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Trade, Growth, Poverty, and Politics: Toward a Unified Theory

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This article takes the opportunity presented by the current global downturn to reassess the latest scholarly work on globalization's long-term implications for economic and political development. Will the market inequities generated by trade and international interdependence systematically undermine the domestic redistributive systems on which poor, redistribution-reliant citizens depend for their economic well-being and continuing engagement with society? Or should we expect to find trade-induced market inequality biasing political systems in exactly the opposite direction—toward more, not less, market-correcting redistribution? To answer these discipline-spanning questions with any degree of confidence, we will first need to develop a more theoretically integrated model of the mechanisms that link market inequality to nonmarket redistribution. Creating that model—and, to that end, unifying major theoretical strands within political science and economics—should be our first priority.

Keywords: Trade and Globalization, Theories of Political Economy, Market Inequality, Redistribution, Poverty, Welfare Policy, International Political Economy, International Relations.

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Este artículo toma la oportunidad que plantea la actual crisis de la economía global para re-evaluar la investigación sobre las implicaciones de largo plazo de la globalización para el desarrollo económico y político. El artículo considera dos escenarios ¿subvertirán sistemáticamente las desigualdades generadas por el comercio y la interdependencia internacional los sistemas redistributivos de los que dependen los ciudadanos pobres para su bienestar económico e inclusión sostenida en la sociedad, o podemos esperar que estas desigualdades sesguen a los sistemas políticos hacia una mayor redistribución? Para responder a esta pregunta con algún grado de confianza necesitamos primero desarrollar un modelo teóricamente integrado de mecanismos que vinculan la desigualdad del mercado a la redistribución originada por fuentes distintas al mercado. Crear tal modelo, y con tal fin, unificar las corrientes principales de la ciencia política y la económica, deberá ser nuestra primera prioridad.

It has been said about Miles Davis, possibly by the great jazz trumpeter himself, that the notes he did not play were more important than the ones he did. Much the same applies to globalization or, more specifically, to the manner in which the topic has been studied by political scientists and economists over the past couple of decades: it is the arguments we do not hear that deserve our closest attention.

This article zeroes in on several of those arguments, or *potential* arguments, with the aim of explaining why research in political economy, international relations, and development, as productive as it has been in helping us understand key aspects of the shift toward globalized markets, has nonetheless managed to sidestep the important theoretical and empirical dimensions spotlighted in this article. These other dimensions, the missing notes I will be trumpeting, are multifaceted and complex and, as such, raise more questions than any one economist or political scientist could reasonably hope to address. My purpose here, however, is not to provide a full accounting of globalization's causes and consequences (some aspects of which I have tackled in other work) but to suggest why these issues deserve a fresh look. That "refreshing" of an older set of globalization debates and a literature which, after many productive years, might seem to have run its course is unlikely to occur until others see the gaps in this accumulated body of work that I see. The work of filling in those gaps is crucial and, from a policy standpoint, the sooner it can begin and the more systematically it is pursued, the better. But engaging in that work strikes me as the second step.

The first step, and the more modest task undertaken here, is to explain why all of this (comparatively) radical new theoretical and empirical work is even needed. Keeping international markets open is surely in the world's long-run interest. Would the United States today, or a hegemonic China tomorrow, really be better off with a world economy less open to trade than the one we currently inhabit? Perhaps not. But even today, after years of high-quality research, there is still a great deal we do not know concerning the long-run impact of open trading regimes and the regulatory environments that support them. One reason we don't know—not the only reason, to be sure, but an important one—is that earlier contributors to the globalization literature neglected to debate, and so never resolved, a number of key theoretical points. It is the key-ness of those points (to stay with the musical metaphor) that I emphasize in what follows, as well as their resounding absence from our existing body of knowledge.

As for what explains that absence, here my analysis singles out the hyperfragmentation of previous theoretical research. What the existing globalization literature offers is less an overarching conceptual framework, I will suggest, than a series of discrete theoretical propositions, each addressed to a single causal pathway, a single piece in globalization's larger theoretical puzzle. We are, as a result, a long way from the "unified theory" toward which international and comparative political economists—or indeed any parsimony-loving social scientist—should be aspiring.

In laying out this argument, I start with the economics: will trade end up inhibiting rather than stimulating growth? Will it drive a wedge between the earnings of the highly educated and the unskilled? On neither question has the economics literature reached a definitive verdict. The first part of the second section describes the evolution of recent economic work on both questions, narrowing the focus to trade's effects on the long-term living standards of each globalizing country's poorest inhabitants. The section then draws on an older notion from economics—the so-called efficiency/equity trade-off—to suggest how these parallel and, until now, largely separate debates might be brought together. That trade-off is one of the issues that economists have not always confronted, but whose ultimate resolution is, I will argue, absolutely critical to answering the larger "is it good for the poor?" question that fuels inquiry into globalization for the majority of scholars studying it.

The third section turns the spotlight on the globalization debate within political science. Here it is the causes and short-term political consequences of openness that have received the lion's share of attention. Inasmuch as longer-term considerations do get discussed, attention usually centers on the welfare state and its precarious future in an era of ever more deeply integrated global markets. For some political scientists—and, frankly, for most—that future looks bleak. But an absence of debate is not the problem here. To the contrary, inquiry into trade's long-term effects has spawned two separate theoretical debates within the political science literature, each of which I briefly discuss. Taken together, these twin debates—one exploring the political foundations of global capitalism, the other focused more narrowly on the state's role is providing social insurance—might give the impression that there is no longer any new theoretical terrain left to explore. Yet that is not the case. The task ahead lies not in uncovering new causal pathways so much as integrating these political science debates into the economic debates referred to in the second section. Although many of us call ourselves "political economists," it is not immediately obvious how this sort of theoretical integration should proceed or, assuming the marriage does eventually take place, how it will help us understand actual events and processes. Here, then, is another debate that is not happening, and should be.

Taking up this challenge—and moving beyond the discipline-by-discipline critiques of the second and third sections—the analysis in the fourth section emphasizes the importance of adding up globalization's economic and political effects, rather than looking myopically at any one discipline's familiar set of causal relationships. By embracing the "additive" political economy framework proposed at the beginning of this section, both disciplines could sharply reduce the potential for theoretical bias that accompanies any partial approach. The virtues of simplicity notwithstanding, however, the second part of the fourth section makes the case for a (very slightly) more complicated theoretical model. What matters, after all, is not just how the economics and politics of globalization net out; it is how they *interact*. A number of these interaction effects have been studied by previous scholars, some taking their lead from discipline-spanning, hence hard-to-classify, political scientists Jeffry Frieden and Ronald Rogowski, others from label-defying economists such as Tim Besley, Avinash Dixit, Daron Acemoglu, and James Robinson. But while some of the interactions between trade's political and economic dynamics have been scrutinized in the literature, the number of discipline-spanning pathways is potentially infinite. Have we drawn the lines in the right places?

In the fourth section, I suggest that the link from earnings inequality—a trade-related variable on the economics side—to domestic redistribution—a political variable—is an especially promising avenue for future research. In part, that is because its underlying theory and empirics are less well understood than those underlying other discipline-spanning interactions. Again, however, there are many trade-related interactions that could be better understood. My main reasons for calling attention to this particular interaction, the link between inequality and redistribution, are normative: from an ethical perspective—if our ultimate objective is to build a more fair and just, and not simply a more prosperous, international economic order—it is the link most urgently in need of clear-headed analysis.

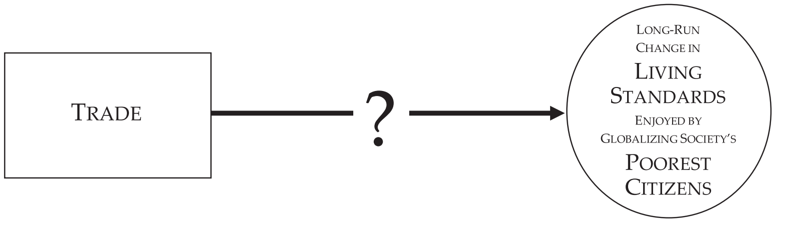
One reason why the link between inequality and redistribution has been overlooked is conceptual. When economists talk about inequality, it is the distribution of market earnings that they have in mind, not justice. And yet, as Amartya Sen (1999) likes to remind his fellow economists, inequality is a broader concept. What matters—insofar as inequality matters—is not that some households earn less than others in the marketplace, but that what those households get to "take home" is also less, perhaps a lot less. Global integration may well be contributing to domestic inequality in the first sense; both theory and a great deal of empirical work suggests as much. But is it fueling inequality in this second, more fundamental sense? Is it creating an earnings gap within globalizing societies, or is it also—and more problematically—creating a living standards gap?

Enter politics. For economists to answer the question on the basis of earnings data alone, without also taking into account the redistributive role of government, is to bias their analyses. With a sufficiently aggressive (and progressive) political intervention, any distribution of market earnings could be altered to ensure that the lowest earners end up with living standards no different from those enjoyed by the highest earners. It is politics, then, at least potentially, "all the way down." For economists to leave government's redistributive function out of the globalization equation is thus to "commit" omitted variable bias, an unpardonable sin in any statistical church. The fourth section concludes by suggesting how this bias might be removed. Rather than exposing theoretical gaps, here I unearth, with the goal of remedying, an empirical deficiency—the redistribution literature's reliance on unnecessarily restrictive measures of progressivity.

One has to start somewhere, of course, and the diverse theoretical and empirical strands I am suggesting we pull together consist, as they must, of a series of relatively narrow studies. Partial-equilibrium analyses are not just useful; they are essential. At the same time, however, the path-dependent processes that generate these analyses can sometimes lead scholars astray, focused on one path or another rather than on how, when viewed from a distance, these paths intersect and, with the addition of just a few new roads, could become an (analytical) superhighway. Even when our eyes are focused on the road, we should never lose sight of our ultimate destination—which, for social scientists, is likely to be some individual's or group's quality of life, including the ability to shape that life in conjunction with others. If we fail to integrate all the different partial equilibria into a general equilibrium, we will never reach that destination; we will never address the fundamental "is it good or bad?" questions that policy makers need our help answering.

