

# Research on Globalization and the Welfare State: Do We Know What We Think We Know?<sup>1</sup>

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The rise and decline of the welfare state has been a constant theme of the political economy literature since at least the 1970s. In the last few years the internationalization of the economy (a.k.a. globalization) has become a major issue. Many suggest that technological and organizational innovations and public policy shifts have disrupted a post-war “closed polity” model of political economy and subsequently ended a period of history when western nations shared rapid domestic economic expansion among broad segments of their citizenries. Instead, the future portends much more integrated international markets, smaller governments, and greater sway of internationally mobile investors.

It is widely recognized that in recent decades countries have faced increased pressure to open their economies to international trade and investment. But among academic political economists, there is a wide consensus, at least from examining the systematic empirical research, that globalization either a) simply does not matter that much for the future of the size of welfare state or its funding basis, or b) that economic integration *increases* overall welfare state spending. (e.g., Castles 2001; Garrett and Mitchell 2001; Huber and Stephens 2001; Iversen 2001 Lindert, 2004; Swank 2002 Steinmo and Swank 2002; Mosley 2003). And, at least one formal model, greeted approvingly for its apparent implications, implies that international capital mobility produces a divergence in state responses, with policy towards capital in more “capital friendly” countries diverging from policy in “less friendly” ones (Rogowski 2000).

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Alternative explanations—deindustrialization, an aging population, prior overcommitments, shifting popular sentiment, conventional political competition, or simply its own success in reducing inequalities—are instead held to be more convincingly linked to the current travails of the welfare state. Some alternative explanations allow for conditional effects of globalization on things like income distribution, tax policy and the like, but emphasize the role of institutional arrangements in producing divergent responses across nations. Normatively, one might characterize these results as vindicating social democracy or embedded liberalism against neo-liberalism.

In this paper, I want to revisit the basis of the sanguine prognosis that we should not “blame” recent economic globalization for many problems that ail the welfare state. Besides missing some important facts by trying too hard to avoid them, I think that comparative political economy’s implicitly “realist” (even mercantilist) perspective on international relations causes it to ignore certain collective solutions, namely forms of international cooperation (or collusion) to regulate globalization. As Polanyi pointed out at the beginning of the current (or perhaps now the previous) epoch, while much economic and technological progress is the result of the territorial expansion of markets, political/social challenges come from controlling that expansion in ways that make the economy serve society rather than the reverse.

The main focus of this paper will actually not be on broad conceptual themes like what to do in response to globalization. A recent paper by Dani Rodrik (2002) suggests that one answer is, in keeping with Polanyi and Ruggie, to allow states to slow it down. Of course, Rodrik seems to leave *how* that might be achieved up in the air-- presumably a minor political detail-- the old framework having fallen apart in the early 1970s.<sup>2</sup> Instead, I want to review some of the ways that we have reached the point we have in this literature. I think it will demonstrate some good things, but also a lot of the problems that social scientists have in conducting meaningful hypothesis testing of substantive issues.

As I will hopefully make clear throughout the paper, I do not intend to suggest in these criticisms that large-n empirical work is the problem *per se*. On the contrary, it is essential to evaluate the veracity of competing models with comparisons across countries

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<sup>2</sup> Insofar as political scientists do not view globalization as much of a problem for the welfare state (or other political problems), they (we) reject Rodrik’s premise.

and history, preferably with as many *different* units as possible. But that is all the more reason to do careful work.

A great deal of empirical work purports to show that something besides globalization is linked to welfare state change, but I want to suggest that a lot of these inferences are not typically conceived in a way to evaluate fairly the impact of globalization as such. Indeed, how globalization is typically characterized theoretically, departs considerably from how it is typically operationalized in empirical models. Instead, comparative political economists are more apt to be interested in demonstrating that an alternative domestic explanation/hypothesis better fits the data, while operationalizations of globalization are caricatures deserving the “strawman” label.

Some of my concerns are based on broader questions about how empirical work easily becomes disconnected from, or never meaningfully connected to, more formal or theoretical work. Many of the hypotheses regarding the effects of globalization are derived from a relatively extreme version of a “race to the bottom” argument more prevalent in the political and business *press* than anywhere else. Obviously engaging these contemporary policy concerns is good (and makes what we do more relevant). But it is not clear that the policy advice our results ultimately provide helps. Should we conclude that proponents of greater state protection should embrace capital flows and trade across the globe (or at least in the Nordic countries)?

The main points I want to make are three. First, I discuss problems with most empirical approaches to capturing the effect of globalization. The better ones, I think, imply that globalization is not great for the welfare state. Second, I discuss why our empirical testing and inferences about the effects of globalization on welfare states is generally wrongheaded. Third, I want to suggest that we might think about the effects of *international* market integration on national welfare states as a process analogous to national/regional market integration: specifically by considering the paths of (sub)national political units within (quasi) federal systems, such as the European Union and the United States. While one must be careful with reasoning by analogy, it may be a better way to move forward.

## I. The decline of the welfare state, again? Government programs 1960-2003

While what is meant by welfare decline has not always been clear, there is a wide consensus, that there was little evidence of decline into at least the late 1980s. In this section I suggest that this view should now change. While it is not correct to say that we are in the midst of collapse of social spending, there are a number of commonly referred to indicators that suggest more long-term change than we have been led to believe, especially if we allow for increased demands based on past commitments (pensions) that are not easily undone.

Cross-national comparisons of spending ratios (total outlays, transfers, consumption, etc.), remain the general basis for comparison and have been used to show that the welfare state pessimists have been perpetually wrong. The welfare state has continued to grow.<sup>3</sup> Even less optimistic voices point out that the 1980s and 1990s represent a period of at worst stasis (“growth to limits”) rather than crisis. Indeed several recent studies point out that the annual average spending ratio (spending/GDP) for the core OECD countries, was higher in 1993 than in *any* other year since 1960 (the earliest year available), and probably since war demobilization (Garrett and Mitchell, 2001; Lindert 2004; Swank 2002). The same could be said of the main subcategories of spending, namely social transfers and government consumption. Fourteen of twenty-one countries making up the average in Figure 1 spent more in 1993 than at any other time since 1960.<sup>4</sup>

Figure 1 here

However, examining the ensuing decade (1993-2003) suggests that 1993 was, in terms of spending, a hospitable vantagepoint for welfare expansion optimists. Figure 1 shows that, while prior economic slowdowns, particularly major recessions-- in the mid 1970s, early 1980s, and in the early 1990s-- resulted in permanently larger governments outlays, the most recent *cyclical peak* of spending (around 2000-1) will be below the

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<sup>3</sup> While the shortcoming of such a measure as an indicator of outcome or effort are well-documented, the critiques tend to emphasize how spending may *overstate* welfare state generosity (e.g., Esping Andersen 1990).

<sup>4</sup> The twenty-one countries are: Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Japan, Netherlands, New Zealand, Norway, Portugal, Spain, Sweden, Switzerland, United Kingdom and United States.

spending level reached of every year since the early 1980s.<sup>5</sup> Most countries today have a much higher percentage of adult dependents (mostly old age pensioners) than in the early 1980s: 12.5% versus 15.3% of over 65s and unemployed as a fraction of the total population, unweighted average. Moreover, there has been a long-term build up of public pension obligations in all countries (though in some more than others) due to the fact that most retirees today have built up full pension system entitlements whereas those in many countries in 1980 had not. This suggests that recent trends make spending *demand* “per dependent” above its growth path from the 1970s, while spending itself seems to be below that path.<sup>6</sup>

Figure 2 reports the country averages for social transfers and government consumption spending ratios.<sup>7</sup> It suggests that the decline in spending since the mid 1990s is attributable to declines in *both* transfers and consumption, although the decline in consumption has been more pronounced.

Figure 2 here

Many criticisms have been leveled at spending (whether aggregate, social transfers or consumption) as a measure of welfare effort, since spending may or may not imply redistribution towards those less well off. One alternative measure of welfare state generosity is core program entitlements and the conditions for receiving them. Figure 3 presents trends in unemployment insurance replacement rates for a notional average worker taken from the Comparative Welfare Entitlements Dataset (Allan and Scruggs 2004). There are two series provided: one for a single person living alone and one for a family with one earner and two children. The data are unweighted averages for seventeen

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<sup>5</sup> There is no data for New Zealand prior to 1985, but adding this country to the analysis would *accentuate* the decline since the mid-1990s. Spending in New Zealand fell dramatically from about 54% of GDP in the mid 1980s to 43% in 1994 and then to less than 40% in 2003.

<sup>6</sup> Though the overall dependency ratio (including children) is lower and has been falling since the 1960s, the per capita burden of retirees and unemployed on welfare state resources—pensions, health benefits, even extra child benefits-- more than compensates for fewer children.

<sup>7</sup> Consumption figures here are somewhat higher than those reported elsewhere (e.g., Garrett and Mitchell 2001 or Iversen 2001) because they include military spending. Excluding the military from the figures does not significantly alter the trends, and, insofar as military spending functions partly as industrial policy in some countries, it should be included in an analysis of determinants of government spending.

OECD countries.<sup>8</sup> Consistent with the spending data, average replacement rates increased during the 1970s. However, unlike overall spending, replacement rates peaked in the early 1980s, after which they have declined considerably.<sup>9</sup> In both household types replacement rates in 2002 are, on average about the same as they were in the mid 1970s, with their retreat being a somewhat slower process than their expansion.

Figure 3 here

Declines in generosity, like those in spending, have been widespread. For instance, single replacement rates declined between 1985 and 2002 in fifteen of the seventeen countries (exceptions were Australia and Japan). Moreover, trends in other aspects of these programs-- qualifying conditions, benefit duration, and so forth-- have become, if anything, more stringent since the 1970s or 1980s, and have tended to impact younger workers more profoundly, and those in less stable employment situations.

The one area where benefits have clearly *not* declined as much today is public contributory pensions. This is due to a number of factors many of which relate to the long lead times of pension programs and reforms. Thus, generosity to today's pensioners reflects commitments a generation ago. However, for *today's workers*, recent pension reforms will almost certainly make pension programs less generous and less redistributive.

