1

Sweet Science

ENGINEERING A NEW APPROACH TO ECONOMIC POLICY

Economic policy does not follow from economic theory. Instead, policy needs to be drawn from a complicated blend of judgments about ambiguous empirical evidence, normative judgements, and sensibilities that may be framed, but are not determined, by scientific theory.[1] Put another way, economic policy is a blend of engineering and judgment—an "art and craft," not a scientific endeavor that follows from economic theory.[2] Debates about policy are best treated as debates about the art and craft of economics, using a methodology appropriate for an art and craft. Policy debates should not be treated as debates about science, and should not be governed by a methodology more appropriate to science.

Unfortunately, modern economics doesn't treat policy in this way. Instead, it conceives of policy as an applied science, and uses the methodology of science to study policy issues. To some degree, that makes sense. Clearly, one wants evidenced-based, objective analysis of policy. An art and craft methodology uses theory and science whenever it can, meaning to the extent it is appropriate. But when dealing with the messy issues of policy, an art and craft methodology takes seriously the fact that statistical evidence needs interpretation and that one's views and analysis are inevitably influenced by one's normative judgments.[3] An art and craft methodology recognizes that policy is intrinsically entangled and must be dealt with using a methodology designed to guide in such ambiguous, messy, and uncertain situations. To pretend otherwise undermines both the science of economics and economic policy discussions.

Early on, economists struggled with this policy/science divide. Advocates of various policy positions all claimed to have science and theory on their side. They inevitably attacked their opponents for being non-scientific. This struggle led Classical Liberal economists to embrace a methodological tradition that interpreted the science of economics narrowly and created a firewall between scientific pursuits and policy endeavors. This tradition is best found in the policy methodology of Classical Liberals such as John Stuart Mill and his followers. That methodology recognized the messiness of policy compared to the elegance of the theory underlying science.

To deal with that messiness, the policy methodology needed a branch of economics that was free of scientific certainty. One way to handle that problem would be to accept that no part of economics was a science.[4] The second way—the path adopted by Classical Liberals—was to divide economics into different branches—a scientific branch concerned with agreed-upon empirical facts and logical implications of assumptions, in which normative values played as minimal a role as possible, and a policy branch in which values were seen as essential elements of the analysis. The policy branch of economics would use a different methodology than the scientific branch, and its conclusions would not be considered scientific conclusions.

Since our goal in this book is to talk about the methodology appropriate for applied policy, we do not distinguish between the "economics is not a science" and the "economic policy branch of economics is not a science" alternatives. The reason is that our interest is in applied policy, not the science of economics. We interpret the "no economic science" alternative, such as that proposed by some philosophers, for example, Hillary Putnam (2002) or Alexander Rosenberg (Rosenberg 1992), as being included in the Classical "separate branch" alternative. The "no economic science" alternative simply makes the further assumption that the science branch of economics is empty, an assumption we do not accept. But for our purposes these come to the same conclusion since, if there is no part of economics that is a science, then the applied policy branch of economics will not be guided by scientific methodology. If one believes, as Putnam and Rosenberg believe, that no part of economics is a science, it does not change our argument that applied policy should not be thought of as applied science—it strengthens it, since if there is no economic science, our argument—that applied policy economics should not be seen as applied science—becomes tautologically true.[5]

Most classical economists accepted that there was a scientific branch of economics.[6] But they also believed that policy did not follow from scientific theory, and they built that belief into their applied policy methodology. By doing so, Classical Liberalism sought to discourage the conflicting advocates of any policy issue from claiming the authority of scientific justification. Only a powerful firewall between theory and policy could accomplish that. For John Stuart Mill, policy was not based on science, and science did not concern policy. Instead, science was about a search for the truth. In pursuit of that truth, in order to see the scientific truth more clearly, one should ideally harbor no policy considerations whatsoever. Since that was practically impossible, one should attempt to guard, as much as possible, against a tendency to claim too much from science. Policy construction was meant to be about the search for answers to specific policy questions. That search required one to go far beyond the limits imposed by science. The objective was to integrate into the argument judgments that had no scientific basis, but that might have a philosophical basis. To keep the two separate, an economist needed to always lean over backward to confess that, in his or her role as an economic scientist, he or she had nothing to say about policy. That doesn't prevent him or her, when operating in a "statesman" role, from expressing views, and if he or she has expertise in that area, from offering them as the views of a specialist. But that expertise has to be broader than that of an economic scientist, and it must involve knowledge that goes far beyond science. Given the complexity of the economy it is an expertise that will emphasize its fallibility and view that often the best we can do in a complex world is to muddle through without definitive answers (Colander 2003).

