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Attitudes, interests, and the politics of trade

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In 2016, for the first time in at least 75 years, major candidates for the presidential nomination of both political parties ran on platforms that were explicitly hostile to international trade, and international economic engagement more generally. One, of course, won the nomination of his party and became the 45th president. In office, Donald Trump was true to his word, pursuing the most aggressively protectionist policies in modern American history.

This shocked many long-time analysts of American trade policy. We were used to trade politics being conducted in the shadows, involving only special interest groups representing industries and labor unions. Not since debates over NAFTA had trade been the subject of mass politics, and even then both the level of hostility to trade and the popularity of this hostility had been an order of magnitude lower.

Scholars who have studied this backlash against globalization in the United States, and analogous instances of a similar backlash elsewhere, have largely followed two broad strands of analysis. One finds a powerful impact of economic trends on political behavior: regions of the United States hit hard by imports, and that have lost substantial numbers of manufacturing jobs, are more likely to have become more protectionist, to have voted for Trump or Sanders, to have switched from the Democratic to the Republican party. Other scholars have found similar relationships elsewhere, especially in Europe, where analogous economic trends have been associated in particular with hostility to European integration.

The second type of analysis focuses on the level of the individual, not the region, and Diana Mutz is perhaps the most prominent exponent of this approach. Mutz finds little or no support for economic sources of protectionist, anti-globalization, or pro-Trump attitudes. Her view is summarized in the sub-title of her book *Winners and Losers: The Psychology of Foreign Trade*. As she puts it, attitudes toward trade are the result of “largely expressive and symbolic reasons such as high levels of nationalism, racial prejudice, competitiveness, and the general desire to “win” and dominate others” (page 18).

In this essay I attempt to address the apparent contradiction between these two well-developed strains in the literature. I suggest that both findings may well be accurate. On the one hand, most individuals’ views on trade may reflect only the vaguest connection to their own economic interests, and be driven by other less direct considerations. On the other hand, the impact of trade (and related economic trends) on recognizable regions may have a real impact on the attitudes of those in those regions, even if they are not directly affected. Individuals live in communities, towns, and cities, and their attitudes are almost certainly influenced by their surroundings. The research frontier, I believe, is to analyze the relationship between geographical socio-economic and political trends and their expression in the views and behavior of individuals.

Winners and Losers summarized

Winners and Losers is a *tour d'horizon* of Mutz's substantial research on public attitudes toward trade and trade policy. I come neither to bury nor to praise (unreservedly) Mutz's work, as it is largely a jumping-off point for my broader purpose; nonetheless, it is useful to provide a quick summary of the book. She starts with a helpful compendium of American attitudes, both quantitatively and with an illuminating array of open-ended survey responses.

Mutz then turns to one of the more startling aspects of trade-related attitudes, the turnaround in partisan opinion. It has been widely noted that while for decades Republicans had typically been more positive about trade than Democrats, this changed dramatically during and after the 2016 election. Pew data show, for example, that while in 2014 Democrats and Republicans had very similar – and strongly positive – views on free trade agreements, by 2017-2018 there was a massive difference: more than two-thirds of Democrats were positive, while barely one-third of Republicans were.¹ For Mutz the reason for these shifts, and in particular the “actual causes of trade opposition” in 2016, were that “white voters in 2016 were expressing a fear that their status in the world as well as their status within the country was suffering” (page 264). Mutz argues that eventually Republicans turned positive, suggesting that this was

¹ <https://www.pewresearch.org/fact-tank/2017/04/25/support-for-free-trade-agreements-rebounds-modestly-but-wide-partisan-differences-remain/>

because, given their concern for social dominance, with President Trump at the helm they now associated trade with “a competition that we are ‘winning’” (page 69).

Mutz turns next to exploring how Americans think about the costs and benefits of trade. She presents results of a survey experiment done in 2013-2014 in which respondents were given choices among agreements in which gains were distributed differently between the United States and a trading partner. Her results, she argues, suggest that Americans value their relative position with respect to another country more than they value absolute gains. That is, they would be willing to forego gains to America in order to limit gains to another country. Mutz emphasizes that Republicans exhibit this tendency more strongly: “Republicans care about dominating” (page 89).

