Computational Economics and Economic Theory: Substitutes or Complements?

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Abstract. This essay examines the idea and potential of a "computational approach to theory," discusses methodological issues raised by such computational methods, and outlines the problems associated with the dissemination of computational methods and the exposition of computational results. We argue that the study of a theory need not be confined to proving theorems, that current and future computer technologies create new possibilities for theoretical analysis, and that by resolving these issues we will create an intellectual atmosphere in which computational methods can make substantial contributions to economic analysis.

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The increasing power of computers presents economic science with new opportunities and potential. However, as with any new tool, there has been discussion concerning the proper role of computation in economics. Some roles are obvious and noncontroversial. Most will agree that computation is necessary in econometric analysis and offers some guidance in policy discussions. Theorists will admit that examples are useful in illustrating general results. However, the discussion frequently gets heated when one raises the possibility of using the computer and computer-generated "examples" instead of the classical assumption-theorem-proof form of analysis for studying an economic theory. In economic terms, the first roles for computation are complements to and a useful ingredient in standard research activities; however, the activity of computational theory appears to be a substitute for conventional theoretical analysis. In this essay, I will focus on the potential role of computational methods in economic theory and their relation to standard theoretical analysis, asking "Are they complements or substitutes?"

We should first realize that the issues raised by the recent surge in the use of computation in economics also arise in general science; in fact, economic science is far behind most sciences. In the past, science proceeded in two fashions. First, there were the observational and experimental modes wherein observations of actual phenomena were used to determine general patterns. Second was the development of theories, wherein formal models of nature were constructed and their logical implications explored through abstract mathematical reasoning. The objective of such an approach is to summarize the implications of a theory in the form of some closed–form expression and/or some general statements, usually called theorems. I shall call that mode of theoretical analysis *deductive theory*. However, the limitation of such an approach is that only simple cases of any general theory can be completely examined in this way. For example, in both classical and quantum physics the general n-body problem can be solved only for $n = 2$. Science has always resorted to approximation methods to extend its analysis, and as computational power has grown science has exploited a wide range of numerical and computational methods to analyze its theories. I shall refer to this activity as *computational theory*.[1]

[1]Since logic as practiced in economic theorizing is finitistic, computer proofs of theorems will surely come to be important in the future. However, this essay ignores this application of computation and treats theorem-proving as an exclusively human activity.

Economics is also undergoing the same transformation, following in the tracks of physics, chemistry, astronomy, and other "hard" sciences. Below, I will give some examples of how we may learn from their experience and some common problems. However, economics does have idiosyncratic features which limits the value of studying how other fields use computation. In particular, physical theories are very specific, such as the inverse square law of gravitation and Schrodinger's equation, whereas economic theories often make qualitative assumptions, such as concavity, and deduce qualitative implications, such as efficiency of equilibrium. Furthermore, the physical behavior studied in physics is presumed to be exact — God may or may not throw dice, but physicists assume that God does not make any mathematical errors in the execution of natural laws — whereas few economists really believe that their objects of study, ordinary economic agents, are infinitely intelligent agents acting with infinite precision. These differences have important implications generally; here they help us interpret the errors which arise in any computational method. In the sections below I will discuss ways in which computational methods can be used to analyze economic theories.

While my main focus will be on the intellectual and scientific potential value of computational methods in theory, I must also discuss several institutional and professional aspects of the economics community which will need to be adjusted if we are to realize this potential. The use of computation has been increasing, as indicated by the examples I cite and the much larger number of examples I do not cite. In preparing for this talk, I did some literature search and was surprised at the extent to which computation has become a common tool. The computational literature is clearly growing rapidly.

However, the progress is uneven. The acceptance of computationally intensive research varies across fields. Even where accepted, there is little agreement on how to present computational results and techniques. Frequently, authors are not allowed to publish the basic computational details of their work, even when the computational innovation is of greater interest than the particular economic application, while others are given substantial space for doing nothing more than reinventing the wheel. There is also uneven awareness of computational methods; in some fields the typical author is acquainted with the latest mathematical developments, whereas authors in other fields use decades-old methods much less efficient than those currently available in the mathematics literature. There is neither a common core of methods nor a language. The vocabulary used by some economists is inconsistent with mathematical practice and authors often do a poor job in describing their methods, making it difficult for readers to understand the descriptions and for techniques to disseminate. These problems keep computational economics from realizing its potential, particularly in theoretical studies. I will attempt to discuss the issues and indicate ways we can increase the value and acceptance of computational work.

Before continuing, the reader should be warned that this is not meant to be a precise, passionless, unbiased analysis of an economic question. This essay is intended to be provocative, to highlight important issues, and to stimulate discussion. Many of the assertions I make will be meant to highlight important issues; they are not meant to be a fair and balanced treatment of the intellectual history of computational economics. The topics and examples I discuss are also idiosyncratic, reflecting my experience and limited awareness of the literature. Other commentators have taken different positions on some of the issues; for example, Bona and Santos[11] present a different perspective. I treat some of these issues in a more balanced, detailed, and expansive way in Judd[39]. Interested readers should also consult Kendrick [**?**], Pakes[57], Rust[63],[64], Marcet[54], Judd[38][40], and the forthcoming *Handbook of Computational Economics* for other discussions of important issues. The purpose here is to dramatize the issues so that we may work to improve the quality, soundness, and appreciation of computational work in economic analysis.

1. Computation and Science

There have been many important developments in the physical sciences which have been made through computational methods. Some examples have a flavor similar to the problems encountered in economics. This section presents a few which I have found to be intuitively valuable; reviewing them gives us an idea about what can be done in computational economics.

One of the great mysteries in astronomy is Jupiter's Red Spot. It is essentially a hurricane, a common occurrence in our atmosphere, but the Red Spot is one which has continued for centuries. Scientists have long wondered how the Red Spot could remain stable for so long. A computer model of Jupiter's atmosphere was run to see if a hurricane of centuries-long duration would arise. The surprising result was that nothing exotic was needed for such Red Spots to arise other than standard interactions of the fluid, gravitational, and energy properties of Jupiter. The importance of the computational approach for this conclusion is obvious since only a computer model could handle the turbulent interactions inherent in such phenomena.

An example a bit closer to home concerns the formation of the moon. Examination of moon rocks showed them to be similar to, but not the same as, rocks here on earth. A popular theory has always been that the earth–moon system formed as a result of a large object colliding with earth. Computer modelling has shown that this is indeed a plausible explanation, and that the differences in earth and moon rocks can also be explained by the mechanics and chemistry of such a collision.

Astronomical and meteorological examples of computational modelling are appropriate for economists. Since astronomy, meteorology, and economics are all largely observational sciences, not experimental,[2] they all have to take what they observe and try to back out the causes. Computer modelling operates as a substitute for experimentation in screening possible explanations for plausibility and consistency.

In fact, some of the successes of the computational approach in economics are similar to these computational successes. Using computational methods, Kydland and Prescott[47] showed that fairly simple dynamic general equilibrium models could display the type of economic fluctuations we observe in the macroeconomy. Prior to that many argued that macroeconomic data were inconsistent with the standard competitive model, just as many thought that the Red Spot was due to special causes. While the full Real Business Cycle (RBC) research program remains controversial and unfinished, it is a major contender in the current intellectual battles and is an example of research conducted in a largely computational manner.

These examples all use conventional methods of numerical analysis ways to compute approximate solutions. Another way to generate such applications is to use asymptotic and perturbation methods. The analysis of the helium atom is a good example of perturbation methods applied in quantum mechanics. The basic problem is that the only atomic quantum systems which can be solved in closed-form are those involving the interaction between a negatively charged particle and a positively charged particle, the hydrogen atom. The helium ion with one electron can be solved because it is basically a $+2$ nucleus and a single -1 electron. However, the helium atom cannot be solved because of the repulsive forces between the two electrons. If electrons did not repel, then a closed-form solution is the sum of two helium ion solutions. The key perturbation idea is to take the closed-form solution without electron repulsion, differentiate the result to find out what happens when one adds a little repulsion (economists call this "comparative statics") and then extrapolate the result to arrive at an approximation of the quantum mechanics solution for the helium atom. This is just one example of the large variety of approximation strategies employed in quantum theory.

General relativity theory uses a similar approach. The nonlinear system of partial differential equations which describe general relativity theory have no general solution. Closed-form solutions generally involve only one body with mass. The first general approach used to analyze general relativity begins with the degenerate case of a universe with no mass; that is, a vacuum. The vacuum case has a simple solution; actually, the solution is the special theory of relativity. One then perturbs the general equations to develop a linear theory which can be analyzed. This sounds crazy

[2] The parallel is not exact. The nonexperimental nature of economics research is a political fact, not inherent in the field. If economic research were given a budget equal to that given to the search for the Higgs boson, top quark, and other exotic particles, economics could also be an experimental science.

because the whole point of general relativity theory is to be a theory of gravity whereas this method begins with a universe in which there is no gravity, but the linear theory and higher-order approximations are the basic tools physicists use to study the implications of general relativity theory.

The value of approximation methods, numerical and asymptotic, in the physical sciences is undisputed. If theoretical physicists insisted on using only closed-form solutions or proofs of theorems to study their models, they would spend their time examining the hydrogen atom, special ions, and universes with one star, and ignore most interesting applications of physical theories. These examples serve two functions. First, they indicate the high cost of staying with closed-form solutions. Second, they dispel any notion that staying with closed-form solutions and deductive approaches to theoretical analysis is a requirement for doing respectable, rigorous science.

