Survey Experiments in International Political Economy: What We (Don’t) Know About the Backlash Against Globalization

Megumi Naoi
Department of Political Science, University of California, San Diego, La Jolla, California 92093-0521, USA; email: mnaoi@ucsd.edu

Abstract
This article reviews the cumulation of evidence from survey experiments in the field of international political economy (IPE) and discusses their strengths and weaknesses in explaining the backlash against globalization. I first review the advancements made by the most commonly used survey experiment design in IPE, namely the Globalization-as-Treatment design, in which scholars randomly assign information about different features of globalization and solicit respondents’ attitudes toward protectionism. Then I discuss three issues with this design in addressing key puzzles in the emergence of globalization backlash: (a) using a coarse informational treatment that stacks the deck against the economic self-interest hypothesis; (b) over-attributing globalization as a source of hardship; and (c) neglecting heterogeneous room-to-maneuver beliefs across and within countries. The article suggests alternative designs and strategies to study these questions. Evidence from survey experiments suggests that much of the globalization backlash we witness today is deeply rooted in domestic politics.

Keywords
survey experiments, IPE, trade, immigration, investment, protectionism, globalization, populism
INTRODUCTION

The durable commitment to an open economy that characterized the post–World War II order is no longer assured. The surge of protectionist rhetoric, sentiments, and policies that we witness today poses two key questions for scholars of international political economy (IPE). The first question is about sources of voter opposition to globalization: whether backlash is motivated by the material interests of voters, such as economic hardship and insecurities, or by nonmaterial interests, such as cultural and status threats. The second question is whether this backlash is generated by a bottom-up process, where voters assign blame for their economic or cultural insecurities to globalization and hold elected officials accountable (known as “blame attribution” or “causal attribution” in the literature), or by a top-down process, where elites create demand for protectionism among otherwise unaware voters by framing, persuasion, or fueling in- versus out-group anxiety toward globalization.

This article reviews studies that leverage survey experiments in the field of IPE and discusses their strengths and weaknesses in explaining backlash against globalization. I begin by noting that emerging findings on globalization backlash from observational studies and survey experiments are at stark odds with each other. The overwhelming majority of observational studies have found support for the economic interests hypothesis, i.e., that the economic hardship of voters drives their support for protectionism. Analyses of Donald Trump’s vote gains in the 2016 US presidential election, especially in the Rust Belt region, corroborate this view (Autor et al. 2016, Jensen et al. 2017). [See Norris & Inglehart (2019) and Mutz (2018) for counterevidence, although both works seek to explain support for right-wing parties or candidates, not necessarily protectionism.]

“Leave” votes in the Brexit referendum were also concentrated in districts with voters with lower educational attainment, lower income, higher unemployment rates, and a historical dependency on manufacturing employment (Becker et al. 2017, Colantone & Stanig 2018). Although these studies consider diverse economic origins of this hardship—e.g., import shock from China (Autor et al. 2013, Kuk et al. 2018), companies outsourcing jobs abroad (Rickard 2018), exogenous exchange rate shock (Ahlquist et al. 2020), or the Great Recession (Mansfield et al. 2016)—economic hardship nonetheless plays a primary role in explaining the shift in public and elite opinion toward protectionism. Beyond explaining protectionist sentiments and policies, furthermore, studies have attributed the rise of xenophobia, racism, support for extreme right-wing parties, and the rise in authoritarian values to economic hardship (Ballard-Rosa et al. 2017).

By contrast, the bulk of findings from survey experiments lend support for noneconomic sources of support for protectionism, such as other-regarding preferences (Naoi & Kume 2011, Lü et al. 2012, Chilton et al. 2020), nationalism, and ethnocentrism (Margalit 2012, Mansfield & Mutz 2013). In fact, survey experimental studies have rarely found support for economic self-interest as a driver of opposition to globalization [exceptions include studies by Rho & Tomz (2017) and Bearce & Tuxhorn (2017), which I discuss in detail below]. Furthermore, these findings from survey experiments corroborate the findings from nationally representative public opinion data that the salience of trade and other globalization-related issues has been extremely low in the public mind, even at the height of the China import shock (mid-2000s; see Guisinger 2009, 2017; Cramer 2016) and during the Great Recession, the largest economic contraction since the Great Depression (Naoi 2018).

How can we reconcile the disparate evidence from observational versus survey experimental studies? I argue that some of the conflicting evidence arises by (research) design. I make three points. First, I demonstrate that giving a precise informational treatment about individual economic self-interest is difficult in survey experiments. As a result, scholars have administered coarse informational treatments about globalization—such as statements about trade policy's effects on
the national economy, its distributonal effects on a particular sector or workers, or immigrants of a particular skill profile seeking entry into a country. It is inherent to the design then that these coarse informational treatments stack the deck against the self-interest hypothesis in favor of the other-regarding or sociotropic hypotheses. Because the coarse informational treatment updates beliefs, knowledge, or salience about a broader community or about the interests of “others,” survey experiments give an edge to the other-regarding and sociotropic arguments about voter preference formation.

Second, observational studies and survey experiments have different comparative advantages in measurement. Observational studies that use labor, firm, and industrial censuses have an advantage in measuring so-called hard and objective economic interests of communities or individuals and their changes over time (Autor et al. 2013; Hummels et al. 2014) match worker–firm data in Denmark, although I would note that any census has a self-reporting component that is susceptible to similar subjectivity and reporting biases as surveys. Measuring citizens’ perceived status or cultural threats and heightening identity is a lot harder than measuring their economic hardship, especially in observational settings, as these variables are chiefly cognitive or psychological (Bullock et al. 2010, Zmigrod et al. 2018). There is no census on the cognitive or psychological state of citizens, although scholars have devised creative ways to measure status threat and nationalist sentiments through behavioral measures such as Google search data on racist terms (Stephens-Davidowitz & Pabon 2017) and barcode data on consumer behavior at grocery shops (Pandya & Venkatesan 2016).

Surveys and focus group interviews have filled this gap by eliciting citizens’ perceived status threat, identity, and nonmaterial dispositions (Cramer 2016, Mutz 2018). Survey research in political science has benefited from the cumulative knowledge in other disciplines about how to measure the cognitive and psychological state of citizens as well as their predispositions. Examples include measurements of authoritarian values (Dunwoody & Funke 2016, Ballard-Rosa et al. 2017) and social dominant orientations (Mutz 2018) developed in psychology, as well as measurements of cosmopolitanism developed in the field of sociology and marketing (Bechtel et al. 2014). For instance, Mutz (2018) uses a social dominance orientation index constructed from panel surveys and responses to other questions about trade agreements and China as a measure of the racial, cultural, and global status threats perceived by the American respondents. Survey experiments in IPE have leveraged this progress in measurements in cognitive, emotional, and dispositional characteristics of individuals, which facilitate their inquiries into how nonmaterial factors can contribute to the globalization backlash (Naoi & Kume 2011, 2015; Margalit 2012; Mansfield & Mutz 2013; Mutz & Kim 2017).

Third, observational studies and survey experiments have different comparative advantages in addressing threats to causal inference. It is widely accepted that randomized experiments, including survey experiments, are the gold standard of causal inference. Random assignment of stimuli embedded in the survey addresses multicollinearity issues (e.g., culturally threatened individuals are often economically threatened, too), the issue of contemporaneous change (e.g., President Obama’s first term coincided with the Great Recession), and reverse causality (e.g., we cannot know whether elite opinion molds public opinion or vice versa) that have plagued observational research on the globalization backlash. Yet, scholars rarely conduct the same survey experiment repeatedly, and with a one-shot experiment, it is difficult to answer a question about the timing of the emergence of the backlash. When, and among what segments of the population, did voters become more protectionist? For instance, Naoi (2018) finds that 2009 is the historic year of partisan reversal in the United States, when Republican voters became more trade protectionist than Democratic voters. She attributes this reversal to “stimulus resentment,” i.e., Republicans
who perceived that they did not benefit from the 2009 American Recovery and Reinvestment Act grew to further oppose government compensation and instead support trade protectionism. Observational studies, especially those that leverage panel data (e.g., panel surveys that follow the same individuals over multiple waves, time-series cross-sectional data by electoral district), offer opportunities to analyze how these sharp and big changes affected public opinion. [Goldstein & Peters (2014), Margalit (2013), and Mutz (2018) leverage panel survey data; see Autor et al. (2013) on the use of first-differenced cross-sectional data over two time periods.]