Views on trade, Mutz continues, vary by trading partner. Americans are much more positive about trade with Europe than they are about trade with China (although many would be surprised to know that a very large majority – 58 percent – thought that trade with China was a good or very good idea). She argues that variation in ethnocentrism, feelings of psychic closeness, and racial or ethnic prejudice, have a strong impact on attitudes toward trade. Canadian respondents, less competitive and less ethnocentric, were less likely to harbor anti-trade attitudes.

Mutz then turns to the electoral connection. She surveys several theoretical approaches that might connect trade to voting behavior, concluding that we do not yet have compelling evidence in favor of any one theory. The 2016 election, then, provides

her with an excellent testing ground. And here she argues that the dominant reason for anti-trade voting was “ingroup status threat.” She notes that “Not long before the 2016 election...white Americans were told that they would soon be the minority race.” In this context, “white male Christians...have the most to lose psychologically if they perceive America and/or men and whites to be no longer dominant” (pages 231-2).

The book concludes with some suggestions about how politicians and others could overcome opposition to trade. Mutz suggests that they focus on trade as cooperation, not competition; and on the similarities we share with those with whom we trade, rather than the differences.

Overall, *Winners and Losers* is a weighty argument for psychological and other non-economic sources of American attitudes toward international trade. It is a worthy representative of one pole in ongoing discussions over American political behavior, and in particular political behavior toward international economic and political integration. Similar debates have been going on about analogous behavior in other advanced industrial countries, prompted by the upsurge in politicians, political parties, and political movements that have – like Donald Trump and Bernie Sanders – expressed hostility toward international and regional economic and political integration. Brexit, populist Euro-skepticism, and related movements all seemed to surge at roughly the same time as the rise of Donald Trump, and this phenomenon remains a topic of lively scholarly investigation.

For the purposes of this essay, what matters is the broad sweep of the argument by Mutz and others: many surveys and survey experiments at the individual level show little or no connection between individual-level economic factors and political attitudes – including toward Donald Trump and toward trade. However, before moving on to consider this, and alternatives, some comments on *Winners and Losers* may also be helpful, inasmuch as they often apply to other work of this type.

Reservations

My principal purpose in this essay is to suggest a way forward that does not require us to reject any of the relevant findings on the politics of trade. However, given the forcefulness with which Mutz rejects findings and views other than her own, it is worth laying out why I think that she falls short of providing a satisfactory, self-contained, explanation of the relationship among socio-cultural, economic, and political factors in the formation of political attitudes and behavior. I leave aside the glaring fact that she completely ignores the candidacy of Bernie Sanders, whose rhetoric in opposition to globalization and trade was virtually indistinguishable from that of Trump, and whose supporters presumably differed in important psycho-social ways from those of Donald Trump.

Mutz is quite insistent about the core arguments of the book. I found it troubling, however, that she did not adequately recognize the enormous quantity of scholarship that might call her argument into question. Here I do not only mean criticisms of her

own work (Morgan 2018, for example), but rather two important and very large bodies of work. The first is made up of scholarship that, like Mutz's, uses individual-level studies – typically surveys and survey experiments – but arrives at different conclusions. There are indeed some classic studies along these lines, and a whole host of more recent ones.² Perhaps just as important, there are a number of rigorous studies that look explicitly at the interaction of economic and psychological (or more broadly socio-cultural) factors. Indeed, a recent (purely theoretical) article by two of the world's leading trade theorists develops a theory of trade policy preferences in which “assessments of wellbeing include both material and psychosocial components” (Grossman and Helpman 2021). Even more relevant, perhaps, are empirical studies that have indeed found important effects from the *interaction* of economic and non-economic considerations. One recent such article, for example, finds evidence that “economic change can shape peoples' values and value change may be an important mechanism

² For the former, see Scheve and Slaughter 2001 and Mayda and Rodrik 2005; for a little sample of the latter see Owen and Johnston 2017, and Choi et al 2021 for an intriguing analysis of the political impact of NAFTA that relies on a raft of measures, including surveys. There is indeed a plethora of survey-based studies whose conclusions are far more nuanced and ambiguous than those of Mutz, for example Rho and Tomz 2017 and Schaffer and Spilker 2019.

linking economic change with opposition to economic integration” (Ballard-Rosa et al. 2021). There are many studies by political scientists and others that use individual-level data, like Mutz, in both the United States and elsewhere, and arrive at very different conclusions about the impact of economic factors on relevant attitudes. It is up to others to adjudicate among these studies, but it is not helpful or appropriate simply to ignore them.

