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Economics Imperialism and Solution Concepts in Political Science

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Abstract

Political science and economic science . . . make use of the same language, the same mode of abstraction, the same instruments of thought and the same method of reasoning. (Black 1998, 354)

Proponents as well as opponents of economics imperialism agree that imperialism is a matter of unification; providing a unified framework for social scientific analysis. Uskali Mäki distinguishes between derivational and ontological unification and argues that the latter should serve as a constraint for the former. We explore whether, in the case of rational-choice political science, self-interested behavior can be seen as a common causal element and solution concepts as the common derivational element, and whether the former constraints the use of the latter. We find that this is not the case. Instead, what is common to economics and rational-choice political science is a set of research heuristics and a focus on institutions with similar structures and forms of organization.

Keywords

economics imperialism; rational choice; political science; equilibrium; unification

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Introduction

Economics imperialism refers to the application of (mainstream) economic methods to the study of phenomena outside the traditional domain of economics. Uskali Mäki (2009) has recently presented a typology of different aspects of economics imperialism in this journal. *Economics expansionism* is "a matter of persistent pursuit to increase the degree of unification and full consilience provided by an economic theory by way of applying it to new types of phenomena." *Economics imperialism* "is a form of economics expansionism where the new types of explanandum phenomena are located in territories that are occupied by disciplines other than economics." (Mäki forthcoming)[1] For many of its proponents, economics imperialism is thus a matter of unification. Mäki also argues that economics imperialism is in principle justifiable precisely because of this unificatory potential. However, he distinguishes between two kinds of unification, derivational and ontological. *Derivational unification* is a matter of deriving large classes of explanandum sentences from a parsimonious set of theoretical sentences or inference patterns. *Ontological unification* involves redescribing large classes of apparently independent explanandum phenomena as forms or manifestations of a common system of entities and causes. Mäki argues that derivational unification has value only if it is accompanied by ontological unification, and that the requirement of ontological unification should therefore be seen as a constraint on economics imperialism.

In this article we take a philosophy of science perspective on an important and prominent example of economics imperialism, the rational-choice branch of political science. Standard economic models rely on some solution concept (equilibrium) for deriving the implications of the model and rational behavior as an apparently common causal postulate. Equilibrium might thus qualify as a common derivational element, and rationality as a common causal element in all rational-choice models. The following quotation from Duncan Black shows that practitioners of rational-choice political science consider economics and political science intimately related to each other through the shared concept of equilibrium:

> The most important feature of the economic mode of abstraction is that the individual is represented by his schedule of preferences. . . . The main other instrument in the two sciences is the conception of equilibrium. In politics the question which should be asked is: What are the

[1]Mäki proposes using the term "economics imperialism" instead of the more common "economic imperialism" because it makes clear the difference between the imperialism of theoretical approaches and the imperialism of nations or capitalist market economy. We adopt Mäki's terminology because it would be confusing to discuss his account as an account of "economic imperialism."

characteristics of political equilibrium in particular cases (Black 1998, 356-59).

Given that many rational-choice theorists themselves (e.g., Coleman 1990; Olson 1990) have been enthusiastic about the approach precisely because it is taken to promise unification of the social sciences, we will attempt to evaluate economics imperialism qua unification. The question to ask is thus whether unification, in either of its forms distinguished by Mäki, is plausible or legitimate in the case of analytical political science, and whether successful unification would have some relevant epistemological implications on which proponents or opponents of rational-choice theory (RCT) may rely in their assessments of economics imperialism. We will seek support in answering such normative questions through the examination of its actual forms. We will thus explore whether and how solution concepts might qualify as common derivational elements, and rationality or self-interest as common causal elements, in economic applications outside the traditional boundaries of economics. Our specific research question is thus the following: does thinking and theorizing in terms of similar equilibrium concepts in two domains of phenomena require that there is something similar in the causal bases of the phenomena, as Mäki seems to suggest? The spatial theory of voting, particularly the closely related work on cyclic preferences and disequilibrium in political science, will be considered as a case. This literature is also compared with that on general equilibrium theory in economics.

According to Mäki, unification should be of the ontological kind if it is to be regarded as genuine scientific progress, i.e., the unified fields of study should share the same causal basis. Economics imperialism should thus not be a matter of arbitrarily extending the scope of application of a given set of theoretical tools. It should also be constrained by the requirement of similarity between the causal bases of economics and of the phenomena under study in the field to be conquered. We argue that ontological unification has not been a constraint on derivational unification in practice, at least if the constraint is understood as requiring similarity in the underlying causal basis of diverse phenomena. What can be said to be common to economics and plausible applications of economic methods in political science is a focus on certain structural features of the choice situations and forms of organization inherent in some social institutions.

Mäki has provided a useful characterisation of economics imperialism as unification. We take issue with him concerning the idea of a constraint, however. The problem is with *applying* the ontological constraint. We propose that there are two (not mutually exclusive) ways of applying the idea of an ontological constraint, the first of which could be called causal and the

second structural. In the context of economics imperialism, the former requires that if the individuals are not really rational in a substantial sense, rational-choice models should not be used because the causal basis supposed by the theory is false. The structural constraint requires that economic models should be applied to topics outside the traditional domain of economics only when the new field might reasonably be expected to exhibit such general structures to which the economic tools can be taken to refer. Mäki does not propose precise standards for applying the constraint but we think that at least the idea of having a common causal basis seems to be too strict: the mere fact of the lack of such common basis should not give rise to the judgment that an imperialistic model is somehow inadequate. The problem with the structural version of the constraint is the converse: it is so flexible and imprecise that it fails to rule out any conceivable research program. Insofar as the ontological constraint can be brought to bear on economics imperialism, it goes very little beyond general, noninstrumentalist criteria for theory assessment.[2]

Before setting off, it is necessary to specify what we take economic methods to be. Mainstream economics is not identical with neoclassical economics given that it nowadays seems to include behavioral and experimental economics as well as some new institutional approaches. Furthermore, even neoclassical economics does not have precise boundaries. Frank Lovett (2006) defines RCT as follows: it posits actors that make consistent and rational choices when rationality means acting according to some solution concept. Given that a large number of distinctly economic contributions include these assumptions, a research practice that employs these assumptions in new domains can be said to apply economic methods.