2. What Can Economists Compute?

The next point which must be made is that there is a wide range of economic models which can be computed in reliable and efficient fashions. Some of this material is old and standard, but the recent upsurge in interest in computational methods has generated a large amount of new work, allowing us to solve models previously considered intractable. Any review of this literature indicates the substantial potential of computational methods, and the breadth of available methods shows that all areas of economic inquiry can profitably use computational methods.[3]

First, we should remember that the linear-quadratic approach to modelling produces computationally tractable analyses of models of dynamic choice in both competitive and game-theoretic contexts, with and without perfect information. Even today, progress is being made in improving the solution methods for the Ricatti equations that arise in linear-quadratic control problems. The excellent manuscript by Hansen and Sargent[29] presents many of these techniques. Despite the fact that these tools are well understood, even this approach has been used relatively little in theoretical work. For example, consider industrial organization theory. The literatures on imperfect competition, learning curves, investment, and informational asymmetries are dominated by static (or nearly static) models which can never be reasonably calibrated to yield quantitatively sensible discussions of these phenomena. The other common approach is to use supergame models in which nearly anything is an equilibrium. Dynamic games are easily computed (see the papers by Kydland[46]), but outside of few exceptions ([34], Reynolds[60], and Fershtman and Kamien[23]) this approach is almost never taken in theoretical industrial organization. In contrast,

[3] The following is by far from a complete listing. I have focussed on numerical methods which are currently on the efficiency frontier of computing. See Judd[39] for more complete listing of alternative methods.

these methods are used extensively in the international policy competition literature and monetary policy games (see Fershtman[22]).

Second, there is the computational general equilibrium literature. This literature took off with the development of Scarf's algorithm thirty years ago; see Shoven and Whalley[67] for a recent survey. Recent advances include the application of variational inequalities; see the book by Nagurney[56] and the papers of Rutherford (e.g., [65]) for recent work in this area and economic applications. Computational general equilibrium is the most mature computational area in economics, but it focuses generally on policy-related research such as taxation, trade policy, regional development and is usually weak on the intertemporal dimension. The recent work of Brown et al.[14] and Schmedders[**?**] now makes it possible to compute general equilibrium with incomplete asset markets.

Recent years has seen much work on developing numerical methods for nonlinear dynamic problems. There has been much effort recently on computing dynamic programming models. Rust[**?**] implemented sparse matrix methods to solve large problems. Johnson et al.[32] has demonstrated the usefulness of the spline ideas exposited in Daniel[19] and the polynomial ideas exposited in Kalaba et al.[7]. Judd and Solnick[45] have demonstrated the usefulness of shape preserving splines to dynamic programming problems.

Perfect foresight models have been developed in the past fifteen years to study intertemporal economic equilibrium. These models typically boil down to two-point boundary value problems, a class of mathematical problems for which there is a wealth of methods. The work of Auerbach and Kotlikoff[5], and Bovenburg and Goulder[12] are typical examples of this class of models.

One of the most interesting problems in computational economics has been the solution of rational expectations models. The first work was by Gustafson [28]; Wright and Williams[72][73] developed efficient methods to compute rational expectations equilibrium even in the presence of frequently binding constraints. Tauchen[70] applied Fredholm integral equation methods to solve asset pricing models. Judd[37] showed how to use projection methods to develop efficient schemes for solving complete information rational expectations models. Laitner[48][49], Basar and Srikant[69], Budd et al.[15], Bensoussan[8], Fleming[25], Fleming and Souganides[26], Judd[35], and Judd and Guu[43] have solved for high-order Taylor expansions of rational expectations models, including dynamic games. Dixit [20], Samuelson[66], and Judd and Guu[44] derived approximation methods for asset problems. Ausubel[6], Judd and Bernardo[41], Bernardo[9] and Corb[17] have solved models of asymmetric information much more general than the ubiquitous exponential-Gaussian example.

There has also been much success in developing algorithms for solving for Nash equilibria of games. Lemke and Howson[50] computed Nash equilibria of two-person games, and Wilson[75] extended this to general n-person games. Wilson[76] also developed an algorithm to compute stable equilibria. Despite the large body of work on this topic, I know of no application of these methods to a specific problem. More recently Maguire and Pakes[52] has applied computation methods to dynamic games of entry and exit, Gowrisankaran[27] has numerically analyzed antitrust policy in those models, and Judd[36], and Miranda and Rui[55] have applied polynomial approximation methods for computing Nash equilibria of general nonlinear dynamic games. Cronshaw and Luenberger[18], Judd and Conklin [42] have developed methods for finding all subgame perfect equilibria in infinite-horizon games, including problems with state variables and asymmetric information.

These examples are general methods which solve general classes of problems. Many others have developed solution methods for specific problems; we will see some in our discussion below. This quick review shows that we now have numerical methods for solving a wide variety of basic problems. In fact, it is difficult to think of a problem in economic theory where there does not now exist a reasonable algorithm to use. After reviewing the array of available methods, I am disappointed with the relatively small role any of them, even the well-known old methods, play in theoretical analysis of economic problems. I suspect that there are many reasons for this. While part of the explanation is that many economists are unaware of these methods, the deeper reason is that even those who do know computational methods don't quite know how to use these tools in a way generally accepted by the profession, and therefore, in many branches of economics, there is little incentive to learn numerical methods. I will return to these issues, but first I will discuss the methodological issues.

3. The Advantages of Deductive Theory

Just as the last section focussed on what computation can do, we should note the comparative advantages of deductive theory. There are many questions which only deductive theory can address. Only deductive methods can prove the existence of solutions as conventionally understood; I hedge now because I will return to this issue below. More generally, only deductive methods can determine the topological and qualitative structure of a model. For example, deductive methods tell us if equilibrium depends on parameters in a continuous fashion. Deductive methods may tell us if an agent's equilibrium decision rule is increasing, continuous, and/or concave in the agent's state.

Deductive methods are also necessary for analyzing infinite-dimensional questions. The utility functions, production functions, and other structural elements which go into any computational analysis will generally be from some finitely-parameterized family. In contrast, deductive methods can analyze models with an infinite number of parameters, such as analyses which derive properties about models with arbitrary concave utility functions. Deductive methods are also useful in telling us if equilibrium is efficient.

4. Theoretical Analysis versus Proving Theorems

The comments above indicate that deductive methods and computational methods have very different strengths. In this section and the next, we begin to be more precise about what we mean by theoretical analysis, argue that theory is not synonymous with theorems, and argue that computations may contribute to an analysis of a theory.

One role of computation in theoretical study is noncontroversial and common. When beginning a study of a theory, one may study computed examples. These computations hopefully reveal patterns which suggest theorems. However, these examples are regarded as strictly subordinate to the deductive theory imperative to produce theorems. The theorems are just as true and publishable without examples; therefore, the patterns discovered with computations are not given any independent value since they are not validated until there is a general theorem, in which case the examples have no further logical value. Contrary to standard practice, I argue that these examples do have value even in the absence of any validation by some theorem. To argue that, I must be clearer about what constitutes a theory and its development, and distinguish it from theorem-proving. This will allow us to appreciate alternative computational approaches analyzing theories.

I want to argue that "theoretical analysis" does not necessarily involve the statement and proof of theorems. A theory, as generally understood in mathematics and science, is a collection of concepts, definitions, and assumptions. The focus of a theoretical study is to determine the implications of a theory. This is conventionally done through proving theorems. Occasionally, we can prove general theorems, such as the existence and welfare theorems of general equilibrium theory. The more common situation finds us unable to prove general results, in which case we turn to special cases which are tractable. The main point is that these special cases are also just collections of examples of the theory, albeit of greater cardinality, that they do not always indicate the true general patterns, and that computational methods can provide insight by examining collections of examples which would otherwise be ignored.

This may sound heretical, but that is partly due to the intellectual history of modern economic analysis. While all of science uses the theorem-proof approach to some extent, the emphasis in economics on theorem-proving is not typical. For example, much of conventional economic theory focuses on existence theorems. Existence results are important for any theory since they establish internal, logical consistency. Much of the effort in economic theory has been on existence proofs, and the professional rewards in that area can be great. For example, when we discuss general equilibrium theory, the names of Arrow, Debreu, and McKenzie come to mind, not because they formulated the basic concept of general equilibrium but rather because of their contributions to existence theory. The activity of providing such proofs is not as well rewarded in physics. For example, when we think of general relativity theory, the name Einstein comes immediately to mind. His contribution was to list the basic concepts, assumptions and equations, and derive some implications of relativity theory. However, he did not demonstrate the logical consistency of general relativity theory by proving the existence of nontrivial solutions to the critical equations. That was done later by Schwartzschild, Gödel, etc., names which are much less honored within physics.

The priorities of economic theory reminds one not of the physical sciences but of the Bourbaki school of mathematics, a movement which insisted on reducing everything in mathematics to pure logic. This is not the approach taken in physical sciences. In physics, there is a sharp distinction between theoretical physics and mathematical physics. Theoretical physics often uses mathematical techniques which are not logically complete. For example, physicists used the elements of distribution theory, such as the Dirac delta function, long before Schwartz developed distribution theory. Asymptotic methods are often used without complete, formal foundations. These ad hoc and informal methods have often stimulated mathematicians to provide the formal foundation.