The second broad category of studies that is in need of much more serious consideration – and whose existence Mutz does, in fact, acknowledge – is the truly enormous body of work that explores the impact of regional economic trends on regional political behavior. Perhaps the best-known class of such studies are those that grow out of the “China shock” approach innovated by David Autor and co-authors (Autor et al 2013; on the politics see Autor et al 2020), but there are many others. One recent survey (Walter 2021 – for which see citations) cites more than 150 studies, and there are many, many more. This research finds systematic impacts of economic trends on political behavior. Typically, the work connects increased import competition from low-wage countries to a loss of many of the region’s unskilled and semi-skilled manufacturing jobs, which leads to a turn toward more populist and protectionist behavior. The behavior measured is usually voting, but also includes legislative behavior by representatives of these regions (Feigenbaum and Hall 2015, for example) and survey responses (Choi et al. 2021, for example). Importantly, there are studies

along these lines of the United States and of many other developed societies, suggesting the scope for both broader empirical regularities and theoretical conclusions.

This work cannot be dismissed, for several reasons. First, it is enormous in its volume and in the similarity in results over time, across countries and regions of countries, and using many different methods – all of which, however, are at the level of a geographic region (county, legislative district, commuting zone, etc.). Second, and perhaps more important, many if not most of these studies can make a serious claim for causal identification. This is because – as in the canonical Autor Dorn and Hanson 2013 paper – they typically explore the impact of a plausibly exogenous event like the creation of NAFTA or the entry of China into the WTO upon the political behavior in question.

The strength of the causal claim here is especially important because the survey-based scholarship of Mutz and others *cannot* make similar causal claims. As is well known, survey research effectively measures the correlation of attitudes with other attitudes (nationalism with protectionism, for example) and *cannot* claim to have uncovered any causal relationship. Indeed, one obvious interpretation of these attitudinal correlations is that they are both due to a third causal factor: perhaps economic distress caused by imports causes both nationalism and protectionism. Mutz in fact is not careful on this score; *Winners and Losers* is full of causal claims that cannot be supported. The only cause possible in her research is with survey *experiments*, and

then all that we can say is that a prime or prompt caused respondents to change their responses to a survey researcher. This is a very weak reed to hang an argument on, much weaker than such consequential behavior as voting or legislative action. One of Mutz's reactions is peculiar, when she writes "as with many observational analyses, it is difficult to make strong causal arguments because so many other characteristics also vary among communities" (page 206). Surely the answer is not to rely solely on surveys and survey experiments that cannot make *any* causal arguments, let alone strong ones. It is true that we need to be careful about inferring a mechanism from a demonstrated causal relationship; but it is also true that we may not get much useful information about a possible mechanism from correlations such as those drawn from surveys. I return to this below, because I believe that a constructive and important goal for scholars is in fact trying to figure out – with the help, I hope, of survey researchers – what mechanisms indeed do lie beneath these demonstrated causal relationships.

To be sure, there are plenty of potential objections to this regional literature, even leaving aside any debates about the strength of the causal claims. Mutz quite rightly highlights the principal weakness of these regional studies. Noting that "communities don't vote; people do" (page 206), she writes, "the mechanism producing these aggregate-level observational relationships is either unclear and/or inconsistent with much of what is known about voting behavior" (page 214). When she acknowledges "aggregate-level data showing that...whites concentrated in manufacturing areas, were

more likely to vote for Trump” she quite reasonably tempers this with the observation that “this relationship...is subject to many alternative interpretations”(page 267).

Mutz is exactly right that the gaping hole in our understanding in this realm is about the mechanism. Those who posit an impact of import competition and job loss have trouble explaining how aggregate regional trends affect individual behavior – especially when survey and other individual-level studies tend not to show a strong direct impact of these trends. Scholars who focus on the regional aggregates may be aware that there is not a very strong relationship between individual experiences and individual political attitudes and behavior, but they have no real explanation as to how socio-economic trends at the regional level affect individuals. The same, of course, is true of individual-level studies, which cannot explain why aggregate outcomes are not observed at the level of the individual.

