

### **2.1.4 Austrian Economic Methodology**

Although Robbins was influenced by certain Austrian ideas, his methodology does not offer a very smooth transition into the Austrian literature discussed in this section. Unfortunately, a more effective segue is not really available. The problem is the rather enigmatic relationship that exists between the Millian and Austrian methodological traditions. On the one hand, Austrian methodology is frequently presented as a special case of Millian *a priorism*, and, yet, on the other hand, the Austrian tradition is both antiempiricist (and, thus, deeply at odds with Mill's fundamental philosophical commitments) and earnestly marginalist in its economics (and, thus, equally at odds with Mill's commitment to classical economics). As we will see in later chapters, the tendency to view these two methodological approaches as fundamentally similar undoubtedly owes more to the influence of mid-twentieth century positivism than to any deep philosophical common ground, but, nonetheless, it still provides the main rationale for adding the Austrian position to this section on Millian methodology.

Austrian methodology is far more difficult to summarize than the methodological writings of Mill, Robbins, or the other authors in this section. Like the work of these authors, the Austrian view is subject to a variety of different interpretations, but the Austrian situation is compounded by the fact that there are so many different economists, with so many different points of view, that can all be (and would probably want to be) classified as "Austrian." The sheer bulk of the literature and the range of diversity within the program combine to make it effectively impossible to examine all, or even the majority, of the work in the Austrian methodological tradition. My approach will be to briefly consider the founder of the Austrian school, Carl Menger, and then turn to the methodological writings of the two most-important figures in twentieth-century Austrian economics: Ludwig von Mises and Friedrich von Hayek. Although the resulting discussion is not a thorough examination of Austrian methodology, it should provide a useful introduction as well as an effective guide for those wishing to delve deeper into the subject.

Carl Menger (1840-1921) was both the architect of the Austrian school and one of the economists sharing responsibility for the early development of neoclassical economics. Menger's *Principles of Economics* (1976), Leon Walras's *Elements of Pure Economics* (1954), and William Stanley Jevons's *Theory of Political Economy* (1879) all appeared in the early 1870s and are generally considered to be the three most important books in what ultimately came to be called the neoclassical (or marginalist) revolution. The works of Menger, Walras, and Jevons do have much in common, but there are also significant differences (Jaffé 1976), and Menger's economics in particular differed substantially from that of Jevons and Walras. One difference was that both Jevons and Walras relied heavily on differential calculus (and thought it was essential for the argument), while Menger avoided the use of advanced mathematics entirely, but the differences run much deeper than simply the use of calculus. Menger advocated a "subjectivist neoclassicism" (Greenfield and Salerno 1983) — that emphasized the subjective goal-directed actions of individual economic agents — a view that continues to characterize the "Austrian" approach to economic theory, but one that ultimately came to be overshadowed by the (now dominant) Walrasian research program.

Although Menger has been the subject of a massive interpretative literature, the customary reading is that while Menger had many intellectual influences (see various papers in Caldwell 1990), his underlying philosophical position is best described as a version of Aristotelian essentialist realism.<sup>13</sup> It is important to emphasize Menger's Aristotelianism, since it represents a radically different point of departure than the empiricism of John Stuart Mill. Although Mill and Menger both end up advocating a deductive *a priori* approach to economics, and although their general approach to theorizing (as opposed to their actual economic theories) may be indistinguishable to the casual observer, they are in fact starting from entirely different philosophical positions (Cartwright 1994b). This tension — the tension between an empiricist-inspired deductivism (the Millian tradition) and the openly antiempiricist deductivism of certain Austrians — has manifested itself in many different ways during the last hundred years of methodological debate.<sup>14</sup>

Although the *Methodenstreit* within the German historical school probably pushed Menger into a rather exaggerated version of his view, it is also clear that his position within the debate reflected his overall methodological convictions. The actual debate between Menger and Gustav Schmoller was remarkably short-lived. It began in 1883 with Schmoller's harsh review of Menger's *Untersuchungen* (translated as *Problems of Economics and Sociology* 1963), and ended in 1884 with Menger's equally strident reply, a reply that took the form of a pamphlet written as letters to a friend. Although the formal exchange between the two individuals ended with Menger's reply, the *Methodenstreit* dragged on throughout Menger's life and ultimately had a profound impact on both the teaching of economics in Germany and the Austrian attitude about the importance of methodology.

It is necessary to realise fully the passion which this controversy aroused, and what the break with the ruling school in Germany meant to Menger and his followers, if we are to understand why the problem of the adequate methods remained the dominating concern of most of Menger's later life. Schmoller, indeed, went so far as to declare publicly that members of the "abstract" school were unfit to fill a teaching position in a German university, and his influence was quite sufficient to make this equivalent to a complete exclusion of all adherents to Menger's doctrines from academic positions in Germany. (Hayek 1934, p. 407)

The standard interpretation of the *Methodenstreit* reduces the entire debate to a disagreement about whether deduction or induction represents the (only) proper method for obtaining economic knowledge. Menger is viewed as a radical deductivist who wanted to deduce all of economic theory from a few basic propositions about economic behavior, while the German historical school is viewed as an equally radical, inductivist sect that wanted to abandon theory altogether in favor of the endless accumulation of empirical and historical data. This portrait of Menger suggests that he was not at all interested in either empirical evidence or the structure of social institutions, while this interpretation of Schmoller makes him into an interminable fact finder: an "inductivist" who never gets around to actually making any inductive inferences. This standard caricature really does an injustice to both sides of the debate. Although sorting out the literature on the *Methodenstreit* is clearly beyond the current project, it should at least be noted in passing that neither side actually advocated a view that was anywhere near as simplistic as that suggested by the standard interpretation. Even recognizing that the heat of the fray often pushes authors into simplistic positions, the arguments of both sides were substantially more complex (and much more philosophically interesting) than merely quarreling over whether pure deduction or pure induction constitutes the proper method of economic science.<sup>15</sup>

The methodological position of one of Menger's most influential followers, the third-generation Austrian economist Ludwig von Mises (1881-1973), does though come fairly close to the caricature

version of Menger's position in the *Methodenstreit*.<sup>16</sup> While Mises's view represents a radical departure from the methodological mainstream in economics — a mainstream that despite its diversity tends to be generally empiricist and methodologically monist (social and natural science should practice the same “scientific method”) — his view is often presented as the paradigm case of Austrian methodology (see, for example, Hutchison 1981). Perhaps commentators equate Austrian methodology with Mises's interpretation because extreme positions make easier targets, or perhaps it is simply because of the vehemence with which Mises advocated the same (rather radical) methodological position throughout his career.

