

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

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## Abstract

How does the extent of policy benefits—not simply their presence or absence—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, 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 whether farmers viewed the intervention as helpful. Our results suggest that even highly salient variation in policy outcomes may have limited mobilizing capacity in a polarized environment.

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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 increasing 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 US-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 US, as incumbents in various institutional settings tend to steer disproportionate economic policy benefits towards reliable partisan allies (Kriner and Reeves, 2015; Nicholson-Crotty, 2015; Ansolabehere and Snyder, 2006). Given the relative stability of US

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” (p. 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 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 non-response 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 non-response can make obtaining representative samples difficult. Survey data is 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 administra-

tive 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 Market Facilitation Program (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.

Finally, our paper also speaks to the longstanding literature in political science on agrarian political behavior (Campbell et al., 1960; Lewis-Beck, 1977).<sup>1</sup> Better 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 “pocket-book” 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

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<sup>1</sup>Our study is also somewhat related to prior papers 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 Urbatsch, 2021; Kim and Margalit, 2021; Ritchie and You, 2021). We connect our findings to this literature in the discussion.

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 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, 2022), 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.”

## 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 U.S. and China. In early July, the Chinese government implemented retaliatory tariffs on nearly all U.S. agricultural products.<sup>2</sup> 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

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<sup>2</sup>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 U.S. and China.

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, cotton) ultimately earned the lion’s share of MFP payments, and are the focus of this study.<sup>3</sup>

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.<sup>4</sup> 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 two weeks after the 25% tariff took effect on July 6.<sup>5</sup> 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

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<sup>3</sup>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.

<sup>4</sup>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 U.S. 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  $(\$956 \text{ million} - \$642 \text{ million}) / (364 \text{ million bushels})$ , or \$0.86 per bushel (USDA Office of the Chief Economist, 2018).

<sup>5</sup>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).



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 agricultural economists estimating the price impacts of the 2018 retaliatory tariffs (see Online Appendix A.4 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 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.

**Table 1: MFP Compensation Levels by Crop**

	Tariff Price Impact ( $A$ )	MFP Rate ( $B$ )	Net Benefit ( $A + B$ )	Compensation Ratio $ B/A $
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.

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 NASS 2018a). 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 US soybean export prices did not diverge from Brazilian export prices until the date of the tariff announcement (Regmi, 2019). Indeed, aggregate planting data confirms 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.<sup>6</sup>

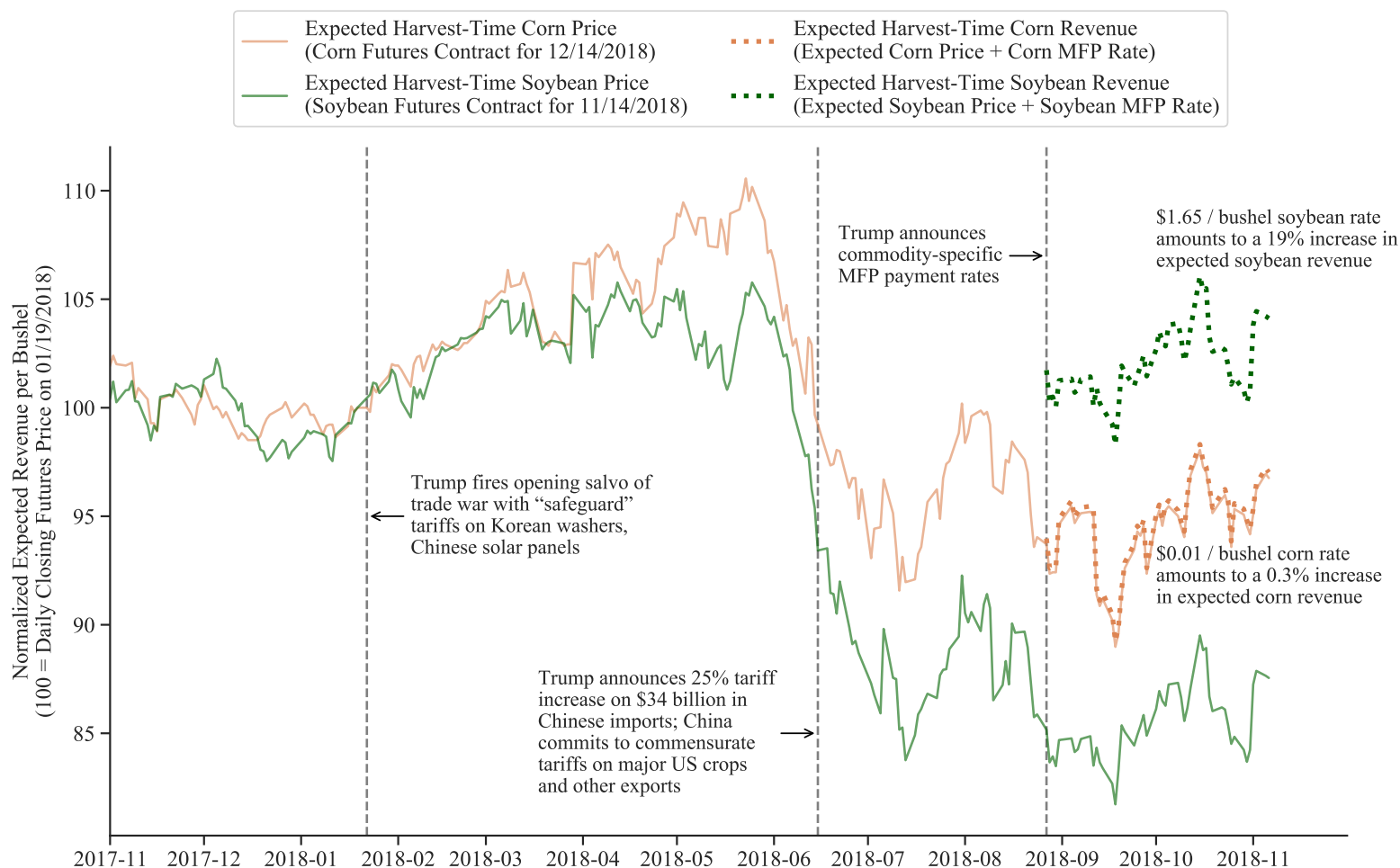
Farmers’ variation in policy outcomes in 2018 was therefore both unanticipated and imposed through idiosyncratic treatment of particular crop portfolios. As such, it presents a valuable test case for evaluating the salience of policy outcomes for partisan mobilization. Through press releases, interviews, and campaign events, President Trump repeatedly took credit for both the trade war and his Market Facilitation Program. As we show in Further Supplemental Materials Section C (Table FSM1 and Figure FSM6), 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.<sup>7</sup>

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<sup>6</sup>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>.

<sup>7</sup>An anonymous version of the Further Supplemental Materials can be found at: <https://osf.io/esarh/>

**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



Notes: 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 01/19/2018, 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 08/27/2018 (the date on which the payment rates were announced).

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 (ASA 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 addressed realized harms from the trade war (NCGA 2018).

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**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



Notes: See Further Supplemental 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.

# 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.<sup>8</sup> 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 Online Appendix A.1 for a discussion of these aspects of our sample

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<sup>8</sup>See Further Supplemental Materials Section E for details.

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 Further Supplemental Materials Section E. By using voter file snapshots from both February 2018 and June 2019, we obtain farmers’ 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.<sup>9</sup>

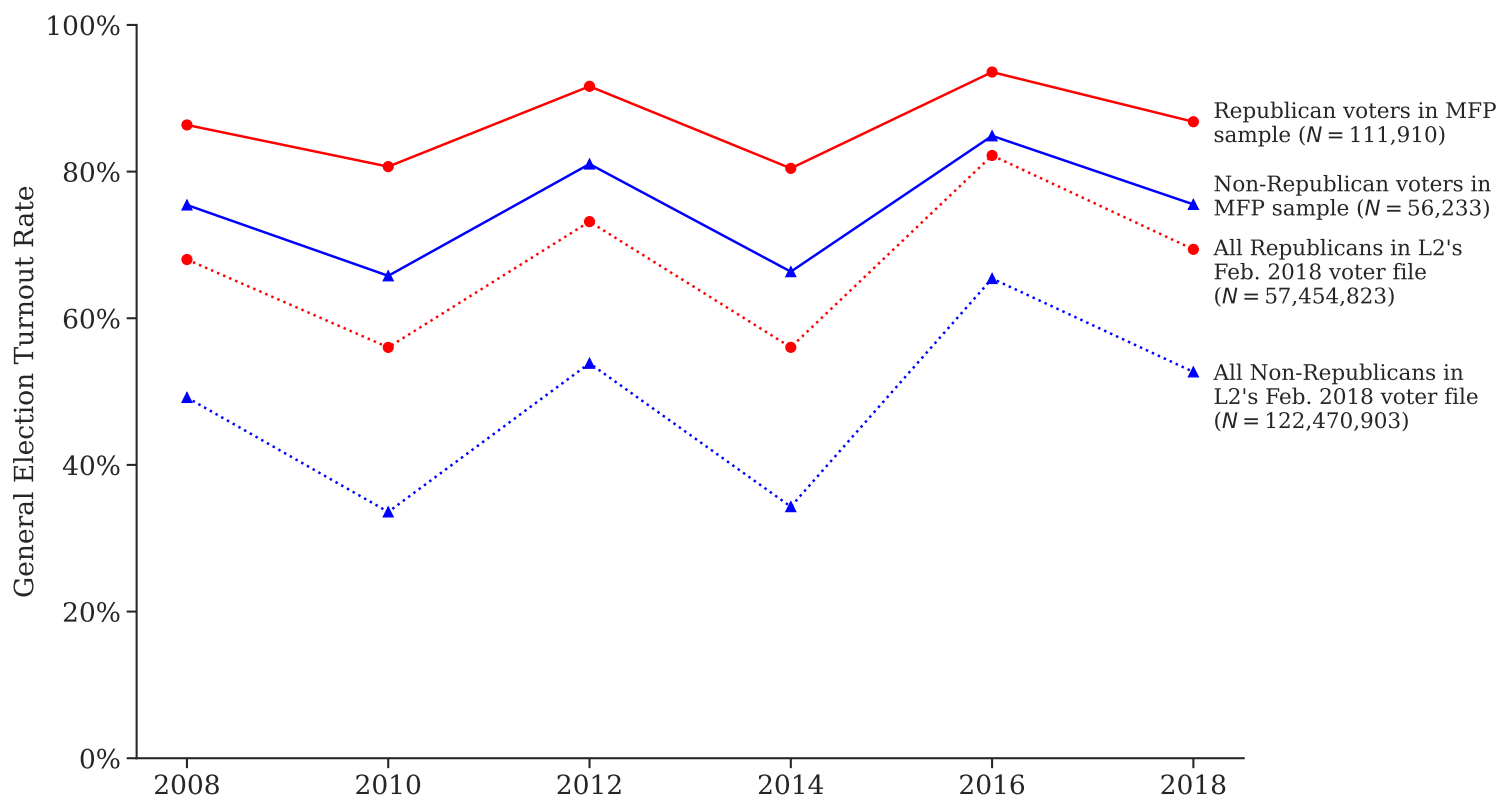
We link this resulting set of 122,157 farms to the DIME 4.0 database of itemized political contributions spanning 1979-2020. The DIME database employs an entity resolution algorithm to assign groups of transactions to distinct contributor identification numbers (IDs). For the purpose of our main analyses, we match specific contributor IDs from DIME to distinct farms (rather than distinct voters), because campaign contributions may often be a joint household

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<sup>9</sup>L2 estimates likely party affiliation in states in which voters do not declare a partisan preference; see Online Appendix A.2 for more details.

decision. As discussed in Further Supplemental 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.

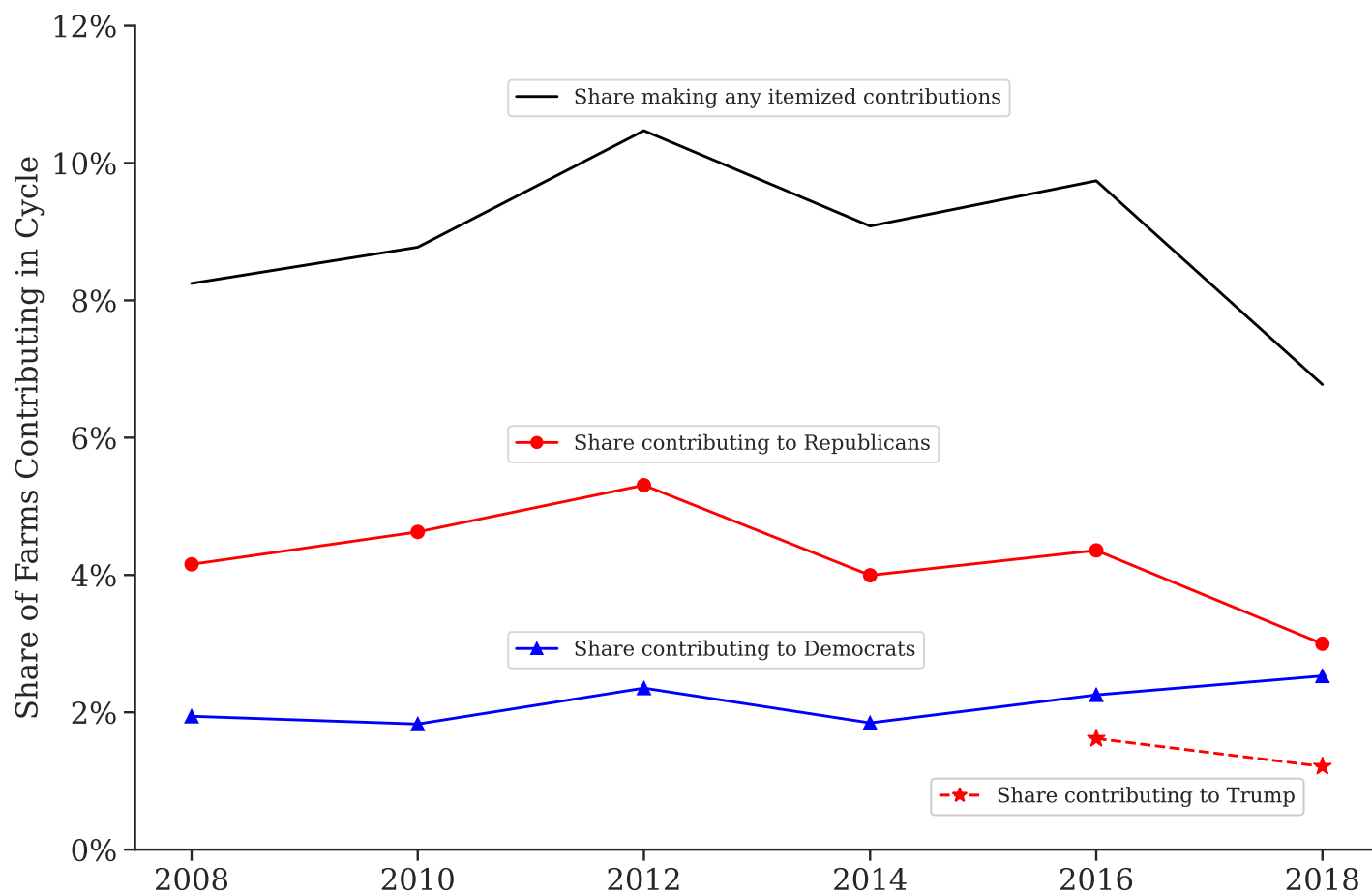
**Figure 3:** General Election Turnout Rates among Voters Linked to CY 2018 MFP Sample of Farms, with Comparison to Broader Electorate



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 levels of political engagement relative to the broader electorate. However, there was still substantial room for improved mobilization in 2018: roughly one in



**Figure 4:** Farm-Level Contribution Rates by Cycle among CY 2018 MFP Sample of Farms



Notes: 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.

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 US 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. As we observe the MFP payment amount each farm received for each covered crop, and as farmers were paid 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.<sup>10</sup> We combine these implied harvest quantities with pre-trade war price forecasts to calculate expected harvest value. We then take the average estimated trade war price impacts presented in Table 1, and estimate each MFP participant  $i$ 's tariff-induced losses as

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

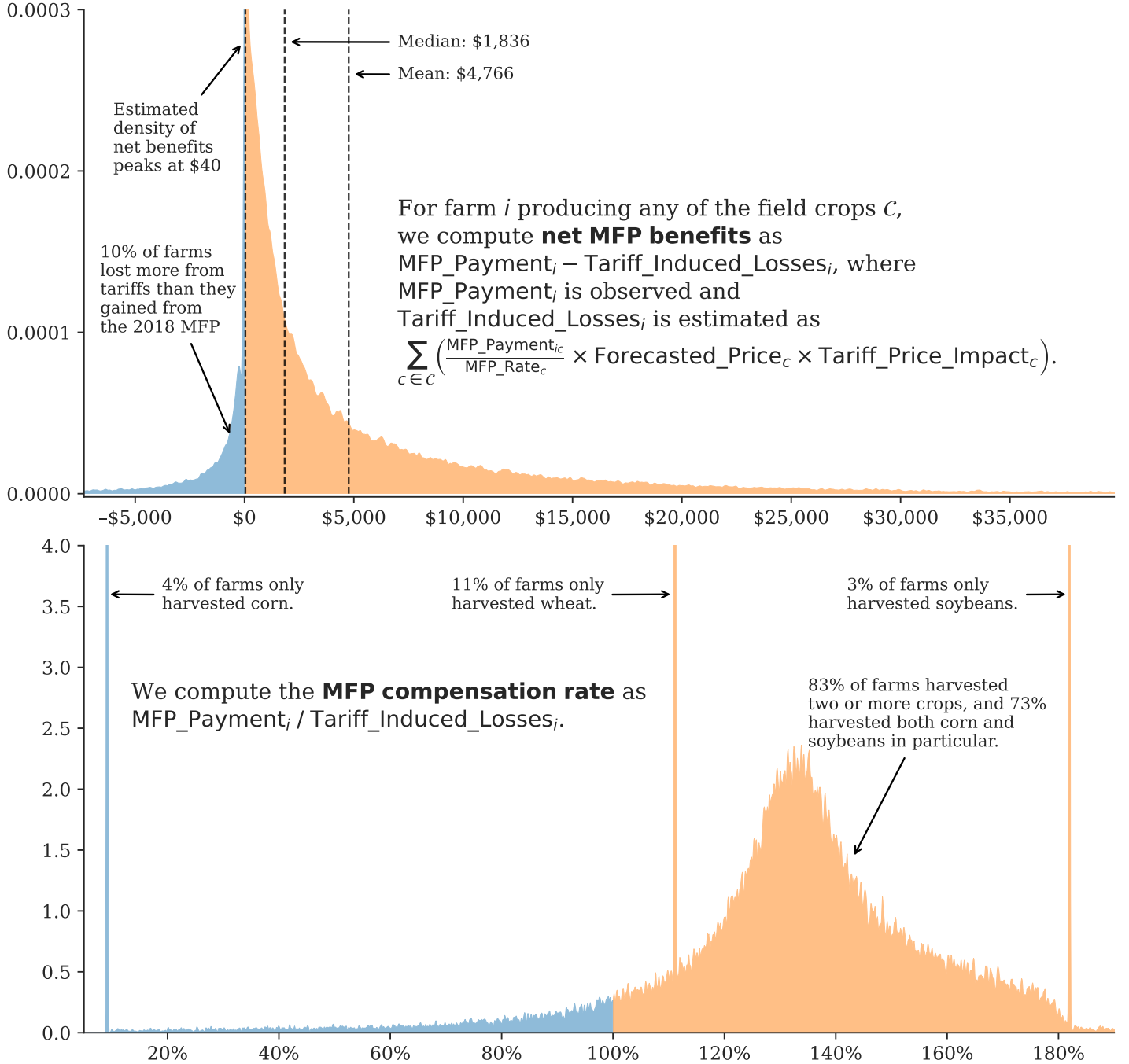
where  $\mathcal{C} = \{\text{corn, cotton, sorghum, soybeans, wheat}\}$  and  $\text{Tariff\_Price\_Impact}_c \in (0, 1)$  is the relative 2018 price decline attributed to the trade war. See Online Appendix A.4 for further details regarding these calculations.

We calculate each farm's net MFP benefits by subtracting these estimates from their actual

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<sup>10</sup>See Further Supplemental Materials B for a validation of our approach to measuring farm-level production by backing out the basis of farm program payments.

**Figure 5:** Distribution of Net MFP Benefits and Compensation Rates Across 122,157 Farms that Harvested Field Crops and Participated in the 2018 MFP



Notes: Kernel density estimation was conducted using a Gaussian kernel. For improved readability, y-axes are truncated at 0.0003 and 4, respectively, and x-axes are truncated at 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.

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 12,000 sample farms were not made whole.

In Further Supplemental 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,<sup>11</sup> changes in this measure parsimoniously reflect shifts in farms' political engagement in favor of the party responsible for both the trade war and 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 non-political 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 longstanding farm size.<sup>12</sup>

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<sup>11</sup>Only 42 out of 122,157 farms in the main analysis sample contributed to both Republicans and Democrats between 08/27/2018 and 05/23/2019, whereas 1,528 farms contributed solely to Republicans and 1,822 contributed solely to Democrats.

<sup>12</sup>As we explain in Online Appendix A.5, we do this by estimating acreage enrolled in DCP/ACRE—the USDA's flagship safety net programs for field crops between 2009 and 2013—from each farm's payment records, known commodity-specific rates, and county-level yields.

## 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 (08/27/2018–05/23/2019). 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 assume that variation in compensation outcomes is unconfounded conditional on prior levels of political engagement and observed pre-treatment 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 Further Supplemental 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 Online Appendix B 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 a “kitchen sink” 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 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. However, it greatly simplifies our analysis while closely mirroring the familiar OLS approach to “adjusting for” pre-treatment 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 a kitchen sink OLS regression is that its implicit use of auxiliary OLS regressions to estimate the conditional expectations for turnout and compensation rate is arbitrary and likely suboptimal.<sup>13</sup> 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 partially linear regression (PLR) model of farmers’ turnout decisions, which we estimate separately for Republicans and non-Republicans.

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

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

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

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<sup>13</sup>As we show in Further Supplemental Materials Section F.1, both simple linear models and “kitchen sink” specifications provide unsatisfactory approximations of the conditional expectation of a voter’s engagement given observed controls.

estimate of the linear causal effect  $\theta$  of better policy outcomes. The essence of the DML approach is best seen by rewriting Equations (1) and (2) via the classic Robinson (1988) transformation:

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

Estimation of the conditional expectation functions  $q(X_i) \equiv \mathbb{E}[Y_i | X_i]$  and  $m(X_i) = \mathbb{E}[B_i | X_i]$  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  $\theta$ .

While our main focus is on estimating the constant marginal effect  $\theta$  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  $\theta$  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 Further Supplemental Materials Section F.2.

## The Effect of Improved Policy Outcomes on Voter Turnout

Figure 6 depicts our main results on voter turnout. For each of the policy outcome “treatments” we consider, we plot DML estimates of the (linear) effect coefficient  $\theta$  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.<sup>14</sup> 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 percentage points. Likewise, a 100 percentage point increase in the compensation rate (as a share of tariff-induced losses) nets an increase of 0.2 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 0.3 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.

