American Political Science Review (2024) 1–23
doi:10.1017/S0003055424000571

Policy Impact and Voter Mobilization: Evidence from Farmers' Trade War Experiences

JAKE ALTON JARES *Stanford University, United States*
NEIL MALHOTRA *Stanford University, United States*

How does the extent of policy benefits—not simply their presence—affect political engagement? While fundamental to understanding the electoral implications of economic policymaking, addressing this question is challenging due to the difficulty of measuring individual voters' policy outcomes. We examine a natural experiment embedded in President Trump's Market Facilitation Program (MFP), which aided a core Republican constituency: farmers harmed by his 2018 trade war. Due to idiosyncrasies of program design, the MFP undercompensated some farmers for their trade war losses—and significantly overcompensated others—based solely on their 2018 crop portfolios. Analyzing over 165,000 affected voters, we show that improved compensation outcomes had negligible impacts on Republican farmers' midterm turnout and campaign contributions, even though such variation in benefits significantly affected farmers' propensity to view the intervention as helpful. This null result is important—our estimates suggest that even highly salient variation in policy outcomes may have limited mobilizing capacity.

INTRODUCTION

An enduring question in political behavior research concerns what motivates citizens to become politically engaged. This issue is not purely theoretical; politicians and campaigns invest millions of dollars in mobilizing supporters. As American politics has become more polarized and the number of swing voters has decreased, there has emerged an increased emphasis on boosting turnout among the base instead of persuading voters to change their minds (Panagopoulos 2016). It is therefore not surprising that there is an extensive literature exploring campaign strategies to convince people to participate in elections, largely driven by the methodological advance of field experimentation (Green and Gerber 2019).

Yet incumbent politicians can do much more to mobilize voters outside of campaigns; they can use policy instruments to provide resources to voters, which in turn may affect political engagement. This study examines the mobilizing effects of one such policy instrument: Donald Trump's Market Facilitation Program (MFP), through which agricultural producers harmed by the 2018 U.S.–China trade war received direct monetary payments to compensate for tariff-induced price declines. We gauge the extent to which variation in the joint economic impact of the trade war and MFP affected voter turnout and campaign contributions among this core Republican constituency. Our research design relies on a natural experiment that induced substantial farm-level variation in compensation outcomes. As we explain below, some farmers were overcompensated by the MFP—while others were left undercompensated—in accordance with planting decisions made *before* retaliatory tariffs emerged on the political horizon.

The strength of policies' mobilization effects may be of first-order importance for distributive politics in the United States, as incumbents in various institutional settings tend to steer disproportionate economic policy benefits toward reliable partisan allies (Ansolabehere and Snyder 2006; Kriner and Reeves 2015; Nicholson-Crotty 2015). Given the relative stability of U.S. party coalitions, such "core voter" targeting might seem difficult to square with incumbents' electoral motivations: how many votes can be gained by delivering good policy outcomes to a committed partisan? For this reason, Cox (2009) argues that it is important to incorporate voter mobilization into canonical theories of distributive politics. While it might not be credible for a Republican voter to threaten a Republican incumbent with a vote for their Democratic challenger, turnout itself is costly, and abstention may well be a rational response to poor policy outcomes. Cox (2009) thus notes that the prospect of affecting turnout reduces the "tension between the goals of maximizing votes and serving the interests of core voters" (343).

These issues have recently come to the fore in debates regarding President Biden's economic policy agenda, as politicians and pundits alike have frequently posited that delivering programmatic benefits is critical to maintaining the engagement of core voters. In particular, progressive voices have forcefully argued that the *extent*—not just the presence—of such policy impacts is critical to

Jake Alton Jares, PhD Candidate in Political Economy, Stanford Graduate School of Business, Stanford University, United States, jjares@stanford.edu.
Corresponding author: Neil Malhotra, Edith M. Cornell Professor of Political Economy, Stanford Graduate School of Business, Stanford University, United States, neilm@stanford.edu.

Received: November 02, 2022; revised: June 07, 2023; accepted: April 24, 2024.

motivating the base and that compromise positions will come at a steep cost in voter engagement (Nova 2022). For example, in August 2022, President Biden announced that he was fulfilling a campaign promise by unilaterally forgiving $10,000 in student loans for most borrowers. While this move received both praise and criticism based on its policy substance, it also generated significant debate over its turnout impacts relative to alternative policies, such as proposals to instead cap relief at $50,000, or eliminate all outstanding debt altogether.

A presumption underlying this sort of electoral appeal for more generous policies is that the political engagement of the incumbent's base increases in accordance with the value of the policy impacts they experience. There are many theoretical reasons why we might expect such a relationship. Along the lines emphasized thus far, voters may express gratitude knowing that government works for them. Formal models of voter mobilization proposed by Cox (2009) and Chen (2013) posit that voters hold incumbents accountable for delivering policy benefits by turning out at higher rates to reward a high-performing copartisan or punish a low-performing out-partisan. However, on a more basic level, policies that provide financial resources to voters can also equip them with the budgetary slack to contribute to campaigns. Easing financial constraints can also free up time to develop civic knowledge and skills and to spend time voting (Verba, Schlozman, and Brady 1995). Finally, there is an extensive literature on policy feedback, which argues that public policies create constituencies who are motivated to participate in politics to defend those programs (Campbell 2012).

Of course, this is fundamentally an empirical question, and indeed one that poses numerous data challenges that have plagued prior research. It is difficult to obtain large-scale administrative records on both policy outcomes and voter engagement. Some papers rely on surveys to ask people about these variables, but reporting bias and nonresponse bias present well-documented issues (Dahlgaard et al. 2019). Voters most likely to be influenced by policies because they see them in a positive light are also the ones most likely to remember their participation and report it in a survey. They are also more likely to overstate the magnitude of the benefit. Voter turnout is systematically overreported in surveys and nonresponse can make obtaining representative samples difficult. Survey data are often small in size, posing challenges to statistical power and the ability to measure effects precisely. Finally, prior research has faced challenges with causal identification given that the scale of program benefits is almost never randomly assigned.

In this study, we address each of these challenges by tying together large-scale administrative records on policy outcomes and political engagement. As previously noted, we assess the joint impact of two innately connected policy events: Donald Trump's 2018 trade war with China, and his MFP, through which agricultural producers harmed by the trade war received direct monetary payments to offset revenue losses. Given the ability of U.S. presidents to take unilateral action on trade policy if they can at least vaguely point to a national security interest, Trump was able to both implement tariffs—as well as dole out MFP payments—without congressional approval. We examine how variation in the joint economic impact of these events affected voters' (a) turnout in the 2018 midterm elections and (b) campaign contributions throughout late 2018 and early 2019.

Although Trump himself was not on the ballot in 2018, the midterm election was widely viewed as a referendum on his presidency; indeed, Jacobson (2019) finds that voters' assessments of the president in 2018 held a nearly unprecedented level of centrality in determining their midterm vote choices. As losing control of Congress would make it difficult for him to implement his agenda going forward, Trump campaigned extensively on behalf of candidates during the election and promoted the MFP to agricultural constituencies (Eller 2018). Likewise, Chinese leadership electorally targeted retaliatory tariffs at the district level, suggesting that they believed voters would tie the trade war to the Republican brand generally (Kim and Margalit 2021).

Our study is composed of three analyses of policy impacts and voter engagement. In our first analysis, we estimate farm-level trade war losses—and thereby, overall compensation outcomes—for over 165,000 registered voters, allowing us to obviate ecological inference problems and estimate turnout and contribution effects with a precision well beyond the scale of prior research. We find negligible effects of improved compensation outcomes on Republican and non-Republican turnout and contributions alike. In our second analysis, we use a survey of Midwestern farmers during the trade war to demonstrate that these null results are not due to a lack of awareness of individual policy outcomes. Indeed, we find that farm-level variation in economic benefits significantly impacted farmers' perceptions of the helpfulness of the MFP. In our third analysis, we look beyond the population of agricultural producers and examine whether exposure to a major policy shock might affect political engagement beyond the extent to which individuals experience monetary gains and losses. We compare the turnout and contribution propensities of farmers affected by the trade war with those of the broader 2018 electorate and thereby assess the effect of overall policy experiences rather than the level of benefits *per se*. Our results suggest that the effects of economic policies on voter behavior may not adhere consistently to a conventional political accountability dynamic of turning out (or abstaining) to reward (or punish) an incumbent. Instead, the political engagement of MFP-eligible farmers in 2018 broadly increased relative to the general population in a manner that cut across individual-level policy experiences.

These results also speak to the long-standing literature in political science on agrarian political behavior (Campbell et al. 1960; Lewis-Beck 1977).[1] Better

[1] Our study is also somewhat related to prior articles that have used aggregate-level data to examine the effect of Trump's trade policies on vote choice (Blanchard, Bown, and Chor 2019; Chyzh and

understanding rural Americans is important to contemporary political science given this population's overrepresentation in American political institutions, as well as the changing nature of geographic polarization (Rodden 2019). Early research conceived of agricultural producers as "pocketbook" voters whose political behavior was driven by personal economic circumstances, particularly those related to stress. As described in *The American Voter*, "as economic pressure on the farm increases, the political involvement of the farmer … increases as well. The suggestion is obvious that short-term economic pressures lie behind the spurts in voter turnout that mark the farm vote" (421). This would suggest that political engagement would be sensitive to the individual-level impacts of distributive economic policies such as the MFP. However, more recent research on rural politics conceives of rural voters being more defined by geographic and cultural identity rather than economics (Cramer 2016; Jacobs and Munis 2023), suggesting that political involvement is mostly driven by post-materialist concerns. Our findings are much more consistent with these accounts than with traditional conceptions of the "pocketbook farm vote."

Our main null result thus advances two important lines of political science scholarship. As even highly salient variation in policy outcomes can have a negligible capacity for mobilizing core voters, scholars of distributive politics may need to undertake a deeper investigation of the political incentives that give core voter targeting its well-documented prominence within economic policy design. Likewise, our rebuttal to the classic narrative of the "pocketbook farm vote" suggests that the fundamental shift in rural political behavior in recent decades might extend beyond the widely recognized partisan realignment.

INSTITUTIONAL BACKGROUND

In late January 2018, President Trump invoked the rarely used "safeguard investigation" trade authority to unilaterally raise tariffs on imported solar panels and washing machines. This marked the beginning of a quickly escalating trade conflict between the United States and China. In early July, the Chinese government implemented retaliatory tariffs on nearly all U.S. agricultural products.[2] The Trump administration responded by authorizing billions in direct payments to affected farmers via the MFP. Payments were distributed in three tranches in late 2018 and early 2019 (which we refer to below as "the MFP" or "the 2018 MFP"). A second series of payments was issued via a sequel program ("the 2019 MFP") in 2019 and 2020.

The MFP was announced in a July 24, 2018 press release that listed seven covered commodities and a $12 billion payment cap that was targeted at relieving "unjustified retaliatory tariffs" that caused an estimated $11 billion reduction in agricultural export value. Concrete program details, including commodity-specific payment rates, were announced on August 27, with the USDA's Farm Service Agency (FSA) taking enrollments starting September 4. Five major field crops (corn, sorghum, soybeans, wheat, and cotton) ultimately earned the lion's share of MFP payments, and are the focus of this study.[3]

The USDA calculated commodity-specific payment rates by simulating the expected decline in export value to trade war participants with a global trade model, and then divided this quantity by total 2017 U.S. production of the given commodity.[4] The resulting ratios yielded payment rates that were 0.3%, 9.2%, 23.9%, 16.5%, and 2.8% of the May 2018 forecasted prices for corn, cotton, sorghum, soybeans, and wheat, respectively. The gap between corn and soybeans, the two largest U.S. crops, was especially notable but clearly reflected the trivial share of corn production exported to China in previous years.

The subtleties of this program design ultimately provided significant variation in constituents' policy experiences. In directing the Secretary of Agriculture to devise a relief package well before the effects of retaliatory tariffs were known, Trump was able to flaunt a $12 billion price tag just 2 weeks after the 25% tariff took effect on July 6.[5] While the USDA's damage methodology proved expedient, it did not accurately and consistently compensate agricultural producers for their losses. Farmers' actual take-home pay depends on the *prices* they sell their commodities at, not on any changes in national bilateral export value. Indeed, Janzen and Hendricks (2020) and Adjemian, Smith, and He (2021) argue that realized price impacts diverged substantially from the MFP's measure of trade war damage, both because U.S. producers were able to find alternative trading partners, and because of cross-price elasticities between commodities (particularly corn and soybeans).

Starting with the eight papers reviewed by Janzen and Hendricks (2020), we identified ten studies by

Urbatsch 2021; Gulotty and Strezhnev 2024; Kim and Margalit 2021). We connect our findings to this literature in the discussion.

[2] The net reduction in Chinese imports of U.S. agricultural products dwarfs the effects of other countries' retaliation (Regmi 2019), thus meriting a focus on bilateral conflict between the United States and China.

[3] Of the $8.6 billion ultimately distributed through the 2018 MFP, 95% went to the five major field crops, with 4% going to hogs and dairy. The remaining 1% went to almonds and sweet cherries via a "specialty crops" category added on September 21.

[4] For example, the USDA's September 2018 MFP white paper describes the sorghum rate determination in terms of three data points: China imported $956 million of sorghum from the United States in 2017, trade model simulations yield expected 2018 imports of $642 million, and 2017 sorghum production was 364 million bushels. The resulting MFP rate equals ($956 million – $642 million) / (364 million bushels), or $0.86 per bushel (USDA Office of the Chief Economist 2018).

[5] Moreover, the July 24 announcement was reportedly moved up several weeks earlier than originally planned (Abbott 2018), and to just two days before Trump spoke at an event in Iowa in which he distributed "Make Our Farmers Great Again!" hats and defended his trade policies (Eller 2018).

TABLE 1. MFP Compensation Levels by Crop

	Tariff price impact	MFP rate	Net benefit	Compensation ratio
	(A)	(B)	$(A+B)$	$\lvert B/A \rvert$
Corn	–2.9%	0.3%	–2.6%	0.09
Cotton	–1.5%	9.2%	7.7%	5.98
Sorghum	–7.6%	23.9%	16.3%	3.15
Soybeans	–9.1%	16.5%	7.4%	1.82
Wheat	–2.5%	2.8%	0.3%	1.11

Note: Tariff price impacts (*A*) and MFP payment rates (*B*) are expressed relative to the USDA's May 10, 2018 forecast of the 2018/2019 marketing year price.

agricultural economists estimating the price impacts of the 2018 retaliatory tariffs (see Section A.4 of the Online Appendix for additional details). We average the estimated tariff-induced price declines among these studies and compare them to the commodity-specific MFP rates in Table 1. In line with with the conclusions of Janzen and Hendricks (2020), these calculations imply that cotton, soybeans, and sorghum were each overcompensated by the 2018 MFP, while corn was undercompensated.

