A similar set of concepts has been central to the literatures on the formation of trade policy coalitions and the "new economics of institutions": the political and economic consequences of the degree to which assets are specific to a particular economic activity. In this survey, the authors take the necessary first step of summarizing the main findings of these two literatures and then suggest ways in which the issue might be joined. In addition to providing a more coherent understanding of the findings of these two literatures and some new directions for them, the authors show that many puzzles remain in the field of trade politics—puzzles for which there are no appealing answers or, where there are answers, no strong evidence in support of them. This essay, then, in addition to being a theoretical review of the literature, puts forward an agenda for future study of international trade politics.

THE POLITICAL ECONOMY OF INTERNATIONAL TRADE
Enduring Puzzles and an Agenda for Inquiry

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I. INTRODUCTION

Over the past decade, the tools of modern political economy have been brought to bear on a wide variety of topics. Two active areas of work have

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been the politics of trade policy (endogenous tariff theory) and the new economics of institutions. A similar set of concepts has been central to both research agendas: the political and economic consequences of the degree to which assets are specific to a particular economic activity. There has been little or no attempt to explore this overlap. In this survey, we take the necessary first step of summarizing the main findings of these two literatures and then suggest ways in which the issue might be joined. In addition to providing a more coherent understanding of the findings of these two literatures and some new directions for them, we show that many puzzles remain in the field of trade politics—puzzles for which there are no appealing answers or, where there are answers, no strong evidence in support of them. This essay, then, in addition to being a theoretical review of the literature, puts forward an agenda for future study of international trade politics.

Many believe that factor specificity is an important determinant of the composition and character of trade policy coalitions. Traditionally, two different economic models have been used to ground the study of trade policy. They are the Heckscher-Ohlin model, in which factor specificity[1] is low (so low that it is assumed that all factors can move costlessly among industries), and the Ricardo-Viner model, in which factor specificity is very high (so high that some factors cannot move at all among industries). We discuss these two polar types in the next section and introduce another interpretation of trade patterns that also generates predictions about trade policy coalitions: the increasing returns to scale (IRS) model common in more recent international trade literature.

In addition to these three economic models of trade patterns, which generate predictions about actors' trade policy preferences given their factor endowments and product characteristics, there is a set of more political models based on collective action costs and governmental institutions. These models offer predictions about how "raw" preferences are translated into actual trade policy coalitions. We argue that collective action problems and political institutions interact with economic characteristics, mainly the degree of factor specificity, to affect trade policy coalitions.

1. By factor specificity, we are referring to the ease with which factors can move among sectors of the economy. Perhaps more accurately, it is the costliness with which factors move from their current use to an alternative one, for example, the costliness with which labor in a textile mill, say, can move to a microchip factory.

AUTHORS' NOTE: *This article grew out of a roundtable on "Factor Specificity, Interest Groups, and the Politics of International Trade" at the 1993 American Political Science Association meetings and the summer of discussions that preceded it. We thank Karen Ferree for many useful comments. Please direct correspondence to Michael Gilligan, Department of Politics, 715 Broadway, 4th Floor, New York, NY 10003; e-mail: gilligan@acfcluster.nyu.edu.*

Virtually no serious student of trade policy believes any particular approach monopolizes the truth on these issues. All of them operate to some degree depending on the circumstances. Therefore, after outlining the various approaches to trade policy coalitions, we suggest empirical challenges that remain for these existing frameworks.

To consider how best to organize the empirical inquiry, we next discuss the explanatory variable, factor specificity. Because most agree that factor specificity is central in some way to the formation of trade policy coalitions, we argue that those who study trade policy should develop hypotheses from insights of the new economics of institutions, where notions of factor specificity are also central. There should be some gains from cross-fertilization. However, testing any such hypotheses would also involve measuring specificity at a variety of levels of aggregation from individual firms to industries and even sectors of the economy. Empirical work using the concept is scattered across many fields, but the third section of the article discusses and compares some promising approaches to measuring specificity.

The fourth section turns from the independent to the dependent variable and puts forward an enduring and little studied problem in the political economy of trade policy. We still do not fully understand why trade barriers are used for policy purposes at all. In this vein, we raise two questions about the politics of international trade, questions with far from obvious answers, namely "Why do we observe trade policy at all given its well-known inefficiency in redistributing income?" and "Why do those favoring trade restriction so often seem to win politically?" Considering these questions forces us to reassess the dependent variables studied by the literature. Further, an answer to these questions is important to the issue of trade policy coalitions because our models of coalition formation will be based on seemingly irrational behavior. Thus the models will not be entirely satisfactory until these questions are answered. We hope our presentation of the state of the discussion on trade policy coalitions and the new directions for their study offered by the new economics of institutions helps resolve these enduring puzzles in the political economy of trade.

II. FACTOR SPECIFICITY, INTEREST GROUPS, AND THE POLITICS OF INTERNATIONAL TRADE

This section describes the strengths and weaknesses of each of the three economic approaches to trade policy coalition formation as well as some more political arguments. It is convenient to start by considering the first two economic models, based on mobile and specific factors models, respectively,

as two polar extremes. We then consider a third trade-theoretic alternative, IRS models, and the political factors in coalition formation. Finally, we relax our initial assumption, treating factor specificity as a matter of degree.

MOBILE AND SPECIFIC FACTORS

First, assume factors of production (e.g., capital, labor) can move costlessly among sectors. This is the assumption of the Heckscher-Ohlin model, and it implies that factor returns are equalized throughout the economy. Therefore, the fortunes of owners of a particular factor rise and fall together regardless of which industry employs them. Second, regions naturally export goods whose manufacture uses intensively factors in which they are abundantly endowed and import goods intensive in their scarce factors.[2] Thus trade benefits owners of abundant factors and, absent compensation, harms owners of scarce factors. Hence owners of abundant factors will favor free trade, owners of scarce factors will be protectionist, and empirically we would expect trade policy coalitions to form along factor or class lines.[3]

At the other extreme is the specific factors model or the Ricardo-Viner model. It assumes that at least some factors cannot move between sectors of the economy, as some (perhaps most) investments are "stuck" in their present occupations. The fortunes of the specific factors in an industry then rise and fall together whether they are the same type of factor or not.[4] Hence we would expect trade policy coalitions to form along the lines of exporting versus import-competing industries or sectors. If the United States is abundant in software-producing capital but scarce in up-to-date auto-producing capital, and if shifts between these uses are costly and slow, then software manufacturers will embrace free trade and automakers will be protectionist.[5]

2. In the classic case, land-abundant Australia exports land-intensive wheat and imports labor-intensive cloth.

3. If, for example, land is abundant and labor is scarce, then peasants will be more protectionist the less land they own (cf. Rogowski, 1989, pp. 19-20). Technically, the model predicts that trade preferences will vary within categories of asset holders according to portfolio composition. For instance, in the present-day United States, where human capital is abundant but unskilled labor is scarce, we would expect workers to be more protectionist the lower their level of skill. (Midford, 1993, advances suggestive evidence in support of this proposition.)

