just a matter of choice, but a natural tendency. Individuals live and interact together, helping to fulfill each other’s desires and goals that they cannot realize independently. Even basic needs are inevitably social. So, a theory of rationality must take account of ‘relations’ in the sense that individuals are more than atomic entities.

The second goal is to discuss one of the assumptions of RCT—the assumption that people are not only rational, but also that they are perfectly rational. If they follow the rules of rationality, as they should, they can make flawless calculations about the best means to achieve their specific ends. Psychological tests have provided evidence time and again that this assumption is no longer tenable and that the application of RCT to culture supports the same assumption. Considering individuals as less than perfectly rational gives a more realistic view of them.

The third goal is to emphasize a point made by Jon Elster in Sour grapes (1983). He gives the following example to criticize the kind of rationality that he calls ‘thin’: “If an agent has a compulsive desire to kill another person, and believes that the best way of killing that person is to stick a pin through a doll representing him, then he acts rationally if he sticks a pin through the doll” (3). Thus, Elster rightfully objects to the idea that rationality can be understood without reference to the contents of desires, intentions, emotions, and beliefs. Pure means-ends rationality justifies all kinds of immoral acts including killing an innocent person in the most efficient way. To avoid such absurd conclusions, a broader concept of rationality is needed to guide us in interpersonal relations towards a more humane world.
Regarding the last point of criticism, I mention Wittgensteinian rule-following as an objection against methodological individualism. It is not meaningful to talk about the correct or incorrect application of a rule, or the correct or incorrect use of a word, a sentence or a statement independently of their usage in social relations and interactions. The individuals’ mental states, beliefs, intentions are inherently and inseparably linked to social practices. So, methodological individualism cannot be successful in explaining complex human behavior.

In the second part of the dissertation (chapter 3), I introduce four more criticisms, which are that (5) following norms is not incompatible with utility maximization; (6) the dynamic nature of norms and interactions among rational individuals can be fully accounted for through integrating an evolutionary approach to choice theories; (7) sympathy, trust, and commitment (among other values) must be an integral part of rational behavior so that complex behavior can be explained consistently in the PCT framework; and (8) PCT fails to produce empirically satisfactory findings for cultural behavior.

These criticisms aim to overcome the shortcomings of current choice theories by providing a complete picture of the determinants of norms, sentiments, and civic engagement. They indicate the evolutionary framework in which these determinants develop. Without taking these significant factors into consideration, we will not be able to comprehend fully the place of rationality in human affairs. A theory that equates rationality with utility maximization is not only wrong, but also harmful and runs into difficulties in the face of the complexity of human behavior. It also falls short of explaining some forms of civic participation such as volunteering, charitable giving, and other forms of altruism.

Second, the application shows that when cultural behavior is analyzed in detail, the model and its application fail to combine individual and social behavior appropriately. When people play the game of norms across generations, they will reach an optimal strategy and equilibrium regarding these norms. Therefore, an evolutionary approach enables us to see the dynamic interactions between norms and rational individuals. Our norms evolve along with our lives and relations, and this has to be taken into account to understand individual action through interaction with others over time.

Third, I discuss an aspect of Adam Smith’s approach to human relations that is usually ignored by economists who concentrate only on
consistency, choices, and ends of economic action. In other words, they concentrate on the formal aspects of rational choice, ignoring the setting in which these concepts occur in relation to virtues such as sympathy, trust, and prudence. As argued by Adam Smith, the need for sympathy urges people to socialize and regulate their behavior to make it conform to moral values.

In the last part (chapter 4), I introduce two more criticisms: (9) making a constitution is not just about proposing fair rules and just laws to be included in a constitution, rather it is also about the relative merits of the constitution makers. Since there is always the possibility of constitutional high-jacking by self-interested and partisan rulers and law-makers, even the fairest rules may be unjustly interpreted and ruthlessly manipulated in the hands of people who are solely motivated by self-interest. Lastly, (10) only a neutral constitution can justify the necessary constraints for cultural behavior.