The Abandonment of Classical Liberal Methodology

In the 1930s, the economics profession began to abandon the policy methodology of Classical Liberalism by removing the firewall between economic science and policy. This book is an exploration of that abandonment and a call for the profession to return to a more Classical Liberal methodology in policy matters. In our consideration of the abandonment of Classical Liberal methodology, we use the University of Chicago as a case study of this largely postwar phenomenon. We focus on Chicago, not because of the political inclinations or ideological leanings that characterized Chicago in this era, nor because Chicago was unique in abandoning this Classical firewall separating scientific theory from policy. Instead, we choose it because the stalwarts of the postwar Chicago School actually imagined themselves to be, and were seen by most economists as being, defenders of Classical Liberalism.[7] In our view, the Chicago School failed to defend what was important in Classical Liberalism, namely its art and craft policy methodology.

The Chicago School was intent instead on maintaining a narrow interpretation of a laissez-faire policy precept.[8] Chicago adopted a viewpoint which insisted that economic science effectively underpinned the conclusion that the market is capable of solving its own problems. Consequently, government policy interventions should be strongly discouraged. There were two problems with this. The first is that, in Classical Liberal thought, no policy precept followed directly from economic science. In this regard, the Chicago School failed to adhere to that Classical Liberal position. The second issue was the failure to recognize that the laissez-faire policy precept of

Classical Liberalism was far more ambiguous than its Chicago interpretation. The Classical laissez-faire understanding could be held by economists with a wide disparity of views of government policy, ranging from John Maynard Keynes's policy activism to Frederick von Hayek's pro-market policy. Moreover, because it was a policy precept, it could change over time, as the problems faced by society changed, as sensibilities changed, and as government structures in turn changed. It was not for economic scientists to settle this debate about policy since the issues debated were, to a large extent, non-economic.

What we are saying is that at the core of Classical Liberalism was a methodology that required separating out, as much as possible, one's consideration of scientific research from one's policy views. One could, and inevitably would, hold ideological and policy views, but, using a Classical Liberal methodology, debates about such matters were best separated from debates about science. As the economics profession progressively abandoned Classical Liberal policy views in a process extending from the 1930s to the 1960s, they simultaneously abandoned the corresponding methodological approach to policy.

The abandonment of these methodological views started with the development and acceptance of what would come to be called welfare economics. Welfare economics provided a formal scientific economic framework for thinking about policy. That framework proved extremely useful in shedding light on many policy questions and incorporating insights from economic thinking. The problem of coordinating responses to scarcity could be captured in a mathematical general equilibrium model and applied to a wide variety of situations. The power of this mathematical model was recognized by the profession, and it became central to the teaching of economics. It was subsequently supplemented by empirical work applying the theory, which could be carried out more rigorously due to developments in statistical analysis. These advances led many economists to believe that economics had become engaged in a series of scientific breakthroughs that would rescue economics from what many considered to be the realm of pseudoscience. Instead, the discipline would be invested with the much more welcomed foundation of formal science.

The general equilibrium model framed policy within a mathematical optimal control structure. The model implicitly assumed that a perfectly competitive market would optimally organize economies in most situations, but that government intervention would be needed to correct for market failures, such as externalities. This approach resulted in what was considered a scientifically based policy conclusion implying some need for government intervention if an economy was to run smoothly. The theory proceeded to develop formal marginal conditions that were capable of guiding policy makers. Laissez-faire was correspondingly non-optimal.

This welfare economics policy framework caught on like wildfire. Since the best way to understand the framework was to understand the mathematical structure of the general equilibrium system, adopting this framework changed the way economics was taught. Students were taught more math and statistics and less moral philosophy and institutional insights. With that change in place, the general equilibrium welfare policy framework eliminated the previously acknowledged firewall. What was considered scientific economic theory was connected directly to policy.

The change to a mathematical general equilibrium welfare economics framework occurred throughout the profession. It started slowly, but resistance decreased as new mathematically and statistically trained economists replaced those trained in the broader Classical Liberal discursive tradition. The art of policy was lost, and the craft of policy became synonymous with scientific theory. The policy/science firewall was fundamentally violated by this change.