4. They act as if they were different factors in the Heckscher-Ohlin model. Factors in the Heckscher-Ohlin model can be costlessly redeployed to more profitable uses but never can be transformed into other factors.

5. Strictly speaking, exporting firms should favor export subsidies and import-competing firms should favor protection. However, because export subsidies are much less common than trade restrictions (see the discussion in the fourth section), we often see export industries lobbying for "antiprotection" rather than export subsidies (Destler, Odell, & Elliot, 1989; Milner, 1988). Our preceding point, then, is defensible as a first approximation.

As to which of these models is superior empirically, the jury is still out. In fact, if they are more a matter of degree than mutually exclusive, then the question may be moot. Frieden (1991a, 1991b) and Gilligan (in press) have used the Ricardo-Viner model, as have most other researchers in the endogenous tariff literature (Findlay & Wellisz, 1982; Grossman & Helpman, 1994). The Ricardo-Viner assumptions often are implicit in the empirical economic literature on tariff setting. (See Anderson, 1980; Baldwin, 1986; Cheh, 1974; Lavergne, 1983; Magee, Brock, & Young, 1989; Pincus, 1977; Ray, 1981a, 1981b, 1987; Ray & Marvel, 1984; and Reidel, 1977, among many others; much of the literature is reviewed by Anderson & Baldwin, 1981.) Rogowski (1989) used the Heckscher-Ohlin model for his book, *Commerce and Coalitions* (see also Mayer, 1984).

IRS MODELS

For much of world trade, however, neither of these "endowments-based" models of interindustry trade patterns is quite sufficient. Neither one predicts the increasing importance—indeed, now the vast majority of world trade—of *intra*industry trade between regions of similar factor endowments (Krugman & Obstfeld, 1991, p. 139), that is, north-north rather than north-south trade. Among industrial countries, the importance of intraindustry trade—two-way trade in similar products—cannot be overemphasized. Depending on how one counts, this kind of trade constitutes as much as 83% of U.S. trade, 79% of British trade, 73% of German trade, and so on (Bergsten & Noland, 1993). What is left over is *inter*industry trade, only a small fraction of the total.[6]

This is important in this context because two-way trade in auto parts or in machine tools cannot easily be explained on the basis of intercountry differences in factor endowments and interindustry differences in factor intensities. We need to rely instead on the existence of product differentiation and IRS, the latter of which requires parting company with the assumption of perfect competition shared by both the Heckscher-Ohlin and Ricardo-Viner models. It is important to understand that in these models with IRS internal to firms and differentiated products, one gets not only intraindustry trade but also

6. None of these statements is uncontroversial, of course. For instance, the intraindustry versus interindustry distinction may simply be a problem with labeling. Many products that are commonly thought of as being in the same industry may in fact be so different as to not really be in the same industry at all. For instance, luxury cars may be such different products from other types of automobiles that although they both are lumped together into the automotive industry, they are in fact different industries with different factor requirements and so forth. If that is the case, then what usually is thought of as intraindustry trade could be analyzed with the older models, particularly the Ricardo-Viner model.

neutral consequences for income distribution and the possibility that everyone gains from increased trade through the expanded number of products available. The last consequence occurs because with differentiated products—no direct substitution of competing products—increased trade causes no product line to shut down or scale down because no identical product is produced by another firm, and so no factor owner loses out through sectoral reallocation. At a minimum, these results call attention to the importance of product differentiation and the absence of substitution, reversing the emphasis of the Ricardo-Viner model on potential losses facing specific investments. (For further discussion of the effects of increasing returns on international trade, see Helpman & Krugman, 1989.)

To see how intraindustry trade can matter, consider the case of Japan. Japan is peculiar because its intraindustry trade is uncommonly low among industrial countries; it stands at only 58%. This fact may provide an important clue as to why Japan's trade is so contentious. U.S. auto imports from Canada are as large as U.S. auto imports from Japan ($27 billion in each case), but we would hardly know it. The reason is that Canada's trade with the United States is heavily intraindustry and therefore does not have the same redistributive consequences (along either industry or factor lines) that Japan's trade has. This is not to say that intraindustry trade always is devoid of distributional consequences; it is only to say that it *can* be (unlike endowments-based trade) and that, in any case, the distributional consequences always are murkier than they are in the endowments-based models and may not follow the well- defined industry or sector lines along which many political organizations form and that scholars use as the basis for analysis.

In terms of political economy, intraindustry trade is distinguished from Heckscher-Ohlin and Ricardo-Viner trade in not necessarily having stark distributional implications. Because the adjustments called for by intraindustry trade take place within (rather than across) sectors, there is at least the theoretical possibility that everyone can gain from such trade. So the question of factor specificity and the associated distributional implications could be irrelevant to a large part of the trade of the countries mentioned in the preceding. Thus the "newer" IRS models that account for the growth of intraindustry trade also predict ambiguous distributional consequences of trade and therefore less conflict on trade issues than do the other two models.[7]

Such IRS models, however, also show how both workers and owners can be threatened by a "tipping" of regional advantage that moves a whole sector

7. We emphasize here that if the IRS effects are external to firms, then intraindustry trade will not result, and that redistribution certainly is possible along geographic lines, particularly in models with costly transportation.

from one location to another (Krugman, 1991). If, indeed, economies of scale are increasing in importance, and if a region's or a country's competitive advantage results more from accident, infrastructure, and infant-industry policy than from endowments of general or specific factors, then it is reasonable for sectors to lobby more and to do so as sectors. Moreover, one might predict that the sectors most characterized by IRS would be likeliest to mobilize as sectors in support of more open trade.

INTERACTION OF POLITICAL AND ECONOMIC FACTORS

The three economic models specify individuals' preferences over trade policies, but none of these models says how its distributional implications find expression in the political arena or predicts the resulting character and composition of trade policy coalitions. Alt and Gilligan (1994) argue that we cannot get from the specification of preferences offered in these three models directly to coalition formation. Simply having a particular set of preferences will not motivate people to take political action.

When deciding to take political action, people make a cost/benefit calculation. The preferences from the Heckscher-Ohlin, Ricardo-Viner, or IRS models can be thought of as the "benefits" side of a calculation—the goods that individuals hope to receive or the ills they hope to avoid by taking political action. But people also take into account the *costs of collective political action*, and, as we know, these costs can be exacerbated by the familiar "free-rider" problem (Olson, 1965).