These ten criticisms are intended to suggest that we need a model that is inclusive of the individuals who belong to different ‘forms of life’ and will explain the reasons of their behavior properly. I believe that this dissertation achieves two things: first, RCT and PCT may have a potential to bring under their scope previously neglected fields such as identity and culture only if they transform their sense of rationality by considering complex behavior, actors, and their interactions. Second, the pervasive opinion that culture and identity are not relevant to rationality and rational choice must be reconsidered.

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PHD THESIS SUMMARY:
From the Lucasian revolution to DSGE models: an account of recent developments in macroeconomic modelling

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In the last four decades, macroeconomic modelling practices underwent a deep transformation. My dissertation provides a history of this transformation, starting from Robert E. Lucas’s work in the 1970s up to today’s dynamic stochastic general equilibrium (DSGE) approach (Smets and Wouters, 2003). Working from a historical perspective, I suggest that the recent rise of DSGE models should be characterized as a shaky compromise between opposing views of modelling methodology—on the one hand, the real business cycle (RBC) view, on the other hand, the new Keynesian view. To justify this claim, my work provides an epistemological reconstruction of the recent history of macroeconomics, building from an analysis of the criteria defining the validity and the pertinence of a model.

My assumption is that recent macroeconomic modelling practices can be described by three distinctive methodological criteria: 1) the internal validity criterion (which establishes the consistency between models’ assumptions and concepts and formalisms of a theory), 2) the external validity criterion (which establishes the consistency between the assumptions and results of a model and the real world, as well as the quantitative methods needed to assess such a consistency), and 3) the “hierarchization” criterion (which establishes the preference for internal over external validity, or vice versa). This epistemological analysis draws primarily from the literature about models in philosophy of science (especially Morgan and Morrison 1999). My work aims to make four contributions to the history of recent macroeconomics. Firstly, to account for the rise of DSGE models without referring to the explanation provided by the macroeconomists

1 Original title (the dissertation is in French): "De la révolution lucasienne aux modèles DSGE. Réflexions sur les développements récents de la modélisation macroéconomique"
themselves, who tend to think that macroeconomics evolved through theoretical consensus and exogenous technical progress. By distancing itself from this perspective, my work draws attention to the disruptive character of methodological controversies and to the interdependence between theoretical activity and the development of statistical and econometric methods. Secondly, to overcome the existing divide between the history of macroeconomic theories (such as De Vroey, 2015) and the history of quantitative methods. Through its epistemological perspective, my work reconciles these two historiographies and specifies the basis for a comprehensive understanding of recent developments in macroeconomics. Thirdly, to put the accent on the external validity condition as the main controversial issue separating different views of macro-modelling methodology. Furthermore, I illustrate how the debate about external validity is closely related to the problem of causal explanation (such as recently discussed in Maki 2005 and Reiss 2012) and to the conditions for providing economic policy evaluation. Fourthly, to characterize the DSGE approach: although DSGE models are often presented as a “synthesis”, or as a “consensus”, they are better described as a shaky compromise between two opposing methodological visions.

Part I of the dissertation analyses Lucas’s methodological view. Relying both on his published works (from the 1970s until today) and on archival material (correspondence, drafts), I emphasize the ambivalence of Lucas’ vision of models, which suggest two incompatible definitions of external validity: on the one hand, ‘black box’ models, characterized by a-realistic assumptions; on the other hand, models as ‘laboratories’ for policy analysis, relying on causal analogies. Furthermore, I discuss how Lucas’s view was implemented during the 1970s by the new classical macroeconometric research program (Lucas and Sargent 1981), and emphasize the shortcomings of this approach in terms of methods and results.

Part II of the dissertation illustrates the competing views on models developed by RBC (Kydland and Prescott 1982) and new Keynesians (Mankiw and Romer 1991) during the 1980s and early 1990s. RBC modellers embraced Lucas’s ‘black box view’ of models, emphasizing a-realistic assumptions. As a result, they departed substantially from new classical economics by abandoning econometric methods and championing calibration. In contrast, new Keynesian economics
emphasized the need for ‘realistic’ assumptions as an alternative to the RBC ‘black box’ models.