The analogy to Miles Davis's missing notes is not quite right, then. Rather than notes, what's missing in the study of globalization—if the thrust of my analysis is correct—are whole lines of debate and the answers to

Figure 1.
The Ultimate Question:
Will Globalization End Up Helping—or Hurting—the Disadvantaged?



discipline-spanning theoretical and empirical questions of major policy importance. Davis's attention-grabbing silences may be beautiful, but the current globalization debate, with such a stellar track record of past success and with so much presently at stake, is no place for silence. It is time to turn up the volume.

The Theoretical Progression in Economics

When economists debate globalization, most of the time they are debating—implicitly or explicitly—whether its long-run effects are good for the poor. This puts the question in Rawlsian terms (Rawls 1971). It is fine if trade benefits wealthy individuals and families, but a country's opening to international markets can be considered just or moral only if that opening also improves the well-being of each globalizing society's poorest inhabitants.[1]

This controversy is depicted in Figure 1, a simplification intended to focus attention on the fundamental question at issue: will globalization ultimately prove helpful to poor individuals and families in the bottom rungs of society, or will its long-run effects leave them in an even worse position?[2]

[1] While my discussion emphasizes globalization's impact on the lower ranks of society, other assessment criteria are, of course, also possible. At least one moral philosopher, Peter Singer (2004), believes the appropriate metric for evaluating globalization is its impact on the poorest people in the entire world—the "bottom billion" in Paul Collier's (2007) useful coinage—rather than the poorest inhabitants of each society, some of whom may be quite well off in global terms. One could also assess a trade regime by reference to its impact on international (e.g., North-South) inequality or global warming. Indeed, the list of criteria one might wish to employ is potentially endless, ranging from national or global security at one end of the (realist) spectrum to identity, culture, and possibilities for civic engagement at the other. How all these alternatives compare with the Rawlsian standard—which criteria are more legitimate and which are less—is not a question I will explore in any depth here, however.

[2] As discussed in the previous note, the term "poor," as I am using it here, is nationally specific. This is an important distinction as, from a global perspective, the poorest inhabitants of industrialized societies are often much wealthier than the poorest citizens of other, less advanced economies.

As policy questions go, it is hard to get more basic than this—or, in the years following the antiglobalization protests at Seattle's World Trade Organization (WTO) ministerial conference in 1999, to incite more controversy and impassioned debate.[3] Over time, much of that earlier passion subsided, although the question continues to excite discussion (if not, thus far, street protests) among academic economists.

Globalization is not entirely a matter of choice, of course, for policy makers or anyone else. If China's recent experience is any guide, cross-border flows of information would be particularly difficult for a "backlashing" government to regulate, let alone shut down. That said, there is more to globalization than the flow of information, and if officials in the industrialized countries were to decide at some future date to clamp down on international flows of capital and, certainly, of manufactured goods—of tangible commodities like lumber or steel or widgets—it would be a mistake to assume they would fail. What, then, *should* they decide?

Given how thoroughly economists have dominated the theoretical analysis of trade policy to this point, both in print and on the public stage, it makes sense to begin with their answer to the question. Several pioneering thinkers have a specific take on the subject (see Bhagwati 2004; Collier 2007; Dollar and Collier 2002; Krugman 1997; Rodrik 1997, 2011; Stiglitz 2006). But stepping back from their individual contributions, one can ask what economists *in general* believe globalization's ultimate impact will be. Do they think that it will it help the poor—or will the opening up of international markets, if permitted to continue, just make matters worse?

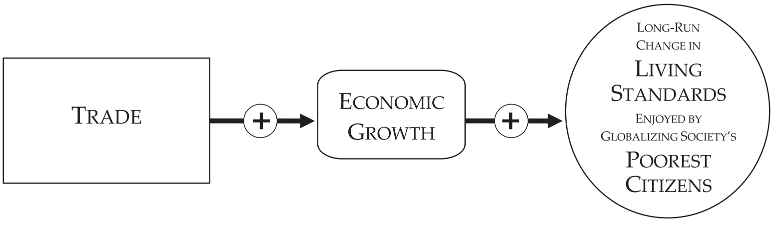
Two Cheers for the Washington Consensus

If asked today, most economists would surely place themselves in the pro-globalization camp (or *camps* since there is not just one). Why? At least part of the answer has to do with the pro-globalization side's longer time frame: the arguments invoked by the antiglobalization side have tended to privilege short-term considerations, the dislocations that can be inflicted upon the poorest households in a society—and the middle classes too, for that matter—during the adjustment period that ensues after foreign goods, services, and capital begin "flooding" the domestic market. While these dislocations can be serious, however, they are also temporary, and their negative consequences are in any case overwhelmed by globalization's longer-run positive consequences. Or so goes the pro-globalization side's economic theory.

And it is, like it or not, a theory: it arises from a theoretical understanding of how globalization influences economic outcomes. And like any good theory, it identifies a causal mechanism, a chain of logic that is, as even its critics would agree, both clear and remarkably simple. "Trade produces growth!" is the

[3] On the persistence of "interdependence sovereignty," see Krasner (1999).

Figure 2.
The Washington Consensus



sound-bite form of the first link in this chain, while the title of a widely cited World Bank study—"Growth Is Good for the Poor" (Dollar and Kraay 2002)—is a wonderfully succinct crystallization of the second.

This two-step causal chain is the theoretical basis underlying what is popularly known as the Washington Consensus (see Figure 2). To describe it as a consensus may be something of a stretch today, but its basic logic was once embraced by the vast majority of globalization analysts—outliers such as Dani Rodrik and Robert Wade excepted (see Rodrik 1997; Wade 2003a [1990])—who grappled with its long-term economic consequences and not just the shorter-run political disequilibria that trade can create along the way.[4]

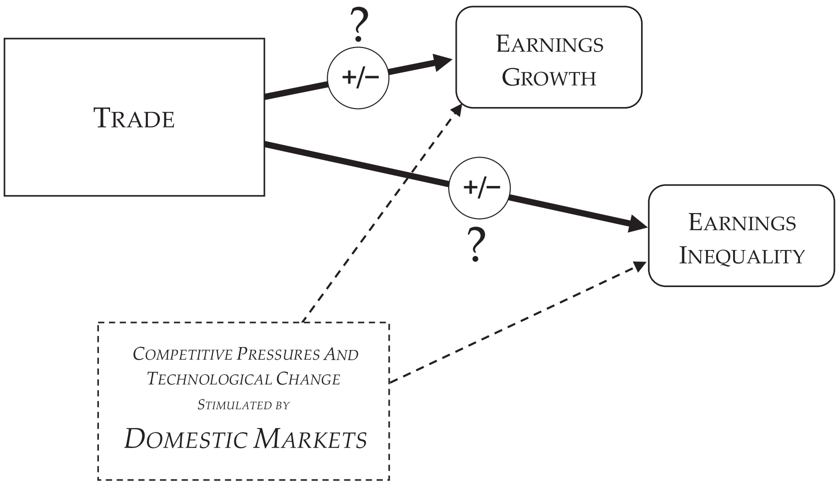
Mechanisms matter, but globalization does not just affect growth rates—and even when it comes to growth, the past ten years have seen a fraying of the near-consensus position within the corridors of the World Bank and university economics departments alike (see e.g., Easterly 2002; Milanovic 2005; Rodrik 2011). In truth, the causal nexus between markets and growth has never *not* been a controversial topic among economists; Adam Smith first put the issue onto the agenda back in 1776, and economists have been arguing about it, painstakingly but productively, ever since.

While some of these arguments—like Smith's own—concern theoretical issues, the empirical task of weighting globalization "versus" other causal mechanisms is the one toward which the economics literature has recently been gravitating. If asked to list what is missing from the globalization debate, then, most economists would probably start here, with the empirics—and with measurement issues more specifically (see Figure 3).[5]

[4] The "basic" causal sequence depicted in the diagram subsumes a great many subarguments and debates, of course—everything from Ricardo's logic of comparative advantage to Krugman's (1992) arguments concerning the geographic clustering of economic activity and its contribution to long-run growth. Bhagwati (2004) and Wolf (2005) provide two especially lucid expositions here; see also Gruber (2011).

[5] Over the past 15 years, economists working on the empirical front have been trying to derive—with, as they themselves would concede, only moderate success—a clean way of

Figure 3.
Globalization and Welfare: The Debate within Economics



Yet this is too easy. An openness variable is unlikely ever to emerge from the econometrics as the only significant factor; no serious economist would ever attribute changes in average household earnings, let alone the distribution of earnings across an entire economy, to a single source. Other sources—other variables unrelated to the lowering of barriers to global trade and investment—will also need to be introduced. And yet, when we look at those other variables, we find that some of the competing explanations from which they originate are endogenous to globalization itself.

measuring a country's openness to international markets. The earliest work incorporated trade shares (the sum of exports and imports as a proportion of gross domestic product [GDP]), with subsequent contributions making greater use of policy-focused indicators of trade openness such as the Sachs/Warner index. See Rodriguez and Rodrik (1999) for an early overview and critique. Not that trade openness is the only variable presenting such complexities—operationalizing economic inequality is no easy task either. Some of these difficulties are deeply rooted and conceptual, raising philosophical questions of the sort I discuss below. But these are not the only obstacles, as exchange rate conversions, household equivalency formulae, and myriad other mundane-but-unavoidable mechanical issues can pose problems as well. Another is the failure of previous researchers to distinguish clearly between earnings inequality and disposable-income inequality. These and other methodological limitations are highlighted—and bemoaned—in Atkinson and Brandolini's (2009) masterful overview of econometric work in this area (see also Gruber 2011; Mahler and Jesuit 2004; Milanovic 2005). Limited by the data, contributors to this empirical literature have tended to concentrate on wealthier countries. For evidence that earning divergences have been growing in a number of the developing world's most open economies, see Goldberg and Pavcnik (2007) and IMF (2007).