Mises (1949, 1978) called his approach to economic methodology “praxeology.” The philosophical origins of praxeology are Kantian: just as Kant answered the question of how our concepts and experiences match up to the objective features of the external world by turning the question upside down — making the objective world match up to our concepts and experiential framework — Mises, too, relied on the essential features of the human subjective constitution to ground his concept of knowledge.<sup>17</sup> For Kant, there were certain basic principles and judgments that formed the basis of our knowledge — things such as the rules of logic, the idea that every event has a cause, and the fact that objects exist — that are so fundamental to our understanding that without them no meaningful experience would be possible at all; because knowledge of such principles is necessary (a precondition) for understanding at all, they can not come from outside, from empirical observation, but must be *synthetic a priori true*. For Mises, economic knowledge also has a (unique) necessary precondition — a synthetic *a priori true* proposition necessary for the possibility of meaningful experience — it is that *human beings act* (engage in intentional or purposive behavior).

The a priori knowledge of praxeology is entirely different — categorically different — from mathematics. . . . The starting point of all praxeological thinking is not arbitrarily chosen axioms, but a self-evident proposition, fully, clearly and necessarily present in every human mind. . . . The characteristic feature of man is precisely that he consciously acts. Man is Homo agens, the acting animal. . . . To act means: to strive after ends, that is, to choose a goal and to resort to means in order to attain that goal sought. (Mises 1978, pp. 4-5)

Knowledge of the fact that humans act purposefully is not only a precondition for all knowledge of human behavior, it is knowledge that we possess, in part, because of our self-knowledge regarding our own actions.

What we know about our own actions and about those of other people is conditioned by our familiarity with the category of action that we owe to a process of self-examination and introspection as well as of understanding of other people's conduct. To question this insight is no less impossible than to question the fact that we are alive. (Mises 1978, p. 71)<sup>18</sup>

This postulate — that agents act and thereby engage in purposeful, intentional, goal-directed behavior — is the starting point for the entire Misesian research program in economics. All legitimate economic theory follows as a deduction from this core *a priori* presupposition.

Praxeology is a priori. All its theorems are products of deductive reasoning that starts from the category of action. . . . Every theorem of praxeology is deduced by logical reasoning from the category of action. It partakes of the apodictic certainty provided by logical reasoning that starts from an a priori category. (Mises 1978, p. 44)

The Misesian approach has at least three important methodological implications: *methodological individualism*, *methodological dualism*, and *a priorism* (Boettke 1998). It is useful to examine each of these in turn.

Methodological individualism is a common position in the philosophy of economics; it was advocated by Mill, Robbins, and most of the others discussed above (and below as well). Although the philosophical literature is replete with numerous specific versions of methodological individualism (see Kincaid 1996, for example), the Misesian variant is based on the simple presupposition that only individuals act: “The collective has no existence and reality but in the actions of individuals” (Kincaid 1996, p. 81). This means (as with Robbins) that all of economics is microeconomics, and although macroeconomic regularities might sometimes be of interest to economists and policy makers, macroeconomic constructs such as the consumption function are totally devoid of any real explanatory power. As Walter Block explains in a reply to a paper on Austrian methodology by the philosopher Robert Nozick (1977):

For the claim of the Austrians is that although *microeconomics* is correct in its own terms, able to trace phenomena back to the causal agents (individual decisions), macroeconomics includes only artificial constructs which, apart from the individual choices upon which they are very indirectly based, have no causal explanatory power on their own. There are, to be sure, statistical correlations between various of the macroeconomic aggregates. But cut off from the *purposes* of human actors, the only causal agent in economics, they are powerless to form part of a causal genetic chain. (Block 1980, p. 407)

Although individualism is a common view among those writing on economic methodology, Mises’s second affirmation — methodological dualism — is quite uncommon. Methodological dualism is the position that the human and social sciences are fundamentally different in character than the natural sciences: that there is not a single scientific method, but rather two different methods, one suitable for studying humans in society and another for studying nonhuman nature. Of course, dualism (two different methods) is a subset of methodological pluralism: the view that there are many different ways of obtaining knowledge depending on the subject at hand. Mill, who was firmly monistic with respect to epistemology (all knowledge was grounded in empirical evidence), was methodologically pluralistic — different sciences have different specific methods for obtaining knowledge in their particular domain — but such pluralism is relatively rare among those writing on economic methodology (and later authors in the Millian tradition played down this aspect of Mill’s view). Mises’s dualism follows immediately from his definition of *human action*. Humans act teleologically — they engage in purposeful goal-directed behavior — rocks and trees do not. Perhaps at one point in our history, when lightning bolts were viewed as a result of purposeful behavior by angry gods, humans explained natural phenomena in teleological terms, but modern science has replaced such concepts with the laws of nature. Whereas modern science may have accomplished a lot with the materialistic point of view, Mises argues that it is not possible to reduce the goal-directed action of humans to physiology or brain chemistry, and our knowledge of human beings must therefore remain grounded in praxeology, not natural science (Mises 1978, pp. 28-34).<sup>19</sup> There are two different ways to do science; economics is not, can not be, and should not try to be, physics.<sup>20</sup>

Finally there is the issue of Mises’s version of *a priorism* and in particular its relation to the empirical testing of economic theories. For Mises, economics is not subject to empirical tests; the fundamental presuppositions of praxeology are *a priori* true, and, therefore, assuming the deduction is done correctly, the conclusions of deductive arguments based on those premises are true as well. There

really isn't any room (or reason) for "empirical testing" of substantive economic theory. In fact the entire notion of testing involves a basic inconsistency (or misunderstanding) of the category of human action. As Bruce Caldwell explains.

The fundamental postulate of human action is that all action is rational. Praxeologists assert that this postulate is known to be true with apodictic certainty; that is, it is a priori true. Mises argues that since attacks on the postulate require purposeful human action, attempts to refute it necessarily involve inconsistency. (Caldwell 1984b, p. 364)

Of course, like many of those in the Millian tradition, Mises would certainly agree that empirical evidence can be useful in deciding about the applicability or relevance of a certain result for a particular problem or in a specific context, but these are questions about history not about economic theory. Again Walter Block:

Clearly, for the Austrians, economic *theory* is completely devoid of any empirical role, while it is necessary, although not sufficient, for an understanding of economic *history*. Experience is also vitally important in determining the *applicability* of apodictically certain economic theory. . . . note how different here is the employment of the term "empirical" from its ordinary use in economics. The Austrians use it to denote the applicability of a *prioristic* economic law to reality... ; on the part of establishment economists, empirical work is done in order to "test" the truth of economic hypotheses. (Block 1980, pp. 419-20)<sup>21</sup>

Needless to say, this contemplated lack of concern over empirical testing of fundamental economic theory will become a significant bone of contention in the later methodological literature. All of the non-Austrian authors discussed in the rest of this chapter will use the issue of empirical testing as their main point of attack as well as a conduit for the presentation of their own methodological views.

