The Distinct Political Economies of Trade and Migration Policy:
Through the Window of Endogenous Policy Models

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The domestic politics of international trade seem to differ in fundamental ways from the domestic politics of immigration, but it is difficult to say exactly how and, more importantly, why. This paper uses a common frame of reference, simple endogenous policy models, as a way into this issue. These models capture the essential insight underlying much political economy analysis that material interests drive policy preferences. Part of the claim made in this paper is that trade politics appear to be essentially about material interest, while immigration politics are not. Related to this claim, however, is the complementary claim that the politics of these two seemingly similar issues are also organized in fundamentally different ways. The argument proceeds in five parts. The first part describes the general structure of endogenous policy models; the second provides a brief overview of theoretical and empirical work on trade; the third provides a similar overview of contemporary research on immigration politics; the fourth compares the political structures of the politics in these two domains; the fifth section concludes.

I. What are Endogenous Policy Models, and What Are They For?

Endogenous policy models formalize the notion, widely applied for virtually as long as there has been systematic study of economic policy, that material self-interest has a sizable effect on the policy preferences of citizens, and that the policy preferences of citizens has a sizable effect on the policies chosen by rulers. Both of these statements should strike us as completely unexceptionable. In particular they commit us neither to a thoroughgoing materialism, nor to any particular understanding of how citizen preferences are mapped into final policy determination. As such they rule out virtually nothing and are, thus, virtually useless on their own as a theory of policy determination. The strategy of endogenous policy modeling proceeds from the radical simplification that these two attributes fully characterize the preferences of citizens and policy-makers. All that remains is to develop the specific political and economic context and results/predictions can be derived.

More formally, endogenous policy models build some form of explicit political structure into some form of neoclassical general equilibrium model. That is, the underlying economy is made up of households, characterized by preferences over final goods and portfolios of productive factors, and firms, which transform factors of production into final goods.[1] We can denote a neoclassical economy: $\mathscr{E} = \{\boldsymbol{Z}, \boldsymbol{F}, \boldsymbol{R}\}$, where $\boldsymbol{Z}$ is a matrix allocating factors of production among households, $\boldsymbol{F}$ a vector of technologies, and $\boldsymbol{R}$ a vector of household preferences over final commodities.[2] To this economy we attach a vector of possible interventions ($\boldsymbol{t}$) and a political mechanism (M) yielding a political-economy: $\mathscr{P} = \{\boldsymbol{Z}, \boldsymbol{F}, \boldsymbol{R}; \boldsymbol{t}, M\}$. The easiest part of the analysis of a system like $\mathscr{P}$ involves the derivation of citizen preferences over policy. For a given household, h ($R_h \in \boldsymbol{R}$, $\boldsymbol{z}_h \in \boldsymbol{Z}$), for a fixed $\mathscr{E}$, we simply ask how any relevant state of the policy variable ($\boldsymbol{t}$) affects household welfare. By answering this question for every feasible state of the policy variable, we trace out household political preferences over that policy.

Just as knowing preferences over final goods is not sufficient for a theory of market equilibrium, knowledge of political preferences is not sufficient to determine political-economic equilibrium. In principle, a model of political action must be combined with a model of policy

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[1]Under constant returns to scale and perfect competition the number of firms is indeterminate, but also irrelevant. That is, the industry acts as if it is being operated by a profit-maximizing/cost-minimizing firm under a zero-profit constraint. Thus the unit of analysis on the supply side of the economy is the industry. As a result, in the formal analysis, we need to keep track of: households ($h \in H$); factors of production ($i \in I$); and final goods ($j \in J$).

[2]One of the extraordinary accomplishments of modern economic theory has been to prove that a decentralized equilibrium of this sort of system exists. However, to go beyond existence (and a couple of technical properties of the equilibrium) requires considerably more structure.

determination to determine a full political economic equilibrium.[3] Here there are many possibilities. Two approaches short-circuit the need for models of this sort: the single-issue referendum approach and the political response function approach. Under the assumption that voting is costless (and the assumption that preferences are single-peaked over a one dimensional *t*), the first approach simply determines the policy outcome at the most preferred point of the median voter. No resources are used up in the political process, there are no gains from misrepresentation of preferences, so once citizen policy preferences have been determined, the step to final policy determination is trivial. This ease is achieved at the cost of radical simplification of both political action and policy determination. Where referendum models adopt a radical simplification, the political response function approach simply does away with any attempt to theorize the supply side. Specifically, following Peltzman (1976), this approach represents unexplained (but completely plausible) differential group organization in terms of differential responsiveness of an untheorized function. Lobbying models, with a passive register state, introduce an explicit analysis of costs of political action. However, since policies are in the nature of public goods and expenditure on policy by any individual has an effect on all individuals, the theory of political action is far from compelling. Furthermore, it is not clear that simple venality (i.e. the selling of policy) is obviously superior to determination by referendum, and both lobbying and voting models can be seen as specific forms of Peltzman's black box. A recent innovation extends the Becker-type model to incorporate an active policymaker by applying the menu auction version of a common agency model (Bernheim and Whinston, 1986), thus endogenizing the political response function (Grossman and Helpman, 1994). That is, the policy-maker offers a menu of policy-outcomes associated with patterns of bids by various organized groups, and the equilibrium is determined by the menu offered and the pattern of bids, where these must form a Nash equilibrium. While formal theorists in political science have developed models of electoral competition and bureaucratic policy determination in considerably

[3]It should be noted that virtually no endogenous policy models involve politically active firms. This follows from zero profits, as discussed in footnote 1. In models with specific factors, one might take the unit of organization to be the firm, just as in models with perfectly mobile factors we might take the unit of organization to be factor-based. However, fundamentally, these

more detail, these have rarely been applied in the endogenous policy context. Where they have, in particular in models of endogenous macroeconomic policy determination, the underlying models of the economy tend to be radically simplified.

Endogenous policy theory, in virtually all applications, was first developed to account for "bad policy"--i.e. deviations from the optimal policies prescribed by economists. For example, the standard motivation for research on the political economy of trade policy proceeds by pointing out that virtually no contemporary or historical illiberal trade regime can be satisfactorily rationalized as welfare maximizing.[4] Various political economy models illustrate the fact that plausible forms of political interaction *can* account for patterns of policy of the sort that we observe.[5] However, this is not much more than providing some substance to assertions of the form "that's a political economy problem". That is, it illustrates a possibility without providing a clear prediction about outcomes. As we have already noted, while bad policy choices are not at all uncommon, it is not clear that they are any more uncommon than good policy choices.

A closely related, also essentially negative, accomplishment is to help desimplify thinking about "good" and "bad" policy. Consider the following rather deep problem from Mayer's (1984) fundamental paper on the political economy of trade policy. For a small, open economy of the Heckscher-Ohlin-Samuelson sort, free trade welfare-dominates protection.

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models run from citizen preferences, with no other input on the demand side.