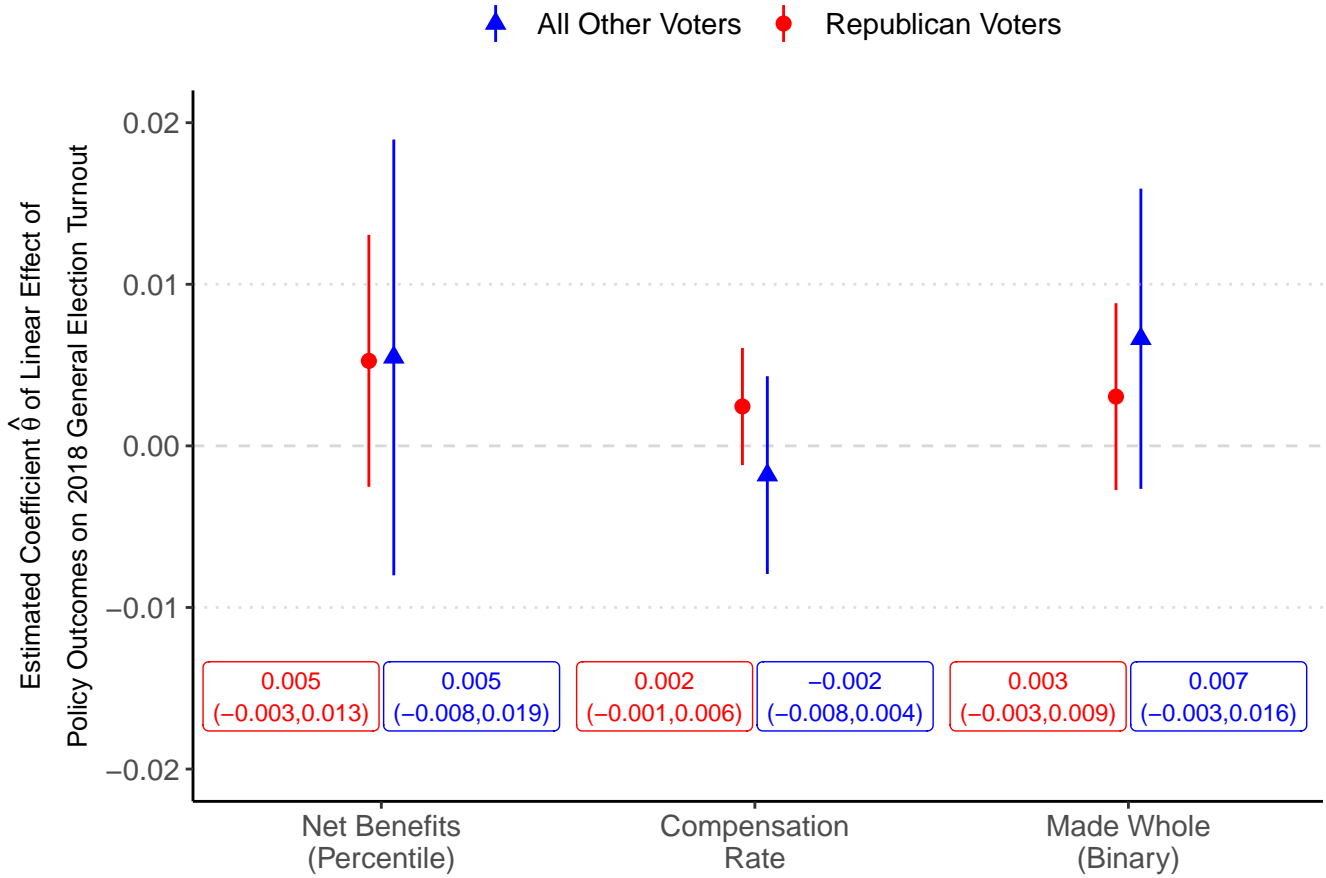
Since our sample of registered voters has a relatively high baseline turnout rate, we also estimate this effect allowing for heterogeneity by prior turnout. Figure 7 presents estimated effects with separate slopes for individuals who voted in the 2014 elections and those who abstained. We find no tangible difference in the effects between these two groups, and thus

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<sup>14</sup>All reported  $p$ -values are two-tailed.

conclude that it is very unlikely a ceiling effect is mechanically driving our null results.

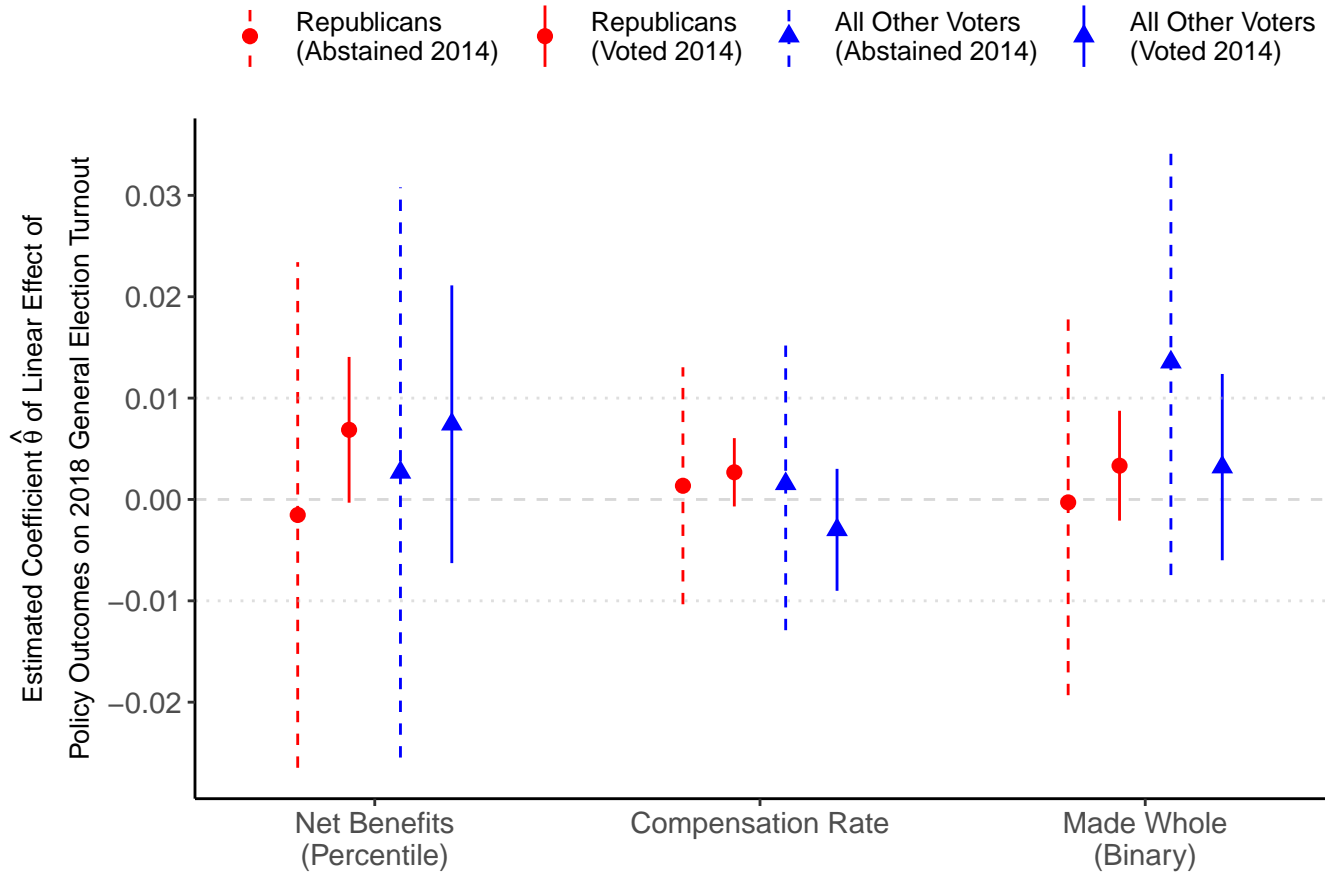
**Figure 6:** Estimated Effects of Improved Policy Outcomes on 2018 Turnout by Party



Notes: 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).

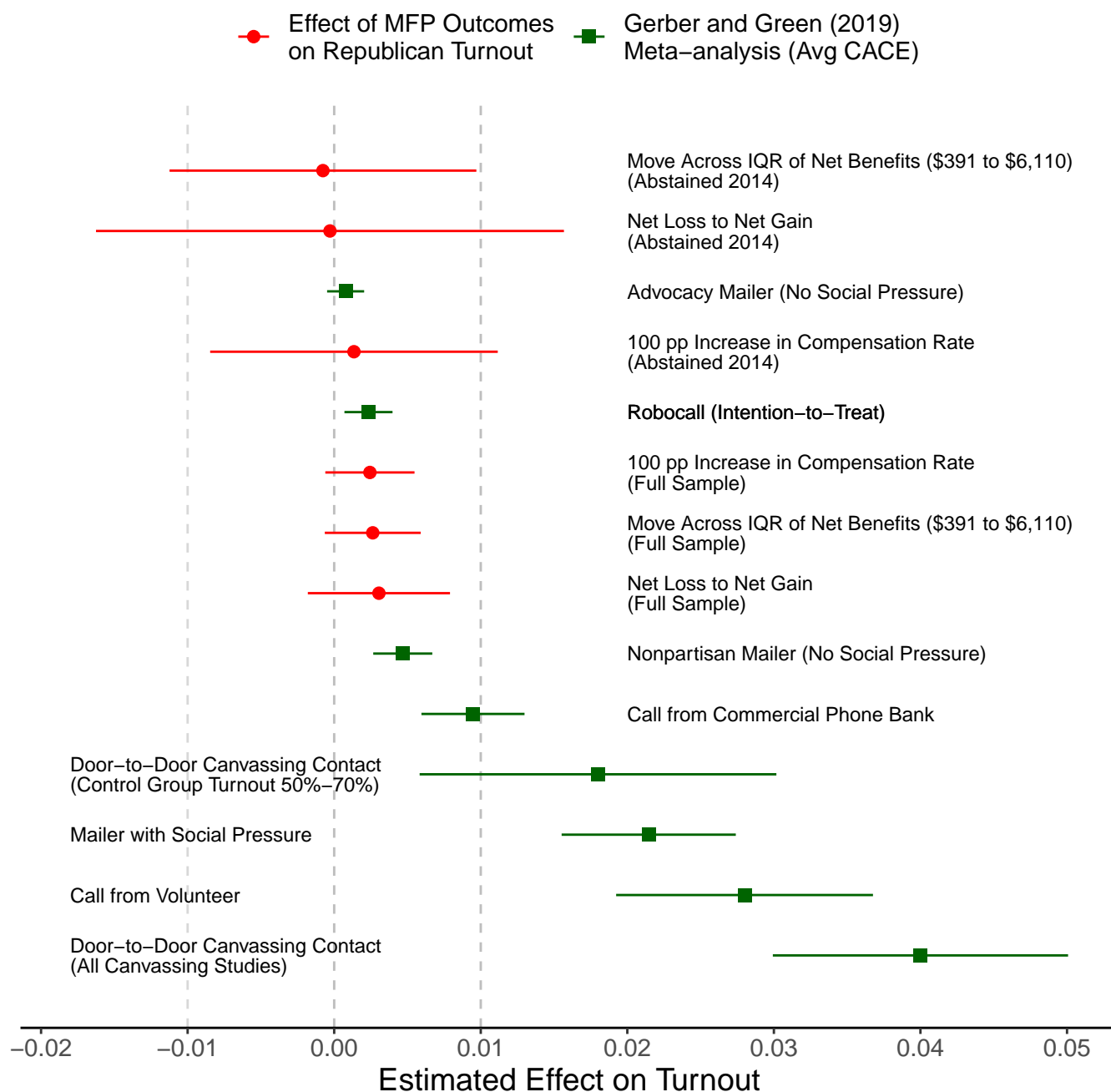
Following Rainey’s (2014) suggestions for arguing for a negligible effect, we conduct “two one-sided test” (TOST) analyses to formally demonstrate that our estimated effect sizes are smaller than proposed bounds on substantively meaningful effects. As Rainey (2014) notes, this is most simply implemented 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

**Figure 7:** Effect of Policy Outcomes on 2018 Turnout (Heterogeneity by Past Turnout)



Notes: 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 Further Supplemental Materials Section I.

**Figure 8:** TOST Analysis for Republican Turnout Effects with Comparison to Meta-Analytic Estimates of Campaign Activity Effectiveness



Notes: Point estimates are depicted alongside 90% confidence intervals. CACE refers to “complier average causal effects”; see Appendices A, B, and C of Green and Gerber (2019) for details on the studies and methodology underlying these meta-analytic estimates.

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 one percentage point, since only the weakest, lowest-cost campaign interventions (e.g., robocalls, 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 (0.1 percentage points among farmers who abstained in 2014, 0.2 percentage points among the full sample) is roughly as large as the average CACE of an advocacy mailer lacking social pressure (0.1 percentage points) or a robocall (0.2 percentage points). Moreover, it is noticeably smaller than the average CACE of a nonpartisan mailer lacking social pressure (0.5 percentage points), 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). Furthermore, this contrast is not simply an artifact of lower baseline turnout among typical experimental samples. Among experiments with 50-70% control group turnout—comparable 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 supplemental 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 Further Supplemental Materials Section H. To summarize our conclusions: (1) we obtain very similar estimates to our main DML results using a simple OLS specification; (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; and (6) we find no evidence that the relationship between payments and turnout is asymmetric with respect to gains and losses. Additionally, in Further Supplemental Materials 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, and nonparametric estimates of heterogeneous effects by date of enrollment do not uncover even a short-lived spike in mobilization effects ahead of Election Day.

## **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

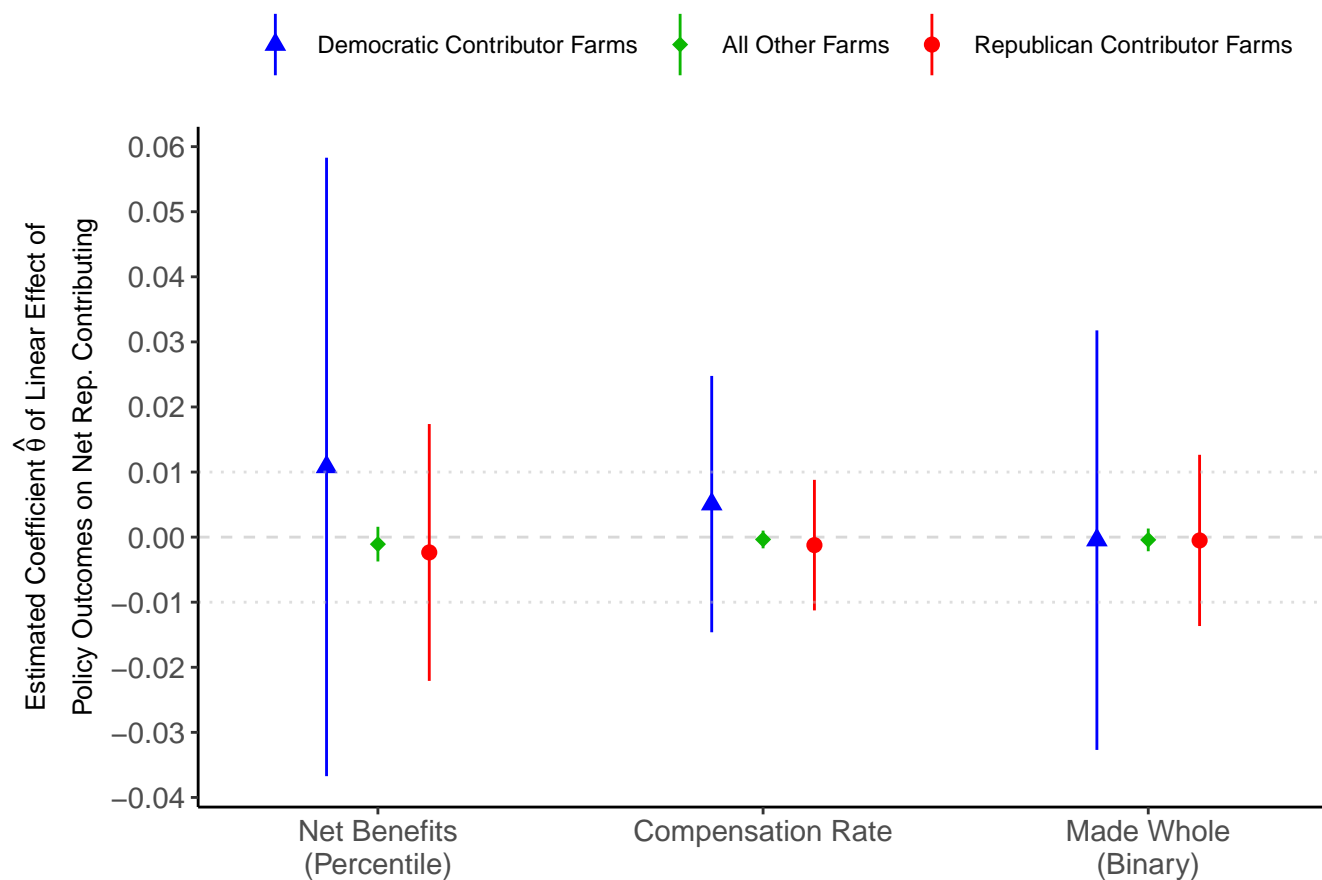


serve as a proxy for higher forms of political engagement, such as canvassing, organizing, and opinion leadership.

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 partially linear regression 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, non-partisan recipients, and Trump in each quarter between 2005 and 2017. To directly control for the longstanding 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 Online Appendix B 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 Online Appendix A.3 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:** Estimated Effects of Improved Policy Outcomes on Net Republican Contributing by Prior Contribution Behavior



Notes: 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 Further Supplemental Materials Section I.

Figure 9 presents our main results from regressing net Republican contributing on each of our three treatments of interest. As with the turnout analyses, we find no tangible evidence that large improvements in farmers’ trade war outcomes affected political engagement benefiting Republican candidates. This is most clear among farms with a distinctly Republican pre-2018 contribution history, whose average net Republican contributing was 0.073 across our period of interest following the MFP commodity rate announcements (08/27/2018–05/23/2019). 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. None of these estimates are statistically different from zero at the 5% level.

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 an 0.005 point increase in net Republican contributing. We likewise estimate a 0.005 point effect for a 100 percentage point increase in the compensation rate, and a  $-0.0005$  point effect for making a farm whole. Again, none of these estimates are statistically significant.

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, -0.0004, and -0.0004, 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 that we lack room to discuss in the main text. While net Republican contributing provides a parsimonious measure of contribution behavior, we demonstrate in Further Supplemental Materials 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 for contribution estimates 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 no meaningful effects 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 Qu et al.'s (2019) February 2019 survey of 693 corn and soybean growers in Iowa, Illinois, and Minnesota,<sup>15</sup> variation in the generosity of the MFP across different planted crop portfolios was clearly reflected in the views of individual farmers.

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<sup>15</sup>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.

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 Online Appendix A.6 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,” 42% “somewhat helpful,” 19% “quite helpful,” and 26% “very helpful.”<sup>16</sup> To examine how farmers’ attitudes towards 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 2013-2017. 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.

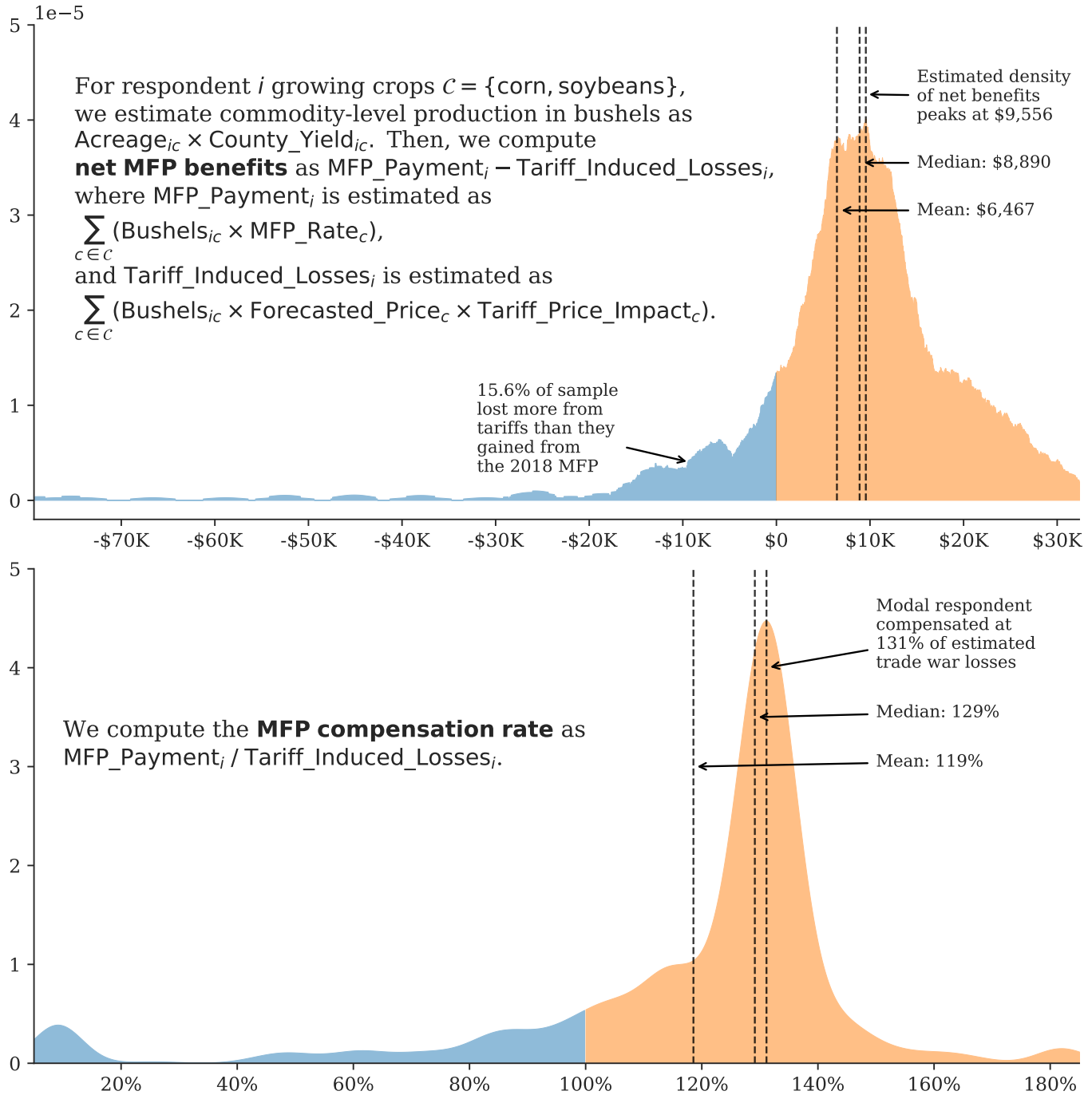
Consistent with the overcompensation of soybeans and undercompensation of corn documented in Table 1, we find that these four-point assessments of the helpfulness of the MFP are strongly correlated with each farm’s soybean share of planted corn/soy acreage. Moreover, perceived MFP helpfulness is separately 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 Further Supplemental Materials H.4.

Instead, we focus in this section on the same three treatment measures considered in

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<sup>16</sup>7% of respondents reported “not sure.” See Further Supplemental Materials H.4 for analyses incorporating these responses.

**Figure 10:** Distribution of Net MFP Benefits and Compensation Rates Among Qu et al. (2019) Survey Respondents



Notes: Kernel density estimation was conducted using a Gaussian kernel. For improved readability, x-axes are truncated at 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.

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 (\$4,106 to \$14,331) is associated with a 0.4 point increase in perceived helpfulness. Given that the total 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 "Institutional Background," Chinese retaliatory tariffs and the MFP compensation 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 corroborates this claim. Among sample members, corn and soybeans averaged 54% and 44% (respectively) of planted acreage for 2013-2017. These shares stood unchanged for 2018. Second, the bivariate regression estimates

**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	0.827*** (0.135)	0.702*** (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 2013-2017)	—	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 (Five-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	—	0.401* (0.235)	—	0.345 (0.233)	—	0.398* (0.231)
Other Livestock	—	-0.148 (0.189)	—	-0.097 (0.184)	—	-0.091 (0.187)
Intercept	1.293*** (0.077)	1.481*** (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
Observations	575	575	575	575	575	575
R-squared	0.063	0.118	0.040	0.120	0.021	0.107

Notes: 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).



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 geographical variation in positivity towards government programs. Further, the results are robust to controlling for farmers’ average corn/soy portfolios across the previous five years. As discussed in Further Supplemental 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.

Although net MFP benefits were recognized by producers, that does not necessarily mean that they made producers more positive toward Trump’s economic policies generally. To assess this, we estimated an analogous set of regressions, but predict support for Trump’s agricultural tariffs, a question that was also asked by Qu et al. (2019).<sup>17</sup> As shown in Tables FSM18 and FSM19 in Further Supplemental Materials H.5, none of our measures of benefits positively predicts tariff support. This may suggest that the disconnect between political attitudes and political behavior that we document may have stemmed from intransigent policy positions.