The Nobel laureate Friedrich Hayek (1899-1992) was a fourth-generation Austrian working in the Mengerian tradition, and although his methodological views certainly overlap with those of Mises (his friend and teacher), there are also substantial differences. Hayek is clearly a methodological individualist, but he substantially softens both the dualism and the *a priorism* of Mises.<sup>22</sup> This softening occurs in a number of different ways.

One of Hayek's most important moves is to distinguish "scientism" from "science" and direct his attack against the former, not the latter.

According to Hayek, scientism "involves a mechanical and uncritical application of habits of thought to fields different from those in which they have been formed" (Hayek 1979, p. 24) and this uncritical application is the problem, not science (or even the philosophy of science): "It need scarcely be emphasized that nothing we shall have to say is aimed against the methods of Science in their proper sphere or is intended to throw the slightest doubt on their value" (Hayek 1979, p. 23). Hayek seems to be much more sensitive to the fact that he is living, writing, and attempting to persuade readers, in the age of science; although Mises is never explicit about it, one gets the feeling that he could just as well do without science entirely (or at least without the whole scientific form of life). In many ways, Mises is a nineteenth-century humanist, idealist-inspired, philosopher. Hayek, although sharing many of Mises's views on politics and economics, seems much more (earnestly or rhetorically) resigned to empirical science as the hegemonic form of intellectual life; meaning has clearly left the stage; the task is to

salvage as many of its best features as possible, and that task may be best accomplished by conciliation with the powers that be.

For Hayek, the aim of a social science such as economics “is to explain the unintended or undesigned results of the actions of many men” (Hayek 1979, p. 41). Such social science must start with human action, the subjective goal-directed action of individual agents, but it is much more. Social science must study the coordination of those individual actions into social phenomena and structures that were not the goal of any individual agent: “To grasp how the independent action of many men can produce coherent wholes, persistent structures of relationships which serve important human purposes without having been designed for that end” (Hayek 1979, p. 141). Hayek calls this approach the “compositive” method, and attributes it originally to Menger (Hayek 1979, pp. 65\_6).<sup>23</sup> An example of the compositive method might be Menger’s discussion of money in Chapter 8 of his *Principles* (1976); establishing money, a means of exchange, is not the purpose of any individual’s action, and yet money emerges as an unintended consequence of that individually self-interested behavior. Those who embrace scientism not only do not practice the compositive method, it has become a “constant source of irritation of the scientistically minded” (Hayek 1979, p. 146). The scientistically minded view institutions as conscious consequences (not unintended consequences) of human design; as it is generally not, at least postmonarchy, the design of a single individual, it must be the result of a conscious group mind. The result is a “collectivist prejudice inherent in the scientific approach” (Hayek 1979, p. 65); this methodological collectivism (Hayek 1979, p. 93) is closely related to various types of political and economic collectivism, which in turn leads to economic planning, social engineering, and Stalin’s “engineers of the soul” (Hayek 1979, p. 166).

In his later methodological work, particularly (1967a) and (1967b), Hayek emphasizes that while economics is capable of making certain types of empirical predictions, the complex nature of economic phenomena prevents economists from making anything more than generic, or what Hayek calls “pattern” predictions. These pattern predictions are associated with a particular type of scientific explanation: “explanations of the principle.” The complexity of economic phenomena, for example, prevents economists from predicting what any particular consumer will buy, but it is possible to predict the general pattern of an individual’s consumption and how it is likely to change in response to taxes or subsidies. What an economist is explaining in such theoretical exercises is the general principle at work behind the scenes of the observed pattern of economic behavior. In Hayek’s own words:

Though we may never know as much about certain complex phenomena as we can know about simple phenomena, we may partly pierce the boundary by deliberately cultivating a technique which aims at more limited objectives — the explanation not of individual events but merely of the appearance of certain patterns or orders. Whether we call these mere explanations of the principle or mere pattern predictions or higher-level theories does not matter. Once we explicitly recognize that the understanding of the general mechanism which produces patterns of a certain kind is not merely a tool for specific predictions but important in its own right, and that it may provide important guides to action (or sometimes indications of the desirability of no action), we may indeed find that this limited knowledge is most valuable. (Hayek 1967b, p. 40)

Again, this is certainly an Austrian argument, but is not as radically *a priorist* as the Misesian version of the Austrian method. Unfortunately, Hayek and his methodological followers often do get caught in what seems to be a rather debilitating crossfire. Critics outside the Austrian school often ignore such moderate views and characterize Austrian methodology solely in terms of Mises’s most radical statements; by contrast, many of those sympathetic to Austrian economics seem to view Hayek’s

methodological moderation as a potentially dangerous slippery slope (with Walrasian or Keynesian economics waiting at the bottom). The result is that Hayek's Austrian methodology, an Austrian view that is more moderate and in many respects philosophically rather contemporary, gets much less attention than Mises's praxeology.

## 2.2 Variations on Positivist Themes

Positivist philosophy of science will not be discussed in detail until the next chapter, but this section continues the theme of examining the "greatest hits" of economic methodology by discussing the methodological writings of three influential economists — Terence Hutchison, Milton Friedman, and Paul Samuelson — who were all, in one way or another, influenced by positivist ideas. These economists clearly represent the "big three" of twentieth-century methodological writing (at least prior to the explosion of literature during the last few decades), and for those of us who are middle-aged American-educated professional economists, they (particularly Friedman and Samuelson) represent the sum total of what we learned about "economic methodology" in graduate school. This section will examine the methodological writings of these three economists as relatively free-standing arguments about the proper way to conduct the science of economics — the methodological rules — without any serious consideration of the underlying positivist philosophy. While this may appear to be an unusual approach — discussing the application of positivist ideas before discussing positivism — it actually works quite well in the case of these authors. Although all three were broadly influenced by positivist ideas, none of them actually employed the positivist philosophical language or literature in a very precise or systematic way. Hutchison's work is by far the most philosophically astute, and yet even he fuses logical positivism and elements of Karl Popper's philosophy in a way that makes his position (particularly the early work discussed in this section) more of a free-standing economic methodology than a particular "application" of either logical positivism or Karl Popper's philosophy. Friedman's methodological writings are basically aphoristic, written by a practicing economist for practicing economists, with minimal donnish ornamentation; and, although Samuelson does endorse "operationalism," a particular version of the positivist tradition, he constructs his own specific version of the operationalist approach. So, yes, the discussion of positivism can safely be deferred until the next chapter.

### 2.2.1 Hutchison on the Significance of the Basic Postulates

Terence Hutchison was only twenty-six years old when *The Significance and Basic Postulates of Economic Theory* (1938) appeared in print.<sup>24</sup> While the book was many things — including the economic profession's first systematic introduction to the philosophical ideas of Karl Popper and Logical Positivism — it was most poignantly an attack on the *a priorist* praxeology of Ludwig von Mises. As Hutchison put it years later in the preface to the 1960 edition, his critique was originally aimed at "the dogmatic and extreme *a priorism* of Professor Mises, which was much more influential in the thirties" (1960, p. xxi). Over the years, Hutchison's exemplar for methodological malpractice shifted a bit toward (or at least to include) Marx and Marxian economics, but in 1938 the target was clearly Mises.