We should also use the same distinction between mathematical economics and economic theory. When mathematical methods were introduced into economics, it was perhaps desirable that a pure, Bourbaki kind of approach be used since economists were not well-acquainted with mathematics. However, now that the profession has matured, an approach closer to that of theoretical physics is perhaps more desirable, particularly given the power of these tools.

Another important distinction is the architecture of a theory. Pure mathematics is a cumulative activity where the result of one theorem is used in proving many others. The path from definitions and assumptions to final proof is very long for most of mathematics. When the structure of a theory is so deep, it is imperative that the foundations and intermediate development be completely justified. It is understandable why theorem-proving is and will remain the dominant mode of analysis in mathematics.

This is not the case in economics. The economic portion of any economic theory is a "shallow" layer at the top in terms of its logical development. The usual proof of an economic theorem relies little on previous economic theorems. There may be similarities across proofs, but each proof uses few if any economic theorems, relying far more on mathematics with a deep logical foundation. Therefore, the errors and imprecision of computational methods in one economic analysis have much less chance of contaminating and undermining later work.

These observations all emphasize the main point that economics is not mathematics. While many of us would have preferred being pure mathematicians, we should not try to turn economics into a branch of pure mathematics.

5. The Excessive Simplicity and Quantitative Irrelevance of Tractable Theoretical Models

Some might wonder why we would ever consider using computation except in the context of empirical analysis. This is an important issue for computational economics since if the primary application is empirical analysis then the focus should be on problems where good data is available in sufficient quantities to make empirical analysis feasible. Computational approaches to theory substantially broaden the variety of problems to which computational solutions would be needed.

The major reason for a computational approach to theory is the excessive simplicity of currently tractable theoretical models. I refer here not to the "high" theory of equilibrium existence theory, but rather to the parametric modelling typical of theoretical analysis in the applied fields. Theoretical models often make simplifying assumptions so that they can get clear, substantive results. These models are also used to interpret empirical results. The results are often unrealistic since the elements which are sacrificed in the interest of simplicity are often of first-order importance.

An excellent example of this occurred recently in the economic literature on executive compensation. Jensen and Murphy[31] in their study of executive compensation found that management earned, at the margin, only three dollars per thousand dollars of profits. They argued that this was far too small to create the proper incentives for managers. They made reference to the fact that risk-neutral managers would earn all the residual profits under the optimal contract, and argued, without making any computations, that observed incentives were too small to be consistent with manager risk-aversion. In response, Haubrich[30] actually computed some optimal contracts and showed that with reasonable estimates of risk aversion the optimal contract would give managers much less marginal incentive; in fact, he showed that for many of the examples in Jensen and Murphy, the marginal incentive would be *three dollars per thousand*! The use of the theoretically simple benchmark model, the risk neutral manager, produced a strong implication (that manager pay should be strongly tied to performance) which was completely reversed by the use of a computational approach to a more sensible model.

This is not atypical. I am sure that there are many other such examples yet to be exposed. The unrealistically simple nature of analytically tractable theoretical models is also a problem for empirical work. The ability of computational methods to explore more realistic cases of a theory is its great value.

6. The "Black Box" Criticism

While most will agree that most theoretical models are too simple, they will also claim that a computational analysis is not a good alternative. Many argue that the results of a computational study are unintuitive and incomprehensible because the computer program which generates the result is essentially an impenetrable "black box".

These criticisms are valid, but not damning. I will give two responses in this essay. First, the black box criticism is more a comment on the poor fashion most computational work is exposited and the general lack of sensitivity analysis. When a computation gives an answer, we do want to know which economic forces and considerations determined the answer. To some extent, the only way to address this is to conduct several alternative computations. I will expand on this point in later sections.

The second response is to recall Einstein's recommendation — a model should be "as simple as possible, but not simpler". We need to remember that we are studying complex questions, a fact which is true if we are macroeconomists or tax economists studying national economies and their policies, microeconomists studying firms, or labor economists studying decisionmaking in a family. This consideration is often ignored in modern applied theory where unicausal analyses dominate. For example, the industrial organization literature is filled with models which explore one interesting feature of economic interaction in isolation. A typical model may study moral hazard *or* adverse selection, *or* entry *or* investment *or* learning *or* R&D, *or* asymmetric information about cost *or* demand, *or* sharing information about cost *or* sharing information about demand, *or* etc. To see how limiting this is, suppose meteorologists took this approach to studying the weather; they would ignore complex models and their "black box" computer implementations, and instead study evaporation *or* convection *or* solar heating *or* the effects of the earth's rotation? Both the weather and the economy are phenomena greater than the sum of their parts, and any analysis which does not recognize that is inviting failure.

This is not to say that the simple focussed studies are unimportant. They do give us much insight. But they can only serve as a step in any substantive analysis, not the final statement. Once we leave simple models, computation is often the only way to get any answers. The issue is not whether we should go to computational methods, but how best to use them.

7. The Real Issue: *Where* to Approximate, not *Whether*

When we face up to the complexities of what we are ultimately trying to study in economic theory, we see that the issue is not whether we use approximation methods, but where in our analysis we make approximations, what kind of approximation errors are we going to tolerate and which ones we try to avoid, and how to determine approximation errors to the extent possible. Simple, unicausal models make approximation errors by ignoring all but one feature of the real world. While we may be able to prove theorems in those one-dimensional models, the results have only approximate, if any, validity concerning the real world. Computational methods can be much more realistic, but bring with them approximation errors of a numerical kind. We are generally presented with a trade-off: achieve logical purity while sacrificing realism, or bring many elements of reality to the analysis and accept imprecision due to numerical error. The proper choice will depend on the context.

Our purpose in these last sections was to argue that neither the purely deductive nor the purely computational mode of analysis is the correct one. Both have much to offer and both have weaknesses. We will now discuss some of the interesting ways in which computational methods and conventional economic theory interact.

8. Partners in Analysis: Strong Complementarities

We now turn to the many ways in which computation and theory can interact. The first way in which computation and theory interact is as strong complements. The goal of theoretical analysis in economics is to describe the nature of equilibrium. A complete characterization is often impossible. However, theoretical analysis can often provide a partial characterization, which can then be used by computational methods to produce efficient numerical approaches, which then produce economically substantive results.

A particularly good example of this kind of partnership is in the literature on dynamic contracts. Spear and Srivastava[68] studied a moderately general model of dynamic principal-agent contracting under repeated moral hazard. A closed-form solution for the optimal contract was not computed. Initially, the problem appears intractable from both a theoretical and computational view. One can express a contract as a payment conditional on the output history of a worker, but that approach is extremely complex since the history of a worker grows in complexity over time. Both theoretical analysis and brute force computation are able to compute the nature of such contracts only for short horizons.

Spear and Srivastava came up with an ingenious insight which reduced the problem to a one-dimensional, dynamic programming problem. This reduction did not make pure theory much easier, but it drastically simplified the computational problem. At that point, Phelan and Townsend[58] computed example contracts and illustrated many important properties of those contracts.

Theory can be very useful in reducing extremely complex problems to equivalent problems with a much simpler structure. Theory can also prove qualitative properties of the solution, such as differentiability and monotonicity, which can be exploited to develop efficient computational approaches. In this way, deductive analysis which only partially characterizes the solution can be extremely useful if it indicates an efficient computational approach.

9. Setting Priorities: Quantitative Tests for Importance

One potential role for computation in theoretical analysis is testing if the implications of a theory are quantitatively important. This is distinct from empirical work: empirical research asks whether the data from actual economies are consistent with the theory in some precise statistical fashion, whereas computational analyses can indicate if the phenomena being investigated by a theory is important for any plausible parameters of the critical parameters. As noted above, the real business cycle literature frequently evaluates theories on the basis of their ability to match quantitative as well as qualitative features of the real world; however, these exercises also have an empirical flavor in that they are attempting to match a few real world examples.

Numerical examples can more generally help us identify what is important and what is not.For example, consider the theoretical model in Fischer[24]. He investigated a Brock-Sidrauski model of money demand and studied whether it displayed the Tobin effect; that is, whether inflation affected growth by encouraging agents to save through real investment instead of monetary assets. He showed that a Tobin effect existed in that model. However, I once computed the Tobin effect in his model for a large range of empirically reasonable values for the critical parameters in a generalization of Fischer's model and found that increasing annual inflation from zero to one hundred per cent would increase net investment by at most *one-tenth of a percent*. An effect that small would not seem worth studying and would be undetectable in the data. Also, if we found a relation between inflation and growth in real-world data, these quantitative results tell us that the explanation does not lie in the elements of Fischer's model. In light of this, do we consider the Fischer[24] a success? Qualitatively, it does deliver the desired result, but not in a quantitatively significant fashion. This observation leads one to ask if it should even have been published[4], particularly in such a high quality journal? I shall return to this question below.

Another example occurs in the theory of equilibrium with adverse selection. Wilson [74] argued that there may be multiple equilibria in an Akerlof-style adverse selection model. However, Rose[61] showed, via an extensive computer search, that multiple equilibria was highly unlikely when the critical probability distributions were taken from conventional families. He showed that most familiar (and quantitatively reasonable) probability distributions implied unique equilibria, and that only a few

[4] I doubt that this paper is any worse than most papers. In fact, it is the clarity of the writing and analysis which makes it an excellent example to use here.

extreme cases using the normal distribution lead to multiple equilibria.

