

**No Theorems without Theory:**

**On Rogowski's Comparative Political-Economy of Trade Policy**

Douglas Nelson

Department of Economics, Syracuse University

## No Theorems without Theories: On Rogowski

Professor Rogowski's recent analysis of the political-economy of trade policy reported in this journal (*APSR*, 1987) and in his *Commerce and Coalitions* has been widely acclaimed as a major step forward in the development of research on comparative political economy.<sup>1</sup> In particular, Rogowski's work is striking in its attempt to develop a firmer theoretical foundation for the analysis of the link between changes in international economic conditions and patterns of group conflict.<sup>2</sup> Given the importance of this general programme, I believe it is essential to be clear about the limits of the particular form in which it appears in Rogowski's work.

The notion that trade policy is, at least in part, determined by the political action of self-interested groups is certainly not new in political science. Several foundational works in the group theoretic tradition were studies of the trade policy process (Schattschneider, 1935; Bauer, Pool and Dexter, 1963; Lowi, 1964). Where this earlier work was primarily concerned with the determination of policy outputs, research of the Gourevitch-Kurth-Rogowski variety is more concerned with the systematic patterns of social conflict in response to changes in the political-economic environment.<sup>3</sup>

Recent research, primarily in Economics, proceeds in a very similar manner: from the identification of interests to political action via a comparative statics exercise. The fundamental difference between the Kurth-Gourevitch-Rogowski type of analysis and endogenous policy theory relates to the explicitness of the development of the theoretical framework. Rogowski (1989) appears to take a major step toward reducing this gap, presenting an analysis based explicitly on a major result from the general equilibrium theory of international trade. Unfortunately, Rogowski never develops (or even describes) the particular details of the underlying model. Because we believe that

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<sup>1</sup> The theoretical framework of Rogowski's work was awarded the Franklin L. Burdette-Pi Sigma Alpha Prize for the best paper presented at the 1987 annual meeting of the American Political Science Association; it was ultimately published in the Association's journal (Rogowski, 1987); and the book was published by a prestigious academic press (Princeton) with accolades from leading scholars in the field on the cover (eg. "I believe that, simply stated, this is the most important book written in comparative politics in the 1980's").

<sup>2</sup> See Gourevitch (1977) and Kurth (1979) for exemplary earlier analyses in this tradition.

<sup>3</sup> Characteristically, pluralist/group-theoretic research on trade has not been particularly concerned about the identification of interests or its aggregation. This derives from the strongly empirical orientation of this tradition. Pluralist scholars tended to assume that, as a research strategy, it was better to let people identify their basic interests through forming and joining groups rather than imposing specific interests *a priori*.

sustained, formal theoretical development is essential to the development of a cumulative body of research in comparative political economy, we offer a critique of Rogowski's analysis. It should be clear that, to the extent that analyses with less formal structure rely on an implicit model of the Rogowski sort, the same critique applies to them.

Rogowski bases his analysis on a result from the theory of international trade called the Stolper-Samuelson (SS) theorem. Note that this result is a theorem, it is derived from a specific set of assumptions which form the basis of what is generally called the Heckscher-Ohlin-Samuelson (HOS) or "modern" theory of international trade. It will be useful to list the assumptions here:

1. With regard to production:

- a. There are two factors of production; fixed endowments of both; both factors are of uniform quality, and perfectly and instantaneously mobile between sectors;
- b. There are two sectors characterized by neoclassical production functions (i.e., both factors are essential and the functions are linear homogeneous, twice differentiable, strictly concave);
- c. There is no joint production and no production externalities;
- d. One sector can be characterized as K-intensive relative to the other at any given relative factor prices;
- e. No factor-intensity reversals;

2. With regard to demand, both goods are normal;

3. With regard to the institutional environment:

- a. All four markets exist and are perfectly competitive;
- b. There is a perfect system of property rights both domestically and internationally;
- c. All agents are economically rational.