This, I believe, points us toward where our research should go. Rather than engage in another fruitless round of debates, with political economists and survey researchers flinging results and abuse at each other, we should address what is a common problem to all the work in this area. We have powerful evidence that economic events at the regional level have important effects on political behavior at the regional level; and we have powerful evidence that individual attitudes and behavior do not seem to respond directly to these economic events. There is a fundamental, theoretically crucial, aggregation problem, and I think it will require both types of researchers to

address. I now turn to what I hope are some more constructive observations on the problems we face as we go about this task.

The meaning of public attitudes toward trade

First let me make clear that while Mutz's book is almost exclusively on trade, very similar findings prevail with respect to virtually every measure of international economic engagement: finance, investment, immigration, regulation, and beyond. In the United States, trade and globalization seem the most common topics; in Europe it is some combination of immigration and European integration writ large. The objections (and support) expressed are very similar, and while it will certainly be important for scholars to differentiate as necessary, I am going to elide the various component parts for the purposes of generalization. In what follows I make some observations about the state of our knowledge and understanding of public opinion on trade and its implications for the politics of trade more generally.

Importance for policy. Most scholars of trade politics think of policy as largely driven by special interests. The canonical Grossman-Helpman (1994) model has a black-boxed government weighing the demands of concentrated industrial lobbies – both management and labor – against the government's concern for aggregate social welfare. There is no particular treatment of the role of the electorate: somehow the government is expected to internalize society's interests and counter-balance it to the special interest pressures (and campaign contributions) to which it must also attend.

While this highly stylized picture is almost a caricature, it is not too far from the reality of most scholars' views of the trade policymaking process. The mass public has little or no understanding of trade – and with good reason. The impact of trade on most Americans is very limited. Manufacturing and agriculture, the two principal traded-goods activities, account for about 10 percent of employment, and many portions of even these sectors are unaffected by exports or imports. Modern trade theories, in fact, focus on firms rather than industries, for the vast majority of American trade involves a tiny minority of its firms – even in the more trade-oriented and trade-affected industries. Certainly trade has been a boon to consumers, but the standard view among students of trade policy is that the concentrated interests of exporting and import-competing firms matter far more than the diffuse interest of consumers.³

This is almost certainly an accurate depiction of the politics of trade at most times, in most countries. The intricacies of trade policy, especially in a modern international economy dominated by intra-industry and intra-firm trade in complex supply chains, are far beyond the ken of all but the most engaged citizens. The average, or median, citizen-voter has no reason to care about the latest anti-dumping suit or WTO dispute. Almost all the time, we can explain actual trade policy in the United

³ For a finding in line with the standard view, see Betz and Pond 2019; but for a different perspective from the developing world see Baker 2005.

States – and elsewhere in the developed world – pretty much entirely on the basis of special-interest politics. Those who focus on popular attitudes toward trade do not normally dispute this, but it is worth remembering that these attitudes do not typically have much impact on the making of trade policy itself.

This reality is tempered in two ways. First, citizen-voters may well care about aspects of trade that affect them directly, or their local community. There are indeed many millions of Americans on firms and farms that either rely on exports or are subject to potential damage from imports. When trade policy matters to these workers, managers and union representatives typically make sure they know about it. While much of the literature focuses on lobbying and campaign contributions, most politicians are probably particularly susceptible to the possibility that a contingent of voters in their district expects to be harmed by trade or a trade measure. Granted, this is a small minority of voters, but it can be a substantial mass of voters in districts – such as congressional districts – in which manufacturing or farming are important. So while the median American voter rarely thinks about trade, and what she thinks is largely irrelevant to the making of trade policy in any case, the median voter in some trade-exposed areas may indeed think about and care about trade policy, and these views may indeed have an impact on policymakers.