Papers like Rose's are unusual. The theory literature is full of qualitative analyses which never consider the quantitative importance of their results. These analyses are valuable in providing basic insight and illustrating new concepts. However, many of these papers also claim real-world relevance and proclaim success when their model produces the desired *qualitative* correlation only, and totally ignore the question of whether the analysis is *quantitatively* plausible. Rather little of the theoretical literature is subjected to any such quantitative testing. It will be interesting to see how much of it survives the kind of scrutiny which computational methods now make feasible.

The division of labor is relatively clear. Deductive theory can establish qualitative features of a general theory, whereas computational methods can investigate the quantitative properties of specific instances of a theory. We now ask how these tools are related to our notion of theoretical analysis.

10. Theory as Exploration

As I asserted earlier, theory is the exploration of implications of a collection of assumptions. The claim of this essay is that both conventional deductive analysis and computational methods can, for many theories, explore these implications with equal validity. In this section, we try to understand how conventional deductive analysis and computational analysis are related.

To highlight the relation between deductive and computational methods, I will use the analogy of exploration. Theoretical analysis of a model is similar to exploring unknown geographical territory. In the case of a scientific theory, the "territory" is defined by the definitions and assumptions of the model. How does conventional, deductive theory proceed? Deductive theory usually formulates a proposition. Sometimes, the validity of the proposition can be established without further restrictions; existence results and welfare theorems of general equilibrium are examples of these results. For many propositions, however, the analyst then adds assumptions (such as linear demand, constant costs, etc.) to the basic ones in order to make a proof of the proposition possible. With these auxiliary assumptions, one can prove precise statements concerning the implications of the augmented theory. It is not that we believe that these added assumptions are true, but we proceed in the belief (or, more precisely, hope) that the results we get are actually robust.

Explorers of unknown geographical territory use a similar strategy. In the case of geographical exploration, the initial explorers of an unknown territory do not examine the entire territory nor take a random path, but instead follow a path in that territory which can be easily traversed. For example, one way to explore a region is by floating down (or rowing up) a river which cuts through it. These explorers report how their view of the unknown territory changes as they move along this path. This is a risky strategy since the path is chosen for its convenience and not likely to be representative of the total region. Similarly, deductive theory can sometimes say how the results vary as we move within the narrow, augmented theory. However, the results in this limited examination are not necessarily robust.

In this exploration activity, computational methods have substantial advantages because they can approximately solve an arbitrary example of the basic theory, and determine the nature of any individual point in the territory being explored. The advantage arises because computational methods do not need as many, if any, auxiliary assumptions to compute the equilibrium; computational methods are not restricted to the easily traversed paths. The supposed weakness is that they can do this only one example at a time with error, and in the end can examine only a finite number of points. Computational methods are similar to a satellite: it can take a picture of any location, but the picture may be fuzzy and there is not enough film to photograph each location.

There are many who will argue that computational methods add little to what is available from deductive theory. We shall take up some of these criticisms now and consider them in the context of this exploration metaphor.

Some will argue that deductive theory is better because its results are error-free. Where deductive theory provides an answer then it will provide a more precise answer. The problem is that deductive theory is limited to the small subset of examples which obey the conditions imposed to make the analysis tractable. If one graphed the tractable territory of a theory, the typical picture is that deductive theory can analyze only a piecewise connected continuum of cases within the space of all models, and that these cases are not dense in the space of all models; just like little rivers. Figure 1 displays such a picture for, say, growth theory. One thread is the linear-quadratic cases, another thread is the linear production cum isoelastic utility cases, and the third is the log utility and Cobb-Douglas production case. If one wanted to understand growth and asset pricing issues, then one could restrict the study to these cases, but that would be dangerous and very limiting. While computational methods often involve error, their ability to look where deductive methods fail offers an important advantage. Even a fuzzy picture of unknown territory is more informative than no picture.

Deductive theorists often say that their methods will provide a guide to the important principles which are robust. In fact, the only reason for analyzing the simple cases is the possibility of gaining general insight. This approach produces candidates for robust principles, but one is just guessing when claiming that these principles actually are robust. The results from special tractable cases may be unrepresentative just as the view from floating down a river may not give a true picture of a territory.

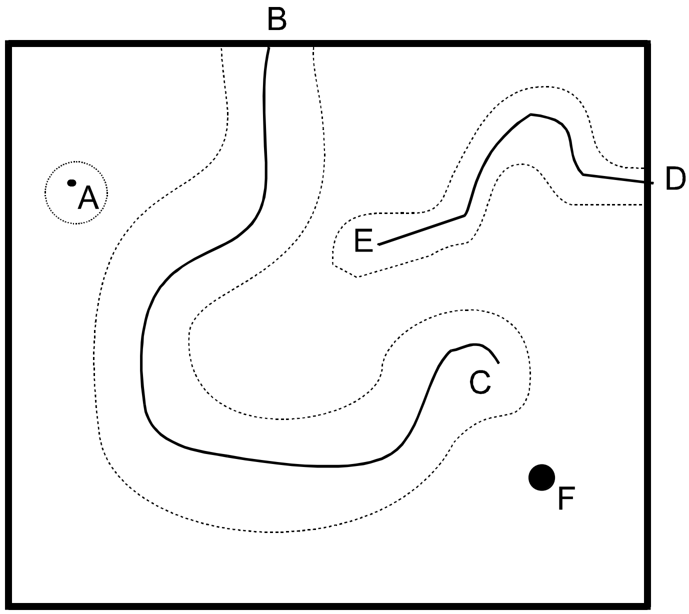

Figure 1: Typical pattern of tractable cases

To establish robustness claims, we need either to tackle the intractable or to use computational methods. In many cases, the supposed basic results break down when analysts begin to examine more general cases. For example, in his work on nonlinear pricing, Wilson[77] argues that the principles of the one-dimensional theory are practically useless when one moves to two-dimensional problems, which he examines numerically.

Theorists may make a great deal about how they can solve whole continuous classes of cases instead versus the finite number of cases which the computational method can deliver. This is an empty claim in many economic theories. In many cases, the important elements of a theory, such as an equilibrium relation between parameters and equilibrium values, are continuous or piecewise continuous functions. Proving such topological properties of models is something at which theory is very good. In cases where theory tells us that these relations are piecewise smooth, the cardinality of the cases deductive theory can analyze is a substantively empty measure of value since piecewise continuous functions can be arbitrarily well approximated by a finite number of cases. It is well known in approximation theory that the best way to approximate continuous functions is to have data at a well-dispersed set of points and that finite collections of well-chosen points can do a very good job of approximating a function even in the presence of error. Knowing a function at each point on a continuum is of little marginal value. Egoroff's theorem supports a weaker formulation of the same assertion for measurable relations. Purely deductive methods are clearly dominant only in the rather exotic case of nonmeasurable relations between structural parameters and equilibrium outcomes, and in such cases one would not dare make the claim that the thread of tractable cases is representative of the general cases.

The excessive simplicity of analytically tractable theoretical models make it imperative that economists explore models which are not analytically tractable. We next discuss distinct ways in which computational methods will help theory move beyond the tractable and focus its energies on important issues.

11. Perturbation Methods: Common Ground

Deductive theorists generally want proofs concerning the nature of a theory's implications. Some computational methods fulfill the demands of the deductive theorist, but are computational methods in that the computer can do the work and the result can be used as numerical approximations. These are called *asymptotic methods*, also known as *perturbation methods*. For many economic models, equilibrium can be expressed as the solution to an equation $f(x, \delta, \epsilon) = 0$ where ϵ and δ are parameters of the model. The equation $f(x, \delta, \epsilon) = 0$ implicitly defines the equilibrium correspondence $x(\delta, \epsilon)$. In particular, ϵ is a parameter such that the equation $f(x, \delta, 0) = 0$ can be solved for x in closed form for arbitrary δ. The result is a parameterized solution set $x(\delta, 0)$ for the special subclass where $\epsilon = 0$. Theoretical analysis will often be able to tell us that the solution manifold $x(\delta, \epsilon)$ for general ϵ is smooth for small ϵ and all δ, but theory may be unable to solve $f(x, \delta, \epsilon) = 0$ for small nonzero ϵ.

In such cases, perturbation methods can often take this smoothness information and compute an approximation of the form

$$x(\delta, \epsilon) \sim x(\delta, 0) + x(\delta, 0)\epsilon^{\nu_1} + x(\delta, 0)\epsilon^{\nu_2} + \cdots$$

for some increasing sequence $\nu_1 < \nu_2 < \cdots$. The case of $\nu_i = i$ is the common Taylor series method, but we are not restricted to that special sequence. The advantage of this method is that any property of the series which holds as $\epsilon \to 0$ holds for the true solution manifold $x(\delta, \epsilon)$ for sufficiently small ϵ. One can thereby solve any case for sufficiently small ϵ and construct proofs concerning the nature of the solution. This is a computational method since the series can be used as an approximation.

Perturbation methods provide a very useful tool for most theoretical applications of quantum mechanics and relativity theory, theories which are generally intractably complex in structure, but relatively easily analyzed through perturbation methods.