[4]This is just as true of illiberal elements of essentially liberal trade regimes. Thus, while there is a substantial literature on the political economy of administered protection in industrial countries, there is virtually no research by economists on the historically striking liberal-ness of all industrial countries. Apparently, there is no need to explain good policy. While the definition of "good" and "bad" policy may vary, essentially the same is true of political scientists. Thus, among those that see trade activism as "good" policy, the question is how government comes to adopt a relatively liberal policy.

[5]There are now some very good general texts in what is essentially endogenous policy theory. The broadest in coverage is Persson and Tabellini (2000); while Drazen (2000) focuses on macroeconomic policy; Laffont (2000) develops informational and constitutional issues; and Dixit (1998) presents a transactions cost approach. Roemer (2001) and Grossman and Helpman (2001) develop advanced methods of political modeling that are consistent with endogenous policy modeling.

However, in the case of single-peaked preferences over one-dimensional policy, majority-rule satisfies all of Arrow's axioms for a democratic social choice function. And yet, except in the razor's edge case where the median voter has the same endowment as the aggregate endowment for the economy, the social choice will be for positive intervention. Thus, if we started from an imposed equilibrium at the welfare-maximizing policy of free trade, the democratically correct social choice would be some welfare-inferior level of protection. What are we to make of "good" policy in this context?

Qua social science, we presumably expect something more from a body of theory than the formalization of an argument or a cautionary tale. Research on endogenous policy modeling has moved beyond these essentially philosophical applications in most subfields, though the extent of motion varies quite widely across them. The most successful extensions from theory to systematic empirical research, and back again, are in local public finance and macroeconomics. In both cases it can be reasonably argued that the policy environment is reasonably well-understood and that policy is determined to some considerable extent by referendum. In the area of school finance, the policy really is often determined by referendum and people really do see the issue quite straight-forwardly in terms of material costs and benefits. As a result, there is now a substantial body of empirical research that quite successfully applies endogenous policy models in this area (Ross and Yinger, 1999; Rothstein, 1994; Epple, Romer, and Sieg, 2001). For macroeconomic policy the links are a bit looser, but the data are quite good and the stakes are high. Thus, macroeconomic models are relatively simple, if highly contested, and it is not too violent a simplification to see national elections as referenda on macroeconomic policy. As a result, there is an enormous literature on the political economy of macroeconomic policy which seems, under any reasonable definition of these concepts, theoretically and empirically progressive (Drazen, 2000). The story is less clear when we come to the cases that concern us most directly: trade and immigration policy. We will see that there are fundamental differences between the two in terms of both the extent to which citizen preferences can be represented by relatively straight-forward materialist self-interest, and the policy-making environment can also be given a relatively simple representation. Finally, it will prove important for systematic empirical work that, as in the cases of local public finance and macroeconomic policy, policy choice is made repeatedly so as to permit the collection and analysis of data.

II. Trade Policy in Endogenous Policy Models

The theoretical and empirical literatures on the political economy of trade policy are both immense, though only very weakly integrated. In the last fifteen years, there have been no fewer than a dozen major surveys of one or another aspect of these literatures, the papers on which they are based run into the hundreds.[6] The area of greatest success in the application of endogenous policy modeling to trade policy has been in the construction of plausible accounts of suboptimal policy of a general sort. That is, this work explains how policies different from welfare-optimal policy *could be* adopted, without making particularly strong predictions about what policies *would* be adopted. The proliferation of variants on referendum, response function, lobbying, electoral competition, and agency models has been rapid. However, given that the essential point is made quite clearly in the key contributions on voting (Mayer, 1984) and lobbying (Findlay and Wellisz, 1982; Young, 1982), the value-added of many of the later papers must be judged to be rather small. Similarly, much of the empirical research, while increasing our knowledge of the correlates of protection, is essentially *ad hoc* and, in particular, unrelated to theoretical models of political economy. This disjuncture should not surprise us given the very different demands on theory for these tasks: the abstract point-scoring task requires a minimal model that illustrates a plausible causal channel; while for a theory to be a compelling basis for empirical work, it must find some fundamental attachment to the empirical phenomenon.

Nonetheless, while recognizing that the literature on the political economy of trade cannot claim the sort of cumulative interaction between theory and research that characterizes work on the political economy of macroeconomic policy or of local public finance, there have

[6]Among the major surveys: Anderson and Baldwin (1987), Ray (1987), and Gawande and Krishna (2003) emphasize the empirical work; Nelson (1988), Hillman (1988), Baldwin (1989, 1996), Riezman and Wilson (1995), and Helpman (1995) emphasize theoretical work; and Rodrik (1995), Frieden and Rogowski (1996), Alt, *et al*. (1996), Deardorff and Stern (1998), and Milner (1999) aim for critical synthesis.

been some genuine attempts to move in this direction: identification of policy preferences; identification of situations in which the model seems to bear some relatively close relationship to the actual environment; and efforts to bring more detailed institutional structure into the model in such a way as to produce plausible hypotheses which can be evaluated empirically.

One fundamental question that has recently seen a revival in interest is the empirical identification of preferences over trade policy. The theoretical problem is straight-forward: under the maintained assumptions of the underlying model of the economy, the welfare of a given individual/household can be represented by it's *indirect utility function*:[7]

$$v^h(\mathbf{p};\gamma_h) := \max_{\mathbf{x}_h}\left\{u^h(\mathbf{x}_h)\middle|\mathbf{p}\bullet\mathbf{x}_h \leq \gamma_h\right\}, \quad (1)$$

where $\mathbf{p}$ is the vector of commodity prices, $\mathbf{x}_h$ is the household's consumption bundle, γ_h is the household's income, and $u^h(\bullet)$ is the household's utility function (derived from R_h–household preferences over possible consumption bundles) . For our purposes, what is relevant is that utility is decreasing in the elements of $\mathbf{p}$ and increasing in γ_h. Furthermore, we suppose that household income is made up of factor-income plus any governmental transfers:

$$\gamma_h = \mathbf{w}\bullet\mathbf{z}_h + G_h, \quad (2)$$

Where $\mathbf{w}$ is the vector of returns to factors of production, $\mathbf{z}_h$ is the household's endowment bundle, and G_h is the government transfer to household h. Most trade interventions will affect $\mathbf{p}$ and any income transfers induced by the policy will enter through γ_h. These effects are easy to sign since the signs are not particularly dependent on the structure of $\mathscr{E}$. The other major channel of effect, however, is generally dependent on the details of $\mathscr{E}$. The problem is that changes in $\mathbf{p}$ will cause changes in $\mathbf{w}$, thus indirectly affecting γ_h. In the 2-factor × 2-good Heckscher-Ohlin model, in which factors are costlessly mobile between productive sectors, the *Stolper-Samuelson theorem* makes a clear prediction that an increase in the relative price of one good will result in

[7] $v^h(\bullet)$ is the maximum value function associated with the consumer's optimization problem

an increase in the real wage of the factor used intensively in the production of that good, and a reduction in the real wage of the other factor. That is, policy preferences will be driven by factor-ownership. By contrast, if factors are completely immobile between sectors, the Cairnes-Haberler model, policy preferences will be determined by the sectoral allocation of factors. Between these extremes are a number of possibilities.[8] As is widely noted, it is a definitional attribute of the long-run that all factors are fully mobile and of the short-run that none are. Thus, as a practical matter, the issue of the relevant model for analyzing the politics of trade is not so much a matter of technology as of perception. That is, rational people will evaluate policies in terms of the time horizon over which they make political calculations (see, e.g. Hall and Nelson, 1989).