The second way that opinions on trade may matter is when trade does in fact become a nationally prominent issue. This happens rarely – in the United States in

recent memory it has only really been the case around NAFTA and 2016. More general cross-national studies indicate that trade comes to the fore primarily when there is a significant trade treaty or agreement being negotiated or ratified (Frieden et al 2022). Certainly the 2016 election campaign, and the subsequent policies of the Trump Administration, are important examples of how trade can rise toward the top of the national political agenda. Scholars may long debate the causes of this rise, but there is no doubt that the fact that trade was a central issue in 2016 and after meant that public opinion on trade was consequential. It is certainly not clear *how much* public opinion mattered to the ultimate evolution of trade policy, even in 2016 and after, but this was one of the rare instances in which the politics of trade emerged from the special-interest shadows to take center stage in national political debates. While this highlights the potential importance of understanding public opinion on trade, it is worth keeping in mind that trade policy is not normally subject to such attention, and is not normally affected by it.

Even when trade does become a topic of general interest, its impact is mediated through the often-complex electoral and legislative institutions of the society in question. The American trade policymaking process is complicated and highly bureaucratized, in ways that can affect outcomes even in the context of an all-out trade

war, such as with the tariff exclusion process.⁴ More broadly, scholars have analyzed the impact of the wide variety of political institutions across the developed world, with the general conclusion that similar public and electoral pressures can lead to very different policy outcomes in, for example, parliamentary and presidential systems (Rickard 2015 is an excellent survey). Certainly students of American politics know that the House of Representatives, the Senate, the President, and the trade bureaucracy are likely to have very different responses to electoral and special-interest pressures on trade policy.

The institutions and realities of American democracy may indeed have played a role in encouraging politicians to focus on trade in recent years. As American politics became more and more polarized after the early 1990s, with most states solidly Democratic or solidly Republican, the Industrial Belt was significant as a region in which states might go one way or the other. This made them central to the politics of trade policy. To take an example, the four states with the most steel employment in the United States are Indiana, Michigan, Ohio, and Pennsylvania. Three of these four are

⁴ Gary Clyde Hufbauer and Zhiyao Lu, “The USTR Tariff Exclusion Process: Five Things to Know About These Opaque Handouts,” at <https://www.piie.com/blogs/trade-and-investment-policy-watch/ustr-tariff-exclusion-process-five-things-know-about-these>

swing states, whose voting behavior has a notable impact on national – especially presidential – electoral outcomes. The median voter in the median state is rarely the country’s overall median, and in 2016, the pivotal voters in pivotal states may well have been trade-sensitive voters in the Industrial Belt. The country’s attitudes toward trade may not have mattered much, but attitudes in the swing states may have been crucial to Donald Trump’s victory.

Popular understanding of trade. No scholar believes that the average voter has anything like an economist’s view of the impact of trade. The concept of comparative advantage is hard enough for undergraduate Economics majors to master; it is not a commonplace of popular wisdom.

Nonetheless, popular attitudes toward trade are not that different from what we might call a “folk Economics” view. Mutz, indeed, provides a useful summary of such a view. She shows, as have others, that Americans generally think trade is good for the country, but that it endangers jobs. Mutz refers to the tendency toward a zero-sum attitude, thinking that with trade the United States will lose jobs and other countries will gain them.

Although the view that trade is bad for jobs is hardly textbook Economics, it is not foolish in the American context. Indeed, America’s factor endowments – poor in unskilled and semi-skilled labor, rich in capital and human capital – imply that trade, especially with poor countries, will be bad for unskilled and semi-skilled workers. To be

sure, trade also creates many jobs, but these are typically – given the country’s endowments – for highly skilled individuals. Generally speaking, we expect trade to lead employers to substitute away from labor and toward capital and skilled labor – as in fact they have. When Americans are asked about trade and jobs, they presumably do not focus on the creation of (prospective future) jobs for software engineers and biochemists, instead recognizing that working people are likely to lose their current jobs. Generation after generation of scholarly study has indeed shown just this, whether about NAFTA or the entry of China into the World Trade Organization. While average Americans do not understand the intricacies of trade theory, they appear to have a reasonably good sense of some of the principal effects of trade, both on the general economy (good) and on jobs (not so good).

This issue is related to one that also makes understanding public opinion on trade complex, the role of “socio-tropic” attitudes. These are attitudes based on understandings about the implications of a phenomenon, such as trade, for society (hence “socio”), rather than the individual. Admittedly, there are many different interpretations of socio-tropic attitudes. Some regard them as pure altruism: people care about their fellow citizens. Other see them as about information: citizens use national economic conditions to draw inferences about how they will fare (Kinder and Kiewiet 1981). A view that trade is good for the country but bad for jobs is ambiguous, so that

citizens have to try to decide whether they think the information about the job impact is more relevant to them than that about aggregate welfare.