4. With regard to the international environment:

- a. The Home country is economically small (i.e., changes in the national economic environment have no effect on international prices);<sup>4</sup>

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<sup>4</sup>Much of the initial development of modern trade theory assumes that there are two countries, distinguished only by their relative capital/labor endowments. This is usually insured by assuming that countries have access to the same technologies and have identical tastes. We will not need this assumption here since, under the small country assumption, the SS theorem relates strictly to domestic economic adjustments.

- b. Goods are costlessly mobile internationally;
- c. Factors are immobile internationally;
- d. There is balanced trade.

Under these assumptions the economy will be characterized by: full employment; zero economic profit; and increasing opportunity costs in the transformation of one good into the other (i.e., the production possibility curve will be bowed out from the origin). Given these assumptions, a change in the relative prices faced by domestic producers will cause perfectly determined changes in the returns to factors of production. Specifically, an increase in, say, the price of the capital-intensive good ( $p_1$ ) will raise the returns to capital ( $r$ ) relative to both commodity prices and will cause the return to labor ( $w$ ) to fall relative to both commodity prices. Using "hats" to denote proportional changes:

$$\hat{r} > \hat{p}_1 > \hat{p}_2 > w.$$

Unlike the impression given by Rogowski, this result is quite sensitive to (at least some of) the assumptions. Let us now compare these assumptions to those in the Rogowski analysis.

The most striking thing about Rogowski's analysis, given the central role played by the SS theorem, is the almost total lack of discussion about the supply-side of the model. The number of goods is never discussed and while most of the discussion could be taken as assuming two goods (urban and rural), at several points the analysis implies a higher dimensionality. A considerably more serious problem, especially given the definite increase in the number of factors from two to three, is the lack of analysis of the technologies of production. In addition to the assumptions about factor intensity and the ways the factors enter into production in general, in higher dimensions the results of the model are also sensitive to the assumed substitutability between factors in production.<sup>5</sup>

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<sup>5</sup> One might also note at this point that the lack of detail with regard to technology is particularly troublesome with respect to the application of the Heckscher-Ohlin theorem. This is the theorem that relates factor endowments to comparative advantage and, although Rogowski does not refer directly to it, much of his discussion relies on being able to identify a country's exportable and importable sectors in a manner consistent with the theory. The difficulty here is that, in addition to factor endowment (which characterizes countries), we also need information on factor intensities (which characterize industries). In a three-factor/two-good model any conclusions about comparative advantage obviously requires much more detail on technology. In addition to Ethier (1984), useful discussions of the link between factor endowment and trade pattern in high dimensional models can be found in Jones (1956/7, 1974), Bhagwati (1972), Mayer (1976); Drabicki and Takayama (1979), and Dixit and Woodland (1982). For multiple countries see: Jones (1961), Krueger (1977), and Deardorff (1984).

With regard to the production structure we have noted one major explicit change in the assumption structure: there are now three factors of production (land has been added to labor, and capital) and an unspecified number of final consumption goods.<sup>6</sup> The inexplicitness with respect to the number of goods is unfortunate because the HOS model is sensitive to this specification.<sup>7</sup> Rogowski's theoretical discussion seems to retain the two good specification with one rural good and one urban good. As long as the rest of the assumption structure remains in place, this structure is consistent with generalizations to the effect that: an increase in the relative price of a good will result in an increase in the return to *some* factor in terms of all goods and lowers the return to *some* other factor relative to all other goods (Ethier, 1984). Unfortunately, Rogowski's analysis is based on the stronger claim that: an increase in the relative price of a good will partition factors between gainers and losers. What is unfortunate is that such a proposition cannot be demonstrated (Ethier, 1984, pp. 152-154).<sup>8</sup>

Let us skip a variety of relatively small deviations from the basic HOS assumption structure to examine the

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<sup>6</sup> We will assume, given the explicit reference to the Stolper-Samuelson theorem, that the original assumptions are retained unless specifically noted. At this point, that means that all three factors are assumed essential and that the production functions are of the usual neoclassical type. Neither of these is trivial, if either fails to hold most of the generalizations of the SS theorem to higher dimensions will also fail to hold.