Of course, it is hard to blame globalization *directly* for the problems leading to these reforms. An aging population is one domestic variable that *not* attributable to globalization. Nonetheless, one needs to keep two facts in mind when considering such welfare state problems in the face of globalization pressures. First, one should consider whether reform choices (e.g., taxes versus benefit cuts and who bears them) would have been different under different degrees of international market integration. One could contrast different responses to the unemployment crises of the 1970s in otherwise similar

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<sup>8</sup> Italy was excluded from the calculations. Its regular unemployment benefit replacement rate increased from about 5% in the 1970s to about 45% by 2002, but the Italian unemployment insurance system offered very generous special benefits (replacement rates of 80%) to many industrial workers under specific circumstances in a program that has become less expansive over time. The net result of these changes is ambiguous. The regular benefits increased the level of benefits for many workers, but reduction of the scope of the special programs reduced benefits for many

<sup>9</sup> Non-contributory pensions (for singles or couples) display a similar pattern.

systems with more open financial systems that are more integrated into larger systems like Denmark, and more (relatively) closed, independent systems, like Sweden.

Second, different starting positions (patterns of commitments) will entail different national adjustment paths, *even if the paths are convergent*, unless one assumes only the crudest (reduced form) of adjustment dynamics, and the complete irrelevance of democracy. If one country with a state-oriented pension system, and another, less state-oriented one each shifted to a purely market based pension system *for all future commitments*, it would likely take at least a generation for the observed public spending of the two to fully converge. And initially, the more liberal regime could easily show up as an “all market” system the fastest.<sup>10</sup>

## **II. What is globalization and what are its effects?**

Globalization is typically referred to as the rapid integration of international markets for goods, services and capital. Thus, globalization is a *systemic* process, made up of national level “responses.” In the system, there may be different starting conditions--some areas/actors are more internationally integrated than others. There may also be different end states, that is, some national areas may be less “integrated” than others. But how would we know integration when we saw it?

In a world of nation-states, integration of national markets into international ones obviously implies a greater scope of international transactions, but in relation to what? Most indicators used in empirical work measure national-level features in isolation. Conceptually, though, a global market is one where national borders are irrelevant, so the “nationality” of the transactions is not the issue. Does more trade with a neighboring country imply more integration than less trade with a more distant (or different in terms of economic structure, say) country? Conceptually, that is not clear. What is clear is that there is generally no distinction made in national measures of external trade.

Consider the European Union and the United States. In the EU, a huge portion of measured trade (in goods or factors) over a given distance is counted as *international* trade

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<sup>10</sup> Even the “radical” UK pension reforms had this quality. The SERPs system was discontinued in the early 1980s, but the government maintained its commitment to existing members. Most public pension reforms, e.g., Italian and Swedish, did the same.

which, in the United States, is trade between constituent units is *intranational* trade. And the EU obviously has a dense network of international governance institutions.

One might think about globalization as a re-equilibration in the spatial distribution of transactions. Under the old “national rules” there was an equilibrium distribution of “domestic” and “foreign” exchanges, while under new “open polity rules,” there is a potential shift in an actor’s distribution of domestic and foreign transactions. Note that “potential” is critical. Any observed shift in the distribution of exchanges (between national and international sources) is neither necessary nor sufficient to indicate “pressure” from outside competition. Think of an undifferentiated good that is affected only by price and transportation cost. Under a closed model, one can think of the price as unaffected externally, but under the new rules, it is.<sup>11</sup> If the country is internationally integrated (i.e., the new rules are in effect) what should we observe? We might observe an international transaction if the domestic price is higher than the world price. Alternatively, we may still observe no foreign transactions if the domestic firm adjusts its price. The latter case, incidentally, is the main argument against use of international “flows” as measures of openness (such as trade or FDI); though flows might still imply “pressures”.

Another element of globalization that contradicts virtually all comparative treatments is that “external” pressures on national actors operate independently of the actions of national actors, particularly if countries are already partly “integrated”. That is, a more integrated global market affects relationships independently of actions to further integrate. This is clearly a factor that we see in many areas of institutional development: e.g., the British are affected by European integration whether they are a formal EU member or not.

Several general implications follow from this. First, globalization should be best also considered as systemic variable that all countries face (excepting explicit conditional factors in our models), and one whose effects *may* be conditional on the level of national integration.<sup>12</sup> This implies that for individual states there is an effect of the level of

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<sup>11</sup> In a completely closed polity, outside trade is necessarily more open polity oriented. Practically speaking, of course, there was trade before, so is expanded trade further integration of markets or not?

<sup>12</sup> In cross-sectional analysis, the common trend is not relevant to explaining variance, but in a cross-sectional time series it could explain be considered important. For example, in Allan and

external integration, an effect of national “integration” and a potential interaction between the two.

A second implication is that if countries differ in their degree of global integration, but also operate within a system that is itself more integrated, a nation’s position *relative* to other nations at a given time may be more relevant than their absolute position on some scale. In a series of paper, Mike Devereaux and several coauthors argue that this is precisely what happens to corporate tax rates. Once countries are modeled as setting corporate tax rates to what other countries are doing, there is good evidence of exactly the “race to the bottom” kind of tax competition suggested in some of the literature. (They also note that the comparative political economy literature may have missed this fact, because it assumes that states act in a vacuum.)

Third, many alleged effects of globalization (good or bad) actually turn on assumptions about international, as opposed to domestic, politics. Thus, “capital” is a possible threat to national welfare states because, when it is internationally mobile, national states may be less capable of colluding to prevent tax/wage competition, and cannot be held to any sort of political (democratic?) check on behavior.<sup>13</sup> This is obviously a point of intersection between comparative and international politics. The possibility of existing (or emerging) governance structures at the international level are likely to affect how much leverage “mobility” entails. This possibility also justifies thinking about the *globalization* of markets in ways similar to the *nationalization* of markets. Historically, nationalization is seen as a struggle to integrate local markets under a spatially larger political authority.

### Measuring Globalization

In the empirical literature, several main variables tend to serve as “indicators of globalization.” Theoretically, some point to a linkage between mobility and enhanced

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Scruggs (2004) we suggest that the effects of partisanship on welfare state generosity change subtly with such a “structural change.” Left party dominance is a significant predictors of welfare expansion up until the early 1980s. After that, in the “era of retrenchment,” right parties become associated with heavier retrenchment.

<sup>13</sup> Note that domestic structural power (and neo-pluralist) theories have generally assigned greater power to capital within a sovereign nation-state. In those cases the reason is that holders of capital can choose whether, and in what, to invest.

“voice” of mobile factors (namely capital and perhaps highly skilled labor) can divert policy away from democratic outcomes, or, alternatively, the exit power of mobile factors serves to divert productive resources away from non-reformers. The logic suggests a situation of declining support and standards, or a reallocation of financing for those standards. In this section I discuss several specific indicators commonly used in the literature. I try to demonstrate where and to what degree these indicators are appropriate for evaluating the impacts of international integration on the welfare state.

### Capital flows

Foreign direct investment and other forms of capital flows (portfolio investment or bank borrowing) is a widespread measure of the internationalization of financial transactions.<sup>14</sup> Globally, and in individual countries, the volume of FDI has clearly increased since the 1970s, and there is a theoretical literature on structural dependence of the state which associates capital with additional political influence (see Swank Chapter 2). Empirical studies typically use some measures of annual flows of FDI into or out of the country in question. Typically, the measure is normalized as a ratio of national GDP. Generally (though I have to admit I’m not sure where it was explained as such), the higher the annual flow in and/or out, the more a country is considered more heavily integrated into the global economy.

But what is FDI supposed to represent exactly? Does a high level reflect that said government is accommodating to capital? While this seems reasonable, capital flight manifests as investment abroad, and thus FDI flows would signal a country is *not* amenable to capital.

Conceptual problems combine with measurement problems in many instances to confound expectations. First, (leaving aside questions about how reliable the existing data is) should one use inward or outward, net or total FDI flows as a measure of globalization? All have been used in different estimates in the literature without much discussion of why one is chosen over the other. Measures of FDI stocks (assets held by foreigners) might provide a better indicator of vulnerability to “external” imperatives.

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<sup>14</sup> Huber and Stephens (2001), Garrett and Mitchell (2001), Swank (2002), I refer to FDI as shorthand in this section, but the arguments generalize to loan of portfolio investment flows.

Second, FDI activity is procyclical. This suggests that *annual fluctuations* in FDI can represent vagaries of the business cycle and not necessarily government policy. Given what we know about welfare spending ratios and recessions, this would imply a negative FDI-welfare spending relationship in a detrended series. Since we know that spending and FDI are positively trended, we expect a positive (spurious) relationship.

Third, FDI is not, as it is typically assumed, usually motivated by straightforward “production cost” criteria as is often (at least implicitly) assumed. Much more than building a factory to produce in country with cheaper labor or lower taxes, FDI is also driven by strategic merger and acquisition activities which involve global companies taking over other (global) companies to position themselves to profit in new markets.<sup>15</sup>

One can also seriously question the cardinality assumption in the measurement of FDI across countries and time. This criticism, of course, applies to any data used in OLS regression analyses, but this does not make it necessarily unimportant to consider in testing models. Does twice the ratio of FDI between countries in a given year indicate double the integration? Was Sweden really three times as internationally integrated in 1998 (FDI was about 18% of GDP) as it was in 1994 or in 2001 (FDI around 6% of GDP)?<sup>16</sup>

Consider the example of Swedish FDI trends in Figure 4. As with most countries, flows were limited until the late 1970s. After lifting some capital controls in the early 1980s, Sweden increasingly experienced net outward flows that were a considerable share of GDP, with little absolute inward investment. Following another round of capital control liberalization, Sweden immediately experienced a peak in net capital flight, as well as a great deal of financial speculation that contributed to a major recession. In the intervening expansion, total FDI has increased but in a volatile manner, while the net position of flows has also fluctuated somewhat wildly. Meanwhile, of course, Sweden did not experience major welfare state spending cuts and reforms until around the time of the recession and a couple of years after capital controls were lifted, and after persistent negative FDI flows

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<sup>15</sup> A good example in this context has to do with the acquisition of Swedish carmakers Volvo by Ford in 1999 (a 50 billion kroner deal equal to 2-3% of Swedish GDP). It was not motivated by costs of production or taxes as much as efforts to acquire brands that catered to in certain market niches that Ford was weak in.

<sup>16</sup> Smoothing the series (e.g., taking a moving average of FDI) does not necessarily alleviate the problem. For instance, the three year average total direct investment for these years is 3% for 1991, 10% for 1994 and 21% for 1998.

has come back to balance. In a regression model with a one-year lag, this sequence would appear to favor the explanation that a spike in unemployment precipitated welfare state cuts. Of course, a two or three year lag would produce a different conclusion.<sup>17</sup>

Figure 4 here

Finally, FDI typically has a source and a destination. The choice to invest out of one country and into another is likely made on cost considerations, but also others. Implicitly, studies assume that FDI decisions are made on the basis of relative competitiveness of the country of origin/destination. However, few if any studies disaggregate the sources of FDI investments in this way.