This is especially the case inasmuch as Americans emphasize the impact of trade – like so much else – on their own region of the country. It is reasonable for individuals to use information about regional effects of economic trends – such as trade – to try to anticipate more direct effects on themselves. And some regions of the country have clearly gained from trade, while others almost as clearly seem to have lost. Notably, many of globalization’s “winners” are found in the prosperous, solidly Democratic, Northeast and Far West, along with the prosperous Democratic urban areas in between. While many traditional Republican areas have also done well, the politically contested Industrial Belt is where the country’s globalization “losers” are concentrated.

In this context, even socio-tropic attitudes are likely to be affected by *regional* trends and realities. There is evidence, indeed, that voters pay special attention to their near surroundings, and form their attitudes accordingly. Such “geo-tropic” views – emphasizing developments in a surrounding area smaller than the country but larger than the individual – may well explain a great deal of relevant public opinion (Ansolabehere et al. 2014). The regionally specific nature of attitudes toward trade – and toward economic and political integration more generally – is hardly a solely American phenomenon. Throughout the developed world, support for political movements, parties, and candidates who express hostility toward trade, international and regional

economic and political integration, immigration, and the like are very noticeably concentrated in specific geographical regions. I now turn precisely to the connection between these regional trends and individual attitudes and behavior.

Trade and politics: economic and political geography

Regions that have been hard hit by the loss of manufacturing jobs are the center of popular skepticism about international trade, and other international economic relations. This has been the conclusion of literally hundreds of scholarly articles, in dozens of countries. Some use the “China shock” instrument pioneered by Autor, Dorn, and Hanson (2013), but there is a very wide variety of methods, all pointing in the same direction. It is true of Western European countries generally (Colantone and Stanig 2018a and 2019), of France (Malgouyres 2017) and Sweden (Dal Bó et al 2021) specifically, and of the British referendum to leave the European Union (“Brexit”) (Colantone and Stanig 2018b). It is also true of analogous trends in American politics: hard-hit regions have become more politically polarized since 2000 (Autor et al. 2016a); their legislators have tended to vote in more protectionist directions (Feigenbaum and Hall 2015); and they were more likely to swing their votes toward Donald Trump in the 2016 presidential election (Autor et al. 2016b; Jensen et al. 2017; Baccini and Weymouth 2021).

The consistent findings of a strong – many would argue demonstrably causal – relationship between regional economic shocks and regional political behavior are a

given. Their claims can be questioned or weakened on many grounds. Trade is almost certainly a less important factor in deindustrialization than skill-biased technological change (automation). In countries other than the United States, the target of skeptics is rarely trade itself, more commonly taking the form of anti-European Union or anti-immigrant sentiment. The political responses in questions vary greatly, as do their reflection in a variety of political systems. Economic factors may interact with non-material ones. There may be methodological challenges to some of the stronger causal claims. And, as previously noted, the mechanisms that connect regional causes to individual-level effects are rarely if ever specified, let alone demonstrated. Nonetheless, the strength and extent of these findings mean that we should simply take these very strong regional connections as given.

However, these findings do not demonstrate direct connections between economic shocks and political behavior at the individual level. This is the point made by many survey researchers, and showcased in *Winners and Losers*. There is a wide variety of approaches to the issue, many of them indeed discussed in Mutz's book. Some emphasize the role of partisan politics; others focus on cultural, ethnic, and other non-material factors. Others emphasize deep-seated moral features of individuals, or groups of individuals (Enke 2020). And, like the regional findings, these individual ones should be taken as given.

The challenge, then, is to understand the relationship between the regional and individual causes and effects. This is, I submit, the clear research frontier for those interested in this very wide range of political and political-economy issues. Indeed, it is not only relevant to attempts to understand the backlash against globalization, but rather implicates the politics of international and domestic economic policymaking more generally. Economic differences among regions appear to have grown significantly in past decades across the developed world, and the political implications of this divergence promise to be a central issue in politics and political economy for a long time to come.