Peter Ordeshook has argued that political science has *not* been the subject of economics imperialism, and one of his reasons for making this claim is that no substantive theorems have been imported from economics (Ordeshook [1992], see also [1993] and [1995]). As we will see, what has been imported is a set of methods and mathematical techniques. However, we think that Ordeshook is using the word "imperialism" in an overly strict sense. If using a distinctly economic (or rather, game-theoretical) style of reasoning, theory structure, and solution concepts in a new field of research does not count as economics imperialism, then what does? It does not seem likely that *theorems* with substantive content related to specific phenomena could have been transported from economics to political science. If the degree of detail in the substantive content of the theorems is specific enough,

[2]It is not surprising from this point of view that Mäki's other two constraints, the epistemic and the pragmatic, seem to provide conditions for good theories in general. It is of course a further issue whether all economics imperialists adhere to conditions for good theories "in general."

it becomes extremely unlikely that the theorems could hold in both economic and political contexts.[3]

The structure of the article is the following. The concept of unification is discussed in Section 2, while Section 3 defines solution concepts and discusses their role in economic models. We present our case study in Section 4 and Section 5 concludes the article.

Two Faces of Unification

The ideal of scientific unification is widely considered one of the most important intellectual driving forces behind the phenomenon of economics imperialism. Unification is a stronger notion than unity of knowledge in the sense of compatibility or coherence. It embodies the idea that there is something intrinsically virtuous in explaining or accounting for as much as possible with as little as possible. From Kant and Whewell to logical positivism and beyond, this kind of parsimony has been seen as a virtue intrinsic and essential to the progress of science. Therefore, to understand and evaluate the imperialistic project, one has to have a firm grasp of the nature and epistemic credentials of unification. Following the lead of Uskali Mäki, we distinguish between two basic forms of unification: ontological and derivational.

Ontological unification amounts to the reduction of the number of entities or phenomena accepted as basic. There are, however, different characterizations and variants of this general idea. According to Friedman (1974), explanatory unification is a matter of reducing the number of facts accepted as primitive or brute; as science progresses, more and more facts are derivable from ever fewer facts and our system of knowledge grows more organized and secure at the same time. However, as forcefully pointed out by Kitcher (1981), there are fundamental problems with this kind of reduction. Mäki characterizes ontological unification as the process of realizing that different classes of phenomena are manifestations of a single or a few causes. Physics underwent a major unification when light and electromagnetic phenomena were seen as manifestations of the very same causal force. In the case of economics imperialism, ontological unification is usually at least supposedly achieved by showing that social institutions or patterns of behavior result from individual rationality.

Derivational unification proceeds by integrating *the way* in which we make sense of the world. After dismissing Friedman's proposal because it

[3]It is clear that even such somewhat broadly applicable theorems as the first theorem of welfare economics and Coase's theorem cannot be applied, mutatis mutandis, in political science.

involves dubious fact counting, Kitcher (1981, 1989, 1993) framed his account of explanatory unification in terms of the use of the same *argument patterns* to derive descriptions of a wide range of phenomena. These argument patterns are sets of argument schemas accompanied by filling instructions that specify what sorts of entities are to be plugged into the schemata. The conclusions of the arguments are descriptions of phenomena within the extant body of knowledge. As theoretical or explanatory progress, unification amounts to maximizing the number of conclusions derived from a minimal number of argument patterns together with their *stringency*, i.e., the strictness of their conditions of application.[4] The use of these argument patterns is a multistage process that Kitcher calls derivation. This process is supposed to account for the cognitive element of understanding that is missing from simple deduction, which is constitutive of the old covering-law model.

This view, which stresses the syntactic or logical form of the unifying theory, the constancy in the style of argumentation, and the importance of derivation, seems to fit nicely with the practice of orthodox economic theorizing. The familiar "intellectual toolbox" of economics could naturally be conceived of as a set of argument patterns applicable to a wide range of problems. Gary Cox emphasizes the fact that equilibrium analysis proceeds according to fixed methodological rules that ought not to be violated.

> . . . all the rules and regulations, especially those involving the core concept of equilibrium, evolved precisely to deal with threats to the internal consistency of reasoned action and interaction, so that nose-to-the-grindstone application of these rules can supply the defect of insight. (Cox 1999, 158)

This nose-to-the-grindstone application is an expression of the idea that equilibrium concepts should be used within a set of fixed argument patterns. As a first approximation, the broad outlines of the core unifying argument schema of economics could be seen as something like the following.

- *Question*: What are the characteristics of equilibrium in this and that institutional setting? (Why is the allocation of goods $S_1, \ldots, S_n$ such and such?)

[4]The stringency requirement is tailored to respond to some well-known counter examples of the covering-law model of explanation as well as some specific problems of the unification account.

- *Answer*: Postulate a model that contains a set of agents with a specific rational preference structure, and a set of institutional, legal or other constraints within which these agents are to make choices.
- Players' choices have to simultaneously satisfy some mutual rationality/consistency condition, which is formalized as a *solution concept*.
- Given the preferences, the solution concept provides a constraint for the possible solutions. If the solution constraint implies a single equilibrium outcome the aggregate-level outcome can be understood as a deductive consequence of it.

We will consider this argument schema again in discussing our case.

What is the relationship between the ontological and the derivational strands of unification, and should either one of them be seen as some sort of progress? As pointed out above, Mäki claims that derivational unification is acceptable only if it is accompanied by ontological unity. To assess this claim, we need to investigate what cognitive or epistemic virtues unification is supposed to have in the first place. Friedman and Kitcher originally intended their theories to be theories of scientific explanation. Unless one is ready to embrace extreme instrumentalism, the view of unification as constituting explanation dictates the required correspondence between derivations and ontology: one may follow Kitcher in advocating a Kantian metaphysics according to which we project a causal structure on the world according to our explanatory practices. Here ontological unification is taken to follow necessarily from derivational unification. If one has more realist leanings, one may follow Mäki in requiring that legitimate theoretical unification should proceed only when the underlying ontology allows it.