Does the fact that FDI has potentially countervailing effects (some positive others negative) call precisely for empirical estimation to let the data determine which effect is most important? In short, no. Generally, indeterminacy of this type is a *theoretical* or a *research design* problem that statistical analysis cannot solve. It will provide an estimated average effect across the sample, but that average may be positive or negative in spite of “mechanical” effects in both directions. For example, imagine the amount of FDI completely determines welfare spending, but the direction of the effect is conditional on direction of the FDI; i.e., inflows increase spending, outflows decrease it. Moreover, assume that there is a 1 to 1 tradeoff within the country, so that net inflows or outflows are the critical factor. The literature is filled with stories and vague hypotheses suggesting that: FDI flows help welfare states grow, or FDI flows produce a race to the bottom. The results of an analysis of overall FDI on spending will depend on the distribution of net flows among the countries sampled. If a few countries are net recipients (and have higher spending), many countries have a rough balance (and thus stable welfare state spending) and a few have large net outflows (and large declines in spending), the results will suggest that FDI does not affect the welfare state. If the distribution of cases *in the sample* was

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<sup>17</sup> Modeling assumptions are critical in these situations. Lags are typically introduced in these models to avoid endogeneity, not discriminate between explanations. Is a one-year lag appropriate to capture a policy lag process? Can this be expected to approximate the lag in all countries?

tilted one way or the other, one might reach one conclusion or the other. Only if one measures inward and outward separately would one find the more nuanced effect.<sup>18</sup>

Generally speaking, capital flows like FDI seem to have some of the biggest current conceptual and measurement problems as they might be incorporated into empirical models of the effects of globalization as it relates to the welfare state. For this reason, it is perhaps not surprising that it seldom has a consistent effect, and is very often imprecisely estimated.

### Trade

Trade openness (total trade/GDP) is perhaps the most widely used measure of economic globalization. Starting with Cameron (1978) and Katzenstein (1985), political economists have suggested that international integration promoted the *growth* of the welfare state. The mechanism proposed was that larger public sectors are “necessary” to cushion the population from external shocks brought on by greater economic integration. More recently, however, Garrett and Mitchell (2001) and Rodrik (1997) both found that, more trade was associated with smaller national governments (or at least slower growth). Moreover, the putative reason for this cross-sectional correlation has been shown to be faulty. A direct empirical test of this mechanism (Iversen 2001) finds that there is no negative relationship between international specialization, export orientation, and output volatility (i.e., risk) as suggested by Rodrik (1997). In other words, he finds that there is not a larger risk to being export oriented. A more recent paper using data up to the peak year of welfare spending reports that increasing trade openness is associated with *less* growth in government Garrett and Mitchell (2001).

Trade exposure, as typically used, is actually an indication of the *intensity* of trade rather than policy openness, as such. (That is, a country might be a very heavy trader, but subject trade to considerable restrictions). Total trade is intended to proxy for risk and

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<sup>18</sup> Of course, if the sample was selected randomly on the independent variable (FDI), the inference would be valid for the population. But we usually do not sample that way. Yet another empirical shortcoming here is that as a ratio measure, the sum of changes in FDI ratios need not equal total global flows, even though total outflows must by definition equal inflows. The reason is the size effect. A ten billion dollar acquisition of a Danish firm by a US firm will show as a large FDI inflow for Denmark (in terms of its GDP), but a trivial outflow for the US in terms of its GDP.

volatility faced by producers, and hence some (potential) demand for social protection.<sup>19</sup> Unlike financial flows measures, trade intensity *is* relatively stable from one year to another. Of course, while consistent with the idea a measure of a country's degree of international integration *not* be highly volatile from year to year, the absence of variation makes it hard to discern statistically any connections between long-term movements in trade exposure and government spending over relatively short spans like thirty years.

Another problem with the trade measure is that it conflates structural/natural constraints and policy ones (Rodriguez and Rodrik 2000). For example, for a given industrial structure, small countries near larger, richer markets (the Netherlands) will be more likely to trade than large, isolated rich countries (the United States) surrounded smaller, poorer neighboring countries. Of course, the policy/structure distinction is less relevant when it comes to volatility risks—citizens do not necessarily care if the risks they face are geographic or policy-induced. However, insofar as we wish to know whether trade liberalization *policies* induce the same behavioral responses as geographic disadvantages, we need to assess this separation.

Figure 1 (referred to earlier) includes, besides trends in total government spending, the 21 country average level of trade openness, (exports+imports/GDP). Up until the very recent past, say the late 1980s, welfare state growth and trade growth both trend upward. Since the late 1980s, however, the two trends have diverged markedly: trade has continued to increase, government spending has decreased. This change of relationship appears to be quite dramatic.

Based on the trade and spending trends in Figure 1, what conclusion can one draw about the relationship between globalization and spending? Is the relationship positive or negative? Is there a change in the *structure* of the relationship between the two that we might attribute to some significant event? The latter interpretation would seem broadly consistent with the idea that absence of capital controls (see next section) *interacts* with higher trade openness to reduce the size of government. The timing is consistent with the decline in capital controls during in the 1980s or early 1990s, but requires that such changes precede spending cuts with a 5-10 year lag. Since virtually all spending studies

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<sup>19</sup> Cameron posited that trade openness was linked with industrial concentration and the development of strong unions. His was in some ways an ad hoc way to account for a correlation between welfare states and trade intensity in Western Europe.

find that it has a long “memory” (i.e., high value for a lagged dependent variable), it is not surprising that any patterns take a long time to reveal themselves.<sup>20</sup>

A regression of the levels of both (trade and spending) variables suggests a strong positive relationship (particularly for the period up to 1993.) Of course, this regression is spurious, as two trending variables appear correlated simply as a function of their trend. A regression using the standard approach of differencing the two series through 1989 produces a *negative* coefficient, but marginally statistically significant ( $p < .10$ ). An estimate based on the entire differenced series (to 2003) is negative *and* statistically significant. Thus, (at least based on these unweighted country averages), there is no support for the trade compensation hypothesis, but there is support for the efficiency hypothesis. That is, trade makes governments expand more slowly (or decline more rapidly).

How does this result square with so much work finding a positive relationship? Cameron relied on a very limited sample of cross-sectional data. Rodrik’s findings were actually ambiguous for the OECD countries. His cross sectional results for the 1980s and early 1990s (1997 p. 52) suggested a positive relationship, as do results among a large set of countries (1997 p. 53, 1998). However, in Rodrik’s 1997 monograph, a later analysis of twenty OECD countries for 1966-91 suggests that greater openness is associated with *lower* spending of all types, and that the negative effects are more pronounced in the presence of greater capital market openness. Some other recent studies also seem to suggest that the positive cross-sectional relationship between OECD country openness and spending does not hold up over time (Garrett and Mitchell 2001).

One can readily see that any positive cross-sectional association between trade openness and spending has been *declining* over time by looking at decade to decade snapshots of correlations. Panels in Figure 5 show, respectively, the ten-year averages for the 1970s 1980s and 1990s plotted against spending for the first three years of the 1980s, 1990s and 2000s.

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<sup>20</sup> A long memory means that “history matters” but its influence *declines* over time. This should not be confused with “path dependence”, where historic events (essentially) last forever. While I accept this as a view of reality, I think it probably renders comparative social science and any attempts to generalize beyond a specific place *and* time inherently difficult if not impossible.

Finally, we should consider whether the trade measure for individual countries is consistent with the idea of increased global integration of those countries. A few facts seem discordant. For example, it appears that Japan has become considerably *less* internationally isolated since the mid-1980s by this measure. Finally, Japan and the United States appear as the least *internationally* integrated countries in the OECD (as they do via measures of capital flows).

Figure 5 here

#### Policy restrictions on international capital movements

Both measures discussed above capture flows, and are subject to the blanket criticism that, in addition to not accurately capturing variations in integration, they may also be beyond the influence of national governments or national actors to affect. That is, such measures do not represent the policy commitments of governments. Moreover, the variations in the year-to-year (capital flows) or decade-to-decade (trade) movements of aggregate flows are not consistent with many qualitative assessments of trends in international economic integration. This seems a basic validity problem with such measures, and as such, makes results relying on them suspect whatever they may indicate.

Efforts to capture liberalization in policy outputs often rely on some measure of legal restrictions on financial market transactions. One of the most popular is the coded from a list of legal restrictions on international capital movements that is compiled annually by the IMF (Quinn 1997). The measure comprises restrictions on the capital account, restrictions on the current account (for both goods and services) and acceptance of international treaties “constrain a nation’s ability to restrict exchange and capital flows (Quinn, 1997, 535).

Capital market openness across the OECD has a strong upwardly trend and is convergent over time. Figure 6 shows the median and interquartile range for 21 countries between 1960 and 1997. Average openness scores rise consistently with particularly large jumps between 1979 and 1980 and between 1989 and 1990. In 1970, only one country, Germany, scores 13 or higher. In 1980, five countries do; in 1990 13 do and in 1997 (the last year with reliable data), 19 of the 21 countries scored 13 or higher.

Figure 6 here

The disappearance of restrictions at the national level serve as a direct indication of openness to capital flows that is consistent with the idea of a steady and largely permanent increase international capital openness. Compared with alternative measures of capital restrictions, and with flows data, this represents, in principle, an excellent source for determining commitments. The main problem lies in the fact that variance is correlated with time. Almost all countries are top coded for openness by the mid 1990s. Of course, if the measure is a valid indicator of the level of mobility in a country, this simply reflects the fact. Nonetheless, this pattern is troubling for us if we are trying to get reasonable estimates of the effects of varying openness “independently.”

Empirical work using capital market openness has, like much of the globalization literature, produced mixed results. Arguably, insofar since these restrictions have a bearing on all types of financial flows, this measure should be a good macro indicator and reduced restrictions have a less ambiguous information content than flows like trade or investment. Rodrik, using a more crude measure of openness, found evidence that openness reduced spending (and even has a negative interaction effect with high levels of trade). Quinn (1997) originally found strong positive associations between a variety of national macroeconomic benefits and financial openness, including equality and social spending. Swank 2002 also finds positive relationship between openness and overall welfare effort, though not on categories of effort like social wage or health spending. Other recent studies, such as Iversen and Cusack (2000) and Garrett and Mitchell (2001) and Huber and Stephens (2001), Bradley, et al. (2003) find negative or at best mixed evidence of any effect.