Understanding the strengths and weaknesses of survey experiments in explaining globalization backlash, relative to observational studies, is the key to reconcile the disparate findings and advance research. The remainder of this article proceeds as follows. The first section below reviews the cumulation of evidence from the most commonly used survey experiment design in IPE, in which scholars randomly assign information about different features of globalization and solicit respondents’ attitudes toward protectionism; I call this the Globalization-as-Treatment design. The next section discusses three issues associated with this Globalization-as-Treatment design in addressing key puzzles in the emergence of globalization backlash, especially the voter attribution process: (a) The design uses a coarse informational treatment that stacks the deck against the economic self-interest hypothesis, (b) it overattributes globalization as a source of hardship, and (c) it neglects the heterogeneity of room-to-maneuver beliefs (i.e., the extent to which citizens believe that their government can control globalization) across and within countries. The article suggests alternative designs and strategies to study these questions that might help us reconcile the disparate findings.

To preview the findings, emerging evidence from survey experiments reviewed below suggests that much of the globalization backlash we witness today is deeply rooted in domestic politics. An income shock or economic hardship alone is often not a sufficient or even necessary condition for backlash to emerge. These shocks are filtered through partisan competition, ideological battles over who deserves protection more than others, and existing (or lack of) welfare programs, to produce a variety of backlash responses across time and space. Survey experiments have much promise to advance our understanding of globalization backlash, but they require proper designs and inference as well as a nuanced understanding of domestic politics.

GLOBALIZATION AS TREATMENT

Who supports and opposes globalization, and why? Do labor market concerns or cultural threat drive voters to oppose free trade or immigration? Are voters more concerned about their economic self-interest in taking a policy position, or are they more concerned about the welfare of others or the broader community that they belong to? What types of voters are swayed by protectionist campaigns by elites, and what is the effect of pro-immigration and pro-trade campaigns? These are some of the key questions that survey experimental studies conducted in IPE have sought to answer.

To address these questions, the most common IPE survey experiment gives a randomized informational treatment about particular features of globalization and solicits respondents’ attitudes toward the outcome of interest, such as support for a policy restricting imports and immigration. The difference in distribution of opinions between the treated group and a control group, which received no information about globalization, or a placebo group, which received information irrelevant to globalization, is the average treatment effect. Substantively, the average treatment effect captures how much the information moves respondent opinion on the outcome question compared to the control group. Hereafter, I call this a Globalization-as-Treatment design. Figure 1 describes typical survey flows of a Globalization-as-Treatment design. Note that
Figure 1: Globalization-as-Treatment design, common in the field of international political economy.

treatment assignment can be embedded in the outcome question. For instance, Hainmueller & Hiscox (2010) randomly assign different versions of an outcome question: Some groups are asked about their support for accepting more low-skilled immigrants, and other groups are asked about their support for accepting more high-skilled immigrants.

Testing Preference Formation I: Material Versus Nonmaterial Sources

Survey experiments have made substantial contributions to our understanding of how much of the backlash is driven by the material versus nonmaterial interests of citizens. Cumulative evidence from survey experiments in IPE lends stronger support to nonmaterial explanations for globalization backlash in policy areas such as trade, outsourcing, and immigration. By contrast, several studies have shown that economic ignorance or the lack of sufficient policy knowledge of citizens might have contributed to the scarcity of evidence in support of the economic self-interest hypothesis. When respondents are sufficiently informed about the predictions of well-established economic models on who should benefit and lose from the policy, they are more likely to take positions that are consistent with the predictions of the models.

While distributional and welfare implications of many policy issues are murky even for scholars, the field of IPE has sufficient cumulation of established economic models that predict who should benefit and lose from globalization. Scholars in IPE have fully taken advantage of this by using existing economic models to benchmark economic self-interest. For instance, to better understand individual attitudes toward trade, scholars have benchmarked economic self-interest based on the prediction of the Stolper-Samuelson theorem—i.e., in advanced industrialized nations, low-skilled citizens will lose from trade liberalization and high-skilled citizens will benefit (Rho & Tomz 2017)—and based on the prediction of the Ricardo-Viner model of trade, which predicts that export-oriented industries will benefit and import-competing industries will lose from trade liberalization (Chiang et al. 2017). Similarly, for benchmarking economic self-interest in immigration policy, scholars use a factor proportions model, in which natives face labor market competition against immigrants with similar skill level (often measured by educational attainment). For benchmarking economic self-interest in the monetary policy domain, Bearce & Tuxhorn (2017) base their measurements of covariates on the factor-, sector-, and firm-based models of monetary policy preferences (to be discussed in detail below).

After recording these labor market characteristics of respondents according to the predictions from economic models before treatments, scholars have used three common strategies to
adjudicate economic versus cultural explanations. One is to increase the salience of economic considerations by randomly assigning an informational treatment that varies economic profiles of goods, immigrants, or investment projects entering respondents’ country. Scholars design the variations in the treatments to test the benchmark predictions from the economic model. When respondents prefer or oppose a particular profile of globalization that is consistent with the prediction from the model, e.g., respondents are more likely to support immigrants with complementary skills, scholars infer that economic self-interest dictates attitudes. When respondents’ attitudes deviate from the prediction, scholars infer that noneconomic factors might be at play.

Hainmueller & Hiscox (2010) use this design to test whether and to what extent concerns about labor market competition and the fiscal burden on natives drive immigration attitudes in the United States. Their survey experiment randomly assigns two versions of the outcome question on opinion on immigration, in which the questions vary only in the skill level of the described immigrants (emphasis original): “Do you agree or disagree that the U.S. should allow more highly skilled (low-skilled) immigrants from other countries to come and live here?” An economic model of factor proportions, which predicts that wages of low-skilled natives should fall with the entry of low-skilled immigrants (the same holds for high-skilled natives facing high-skilled immigrants), guides the design of these treatments (Borjas 1999). Random assignment of information about immigrants’ skill levels, interacting with the respondents’ own skill levels, provides variation to test the labor market competition hypothesis.

Hainmueller & Hiscox (2010) find that both high-skilled and low-skilled respondents prefer that the government allow more high-skilled immigrants, contrary to the prediction from the factor proportions model. They also find no support for the tax burden hypothesis, which predicts that rich natives will oppose low-skilled immigration in states where welfare spending for native households and the ratio of immigrant to native households are both high (Hanson et al. 2007). Rather, they find the opposite—that rich natives in high-fiscal-exposure states oppose low-skilled immigrants less. The authors interpret the results as a rejection of the material interest hypothesis and discuss alternative explanations, such as ethnocentrism and sociotropic concerns (that high-skilled immigrants are preferred by natives due to their contribution to the national economy). The only finding that lends support to the material interest hypothesis is that low-skilled respondents oppose low-skilled immigrants in high-fiscal-exposure states, which suggests that natives’ concerns about competition over public and welfare service provisions affect attitudes.

