

Final Proofs for: "GP08 is the New F53: Gul and Pesendorfer's Methodological Essay from the Viewpoint of Blaug's Popperian Methodology," Chapter 17 in Mark Blaug: Rebel With Many Causes. ISBN:9781781955666, M. Boumans and M. Klaes (eds.), Edward Elgar, 2013, 245-65.

17. GP08 is the new F53: Gul and Pesendorfer's methodological essay from the viewpoint of Blaug's Popperian methodology

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It would be foolhardy to tell fellow economists how to amend mainstream economics to take account of choice anomalies or even to abandon standard microeconomics in favor of one of the dissenting brands of economics ... However, what is clear is that the direct investigation of rational action, the attempt to test the urgency of the assumption of rationality, should not be dismissed out of hand. (Blaug 1992, p. 233)

17.1 INTRODUCTION

Mark Blaug was not only a methodologist; he was also a critical commentator on current methodological debates, and right now Faruk Gul and Wolfgang Pesendorfer's paper 'The Case for Mindless Economics' (hereafter GP08) is the most debated methodological paper among practicing economists. It was widely circulated prior to publication and the volume it was published in (Caplin and Schotter 2008) was entirely dedicated to the essay; GP08 was chapter one, followed by 14 chapters of commentary. For the younger generation of economists GP08 may now have replaced Milton Friedman's 1953 classic 'The Methodology of Positive Economics' (hereafter F53¹) as the 'only essay on methodology that a large number, perhaps a majority, of economists have ever read' (Hausman 1992, p. 162). Notice I say the popularity is 'among practicing economists'. Unlike when F53 was published, there is now an active community of scholars working in the field of economic methodology and GP08 has not received the attention from this community that one would expect.² Similarly, those who have written on GP08 have revealed very little interest in the literature produced by the methodological community. For example, of the 14 chapters of commentary on

GP08 in Caplin and Schotter (2008), only one chapter – Hausman (2008) – was written by someone who regularly contributes to the methodological literature.

The purpose of this chapter is to bring GP08 into closer contact with the traditional methodological literature in general and Mark Blaug's work in particular. This will be done in two related ways. First I will compare GP08 and Friedman's F53. There are a number of similarities between the two essays and as the title 'GP08 is the new F53' suggests, I believe they are significant. Second, I will turn to Blaug's Popperian economic methodology to explore the question of how Gul and Pesendorfer's arguments might look through Popperian spectacles.

17.2 GP08

GP08 can be divided into three parts. First, it is an endorsement of a particular approach to economic analysis (a version of revealed preference theory) which Gul and Pesendorfer consider to be 'standard economics'.³ Second, it is an argument that this particular approach to economic analysis is the way that scientific economics (at least choice theory)⁴ is in fact done and the only way that it can be done. Finally, it is a sustained attack on what Gul and Pesendorfer call 'neuroeconomics', in general, but particularly various neuroeconomics-based criticisms of standard theory.

It is important to note that Gul and Pesendorfer do not use the term 'neuroeconomics' in the way that most researchers who self-identify their work as neuroeconomics use the term; Gul and Pesendorfer define it much more broadly. For Gul and Pesendorfer, 'neuroeconomics' is any economic analysis that (a) employs psychological or physiological evidence and/or (b) interprets 'utility' as anything other than an index of observed choice (p. 3).⁵ Of course such a definition would include certain types of heterodox theorizing, but it also includes much of mainstream economics as well. For example, based on (a) 'neuroeconomics' would include almost all of the economic literature inspired by experimental psychology (Kahneman and Tversky and so on), a significant portion of current behavioral economics as well as the first generation behavioral economics associated with Herbert Simon and others,⁶ some of experimental economics, as well as traditional neuroeconomic research (that self-identified as such by its authors); and based on (b) 'neuroeconomics' would include all of the early neoclassicals from the late 19th and early 20th centuries who were hedonistic about utility and utilitarian about welfare economics (William Stanley Jevons, Alfred Marshall, and so on),

the recent neo-hedonist literature on behavioral welfare economics (Kahneman, Wakker and Sarin 1997 for example), the behavioral economics-inspired welfare economics of ‘libertarian paternalism’ (Sunstein and Thaler 2003, 2008), and a variety of other types of economics.