#### Taxation: The Thin edge of the wedge against big government?

A specific avenue by which many argue that globalization (especially of capital) erodes the welfare state is via taxation. As capital becomes more mobile, so the argument goes, it will tend to induce (via tax competition) a shift in the *distribution* of taxation. Since the tax structure of the welfare state has relied on taxing capital *and* labor in the past (and has

often taxed capital heavily), globalization either mean that taxes are raised on things besides mobile capital, or social benefits (or some other element of public spending) must be cut. This makes the tax angle “the thin edge of the globalization wedge against big government” (Garrett and Mitchell 2001, 173). Indeed, while “pro-globalization” organizations like the OECD rail against labor market rigidities and barriers to international integration, they also devote special attention to avoiding tax competition.

An increasing amount of work is being done on the implications of capital mobility on the distribution of taxation between factors (Swank 1998; Steinmo and Swank 2002, Garrett 1997, Rodrik 1997, Garrett and Mitchell 2001). Much of this work is motivated by a suggestion that capital’s increased mobility makes it increasingly capable of demanding and receiving a lower tax rate as a condition for investment. For example, it was suggested by results in Rodrik (1997) and a formal model in Rodrik and Ypersele (2001) that capital mobility limits the ability of states to tax capital and ensure that the net efficiency gains to capital mobility might be shared in part with labor.

More or less consistent with other work on the direct effects of globalization on taxation, these authors have tended to find that there is little systematic evidence that globalization shifts the burden of taxation to labor. Several results even suggest that capital mobility is linked to *increased* taxation of capital.

Swank and Steinmo’s (2002) study is one of the more recent published studies in comparative political economy, and perhaps the most careful one, so I focus on that here. They begin their paper by noting that a number of countries instituted tax reforms in the 1980s that substantially undid “a regime of high marginal tax rates—combined with generous tax incentives for investment and controls regulating capital export—[that] was a centerpiece of compromise between capital and labor in all industrialized countries and contributed to the joint pursuit of equity and growth in the Keynesian welfare state. (p 643).” In a pooled analysis of 14 countries for the period from 1980 to 1995, they find strong support for negative effects of (legal) capital market liberalization (Quinn’s 0-4 measure dealing only with the capital account ) and trade intensity on reductions in the top marginal corporate tax rate. They affirm that the timing of changes in capital market liberalization generally *precedes* tax rate reductions, and cite qualitative evidence from their own personal interviews suggesting that policymakers were often motivated by

“competitiveness concerns” stemming from capital market liberalization when reducing rates (which were incidentally part of a larger wave of tax reforms).

In estimates of a model of determinants of effective corporate tax rates (rates of tax actually paid), they find that the relationship between capital market liberalization and effective corporate tax rates has the wrong sign and is not statistically significant). Instead they find that long term unemployment rates, levels of public debt, and a series of mostly macro-economic controls are better explanations for changes in effective rates. The authors devote a long discussion to explaining how this (non)result for globalization is “robust” to the use of different measures of openness of the capital account (e.g., FDI and total capital flows as a portion of GDP), a slightly different measure of corporate tax rate, and (though somewhat opaquely) the erosion of the corporate profit tax base due to transfer pricing. They note that many of the changes to corporate tax laws broadened the tax base, and these may make up for nominal rate reductions.

The authors estimate essentially the same empirical model on effective tax rates on labor and on consumption. For labor they find strong negative effects of liberalization and long-term unemployment rates. For consumption taxes, they find some evidence that higher trade and a larger portion of elderly are associated with a higher take from consumption taxes. Based on the much stronger effect of capital market liberalization to reduce taxes on labor (as opposed to capital), they conclude that the effects of globalization on the tax system have been exactly the opposite of what fear-mongers of globalization predict. The tax burden on capital has gotten relatively worse as markets have become more open, while the burden on labor has been reduced. Moreover, they note that some structural factors (accumulated debt) are systematically checked on government’s ability to cut taxes.

One significant estimation problem in the paper is the lagged dependent variable. In all models, the coefficient is extremely high (.875 for corporate top rates, .93 for effective capital rates, .99 for effective labor rate, and .98 for effective consumption rate) implying possibly severe biases in substantive estimates (many of which are likely autoregressive series).

The most substantive criticism, however, relates to the quality of the tax data itself. The authors of a recent OECD report note a number of serious questions about the

usefulness of the average effective tax rates estimated in a paper by Mendoza, et al, (1994) and used in the recent literature (Carey and Tchilinguirian 2000). Such concerns are reflected in other studies, some of which strongly suggest that the Mendoza, et al. methodology and associated data make it impossible to adjudicate whether globalization lead to a “race to the bottom” (e.g., Devereux, Lockwood and Redoano, 2002; Stewart and Webb, 2003). Carey and Tchilinguirian conclude that:

“...considerable caution is required in interpreting AETRs, for both individual countries and in assessing general trends for OECD countries as a whole. The fundamental problem of splitting income tax between capital and labor is hard to solve... [E]stimates rest on the assumption that all capital income is taxed at the same rate. A brief inspection of most OECD countries’ tax systems shows that this assumption is unrealistic, particularly for countries with preferential treatment of pension funds, and largely funded retirement income systems... In addition, they are not very useful for assessing the effects of international tax competition because this can reduce both the numerator and denominator of the AETR. For such reasons, AETR estimates need to be corroborated by a significant volume of other information before conclusions can reasonably drawn. (p. 26)

First, the Mendoza, et al methodology assumes that all self-employment income is entirely capital income, when it should be more correctly treated as partly wage income. Second, the methodology assumes that the rate of tax paid on capital income earned by households is equal to its effective labor tax rate. This over-estimates that tax rate when capital income is taxed at a different rate or when there are provisions against double taxation of dividends. In recent years, many countries have tended to move towards differential treatment that benefits capital income earned by households (a so-called dual tax system). Third, there are a variety of problems in converting categories from the tax revenue sources (numerator) into those for National Accounts sources for income (denominator). This means that labor income (tax revenue) may be categorized as capital

income (tax revenue), including accounting procedures for depreciation of fixed capital, which has important effects on net operating surplus (the denominator for the effective tax rates for capital). Fourth, and perhaps most critically, they note that the calculations using the same base sources as Mendoza ,et al, will greatly underestimate the effects of international tax competition (precisely what the use of this data is intended to capture). Fifth, Carey and Tchilinguirian also note that the accounting methods for the revenue and national accounts data differ. Essentially it means that the two systems observe money at different points (cash versus accrual basis).

An alternative indicator of corporate tax rates that rely on more detailed national tax systems suggests that relatively large declines in corporate tax rate (nominal and effective) and convergence for 21 high-income OECD countries between 1983 and 1999 (Devereaux and Redaono 2002; Devereaux and Griffith, 2002). These authors find, moreover, that governments do react to fluctuations in FDI by adjusting their average and effective corporate tax rates. This work is much more explicit and elaborate in building and evaluating a model of how government react to capital mobility.

One thing that Mendoza, Carey and Tchilinguirian, Swank and Steinmo, and Devereux do agree on is the fact that aggregate national trends in the relative tax burdens on capital and labor have moved over the last 20-25 years towards labor. It is also reasonably clear from the tax rate data that top tax rates on both capital and labor income have tended to come down. This implies that trends are consistent with “efficiency” effects from globalization, as high earners and capital income are the most mobile elements of the tax base.

Regardless of whether falling capital tax burdens are having an impact on the size of the welfare state, one other element of the tax structure has gone largely not remarked upon: the distribution of the labor tax burden. Across the OECD, there has been a trend towards reduction in the number of brackets and of elimination of allowances, and increases in social insurance taxes. Consumption taxes (value added, excise, or “pollution taxes”) have also increased somewhat over the years. Combined with more dual tax systems that tax household capital income at lower nominal rates (and generally no social insurance taxes) than wage income, these shifts have made labor tax systems more

regressive.<sup>21</sup> In that respect, even if there has been little impact on state spending as such, and even if capital's share has not changed much, the tax system has become increasingly less progressive. Clearly this should count against maintenance of most welfare state *goals*, even if such changes are not captured in spending ratios or benefit entitlements.

Or should it? Several recent books and articles claim that big, egalitarian welfare states have been paid for by much *more* regressive systems of taxation than previously appreciated, and that many small states have historically been comparatively kind on capital taxation (Kato 2003). Though it is not clear from the *effective* tax rate data that this is not true-- it certainly is true for nominal capital tax rates-- assume that it is true. This suggests that small states (generally with big governments) may enjoy a comparative corporate tax advantage, allowing them to attract/retain enough capital to grow and impose larger tax burdens on their labor force. (Of course, they also tended to use to capital controls to restrict capital movements.) By itself, the reduction of many capital controls in the 1980s may not have been devastating, since these countries had somewhat kinder capital tax regimes. However, when big high corporate tax countries move to introduce tax reforms that become more favorable to capital, the small state comparative corporate tax advantage slips. Now, absent capital controls, some investors and re-investors may look at indirect wage costs in making location decisions. (Despite the fact that social security and payroll taxes are accounted for as taxes "on labor," they can affect capital investment decisions via the implicit labor portion of that investment.)

### **III. Model building, statistical tests and inference**

The previous section highlighted a number of basic validity problems with the frequently used measures of globalization. In this section, I discuss some basic modeling, testing, and inference problems that follow from the above criticisms. In some cases, I simply refer to previous discussion in the last section.

One of the most basic implications of using measures that are not valid is that we cannot infer anything about relationships between more abstract concepts to which the measures refer. A bad measure is *not* better than no measure. Insofar as we have

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<sup>21</sup> Many withdrawn deductions are flat rate, while new "means-tested" allowances tend to provide some cutouts for the lowest incomes, but not others.

conceptual reasons to think that our well measured concepts might be correlated with concepts we do not measure well, we cannot parse the effects of the concepts. This means that including the poor measures of globalization in our models tells us nothing about whether or not globalization matters, and nothing about whether estimates for our other variables are affected by taking globalization into account. This seems obvious enough, but we often feel compelled to include things that we know to be very conceptually poor, and then pretend our results “controlled” for something. Another basic shortcoming in many analyses of the “non-effects” of globalization involves the erroneous conclusion that the inability to reject the null hypothesis ( $H_0$ : globalization has no effect) suggests that the null hypothesis (no effect) is true. While this seems a trivial problem (most of us know it well from statistics class and repeat it in our sleep), we unwittingly tend to ignore it when talk about results. The main problem may simply stem from the fact that most “tests” of globalization hypotheses are poorly conceived controls, and so are arguably not really tests where a failure to reject the null hypothesis should lead us to alter priors at all.