Given the centrality of policy preferences to endogenous policy models, it is of considerable importance to know something about this time horizon, but this is an essentially empirical question. Here there are three kinds of evidence. There is economic evidence that capital responds to shocks as if it were relatively fixed. Specifically, an important paper by Grossman and Levinsohn (1989) used a capital-market event study methodology to examine the response of owners of firms to a variety of shocks, concluding that these responses were consistent with perceptions of relative capital fixity.[9] Magee (1978) developed a clever indirect methodology of using information on the trade lobbying behavior of labor and capital as evidence of revealed policy preference, and found that labor and capital tended to lobby together, concluding that the politically relevant time horizon was relatively short. There has recently been an upsurge of interest in this method with papers by Beaulieu (2002 a&b), Beaulieu and Magee (2004), and Hiscox (2001). Hiscox develops a sophisticated extension of Magee's method, and extends the analysis to the period 1824-1994, providing support for Magee's

[8] The most commonly studied, primarily because of its tractability, is the specific-factor (or Ricardo-Viner) model, in which some factor is costlessly mobile and all other factors are sector-specific. See Jones (1971) or Mussa (1974). Among others, Hill and Mendez (1983) consider the case of general imperfect mobility.

[9] Thompson (1993, 1994) are similar studies. Recent papers by Ramey and Shapiro (1998, 2001) provide some direct evidence of capital fixity, while a substantial literature in labor economics finds labor to quite fixed over economically relevant periods (e.g. Topel, 1994 a&b).

general conclusion, while showing considerable variance over time. Beaulieu and Magee use campaign contribution data, concluding that, while there is some evidence of both factor and industry effects, the effects of factor ownership (i.e. long-run calculation) seem to be dominant for capital, while labor may be more immobile over the politically relevant period. Finally, recognizing that these politically revealed preferences contain information about both preferences and political institutions, there have been some recent attempts to study preferences directly. Balistreri (1997), Scheve and Slaughter (2001a), Mayda and Rodrik (2003) and O'Rourke and Sinnott (2002) have all used public opinion data to examine the pattern of preferences on trade policy, with Balistreri and Scheve/Slaughter reporting findings supportive of the HO/long-run/mobile factor model, O'Rourke/Sinnott also providing support of a more nuanced sort, and Mayda/Rodrik providing an argument in favor of a shorter-run perspective. The latter two studies also find considerable support for ideological factors in determining policy preferences. While there is still considerable disagreement in this literature, all of this work has attempted to more closely link the underlying theory to the empirical work in ways that hold great promise. However, the study of preferences is, in some sense, the easiest part of the program of systematically evaluating and applying endogenous policy modeling to trade policy.

The essential problem in developing empirical frameworks embodying endogenous policy models is that, over the period for which we have relatively reliable data on the relevant variables, trade policy has rarely been determined by referendum. However, there have been a small number of situations that were either, like school bonds, actual referenda on trade questions or electoral contests in which trade was a sufficiently prominent issue that, as with macroeconomic policy, they could be treated as referenda. In either case, there are two sorts of research question: the first asks whether the electorate behaves as predicted by the model; the second derives a comparative static implication of the model and asks whether the implication is observed. We have just seen that the first type of question faces certain difficulties in that there are no clear predictions about behavior without essentially empirical auxiliary assumptions about the underlying economic model in use by agents. The largest problem is the rarity of trade policy referenda. However, Switzerland has, on at least two occasions, held referenda on trade policy questions: a proposal to impose tariffs on processed food (1975); and a proposal to replace subsidies on sugar with tariffs (1986). Weck-Hannemann (1990) presents an analysis of these. The author focuses on industry as the basic unit of political organization/calculation, and finds statistically significant evidence of industry effects on proportion of vote in favor of both measures. That is, the presence of an industry protected by the tariff increased support it. Other than this exceptional case, attempts to directly evaluate referendum models involve a claim that some general election, or set of general elections, turned to a considerable extent on a trade policy question. Most of these analyses involve periods prior to the development of Keynesian policy instruments. Thus, Johnston and Percy (1980) examine patterns of voting in Canada's landmark 1911 federal election in which liberalization of trade with the U.S. ("reciprocity") was a key issue, while Irwin (1994, 1996) considers British general elections in1906 and 1923 in which Conservatives made "tariff reform" (i.e. increased protection) a key element of their platform. As with Weck-Hannemann, all three of these analyses find some support for an industrial basis of voting. Of particular interest is Irwin (1996), in which the author also attempts to carry out a class/factoral analysis of voting, ultimately concluding that "the occupational regressors explain the election outcome better than the class regressors" (pg. 71).

An alternative approach to estimating/evaluating the referendum model involves deriving a comparative static proposition from the model. Dutt and Mitra (2002) focus on the relationship between inequality and tariff in a median voter model with a H-O structure. Dutt and Mitra consider a model, like that of Mayer (1984), in which every household is endowed with one unit of labor and some amount of capital k_h. Thus, they treat inequality as inequality in the distribution of capital. In this model, it is straightforward to show that increased inequality in a capital rich country results in an increased tariff, and vice versa for a capital poor country.[10] The authors, in fact, find support for the prediction in their empirical work. This work, however,

[10] That is, for the capital rich country, a tariff raises the return to the scarce factor (labor) and, as a result of unequal distribution, the median voter has a large endowment of labor relative to the economy as a whole. Thus, as inequality rises, the median voter is increasingly capital poor and prefers an increasing tariff. *Mutatis mutandis*, the same logic applies to a labor rich country.

makes the very strong assumption that trade policy (the average tariff) is determined in all countries in their sample by referendum. While the authors are explicit that this is not the case, the argument they give to support the assumption is weak. A somewhat less heroic approach is taken by Hall, Kao, and Nelson (1998). As in the work of Johnston and Percy, and Irwin, the authors appeal to the historical record to argue that the tariff was a fundamental continuing issue between Republicans and Democrats in the period from the end of the Civil War (or at least from the 1880s) until the Reciprocal Trade Agreements Act of 1934. With this warrant to treat general elections as tariff referenda, the authors then argue that female franchise would tend to produce a downward shift in the equilibrium tariff. The empirical analysis presented in the paper is strongly consistent with this hypothesis.