Two of these other dynamics are the weakening of labor unions and the development of new technologies biased in favor of highly skilled workers.[6] In each case, the dynamic in question would itself seem to be a product, to at least some extent, of trade-induced competitive pressures. Given this endogeneity, we may never know exactly how much weight to assign trade's independent contribution, and even the direction of its effects—the signs on the coefficients—are likely to remain matters of intense debate.

But while all these things may be controversial, the idea that countries have recently been growing ever more deeply integrated into the world economy is not. On this, at least, everyone can agree. And if globalization matters today, when it is still, as many have argued, in its infancy, it seems certain that its effects will magnify over time. The empirical literature's trade coefficients, as large as they may be today, are destined to grow in size and significance as we move into the future.

Moving, then, from the empirics of trade back to the theory, I want to suggest that the theoretical debate within economics, as multidimensional and vigorous as it has been, still leaves something out. Normally that would not be a problem. What Roger Federer once said about his tennis career also applies to the social sciences: there is no finish line. The trouble here is that, while some of these missing pieces are on the verge of being discovered, others are not. For, in truth, many economists do not know they are missing.

What's (Still) Missing from the Economic Debate?

The main pieces I have in mind here are political pieces. There are political dimensions to globalization that have occupied political scientists but that economists tend to ignore. Not that political scientists aren't missing pieces of their own, of course. Later on I lay out what I regard as a major deficiency in the political science slice (sliver?) of the globalization literature to date—that it pays too little attention to the link between political fundamentals, such as the size of the welfare state, and what I would term "ultimate" fundamentals, such as poverty, living standards, and the like. The economics literature at issue in this section can be faulted for much the same thing, however: economists have been more interested in rooting out the causes of intermediate fundamentals—earnings growth on the one hand, earnings inequality on the other—than in exploring how they combine to produce the end-state fundamental that is an individual citizen's or family's subjectively evaluated quality of life. Yet, from a strictly theoretical standpoint, how growth and inequality combine is absolutely crucial.

In theory, for example, the growth stimulated by globalization could have the effect of lifting the earnings of a society's average household while at the

[6] Deindustrialization could perhaps be added to the list (cf. Iversen and Cusack 2000), although the forces driving deindustrialization are themselves driven by technological changes and weakening unions—or so, as a first approximation, one might reasonably assume.

same time depressing the earnings of that same society's bottom-end households. And there are other possibilities as well. What if disadvantaged households at the bottom tail of the earnings distribution, rather than suffering an absolute globalization-induced decline in their market incomes, were to endure "only" a relative decline? Would we now regard globalization as a win-win proposition?

It is not an easy question to answer and not only because it touches on long-standing philosophical debates located outside the realm of economics. The question touches on practical issues as well, issues amenable to empirical, even quantitative, analysis. To take just one of many possible avenues warranting further investigation, return to the moral philosopher John Rawls and consider how he suggests we ought to evaluate departures from strict equality. If the departure is one that would advantage my own family—and I know this in advance—I myself would be likely to assess it positively. But my neighbors could object, and legitimately so, on ethical grounds. In *Theory of Justice*, Rawls (1971) argued that inegalitarian distributions were morally acceptable only if they would have been approved by people who did not know ahead of time how they themselves would fare in this new, less egalitarian society. In that hypothetical state of affairs, where the evaluators were called upon to render their judgment from behind what Rawls termed a "veil of ignorance," he suggested that departures from equality would be considered fair, hence acceptable, only as long as the gains accrued by the departure's beneficiaries would be unlikely to result in any losses for other members of society. Fair enough.

Or is it? In deriving his so-called Difference Principle, Rawls assumed that the poor (or those who could imagine themselves becoming poor) would consent to a dramatic increase in the material well-being enjoyed by their better-off neighbors for the sake of a small improvement in their own. Would the poor really tolerate this sort of exchange? One's well-being is a subjective matter, after all, and there are reasons to think that globalization-induced inequality could, by its very existence, drag it down—at least as far as well-being is subjectively understood and experienced by real people. For all the difficulties involved in quantifying happiness, recent efforts suggest that inequality does tend to depress subjective well-being (see e.g., Solnick and Hemenway 1998; Stevenson and Wolfers 2008; Wilkinson and Pickett 2010).

To be sure, globalization may not be "inducing" inequality after all. As noted earlier, the link between trade and inequality remains a hot topic among economists, both conceptually and empirically. On one scenario—and at this point hardly any possibility can be ruled out—globalization will widen the earnings gap in the capital-rich countries of the industrialized North while, in keeping with the familiar Heckscher–Ohlin trade model, it reduces the gap in the labor-abundant South. Assuming (controversially) this is how it plays out, one might be tempted to conclude that the growth-versus-equity trade-offs that I have been emphasizing here no longer apply universally, that in the developing world, at least, the poor will enjoy both growth *and* equity. Thanks to globalization, they can have it all.

Before we celebrate the liberalizing forces responsible for this achievement—or partial achievement (what about the labor-scarce economies of the industrialized North?)—it is worth considering whether the "all" in question is one that poor families would actually want to have. Growth is mostly a good thing, of course, but some forms of equality, even when combined with high growth, may not be worth very much. For just as the concept of well-being is subjective, so too is equity. Anyone viewing the world through Rawls' veil of ignorance may be able to agree that equity is conducive to one's overall well-being. But transitions from low to high equality do not always bring societies closer to equity in this Rawlsian sense. In theory, a developing country could undergo a globalization-induced shift toward higher incomes *and* greater equality without at the same time improving the plight of its poorest citizens. Higher growth, yes. Greater equality, yes. But equity, not so much.

Neglected though it has been by the existing globalization literature in economics, this discomfiting possibility warrants closer investigation. Let us set to one side any concerns we might have about globalization's impact on political polarization or spatial segregation so as to focus on the most basic form of inequality, the kind of inequality measured by your run-of-the-mill Gini coefficient.[7] One may be concerned that globalization is working systematically to lift that type of inequality. But suppose it is not. Even if openness and Gini-metered inequality are inversely related (as the Heckscher–Ohlin model would happily predict for developing countries), there are different ways in which inequality could fall and, whatever their political effects, some would be more *subjectively* problematic than others (see Figure 4).

Depicted in Figure 4 is a stylized society in which all ten inhabitants, as they move from time 1 to time 2, see their earnings rise and their society's aggregate (nonspatial) inequality fall. That fall, indicated by the Gini coefficient's decline from .30 to .23, reflects the doubling of earnings enjoyed by the bottom eight members of this society, an increase greater than the 50 percent rise enjoyed by the society's smaller elite. And yet, in absolute terms, the $20 increase in the earnings of each member of this elite dwarfs the absolute $10 gain accrued by

[7] For the theory underlying concerns about political polarization and spatial segregation, see Esteban and Ray (1994), Wilson (1997), Baldwin and others (2003), Dreier, Mollenkopf, and Swanstrom (2004), Kanbur and Venables (2005), and Stewart (2010). For present purposes, the main points are that: (1) polarization and segregation are different, both from each other and from the types of inequality proxied by Gini coefficients; (2) a number of societies, as they have become more open to international market forces, have also become more polarized politically *and* more internally divided in spatial terms; and (3) there are good theoretical reasons to think that, in both cases, the association is causal, that is, that globalization has been accentuating these trends. I am resisting the temptation to delve any further into these issues here as the theoretical correspondence between openness and nonspatial (Gini) inequality is plenty enough to delve into for one study, and we will only be dipping our toes into the water as it is.

Figure 4.
More Is Less: Earnings Growth and Inequality in a Stylized Ten-Person Society

	Time 1 →	*Time 2* →	*Time 3* →	*Time 4*
elites	$40	$60	$90	$135
	$40	$60	$90	$135
masses	$10	$20	$25	$30
	$10	$20	$25	$30
	$10	$20	$25	$30
	$10	$20	$25	$30
	$10	$20	$25	$30
	$10	$20	$25	$30
	$10	$20	$25	$30
	$10	$20	$25	$30
gini	0.30	0.23	0.27	0.33

each of the other eight citizens. Would the lower eight be content with this situation? Can we say with any confidence that they would prefer time 2's distribution to time 1's?

Perhaps even more significant—and, from the perspective of the lower eight, even more worrisome—is the increasing absolute gap separating the lower eight from the upper two, a gap that rises sharply as we move from time 1 to time 2. While the have-nots at the bottom were once but $30 away from joining the haves at the top, by the time we reach the second period the gulf separating them from the elite has risen to $40.

In a myopic sense, our stylized society meets the Rawlsian criteria for justice: as the economy transitions from time 1 to time 2, the earnings of the poorest eight individuals do not just grow; they double. Applied correctly, however, the Rawlsian standard requires that the gains enjoyed by a society's haves leave the have-nots better off in an *overall* sense, and looking at the time 2 distribution, it is not at all clear that this threshold has been reached.

Figure 4 is also useful for making another point about the dynamics of inequality, although in this case less about the way those dynamics are perceived and evaluated than about the objective trends themselves. Return, then, to our stylized ten-person society and consider what happens as it moves—and its economy grows—through time. As just noted, the 50 percent increase in the earnings of the elites that occurred during the transition from time 1 to time 2 "pulled up" the earnings of the other eight members of society by a full 100 percent. Here then is a case of rising tides lifting all boats (as the familiar expression goes). The description also applies to the transition from time 2 to time 3—the rising tides enjoyed by the elites in time 2 have a similar boat-lifting effect in time 3—only this time the elites' boat-lifting power has declined considerably: instead of the elites' 50 percent earnings increase translating into another 100 percent increase for the lower eight, the shift from time 2 to time 3 sees the earnings of the latter rise by only 25 percent, from \$20 to \$25. And as the society transitions from time 3 to time 4, the same proportionate 50 percent increase in the earnings of the elites has an even weaker effect on their less fortunate neighbors, whose earnings rise as before, but only by 20 percent, from \$25 to \$30. Note further that the Gini is now, in time 4, higher than it was back in time 2.