Hutchison was aggressively committed to the position that economics should be (and praxeology was not) a *Science* in the image of the natural sciences. Economics should be above the political and ideological fray: a science clearly differentiated from metaphysical speculation and whose propositions were systematically disciplined by objective empirical facts.

If there is any object in pursuing an activity one calls "scientific," and if the word "science" is not simply to be a comprehensive cloak for quackery, prejudice, and propaganda, then there must be

a definite objective criterion for distinguishing propositions which may be material for science from those that are not, and there must be some effective barrier for excluding expressions of ethical or political passion, poetic emotion or metaphysical speculation from being mixed in with so-called “science.” (Hutchison 1960, p. 10)

Gone from Hutchison’s view of scientific inquiry is the “moral science” of Mill and the “normative science” of Keynes; gone is the plurality of disparate scientific endeavors each with its own discipline-specific characteristics. For Hutchison, only one unique and narrowly defined type of intellectual activity should be allowed to sit at the captain’s table of science, and an “effective barrier for excluding” all others should be strictly enforced. He drew a demarcational line in the sand; on one side was a relatively homogeneous set of activities that had earned the right to be designated “Science” and on the other side was basically everything else: metaphysics, religion, ideology, ethics, poetics, praxeology, and all the other intellectual activities that, however interesting and passion-inspiring they might be, remain epistemically trifling.

Hutchison’s criterion for demarcating the scientific and empirically meaningful from the non-scientific and meaningless resides in the *empirical testability (potential falsifiability)* of the proposition in question.

We suggest that the economic scientist is transgressing the frontiers of his subject whenever he resorts to, or advances as possessing some empirical content, propositions which, whatever emotional associations they may arouse, can never conceivably be brought to any intersubjective empirical test, and of which one can never conceivably say that they are confirmed or falsified, or which cannot be deduced from propositions of which that can conceivably be said. (Hutchison 1960, p. 10)

If the proposition is subject to “intersubjective empirical test” — if it is subject to potential refutation by the empirical evidence — then it is “scientific”; if not, then it is not. As Hutchison put it in his reply to Frank Knight (1940): “Scientific propositions in question must be testable. . . . The difference between the propositions about snakes of the scientific zoologist and those of the sufferer from delirium tremens is just that” (Hutchison 1941, p. 738).

As the discussion in the next chapter will make clear, Hutchison’s demarcation criterion seems to amalgamate at least three different ways that philosophers have tried to differentiate the scientific or cognitively meaningful from that which is non-science or cognitively meaningless: the logical positivist criterion of cognitive meaningfulness, the logical empiricist criterion of empirical testability, and the falsificationist demarcation criterion of Karl Popper. In later work, Hutchison became more attuned to the subtle distinctions between these three criteria — and sided with Popperian falsificationism — but in 1938 he was not concerned with such philosophical nuances (nor, frankly, were the relevant philosophers yet clear about the distinctions themselves). In *Significance*, Hutchison was making a simple, if rather doctrinaire, point; economics should be a *Science* and science involves propositions that can be empirically tested. Theorizing based exclusively on propositions that are not subject to empirical test, such as the “synthetic *a priori* true” propositions of Misesian praxeology, is simply not science and has no place in scientific economics. As Hutchison restated the argument fifty years after the publication of *Significance* (adding the Marxists to the *a priorist* roll),<sup>25</sup> the argument is simultaneously epistemological and political.

A priorism rejects fundamentally the falsifiability principle (FP) and all empirical testing. . . . Long supported in economics by Misesians . . . a priorism has now found support among Marxians . . .



Misesians and Marxians presumably claim authority, and reject all testing and falsifiability, for quite different, perhaps flatly contradictory, fundamental axioms. . . . The political implications are alarmingly hostile to freedom of economists, or of any group or authority, claiming infallibility, or “apodictic certainty,” for selected axioms, and conclusions deduced from them that are claimed to possess significant economic content, but for which testing, and falsifiability are comprehensively rejected. The FP, on the other hand, is a truly libertarian principle because, in demanding testing and falsifiability, it is based on human fallibility and denies the infallibility claimed by the a priorists, Misesian, and Marxian. (Hutchison 1988, p. 176, note 3)

Although empirical testability was necessary for scientific economics, Hutchison realized that economic science would also contain non-empirical propositions; in fact, he insisted that “pure theory” was entirely deductive and not empirical at all. According to Hutchison, pure theory simply involved the (deductive) drawing out of the implications of various analytical presuppositions. Quoting the positivist philosopher Moritz Schlick, Hutchison called such exercises “a game with symbols” (Hutchison 1960, p. 33). It is a game that is quite useful because it allows us to ferret out the various implications of our analytical definitions, but since they are “neither confirmable nor contradictable by an empirical synthetic proposition, propositions of pure theory cannot tell us anything new in the sense of telling us new facts about the world” (Hutchison 1960, p. 34). Hutchison claimed — a claim harshly criticized in the later literature — that such propositions were necessarily “tautological” (i.e., true by the definitions of the terms).<sup>26</sup> Whether or not “tautological” is the proper term, it is clear that Hutchison saw a role for pure theory, but it is also clear that he viewed pure theory as merely a useful accouterment to the main project of empirical economic science.

Although Hutchison admitted the usefulness of (nonempirical) pure theory, he did not consider the main “laws” of economics to be of such analytical character. The laws of economics were testable empirical propositions. The primary law of economic motivation — the assumption of rational economic man — was not simply an *a priori* proposition; it was a testable empirical proposition about human behavior.

It is . . . an empirical generalisation capable of being tested empirically and of being falsified, possessing therefore *some* empirical content, however insignificant this may be. It is not simply an empirically empty definition, which is what is sometimes offered as a “Fundamental principle” of economic conduct. (Hutchison 1960, p. 114, emphasis in original)

Hutchison not only considered general principles like the rationality assumption to be testable, he also considered more specific restrictions such as the law of diminishing marginal utility (Gossen’s law) to be testable as well. According to Hutchison, the problem is not with such “laws” but with the way that economists have traditionally thought about them.