<sup>7</sup> Rogowski cites Ethier's (1984) excellent survey of the effect of changes in the dimensionality of the basic (2x2) trade model. His claim that the SS theorem is relatively insensitive to changes in dimensionality cannot be supported by even a cursory reading of Ethier. Consider the following:

" The attempt to extend to higher dimensions the strong Stolper-Samuelson property that commodity price changes produce unambiguous changes in all factor rewards thus runs into serious limitations. First, either the class of applicable relative commodity price changes was restricted to the two-dimensional one or the conclusions about factor rewards were obtained only on average; secondly, the presence of the strong property for this restricted class was shown to be equivalent to the imposition on the technology of a strong factor intensity condition that can be interpreted as requiring the technology to be in some sense essentially two dimensional. The most significant accomplishment of this branch of international trade theory must surely be the basic elucidation of the notion that the strong Stolper-Samuelson property is in its very essence largely a two dimensional one". (pp. 158)

In addition to Ethier, other useful treatments of generalized, neoclassical trade models can be found in: Dixit and Norman (1980), Woodland (1982), and Takayama (1982).

<sup>8</sup> The point here is not that we cannot construct a specific version of the HOS model yielding the specific pattern of returns required by Rogowski's analysis, we certainly can. The point is that such a pattern is not a general result of the HOS model. To construct a model yielding the required pattern of factor price reactions to a change in relative commodity prices requires specific assumptions about the production functions for the final consumption goods (especially with respect to elasticities of substitution between factors of production); about the country's factor; and, for more than two final consumption goods, about the elasticities of substitution between the goods in consumption. It would, however, be pointless to speculate about these because Rogowski presents no assumptions at all relating to technology and demand.

assumption that factors are internationally immobile.<sup>9</sup> Rogowski (1987) commonly uses the evidence of substantial international factor flows as indirect evidence of national factor endowments. The problem with this procedure is that, as Mundell's (1957) classic paper clearly demonstrates, with perfect international factor mobility there will be no SS effects. The reason is clear enough: since a good is simply a bundle of factors, international factor mobility is a substitute for international good mobility, and vice versa. A barrier to the mobility of one simply induces an accommodating movement of the other.<sup>10</sup> A trade policy action that raises the return to, say, labor via SS effects will simply induce an inflow of foreign labor driving down the wage to the equilibrium level. If international factor mobility is assumed to be imperfect, then: 1) mobility of the magnitude discussed by Rogowski will still have a substantial depressing effect on the SS effects; and 2) both the mobility and the imperfection should be explicitly included so that their effects on the relevant parts of the model (i.e., the SS theorem) are known.

By comparison with the discussion in Rogowski's work, the framework developed in endogenous policy theory is explicit in its assumptions and, as the preceding discussion should suggest, considerably more general than Rogowski's. For example, the framework developed in Hall and Nelson (1991, 1992, 1998) permits a wider variety of potential cleavage patterns while retaining the general neoclassical structure implied by Rogowski. That is, while capital-labor conflict is a possible axis of conflict, so is inter-industrial conflict. In other extensions, gender-based and age-based conflicts can also be accommodated. The benefit of the explicitness is not only in helping to avoid logical errors, but also in permitting clear development of more complex analytical structures.

The issues that we have raised to this point are purely theoretical. What we have suggested is that the SS theorem cannot be generally derived from the explicit and implicit assumptions of the Rogowski analysis. Since this result lies at the core of Rogowski's theory, this conclusion effectively leaves his interesting historical discussions

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<sup>9</sup> One other significant error can be briefly noted. In a footnote on the first page, Rogowski seeks to "dispel a misunderstanding that occasionally arises: these [SS] effects befall both the country that imposes protection and its trading partners" (1989, pg 3). In fact, this is not correct. The HOS model assumes that the relevant country is economically small, which means that its policy actions have no effect on world prices. As Lerner (1936) and Metzler (1949) demonstrated long ago, if the policy actions of the Home country can affect its terms of trade, the normal SS pattern of income distribution effects may not obtain.