The Hall/Kao/Nelson analysis constitutes a nice bridge to the final body of work seeking to forge a closer relationship between theoretical and empirical work: institutional comparative statics. The virtue of a comparative static analysis in the referendum case derives primarily from the lack of access to detailed data related to policy preferences. When we move into the modern period, the need for some form of comparative static analysis becomes more pressing. The problem is that, with the exception of the two Swiss referenda already mentioned, trade policy is virtually never the policy being considered by parties or voters. Trade policy is important continuing policy in many countries, but the form of politics is overwhelmingly lobbying. This implies two fundamental problems, one practical, and the other conceptual. Practically, the data on lobbying take two forms: public expressions of interest (mainly in the form of testimony); and general lobbying expenditures. The former give little information about intensity of interest, while the latter cannot generally be associated with a particular policy. At a conceptual level, while virtually all endogenous policy models applied to trade policy to date treat lobbying as simple venality, contemporary research on lobbying by political scientists seems to have increasingly moved away from this interpretation in favor of interpretations stressing access and information transfer (Austen-Smith, 1997).[11]

Thus, while there are a number of analyses of Congressional voting on trade legislation that introduce contribution data into essentially *ad hoc* econometric models, along with a variety of other variables, it would be hard to treat these as derived in any way from an underlying model of the political economy.[12] By contrast, analyses that seek to introduce explicit institutional structure in such a way that lobbying incentives change, producing a change in the equilibrium permit explicit evaluation in much the same way as in the Hall/Kao/Nelson analysis. Here we briefly consider the two main cases that have been extensively analyzed: the Reciprocal Trade Agreements Act of 1934; and changing terms of partisan competition. At least since Lowi's (1964) classic reconciliation of the accounts of trade policy making in Schattschneider (1935) and Bauer, Pool and Dexter (1963), it has been clear that trade policy making changed fundamentally around the time of the Reciprocal Trade Agreements Act of 1934. While much of the subsequent literature on the RTAA has sought to explain the legislation itself, several papers have looked directly at the effect of the legislation on the incentives to engage in lobbying. Broadly speaking there are two approaches here: one emphasizes delegation to the executive branch; the other the adoption of a rules-based definition of trade policy. Examples of the first are Lohmann and O'Halloran (1994), who argue that the executive is more prone to consider consumer costs than the legislature, and Bailey, Goldstein, and Weingast (1997), who argue that general authority to negotiate trade agreements induced greater participation by liberalization-seeking firms. In either case, the analysis predicts a downward shift in the equilibrium tariff. Nelson (1989) and Hall and Nelson (1992) offer an account which, following Lowi (1964), emphasizes the changed incentives to lobby when trade policy is defined as a collection of private goods (distributive politics) and when it is a general rule to be applied across a number of industries (regulatory politics). Loosely speaking, this work accounts for a shift, though each implies somewhat different timing. Thus, there is the possibility of systematic time series work on this topic.

An alternative institutional comparative static compares the incentives facing government

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[11] A recent paper by Anderson and Zanardi (2004) develops a model of the political economy of antidumping as deflection based on a model with a sophisticated analysis of political money and information.

[12] Examples of such work include Goldberg and Maggi (1999), Bandyopadhyay and Gawande (2000), and Baldwin and Magee (2000).

under conditions in which the same party controls both the executive and the legislature, and conditions of divided government. In particular, O'Halloran (1994), Epstein and O'Halloran (1996), and Milner and Rosendorff (1997) argue that legislatures under divided government will want to delegate less authority to the executive than under unified government and, under the auxiliary hypothesis that the executive generally prefers greater liberalization than the legislature, conclude that divided government impedes liberalization. This conclusion has been criticized on empirical grounds by Coleman (1998) and Hiscox (1999), while Karol (2000) reinterprets the theory before undertaking a systematic analysis of the data, finding that simple divided government hypotheses are essentially groundless. More than any of the research considered to this point, the work discussed in this and the preceding paragraph seeks to construct a more fully satisfactory account of the political structure generating trade policy outcomes.

III. Immigration Policy in Endogenous Policy Models

When we turn to the analysis of immigration policy we find considerably less application of endogenous policy modeling and most of that is of more recent vintage. The concomitant lack of empirical research on immigration policy with a direct link to endogenous policy modeling is partially a function of the relative immaturity of such modeling, but I will also argue that there are two kinds of problem beyond immaturity: weak empirical evidence of economic impact and strong evidence of non-economic forces in determining preferences; and lack of regular, ongoing politics of immigration of the sort that permits systematic study.

As with trade policy, deriving individual/household preferences over immigration policy is, in principle, a simple task (see Bilal, Grether, and de Melo, 2003). Once again using the indirect utility function in equation (1), and the definition of income in (2), we can begin to see how immigration differs from trade. To focus directly on immigration, suppose that commodity prices (**p**) are fixed. Equation (1) makes clear that the only channel via which immigration can affect household welfare is its effect on income. Furthermore, since there is no initial revenue consequence of immigration we start by assuming that $G = 0$. Now the underlying structure of the economy becomes even more important than in the case of trade policy. Without getting bogged down in the technical details, we note that if there are more factors of production than final commodities, changes in endowments of factors will generally have an effect on relative returns to factors; while in the case of more final commodities than factors of production, as long as certain commodities do not go out of production, a change in the factor endowment has no effect on relative wages.[13] This has implications for interpreting the results of econometric research on the link between immigration and wages as well as for political economy modeling.

Most of the sizable literature seeking to estimate the labor market effects of immigration proceeds from a many-factor × one-final commodity model of the economy, where the commodity is interpreted as national output.[14] While a wide range of econometric techniques, data sets, definitions of the input vector, and time periods have been used, the results have pretty consistently found only very small effects on any native group, with the exception of least skilled workers (defined as high school dropouts) and previous immigrants with the same ethnographic and labor market traits as the current immigrants. Building on a substantial review of this literature, Gaston and Nelson (2002) argue for a presumption that, at least in the long-run, there are more produced goods than factors of production, with the implication that, unless immigration transforms the production structure, adjustment to immigration occurs primarily on the output margin, not on the wage margin.[15] However, if we adopt this presumption, along with the standard assumptions that underlie endogenous policy models, there can be no material

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[13]This latter is often referred to as *factor-price insensitivity* (the single-country version of the more well known *factor-price equalization theorem*). An elementary presentation of this result can be found in Gaston and Nelson (2000), while the appendix to Jones and Scheinkman (1977) presents the relevant analytics in full detail. The expression "factor-price insensitivity", as well as an alternative presentation of the analytics comes from Leamer (1995).

[14]See Hammermesh (1993) for a survey of the relevant methods, and Johnson (1998) or Borjas (1999b) for application to the labor market effects of immigration.