The point here is straightforward: the boat-lifting potential of a society's rising tides should be treated as a variable rather than a (positive) constant. There is, to my knowledge, no *a priori* reason to assume that, as a society grows, the earnings accrued by households at the top of the distribution will continue to have the same boat-lifting effect on households at the bottom. For what connects the elites to the others is not a rope, as the standard way of thinking about these things implicitly assumes, but a rubber band, the elasticity of which deserves closer scrutiny than it has been given thus far. Are trade and globalization operating in ways that tighten the band or loosen it? And if the band is loosening, how long before the "masses" populating globalizing societies see their boats begin taking on water? Instead of sink or swim, might the world economy's move toward greater openness be better characterized for some societies as sink *and* swim, the swimming coming first, the sinking later?

Globalization and Political Science

Explaining—and conjoining—the separate growth and inequality effects of openness is a big project and certainly one worth doing. That said, the theoretical progression I began tracing in the second section has more than two strands, and the two on which economists focus—the growth-of-earnings strand and the distribution-of-earnings strand—may not actually be the most important. Or that, at any rate, is how political scientists see it.

Unlike many activists and political pundits, but *exactly* like their economist colleagues in the academy, political scientists appreciate the importance of theory. It should come as little surprise, then, that when political scientists debate globalization, they too start with fundamental theoretical questions. In fact, inquiry into the long-term consequences of international economic interdependence has sparked not one, but *two* distinct debates within the discipline.[8]

The "Intermediating" Politics of Growth and Market-Driven Inequality

While both debates have been productive, the causal claims underlying the Washington Consensus are the focus of only this first one, which is why it makes sense to begin with it. At issue here are the ways in which—and also the extent to which—political variables can influence the growth and inequality dynamics emphasized by economists. Economists do not dispute that political realities often get in the way, weakening the associations we have just been discussing between globalization and growth, on one hand, and globalization and inequality, on the other. But while they do not dispute that politically induced de-linkings of this sort can occur, neither do economists spend a great deal of time analyzing why and how they occur, or how seriously they can skew things when they do.

That is where the political scientists engaged in this first debate enter the scene. Because the links between trade policy, growth, and inequality are so heavily contingent on political conditions, these conditions should, they argue, be a major focus of theoretical and empirical investigation. On this point, political scientists are in broad agreement (Keohane and Milner 1996; cf. Moe 2005). Where the controversy begins is with the investigations themselves as well as the methodologies employed in undertaking them.

Take, for instance, the Varieties of Capitalism debate kicked off by Peter Hall and David Soskice in the late 1990s and raging ever since (see Hall and Soskice 2001). A central theme in that literature—perhaps *the* central theme—is that the extension of markets, first domestically and now globally, need not result in inegalitarian market outcomes. That may be how it has played out in the United States, but, owing to their very different constellations of political actors and institutions, other industrialized countries—Hall and Soskice's "social market economies"—have ended up with considerably smaller wage gaps. Politics intervened.

Or maybe not. Maybe the economic logics discussed earlier have been intermediated by politics, just as Hall and Soskice (2001) suggest, but the specific political and labor market institutions that Hall and Soskice identified were not the decisive ones. Others mattered more. Even today there is, as I said, a raging debate, and no one rages over points of agreement.[9]

[8] To be fair, the globalization debate within political science goes well beyond the two controversies spotlighted here. As noted, however, most of the studies contributed by political scientists focus on globalization's political origins—America's "go it alone power" (Gruber 2000, 2001), for instance, or the diffusion of ideas (e.g., Goldstein and Keohane 1993; Ikenberry 2000; Slaughter 2005; cf. Koremenos, Lipson, and Snidal 2001)—rather than its longer-term effects on democratic institutions or living standards more generally.

[9] Another issue of contention is the extent to which the political and labor market institutions depicted in the varieties-of-capitalism literature as "intervening" between globalization and

While contributors to the varieties-of-capitalism literature, in the course of their disagreements, have been highlighting the political intermediation of market-driven inequality, other political scientists have begun integrating political considerations into the depoliticized growth models their literature had inherited from "pure" economic theory. That theory's emphasis on the positive growth effects of intra-industry trade and the geographic clustering of economic activity is a case in point (Fujita, Krugman, and Venables 1999; Krugman 1992). Political scientists do not dispute that geographically concentrated economic hubs emerge through a self-reinforcing process of agglomeration, just as Krugman and other new trade theorists have argued. What they dispute are the forces underlying that process—the factors that make it self-reinforcing.

Not surprisingly, economists have tended to emphasize economic forces: as countries acquire profitable world-class industries, the firms in those industries generate positive spillovers for the rest of the economy. Those externalities then act as natural inducements for further investment, out of which flow new externalities (or more of the old ones) and the process snowballs. This is the economic explanation, and it is undoubtedly correct—as far as it goes. But there is also a political dimension to new trade theory's agglomeration dynamic, and the force it exerts may be even stronger. Yes the snowballs keep getting bigger. But perhaps that is because they are rolling down a hill that has been tilted—for political as much as for economic reasons—in their direction.

Although the term agglomeration may be new, the process it describes is a long-standing and familiar theme in international relations theory. In the global race for whatever it is countries are seeking—power, security, wealth, their vision of global justice—rich countries enjoy a head start. Fair or not, it is an advantage they hope to maintain and, if possible, increase. How? By trying as best they can to convert that advantage, their initial economic superiority, into lasting political influence. Once successful and in command of the global agenda, the big powers then throw up additional barriers to entry for poorer countries eager to join them at the table.

This casts agglomeration in a rather different, more political light. It also clarifies aspects of the process difficult to reconcile with economic accounts. When developing countries enter international markets, for example, they do so under terms established, in most cases, by multilateral agreements and, increasingly, regional and bilateral accords as well. These are highly elaborate deals typically involving one or more of the already-developed countries.[10] And as one might expect—especially given the enormous go-it-alone power of the

long-run economic fundamentals may in fact be endogenous to globalization. I return to this in the fourth section.

[10] Whether the recent rejection of the Doha Round will alter this dynamic remains to be seen. A likely outcome is slower liberalization on the multilateral front combined with faster liberalization on the preferential/regional front—or fronts given that the number and variety of regional agreements have continued to increase.

North (Gruber 2000, 2005)—the Southern governments that participate in these arrangements often end up with the short ends of the stick, agreeing to stop their countries' production of inexpensive generic drugs, for instance, or to scale back their agricultural subsidies long before their economies have reached the levels of development that the already-developed countries had attained when they began scaling back on their own subsidies (see e.g., Chang 2002; Shadlen 2007). The result is what Robert Wade (2003b) has described as a "shrinking of development space" in the South—and, he might have added, its further expansion in the North. And so the poor countries get poorer while the rich, having kicked away the ladder, get richer.

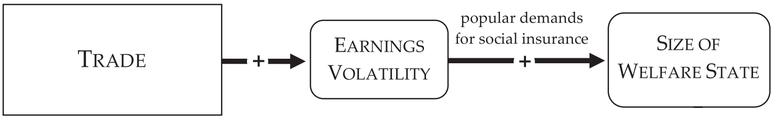
Mushtaq Khan (2001, 2006) has made a similar point about corruption. Convinced that institutions matter for development, and governance institutions in particular, Northern aid agencies and donors have made a practice of attaching anti-corruption strings to the foreign aid they offer developing countries. Lacking the power to say no, recipient governments accept the aid, strings and all, and take up the work of rooting out corrupt officials and reducing opportunities for rent-seeking. But while it would be hard to make a case against good governance, the anti-corruption campaigns waged by recipient governments at the behest of the North (most recently in Afghanistan) end up weakening the structures of public authority that previously existed in their countries—as, for the campaigns to work, they must. As that authority weakens, however, so too do the recipient countries' long-term prospects for development, for if Khan is correct (cf. Huntington 1968; Kohli 2004; Wade 2003b), a strong state is essential for the capitalist transformation on which long-run growth ultimately depends. By this logic the North, in using its power to impose good governance on the South, is throwing the development baby out with the anti-corruption bathwater.[11]

Globalization and the Politics of Volatility

When we turn from this first political science debate to the second, things continue to heat up. For here, once again, the long-term impact of globalization is the main source of controversy, and its impact on the poor above all.

[11] Not that corruption is always consistent with high growth, of course. In some ways, the most interesting question here is the one that Khan does not address: why do corrupt government officials in some countries (see: China) have a track record of channeling economic resources to productive uses, a cut of the profits from which they then take for themselves, while corrupt officials in other countries (see: Nigeria) consistently pick losers, showering resources upon their economies' weakest performers? Is the prospect of a bigger pie more attractive to some "stationary bandits" than to others (Olson 1993)? Or is it equally attractive to all corrupt leaders, the distinction being that some—and only some—operate within a party structure organizationally sophisticated enough to prevent lower-level functionaries from free riding on the bandit party's long-term collective good?

Figure 5.
Globalization and Welfare II: The View from Political Science



Of particular interest in this controversy is how (and how much) globalization may be influencing the willingness and capacity of states to cushion their citizens from the effects of market volatility. All markets are volatile—creative destruction and all that—but highly specialized markets, the types of markets implied by Ricardo's comparative advantage or Krugman's new trade theory, are especially prone to volatility. And just as globalization is economically disruptive, it is also politically disruptive, with those who fare poorly in the international marketplace pressing their demands on governments that have, in their view, lost touch.

This, then, is why political institutions—not just economic institutions—are such an important part of the globalization story. It is through well-established institutional procedures that these conflicts normally get resolved.[12] Given that winning and losing is (mostly) what politics is about, it is only natural that political scientists would want to inquire into the outcomes of these new, globalization-fueled contests. And with few exceptions, variation in the political support for, and ultimately the supply of, state-funded social insurance is the particular outcome variable on which their attention has focused (see Figure 5).