#### Model specification and globalization

Most large-n empirical work on the relationship between globalization and the welfare state confronts a difficult task. Many of the claims about the pernicious effects of globalization on the welfare state are not very clearly specified. Some factor called globalization is alleged to act to constrain “sovereign” states’ choices that were heretofore unconstrained. This changed structure alters the probability that states reform to become leaner, market-conforming, “commodifying” (and by extension usually smaller) states. In the extreme form of the argument, states are forced into competition with each other for investors (and highly mobile labor), reducing their demands/conditions for investment (i.e., various taxes, etc.).<sup>22</sup> Thus, social regulations and taxes on capital fall to zero, as states compete with each other for investment. The implications are that tax burdens are pushed onto labor or assets that cannot easily move.

Empirical research that has attempted to evaluate this argument has tended to be quite hostile to it in principle. Skepticism is justified, insofar as the extreme view tends to

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<sup>22</sup> On the governmental competition for high-skilled human capital, see Florida (2002) and Florida and Tinagli (2004).

be used by proponents and critics of globalization for partisan purposes. However, I think this academic skepticism has often led to ways of specifying the “race to the bottom” or “tax competition” hypotheses that are not so well thought out.

For instance, the standard way that globalization has entered is simply as (a set of) variable(s) added to the end of regression equations meant to explain welfare state growth. Often, this is done by including “domestic” and “globalization” variables that are likely correlated because they are causally related. For example, spending models include unemployment or budget deficits as “domestic” level variables.

The problem with this specification is that they control for proximate causes of cutbacks that are alleged to be caused by international economic integration if states fail to conform to its strictures. That is, if high spending in an integrated environment induces budget crises or higher unemployment problems, then globalization may work indirectly through those channels, *particularly in countries that do not adapt by reducing spending*.

Similarly, many have argued that the institutional bases of large welfare states (left government linked to strong trade unions and cooperative policy making institutions) have also been eroded by globalization, leading to welfare state decline (cf. Swank 2002). It is certainly true that, except for the countries with Ghent unemployment insurance, union density has declined (in some cases dramatically), and collective bargaining institutions have been weakened in much of the OECD, even if cross-national variations remain. How much of this change is due to “globalization” or “change in the structure of employment” and how much these two forces interact with each other is not clear enough to assume that the two are “independent” sources of change.

These types of arguments globalization argument can unyielding, in the sense that they suggest that any “independent” variable is simply affected by international globalization. Yet so can a search for displacing international “causes” with proximate national ones.

Globalization is too often presented as inevitable, when it may simply be a choice. One potentially useful way to portray the possible effects of the global mobility of investment (and highly skilled labor) is to think of mobility as altering the steepness of the trade-off between political objectives (call it, growth and equity). This view implies that governments are not really compelled to do anything with respect to spending, but may

suffer consequences (like lower growth). Thus, one may *observe* adjustments via reductions in the size of the state, but we may also observe states choosing slower growth paths, that is, paying the (now) higher price of domestic equity.

In addition to the extreme “goals” of choosing equilibria, we must think about transition paths. Much of the dismissal of “race to the bottom” argument sometimes seems to be based on the observation of the continued existence of redistributive programs or a non-zero tax rate on capital. The other possibility is that it is a pressure that is adjusted with different end choices over different time periods.

We need to think harder about time dimensions in our models. One reason we are reticent to difference series is that we are interested in long-term effects. (Another is that we are really not so good at the year-to-year dynamics with any substantive variables.) But in moving to pooled time series models, we have tended to ignore time for the most part and are estimating old cross-sectional models. The previous example of how to resolve the explanatory conundrum of “what caused” the Swedish welfare cutbacks in the mid 1990s: the economic crisis or globalization provides some clue as to how hard this can be. The data just won’t answer that story, because the events are actually rare.

### Taking the con out of the econometrics<sup>23</sup>

A major problem in the estimation of empirical models in the comparative welfare state literature on globalization is the huge amounts of data (much of which has unattractive statistical properties like multicollinearity, trends, unit roots, etc.), modeling assumptions, and large numbers of parameters. To this, add the fact that there is generally limited attention to modeling of specific processes being estimated (lag structures, alternate function forms, and opportunistic specification searches i.e., running regression models on the same data until you achieve what you expected). Empirical modeling in a world of cheap computers and access to Stata can easily become, like formal modeling, all about assumptions. It is generally not clear whether current modeling approaches really contribute to expanding our knowledge about processes we are interested in (Achen 2002). Sophisticated techniques are not solutions to these basic modeling problems. Out of sample testing might be, but that is almost never done.

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<sup>23</sup> The title of this section borrows from a famous paper by Ed Leamer cited later in the section.

Most people do not tend to think of it this way, but linear parameters, homogenous effects, normal error structures, fit between concepts and variables, when to stop trying different model specifications, are all assumptions. Assumptions are unavoidable, but many of them, like opportunistic specification searches, are very *bad* ways to behave as social scientists, and we often fail to appreciate that we are making them.

If you combine with that starting with poorly developed concepts and empirically invalid measures, things are bound to go poorly. Table 1 presents the results of a replication of a published paper modeling determinants of government spending for the 1961 to 1993. (Though the data matrix is not identical, my estimates are reasonably close to those in the paper. The data and code in Stata format is available on my website.) It contains all of the measures of globalization just discussed above, dropped in largely “off the shelf.” The estimation procedure uses OLS with panel corrected standard errors and dummy variables for each country and for each year. In all, the model estimates 62 parameters on 550 observations (the *n* in the paper was slightly smaller). That is a ratio of around 9 observations per parameter (never mind the actual size of the parameter space). If you estimated a voting model from the NES data set with the same ratio, you could estimate 200-250 parameters!

Among the substantive parameters in the model is a lagged dependent variable, with an estimated effect of between .91 and .94. I also estimated the models with additional data for years 1994-2001. The results suggest large negative effects for the three globalization variables: FDI, openness and trade, and thus appear to confirm the idea that globalization is constraining the growth of government.

These procedures are relatively standard modeling approach in the literature. Indeed, the model here might be considered relatively simple. Most of the RHS variables are binary dummies (they take only two values), and there are no interaction terms. Several recent studies have many more variables, many of them highly collinear.

So what’s wrong with it? The extremely high R2 is a signal that something is amiss. Even if the *model* is good enough to get this, the *data* is not. Indeed, simply regressing the dependent variable on its lag, shows the problem. The estimate is .993, and itself explains more than 97% of the variance. Add the country dummies and we explain

99.8% of the variance, add the year dummies and we are up to 99.92%. Now we can add the substantive variables! They contribute an extra .02% (.0002) to the variance explained.

Table 1 here

So which of these is the best model? For predicting what will happen to outlays in the next seven years (1994-2001), the correlation is .979 for the first model estimated in Table 1, .979 for that model without year dummies, and .989 for a model using only a lagged DV. In this paper, the authors suggest that their modeling strategy was extremely conservative, particularly the use of panel corrected standard errors, making them confident that their results are strong. It is true that PCSE inflate standard error estimates, making one less likely to reject a null hypothesis. But this paper's main conclusions are based on tallying the number of statistically significant estimates of the globalization estimates from regressing the model on a number of number of different spending measures. A tallying of separate "conservative" estimates does not necessarily lead to a conservative conclusion, however.

The presence of a lagged dependent variable in the model tends to bias exogenous variable estimates towards zero, especially when the exogenous variables exhibit a high degree of autocorrelation (as is the case for almost all of these variables).<sup>24</sup> Thus, while the statistically significant results are so by a conservative procedure, signs of the non-significant estimates are biased towards zero, implying that the tally of downwardly biased significance estimates. (There is no attempt to compare magnitudes.)

The paper, like many, are estimates effects that are considered "conservative" by virtue of the fact that the full set of dummy variables are included as regressors. The use of dummies *can* avoid situations in pooled data sets where the real variation is simply cross-sectional or simply across time. However, dummies can also "explain away"

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<sup>24</sup> Based on Achen (2000), LDV models should not be used when the dependent and independent variables tend to be highly autoregressive, which is the case with most series in the model under discussion, and many others used in comparative political economy.

variance that would otherwise contribute to the standard error of a parameter estimates of substance, making such estimates appear more precise than they otherwise would.<sup>25</sup>

In addition to, but also related to, problems of overfitting models like the example, many very complicated models are, if meaningfully specified, often extremely fragile: the results do not withstand the common types of robustness tests. While the best case for a model would be to predict out of sample, at the *very least*, researchers need to “tweak” their empirical models, drop random sets of observations, controls, examine residuals to ensure that obvious subsets of the data are not driving results. And that should be done *after* they get the results that they want with the correct variables (Leamer 1983). In the present example, eliminating year dummies or country dummies significantly alters the substantive and statistical significance of several estimates. That should elicit concerns that *Stata*, not the *data*, is speaking.

The approach of running the same model on different dependent variables and “tallying results” is quite common. For example, many studies on determinants of welfare spending develop a generic model to explain “generic” spending, and then apply that set of explanatory variables to specific categories of spending. While it is not necessarily wrong, it is quite often poor way to test a model. Is a variable that is significant across four different dependent variables a “better result” than variable that is significant for one? What leverage are we looking for?

First, it is quite plausible that a model (and its accompanying set of explanatory variables) that explains one dependent variable might do poorly as an explanation of another. Just as aggregate growth models do not pretend to explain growth of specific sectors of the economy (at least without substantial revisions in the granularity of data and with augmentation), so too should we not develop, or expect, models of total spending and expect them to perform in the same way on health spending or military spending. A good solution to this problem is to find other empirical models *successfully* explaining the dependent variable (which may be unrelated to you explanation of interest), and use those as controls. Unexpected results that contradict what others found (or everyone is saying) should be initially troubling, though, of course not necessarily wrong.

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<sup>25</sup> Consider a bivariate relationship with 20 observations that is highly correlated (say .7) save for one outlier. Create a dummy variable for the outlier, and you can created statistical significance!

A second issue concerns the relationship between variables/measures and concepts. Bad measures are bad measures. The globalization literature has focused primarily on employing variables that describe the *aggregate* market integration situation well, but which are not applicable indicators of the degree national level integration. In other cases, the actual relevance of indicators is less well worked through. This results in at least vagueness as to what inferences one can draw, and interferes with clear evaluations of concepts.

An important example in this regard is the lack of a coherent model of national responses to globalization. Many existing academic accounts of globalization effects by political economists really resemble the unrealistic and often hysterical popular press accounts. The expectation is that, in the absence of legal restrictions on capital movements in most (if not all) states, states become caught in a war of all against all for mobile investment. All states, via backward induction and an inability to act collectively under international anarchy, recognize that a failure to succumb implies a capital strike, and quickly and dramatically reduce their demands on capital.