[15] At least some people at the Kiel conference thought notice should be taken of Borjas' (2003) most recent attempt to find large labor market effects of migration. From the perspective of this paper the essential fact is that this work presumes that the relevant labor market is national. That is, Borjas essentially argues that the measured local labor market effects are small due to their embeddedness in a national labor market. Thus, Borjas' results suggest that we should see national politics of immigration. However, as we shall argue below, the politics of migration seem to be much more local than the politics of trade.

income distribution effects from immigration and, thus, no basis for an endogenous policy analysis. Of course, these results are far from dispositive with respect to the relevant assumption for political economic analysis. Perhaps most importantly, they say nothing about the politically relevant time horizon nor about the way in which adjustment costs are considered by agents in their political calculus.

With respect to the first, it is trivially true (as long as production generally involves the application of more than one factor of production and factors are not instantaneously mobile) that there is some period in which there are more factors and commodities. In such an environment, as well as in any environment in which there are some immobile factors in any industry, changes in endowments will generally have distributional effects. As with trade, it becomes important to the analysis to know the time horizon of political calculation. That is, regardless of the long-run consequences, if households generally focus on the short-term effects of immigration, there may well be a disjoint between the econometric results and effects revealed by political behavior. Similarly, the econometric analysis and endogenous policy theory are based on comparative static analysis. Neither allows us to raise the question of how adjustment costs enter the economic or political calculus. However, there is considerable evidence that losing a job entails considerable cost and, less compelling (because less general) evidence from ethnographic studies of the changing ethnic structure of very local job markets, that jobs are lost in ethnic transitions in these local markets (Waldinger 1996 a&b; Rosenfeld and Tienda, 1999). Fears of such transitional costs could well be a substantial source of mobilization on the immigration issue.

Our empirical options for getting a grip on this question are fewer. We don't have studies, like Magee's (1978) and those that followed on trade, of the policy preferences revealed by political activity on immigration.[16] Similarly, while there are studies of labor mobility, on the whole they don't ask precisely the question we would like–i.e. how responsive to wage differentials are internal/native migrants– and we have nothing like the capital market studies

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[16]The occasional flaring up of active migration politics seems not particularly helpful in settling this question. The only strong explanatory factor seems to be economic hard times, not particularly moments of great immigration (in fact, Hatton and Williamson (1998), among many others, suggest that immigration seems to be cyclical–increasing in good times falling in bad).

that shed light on capital mobility in response to trade shocks.[17] Nonetheless, by appealing to the studies on trade, as well as the general studies of local markets (e.g. Topel, 1994 a&b), we can conclude that there is some warrant for a presumption of a non-trivial degree of stickiness in adjustment to economic shocks in general, and there is no reason to assume that migration shocks differ from other macroeconomic shocks in this regard. Thus, as with trade, we turn to public opinion data for some information that might help us understand the link between material conditions and policy preferences. Interestingly, unlike trade policy, where public opinion research has been rather scarce, the opinion literature on immigration is substantial.[18]

Most of the research attempts to distinguish between economic factors (labor market competition and fiscal redistribution effects) and social factors (proximity, political ideology, racism), not between factor-based and industry-based preferences. However, the results of this research are far from irrelevant. A fairly strong finding is that, while economic factors do have an effect on restrictionist attitudes, social factors have substantially larger effects (Espenshade and Calhoun, 1993; Citrin, *et al*., 1997; Burns and Gimpel, 2001; Fetzer, 2000; Dustmann and Preston, 2001; Gang, *et al*., 2002). Other studies have found larger effects of economic factors, while still finding significant effects from social considerations (Espenshade and Hempstead, 1996; Kessler, 2001; Mayda, 2003). Finally, a recent paper by Scheve and Slaughter (2001b), finds strong evidence of a link between labor market skills and immigration preferences. However, since they do not particularly study the effects of social factors, the results are not strictly comparable with the other studies. Overall, just as it is hard to conclude that there is much systematic evidence of economic effect on labor market outcomes, it is hard to conclude from the public opinion literature that there is much evidence of economic motivation behind

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[17]Card's (2001) recent paper on the responsiveness of internal migration patterns to immigration shocks, however, strongly suggests that "intercity mobility rates of natives and earlier immigrants are insensitive to immigrant inflows" (abstract).

[18]Characteristic recent research includes: Espenshade and Hempstead (1996), Citrin, *et al*. (1997), Burns and Gimpel (2000), and Scheve and Slaughter (2001) on the US; Bean (1995) and Betts (1996) on Australia; and Lahav (1996), Dustmann and Preston (2000), and Gang, Rivera-Batiz and Yun (2002) on the EU. Simon and Lynch (1999), Fetzer (2000), and Mayda (2003) provide a comparative perspective.

anti-immigrant politics. Before turning to endogenous policy models of immigration policy, it is worth noting an empirical finding reported in several of the above studies: Hispanic and black respondents tend to express more positive views of immigration than non-Hispanic whites (Espenshade and Hempstead, 1996; Citrin, *et al*., 1997; Scheve and Slaughter, 2001). Since these are precisely the groups that, at least according to the econometric studies of labor market impact, might experience measurable effects, results such as these strongly raise the question of the adequacy of labor market competition as an explanator of immigration policy attitudes.

Not surprisingly, virtually all theoretical treatments of migration policy in an endogenous policy framework adopt some assumption that generates material distribution effects from immigration. The most straightforward approach is to follow labor economists in treating the economy as producing a single final consumption good from the input of whatever factors the analysis chooses to consider. A number of analyses develop this in some detail. As we shall see, this is primarily an input to some form of analysis which seeks to identify political-economic equilibria or, more commonly, to perform comparative static analysis on such equilibria. If we accept, contrary to most of the empirical work that directly addresses this question, that immigration policy is driven by the indirect distributional effects of immigration, endogenous policy theory offers a number of relatively simple strategies for representing the political economic equilibrium.

As in the case of trade policy models, the approach with the least theoretical overhead assumes that policy is determined by direct referendum. If sufficient structure on preferences is assumed to yield single-peaked preferences, the equilibrium policy is determined by the most preferred point of the median voter. A number of analyses pursue precisely this approach (Benhabib, 1996; Hillman and Weiss, 1999; and Grether, de Melo, and Muller; 2001) Because of the simple political structure, referendum models are often a convenient reduced form representation of more complex political processes. As with trade there are not many referenda on immigration, but there was one recent, highly visible and widely studied, example: Proposition 187 in California. Most of these studies emphasize the importance of social factors (Tolbert and Hero, 1996; Alvarez and Butterfield, 2000; Newton, 2000; Hood and Morris, 2000); broad macroeconomic conditions (Alvarez and Butterfield, 2000); and linkage to welfare reform/race relations (Calavita, 1996; Salvanto, 1998); but only weak support for individual effects, party, or ideology (Alvarez and Butterfield, 2000; though see MacDonald and Cain, 1997 for the effects of partisan identification). Thus, at least with respect to the one clear example of a referendum on the question, standard endogenous policy models in which factor-market effects drive policy preferences must be seen as being more-or-less rejected by the data to date.[19]