The first link of the causal chain depicted in Figure 5 associates trade with volatility: whatever its impact on growth or inequality, openness is presumed to increase the instability of household earnings. The second link examines how that instability in turn generates widespread politically expressed demands for social insurance, culminating—although the process has yet to reach anything that might be called a stable equilibrium—in a marked expansion of government activity. Far from disempowering or destroying the state (cf. Kapstein 1994; Strange 1996), globalization just makes people want the state—and, particularly when it comes to volatility-mitigating social insurance, demand state intervention—even more.

[12] The exceptions are not unimportant, but they are rare. Mob violence is an example, as is tribal warfare (although neither is as rare an exception as one might like). These seemingly spontaneous outbreaks do arise sometimes, and when they do, things certainly appear anarchic at the time. Although the societies in which they occur may be stateless in a formal sense, however, closer inspection after the fact typically reveals not random acts, but patterns of behavior—patterns that have been shaped, if not scripted, by preexisting rules and structures of authority. Terrorism is another exception that proves the rule: its practitioners are insane at one level, but following institutionally mandated scripts at another (Krueger 2007; Pape 2005; cf. Scott 1987).

This simple version of the argument is highly contested, however, and the two plus signs in the diagram might more accurately be represented by question marks. To begin at the left of the figure, some political scientists have questioned the degree to which trade is responsible for the instability we observe in many countries' recent growth trajectories. For advanced countries, one could make a plausible case—one forcefully argued by Iversen and Cusack (2000), among others—that international markets, being more diversified, are actually less volatile than domestic markets.

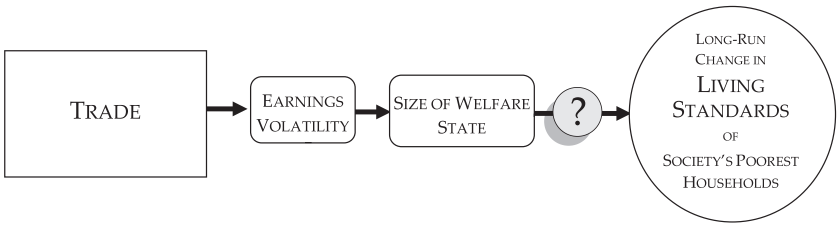
A second point of controversy among political scientists is the extent to which the political demands fueled by rising volatility, whatever their source, have actually been influencing the size of government. Even if the demands are strong, government officials who exercise power may not be beholden to the particular coalitions of voters making them. Their constituents may come from those segments of the electorate whose earnings have *not* been growing more volatile (or who can afford to insure themselves privately). Or perhaps these officials do have an interest in responding to popular demands for an increase in welfare outlays, but their hands have been tied by budgetary realities and the need to satisfy foreign investors, and there is nothing they can do. Not surprisingly, then, political scientists have begun investigating whether "welfare effort" has in fact been rising or, to the contrary, whether the welfare state as we used to know it is no more. It is one thing for the poor to fight back against the volatility and disruption they believe trade has been injecting into their lives; it is quite another for the poor to win.[13]

Third, inasmuch as the diagram has it right and the size of the welfare state has been growing in response to rising earnings volatility, where are all those "popular" demands for Big Government coming from—the poor or the rich? Looking at the politics, one might think the welfare state's biggest champions would be parties of the left; these are the parties to which the working classes and poor have historically thrown their support, after all. On closer inspection, however, things are more complicated. Although left-wing parties have traditionally been supportive of welfare spending, this stance may have reflected their interest in currying favor with higher-earning segments of the electorate, without whose votes no left party stands any realistic chance of being elected, let alone returned to office (Przeworski and Sprague 1986).[14] As further evidence that the haves—not the have-nots—are the welfare state's true constituency, one

[13] On one side of this debate are scholars—typically, although not exclusively, political scientists (Kapstein 1994; Strange 1996; cf. Rodrik 1997)—who predict an imminent, globalization-induced withering of the welfare state. Arguing against this view are those from the comparative-politics branch of the discipline, many of whom cast a skeptical eye on the race-to-the-bottom logic that led Rodrik to question whether globalization had "gone too far" (Burgoon 2001; Garrett 1998; Hall and Soskice 2001; Iversen and Cusack 2000; Rudra 2002).

[14] The point here, although simple, directs attention to a larger problem in political research: the preferences of political actors often diverge quite substantially from the preferences of the societal actors they claim to represent (Frieden 1999; Garrett and Lange 1995; cf. Olson 1971 [1965]).

Figure 6.
For Richer or for Poorer?
The Ambiguous Welfare Consequences of Public Spending on Social Insurance



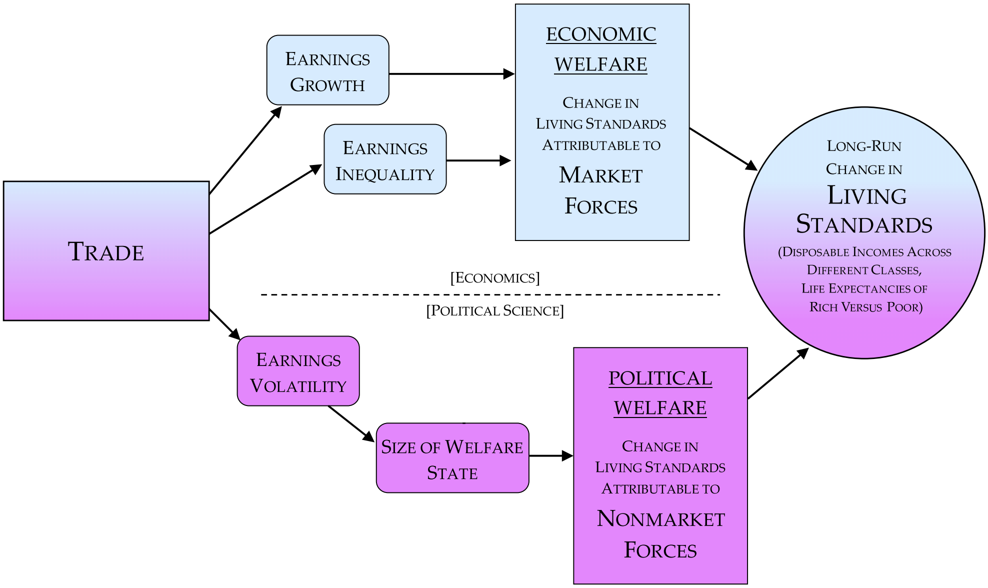
could point to the strong association between welfare spending and Christian Democratic government (Swank 2003; Wilensky 1981). And although she served more for than a decade as Britain's Tory Prime Minister and was, it seems safe to say, no great champion of the downtrodden, even Margaret Thatcher found it politically expedient to leave the country's National Health Service intact (Pierson 1994). Although the welfare state's empirical association with Conservative and Christian Democratic rule may run against the conventional notions of partisan ideology, the work of Michael Wallerstein (see e.g., Moene and Wallerstein 2001) highlights the correlation's underlying economic rationale: earnings volatility may affect everyone, but the middle classes are the ones most eager to protect themselves against the associated risk. Not only do they have a greater ability to pay for this protection (in the form of higher taxes), but because their average earnings are higher, the downside risks for these individuals and families are higher as well.

Finally, looking at those welfare programs that do find support in the electorate, from whatever corner, one can ask how well they have actually done at insuring citizens against the risk of exogenous shocks to their flow of earnings (see Figure 6). So far, this last question mark has been the near-exclusive preserve of economists and sociologists rather than political scientists (see e.g., Heckman and Smith 1998; Mayer 1997; van Ginneken 2003). But given how directly it bears on any overall assessment of trade's long-term utility consequences—the overarching question mark—there seems little reason for this.

Putting It All Together

So as with the economic debate on globalization, the debate among political scientists presents a number of open questions. But even if—or, more optimistically, when—each discipline's questions are answered, an answer to the "ultimate" question could still elude both disciplines. For any comprehensive

Figure 7.
Globalization and Welfare III: An "Additive" Political Economy Model



assessment of trade's long-run welfare consequences will require just that: a more comprehensive, that is to say integrated, theoretical framework.

Both Sides Now: An Integrated Framework

By pulling together the three causal pathways I have just discussed, the simple framework depicted in Figure 7 makes a small but, I think, important step in this direction. Strictly speaking, what the diagram displays is not an integrated model so much as an additive one, with political scientists appending a new branch to a tree firmly rooted in economics. For now, though, rather than disparage the model, I want to emphasize what an important conceptual leap it represents.

Good empirical research takes time in the best of circumstances, and the circumstances in this case—the data—are not always of the highest quality. Sometimes the relevant data do not exist at all, or, when they do exist, the data sets we employ to investigate globalization's impact are too small to allow for statistically meaningful generalizations. As serious as these problems may be, however, they are all on the empirical side. And yet there have also been stumbling blocks—equally, if not more, serious—on the theoretical end. Of these, the bifurcated nature of previous theoretical research is the one I suggest we should be most concerned about. Economists have tended to approach trade policy in one way and political scientists, as we have seen, in quite another. This may be natural, but the instinct should be resisted. For it is only by conjoining the approaches of the two disciplines—by putting them together—that we are likely to get an unbiased estimate of an open trade regime's long-term future.

This is why Figure 7's additive model depicts living standards as only partly deriving from market transactions; nonmarket transactions, as when citizens offer their votes to politicians in exchange (they hope) for policies that will benefit them, also matter. While political scientists can be criticized for being excessively preoccupied with these nonmarket transactions, and with the vicissitudes of the welfare state in particular, economists are equally preoccupied, and just as excessively, with the market side of the equation.

Again, all of this is understandable. But to understand these biases is not to endorse them. As a general practice, the division of labor between political scientists and economists has been extraordinarily productive. Still, this may be the one case where a little *less* division would result in even larger mutual gains.

In fact, trade's impact on a poor family's market earnings and the impact on its nonmarket disposable income need not be same, or even in the same direction. If the dismantling of trade barriers dramatically increases a family's earnings, the end result could still be a decline in that family's standard of living, or even "just" in the amount of income each of its family members ultimately gets to enjoy. For our hypothetical family, if it is poor, is likely to derive a large fraction of its disposable income—and potentially all of it—from nonmarket sources: government transfers, unemployment insurance, social security payments, and the like. And while the market may be booming, the nonmarket (i.e., government) may be struggling—with large budget deficits, for example. Or elected officials may simply be feeling ill disposed toward the poor and thus disinclined to send additional material resources their way.