Gul and Pesendorfer argue that GP08 provides a defense of ‘standard economics’ (pp. 3, 4, 6, 5, 7, 8, 13, 17, 21, 26): the ‘common practice of economists’ (p. 3) that ‘can be found in a standard graduate textbook’ (p. 7). So what exactly is standard economics according to GP08? First of all, it starts with empirical choice data⁷ – most often prices and the quantities purchased at those prices – and employs a version of revealed preference theory to identify a utility function that would rationalize that data (or a consistent subset of it). A rationalizing utility function is one that if maximized subject to the relevant constraints would produce the consistent choices revealed in the initial choice data. This utility function can then be used to predict the agent’s choices for out-of-sample data and can also be integrated into more elaborate models of economic interaction (often, but not exclusively, game-theoretic).⁸

One of the implications of this approach is that the terms ‘preference’ and ‘utility’ do not necessarily refer to mental states that cause and explain economic behavior. According to GP08, standard economics does not explain economic behavior (choices) by means of the agent’s preferences or utility; it defines what the agent prefers, or that which is associated with the highest level utility, solely in terms of observed consistent choices and then uses the preference/utility so derived to predict the agent’s choices in other possible choice situations. As Gul and Pesendorfer explain:

In the standard approach, the terms ‘utility maximization’ and ‘choice’ are synonymous The relevant data are revealed preference data, that is, consumption choices given the individual’s constraints. These data are used to calibrate the model (in other words, to identify the particular parameters), and the resulting calibrated models are used to predict future choices and perhaps equilibrium variables Standard economics focuses on revealed preference because economic data come in this form Such data do not enable the economists to distinguish between what the agent intended to choose and what she ended up choosing. (pp. 7–8)

This is standard economics according to Gul and Pesendorfer and this is the scientific practice they defend against criticism from ‘neuroeconomics’.⁹ Given this characterization of standard economics, it is fairly easy to see how Gul and Pesendorfer can dismiss the various criticisms of

rational choice theory coming from behavioral economics and experimental psychology, since much of this criticism focuses on trying to get behind, or more realistically represent, the preferences that cause economic behavior. As Colin Camerer and George Loewenstein say in the first sentence of their survey of behavioral economics: ‘behavioral economics increases the explanatory power of economics by providing it with more realistic psychological foundations’ (Camerer and Loewenstein 2004, p. 3). But if standard economics is as Gul and Pesendorfer say – deriving a utility function that rationalizes a consistent set of choice data to extrapolate choices that would be associated with different parameters – then what could ‘greater psychological realism’ (ibid.) possibly mean? According to Gul and Pesendorfer, economists are not looking for the underlying psychological or physiological causes of economic choices and they are not trying to explain behavior in terms of mental or brain states – these are the objectives of psychologists and neuroscientists – they have entirely different goals: ‘The neuroeconomics program for change in economics ignores the fact that economists, even when dealing with questions related to those studies in psychology, have different objectives and address different empirical evidence’ (p. 4).

For example, in response to evidence that similar physiological causes of addiction (and the associated choices) are at work in humans and rats, Gul and Pesendorfer simply say:

That substances addictive for rats are also addictive in humans is not relevant for economists because (standard) economics does not study rats. It also does not study the causes of preferences. To say that a decision maker prefers x to y is to say that he never chooses y when x is also available, nothing more. (p. 20)

According to Gul and Pesendorfer, neuroeconomic critics want economics to be a kind of *therapy*: trying to understand what is really going on behind the choices that agents make in order to improve those choices. But economics is not therapy.

Neuroeconomics is therapeutic in its ambitions: it tries to improve an individual’s objectives. The central questions of neuroeconomists are: how do individuals make their choices? How effective are they at making the choices that increase their own well-being? By contrast, economists analyze how the choices of different individuals interact within a particular institutional setting, given their differing objectives (p. 9).

Given the goals of economists and the type of data that economists must deal with, choice-theoretic economics and the psychology and behavioral economics that Gul and Pesendorfer call neuroeconomics

simply represent incommensurable paradigms (note this is my term, borrowed from Thomas Kuhn (1970), not Gul and Pesendorfer's) and economic choice theory can only be the type of revealed preference analysis they endorse. As they summarize their position at the end of the essay: 'economic models can only be evaluated on their own terms, with respect to their own objectives and evidence A choice theory paper in economics must identify the revealed preference implications of the model presented and describe how revealed preference methods can be used to identify its parameters' (p. 36). The GP08 bottom line is that 'the neuroeconomic critique fails to refute any particular (standard) economic model and offers no challenge to standard economic methodology' (p. 7).

17.3 SIMILARITIES BETWEEN GP08 AND F53

GP08 and F53 have much in common. I will just discuss the two similarities that seem to be most important: (a) both F53 and GP08 are *ambiguous* essays that have *economists as the intended audience* and (b) they are both attempts to *defend a version of mainstream economic practice* from specific criticisms.