Instead, one should probably start with a more reasonable assumption about adjustments to new equilibria (or perhaps even smaller elements of the relationship, like whether states use taxes to compete for foreign investment or highly skilled labor.) It is probably plausible to assume that globalization means that investors are *more likely* to tax shop, but there will be a distribution of investors leaving many who remain “home bound.” Thus, states that do not adjust immediately to the new equilibrium, do not face a world of zero investment, though there may be signals (or facts) which imply that something is “amiss.” States may adjust, or may decide, like many states did in responding to macro-economic crises in the 1970s, that they will weather the storm (i.e., they do not observe the new equilibrium). Since investment rates and the pool of investors in a given year is noisy (some years economic nationalists are building new plants, in other years rational globalists), one might even find it hard to evaluate whether policies are actually altering investment patterns.

In summary, even if globalization points toward a new economic equilibrium of smaller states, lower tax burdens on capital, etc., the path to that equilibrium may be long and varied. Does globalization means a steeper implicit trade-off between national growth

and national equity? If it does, states may still choose whether or not to pay that price, with a cost different than reduced welfare spending --reduced growth.<sup>26</sup> Does the fact that we can certainly reject the idea of a *race* to the bottom (and virtually all results suggest that this much is true) imply reassure us that we are not *walking* there?

#### **IV. Models of the globalization process: From contemporary internationalization to historical nationalization**

One of the more obvious, but to my knowledge overlooked, sources for trying to come to grips with the political and economic implications the process of international economic integration is the contemporary history of *national* economic integration. Historians like Tilly (1995) have long discussed market integration as a process that “created” *our* units of observation (national states). While changed technological conditions may make it difficult for us to look to the consolidation of, say, the English state as relevant data for understanding the impact of capital mobility on today’s national states in a more global world, some situations might serve as better analogues for inspiration.

For instance, contemporary, strong federal systems like the United States, Canada and Australia or confederal systems like the EU can be thought of as mixed institutional arrangements where autonomous units have been subjected to processes of economic (and political) integration, yet retain many aspects of political independence. (Whether one considers these as systems “in the process of reaching an end-state or in such units is not immediately relevant.) These seem like good environments in which to evaluate the effects of integration on the subunits.<sup>27</sup> Of course, knowing how integration affected states does not allow us automatically to draw inferences about what will happen from international integration of national states in a world without a US constitution, but they may be instructive.

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<sup>26</sup> Note that if two identical states choose different points on a new equilibrium with a higher growth equity elasticity, state size/GDP could diverge because of differences between states in the growth of their numerator *and* denominator.

<sup>27</sup> For example, comparisons of US states have been used by economists studying whether there is convergence in income as neo-classical theory suggests (Barro and Sala-I-Martin 1992) or the tax effects of competition for investment (see Devereux, Lockwood and Redaono, 2002.)

If we think of the US states or EU member countries as political units that have been subjected to pressures for economic integration with their fellow (member) states, we might ask how those pressures have shaped their welfare state policies. It seems that the process of economic integration in the EU (Europeanization) has provoked some of the arguments and fears that are provoked by globalization. Focusing primarily on the richer EU states (who would be analogous to the rich countries in the global context) it is clear that citizens in many of these countries have had concerns that economic integration and free labor mobility might affect their national welfare systems by threatening “social dumping” or the movement of capital investment to “lower cost” areas. One important contrast between the EU situation and international globalization is that there are stronger institutions of international governance among EU countries. This cuts both ways, however: it probably allows for possibilities to alter unfavorable developments ex post via EU institutions (e.g., tax harmonization or higher EU social standards closer to the rich country level); but it also makes migration or investment abroad also less risky.

We might ask whether increasing economic integration has had those effects on European welfare states. Although hardly at a steady pace, barriers to trade and investment have fallen among EU members, especially since the late 1980s with the passage of the Single Europe Act, Maastricht, free labor movement and eventually a single currency. Several studies have looked at overall social spending, or components and concluded that, while spending as a share of GDP across countries has converged, on average spending is higher (even controlling for dependency ratios, etc.) (Cornelisse and Goudswaard 2002; Wolf 2002). One problem with this overall spending approach is that is not a particularly good test of generosity, since an increase in welfare dependents can lead to higher spending, even if state spending per dependent is declining.

We can examine program rules to detect changes in generosity, under the assumption that these are more directly manipulated than spending levels, and are not likely to be driven by differences and changes in age structure, the pension structure, or the economic cycle. Figure 7 plots the mean and coefficient of variation of the single unemployment benefit replacement rate against time for the 8 countries in the group of 18 that were EU members from 1971. The figure shows that, based on the generosity of unemployment benefits, there has been both a decline in the mean since the 1980s and well

as a decline in the coefficient of variation (since around 1990). This suggests that there has indeed been some downward convergence in replacement rates since the early 1980s.<sup>28</sup> While it hardly constitutes an abandonment of social protection, it is more supportive of a tightening of social protection than spending figures sometimes suggest.

Figure 7 about here

As integrated as the EU has become, there remain considerable barriers to exchange and major adjustments will be made before Europe could be considered in a truly integrated market. Even if one argues that all or most of the institutional/policy conditions for a fully integrated market are in place (single currency, basically free trade and mobility of labor), one could still argue that any adjustment to a European regional integration that corresponds to some state of affairs akin to national economic integration's of the past would take some time to become manifest in something like social policy. Some would certainly have grounds to argue that, like the national state's development, that the economic process only progresses with the dual development of a supra-regional political order.

Perhaps the closest thing to a fully an integrated market economy that retains quasi-sovereign political units would come from strong federal states. In some respects the history of US federal development seems to offer some interesting parallels. The United States permits a high degree of autonomy to its member states, and this was particularly true as recently as the New Deal. Most modern social regulatory functions (occupational health and safety, environmental protection) that today are formally determined nationally are still mostly implemented by states, and were almost completely state regulated prior to the 1960s. Most criminal law, major elements of civil law, regulation professions (lawyers, doctors, teachers, etc.), education, banking, and utilities are largely or exclusively state prerogatives. State governments have their own constitutions, are free to raise taxes

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<sup>28</sup> Recall that Italy was something of an outlier. Within Italy, the replacement rate conforms to a downward convergence: the special industry system had paid a higher than average replacement rate (80%) but its coverage was curtailed over time, while the regular system had the benefit amount increased, from practically nothing to a level still well below the average EU 9 country, while its coverage expanded. Thus, the "average" replacement rate probably decreased slightly, while the variance decreased considerably.

on their citizens, control important procedures for their representation in federal government, and have basic authority to administer their territory (including creating and dissolving local administrative units). Some prerogatives of these have been encroached upon by the federal government in various ways in recent decades, but perhaps as importantly, technological and legal changes have done much to motivate national integration in the US in relatively recent times. For example, banking deregulation in the 1980s did much to contribute to consolidation and nationalization of what had been a very state-limited banking system.

In areas of social welfare policy, states also have had a wide degree of latitude. Unemployment insurance systems are state created and administered but in conjunction with general federal guidelines. Program entitlements are not mandated and vary considerably between states (though not as much as in the EU). For most types of federal benefits (supplemental security, disability, Medicaid, housing, social assistance (TANF, formerly AFDC), states are permitted to provide additional benefits or to have a large hand in determining eligibility criteria.

Insofar as US states have considerable policymaking autonomy in areas of social welfare policy, comparisons of state policy within the United States represent an ideal laboratory to explore the social welfare policy effects of market integration. In the area of social welfare policy considering the experience of US states might be considered a useful arena to explore the “race to the bottom” versus Rogowski’s “race apart” theories of the effects of globalization processes.

Here I present a preliminary descriptive analysis of maximum AFDC+Food Stamp benefits among the US states for years 1972, 1980, 1985, 1989, and 1996 (the last year before the program was substantially changed). As an overview, US states set the eligibility and maximum benefit limits of AFDC programs and pay parts of the costs, the federal government sets the standard and pays for Food Stamp benefits. Figure 8 provides the state median and interquartile range for the benefit entitlement that a single mother with two children and no income would receive in constant 1996 dollars. Over time the level *and* variation in benefits falls dramatically. In 1972 the median state paid a benefit of about \$10,000 dollars. By 1980, this had fallen to just over \$9000, and by 1996 it was approximately \$7800. Benefits declined in every state. Over this period real median

household income and the poverty rate rose by around 16%. (In 1972 the median state's maximum benefit for a family of 3 was about 85% of the poverty line, in 1996 the median states poverty line was about 60% of the poverty line!) Indeed, the combined AFDC and Food Stamps benefit hides the level of cuts in state-determined benefits, because the value of maximum Food Stamp benefits declined only marginally over this period. This result is consistent with other research on AFDC and other state determined welfare benefits (Bailey and Rom 2004).

Figure 8 here

Obviously, these declines were not driven simply by market integration in the United States during this twenty-five year history. While it is often suggested that United States' residualism stems from institutions and values "unique to the United States," the pattern of spending in the 1970s suggests considerable disparity among what states initially valued. Many of the cuts and much convergence occurred *before* the "anti-state" backlash of the 1980s. It is possible that initial differences failed to reflect the uniform set of national preferences observed in the late 1990s, or that tax competition and fears of "adverse migration" played little role. The level of state spending has generally increased over the same period. Of the elements of US state spending, however, AFDC benefits probably constitute among the most redistributive elements of welfare state policy in a society that was a universe of imperfectly autonomous states in a very liberal economic national order and see an existing model of international globalization with an unhappy ending for the international distribution of income.

## **Conclusion**

The main purpose of this paper is to be critical and provocative about the way we have tended to go about trying to figure out what is going on between globalization and the welfare state. It has been critical at a number of levels of how we conceptualize globalization. Problems here make it hard to delineate clearly how we think globalization manifests itself in choices that state policymakers and democratic societies make, which in turn affects how we collect data to examine what the effects have been and will be.

Ultimately, this all affects whether the procedures we use to test hypotheses tell us anything about the ultimate question. The paper has been provocative, besides in its criticism, by suggesting some ways in which a slightly different perspective on the aggregate data and problem might paint a less sanguine picture about globalization as it will unfold than has been painted.

Of course, the paper has also been a little hypocritical as well. I have complained about the lack of models and myself not developed a model which derives hypotheses and tests them in a way that would does inspire more confidence in the empirical answers. For that, the only appeal is that you have to articulate a reason for change first.