In theory, then, globalization could be lifting the market incomes of a society's lowest-earning individuals and families at the same time that, by encouraging the election of political parties opposed to welfare spending, it was depressing the nonmarket social insurance benefits flowing to them. Would we regard globalization as a net gain for the poor in this case, or as just the opposite—a net loss?

Good question. And here's another: what if we were to reverse the situation so that our poor households, including those receiving no market earnings at all, were now on the receiving end of an *increase* in government transfers, all thanks to a globalization-fueled groundswell of support for populist candidates and the rise to power of a welfare-state-friendly government? Before giving globalization credit for enriching the poor, surely we would first want to inquire into globalization's market consequences. If the poor were earning less and their vulnerabilities to unemployment or other earnings shocks were increasing, the fact that government officials had been showering more welfare benefits on them may not make them feel, or live, any better.

Factor the distribution of earnings into the equation and it is easy to see how the same sort of trade-off could occur in an economy that was performing well, not poorly, in aggregate terms. An activist and progressive left-wing government, a buoyant macroeconomy, rising average (and per capita) earnings—what's not to like? Maybe nothing. But again, an increase in trade could provide all of this and yet, owing to its distributional consequences, still not benefit the poor.

Spanning the Disciplinary Divide: Where to Draw the Line?

But what about the *interaction* between those economic fundamentals and the "political fundamentals" emphasized, naturally enough, by political scientists? While Figure 7's additive model represents progress, it is also incomplete—and not just in the obvious-but-trivial sense that all models are incomplete: it is missing some pretty major causal arrows.

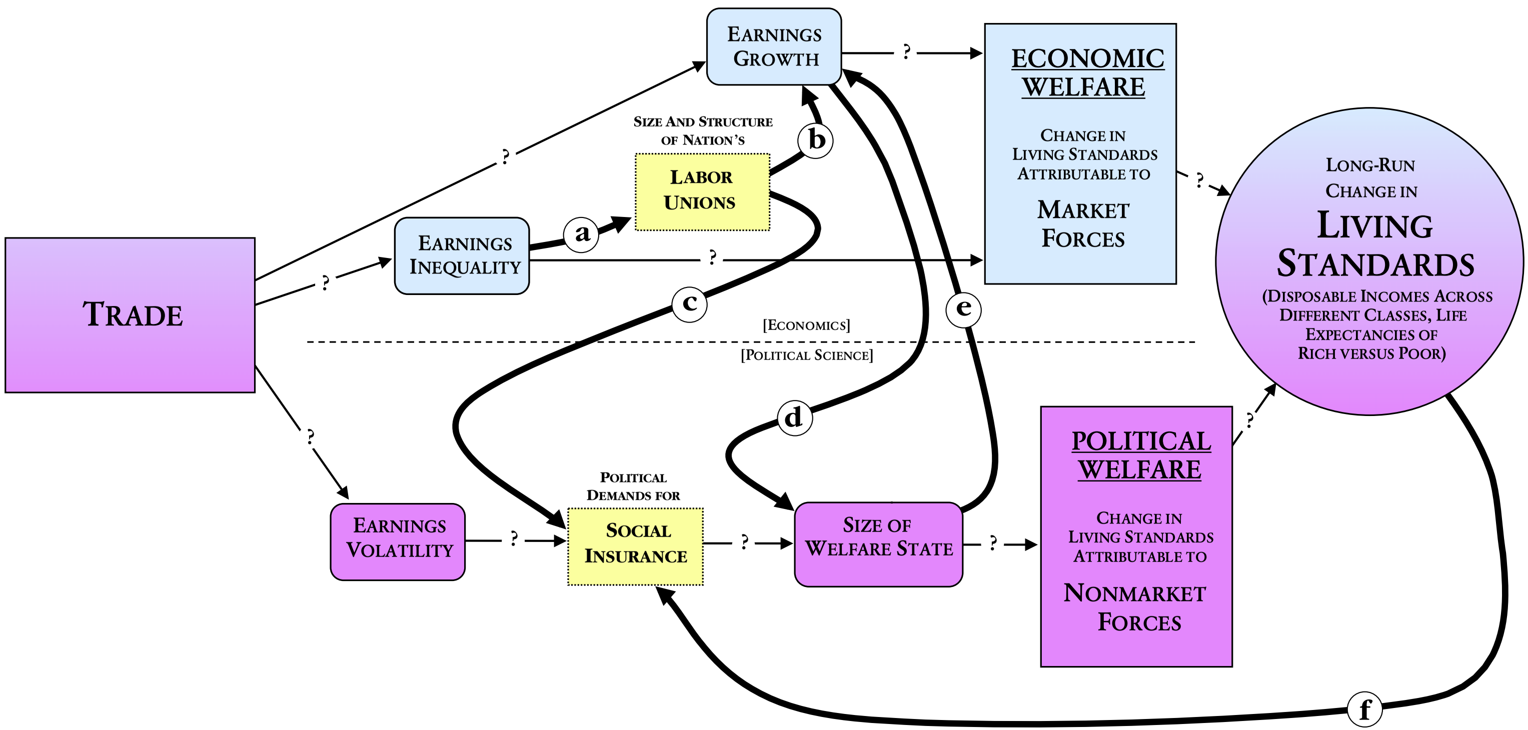
Arrows are not models, of course, but, particularly if they help clarify lines of debate, they can be a useful point of departure. In that sense, the problem with Figure 7 is not that there are too many arrows, but that the arrows, although numerous, never extend out across the economics/politics divide. So how to proceed?

As it happens, there are *many* ways to proceed—the literatures in economics and political science provide a veritable quiver's worth of discipline-spanning arrows. A few of these have already been mentioned. Recall the third section's examination of the ways in which go-it-alone power, a political variable, can "intermediate" the economic relationship between globalization and growth by artificially inflating the North's first-mover advantage over the South. That is just one example, however, and there are plenty more interdisciplinary arrows shooting through—and between—the economics and political science literatures.

In fact, there are too many. A quick glance at Figure 8, which displays a few more, makes the point: once the analyst starts adding causal pathways, it is hard to stop. And yet each additional pathway reduces the original model's theoretical parsimony to the point that, strewn with arrows, the model becomes useless.

Each of Figure 8's new arrows flies—er, flows—out of an established body of literature. Starting at the left, an open international economy increases the credibility of capital's exit option that, in addition to weakening the bargaining power of individual workers, also undermines the organizational cohesion of labor unions. That is arrow a. In turn, this globalization-induced weakening of organized labor has the effect of eroding the "corporatist" wage bargains upon which, according to another body of literature, an economy's long-term (noninflationary) growth depends. And as unions weaken, so too does the labor movement's capacity to mobilize voters to turn up at the polls and support pro-welfare parties on the left of the political spectrum. Those are arrows b and c, respectively.

Figure 8.
Arrow Problems: Globalization and Welfare IV, V, VI, . . .



Staying on the topic of welfare, it is far easier to finance social transfers when a society's economy, and hence its tax base, is growing than when its economy is performing poorly (arrow d). Moving now from the political branch back to the growth branch, some analysts worry that the high taxes needed to sustain large, publicly funded social insurance schemes reduce work incentives and, for that reason, slow economic growth (arrow e). And if the living standards of the poor begin to decline, whether because their society's aggregate growth is declining or just their own, it is a safe bet that sooner or later their demands for social insurance will increase (arrow f).

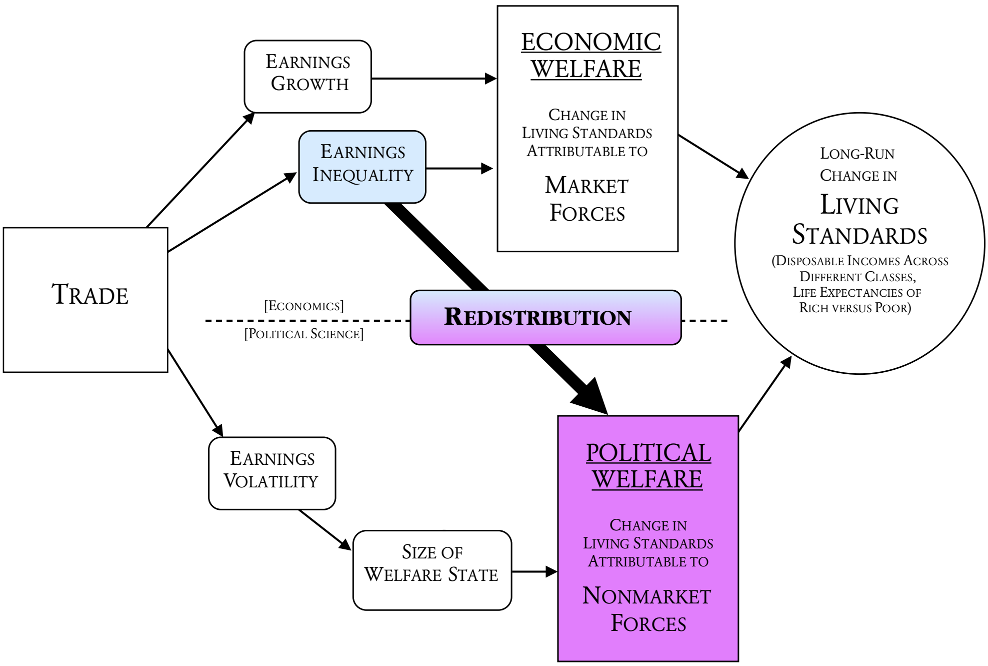
What we have here is not an Arrow Problem but an *Arrows Problem*: ideally we could integrate the upper (economic) pathways with the lower (political science) pathways, but it is hard to know where to start—or, more importantly, where to end.