(a) The Economist as Audience and Ambiguity

Both essays were written by economists for economists, and both are relatively ambiguous and difficult to understand as a coherent argument (particularly as a coherent philosophical argument). These two aspects are interrelated because the narrowness of the audience contributes to the sense of ambiguity that philosophically-minded readers feel when they try to uncover the philosophy of science that undergirds the essays' claims and admonitions about economic knowledge. This results in a large number of often quite different interpretations of the central methodological message. I will argue this is true of both F53 and GP08, but since there is so much more literature on F53 and the evidence for its ambiguity and interpretative diversity is so clear, I will start with F53 then turn to ways that GP08 is similar.

The ambiguity of F53 is well-documented within the methodological literature.¹⁰ For example, as Uskali Mäki noted in 1992, 'The essay lacks coherence and often puts its points ambiguously. This both gives room and brings about a need for interpretation. Different interpretations fit different background commitments and serve different interests' (Mäki 1992, pp. 171–72), and continued to argue over the years: 'Friedman's essay is ... terribly confused and ambiguous, so readers can take liberties

to provide their own favorite interpretations' (2012, p. 13). This ambiguity of course facilitates 'multiple justifiable readings' (2009b, p. 49) and since ambiguity 'sometimes *helps* a piece of writing to make a popular impact' (ibid., p. 48), it is one of the reasons F53 was 'found persuasive by many economists' (Mäki 1992, p. 192).

But philosophical ambiguity and support among economists are not the only effects of the narrow audience. Mäki also argues this makes the reception of F53 more socially conditioned than is generally recognized. He notes: 'In passages mostly neglected by commentators, Friedman stresses the roles of subjective judgment, disciplinary tradition and institutions, and consensus among economists, in shaping theory choice' (Mäki 2012, p. 16). This implies that

What F53 suggests ... is that the *social performance* (embeddedness in disciplinary institutions and culture, continued use, collective acceptance, tenacious grip) of a theory provides *indirect evidence* in its support ... acceptances and rejections of theories are not strictly rule-governed responses to direct empirical evidence, they rather depend on the subjective judgments of economists whose behavior is shaped by their background and social context. (Mäki 2009c, p. 111, emphasis in original)

Such discipline-based appeals are also a common feature of GP08. For example in their discussion of why evidence of hedonic experiences should be ignored by economists Gul and Pesendorfer say:

Discussions of hedonic experiences play no role in standard economics models. Discussions of hedonic experiences play no role in standard economic analysis because economics makes no predictions about them and has no data to test such predictions. Economists also lack the means for integrating measurement of hedonic utility with standard economics data. (p. 4)

This may in fact be true, but why? Why does economics make 'no predictions' about, and have 'no data to test' hedonic experiences? Economists certainly could have both; 'neuroeconomists' are more than willing to provide such data and explain how to make such predictions. This is less an argument about what could or should be done within economics, than an appeal to certain existing professional conventions.

Gul and Pesendorfer also use the example of how 'risk aversion' is used by economists and 'neuroeconomists' and make the case that there is no way to decide which is better because the different definitions are based on 'disciplinary-specific abstractions'. How could one motivate the claim that 'economic models can only be evaluated on their own terms, with respect to their own objectives and evidence' (p. 36) without at least

implicitly assuming that the readers of the essay shared the authors' views about economic 'objectives and evidence'?

Ross makes similar disciplinary-focused remarks about *both* F53 and GP08, but for him it is not just a matter of audience, intuitions and institutions, but rather one of tapping into the 'deep methodological assumptions' of the economics profession. As he explains, both essays contain (at best) very weak arguments, particularly to the philosophically informed, but for Ross the popularity of the essays among economists in spite of this attests to their connecting up with the methodological message in a much more substantive way.

Their [GP08] paper is a general, almost entirely unqualified rejection of the relevance of neuroscience or psychology to economics. As with Friedman's essay, Gul and Pesendorfer's consists mainly of assertions and examples rather than arguments. And also as in the case of Friedman, it amplified a core observation about a deep methodological assumption animating much or even most economists' practice that had gone largely un-noticed by either methodologists within the discipline or commentators from outside it. (Ross 2011a, p. 218)

Of course this is a stronger claim than Mäki's arguments about the relevance of disciplinary context on the acceptability of economic theories and the persuasiveness of the essays to economists, and frankly it is not obvious why the essays need to touch anything methodologically 'deeper' than the desire to keep doing the same thing to explain support for essays which justify ignoring demands for more realistic assumptions (F53) or more psychological realism (GP08). But it is not necessary to take sides on these two arguments to make the case that the essays were written by economists for economists and (for a number of possible reasons) that has contributed to their appeal among members of the economics profession as well as to the relatively unsympathetic reading they received by philosophers and others. And this is a common feature of the two essays.