Given the relative lateness of these events within the growing season, farmers experienced both tariff-induced price declines and MFP compensation as exogenous income shocks that they were unable to plan around for the 2018/2019 crop marketing year. By the time China announced tariffs in the middle of June, nearly 100% of the corn crop had already been planted, as well as 97% of the soybean crop, 96% of the cotton crop, and 89% of the sorghum crop; the harvest was already underway for winter wheat (the predominant wheat variant) (USDA National Agricultural Statistics Service 2018). Even the most sophisticated market actors appear to have been surprised by the initial tariff announcement, as soybean futures prices did not begin their tumble until June (see Figure 1), and U.S. soybean export prices did not diverge from Brazilian export prices until the date of the tariff announcement (Regmi 2019). Indeed, aggregate planting data confirm that farmers of affected crops were "locked-in" to their pre-trade war choices for the 2018/2019 marketing year. For corn, soybeans, and wheat—the three largest field crops by far—actual planted acreage in 2018 amounted to 98%, 98%, and 106%, respectively, of projections the USDA completed in January 2018.[6]

Variation in farmers' policy outcomes in 2018 was therefore both unanticipated and imposed through idiosyncratic treatment of particular crop portfolios. As such, it presents a valuable test case for evaluating the salience of policy outcomes for partisan mobilization. Through press releases, interviews, and campaign events, President Trump repeatedly took credit for both the trade war and his MFP. As we show in Dataverse Materials Section C (Table DM1 and Figure DM6), the farmers who produced affected commodities were overwhelmingly within Trump's Republican base, and yet incurred drastically different policy outcomes for the 2018/2019 marketing year based solely on apolitical, pre-trade war planting decisions.[7]

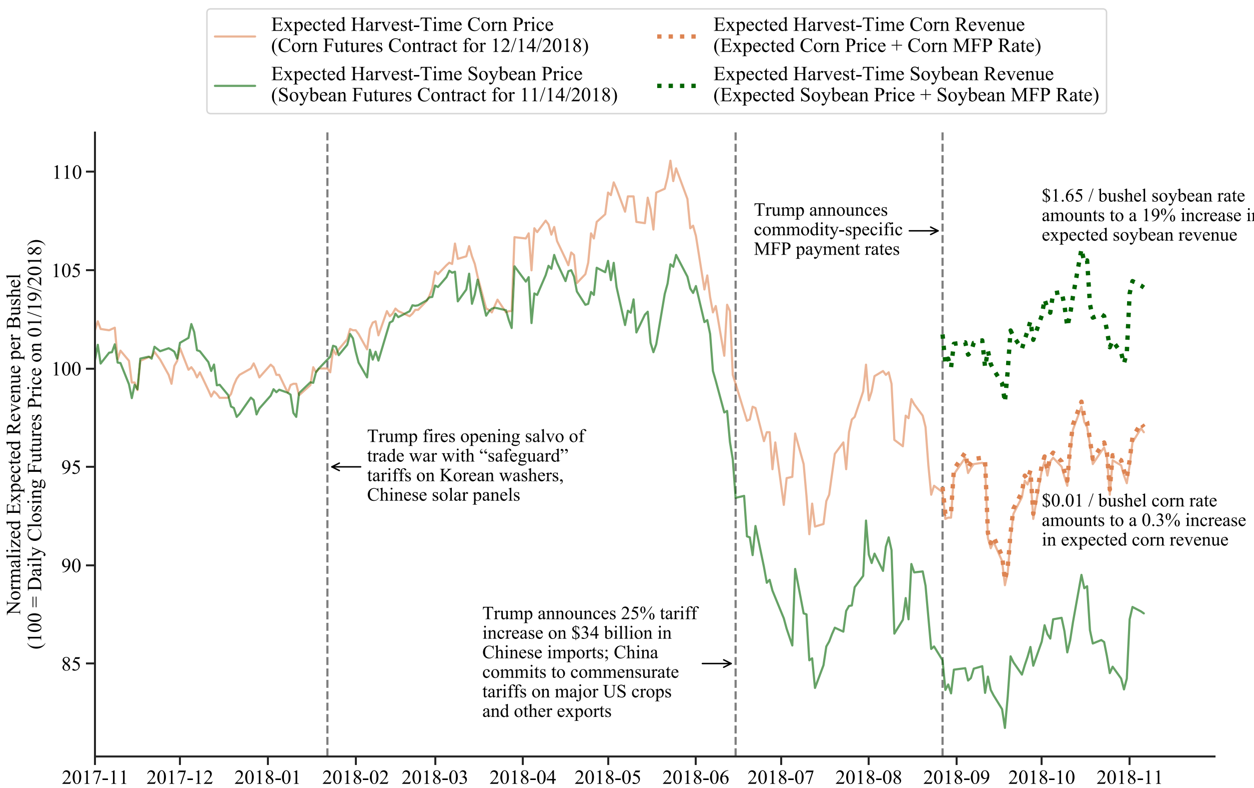
Many farmers in 2018 had spent the past three years consistently struggling to break even under prevailing commodity prices, and 2018 would have been a year of tight margins regardless of the trade war. Figure 2 illustrates the contributions of trade war-induced price declines and MFP payments to farm profits using a model Iowa farm that planted the modal 50/50 corn–soy split in 2018. As shown, given the high cost of agricultural inputs, even seemingly modest price shocks—such as the estimated 2.9% decline in the corn price noted in Table 1—can significantly affect a farmer's net income. Indeed, in our illustrative example, tariff-induced price declines and MFP payments accounted for 27% and 37% of net farm income, respectively.

As such, the crop-level differences depicted in Table 1 were quite meaningful to individual farmers' bottom lines. Befitting its MFP windfall, the American Soybean Association responded to the second 2018 MFP tranche in mid-December with a press release titled "Soybean Farmers Thankful for Final Installment of Market Facilitation Aid" that featured a similarly positive quote from the association president (American Soybean Association 2018). However, as the only clear net-losers from the 2018 MFP formula, the National Corn Growers Association released a statement with the subtitle "USDA Trade Aid Comes Up Short, Again" and an expression of "disappointment that corn farmers impacted by trade tariffs and ongoing trade uncertainty would receive virtually no relief." The association president complained that the $0.01 per bushel rate was "woefully inadequate" and accused the USDA of failing to craft a policy that

[6] For contrast, actual planted acreage of these crops in 2019 was 100%, 84%, and 99% of 2019 acreage forecast by the same report, suggesting that farmers significantly shifted away from soybeans once they had the flexibility to do so. This endogenous response to the 2018 MFP, coupled with program changes to the 2019 MFP that mitigated these crop-level discrepancies, means that we cannot leverage the natural experiment to study voting behavior in the 2020 election. See USDA Agricultural Projections to 2027, 2029, and 2030, available at https://www.ers.usda.gov/publications/pub-details/?pubid=87458.

[7] Due to space constraints in the formal Online Appendix, we present additional supplementary information and analyses in the Dataverse Materials (see Jares and Malhotra 2024).

FIGURE 1. Changes in Harvest-Time Revenue Expectations for Corn and Soybeans across Key Phases of 2018 Trade War, Based on Futures Prices and Announced MFP Payment Rates



Note: The figure presents daily closing prices through Election Day 2018 for harvest-time corn and soybean futures contracts, as well as the sum of each commodity's futures price and MFP payment rate. Each series is normalized to take a value of 100 on January 19, 2018, the last trading day prior to Trump's initial safeguard tariff announcement. Cited percentage increase in revenue from MFP rates is calculated by dividing each rate by the futures price on August 27, 2018 (the date on which the payment rates were announced).

addressed realized harms from the trade war (National Corn Growers Association 2018).

DATA ON POLICY OUTCOMES, VOTER TURNOUT, AND CAMPAIGN CONTRIBUTIONS

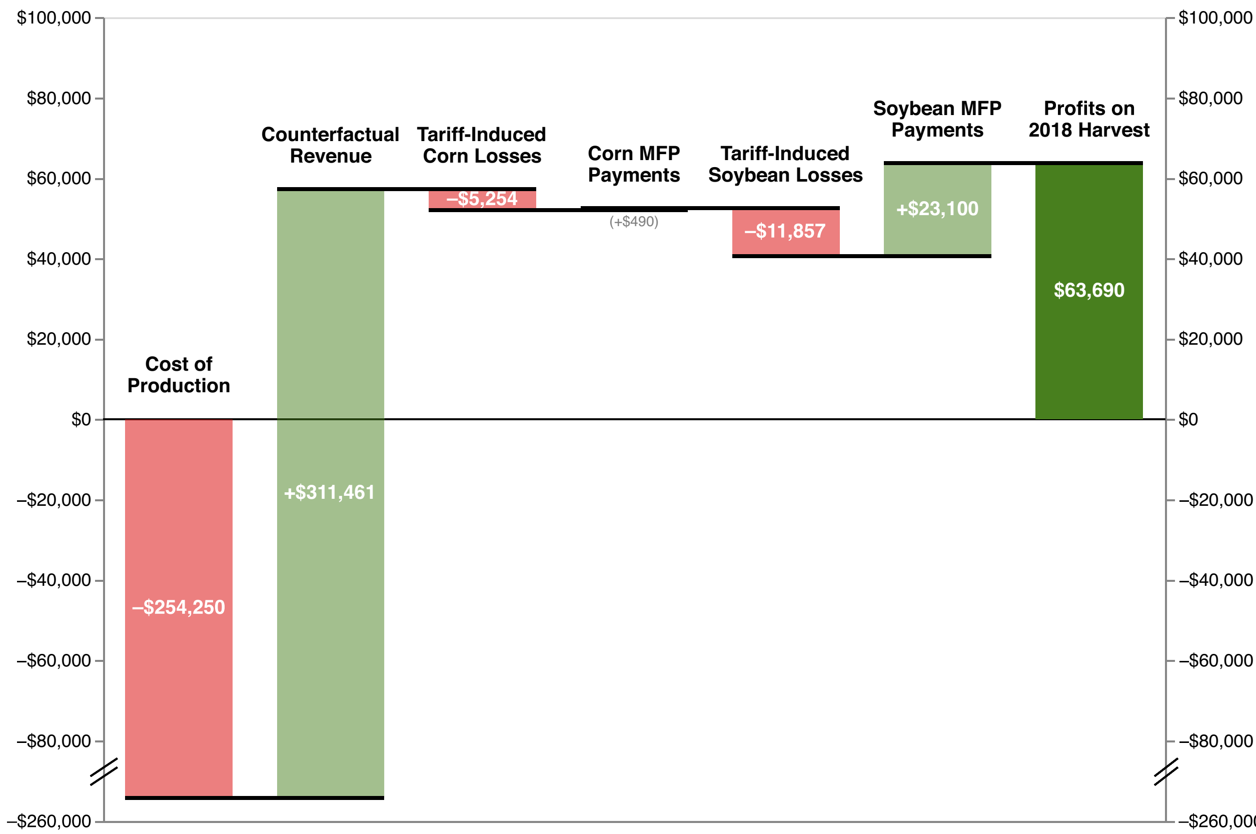
Our empirical analysis of voter engagement relies on transaction-level MFP payment data linked to (a) a series of voter file snapshots for each of the 50 states and (b) campaign contributions from the Database on Ideology, Money in Politics, and Elections (DIME) (Bonica 2014). Through a series of FOIA requests to the Farm Service Agency, we obtained nearly the universe of USDA farm program transactions (including MFP payments) for 2004–2020. Given information on the names, addresses, and ownership relationships of the several million individuals and businesses featured in this database, we employed a bespoke entity resolution algorithm to cluster recipients into groups that generally reflect distinct farming households.[8] For brevity, we refer to such clusters as "farms" in the following discussion, and we render all measures of farm program payments and trade war outcomes at the farm level.

Our analysis of policy outcomes relies on inferring farms' 2018 crop portfolios from their MFP payments and known commodity-specific payment rates. As such, we draw the corresponding sample of farms from a special commodity-by-commodity tabulation of calendar-year 2018 enrollments in the 2018 MFP. Using two sources of auxiliary information, we carefully select farms from this database for which payment information allow us to confidently infer 2018 harvest records—and thereby ensure minimal measurement error in estimating farm-level trade war outcomes. We likewise limit our main analysis to farms with at least 10 acres of cropland, thereby ensuring that the 2018 policy shock was economically meaningful to the voters we study (see Section A.1 of the Online Appendix for a discussion of these aspects of our sample construction).

We link the resulting sample of farms to several snapshots of a national voter file from the vendor L2 using a highly customized probabilistic matching algorithm. For a detailed description of our entity resolution and record linkage algorithms, see Dataverse Materials Section E. By using voter file snapshots from both February 2018 and June 2019, we obtain farmers'

[8] See Dataverse Materials Section E for details.

FIGURE 2. Contributions of Trade War and MFP to Farm Profits on a 500-Acre Iowa Farm Planting a 50/50 Corn–Soybean Split on 250 Operator-Owned Acres and 250 Rented Acres



Note: See Dataverse Materials Section A for further details on this figure, including cost and revenue assumptions and a discussion of representativeness. Note that the implied crop-specific compensation rates differ slightly from those depicted in the fourth column of Table 1 due to the use of actual Iowa marketing year prices.