However, explanatory progress may not be the essence of unification. Unificationism as a philosophical theory of explanation faces serious difficulties that need not be addressed here in their entirety.[5] However, even though unification as such does not *constitute* explanatory progress, it may contribute to it in a number of ways. Another virtue considered by Friedman is epistemic warrant: more unified theories are more likely to be true. This idea is familiar from Whewell's discussion on the notion of the consilience of inductions, and Mäki also uses it as a *prima facie* reason in favor of economics imperialism. However, as Margaret Morrison (2000) documents in great detail, the unification promised by the theories in the major unificatory feats in physics and biology[6] did not in itself provide any reason for believing in the truth of the theory. Instead, unification was usually a byproduct of heuristic analogies and the use of similar mathematical techniques. The actual

[5]See Barnes (1992), Humphreys (1993), Morrison (2000), and Woodward (2003, chap. 8).

[6]These include Maxwell's electromagnetic synthesis, the Weinberg-Salam model, and the modern synthesis in biology.

make-or-break factors for the acceptance of the unified theories were mathematical convenience or the purely empirical results of new experiments. Morrison thus claims that unification in itself carries no epistemic value, at least according to the scientists. In fact, she goes even further. The main thrust of her argumentation is that successful theoretical unification has no implications for fundamental ontology or metaphysics. Spectacular examples of ontological unification like that of Maxwell's electromagnetic theory are the exception rather than the norm.

The diversity of possible relevant evidence is greater for a more unified theory than for single disjoint theories with a jointly similar scope, and the unified theory might therefore be taken to be better supported by the evidence. However, it is not clear whether it is the unification, the variety or novelty of the evidence, or some other factor that is responsible for any "increase in confirmation." There are some Bayesian analyses that purport to establish a link between unification and higher posterior probability, but the concept of unification used in these arguments bears little resemblance to any intuitive notion of explaining much with little (see Schupbach [2005]).[7] Perhaps the liveliest discussion on unification concerns it as a model-selection criterion that increases the predictive power of the model by guarding against overfitting a model to a single sample (this idea is based on the Akaike Information Criterion). However, even the proponents of such views are careful in admitting that the instrumentalist goal of predictive accuracy is separate from and independent of the goal of finding the true theory (Forster and Sober 1994). Whatever the cognitive virtues of unification are, they seem to have little to do with explanation and the jury is still out on whether it has anything to do with confirmation or truth.

One consequence of the lack of connection between explanation, evidential value, and unification is that it becomes less clear why ontological and derivational unification should go hand in hand. Morrison has emphasized that there are many ways in which unification could take place, and that theoretical unity need not be, and indeed in most cases is not, accompanied by reductive unity in the fundamental matter the theories supposedly concern.[8] Morrison concludes that unification usually results from the use of similar mathematical techniques, and that any underlying unity resides in the perceived common general structures or systemic properties in apparently diverse phenomena. Her position on unification thus straddles instrumentalism and unbridled realism; identity in the causal basis of the phenomena to be unified is not required, but there should be some common structural features or forms of organization in the unified domains.

[7]For example, these studies posit that common-cause structures are necessarily disunifying.
[8]Note that Morrison's concepts do not map exactly to the concepts of ontological and derivational unification as they are used here.

What can we now make of Mäki's (2001) suggestion that ontological unification should be imposed as a *constraint on economics imperialism*? The requirement of ontological unification is essentially a realist attack against the commonplace instrumentalist rhetoric used by many economists and game theorists (see MacDonald [2003]): unification should be a matter of empirical truth, not of arbitrary preferences over mathematical modelling techniques. We specify the causal basis of economic phenomena in terms of the assumption of rational behavior mainly because it seems to correspond to the way rational-choice theorists conceptualize causes:

> Physical and social equilibria have different properties. Physical equilibria occur when forces balance one another so that a process repeats itself or comes to rest. . . . What must be balanced in the social sciences are choices of actions—that is, intentions, which are thus analogous to physical forces. (Riker 1990, 176-77)

Sometimes the methodological requirement of ontological unity in the causal basis seems to motivate theory evaluation among rational-choice theorists. If this was not the case, it would be difficult to understand why such an enormous amount of scholarly attention has been lavished on the so-called paradox of turnout. Anthony Downs (1957) argued that rational voters should not go to the voting booth since the expected benefits in terms of the influence on policies because of a person's vote are minuscule, and there are some costs involved in the act of voting. The problem in terms of RCT is that people do go to the ballot box.[9] The question of why people vote voluntarily would certainly not be subject to such wide scholarly interest if it did not provide an example of a phenomenon for which it has proven to be impossible to derive an explanation that is based on self-interest. It provides a counter-example to the possibility of thoroughgoing ontological unification in terms self-interest as the common causal basis.

There are, however, two different notions of rational choice. One involves *consistency* of choices, and the other *self-interest* (Sen 1985). The former has also been called *thin rationality* or *rationality as consistency* (e.g., Ferejohn and Satz [1996]), and the latter *thick rationality* or *substantive rationality*. Thin rationality does not really explain individual action but only describes it in a systematic way. On the other hand, rationality as self-interest is an *assumption* in empirical models that specifies what is assumed to motivate the individuals in the model.[10] The paradox of voting is problematic only in terms of the thick conception of rationality, and not of the thin one.

[9]See, e.g., Blais (2000) and Brennan and Lomasky (1993).

[10]The self-interest assumption may take many different forms and always requires additional arguments to be operationalized (Kavka 1991; Lehtinen and Kuorikoski 2007). These arguments cannot be derived from any grand philosophical theory.