Alt and Gilligan (1994) argue that *domestic political institutions* matter as well. If the political system is such that it rewards small sectoral groups, then people will not be willing to pay the substantial costs of organizing large intersectoral coalitions. So, for instance, if all one has to do to receive protection is lobby a few legislators on the House Ways and Means Committee and the Senate Finance Committee, then there is no reason for groups to waste resources developing large intersectoral coalitions. On the other hand, if the political system rewards large mass movements (e.g., coalitions such as urban vs. rural or labor vs. capital), then intersectoral groups will find it necessary to coalesce if they hope to receive benefits through the political system.

Alt and Gilligan (1994) go on to argue that these two variables (collective action costs and political institutions) are *interactive* with factor specificity. For instance, the kinds of coalitions Rogowski (1989) mentioned require (a) low factor specificity, (b) low collective action costs, and (c) domestic political institutions that reward mass movements. (Alt & Gilligan call such institutions "majoritarian," whereas Verdier, 1994a, speaks of decisions

requiring a "large" quorum.[8]) If one of these is missing, then a different set of coalitions will form. If there are high collective action costs, then no coalitions will form because they will not be able to overcome those costs; if institutions are less majoritarian, then interests will have no reason to pay the costs of forming such massive movements and will resort to back-room deals and lobbying.

Similarly, for the kind of politics described in most of the endogenous tariff literature, (a) factors must be specific, (b) collective action costs must be high, and (c) institutions must be less majoritarian. If one of these is missing, then a different set of coalitions will form. If collective action costs are low, then consumer groups will form against protectionist groups and swamp them unless the protectionist groups make intersectoral coalitions. If institutions are majoritarian, then sectoral groups will have to make alliances with each other or with mobile factors to convince the political system to grant them their preferred policies.

In short, to get the two "ideal" types of trade policy coalitions associated with the Heckscher-Ohlin and Ricardo-Viner models, researchers really must make assumptions not only about factor specificity but also about collective action costs and policy-making institutions. Further, because these three variables are interactive, altering assumptions about each of them, one by one, yields different outcomes for trade policy coalitions, including no coalitions at all, and some unstable coalitions (see Alt & Gilligan, 1994).

Another implication of Alt and Gilligan's (1994) argument is that if a particular group has long since paid up the fixed costs of creating the coordinating agencies and establishing the selective incentives that permit collective action, and if it has well-worn channels of access to public officials, then it may defend its trade-related interests even when little is at stake because the marginal costs of action are low. At the same time, a much more affected but inchoate group does nothing because the start-up costs of organization are too daunting.[9] Similarly, well-established coalitions of groups are likely to survive simply because new ties are costly.

If these considerations are important—that is, if the past strength of an organization is a reasonable proxy variable for current collective action costs—then we should expect, ceteris paribus, that the best organized and most influential groups would intervene most on trade issues or that previous strength of organization should prove a powerful intervening variable in

8. For instance, in our usage, large-district proportional representation systems are more majoritarian than small single-member districts. Rogowski (1987) argues that, with respect to trade, the institutions themselves are endogenous in the long run.

9. A concrete example probably is steelmakers versus steel users.

predicting group actions on trade. Other tests might use more direct proxies of collective action costs such as geographic concentration, four-firm concentration ratios, or even the costs of telecommunication or mass communication. Surely, further tests of these hypotheses should be undertaken; however, existing tests of some of these predictions have produced contradictory results. (See the review by Anderson & Baldwin, 1981, and sources cited therein as well as Anderson, 1980; Baldwin, 1986; Cheh, 1974; Lavergne, 1983; Pincus, 1977; Ray, 1981a, 1981b, 1987; Ray & Marvel, 1984; Reidel, 1977.)

TREATING SPECIFICITY AS A MATTER OF DEGREE

The two sets of assumptions regarding specificity with which we began really are ends of a continuum ranging from no costs of mobility to prohibitive costs of mobility with various gradations in between. Once the extreme assumptions of both endowments-based models are eased, their predictions about coalitions, although probably superior empirically, no longer are as clear-cut. For the sake of argument, we can move from the classic Ricardo-Viner model, which assumes that some factors are not mobile *at all*, to a more generalized Ricardo-Viner model, in which the specific factors have imperfect but *some* mobility.

To give a concrete example, unless textile looms and the workers who tend them are completely specific to the textile industry, there is no guarantee under the generalized Ricardo-Viner model (with imperfect but *some* mobility) that both of the groups must necessarily lose from greater import competition. In particular, workers, who will bear the cost of retraining and adjustment (and so are specific to textiles in that sense) may still win from the decline of the textile industry provided the economy exports labor-intensive commodities. Of course, they do not gain as much as they would have had they been perfectly mobile, as in the Heckscher-Ohlin model. So different degrees of specificity among factors is compatible both with cross-class coalitions (à la pure Ricardo-Viner) and class-based coalitions (à la Heckscher-Ohlin), and this is not surprising given that the model is intermediate between the two polar cases.

Aggregating to higher levels, such an intermediate model predicts empirically that owners of an abundant general factor (e.g., U.S. capitalists, Mexican workers) in an import-competing industry will be more susceptible to protectionism the more specific (i.e., nonredeployable) their present investments or skills happen to be. If auto factories have but a few uses whereas warehouses have many uses, then U.S. automakers will be more likely to embrace

protection than will U.S. warehouse owners, given the same degree of import competition.

In addition to admitting various degrees of specificity, economists traditionally have viewed the sector-specific model as a short-run version of the long-run Heckscher-Ohlin model. According to this view, in the long run there are no assets that are specific to anything, whereas in the short run very few assets are costlessly transferable. This view implies that analyses using the Heckscher-Ohlin assumptions (e.g., Rogowski, 1989) are correct when focusing on long historical stretches (i.e., over the *longue durée*). At the same time, positions in the short run (e.g., who will be in favor of the North American Free Trade Agreement or the Uruguay Round) probably are better predicted by the sector-specific model, which often is used in the endogenous tariff literature. According to this view, the time horizon may well be the key determinant of the appropriateness of the two models.

This is not an uncontroversial view, however. Alt and Gilligan (1994) argue that time horizon does not necessarily choose the model. Factors of production will have to pay costs of adjustment in the long run just as they would in the short run; the costs only would be spread out over a longer period of time. More to the point, if economic agents can secure protection of a declining industry from the government, then they would have no reason to move to another industry. On the contrary, in a game between economic agents and the government, the former actually may invest in a declining industry as a costly signal of their intention not to move from the industry, thereby forcing a government concerned with the welfare of its constituents to continue granting protection. This may be why certain specific factor owners in would-be declining industries do not move from those industries and even continue to invest in them over the very long term. Continued reliable protection of their industries has given them no reason to move. The wool-growing industry in the United States comes to mind.