(b) Defends (at least a version of) Mainstream Practice Against Specific Alternative Approaches and the Criticisms they Raise

The impact of the disciplinary context on F53 is also generally well-accepted. Friedman wanted to defend a version of standard practice against attacks from specific economic theories and he clearly saw more than one such threat. The 'marginalist controversy' sparked by Lester (1946) and the earlier 'full-cost-pricing' debate associated with Hall and Hitch (1939) were an influential part of the background context for F53.

The critics argued, based on surveys of business managers, that firms did not employ marginalist reasoning and marginalist rules for profit maximization when making business decisions. As Roger Backhouse explains:

It was only after Lester's challenge in 1946 that the main issue came to be seen as marginalism *versus* a non-marginalist alternative. After the marginalist controversy of 1946–7, the debate over the theory of the firm took on a different character. Debate over markets was transformed into debate over marginalism. This was precisely the time when Friedman began working on F53. (Backhouse 2009, p. 228)

The consensus is that the debate was won by the marginalist and that F53 played an important role in the victory.

But the marginalist and full-cost controversies were not the only debates important to F53; there was also the theory of 'imperfect competition' associated with Edward Chamberlin (1933) and Joan Robinson (1933) which offered a serious challenge to the traditional model of firm behavior. There were many aspects to the imperfect competition critique, but the most important was the lack of realisticness (or as Friedman would put it 'realism') of the assumptions of the standard models of firm behavior (and the realisticness of the imperfectly competitive model). As Friedman explained in F53:

The theory of monopolistic and imperfect competition ... was explicitly motivated, and its wide acceptance and approval largely explained, by the belief that the assumptions of 'perfect competition' or 'perfect monopoly' said to underlie neoclassical economic theory are a false image of reality. And this belief was itself based almost entirely on the directly perceived descriptive inaccuracy of the assumptions rather than on any recognized contradiction of predictions derived from neoclassical economic theory. (F53, p. 15)

Finally, there was the Cowles Commission's residence at the University of Chicago 1935–55 which brought with it Keynesian macroeconomics, Walrasian general equilibrium theory, and structural equation econometrics – none of which was the kind of economics (for a variety of reasons) supported by Friedman or most of his Chicago colleagues. As Friedman explained in an interview with Daniel Hammond: 'I was a major critic of the kind of thing they were doing in Chicago. I introduced the idea of testing their work against naïve models, naïve hypotheses, and so on. So I was very unsympathetic ... from the beginning' (Hammond 1993, p. 231). Here too many of the features of Cowles economics that Friedman was most concerned about became the targets of F53. All in all Friedman had in mind very clear examples of flawed economic theorizing and the methodological imperatives that supported them, as well as

the ways that defenders of these approaches had criticized the standard economics that Friedman defended.

GP08 is also clearly a methodological essay that self-consciously defends what the authors see as mainstream practice against specific alternative theoretical and methodological approaches that are explicitly critical of such practice. GP08 is a relentless attack on 'neuroeconomics' and the associated critique of rational choice theory. The first two sentences of GP08 read: 'Neuroeconomics proposes radical changes in the methods of economics. This chapter discusses the proposed changes in methodology, together with a neuroeconomic critique of standard economics' (GP08, p. 3). And the defensive emphasis on 'neuroeconomics' continues throughout. They eventually make a case for how choice theory is, must be, and should be done, but that methodological message always plays a secondary role to the defense against the 'neuroeconomic' critique.

As Mäki notes (without reference to GP08) this is quite similar to the disciplinary context of F53.

F53 became a major defensive statement that sought to undermine the empirical criticisms that were leveled against marginalist maximization assumptions. Today we are again witnessing massive attacks against the standard assumptions of rationality, and there are new attempt to replace them by more realistic assumptions. The difference is that in the late 1930s and early 1940s, the critical strategy was one based on field survey, simply asking business managers what they were doing, while nowadays the critical conclusions derive from experiment and brain scan, with psychologists and neuroscientists contributing to the collection of critical evidence. (Mäki 2009b, p. 49)

Hausman has explicitly drawn out broad similarities between F53 and GP08 on the basis of this similar context. He argues that 'The context in which Milton Friedman wrote his renowned essay, ... is in important ways analogous to the context in which Gul and Pesendorfer are writing, and their central arguments ... are, I believe, similar to Friedman's' (Hausman 2008, p. 139). He discusses the context of Hall and Hitch (1939) and Lester (1946) and how F53 seemed to effectively diffuse these critiques. In addition 'Gul and Pesendorfer, like Friedman, argue that certain data that apparently bear on generalizations employed in economic models are in fact irrelevant to the acceptance or rejection of those models' (Hausman 2008, p. 140). The problem situation is the same and the defensive strategy is the same, only the goals of the profession have changed in response to changes in the nature of the threat.