Let us now reconsider the thesis of ontological unification as a causal constraint. Our working hypothesis is that substantive rationality or self-interest is the most obvious candidate for a unifying causal basis in rational-choice models. It seems natural to require that models that do require and depend on substantive self-interest assumptions, i.e., thick rationality, should not be used when people do not act consistently with their self-interest. In contrast, utility maximization is not a plausible candidate for being a common causal basis because it does not yet involve any causal assumptions. If some people's behavior is described as if they were maximizing expected utility, this tells us nothing about why they act that way. This is why we do not count appeals to thin rationality as possible instances of causal unification: as-if maximization is not a sensible candidate for being a causal basis for anything. Rational-choice theorists do not interpret "maximizing" as a mental operation, but rather as a description of consistent behavior.[11] Mere maximizing is thus not causal because it is "formal," i.e., it lacks empirical content.[12]

The question to be asked is thus whether unification (in political science or economics) is a matter of applying the assumption of substantive self-interest in ever new circumstances. On a superficial analysis this would seem to be the case, since many applications of economic models do assume self-interest. On the other hand, ontological unification as the requirement of a common causal basis could function as a constraint on economics imperialism only if substantive self-interest were a *necessary* ingredient of rational-choice models. However, it is not. Rational-choice models may be used to model any kind of motivation. Furthermore, in many applications, e.g., most studies of voting, the content of the preferences simply does not matter. Indeed, this may be the main reason why these models have been so immensely successful in conquering new fields of research. Thin-rationality accounts allow for derivational unification without a concomitant underlying ontological unity in the causal basis of the behavior.

[11]Indeed, maximization could not be understood as a mental notion because according to expected utility theory, it is meaningless to say that a person *tries* to maximize utility. Utilities are numbers that describe a person's preferences. The meaninglessness thus derives from the logical form of the notion of utility in this theory. It makes no sense to say that a person tries to maximize a number.

[12]"In our view, game theory . . . should be regarded as a purely formal theory lacking empirical content. Both theories merely state what will happen if all participants have consistent preferences and follow their own preferences in a consistent manner—whatever these preferences may be. Empirical content comes in only when we make specific assumptions about the nature of these preferences and about other factual matters" (Harsanyi 1966, 413-14).

Solution Concepts

What economics brings to a field of study is a particular style of argumentation accompanied by a reconceptualization of phenomena and a shift in explanatory focus. All these facets are particularly clear in the pride that economists take in the fact that their discipline incorporates "solution concepts" or equilibria (e.g., Lazear [2000]). In this section we provide a preliminary discussion for our case by exploring the theoretical role of solution concepts and the different ways of conceptualizing the key notion of equilibrium.

At first glance, equilibrium seems to be a property of a causal system. A system is in equilibrium if the causal forces affecting it balance each other out. Usually equilibria are also thought to be stable in that they would return to their initial state, or to a new stable state, after the causal forces affecting them have been perturbed. Walras (1954 [1874-77], 109) compares an equilibrium to a suspended body of which the center of gravity lies directly beneath the point of suspension. If this center of gravity were to be displaced from this vertical line, it would automatically return to its original position through the force of gravitation. When equilibrium is understood as a property of a causal system, it is quite obviously an ontological feature of the world and independent of our ways of conceptualizing and explaining it.

However, in economics and political science equilibria are conceived of not as balances of forces, but as mutual consistency of plans, choices or strategies (see the quotation from Riker above).[13] We use the plural here because there are several equilibrium concepts in game theory, economics, and political science. In contrast to the natural sciences, where equilibria usually involve straightforward physical forces or selection pressure, it is not always clear in the social sciences how the equilibrium should be defined and which equilibrium concept should be used. There is always some leeway in modelling the players' decision-making processes. Indeed, the ad-hoc use of equilibrium refinements has been considered a problem with RCT models (Diermeier 1996, 68). All game-theoretic models are based on expected utility maximization since the payoffs are interpreted as von Neumann-Morgenstern utilities. Solution concepts add a further characterization of rationality, and this may be somewhat different with different solution concepts.[14] The most commonly used equilibrium concept in rational-choice political science is that developed by Nash. A set of strategies constitutes a Nash equilibrium in a game if no individual has an incentive to *change* strategy, assuming that all other players use their equilibrium strategies. As Riker acknowledges above,

[13]Giocoli (2003) traces the transformation of the image of economics from a science concerned with systems of forces to one concerned with systems of abstract relations.

[14]In some cases, two different solution concepts yield conflicting predictions for a given configuration of preferences; see, e.g., Grüne-Yanoff and Lehtinen (forthcoming).

this definition is still somewhat similar to the idea of a system returning to its initial state in that it refers to the absence of incentives for, and thus the absence of, a change in equilibrium.

We can now distinguish between two conceptions of equilibrium, the derivational and the ontological. A *solution* is a specification of the strategies that the players are predicted to use, and it is derived from a *solution concept*. It is also common to use the term "equilibrium concept" for this derivational concept of equilibrium. While an equilibrium may be interpreted ontologically, a solution concept refers only to derivation. Solution concepts can, in principle, be applied without ontological commitments if their only job is to assure that modelling assumptions are logically coherent, and that they imply a well-defined result or prediction. Aumann (1985), for example, describes a solution concept as "a way of organizing in a single framework many disparate phenomena and many disparate ideas" (pp. 34-35). Correspondingly, Mäki takes the application of a similar solution concept to be a prime example of derivational unification.

Rational-choice models proceed by deriving predictions and explanations from postulates concerning individual behavior and some constraints on their choices. The role of solutions is to provide the necessary deductive connections between the behavioral postulates and the predictions of RCTs. In other words, rationality postulates are *usually* insufficient in themselves for deriving predictions (Arrow 1986).[15] In political science the optimal decisions of the players usually depend on the other players' strategies, and equilibrium concepts are needed. Indeed, they are even more important in political science than in economics because in some important cases, economic models may assume that individuals consider other people's actions as given. Solution concepts resolve the infinite regress of conjectures that would result from the simple application of constrained maximization to strategic contexts by imposing additional assumptions on the players' beliefs. Rational-choice models are usually not constructed by eliciting the preferences and beliefs of the relevant individuals, and calculating their expected utilities on the basis of this information. It would, in principle, be possible to do so, but usually the solution concept can be used to alleviate the problem of getting to know what the individual preferences and beliefs are. Solution concepts may be used to derive *conditions* for the players' beliefs and thereby provide a solution.