Part III of the dissertation analyses two debates, which illustrate the shift from the open conflict between RBC and new Keynesian views to a compromise—the DSGE approach—embodying these tensions. First, the famous Prescott (1986a, 1986b) and Summers (1986) controversy is reinterpreted within my methodological criteria to illustrate how methodological views on modelling were the underlying crucial issue of this debate. Inside the current DSGE approach, these two interpretations are still in conflict, as shown by the recent literature. In the final chapter of the dissertation, the on-going discussions on the vulnerability of DSGE models to the Lucas critique are shown to be a crucial example of this tension. I argue that two competing interpretations of the Lucas critique arose from opposing views on modelling methodology: whereas RBC conceived the Lucas critique as a theoretical proposition, pertaining to the internal validity criteria, new Keynesians rather interpreted the Lucas critique as an empirical proposition, pertaining to the external validity set of conditions. Today’s DSGE modellers inherited these competing interpretations. Moreover, these competing interpretations result in alternative research paths that tend to destabilise the compromise about DSGE models.

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PHD THESIS SUMMARY:
What is required for (r)evolutions? The case of economics

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This thesis consists of four parts, all of which concern one topic: (r)evolutions in economics. Part I, entitled Problem context, involves an analytical and critical description of the dominant discourse in economics, juxtaposed with an overview of the contemporary world economy and humanity. It has four main findings. The first one asserts that the dominant economic discourse provides the intellectual backbone to a world economy in which severe economic imbalances are regenerated and widened, mainly in the forms of extreme poverty, extreme wealth and associated inequality. Secondly, the dominant economic discourse provides the intellectual backbone to an elite-oriented, subjugated humanity, mainly by encouraging ethical behaviour based on destructive selfishness and competition. Thirdly, these economic and human imbalances regenerate severe power imbalances so that societies suffer from lower quality of democracy, well-being and further human polarisations, as well as more plutocracy and economic inequality. Finally, the three subjugatory channels generate societies that oscillate within a vicious cycle of development towards even more subjugatory and destructive imbalances in terms of economy, ethics and power. Moreover, since the outcomes seem to be quickly worsening, (r)evolutions are imperative. The final conclusion to part I is, therefore: (r)evolutionise economics, the sooner the better.

Part II, entitled Solution orientation: how to (r)evolutionise economics?, employs two lines of enquiry in order to assemble inferences on what is required to actually (r)evolutionise economics. The first involves a philosophical appraisal, which attempts to outline important perspectives, approaches, and accounts to transform an academic field such as economics. The second line of enquiry involves a historical appraisal, which attempts to outline the economic history of an acknowledged (r)evolution in economics: the neoclassical economics take-over during the 1970s. The findings lead us to conclude that there
are five overarching criteria that need to be fulfilled in order to realise a (r)evolution in economics: *critical juncture, dissimilarity, sensibility, scholar validation*, and most importantly, *elite appropriation*. In relation, part II concludes that an academic field such as economics cannot be changed simply by intra-scientific support, but must be coupled with extra-scientific factors since economics is significantly value-, interest- and ideology-laden.

Part III, entitled *Solution assessment: to (r)evolutionise economics today!*, appraises the criteria from part II within the context of the contemporary state of economics. It comprises of five sections, corresponding to the five criteria identified in part II. Each criterion is assessed through relevant research findings and, when applicable, economic indicators and other statistics. The first criterion, ‘critical juncture’, is fulfilled because the global financial crisis (GFC) of 2007-2008 and its aftermath form a major economic crisis, and a significant crisis in economics. Furthermore, it is widely seen that the dominant economics has not, and cannot, (re)solve the continued repercussions of the GFC. The ‘dissimilarity’ criterion was also found to have been fulfilled, given the number of well-researched alternative discourses. The ‘sensibility’ criterion is only partly fulfilled given, for instance, the limited success in dissemination and exposure while failing to make a significant impact on the mantra that ‘There is no alternative’. However, sensibility is a particular challenge in the face of elite appropriation, which involves obstructing exposure to alternative ideas, as well as the existence of prevailing cognitive maps, to the audience. The fourth criterion, ‘scholar validation’, has also been only partly fulfilled, since dissimilar discourses continue to face major hurdles in the face of entrenched scholarship structures and mechanisms favouring the dominant discourse, such as university education, funding, citations, journal rankings. However, we were able to show the growing interactions and collaboration among heterodox economists, as well as the existence of dissenting economics students. The final criterion, ‘elite appropriation’, has certainly not been fulfilled. The dominant elites continue to support the dominant discourse in various ways, particularly in terms of funding, but also through the processes of domination (political power, corporate power, ethical power and through the economics profession).