In closing this discussion of the similarities between F53 and GP08, I want to briefly mention a topic that might have been raised as an additional similarity and explain why I did not offer it as such. It might be argued that both F53 and GP08 are consistent with an *instrumentalist* view of scientific theories. Put simply, instrumentalism is a philosophy of science that considers scientific theories to be nothing other than tools for organizing empirical data, and given this, the only way to evaluate the success of a scientific theory is the degree to which the theory accurately predicts relevant empirical evidence (in other words, saves the relevant phenomena). A long line of interpreters – starting with Bear and Orr (1967), Boland (1979) and Wong (1973) – have argued that instrumentalism is the most reasonable philosophical interpretation of F53, and since according to GP08 preferences do not cause or explain the choices that agents make, and the main purpose of choice theory is to predict out-of-sample empirical data, it could be interpreted in instrumentalist terms as well. So given this, why didn't I list instrumentalism as a third similarity between F53 and GP08? The problem is that it is not entirely clear that the view of scientific theories at work in either F53 or GP08 is best described as instrumentalism. Although instrumentalism was once the dominant interpretation of F53, there have always been many others, and at this point it seems to be losing support.¹¹ The situation with GP08 is similar. Gul and Pesendorfer repeatedly make statements that are contrary to an instrumental (non-causal and non-explanatory) interpretation of the revealed preference theory they defend.¹² For example, they tell us that for 'standard' welfare economics 'what is relevant are the agent's interests (or preferences) as perceived by the agents themselves' (p. 5). Perhaps, but how could fitting a rationalizing utility function onto objective choice data tell us anything about what the agent 'perceived' (as opposed to what they did)? And later we are told that economics studies 'the purposeful behaviors of different individuals' (p. 34). Perhaps, but how could choice data with no presumption that it was caused by underlying mental states say anything about 'purposeful behaviors'? There is simply too much philosophical and methodological ambiguity in both F53 and GP08 to be confident about the appropriateness of the instrumentalist interpretation and so there is no reason to consider this a similarity between the two essays.

17.4 A POPPERIAN EXAMINATION OF GP08

Perhaps the most direct way to bring Mark Blaug's economic methodology into the discussion about GP08 would be to examine Blaug's

criticisms of F53 and then, given similarities between F53 and GP08, attempt to show those criticisms also apply to GP08. This seems like an obvious way to organize this section, but unfortunately it does not work. The problem is that Blaug was not consistent over time about his critical evaluation of the methodological prescriptions of F53. On his first few readings early in his career, he thought F53 ‘was a good argument’ (Blaug 2009, p. 34). By the first edition of *The Methodology of Economics* in 1980 he accepted the instrumentalist interpretation of F53 and was extremely critical of it (following Karl Popper’s criticism of instrumentalism in *Conjectures and Refutations* 1965). One of his main points involved the symmetry thesis: the logical empiricist idea that empirical prediction and scientific explanation are symmetric. He argued that ‘instrumentalism is untenable because the symmetry thesis is false’ (1980, p. 113), and since F53 was linked to instrumentalism, it was false as well. In the second edition of *The Methodology of Economics* (1992) Blaug continued to be critical of F53, but his criticism was significantly softened. He still endorsed the instrumentalist reading, but he eliminated the entire discussion of the symmetry thesis and substituted a short section arguing that the main problem was that ‘instrumentalism is an excessively modest methodology’ (1992, p. 99). Finally, in a concluding chapter to Mäki (2009a) Blaug moved entirely away from the instrumentalist criticism of the Friedman essay:

it is difficult to see any explicit endorsement of F53 on the ... philosophy of instrumentalism. Uskali Mäki (1986) is quite right, it seems to me, to reject Boland’s claim that when F53 is read carefully it will be found to be ‘both logically sound and unambiguously based on a coherent philosophy of science – instrumentalism’. (Blaug 2009, p. 351)

This is a significant change from his earlier criticisms of F53 which were based entirely on its commitment to instrumentalism. Now of course there are often very good reasons for changing one’s mind, and perhaps Blaug’s reasons changing his view of F53 were quite good, but it does make it difficult to simply apply what Blaug said about F53 to directly GP08 since he said very different things at different times.

So given that the most obvious door to a Blaug-type analysis of GP08 seems closed, how will I proceed? I will do so by taking a less direct approach. Since Blaug was always sympathetic to Popperian philosophical ideas and used them as the overarching philosophical framework for thinking about economic methodology, I will examine GP08 through some very broad/generic Popperian spectacles and see how it looks. I say ‘broad/generic Popperian spectacles’ because there are a number of

different sets of Popperian spectacles to choose from and each provides a slightly different view. Although it is potentially a very large set, the four main Popperian positions that have played a role in economic methodology are: Popper's well-known falsificationism (Popper 1965, 1968; most associated in economics with the writings of Mark Blaug¹³ and Terence Hutchison), Lakatos's methodology of scientific research programs (De Marchi and Blaug 1991; Latsis 1976), Popperian critical rationalism (most associated with the writings of Lawrence Boland and Klappholz and Agassi 1959), and Popper's own (few) writings explicitly on the science of economics (Caldwell 1991; Popper 1985, 1994).