In the American context, a starting point to understanding this complex reality is to look at the recent history of these regional trends.⁵ Manufacturing employment began to decline in the United States in the early 1970s. Technological change was an important source of this decline, but so too was the entry into world markets of a phalanx of low-wage developing countries in East Asia and Latin America. Indeed, in this period, for the first time, developing countries began emphasizing the production of manufacturing goods for export to rich nations (where they had previously exported primary products and focused their industrialization drives on their domestic markets).

⁵ For more details on what follows see Broz et al (2021).

China and several other East Asian countries came relatively late to this process, but their full-scale entry into international trade accelerated the trend.

The impact of manufacturing job loss was especially severe for small cities and towns that did not have a diversified economic base. Large cities lost manufacturing jobs, of course – the massive manufacturing complexes of New York and Los Angeles collapsed – but employment was picked up in other sectors. In the smaller cities and towns the loss of a few manufacturing plants, or a single large one, could be devastating. And the economic decline fed on itself. As employment and income fell, so too did housing values; local tax revenue declined and the ability of local governments to provide local public goods suffered. The manufacturing decline associated with increase manufactured imports, then, fed through to local spending on public welfare, public transport, and public housing; to increased crime; and increased class sizes in public schools (Feler and Senses 2017; Xu et al 2019). Eventually economic decline contributed to broader social problems, as documented by Case and Deaton (2020) in their work on “deaths of despair” due to alcohol and drug abuse and suicide (see also Pierce and Schott 2020 and Dean and Kimmel 2019). Reduced opportunities in trade-affected areas led better-educated residents to leave, and were also associated with increased levels of enlistment in the armed forces by those who remained (Dean 2018; Dean and Obert 2020).

For many regions of the United States, deindustrialization has been an extended, agonizing downward spiral. It was a spiral in which many individuals found themselves stuck. Regional mobility in the United States has declined dramatically, in large part due to soaring housing-price differentials between booming and struggling areas. For lower-wage workers such as those hit by manufacturing job loss, the ability to move to more promising areas is strictly limited by the exorbitant cost of housing in the prosperous big cities (Ganong and Shoag 2017; Hoxie et al. 2020). And it is well-known that the declining regions of the country are precisely those in which *social* mobility is lowest as well (Chetty et al. 2014).

For our purposes, the important thing about these powerful and disturbing regional realities is that to a great extent they affect *everyone* in a region. Certainly the factory workers who lose their jobs as industry declines are on the front lines, but eventually the kind of community decay that these studies describe has an impact on everyone in the area. Out-migration of the more promising young people, a deterioration of social services, a rise in social ills are *community* problems. And such community problems will be a prominent feature in the lives of local schoolteachers, dentists, and shopkeepers, along with the directly affected factory workers.

This can be an important starting point to understanding how regional economic problems translate into individual political beliefs and behavior. It is only a starting point, and there are many directions worth exploring. Does the nature of the economic

trends or shocks (automation or trade, for example) matter? How about the length of time the decline has been in train? Do local social networks mitigate the blow of economic distress – or, perhaps, magnify it? Do local political leaders address the problems effectively, or ignore them? Are the region’s representatives at the state and federal level focused on the problems, or do they turn their attention to their more prosperous constituents? Do local social and political leaders suggest narratives to explain the problem – and potential solutions? How realistic are the explanations and the solutions – and to what extent do they focus on assessing blame as opposed to addressing alternatives? All of these questions, and many more, are relevant to trying to figure out the transit from regional economic trends through to individual (and thence regional, again) political behavior and outcomes.

This work can best be carried out with *collaboration* among researchers who focus on the regional factors and those who emphasize the individual ones, among those who incline toward economic causes and those partial to non-material ones. All serious scholars understand that *all* of these dimensions matter, even if we do not know how. It is in all our interests to figure out the relationships in question.

Why it matters

These questions are extraordinarily and inherently interesting, which is why so many scholars have been working on them. But they are also extremely important for the future of our societies. Political leaders, movements, and parties that question most

aspects of international economic (and political) integration and cooperation remain powerful in most developed, and many developing, societies. Most scholars, I think, believe that the policies these populist and related movements propose are both damaging and unlikely to do anything to ameliorate the problems they purport to address. Yet it is not clear that existing governments and mainstream political parties have much of an answer to the discontent populist movements appear to have harnessed.