[15]We emphasize the word "usually" here because it is not impossible that rationality postulates are sufficient for deriving predictions. It is, of course, possible in some cases to specify a model in which the players' preferences are defined for some choice options. If the players have dominant strategies, or if we can specify their beliefs in addition to their preferences, deriving a prediction simply involves calculating the players' expected utilities (e.g., Harsanyi [1966]).

Equilibrium concepts provide the link between individual preferences and aggregate-level outcomes. They allow for *aggregating* individual choices (strategies) into collective-level outcomes, without necessarily knowing all the details about the individual choices. Since RCT is predominantly based on methodological individualism, it is easy to understand why the notion of equilibrium is of such paramount importance in economics and rational-choice political science. The primary explananda of equilibrium models are thus aggregate-level properties, such as stabilities and functional relationships between distributions of properties (allocations, prices, votes, alleles, for example). Indeed, some rational-choice political scientists have argued that political science and economics study the *same kind of relationship*: that between individual preferences and macro-level outcomes (Riker 1983, 47). In principle, it does not really matter for this particular explanatory task whether the individual behavior doing the explanatory work is itself explained (as intentional action)—i.e., whether the rationality in the model is thick or thin. Equilibrium is a form of organization, itself independently characterizable regardless of the causal basis realising it. It is a paradigm example of a general systemic property. As such, and given Morrison's observations, theories based on equilibrium solutions could be thought of as natural candidates for drivers of unification, not only in a purely instrumentalist derivational sense but also in the sense of tracing similar structural features, or at least the possibility of similar structural features, in domains that were previously thought to be distinct.

Equilibrium in Political Science

We will now study the role of equilibrium concepts in political science by discussing the well-known literature on preference cycles and chaos. The purpose of this case is to show that although the research practice in political science does not fit with all the details of our characterisation of the rational-choice argument schema, a common form of equilibrium theorizing does underlie the research efforts. Furthermore, the way in which the research heuristics in political science differ from the argument schema is highly similar to the way in which they do so in general equilibrium theory (GET) in economics. The research heuristics in GET and in the theory of political disequilibrium will thus be compared. We find this comparison interesting because if the two theories were literally correct, they would imply that there is a general equilibrium in markets but no corresponding equilibrium in political institutions.

By GET, we refer mainly to the literature (stemming from Arrow and Debreu [1954]) that explores conditions under which an abstract economy of multiple sectors could even in principle be in equilibrium (no excess demand), i.e., whether a certain kind of mapping from a commodity space unto itself has a fixed point and whether these fixed points are stable. Hausman (1984) distinguishes these abstract topological exercises from computational or partial equilibrium models that can be applied to real economies. It is almost universally admitted that the conditions for the existence of a general equilibrium are unlikely to be realized in the real world (e.g., Hahn [1973]; see also Hausman [1984]). For example, we know that there are externalities, that individuals do not have perfect knowledge about the commodities and that there are no efficient futures markets for all commodities. Existence proofs for general equilibria are perhaps best seen as consistency proofs for the standard neoclassical modeling assumptions, or, as Hausman puts it, conceptual assurance that the general economic modeling framework is at least on the right track. We concentrate on abstract GET, because the studies done in political science seem to share the same intellectual flavor and outlook. One important role of this theory is to provide a framework for specifying the conditions in which the state should interfere with the workings of the free market. The strategy is thus to see *how* the conditions of the existence theorems are *violated* in the real world, and to offer normative advice for what should be done about it. The theory of general equilibrium is thus important not only because it shows that free markets will provide Pareto-optimal allocations in certain conditions, but also because if and when these conditions do not prevail, it could be taken to imply that the market will *not* provide such allocations.

Let us now move into questions of equilibrium in voting. It has been known since the end of the eighteenth century that majority rule is vulnerable to *cyclic preferences*. The so-called Condorcet paradox provides an example of a preference cycle. Assume that three voters A, B, and C have preferences for alternatives X, Y, and Z: let R_i denote individual i's preference relation. The paradox occurs when XR_AYR_AZ, YR_BZR_BX, and ZR_CXR_CY. X has a majority over Y, Y over Z, and Z over X, but no alternative has a majority over all the other alternatives. The majority preference relation R thus includes a cycle: XRYRZRX. An alternative that has a majority over all other alternatives is called a *Condorcet winner*. The notion of a Condorcet winner is closely related to the game-theoretical equilibrium notion of the *core*. In general, the core is the set of allocations such that no coalition of individuals has an incentive to change the allocation, given that all other coalitions play their equilibrium strategies.[16]

[16]The difference between the core and the Condorcet winner is just that if some coalitions (or individuals) are *indifferent* between one alternative and another, one may belong to the core even though it is not a Condorcet winner.

Positive political theory and social-choice theory have studied the possible consequences of the fact that there may be no core under majority rule and therefore no equilibrium. Some well-known contributions have shown that the problem is not restricted to the mere possibility of no equilibrium: it is rather that disequilibrium is likely to prevail. Plott (1967), for example, showed that the existence of a core is highly unlikely if the number of alternatives is large. Various contributions show that if each preference ordering is equally likely (this is the "impartial anonymous culture" assumption), it is very unlikely that there is a Condorcet winner and thereby an equilibrium.[17] McKelvey (1976) and Schofield (1978) showed that if there is no equilibrium, the preference cycle spans the whole outcome space in the sense that there is a path from any point in the space to any other that could be reached by a sequence of majority votes. These results are now widely, albeit misleadingly, referred to as "chaos" results.