Another variant of this inquiry into economic self-interest is to randomly give respondents information about who benefits and loses from globalization that is essentially a summary of a well-established economic model. The idea is to inform respondents of what economics textbooks would tell them about their self-interest and analyze whether this treatment increases the concordance between their labor market characteristics, which scholars have used to proxy economic self-interest, and their policy positions.

For instance, Rho & Tomz (2017) test how informing respondents of the Stolper-Samuelson predictions about trade makes them more aware of their self-interest in trade policy. Their survey experiment with Mechanical Turk respondents in the United States randomly assigns the distributional implications of the Stolper-Samuelson theorem (e.g., low-skilled American workers lose from imports of products that are made by low-skilled workers, and high-skilled American workers benefit from low-skilled imports) and then solicits respondents’ support for limiting low-skill versus high-skill imported goods. The paper finds that the Stolper-Samuelson theorem treatment increases the concordance between respondents’ skill levels, measured by their educational attainment, and their attitudes toward limiting low- versus high-skill imports. The treatment especially shifts opinions of high-skilled respondents to oppose high-skill imports.
The implication of this finding, Rho & Tomz (2017) argue, is that economic ignorance might account for citizens’ inability to assess self-interest in trade and that correcting this ignorance might increase their self-interest awareness.

Similarly, Bearce & Tuxhorn (2017) test whether informational treatments (“contextual information”) about the costs and benefits of two monetary policy options and their trade-offs can inform self-interest regarding monetary policy among American citizens. Their informational vignette provides an accessible, textbook-type explanation of two conflicting goals for monetary policy, in which a government can either stimulate domestic growth or stabilize the US dollar in the international market, but not both. A respondent’s economic self-interest is measured according to the three existing models—factor-, sector-, and firm-level theories of monetary policy preferences—after the treatment. Bearce & Tuxhorn (2017, p. 184) justify this decision as a strategy to avoid “priming” respondents too much about their economic standing vis-à-vis monetary policy before the treatment. Ideally, however, scholars would want to measure economic self-interest before the treatment to avoid conditioning on post-treatment variables (see Montgomery et al. 2018). The ordering of questions might not be consequential in this case, however, because they measure hard-to-move, nonattitudinal characteristics of respondents, such as education, income, and occupational profiles.

Substantively, the experiments find that the contextual information weakens preferences for domestic monetary policy autonomy among those respondents who work for companies that export or do business abroad, consistent with the prediction from the firm-level model. The authors’ interpretation of this concordance between the firm-level model’s prediction and the heterogeneous treatment effects found along the firm dimension is that the contextual informational treatment informed the self-interest of respondents.

The second strategy to adjudicate material and nonmaterial explanations using survey experiments is to do the opposite of the first strategy—in instead of manipulating salience of economic self-interest, scholars can heighten the salience of respondents’ cultural considerations and analyze how this informational treatment mobilizes respondents’ support for protectionist policy.

The experiment by Margalit (2012), for instance, is one of the first to provide evidence that heightening the feeling of cultural threat, unrelated to globalization, can cause citizens with low educational attainment to increase their opposition to globalization. Margalit (2012) tests the cultural threat hypothesis with a survey experiment using a nationally representative YouGov/Polimetrix sample in the United States. The experiment primes respondents with four sets of questions relating to cultural threat in the United States (e.g., questions about allowing students to sing the national anthem in Spanish at schools, gay marriage, and the decline of American culture). None of these primes directly discuss globalization or its threat to the labor market, although one could argue that a question about allowing the national anthem to be sung in Spanish at schools can remind respondents of labor market threats from immigrants from Latin America. The study finds that the cultural threat prime increases opposition to “the growing trade and business ties between our country and other countries” by 10 percentage points (from 44% to 54%), and this effect is only found among respondents without a college education.

Mansfield & Mutz (2013) examine how the in-group/out-group distinction, manifested through a sense of nationalism, can raise opposition to American companies relocating their production and service sites abroad (i.e., outsourcing). The experiment randomizes treatments that boost respondents’ sense of nationalism, such as a statement that emphasizes the traditional values of Americans. The study finds that boosting respondents’ sense of nationalism together with the use of the symbolic term “outsourcing” in the outcome question increases opposition to outsourcing. Yet, nationalism treatments or outsourcing treatments alone have no systematic
effect on respondents’ opinions toward outsourcing abroad. Another key finding that stands in stark contrast to Guisinger’s (2017) is that racial minorities express greater support for outsourcing than white respondents.

The third strategy to adjudicate material versus nonmaterial explanations is to horse-race two arguments by designing informational treatments that describe both economic and cultural characteristics of goods (for trade policy) or immigrants (for immigration policy) and solicit respondents’ support for limiting or encouraging their entry. This can be done with a factorial design, where some treatment conditions describe economic characteristics of goods or immigrants and other treatment conditions describe cultural characteristics, or with a conjoint design, where scholars randomly assign paired choice sets, which provide information on the economic and cultural characteristics of two goods or immigrants, and then ask respondents to indicate their preferred choice (see Hainmueller et al. 2014 for more detailed discussion of conjoint designs).

In both the factorial and conjoint designs, scholars adjudicate economic versus cultural explanations by measuring whether the economic or cultural treatment causes respondents to have a different distribution of opinions compared to the control group, as well as the relative magnitudes of these effects. Null results—where a treatment does not cause respondents’ opinions to differ systematically from the control group—are then interpreted as a rejection of the theory (I discuss the problem with this interpretation below).

Hainmueller & Hopkins (2015) employ the third strategy and test whether citizens’ concerns about labor market competition (self-interest), immigrants’ contribution to the national economy (sociotropic interest), impact on taxes and welfare provisions, and cultural threat drive the immigration attitudes of Americans. To test which concerns influence attitudes, the authors administered a conjoint experiment, where paired immigrant candidates are described along nine dimensions and a respondent chooses one preferred candidate for admission.

The results suggest that regardless of respondents’ levels of education (skill), income, partisanship, or ethnocentrism, respondents prefer admission of an immigrant with high educational attainment, a highly skilled and prestigious job, and fluency in English. Respondents with the same occupation as the immigrant described in the treatment do not oppose their entry more than respondents with a different occupation. Respondents overall are less likely to prefer an immigrant from Iraq, but otherwise do not differ systematically in their views of an immigrant from a developed or a developing country (India, Sudan, Somalia or China). As the immigrant’s country of origin, Mexico (the largest sender of immigrants to the United States) was not less preferred than France. In Hainmueller & Hopkins’ (2015) interpretation, the overall results support norm-based and sociotropic explanations and challenge the labor market competition hypothesis.

In summary, cumulative evidence from survey experiments in IPE lends overwhelming support to nonmaterial sources of citizen opposition to globalization. I return to the potential issues with this interpretation in the section titled Limitations of the Globalization-as-Treatment Design.

Testing Preference Formation II: Self-Interest Versus Other-Regarding Preferences

Another issue on which survey experiments in IPE have made substantial progress is whether citizens’ self-interest guides opinion formation or whether their concern for the welfare of others, such as the broader community that they belong to and declining industries, has dictated their attitudes. Studies have found that citizens might view trade policy as redistributive policy and support protectionism to help out lower-income workers (Naoi & Kume 2011, Liu et al. 2012). Other-regarding preferences can prevail in citizen support for protectionism even in cases where the majority of citizens must shoulder the cost of protectionism. Scholars have also found
that citizen support for protectionism differs depending on who suffers from adverse income shocks and reasons for the suffering—because citizens have predispositions about who deserves protection more than others.