If one conceives of Gossen’s Law as an empirical generalisation one can, when one wants to, go to the facts of economic behaviour to test it. On the other hand, simply to rely on dogmatic assertion even when supported by phrases like “inner feelings of necessity” and “*a priori* facts,” is to commit scientific suicide. (Hutchison 1960, p. 135)

The bottom line for Hutchison (at least in *Significance*) seemed to be that there was not really anything much wrong with the practice of economics along the lines of Cairnes’s “hypothetical” method or Keynes’s positive economics; the problem was mostly in how economists thought (and defended) the propositions of economic theory. Hutchison, of course, thought that some economics (Mises, Marx, etc.) was clearly bad science, but for the most part the mainstream economic tradition from Mill through

Marshall seemed to be defensible on the basis of Hutchison's scientific demarcation criterion. This attitude changed in Hutchison's later work (see 1992b, in particular). Here, Marx and Mises remain villains, but now the "formalist-abstractionist" mathematical theorists that dominated Anglo-American economics in the latter half of the twentieth century also become the subject of rebuke. Economics is no longer about policy or "the real world" but a game to be played for the respect of (only) other professional economists. According to Hutchison, the result is an "abstractionist-mathematical blight" (Hutchison 1992b, p. 102) that has divorced economics from both social engagement and the rules of proper scientific method. In recent work, Hutchison has even blamed certain economic methodologists for many of these troubling developments. Evidently those writing on economic methodology during the 1960s and 1970s were influenced by the "ultra permissive attitude" of the "flower children" (Hutchison 1992b, p. 48) and were thus unable (or unwilling) to keep the economics profession's feet to the scientific fire. But, of course, consideration of such accusations would carry us way ahead in our story. For now, let us "drop out" of such recent debates and return to the second of the three main characters in mid-twentieth-century economic methodology: Milton Friedman.

### ***2.2.2 Friedman on the Methodology of Positive Economics***

Milton Friedman's essay on "The Methodology of Positive Economics" (1953) is clearly the best-known work in twentieth-century economic methodology. It was "a marketing masterpiece" (Caldwell 1982, p. 173) that is cited in almost every economics textbook and it remains, almost a half-century after its publication, "the only essay on methodology that a large number, perhaps majority, of economists have ever read" (Hausman 1992, p. 162).

Unlike Hutchison, Friedman was not writing so much in response to a debate about the philosophical foundations of economics but rather in response to certain contemporary debates regarding the theoretical and empirical practices of the economics profession. There were, of course, many such debates — recall this was a period of great change in economics, marked by the rise of Keynesian macroeconomics, Walrasian general equilibrium theory, mathematical economics, and econometrics, as well as by the decline of various indigenous American strains of economic theorizing — but I will limit my comments to three controversies that seemed to bear most directly on Friedman's methodological views.

First, and the issue that gets the most press in the methodological literature, was the debate over the appropriateness of "marginal analysis" in the study of labor markets and the theory of the firm. Richard Lester (1946) and others (Hall and Hitch 1939) had made the case (in part based on survey data from business managers) that firms do not actually maximize expected returns as assumed in the standard marginalist

#### *54 Reflection without Rules*

framework.<sup>27</sup> Second, and related to the marginalist controversy, was the "imperfect competition revolution" — initiated by Chamberlin (1933) and Robinson (1933) — which offered a major challenge to the assumption of perfectly competitive markets that had dominated economic analysis since the time of Adam Smith. Third, and most relevant in light of later developments in economic theory, was the so-called measurement without theory debate between representatives of the Cowles Commission (Koopmans 1947 and 1949) and the Chicago school of economics (Vining 1949a and 1949b).<sup>28</sup> This debate was ostensibly about the proper role of "theory" and "empirical observation" in the analysis of business cycles (Burns and Mitchell 1946), but actually reflected a much deeper schism

between the members of the Chicago economics department (including Friedman) and the members of the Cowles Commission who were physically (but not intellectually, methodologically, or politically) housed at the University of Chicago from 1939 to 1955.<sup>29</sup> The stable equilibrium that Friedman sought to negotiate among, and in response to, these (and other) disruptive forces was a type of Marshallian, partial equilibrium, small-number-of-equations, micro and monetary economics that would steer a theoretical middle ground between the abstract Walrasian theorizing of Cowles on one hand, and the more-broadly social theorizing of certain Institutionalists on the other. This equilibrium also needed to sustain the use of the available empirical evidence and statistical techniques without being forced into the Procrustean bed of Cowlesian structural equation econometrics; allow for the use of certain Keynesian-based theoretical constructs (like the IS-LM model) without buying into Keynesian-interventionist policy or political philosophy; and preserve both the two-hundred-year-old framework of economic analysis based on competitive markets, and the neoclassical assumption of rational maximizing agents. Keeping all of these balls in the air at the same time was not an easy job.

Friedman's main argument in "The Methodology of Positive Economics" was that for the purposes of positive (as opposed to normative) economics, *the truth of the assumptions of a theory do not matter all*. The only thing that matters in deciding among various economic theories is which one is most successful in making empirical *predictions*. The theory that makes the most accurate predictions in the relevant domain is the best theory, and if it employs "unrealistic" assumptions this should not in any way detract from its success as a positive scientific theory.

Viewed as a body of substantive hypotheses, theory is to be judged by its predictive power for the class of phenomena which it is intended to "explain." Only factual evidence can show whether it is "right" or "wrong" or, better, tentatively "accepted" as valid, or "rejected." . . . the only relevant test of the *validity* of a hypothesis is comparison of its predictions with experience. (Friedman 1953, pp. 8-9, emphasis in original)

While only predictions matter, Friedman does argue that some predictions are more important than others. Predicting a *novel fact* — evidence not yet observed — is the key determinant of a successful economic theory. In Friedman's own words, the "ultimate goal of a positive science is the development of a 'theory' or 'hypothesis' that yields valid and meaningful . . . predictions about phenomena not yet observed" (Friedman 1953, p. 7). Now, since economics often predicts things that happened in the past, whether that past is nineteenth-century economic history or this morning's stock market, Friedman also makes it clear that "novel" does not necessarily mean "in the future," but rather "unknown" to the economist proposing the theory in question: "they may be about phenomena that have occurred but observations on which have not yet been made or are not known to the person making the prediction" (Friedman 1953, p. 9). It is useful to note that Friedman has consistently maintained the importance of novel facts throughout his career — from his critique of Lange in 1946 ("the ability to deduce facts that have not yet been observed," p. 631) to the Friedman and Schwartz response to Hendry and Ericsson in 1991 ("any hypothesis must be tested with data or nonquantitative evidence other than that used in deriving the regression or available when the regression was derived," p. 49) — it is certainly not an argument that just appeared in the 1953 methodological essay.<sup>30</sup>

Of course, if prediction is all that matters, novel or otherwise, then the "realism" of the assumptions are entirely irrelevant to the importance of an economic theory.<sup>31</sup> To use two of Friedman's own examples, objects fall *as if* they were falling in a vacuum and the leaves on a tree arrange themselves *as if* they were trying to maximize the sunlight they receive; these assumptions — the presence of a vacuum and leaves acting rationally — are highly unrealistic, and yet scientific theories

based on such unrealistic assumptions yield highly reliable (and often novel) empirical predictions. According to Friedman, “the relevant question to ask about the ‘assumptions’ of a theory is not whether they are descriptively ‘realistic,’ for they never are, but whether they are sufficiently good approximations for the purpose in hand” (1953, p. 15), and “in general, the more significant the theory, the more unrealistic the assumptions” (1953, p. 14). Such arguments about the irrelevance of unreal assumptions led Paul Samuelson to characterize Friedman’s methodological position as the *F-twist* (a label that has stuck in the literature): “A theory is vindicable if (some of) its consequences are empirically valid to a useful degree of approximation; the (empirical) unrealism of the theory ‘itself,’ or of its ‘assumptions,’ is quite irrelevant to its validity and worth” (Samuelson 1963, p. 232).