While immigration politics, contemporary and historical, may be more naturally analyzed in the context of referendum models, it is also useful to consider lobbying-based models. Unlike voting models, in which preferences are costlessly transformed into policy outcomes via the referendum, lobbying is a resource using activity. One specific policy that might be thought of as lobbying-related is the allocation of resources to enforcement of immigration law. Nelson and Xu (2001) build on the important work of Ethier (1986) and Bond and Chen (1987), to endogenize the determination of enforcement activities against illegal immigration. While this paper derives a number of formal results, they rely on a model with more factors and goods, which we have seen has dubious empirical foundations. Hanson and Spilimbergo (2001) develop a theoretical analysis of enforcement activities based on a similar model but focus on the implication that enforcement is negatively correlated with prices in sectors that are potentially high users of illegal immigrant labor (apparel, fruits and vegetables, livestock, and construction). Their empirical work is supportive of this hypothesis.[20]

Recent papers by Facchini and Willmann (2001) and Bellettini and Ceroni (2004) adopt a

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[19]Timmer and Williamson's (1998) study of immigration policy in North and South America in the late-19th and Early 20th centuries comes to exactly the reverse conclusions: income distribution effects dominate (at least in the US and Canada); and neither broad macroeconomic trends nor racism play any role. While interesting, these results rely heavily on constructed data.

[20]Other empirical work has also examined the political economy of enforcement activities over time, focusing on broad macroeconomic variables as explanators: Shughart, *et al*. (1986) also examine output measures (i.e. number deported) but motivate their analysis via a regulation theoretic model; Moehring (1988) studies the evolution of the INS budget over time; and Dávila, et al. (1999) use a Niskanen-type model of a budget maximizing bureau to account for variation over time in the relative allocation of manpower to border versus interior enforcement activity.

variant of the lobbying model based on treating political competition as a menu auction game (Bernheim and Whinston, 1986; Grossman and Helpman, 1994). Specifically, the authors consider factor-protection in a multi-factor $\times$ one final-good economy in which government policy regulates both inflow and outflow of factors.[21] Facchini and Willmann use Grossman and Helpman (1994) strong assumptions on both economic and political structure to derive a simple framework for estimation, which is then applied to data from OECD countries. While the model performs well empirically, it is difficult to know how to interpret the findings. Not only are the results apparently inconsistent with the existing small-to-zero estimates of labor market effects of immigration, but the reliance of the estimating framework on very strong economic and political assumptions is also problematic.

As with trade policy, lobbying models constitute a fairly natural motivation for empirical analyses of Congressional voting, on which there is a small literature. As part of a broad historical analysis of the political economy of immigration restriction from 1890-1920, Goldin (1994) presents an econometric analysis of voting on two bills (1913 and 1915) that would impose literacy tests on immigrants, and one vote to overturn a presidential veto (1915). In the first two analyses, vote is regressed on percent foreign born, percent urban, and immigration rate. The results show the first two negatively related to support for the literacy test and the last positively related. Once again, it is hard to rationalize such a result in a standard endogenous policy model: while the increase in immigration rate might be associated with a negative impact on wages, the effect should be strongest on immigrants. Lowell, Bean and de la Garza (1986) examine voting in the U.S. House of Representatives on the Simpson-Mazzoli bill(1984). In the context of concern with Mexican immigration, this analysis controls for Hispanic population, finding it negatively associated with support for restriction. The authors argue, correctly, that this is not consistent with explanations based on labor market effects. The most sustained analysis of Congressional voting on immigration issues is the important book by Gimpel and Edwards (1999) which reports econometric analysis of every major vote on immigration from

[21] Where Facchini and Willmann consider a competitive labor market, Bellettini and Ceroni consider a unionized labor market. Given the active participation of unions in immigration politics, the latter is a particularly useful innovation. More recently, papers by Amegashie (forthcoming) and Espstein and Nitzan (2002) have also developed lobbying models in which the policy is the size of an immigration quota rather than the degree of enforcement.

1965 through 1996. Consistent with the findings of Lowell, Bean and de la Garza, Gimpel and Edwards find little support for effects of indirect redistributive effects, but find that the issue becomes increasingly partisan over the period of study. Overall, then, nothing in the empirical work– whether focused on actual labor market outcomes, citizen preferences, or the political behavior of citizens or their representatives–provides support for a model of immigration policy-making in which indirect redistributive/labor-market effects drive outcomes.

To this point, we have assumed that the government's only role is to set a level of immigration of some class of factor-owner or to set some form of immigration enforcement. However, precisely because the measured labor market effects of immigration appear to be so small, a considerably greater effort has been made in the political economy of immigration to build in explicit redistributive mechanisms. That is, there is substantial evidence that the presence of welfare state policies, and the taxation necessary to support them, results in considerably larger redistributive effects from immigration than those deriving from labor market effects.[22] As a result, a substantial body of work analyzing the political economy of immigration in the context of a redistributive state has developed (see Cremer and Pestieau 1998). That is, in terms of equation (2), we need to consider the government transfer term, *G*, in considerably more detail. Building on important early work on local public finance, in which people can move among jurisdictions within a federation and then choose (by referendum) redistribution schemes knowing that this will affect interjurisdictional mobility, this work considers labor mobility between nations in which citizens vote on redistributive policies to which immigrants have access.[23] These papers tend to focus on: level of immigrant admission (Lejour and Verbon, 1994; Mazza and van Winden, 1994; Flores, 1997); generosity of transfers/magnitude of tax (Mazza and van Winden, 1994; Scholten and Thum, 1996; Haupt and

[22]The literature here is large and contentious. Some representative studies are: Simon (1996); Borjas (1995); VanHook, Glick, and Bean (1999); and Canova and Ravn (1998). Smith and Edmonston (1997, chapters 6 and 7) and Espenshade (1998) are useful overviews of the issues.

[23]Representative examples of research in local public economics are Rose-Ackerman (1983), Yinger (1985), and Epple and Romer (1991).

Peters, 1996; Cremer and Pestieau, 1998; Razin, Sadka, and Swagel, 1998); and the extension of the franchise to immigrants (Cukierman, Hercowitz, and Pines, 1993; Michel, Pestieau, and Vidal, 1998). Razin, Sadka, and Swagel (1998) contains an interesting empirical analysis, of European countries 1974-1992, in which they find support for the prediction that an increase in the number of unskilled immigrants tends to reduce the generosity of social insurance programs, even after controlling for overall generosity of the welfare state, demographic makeup of the country, and international openness.