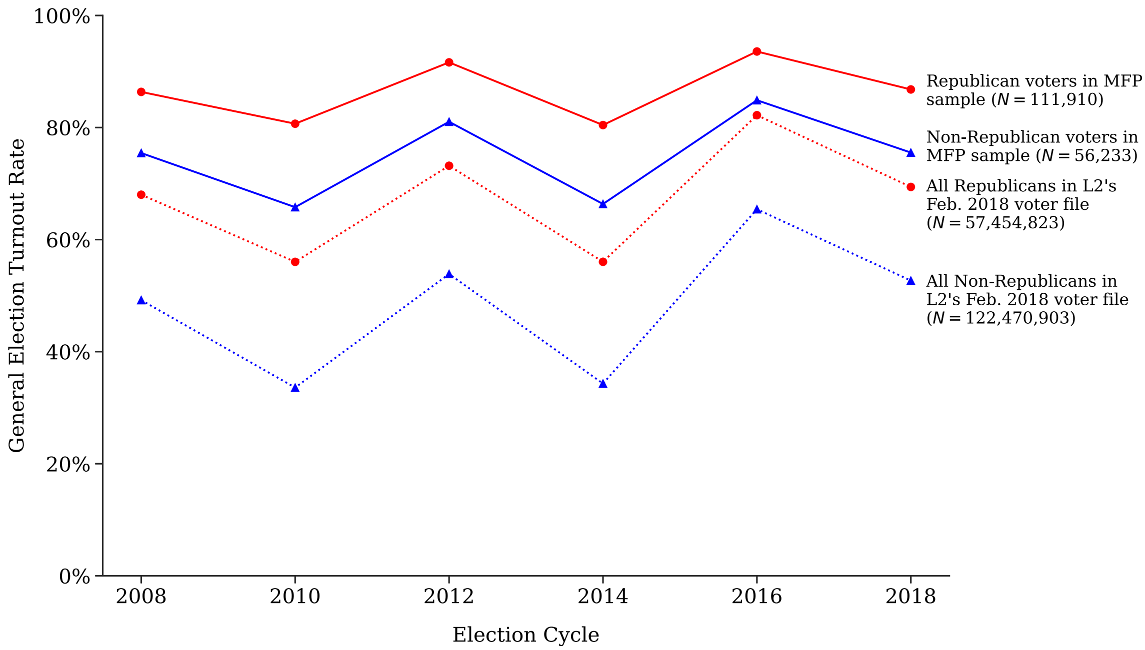
turnout records in the 2018 and preceding elections while mitigating any possible survivorship bias that might arise from voter file attrition. We also incorporate records from a May 2021 snapshot of L2's voter file, as well as L2's national consumer file, in the interest of maximizing the accuracy of our record linkage and individual-level demographic information. Our main analysis of MFP outcomes and turnout centers on voters who (a) were listed in the February 2018 snapshot of the L2 voter file and (b) were linked to one of the farms satisfying the criteria described above. After applying each of these restrictions, our main analysis sample consists of 168,143 voters linked to 122,157 farms. According to the February 2018 snapshot of the L2 voter file, 111,910 (67%) of these voters were affiliated with the Republican party, 32,835 (20%) were affiliated with the Democratic Party, and the remaining 23,398 (14%) were flagged as independents or third-party members.[9]

We link this resulting set of 122,157 farms to the DIME 4.0 database of itemized political contributions spanning 1979–2020. The DIME database employs an entity resolution algorithm to assign groups of transactions to distinct contributor identification numbers (IDs). For the purpose of our main analyses, we match specific contributor IDs from DIME to distinct farms (rather than distinct voters), because campaign contributions may often be a joint household decision. As discussed in Dataverse Materials Section E, each of our contributor-farm matches is made according to one of two methods: (a) a bespoke probabilistic matching algorithm directly linking contributor profiles to farm profiles or (b) by way of the previously established voter-farm matches using a crosswalk between recent DIME contributors and L2 voter profiles (Bonica and Grumbach 2022). Together, these two approaches match 35,401 of the 122,157 farms (29%) to one or more contributor profiles in DIME.

In Figures 3 and 4, we present voter-level turnout rates and farm-level contribution rates by cycle, respectively, for our main analysis sample. As shown in Figure 3, MFP recipients consistently demonstrate high

[9] L2 estimates likely party affiliation in states in which voters do not declare a partisan preference (see Section A.2 of the Online Appendix for more details).

FIGURE 3. General Election Turnout Rates among Voters Linked to CY 2018 MFP Sample of Farms, with Comparison to Broader Electorate



levels of political engagement relative to the broader electorate. However, there was still substantial room for improved mobilization in 2018: roughly one in four sample voters sat out the previous midterm election in 2014, and the vast majority of sample farms did not make itemized political contributions in any given cycle. In studying both turnout and contributions, our analyses are therefore able to gauge mobilization effects across both high-propensity and low-propensity forms of political engagement.

Our treatment variables of interest are based on measures of farm-level compensation outcomes stemming from farms' variable crop portfolios, which—as previously discussed—were predetermined by planting decisions made prior to the announcement of Chinese retaliatory tariffs on U.S. agriculture. For each of the 122,157 farms in our sample, we construct measures of net MFP benefits and MFP benefits as a share of tariff-induced trade war losses, inferred using national crop-specific price impacts. As we observe the MFP payment amount each farm received for each covered crop, and as farmers were payed a fixed rate per unit of certified, harvested production (e.g., \$1.65 per bushel of soybeans), it is straightforward to calculate farmers' harvest records based on observed payment records.[10] We combine these implied harvest quantities with pre-trade war price forecasts to calculate expected harvest value. We then take the average estimated national trade war price impacts presented in Table 1, and estimate each MFP participant i's tariff-induced losses as

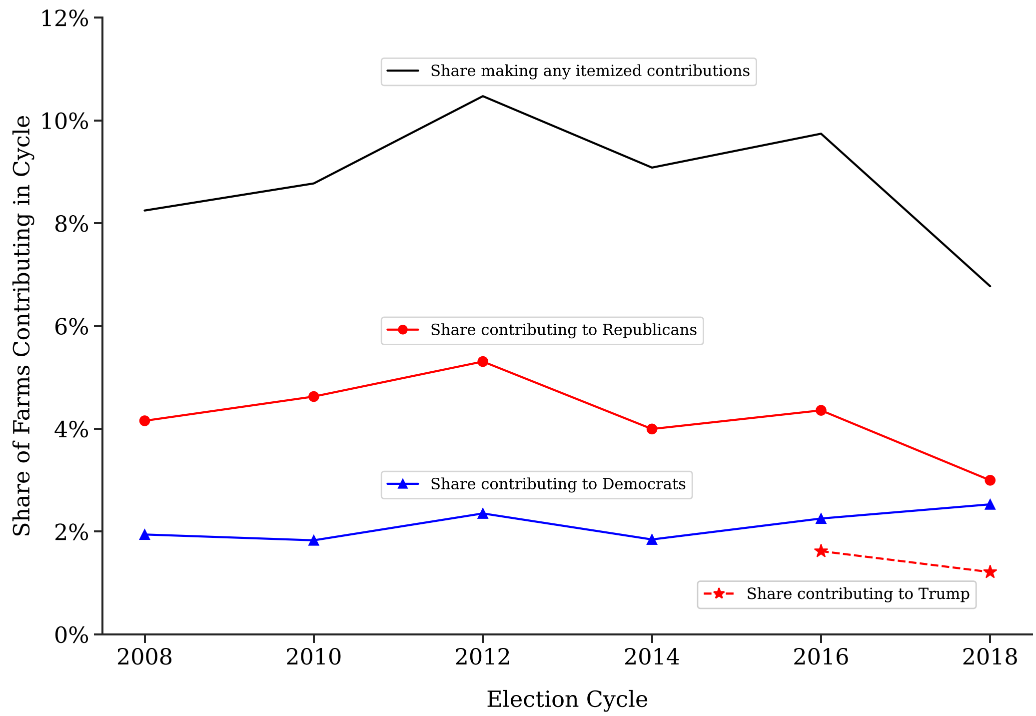
$$\text{Tariff\_Induced\_Losses}_i = \sum_{c \in \mathcal{C}} \left(\frac{\text{MFP\_Payment}_{ic}}{\text{MFP\_Rate}_c} \times \text{Forecasted\_Price}_c \times \text{Tariff\_Price\_Impact}_c \right),$$

where $\mathcal{C} = \{\text{corn, cotton, sorghum, soybeans, wheat}\}$ and $\text{Tariff\_Price\_Impact}_c \in (0,1)$ is the relative 2018 price decline attributed to the trade war (see Section A.4 of the Online Appendix for further details regarding these calculations).

We calculate each farm's net MFP benefits by subtracting these estimates from their actual MFP payments, and we divide MFP benefits by tariff-induced losses to obtain each farm's compensation rate. Figure 5 depicts the distribution of each of these measures across sample farms. While farmers' perceptions of their tariff-induced losses may have varied idiosyncratically from our estimates derived from agricultural economics studies, these two measures allow us to credibly distinguish between farms that achieved relatively better or relatively worse outcomes from Trump's 2018 policy endeavors. Indeed, Figure 5 demonstrates that there was substantial variation in actual compensation outcomes, even though the Trump administration's clearly stated goal was to compensate all producers at 100% of their losses. While the large majority of farms were overcompensated by the MFP for their tariff-induced losses, over twelve thousand sample farms were not made whole.

[10] See Dataverse Materials Section B for a validation of our approach to measuring farm-level production by backing out the basis of farm program payments.

FIGURE 4. Farm-Level Contribution Rates by Cycle among CY 2018 MFP Sample of Farms



Note: A farm is recorded as contributing to a Republican (or Democrat) if they are linked to an itemized contribution to a Republican (Democratic) candidate or PAC within the specified cycle. Contributions to Trump-affiliated PACs factor into the “share contributing to Trump” statistics.

In Dataverse Materials Section C, we provide further descriptive statistics on trade war losses, MFP benefits, joint policy outcomes, and these outcomes’ relationship with partisanship. In particular, we find that the treatment of particular crop mixes (as measured by the MFP compensation rate) is uncorrelated with Republican party affiliation. Indeed, corn growers appear to be just as solidly Republican as soybean growers, corroborating our conjecture that the over-compensation of soybeans and under-compensation of corn reflected hasty policy design rather than partisan particularism.

Our primary outcomes of interest are (a) each voter’s turnout in the 2018 midterm elections, as recorded in L2’s national voter files and (b) each farm’s “net Republican contributing” status, which reflects the partisan orientation of a farm’s contributions made between August 27, 2018 (the day on which commodity-specific MFP rates were announced) and May 23, 2019 (the day before the 2019 MFP was announced). This latter measure takes a value of 1 if individuals associated with a farm contributed to Republicans but not Democrats, a value of –1 if associated individuals contributed to Democrats but not Republicans, and a value of 0 otherwise. Since only a handful of farms contributed to both Republicans and Democrats within the specified period,[11] changes in this measure parsimoniously reflect shifts in farms’ political engagement in favor of the party responsible for both the trade war and the MFP. The date range across which net Republican contributing is measured constitutes the broadest time frame in which farmers could be aware of their own 2018 MFP benefit amount while remaining unaware of benefits from the 2019 MFP sequel program. It also happens to span the beginning of the corn and soybean harvest seasons to the end of the 2018/2019 marketing year, a sequence of months in which MFP and trade war outcomes should have been the most salient.

Beyond the aforementioned treatment and outcome measures, we draw on L2’s voter and consumer files, the DIME database, and auxiliary election data to construct a detailed array of individual and geographic controls. These include past voter-level turnout in general and primary elections, quarterly farm-level contribution histories, a number of demographic fields, past precinct and county-level voting patterns and turnout, and several nonpolitical geographic characteristics (including, most simply, each individual’s congressional district). Additionally, we draw on our database of USDA farm payment records to construct a measure of 2009–2012 row crop acreage, which we use to control for long-standing farm size.[12]

[11] Only 42 out of 122,157 farms in the main analysis sample contributed to both Republicans and Democrats between August 27, 2018 and May 23, 2019, whereas 1,528 farms contributed solely to Republicans and 1,822 contributed solely to Democrats.

[12] See Section A.5 of the Online Appendix for details regarding this measure.

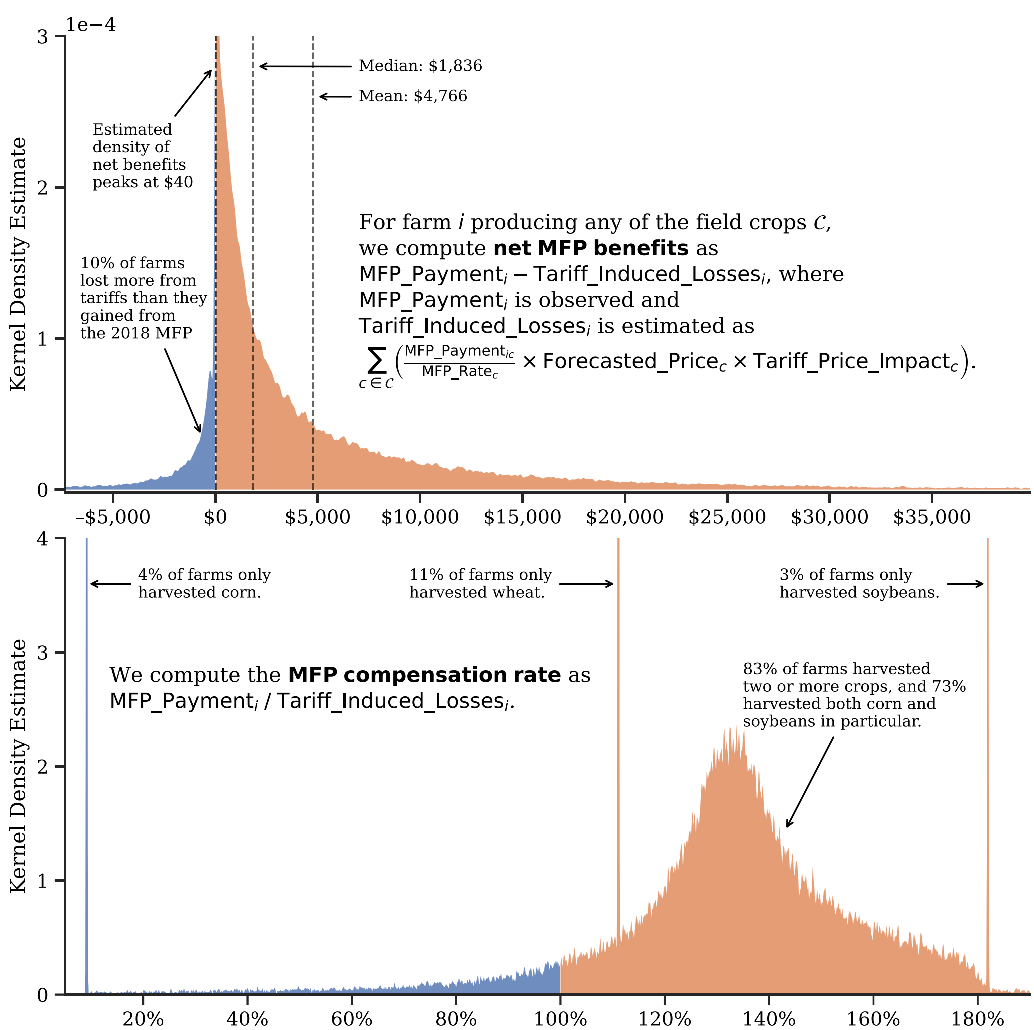
EMPIRICAL STRATEGY

To gauge the efficacy of improved policy outcomes in mobilizing constituents, we estimate the effect of farm-level variation in compensation on: (1) turnout in the 2018 midterm elections and (2) campaign contributions in the nine months following the announcement of commodity-specific MFP rates (August 27, 2018 to May 23, 2019). In each analysis, we operationalize better MFP compensation using three alternative treatment variables based on the policy outcome measures described in Figure 5. These include: (1) MFP benefits net of tariff-induced losses, specified in percentiles due to the distribution's fairly long right tail; (2) benefits as a share of losses; and (3) an indicator for whether a given farm was made whole through the MFP.