Part IV provides the conclusions and recommendations to materialise (r)evolutions in economics today. Given that this project
needs to involve a process in which economics, the economy and
democratic power, as well as cognitive maps, are emancipated from elite
appropriation, two further criteria are added: plutocrat disempowerment
and emancipation.

When it comes to the first criterion, ‘critical juncture’, it is
recommended that one be established. There are a large number of
crises around the world today, which are all, more or less, linked
together to form one massive, overarching crisis. These crises are in the
form of widespread poverty, inequality, unemployment, vulnerable
employment, environmental degradation, racism, sexism, as well as high
number of wars, authoritarian regimes, and de-democratisation
processes. However, in light of the ‘sensibility’ criterion, it may be
worthwhile showcasing such various dimensions of a holistic crisis not
only at the global, but also at the continental, national, or even local
levels, so as to garner sufficient attention to the issues at hand. In brief,
for the criteria ‘dissimilarity’, ‘sensibility’, and ‘scholar validation’, it is
suggested that it is essential to transcend interests, values and ideology
so as to shift cognitive frameworks towards alternative, or rather
emancipatory, ethics, economics and economy. In this endeavour, the
more the subjugatory structures and mechanisms are made visible, the
better for the (r)evolutionary project, as our findings suggest that power
is most effective when invisible. The fifth criterion this time around is
‘plutocrat disempowerment’. This is, of course, the most difficult
challenge of the (r)evolutionary project. Dominant economic elites have
managed to generate an excessive form of capitalism around the world,
in which capital is almost entirely equal to power. In other words,
excessive capitalism leads to a form of plutocracy that is equal to the
absence of real democracy. Therefore, we may conclude that the
dominant economic discourse helps to generate ever more totalitarian
governance systems, including fascism. The circle is complete. The way
out is through emancipation of individuals and institutions.
Emancipation is the process of taking someone or something from the
state of being subjugated to the state of being free. As such,
emancipation precedes freedom. (R)evolutions involve the start of a
change process going from subjugation and embarking on transitional
pathways toward freedom.
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PHD THESIS SUMMARY:
Polycentric democracy: using and defusing disagreements

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One of the main debates in political philosophy throughout the past four decades has been whether modern societies, despite their profound and ubiquitous disagreements, can find forms of association or institutional settings that are superior to our current modus vivendi arrangements. It is this question, I argue, that has motivated some of the most important recent contributions to political philosophy, such as John Rawls’s *A theory of justice* (1971) and *Political liberalism* (1993), James Buchanan’s work on constitutional economics (cf. 1975), and Gutmann and Thompson’s approach to deliberative democracy (cf. 2004). We might think of these contributions as constituting the philosophical canon with respect to the question of how we should deal with deep and pervasive disagreements in society. While the philosophical approaches of the above-mentioned authors are construed around a diverse set of moral and methodological priors, they also have something important in common: these authors view disagreements about facts and norms not only as a problem of pure theory but also as a predicament that fundamentally endangers the stability of society. Not surprisingly, they regard pervasive political disagreements in modern liberal societies mainly as a threat that needs to be contained.