These chaos results were instrumental in making clear that something is wrong with the theory of political disequilibrium. Casual observation and laboratory experiments (Fiorina and Plott 1978) indicated that real-world social choices do not cycle in the way that the theory seemed to imply. "Why so much stability?" Gordon Tullock (1981) asked, because there seemed to be a discrepancy between the chaos "predictions" of the theory and the observation of stability in the real world. Given that Tullock evidently took "stability" to mean lack of change, he seems to have thought that the theories dealing with cyclic majority preferences predict endless changes in the policies adopted. The theory thus seemed to explain phenomena we have no reason to believe really exist (e.g., Murphy [1996, 167]). This perceived incongruence between theory and observation prompted two kinds of responses. The first seeks to explain the observed discrepancy, but the second denies that the theory says anything about the world in the first place.

According to textbook philosophy of science, an ordinary scientific theory should be discarded if it is falsified by the evidence. Some authors, however, have suggested that it is questionable whether the chaos theorems predict anything at all (Austen-Smith and Banks 1998), and therefore whether the casual observation of stability constitutes evidence against the theory. It is difficult to specify what these theorems say about the real world (Hall 1995). It is not evident that the literature on cyclic preferences can be taken to predict actual cycling in legislatures. The main reason for this is that the theory does not really contain an explicit account of how preferences are supposed to give rise to behavior, i.e., there is no explicit theory of individual behavior. Furthermore, chaos theorems assume, among other things, that the

[17]See, e.g., Gehrlein (2002) for references.

agenda-setter has complete information on voters' preferences, and the power to single-handedly determine the voting order. These assumptions are so wildly inaccurate that it is virtually sure that the conditions assumed in the theorem will never be exemplified in the real world. It is nowadays widely held that instability and impossibility theorems do not predict anything in any particular voting situation (e.g., Huber [1996, 40]). Some authors have suggested that they demonstrate how badly any "minimally democratic social preference relation" can behave (Austen-Smith and Banks 1998) or provide "a core theoretical structure for integrating" first principles (Ordeshook 1993, 76). This line of argumentation is similar to the interpretation of general-equilibrium theories in economics: the value of GET lies in the fact that it provides a conceptual framework and a "baseline" theory for an integrated study of the whole economy.

Despite this, there are plenty of authors who think that the theorems have important implications for the functioning of democracy, and for the right interpretation of democracy in general (see Riker [1982], Coleman and Ferejohn [1986], and Mackie [2003] for discussions). Cyclic preferences have been associated with strategic voting, path-dependence, and agenda-setter's excessive power. The contributions that try to determine how common are preference cycles typically do not specify how exactly the cycles translate into strategic voting, etc. As Hammond (1987) once stated, social choice theory is the "science of the possible"; it has concentrated on studying theoretical possibilities the relevance of which is unknown, and in some cases known to be irrelevant. Similar assessments have been made about the role of GET in economics.

Whether one believes that real-world voting situations are realistically characterized by preference cycles is important in terms of how one explains the observed stability in legislatures. Two different *kinds* of argument have been presented. Some authors maintain that most voting situations are characterized by only one dimension so that the preferences are usually single-peaked (e.g., Niemi and Wright [1987]). However, others emphasize the fact that real-world distributions of voter types are so far from the assumption of an impartial anonymous culture that Condorcet winners exist more often than theoretical investigations into impartial cultures have suggested (Regenwetter, Adams, and Grofman 2002; Tsetlin, Regenwetter, and Grofman 2003; Regenwetter et al. 2006). These explanations deny the idea that preference cycles are common in the first place.

The literature on the *structure-induced equilibrium* (SIE)[18] takes cyclic preferences more seriously, taking the ubiquitous existence of cycles as its

[18]See Shepsle (1979) and Shepsle and Weingast (1981, 1984).

starting point. The basic idea of SIE models is that although we observe relatively stable voting results, this is not because the underlying preference profile does not contain cycles, but rather because the constraints and procedures of voting in the parliaments create stability. This viewpoint is consistent with the idea that voting outcomes are not necessarily the best alternatives, because the process by which the votes are taken may be path-dependent.

Riker (1980) argued that chaos results mean that *anything can happen* in politics, and that the results of voting institutions are intrinsically *unpredictable*. Those who took his interpretation of chaos theorems seriously thought that structure-induced equilibrium explained why political decisions made in parliaments did not seem to be part of a neverending cycle. Indeed, Riker (1980) himself emphasized that the results merely showed that there was no "equilibrium in tastes." There is an *institutional equilibrium* (see Shepsle [1987]), however, in the sense that given the constraints provided by the institutions, no agent has an incentive to change her strategy.

Riker concluded that the science of politics rested on a dubious basis because the chaos results implied the impossibility of prediction—a conclusion that was immediately contested by various authors. Ordeshook (1980) argued that the results merely concerned one particular solution concept (the top-cycle set), and that there could be other solution concepts that implied stability. Aldrich and Rohde (1982) suggested that the absence of equilibrium in tastes was not methodologically devastating for political science because knowledge of individual tastes would have given a misleading picture of the outcomes of the political process in any case. They argue that the liberals thought the political institutions worked systematically in favor of the Republicans in the 1960s. If these liberals were correct, "knowing where the preference equilibrium was in the system would have provided incomplete and misleading predictions about policy outcomes" (p. 94). In other words, Aldrich and Rohde claim that mere knowledge of preferences would not have been sufficient for predicting voting outcomes even in the absence of cycling problems because institutional factors may systematically distort the political outcomes.

These comments bring us to an important aspect of derivational unification between economics and political science. Riker's concern that political science is doomed to failure because of the chaos theorems can best be understood if he is assumed to take our rational-choice argument schema seriously. He takes the task of political science to be predicting or explaining *voting outcomes* on the basis of knowledge concerning individual preferences. The problem, for him, is that we cannot predict voting outcomes on the basis of

knowledge concerning individual preferences because of the chaos results. He thus takes the *explanandum* to be the observed outcomes, and the *explanans* the preference profiles. We hesitate to say, however, that rational-choice theorists really have been motivated to explain voting outcomes on the basis of preferences in specific situations. After all, given the chaos theorems, we know that this could not be done, even if we knew the individual preferences, without some information on the institutional details and the behavioral dispositions of the voters.