Lü et al. (2012) ask why both developed and developing countries protect low-skilled workers more than high-skilled workers with higher tariffs—the puzzling empirical pattern that deviates from the Heckscher-Ohlin model of trade, which predicts that a developed country should protect low-skilled workers more and a developing country should protect high-skilled labor more given their comparative disadvantages in the world economy. In coordinated, nationally representative survey experiments conducted in the United States (where low-skilled workers should be trade losers) and China (where low-skilled workers should be trade winners), the authors randomly assigned treatments that describe different wage brackets of workers and solicit respondents’ support for trade protection. They find that other-regarding concerns for redistribution [“inequity aversion” (Lü et al. 2012, pp. 638–39); also see Fehr & Schmidt 1999], not self-interest, dictate citizens’ attitudes toward protecting the low-skilled (proxied by their wage levels) workers regardless of low-skilled workers’ position in the international economy.

Naoi & Kume (2011) ask why voters support agricultural protectionism in advanced industrialized nations, even though only a fraction of the workforce engages in farming and protectionism raises food prices, especially burdening low-income consumers. They conducted a priming survey experiment with opt-in respondents in Japan, where consumers are taxed with an average 40% price premium for food due to trade protection, price supports, and subsidies given to farmers. The experiment randomly primes respondents to think about worker/producer versus consumer interests by assigning visual images of three work sites versus three consumption sites (e.g., retail stores), with a control group that sees no photos. The consumer treatment has a null effect on respondents’ support for further liberalizing food imports, while the producer treatment mobilizes support for agricultural protectionism by nine percentage points compared to the control group. With further mechanism tests, Naoi & Kume (2011, p. 791) find a “coalition of losers”—the producer treatment increases support for agricultural protection among citizens with high job insecurity through a psychological mechanism of projection. Agriculture is a symbolic declining industry and the Japanese public perceives farmers to be hardworking and low-income, which accounts for the formation of other-regarding support for protection. The authors further note that farmers being poor is a misperception long held among the Japanese public—the average household income of farmers has exceeded those of urban employees in Japan since 1975.

Finally, other-regarding preferences can account for why citizens are often willing to shoulder the costs of helping other countries, through various policy instruments such as foreign aid (Baker 2015) and financial bailout. Bechtel et al. (2014) fielded a nationally representative survey as well as a survey experiment in Germany to test economic and noneconomic explanations for citizen support for financial bailouts, e.g., German taxpayers shouldering the cost of bailing Greece out from the Eurozone crisis. They find that economic explanations, such as levels of education and tax burdens falling onto high-income Germans, do not fare well in explaining who supports the bailout in Germany. Instead, altruism (reflected by a quasi-behavioral measure of what proportion of money respondents would donate to charity causes if they were to win the reward lottery run by the survey company) and cosmopolitanism (measured via survey) are two explanations that find stronger support.

Baker’s (2015) survey experiments show that racial prejudice in the United States, where white citizens are less supportive of welfare programs if recipients are African American, manifests as paternalism in shaping white respondents’ attitudes toward foreign aid. He finds that, among white Americans, support for foreign aid to African recipients is higher than support for aid to Eastern
European recipients, and this difference is not due to perceived differences in needs but due to underlying paternalism: They perceive black recipients to be lacking in human agency and thus more deserving recipients of foreign aid.

In summary, contrary to the standard political economy approach where economic self-interest guides policy preferences, survey experiments in IPE have rarely found evidence for the egocentric view of individuals dictating their attitudes.

Fairness, trust, and reciprocity. Another other-regarding source of protectionist sentiments depends on whether citizens and elites see their country’s relationship with other countries as fair, trustworthy, or reciprocal. President Donald Trump’s statements about the US trade imbalance with Japan and China, characterizing it as unfair, and about China’s alleged currency manipulation are a throwback to US–Japan trade conflicts in the 1970s and 1980s. This fair trade discourse was also embraced by the previous, Democratic, president, Barack Obama. He celebrated the signing of the Trans-Pacific Partnership (TPP) Agreement, from which the Trump administration later withdrew, as “a new type of trade deal that puts American workers first. . . . It includes the strongest labor standards and environmental commitments in history—and, unlike in past agreements, these standards are fully enforceable. TPP allows America—and not countries like China—to write the rules of the road in the 21st century, which is especially important in a region as dynamic as the Asia-Pacific” (White House 2016).

Do concerns for fairness and reciprocity shape public opinion on globalization? To what extent does elite discourse about fairness and reciprocity sway public opinion? Scholars have long debated why governments cannot cooperate even if an agreement is Pareto-improving and whether elites and the public are more concerned about relative gains than absolute gains.

Mutz & Kim (2017) find that the in-group/out-group distinction shapes Americans’ preferences for trade policies that favor the United States and disfavor its trading partner, even if the alternative agreement is Pareto-improving for both countries. The experiment randomly assigns information to respondents about whether the United States or the other country stands to gain or lose from a trade policy and then solicits respondents’ support for that policy. They find that Americans prefer a trade policy that benefits Americans more than citizens of another country and this effect is stronger among those who believe in American superiority, recorded before treatments. In other words, those who believe that the United States deserves to gain more than the other country are more likely to support a trade policy that disproportionately benefits Americans.

Chilton et al. (2020) find that reciprocity concerns substantially shape both American and Chinese citizens’ attitudes toward limiting foreign firms’ mergers and acquisitions. Conjoint experiments vary information about the degree to which the other country restricts American/Chinese firms’ entry (i.e., reciprocity), in addition to varying features such as country of origin, ownership, size of targeted firm, and whether a targeted industry is in distress. The experiment finds that both American and Chinese respondents are more likely to support blocking inward foreign direct investment when the treatment says that the other government restricts US or Chinese firms’ acquisition of local firms.

Similarly, a series of experimental studies that manipulate country-of-origin information about incoming immigrants, goods, or trade/investment agreements suggest that citizens’ support for the policy differs substantially across different countries of origin. Although it is often difficult to interpret the treatment effects of country-of-origin information due to multicollinearity and misinformation (e.g., profiles of goods imported from China differ from goods imported from Canada), studies have shown that concerns for national security, economic rivalry, and fairness can be a source for this divergence.
Guisinger (2017, ch. 4), for instance, finds that whom the United States is trading with has substantial effects on American citizens’ support for trade protection. In her survey experiment, she first quizzed American respondents on who is the United States’ largest trading partner; the correct answer is Canada, but many respondents answered China. Then, she randomly assigned a treatment that corrected this misperception. She finds that correcting the impression that China is the largest trading partner to the United States increases white male respondents’ support for free trade, but women and minorities oppose free trade regardless of their (mis-)perception about who the United States’ largest trading partner is.

Tella & Rodrik (2019) also find that information about outsourcing to a developing economy mobilizes higher support for protectionism than information about outsourcing to developed economies, and within developing economies, information about “labor abuse” mobilizes higher support for trade protection among Democrats.

In summary, citizens’ support for protectionism can be highly dependent on who experiences hardship, who gains and loses from the hardship relative to one’s in-group, and what gives rise to the hardship.

**The puzzle of instrument choice and deservedness.** Survey experiments have also made substantial progress in better understanding how domestic politics, especially partisan polarization and race and ethnic politics, shape citizens’ beliefs about who deserves protection and government assistance in the global economy.

An emerging puzzle about the backlash that we see today concerns the failures of the embedded liberalism compromise and policy substitutions. Domestic compensation programs are either considered to be “insufficient” to tame the backlash or a “waste” of taxpayers’ money (Naoi 2015). Studies have documented that citizens’ distrust in political authorities (such as political parties and governments) has grown since the Great Recession and that this distrust can translate into a new mix of policy preferences, one that combines their support for smaller government with higher barriers in trade and migration (Naoi 2019). This is a reversal of “embedded liberalism” (Ruggie 1982), when European governments mobilized support for open economies by expanding government programs to shield economic losers from adverse economic shocks after World War II.