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Table 1: Replication of Social Spending Models

	Total Spending	Social Spending	Govt Consumption
Lagged Spending	0.923 ***	0.889 ***	0.893 ***
Unemployment	0.03	0.063 ***	-0.004
Economic Growth	-0.299 ***	-0.112 ***	-0.113 ***
Dependency	0.076 *	0.06 ***	0.04 **
Left Cabinet share	-0.001	0	0.001
Christian Dem Cabinet Share	-0.007 **	-0.001	-0.002 +
<b>Trade</b>	<b>-0.043 ***</b>	<b>-0.016 ***</b>	<b>-0.013 ***</b>
<b>FDI</b>	<b>-0.041</b>	<b>-0.052 *</b>	<b>-0.019</b>
<b>Financial Openness</b>	<b>-0.138 **</b>	<b>-0.001</b>	<b>-0.032 +</b>
n	536		550
	1962-1993		1962-1993

+ t>1, \*p<.10, \*\*p,.05, \*\*\* p<.01

Figure 1 Outlays and Trade for 20 OECD Countries

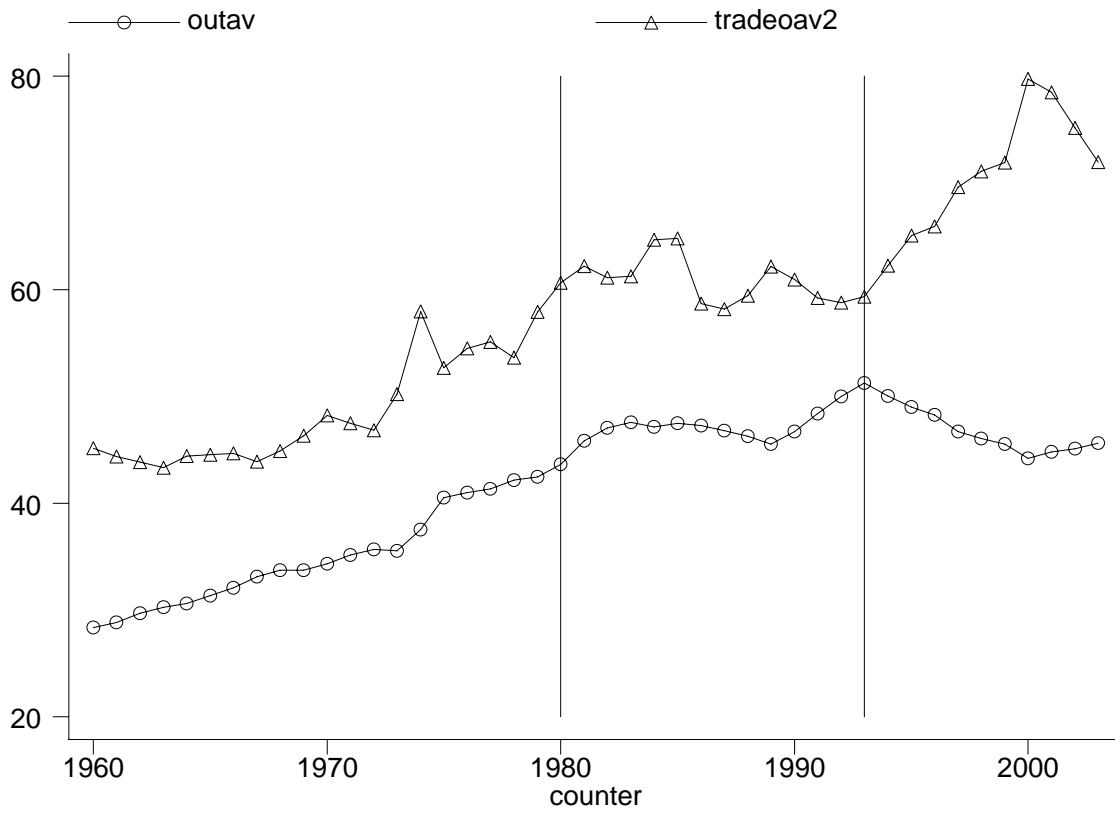


Figure 2: Transfer and Consumption Spending in 20 OECD countries