The standard conception of the structure of a rational-choice model is that once we have the initial conditions, i.e., the individual preferences, and the solution concept, an aggregate-level consequence can be deduced. Despite appearances, however, this is not how rational-choice modeling proceeds in our case study, or more generally. In particular, even though it is always possible in principle to find out about individual preferences, this is often practically very difficult (e.g., Chong [1996]), and is usually not done. Perhaps the main reason why the equilibrium derivational pattern is so tremendously popular is that it often allows the rational-choice theorist to draw interesting conclusions despite *not* knowing the individual preferences. This may be because an equilibrium model may (a) allow for deriving a prediction for virtually any configuration of preferences, or (b) require only a very broad characterization of the individual's incentives, or (c) may use random preferences for a comparative institutional analysis. Examples in political science include the median voter theorem (a), Duverger's law (b), and computer simulations under various voting rules (c).

Rather than really predicting or explaining outcomes in specific situations, rational-choice theorists have been mainly motivated to evaluate the functioning of various voting *institutions* normatively.[19] There are also two interconnected senses in which the chaos theorems and the SIE could be viewed as explanatory rather than normative. On the one hand, insofar as the theorems imply that voting would be chaotic or cyclic forever in the absence of rules and procedures, the existence of these procedures explains why we do not observe cycling. Van Mill (2002) gives credence to this kind of analysis by presenting an empirical test of some procedures that SIE models investigate. He shows how an institution, the American Continental Congress (1774–1789), which almost completely lacked the procedural constraints under which current parliaments operate, was indeed prone to instability: the same issue (the location of congress) was brought into the

[19]There is, in fact, a well-known discipline within political science, the study of *voting behavior*, which attempts to predict and explain voting outcomes. This discipline has been predominantly driven by behaviorist methods rather than rational choice.

discussion time after time. On the other hand, the underlying instability of majority rule explains why some current procedural rules are in use. Huber (1996) uses this strategy to explain the existence of some parliamentary procedures in present-day France.

These two examples illustrate the fact that, despite appearances, the argument pattern for equilibrium explanations discussed earlier in this article mis-specifies the *explanandum* in a number of RCTs in politics: it stated that given the preferences, the aggregate-level outcome could be understood as a deductive consequence of the solution concept. We have seen that the *explanandum* in the first explanatory case is the *falsehood* of the deductive consequence of the equilibrium model, the lack of chaos. In the second case, the *explanandum* is the existence of some voting institutions. The existence of these institutions is not a deductive consequence of the equilibrium model either. The role of equilibrium analysis here is that it provides a background theory in an account of institutions that obtains its epistemic credentials from functionalist or selection arguments (Krehbiel 1991, 28-29; Miller 2000).

GET has been used for analogous analyses. The structure of these analyses is the following. Given that we know the way in which the abstract institution-free model is unrealistic, the existence of institutions can be explained by appealing to the need for them (hence the functional tone of some of these analyses). Thus the standard economic explanation for the existence of the state is that this institution is needed because completely free markets would not provide an optimal amount of public goods, it would not take into account externalities, and so on. Few think that GET actually explains prices or price stability. In any case, the well-known Sonnenschein-Mantel-Debreu (SMD) results establish that a postulation of a general equilibrium does not in any way restrict the aggregate behavior (price movements) of the economy. Thus GET cannot be used to draw any macro conclusions from aggregated micro-level data.[20] As in the literature on disequilibrium in political science, the role of GET could (perhaps a bit charitably) be seen as providing a theoretically rigorous default contrast or baseline, against which empirically more meaningful research questions can be formulated.

The most obvious difference between voting theory and GET concerns the very existence of a theoretical equilibrium and the role this question plays in the larger theoretical context. In both cases a large amount of effort has been expended on specifying the conditions for the existence of an equilibrium. The GET contributions characteristically consist of proofs that provide

[20]See Rizvi (2006) for an account of the more recent and related literature on the "equilibrium manifold," and Kirman (2006) for an elucidating interpretation of the SMD results.

sufficient conditions for the existence or stability of a general equilibrium, whereas contributions to abstract political theory characteristically consist of proofs showing that there is no equilibrium. While economic equilibrium putatively explains price stability, political disequilibrium putatively explains chaos. A comparison between GET and abstract political theory thus shows that if equilibrium is viewed as an ontological element, the two fields display disunity, but if we look at their derivational elements they seem to be similar in the sense that similar (or the same) equilibrium concepts are used. What is common to both fields is that the question of the existence of equilibrium is largely viewed as a question of consistency in the basic modeling assumptions in both fields.

Even though the equilibrium-argument patterns seem to have become ubiquitous in political science, the ways in which they are used vary a great deal. As repeatedly stressed above, the conclusions derived from the core argument pattern (i.e., the putative explananda) are not the only things that are of interest. For example, in the institutionalist program, the argument schema is used by varying the game form and seeing "how and why" institutions matter. Moreover, the equilibrium argument schema is also used without any explanatory ambitions concerning the normative appraisal of possible political institutions.

It is reasonable to model the institutional constraints into the *game form* and to keep the behavioral postulates fixed (Diermeier and Krehbiel 2003, 129). Methodological unity in assuming fixed behavioral dispositions is thus necessary for such comparative institutional analyses irrespective of the (dis)similarity of the actual causal factors. What has crossed borders is a general style of theorizing, not the mechanical application of fixed argument patterns in the derivation of descriptions of empirical phenomena. However, there is a minimal shared ontological commitment implied by the use of equilibrium theorizing: the systems to which the equilibrium models are being applied should be such that it makes sense to ask whether and under what conditions they are in equilibrium (in a causal sense).