To ensure that our results can speak to different theoretical mechanisms of voter mobilization, we estimate effects on Republican and non-Republican engagement separately. In particular, Chen's (2013) political accountability theory of mobilization posits that improved policy outcomes will increase farmers' preferences for keeping the incumbent party in power, and thus will only incentivize greater engagement among Republicans. In contrast, a similarly positive effect for Republicans and non-Republicans alike could be consistent with farmers becoming more politically engaged due to positive experiences with government or increased financial resources, two mechanisms emphasized in policy feedback theories.

Our identification strategy is fundamentally the same across our analyses of turnout and contributions. We

FIGURE 5. Distribution of Net MFP Benefits and Compensation Rates across 122,157 Farms That Harvested Field Crops and Participated in the 2018 MFP



Note: Kernel density estimation was conducted using a Gaussian kernel. For improved readability, the *y*-axes are truncated at 0.0003 and 4, respectively, and the *x*-axes are truncated at the 1st and 99th percentiles. $\text{Tariff\_Price\_Impact}_c$ refers to the proportional decline in a crop's price due to retaliatory tariffs, as depicted in Table 1. $\text{Forecasted\_Price}_c$ denotes the USDA's May 10, 2018 price forecasts for the 2018/2019 marketing year.

assume that variation in compensation outcomes is unconfounded conditional on prior levels of political engagement and observed pretreatment characteristics. However, the unit of analysis differs across these two analyses (voter-level turnout and farm-level contributions), and so—for ease of exposition—we focus here on describing the empirical strategy underlying our turnout analysis. We provide the details of how this approach maps onto our analysis of farm contributions in the section describing the results of that analysis.

Reflecting the richness of the large, individual-level database we have constructed, we utilize a three-pronged research design to identify the causal effect of improved trade war outcomes on partisan turnout. First, at the core of our study, is the natural experiment of the MFP itself. The combination of the trade war and MFP was an unprecedented (and likely unanticipated) shock to row crop farmers, and as noted previously in "Institutional Background," it arrived late enough in 2018 that crops were already in the ground and farmers were unable to adjust their crop portfolios. While the joint impact of these two events produced substantial farm-level variation in net policy outcomes for the 2018/2019 marketing year, this variation was not politically targeted, but the incidental result of a compensation package that the administration rushed to announce in the face of a critical media firestorm and the looming midterm elections. In Dataverse Materials Section D, we show that this variation in policy impact is generally orthogonal to past levels of political engagement as well as key individual and geographic covariates.

The panel structure of our turnout and contribution history data, as well as a rich set of covariates pulled from L2 and USDA administrative records, provide the second and third prongs to our research design, respectively. In particular, voters' turnout decisions in previous elections are highly indicative of their baseline propensity to turn out in 2018. Each farm's history of campaign contributions prior to 2018 is likewise informative of a household's baseline level of political engagement. A number of individual and geographic controls, including each voter's age and congressional district, should also be predictive of turnout. Moreover, since we are able to control for long-standing farm size, we can ensure that our identifying variation is driven by idiosyncratic farm-level variation in crop mixes, rather than economic status or wealth. Altogether, we control for a farm's 2012 acreage, 2010–2016 general election turnout, 1992–2008 general election and 1992–2018 primary election turnout (with varying completeness by state), quarterly farm-level contribution amounts spanning 2005 to 2017, congressional district, gender, age, education, ethnicity, religion, military/veteran status, gun ownership, census block population density, and a number of geographic measures concerning past local turnout and partisanship. We direct readers to Section B of the Online Appendix for further details on the specifications of these controls.

While this broad list of controls helps us to relax our identification assumptions to a credible unconfoundedness design, it would also present a host of difficult modeling decisions for OLS estimation. Instead of turning to an OLS regression with an arbitrary assortment of interactions between past turnout fields and demographics, our main analyses employ a data-driven regression approach to extract the critical information contained in our controls.

Specifically, we estimate a partially linear regression (PLR) model using the "Double Machine Learning" (DML) estimation framework of Chernozhukov et al. (2018). As DML is a relatively recent advancement in causal inference, this particular approach may be unfamiliar to some readers.[13] However, it greatly simplifies our analysis while closely mirroring the familiar OLS approach to "adjusting for" pretreatment covariates. According to the classic Frisch–Waugh–Lovell (FWL) theorem, in an OLS regression of 2018 turnout on MFP compensation rate and controls, the estimated coefficient on MFP compensation rate can be expressed as a bivariate residual-on-residual regression coefficient: the estimated slope from regressing turnout minus predicted turnout on compensation rate minus predicted compensation rate. In this result, the *predicted turnout* and *predicted compensation rate* have themselves been obtained by auxiliary OLS regressions of turnout and compensation rate, respectively, on the controls, and by subtracting out these predictions one aims to purge the bias from the bivariate relationship between the outcome and the treatment.

Our rationale for eschewing an OLS regression with multiple controls is that its implicit use of auxiliary OLS regressions to estimate the conditional expectations for turnout and compensation rate is arbitrary and likely suboptimal.[14] Instead, the DML approach lets us use a high-dimensional nonparametric regression (i.e., supervised machine learning) to obtain the best possible predictions for individuals' turnout propensity and MFP compensation rate from our broad array of controls. Then, we obtain a linear treatment effect estimate using a bivariate residual-on-residual regression, as one implicitly would with OLS.

To formalize this discussion, we present the PLR model of farmers' turnout decisions, which we estimate separately for Republicans and non-Republicans:

$$Y_i = \theta B_i + g(X_i) + \varepsilon_i, \qquad \mathbb{E}[\varepsilon_i \mid X_i, B_i] = 0, \tag{1}$$

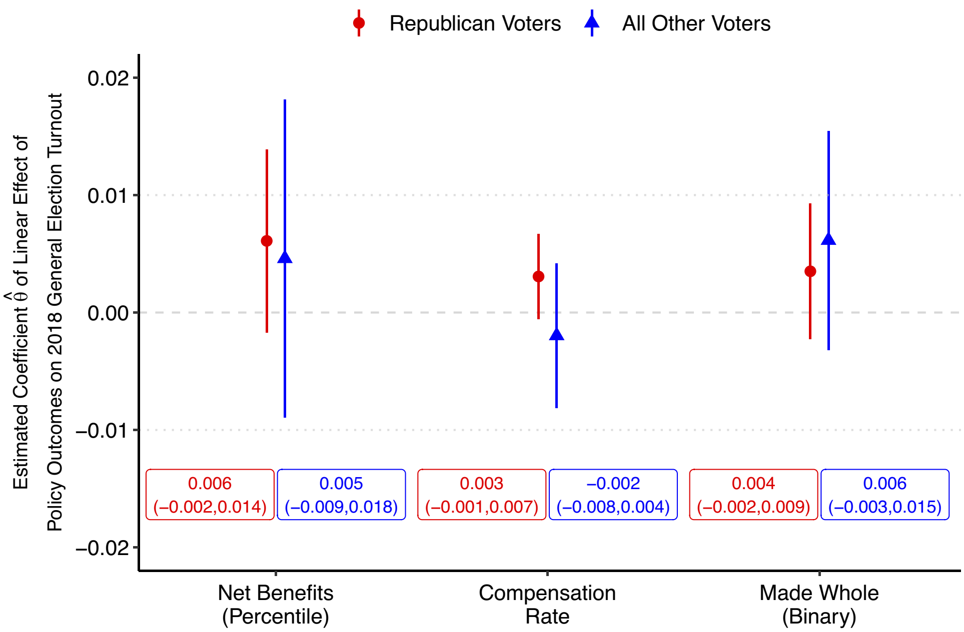
$$B_i = m(X_i) + \eta_i, \qquad \mathbb{E}[\eta_i \mid X_i] = 0. \tag{2}$$

The outcome $Y_i \in \{0,1\}$ denotes producer i's turnout in 2018, B_i denotes i's policy outcomes (MFP benefits net of trade war losses, benefits as a share of trade war losses, or an indicator for whether benefits exceeded losses), and X_i is a vector of controls. We seek a consistent estimate of the linear causal effect θ of better

[13] A more detailed presentation and explanation of the method's advantages can be found in Ratkovic (2023).

[14] As we show in Dataverse Materials Section F.1, both simple linear models and fully specified models provide unsatisfactory approximations of the conditional expectation of a voter's engagement given observed controls.

FIGURE 6. Estimated Effects of Improved Policy Outcomes on 2018 Turnout by Party



Note: Effects are estimated separately for Republicans and non-Republicans. Point estimates are depicted with 95% confidence intervals. The "Net Benefits (Percentile)" treatment ranges from 0 to 1; "Compensation Rate" ranges from 0.09 (corn-only portfolio) to 5.98 (cotton-only portfolio). To view these results in table form, see Dataverse Materials Section I.

policy outcomes. The essence of the DML approach is best seen by rewriting Equations 1 and 2 via the classic Robinson (1988) transformation:

$$Y_i - \mathbb{E}[Y_i \mid X_i] = \theta(B_i - \mathbb{E}[B_i \mid X_i]) + \varepsilon_i.$$

Estimation of the conditional expectation functions $q(X_i) \equiv \mathbb{E}[Y_i \mid X_i]$ and $m(X_i) = \mathbb{E}[B_i \mid X_i]$ amounts to a high-dimensional nonparametric regression task, for which modern supervised learning algorithms are well-suited. We use CatBoost, a gradient boosting decision tree algorithm, to obtain estimates $\hat{q}$ and $\hat{m}$, allowing us to compute residuals $\tilde{Y}_i \equiv Y_i - \hat{q}(X_i)$ and $\tilde{B}_i \equiv B_i - \hat{m}(X_i)$, which leaves us with the simple binary regression problem

$$\tilde{Y}_i = \theta\tilde{B}_i + \varepsilon_i. \tag{3}$$

Chernozhukov et al. (2018) show that this "orthogonalized" residual-on-residual regression, in conjunction with a sample splitting technique termed cross-fitting, efficiently removes the effect of regularization bias that would generally be induced by using machine learning estimators, and can deliver a $\sqrt{n}$-rate consistent (and asymptotically normal) estimate of θ.

While our main focus is on estimating the constant marginal effect θ from Equation 1, we also investigate several potential sources of treatment effect heterogeneity. Following an approach from Battocchi et al. (2019), we maintain the assumption that treatment effects are linear in policy outcomes, but model heterogeneity by allowing θ to be a function of a low-dimensional vector of covariates V_i. In particular, to gauge heterogeneity by baseline turnout propensity, we set $\theta(V_i) = \alpha + \beta \cdot \text{turnout\_2014}_i$, thereby estimating separate conditional marginal effects for farmers who voted or abstained in the previous midterm election. For further details on DML estimation, as well as our particular implementation, see Dataverse Materials Section F.2.

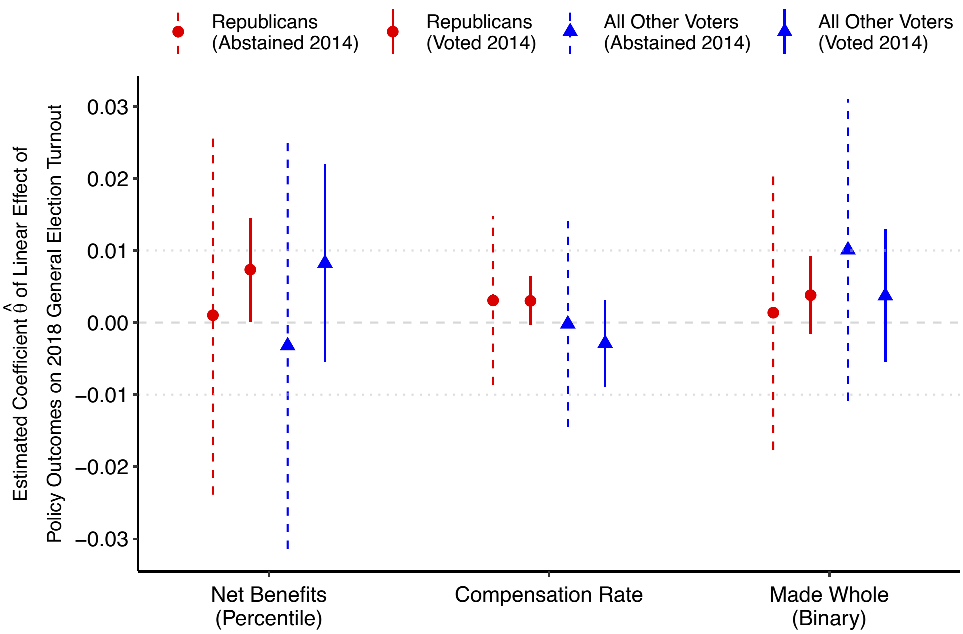
THE EFFECT OF IMPROVED POLICY OUTCOMES ON VOTER TURNOUT

Figure 6 depicts our main results on voter turnout.[15] For each of the policy outcome "treatments" we consider, we plot DML estimates of the (linear) effect coefficient θ from our baseline partially linear model (see Equations 1 and 2). We do not find a statistically significant effect of any of our policy outcome specifications on turnout among Republicans or non-Republicans.[16] More importantly, the substantive effective sizes we estimate are very small and—due to the large sample size—quite precise. Our point estimate for the effect of net MFP benefits (specified in percentiles) implies that moving a Republican farmer across the interquartile range of outcomes ($391 to $6,110) only increases her turnout rate by 0.3

[15] See Dataverse Materials Section F.3 for measures of covariate feature importance.