MANY REMAINING CHALLENGES

It would be quite inappropriate to move on without acknowledging that each of the models has significant weak spots and that many puzzles remain unresolved. What the mobile factors model does not readily explain is variations among capitalists and among workers (e.g., why automakers are protectionist but software manufacturers are not) as well as the rise of sectoral coalitions (e.g., between U.S. automakers and autoworkers) in support of protection. At the same time, the breakdown of factoral solidarity on trade

issues should imply ever greater specificity of assets. Yet, as Kim (1992, pp. 20-26; 1993, esp. pp. 12-13) shows for the U.S. case, both capital and labor have become far more mobile among regions and employments since World War I, and a three-factor Heckscher-Ohlin model predicts regional specialization within the United States far better after 1967 than before (and much better than before 1939).[10]

Nor are such empirical puzzles only characteristic of the classical models. Some evidence suggests that economies of scale may be less significant than early enthusiasts supposed. According to some analyses (notably Krugman, 1991), for example, IRS sectors normally should exhibit extreme geographic concentration of production. In fact, however, as Kim (1992) shows, regional concentration of U.S. industry peaked during the early 1930s and has since declined steadily; today, American manufacturing is less concentrated geographically than it was in 1860.[11] We need more evidence about the prevalence of economies of scale before attributing too much to it. Even the stress on organization, and particularly on its effect of retarding and delaying adjustment and mobility, does not explain some of the most striking historical phenomena, namely the swift rise of seemingly new organizations (unless the costs of collective action have fallen substantially due to new technologies) or the reversal of traditional alliances that trade crises often seem to have provoked (Gourevitch, 1977, 1986).[12]

Most of the disagreement seems to exist among the economic models, but no two are mutually exclusive. Each appears to explain some things the others do not, and the phenomena each emphasizes are likely to be at work to some degree. More soberingly, each seems to be contradicted in some significant way by the available data, and, even in combination, the models do not appear to entirely explain the observed variation. We may well have to turn to some theory that has not yet been widely discussed.

10. It is essential to distinguish regional mobility from employment mobility. This survey is predominantly about the latter. Kim suggests that both have increased, but this assertion is far from uncontroversial.

11. It is possible that because products are more differentiated now than they were in the past, there is greater geographic concentration of products despite the lower concentration of industries that Kim finds. However, Kim's results hold whether he uses less differentiated (two-digit) or more differentiated (three-digit) industry categories.

12. Further, the argument will have to be extended to account for the manifest endogeneity of organizational patterns, particularly the degree to which they have been shaped by government (Katzenstein, 1985; Wallerstein, 1989).

III. FACTOR SPECIFICITY AND "GAINS FROM TRADE" WITH INDUSTRIAL ORGANIZATION

THEORY

This section develops the measurement and use of factor specificity as an explanatory variable. All models that use factor specificity to explain trade policy make the general claim that the greater potential loss facing the owner of a specific asset rationalizes greater expenditure of resources on activities (e.g., political, contractual) that offer the prospect of averting the loss (or reaping a gain). They do so whether the potential loss arises through exogenous competitive forces or opportunistic behavior by others and whether factor specificity is used interactively with political factors or not. Thus a crucial determinant of the incentives for an economic agent to seek trade protection (or, more broadly, subsidies) for his or her economic activity is the degree to which the agent's assets are specific to this activity.

Put differently, the specific factors view is based on the observation that specificity creates *ex post* exit barriers, which ultimately constitute *ex ante* entry barriers, whether these reflect actual political rules and regulations or simply the recognition by other economic agents of the risks of opportunism. Such barriers allow members of the sector to realize sector-specific rents. Of course, such rents are not restricted to trade protection, but for the purposes of this article we can focus on this. (In this argument, there does not appear to be much difference between the factor specificity of the trade policy literature and the asset specificity of the "new institutional economics" literature.)

At the limit, if the asset is completely mobile from use to use, then there is less incentive to expend resources to lobby for protection, whereas if the asset is completely specific to its current use, then its owner has a strong incentive to seek protection. The returns to political lobbying, in this context, are increasing in the specificity of the asset. This is a purely economic consideration with no institutional or informational components.

Adepts of modern industrial organization may indeed find this striking, given that the centerpiece of much exciting work in this field has to do precisely with the implications of asset specificity for the organization of economic activity. Without entering into the myriad twists and turns of the literature, it is safe to observe that most scholars believe that asset specificity is of central importance in determining the form of long-term contracts and the organization of firms. In its strongest form, this strain of thought asserts that the more specific the assets in question to their current use, the greater the incentive for their owners to carry out economic activity within one

economic entity rather than in spot markets at arm's length. This leads to predictions about the degree to which industries or firms will be characterized by vertical integration and long-term contractual arrangements, on the one hand, as opposed to arms-length or spot market interactions, on the other.

The reasons for these industrial organization conclusions largely have to do with the observation that the more specific the asset (i.e., the greater the quasi-rents), the more subject its owner to being held up by others involved in joint production. The internalization of exchange within the firm can help reduce the costs of monitoring and enforcing compliance with contractual or quasi-contractual agreements among those engaged in the joint production activity. Of course, the benefits of internalization (presumably primarily in reducing transactions costs) must be weighed against its costs (presumably primarily administrative).

This approach has indeed been applied quite widely in political science. The most common adaptation has been to relations among politicians and bureaucrats with the intention being to explain why Congress or regulatory agencies are organized as they are. In international relations, applications have emphasized the analogy between firms and international organizations; just as economic agents can create institutions to reduce transactions costs, so too can states create such institutions.

These all are important points and represent a fruitful interplay of work in industrial organization and political economy. However, we are intrigued by the largely unexplored relationship between factor specificity in the trade-theoretical sense and asset specificity in the new institutional economics sense. We present a few possible areas in which cross-fertilization might be possible in the following.

THE ORGANIZATION OF POLITICAL ACTIVITY

Just as politicians and bureaucrats organize themselves to reduce transactions costs, so too may socioeconomic agents. In this sense, long-lasting or institutionalized political aggregations are more likely where the actors hold political or economic assets specific to their current use. Individuals or firms with highly mobile or diversified interests are unlikely to enter into long-term organizations; those with very specific interests are more likely to do so.