Friedman’s position on the importance of prediction and the irrelevance of unrealistic assumptions both have important implications for the theoretical debates in which he, and the economics profession more generally, was embroiled at the time and for the next few decades. The sole criteria of predictive accuracy bore directly on his debates with Cowles and other Keynesians, since their many-equation big econometric macro-models didn’t seem to perform predictively any better than the small, often single-equation, models of Friedman and other monetarists. The irrelevance of unreal assumptions had an obvious impact on the “marginalist controversy” and debates about the appropriateness of the assumption of perfect competition. If models assuming profit maximization and perfect competition were more predictively successful than the available alternatives (which Friedman certainly assumed), then the purported unrealism of their assumptions was entirely irrelevant to their scientific usefulness; and, perhaps even more important, one could just drop the entire irrelevant debate about whether such assumptions were unrealistic or not and get on with actually doing economics (i.e., making economic predictions). Friedman made these implications quite clear in his original essay.

The abstract methodological issues we have been discussing have a direct bearing on the perennial criticism of “orthodox” economic theory as “unrealistic” as well as the attempts that have been made to reformulate theory to meet this charge. . . . As we have seen, criticism of this type is largely beside the point unless supplemented by evidence that a hypothesis differing in one or another of these respects from the theory being criticized yields better predictions for as wide a range of phenomena. (Friedman 1953, pp. 30-1)

This message — essentially “don’t criticize until you have a theory that predicts better” — seems to have been greeted with a sense of *liberation* by the economics profession. Economists “could now get on with the job of exploring and applying their models without bothering with objections to the realism of their assumptions” (Hausman 1992, p. 164, note 18).

Friedman’s essay has generated a massive critical and interpretative literature.<sup>32</sup> The first round of these debates was dubbed the “assumptions controversy” and contributions to it have proceeded relatively unimpeded since its beginnings in the mid-1950s until the current time. There also have been other subdebates that have emerged along the way (some of these will be discussed in Chapters 6 and 7) and Friedman’s position has consistently served as a foil for, or as the backdrop to, authors presenting other methodological views. Although it has died down in recent years, there were a few decades where almost everything written about economic methodology seemed to start with Friedman’s essay. Given the extent of the debate, I will not attempt to summarize the literature on the assumptions controversy; instead, I will just pick two authors — Musgrave (1981) and Hausman (1992) — that have made particularly influential remarks regarding Friedman’s essay.<sup>33</sup>

Alan Musgrave's (1981) rather simple, but very important point, is that not all assumptions play the same role in economic (or for that matter any scientific) theory. Friedman just talks about "assumptions" without specifying exactly what type of assumptions he is talking about. Musgrave simply argues that for certain types of assumptions, Friedman is right — they don't matter — but for other types of assumptions, they do. He divides the "assumptions" in economics into three main types: negligibility, domain, and heuristic. Musgrave discusses each of these types, but also notes that his threefold classification does not exhaust all of the various types of assumptions that appear in Friedman's paper.

Negligibility assumptions simply specify that some factor  $x$  is negligible; in other words things act as if  $x$  were the case. The way to think about negligibility assumptions is not that such factors are absent, but rather that they are "irrelevant for the phenomena to be explained" (Musgrave 1981, p. 380). Musgrave gives the example of a "no government sector" assumption in a macro-model, but perhaps a better example would be the assumption of perfect competition in the analysis of short-run (qualitative) comparative statics. An increase in demand will increase the price of the good whether the firm is competitive or a monopoly; the assumption that the market is competitive is irrelevant for this particular phenomena. Musgrave argues that Friedman is basically correct about negligibility assumptions — some of the things that Friedman says about them are not exactly right — but Friedman is correct that the realism of such assumptions is irrelevant to the validity or usefulness of an economic theory.

Musgrave's second type of assumption is a domain assumption; it specifies that a theory works (perhaps only works) in some particular domain. To pursue the macro example; a domain assumption that there is "no government sector" would say that the theory works (perhaps only works) in an economy without a government sector. Musgrave argues, contra Friedman, that such assumptions do matter. In particular, if one converts a falsified negligibility assumption into a domain assumption, one decreases the testability of the theory.

Finally, heuristic assumptions are assumptions that are initially assumed to be negligible, but eventually, at a later stage, will be weakened to see if they have any impact. Continuing with the example of "no governmental sector"; as a heuristic assumption, it would say "let's assume for the moment that there is no government sector, but later we will relax the assumption and see if it has an impact on the results." Heuristic assumptions, according to Musgrave, are extremely important in a scientific theory such as economics where the "logico-mathematical machinery is so complicated that a *method of successive approximation* has to be used" (Musgrave 1981, p. 383, emphasis in original). Because of the tentative nature of such assumptions, they are involved more in the process of theory refinement than in empirical prediction.

Musgrave concludes his analysis of Friedman's essay with the following summary of his position.

I have claimed that the so-called "assumptions" of economic theories (and of other scientific theories) play at least three different roles within those theories, and are assertions of (at least) three different types. I have argued that Friedman overlooked these distinctions, and was led thereby to the mistaken thesis that "the more significant the theory, the more unrealistic the assumptions" (Musgrave 1981, p. 385)<sup>34</sup>

Daniel Hausman has been a prolific contributor to the recent methodological literature (his work will be examined in more detail in Chapter 7) and has made critical remarks about Friedman's

methodology in a number of different contexts. The criticism that I will discuss in this section is the criticism he raises in Chapter 9 of *The Inexact and Separate Science of Economics* (1992). Here, Hausman makes the argument that Friedman's claims about the realism of assumptions do not stand up *even if* one accepts empirical prediction as the sole criterion for scientific success: Hausman's criticism should (for reasons that will be obvious in a moment) be called the "used car argument." He begins by summarizing Friedman's argument in the following way:

1. A good hypothesis provides valid and meaningful predictions concerning the class of phenomena it is intended to explain (premise).
2. The only test of whether an hypothesis is a good hypothesis is whether it provides valid and meaningful predictions concerning the class of phenomena it is intended to explain (invalidly from 1).
3. Any other facts about an hypothesis, including whether its assumptions are realistic, are irrelevant to its scientific assessment (trivially from 2). (Hausman 1992, p. 166)