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 could not simply be due to a lack of understanding about the MFP or its helpfulness.

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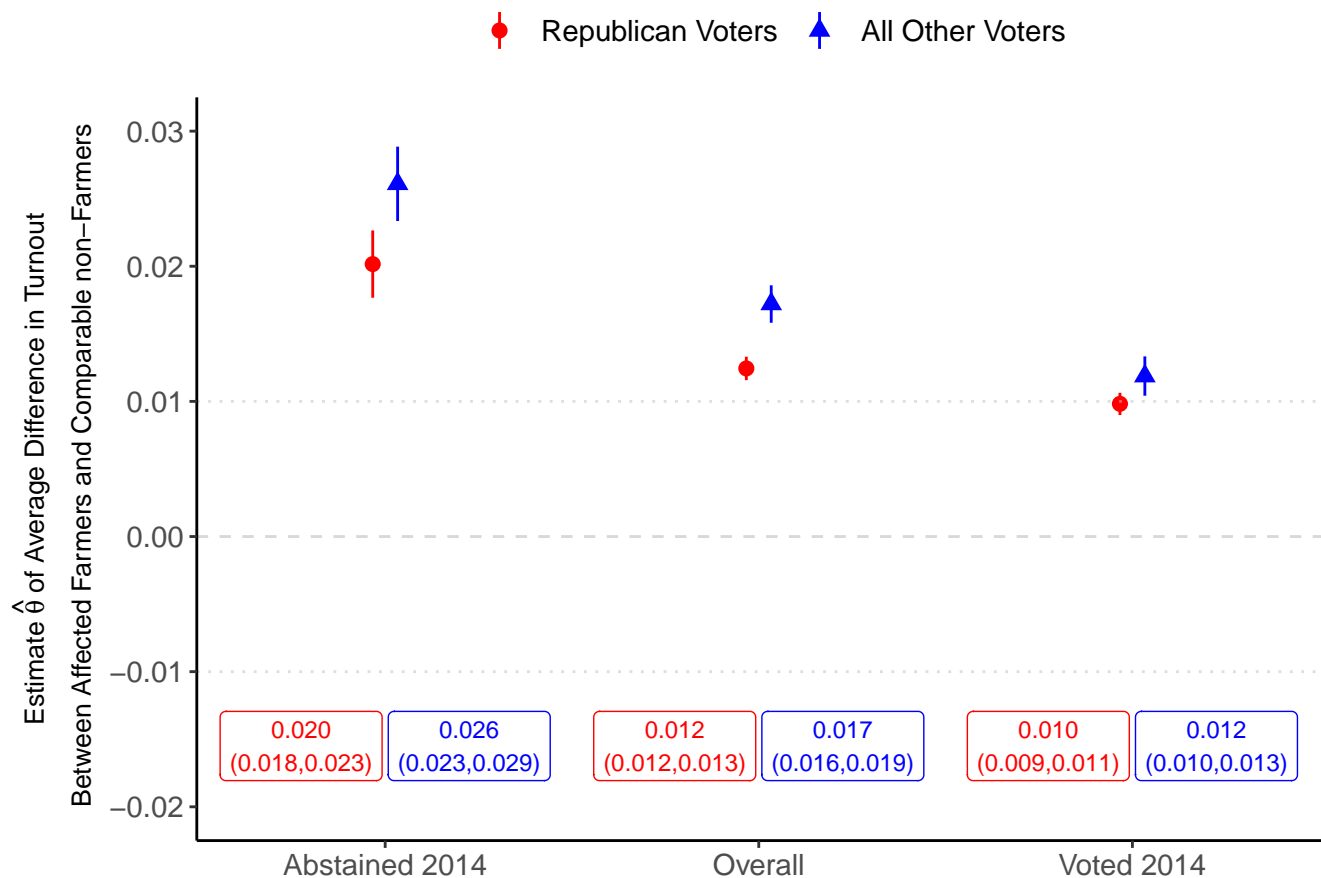
<sup>17</sup>Producers were asked “To what extent do you support or oppose raising tariffs on products imported from China?” with response options ranging from “strongly oppose” to “strongly support.”

# 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 US-China trade war was a once-in-a-generation scale shock to US 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 towards 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 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

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



Notes: 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.

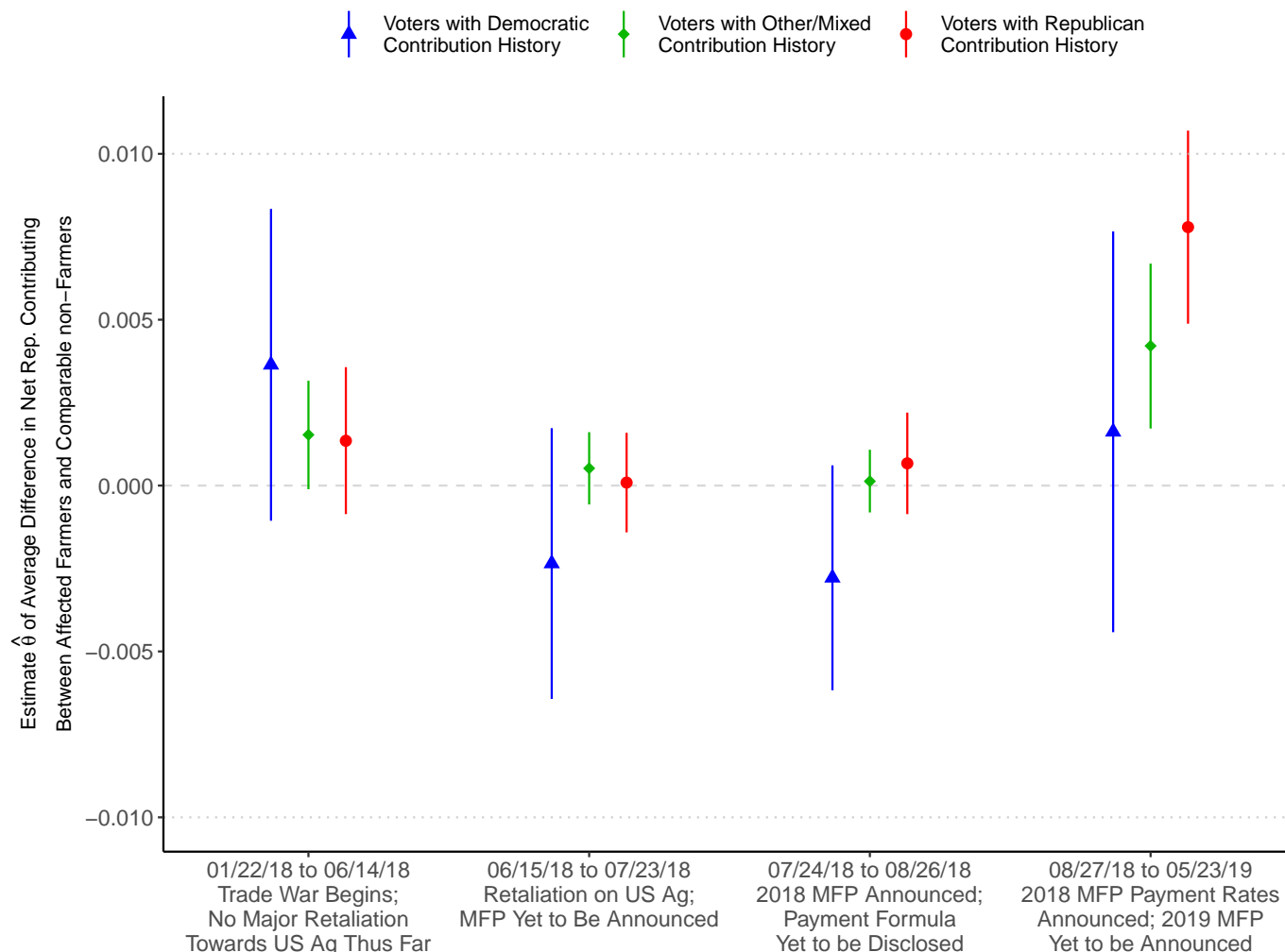
treatment. See Further Supplemental Materials Section G for details, including a description of the 2013-2017 programs used to indicate treatment status and a discussion of the merits of this “intention-to-treat” estimand.

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  $p < .01$ ). These differences were noticeably larger among voters who abstained in the previous midterm election (2.0 and 2.6 percentage points, respectively) ( $p < .01$ ). 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.

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



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

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 towards 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 Further Supplemental 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 longstanding 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 US 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 baseline levels of

support, average net Republican contributing was virtually unaffected among farmers with a Republican contribution history ( $0.0223 \rightarrow 0.0224$ ) and farmers with a mixed/bipartisan contribution history ( $-0.0017 \rightarrow -0.0012$ ). Net Republican contributing was 0.002 points below baseline ( $-0.086 \rightarrow -0.089$ ) 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 ( $0.0232 \rightarrow 0.0239$ ) and farmers with a mixed/bipartisan contribution history ( $-0.0003 \rightarrow -0.0001$ ). Net Republican contributing was 0.003 points below baseline ( $-0.049 \rightarrow -0.052$ ) for farmers with a history of contributing to Democrats.

Finally, we do observe statistically significant differences between affected farmers and comparable non-farmer 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 ( $0.101 \rightarrow 0.109$ ), 0.004 above for farmers with a mixed/bipartisan contribution history ( $-0.002 \rightarrow 0.002$ ), and 0.002 points higher for farmers with a Democratic contribution history ( $-0.244 \rightarrow -0.243$ ). These effects for farmers with Republican and mixed/nonpartisan contribution histories are statistically significant at the 1% level, whereas the effect on Democratic farmers is statistically insignificant.<sup>18</sup>

Altogether, these estimates imply that politically active farmers may have modestly but noticeably increased support for Republican candidates after relief was tangible. However, we find little evidence that impending losses from cratering futures prices in June had much of an effect on farmers' political engagement. In conjunction with our earlier results concerning 2018 voter turnout, these results suggest that the Republican Party may have been able to profit slightly from the MFP, but the broader economic shock from the trade war was more politically salient than farmers' particular policy outcomes.

## 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 US finds that benefits tend to flow disproportionately to constituencies that already support the

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<sup>18</sup>In Further Supplemental Materials H.7, we complement these estimates with a series of analyses looking at contribution rates to Republicans, Democrats, and Trump specifically.



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 towards 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, p. 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 towards 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.

Nonetheless, we must emphasize several caveats in interpreting our results. There are a few notable factors that might make farmers’ 2018 trade war experiences distinct from other distributive politics settings that political scientists care about. 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. Indeed, farmers may have been predominantly concerned with long-term uncertainty related to agricultural policy. Variation in such concerns would have been somewhat orthogonal to the farm-level variation we studied, given producers’ propensity to shift crop portfolios from year to year.

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—disaster relief can often be necessitated by insufficient preparation on the part of the incumbent, and a voter might evaluate an incumbent’s economic stimulus package

during a recession against the pain of the recession itself. And more broadly, 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.

As a final caveat, 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 paper 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 (Kinder and Kiewiet, 1981). 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 intran-

sigence 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 (Mutz, 2018; Ellis and Ura, 2021).

To be clear, we do not interpret our findings as contrary to the claim—well-studied in the policy feedback literature (Campbell, 2012)—that policy shocks can be important drivers of political engagement. 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 US 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.

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# Online Appendix for Policy Impact and Voter Mobilization: Evidence from Farmers' Trade War Experiences

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# A Detailed Data Description

## A.1 FSA Payment Records

Through a series of Freedom of Information Act (FOIA) requests, we obtained payment data from the USDA Farm Service Agency (FSA) in two distinct formats. First, we obtained a series of annual files that together document nearly the universe of USDA farm program payments for 2004–2020 at the transaction level. Over the past few decades, USDA farm programs have generally fallen into one of three bins: (1) *commodity programs*, in which farmers are given financial support to grow crops, (2) *disaster programs*, which provide financial relief to farmers and ranchers harmed by hurricanes, droughts, frost, wildfires, tornadoes, and other natural disasters, and (3) *conservation programs*, which pay farmers to leave farmland fallow for the span of a 10–15 year lease. The yearly payment files we obtained feature the universe of Market Facilitation Program (MFP) payments, in addition to payments issued through the three traditional categories of farm support. Each transaction record lists the name of the recipient, the recipient’s mailing address, the payment amount, the name of the program, the payment disbursement date, and the FSA county office issuing the payment. For clarity, we adopt the FSA’s terminology and refer to these datasets as “name-address-payment” files.

Through a second group of FOIA requests, we obtained a patchwork of transaction-level databases covering 2004–2018 in older formats that feature internal recipient identifiers and (for certain key programs) attributions of payments to particular crops. Among the databases obtained was a set of records for MFP payments issued in calendar year 2018. This separate tabulation of MFP records contains all of the identifying fields in the name-address-payment files, but is organized at the transaction-crop level, and thus allows us to distinguish how much of each payment is attributed to, say, corn or soybean production. By contrast, MFP payments in the name-address-payment files have been aggregated to the MFP subprogram level,

such that we can only distinguish payments for “commodity crops” (corn, cotton, sorghum, soybeans, wheat), “animal products” (milk or hogs), or “specialty crops” (almonds, cherries). Additionally, the separate calendar year 2018 MFP tabulation features the USDA’s distinct customer identifiers (which were discontinued after 2018, and thus not provided with the name-address-payment files), as well as the “record creation date” for each MFP transaction. The latter date is generally one business day prior to the first disbursement date listed in the name-address-payment files, and thus constitutes the best available indicator of when program enrollment actually occurred.

Given the importance of crop-specific MFP payments to our research design, our main analyses focus exclusively on individual agricultural producers featured in the separate 2018 MFP tabulation. We nonetheless use the set of name-address-payment files to filter out farms that received some of their 2018 (program-year) payments in (calendar-year) 2019. Using the “name-address-payment database” of 2018 MFP payments—which also contains payments released after CY 2018 but lacks information on which commodities are enrolled—we limit our sample to farms in which all payments are reflected in our commodity-level tabulation. Using measures of recipients’ historical production inferred from past commodity-level program payment records, we further limit our sample to farms that enrolled each crop in the MFP that historically constituted a major fraction of harvested production. These two methods together screen for farms for which our MFP records are very likely to reflect the full extent of 2018 production, and thereby allow us to minimize measurement error in the three policy outcome “treatments” we construct. See Online Appendices A.4 and A.5 for further details. Finally, we drop all farms for which recipients’ MFP payment limits are binding, and limit to farms with at least 10 acres of cropland that reported producing one or more of the five major MFP-eligible crops in their MFP application, but none of the other four covered commodities (milk, hogs, almonds, cherries).<sup>1</sup>

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<sup>1</sup>We make this last restriction for a few reasons. First, we include a control for historical farm size in our

## A.2 L2 Voter and Commercial Files

Individual-level turnout in the 2018 general election and many of our controls come from national voter and consumer files provided by the vendor L2. L2 maintains panel data on individual voter turnout for over 180 million voters. Their national voter file provides unique identifiers for each voter, tracks individual voters who change addresses across election cycles, and is augmented by a number of demographic fields either provided in the constituent state voter files or merged in from other commercial sources. L2 features turnout histories for each state (and D.C.) in each election cycle from 2004 onward, and for some areas it features records as far back as the late 1980s. L2’s voter files contain numerous, useful covariates that we augment by merging in L2’s separate collection of state commercial files, which feature detailed data on over 200 million U.S. consumers.

We take great care to consider the particular panel nature of voter file turnout history data. State voter rolls evolve over time, adding voters as they register to vote and removing others as they become inactive or move out of state. As such, the “voter files” that researchers work with to study individual-level turnout decisions are actually snapshots of states’ administrative databases at given points in time. This feature of voter files raises concerns for causal inference, as the periodic removal of inactive voters may cause survivorship bias if the voter file snapshot reflects the membership of a state’s voter rolls *after* a treatment or event of interest has transpired. To mitigate such biases, we use L2 national releases from February 2018, June 2019, and May 2021, and limit our analysis to individuals featured in the February 2018 release. We obtain one of our two central outcomes in this paper—turnout in the 2018 general election—by creating a variable that takes a value of 1 if such a voter is noted to have turned out in 2018 in either the 2019 or 2021 releases, and a value of 0 otherwise.

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main analyses, but we are only able to construct this measure for field crops. Second, the payment basis for animal products is not a perfect measure of 2018 production; see Online Appendix A.4 for details. Finally, we do not know of estimates of tariff-induced price declines for cherries and almonds.

Importantly for our study, L2 has a party affiliation field that reflects each registered voter’s declared party preference in the 35 states (plus the District of Columbia) that collect such information. In the remaining 15 states,<sup>2</sup> L2 party affiliation reflects a modeled “likely” party affiliation based on supervised learning algorithms that take into account an array of public and propriety data, from primary election participation to local election results and voter file demographics.<sup>3</sup> After matching transactions from the FSA payments data to individuals in the consolidated L2 voter and consumer files (see Further Supplemental Materials Section E), we flag individual agricultural producers as “Republican” if they receive this label in the voter file, and categorize all other registered voters as “non-Republicans.”

### A.3 Campaign Contribution Records

Farm-level measures of campaign contributions rely on itemized contribution records in the Database on Ideology, Money in Politics, and Elections (DIME) (Bonica, 2014). Adam Bonica generously provided us with a pre-release version of the DIME 4.0 database that extends coverage through the 2020 election cycle. This extended database features over 600 million itemized donations made between 1979 and 2020. Critically, DIME assigns unique identifiers to individual donors that track their donations across recipients and election cycles. As discussed in detail in Further Supplemental Materials E, we link each farm in our USDA database of farm program records to a distinct set of DIME contributor identifiers. Having done so, each transaction in DIME is associated with at most one distinct farm ID.

We utilize several pieces of information contained in the DIME transaction records. Several fields are used for record linkage, including contributor name, address, occupation, and employer. The other fields we use include the transaction amount, each recipient’s name and party affiliation, and Bonica’s measure of each recipient’s ideological ideal point: the

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<sup>2</sup>The 15 states with modeled party affiliation are HI, IL, WA, MT, ND, MN, WI, MI, VT, SC, MO, AL, TX, VA, and GA.

<sup>3</sup>See Spenkuch, Teso, and Xu (2021) for a discussion of L2’s party affiliation field.

common-space campaign finance score (“CFscore”).

Using a combination of the party affiliation fields and manual review, we calculate the dollar amount and transaction count of each farm’s itemized contributions to (a) Republican recipients, (b) Democratic recipients, (c) President Trump’s campaign and affiliated PACs, and (d) other recipients. We include PACs closely affiliated with the Republican Party and Democratic Party in our Republican and Democratic contribution figures, respectively. We aggregate all contribution measures to the farm level, because farm units within our data build generally correspond to distinct households, and political contributions are plausibly a household financial decision.

CFscores provide an appealing way to capture contributing farms’ ideological lean in a single parsimonious measure. The CFscore places political contributors and recipients across all levels of US politics and across time on a unidimensional spectrum of left-right political ideology. This measure is scaled such that it is centered at zero and has standard deviation one. Importantly, the CFscore is a static measure: a single value is assigned to a contributor or recipient profile for their entire 1979-2020 contribution history. The fact that this measure is partly based off of 2018-2020 contributions suggests that we cannot be sure that contributors’ CFscores embody a pre-treatment measure of individuals’ political ideology. Consequently, we construct a farm-level pre-treatment CFscore by identifying all contributions made by a farm *prior* to 2018 for which the recipient has been assigned a CFscore. We then take an average of these pre-2018 recipients’ CFscores weighted by contribution amount to arrive at a farm’s pre-2018 CFscore. Among the 122,157 farms in our main analysis dataset, we are able to assign a pre-2018 CFscore to 27,060 (22%). We impute a default score of zero for the remaining farms.

For our analysis of farm-level contributions, we allow effects to be heterogeneous by partisan affiliation (as we do in our turnout analyses). However, we partition the sample according to contribution history, because (a) farms do not have a party registration, and (b) farms that

do not have a history of contributing are fundamentally distinct from those who do. We thus break up our sample into “farms with a distinctly Republican contribution history,” “farms with a distinctly Democratic contribution history,” and “all other farms.” We consider a farm to have a “distinctly Republican” contribution history if it satisfies three conditions: (i) it is associated with some contribution to a Republican candidate or PAC prior to 2018, (ii) it is associated with a greater dollar amount of pre-2018 contributions to Republicans than Democrats, and (iii) if it is assigned a pre-2018 CFscore, this CFscore is strictly greater than zero (indicating a history of supporting conservative candidates). The criteria for a “distinctly Democratic” contribution history mirror this exactly, and all farms who do not fall into one of these two bins are included in the “all other farms” category.

## A.4 Construction of Policy Outcome Measures

We study the effects of changes in individual producers’ short-run economic outcomes resulting from the 2018 trade war and the Market Facilitation Program. We consider three measures of policy outcomes in this vein: (1) 2018 MFP benefits net of 2018/2019 marketing year revenue lost due to commodity-specific price declines caused by retaliatory tariffs, (2) MFP compensation as a share of tariff-induced losses, and (3) whether or not an individual was “made whole” by the MFP, which corresponds to whether the first measure exceeds \$0 and equivalently whether the second exceeds 100%. Each of these measures requires a measure of an individual’s MFP benefits and tariff-induced losses. In our survey analysis, we estimate both of these quantities from each respondent’s reported 2018 corn and soy acreage in conjunction with their primary county of operation. In our turnout and contribution analyses, we observe MFP payments and use them to estimate tariff-induced losses. The resulting farm-level estimates are plausible because the MFP was paid out using a simple, known formula, and because a relatively large number of studies have estimated the impact of the 2018 trade war on agricultural commodity prices.



The 2018 MFP paid out \$0.01 per certified harvested bushel of corn, \$0.06 per pound of cotton, \$0.86 per bushel of sorghum, \$1.65 per bushel of soybeans, and \$0.14 per bushel of wheat. As such, we estimate harvest quantities for each farm in our survey sample by multiplying each farm’s 2018 average county crop yields by planted acreage; we then obtain crop-specific MFP payments by multiplying harvest estimates by their respective MFP rates. We move in the opposite direction in building treatment measures for our turnout analysis; since we observe commodity-specific payments, we divide farmers’ recorded payments by their respective rate to obtain the amount of each of these crops that the farmer harvested in 2018.<sup>4</sup> The 2018 MFP also paid agricultural producers \$8.00 per head for each live hog on August 1, 2018, and \$0.12 per hundredweight of milk produced during a historical benchmark period.<sup>5</sup> Thus, we can also infer the size of recipients’ hog and dairy operations at the trade war’s commencement. However, the resulting figures are somewhat less useful for us than those obtained for commodity crops. Hogs and milk are not quite as homogeneous commodities as any of the five field crops; hogs are generally priced by weight (not per head) and milk is sold at different prices depending on which of four tiers it falls into under the Federal Milk Order System. Moreover, we are only able to infer dairies’ recent historical production. As such, our main analyses focus only on the five major field crops (for which we can obtain a damage measure with minimal measurement error), and we limit our analysis of hog farmers and dairy owners to robustness checks.

With measures of production in hand for our survey and turnout analyses, we obtain farm-level measures of expected revenue for each commodity by multiplying production (in bushels

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<sup>4</sup>The 2018 MFP limited payments to \$125,000 per recipient each separately for dairy, hogs, and field crops. We drop producers from our sample if they meet (or exceed) any of these caps, as for these individuals we cannot infer 2018 production from MFP payments alone. This drops 2.6% of our sample. Note that some individuals were associated with multiple legal entities in the FSA payment system, and thus remain in our sample despite receiving total benefits in excess of \$125,000.