For example, unskilled laborers who can move readily from job to job will be less likely to join together politically than will skilled workers tied to a particular industry or set of policies. By the same token, farmers producing a wide range of crops—that is, those whose land and human or physical capital are suited to many agricultural uses—will be less likely to organize than will highly specialized farmers. This may help explain a historical

pattern that Rogowski (1989) has noted, that is, the greater tendency of trade policy divisions to be along class lines in the 19th century than in the 20th century. Inasmuch as workers, farmers, and managers develop ever more specialized skills as levels of development rise, their political aggregations should become ever more specific to particular industries and activities.

This should be true for primordial interest groups, coalitions, and political parties. That is, broad-based political parties or movements with general goals are more likely when economic activities are simpler. More complex congeries of interests with a network of particularistic goals are more likely where the average socioeconomic agent's assets are more specific as to use.

In other words, just as the incentives to internalize economic exchange within a firm are a function of asset specificity, so too may the incentives to develop long-term political organizations and alliances be a function of the specificity of socioeconomic agents' assets. Inasmuch as this is the case, we expect those with more specific assets to be both better organized and more likely to engage in political activity than those with less specific assets. The reason is that although even perfectly mobile asset holders have incentives to lobby generally for protection for all industries that use them intensively, specific asset holders' real incomes will vary much more with the fortunes of the particular industry to which they are specific. Therefore, specific asset holders generally have more incentives to lobby than do owners of mobile factors.[13]

PUBLIC POLICY AS CONTRACT

As in trade theory, we might regard public policies as running a range from those that affect very broad groups of individuals who share general attributes (e.g., over age 65 years, below the poverty line) to those targeted to much more limited groups (e.g., microchip producers, construction companies in one congressional district). The former reward politicians for favorable policies by voting; the latter reward politicians by lobbying or making campaign contributions. Policies that benefit broad segments of the electorate are analogous to spot or arms-length economic transactions. Large unorganized groups of politicians provide benefits to large unorganized groups of voters and receive votes in return. More specific policies are analogous to economic transactions involving important contractual considerations. Small groups that rely on favorable policies develop long-term relationships with

13. This effect is stronger inasmuch as the costs of organization are lower for factor owners specific to a certain industry than they are for a mobile factor that is employed throughout the economy (Alt & Gilligan, 1994).

politicians in which both parties to the political exchange are to an extent "locked in," such that the politicians rely on the groups' support whereas the groups rely on the politicians' delivery of benefits.

We would expect the former sort of politics in arenas where assets are not particularly specific to policy and where monitoring and enforcing the delivery of benefits (and the identities of those entitled to benefits) is relatively easy. We would expect the latter sort of politics in arenas where assets are specific to policy and where monitoring and enforcing the delivery of benefits is relatively difficult. This would appear to explain some of the difference between, say, the politics of Medicare and that of trade policy. Rubin (1975) speculates about interest groups and legislation in these terms, and Verdier (1994b) offers a parallel view of welfare policies.

INTERDEPENDENCE OF POLITICAL CONSTITUTIONS AND ECONOMIC STRUCTURE

Let us take the argument a step further to see how the degree of asset specificity in an economy could come to depend on political institutions as well as these lock-in incentives. Suppose that in some sector, firms cannot invest in specific assets unless some form of protection is guaranteed. Also suppose that the marginal benefits provided by such specific investments (with protection) would be instrumental in securing majority support for the political representative. Then the political representative would improve reelection prospects by providing the protection that keeps the firm tied in to a specific location. Where political representatives have less incentive to provide protection—for example, because they do not represent geographically based constituencies—firms will on average have less incentive to invest in specific assets. In such cases where, for instance, the national parliament is elected from a national list of candidates, the economy should be (other things equal) characterized by greater factor mobility. We would expect less factor mobility (other things equal) where members of parliament are powerful, tied to single-member districts, and independent of party. Thus we can hypothesize that an economy characterized by a high level of asset specificity is likely to be the product of a political system that allows firms to protect specific assets by forming durable relationships with political representatives.[14]

14. Williamson (1988) makes a similar point, namely that countries whose laws inhibit equity ownership (i.e., force firms to finance through debt) will have suboptimal levels of investment in specific assets because only nonspecific assets have ready resale markets in case of foreclosure.

PREFERENCE INTENSITY AND ASSET SPECIFICITY

In the industrial organization literature, asset specificity is a crucial explanatory variable that takes a whole range of values. However, much of the endogenous tariff theory typically assumes that factor specificity takes only one of two values, full or none.[15] It is obvious that this simplification, perhaps justified for simple modeling purposes, is not particularly useful for analytical applications. It is precisely the range of variation in factor or asset specificity that can provide explanatory power.

This implies that one major goal of modern political economy should be to come up with theoretical and empirical analyses incorporating variation in asset specificity. For example, endogenous tariff analysts might try to find proxy measures for factor specificity and see whether they are correlated with the demand for trade protection across a wide spectrum. The same exercise might be undertaken in regard to other government subsidies and incentives; the more specific the asset, the more its owner will have "intense preferences" for policies to increase the rents accruing to it.

MEASUREMENT

In these and other ways, students of trade policy can adapt insights from industrial organization and the new economics of institutions to test the extent to which specificity is an important explanatory variable. Particularly, we can learn whether it explains the propensity to participate in various forms of political activity and the resulting patterns and extent of political coalitions and interest groups as well as whether the decision to undertake highly specific investments is itself conditionally more likely under certain institutional conditions. But to do any of the investigations described in the preceding, one would require measures of specificity across either firms, industries, or sectors. Because trade policy is by no means the first or only field in which scholars have explored such possibilities, there may be much to learn about measurement in the research of others.

However, while the studies proliferate, the situation that Joskow (1988) describes of wishing for "more empirical support" for the perspective he "found so intuitively appealing" still pretty much holds, even though the number of other subfields in which some form of specificity appears as an independent variable is rising rapidly. One can, for example, find such work

15. We are referring here not to trade theory per se, which has extensively modeled situations with some but not full mobility (Grossman, 1983; Mayer, 1974; Mussa, 1974; Neary, 1978), but to the endogenous tariff literature that seeks to explain trade policy outcomes (Findlay & Wellisz, 1982; Grossman & Helpman, 1994; Magee et al., 1989; Mayer, 1984, among many others).

in industrial relations literature. Jacoby (1992) reviews efforts to examine whether skilled labor is less mobile and whether this is reflected in contracts or other policy-related indicators. (By the way, the studies cited by no means show unambiguously that skilled workers reveal greater specificity.) Norris (1992) provides an example from development economics, arguing that if women's labor is specific to food production, then their economic fate is tied to that of land. In that case, the effect of economic development on the female real relative wage is not necessarily positive. In an application from corporate finance, Boot (1992) argues that if specific assets cannot be sold off as easily, then posttakeover divestitures should be less common in the case of taken-over firms whose assets were more specific.