Classical Liberals objected, arguing that thinking about policy in too rigid a mathematical fashion eliminated all types of issues that were important components of the policy debate. The general equilibrium model wasn't wrong, but it wasn't "the" sole economic model that should be employed. It was a model based on assumptions that for some issues was useful, but for others was not. Different assumptions could lead to different results. The problem with the model was that it didn't appropriately capture many of the debatable issues relevant to policy, and thus inappropriately limited the scope of policy discussion. Those objections were vigorous at first, and then tended to fade away. Economists advancing those arguments were attacked as being old-fashioned and unscientific. They were dismissed as lacking mathematical knowledge and skills. This change in methodology occurred throughout the English-speaking economics profession, but was led by economists at LSE (Hicks and Lerner), Cambridge (Pigou), as well as Harvard and MIT (Samuelson).

The Chicago Response

The incipient Chicago School (George Stigler, Milton Friedman, Aaron Director, and their associated colleagues) objected to the development of this general equilibrium welfare frame as a basis for policy. They saw it as a rejection of Classical precepts guiding both microeconomic and macroeconomic policy. Particular ire was directed at what they viewed as the malignant travesty encapsulated by the developing Keynesian (collective) policy.[9] These Chicago economists insisted that the Keynesian model was a Trojan horse being used to advance statist ideology and collectivist ideas. They chose, however, not to argue in favor of Classical Liberal methodology. Nor did they reject the implicit contention that policy must follow from mathematically rigorous models. Instead, they responded to this challenge by developing an alternative "scientific" pathway that would lead to the desired laissez-faire policy precept. Specifically, they developed an alternative model demonstrating that the types of government intervention supported by the welfare economic framework, as well as the Keynesian macro framework, were fundamentally flawed theories. In their alternative scientific model, an economy would work best when left to its own devices.

Because of their impressive rhetorical and intuitive marketing skills, the Chicago economists eventually managed to engineer a successful partial counterrevolution against this general equilibrium welfare economics framework. But, in engineering that counter-revolution to save the Classical Liberal policy precept of laissez-faire, they abandoned the most essential part of Classical Liberalism—its methodological foundations. In doing so they, like the advocates of the general equilibrium welfare policy framework, abandoned the Classical Liberal firewall that had previously separated science from policy. Once they accepted that policy necessarily followed from a scientific model, the economic policy debate became inextricably focused on whose science was correct. The bone of contention no longer survived as a debate focused on judgments and sensibilities, which is where Classical Liberal methodology placed it. Serious discussion concerning the subtleties and judgments underlying these differing views became impossible. Each side characterized the other as ideology masquerading as science. These pointed accusations were precisely the type of futile debate that the Classical Liberal firewall had been designed to avoid.

Our concern in this book is not focused on which policy view is correct. Instead, our interest lies in the manner in which the debate about those policy views should be conducted. Should it be primarily a debate about science, formal models, and statistical tests of empirical evidence? Or should it be primarily a debate about sensibilities and judgments that are informed, but not determined, by science? Our argument is that Classical Liberal support for the market was a precept upon which good economists could disagree. It was not a fundamental conclusion based on economic science. Although science and theory can provide some guidance about policy, resolution of such debates is not to be found in theory, but rather in vigorous "argumentation for the sake of heaven," a term that designates a process of cordial argumentation that attempts to seek out the best estimate of truth that is possible.[10] It is not argumentation that focuses primarily on winning policy debates.

Had the fight been about Classical Liberal methodology, not policy, the battle lines would have been drawn quite differently. For example, in terms of policy, the Chicago School vehemently opposed John Maynard Keynes. But in terms of methodology alone, Keynes was an ally of Classical Liberalism. Throughout his career he steadfastly stayed within the Classical Liberal methodological tradition. He questioned mathematical models, econometric models, and tended to use discursive arguments to make his key points.[11] While in terms of specific policies, Frank Knight, a guiding light of Chicago in the 1930s, was often diametrically (and vehemently) opposed to Keynes, methodologically Knight had much more in common with Keynes than he did with either Samuelson or Arrow (or Friedman for that matter). That methodological connection between Keynes and Knight was lost when policy (and ideological) fidelity trumped methodological fidelity as the litmus test for Classical Liberalism. By surrendering Classical Liberal methodology, the economics profession steered the policy debate to its current sterile state.