<sup>5</sup>The MFP 2018 dairy rate was paid on the highest annual milk production marketed during the full calendar years of 2011, 2012, and 2013. However, only dairies in operation on June 1, 2018 were eligible for this payment.

or pounds) by expected commodity prices. Following Janzen and Hendricks (2020), we use USDA price forecasts published on May 10, 2018 as a benchmark to gauge MFP receipts and tariff-induced price declines against farmers’ pre-trade war revenue expectations.<sup>6</sup> Specifically, the USDA forecast that farmers would receive \$3.80 per bushel of corn, \$0.65 per pound of cotton, \$3.60 per bushel of sorghum, \$10.00 per bushel of soybeans, and \$5.00 per bushel of wheat. In our robustness checks that take into account covered animal products, we use the USDA’s February 2018 forecast of the average milk price in the fourth quarter of 2018<sup>7</sup> (\$16.60 per hundredweight), and the (ex-ante) expected 2018 price per hog (\$124.44) calculated by Swanson et al. (2019).

To gauge the share of revenue lost from tariff-induced price declines in 2018, we turn to published estimates from the agricultural economics literature. Starting with the most updated versions of papers reviewed by Janzen and Hendricks (2020), we ultimately find 10 studies that estimate the effect of retaliatory tariffs on prices received by U.S. agricultural producers. The most cited of these studies, Adjemian, Smith, and He (2021), uses the relative price of a substitute method to conclude that China’s 25% retaliatory tariffs on U.S. soybean imports depressed U.S. soybean export prices between late-June and late-November 2018. The authors’ methodology treats Brazilian producers—the second largest source of Chinese soybean imports before 2018—as a comparison group, and identifies the effects of retaliatory tariffs through structural breaks in the ratio of U.S. and Brazilian export prices. They estimate that prices received by U.S. producers were \$0.74 lower on average across the five-

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<sup>6</sup>We use the midpoints of the USDA’s commodity-specific forecasted price ranges published in the May 10, 2018 World Agricultural Supply and Demand Estimates (WASDE) report, available at <https://usda.library.cornell.edu/concern/publications/3t945q76s?locale=en>. This is the earliest WASDE report forecasting prices for the 2018/2019 crop marketing year, and its analysis makes no mention of the coming trade war. Indeed, it seems unlikely that either market or government entities anticipated trade war impacts at this point, as U.S. and Brazilian soybean export prices did not begin to diverge until mid-June, when China announced its 25% tariff on U.S. soybeans (see Figure 4 in Regmi (2019)).

<sup>7</sup>See the February 16, 2021 USDA Livestock, Dairy, and Poultry Outlook report, <https://www.ers.usda.gov/publications/pub-details/?pubid=100519>.

month period; this amounts to 7.4% of the USDA’s forecasted 2018/2019 marketing year price, and 45% of the rate at which the 2018 MFP compensated soybean farmers. In a different time series approach, Swanson et al. (2019) construct a counterfactual price trend for corn, soybeans, and hogs by estimating the historical relationship between harvest-time price declines (relative to pre-harvest crop insurance prices) and realized levels of production. The authors estimate that counterfactual prices in October 2018 exceeded actual prices by \$1.02 per bushel of soybeans, \$0.08 per bushel of corn, and \$2.21 per hog, amounting to 10.2%, 2.1%, and 1.8% of expected prices and 62%, 800%, and 28% of 2018 MFP rates, respectively.

Each of the remaining eight studies estimates the effect of retaliatory tariffs by calibrating trade models with pre-trade war agricultural market parameters, and then simulating the relevant policy changes. While each study specifies a full model of supply and demand within the relevant market(s), each ultimately features a distinct modeling approach. Zheng et al. (2018) use a version of the Global Simulation Model (GSIM), an Armington partial equilibrium model of trade that was also the basis for the USDA’s calculations underpinning the 2018 MFP payment rates. Balistreri et al. (2018), Taheripour and Tyner (2018), and Yuan et al. (2020) each rely on particular versions of the Global Trade Analysis Project (GTAP) computable general equilibrium model. On the other hand, Sabala and Devadoss (2019) develop and calibrate a novel spatial equilibrium trade model, and the remaining three studies apply the particular agricultural trade modeling systems developed by research centers at Iowa State University (CARD Model, Elobeid et al. (2021)), University of Missouri (FAPRI-MU, Westhoff, Davids, and Soon (2019)), and Texas Tech University (World Fiber Model, Liu and

Hudson (2019)).

**Table OA1: Estimated Producer Price Declines Caused by 2018 Tariffs**

	Soybeans	Corn	Wheat	Sorghum	Cotton	Hogs	Dairy
Time Series Analyses							
Adjemian et al (2021)	7.4%	—	—	—	—	—	—
Swanson et al (2019)	10.0%	2.1%	—	—	—	1.8%	—
Trade Model Estimates							
Balistreri et al (2018)	10.2%	4.0%	—	—	—	—	—
Elobeid et al (2021)	14.8%	3.6%	3.9%	—	—	2.1%	—
Liu and Hudson (2019)	—	—	—	—	1.5%	—	—
Sabala and Devadoss (2019)	11.9%	—	—	—	—	—	—
Taheripour and Tyner (2018)	4.9%	1.5%	1.1%	2.1%	—	—	0.5%
Westhoff et al (2019)	9.4%	3.1%	2.6%	10.0%	2.6%	1.6%	0.2%
Yuan et al (2020)	—	—	—	—	0.9%	—	—
Zheng et al (2018)	3.9%	—	—	10.6%	1.2%	0.6%	—

Notes: When a study features an estimate in terms of dollars and a % of a specified baseline, we take the dollar amount and divide it by the USDA’s May 2018 forecasted 2018/2019 marketing year price.

Price impact estimates from each of these ten studies are presented in Table OA1. Altogether, eight studies estimate price impacts for soybeans, five for corn, three for wheat, three for sorghum, four for cotton, four for hogs, and two for dairy. In line with the conclusion of Janzen and Hendricks (2020), these studies unanimously find that MFP payment rates for soybeans, sorghum, and cotton exceeded short-run price impacts of the 2018 trade war, but undercompensated corn farmers. However, given the diversity of methodological approaches, there is (perhaps unsurprisingly) some dispersion among the estimates for particular commodities. Rather than litigate the relative advantages and disadvantages of different studies’ methodologies, we obtain price impact measures for our main analyses by assuming that the true impact of the trade war is likely somewhere near the average of the different approaches. For our preferred measure of trade war damage (which we use for our main results), we consider only the five major field crops, and take the simple average of estimated price impacts across the ten studies featured in Table OA1. Then, as a robustness check, we replicate our

main analyses using four alternative aggregations of these commodity-specific estimates. In increasing order of restrictiveness, we consider: (a) the simple average across all studies for all commodities (five field crops, hogs, dairy), (b) the simple average across all studies for the five field crops and hogs, (c) averaging estimates with equal weight given to the two high-level methodological approaches (time series analysis / trade model simulation) for each of the five field crops, and (d) taking the average estimates for corn and soybeans among only the time series analyses. The resulting commodity-by-commodity aggregates are presented in Table OA2. Note that we only calculate trade war damage under each aggregation method for producers of commodities included in said aggregation; as such, none of the producers in our main specifications reported raising hogs or running a dairy in 2018. To compute tariff-induced losses for our survey, turnout, and contribution analyses, we take the aforementioned revenue measures and multiply them by the proportional declines features in Table OA2.

**Table OA2: Methods for Aggregating Price Impact Estimates**

Commodities	Aggregation	$N$	Soybeans	Corn	Wheat	Sorghum	Cotton	Hogs	Dairy
Crops, Hogs, Dairy	Simple Avg	264,407	9.1%	2.9%	2.5%	7.6%	1.5%	1.5%	0.4%
Crops, Hogs	Simple Avg	246,891	9.1%	2.9%	2.5%	7.6%	1.5%	1.5%	—
<b>Crops</b>	<b>Simple Avg</b>	<b>242,575</b>	<b>9.1%</b>	<b>2.9%</b>	<b>2.5%</b>	<b>7.6%</b>	<b>1.5%</b>	—	—
Crops	Avg by Method	242,575	9.0%	2.6%	2.5%	7.6%	1.5%	—	—
Soybeans, Corn	Avg Time Series	170,357	8.8%	2.1%	—	—	—	—	—

Note: Third row depicts parameters reflected in the net benefit measure used in the main text.

## A.5 Construction of Historical Farm Size Measure

To control for baseline differences in political engagement that may stem from long-standing differences in farm size (and therefore wealth), we construct a measure of historical farm acreage using each farm’s prior payment records. We take advantage of the relatively simple connection between payments issued through flagship USDA farm programs in 2009-2012 and each recipient farm’s historical acreage and yields. Since the passage of the 1996

farm bill, the USDA’s predominant farm programs have made payments on historical planted acreage rather than current planting decisions. The 2002 farm bill maintained this program design principle in authorizing the Direct and Counter-Cyclical Program (DCP), which made payments on a farm’s “base acres,” which mostly reflected the farm’s plantings of covered commodities between 1998 and 2001. Farms enrolled in the DCP received a “direct payment” each year, a constant annual sum paid out at commodity-specific rates according to base acreage and yields. Farms also received a conditional “counter-cyclical” payment when prices of particular commodities fell below statutorily-fixed thresholds. In the 2008 farm bill (in effect between 2009 and 2013), farms had the option to enroll in the Average Crop Revenue Election (ACRE) program instead of the DCP, which traded a 20% reduction in the direct payment rate for a counter-cyclical payment that would be made on current plantings instead of base acres.

By construction of our turnout sample, all farms we study enrolled in either DCP or ACRE between 2004 and 2012.<sup>8</sup> While DCP/ACRE made payments for 17 distinct crops, the five crops we consider in our turnout analysis accounted for over 92% of 2009-2012 enrolled base acreage. As such, inferring the basis of 2009-2012 DCP/ACRE payments for each farm in our sample should provide a compelling measure of longstanding farm size.<sup>9</sup>

Since we observe commodity-specific DCP/ACRE payment amounts for each farm in each year, we divide payment amounts by payment rates to infer each farm’s “base production.” Specifically, for each covered crop  $c \in \mathcal{C}$ , the DCP/ACRE formula specifies farm  $i$ ’s fixed annual direct payment in year  $t \in \{2009, 2010, 2011, 2012\}$  as:

$$\text{Payment}_{itc} = \text{Base\_Acres}_{ic} \cdot \text{Acreage\_Prop}_t \cdot \text{Base\_Yield}_{ic} \cdot \text{Payment\_Rate}_c \cdot \text{ACRE\_adj}_i,$$

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<sup>8</sup>While the DCP/ACRE programs were in effect between 2004 and 2013, our commodity-level transaction data for DCP/ACRE only spans 2004-2012.

<sup>9</sup>Indeed, we validate the resulting measure of farm size in Further Supplemental Materials B.

where  $\text{Base\_Acres}_{ic}$  denotes the farm  $i$ 's base acres associated with commodity  $c$ ,  $\text{Acreage\_Prop}_t$  reflects the fact that the 2008 farm bill specified payments to be made on 83.3% of base acres in 2009-2011 and 85% of base acres in 2012,  $\text{Base\_Yield}_{ic}$  denotes the farm's historical yields for commodity  $c$ ,  $\text{Payment\_Rate}_c$  denotes the direct payment rate per harvested unit of commodity  $c$  (fixed for the duration of the farm bill), and  $\text{ACRE\_adj}_i$  denotes the fact that farmers electing the ACRE option incurred a 20% reduction in direct payments. Since we observe each farm's county of operation,<sup>10</sup> we proxy each farm's base yield for each commodity with their county's historical yields, and then estimate farm  $i$ 's total enrolled base acreage in year  $t$  as

$$\widehat{\text{Base\_Acres}}_{it} = \sum_{c \in \mathcal{C}} \frac{\text{Payment}_{itc}}{\text{Acreage\_Prop}_t \cdot \text{County\_Avg\_Yield}_{ic} \cdot \text{Payment\_Rate}_c \cdot \text{ACRE\_adj}_i}.$$

We average the resulting measure across 2009-2012 to obtain a pre-treatment measure of longstanding farm size.

As referenced in the main manuscript ("Data on Policy Outcomes, Voter Turnout, and Campaign Contributions"), limiting our sample to 2004-2012 DCP/ACRE recipients also comes with the advantage of allowing us to screen out farms whose records may not reflect the totality of their 2018 harvest. Enrollments in the 2018 MFP for a given commodity were made after the farm finished harvesting said commodity. Since enrollments were made using formally certified harvest records, MFP transactions within the \$125,000 payment cap should precisely reflect the farm's total 2018 harvest for each enrolled commodity. Farmers we have spoken with have confirmed that it would be implausible for a farm to enroll anything other than their full harvested amount of a given crop. However, we consider the possibility that certain farmers may not have bothered to enroll *particular crops*—particularly corn, given the pittance it earned from the MFP. To mitigate this, we took each farm's DCP records and

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<sup>10</sup>We observe the FSA county office through which each transaction is processed. According to a FSA employee we spoke with, this is generally a very good indication of where the actual farm in question is located.

backed out “base production” of each crop  $c \in \mathcal{C}$  (in bushels or pounds) as

$$\widehat{\text{Base\_Production}}_{itc} = \frac{\text{Payment}_{itc}}{\text{Acreage\_Prop}_t \cdot \text{Payment\_Rate}_c \cdot \text{ACRE\_adj}_i}.$$

We then multiplied these base production measures by average 2009-2012 commodity prices to obtain a measure of “base revenue” that is comparable across crops. We dropped any farm from our main turnout analysis sample if (a) any MFP-eligible crop constituted at least a third of base revenue and (b) said crop was not enrolled in the MFP.

## A.6 Survey of Midwestern Corn and Soy Growers

Our analysis of farmer attitudes towards the MFP relies on survey data first reported in Qu et al. (2019), and also analyzed in Li et al. (2022). In March 2019, the authors sent survey invitations to a random sample of 3,000 corn and soy farmers operating at least 250 acres in Iowa, Illinois, and Minnesota (three of the top four corn and soybean producing states). The firm Survey Sampling International recruited the sample based on government records of benefit receipt. The initial invitations referred potential respondents to an online survey; the authors followed up with farmers who did not complete the online survey by mailing printed questionnaires on April 15 and May 7.

The authors kindly agreed to share a replication dataset with us. This dataset features responses from 783 farmers, and includes a number of useful items for our analysis: (1) planted acreage of corn, soybeans, and all other crops in 2013-2017 (average), 2018, and 2019 (planned), (2) an item asking respondents how informed they considered themselves with regards to the trade disruption, (3) an item testing respondents on their knowledge of the Market Facilitation Program by asking them to identify the correct soybean payment rate from a lineup of five rates, (4) an item gauging how helpful the farmers perceived the 2018 MFP to be, and (5) the respondent’s primary county of operation.



As referenced in “Farmer Perceptions of the 2018 MFP” in the main text, we use items (2) and (3) from this list to conclude that respondents were generally well-informed about the trade war and the MFP. Among the farmers who reported any corn/soybean acreage in 2018 and 2013-2017, 7% claimed to be “slightly informed” about the trade war, while 42% considered themselves “moderately informed,” 40% “very informed,” and 12% “extremely informed.” None identified as “not informed.” More concretely, when asked to identify the MFP payment rate for soybeans out of a lineup of five options, over 90% correctly identified it as \$1.65/bushel. We use the remaining fields to demonstrate that the perceived helpfulness of the MFP was generally increasing in soybean plantings and decreasing in corn plantings, and that better overall policy outcomes from the trade war and the MFP were associated with greater perceived MFP helpfulness.

Since we use this data to explore the relationship between farmers’ policy outcomes (by way of their crop portfolios) and policy attitudes, we limit our analysis sample to respondents who (a) reported positive corn/soy acreage in 2013-2017 and 2018 and (b) reported their perceived helpfulness of the MFP. These deletions leave us with 621 responses.

## **B Description of Covariates and Controls**

Table OA3 describes all controls included in our main voter-level turnout analyses, as presented in the main text (“The Effect of Improved Policy Outcomes on Voter Turnout”) and Further Supplemental Materials Sections H.1 and H.2. These include fields representing individual turnout and farm contribution histories, individual demographics, historical farm size, and geographic characteristics relating to voters’ residential addresses. In addition to the fields listed in Table OA3, our robustness checks examining heterogeneous treatment effects occasionally include an additional covariate. For example, our analysis of heterogeneity by payment timing makes use of an indicator for whether an individual’s first MFP record in the FSA system was created before Election Day 2018 (Figure FSM37) or the number of days since

Election Day (Figures FSM38, FSM39, FSM40, and FSM41). In our DML analyses, we do not need to one-hot-encode categorical variables or check for collinear feature combinations, as CatBoost (the supervised learning algorithm we use to estimate the first-stage conditional expectations  $q$  and  $m$ ) handles categorical variables natively, and does not require the feature matrix to be nonsingular. Our measure of historical farm size was inferred from farm program payment information obtained via FOIA request (see Online Appendix Subsection A.5), and county-level Presidential margins in 2000 and 2004 are from data provided by the MIT Election Lab. Contribution fields are sourced from DIME. All other fields are sourced from L2’s voter and commercial files. In the interest of ensuring that all covariates are observed pre-treatment whenever possible, we source these measures from the February 2018 snapshot of L2’s voter files whenever available; this is our data source for relevant covariates for the vast majority of sample members. In the rare occasion in which covariates are only available from later sources, we obtain these measures from L2’s 2021 commercial file, or L2’s June 2019 or May 2021 snapshots of their voter files.

Similarly, Table OA4 presents controls used in our main farm-level campaign contribution analyses, as presented in the main text (“The Effect of Improved Policy Outcomes on Campaign Contributions”) and Further Supplemental Materials Section H.3. Our large-scale analysis comparing affected farmer turnout to non-farmer turnout in Figure 11 relies on all covariates in Table OA3 save historical farm size and fields constructed from campaign contribution records.<sup>11</sup> Our large-scale analyses comparing affected farmer contributions to non-farmer contributions likewise rely on a subset of the covariates described in Tables OA3 and OA4. See Further Supplemental Materials G for a full list.

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<sup>11</sup>Given our focus to the broader electorate, we also do not limit our congressional district controls to the 200 most common districts, as we do in our between-farm analysis of voter turnout.

**Table OA3: Description of Controls in Voter-Level Turnout Analyses**

Field(s)	Description
<code>turnout_[cycle]</code> for <code>cycle</code> in 2008, 2010, 2012, 2014, 2016	Indicator for whether the individual voted in the specified general election.
<code>turnout_[cycle]</code> for <code>cycle</code> in 1992, 1994, ..., 2004, 2006	Categorical variable specifying turnout records for the given general election. This variable takes a value of 4 if a vote was recorded in the individual's L2 turnout history; 3 if the individual was not yet 18 on Election Day; 2 if L2 does not have records for that state-cycle combination; 1 if turnout in the election was less than 20% among all individuals in the L2 voter file currently residing in said state; 0 if there is no record of a vote cast in said election, and none of the previous factors apply.
<code>primary_turnout_[cycle]</code> for <code>cycle</code> in 1992, 1994, ..., 2014, 2016	Indicator variable specifying turnout records for the given primary election. Takes a value of 1 if a vote was recorded in the individual's L2 turnout history; 0 otherwise.
<code>democrat</code>	Indicator for Democratic party affiliation (L2 voter file); only included for the non-Republican effect estimates
<code>education_5pt_[level]</code> for <code>level</code> in 1,2,3,4,5	Indicators for highest level of education obtained; levels are "less than HS diploma", "HS diploma", "some college or vocational/technical degree", "bachelor's degree", "graduate degree"; left-out category is "missing". In the instance in which the L2 voter and consumer profiles disagree, we take the greater of the two.
<code>white_not_hispanic</code>	Ethnicity from L2; in the instance in which the L2 voter and consumer profiles disagree, we use the value from the voter file.
<code>race_ethnicity_missing</code>	Ethnicity from L2; in the instance in which the L2 voter and consumer profiles disagree, we use the value from the voter file.
<code>protestant</code>	Indicator for whether L2 classifies voter as Protestant.
<code>catholic</code>	Indicator for whether L2 classifies voter as Catholic.
<code>female</code>	Indicator for voter's gender.
<code>age</code>	Integer reflecting voter's age on Election Day 2018.
<code>log_pop_density</code>	Natural logarithm of the estimated number of adults per square mile calculated for all adults living within the voter's census block.
<code>military_or_veteran</code>	Indicator taking a value of 1 if the L2 voter file labels the individual as "military,veteran", the L2 commercial file labels them a "veteran", or the commercial file indicates that there is a veteran in the household.
<code>christian_family</code>	Indicator taking a value of 1 if the L2 commercial file considers the individual to be in the "Christian families" demographic.
<code>gun_owner_or_concealed_carry</code>	Indicator taking a value of 1 if either the L2 voter or commercial file notes that the individual is a gun owner or holds a concealed carry permit.
<code>log_historical_acreage_09_12</code>	Natural logarithm of estimate of average 2009-2012 acreage enrolled in flagship USDA farm programs. Proxies for farm's longstanding row crop acreage. See Online Appendix A.5 for details on construction of this measure.
<code>county_GOP_pres_margin_[cycle]</code> for <code>cycle</code> in 2000, 2004, 2008, 2012, 2016	County-level Republican two-party vote share margin in the specified presidential election.
<code>precinct_turnout_G[YY]</code> for <code>YY</code> denoting 2010, 2012, 2014, or 2016	General election turnout within the voter's precinct in the specified general election.

Continued on next page

**Table OA3 – continued from previous page**

Field(s)	Description
<code>precinct_turnout.G[YY]_[party]</code> for YY denoting 2010, 2012, 2014, or 2016 and party denoting Republicans, Democrats, or independents	General election turnout among voters affiliated with the specified party within the voter's precinct in the specified general election.
<code>CIDs_active_before_2018_count</code>	Number of distinct contributor IDs in Bonica's DIME 4.0 database that made itemized contributions prior to 2018 and were linked to the same farm as the voter.
<code>total_amount_pre_2018</code>	Total dollar amount of 1979-2017 itemized contributions made by DIME contributor profiles linked to the farm.
<code>pre_2018_cfscore</code>	Pre-2018 analogue of Bonica's (2014) common-space campaign finance score ("CFscore") measure of donor ideology. For each farm linked to contributors in DIME that made itemized contributions prior to 2018, we take the average of pre-2018 contribution recipients' CFscores, weighted by the dollar amounts of the respective donations. For farms not linked to pre-2018 contributions, we impute a value of 0.
<code>rep_contribution_history</code>	Indicates farm had a distinctly Republican pre-2018 contribution history. Takes a value of 1 if three conditions satisfied: (i) farm is associated with some contribution to a Republican candidate or PAC prior to 2018, (ii) farm is associated with a greater dollar amount of pre-2018 contributions to Republicans than Democrats, and (iii) if farm is assigned a pre-2018 CFscore, this CFscore is strictly greater than zero.
<code>dem_contribution_history</code>	Indicates farm had a distinctly Democratic pre-2018 contribution history. Takes a value of 1 if three conditions satisfied: (i) farm is associated with some contribution to a Democratic candidate or PAC prior to 2018, (ii) farm is associated with a greater dollar amount of pre-2018 contributions to Democrats than Republicans, and (iii) if farm is assigned a pre-2018 CFscore, this CFscore is strictly less than zero.
<code>net_rep_amount_before_2005</code>	Total dollar amount of farm's 1979-2004 political contributions to Republican candidates and PACs, minus total dollar amount to Democratic candidates and PACs.
<code>net_rep_amount.[quarter]</code> for quarter in 2005-Q1, 2005-Q2, ..., 2017-Q3, 2017-Q4	Total dollar amount of farm's political contributions to Republican candidates and PACs in the specified quarter, minus total dollar amount to Democratic candidates and PACs.
<code>net_rep_amount.[quarter]</code> for quarter in 2005-Q1, 2005-Q2, ..., 2017-Q3, 2017-Q4	Total dollar amount of farm's political contributions to Republican candidates and PACs in the specified quarter, minus total dollar amount to Democratic candidates and PACs.
<code>rep_amount.[quarter]</code> for quarter in 2016-Q1, 2016-Q2, 2016-Q3, 2016-Q4, 2017-Q1, 2017-Q2, 2017-Q3, 2017-Q4	Total dollar amount of farm's political contributions to Republican Party candidates and PACs in the specified quarter.
<code>dem_amount.[quarter]</code> for quarter in 2016-Q1, 2016-Q2, 2016-Q3, 2016-Q4, 2017-Q1, 2017-Q2, 2017-Q3, 2017-Q4	Total dollar amount of farm's political contributions to Democratic Party candidates and PACs in the specified quarter.
<code>other_amount.[quarter]</code> for quarter in 2016-Q1, 2016-Q2, 2016-Q3, 2016-Q4, 2017-Q1, 2017-Q2, 2017-Q3, 2017-Q4	Total dollar amount of farm's political contributions in the specified quarter to candidates and PACs not affiliated with the Republican or Democratic parties.