<sup>10</sup> As with everything else in trade theory, Mundell's conclusion rests on a very specific model. The existence of international mobility of both goods and factors leads to a variety of complex models. Useful presentations of these issues can be found in: Ruffin (1984); Neary (1985); Ethier and Svensson (1986); and Wong (1986).

without theoretical foundation. The moral of this part of the story is simply that one cannot buy a theorem without buying the theory from which it is derived. Passing from the relationship between the HOS assumptions and Rogowski's presentation, there are a couple of additional points worth considering: the political time horizon and the degree of inter-sectoral factor mobility; the lack of any explicit treatment of political action; and the types of parametric changes to which the analysis applies.

It is standard in economics to treat the SS theorem as effectively a statement about the long-run effects of a change in relative commodity prices on relative factor prices.<sup>11</sup> The use of the SS theorem in Rogowski's work, thus, implies either that persons make their political calculations with respect to the long-run effects of changes in the system's political and economic parameters; or that the conclusions of the theory refer to some aggregate relationship without obvious micro-foundations. While a perfectly respectable account of the political-economy of trade can be constructed without choice theoretic foundations, Rogowski's work (as well as our own) clearly is based on such foundations. Thus we can ignore the second possibility. What is at stake here is the form of the relationship between changes in the political-economic environment and political action as mediated by an individual's position in the economy (defined by factor ownership, sectoral location, and tastes). The key to this, as we argue in our own work (Hall and Nelson, 1989), is the time horizon over which political calculation occurs relative to the time period over which the economy adjusts to a change in its parameters.<sup>12</sup>

Most research on the United States strongly supports the hypothesis that, at least for trade policy, the political time horizon is shorter than the period of economic adjustment.<sup>13</sup> Rogowski (1989, pp. 18-19) responds to

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<sup>11</sup>While Rogowski strongly and explicitly defends his use of a long-run framework, it should be noted that he is not completely consistent in this. For example, on page 7 (footnote 15) he makes an argument that rests on an assumption of "rural labor", i.e. labor which is sectorally immobile. At a later point (page 18), he argues from the premise that "in some cases trade expands or contracts so rapidly and surprisingly as to frustrate rational expectations". Since no basis for the "frustration of rational expectations" is laid in his analysis, this is presumably short-hand for not focusing on the long-run effect of the change in the system's parameters.

<sup>12</sup>The issue of time horizon is, at least indirectly, related to the status of Rogowski's use of the notion of "potential gains" as a stimulus to political action. Both Rogowski's approach and our own are based on prospective political calculation: agents are motivated to engage in political action in response to rational evaluations of the (future) effects of those actions. It should be noted that neither model treats the future in a particularly compelling way. Specifically, both lack explicitly modelled uncertainty and both treat capital as an endowment. Thus, two of the most fundamental aspects of prospective calculation are ignored.

<sup>13</sup>The most direct test of this, as Rogowski notes, is found in Magee (1978). Magee's results were supported for several additional years as part of research reported in Nelson and Wasley (1990). These are merely

this in three ways. First, he simply reasserts that the economy will eventually adjust to the long-run equilibrium. There are two problems with this: first, the issue is not what the equilibrium looks like in the long run, but what people base their political action on; and second, it is not correct if the adjustment resisting forces are successful in altering government policy to offset the original change. Rogowski's second line of defense is that factors should be becoming more mobile over time so that, even if the specific-factor formulation was appropriate at some point, it is becoming less appropriate over time. This may be, but the most systematic evidence in favor of the short-run formulation comes from the last 30 years. Thus, there is no evidence, whatever the trend, that the long-run formulation has ever actually been appropriate. The second line of defense appears to be in contradiction with the third line, that the evidence supports the long-run formulation. The "evidence" to which Rogowski apparently refers is the fact that he can tell a compelling story about aggregate interests and aggregate outcomes. The problem, of course, is that these aggregates do not provide compelling support of an individual level hypothesis (this is the ecological fallacy), especially in the face of relatively direct evidence to the contrary. The point is not that the historical discussion is in any way in error, only that it can neither support nor be supported by the SS theorem. If the historical analysis is, in fact, correct, it is interesting to speculate on the mechanisms that transform political behavior based on short-run economic interests into political structures reflecting long-run economic interests.