While political scientists and economists have considered trade protection and government transfers as substitutive policy instruments (i.e., both address income losses of citizens), emerging experimental studies have found that the government’s use of some policy instruments is more polarizing than its use of others, and they are not substitutes in citizens’ or legislators’ minds (Rickard 2015, Naoi 2019). An implication of this insight for the measurement of outcomes is that we must measure support for protectionism and other domestic measures (e.g., transfers, welfare programs) independently. Most survey experiments in IPE tend to gauge respondents’ support for a single policy instrument or the composite index of policy preferences across a range of substitutive policies (such as tariffs and subsidies; see Guisinger 2017, Tella & Rodrik 2019). These approaches do not accommodate how citizens’ support for one policy instrument (e.g., trade protection) can be associated with their opposition to another, seemingly substitutive, instrument (e.g., domestic compensation).

Tella & Rodrik (2019) ask whether American citizens’ support for two policy instruments, trade protection and domestic compensation, differs depending on the reason for the manufacturing plant closure described in the treatments. In the majority of labor shock treatments, the authors find that American respondents from the convenience sample generally prefer trade protection over government transfers to workers, in both trade- and nontrade-origin shocks. The only exception to this finding is when job losses are described in the treatment as “due to bad management in
the plant,” which increases support for government assistance to workers. The finding is contrary to the established wisdom from political economy research that government transfers are more efficient and less distortive than trade protection (Acemoglu & Robinson 2001). Tella & Rodrik (2019) explain this anomaly by arguing that trade protection helps both firms and their workers, while government transfers are for workers only, although studies have found that governments of advanced industrialized nations generally devote a high proportion of their expenditures on subsidies to firms and industries (Rickard 2012).

Guisinger’s (2017, ch. 6) experiment provides another explanation for why American voters might prefer protectionism over domestic compensation: that there is no racial or income association with trade protection, while many Americans associate welfare programs with particular race or income groups. Guisinger conducted a survey experiment where half of Mechanical Turk respondents were asked to watch a two-minute campaign ad run by Sal Pace, a Democratic presidential candidate, in Colorado for the 2012 presidential election. The ad visually shows all racially white steel mill workers and narrates that Pace protected their jobs by not allowing steel imported from China to be used for building bridges. The author finds that the ad mobilizes 20 percentage points higher support for limiting imports. In another set of experiments, Guisinger (2017, ch. 6) varied a photo image and the name reported in a news clip about the manufacturing sector struggling with import competition, between an African-American man and a white man. After reading the news clip, respondents were asked to take a position on limiting imports. The author finds that white respondents oppose trade protection more when the treatment depicts a worker as an African-American man than when the worker is shown as a white man. Race, she argues, is a key reason why the American public polarizes over welfare programs and is much less divided over trade protection.

While citizens in advanced industrialized nations show high support for trade protection for farmers, their support for government compensation of farmers for trade-induced income shocks is polarized. Naoi (2019) fielded coordinated survey experiments in the United States and Japan that manipulated information regarding the sources of industries’ financial losses (trade agreement, natural disaster, or unspecified cause) as well as who suffers from the losses (manufacturing industries versus farmers). Two key innovations of her design are to vary the source between an explicitly policy origin (a trade agreement) and a nonpolicy origin (a natural disaster) and to ask about respondents’ support for the two instruments—trade protection and transfers—individually, rather than making them choose one over the other. The results suggest that trade treatments mobilize higher support for trade protectionism but do not mobilize higher support for domestic compensation. Rather, treatments that explicitly link farmers’ income losses to a trade agreement produce backlash against domestic compensation in both US and Japanese samples. The results provide a micro-foundation for why, despite its economic efficiency, domestic compensation cannot substitute for protectionism—a failure of embedded liberalism.

In summary, other-regarding preferences of citizens about who deserves government protection and assistance account for the failure of embedded liberalism in polarized countries like the United States. Domestic compensation is not able to substitute for protectionism.

LIMITATIONS OF THE GLOBALIZATION-AS-TREATMENT DESIGN

The Globalization-as-Treatment design has three limitations in probing several key questions about globalization backlash: (a) using a coarse informational treatment that stacks the deck against the self-interest hypothesis, (b) overattributing globalization as a source of hardship, and (c) neglecting heterogeneous room-to-maneuver beliefs across and within countries. I discuss
each of these issues below and suggest alternative designs and strategies that scholars have used to address them.

**Problem I: Testing Economic Self-Interest—The Coarse Informational Treatment**

The first limitation of survey experiments is that individual experience of economic hardship and individual information about economic hardship are nearly impossible to randomly assign. Commonly used informational treatments about globalization are coarse. As discussed above, examples of the Globalization-as-Treatment design include providing information about the expansion of flows (trade, capital, or labor) (Hiscox 2006, Ardanaz et al. 2013), particular profiles of trade or immigration inflows or outflows (Rho & Tomz 2017, Hainmueller & Hopkins 2015), trade or investment agreements that governments negotiate and sign (Kim et al. 2019), or economic hardships of particular sectors, workers, or towns (e.g., a news clip about a manufacturing plant closure) (see Guisinger 2017, Naoi 2019, Tella & Rodrik 2019).

These coarse informational treatments about globalization, however, do not sufficiently inform the self-interest of respondents, and they do not approximate the fine-grained, micro-level distinctions between the beneficiaries and losers of globalization that observational studies in IPE have discovered over the past two decades. For instance, studies have found that the effect of globalization varies substantially within the national economy, among low-skilled workers (Owen & Johnston 2017), across localities, within industries (Kim 2017), and even within each individual depending on whether the worker or consumer dimension of interests is salient (Naoi & Kume 2011, 2015). Likewise, a single trade agreement can involve more than 5,000 product-level tariff lines in addition to complex nontariff and nontrade provisions (e.g., provisions related to investment, the environment, labor, and intellectual property rights) (Naoi & Urata 2013, Kim et al. 2019). Even with a conjoint experimental design that can accommodate 240 or more agreement profile combinations, providing respondents with details about how the policy will affect their self-interest in survey treatments is a difficult, if not impossible, task.

These coarse informational treatments, moreover, are generally about economic interests of a broader community or other citizens than the respondent herself. The commonly used treatment that primes respondents to think about economic hardship is about the national economy or workers in general (Hiscox 2006), manufacturing plant closures (Guisinger 2017, Tella & Rodrik 2019), or income loss of a particular sector or industry (Lü et al. 2012, Naoi 2019).

Therefore, the coarse informational treatment about globalization stacks the deck against the self-interest hypothesis in favor of the sociotropic or other-regarding hypothesis (Mansfield & Mutz 2009, Naoi & Kume 2011, Lü et al. 2012, Bechtel et al. 2014) by design. Because it is difficult to update respondents’ beliefs about their self-interest using the coarse informational treatment, the treatment effect we end up estimating in the Globalization-as-Treatment design may be respondents’ receptiveness to information (i.e., the likelihood of believing the statement or resonating with the statement), not necessarily their recognition of their self-interest or sociotropic interest in response to the treatment.

Mansfield & Mutz (2013, pp. 584–91) provide the most careful examination of this issue. In their survey experiment on American attitudes toward outsourcing, they prerecorded respondents’ responses to the following question: “Have you or has anyone in your family been positively or negatively affected by outsourcing?” They argue that this question may measure one of, or the combination of, three things: (a) respondents’ subjective self-interest over the issue of outsourcing, (b) information about outsourcing that respondents have been exposed to before participating
in the experiment, and/or (c) respondents’ predisposition about outsourcing. Mansfield & Mutz (2013) carefully interpret the substantive and statistically significant effects of this covariate (i.e., subjective reporting of self-interest) on attitudes toward outsourcing and find strong support for the information hypothesis over the self-interest hypothesis. This study suggests that, even when a survey prerecords respondents’ subjective self-interest in a given policy, interpretation of their responses is not straightforward.