Talk Is Cheap: Paying Lip Service to the Science/Policy Firewall

Even as economists were tearing down the firewall erected between theory and policy, they implicitly recognized what they were doing. They even paid lip service to maintaining that traditional separation, as they were busily blending the two. For example, Milton Friedman, who most observers saw as constructing scientific arguments supporting the superiority of the market, states:

> I have tried to influence public policy. I have spoken and written about issues of policy. In so doing, however, I have not been acting in my scientific capacity but in my capacity as a citizen, an informed one, I hope. I believe that what I know as an economist helps me to form better judgments about some issues than I could without that knowledge. But fundamentally, my scientific work should not be judged by my activities in public policy. (Friedman quoted in Overtveldt 2007: 91)

His counterpart, Paul Samuelson, similarly acknowledged this need (as well as the difficulty anyone would have in preserving it), writing:[12]

> I could disagree 180 degrees with his [Milton Friedman's] policy conclusion and yet concur in his diagnosis or the empirical

> observations and inferred probabilities. Yet such is the imperfection of the human scientist, an anthropologist studying us academic guinea pigs will record the sad fact that our hearts do often contaminate our minds and eyes. The conservative will forecast high inflation danger on the basis of the same data that leads the do-gooder to warn against recession. (Conscious of this unconscious source of bias, as the subsequent discussion will elaborate on, I make a special effort toward self-criticism and eclecticism—with what success, the record must testify to.) (Samuelson 2011: 888–889)

Our argument is that, while recognizing the need for a firewall when forming policy arguments, as well as the use of economic theory to support that policy, MIT economists such as Samuelson, Chicago economists such as Friedman, and Keynesian economists such as Abba Lerner and Jim Tobin[13] continually violated the science/policy firewall.[14] Doing so forced the opposing side to feel as if it had to do the same. Neither side could bring itself to hold firmly to the Classical Liberal position that the basis for their policy recommendations did not, and could not, directly follow from economic theory or science. Instead, their policy conclusions should have followed from a much more complex set of contestable arguments, reasoning, and sensibilities. Policy involves matters on which reasonable people, who share the same deep understanding of scientific economic theory, can differ and which cannot be resolved satisfactorily in terms of economic theory or science. The best we can hope for is resolution through "argumentation for the sake of heaven," which requires good faith efforts from both sides to communicate.[15] So, while both sides instinctively comprehended the gulf dividing economics as a science from its role in guiding policy, they also found following these instincts almost impossible.[16] Expediency, as it often does, triumphed. In a suitable phrase that one can imagine being employed over lunch at the University of Chicago's Quadrangle Club, the gulf existing between intentions and actions can be summed up by an all-purpose dismissal, namely that "talk is cheap."

In the postwar struggle for the soul, or at least the lungs of the discipline, most of the niceties separating the rough and tumble of policy hawking from the more imposing heights of economic science became blurred. The application of economics would come to be seen as grounding policy on a firmer and more scientific foundation. Doing so involved replacing discursive economic analysis, which touched on the sensibilities and judgments underlying policy differences, with the supposed rigor of "scientific" analysis. Math replaced words whenever possible. Statistical studies replaced case studies, and empirically tested theory replaced reasoned discursive argument. Economic policy analysis evolved into an applied science that was no longer seen as an art and craft. Discursive argument, dealing with judgments and sensibilities, no longer had a featured place in the applied science of policy economics.

Seeking a scientific basis for policy was by no means a radical procedural departure that definitively separated the Chicago School from the broader swath of the more mainstream economics profession of that era. As we have emphasized, it was a movement that transcended the confines of Chicago. As previously mentioned, throughout the Classical period of economics, there had been continual attempts by one side or the other to claim a scientific foundation for its policy views. The evolving acrimony in dealing with those competing claims led Classical Liberal economists to develop their science/policy firewall. Precisely because the temptation to assert such a scientific basis for one's policy views was so powerful, that claim had to be continually challenged and contained.[17] However, in the rising optimism characterizing the postwar era, economic policy seemed to present no more of a challenge to a rising set of engaged professionals than did the previous daunting tasks of mobilizing resources to conquer the Great Depression or to successfully vanquish wartime foes. The solution to any policy problem simply involved the appropriate application of measured scientific methods.[18] To manage this task, a new cohort of young economists achieved positions of increasing prominence,[19] implicitly promising to shift away from the sort of judgment-making and ill-defined interpretative skills required by the older, Classical Liberal economists still trapped by the past.[20] The prevailing conviction was that by employing scientific methods, ambiguity-ridden policy pronouncements would be significantly reduced if not eternally banished.