Continued on next page

**Table OA3 – continued from previous page**

Field(s)	Description
<code>consolidated_cong_district_[i]</code> for $i$ in $1, \dots, 200$	Indicator for whether voter resides in congressional district $i$ , where districts have been sorted from most to least common within our sample. In practice, this serves as adding in district fixed effects into our set of controls. We limit this array of district indicators to the top 200 districts within our sample, as 98% of our sample resides within these districts, and this restriction substantially improves the computational performance of our DML estimators.

**Table OA4: Description of Controls in Farm-Level Contribution Analyses**

Field(s)	Description
<code>pre_2018_cfscore</code>	Pre-2018 analogue of Bonica’s (2014) common-space campaign finance score (“CFscore”) measure of donor ideology. For each farm linked to contributors in DIME that made itemized contributions prior to 2018, we take the average of pre-2018 contribution recipients’ CFscores, weighted by the dollar amounts of the respective donations. For farms not linked to pre-2018 contributions, we impute a value of 0.
<code>rep_contribution_history</code>	Indicates farm had a distinctly Republican pre-2018 contribution history. Takes a value of 1 if three conditions satisfied: (i) farm is associated with some contribution to a Republican candidate or PAC prior to 2018, (ii) farm is associated with a greater dollar amount of pre-2018 contributions to Republicans than Democrats, and (iii) if farm is assigned a pre-2018 CFscore, this CFscore is strictly greater than zero.
<code>dem_contribution_history</code>	Indicates farm had a distinctly Democratic pre-2018 contribution history. Takes a value of 1 if three conditions satisfied: (i) farm is associated with some contribution to a Democratic candidate or PAC prior to 2018, (ii) farm is associated with a greater dollar amount of pre-2018 contributions to Democrats than Republicans, and (iii) if farm is assigned a pre-2018 CFscore, this CFscore is strictly less than zero.
<code>CIDs_active_before_2018_count</code>	Number of distinct contributor IDs in Bonica’s DIME 4.0 database that made itemized contributions prior to 2018 and were linked to the farm.
<code>total_amount_pre_2018</code>	Total dollar amount of 1979-2017 itemized contributions made by DIME contributor profiles linked to the farm.
<code>log_historical_acreage_09_12</code>	Natural logarithm of estimate of average 2009-2012 acreage enrolled in flagship USDA farm programs. Proxies for farm’s longstanding row crop acreage. See Online Appendix A.5 for details on construction of this measure.
<code>log_pop_density</code>	Natural logarithm of the estimated number of adults per square mile calculated for all adults living within the voter’s census block; we take the average population density for voters linked to the farm, and then apply the log transformation.
<code>consolidated_cong_district_[i]</code> for $i$ in $1, \dots, 200$	Indicator for whether farm resides in congressional district $i$ , where districts have been sorted from most to least common within our sample. In practice, this serves as adding in district fixed effects into our set of controls. We limit this array of district indicators to the top 200 districts within our sample, as 98% of our sample resides within these districts, and this restriction substantially improves the computational performance of our DML estimators.

Continued on next page

**Table OA4 – continued from previous page**

Field(s)	Description
<code>county_GOP_pres_margin_[cycle]</code> for cycle in 2000, 2004, 2008, 2012, 2016	County-level Republican two-party vote share margin in the specified presidential election; we take the average among voters linked to the farm.
<code>precinct_turnout_G[YY]</code> for YY denoting 2010, 2012, 2014, or 2016	General election turnout within the voter's precinct in the specified general election; we take the average among voters linked to the farm.
<code>precinct_turnout_G[YY]_[party]</code> for YY denoting 2010, 2012, 2014, or 2016 and <code>party</code> denoting Republicans, Democrats, or independents	General election turnout among voters affiliated with the specified party within the voter's precinct in the specified general election; we take the average among voters linked to the farm.
<code>net_rep_amount_before_2005</code>	Total dollar amount of farm's 1979-2004 political contributions to Republican candidates and PACs, minus total dollar amount to Democratic candidates and PACs.
<code>net_rep_amount_[quarter]</code> for quarter in 2005-Q1, 2005-Q2, ..., 2017-Q3, 2017-Q4	Total dollar amount of farm's political contributions to Republican candidates and PACs in the specified quarter, minus total dollar amount to Democratic candidates and PACs.
<code>net_rep_trx_count_before_2005</code>	Number of itemized contributions made to Republican candidates and PACs 1979-2004, minus number made to Democratic candidates and PACs.
<code>net_rep_trx_count_[quarter]</code> for quarter in 2005-Q1, 2005-Q2, ..., 2017-Q3, 2017-Q4	Number of itemized contributions made to Republican candidates and PACs in the specified quarter, minus number made to Democratic candidates and PACs.
<code>[party]_amount_before_2005</code> for each party in "Republican", "Democrat", "Other"	Total dollar amount of farm's 1979-2004 political contributions to party candidates and PACs.
<code>[party]_amount_[quarter]</code> for quarter in 2005-Q1, 2005-Q2, ..., 2017-Q3, 2017-Q4 and for each party in "Republican", "Democrat", "Other"	Total dollar amount of farm's political contributions to party candidates and PACs in the specified quarter.
<code>trump_amount_[quarter]</code> for quarter in 2014-Q3, 2014-Q4, ..., 2017-Q3, 2017-Q4	Total dollar amount of farm's political contributions made to Trump campaign and affiliated PACs in the specified quarter.
<code>[party]_trx_count_before_2005</code> for each party in "Republican", "Democrat", "Other"	Number of itemized contributions made to party candidates and PACs 1979-2004.
<code>[party]_trx_count_[quarter]</code> for quarter in 2005-Q1, 2005-Q2, ..., 2017-Q3, 2017-Q4 and for each party in "Republican", "Democrat", "Other"	Number of itemized contributions made to party candidates and PACs in the specified quarter.
<code>trump_trx_count_[quarter]</code> for quarter in 2014-Q3, 2014-Q4, ..., 2017-Q3, 2017-Q4	Number of itemized contributions made to Trump campaign and affiliated PACs in the specified quarter.
<code>republican_voter_count</code>	Number of Republican Party voters linked to the farm, per the February 2018 L2 voter file snapshot.
<code>democratic_voter_count</code>	Number of Democratic Party voters linked to the farm, per the February 2018 L2 voter file snapshot.
<code>turnout_[cycle]_count</code> for cycle in 2008, 2010, 2012, 2014, 2016	Number of voters linked to farm who turned out to vote in specified general election.

Continued on next page

**Table OA4 – continued from previous page**

Field(s)	Description
<code>primary_turnout_[cycle]_count</code> for cycle in 2008, 2010, 2012, 2014, 2016	Number of voters linked to farm who turned out to vote in a primary election in the specified cycle.
<code>military_or_veteran_count</code>	Number of Democratic Party voters linked to the farm, per the February 2018 L2 voter file snapshot.
<code>christian_family_count</code>	Number of linked voters that the L2 voter file labels as “military, veteran”, the L2 commercial file labels them a “veteran”, or the commercial file indicates that there is a veteran in the household.
<code>gun_owner_or_concealed_carry_count</code>	Number of linked voters that the L2 commercial file considers to be in the “Christian families” demographic.
<code>precinct_turnout_G[YY]</code> for YY denoting 2014 or 2016	Number of linked voters for whom either the L2 voter or commercial file notes that the individual is a gun owner or holds a concealed carry permit.
<code>precinct_turnout_G[YY]_[party]</code> for YY denoting 2014 or 2016 and party denoting Republicans, Democrats, or independents	General election turnout within the voter’s precinct in the specified general election; we take the average among voters linked to the farm.
<code>county_GOP_pres_margin_[cycle]</code> for cycle in 2008, 2012, 2016	General election turnout among voters affiliated with the specified party within the voter’s precinct in the specified general election; we take the average among voters linked to the farm.
<code>county_GOP_pres_margin_[cycle]</code> for cycle in 2008, 2012, 2016	County-level Republican two-party vote share margin in the specified presidential election; we take the average among voters linked to the farm.
<code>voter_[i]_age</code> for $i \in \{1, 2, 3\}$	County-level Republican two-party vote share margin in the specified presidential election; we take the average among voters linked to the farm.
<code>voter_[i]_female</code> for $i \in \{1, 2, 3\}$	Integer reflecting age on Election Day 2018 of the farm’s $i$ th oldest linked voter; if fewer than $i$ voters have been linked, value defaults to -1.
<code>voter_[i]_ethnicity</code> for $i \in \{1, 2, 3\}$	Indicator for whether farm’s $i$ th oldest linked voter is female; if fewer than $i$ voters have been linked, value defaults to -1.
<code>voter_[i]_education</code> for $i \in \{1, 2, 3\}$	Categorical variable indicating ethnicity of farm’s $i$ th oldest linked voter; if fewer than $i$ voters have been linked, value defaults to -1.
<code>voter_[i]_republican</code> for $i \in \{1, 2, 3\}$	Categorical variable indicating highest level of education achieved by farm’s $i$ th oldest linked voter; if fewer than $i$ voters have been linked, value defaults to -1.
<code>voter_[i]_democrat</code> for $i \in \{1, 2, 3\}$	Indicator for whether farm’s $i$ th oldest linked voter is listed as Republican in February 2018 L2 voter file snapshot; if fewer than $i$ voters have been linked, value defaults to -1.
<code>voter_[i]_turnout_ge_[cycle]</code> for $i \in \{1, 2, 3\}$ and cycle denoting 2014 or 2016	Indicator for whether farm’s $i$ th oldest linked voter is listed as Democrat in February 2018 L2 voter file snapshot; if fewer than $i$ voters have been linked, value defaults to -1.
<code>voter_[i]_turnout_pe_[cycle]</code> for $i \in \{1, 2, 3\}$ and cycle denoting 2014 or 2016	Indicator for whether farm’s $i$ th oldest linked voter turned out to vote in the general election in the specified cycle; if fewer than $i$ voters have been linked, value defaults to -1.
	Indicator for whether farm’s $i$ th oldest linked voter turned out to vote in the primary election in the specified cycle; if fewer than $i$ voters have been linked, value defaults to -1.

## C Contents of Further Supplemental Materials

Additional information is available in our secondary appendix (“Further Supplemental Materials”), accessible at the following anonymized OSF link:

[https://osf.io/esarh/?view\\_only=643735750ca64066a12fb317f7761f86](https://osf.io/esarh/?view_only=643735750ca64066a12fb317f7761f86)

This includes: (1) a discussion of what might constitute a “typical” farm experiencing the trade war and MFP (and the calculations that went into Figure 2); (2) a validation exercise for our measures of historical farm size and 2018 crop portfolios; (3) additional descriptive statistics; (4) a discussion of pre-treatment covariate balance across the distributions of our policy disposition treatments; (5) a detailed description of the entity resolution and record linkage algorithms used to cluster USDA farm program recipients into distinct farms and link these farms to the L2 voter files and DIME database of itemized political contributions; (6) further details on our empirical strategy, including the mechanics of Double Machine Learning estimation and a discussion of our use of CatBoost to estimate the first-stage relationships  $q(X_i) = \mathbb{E}[Y_i | X_i]$  and  $m(X_i) = \mathbb{E}[B_i | X_i]$ ; (7) supporting information for the large-scale analyses presented in Figures 11 and 12, including a detailed description of how we categorized farms as “directly affected by the trade war” using 2013-2017 administrative data; (8) additional results and robustness checks for our survey, turnout, and contribution data analyses; and (9) a single table listing all Double Machine Learning PLR estimates presented in this paper.



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# Further Supplemental Materials for Policy Impact and Voter Mobilization: Evidence from Farmers' Trade War Experiences

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These “Further Supplemental Materials” provide supporting information and analyses for claims made in the main text. Materials presented in this document are in addition to the formal Online Appendix, which features detailed descriptions of (i) the source datasets underlying all analyses, (ii) the construction of the policy outcome and historical farm size measures utilized in the main turnout and contribution analyses, and (iii) the set of covariates controlled for in all of the main turnout and contribution analyses.

## A Typical Farm Size, Renting Practice, and Policy Impacts

To consider what a “typical” farm affected by the trade war and MFP might look like, we took our main analysis sample of 122,157 farms and inferred harvested production in bushels (for corn, sorghum, soybeans, and wheat) or pounds (for cotton) as discussed in Online Appendix Section A.4. We then divided harvested production by county-level 2018 yields to infer farmers’ 2018 planting decisions in terms of acreage.<sup>1</sup> The (mean) average farm harvested 505 acres of covered commodities in 2018, though as shown in Figure FSM1, farm size was highly skewed to the right. 73% of sample farms (89,241) harvested both corn and soybeans, and 61% (74,573) harvested both corn and soybeans and did so exclusively. Among these corn/soybean farms, the distribution of 2018 crop acreage allocations were (roughly) symmetrically centered around a 50/50 corn-soybean split, though as shown in Figure FSM2, there was substantial variation in this ratio.

Guided by these descriptive statistics, in Figure 2 in the main text, we illustrate the significance of trade war policy outcomes to individual farmers using a model 500 acre Iowa farm planting half corn and half soybeans. We use Iowa State University’s Ag Decision Maker calculators for corn and soybean profitability to estimate costs for such a farm in November 2018.<sup>2</sup> These calculators track and aggregate relevant per-acre expenses regarding direct crop inputs (e.g. seeds, fertilizers, herbicide, insurance, hired labor, drying, gas), machine ownership, storage interest, and land costs.

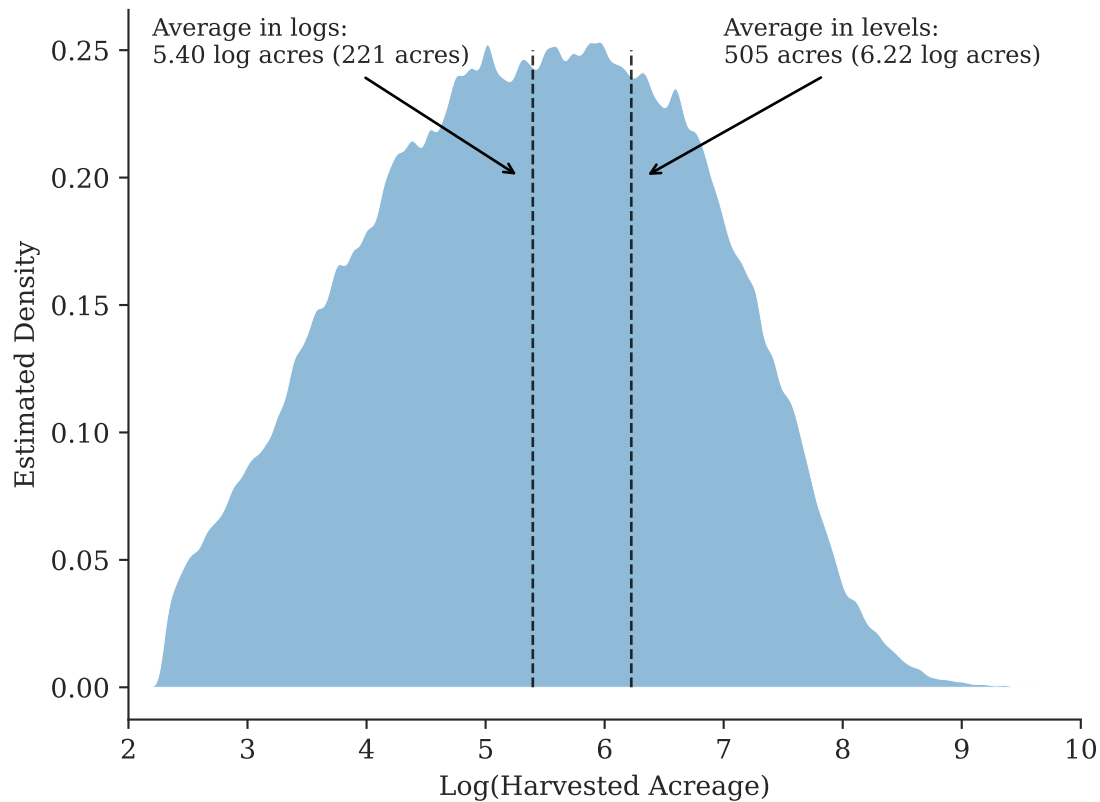
A particularly influential input for these cost calculations is the share of acres rented by a farm. Cropland rental prices in the Midwest account for a large share of revenue on rented acreage, and so profit margins on rented acres are significantly lower than margins on

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<sup>1</sup>When county-level yields were not available for a given crop/farm pair, we used USDA agricultural district yields, state yields, or national yields as available in decreasing order of granularity.

<sup>2</sup>See “Monthly Profitability of Corn Production” and “Monthly Profitability of Soybean Production” Excel spreadsheets on the Ag Decision Maker website at <https://www.extension.iastate.edu/agdm/cdcostsreturns.html#profitability>.

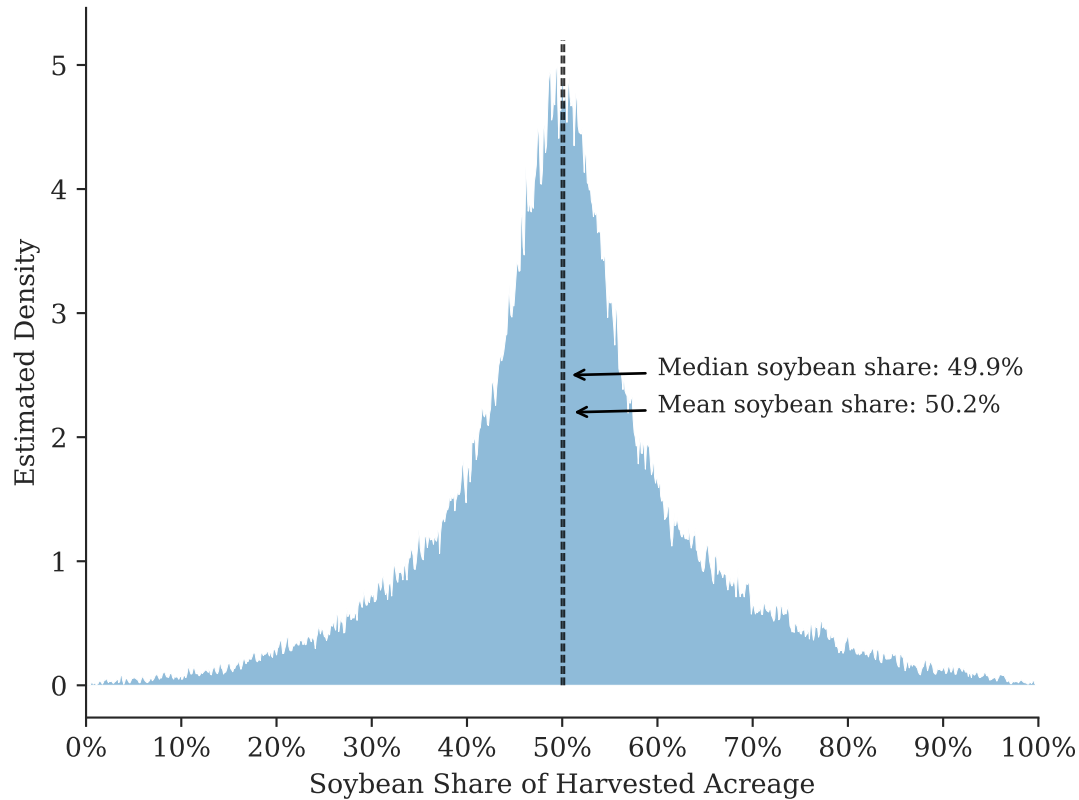
**Figure FSM1:** Distribution of Farm Size Inferred from MFP Payments and County-Level Yields Among Main Analysis Sample Farms ( $N = 122,157$ )



Note: Kernel density estimation was conducted using a Gaussian kernel. By construction, sample is limited to farms that we have inferred harvested at least 10 acres of cropland.

owned acres. This difference is important for gauging “typical” overall farm profits because, as reported in the 2016 U.S. Farmland Ownership, Tenure, and Transfer survey, 55-60% of corn and soybean acreage in the U.S. is rented, but the farm-level rented share is decreasing in farm size (Bigelow, Borchers, and Hubbs, 2016). To determine an appropriate rental share for our stylized Iowa farm example, we turn to the Qu et al. (2019) survey of Midwestern corn and soybean farmers discussed in the main text (“Farmer Perceptions of the 2018 MFP”) and Online Appendix Section A.6. Quite usefully for our purposes, the authors ask farmers responding to the survey not only how many acres they operated, but also what share of those acres were rented vs owned. As shown in Figure FSM3, LOWESS analysis of the relationship between survey respondents’ planted corn/soybean acreage in 2018 and their share of acreage rented suggests that a 500 acre farm—on average—tended to rent about 50% of its acreage 2018.

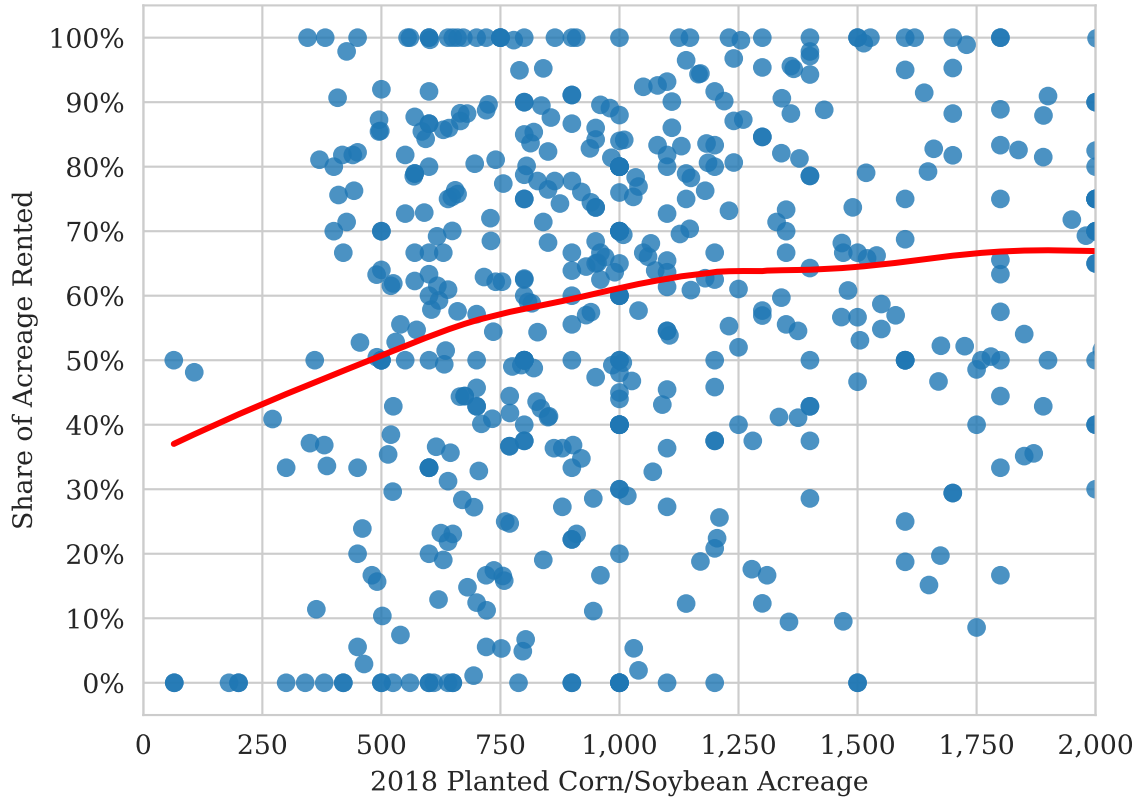
**Figure FSM2:** Distribution of Inferred Soybean Share of Acreage among Corn/Soybean Farms ( $N = 76,167$ )



Note: Kernel density estimation was conducted using a Gaussian kernel. Sample is farms from the main sample that harvested corn and soybeans and did so exclusively. Farm-level acreage is estimated by dividing implied farm-level production of each commodity and dividing by average county-level yields for each crop.