By introducing a redistributive state, this research has introduced a plausible channel through which material self-interest can, at least in principle, drive the political-economy of immigration policy. However, there are still at least two problems in making this link fully compelling: first, the politics of immigration policy were more public, more aggressive, and more sustained in the late-19th and early-20th Centuries, when there was essentially no welfare state, than today; and second, not only are the politics of immigration broadly responsive to general business cycles, but they are only occasionally responsive. By contrast, the politics of trade policy appear to have been, at least since the end of the 19th Century, substantially about their redistributive effects and, while the particular modes of domestic politics have changed, trade policy has never ceased to be a major issue.

IV. Trade and Migration Policy in the World

The argument, to this point, has been that a reasonably good first cut at understanding where trade policy comes from can be had by applying quite standard endogenous policy models. As with any first approximation, such models do not tell the whole story, but by capturing a fundamental element they help identify promising directions for extension and issues that need to be addressed outside the model. By contrast, endogenous policy models of migration policy seem to provide very little analytical leverage. While the patent failure of these models in their usual form did lead quickly to models emphasizing the role of a redistributive state, to date these models have not been the basis of much empirical work. Thus, this work is still at the stage of providing a plausible account of broad stylized facts, but as we argued, the stylized facts at which this analysis aims are themselves problematic. This state of affairs is troubling. Why should two obviously economic phenomena, both of which have been the focus of politics over long periods of time, have such different relationships to what is, after all, just a very stripped down version of what lies behind virtually any political economy analysis.[24]

In an effort to organize our thoughts about this problem, we begin with a distinction, due to Schattschneider (1960), between democratic politics and group politics. For Schattschneider, *democratic politics* refers to the public politics through which a democratic civil society constitutes itself and through which it is linked to the policy making apparatus. While elections are the final defense of democratic politics, as well as the key stimulus to public discourse, as stressed by theorists of deliberative democracy, the core of democratic politics is the public discourse itself. Furthermore, the terms of this discourse emphasize public interest and downplay private/individual interest. There is considerable evidence that the public discourse, and its emphasis on some notion of public interest, affects both attitude formation and voting behavior, (see Kinder and Mebane 1983). By contrast with democratic politics, *group politics* is explicitly about private interests. Furthermore, where democratic politics are public politics, group politics happen behind closed doors. Where democratic politics are inclusive, group politics is a game played by insiders. Because group politics are solidly rooted in relatively stable interests, they are predictable and they change in predictable ways in response to the, generally marginal, changes in the environment embedding those interests. Democratic politics are not tied down in the same way. While the location of individuals within the terms of the public discourse is certainly not independent of individual interest, not only do the terms of that discourse change, more quickly and on a much greater scale than the terms of group politics, but individual locations within that discourse (i.e. preferences) change as well.

Political economy analysis must proceed from the assumption that there is an ongoing material basis to the observed politics we seek to explain. At least since the emergence of the

[24] As Timothy Hatton commented at the Kiel conference, the problems are even more complex. Given that the public opinion data seem to show a general public opposition to increased openness to any kind of globalization (trade or immigration), it is the politics of trade that are puzzling. That is, the average level of protection is surprisingly low and standard models have literally no compelling explanation for this (the currently popular Grossman-Helpman model simply treats this as a function of a government taste parameter). Immigration policy is rather more restrictive. On the other hand, the main focus of this paper has been on the ability of these models to explain "local" political economies—i.e. political economic outcome is the neighborhood of equilibrium. That is, the politics of dispersion (of tariffs across sectors and of immigration across locations) rather than averages.

classic tariff system, trade policy has been, and has been understood to be, economic policy. In the public discourse the mix of fiscal, national development, industrial, and income distribution issues has varied, but the language of discussion has always been essentially economic. As a matter of group politics, trade policy has always been quite clearly understood as being about distribution broadly construed. During the period of Classic tariff politics, it was widely understood that general elections were, to some extent, a referendum on “The Tariff” (loosely the average tariff), an important issue of public policy that helped define the difference between Republicans and Democrats over the entire period. However, once a Congress was elected, dispersion around that average was going to be determined very much by group politics.[25] The self-interested nature of these politics is a constant linking Classic tariff politics to the modern politics of the post-Reciprocal Trade Agreements Act system. There are important and difficult issues of accounting for shifts in these interests and shifts in the institutions that channel these interests, but at base the politics of trade policy making remain anchored in material interests.

We have already noted that, unlike trade, there is no systematic evidence of a material basis for ongoing politics of immigration. One of the most telling facts is that there is no equivalent, long-lived, group-based politics surrounding immigration. Following the establishment of the national origin quotas in the Johnson-Reed Act (1924), immigration more or less disappears as a political issue (democratic or group) for forty years. Interestingly, the Immigration and Nationality Act of 1965, which ended the quota system, reflected neither the emergence of new public pressure nor the operation of group politics, but rather derived from its attachment to civil rights issues and, to some extent, to a liberal framing of US international obligations (Gimpel and Edwards, 1999). By the time of the landmark Immigration Reform and Control Act of 1986, while there was a more established set of groups in play: 1) these groups do not have the long history that groups on trade do (i.e. most of the established groups go back no further than the politics surrounding the 1965 Act); and, more importantly, there is not the same

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[25]Even before the Civil War, at least from the early 19th Century, narrative accounts emphasize the role of interest (e.g. Taussig, 1931), while quantitative analyses of the income distribution effects (e.g. Pope, 1972; Baack and Ray, 1973/4; James,1978), and a formal quantitative analysis based on a Chicago school political economy framework (Pincus, 1977) all

straightforward material foundation, or the broad base, in immigration-related groups.[26] It is interesting that, although there is interest based organization on the immigration issue, this organization does not cover the wide range of economic interests that organization on trade does, and, as we have just noted, much of the organization focuses on issues that are essentially orthogonal to economic issues in general, and distributive issues in particular. Comparing the lack of both broad interest based organization and sustained interest based politics on immigration, to the presence of both on trade would seem to provide very strong evidence in favor of our central claim.[27]

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provide evidence of interest based politics in the Ante-Bellum period.

[26]The best treatment of the politics of this period is Schuck (1992). A couple of exceptions require careful consideration. On the one hand, there are a small number of groups with clear material interests that have been involved in immigration politics on more-or-less the same terms as trade-related groups. Southwestern farmers, orchard owners, and ranchers have been actively involved in immigration politics. More recently are employers in the computer industry that have aggressively sought liberalization of entry for skilled labor. However, the narrowness of these interests relative to the wide base of economic interests makes the immigration groups exceptions that prove the rule. On the other hand, immigration lawyers have played an important role in the politics of immigration policy. In understanding their role, however, it is useful to compare the immigration bar with the trade bar. Both have an obvious interest in the details of the law regulating their areas of practice, but these two groups of lawyers do very different things: the trade bar is essentially in the lobbying business, they represent broad parts of American industry and labor; the immigration bar represents a much less obviously material interest and what they do seems different. In addition, a range of humanitarian, religious, and other groups play large roles that they do not play in the trade context. Extremely useful discussion with Gary Freeman led to this footnote and to a substantial improvement in this whole section of the paper.