As I show in the first chapter of the dissertation, the canonical approaches fail to produce institutional recommendations that can be viewed as an advancement beyond the current modus vivendi from a wide variety of comprehensive views. This motivates the thesis to propose a philosophical U-turn in the spirit of Adam Smith. This U-turn consists of three guiding ideas. The first is that a diversity of perspectives is not merely a nuisance, but an asset when solving hard problems. The second idea builds on the founding insight of institutional economics. Institutional economists have long held that whether self-interest works to the disadvantage or advantage of society depends on the rules of the game. By the same token, the dissertation

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argues that whether diversity—the cause of political disagreements—works to the advantage or disadvantage of society is a function of the rules of the game. The third idea is that an institutional structure that is construed around the idea of using diversity might have a better chance at generating an overlapping consensus than the canonical approaches that are based on a taming approach towards diversity.

The second part of the dissertation aims at getting a better conceptual understanding of the gains of diversity by comparing the two main approaches for reaping the benefits from diversity: expert deliberation and polycentric systems. The core insight of this analysis is that polycentric systems outperform expert deliberation with respect to hard problems because they are able to ameliorate the psychological and epistemic inhibitors of collective deliberation. In other words, the core insight of the second part is that competition—allowing diverse people to act on their idiosyncratic hunches—is crucial to unlocking the gains from diversity.

In the third part of the dissertation, I apply these insights to the political realm and advance the concept of a polycentric democracy. I argue that by creating more space for diverse political experiments, a polycentric democracy can leverage diversity’s full potential for the benefit of society. The concept of polycentric democracy is defined as an institutional arrangement involving a multiplicity of polities acting independently but under the constraints of a democratically supervised framework designed for institutional competition. A well-regulated competition between polities can be expected to achieve three outcomes that are especially interesting from a normative perspective.

**Discovering new heights:** Since institutional competition is a discovery process, a political polycentric system should constantly find new and better ‘ways of living’ or simply better modus vivendi arrangements.

**Reducing shallow disagreement:** Much disagreement in political philosophy is, as many commentators note, most likely due to our disagreement on non-normative facts. In political polycentric systems, a larger number of socio-economic theories can be tested than in monocentric ones. This should reduce disagreement.

**Defusing deep disagreement:** It is highly likely that not all of our disagreements are ultimately based on non-normative facts. In a polycentric system, people who disagree are allowed to enter into
polities with more like-minded people. Thus, tensions in society could be reduced.

Informed by the epistemic advantages of polycentric systems, the dissertation develops a contractarian argument in favor of polycentric democracy. The dissertation defends the normatively modest claim that for a very diverse set of reasonable political factions, choosing polycentric democracy over our current democratic modus vivendi arrangements would be rational.

The approach developed in the thesis is inspired by the work of several political economists on the advantages of federalism. At the same time, polycentric democracy has different explanatory and normative ambitions than the accounts of federalism developed by political economists such as Paul D. Aligica, Viktor Vanberg, Elinor Ostrom, James Buchanan, and Roland Vaubel.

In principle, there are a number of options available for implementing the ideal of polycentric democracy. The most prominent institutional proposals for implementation are the ones set forward by Chandran Kukathas (2007) and Robert Nozick (1999). Since none of their proposals could provide the basis for an overlapping consensus the dissertation offers a new approach for implementing polycentric democracy that should generate less resistance: the free city approach.

The main idea of this approach consists in adding special administrative zones or free cities to the structure of existing national states rather than changing the basic structure of society in its entirety.

A polycentric democracy constructed along those lines, the thesis conjectures, should be much more capable of using and defusing political disagreements than our current democratic orders.

REFERENCES


Julian F. Müller is a postdoctoral research associate at the Political Theory Project at Brown University. He studied philosophy, sinology, and economics in Tuebingen, Beijing, and Hamburg. After graduating in 2011, he became a research associate at the Peter Loescher Chair of Business Ethics at TU Munich. During the last years, he was a visiting scholar at the People's University of China and the University of Arizona. In 2015, he received his doctorate from the Technical University of Munich (summa cum laude). His thesis received two prestigious dissertation awards: the Werner von Melle award in 2016 for advancing the concept of the open society and the Roman Herzog research award in 2017 for advancing the concept of the social market economy. His articles have appeared in journals such as Philosophical Studies, Moral Philosophy and Politics, Journal of Business Ethics, and Science and Engineering Ethics.

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