For example, the only quantum system which can be solved in closed form is the hydrogen atom. Conventional closed-form analysts would spend their time studying the hydrogen atom and trying (in vain) to find closed-form solutions for other atoms. Perturbation methods are extensively used throughout applied physics and mechanics. Of particular relevance to economics, perturbation methods are frequently used in control theory, nonlinear filtering theory, and statistical theory.

Economists do use these methods, but often in an ad hoc fashion. The quadratic loss function for taxation is an example of such an approximation. The single-good version states that the welfare loss of a tax on a good approximately equals the one-half the product of the square of the tax and the elasticity of demand (assuming constant cost). It is valid for small tax rates, and accurately indicates the direction of tax effects and costs as the revenue needs increase from zero. The multiple good versions of this approximation can be used to solve for the optimal tax structure for small revenue needs. These quadratic approximations also work in practice as good approximations for moderate revenue cases.

Perturbation methods are explicitly used in dynamic public finance.[5] For example, let $C(k, \sigma^2)$ denote aggregate consumption when the capital stock is k and the variance of the productivity shock is σ^2. It is easy to compute the steady state capital stock, denoted k^*, and steady state consumption of the simple one-good representative-agent deterministic growth model with a constant income tax; $C(k^*, 0)$ denotes that consumption level. It is not easy to compute the consumption policy function for general k and general σ^2. Perturbation theory focuses on capital stocks close to k^* and small values of σ^2, and computes the linear approximation of the consumption function

$$C(k, \sigma^2) \sim C(k^*, 0) + C_k(k^*, 0)(k - k^*) + C_{\sigma^2}(k^*, 0)\sigma^2 \cdots$$

where $C_k(k^*, 0)$ and $C_{\sigma^2}(k^*, 0)$ are easily computed derivatives. If one wanted greater accuracy (as demonstrated in Judd and Guu), one just takes higher-order Taylor series.

Figure 1 displays the typical value of perturbation methods. We begin with one of the threads of analytically tractable models. Perturbation theory will then solve models close to those threads, generalizing the analysis to an open set of models around the "measure zero" set of cases solved by analytical methods. Perturbation methods can be used to test robustness of the principles derived in the simple cases which direct theory can handle. This approach can be used in growth models[6], with

[5] See Judd[40] for more discussion and citations.

[6] These methods could be used in RBC theory, but RBC macroeconomists eschew this formal approach, using less general methods. This is clear in the expansion of $C(k, \sigma^2)$ above, which is

and without distortions, in dynamic game models (see Judd[33], Budd et al.[15], Basar and Srikant[69]), sunspot theory (Chiappori et al.[16]), and in asset market analysis (Samuelson[66], Judd and Guu[44]). Brock [13] outlines their use in economic models of complex economic systems with local interactions.

Some think of these perturbation methods as theoretical methods. That may be, but they can be automated. In some sense, theorems concerning local behavior can be computer generated since they follow clear, straightforward applications of basic formulae. Whether we think of perturbation methods as a theoretical tool or computational tool, there value here in theoretical analysis is clear but it is underused in economic analysis.

12. Computational Analyses in the Absence of Theorems

The most controversial use of computers in economic theory would be the use of computations instead of proofs to establish general propositions. One example is not a proof of a proposition; neither does a million examples constitute a proof. However, the latter is far more convincing than one example. Also, what is the marginal value of a proof once we have a million confirming examples? In some cases, that marginal value is small, and may not be worth the effort.

In some cases, there may not be any comprehensible theorem. A problem may have a very complicated pattern of results which defies summarization in a tidy theorem. What are we to do then? The following is a good example of what is probably not an unusual situation.

A paper by Herman Quirmbach[59] is an example of what computation can do and displays what I think will become more common. He asked a very basic and important question in the economics of innovation and antitrust policy. Suppose that several firms can expend R dollars to finance a research and development project which will have a probability of success equal to τ, independent across firms. The successful firms then all produce the new product (patenting is presumed unavailable). The issue is how the market structure and conduct of the post-entry market affects the ex ante R&D effort and net expected social welfare. For example, some might argue that excessive ex post competition will reduce profits among the successful innovators, and discourage ex ante R&D effort. This line of argument may lead to the conclusion that antitrust policy should be lax when it comes to high tech industries. The basic question addressed by Quirmbach is what form of ex post oligopolistic interaction and regulation will lead to the greatest social welfare.

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not certainty equivalent, whereas the approximation developed in Magill and used by Kydland and Prescott[?] and others, are certainty equivalent approximations, and thereby not asymptotically valid. For a more detailed discussion of these issues see Judd[?].

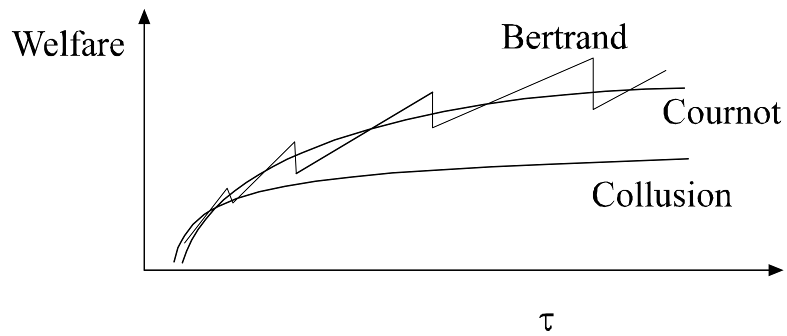

Figure 2: Welfare and market structure

In the typical industrial organization paper, one would make highly specific assumptions for the demand function, for the cost function, and for the specification of imperfect competition. In particular, the typical paper has a finite number of parameters in the model and makes very special functional form assumptions. There is seldom any attempt to generalize the results for general tastes, technology, or mode of competition. Given the finite- and low-dimensional nature of the class of models explored, computation has a chance to be as complete as the typical paper adopting the deductive approach.

Instead of attempting to prove a theorem ranking the ex post market structures, Quirmbach computed the social welfare at a wide collection of values for the critical parameters. Figure 2 displays one of his graphs. The one graph I reproduce here illustrates many critical facts. First, there are no "theorems" if by "theorem" we mean a precise, compact, and understandable statement summarizing the results. Note that each market structure dominates the others at some parameters. It is not even possible to give ranks conditional on τ since Bertrand and Cournot ranks switch due to discontinuities in the Bertrand performance. Any "theorem" which tries to summarize the results just in Figure 2 would be a long, twisted, and incomprehensible recitation of special cases.

Second, despite the absence of simple theorems, there are important and robust findings illustrated in Figure 2 and its companions. Even though we cannot rank the market structures absolutely, it is clear that perfect collusion is usually much worse, and that even when it outperforms Bertrand and Cournot the differences are not significant. Even though the results are muddled, and no simple theorem can summarize these facts, these pictures contain much economic content and clearly reject the argument that collusion should be tolerated because of innovation incentives.

Quirmbach produced many such graphs, exploring various values for the parameter R, alternative demand curves, and alternative R&D games. Other graphs designed to address other questions also showed that there were few if any simple theorems. A total of fifteen graphs were used to illustrate the results of the computations, all presenting economically interesting patterns, but also demonstrating the absence of any general theorems.

I suspect that the true answers to many important questions in economics look like Figure 2. The normal approach of finding a model simple enough to come up with a clean result can do great violence to the real truth.

13. Computational Considerations in Modelling Rationality

My focus so far has been on using computation to solve conventional economic models wherein agents optimize and markets clear. Many have been bothered by the assumption of such perfection on the part of economic agents and markets. In this section, we take a slight detour and discuss how computational ideas have contributed to modelling agent rationality. We discuss this issue here for two reasons. First, it is an excellent example of where computational ideas have played important roles in theoretical analysis. Second, an appropriate resolution of this issue will be important for computational approaches to theory since it will help computational analysts deal with the errors which are always present in computational methods.

Computational ideas can and have contributed to economic theory is by suggesting ways to model the rationality of economic agents. Economists are beginning to investigate the idea that understanding how economic agents with finite computational abilities solve the problems they face may help us in understanding their behavior in complex environments. Various examples of this exist in the economics literature. We next consider the computational ideas which have been used in modelling "rationality" in this section since, first, it is an interesting example of how computational ideas can be used in theory, and, second, notions of bounded rationality will help us interpret and deal with the errors which invariably accompany numerical methods.

Game theorists, beginning with Rubinstein[62], have used the Turing machine notion of computation to model bounded rationality. In this approach, we assume that infinitely rational players choose automata to execute strategies, but we assume that there is a cost to using a large, sophisticated automaton which forces the players to trade-off the computational cost against the payoffs. This idea has received much attention in the game theory literature. However, it is difficult to use these notions in a precise manner when modelling agents in complex environments since Turing machines are not easy to analyze.

Economic theorists sometimes use the concept of the idea of ϵ-equilibrium. Akerlof and Yellen[1] discuss the relation between the agents' ϵ-rationality and the multiplicity of plausible ϵ-equilibria. However, the concept of ϵ-equilibrium is confusing from an operational point of view. A key feature of most economic problems is that each agent must solve an optimization problem. The basic assumption in ϵ-equilibrium is that an agent cannot perfectly solve his problem but will make some choice which yields him a payoff within ϵ of the best possible choice. Upon reflection, this is somewhat confusing: how can an agent be assured of getting a payoff with ϵ of the optimum payoff without knowing that maximal value? Such an agent is using some algorithm to solve his problem. There is no such algorithm available in the numerical analysis literature, forcing us to ask what algorithm can this agent be using which assures us that he will choose only such points?