So what might one come up with if one were to try to meld the central ideas of these different versions of Popperian economic methodology into a single broad/generic Popperian methodology? The five most important points (P1)–(P5) seem to be:

(P1) *The best of all circumstances for the growth of scientific knowledge involves severe empirical testing of empirically falsifiable scientific theories and the rejection of those that are falsified.*

(P2) *Under conditions that are less than ideal (which are more likely in the social and biological than the physical sciences) theories may be modified in response to a negative evidence as long as the modifications are done in an empirical content increasing, non-ad hoc, way (that is not making changes to the theory specifically designed to protect it from known falsifying evidence).*

(P3) *A research program or set of closely connected theories can be progressive even if various parts are falsified as long as the overall ensemble systematically produces novel corroborations (that is those that increase the empirical content of the program and have not been produced by ad hoc theory modifications).*

(P4) *The key to the growth of scientific knowledge is criticism; severe empirical tests are the strongest form of criticism, but there are many other weaker, yet effective, forms of criticism within science. Protective moves that are ad hoc and empirical content reducing are the worst ways for scientists to respond to negative empirical evidence, but other less dramatic protective moves should be avoided as well.*

(P5) *The rationality assumption plays a unique role in economics and can be given a modest degree of protection from falsification as long as the other aspects of the research program (auxiliary hypotheses, and so*

on) are tested and not modified in ad hoc ways. But the rationality assumption should not be completely immune to criticism.

So how does GP08 stand up under the scrutiny of (P1)–(P5)? The first thing to note is that strict falsificationism does not provide us with a tool that will allow us to evaluate GP08, or for that matter any other economic theory or methodological approach concerned with the rationality principle or rational choice. As Popper himself argued (1985, 1994) if the rationality principle is stated narrowly enough to be falsifiable (for example all agents maximize a strictly quasi-concave homothetic utility function) then it will be false, but if the rationality principle is stated quite broadly (all agents act appropriately) it will be nonfalsifiable. Surely the ‘rationalizing’ utility function of Gul and Pesendorfer’s revealed preference theory is of the latter sort. One is not testing the presupposition of the existence of a rationalizing utility function, one is ‘fitting’ such a function to a set of choice data on the basis of a theorem (from Houthakker 1950) that if the data is consistent with the strong axiom of revealed preference, then such a rationalizing utility function always exists. As (P5) notes, economics involving some version of the rationality principle will require special, more lenient, dispensation against falsificationism.¹⁴ Economics is not the best of all scientific circumstances and (P1) does not apply; the required criticism will need to come from elsewhere (P2)–(P4).

So how can we address the question of whether, or the degree to which, GP08 requires a sufficient degree of criticism to meet Popperian methodological standards, while recognizing that a version of the rationality principle is involved as well as a variety of discipline-specific constraints? One consideration is an issue that Blaug often stressed, the problem of being a ‘defensive methodology’. A defensive methodology is one that justifies or rationalizes the over-protection of favored theories from various types of criticism (empirical attempted falsifications of course, but not exclusively).¹⁵ How defensive is GP08 in this respect? Well, actually, it seems quite defensive.

One issue is the narrow way that GP08 defines what economists do and must do. Part of the story is that this is because economics has different goals than other human sciences (particularly ‘neuroeconomics’), but it is also because economic data takes a very specific form: ‘this is the form of evidence that is available to economists’ (p. 36). As I noted at one point above, why? Why is this the form? The point of much of ‘neuroeconomics’ is precisely that such need not be the case – perhaps it was once the case, but technology and advancements in neuroscience now provide a much richer range of data and tools. Of

course Gul and Pesendorfer would reply: 'But those are not economic data or economic tools'. But then again why not? Such digging in of one's heels and insisting 'I can't hear you, I'm an economist' is what some commentators have called 'willfully and obstructively isolationist' (Harrison and Ross 2010, p. 194). Such obstructive isolationism is a very defensive methodological stance and one that has much in common with the Kuhnian defensive methodology that Blaug so strongly opposed (Blaug 1976).