Relative to the amount of use the concept of specificity is getting, attempts to do serious measurement work are surprisingly infrequent. In the example just mentioned, Boot (1992) cites "high-tech" industries as an example of those in which assets were more specific, but he presents no data. Nor is this unusual. For the most part, the literature treats specificity as parametric. Studies in many fields assume that if specificity is present, then its derived consequences (e.g., relative frequency of posttakeover divestitures) serve as measurements. In some cases, where the literature seems to have established some agreement on the explanatory power of the concept—as, for example, in the role of specificity in motivating vertical integration—using such derived measures may be appropriate. In many other cases, this would be less true.

In principle, the measurement of asset specificity has to take account of the multiple sources of specificity and be capable of measuring specificity at a variety of levels of aggregation. Joskow (1988) summarizes Williamson's (1983) definition of four types of specificity. At least at the firm level, he regards the concept as referring to the following:

a. *site specificity*: that assets are highly immobile and that their value depends on their location, which maximizes value as it minimizes transport costs;
b. *physical specificity*: that investments involve design characteristics specific to particular transactions;
c. *human specificity*: relationship-specific human capital often derived from learning by doing; and/or
d. *dedicated assets*: general investments based on the prospect of continuing sales to a particular customer that would otherwise result in significant excess capacity.

Recombining a little, we can see that *physical* specificity is in a sense the most general, referring to an asset's use in one or a few processes or products. Sometimes the nature of the process or product makes the asset specific, but

other things count as well. Another source of specificity is *geography* or location of assets. Joskow and Schmalensee (1983) discuss the classic example of a power generator located at the mouth of a coal mine. Because neither the mine owner nor the generator owner particularly wants to move, each protects himself or herself against the other's ex post opportunism with detailed contingent contracts. (The categories are not mutually exclusive. Some assets might be both most useful in one transaction and costly to move to a similar use elsewhere.) The specificity that inheres in *humans* really is of two quite different sorts. First, there is the capital that people internalize in the form of education, skills, and specialized knowledge that is specific to individuals but not necessarily to particular uses.[16] Then there is the relationship-specific capital that can exist between people: feelings of trust, familiarity, understanding, and/or predictability that may not transfer out with just one of the parties in a relationship.

We can distinguish four approaches to the measurement of specificity, which we discuss in the following. We call these a "rate-of-return" approach, a "lobbying" approach, "expert survey" measures, and "indirect" measures. The last approach, in particular, covers a wide range of possibilities. Because specificity could be a characteristic of individuals, we would like to be able to measure it at that level or at least within and across firms. Ideally, we would want to be able to measure average specificity at higher levels of aggregation as well for industries, sectors, and even whole economies. As we shall see, different ways of measuring specificity have varying feasibilities at different levels.

The *rate-of-return* approach to specificity is exemplified in the Grossman and Levinsohn (1989) rate-of-return calculation. Theirs is a significant method of demonstrating at least the existence of significant entry barriers (which would be a consequence of specificity) by measuring and estimating continued excess (above market average) returns to the shares of companies in certain sectors or industries. The basic idea is that one first allows for other sources of variation in rates of return on shares. These include the effects of changes in income, relative prices, and so on as well as a variety of industry-specific and time-specific "fixed" effects. Then one sees, using regression methods, whether firms in some industries experience *persistently* above-average returns following an unanticipated increase in the price of competing imports, with the extent of such excess returns indicating the degree of specificity. The point is that if excess returns persist, then capital cannot be flowing freely into those industries; that is because if capital flowed freely,

16. This also includes firm-specific human capital such as knowledge of a firm's procedures, machinery, and so on.

it would equate returns across industries or sectors over time. Thus there must be some barriers contributing to, and specificity reflecting, the excess returns. Again, to some extent, the asset specificity might be the source of these barriers, whose nature is not directly addressed in this method. This approach applies easily at the industry and higher levels of aggregation, something not true of every other approach.

The pioneering *lobbying* article that demonstrated the existence of specificity (even if not measuring its extent) was Magee's (1978) "Three Simple Tests of the Stolper-Samuelson Theorem." Magee's idea was to look at the "policy stance" of interest groups representing different factor owners within industries or sectors by counting the frequency of pro- and anti-protection testimony at congressional hearings. The logic of the method is simply explained. As we already have seen, if factors all are mobile, then all owners of a factor benefit from a change in relative prices affecting their factor, regardless of the industry in which it is employed. Therefore, if factors are mobile, then all owners of capital, regardless of industry, should join together in favoring or opposing some change in trade policy that affects the relative value of capital. On the other hand, if factors are specific and do not flow easily across the boundaries of sectors or industries, then one should find representatives of, say, capital and labor joining together within industries around common policy stances, collective action costs apart.

One could perfectly well count other forms of activity besides testimony. However, the difficulty of taking this approach beyond a qualitative comparison of patterns into measuring the extent of participation across industries is that one is measuring the dependent variable rather than the independent variable to some extent. That is, this testimony is the behavior that a measure of specificity should help you predict; therefore, if one is trying to explain policy outcomes, then there is a big risk of rendering one's conclusions circular. On the other hand, this approach works well at the level of industries and can be extended to the level of firms easily by *survey* methods, and this is an advantage. For example, two Norwegian economists, Berrefjord and Heum (1993), survey changes in the frequency with which firms report contacting members of parliament in ways that could readily be modified to fit an investigation of asset specificity and trade policy.

A common practice in the industrial organization and transactions costs literature has been to use experts and questionnaires to establish the extent of specific assets by getting directly at dependencies, either product-, transaction-, or relationship-specific. Good examples of physical specificity cited by Joskow (1988) include Monteverde and Teece (1982). They consulted an engineer to get an expert opinion about the degree to which the design of different components was specific to different manufacturers' product lines

as well as a wholesaler to determine whether the "brand name" was needed to identify a spare part satisfactorily. (The intuition is that if it is necessary to name a brand to identify a part, then there must be few substitutes and thus probably a good deal of specificity in the assets used in making the part.) Measurement difficulties and requirements in using this approach, other than for a few specific firms, would be formidable. Moreover, few experts are likely to know more than one industry, and so interindustry comparisons may be confounded with interexpert variability, and sectoral comparison are likely to be hard to make using this method. On the other hand, the approach goes inside the firm easily, and, fortunately, we are trying to explain not a characteristic of industry but an amount of policy subsidy or protection distributed across industries; so, in consequence, there is little risk of circularity. A similar approach to human capital was taken by Anderson and Schmittlein (1984), who used a questionnaire about territory characteristics and firm size to get at the specificity of the knowledge of sales forces so as to make predictions about whether they would be integrated into the firms they served.