An improvement to the ϵ-equilibrium concept may be based on numerical analysis notions of computation. For example, a player in a game must solve an optimization problem. Instead of asking which actions would put him within ϵ of the best possible payoff, we could ask what he may choose if he used a standard optimization method. If the problem is concave then most optimization methods would stop when it found a point where the marginal benefit of changing his choice is below some critical value. This may be similar to the conventional ϵ-equilibrium, but there would be a difference depending on the curvature of the objective. However, if the player's optimization problem had multiple optima then a player may have the same difficulty in finding the best point as does a maximum likelihood econometrician. By assuming that the agents actually use standard optimization algorithms and using the stopping sets of such methods be our predictions of behavior, we avoid the epistemological difficulties of the conventional ϵ-equilibrium concept. This strategy has been used; Marimon et al.[**?**] study economies wherein agents use genetic algorithms to solve their problems.

Rational expectations theorists have used the notion that agents' abilities to form conditional expectations are limited to particular forms of regression, and then compute rational expectations equilibrium given this limitation. Examples of this include Anderson and Sonnenschein[4], Allen[2], and Marcet and Sargent[53].

We should note the different ways in which the computational ideas have been used and analyzed, and distinguish between computational and deductive methods which arise in these models. Rubinstein created a model in which the agents behaved as automata, a computational idea modelling behavior, and then proceeded to analyze the resulting theory in a deductive fashion. Anderson and Sonnenschein assume that agents use regression in forming their expectations, again a computational approach to modelling behavior, and then proceeded in a deductive fashion to prove existence. The theoretical examples mentioned above made theoretical assumptions concerning the way in which economic agents use their information and compute their response to economic stimuli. In contrast, Marimon et al. assumed that agents used particular numerical method to solve their optimization problems, formulating another theory of bounded rationality, and then studied that theory by simulating the resulting model, thereby using a computational approach to analyzing a theoretical model of behavior. Therefore, the literature contains examples of both deductive and computational analysis of theoretical models of bounded rationality. Incorporating computational ideas into a theory does not mean that one must analyze the resulting model in a computational manner.[7]

14. Numerical Error: Estimation and Interpretation

Any approximation scheme has some error; that is a fact of life in computational approaches. We need to be able to control that error and it would be desirable to have some economic interpretation of that error.

Many numerical methods have desirable asymptotic properties. Typically these properties do not produce a bound on the error, but just a statement that if the algorithm uses a size parameter h then the error is proportional to h^k for some $k > 0$. This can be difficult to interpret except for the infrequent case where one has a good estimate of the proportionality constant. Economists will often face a choice between using slow methods which have good asymptotic properties, and alternative procedures which are fast and typically produce results which are ϵ-equilibria for small ϵ, but have no known good asymptotic properties. Even if one has a convergent scheme, one must still choose a stopping criterion since we cannot wait for the infinite sequence to converge.

These considerations of bounded rationality, numerical error, and stopping criterion lead to some important conclusions. Since it is the convergence criterion which defines what is an acceptable end, it is not clear why one demands convergent methods. Instead, one can just compute a candidate approximation and then check to see if the candidate satisfies the stopping criterion. Since it is the stopping criterion which determines when we end our search, it is there where we can impose our notion of ϵ-equilibrium. I call this approach "compute and verify".

The connection between bounded rationality and numerical methods is one which will be increasingly important. My claim is that in many economic models and computational methods, the numerical error can be related to the optimization error of agents. In Judd[37], I focussed on the magnitude of the Euler equation error, a criterion commonly used in the stopping rules of optimization algorithms. The result allows one to use reasonable assumptions about agent rationality to set standards

[7] I should also make clear that I am not here trying to define what is and is not computational economics. In particular, I am not saying that research which uses deductive methods to analyze models of bounded rationality is not computational economics. I just want to make clear the distinction between computational assumptions about economic agents in theoretical models and the use of computational methods to analyze theories.

for the acceptability of numerical approximations. For example, if an approximate equilibrium strategy has a consumer making optimization errors of a dime per dollar of expenditure, then the approximation is unacceptable if one believes that people do better; if the implicit optimization errors are a penny per thousand dollars, then the approximation is acceptable if you doubt that people can do better.

At this point, I will go way out on the limb and make a radical proposition: once we have agreed to a notion of ϵ-equilibrium, existence theorems may not be necessary. It is not generally possible to prove the existence of an equilibrium through computational means, but numerical demonstrations can be sufficient to prove that a candidate equilibrium is an ϵ-equilibrium. Since agents in the real-world economy are likely to make mistakes, the best we may ask of them is ϵ-optimality. Therefore, if we can computationally prove the existence of ϵ-equilibria through the construction and analysis of numerical approximations, the value of existence theorems is reduced.

There are many issues raised by moving to this kind of analysis. In particular, there are generally a multiplicity of $\epsilon-$equilibria. Some would dislike this multiplicity. I would argue that the size of the set of $\epsilon-$equilibrium is interesting for small ϵ since the size of that set indicates the ability of our theory to predict outcomes of interactions among real people. This supposedly problematic feature of $\epsilon-$equilibrium concepts can really be turned around and used to investigate new economic issues.

15. A Computational Approach to Analyzing a Theory

In the examples above, I have emphasized how computational methods can examine a wider range of models than analytical theory can plausibly analyze. Any analysis, deductive or computational, consists of two steps: first, determine the facts about the theory, and, second, express them in intelligible form. In the case of the deductive approach to theory, the strategy is to find sufficient conditions under which some proposition is provable, and then express that proposition in the form of a theorem. The theorem could just list the results of the analysis in various instances, but that is inelegant. The deductive mode attempts to find a simple, elegant proposition which is true under simple conditions.

A major problem with computational theory is that while the first step of finding facts is not difficult since it consists of solving various instances, the second step of expressing those facts is very difficult. The kind of tabular and graphical expressions displayed above can be used for low-dimensional models, but when one leaves a two-dimensional universe, such tools are difficult to use without excessive consumption of paper and a reader's time.

In this section, I offer various schemes which could lead to modes of numerical investigation and relatively easy exposition. I will lay out an approach which ties together the common deductive methods with the computational methods. I do not claim that this is the typical approach used; in fact, the numerical literature has no such general strategy. I argue that the strategy below allows us to exploit numerical tools to analyze theories and economic issues in ways which satisfy the legitimate methodological concerns of sceptics. Some will find the strategy and its requirements too demanding, and think that this is a proposal which would deter, not encourage, the use of numerical methods; others will find it an attempt to disguise the logical flaws of numerically intensive research. The main objective here is to get us to think seriously about where numerically intensive research fits into the economic literature.

In the discussion below, we let the generic "Proposition P" be a statement about the model. It could be a comparative static, a statement stating the relative size of two quantities in equilibrium, or another kind of statement usually found in a theorem. For the purposes of specificity, suppose that proposition P is "The optimal consumption tax results in greater social welfare than the optimal income tax." To investigate this proposition we would like to vary at least the revenue needs, the social welfare function, the technology, and heterogeneity in tastes and endowments. For the purposes of this discussion, we will assume that there are computational methods which can determine P's truth in any specific case.

15.1. Find Examples with Closed-Form Solutions. The first step in analyzing any theory should be determining cases with closed-form solutions, if that is at all possible. Sometimes these examples may be quite trivial. For example, in discrete-time dynamic games, if we assume that the discount factor is zero then the dynamic game is just a succession of static games. Budd et al.[15] and Judd[33] begin with the closed-form solution to a dynamic game where the payoff was zero, a rather trivial instance. One may have more substantial cases with closed-form solutions. For example, linear-quadratic examples can be solved and treated as essentially closed-form solutions since the error can be reduced almost to machine zero. There may also be special cases with solutions. The result of these special cases will be the threads of tractable cases in Figure 1. In the case of our proposition P, the special case of zero revenue needs and identical agents is easily solved since both the optimal consumption and income tax policies which raise zero revenues impose zero taxes and reduce to the same competitive equilibrium.

15.2. Perturbation Methods around Tractable Cases. After theory produces simple analytically tractable cases, one can measure the robustness of the properties of these cases by applying perturbation methods to determine what happens in "nearby" cases. In our example, we could take the zero revenue, representative agent case and perturb the revenue needs and add a small amount of agent heterogeneity. The tractable examples may be silent on some important issues, whereas the perturbation methods may tell you interesting answers nearby. Perturbation methods begin with points on the tractable threads in Figure 1 and effectively "fatten" them since it gives us information about points near those "threads." The broken lines in Figure 1 display the results of this "fattening" process, representing the expanded class of models for which we have essentially closed-from solutions and which we now understand.

15.3. Test Numerical Methods on Tractable and Perturbed Cases. Since tractable cases are isolated and perturbation methods yield only local information, we need to examine other cases if we are to obtain a robust picture of our theory. However, before computing these other cases, we can use the analytically tractable cases and the perturbation results as cases on which we can test the reliability of candidate numerical methods. By testing possible numerical procedures out on these cases, we will have a better idea as to their numerical errors and speeds, help us choose among these possibilities, and will allow us to fine tune the chosen methods to attain the desired accuracy in these test cases. Such information is likely to be informative when applying the procedure to other cases. This also gives us more reason to find tractable cases and perform perturbation calculations.