Another aspect of GP08 that seems very defensive is the relentless focus on the 'neuroeconomic' enemy. Of course great economists have always presented an alternative against which to make their case. Adam Smith had his mercantilism and John Maynard Keynes his classical theory. But while these theories serve as a foil and critical starting point for their own analysis, they do eventually end up spending most of their energy articulating their own theory, its features and advantages. It seems this has been the case in economic methodology as well. John Stuart Mill (1874) had his practical man and Lionel Robbins (1935) had his institutionalists and behaviorists – approaches that serve as negative examples and conceptual motivators for their own methodological positions – but they were not the main argument. The main thrust of most influential methodological research has been to explain one's own position and demonstrate how it answers various questions about economic knowledge. But this is not the case for GP08. 'Neuroeconomics' is Gul and Pesendorfer's main focus; they just keep coming back to it over and over. Although both F53 and GP08 do this to some extent, the problem seems to be much more serious in GP08. There is a lot of ambiguity in Friedman's attempt to articulate his methodological position, but he does in fact spend the majority of time attempting to do just that: to articulate his methodological position. This is not the case with GP08.

There is a Popperian issue about both of these aspects of GP08. Although Popper never considered philosophy of science to be a scientific endeavor – falsificationism need not be empirically falsifiable – he did argue that methodological programs should stick their necks out; they should actually say something about what scientific knowledge is and what practices will bring it about; they should expose themselves to criticism. Over-emphasis on how a particular field is, and must be, isolated from criticism from other scientific fields and paying an excessive amount of attention to defending the preferred practice from alternative approaches, is not sticking one's neck out. It is precisely the type of defensive methodology that Blaug so often warned against.

So keeping all this in mind I will end with a sweeping Blaug-like Popperian assessment of GP08.¹⁶ What should they – supporters of GP08 – do to be consistent with this Popperian vision? I suggest this: supporters of GP08 should stop paying so much attention to ‘neuro-economics’ and get on with the positive project of thinking carefully about the version of revealed preference they support. They should fully develop that approach based on the (positive) methodological principles of GP08 and try to apply the approach to a wider and wider range of choice data and areas of economic analysis. Stick the theory’s neck out. If it holds up relative to the best alternative choice theories – without resorting to defensive or ad hoc moves that dismiss significant evidence as non-economic (note Blaug’s remarks in the epigraph) – then continue to expand the domain of application. If there are occasional negative empirical results, the program can be revised as long as it is in a non-ad hoc way. If all goes well the program will gain influence and the (positive) methodological strictures of GP08 will have contributed to the development of a progressive scientific research program in applied revealed preference analysis. But if not, if it runs into massive amounts of contrary evidence and/or needs to continuously make ad hoc defensive adjustments to get tractable empirical results, then something will be learned from that as well. Be bold and stick your neck out, and most importantly quit talking about ‘neuroeconomics’. The problem for supporters of GP08 is to develop a new empirically progressive research program in revealed preference analysis; it is not ‘neuroeconomics’.

16.5 CONCLUSION

This chapter has tried to bring Blaug-type Popperian methodological analysis to bear on GP08. The main points of the GP08 essay were discussed in section 17.2. Section 17.3 pointed out several similarities between GP08 and F53 and section 17.4 provided a broad-based Popperian analysis of GP08. To the best of my knowledge Blaug did not write anything on GP08, but I believe the Popperian assessment above is representative of what he would have said had he done so. I also believe it points out some serious problems with GP08.

NOTES

- * I would like to thank Bruce Caldwell, John Davis, and two anonymous referees for helpful comments on earlier versions of this paper.
1. Following the convention established in Mäki (2009a).