Our conclusion is that whatever ontological unity is present in the case of rational-choice political science, it is not similarity in the underlying causal basis of behavior but similarity in the highly abstract structural features and forms of organization that economic and political institutions could be conceived as sharing. In both cases, it at least makes sense to ask whether economic or political systems could, in principle, be in equilibrium, and then use the answer as a default contrast or baseline in the formulation of more advanced research questions. As an example of what we mean by a shared structure or a form of organization tracked by an equilibrium concept, let us consider

Hotelling's (1929) model of spatial competition in one dimension. The basic idea of this equilibrium is that if the agents that try to sell products or obtain votes can reasonably be arrayed on one dimension, it is always in their interest to locate themselves in the middle of the one-dimensional continuum. In economics the model may be applied to firms' location decisions, and in political science to the positions of the candidates in the political left–right landscape. It is clear that the intuitive and/or empirical credibility of this model may vary from one context to another, but it depicts similar structural characteristics—the consequences of strategic location choices in a one-dimensional world—irrespective of the context in which it is used.

Although we do not consider causal and structural commonality as mutually exclusive alternatives, it may be worth noting that some mathematical forms may track common structural characteristics that have nothing to do with causes. Consider, for example, the mathematical form underlying the so-called 'effective number of parties' (Laakso and Taagepera 1979). Let q_i denote the share of votes of party i, i=1,2, . . . ,n. The effective number of parties is then $\sum_{i=1,n} (q_i)^2$. This particular form goes back at least to the Italian statistician Corrado Gini, and it has been applied to model some kind of homogeneity in a large number of seemingly unrelated contexts such as the inequality of the income distribution (Gini 1912), biodiversity, and industrial concentration (the Herfindahl index) and even the "uniformity of the world" in general (Carnap 1952, 65-68).[21] Although the causal reasons for the differences in realized homogeneity vary from one context to another, the structure to which this mathematical form refers is, in some essential sense, always the same.

What does this mean for Mäki's idea that ontological unity should be seen as a constraint for derivational unification? On the one hand, the ontological constraint could be used if substantial assumptions about the content of self-interest were being made in imperialistic applications. However, at least in the case of analytical political science, this is not always the case. Moreover, applying the constraint would simply amount to saying that one should not posit false theories. On the other hand, derivational unification via the use of similar equilibrium concepts does require minimal structural similarity in the systems under study, but this structural unification concerns properties that are so general that it does not really make sense to use it as a criterion of theory assessment: economies and democracies share the potential for similar equilibrium structures, but so does pretty much everything else under the sun. We propose instead that what should be at issue is whether looking at

[21]See Patil and Taillie (1982) for further references and examples.

these particular properties is likely to constitute a progressive research program with potential societal relevance.

Conclusion

We find Mäki's characterization of economics imperialism as explanatory unification to be broadly appropriate. However, we have discovered that it is difficult to use his formulation of ontological unification as a constraint for deciding which imperialistic endeavors are to be considered acceptable. The problem with using self-interest as a common causal basis is that it is not actually necessary for all RCT models. Using similar equilibrium concepts does not require that the things thought to be in equilibrium should be of the same (causal) kind. We do not wish to argue that the lack of unity in the causal basis implies a complete lack of imperialism: it may well make sense to talk about imperialism on the basis of similar research heuristics and forms of derivation.

If imperialism in the case of analytical studies of politics boils down to the use of certain argument schemas with little regard to the strictness of the corresponding filling instructions, one might ask if there is anything specifically economic about it. Sonja M. Amadae (2003, 146-48) notes that many of the methods currently used in rational-choice political science were developed only after World War II, and they address questions that were new at that time. Most of these methods were thus invented, rather than imported from economics and reinterpreted. Accordingly, it might be better to talk about *game theory imperialism* rather than economics imperialism, because game theory invaded not only political science but also economics.[22] On the other hand, "game theory imperialism" is a misleading term given that there is no domain of phenomena studied only by game theory. It is rather a collection of methods that can be applied to a wide range of topics. Most importantly, even though no theorems have crossed the disciplinary boundary, economics or game theory imperialism is definitely not without consequences, since the use of certain derivational techniques and heuristics changes the *kinds* of questions that are being asked and the standards of theory evaluation.

Our case study supports the view of Morrison that unification has, to a large extent, to do with perceiving common structural features across different domains of scientific investigation and the accompanied use of similar mathematical techniques. However, it should be stressed that we do not endorse the

[22]Perhaps imperialism here is merely the continuation of the same mathematics (topology) imperialism that, according to Giocoli, conquered economics somewhere around the middle of the twentieth century.

instrumentalism of Aumann that Mäki takes to be part of the derivational package. Although the exclusive use of certain kinds of formal methods does, to a large extent, dictate *the form* of knowledge and understanding to be gained, whether the knowledge itself is valid remains an empirical issue; i.e., whether structural unification in this sense is a justified enterprise depends first and foremost on the contingent issue of whether or not the political systems under scrutiny are likely to exemplify these very general forms of organization. Structural unification is thus also based on the requirement for a certain kind of ontological unity, a similarity in structural or organizational properties, but it is clearly a distinct and weaker notion than the unity of fundamental entities and causal forces.

Whether the questions that are answerable with equilibrium models are the kinds of questions that we should be asking is, of course, a further matter worth considering. If rational-choice models were to completely crowd out other forms of inquiry, as the unificationist rhetoric of many of the protagonists seems to suggest, a lot of important questions would be left unasked. We do not want to provide an argument *for* economics imperialism in political science or anywhere else. We fully acknowledge that the theories we have compared (GET and the literature on cycles in political theory) are both fields of research characterized by highly abstract theorizing with all but nonexistent possibilities for empirical testing. Merely in terms of epistemic virtues, both could be said to be wanting because they make assumptions that are so wildly false that they are considered untestable for this reason. However, if the theories on cyclic preferences are questionable, they are questionable on these epistemic grounds rather than because they provide an example of mere derivational unification. A similar comment applies to spatial competition: whether or not this theory is considered a success story depends on how well it fares in empirical tests, not on whether the solution concept was suggested by an economist or not.

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Bios

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