What developed in the postwar period was in essence a transformation in the approach economists accepted when formulating economic policy. This book explores that transformation and argues that it contained within it a fatal flaw. In an attempt to achieve a more scientific foundation for its policy analysis, economics violated Hume's Dictum that a "should" cannot follow from an "is." By doing so, this highly touted scientific approach shifted policy debate away from the nuanced understanding that policy required, namely a subtle blend of theory, moral philosophy, and judgment. Instead, the discipline maneuvered itself into an untenable corner where differences in policy unswervingly followed from differences in scientific theory. Consequently, they could only be resolved by using scientific methodology.

Why Did the Profession Abandon Classical Liberal Methodology?

Classical Liberal methodology developed in response to a recognition of the problems created by too closely linking science with policy. This methodology was a correction of a misconceived reaction. Initially, for example, Adam Smith's broad-ranging discursive style failed to lead to any definite policies. Instead, he seemed to provide qualifications, which allowed for different views and interpretations. He supported the market, and laissez-faire, but the support was highly qualified. As a counterpoint to Smith's approach, later economists felt compelled to construct more definitive formulations. David Ricardo proved more than willing to oblige. But, as Classical economists discovered, doing so came with attached problems. For example, as Alfred Marshall noted:

> Their [Ricardian economists'] agreement with one another made them confident, the want of a strong opposition made them dogmatic; the necessity of making themselves intelligible to the

> multitude made them suppress even such conditioning and qualifying clauses as they had in their own minds. Therefore, although their doctrines contained a vast deal that was true, and new, and very important, yet the wording of these doctrines was often so narrow and inelastic that, when applied under conditions of time and place different from those in which they had their origin, their faults became obvious and provoked reaction. (Marshall 1923: 759–760)

In response, subsequent Classical Liberal economists vowed to do better. They developed a firewall separating policy from economic science to help them keep their vows. Since a key argument in our book is that Classical Liberal economists were correct in erecting and preserving a firewall, a natural question is: Why was that methodology abandoned by the profession? We argue that the abandonment was related to, but not synonymous with, the movement from classical to neoclassical economics. Neoclassical economics could be quite consistent with Classical Liberal methodology as long as the neoclassical economist treated the model as a suggestive tool, to be used as only one of many inputs into policy, and not as a direct guide.[21] Through the 1930s, and even into the 1950s and 1960s, vestiges of that Classical Liberal methodology survived despite the onslaught of a newer, more scientific approach. For example, Alfred Marshall, the leading neoclassical economist throughout the 1920s, remained committed to Classical Liberal methodology, although, to be honest, Marshall waivered depending on how the political winds were blowing, as did other supporters of that methodological persuasion. The temptation to claim science as being on your side is often almost overwhelming.[22]

Marshall conceived of economics as operating most successfully when it performed as an applied discipline, in the sense of a tool or way of thinking that allowed insight and understanding of common problems and situations. He termed this formulation the study of commonplace business of everyday life. The reason for studying the subject was not merely to gain an understanding of how the economy worked, but more importantly to provide the basis for sound policy measures. Similarly, John Maynard Keynes also maintained a Classical Liberal methodology, which separated him from those that came to be labeled as his neo-Keynesian followers. They readily abandoned Classical Liberal methodology and entered almost without exception into the scientific policy debate.[23]

As we discussed above, the Classical Liberal method had its origins in Adam Smith's studied and educated commonsense approach to problem-solving. His recognition of the need to yoke the moral to the economic originated in the linkage between his two great works, *The Theory of Moral Sentiments* and *The Wealth of Nations*. In his grand schema, economic analysis and policy provisions were necessarily interwoven with ethics and moral philosophy. He intertwined the two by employing the viewpoint of a hypothetical impartial spectator who was able to serve as the basis for evaluating proposed policies. Consequently, the goal posed by any discussions or written monograph was to convince such a spectator that the advocated policy offered a preferable, rather than an ideal, option.[24] From this perspective, no aspect of the argument was incontestable or off the table.