2. Of course this is not to say that GP08 has not received any attention from the methodological community. Alexandrova and Haybron (2011), Hands (2011b, forthcoming b), Hausman (2008, 2012), Ross (2008b, 2011a, 2011b, 2011c), a number of the papers in neuroeconomics special issues of both *Economics and Philosophy* (Bonanno et al. 2008) and *The Journal of Economic Methodology* (Marchionni and Vromen 2010), as well as a few others, have engaged GP08 in various ways. Still, given the reception from economists in general, the relative lack of attention from within the methodological community is surprising.
3. Gul and Pesendorfer also discuss how 'standard economics' functions in the domain of normative welfare economics, but I will focus exclusively on their characterization of positive economics. See Hands (forthcoming b) for a discussion of the relationship between GP08 and welfare economics.
4. GP08 often sounds like the authors are arguing that such 'standard economics' constitutes all of economics: econometrics, macroeconomics, everything. Given the way they characterize it and its revealed preference origins, it seems more charitable to interpret them as saying that 'standard economics' exhausts all of (rational) choice theory and not literally all of economics, and I will employ this interpretation throughout. As one reader of an early version of this paper noted, this is a 'massive domain-retreat' from the 'ambitions common to earlier generations of economists'.
5. Page numbers without additional citation information will refer to GP08, Gul and Pesendorfer (2008), throughout.
6. As well as the work of the early ordinalists who addressed some of the same issues as behavioral economists (see Hands 2006, 2010 and 2011a for a discussion of this literature).
7. For Gul and Pesendorfer it is individual choice data – the purchases of a particular economic agent – but the approach could also be applied to more aggregate data where no obvious economic 'agent' can be identified (like market data). Some who generally support this version of revealed preference theory, such as Ross (2008b, 2011a, 2012) and Harrison and Ross (2010), consider the extension to aggregated data to be an important advantage of this approach over more traditional, methodologically individualist, approaches and often criticize Gul and Pesendorfer for not emphasizing this: for making 'a methodological fetish out of choice at the level of the individual personal agent' (Harrison and Ross 2010, p. 187) which 'impedes their capacity for self-defense' (Ross 2008b, p. 57).
8. In Hands (2011b, forthcoming a, forthcoming b) I identified a body of recent economic research called Contemporary Revealed Preference Theory (CRPT) and offered GP08 as a paradigmatic example of that approach. I also traced the historical relationship between this version of revealed preference and other members of the revealed preference family. But the distinction between this and other forms of revealed preference is not necessary here since the focus is entirely on GP08.
9. It is surprising how much disagreement there is about the *fact of the matter* regarding the relationship between the GP08 version of revealed preference theory and what is in fact standard in modern economic analysis. It would appear to be a fairly straightforward empirical task to find out whether most economists – in research papers, in textbooks, and in the way they normally think/talk about choice behavior – view preferences as determining choices (as in the traditional folk-psychological way of explaining human behavior) or view choices as determining preferences (as with GP08 and the related revealed preference literature). This seems like a straightforward empirical question, and yet there is extremely wide disagreement on the topic. Even if we just focus on views originating from within the methodological community there is a vast array of different opinions on this empirical question. For example, Anna Alexandrova and Daniel Haybron argue that the GP08 version of revealed preference is in fact how most contemporary economists view choice theory

and trace it back to ‘methodological minimalism, born of the positivist doctrines that prevailed for much of the last century’ (2011, p. 96); this is somewhat surprising since they also argue that it is *not* the way that choice theory *should* be done (they criticize GP08, but consider it to be a criticism of the actual practice of most economists). Francesco Guala (2011) also traces this view back to the influence of positivism and behaviorism, but unlike Alexandrova and Haybron, he argues that economists have softened on this issue (with the demise of the positivist received view) and that economists now generally see preferences as mental states that cause choices. Daniel Hausman (2008, 2012) argues that the majority of economists have always thought of preferences (along with beliefs) as causing choices (as does Mäki 2000, 2002, 2012); according to Hausman this is how the majority of economists have in fact thought about preferences and it is also how they need to think about preferences to achieve the goals of the profession. For Ross (2005 and elsewhere) the GP08 view is a bright thread that runs throughout the history of neoclassical economics (at least from Robbins’s and Samuelson’s work during the 1930s and 1940s) and the profession would be well-served by fully embracing it as the standard view (providing a structural realist defense of such economics has been one of Ross’s main goals). I have criticized Ross’s interpretation of Robbins and Samuelson (Hands 2008, 2009) and I do not believe the GP08 view has ever been dominant, or even a serious contender, within modern economics (although it has played a significant rhetorical role), but I would also note that its support has been increasing during the last decade or so. Given such disagreement, this topic certainly seems to be ripe (perhaps overripe) for some definitive empirical research.

10. And often painfully clear to those of us who regularly teach F53.
11. Uskali Mäki (1992 and elsewhere) has long defended a scientific realist view of F53 and over time that interpretation has gained supporters (and versions of realism are not the only non-instrumentalist interpretations). See the collection in Mäki (2009a) for example for a variety of different interpretations of F53, with none really defending a strict instrumentalist reading. Of course, this does not mean that instrumentalism is entirely dead in economic methodology (Reiss 2012).
12. GP08, like F53, also has realist interpretations (Ross 1995, 2005, 2008a). Actually to be more accurate, Ross is not providing a realist interpretation of GP08, but rather of the type of revealed preference analysis Gul and Pesendorfer support.
13. Although over the course of his career Blaug’s Popperianism was not always of the purely falsificationist sort; he also employed Imre Lakatos’s approach as well as other parts of Popper’s writings.
14. As Popper himself noted: it is ‘sound methodological policy to decide not to make the rationality principle, but the rest of the theory – that is, the model – accountable’ (Popper 1994, p. 177).
15. As Blaug noted about F53, it is ‘a defensive methodology whose principal purpose seems to be to protect economics against the carping criticism of unrealistic assumptions, on the one hand, and the strident demand of severely tested predictions, on the other’ (Blaug 1980, p. 115 and 1992, p. 99).
16. Noting that Blaug was never shy about such assessments: of economic theories, of economic research programs, or of methodological treatises.

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