What further complicates our interpretation of the treatment effects on self-interest is that respondents’ receptiveness to information or their likelihood of resonating with the treatment can correlate highly with common measures of labor market standing, such as education, race, and gender. [See experimental studies on empathy and altruism in psychology and behavioral economics (e.g., Andreoni & Vesterlund 2001); see also Guisinger (2017) on the malleability of trade opinion among white respondents.] Complicating the inquiry still more, education can also highly correlate with predispositions and values, such as cosmopolitanism, cultural tolerance, and openness to out-groups (Hainmueller & Hiscox 2006).

Recall the survey experiments by Rho & Tomz (2017) and Bearce & Tuxhorn (2017), discussed above. Both studies seek to test whether citizens’ economic ignorance or insufficient policy knowledge is the obstacle preventing recognition of self-interest, by randomly assigning information about who benefits and loses from trade openness and the two monetary policy options, respectively. Both find that when respondents are informed, there is a stronger concordance between respondents’ policy positions and economic models’ predictions of who should support or oppose policy according to labor market characteristics. There are two things we cannot know from the Rho & Tomz (2017) or Bearce & Tuxhorn (2017) experiments, however. One gap is due to the coarse treatment problem discussed above. For both trade and monetary policy domains, there are multiple economic models of self-interest-driven policy preferences at the factor, sector, and firm levels. The informational treatment, however, is generally a short description of the distributional implication of a trade or monetary policy within one particular economic model. For Rho & Tomz (2017), the treatment is predictions from the Stolper-Samuelson theorem: that high-skilled workers are trade winners and low-skilled workers are trade losers. The respondents’ personal experiences with or perception of distributional effects of trade might differ from the predictions offered by the Stolper-Samuelson theorem. This divergence can occur even if respondents are perfectly informed about trade models. For instance, the Ricardo-Viner model, Melitz’s (2003) model of firms and heterogeneous trade, and a model of task routine-ness (Owen & Johnston 2017) all predict the divergence of trade attitudes within low-skilled or high-skilled sets of respondents. When the Stolper-Samuelson treatment does not shift respondents’ attitudes, then, we cannot know whether respondents did not update their self-interest perception or whether they were aware of self-interest but simply did not resonate with the statement because they had experienced income shocks that differ from the Stolper-Samuelson predictions.

Similarly, for Bearce & Tuxhorn (2017), the contextual treatment is a coarse description of a trade-off between domestic monetary policy autonomy and the stability and strength of the US dollar. The treatment includes broad terms such as “economic growth,” “price,” “inflation,” and “competitiveness of export industries” but mentions almost no specific information about the distributional implications of monetary policy at factor, sector, or firm levels, except for export industries. It is no surprise, then, that respondents with businesses that “export or do business abroad” respond to the treatment, while the authors find that respondents do not shift opinions with the treatment as predicted by factor- or sector-based models.

The second problem with this design is that, given that college education and occupational standing in the global economy are key covariates, we cannot know whether respondents who
are highly skilled or employed by global firms update their understanding of self-interest with the treatment or whether they are simply more receptive to the treatment because they are better learners or test takers, as suggested by their college degrees.\(^1\) For instance, Hiscox (2006) and Ardanaz et al. (2013) both employ the Globalization-as-Treatment design to study framing effects—whether citizens shift their trade opinions when exposed to positive versus negative framing about trade’s effect in the United States and Argentina, respectively. Hiscox’s (2006) treatment is a random assignment of anti- and protrade statements, such as “Many people believe that increasing trade with other nations leads to job losses and exposes American producers to unfair competition” for an antitrade introduction. Respondents are then asked to answer the question: “Do you favor or oppose increasing trade with other nations”? He finds that the antitrade framing makes American respondents more protectionist, while the protrade framing does not move respondents’ attitudes in a protrade direction. Hiscox further finds that less educated respondents are generally more sensitive to issue framing, and in particular to the antitrade framing. These findings are consistent with the popular discourse that protectionist elite rhetoric is effective in mobilizing support for protectionism among less educated and economically insecure citizens. However, there remains a question regarding whether the large effect of antitrade framing for less educated respondents is due to their labor market standing being more vulnerable to trade shocks (i.e., low-skilled citizens are more likely to lose from free trade) or to their sensitivity to the loss framing in general (i.e., less educated respondents might be more likely to be swayed by political rhetoric).

Ardanaz et al. (2013) address this question by replicating the Hiscox experiment in Argentina, where factor endowment is the reverse of that of the United States: High-skilled labor is scarce and low-skilled labor is abundant. This allows them to separate out the effects of educational attainment and job security on respondents’ sensitivity to framing. They find that regardless of their levels of educational attainment or skills, respondents’ strength and clarity of material interests shape their sensitivity to framing. The framing is most effective among those whose economic interests are ambiguous, such as service sector workers. When respondents have clearly defined economic interests in trade, such as employment in import-competing manufacturing sectors, their trade positions are more stable and resistant to various types of framing. This finding stands in stark contrast to the popular discourse that protectionist elite rhetoric is more effective in shifting public opinion of those who suffer economically—and in contrast to the popular interpretation of Republican vote gains in the Rust Belt region during the 2016 presidential election in the United States.

In summary, these two limitations of common survey experiments in IPE—coarse treatments and receptiveness to the treatment potentially correlating with labor market characteristics of the respondents—are deeper than the issue of treatment compliance. A possible solution might be to leverage the technical advantage of online surveys in customizing and tailoring the treatment to individual respondents, just as microtargeted online advertisements or microtargeted election campaigning do. Another step forward is to oversample respondents who work in particular sectors that facilitate the assessment of how respondents perceive their occupational economic interest, as done by Hainmueller et al. (2015) and Malhotra et al. (2013). Finally, prerecording respondents’ experiences with globalization or how they evaluate their economic or cultural conditions in the past few years as much as possible before treatment might help us better substantively interpret the treatment effects.

\(^1\)This receptiveness to the treatment can also arise with the experimenter demand effect, discussed by Mummolo & Peterson (2019).
Problem II: Overattributing Globalization as a Source of Hardship

The second problem with the Globalization-as-Treatment design is that treatment conditions that vary solely in the different features of globalization do not allow us to differentiate two mechanisms underlying globalization backlash: economic/cultural hardship fueling in-group/out-group anxiety without explicit attribution to globalization (“in vs. out-group anxiety mechanism”; see Bisbee 2019 and Mutz 2018) versus economic/cultural insecurities originating in trade or immigration shocks translating into the demand for trade protectionism or immigration control via the blame attribution mechanism (I call this “policy accountability mechanism” hereafter; Naoi 2019).

Two experimental designs address this overattribution issue. The first alternative design is to use treatments that manipulate respondents’ perceived threat or benefits that seem unrelated to globalization and solicit respondents’ attitudes toward globalization; this is discussed below as the “domestic threat/benefits design.” If a heightened sense of threat alone can trigger protectionist sentiments without voters attributing the blame to globalization, a mechanism underlying backlash might be in-group/out-group anxiety. The second alternative design is to test the blame attribution process directly by randomizing actors or factors to be blamed for citizens’ economic (or cultural) losses and solicit citizen attitudes toward policies related to globalization or their support for the incumbent party or government. This is discussed below as the “blame (or credit) assignment design.”

Alternative design 1: domestic threat/benefits design. Examples of a domestic threat design include a series of studies that test how perceptions of threats that are unrelated to globalization or have unspecified sources (e.g., demographic changes and cultural changes) mobilize higher support for protectionism.