The forgoing point leads us to one of the really curious parts of the Rogowski analysis: although the theorem on which the entire analysis is putatively based assumes rational behavior on the part of all persons in their economic life, there is no rational analysis of their political behavior. The SS theorem is presumed to identify the gains from changes in the economic environment (whether induced by technological or policy-induced shocks), but there is no equivalent analysis of the costs of political action or of the way in which the political system determines the political-economic equilibrium. Rogowski is careful to say that he is not *really* predicting outcomes, only cleavage patterns. But one has to be careful with this assertion since he has already assumed that the only relevant cleavages are defined by factor ownership. This is certainly not necessary. As we suggested above, in a general

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the most systematic presentation of the strong tendency for the politics of U.S. trade legislation to be overwhelmingly based on inter-industry patterns of lobbying, rather than inter-factor patterns. It is important to note, given Rogowski's assertion to the contrary, that the pattern of lobbying in the U.S. is a particularly good test of the general time horizon of political calculation. The existence of an institutionalized class basis of politics in many Western European countries could bias the results in a way that the more pluralistic politics of the U.S. do not.

equilibrium sense there are a variety of possible coalitions possible, based, at a minimum, on: sectoral location of employment; nationality of factor (if we permit factor mobility); and taste patterns; in addition to simple factor ownership. Rogowski seeks to demonstrate that factor-based (i.e., class) cleavages do, in fact, drive the macro politics of trade. This is not derived from the theory in any way, but simply emerges from a sort of functionalist stew.<sup>14</sup> Although the political structure of endogenous policy models is extremely spare, it provides a framework for incorporating, and studying the effects of, a variety of alternative assumptions about the political system.

We should note one last important ambiguity in Rogowski's presentation. It is not at all clear what events the framework he presents is intended to accommodate. We argue elsewhere that models of this sort only apply strictly to "small" changes (Hall and Nelson, 1991). This is not so much a reference to the use of calculus in deriving the results (though it is useful not to forget this), but rather refers to the implicit assumption that the overall political environment can be taken to be fixed. Although Rogowski makes occasional reference to the marginal nature of the analysis (eg. page 17), it should be clear that his historical cases are drawn almost exclusively from major conjunctural moments (i.e., realigning episodes). Just as the transition from the politically relevant period to the economic long-run is trickier than it seems, the application of models of marginal adjustments to moments of world historical change cannot simply be assumed. As before, Rogowski's analysis may be absolutely correct, but that correctness has nothing to do with the model. There must be a meta-model somewhere that explains the link between the micro and the macro. Without it, we are really unable to evaluate the significance of Rogowski's programme to this point.

The point of this discussion is not that Rogowski was wrong-headed in choosing an essentially formal model as the basis for his research. Quite to the contrary. We believe such a strategy is essential. Our argument is that this strategy was not pursued far enough. Furthermore, we do not believe that the problems noted in this comment are unique to Rogowski's work. Rather, they are merely thrown into high relief in that work and should

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<sup>14</sup>This is probably a good place to note that Rogowski's analysis seems much closer, on all dimensions, to the Pirenne-Sweezy-Wallerstein (PSW) tradition than to either neoclassical economic history (eg. North, 1981) or economic sociology (eg. Moore, 1967). Rogowski's analysis lacks the theoretical discipline of the former and the historico-institutional subtlety of the latter. Like PSW international exchange is linked to political outcomes via a simple, mechanical functionalism that operates through class struggle. Also like PSW there is systematic analysis of neither political process nor political structure. Most reviews of Wallerstein's world system theory could be applied to Rogowski's work without substantive change (except of course for empirical domain. In a sense, Rogowski picks up the historical story where Wallerstein has, to date, left off.).

cause us to reassess the broader body of research on comparative political economy.

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