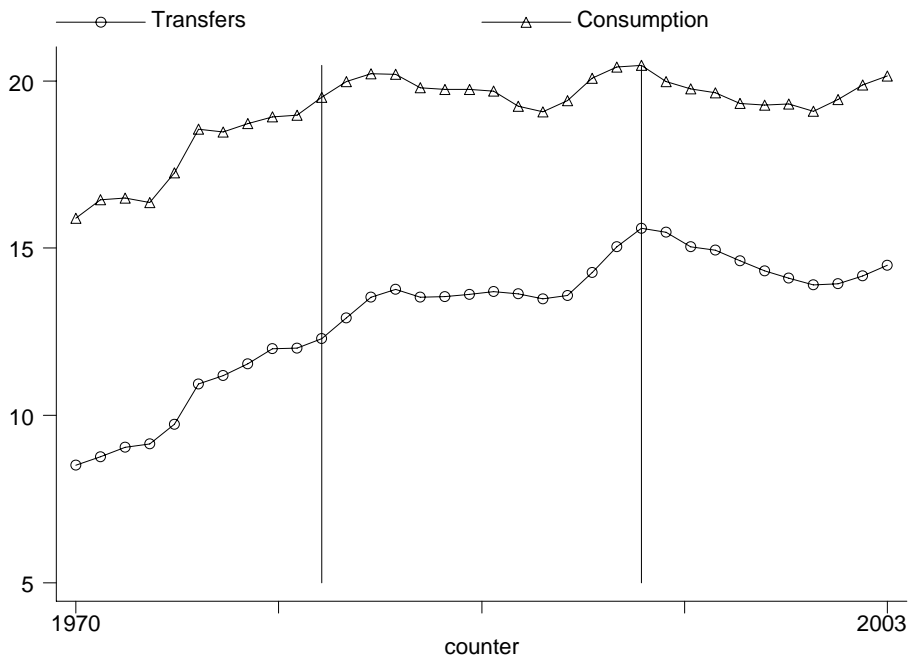


Figure 3: Average Net Unemployment Replacement Rates for 16 OECD Countries

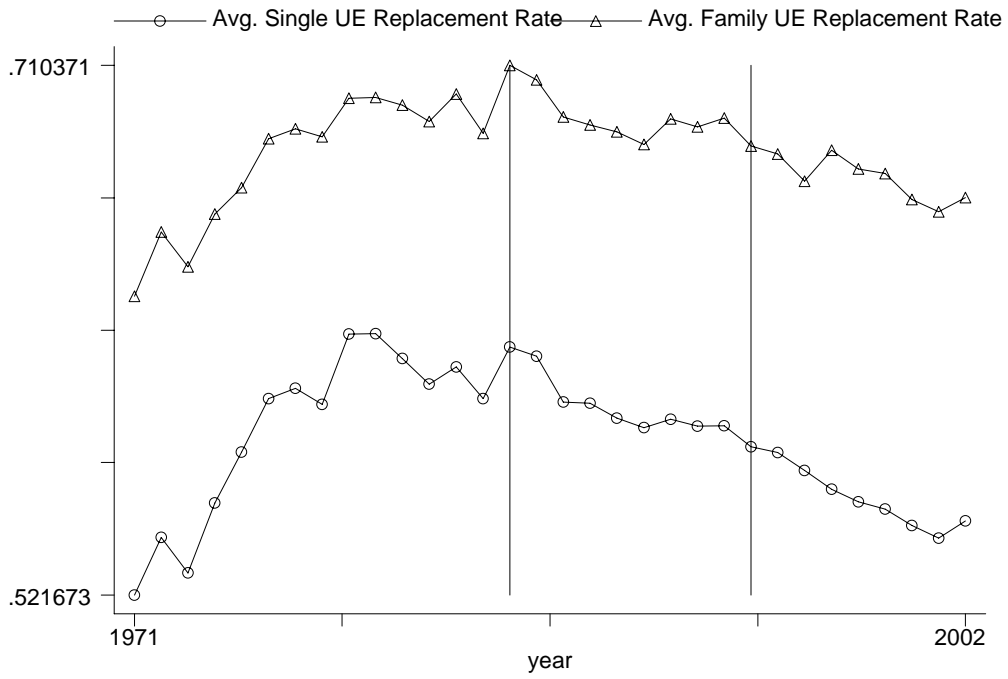


Figure 4: Swedish Foreign Direct Investment Flows

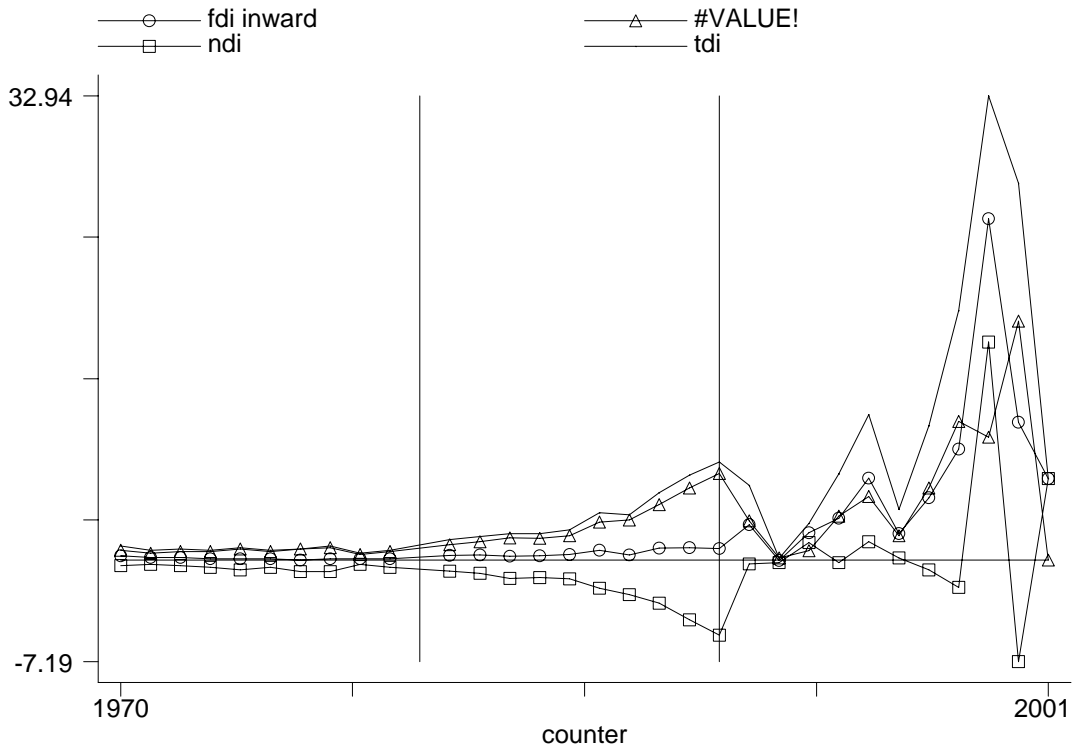


Figure 5: Trade Openness and Government Spending

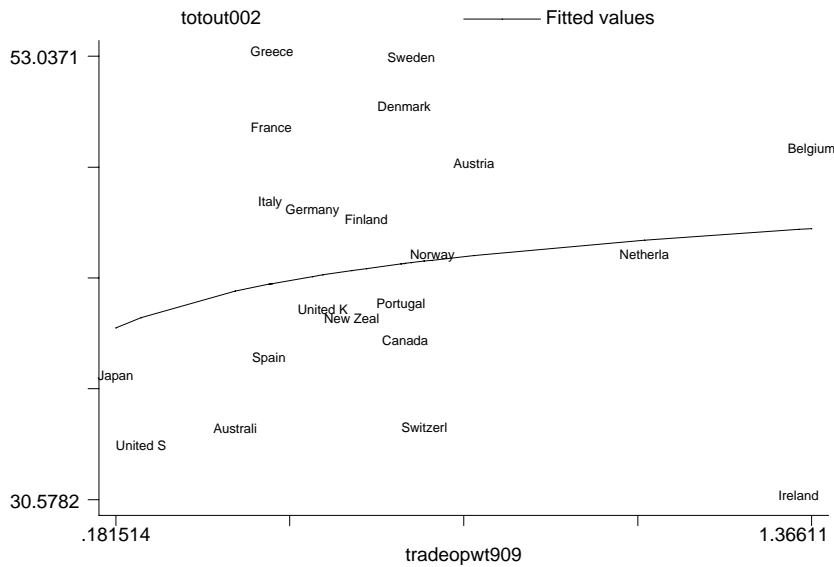
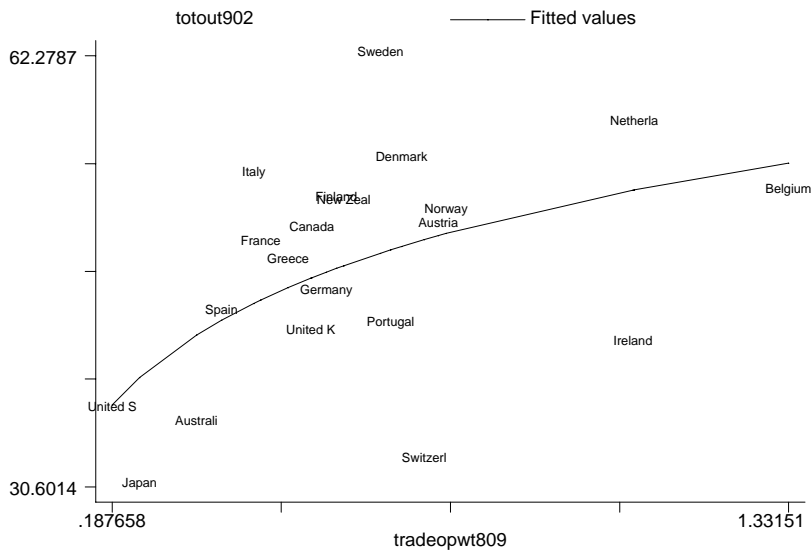
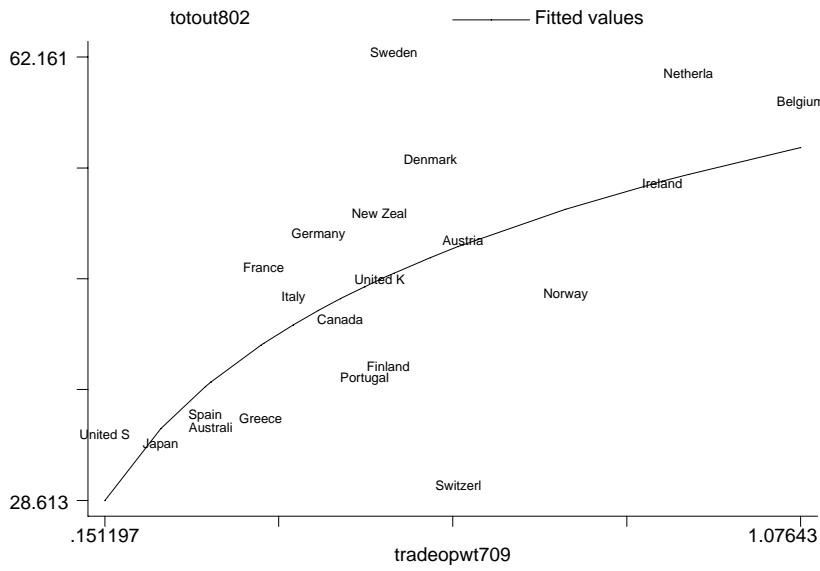


Figure 6: Median and IQR of Financial Market Openness for 21 OECD countries

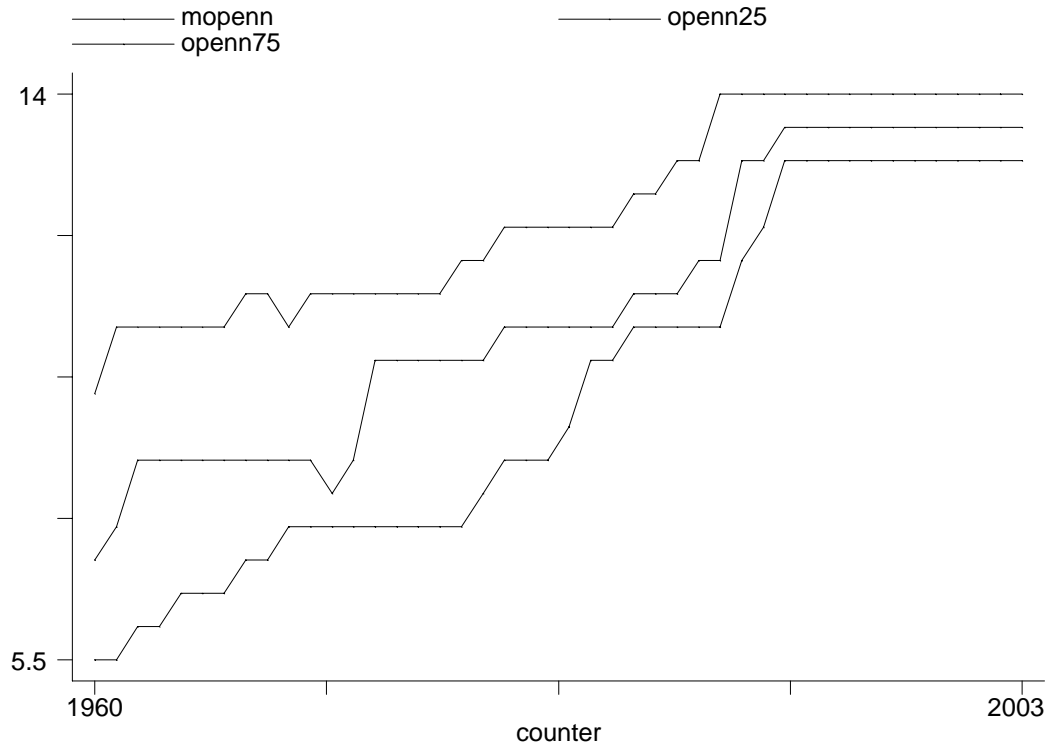


Figure 7: Average Unemployment Replacement Rate and Rate Convergence in 8 EU Member States

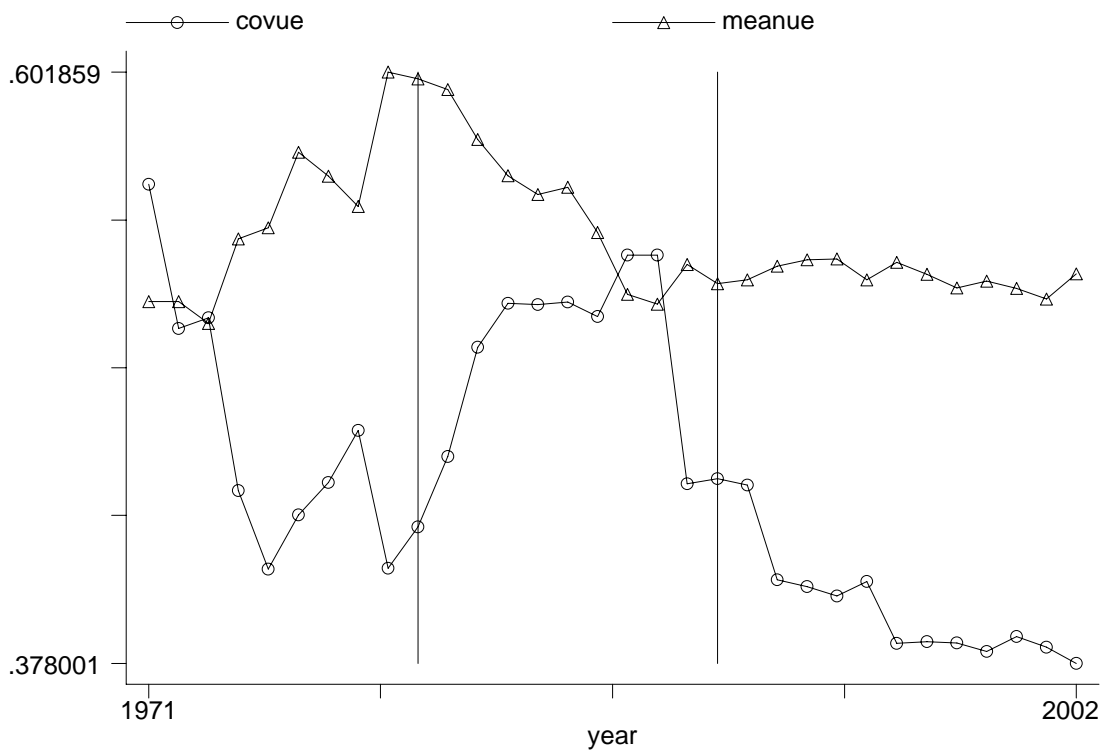


Figure 8: Median and IQR of State Maximum AFDC+Food Stamp Payments (Mother and Two Children, No Income)