[16] All reported p-values are two-tailed.

FIGURE 7. Effect of Policy Outcomes on 2018 Turnout (Heterogeneity by Past Turnout)



Note: Effects are estimated separately for Republicans and non-Republicans. Point estimates are depicted with 95% confidence intervals. The "Net Benefits (Percentile)" treatment ranges from 0 to 1; "Compensation Rate" ranges from 0.09 (corn-only portfolio) to 5.98 (cotton-only portfolio). To view these results in table form, see Dataverse Materials Section I.

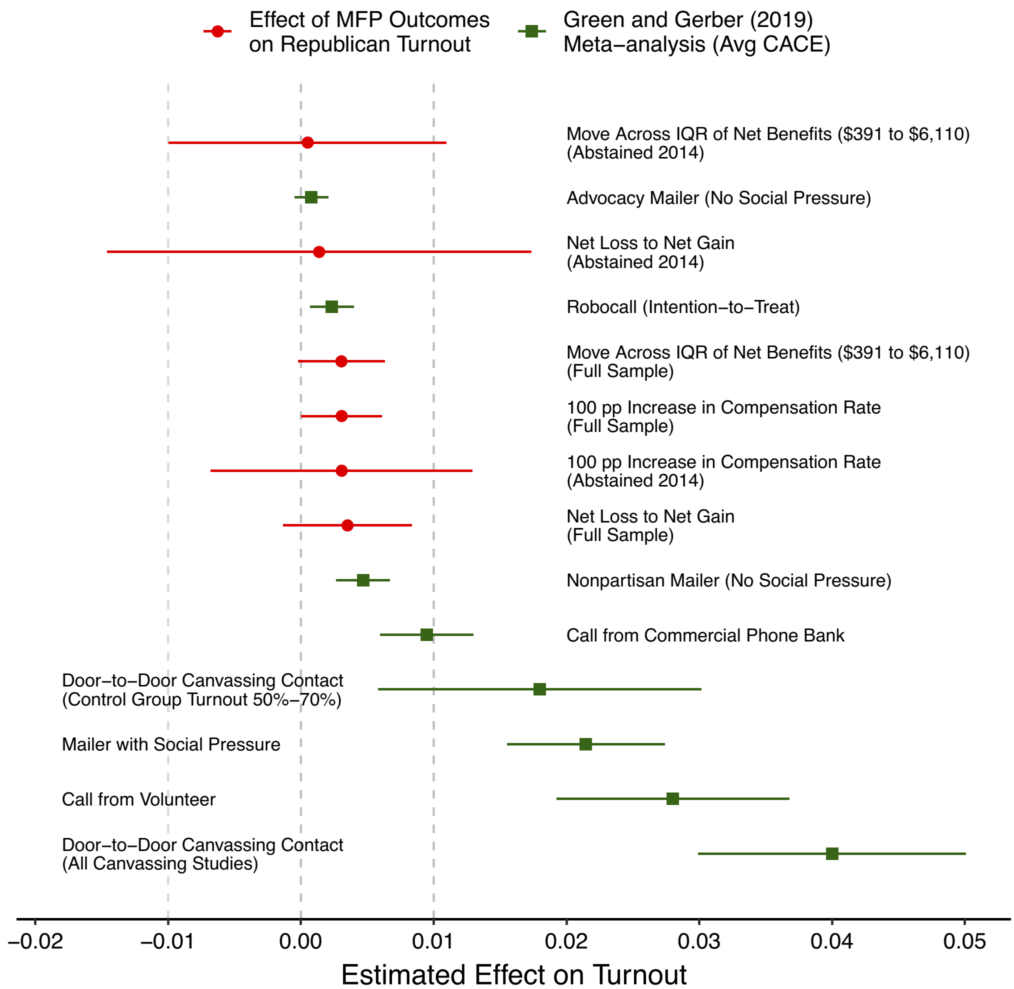
percentage points. Likewise, a 100 percentage point increase in the compensation rate (as a share of tariff-induced losses) nets an increase of 0.3 percentage points in the Republican turnout rate, while the turnout difference between a Republican farmer who is made whole by the MFP and one who is not is 0.4 percentage points. Just as we obtain null results for Republicans, our estimates for non-Republicans provide no evidence that positive policy outcomes significantly reduced the turnout motivations of Democrats and independents.

Since our sample of registered voters has a relatively high baseline turnout rate, we also estimate this effect allowing for heterogeneity by prior turnout. Figure 7 presents estimated effects with separate slopes for individuals who voted in the 2014 elections and those who abstained. We find little difference in the effects between these two groups, and thus conclude that it is very unlikely a ceiling effect is mechanically driving our null results.

Following Rainey's (2014) suggestions for arguing for a negligible effect, we conduct "two one-sided test" (TOST) analyses to formally demonstrate that our estimated effect sizes are smaller than proposed bounds on substantively meaningful effects. As Rainey (2014) notes, this is most simply implemented by checking that an estimate's 90% confidence interval does not contain the specified bounds. Given our focus on voter mobilization, we choose bounds by considering turnout effects estimated for standard campaign activities aimed at mobilizing voters. In conducting a meta-analysis of 56 door-to-door canvassing experiments, 104 direct mail experiments, and 51 phone call experiments, Green and Gerber (2019) calculate average complier average causal effect (CACE) estimates for various types of interventions. We formally conduct TOST equivalence tests to examine whether effects on Republican turnout are less than 1 percentage point, since only the weakest, lowest-cost campaign interventions (e.g., robocalls and mailers without social pressure) yield average turnout effects lower than this threshold.

As shown in Figure 8, we find that we can easily reject average effect sizes at this level. Indeed, we find that even vastly improved policy outcomes in our setting earned Republicans less mobilization among the targeted population than their campaigns might reap from some of the most economical and standard outreach tactics. In particular, our estimated effect of a 100 percentage point increase in the MFP compensation rate (0.3 percentage points) is roughly as large as the average CACE of an advocacy mailer lacking social pressure (0.1 percentage points), a robocall (0.2 percentage points), or a nonpartisan mailer lacking social pressure (0.5 percentage points). Moreover, it is noticeably smaller than the average CACE of a call from a commercial phone bank (0.9 percentage points), a mailer invoking social pressure (2.1 percentage points), a call from a campaign volunteer (2.8 percentage points), or a door-to-door canvassing contact (4.0 percentage points). These differences cannot be dismissed as an artifact of the relatively high baseline turnout rate among our sample of farmers. Among experiments with 50%–70% control group turnout—comparable

FIGURE 8. TOST Analysis for Republican Turnout Effects with Comparison to Meta-Analytic Estimates of Campaign Activity Effectiveness



Note: Point estimates are depicted alongside 90% confidence intervals. CACE refers to "complier average causal effects"; see Appendices A–C of Green and Gerber (2019) for details on the studies and methodology underlying these meta-analytic estimates. To view these results in table form, see Dataverse Materials Section I.

to our 63% 2018 turnout rate among Republicans who abstained in 2014—door-to-door canvassing was found to obtain a CACE of 1.8 percentage points.

We conduct a number of supplementary analyses to demonstrate that the null results described above are not artifacts of our particular empirical strategy, unobserved confounders, or unusual mitigating factors within the substantive setting we examine. We do not have space to present these robustness checks in the main text, but interested readers can find them in Dataverse Materials Section H. To summarize our conclusions: (1) we obtain very similar estimates to our main DML results using simpler OLS and logit specifications; (2) the linear effect specification in the partially linear model is not masking a more substantial (but nonlinear) effect; (3) the effects are similar across electorally competitive and uncompetitive areas; (4) we obtain similar results using four alternative methods of aggregating price impact estimates to construct our individual-level measure of trade war damage; (5) placebo estimates of MFP turnout effects in the prior election provide no evidence that our results are spurious; (6) we find no evidence that the relationship between payments and turnout is asymmetric with respect to gains and losses; and (7) incorporating estimates of spatial heterogeneity into our damage measures (i.e., local variation in price impacts) does not alter our conclusions. Additionally, in Dataverse Materials Section H.2, we conduct an in-depth analysis of MFP enrollment timing. We find no evidence that our results are driven by the timing of MFP disbursements relative to the election.

THE EFFECT OF IMPROVED POLICY OUTCOMES ON CAMPAIGN CONTRIBUTIONS

Having established that better compensation outcomes did not deliver Republican candidates a turnout advantage, we turn to estimating the impact of better outcomes on farms' campaign contributions. This complementary set of estimates is valuable because contributions are a low-propensity form of political engagement, and because donations plausibly serve as a proxy for higher forms of political engagement, such as canvassing, organizing, and opinion leadership.

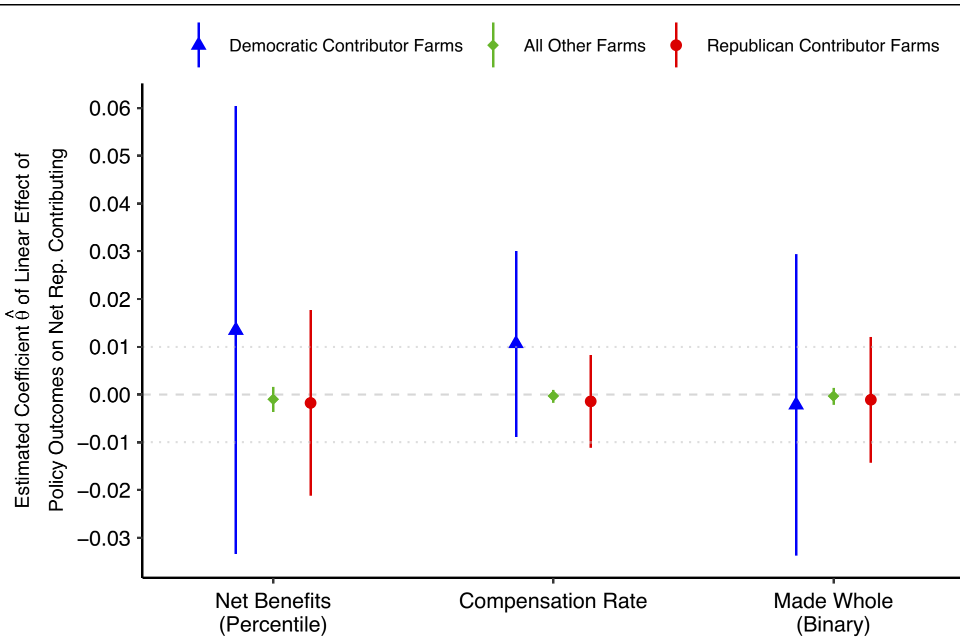
Mirroring our turnout analysis, our estimate of the linear causal effect of compensation outcomes on net Republican contributing is obtained using DML estimation of a PLR model. However, as we measure contributions at the farm level, we necessarily must employ a distinct set of controls. Most simply, we control for the number and total amount of contributions to Republicans, Democrats, nonpartisan recipients, and Trump in each quarter between 2005 and 2017. To directly control for the long-standing ideological lean of each contributing farm, we calculate a farm-level pre-treatment analogue of Bonica's (2014) common-space campaign finance score ("CFscore") measure of donor and recipient ideology. We furthermore control for historical farm size, congressional district fixed effects, other geographic characteristics, and measures of linked voters' political engagement and demographics (see Section B of the Online Appendix for a full list of controls).

It is critical that we allow for effect heterogeneity by prior contribution status, as most farms in our sample had never made an itemized contribution prior to the commencement of the trade war. It is implausible that a significant share of such farms would begin contributing during the trade war, and so documenting a negligible average effect across the entire sample would not be particularly informative. As such, we bin farms into three mutually exclusive categories: farms with a distinctly Republican pre-2018 contribution history ($N = 15{,}192$), farms with a distinctly Democratic pre-2018 contribution history ($N = 5{,}620$), and all other farms ($N = 101{,}345$) (see Section A.3 of the Online Appendix for details on this categorization). We estimate partially linear models that allow effects to vary linearly across these groups by setting $\theta(V_i) = \alpha + \beta_R \cdot \text{Rep\_Contributor}_i + \beta_D \cdot \text{Dem\_Contributor}_i$.

Figure 9 presents our main results from regressing net Republican contributing on each of our three treatments of interest. Echoing our turnout analysis, we uncover no statistically significant effects on the contribution behavior of farmers of any partisan affiliation.

Most notably, improved policy outcomes did not increase the engagement of farms with a distinctly Republican pre-2018 contribution history, whose average net Republican contributing was 0.073 across our

FIGURE 9. Estimated Effects of Improved Policy Outcomes on Net Republican Contributing by Prior Contribution Behavior



Note: Effects are estimated jointly among sample of 122,157 farms, with treatment interactions allowing for separate slope estimates among (a) farms with distinctly Republican contribution histories before 2018, (b) farms with distinctly Democratic contribution histories, and (c) all other farms. Point estimates are depicted with 95% confidence intervals. The "Net Benefits (Percentile)" treatment ranges from 0 to 1; "Compensation Rate" ranges from 0.09 (corn-only portfolio) to 5.98 (cotton-only portfolio). To view these results in table form, see Dataverse Materials Section I.

period of interest following the MFP commodity rate announcements (August 27, 2018 to May 23, 2019). Per our estimates, moving a Republican-contributing farm across the interquartile range of net MFP benefits, a 100 percentage point increase in the MFP compensation rate, and making a farm whole each produced an expected 0.001-point decline in net Republican contributing. As such, variation in compensation outcomes had a negligible impact on Republican-contributing farms' engagement even relative to their modest baseline levels of engagement.

The 5,620 farms in our sample with a distinctly Democratic contribution history were quite active contributors in our period of interest, and had an average net Republican contributing of –0.207. However, our estimates indicate that this was not meaningfully caused or mitigated by the sample's extensive variation in compensation outcomes. Among these Democratic-contributing farms, moving across the IQR of net benefits yielded a 0.007 point increase in net Republican contributing. We likewise estimate a 0.011 point effect for a 100 percentage point increase in the compensation rate, and a –0.002 point effect for making a farm whole.