Since Joskow's article, there have been few published attempts to measure specificity directly, although *indirect* measures have proliferated. An indirect measure is one that is used to measure specificity, even though it actually is something that normally is thought to be a consequence of specificity. If specificity produces complex contracts, for example, then the relative prevalence of complex contracts across industries implicitly measures the underlying specificity. The same logic would allow one to use various forms of organization as indicators of specificity. An article by Acs and Isberg (1991) refers to "asset specificity as measured by innovation" for which they apparently have counts by industry. They predict specificity from research and development (R&D) expenditures, writing that "R&D creates asset specificity because firms that sell products with close substitutes are likely to do less research and development" (p. 324).

Politically, other forms of organization could be used in the same way. The existence of professional societies, craft unions, and trade associations implies a willingness on the part of members to bear some costs to undertake collective action. Their existence, like Magee's (1978) lobbying measure, could be assumed to indicate underlying specificity. They seem like promising ways in which to get at specificity with a human source. Unlike the industrial organization literature, however, the necessary theory is much less developed, and thus the probability of measuring with considerable error is consequently greater.

Given the importance of the concept, there still is much work to be done measuring it. We do seem to be pursuing a closely related set of questions

with the industrial organization literature, and so the exchange should be productive.

IV. FUTURE RESEARCH: THE DEPENDENT VARIABLE

So far, we have assumed that the appropriate dependent variable was *trade policy coalitions*. We have asked "What is the role of factor specificity in explaining trade policy coalitions?" and "Why do certain societal groups choose to take action to restrict trade and other societal groups choose to take action to enhance trade?" Perhaps these questions jump the gun, however, on a couple of issues that are in a sense more fundamental and to which we still are lacking reasonably appealing approaches.

First, an enduring question to which the literature has paid insufficient attention is "Why do we observe trade restrictions in the first place?" This might seem like a silly question. The answer apparently is straightforward: We have protection because import-competing groups are politically powerful. Indeed, there is a remarkable convergence in the economics and political science literatures on what constitutes an "appropriate" model of the political economy of trade policy. As we discussed earlier, the typical strategy is (a) to lay out the implications of different trade policies on the incomes of sectoral groups or of broad factor owners and (b) to deduce the policy outcomes from the organizational strength of these actors and the nature of prevailing political institutions.

Where these models fall short is that they demonstrate only how trade policy *can* play a redistributive role, not why it *should* do so. This is not hair-splitting because trade policy is a tremendously ineffective and inefficient tool for redistributing income in any society. If we take the existing models seriously, then we are left with the following puzzle. If the beneficiaries of trade restrictions are indeed powerful enough to redistribute income to themselves, then why do they not do so more directly? And if indeed there are more direct ways in which to transfer income, as there surely are, then does it not follow that the existing models qualify as models of *trade* policy only because those models have implicitly or explicitly denied the state or the affected groups the use of any other redistributive policies besides trade policies? The puzzle here is not "Why do policymakers choose inefficient *ends*?" Many political economy models answer this question: Industries earn more money from such programs, and legislators gain support by supplying it. The puzzle instead is "Given these desired ends (inefficient or not), why do politicians and interest groups choose such inefficient *means* of achieving them?"

Hence these models do not really address the question at hand. The existing theories fall short because they simply assume (rather than demonstrate) that the end product of political influence is trade policy. The question ought to be not only why certain groups can obtain privileges from the state (which is what the existing theories try to answer) but also why these privileges are granted in the form of trade restrictions. The latter question constitutes a true puzzle in view of the fact that trade policy is a highly inefficient instrument for redistributive purposes. A robust theorem in economics states that, save for one relatively insignificant exception,[17] any policy objective can be fulfilled at lower cost by an alternative policy.

Take the Common Agricultural Policy (CAP) in Europe as an example. The various import restrictions and export subsidies that constitute the CAP are notoriously inefficient. Now, a political economy model that attempts to explain their presence would emphasize the organizational strength and historical importance of French farming groups and the positive income consequences of these trade policies on those groups. Unfortunately, this explanation says nothing about why these income transfers to the farmers have to take the form they do. There are at least three alternative sets of policies (alternative to trade policy, that is) that would achieve the same income transfer at less cost to French society at large and hence, one would think, be politically more palatable than the policies currently in use. In order of decreasing efficiency, these policies are (a) an annual and continuing lump-sum transfer to all French citizens who can demonstrate that farming comprised their primary source of income as of some recent date, (b) an annual and continuing lump-sum transfer to all current and *future* farmers (note that this would be more expensive and in some sense unnecessary given that the government would need the support only of existing farmers, not of future ones), and (c) a production subsidy to farm products, which has the advantage relative to trade policy of increasing prices received by the farmers without raising prices to consumers.

This kind of logic is perfectly general and can be applied to any use of trade policy for distributive purposes. The point is that we will not have an adequate political economy model of trade policy until we confront this issue and try to understand why trade policy is so prevalent when superior alternatives almost always exist. This does not imply that we should hold all other trade policy questions in abeyance until we resolve this one, but it does imply that any answers to those other questions will be based on a model that is not entirely "closed."

17. This is the optimum tariff argument for a country with monopoly power in foreign trade.

Although there are some attempts in the economics literature to deal with this difficult puzzle, they are somewhat far-fetched and on the whole unconvincing (Coate & Morris, 1993; Rodrik, 1986; Staiger & Tabellini, 1987). Rodrik (1994) discusses in greater detail this issue and the economics literature bearing on it. In political science, there is no work on this of which we are aware besides the rather suspect "lack of transparency" argument.

A second question left unanswered by the literature is why import-competing groups on average seem to win out in net against exporting groups. That is, why do trade policies on balance restrict, rather than augment, trade? There exist few countries in the world in which the net effect of trade policies is not to repress trade. (Hong Kong might be the only exception, and even there it is unclear whether there is a *net* bias in favor of exports.) Why do we not see more countries in which export (or import) subsidies dominate? Certainly, there are cases of export subsidies, but their effects almost invariably are more than offset by import restrictions (tariffs and nontariff barriers). Of course, the post-World War II period has been characterized by a relentless liberalization of trade, but this fact only serves to highlight the puzzle. It is trade *restrictions*, not enhancements, that had to be negotiated down. In any case, despite 50 years of liberalization, the net bias of most countries' policies is still against trade.[18]

An interest group approach, whether it focuses on specific sectors or on broad classes, does not account for why interests against trade are invariably dominant over interests in favor of expanding trade.[19] Consider the sector-specific factors model. Here explanation for protectionist bias in industrial countries must be in terms of the superior organizational/political ability of import-competing sectors, such as steel and garments, relative to other sectors. But then consider countries that export steel and garments. If the same organizational/political stories were to apply there as well, then the latter countries would have trade policies that were *pro-trade*, not anti-trade. That is hardly the case.