Naoi & Kume (2015) employ the domestic threat/benefit design and seek to disentangle “the duality of citizen[s’] interests” as workers and consumers in trade policy. They argue that low-income citizens in advanced industrial nations are torn between protectionism and free trade, as trade liberalization threatens their job security as workers while it benefits them as consumers through lower prices. As discussed above, they conducted a priming experiment in Japan during the Global Recession that increased the salience of the worker identity or the consumer identity by assigning photos of work sites to one group and photos of consumption sites to another, with a control group that was not shown any photos. While none of these images are directly linked to trade or globalization (e.g., the workers all appear to be Japanese and the grocery shop photo shows what appears to be domestically produced food), the consumer priming increases support for free trade by nine percentage points, and this effect is substantially higher for low-income citizens and for workers with high job insecurity.

Psychologists have conducted several survey experiments that heighten the salience of so-called demographic threat in the United States and gauge the effect of this threat perception on both race-related and race-neutral policy positions. Craig & Richeson (2014) and Major et al. (2018) conducted survey experiments in the United States in which they randomly assigned information about future demographic shifts, stating that racial minority voters will outnumber the majority (white citizens) in America. Craig & Richeson (2014) finds that heightening the salience of a demographic shift in the treatment makes white and independent Americans lean toward Republican candidates and political conservatism. Major et al. (2018), during the primary campaign period for the 2016 election, randomly assigned information reminding white Americans with high ethnic identification that minorities will outnumber whites by 2042. The treatment made these respondents support candidate Donald Trump and anti-immigration policy more, and oppose political correctness more.
Singer & Quek (2017) exploit the difference in citizens’ attitudes toward internal versus international immigration in China to test the labor market competition hypothesis. Their survey experiment randomly assigned treatments that describe immigrants with different skill levels who are either from foreign countries or from other provinces in China. They find that cleavages form according to immigrants’ described skill levels, not along the foreign–internal dimension. However, they further find that natives prefer high-skilled immigrants when they are foreign and low-skilled immigrants when they are internal. The results overall reject the labor market competition hypothesis as well as the cultural threat hypothesis. The findings from the interaction of the two dimensions, however, suggest that low-skilled immigrants from foreign countries are also double-penalized in a country with an abundance of low-skilled domestic workers such as China, which might lend suggestive support to the sociotropic hypothesis.

These studies together suggest that domestic threats unrelated to globalization can trigger protectionist sentiments and that globalization backlash might originate from cultural and status threats that are not explicitly linked to globalization. Moreover, globalization backlash can arise without sophisticated voters who can assign blame for their economic or cultural hardship to globalization and/or elected officials.

**Alternative design 2: blame (or credit) assignment design.** The second alternative design is to test the blame attribution assumptions directly by randomizing actors or factors to be blamed for citizens’ economic (or cultural) losses and gauge protectionist attitudes or their support for the incumbent.

The advantage of this design is that the treatment does not posit globalization as the only source of economic losses (or benefits). Rather, an experimental treatment primes respondents to think about different origins of economic or cultural losses, including options to blame globalization, elected officials, or themselves. This design allows scholars to test how citizens assign blame and whether policy accountability or the out-group anxiety mechanism is more dominant among various subgroups.

In one example of the blame assignment design, Hellwig et al. (2008) conducted a set of survey experiments of nationally representative Americans that randomly added the option to blame “national and international business cycles” for macroeconomic outcomes, along with the consistent multiple-choice options of “Congress,” “president,” “working people,” and “business people.” In groups that were assigned the additional option to blame business cycles, the proportion of respondents who blamed Congress and the president decreased by 29 percentage points, and 35.7% of respondents chose to blame business cycles. Furthermore, educated respondents were more likely to blame business cycles than elected officials, while less educated respondents were more likely to blame elected officials. Finally, the authors find that Republican supporters are more likely to blame the market (i.e., business people) for economic downturns than Democrats, who are more likely to blame elected officials.

Some of these findings are consistent with the backlash we see today among citizens with lower levels of education but suggest a different mechanism than simple pocketbook voting. Less educated citizens might be holding elected officials more accountable for their economic hardship regardless of its origin, while citizens with higher levels of education might blame market forces or multiple actors [“diffused attribution” (Hellwig et al. 2008, p. 858); also see Gomez & Wilson (2001, 2003)] as causes of their hardship. Yet, Hellwig et al.’s (2008) findings on the partisan pattern of blame attribution suggest a sea change since 2005–2006, when they conducted their study. Their work would predict that Democratic supporters will hold the government accountable for their economic hardship precisely because of their belief in the larger role that government plays and should play in the economy. Margalit’s (2013) study corroborates this finding: Republican
respondents who lost their jobs during the Great Recession were more likely to become Democratic supporters, but there was no tendency for Democrats who lost their jobs to vote Republican. According to these studies, economic hardship should increase support for left-, not right-leaning parties.

Tella & Rodrik (2019) also employ the blame assignment design. As discussed above, they conducted a survey experiment in 2018 with a convenience sample in the United States, positing different reasons for a manufacturing plant closure in a news clip. The reasons for the plant closure included a technological change, demand shift, bad management by the plant, and three kinds of international outsourcing: outsourcing to a developed economy, a developing economy, and a developing economy with bad labor standards. With the exception of the “bad management shock,” the demand for trade protection increases across all causes of job losses, regardless of policy or nonpolicy origins. Among the three treatment groups, the evidence suggests that plant closure due to outsourcing generates the highest support for trade protection, compared to nontrade-origin shocks such as technological changes or bad management. Overall, these results suggest that information about income shocks unrelated to trade but related to market changes (e.g., technological change and demand shift in this example) can trigger demands for protectionist policies. Note that this preference for protectionism is derived from information about an adverse income shock to others (in a news clip) and that the effectiveness of trade protection in reducing or preventing offshoring has been hotly debated among economists (Antràs & Staiger 2012, Díez 2014).

Problem III: Constraints on Government and Room-To-Maneuver Beliefs

Another missing inquiry in the Globalization-as-Treatment design is whether and how citizens’ heterogeneous beliefs about external constraints on domestic government mediate the link between economic hardship and protectionist attitudes. Citizens’ belief that their government can control globalization is often an assumption underlying the link between citizens’ economic hardship and their demand for government protection, yet the extent to which citizens hold this belief varies across countries and over time. Hellwig et al. (2008) show through a series of existing public opinion surveys that American respondents are generally more likely to blame elected officials, such as Congress and the president, for macroeconomic outcomes (64%) than their Western European counterparts. In contrast, the majority of European respondents in a 2001 survey agreed with the statement that “globalization cannot be controlled by governments” (Hellwig et al. 2008). Naoi (2019) original surveys in Japan and the United States show that US respondents are three times more likely to report that their government has “large” or “very large” influence over the contents of a trade agreements (53%) than Japanese respondents (17%). This implies two things. First, the United States, where the majority of voters believe that their government can control globalization, might be more susceptible to voter backlash against globalization than Western Europe or Japan. Second, we should thus expect the policy accountability mechanism of voter backlash to be more prevalent in the United States than in Europe or Japan.

An alternative design to probe these questions is to randomize information about external constraints on domestic government and analyze how this information can shift citizens’ assignment of blame for economic hardship and lead to different attitudes toward policies related to globalization or their support for the incumbent.

Hellwig et al. (2008) test how manipulating citizens’ beliefs about the room to maneuver—how much their government can control macroeconomic outcomes—influences citizen support for an incumbent party or government or their assignment of blame/credit for economic conditions. They randomly assigned a preface, “In terms of trade and finance, the United States is now deeply involved in the world economy,” and then followed up with the question: “In view of this, who is
most responsible for the economic conditions in our country in the last few years, the Congress, the president, working people, or business people?" Some experimental groups received an additional option for blame, “national and international business cycles,” as discussed above. The authors find that this no-room-to-maneuver treatment does not change the distribution of opinions about who is responsible for macroeconomic outcomes in the American sample. The important implication of this finding is that American voters are more likely to believe in their government’s ability to control the economy and expect legislators and the president to adopt policy changes to address their economic hardship. And this belief is quite stubborn.