In this context, it is particularly important to have a clear diagnosis of the source of the discontent. To state my concern along these lines explicitly, I find it troubling that some of the survey-related analyses of the past decade seem to imply that working-class supporters of Donald Trump, or Marine Le Pen, or Brexit, are ignorant racist yahoos whose support for anti-elite, populist, candidates is an expression of their psycho-social hatred for others. These views, I might note, have trouble with powerfully anti-globalization Left populists such as Bernie Sanders; and with the fact that millions of white working-class Americans voted for Barack Obama twice, then for Donald Trump. But more to the point, they seem to simply write off a large segment of American society (and others), and they also seem to imply that their grievances are not legitimate. These are positions that I find questionable, if not dangerous.

The two mainstream American political parties have indeed, in my view, too long ignored the real and serious concerns of regions of the country, and portions of the

working class, who have been facing real hardship as manufacturing declined. This decline was almost certainly inevitable; it was counter-balanced by rapid growth and prosperity elsewhere; and the trade and technological changes that caused it were beneficial to the country as a whole. Nonetheless, this is not moral or ethical justification for disregarding the very real suffering that economic decline and social dysfunction have created in large swaths of the industrial Midwest and South.

Perhaps more important, if we continue to ignore the plight of those disfavored by past and ongoing socio-economic change, we will almost certainly see a continuation and intensification of the kinds of populist movements that have grown rapidly in the past decade. The fact that the policy proposals of these movements are misguided is unfortunate; the fact that many of these movements have taken increasingly authoritarian turns is alarming. This makes it urgent to develop a realistic and effective way of addressing the sources of support for authoritarian populism.

Those who see the supporters of Trump, Brexit, Le Pen and others as morally reprehensible and politically hopeless must hold out little prospect for defusing or redirecting their anger and concern. But these views – to return to our analytical focus – would appear to ignore the regional economic realities these supporters face, whatever their individual fortunes.

To emphasize that entire regions of the United States – and other countries – have been “left behind” over the past few decades is to insist that policymakers should

pay more attention to “the politics of place.” American attempts to address the damage trade may have done have been almost exclusively at the individual level, and there are continuing debates over how effective they have been – or can be (Rodrik 2017).

Whatever the reality about individual compensation, there is a strong view emerging that “place-based policies” are the best way to address these problems (Austin et al 2018; Shambaugh and Nunn 2018). Inasmuch as *regional* factors affect *individual* political behavior, a realistic and reasonable response to regional economic distress may well have an impact on the regional political environment.

This is where scholarly research remains crucially important. A fuller understanding of *how* regional economic developments affect politics within a region – whether the region is a town, county, or state – is central to understanding how policy can appropriately address the concerns of troubled regions in ways that are both economically, socially, and politically constructive. As always, if our diagnosis is flawed or incomplete, our prescriptions will be inadequate to the task – and the prognosis will be pessimistic.

Conclusion

Diana Mutz’s *Winners and Losers: The Psychology of Trade* is a worthy continuator of ongoing discussions about the sources of political attitudes toward trade, as well as toward other aspects of international economic engagement. It makes a forceful argument for non-economic, and more broadly non-material, factors in affecting the

way that Americans think about trade and the international economy. Other scholars have made equally forceful arguments for economic causes of political attitudes and behavior. A striking feature of scholarly debates is that those who focus on non-material influences largely rely on individual-level studies, especially opinion surveys and survey experiments, while those who focus on economic influences largely look at causes and effects at the level of such geographically specific regions as counties, commuting zones, or legislative districts.

It seems clear that regional economic developments, such as deindustrialization, have an impact on the politics of the regions in question – such as spurring anti-trade or populist sentiment. It also seems clear that the responses of individuals, even in these regions, are not so easily explained on the basis of their personal economic experiences and expectations.

The research frontier in this important area of political economy, I suggest, is to analyze the relationship between geographically specific economic trends and individual responses to those trends. These responses are undoubtedly mediated through the social, political, economic, and other characteristics of the region in question. But the way in which this mediation takes place, and the features that matter to it, are still very much a mystery. Both our understanding of the contemporary domestic politics of international economic relations, and policymaking in this realm, depend on clearing up this mystery.

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