[27]It may be that part of the reason the group politics of immigration appear so different from those of trade is that the opportunities to engage in group based politics are so few. In addition to fairly regular legislation on trade issues, there are anti-dumping, countervailing duty, escape clause, unfair trade practices (301), (a few) national security cases, etc. In all of these the plaintiff is an industry. This is also, indirectly, true in the Court of International Trade cases. And we shouldn't forget that there is virtually always Geneva-based action of one kind or another. All of these induce broad sector-based, and, since unions are actively involved, factor-based organization on the issue. There do not appear to be nearly the range of opportunities for group-based politics on immigration.

In some ways, however, a comparison of the democratic politics of trade and immigration provide even more insight on their differences. In both the 19th and 20th Centuries the public politics of trade and immigration are distinctive, in both centuries: trade is seen as national and essentially economic; while immigration is local and essentially social. First consider the public politics of trade. Due to its position as one of the very few major sources of revenue for the Federal Government, the tariff had been an important issue to Congress from the founding of the Nation. However, while there is evidence of partisan difference on the tariff even before the Civil War, it remained inchoate as an axis of systematic partisan conflict until the 1880s. During the 1880s first the Republicans and then the Democrats seized on it as a central post-Reconstruction issue. From the 1888 election until the collapse of the Classic tariff system with the Great Depression and the emergence of the modern trade policy system following the Reciprocal Agreements Act of 1934, the tariff was the most important continuing public issue between Republicans and Democrats. While the meaning of "The Tariff" varied across regions, standing for on the one hand, nation-building, economic development, national unity, and anti-imperialism; and on the other, sectional and industrial special interest, corruption, and centralism, the underlying claims were always economic, and understood as such. Then as now, there may well have been uncertainty about the economic effects of the tariff, and a certain amount of active disingenuity on the part of political leaders in their claims, but no one was confused that "The Tariff" was a tax on imported goods, nor that the connection from the tariff to the broader goals it was being used to support had to rest on an essentially economic analysis.

The public politics of immigration in the 19th Century were different from those of the tariff in almost every way. Most importantly, while the parties certainly differed, immigration was never a major axis of national partisan conflict. Where the tariff was always relatively central, immigration politics always seem to emerge from the margins of the political system. The first major move to restrict immigration (of Chinese, then Japanese), like the most recent one, emerged in California and elicited very little interest anywhere else. Not much later, fueled by pseudo-scientific notions of a "hierarchy of races", a small group of, mainly Boston-based, intellectuals began to lobby for general quotas. In both cases, the local concerns driving these movements were essentially political. That is, existing hierarchies seemed to be under pressure to change and a public rhetoric focused on an alien threat was an obvious way of reestablishing control. Thus, unlike trade, the public discourse was primarily about identity and community, with only the loosest claims about economic effects. As we have already noted, broad-based politics on the immigration issue have generally been related to broad macroeconomic or political conditions, not labor market conditions. Thus, the first major episode of national anti-immigrant politics, in the pre-Civil War period, was parasitic on fears associated with a collapse of the previous party system. Economic and political conditions were unsettled, and the enormous wave of, especially Irish and German, immigrants at mid-Century created a more-or-less obvious focus for the politics of fear. The conclusion of Jones (1992) is worth quoting at length:

> In considering nativism it is important to distinguish between a general dislike of foreigners resulting from a recognition of cultural differences and a similar but more deep-seated antipathy based on emotions of fear and hatred. The former, present to some extent in all societies, has been a constant factor in America from colonial days to the present. Using stereotypes of the foreign-born which only rarely correspond to reality, this sentiment has been essentially passive, probably reflecting no more than a general ethnocentrism. Much more, however, has been involved in the usually short-lived but highly concentrated outbursts of xenophobia which have erupted in America from time to time. *These have been the product of loss of national confidence owing to **internal** stress of one kind or another*. Cyclical in character, strongly marked by hysteria and irrationality, and generally inspired by a specific political purpose, such nativistic movements have been essentially attempts to safeguard American nationality from the foreign influences which were believed to threaten it. ... That the generation preceding the Civil war witnessed a recrudescence of nativism was not so much because the period was one of heavy immigration but that it too was one of internal crisis, when national unity was increasingly threatened by sectionalism. (Jones, 1992, pg. 126, my italics)

Olzak (1992) draws similar conclusions from her careful study of patterns of inter-ethnic violence in the US at the turn of the century. Thus, while occasionally intense, the public politics of immigration were essentially episodic and marginal, while the public politics of trade were constant and central. Combined with the constant presence of group politics of trade and the absence of broadly institutionalized group politics of immigration, and the fact that the group politics of trade have always revolved around economically defined interests and that the group politics of immigration (when they arise) are virtually never about economically defined interests, we begin to see why endogenous policy models seem to perform relatively well as a framework for organizing our thinking on trade, while they fail in this purpose for immigration.

V. Conclusion

This paper has sought to argue that endogenous policy models, and by extension any political economy analysis that rests essentially on material interest as derived from the effect of policy on the prices of goods consumed or the return on factor-ownership, can be expected to aid our understanding of the politics of trade policy but not our understanding of immigration policy. This has been a schematic argument, and in particular the historical argument could do with close auditing and further development. It should also be very clear that we are not arguing against political economic analysis of immigration policy. Immigration may well have effects that work via more complex mechanisms. For example, Roemer (2001b) has developed an analysis of the welfare economics of migration in which immigration affects labor market outcomes via effects on working class solidarity (and thus union cohesion) and community identity (and thus willingness to support the welfare state). This certainly leads to an analysis of material interests, but it proceeds from an essentially social foundation–i.e. the links between immigration and both working class solidarity and community identity, for all their importance, are not ultimately material facts.

In thinking about the future of both of these issues, current work on the politics of immigration suggests additional directions for research in terms of the framing of issues that would be interesting to consider in a more formal analytical framework. For example, we might think of the effect of *positive framing*: trade in commodities induces a politics about commodities, immigration induces a politics about people. Unlike the claims in, for example, Hall and Nelson (1989) or Hiscox, (1999), the temporal or material element may be trumped by framing effects. In addition, current research on the politics of immigration suggests the importance of *normative framing*: focus on foreign *countries* has no negative, and may have positive, political affect; focus on foreign people, especially in a multi-ethnic society, may have considerable negative affect in an era of strong rights-based normative notions. Thus, the normative framing effects may have been considerably weaker in the 19^{th} century. Similarly, they may be weaker in countries in which the discourse of citizenship is defined less in terms of abstract rights and more in terms of descent (i.e. blood) or even cultural acceptance (the Roman tradition).

The prospects for an enriched political economy that applies the tools of formal theory to questions that are better grounded in social and political structures, and more sophisticated characterizations of social learning suggest nearly boundless possibilities for growth.

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