Turning to the Iowa State University’s Ag Decision Maker calculators, we assume that our illustrative 500 acre Iowa farm rented half of the acreage it operated. We compute total costs of \$476 per corn acre on owned land, \$673 per corn acre on rented land, \$305 per soybean acre on owned land, and \$580 per soybean acre on rented land. Altogether, the farm faced \$254,250 in production costs in 2018. On the other side of the ledger, we assume that the farm received the average Iowa marketing year price for each crop (\$3.59 per bushel of corn, \$8.46 per bushel of soybeans), and obtained the statewide average yields on each crop (196 bushels per acre of corn, 56 bushels per acre of soybeans). Per the estimates depicted in column (B) of Table 1, we assume that the corn price fell by 2.9% and the soybean price by 9.1% because of retaliatory tariffs on U.S. agriculture. As such, we conjecture “counterfactual” 2018 marketing year prices (i.e. absent the trade war) of \$3.70 for corn and \$9.31 for soybeans.

**Figure FSM3:** LOWESS Estimation of Average Share of Rented Acreage given Farm Size



Notes: Red line depicts conditional expectation of rented share of acreage (out of total operated acreage) given a farm's 2018 total planted acreage of corn and soybeans. Estimates are obtained via locally weighted linear least squares regression. The sample presented is the set of Qu et al. (2019) survey respondents that planted corn and soybeans in 2018.

We estimate total farm revenue from corn and soybeans sales to be \$294,350, and we estimate counterfactual revenue of \$311,461, with \$5,254 in tariff-induced losses coming from corn price declines and \$11,857 coming from soybean price impacts. Given the \$0.01 and \$1.65 per bushel MFP payment rates for corn and soybeans, respectively, we estimate corn MFP receipts of \$490 and soybeans MFP receipts of \$23,590. Altogether, we estimate farm profits as actual revenue minus actual costs plus MFP payments, yielding \$63,690 in net income for our model farmer.



## B Validation of 2018 Production and Historical Farm Size Measures

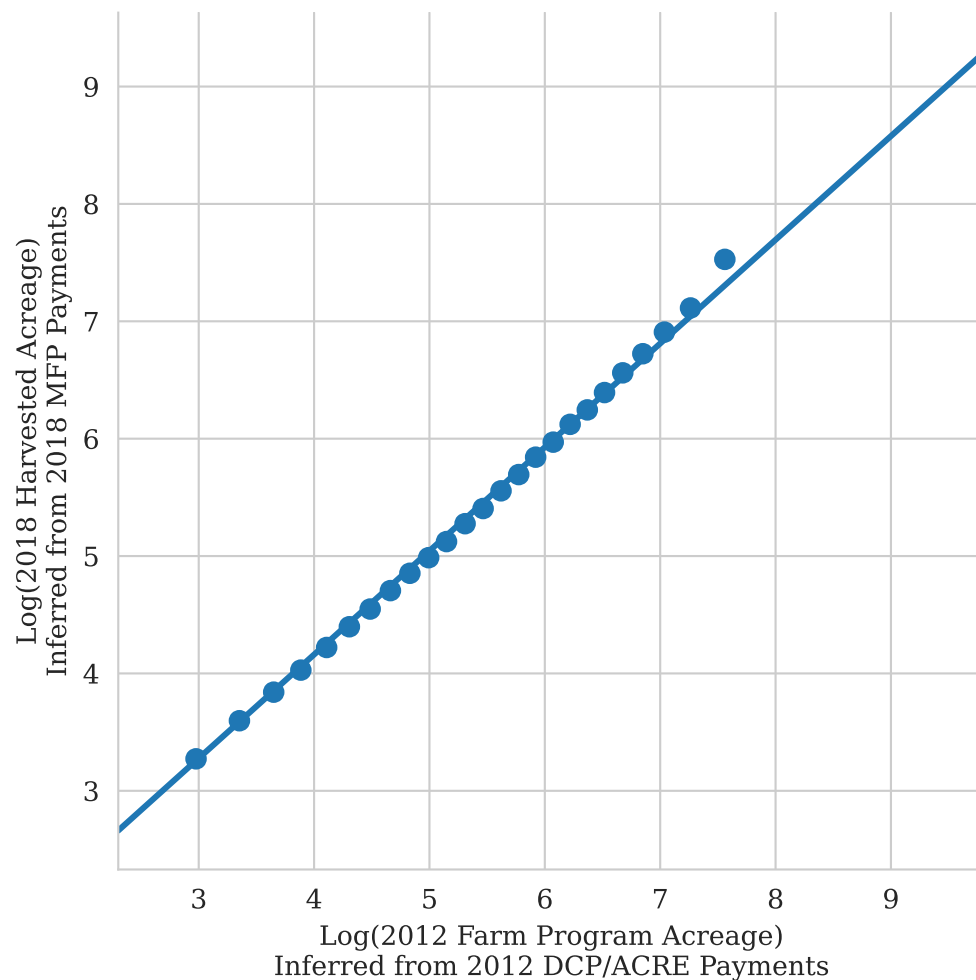
Our study relies on a number of farm production attributes that we infer from USDA administrative payment records by inverting payment formulas: the amount of corn, cotton, sorghum, soybeans, and wheat harvested in 2018, as well as the number of cropland acres operated by the farm in 2012. We obtain the former five items from crop-specific MFP payments and the known MFP payment formula. Since MFP payments are made according to certified actual 2018 production (for instance, using grain elevator receipts as proof of production), measures backed out from MFP payments should be almost completely devoid of measurement error. As discussed in Online Appendix A.5, our measure of historical farm acreage is less precise, as the more traditional DCP/ACRE payments were based on historical plantings and yields (rather than current production).

In Figure FSM4, we show that both sets of measures nonetheless track each other quite well across the distribution of farm size. Using the approach described in the previous section (Further Supplemental Materials A), we convert our MFP production measures to acreage measures using county-level crop yields, and we present a binned scatterplot of (logged) 2018 MFP acreage against 2012 (logged) DCP/ACRE program acreage. The tight linear relationship between the two measures sits close to the 45-degree line. This provides confirmatory evidence that MFP payments were made on sample farms' entire harvest, and that our measure of farm size inferred from 2009-2012 DCP/ACRE payments is an adequate pre-treatment control for historical farm size.

In Figure C, we demonstrate that this relationship is very similar for (a) farms for whom corn constitutes a majority of 2018 acreage, (b) farms for whom soybeans constitute a majority of 2018 acreage, and (c) farms for whom neither constitute a majority of 2018 acreage. This latter exercise eases our concerns that farmers might not have enrolled their corn harvest into the MFP because the pitiful payment rate might not even merit the paperwork. While this

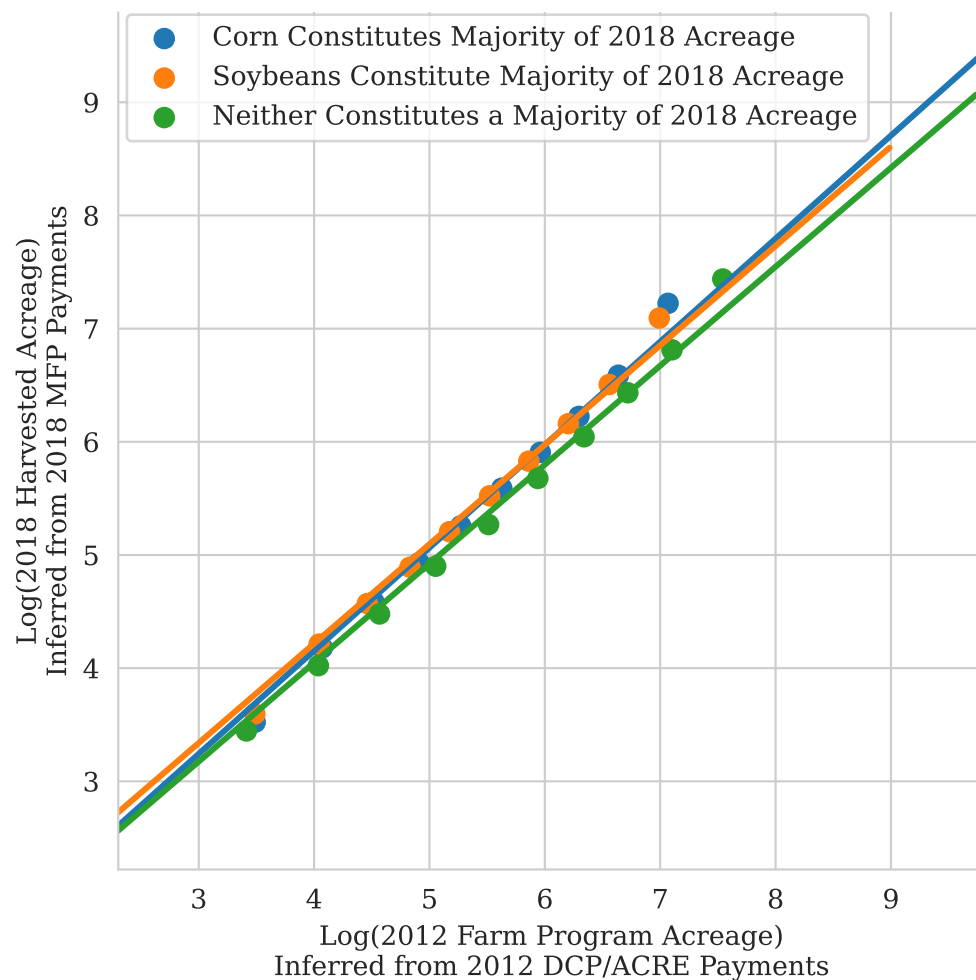
may have occurred in a limited capacity for some farms, Figure C suggests that our sample restrictions (as described in Online Appendix A.5) are sufficient to ensure that sample farms enrolled all of their eligible harvest.

**Figure FSM4:** Binned Scatterplot of 2018 Acreage Inferred from MFP Payments versus 2012 Acreage Inferred from DCP/ACRE Payments



Notes: The measure of 2018 acreage inferred from 2018 MFP payments reflects harvested acreage for corn, cotton, sorghum, soybeans, and wheat. The measure of 2012 acreage inferred from 2012 DCP/ACRE payments reflects acreage enrolled in the DCP/ACRE programs, the vast majority of which have historically been planted to corn, cotton, sorghum, soybeans, and wheat.

**Figure FSM5:** Binned Scatterplot of 2018 Acreage Inferred from MFP Payments versus 2012 Acreage Inferred from DCP/ACRE Payments, Separately by Majority Crop



Notes: The measure of 2018 acreage inferred from 2018 MFP payments reflects harvested acreage for corn, cotton, sorghum, soybeans, and wheat. The measure of 2012 acreage inferred from 2012 DCP/ACRE payments reflects acreage enrolled in the DCP/ACRE programs, the vast majority of which have historically been planted to corn, cotton, sorghum, soybeans, and wheat.

## C Voter-Level Stylized Facts and Additional Descriptive Statistics

We turn to describing the voter-level relationships between policy outcomes, farm size, and the partisanship of the farmers at the center of the trade war. Of the 168,143 voters directly linked to farms in our main sample, 67% were affiliated with the Republican party according to the February 2018 L2 voter file; a further 20% were Democrats, while nearly all of the remaining 14% were independents. The same three percentages are nearly identical for the full set of 592,168 voters linked to farms with any enrollment in the 2018 MFP: 66%, 20%, and 14%, respectively. This rate of Republican affiliation is a little higher than the 61% we obtain for the entire set of 1,996,627 2004–2020 USDA program recipients matched to the voter file. However, the Republican party can also claim 65% of the 809,938 2015–2017 participants in the ARC and PLC programs—the USDA’s largest extant farm aid programs prior to the MFP, and the successor to the long lineage of “farm safety net” programs that have supported U.S. row crop farmers for decades. Thus, we note that to the extent that our sample does lean distinctly towards Republicans, it appears to do so to a degree similar to the national population of row crop farmers that characterizes USDA farm aid recipients.

In Table FSM1, we present voter-level descriptive statistics for the 168,143 members of our main turnout analysis sample. We depict the distribution of payment amounts received by producers, in addition to estimated losses from tariff-induced price declines, separately for Republicans and non-Republicans. As shown in the top panel, Republican producers received \$1,816 more in MFP benefits than non-Republicans on average (difference significant at  $p < .001$ ). This difference is also reflected in the median and interquartile range. Was this discrepancy simply due to the fact that Republican producers experienced greater trade-related damage? As shown in the bottom two panels of Table FSM1, the answer is “not completely.” The gap in trade-related damage between partisan subgroups is only \$1,489, meaning that the average Republican producer enjoyed \$326 greater *net* MFP benefits than

**Table FSM1: Voter-Level Descriptive Statistics on Trade-Related Relief and Damages**

<b>MFP Benefits</b>								
	<i>N</i>	Mean	Std. Dev	1%	25%	Median	75%	99%
Republicans	111,910	\$23,168	\$31,242	\$56	\$3,176	\$10,681	\$30,786	\$136,575
Non-Republicans	56,233	\$21,351	\$30,198	\$44	\$2,712	\$9,236	\$27,584	\$133,178
Difference		\$1,816						
t-statistic		11.4						
<b>Damage from Tariff-Induced Price Declines</b>								
	<i>N</i>	Mean	Std. Dev	1%	25%	Median	75%	99%
Republicans	111,910	\$17,414	\$23,262	\$139	\$2,572	\$8,240	\$23,148	\$104,565
Non-Republicans	56,233	\$15,924	\$22,205	\$119	\$2,176	\$7,150	\$20,625	\$101,443
Difference		\$1,489						
t-statistic		12.8						
<b>Net MFP Benefits</b>								
	<i>N</i>	Mean	Std. Dev	1%	25%	Median	75%	99%
Republicans	111,910	\$5,754	\$10,736	\$-10,769	\$458	\$2,244	\$7,673	\$45,961
Non-Republicans	56,233	\$5,427	\$10,161	\$-8,074	\$422	\$1,982	\$6,814	\$46,844
Difference		\$326						
t-statistic		6.1						
<b>Compensation Rate</b>								
	<i>N</i>	Mean	Std. Dev	1%	25%	Median	75%	99%
Republicans	111,910	133.5%	53.1%	9.1%	111.1%	131.8%	147.0%	296.9%
Non-Republicans	56,233	133.2%	52.8%	9.1%	111.6%	131.6%	147.1%	295.6%
Difference		0.3%						
t-statistic		1.1						

Notes: Statistics are calculated for the sample of voters considered in our main across-farm analyses. Columns 1%, 25%, 75%, and 99% denote the 1st, 25th, 75th, and 99th percentiles of the specified measure within the specified subgroup. All t-statistics are Welch's t-statistics. Note that means and medians differ moderately from those depicted in Figure 5 because the figures in this table are computed at the voter level, and larger farms are more likely to be linked to multiple voters.

non-Republican producers (difference significant at  $p < .001$ ).

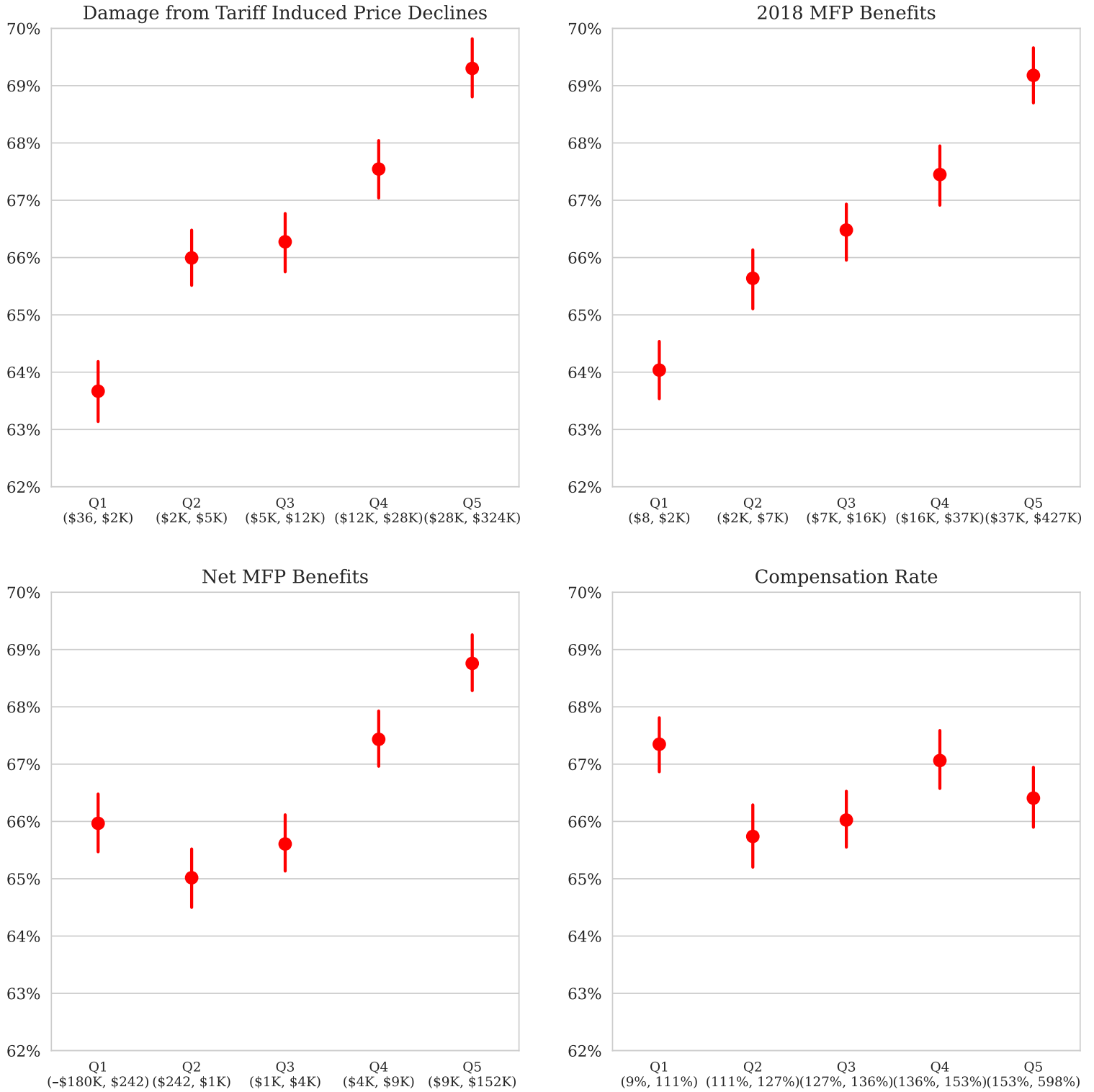
These partisan differences can be explained fairly simply in terms of two general patterns of partisan affiliation among agricultural producers, in combination with a couple of key stylized facts about the 2018 MFP and the trade war. First, agricultural producers are, as a whole, predominantly Republican. Second, even conditional on being a producer, Republicans operate larger agricultural enterprises than non-Republicans. Third, producers who engaged in greater agricultural activity received greater MFP payments and experienced greater trade war damage. Fourth, the MFP overcompensated farmers on average. Together, these four stylized facts explain how Republicans disproportionately benefited from the MFP, even taking into account the actual amount of damage suffered.

Since our “MFP compensation rate” measure is invariant to farm size, the fourth panel of Table FSM1 corroborates our claim that the pro-Republican bias of the MFP arises through preferential treatment of large farms, rather than preferential treatment of any particular crop. The average compensation rate of Republican crop portfolios was only 0.3 percentage points greater than that of non-Republican portfolios. We corroborate this point with two additional analyses. In Figure FSM6, we explicitly demonstrate that the rate of Republican party affiliation is increasing in trade war losses, MFP compensation, and net MFP benefits, but not the compensation *rate*. To directly show that differential compensation rates for particular crops did not correspond to differing levels of partisan loyalty, in Figure FSM7, we plot Republican party affiliation by quintile of revenue attributed to each individual crop.<sup>3</sup> The figure makes clear that Republican affiliation is increasing in the scale of production of each crop. Moreover, Figure FSM7 provides no indication that the soybean farmers who took home the largest MFP payments (in the aggregate) were any more Republican than their corn/sorghum/wheat peers. Indeed, all major U.S. row crops are grown by highly Republican constituencies, and the differentially poor treatment for corn growers in 2018 can likely be attributed to the USDA’s rush to estimate damages before retaliatory tariffs even went into

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<sup>3</sup>We excluded cotton from Figure FSM7 because there were too few sample members growing cotton (2,086 farmers, 1.2% of the sample) to obtain precise estimates by quintile.

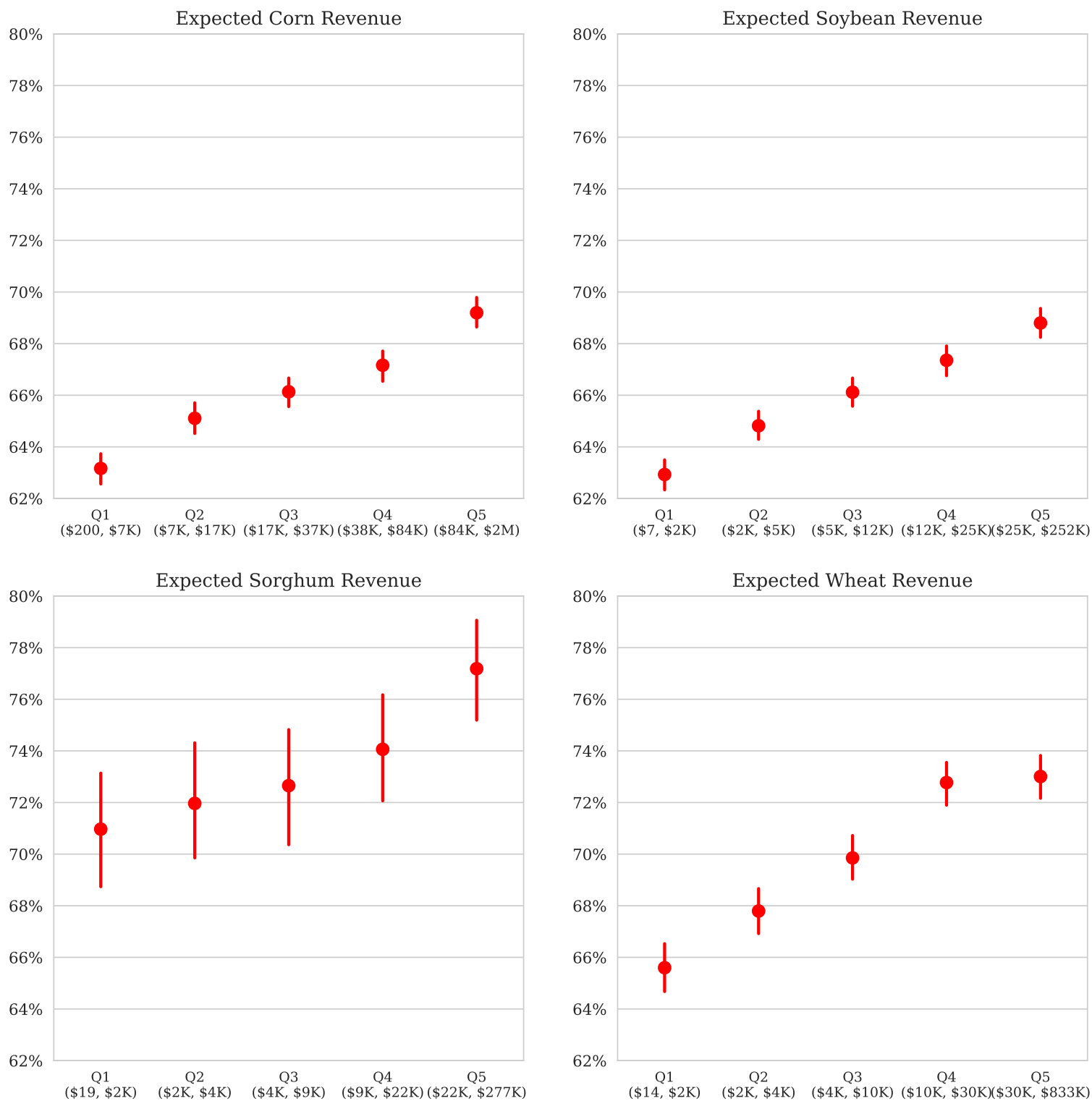
**Figure FSM6:** Republican Party Affiliation by Quintile of Trade War / MFP Exposure



Notes: Each panel depicts the percentage of voters in the main analysis sample quintile who are flagged as Republican in L2's 2018 voter file, with 95% confidence intervals.