The main problem with the argument is that it is not a valid "argument" at all: Statement 2 is not true and it does not follow from statement 1. Hausman uses the following analogous argument to make his point:

- 1' A good used car drives reliably (over-simplified premise).
- 2' The only test of whether a used car is a good used car is whether it drives reliably (invalidly from 1').
- 3'. Anything one discovers by opening the hood and checking the separate components of a used car is irrelevant to its assessment (trivially from 2'). (Hausman 1992, p. 166)

The problem is of course that with a used car or an economic model the relevant issue is how well it will perform in the future and in other circumstances. Theory should be a guide — even if we focus on empirical prediction — to new circumstances and new situations, and for those forward-looking applications examining the parts (the assumptions) matter. In fact, though Hausman does not make this point, Friedman's emphasis on novel facts gives away his commitment to successful future performance, but Friedman never closes the circle. Friedman seems to be making the implicit assumption that success in one novel situation improves the probability of success in additional and/or future novel situations that we might have an interest in, but there is no obvious reason for this to be the case. Such issues actually carry the discussion beyond Friedman's essay and into debates about "realism" and "instrumentalism" in the philosophy of science: a discussion that must wait until the next chapter. At this point I just want to note that Hausman's criticism of Friedman seems to be correct — even if one is only interested in prediction, the assumptions still matter.

### **2.2.3 Samuelson and Operationalism in Economics**

Paul Samuelson had a profound impact on the shape and structure of postwar economics. Not only was he an economist with arresting technical abilities, he was also the second individual (and first American) to receive the Nobel Prize in economic science, and, he was also, more than any other individual, responsible for the structure and content of economics education in postwar America. During the 1950s and 1960s, the teaching of college-level economics in the United States stabilized around two key texts: Samuelson's *Economics* (1948a) at the undergraduate-introductory level and Samuelson's *Foundations of Economic Analysis* (1947) at the graduate level. Although these two books were

ultimately replaced in their respective markets by more user- friendly spin-offs from other authors, they nonetheless effectively defined (and to a lesser extent continue to define) the teaching of “modern scientific” economics in both form and content. In terms of pedagogical form *Economics* gave us the framework for the two-part, micro and macro, introductory sequence familiar to many (even noneconomist) readers from their own undergraduate education, whereas *Foundations* sent the clear signal that students should not even think about graduate work in economics until they have jumped through the appropriate mathematical hoops (demonstrating competency in at least multivariate calculus, real analysis, and linear algebra). With respect to theoretical content, both texts affirmed the “neoclassical synthesis” of Walrasian microeconomics and Keynesian macroeconomics; at the introductory level, the micro was a bit more Marshallian with its focus on single markets and firms, but even there the tone was firmly Walrasian.

Samuelson clearly demonstrated technical brilliance in economic theory and he certainly had an important impact on the teaching of college-level economics, but even these two factors together are not sufficient to account for his wide-ranging influence on economics and the image of the economics profession. Another contributing factor was undoubtedly Samuelson’s reputation as “Mr. Science” (Pearce and Hoover 1995, p. 184); it was actually “Samuelson, and not Friedman, who by both word and deed was responsible for the twentieth century self- image of the neoclassical economist as ‘scientist’ “ (Mirowski 1989c, p. 182). Samuelson offered the economics profession, and those in government and business associated with the profession, an image of scientific economics that was above the political fray, neither extreme right nor extreme left (neither Mises nor Marx), but an objective disinterested instrument of scientific analysis that could be used to reconcile and harmonize the various conflicting interests in postwar economic life. As Pearce and Hoover put it in a recent study of Samuelson’s introductory text:

His *Economics* is above all a harmonist book. The core model continues in its sanctified role as the Prince of Peace among competing economic doctrines. The foundations of the peaceable kingdom are, above all, in *scientific* economics. . . . Science, for Samuelson, is not just a matter of naive realism; it also relies on a neutral and generally applicable analytical framework. (Pearce and Hoover 1995, p. 198, emphasis in original)

While these motivations seem similar to the motivations of Hutchison and J. N. Keynes discussed above, in Samuelson’s case (and in the post- Hiroshima era) they manifest themselves in a fundamentally different set of methodological recommendations.

Samuelson’s stated economic methodology is *operationalist* and *descriptivist*, and although both of these philosophical positions will be examined in more detail in the next chapter, Samuelson was fairly clear what he meant by both terms. Consider operationalism first.

Although operationalist ideas go back at least to the nineteenth century, operationalism was firmly established as a reputable philosophical position by the publication of Percy Bridgman’s *The Logic of Modern Physics* in 1927.<sup>35</sup> Bridgman was a practicing physicist (Nobel Laureate in 1946) who wrote widely on operationalist philosophical ideas and their implications for contemporary physical theory. The first reference to Bridgman’s operationalism in economics seems to have been in Henry Schultz’s *Theory and Measurement of Demand* (1938), but, since operationalist ideas were widely discussed during the 1930s and 1940s (in psychology as well as philosophy and physics), it is not clear whether Samuelson picked up these ideas from Schultz during his undergraduate years at Chicago, or from elsewhere on the intellectual landscape.<sup>36</sup>

The core operationalist idea is that a question has *meaning* only if there exist a set of operations that will provide a definitive answer to it. Correspondingly, a concept or term is *operationally meaningful* if it can be characterized by a particular set of operations, and the meaning of a concept or term is *defined by* that set of operations. Bridgman himself used the concept of “length” as an example.

What do we mean by the length of an object? We evidently know what we mean by length if we can tell what the length of any and every object is, and for the physicist nothing more is required. To find the length of an object, we have to perform certain physical operations. The concept of length is therefore fixed when the operations by which length is measured are fixed: that is, the concept of length involves as much as and nothing more than the set of operations by which length is determined. In general, we mean by any concept nothing more than a set of operations; *the concept is synonymous with the corresponding set of operations.* (Bridgman 1927, p. 5, emphasis in original)

Samuelson’s *Foundations* was based on his 1941 doctoral dissertation, which carried the subtitle “The Operational Significance of Economic Theory,” and from the very first page of the book he makes it clear that he is exclusively concerned with (and also that he thinks that not enough previous economists have been concerned with) “the derivation of *operationally meaningful* theorems” (Samuelson 1947, p. 3, emphasis in original). For Samuelson, a theorem is operational if it can be empirically tested; a meaningful theorem is “simply a hypothesis about empirical...”

-----

13 See, for example, Cartwright (1994b), Hutchison (1973), Kauder (1957), Klant (1984, pp. 66-71) Clive Lawson (1996), Maki (1990a, 1990b, 1992c, 1997), Mirowski (1988, pp. 22-5; 1989a, pp. 260-2), Oakley (1997), and Smith (1990).