We likewise find no meaningful effect sizes among the large majority of sample farms that had not made itemized contributions to Republicans or Democrats prior to 2018. Among such farms, estimated effects for (1) moving across the IQR of net benefits, (2) a 100 percentage point increase in the compensation rate, and (3) making a farm whole are –0.001, –0.0003, and –0.0003, respectively. We conclude that even very large variation in compensation outcomes was insufficient to drive any inactive farms into becoming political contributors.

We also conducted a series of robustness checks for which we lack room to discuss in the main text. While net Republican contributing provides a parsimonious measure of contribution behavior, we demonstrate in Dataverse Materials Section H.3 that the variation we study has no meaningful effects on farms' rates of giving to particular recipients (e.g., Republican candidates more broadly or Trump in particular). Further, we conduct similar robustness checks as we did for the turnout analyses (OLS vs. DML; alternative price impact measures; placebo estimates of contribution effects prior to the announcement of retaliatory tariffs). Lastly, the results are qualitatively unchanged if we specify our outcome as the *number* of contributions to Republicans net of the number of contributions to Democrats.

FARMER PERCEPTIONS OF THE 2018 MFP

Our findings presented in the previous sections clearly indicate that variation in compensation outcomes had minimal influence on farmers' political engagement, either as manifested in voter turnout or campaign contributions. However, these null results are not due to a lack of farmers' awareness or appreciation of their own compensation outcomes. As we show by analyzing responses to Li et al.'s (2023) February 2019 survey of 693 corn and soybean growers in Iowa, Illinois, and Minnesota,[17] variation in the generosity of the MFP across different planted crop portfolios was clearly reflected in the views of individual farmers.

Through self-assessments and tests of factual MFP details, responses to the survey suggest that farmers were fairly well-informed regarding the trade war and MFP (see Section A.6 of the Online Appendix for details). Open-ended comments from respondents also reflected a relatively clear understanding of tariff-induced price declines and the Trump administration's relief package.

Critically for our analysis, respondents were asked “How helpful do you think President Trump's \$12 billion trade relief plan will be to your farm?”; 6% selected “not at all helpful,” 41% “somewhat helpful,” 19% “quite helpful,” and 26% “very helpful.”[18] To examine how farmers' attitudes toward the MFP varied with their crop portfolios, we create a four-point scale of perceived MFP helpfulness. Respondents also reported their planted acreage of corn, soybeans, and “other crops” for both 2018 and the average of 2013–17. Since respondents reported their primary county of operation, we can combine 2018 county-level yields for corn and soybeans with pre-trade war forecasts of corn and soybean prices to estimate each farm's revenue stake in corn and soy production—and thus the corn and soy MFP rates, respectively.

Consistent with the overcompensation of soybeans and undercompensation of corn documented in Table 1, we find that these four-point assessments of the helpfulness of the MFP are strongly correlated with each farm's soybean share of planted corn/soy acreage. Moreover, perceived MFP helpfulness is separately increasing in soybean production and decreasing in corn production, which offers support for our focus on the *net* farm-level impact of retaliatory tariffs and the MFP. Due to space constraints, we present and discuss these particular results in Dataverse Materials Section H.4.

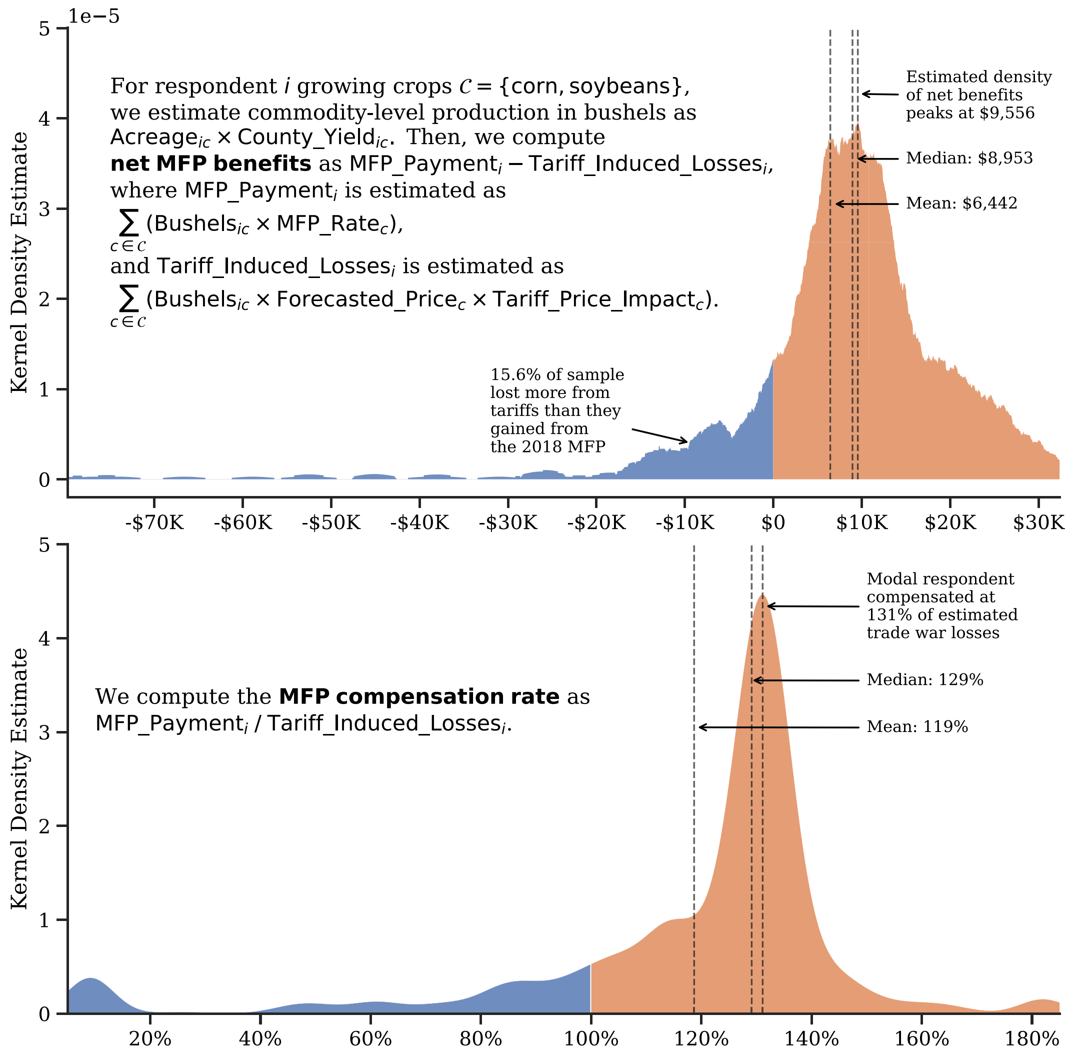
Instead, we focus in this section on the same three treatment measures considered in our main turnout and contribution analyses. As explained in Figure 10, we have sufficient information for each farm in our survey dataset to infer MFP payments net of losses and MFP payments as a share of losses.

In Table 2, we present regressions of our four-point scale of perceived MFP helpfulness on each of the three direct measures of MFP policy outcomes (net MFP benefits, compensation as a share of damages, and whether an individual farm was made whole). The resulting regression coefficient in column 1 implies that moving across the interquartile range of net benefits (\$4,144 to \$14,340) is associated with a 0.4 point increase in perceived helpfulness. Given that the total

[17] Critically, the timing of this survey allows us to evaluate losses sustained on the 2018/2019 marketing year harvest, alongside perceptions of the 2018 MFP. In particular, this precludes any effects from the 2019 MFP, which was first announced on May 23, 2019 and made payments on a different basis.

[18] 7% of respondents reported “not sure.” See Dataverse Materials Section H.4 for analyses incorporating these responses.

FIGURE 10. Distribution of Net MFP Benefits and Compensation Rates among Li et al. (2023) Survey Respondents



Note: Kernel density estimation was conducted using a Gaussian kernel. For improved readability, the *x*-axes are truncated at the 1st and 99th percentiles. $\text{Tariff\_Price\_Impact}_c$ refers to the proportional decline in a crop's price due to retaliatory tariffs, as depicted in Table 1. $\text{Forecasted\_Price}_c$ denotes the USDA's May 10, 2018 price forecasts for the 2018/2019 marketing year. Though omitted from the formulas above, we applied the MFP's $125,000 cap on payments for field crops.

range of the scale is three points, these effects are substantively meaningful. The regression coefficient in column 3 implies that a 100 percentage point increase in MFP compensation as a share of tariff-induced losses (58% of the gap between corn and soybeans) is associated with a 0.6 point increase in MFP helpfulness. Finally, the coefficient in column 5 indicates that respondents who were made whole by the MFP perceived the program to be 0.4 points more helpful than did respondents who lost out on net. Each of these estimates is statistically significant at the 1% level. Hence, farmers' evaluations of the helpfulness of the MFP were intrinsically linked to the generosity of MFP compensation they experienced.

There are several reasons these estimates likely reflect a causal relationship between policy outcomes and perceptions of the MFP. First, as argued in the "Institutional Background" Section, Chinese retaliatory tariffs and the compensation through the MFP were unanticipated policy shocks that were announced late enough in the growing season that farmers were unable to actively select into a treatment disposition. Our survey data further corroborate this claim. Sample members' planting decisions in 2018 were almost identical (on average) to 2013–17, both for corn (53.7% → 53.5% of acreage) and for soybeans (44.3% → 44.3% of acreage). Second, the bivariate regression estimates presented in Table 2 are fairly similar to those obtained after adding in demographics, farm characteristics, and state indicators as controls. In particular, this suggests that our estimates are unlikely to be artifacts of demographic or geographic variation in positivity toward government programs. Further, the results are robust to controlling for

TABLE 2. Farmers with Better Policy Outcomes Viewed MFP as More Helpful

	Outcome: Four-point scale of perceived MFP helpfulness					
	(1)	(2)	(3)	(4)	(5)	(6)
Net MFP benefit percentile	0.830*** (0.135)	0.704*** (0.146)	—	—	—	—
MFP as % of damage	—	—	0.626*** (0.127)	0.661*** (0.124)	—	—
MFP made whole	—	—	—	—	0.378*** (0.107)	0.446*** (0.105)
Log(total acres 2013–17)	—	0.090 (0.063)	—	0.211*** (0.060)	—	0.206*** (0.060)
Female	—	−0.268 (0.211)	—	−0.297 (0.219)	—	−0.313 (0.225)
Education (5-point scale)	—	0.017 (0.042)	—	0.022 (0.042)	—	0.032 (0.042)
Age	—	−0.013*** (0.004)	—	−0.012*** (0.004)	—	−0.012*** (0.004)
Off-farm income	—	0.025 (0.086)	—	0.020 (0.085)	—	0.021 (0.087)
Raised hogs	—	0.081 (0.139)	—	0.083 (0.138)	—	0.052 (0.139)
Dairy cattle	—	−0.459* (0.278)	—	−0.429* (0.223)	—	−0.442* (0.255)
Beef cattle	—	−0.058 (0.086)	—	−0.060 (0.086)	—	−0.059 (0.086)
Raised poultry	—	0.400* (0.235)	—	0.345 (0.233)	—	0.398* (0.231)
Other livestock	—	−0.149 (0.189)	—	−0.097 (0.184)	—	−0.091 (0.187)
Intercept	1.293*** (0.077)	1.482*** (0.537)	0.962*** (0.155)	0.114 (0.550)	1.387*** (0.098)	0.572 (0.532)
State fixed effects	No	Yes	No	Yes	No	Yes
No. of obs.	575	575	575	575	575	575
R^2	0.063	0.118	0.040	0.120	0.021	0.107

Note: Robust standard errors in parentheses. Dependent variable is a four-point scale indicating whether the respondent found the MFP to be "not at all helpful," "somewhat helpful," "quite helpful," or "very helpful," respectively. We impute missing values using sample means. Accordingly, columns (2), (4), and (6) include indicator variables denoting the missing status of each of these fields. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$ (two-tailed).

farmers' average corn/soy portfolios across the previous 5 years. As discussed in Dataverse Materials Section H.4, this is feasible due to year-to-year variation in planting schedules, and it allows us to rule out the influence of any systematic long-standing differences between "corn-heavy" and "soybean-heavy" farms.

In summary, our analysis of a survey of Midwestern corn and soybean farmers suggests that producers were informed about how the trade war and ensuing relief package affected their bottom lines in 2018. Furthermore, their perception of the helpfulness of the MFP was strongly affected by its treatment of their individual 2018 crop portfolios. Hence, the limited effects of economic benefits on turnout and contributions could not simply be due to a lack of understanding about the MFP or its helpfulness.[19]

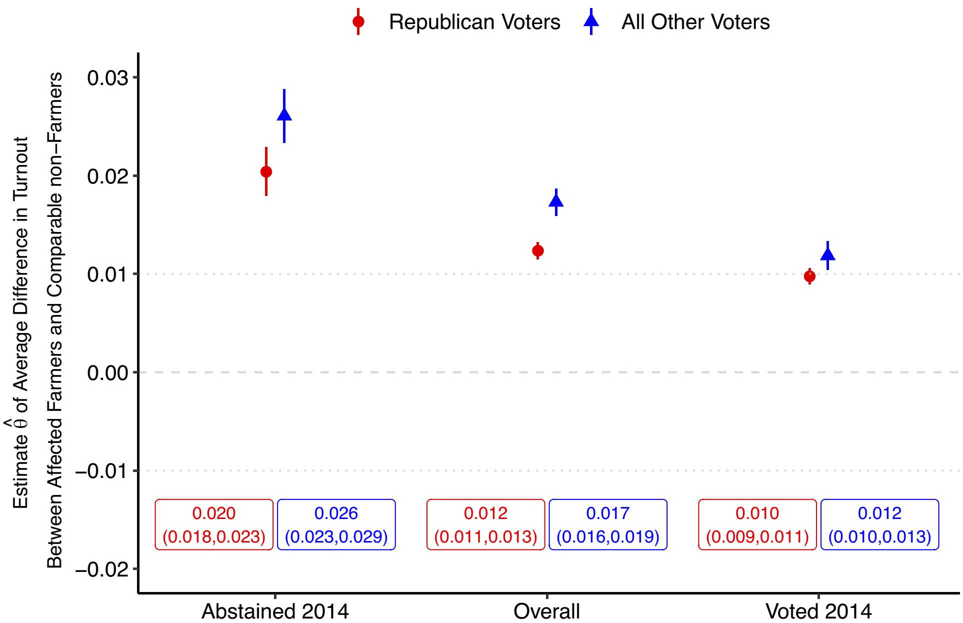
BROADER EFFECTS OF THE TRADE WAR AND MFP ON FARMERS' POLITICAL ENGAGEMENT

To contextualize our main results, and to more broadly contribute to the literature on policy shocks and voter behavior, we zoom out and assess whether direct exposure to the 2018 policy shock—in and of itself—had an effect on farmers' political engagement. The U.S.–China trade war was a once-in-a-generation scale shock to U.S. farm policy; as illustrated in Figure 2, trade war losses and MFP payments each constituted very large shares of farms' net income in 2018. Is it possible that this steep shift in policy salience may have had impacts on political engagement that were orthogonal to individual outcomes?