**Figure FSM7:** Republican Party Affiliation by Quintile of Expected Harvest Value, by Crop



Notes: Each panel depicts the percentage of voters in the main analysis sample quintile who are flagged as Republican in L2's 2018 voter file, with 95% confidence intervals.

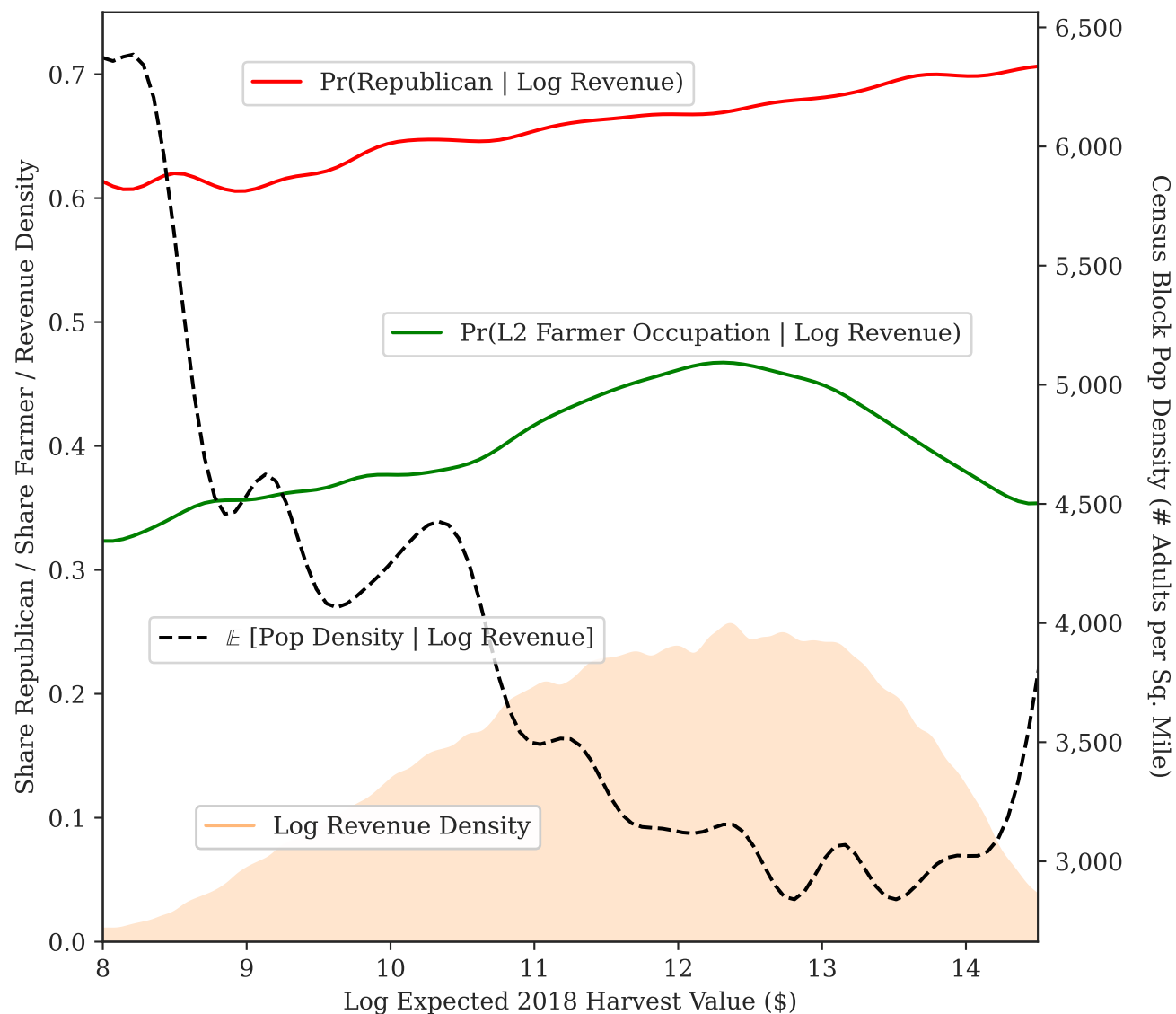
effect.<sup>4</sup> We conclude that it is precisely because farmers are such a distinctly Republican constituency, and because the likelihood of Republican affiliation is increasing in farm size, that Trump was able to craft a program that primarily benefited his electoral base, and (generally) provided the greatest benefits to those most likely to be in his core coalition.

We conclude this broader discussion by noting that, while we cannot provide a full explanation of why Republican farmers tend to operate larger farms (and thus, receive greater net benefits on average) than non-Republicans, we can speculate that the connection may lie in the growing urban-rural political divide. Many farmers actually spend much of their workweek in towns and cities; according to the 2017 Census of Agriculture, only 42% of U.S. agricultural producers consider farming their primary occupation. However, this figure is sharply increasing in farm size: 78% of farmers who operate more than 2,000 acres do so as their primary form of employment (USDA NASS, 2019, pp. 102–103). Figure FSM8 confirms this relationship for our sample. Using locally linear regression, we model the rate at which L2’s voter and commercial files identify an individual as a “farmer” as a function of total farm revenue. While L2’s occupation field is often missing, we nonetheless see that the rate at which members of our sample are flagged as farmers is increasing across most of the range of total revenue. Furthermore, this relationship mirrors a corresponding decrease in the average population density as farm size increases. Thus, the high rates of Republican affiliation among large farms may plausibly stem from the more general social and geographic forces that have turned rural areas throughout the U.S. into Republican strongholds in recent decades (Gimpel et al., 2020).

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<sup>4</sup>In further support for this conjecture, we note that the 2019 MFP (paid out in late 2019 and early 2020) considerably improved the relative treatment of corn farmers (Janzen and Hendricks, 2020).

**Figure FSM8:** Relationship of Farm Size (Measured by Log Farm Revenue) with Partisanship, Farmer Occupation, and Local Population Density



Notes: Kernel density estimation was conducted using a Gaussian kernel. Each of the three bivariate relationships is estimated using locally linear kernel regression.

## D Covariate Balance Across Policy Outcomes

We assess the contribution of our “natural experiment” in MFP compensation variation to our identification strategy by examining how our treatment measures covary with past political engagement and demographics. For the sake of brevity, we focus in this section on our voter-level turnout analyses. Given the panel structure of our data, as well as our rich, individual-level array of useful controls, we emphasize that any claims to causal identification in this paper rest on an unconfoundedness (i.e. “selection-on-observables”) design. As such, we neither expect nor require that covariates be perfectly uncorrelated with our treatments (as one might in a true experimental design). However, by examining covariate balance across the distributions of our treatments, we can gauge the extent to which treatment variation appears exogenous in its own right. Policy variation that is highly orthogonal to *observed* correlates of baseline turnout propensity before any covariate adjustment is likely also more orthogonal to *unobserved* correlates of turnout after adjustment. Examining covariate balance thus provides suggestive evidence regarding which of our analyses also benefits from an additional layer of robustness beyond what is afforded by our Double Machine Learning adjustment for past histories of political engagement and other covariates.

We first gauge treatment correlations by independently regressing eight binary fields and four (standardized) quantitative fields on our three treatment measures, plotting the resulting coefficients in Figures FSM9, FSM10, FSM11, FSM12, FSM13, and FSM14. Then, in Figures FSM15 through FSM20, we compare average covariate values across quintiles of MFP compensation rate and net MFP benefits. We observe the best covariate balance for our MFP compensation rate measure. This corroborates our narrative of idiosyncratic (dis)advantageous treatment of particular crops arising from the administration’s decision to rush out a \$12 billion total price tag without any eye towards individual commodity price impacts. Among Republican members of our sample, a 100 percentage point difference in the compensation rate was associated with less than a one percentage point difference in turnout in any of the previous four elections, the share of voters that are male, the share of voters that are white,

the share of voters with a bachelor’s degree, and the share of voters who are gun owners. Such a large difference in the compensation rate is also associated with only minute differences in voters’ census block population density, farm size, age, and county-level Presidential voting.

Our net benefit percentile treatment measure is also mostly uncorrelated with pre-treatment covariates, though it is not quite as orthogonal as the MFP compensation rate. This appears to derive from the fact that net benefits (in dollar terms) are in part driven by farm size. Unlike harvest (crop) composition, farm size does not translate into policy outcomes by way of idiosyncratic errors in program design, and furthermore farm size *does* appear to be at least slightly correlated with certain demographic and political characteristics. In particular, moving across the interquartile range of net MFP benefits (\$391 to \$6,110) is associated with a 0.8 standard deviation increase in farm size among Republican sample members, as well as a 0.2 percentage point greater turnout rate in 2016, a 3.1 percentage point lower turnout rate in 2014, a 0.6 percentage point lower turnout rate in 2012, and a 2.0 percentage point lower turnout rate in 2010. These modest differences in past turnout propensity may arise from the fact that larger farms tend to be operated by individuals who are younger and more likely to be male — two demographic characteristics that have previously been found to be negatively correlated with turnout rates.

We conclude from these results that our estimates based on MFP compensation rate are our most robust to any putative unmeasured confounders, and thus reflect our “best” estimates of the effects of (better) policy outcomes on voter turnout. Nonetheless, we emphasize that we still consider our DML estimates corresponding to the other two treatments to be highly credible and accurate. Not only are the baseline differences in turnout relatively small, we are able to nonparametrically control for differences in each of the 12 covariates examined in this section, as well as the dozens of further controls described in Online Appendix Section B.

In contrast, we are somewhat more cautious in assigning a fully causal interpretation to the estimates depicted in Figure 11 in the main manuscript. As described in “Broader

Effects of the Trade War and MFP on Farmers’ Political Engagement,” these estimates are obtained by comparing 2018 turnout rates between (a) farmers with a predisposition to be directly affected by the trade war and MFP and (b) the rest of the electorate. Even though we use the same DML estimator employed in our main analyses with nearly the same list of controls,<sup>5</sup> completely eliminating bias from this series of estimates is likely more difficult than in our main analyses. This is because baseline differences between farmers and non-farmers are considerably larger than differences between corn-heavy farmers and soy-heavy farmers—or even differences between large-scale farmers and small-scale farmers. As shown in Figures FSM21 and FSM22 below, farmers affected by the trade war and MFP had higher past turnout rates than the rest of the electorate, and were demographically quite distinct. Among Republicans in the February 2018 release of the L2 national voter file, affected farmers were 10 percentage points more likely than the rest of their copartisans to have voted in 2016, and 22 percentage points more likely to have voted in 2014. Republican farmers were also 21 percentage points more likely to be male, 18 percentage points more likely to own a firearm, and 6 percentage points more likely to be non-Hispanic white. Unsurprisingly, they also tended to live in far more rural areas.

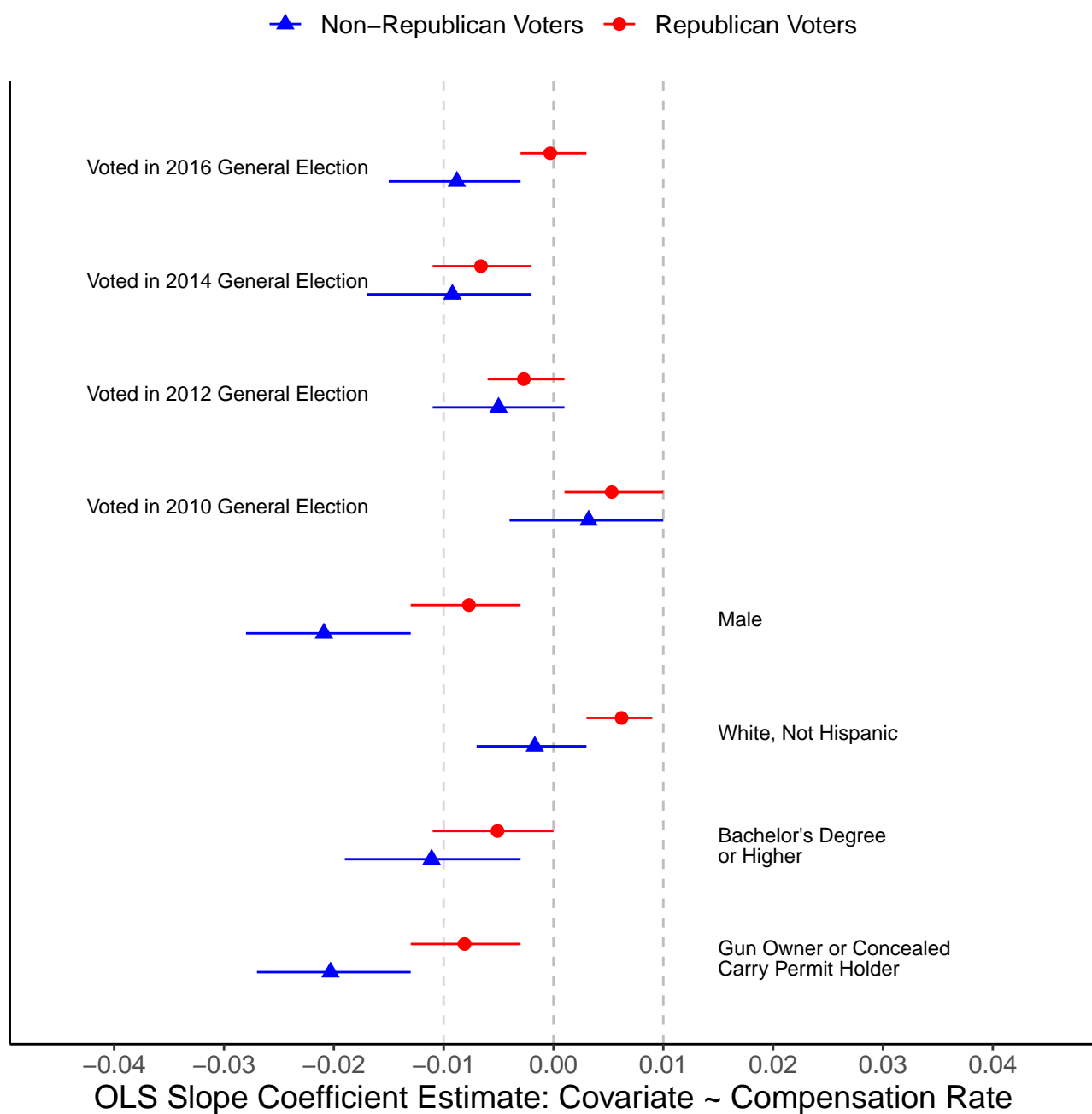
Given that we observe each voter’s individual turnout history, we expect that our DML estimator will be able to account for the vast majority of the difference in baseline turnout

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<sup>5</sup>There are two main differences in covariate adjustment for the electorate-scale turnout analysis (Figure 11): we omit historical farm size from our list of controls, and we do not control for campaign contribution history. With respect to historical farm size, as discussed in Online Appendix Section A.5, we construct our measure of historical farm size by inferring the basis of 2009-2012 DCP/ACRE payments, the USDA’s flagship “farm subsidies” issued for the duration of the 2008 farm bill. And as discussed in Further Supplemental Materials Section G, we designate voters as directly affected by the trade war / MFP if they are associated with a farm that received support from one of several major USDA commodity-support programs (one of which is DCP/ACRE). Given the near-universal enrollment of row crop farmers in DCP/ACRE in 2009-2013, controlling for historical farm size would therefore nearly perfectly predict treatment status in the analyses presented in Figure 11. We do not control for campaign contribution history because record linkage between DIME and the nearly 180 million voters in the L2 2018 voter file snapshot would be computationally intractable. Indeed, our large scale analysis of campaign contribution behavior—as presented in Figure 12 and Further Supplemental Materials H.7—is limited to L2 voter profiles featured in the crosswalk of 2011-2020 contributors produced by Bonica and Grumbach (2022).

propensity between farmers and non-farmers. However, our analyses in this section demonstrate that baseline selection biases in the comparisons considered in Figure 11 are greater than those dealt with in our main analyses. As such, it is difficult to assert that our point estimates in Figure 11 perfectly isolate the effect of trade war / MFP exposure on voter turnout. Instead, we interpret these estimates as suggestive of the sign and approximate scale of such turnout effects, and encourage other researchers to bring alternative evidence to bear in investigating these mechanisms.

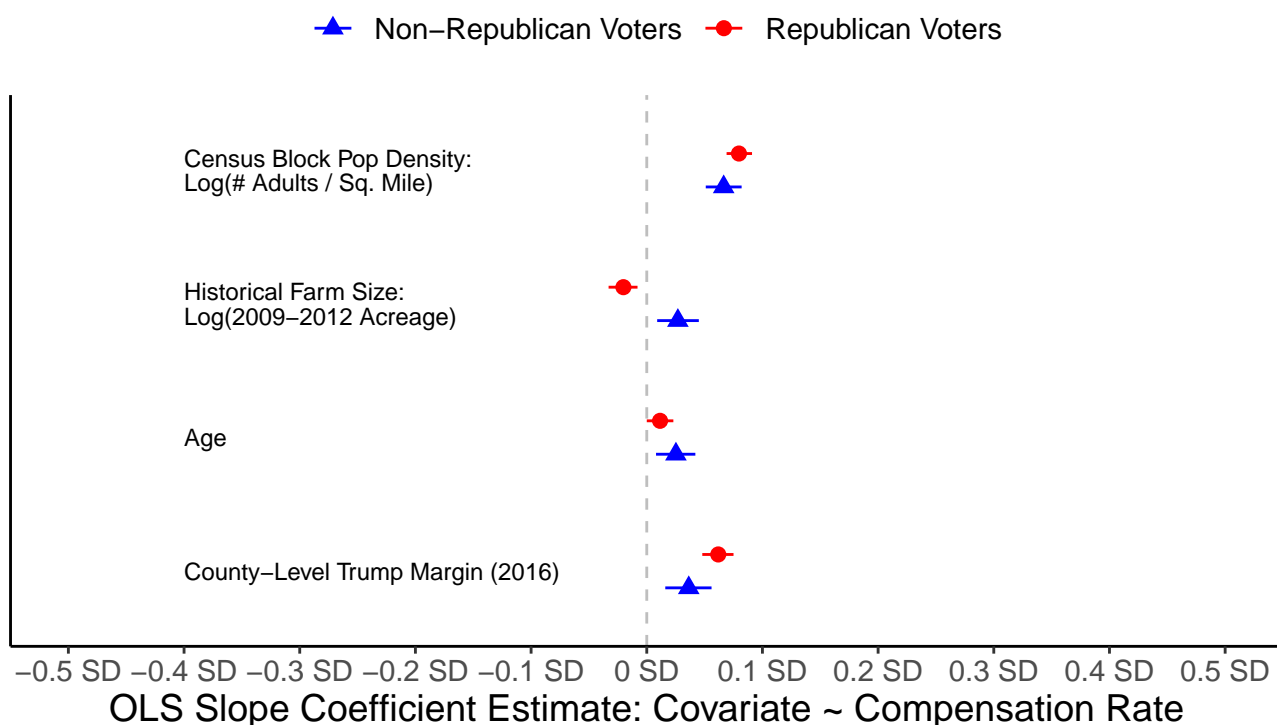
**Figure FSM9:** Correlation of Compensation Rate with Pre-Treatment Covariates: Binary Fields



Notes: Plot depicts slope coefficients from bivariate OLS regressions of pre-treatment covariates on MFP compensation rate as a share of tariff-induced losses, estimated separately for Republicans and non-Republicans. Point estimates are depicted alongside 95% confidence intervals constructed using robust standard errors. Each estimate can be interpreted as the expected increase in a covariate associated with a 100 percentage-point increase the compensation rate.

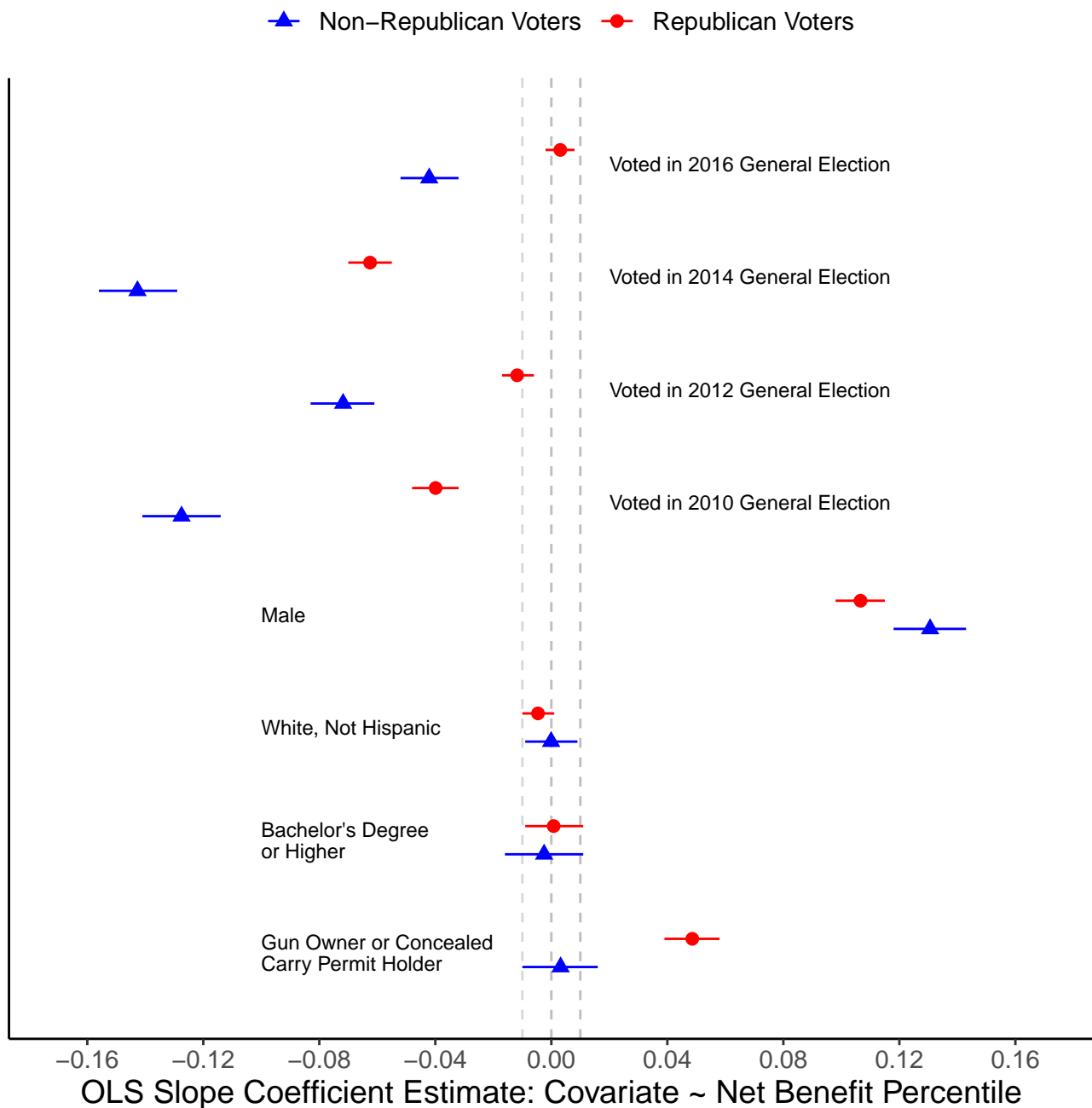


**Figure FSM10:** Correlation of Compensation Rate with Pre-Treatment Covariates: Standardized Quantitative Fields



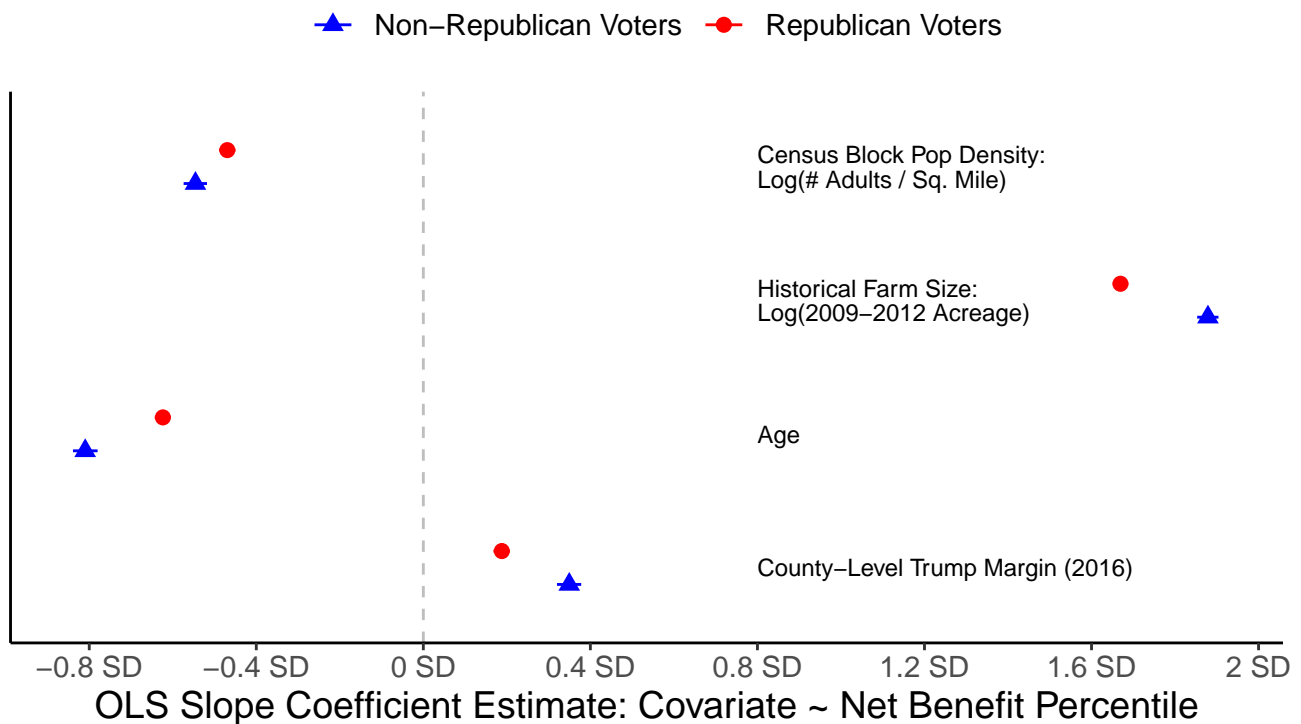
Notes: Plot depicts slope coefficients from bivariate OLS regressions of pre-treatment covariates on MFP compensation rate as a share of tariff-induced losses, estimated separately for Republicans and non-Republicans. Point estimates are depicted alongside 95% confidence intervals constructed using robust standard errors. Each estimate can be interpreted as the expected increase in a covariate associated with a 100 percentage-point increase the compensation rate. Each of the four fields depicted in this figure has been standardized by dividing by its standard deviation.