14 Lionel Robbins seems to be a good example of this tension; it is never entirely clear (particularly in the 2nd edition of his Essay) which side of this philosophical fence he is on, and this foundational bipolarity seems to open the door to a number of different criticisms.

15 There is surprisingly little English language literature on the Methodenstreit, given that it lurked in the background of most late nineteenth- and early twentieth-century methodological writing. As we saw in the previous discussion of Keynes and Robbins, a common approach was to use it as a kind of ominous threat; “Listen to my, more moderate, methodology, so we do not fall into extreme (and unproductive) views like those.” Certain later Austrians even seemed to take such a stance (see Böhm-Bawerk 1890, for example). Some of the more contemporary literature on the Methodenstreit includes Barkai (1996), Bostaph (1978), Fri8by (1976), Hutchison (1973), and Mäki (1997).

16 Menger is considered to be a first-generation Austrian; his younger colleagues, such as Wieser and Böhm-Bawerk, constituted the second generation, making Mises a member of the third generation.

17 See Barrotta (1996) and Parsons (1997b) for a recent exchange concerning the connection between Mises and Kant.

18 The claim that our understanding of the actions of others comes from sharing a common interpretive framework opens the door to Verstehen or hermeneutic approaches to the social sciences: approaches often considered to be the polar opposite of an economic approach to human behavior.



The economist qua acting individual “understands” intent by virtue of personally engaging in purposeful action. A consequence of this *Verstehen*, or “interpretive understanding,” is that one imputes meaning to the action or object on the basis of analogy with one’s own pattern of purposeful action. (Greenfield and Salerno 1983, p. 49)

This has led to a fairly extensive literature on the relationship between economics, particularly Austrian economics, and the triad of *Verstehen*, hermeneutics, and interpretation. See Bacharach (1989), Gordon (1991, Ch. 14), Greenfield and Salerno (1983), Hayek (1973), Klant (1984, pp. 76-82), Lavoie (1990, 1991b), and Lewin (1996) for a wide range of different views on the subject. See Winch (1990) for a classic statement of the interpretive view of social science and Rosenberg (1995a) for a recent survey of the subject.

19 Mises anticipates, and critiques, the “eliminative materialist” claims discussed below (at the end of Chapter 4).

20 According to Mises, attempts to try to do economics like physics lead to undesirable political consequences. The desire to make the social sciences universal — a tendency that Mises rightly identifies with logical positivism (see Chapter 3) — stems, he argues, from a “dictatorial complex” to “see themselves in the role of the dictator — the duce, the Führer, the production tsar — in whose hands all other specimens of mankind are mere pawns” (Mises 1978, pp. 40-1).

21 It is useful to note that Mises’s attitude about empirical testing seems to be much easier to defend now that problems like theory-ladenness and underdetermination (discussed in detail in Chapter 3) are generally accepted within the philosophical literature (see Caldwell 1984b and Boettke 1998). Of course, this does not vindicate Mises’s position, but it does legitimize many of his criticisms of empiricism and positivism in ways that would have been inconceivable only a few decades ago.

22 There is some debate about when (or if) Hayek made the “transformation” from Mises’s methodological views. Bruce Caldwell (1988) has argued that there was a transformation that began around the time of Hayek’s “Economics and Knowledge” (1937), but the transformation had less to do with Mises than with Hayek’s growing discomfort with equilibrium analysis for dealing with important questions like the coordination of knowledge. See Caldwell (1992a, 1992b, 1998a) and Hutchison (1981, Ch. 7, 1992a).

23 As we will see (in Chapter 7), the philosopher Karl Popper also characterized social science as the study of the unintended consequences of individual rational action. Although it is clear that such ideas go back at least to Bernard Mandeville, Adam Ferguson, and Adam Smith, Hayek suggests that Popper got the idea of unintended consequences directly from him (Hayek 1967c, p. 100). See Caldwell (1991a, 1992a, 1992b, 1998a) and Hutchison (1981, Ch. 7, 1992a) for different views of the Popper-Hayek connection.

24 See Caldwell (1998b) and Coats (1983a) for a general discussion of Hutchison’s work and Coats (1983b) for a bibliography of his writings (prior to 1983).

25 Hollis and Nell (1975), in particular.

26 critical reviews of Hutchison (1938) — particularly Klappholz and Agassi (1959), Knight (1940), and Machlup (1955) — constitute some of the most important methodological literature of the middle of the twentieth century. Although many of the issues raised by these critics were unappreciated (or misunderstood) at the time, recent methodological debates have given us a new respect for many of the arguments raised in these papers.

27 Machlup (1946) and Stigler (1947) for other responses to this literature.

28 The papers associated with the “measurement without theory” debate are reprinted in Volume II of Caldwell (1993); also see Mirowski (1989b). Daniel Hammond’s (1993) interview with Friedman provides some insight into the factors that Friedman himself (at least with hindsight) felt were most important in the development of his methodology.

29 See Mirowski and Hands (1998) for a discussion of Friedman’s involvement in the disagreements between the Cowles Commission and the Chicago economics department during this period.

30 We will discover in Chapters 3 and 7 that novel facts are also important for other methodological approaches.

31 Maki (1989, 1992b, and elsewhere) has, I think correctly, argued that “realisticness” is a much better term for what Friedman is interested in than “realism,” but I will follow tradition and continue to use the term realism. See Section 7.3.2 below for more discussion of Maki’s argument.

32 The preponderance of this literature has been critical (Mayer 1993 and 1995 are exceptions). This creates a rather quizzical situation where many, perhaps even most, practicing economists endorse Friedman’s view (at least in a pro forma way), while almost all of the commentary written on the paper is quite critical. This reflects in part who has written on the subject of economic methodology in the latter half of the twentieth century, but there are undoubtedly other factors as well. At this juncture, I only want to point out how different this is from say, Mill.

33 Other key contributions to the assumptions controversy include Bear and Orr (1967), Klappholz and Agassi (1959), Koopmans (1957), Maki (1989, 1992b, 2000a), Melitz (1965), Nagel (1963), Rotwein (1959), Samuelson (1963), and Wong (1973). See Hausman (1992, p. 163, n. 17) or Redman (1991, p. 99, n. 4) for a more complete list.

34 Maki (2000a) presents a number of criticisms of Musgrave’s interpretation of the assumptions controversy.

35 Although it is clear that Bridgman disliked the term “operationalism” and felt that in some ways he had “created a Frankenstein” (Green 1992, p. 310).

36 Unlike most of the economists discussed in this chapter, it also is less clear what specific intellectual concerns motivated Samuelson’s methodological commitments. I personally suspect that it was a series of deeply disturbing run-ins with Frank Knight during Samuelson’s years at Chicago, but this is purely speculation on my part.