Next, consider the factor endowments model. Here we would have to argue that scarce factors hold political power. But why always scarce factors,

18. The most detailed and objective sources of comparative information on trade policies are the *Trade Policy Review* reports of the General Agreement on Tariffs and Trade (now the World Trade Organization). A good number of these have been published for individual countries. A sampling of these reports will convince the skeptical reader that governments do indeed devote far greater attention to restricting trade than to encouraging it.

19. Note that this question cannot be answered simply by appealing to collective action problems faced by consumers in organizing against trade protection. Consider export subsidies. The beneficiaries of export subsidies are export industries (or the factors used intensively in such), whereas the costs are borne by the general taxpayers and the domestic consumers of the exportables. Hence the situation is analogous to the import protection case.

even though the identity of the scarce factors differs from country to country? In industrial countries, it is unskilled labor that is scarce; in developing countries, it is skilled labor and capital. Why is it that unskilled labor dominates in rich countries, whereas capital dominates in poor countries? Hence it appears that political power in the trade arena is not associated with properties of specific industries or of specific classes (productive factors). It is associated with what side of the trade divide you are on. Are you an import-competing sector or factor, or are you an exporting one? But why should that be so?

One potential explanation combines the state's historical reliance on trade taxes with an argument about status quo bias in policy. Poor countries tend to have few "tax handles." Therefore, taxing trade is a natural solution to such states' revenue needs. This was as true in the past in the currently industrialized countries as it is today in, say, sub-Saharan African countries. This explains why the status quo in most instances comes in the form of anti-trade policies. Next, we need to add an argument about why the status quo exerts a powerful influence on future policies, even when the set of available tax handles expands during the process of development. Fernandez and Rodrik (1991) provide one such argument: As long as changes in policies create some winners (and losers) whose identities cannot be determined ex ante, policies tend to perpetuate the status quo ante. This is so even if the status quo is politically inefficient (in the sense that all relevant political groups would be made better off after the policy change) and individual decision makers are risk neutral, fully rational, and forward looking. The reason is the following. Due to uncertainty, collective choice may result in two kinds of "errors": Reforms that make some veto groups worse off are adopted or, conversely, reforms that would have made all relevant groups better off are *not* adopted. The bias toward the status quo results from the fact that the former kinds of mistakes are reversible (by a return to the status quo), whereas the latter kinds are not.

V. CONCLUSION

This article has reviewed the existing literature on the formation of interest groups in trade policy. The literature relies heavily on concepts of factor specificity—the costliness with which factors of production can move from their present use to another use—but other concepts such as the stringency of collective action costs and the types of political institutions are also important determinants. There are two classes of models that help us predict trade policy coalitions. The first class includes economic models that generate

predictions about the preferences over trade policy. They are the Heckscher-Ohlin, Ricardo-Viner, and IRS models. The first two are endowments-based models of trade patterns and are polar opposites on the continuum of factor specificity. The Heckscher-Ohlin model assumes none and, as such, predicts class-based political cleavages on trade policy. The Ricardo-Viner model assumes that some factors are highly specific and, as such, predicts industry-based coalitions on trade policy. The IRS model predicts intraindustry trade with less clear-cut distributional implications; as such, it predicts that trade policy will not be as contentious an issue as the endowments-based models predict it will be. The second class of models offers predictions as to how the preferences predicted by the economic models will be translated into trade policy coalitions and, ultimately, policy outcomes. They point to the importance of collective action costs and political institutions.

We have reviewed the predictions from each model and compared them to the empirical evidence that has been gathered so far. In many cases, predictions have yet to be fully tested (particularly with respect to the role of political institutions). In a sense, then, this survey is a call to the field to begin the work of testing the implications of these models of coalition formation. However, in the cases where empirical evidence has been brought to bear on each model, the evidence is somewhat contradictory. Models perform well in some tests but not in others.

Given this empirical record, there is ample reason for pessimism about the ability of any of these perspectives alone, or even all of them in tandem, to explain the patterns of lobbying on trade issues that we currently observe. As the discussion here has indicated, we first must settle some more basic empirical questions. First, how specific are particular assets, and is this specificity actually increasing or decreasing? Second, to what degree are specific sectors characterized by IRS, and is this aspect increasing or decreasing in importance?

This survey has also presented a framework for the future study of trade politics. Four main conclusions arise from our discussion. First, there are enduring puzzles in the trade policy issue area. We have begun asking why certain *groups* coalesce on trade policy. In addition, we should be asking why certain groups coalesce on *trade policy*. In other words, a different emphasis to the question should be addressed. What is perhaps more puzzling than whether this or that segment of society lobbies for redistribution to their group is why they would lobby for redistributions through such an inefficient policy. A second puzzle is why protectionist groups always seem to remain politically powerful, even though they have weakened over time. There are virtually no cases of countries whose policy is in net to enhance rather than

restrict trade. Why can't exporters effectively compete politically with import competers?

With those issues still very much unresolved, we agree that on the simpler issue of which groups form and why, factor specificity must play some role. We also admit that each model is in some way correct depending on the circumstances. Our second point, then, is to make a call for eclecticism. We will never have a single model that explains most of what we want to understand about international trade. A model that can shed light on broad historical changes in trade policy can be entirely inappropriate for understanding the CAP or U.S.-Japan trade. A model that is relevant to the latter type of issues in turn can be entirely unhelpful in understanding why so many Latin American countries recently have rediscovered the virtues of free trade. Right now, our problem is not that we have too many models but that we have too few. Let's let the few that we have happily coexist.

In that quest for new models, our third recommendation is to look to the new economics of institutions for theoretical insights. Notions of factor specificity in trade are very close to notions of asset specificity in industrial organization. We believe there would be "gains from trade" between these two literatures.

Our fourth suggestion stems from the others: Rather than worrying about the answer to the impossible question of which model is better for all time and space, we should get on with the business of measuring specificity and test its effects on interest groups' behavior. We have borrowed several attempts at measuring specificity from the industrial organization literature and suggested that these may be promising avenues for research in our own field.

Our main purpose on this article, then, has been to point out that puzzles remain even in the well-tilled territory of trade politics—puzzles for which models or explanations of any kind have yet to be created. Further, even where models proliferate, our understanding is far from complete. Clear and testable predictions flow from these theories, but testing has barely begun or has produced contradictory results. We encourage the field to get on with tackling these new puzzles and testing the literature's answers to the old puzzles. The result will be a useful and important body of work about societal groups' political action.

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