We obtain suggestive evidence toward this question by comparing turnout between farmers who were acutely and directly affected by the trade war, and the broader electorate—which experienced the trade war much less directly. We identify farmers who were likely affected by the trade war and MFP by examining

[19] As shown in Dataverse Materials Section H.5, although net MFP benefits were recognized by producers, they did not change support for Trump's tariff policies.

FIGURE 11. Overall Impact of Increased Policy Salience on 2018 Turnout: DML Estimates of Difference in Turnout between Affected Farmers and Rest of Electorate



Note: Plotted estimates reflect difference in turnout among affected farmers and rest of electorate, with DML adjustment for all covariates from main analyses save historical farm size and campaign contributions. Four models are estimated: separate constant effect specifications for Republicans and non-Republicans (from which the "Overall" effect estimates are obtained), and separate specifications allowing for heterogeneity by 2014 turnout for Republicans and non-Republicans (from which the "Abstained 2014" and "Voted 2014" effect estimates are obtained). Point estimates are depicted with 95% confidence intervals.

2013–2017 enrollment in traditional USDA farm programs relevant to affected commodities. Altogether, we identify 915,768 individuals from the February 2018 L2 voter file who were associated with such farms. We find this group to be a good approximation of the set of individuals who were directly impacted by the trade war and eligible for the MFP, as 61% of these voters were connected to farms that enrolled in the 2018 MFP, and 94% of voters connected to MFP-enrolled farms belong to this group. We define a treatment indicator that takes a value of 1 if a voter belonged to this group, and we use our DML estimator from the prior section (with the same set of controls save historical farm size and campaign contribution history) to estimate the effect of this treatment. See Dataverse Materials Section G for details, including a description of the 2013–17 programs used to indicate treatment status and a discussion of the merits of this "intention-to-treat" estimand.

For computational tractability, in each analysis, we randomly sample 25 million control units from the 176,039,979 individuals in the February 2018 voter file that resided in households lacking any connection to our administrative database of USDA farm program participants. As such, our Republican estimates are based on comparisons between 593,018 Republican farmers and 25 million Republican non-farmers, and our non-Republican estimates compare 322,750 non-Republican farmers to 25 million non-Republican non-farmers. We present the resulting estimates in Figure 11. We find that the average Republican farmer exposed to the trade war turned out at a rate 1.2 percentage points higher than a comparable non-farmer, while non-Republican farmers were 1.7 percentage points more likely to vote than comparable partisans (differences significant at $p < 0.01$). These differences were noticeably larger among voters who abstained in the previous midterm election (2.0 and 2.6 percentage points, respectively) ($p < 0.01$). Given our massive sample sizes, these estimates are extremely precise, though the effect sizes are arguably modest in light of the scale of the policy shock.

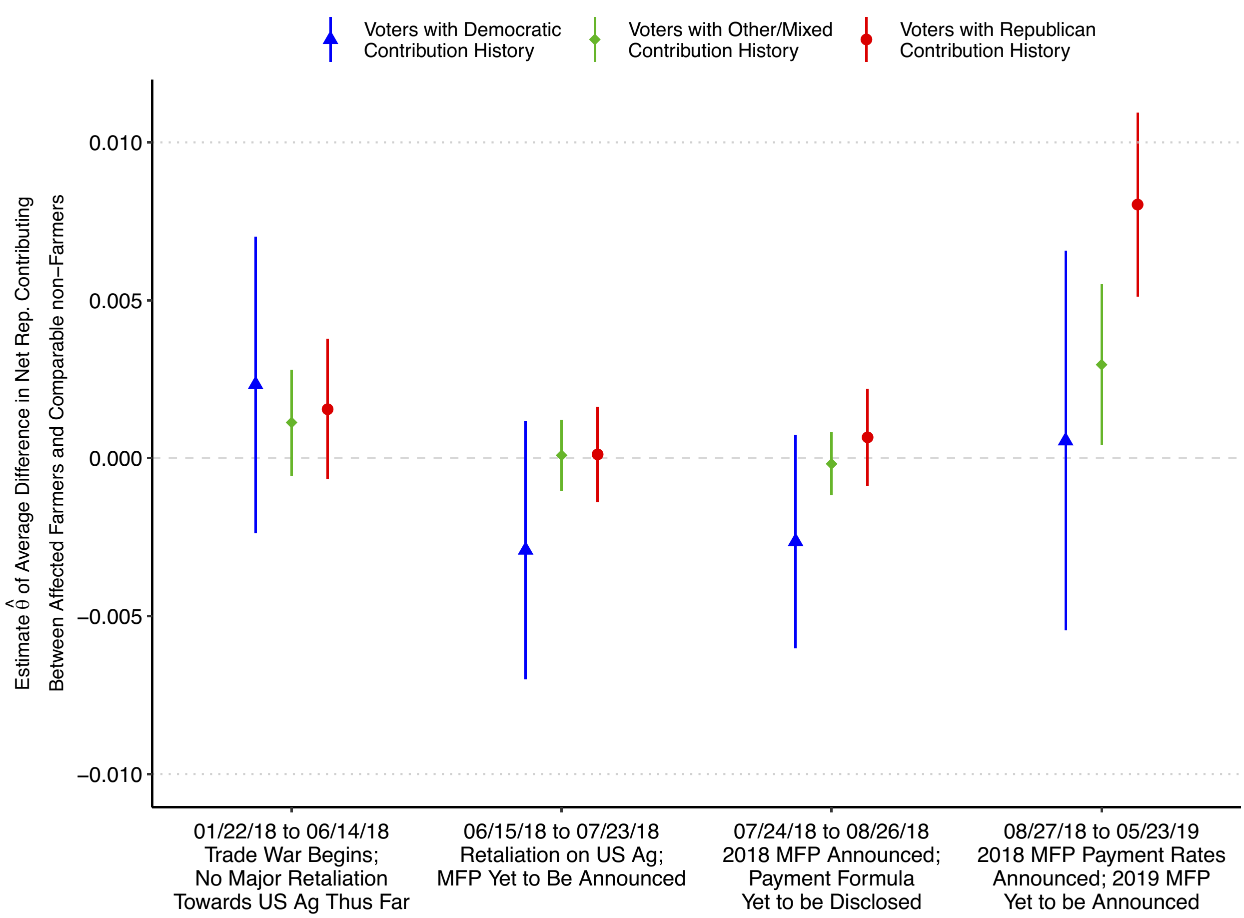
We interpret these results as providing suggestive evidence that the increase in policy salience in 2018 increased turnout among affected voters in the 2018 midterms. However, we emphasize that we cannot conclusively attribute the entire difference in turnout to the causal effect of higher policy salience as—relative to our between-farm analyses presented earlier—this result is more reliant on our covariate adjustment strategy, and thus less robust to unobserved confounders. With this caveat acknowledged, the results presented in Figure 11 are useful for triangulating the mechanisms by which exposure to economic policies might affect political engagement. Specifically, they suggest that exposure to a major policy shock may in and of itself increase political engagement.

As before, we conduct an analogous exercise to estimate the broader impact of increased policy salience on contribution behavior. As contributions occur throughout the election cycle, we can exploit the staggered announcements of (1) Chinese retaliatory tariffs; (2) plans for the MFP; and (3) the actual MFP payment formula to take a step toward disentangling the impacts of the trade war and MFP. To ensure an apples-to-apples comparison of contribution behavior for this broader analysis, we measure contributions at the voter level, and limit our focus to voters who (1) are present in the February 2018 snapshot of L2's national voter file, (2) are linked to a contributor profile in DIME via Bonica and Grumbach's (2022) DIME-L2 crosswalk, and (3) made at least one contribution prior to 2018. This leaves us with 85,012 voter-contributors associated with farms producing affected commodities, who we compare with 7,850,104 voter-contributors not associated with any farm in our database of USDA farm program records. Mirroring our analysis of the overall effect of the trade war and MFP on turnout, we use DML to estimate the average difference in net Republican contributing between farmers and non-farmers after controlling for pre-treatment covariates —including each voter's history of political engagement. See Dataverse Materials G for a full list of controls.

Figure 12 presents estimated effects across four phases of the trade war. We find no statistically significant or substantively large differences in contribution behavior in the early months of the trade war leading up to retaliatory tariffs on agriculture. As such, we conclude that long-standing differences in political engagement between farmers and non-farmers are adequately accounted for through our covariate adjustment strategy, and larger differences in succeeding periods might plausibly reflect the causal effects of policy changes.

Following Trump's June 15 announcement of a tariff increase on Chinese imports, and a same-day announcement of retaliatory tariffs on U.S. agriculture, growers saw expected harvest-time prices for their crops collapse (see Figure 1). While there was no relief in sight over the next five weeks, there is no indication that this meaningfully affected the contribution behavior of farmers of any political stripe. Relative to PLR counterfactual

FIGURE 12. Overall Impact of Increased Policy Salience on Contributions: DML Estimates of Difference in Net Republican Contributing between Affected Farmers and Other Contributors



Note: Point estimates are depicted with 95% confidence intervals. To view these results in table form, see Dataverse Materials Section I.

baseline levels of support, average net Republican contributing was virtually unaffected among farmers with a Republican contribution history (0.0223 → 0.0224) and farmers with a mixed/bipartisan contribution history (–0.0013 → –0.0012). Net Republican contributing was 0.003 points below baseline (–0.086 → –0.089) among farmers with a history of contributing to Democrats. None of these estimated effects are statistically significant, though our confidence intervals do not allow us to rule out modest effects relative to the low baseline propensities to contribute in this short time interval. It is notable that collapsing futures prices did not spur a large share of active Democratic contributors to deliver a rebuke to Trump, and neither did they cause Republican farmers to hold off on funding Trump's co-partisans in the upcoming midterms.

Trump's July 24 announcement of $12 billion in funding for a relief package yielded a similarly mild response among politically active farmers. As with the period following the tariff announcement, we find no statistically significant effects on the contributions of affected farmers in the month following Trump's initial MFP announcement. Relative to PLR counterfactual baseline levels of support, average net Republican contributing was virtually unaffected among farmers with a Republican contribution history (0.0232 → 0.0239) and farmers with a mixed/bipartisan contribution history (0.0000 → –0.0001). Net Republican contributing was 0.003 points below baseline (–0.049 → –0.052) for farmers with a history of contributing to Democrats.

Finally, we do observe statistically significant differences between affected farmers and comparable nonfarmer contributors *after* MFP benefit rates were announced on August 27 and program enrollments began a week later on September 4. In the nine months leading up to the announcement of the 2019 MFP on May 24, 2019, net Republican contributing was 0.008 points above the counterfactual baseline for farmers with a Republican contribution history (0.101 → 0.109), 0.003 above for farmers with a mixed/bipartisan contribution history (–0.001 → 0.002), and 0.001 points higher for farmers with a Democratic contribution history (–0.2434 → –0.2428). These effects for farmers with Republican and mixed/nonpartisan contribution histories are statistically significant at the 1% and 5% levels (respectively), whereas the effect on Democratic farmers is statistically insignificant.[20]

Altogether, these estimates imply that politically active farmers may have modestly but noticeably increased support for Republican candidates after relief was tangible. In conjunction with our earlier results concerning 2018 voter turnout, we conclude that the broader salience of the policy shock had a greater impact on political engagement than each farmer's own particular policy outcomes.

[20] In Dataverse Materials Section H.7, we complement these estimates with a series of analyses looking at contribution rates to Republicans, Democrats, and Trump specifically.

DISCUSSION AND CONCLUSION

Our results in this case study cast doubt on the claim that an incumbent party can easily mobilize its base by delivering better economic policy outcomes. To the extent that this finding generalizes beyond our setting, it has important implications for how political scientists should view the electoral consequences and determinants of economic policymaking in the United States. In particular, if incumbents had a "fiscal policy dial" which they could turn to crank up core voter engagement, it would induce distinct incentives for economic policy design. Indeed, at the outset of this article, we noted that the literature on distributive politics in the United States finds that benefits tend to flow disproportionately to constituencies that already support the incumbent's party. However, without the prospect of increasing vote share through turnout or campaign contributions, prioritizing benefits to solid supporters presents a distinct trade-off. If the budgetary or political resources used to deliver such outcomes could have instead been employed to sway swing voters' choices at the ballot box, then the objective of maximizing electoral dividends might be far from paramount in economic policy design. Indeed, core voter targeting without any resulting impacts on core voter engagement might point toward an economic policy arena that is—as posited by Hacker and Pierson (2014)—less characterized by the "electoral connection" and more by the "*policy* connection that promotes and sustains coalitions of (partisan) politicians and organized interests" (emphasis in original, 644).

Of course, we note that it is not possible to draw such sweeping conclusions from a single case study alone. We do, however, maintain that our examination of the MFP is in many aspects an ideal test of the mobilizing capacity of good policy outcomes, and on the margin it should shift our priors regarding how much the design of economic policies can influence core voter engagement. For a major fiscal policy, the MFP was unusually targeted toward the incumbent president's partisan base. The trade war and MFP were both unprecedented and highly salient shocks to farmers' economic conditions. Attribution of partisan responsibility for these policies was about as easy as possible: not only did Trump unilaterally authorize the MFP and the tariffs that started the trade war, he openly campaigned on both and repeatedly took credit for them in rallies held throughout farm country. Ascribing credit for personal economic consequences to particular policy changes can often be difficult for voters, especially if benefits must first "trickle down" to voters through general equilibrium effects (as might be the case for infrastructure spending or subsidies for large corporations). However, farmers saw the effects of retaliatory tariffs first-hand as commodity prices moved, and the simple linear MFP payment formula—advertised months in advance of the midterms—made it relatively straightforward for farmers to gauge how policy was going to affect their bottom lines for the 2018/2019 marketing year. Indeed, we provide evidence that farmers did understand their individual policy circumstances, as both commodity group press releases and survey data show that farmers with soybean-heavy portfolios had much more positive policy attitudes than those with corn-heavy portfolios. It is notable, then, that we find very negligible effects on turnout and contributions from very salient differences in policy outcomes.