My own view is that we can make greatest headway by focusing our attention on a discipline-spanning arrow missing from all my earlier diagrams, even the hyperendogenized one I have just been discussing. The most natural way to draw a line between the two disciplinary perspectives is, it seems to me, through redistribution. Not only does redistributed wealth make up a large share of the disposable incomes of poor families in many societies, but those societies' redistribution levels at any point in time are themselves likely to be an outgrowth, an endogenous byproduct, of the degree of earnings inequality that existed at an earlier point (see Figure 9).

In failing to integrate this simple insight into their larger trade models, political scientists are perhaps more culpable than economists, for whom redistribution's intrinsically coercive nature removes it from their modeling sweet spot. More surprising is the variable's neglect by students of politics, an oversight made all the more glaring (if oversights can blind) when one considers the potential importance of redistributive taxation, spending, and regulatory policies as a counterweight to market-driven inequality. That globalization seems likely to inflate existing disparities in the earnings of different members of society need not be a problem for those on the wrong end of the divide if public officials rise to the redistributive challenge, transferring resources away from the poor's higher-earning fellow citizens. The question, of course, is *will* governments step in—and, if not, why not? Might trade itself, and trade-induced inequality in particular, be responsible for undermining redistribution's appeal as a political choice? It is time to begin answering these questions.

I say *begin* because, as noted in the third section's survey of political science's theoretical progression, these are not the questions on which students of politics have traditionally focused their energy. Instead, the relationships depicted in the lower branch (and discussed with reference to Figures 5 and 6) are what have commanded their attention: Iversen and Cusack's (2000) penetrating work on trade-induced earnings volatility, for example, or Moene and Wallerstein's (2001) game-theoretic studies of welfare policy.

Figure 9.
Globalization and Redistribution: The Missing Link?



But perhaps this draws too stark a contrast. Is it really fair to depict the study of redistribution as uncharted territory? Even if redistribution, as distinct from "welfare," has yet to emerge as a central focus of the globalization literature—the place where I am suggesting it belongs—it is not as if redistribution is an entirely unknown quantity. Although a bit player in the wider literature, it has captured starring roles in the works of a small but justifiably distinguished group of scholars, tilted perhaps toward political scientists (see e.g., Bartels 2008; Boix 2003; Kaufman 2009; Mahler and Jesuit 2004; Moene and Wallerstein 2001; Pontusson 2005; Steinmo 1996; Swank 1998) but including a number of influential economists, sociologists, and legal scholars as well (see e.g., Acemoglu and Robinson 2005; Alesina and Glaeser 2004; Atkinson 2004; Cafaggi and Pistor 2012; Kenworthy 2004; Milanovic 2000; Robinson 2010).

The problem with these redistribution studies—and the reason "bit player" is still, in my view, an apt description of the variable's standing within the wider globalization debate—is not that the *rest* of the literature neglects redistribution. The problem is that the redistribution literature *itself* neglects redistribution: the variable on which its contributors have been shining their analytical spotlight bears only a passing resemblance to the real thing. If we are going to focus on redistribution (as we should), we need to do a better job of measuring it. Operationalizing redistribution may seem a simple matter. But as I point out below, the operationalizations currently in use are too restrictive to support the empirical project one would hope to see developing around this most important branch of globalization research. Here, then, is an *empirical* gap that warrants greater exposure. And like the theoretical gaps on which I have been focusing to this point, it too needs filling.

Wanted: A Better Measure of Redistribution[15]

While economists continue to debate trade's impact on inequality, the prediction that a globalized world will be characterized (at least in its Northern half) by a sharp and enduring divergence between the earnings of skilled and unskilled workers is not particularly controversial. What is controversial—and yet, from the standpoint of assessing globalization's long-run desirability, absolutely critical—is the long-run political impact of these inegalitarian market dynamics. It is one thing for governments to redress an internal earnings imbalance, as to varying degrees they have done effectively, when these imbalances are low. But as globalization proceeds and the domestic earnings disparities grow, it requires a leap of faith to believe that each country's *ex ante* political balance will remain unaltered. If rising inequality empowers the disadvantaged, we should expect to see more domestic redistribution—perhaps a great deal more, to the point that rising redistribution, rather than merely preserving the *status quo* distribution of income, ends up retilting the economic balance in the poor's favor. Alternatively, inequality could *dis*empower a society's lower-income households, with domestic redistribution, as it falls, adding political insult to the poor's market injury.

So which scenario will it be? To answer with confidence, we first need to develop better tools for measuring redistribution. Take public spending, for example. A state could increase dramatically in size and its government in assertiveness without the result being anything we would want to describe as an increase in redistribution. Much would depend on how that government put its resources to use and, just as importantly, how—or, more precisely, from whom—it collected those resources.

It would be hard to imagine anyone disagreeing with either of these simple propositions. Yet for many years, through the 1990s in fact, the quantitative welfare state literature took total government expenditure—minus military spending—as its baseline indicator of redistribution (Cameron 1978; Garrett 1998; Hibbs 1987; Lindert 2004; Rodrik 1997). More recent years have seen a shift, exemplified by Iversen and Cusack (2000), Burgoon (2001), and Huber and Stephens (2001), toward more refined operationalizations that isolate expenditures specifically geared toward "social" purposes. These new data,

[15] Gruber (2008) provides a more in-depth discussion.

taken from the Organisation for Economic Co-operation and Development (OECD)'s Social Expenditures Database, revealed a dramatic increase in social spending over the course of the early 1990s, a period when (among other things) the U.S. unemployment rate shot up, triggering a correspondingly large increase in public unemployment compensation outlays.

This new effort to look more microscopically at particular aspects of public spending—at spending on state-financed health care, say, or on public pensions—is clearly a good move if social insurance is the dependent variable of interest. If that variable is redistribution, however, things are less clear. For government monies, even when earmarked for social purposes, may not always be allocated progressively. One need not invoke images of Ronald Reagan's "welfare queen" to see the potential for abuse. But abuse, if that is the right term for it, is virtually built into the system when it comes to social security. Rewarding affluent pensioners might be a good idea for political reasons; universalistic programs tend to have greater staying power than targeted ones that means-test their beneficiaries. But schemes of this kind do not redistribute to the poor. That is not their purpose.

The job of the welfare state is to protect its citizens from the undesirable consequences of earnings volatility. The income shocks that arise when people unexpectedly lose their jobs or, less unexpectedly, retire from the labor force, have the potential to cause enormous human suffering. By mitigating the financial burdens associated with these temporary shocks, the welfare state eases this particular type of suffering. But it is neither the only nor, I think upon reflection most analysts would agree, the most important source of financial pain that individuals and families can encounter. People also suffer when their earnings are stable but *too low* (at least as they themselves see the situation). The welfare state was not designed to handle this second type of suffering, which is longer in duration and, indeed, a permanent fixture of any less-than-perfectly-equal society. That is the job—the core function—of redistribution, to mitigate "structural" earnings inequality by taking from the rich and giving to the poor.

And what about the other side of the welfare spending equation—taxes? If social insurance programs are financed primarily by the middle classes and the poor, it is hard to see how they could be generating a lot of redistribution or (for that reason) mitigating a lot of inequality. To be fair, some contributors to the literature do analyze taxation variables (see e.g., Garrett and Lange 1991; Steinmo 1996; Swank 1998; Swank and Steinmo 2002), but just as the distributional impact of government spending is a function of the way it is targeted, the distributional impact of tax policy depends on the way *it* is targeted—on who pays what—and tax incidence is much harder to measure than, say, total tax revenues as a share of GDP.

Here, the state of the art exploits the detailed household-level data of the Luxembourg Income Study (LIS) to derive a measure of what Branko Milanovic of the World Bank terms the "share gain" (Milanovic 2000; see also

Kenworthy 2004; Kenworthy and Pontusson 2005; Mahler and Jesuit 2004). This is a true measure of redistribution and, better still, the statistic is fairly easy to compute.[16] To obtain the measures for the United States, for example, one could compare the earnings of the country's bottom-quartile household with those of its 90th-percentile household, then compute a similar ratio for disposable incomes, and, finally, compare these two ratios. In this way, the procedure makes it possible to isolate the difference between the poor's (typically very low) relative earnings with the (typically higher) relative income they enjoy after governments have entered the scene, offering transfers, providing tax rebates, and employing all the other nonregulatory instruments of government policy that affect the bottom line. The greater this difference, the more "redistributive" a country's tax and transfer system can plausibly be said to be.

Milanovic's (2005) new indicator of redistribution is a welcome addition. It incorporates information on taxes, for one thing, which immediately distinguishes the statistic from the spending-side measures that have dominated the literature. And because the tax data incorporated into the share gains statistic are taken at the level of the household, Milanovic and his followers can begin to talk about tax incidence—and progressivity more generally—in a way that previous analysts such as Swank and Steinmo (2002) could not. But before we get too carried away with taxes, it is important to remember that the share gains statistic is still, at least in part, a measure of government spending, and therein lies its major weakness.

Or weaknesses, for the deficiencies in the statistic can be divided into two types: mechanical and conceptual. Mechanically, there is the problem that the particular form of spending incorporated into Milanovic's (2005) measure is transfer spending, that is, payments sent by the government directly to qualifying households. Transfer spending has peculiar properties, however, that reduce its attractiveness as a proxy for progressivity, if not for redistribution more broadly. During recessions, for instance, they act as automatic stabilizers. Yet the fact that governments send out more unemployment compensation checks (say) during these periods does not mean they have become more progressive. By the same token, social transfers can also rise and fall because of exogenous changes in the number of dependent children and retirees a society

[16] Maybe too easy. A number of the works that make use of the concept and the LIS data fail to distinguish between net and gross household income. Restricting one's sample to countries and years/waves for which gross figures are available is essential, although it severely limits the total number of usable observations. Existing studies also suffer from some other methodological pitfalls (see especially Atkinson and Brandolini 2009). For example, some analysts—including Milanovic himself—fail to equivalize households, or—again like Milanovic—they examine the same households across the pre- and post-intervention distributions, or they omit sampling weights. One can also question the desirability of including households whose initial (pretax and pretransfer) earnings are zero or close to zero in the sample, although one can *also* question whether the bias entailed in omitting these households would be warranted.

has at any given time. To take the second case, the populations of many European countries are aging rapidly. This means that, over time, more and more European households will be collecting their public pensions. Surely, though, one would not want to describe the resulting increase in transfer payments as an increase in the progressivity of European states. There is, unfortunately, no obvious solution to these sorts of mechanical problems, short of scrapping the measure entirely.[17]

As for the measure's deeper, nonmechanical problems, one is the risk—analogous to omitted variable bias—that in focusing on household transfers, the analyst is discounting the progressive (or regressive) impact of all of the *other* spending governments do. Much of that nonhousehold-specific public expenditure—on universal health care and public schooling, say—is "expended" precisely for the benefit of poor families whose low earnings would otherwise put them out of reach. If household transfers were falling but state monies earmarked for universalistically provided public goods of this kind were increasing dramatically (cf. Kaus 1992), would we really want to say that redistribution was withering?