We are now ready to conduct a more global analysis. The following methods could be used to generate and summarize global information concerning the validity of proposition P which can complement the local information produced by the tractable examples and perturbation methods.

15.4. Search For Counterexamples. Ultimately the special cases and the local analyses are exhausted, and we must move to more global methods of evaluating the question at hand, that is, the truth of some proposition P. The next logical step is to search for counterexamples[8]. In our example, we would form the function SW expressing the social welfare of the optimal consumption tax minus that from the optimal income tax, and feed SW to a global minimization algorithm. This global optimization approach will implicitly produce strategies to find counterexamples to the hypothesis that $SW > 0$ always. The results from the perturbation analysis may indicate which directions are most likely to produce cases where $SW < 0$.

Searching for counterexamples via optimization routines may be a good method to test proposition P, and failure to find a counterexample strong evidence for proposition P. If we want to make a case for the general validity of proposition P, then we need to focus on finding counterexamples. If there are counterexamples, the optimization approach is designed to get to them quickly, ignoring confirming examples.

[8]Scott Page suggested this step to me.

However, the lack of a counterexample is difficult to express. If one reports this failure, the reader (journal editors and referees, in particular) may worry that the search procedure was not competently executed or that the optimization procedure was not appropriate for the problem. If we fail to find a counterexample, we need to consider cleaner ways to express the apparent global validity of proposition P.

15.5. Monte Carlo Sampling. Once one is fairly convinced of a proposition's truth, then one wants to express that in some compact way. Monte Carlo sampling can produce results which are easy to report in either classical or Bayesian fashion.

The first procedure for a computational theory I will describe involves the computing numerous examples of a model and then using statistical inference language to summarize the finding[9]. Suppose that one wants to investigate a set of parameterized models of a theory on which one has imposed a probability measure, μ. Suppose we want to evaluate our proposition P. We could draw N models at random according to the measure μ, and use computation to determine the truth of the proposition in those cases. If computation showed that proposition P held in each case, then one could say "We reject the hypothesis that the μ-measure of counterexamples to proposition P exceeds ϵ at the confidence level of $1-(1-\epsilon)^N$." Note the crucial role of the randomization; the fact that we randomly drew the cases allows us to use the language of statistical confidence.

One could also use Bayesian methods to express his beliefs after several computations. Let p be the probability that a μ-measure randomly drawn point satisfy proposition P, and suppose that one has a uniform prior belief about the value of p. Then one's posterior belief about p after N draws which satisfy proposition P can be directly computed.

The advantage of Monte Carlo sampling methods is the ease of expression, and little question about meaning since independent draws are easy to implement and well-understood. The ability to express the results in both classical and Bayesian ways make it easy to communicate the result.

15.6. Quasi-Monte Carlo Sampling. Some have told me that they would prefer to use a prespecified, uniform grid instead of random draws. This approach would be more efficient since it would avoid the clumping and gaps which naturally occur with Monte Carlo sampling[10]. The disadvantage of any deterministic sampling

[9] This is an idea which I outlined in [**?**], have discussed with several colleagues, and which many others have proposed. Despite the fairly wide discussion of this idea, I am unaware of anyone actually implementing this approach.

[10] Monte Carlo sampling is ex ante uniform, but ex post it is most likely locally normal by the Central Limit Theorem.

method would be the inability to use statistical language to express "confidence levels." The alternative expression would be the maximal size of a ball or cube of counterexamples; that is, if proposition P is true at each point on a grid and the largest ball which can miss each point on the grid is of diameter δ, then δ could be used as a measure of the strength of proposition P. Sometimes we could do better. Suppose that the proposition is "$f(x,\mu) > 0$ at equilibrium values of x in model μ." It may be possible to show that f_μ is bounded by M. Then if we can, via computation, prove the truth of proposition P on a grid with mesh $\delta < 1/M$, then we actually have a proof of Proposition A for all models μ.

15.7. Regression Methods. Often, the results of computations will be similar to the Quirmbach example cited above, that is, there is no definitive result and the patterns we find are complex. In those cases, we need other ways to analyze and express the results. The graphical approach in Quirmbach is one way, but it is limited by dimensionality. In problems with higher dimensions, one could use curve fitting methods, such as regression, to express one's findings. In the Quirmbach case, we could draw a random number of models and fit a probit expressing the probability that one market structure dominates the others, or we could regress SW against the model's parameters. Since the objective is to find SW as a function of the parameters, we could use approximation theory and choose a collection of points which are optimal in terms of fitting such functions.

The main point is that approximation and regression methods could be used to summarize the results of a computational study of a theory. As long as the topological analysis of the model indicates that the function of interest is smooth (or, piecewise smooth at least) then we can implement the appropriate approximation method to fit the surface.

16. The Problems Facing Computational Economics Today

I have been discussing the potential of computational theory. I suspect that few of these ideas are new, as is indicated by the many examples I have cited. The question is why computational theory is not exploited more fully. Papers like this one generally focus on the unrealized and unappreciated value of computational methods. It is common to blame the rigid methodologies adopted by journals and others in the profession. However, economists who use computational methods are often their own worst enemy. Part of the problem is that computational economics has not yet developed the standards, the discipline, and the coherent core of techniques which characterize other subdisciplines in economics, such as econometrics. Computational economists often do not take their computations seriously, inviting others to also discount them. These problems make it easy to criticize much of what passes for computational economics. Any balanced consideration of computational economics should face up to the problems which exist, and develop solutions. In this section, I will focus on these problems[11].

First, many of us are guilty of poor scholarship regarding both the relevant mathematical and prior economic literature. There is a tendency to use standard mathematical terms in ways inconsistent with the standard mathematical concept. This alienates a natural audience for the work; if the mathematically well-educated reader is alienated, then how can we expect others to value and respect the work. Economists working in one area make little effort to learn what has been accomplished in other areas. For example, in our paper on computing rational expectations equilibrium, Bizer and Judd[10], we ignored the relation between the methods we used and the earlier work in Agricultural Economics by Gustafson, and Wright and Williams, work which did appear in standard economics journals and was well known among rational expectations economists. Journal editors and referees are equally poorly informed, seldom enforcing minimal standards of scholarship. Ignorance of past work makes it inevitable that the wheel is frequently reinvented, often with "innovations" tantamount to trying a square wheel, and keeps the field from truly advancing. This poor scholarship makes it much more difficult for good work to disseminate properly and for a coherent literature to form.

Part of this scholarship problem is that journals do not take computational work as seriously as they do empirical or theoretical work. Theoretical papers (i.e., both economic theory and econometric theory), must contain the proofs of any proposition. In many cases, the techniques used to prove a theorem are as useful and interesting as the actual theorem. Empirical papers must clearly indicate the statistical procedure used and their properties. Similarly, the empirical procedure used in an empirical paper is often its most valuable contribution. In both cases, journal editors and referees not only permit but insist on full disclosure of the details. Experimental work is also allowed to publish the details of experiments. The treatment of computationally intensive and innovative work is very different. Computational economists are often told to limit severely, if not eliminate, the discussion of their methods, even when those methods are as interesting as the paper's economic content. I expect that the *Journal of Computational Economics* and the *Journal of Economic Dynamics and Control*, with their declared interest in computational methodology, will help to reduce this problem in the future, but their existence is no excuse for bad policies at other journals.

[11]Exact citations of the "sins" discussed in this section serve no purpose in this context. Also, specific citations would have the unfair feature of pointing to writers who were clear enough in their writing so that their sins are clear.

A second (not unrelated) problem is that many economists who use computational methods know little about computing. Recently I was at a conference where a presenter, a well-regarded professor at a well-regarded department[12], discussed how difficult it was to solve his problem, how his solution method (commonly used in his subfield) took a long time to converge, and that it was therefore unreasonable to ask that he make a more refined calculation. After the presentation, a couple of conference attendees discussed these problems with him. We first pointed out that he was essentially solving three smooth equations in three unknowns, where each equation was easy to evaluate. After he agreed to that, we then asked him why he did not use Newton's method to solve the problem[13]; his blank stare told us that he had never heard of Newton's method for solving nonlinear equations!

This is not limited to a few, bad papers. Numerically intensive papers often contain assertions which are inconsistent with the mathematical literature. Sometimes these assertions are even contradicted by the literature they cite. My favorite pet peeves are "only Monte Carlo methods can be used for high-dimensional integration", "to compute a linear approximation to a nonlinear stochastic control problem you take a linear approximation of the law of motion and a quadratic approximation of the objective at the deterministic steady state," and "you cannot generally interpolate data with smooth approximations and impose shape (concavity, monotonicity) restrictions." These three statements are often made explicitly, and are more often implicit in the techniques authors choose to use. They are all misleading today, were known to be misleading even twenty years ago, often lead to inefficient methods, and in many economically relevant contexts are just plain wrong.

A third problem is that computational economists are often sloppy concerning the reliability of their methods and accuracy of their results. At a recent conference, I pointed out to an author that his results were unreliable since the two algorithms he used were both very ad hoc and that he had not really done anything to convince us of their reliability. His response was that he had demonstrated their reliability since the answers of the two methods were very close. We then looked at his tables to find that the first method produced an answer of 2 and the other produced an answer of 4. Apparently "close" to him just meant "the same order of magnitude", a standard far weaker than we usually expect of numerical procedures and a standard which would surely alienate many economists.