We note the importance of contributing this null result to the literature (Alrababa'h et al. 2023). It adds an important counterpoint to the vast majority of older studies which illustrate the electoral returns of distributive politics. In addition to leading us to update our priors, this study helps us see results from the full population of research designs, thereby reducing bias and enhancing knowledge accumulation.

Nonetheless, we must emphasize several caveats in interpreting our results. First, the type of policy variation we study has some key differences from the traditional pork-barrel spending (e.g., infrastructure projects) that takes such a central focus in debates over distributive politics. Our main estimates focus on the effects of short-term policy benefits, but voters may only be moved by economic policy shifts over a longer time horizon, such as uncertainty related to agricultural policy.

Moreover, the MFP was publicly justified as compensation for losses that Trump himself caused by starting a trade war, and both the over-compensation and under-compensation that we leverage arose from the administration arguably making a mistake in program design. However, we must note that this policy attribute is not altogether rare in U.S. economic policymaking (e.g., disaster relief can often be necessitated by insufficient preparation on the part of the incumbent). As growing policy challenges around globalization and climate change necessarily create economic winners and losers, the political ramifications of compensatory policies such as the MFP are increasingly relevant.

Finally, we note that the MFP's broader political salience (and that of the trade war itself) may have had spillovers outside of those employed in agriculture directly. One question we do not study in this article is how non-farmers living in agricultural areas reacted to the program, an important area for future research given the prospect of sociotropic and identity-based voting described above. While the MFP compensated producers quickly, this was not the case for agricultural communities more broadly, who may have suffered from the economic fallout of the immediate effects of the trade war without direct compensation. This could reconcile the lack of significant mobilization effects among farmers with prior county-level studies' findings that the 2018 agricultural policy shocks affected electoral outcomes in rural districts (Blanchard, Bown, and Chor 2019; Chyzh and Urbatsch 2021; Kim and Margalit 2021).

Returning to our opening discussion of trends in agrarian politics and rural political behavior, our results suggest that *The American Voter*'s depiction of farmers as "pocketbook voters" may not reflect contemporary reality. This may be a product of either identity-based concerns or sociotropic considerations taking a larger role in determining producers' political behavior. Indeed, congressional Democrats' support for agricultural assistance over President Bush's veto of the 2008 farm bill, coupled with President Obama's passage of the 2014 farm bill, seemed to have little efficacy in lifting Democrats' prospects in rural areas during the 2010 and 2014 midterm elections. This putative intransigence among the rural electorate may reflect national trends in economic voting, and it is possible that the mobilizing effects of distributive politics have more broadly declined over time and may no longer hold in an era marked by extreme ideological polarization. Indeed, cultural and identity-based concerns have increased in salience among voters and may now trump materialistic concerns as motivators of electoral participation (Ellis and Ura 2021).

To provide some initial evidence for this conjecture, we reanalyze data from Anzia, Jares, and Malhotra's (2022) summer 2020 survey of agricultural producers and show that noneconomic identity better explains attitudes toward Trump than economic policy outcomes (see Dataverse Materials Section H.9). Compared to farmers that prioritize economic identities, farmers with predominantly noneconomic identities were on average 0.056 more supportive of Trump's job performance on a 0–1 scale ($p = 0.03$) and 8.1 percentage points more likely to plan to vote for him ($p = 0.01$). On the other hand, MFP net benefit receipt did not significantly predict evaluations of Trump. Although these results are not based on a natural experiment, they suggest that further research into the salience of rural Americans' noneconomic identities may be a fruitful path forward in building on our broader findings.

To be clear, we do not interpret our findings as contrary to the claim that policy shocks can be important drivers of political engagement (Campbell 2012). Our analyses in "Broader Effects of the Trade War and MFP on Farmers' Political Engagement" suggest that the massive shift in farm policy in 2018 modestly increased overall turnout and campaign contributions among U.S. farmers. However, additional case studies of different types of policies in different settings are needed to further understand the nuances of how public policies affect voting behavior. In particular, political scientists need to conduct further large-scale studies that focus on gauging the substantive effect sizes of various policy interventions. By cobbling together a diverse array of such studies, scholars of political behavior may come to have a deeper understanding of how and when economic policy matters to voters.

SUPPLEMENTARY MATERIAL

To view supplementary material for this article (including the "Online Appendix"), please visit https://doi.org/10.1017/S0003055424000571. Additionally, the secondary appendix ("Dataverse Materials") is available in this article's Dataverse, available at https://doi.org/10.7910/DVN/S7TOGV.

DATA AVAILABILITY STATEMENT

Research documentation and data that support the findings of this study are openly available at the American Political Science Review Dataverse: https://doi.org/10.7910/DVN/S7TOGV.

ACKNOWLEDGMENTS

We thank Michael Adjemian, Sarah Anzia, Adam Bonica, Olga Chyzh, Jordan Gans-Morse, Andrew Hall, Caroline Hoxby, Sung Eun Kim, Apoorva Lal, Dale Lassek, Rebecca Lester, Robin Lincare, Yotam Margalit, Simeon Nichter, Jonathan Rodden, Robert Urbatsch, and Wendong Zhang for their assistance with this study. We acknowledge participants at seminars at the American Political Science Association, Harvard University, Yale University, the Stanford GSB PhD Alumni Conference, and the Stanford Democracy and Polarization Lab for valuable feedback.

CONFLICT OF INTEREST

The authors declare no ethical issues or conflicts of interest in this research.

ETHICAL STANDARDS

The authors declare the human subjects research in this article was deemed exempt from review by the Stanford University Internal Review Board. The human subjects determination form was submitted and approved as Protocol 64021.

REFERENCES

Abbott, Chuck. 2018. "Administration Earmarks up to $12 Billion for Agriculture to Offset Trade War." *Successful Farming*, July 24.

Adjemian, Michael K., Aaron Smith, and Wendi He. 2021. "Estimating the Market Effect of a Trade War: The Case of Soybean Tariffs." *Food Policy* 105: 102152.

Alrababa'h, Ala', Scott Williamson, Andrea Dillon, Jens Hainmueller, Dominik Hangartner, Michael Hotard, David D. Laitin, et al. 2023. "Learning from Null Effects: A Bottom-Up Approach." *Political Analysis* 31 (3): 448–56.

American Soybean Association. 2018. "Soybean Farmers Thankful for Final Installment of Market Facilitation Aid." Press Release. https://soygrowers.com/news-releases/soybean-farmers-thankful-final-installment-market-facilitation-aid/.

Ansolabehere, Stephen, and James M. Snyder. 2006. "Party Control of State Government and the Distribution of Public Expenditures." *Scandinavian Journal of Economics* 108 (4): 547–69.

Anzia, Sarah F., Jake Alton Jares, and Neil Malhotra. 2022. "Does Receiving Government Assistance Shape Political Attitudes? Evidence from Agricultural Producers." *American Political Science Review* 116 (4): 1389–406.

Battocchi, Keith, Eleanor Dillon, Maggie Hei, Greg Lewis, Paul Oka, Miruna Oprescu, and Vasilis Syrgkanis. 2019. "EconML: A Python Package for ML-Based Heterogeneous Treatment Effects Estimation." https://github.com/microsoft/EconML.

Blanchard, Emily J., Chad P. Bown, and Davin Chor. 2019. "Did Trump's Trade War Impact the 2018 Election?" *NBER Working Paper* 26434. http://www.nber.org/papers/w26434.

Bonica, Adam. 2014. "Mapping the Ideological Marketplace." *American Journal of Political Science* 58 (2): 367–86.

Bonica, Adam, and Jacob M. Grumbach. 2022. "Old Money: Campaign Finance and Gerontocracy in the United States." Working Paper.

Campbell, Andrea Louise. 2012. "Policy Makes Mass Politics." *Annual Review of Political Science* 15: 333–51.

Campbell, Angus, Philip E. Converse, Warren E. Miller, and Donald E. Stokes. 1960. *The American Voter*. Chicago, IL: University of Chicago Press.

Chen, Jowei. 2013. "Voter Partisanship and the Effect of Distributive Spending on Political Participation." *American Journal of Political Science* 57 (1): 200–17.

Chernozhukov, Victor, Denis Chetverikov, Mert Demirer, Esther Duflo, Christian Hansen, Whitney Newey, and James Robins. 2018. "Double/Debiased Machine Learning for Treatment and Structural Parameters." *Econometrics Journal* 21 (1): C1–68.

Chyzh, Olga V., and Robert Urbatsch. 2021. "Bean Counters: The Effect of Soy Tariffs on Change in Republican Vote Share between the 2016 and 2018 Elections." *Journal of Politics* 83 (1): 415–19.

Cox, Gary W. 2009. "Swing Voters, Core Voters, and Distributive Politics." In *Political Representation*, ed. Ian Shapiro, 342–57. Cambridge: Cambridge University Press.

Cramer, Katherine J. 2016. *The Politics of Resentment: Rural Consciousness in Wisconsin and the Rise of Scott Walker*. Chicago, IL: University of Chicago Press.

Dahlgaard, Jens Olav, Jonas Hedegaard Hansen, Kasper M Hansen, and Yosef Bhatti. 2019. "Bias in Self-Reported Voting and How it Distorts Turnout Models: Disentangling Nonresponse Bias and Overreporting among Danish Voters." *Political Analysis* 27 (4): 590–8.

Eller, Donelle. 2018. "Did Trump Violate Deere's Trademark with His 'Make Our Farmers Great Again' Hat?" *The Des Moines Register,* July 27.

Ellis, Christopher R., and Joseph Daniel Ura. 2021. "Polarization and the Decline of Economic Voting in American National Elections." *Social Science Quarterly* 102 (1): 83–9.

Green, Donald P., and Alan S. Gerber. 2019. *Get Out the Vote: How to Increase Voter Turnout*. Washington, DC: Brookings Institution Press.

Gulotty, Robert, and Anton Strezhnev. 2024. "The Political Benefits of the Monoculture: Estimating the Electoral Effect of the Market Facilitation Program." Working Paper. https://osf.io/preprints/socarxiv/b84yn.

Hacker, Jacob S., and Paul Pierson. 2014. "After the "Master Theory": Downs, Schattschneider, and the Rebirth of Policy-Focused Analysis." *Perspectives on Politics* 12 (3): 643–62.

Jacobs, Nicholas, and B. Kal Munis. 2023. "Place-Based Resentment in Contemporary US Elections: The Individual Sources of America's Urban-Rural Divide." *Political Research Quarterly* 76 (3): 1102–18.

Jacobson, Gary C. 2019. "Extreme Referendum: Donald Trump and the 2018 Midterm Elections." *Political Science Quarterly* 134 (1): 9–38.

Janzen, Joseph P., and Nathan P. Hendricks. 2020. "Are Farmers Made Whole by Trade Aid?" *Applied Economic Perspectives and Policy* 42 (2): 205–26.

Jares, Jake Alton, and Neil Malhotra. 2024. "Replication Data for: Policy Impact and Voter Mobilization: Evidence from Farmers' Trade War Experiences." Harvard Dataverse. Dataset. https://doi.org/10.7910/DVN/S7TOGV.

Kim, Sung Eun, and Yotam Margalit. 2021. "Tariffs as Electoral Weapons: The Political Geography of the US–China Trade War." *International Organization* 75 (1): 1–38.

Kriner, Douglas L., and Andrew Reeves. 2015. "Presidential Particularism and Divide-the-Dollar Politics." *American Political Science Review* 109 (1): 155–71.

Lewis-Beck, Michael S. 1977. "Agrarian Political Behavior in the United States." *American Journal of Political Science* 21 (3): 543–65.

Li, Minghao, Xi He, Wendong Zhang, Shuyang Qu, Lulu Rodriguez, and James M. Gbeda. 2023. "Farmers' Reactions to the US–China Trade War: Perceptions versus Behaviors." *Journal of the Agricultural and Applied Economics Association* 2 (3): 386–401.

National Corn Growers Association. 2018. “NCGA: USDA Trade Aid Comes Up Short, Again.” Press Release.

Nicholson-Crotty, Sean. 2015. *Governors, Grants, and Elections: Fiscal Federalism in the American States*. Baltimore, MD: Johns Hopkins University Press.

Nova, Annie. 2022. “‘No Different than the Mortgage Crisis’: NAACP President Derrick Johnson on Why Student Loan Cancellation is Needed.” *CNBC,* June 6.

Panagopoulos, Costas. 2016. “All about that Base: Changing Campaign Strategies in U.S. Presidential Elections.” *Party Politics* 22 (2): 179–90.

Rainey, Carlisle. 2014. “Arguing for a Negligible Effect.” *American Journal of Political Science* 58 (4): 1083–91.

Ratkovic, Marc. 2023. “Relaxing Assumptions, Improving Inference: Integrating Machine Learning and the Linear Regression.” *American Political Science Review* 117 (3): 1053–69.

Regmi, Anita. 2019. “Retaliatory Tariffs and U.S. Agriculture.” Congressional Research Service Report R45903. https://fas.org/sgp/crs/misc/R45903.pdf.

Robinson, P. M. 1988. “Root-N-Consistent Semiparametric Regression.” *Econometrica* 56 (4): 931–54.

Rodden, Jonathan A. 2019. *Why Cities Lose: The Deep Roots of the Urban-Rural Political Divide*. New York: Basic Books.

USDA National Agricultural Statistics Service. 2018. “Crop Progress Report for 06/18/2018.”

USDA Office of the Chief Economist. 2018. “Trade Damage Estimation for the Market Facilitation Program and Food Purchase and Distribution Program.”.

Verba, Sidney, Kay Lehman Schlozman, and Henry E Brady. 1995. *Voice and Equality: Civic Voluntarism in American Politics*. Cambridge, MA: Harvard University Press.