So what is the solution? Inasmuch as the share gains statistic leaves out government spending on public goods, one might think the answer would be to fuse the household-level transfer data with data on more universalistic forms of public expenditure. But, even if we could figure out a way to meld the two types of data without inciting new methodological controversies, this mother-of-all-indicators approach would still be unsatisfactory. At some point, the analyst would have to decide (on what basis?) which forms of universalistic spending passed the test for inclusion into the new redistribution index and which did not. Should public subsidies for public transportation count as "social" spending? Should public employment? There is also the matter of quantification. Knowing that government spending on a particular service is progressive would not be sufficient; we would also need to assess the degree of progressivity. How progressive is universal health care? How progressive is state-financed primary education? These questions are as thorny as they are inescapable.

Before concluding this discussion, it is worth mentioning one additional conceptual problem with spending measures, the deepest and thorniest of them all. My focus thus far has been on quantification: I have been following the (public) money. The progressivity of government is not only a function of money, however; spending money on transfers and services aimed at poor

[17] Analysts have wrestled with various possibilities, including—as noted previously—omitting pensioners from the sample altogether (see e.g., Kenworthy and Pontusson 2005). Another approach would be to include a comprehensive set of unemployment and demographic control variables in the relevant regressions (see e.g., Iversen and Cusack 2000). As a "quick fix" this is sensible, but it does raise questions concerning what precisely the share gains variable is measuring.

families is important, but the amount of funding may be less important than how well or, as is all too often the case, how inefficiently it is allocated. Any number of examples could be cited here. The quality of public education is only partly determined by the amount of money a district spends on its teachers and buildings; how schools are organized matters at least as much (Chubb and Moe 1990). Or take the current health-care debate in the United States where, again, how institutions are structured is the main focus and the amount of funding a secondary issue. Even when it comes to household transfers, a case can be made that the larger the amounts of money involved—the bigger the checks a government sends out to poor individuals and families—the less, not more, progressive is a country's redistributive system. Large transfers (according to one version of this argument) trap the poor in situations of dependency, reducing their incentives to retrain and increase their long-run earnings power.

In short, the redistribution literature's new share gains and social spending indicators may be more refined than the earlier (and still widely employed) size-of-the-state indicator, but *all* these indicators anchored in measures of public spending ignore the "new institutionalist" revolution that has been sweeping through the social sciences for well over a quarter of a century (Acemoglu and Robinson 2012; March and Olsen 1990; Moe 1984). While contributors to the redistribution literature have been following the money, their colleagues have been thinking about the institutions through which that money is channeled. In fact, much of what government does—with consequences ranging from the highly progressive to the highly regressive—involves no monetary outlays at all. Passing minimum wage laws and protecting a worker's right to organize are examples that fall on the pretax side, but the organization of a country's tax system is where the real action is. If any money is to be followed, it should be money paid *into* the system, not out of it.

When operationalizing redistribution, then, tax policy should be given special priority. Social insurance schemes may go a long way toward insulating the poor (and the rich too) from the disposable-income effects of unexpected earnings shocks, but the welfare state is not necessarily a *progressive* state. If shrinking the gap is what counts, it is a measure of progressivity we need—and of tax progressivity in particular. Tax systems can be extraordinarily powerful engines of progressivity even when "social spending" is but a trickle. And vice versa: if social spending is high, but the tax revenues used to pay for that spending come disproportionately from the middle classes and the poor, it is as if the system is giving with one hand and taking away with the other.

Tax-based progressivity measures have a number of other virtues as well. First, they sidestep many of the complexities that arise when one tries to measure progressivity in terms of the amount, or even the allocation, of public spending. By comparison, tax progressivity is much simpler. If a government starts taking more from the rich and less from the poor, it is moving policy in a progressive direction even if its spending amounts—or the ways it organizes that spending—remain as they were.

Why focus on personal income taxes rather than corporate, estate, or consumption taxes? Most countries have already reduced their corporate taxes to the bare minimum, and in a globalized economy it is not hard to see why (Rodrik 1997), nor would it be realistic to expect this to change. Regarding estate or inheritance taxes, these are also low, although for a different reason: they are politically unattractive. But even if they do rise in the future—as they have recently done in France—the resulting contribution to progressivity is unlikely to be very great. Individuals with large estates can afford expensive lawyers skilled in the art of tax avoidance, and it would be naïve to think that revenues would come flooding in rather than be diverted into trust funds and myriad other tax loopholes.[18] That leaves consumption taxes, which, given their prevalence in developing countries, would constitute a serious omission from any study of redistribution that purported to be comprehensive. In most industrialized countries, however, the bulk of tax revenues continue to be collected through personal income taxes (Acosta-Ormaechea and Yoo 2012).

When assessing the progressivity of income taxes, or indeed any taxes, we should be careful not to focus exclusively on the sums paid by very high earners. By calling attention to the low personal income tax burdens of the very rich, the economists Thomas Piketty and Emmanuel Saez have done the literature—and perhaps the world—a great service (Piketty and Saez 2007; see also Atkinson, Piketty, and Saez 2011). But the low rates applied to economic elites could simply reflect low government spending, with lower-earning households paying marginal tax rates only slightly below those owed by the top 1 percent. When it comes to tax progressivity, the real question is how the tax treatment of higher- and lower-earning households *compare*, a progressive tax code being one that forces higher-earning households to pay higher effective marginal rates than lower-earning (or zero-earning) households.

Fortunately, a little creativity may be all that is needed to remedy these essentially technical issues. And as more refined measures of redistribution are developed and the data needed to operationalize them improve, future contributors to the globalization debate may finally be in a position to say with confidence that governments confronted with large and rising wage gaps are likely at *some* point to take remedial action to prevent those gaps from translating into a more serious living standards gap. As for precisely what point, the duration of the lag would itself be open to systematic empirical analysis. Of course, these new, more firmly grounded empirical investigations may turn out to reveal something rather different: that contrary to the so-called compensation model (or the *over*compensation model proposed by Meltzer and Richard,

[18] This points up the importance of looking at what taxpayers actually pay—a real possibility with the detailed LIS microdata exploited by Milanovic (2000, 2005) and others—as opposed to changes in legislated tax rates. Progressivity is a function of the former, not the latter.

whose 1981 article still orients much of the research in this area), governments presiding over increasing earnings gaps are the *least* likely to boost their redistributive efforts on behalf of disadvantaged individuals and groups.

Conclusion: Mind the Gaps

This article began by suggesting we use the opportunity presented by the current global downturn to reassess the long-term welfare implications of globalization writ large. An updating of this sort is already under way in global financial relations, where the tide of economic opinion has recently been shifting in favor of greater regulation. When it comes to global *commercial* relations, however, one finds comparatively little sustained enthusiasm for new thinking.

That policy makers have yet to update their views on trade and its ultimate impact on poverty is not surprising; they have bigger, or at least more immediate, fish to fry. But academics, who can afford to take the longer view, have not engaged in any sort of full-scale reassessment either. If economists and political scientists had thought the current crisis called for a fresh reconsideration of trade's poverty-alleviating implications, they would be engaging in that process now. But they don't, and so—with few exceptions—they aren't.

This is letting a good crisis go to waste. For if the arguments developed here are correct, the old ways of thinking about globalization's poverty-reducing potential are just as inadequate, and just as in need of *re*thinking, as the old debates in finance.

When we turn to those old debates, whether in economics or in political science, what we discover—or, rather, fail to discover—are a number of missing pieces. While economists focus on trade's long-run growth effects and its impact on the distribution of earnings, and while political scientists debate trade's long-run impact on the welfare state, the question that too often gets overlooked is how these different causal pathways add up and, even more importantly, how they interact.

In calling attention to these understudied areas of interaction and overlap, my analysis has raised hard questions for stalwart proponents of globalization not only within economics (where "heterodox" skeptics are a small minority) but in political science as well. Along the way, this broader perspective has suggested a new way of conceptualizing the distributional consequences of trade liberalization, and it recommended several new avenues for research.[19] Highlighting the most promising of these, I concluded my analysis with an appeal for new work on the nexus between trade-induced market inequality on

[19] See Gruber (2013) for a further elaboration of some of these suggestions.

one hand, and politically induced, market-correcting redistribution on the other.

Right now would seem to be a particularly good time to be studying this domestic inequality/redistribution interaction, a discipline-spanning pathway that, although something of a sideshow in the literature thus far, bears directly on current policy debates. Even if new financial regulations do spare the world from future bubble-to-meltdown cycles—as one can only hope will be the case—the long-run economic health of any society will still require well-functioning domestic redistributive institutions. Might the market inequities generated by trade and international interdependence end up destroying these institutions and, as they deteriorate, the *domestic* interdependence on which poor families (and ultimately all of us) depend for survival? In closing, I would submit that trade's long-run impact on domestic interdependence is the largest gap, or missing note, in the globalization literature's current research agenda. If political scientists and economists are to fill that gap, their first step must be to *mind* the gap—not just to see it, but to be bothered by it, and so resolve, by working together, to make it disappear.

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