Another example of sloppiness often occurs in the use of Monte Carlo-based simulation solution methods. Many rational expectations methods use realizations of

[12] Any further identification would be inappropriate since this was by no means a unique incident.

[13] He was instead using the iteration $x^{k+1} = g(x^k)$ and variations thereof to solve the fixed point $x = g(x)$ where $x \in R^3$.

random number generators in their algorithms. The result of the computation is then a random variable. Sometimes this is exploited usefully, as in the case of estimation methods which use simulation. In fact, one of the advantages of Monte Carlo methods in simulation methods is that one can fold numerical error into the estimate of the standard error. In any case, it is recognized that the Monte Carlo simulation produces random errors whose approximate magnitude must be reported. Unfortunately, it is more typical for the non-econometric uses of such methods to just report the computed result for one sample, give no report of the standard deviation of the random result, and, when challenged, claim, without documentation, that the variation is trivial. In one such case, I got the authors' program, changed the seed in their random number generator, and reran their computations. The new results were at least two per cent different and in many cases twenty per cent different from the answers the authors computed. Standard practice in the empirical literature insist that standard errors be reported with point estimates; the same should be demanded of Monte Carlo-based computational methods, and of any application where comparable concepts are available[14].

The combination of poor scholarship, poor grasp of basic computational methods, and sloppy standards combine to make computational economics look bad in the eyes of those aware of the problems, and invite disaster by risking embarrassingly bad results. These problems are interrelated. Ignorance of efficient methods leads to inefficient programming which is incapable of meeting high standards. The unwillingness of journals to publish the computational details of a paper seriously impedes dissemination of critical computational ideas. The lack of a full discussion of computational methodology makes it impossible for peer review to impose the usual discipline. The result is that many refuse to take the computational results seriously unless the computational results just serve to illustrate theorems.

These problems are not present equally in all branches of computational economics. More mature areas have worked out these issues. The difficulty is that the standards which work, for example, in computable general equilibrium (CGE), may not work or apply to, for example, computational methods for rational expectations models. Also, the extensive mathematical training which the typical CGE economist has is not adequate for solving rational expectations models. The development of a common, general core of techniques and a common language, such as is done in econometrics, is a task which would help greatly.

[14] I mention the Monte Carlo case because it is one which economists can easily understand. Similar demands can be made of most numerical methods since procedures to estimate errors are often available. For example, conditioning numbers serve an analogous purpose in linear and nonlinear equations, and asymptotic error bounds exist for integration methods. Reporting these diagnostics can go a long way in answering criticisms and revealing problems.

While I do believe that the skepticism often expressed towards computational work can be successfully addressed, this skepticism is valuable. Any new tool or approach will be viewed with skepticism, and that is as it should be. Otherwise, we would be whipped by constant motion from one new fad to another. Only those new tools and approaches which can successfully meet such skepticism deserve to be adopted as part of the core of economic methodology. One of the problems in computational economics has been little consideration of how to address the skepticism. The expositional tools and research strategies discussed above are suggested patterns of research and exposition which help one to communicate across methodological differences.

Of course, bad practice exists in all areas of economics, and standards must be reasonable, tuned to what is practical. Unfortunately, there has been little discussion of these issues, little effort in teaching graduate students a core of basic methods, and little effort to set and enforce standards. Computational economists have some housecleaning to do. I suspect that we do not agree on what those standards should be and what is appropriate graduate training, but it is clear that we can do better. Only when computational economists begin following serious standards of scholarship will computational methods be taken more seriously.

17. Computational Economics and Future Technology

The trends in computational methods and power are all promising for the kind of approaches described above. First, computational speed is increasing at a steady rate. Better yet, computational costs are declining even faster. Some might argue that we are fast approaching the limitations of the silicon-based technologies relied on in current computing — the etchings can be only so small. This is not likely to be a problem. We could go to other materials which allow even smaller components and faster speeds. The other direction is three-dimensional chips. Beyond that are optical switching methods, and, possibly, quantum mechanical computers. There is little doubt that computational speed will continue to rise at a rapid pace.

The new directions in supercomputing are also beneficial to computational economics. Supercomputing, as in the Cray series, used to mean vector processing, a mode of computation which economists often found difficult to exploit. The new mode in supercomputing (also called high performance computing) is massively parallel and distributed computing. In these environments, many moderately powerful processors are networked and together solve a problem. In massively parallel machines these processors are all in one computer, whereas distributed computing is the strategy of linking several computers in a network to cooperate on solving a problem. The power of such computing structures depends on the problem. Some problems are not easily decomposed into subproblems. Fortunately, most of the methods discussed above can easily make full use of the computational power of such systems.

These items concern the development of faster machines. Equally important are the improvements in the algorithms available to solve problems. It is not generally appreciated that, for many problems, there has been as much progress in software in the past forty years as in hardware. This is particularly true for the multidimensional problems which naturally arise in models of uncertainty, information, and risk. There is no reason to think that progress in numerical analysis will slow. This is particularly true in many areas of computational economics where standard practice is decades behind the frontier of the numerical analysis literature. Even when computational economists catch up with the frontier, it is plausible that economists will push out that frontier in directions particularly useful to economists.

The combination of advances in hardware, computer organization, and software all indicate that computing power available for computational economics will continue to increase dramatically in the near future. Luckily, the nature of computational theory is such that it will be able to efficiently exploit these advances. The result will be dramatically faster computing, far beyond current practice.

18. An Economic Theory of Computational Economics

Being economists, we believe that the evolution of practice in economics will follow the laws of economics and their implications for the allocation of scarce resources. The objective of economic science is understanding economic systems. Theories and their models will continue to be used to summarize our understanding of such systems, and to form the basis of empirical studies. We have argued that the implications of these theories can be analyzed by deductive theorem-proving, or they can be determined by intensive computations. The inputs of these activities include the time of individuals of various skills and the use of computers, either as word processors or number crunchers. Theorem-proving intensively uses the time of highly trained and skilled individuals, a resource in short supply, whereas computation uses varying amounts of time of individuals of various skill levels plus the use of computers.

The output of economic research will continue to be used to guide decisionmaking by governments and firms, and train students. Many of these end-users care little about the particular mode of analysis. If a million instances covering the space of reasonably parameterized models of a smooth theory all follow a pattern, most decisionmakers will act on that information and not wait for an analytical theorist to prove a relevant theorem. In the absence of a proof, most will agree that the computational examples are better than having nothing. Most end-users will agree that the patterns produced by such computations are likely to represent general truths and tendencies, and form a reasonable guide until a conclusive theorem comes along.

The picture drawn here is one where alternative technologies, deductive analysis and intensive computations, can produce similar services for many demanders. Economic theory tells us what will likely happen in such a circumstance. In the recent past, the theorem-proving mode of theoretical analysis was the efficient method; computers were far less powerful and computational methods far less efficient. That is all changing rapidly. In many cases, the cost of computation is dropping rapidly relative to the human cost of theorem-proving. I anticipate that in a the next decade, it will be typical for an individual to outline a theory, describe it to his desktop computer, and, in matter of days, have the computer produce a summary of the results it found after working through an computationally intensive analysis. The clear implication of standard economic theory is that the computational modes of theoretical analysis will become more common, dominating theorem-proving in many cases[15].

Does this make deductive theory obsolete? Absolutely not. In fact, as discussed above, the presence of computational methods *raises* the value of some kinds of deductive analysis. Proving existence theorems, deriving the topological and analytical properties of equilibrium correspondences, and finding efficient ways to characterize equilibrium will all assist in the computational step. Even the ability to come up with special cases with closed-form solutions will be useful in giving the computations a beginning point. A computational approach to theory may alter the relative value of particular types of deductive analysis, but does not reduce the value in general.

19. Complements or Substitutes?

At the outset, I posed the question "are computational and theoretical methods complements or substitutes?" As is typical of economists, my answer is a resounding and clear "both." In some activities, they are clearly complements with their complementary strengths and weaknesses indicating that they can be very successful as partners. Deductive theory is necessary in reducing an economic question to a finite set of mathematical expressions which a computer can then analyze to produce economically useful results. The greater the analytical knowledge we have of a model, the better we can do in developing computational methods for solving instances of the model, and greater computer power allows the investigation of more general and complex models. Also, numerical examples can help the analytical theorist in determining the likely quantitative importance of various features of a theory.

On the other hand, computation can also be, and will sometimes be, a substitute for deductive theory. First, computation can inform us of patterns which analytical theory would have great difficulty discerning or expressing. Second, it may be cheaper to use computationally intensive methods instead of theorem-proving to analyze a

[15]This section owes much to and freely borrows from a George Stigler talk on the mathematization of economics. While less dramatic, the computerization of economics may be similar in terms of how it affects the style, emphasis, and allocation of effort in economic research.

theory; given likely technical improvements in computing, this controversial direction has great potential for growth if it becomes accepted.

Whether complements or substitutes in specific activities, theory and computation should never be viewed as enemies in the general development of economic understanding. Computation cannot achieve its potential without the use of theory, and theory will become increasingly dependent on computation to answer theoretical questions and guide it in directions of greatest economic value. The ultimate focus of these discussions should be on what is good for economic science. Clearly, economic science will thrive best by harnessing the power of both theory and computation.

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