To move from the realm of theoretical argument to that of policy, one required this notion of a hypothesized impartial spectator. This imagined umpire would be capable, by means of appeal, of arbitrating and sorting out conflicts and differences attached to the relevant issues. Smith's argumentation was directed at those impartial spectators. The qualifications he placed on his laissez-faire policy precepts were ones that any group of reasonable people could be expected to place on such proposed policies.[25] Formal mathematical and theoretical arguments would not serve the purpose. They might inform the thinking of an advocate of a particular policy position, while still failing to be decisive. All reasonable qualifications about a given policy would be part of the debate. Equally, the question of what precisely was meant by designating a privileged "impartial spectator" to perform this essential role remained far from settled and should itself be subject to debate. Ultimately resolving such debates was distinctly not the job of the economist, but rather of the statesman or of the economist acting in the role of statesman, not scientific economist.

Whereas Smith concretely grounded his policy analysis in *The Theory of Moral Sentiments* as we stated above, Ricardo did not. Joseph Schumpeter would later characterize the resulting outcome as reflecting the "Ricardian Vice."[26] Smith's approach, clothed in much more sophisticated garb, would later be reinstated by John Stuart Mill, the philosopher/economist who best represents the Classical Liberal methodology we advocate. Mill curbed Ricardo's overly optimistic, and quite immodest, enthusiasm by insisting on the inherent limitations constraining theoretical and scientific policy conclusions. Policy was not an exact science but could be more productively thought of as an art that demanded judgment and skill. Theory did not imply specific policies but might prove useful in thinking about them. That would only hold true as long as one had a deep understanding of all the other necessary dimensions involved in arriving at policy conclusions.

Smith's impartial spectator device provides insight into understanding Classical Utilitarianism, an approach that defined the normative goal of economic policy as "the greatest good for the greatest number."[27] Classical utilitarianism was a normative touchstone that one could reasonably argue would be arrived at by employing the impartial spectator perspective.[28] Classical economists saw the utilitarian rules of thumb they developed as the appropriate ways to make needed interpersonal comparisons when engaged in policy analysis. Those rules of thumb did not follow from science (or theory), and an economist accordingly was well advised not to pretend that they did. They were just useful reference points in the inevitable debates arising during policy discussions.

This normative version of utilitarianism was in no sense science, nor was the "utility" it referred to a concept that could be encapsulated by empirical measurements of preferences. Utility was not a concept that could be made empirically observational, as some later neoclassical economists would attempt to accomplish.[29] As Vilfredo Pareto pointed out, what might be measured by scientific methods was something quite different than the normative concept of utility. What could be measured, and what might be shown by revealed preference, was a concept that captured people's choices, which Pareto called ophelimity to distinguish it from utility.[30]

The Classical Liberal Laissez-Faire Precept

If economic theory does not lead to policy results, how did Classical Liberal methodology come to achieve its close association with laissez-faire policy? The answer is that Classical Liberal's laissez-faire policy was based on a set of debatable judgments about the working of the political process, as well as a normative commitment to individual freedom.[31] Classical Liberals did not claim that these judgments were based on science or economic theory. Instead, they were based on observations of how governments and the market actually worked, including their sensibilities about the normative beliefs that were most important to them. Both judgments were recognized as debatable, and given to change.[32] Nonetheless, they were largely shared by Classical Liberals of varying normative and ideological persuasions. They were judgments that many Classical Liberals believed a relatively neutral person would likely arrive based on all the arguments, not just a subset of economic arguments.

These shared normative and institutional judgments were the reason Classical Liberals often came to similar policy conclusions. Many at that time shared the belief that policy of real-world governments would generally not achieve the ends that policy advocates claimed.[33] (It can be argued that governments then were even less transparent, and more controlled by vested interests, than they are now because democracy was only in its beginning stages.) This perspective strongly implies that their support of laissez-faire policy *was not based on economic theory*. It was based on shared judgments about the difficulties connected with how government policy worked, and should be presented as such. Jacob Viner, a neoclassical economist who firmly embraced a Classical Liberal methodology, points this out.[34] He writes:

> Adam Smith was not a doctrinaire advocate of laissez faire . . . He had little trust in the competence or good faith of government. He knew who controlled it, and whose purposes they tried to serve . . . He saw, nevertheless, that it was necessary, in the absence of a better instrument, to rely upon government for the performance of many tasks which individuals as such would not do, or could not do, or could only do badly. (Viner quoted in Overtveldt 2007: 90)

These critical judgments underlying policy were based on historical and empirical judgments upon which reasonable people might differ. The arguments for laissez-faire might refer to theorems developed in economic theory as a framework to capture insights that were not apparent at first sight, but laissez-faire was not based on such theorems. Viner serves as a striking representative of those neoclassical economists who strongly insisted on the need to maintain an imperturbable classical methodology. He writes:

> I do not think it is practical to write an elaborate work on the working of economic process in modern society on a completely "objective" basis . . . Anyone who could do so would be pathological and the pathology would be likely to extend to his selection of premises—it is always necessary to begin somewhere, but where one begins can have great influence on where one ends up—and on his decisions as to what are facts and what myths. In so far as is possible, value-judgments should be labelled as such, but their systematic exclusion is, I am convinced, not in practice either possible or desirable. They should not be concealed, they should not be eccentric, and they should not be elaborated or didactically pressed. (Viner quoted in Van Horn 2011: 291fn)

Viner followed the lead of Classical Liberal economists in believing that price-coordinated markets held both advantages and costs when observed in the actual world. Markets were necessarily embedded in institutional and social structures subject to unforeseen transformations. Both markets and the governments within which they must necessarily operate evolved over time and were consequently co-dependent rather than opposing forces. What Adam Smith and his subsequent followers understood specifically was that questioning whether governments should retain an active role within the operations of a market economy was not a theoretical issue. The effective contestable terrain, one that nourished fruitful debate, encouraged opposing sides to argue the nature, rather than the existence, of that role.

Accordingly, policy debate from a Classical Liberal viewpoint was not about the existence, but rather about the nature of a government's role. The adoption by the profession of the general equilibrium welfare policy model, as well as the neo-Keynesian macroeconomic model, violated this precept because these models were designed to directly lead to answers about the policy role of governments. Classical Liberal economists didn't believe any specific model could answer such questions. At best, a model might shed some useful light. True, when interpreted by more sophisticated economists, the limited relevance of formal models to policy would be admitted, but that was not the way such models were used in practice. In the immediate postwar texts, and in the relevant policy discussions, models directly supported government intervention into a market economy. However, the users of that general equilibrium policy model and the Keynesian model claimed to be ideologically neutral despite such unvarying conclusions.

Lost in the use of these models were the concerns voiced by Classical economists that the policy levers of government were controlled by vested interests, not by benevolent public engineers. Inevitably, these vested interests would use policy to advance their objectives in subtle and devious ways. It was primarily this concern about vested interests and the working of governments, not economic science or theory, that led Smith and later Classical Liberal economists to drift toward supporting a relatively laissez-faire policy. Therefore, their sensibility was that policies, when formulated and enacted, would be dominated by the powerful, which makes the fruit of such policies highly tainted. Those in power could not be trusted to design programs in the public interest unless they faced an effective countervailing power. Consequently, every policy proposal had to be met with unbridled skepticism. True, Chicago voiced a similar skepticism, but theirs appeared to be more an ideological conviction outfitted with the benefit of scientific justification. In contrast, the Classical Liberal position was grounded, not in science, but in judgment from experience. They found that generally, hidden in the footnotes and subclauses of any such policy act, lurked an intention at odds with its more publicized objectives. Given such inherent limitations, rather than engaging in active economic planning and restructuring, government policy should preferably focus on maintaining competition. Intervention by government remained an option of last resort. But long-term policy success depended on creating competitive institutions that managed to use selfishness to counterbalance selfishness.

Conclusion

In summary, the story we tell in this book about policy is one of how economists lost what might be called the "least worst" justification for markets and replaced it with competing scientific justifications. These substitutions alternatively supported either laissez-faire or government intervention. One side claimed that though markets were efficient, government intervention was needed to correct imperfections, thus enabling markets to work even better. The other side claimed that markets were efficient and that government intervention would only precipitate cascading problems. What both sides lost by shunning the Classical Liberal approach was the acknowledgment that scientific inputs into policy form only a limited part of any economic policy debate. A fruitful debate would necessarily address a wide array of issues that go far beyond traditional economic concerns.