Kosmidis’s (2018) survey experiment conducted in Greece during the debt crisis also employs the room-to-maneuver design. He randomly assigned two informational treatments, one with the preface that the International Monetary Fund will design and implement economic policies in Greece (no room to maneuver) and another with the preface that elected officials in Greece will design and implement economic policies in the next few years (room to maneuver). A control group received no preface. Kosmidis finds that manipulating room-to-maneuver perceptions does not change the prevalence of economic votes. Communicating that Greek elected officials have room to maneuver does not increase the association between respondents’ economic evaluations and expressed support for incumbent parties. The finding is contrary to the expectation that economic votes would be less prevalent in the no-room-to-maneuver treatment group than the control group, and more prevalent in the room-to-maneuver treatment group.

This paper raises an important question about whether we should consider citizens’ (retrospective or prospective) economic evaluations as endogenous to their beliefs about who controls the economy or exogenous to these beliefs. This is ultimately an empirical question. Studies have shown that citizens expect different sets of winners and losers when the International Monetary Fund, as opposed to domestic governments, controls reforms (Vreeland 2003). Emerging studies on economic votes in comparative and American politics have also demonstrated that citizens exhibit partisan bias in economic evaluations (Gerber & Huber 2010, Bullock et al. 2015).

The distinction between endogenous and exogenous evaluations is important substantively as well as for experimental designs. The two theories require different sequencing of treatments and economic evaluation questions in the experiment. Kosmidis’s (2018) survey asks an economic evaluation question after the treatment, which means that he considers economic evaluation to be potentially malleable (or endogenous) to treatments. If this is indeed the case, respondents’ evaluation of the economy is a mediating factor between the treatment and outcome, and experimenters should use mediation analysis (Imai et al. 2011) instead of treating economic evaluation as a prerecorded subgroup (see Montgomery et al. 2018 on the danger of treating post-treatment attitudinal variables as conditioning variables). Kosmidis (2018) justifies the sequence by showing that economic evaluation does not differ across experimental groups, yet this is a second-best alternative to either asking the economic evaluation question before the treatment or using the evaluation question as a mediating variable. Unless we have a clear idea about endogeneity versus exogeneity between citizens’ perceptions of who controls the economy and citizens’ economic evaluations, the best course of action would be to ask economic evaluation questions both before and after the treatment to establish whether treatments shift economic evaluations.

Kosmidis’s (2018) paper reminds us that, to the extent that debates about globalization take place in domestic politics, we must address partisan and ideological heterogeneity in voters’ blame and credit attributions and evaluations of the economy. In politically polarized environments, in particular, zbo endorse an opinion can have substantial effects on voter attitudes even if we hold the content of the opinion constant.

Fernández-Albertos et al. (2013) employ a variant of the blame assignment design in an experiment that randomly varies an endorser (incumbent versus opposition party and nonpartisan
sources) and framing of six statements about who is responsible for a given crisis in Spain (the Spanish government, Spanish banks, Spanish labor market regulation, the Euro, foreign investors, or European governments). In their analysis, they find that right-leaning People's Party supporters blame domestic governments and labor market regulations more, and left-leaning supporters of the Spanish Socialist Workers Party (PSOE) blame Spanish banks, foreign investors, and European governments more. These patterns are consistent with each respective party’s ideological platform. Yet, a treatment where the PSOE endorses globalization as the source of the crisis shifts incumbent party (PSOE) supporters to be even more likely to blame globalization for the crisis. This study helps us reconcile mixed evidence from observational studies regarding whether globalization weakens or strengthens the economic vote (Duch & Stevensen 2008; see Kayser & Peress 2012 and Campello 2015 for counterarguments). Partisan heterogeneity in blame and credit attribution can explain why globalization weakens the economic vote for some governments but not others.

While much of the published work using survey experiments in IPE focuses on advanced industrialized nations, expanding the scope to developing economies can give us very different insights. In developing countries where citizens perceive their government to be corrupt or incompetent, constraints imposed on the domestic government by multilateral organizations or a foreign government are a welcome feature of globalization. Findley et al. (2017) conducted coordinated survey experiments questioning Ugandan members of parliament and voters on their support for foreign aid projects. Their main question is whether voters prefer aid projects distributed and managed by multilateral organizations, such as the World Bank, over the domestic government’s projects (and why). The first innovation of the authors’ design is to randomly assign treatment conditions where either the Ugandan government or the World Bank provides projects, probing how “foreignness” of publicly funded projects differs from domestic projects in the eyes of voters. A second innovation is to conduct the same experiment with elected officials and voters, which allows the authors to further test why mass preferences differ from elite preferences. Findley et al. (2017) find that Ugandan voters prefer multilateral aid projects over those of the domestic government, as the former are more effective and less corrupt, while Ugandan legislators prefer domestic government projects over multilateral projects. This experiment lends strong support to the idea that who controls and manages projects has a substantial effect on citizen support for the policy.

These new experimental findings encourage us to revisit the well-established argument that adverse income shocks, originating from globalization, produce demand for protectionism. Income shocks alone, it seems, are neither a necessary nor a sufficient condition for backlash to emerge. How citizens attribute the blame of economic hardship, whether they believe that their government can control and manage globalization, and what they believe regarding the deservedness of protection can have substantive effects on how hardship translates into backlash. These citizen beliefs are all deeply rooted in domestic politics.

CONCLUSION

This article has reviewed the cumulation of evidence from survey experiments in the field of IPE and discussed their strengths and weaknesses in explaining the backlash against globalization. Evidence from survey experiments suggests that much of the globalization backlash we witness today is deeply rooted in domestic politics. Income shocks or economic hardship alone is often not sufficient or even necessary for public support for protectionism to emerge. Even when blame is clearly attributed to globalization, income shocks are filtered through partisan competition, ideological battles over who deserves protection more than others, and existing (or lack of) welfare programs, and these factors produce a variety of backlash responses across time and space.
In concluding, I discuss several conditions common in politically polarized environments that complicate theory testing with survey experimental data. The first condition is respondents’ pre-treatment exposure to information—that respondents are already exposed to similar information or treatments in the real world before participating in experiments, making the effect of treatment on public opinion marginal or null. The paradox here is that the more exposed respondents have been to similar information or stimuli before experiments, the more marginal or null the estimated treatment effects will be, because a control group has essentially been treated before the experiment. A sensible effort to address this concern is to record respondents’ exposure to information, policy knowledge, and baseline attitudes as much as possible before treatments.

The second condition that can compromise theory testing with experimental data is the predispositions of respondents. In a politically polarized world where globalization is a divisive and highly salient issue, we need to be careful about wording and phrasing of survey instruments to avoid knee-jerk partisan responses, unless scholars are interested in gauging such biases (see Mansfield & Mutz 2013, Naoi et al. 2019). Given hard-to-move respondents, scholars need to carefully interpret the substantive meaning of treatment effects and null results. Again, prerecording respondents’ predispositions and sources of such predispositions as much as possible before treatments is an important step forward.

Survey experiments have much promise to advance our understanding of the globalization backlash, but they require proper designs and inference as well as a nuanced understanding of domestic politics.

DISCLOSURE STATEMENT

The author is not aware of any affiliations, memberships, funding, or financial holdings that might be perceived as affecting the objectivity of this review.

ACKNOWLEDGMENTS

I thank Dan Butler, Seth Hill, D.G. Kim, Stephanie Rickard, Peter Rosendorff and Ken Scheve, and an anonymous reviewer for helpful discussions; I thank Nate Mariano and Charlie McLean for excellent research and editorial assistance.

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