**Figure FSM11:** Correlation of Net Benefit Percentile with Pre-Treatment Covariates: Binary Fields



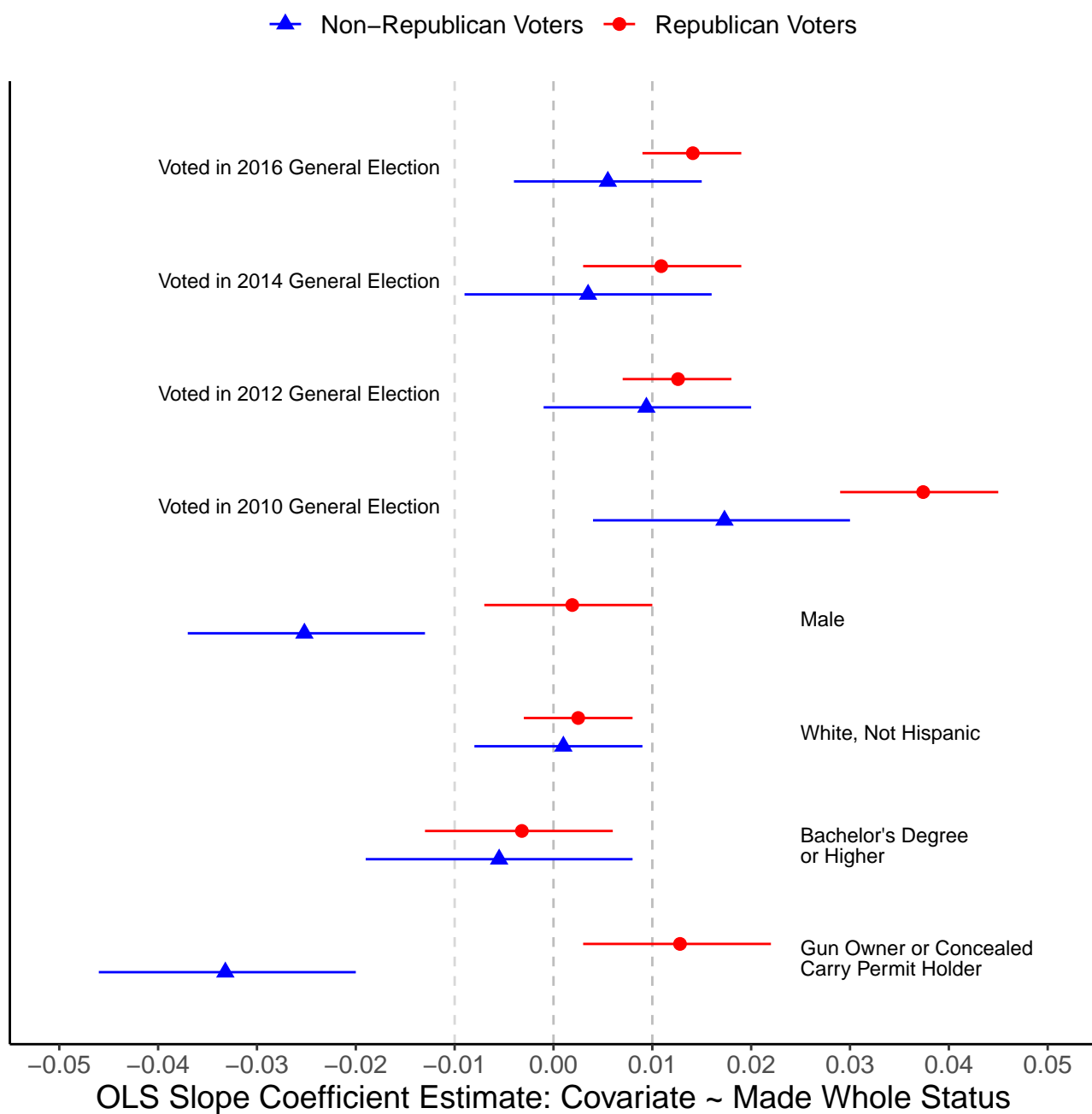
Notes: Plot depicts slope coefficients from bivariate OLS regressions of pre-treatment covariates on net MFP benefits specified in percentiles, estimated separately for Republicans and non-Republicans. Point estimates are depicted alongside 95% confidence intervals constructed using robust standard errors. Each estimate can be interpreted as the expected increase in the specified covariate from moving across the entire range of net MFP benefits.

**Figure FSM12:** Correlation of Net Benefit Percentile with Pre-Treatment Covariates: Standardized Quantitative Fields



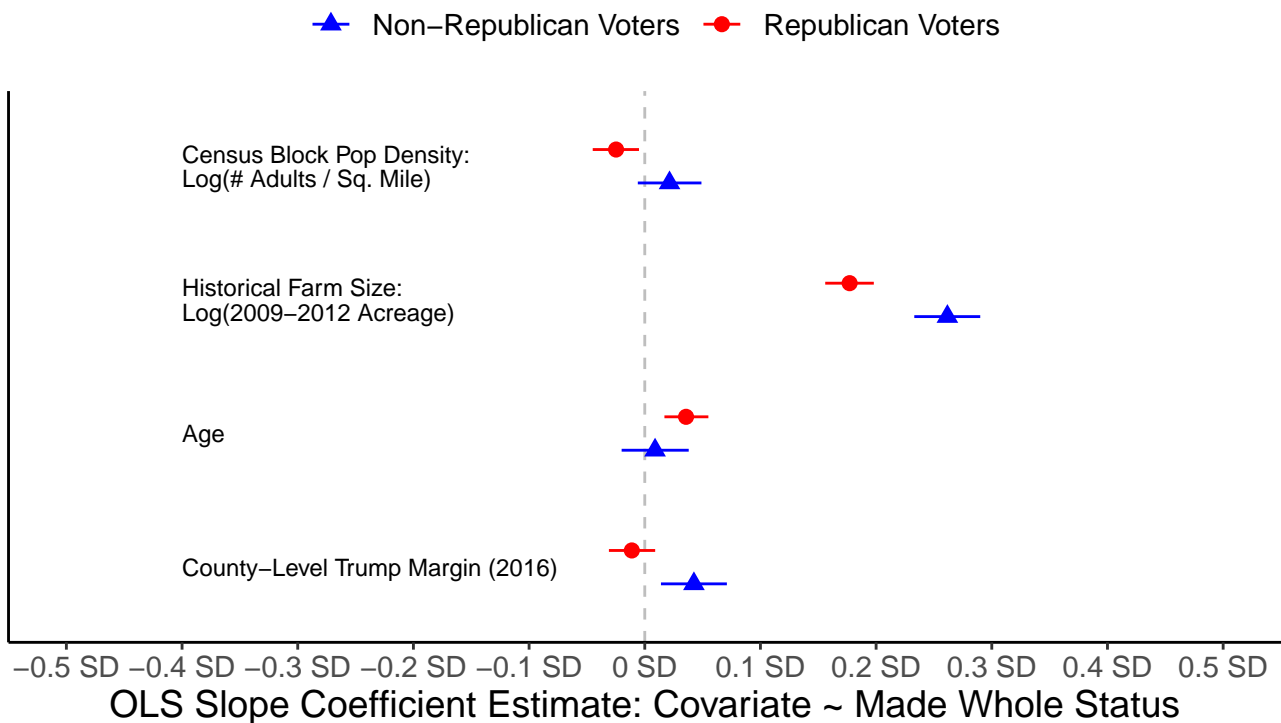
Notes: Plot depicts slope coefficients from bivariate OLS regressions of pre-treatment covariates on net MFP benefits specified in percentiles, estimated separately for Republicans and non-Republicans. Point estimates are depicted alongside 95% confidence intervals constructed using robust standard errors. Each estimate can be interpreted as the expected increase in the specified covariate from moving across the entire range of net MFP benefits. Each of the four fields depicted in this figure has been standardized by dividing by its standard deviation.

**Figure FSM13:** Correlation of Made Whole Status with Pre-Treatment Covariates: Binary Fields



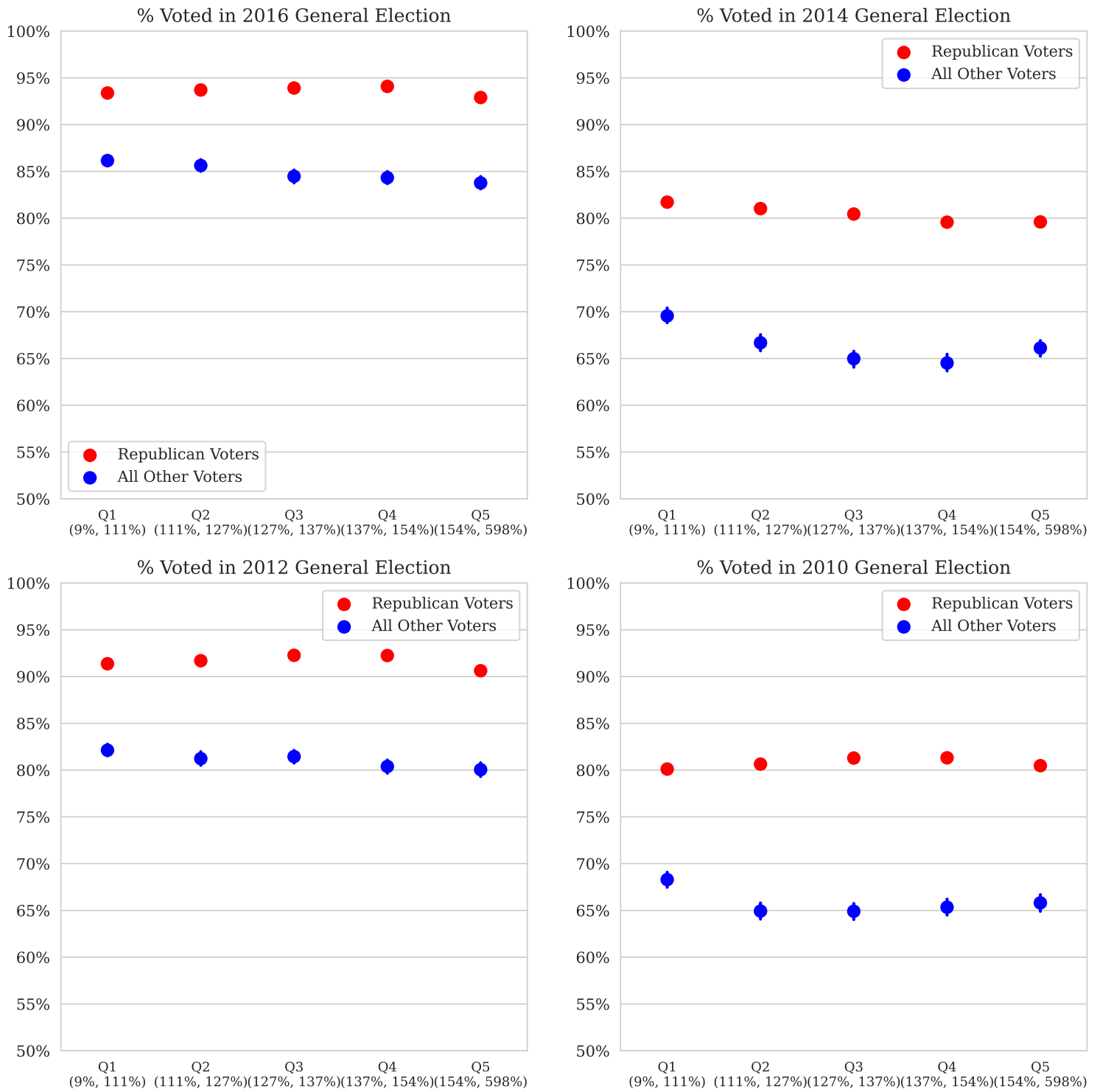
Notes: Plot depicts slope coefficients from bivariate OLS regressions of pre-treatment covariates on an indicator for whether MFP benefits exceeds tariff-induced losses, estimated separately for Republicans and non-Republicans. Point estimates are depicted alongside 95% confidence intervals constructed using robust standard errors. Each point estimate is algebraically equivalent to the difference in sample means between sample members who were made whole and those who were not.

**Figure FSM14:** Correlation of Made Whole Status with Pre-Treatment Covariates: Standardized Quantitative Fields



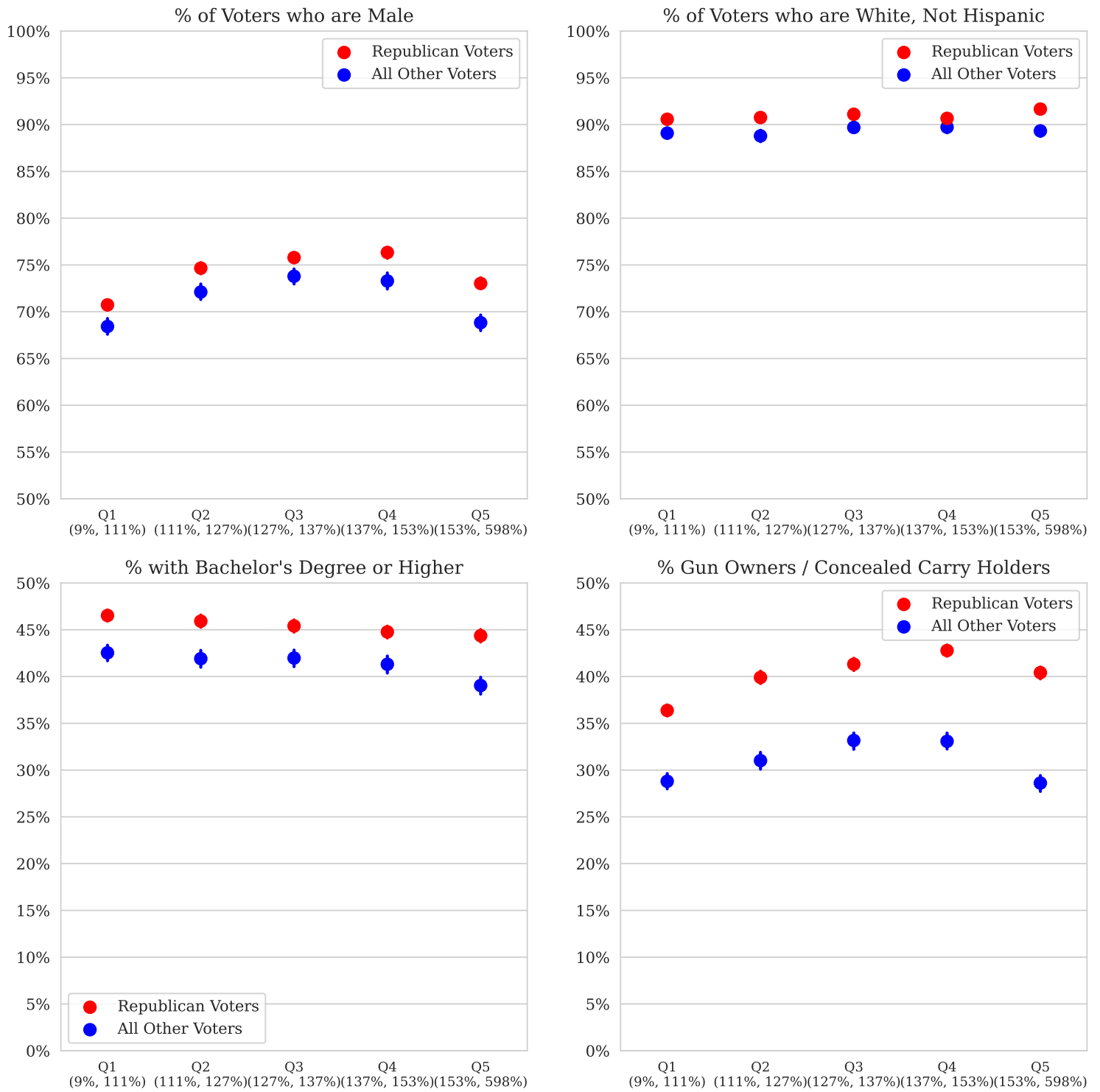
Notes: Plot depicts slope coefficients from bivariate OLS regressions of pre-treatment covariates on an indicator for whether MFP benefits exceeds tariff-induced losses, estimated separately for Republicans and non-Republicans. Point estimates are depicted alongside 95% confidence intervals constructed using robust standard errors. Each point estimate is algebraically equivalent to the difference in sample means between sample members who were made whole and those who were not. Each of the four fields depicted in this figure has been standardized by dividing by its standard deviation.

**Figure FSM15: Variation in Past Turnout by Compensation Rate Quintile**



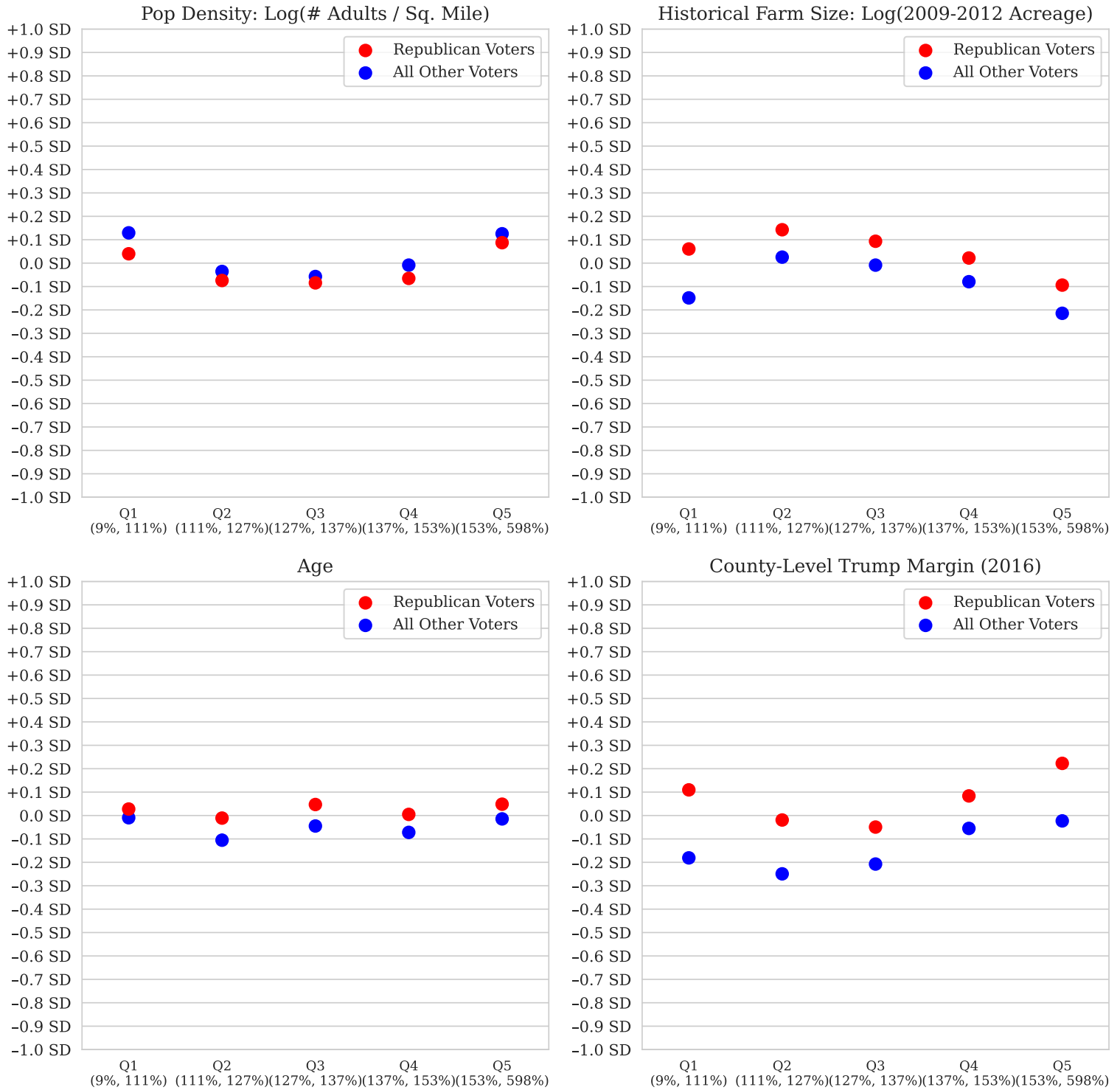
Notes: Each panel depicts sample averages among voters in the main analysis sample, with bootstrapped 95% confidence intervals.

**Figure FSM16:** Variation in Categorical Demographics by Compensation Rate Quintile



Notes: Each panel depicts sample averages among voters in the main analysis sample, with bootstrapped 95% confidence intervals.

**Figure FSM17: Variation in Quantitative Demographics by Compensation Rate Quintile**



Notes: Each panel depicts sample averages among voters in the main analysis sample. Fields are normalized before taking average to facilitate comparison and interpretation. Bootstrapped 95% confidence intervals are plotted but not visible due to precision of estimates.

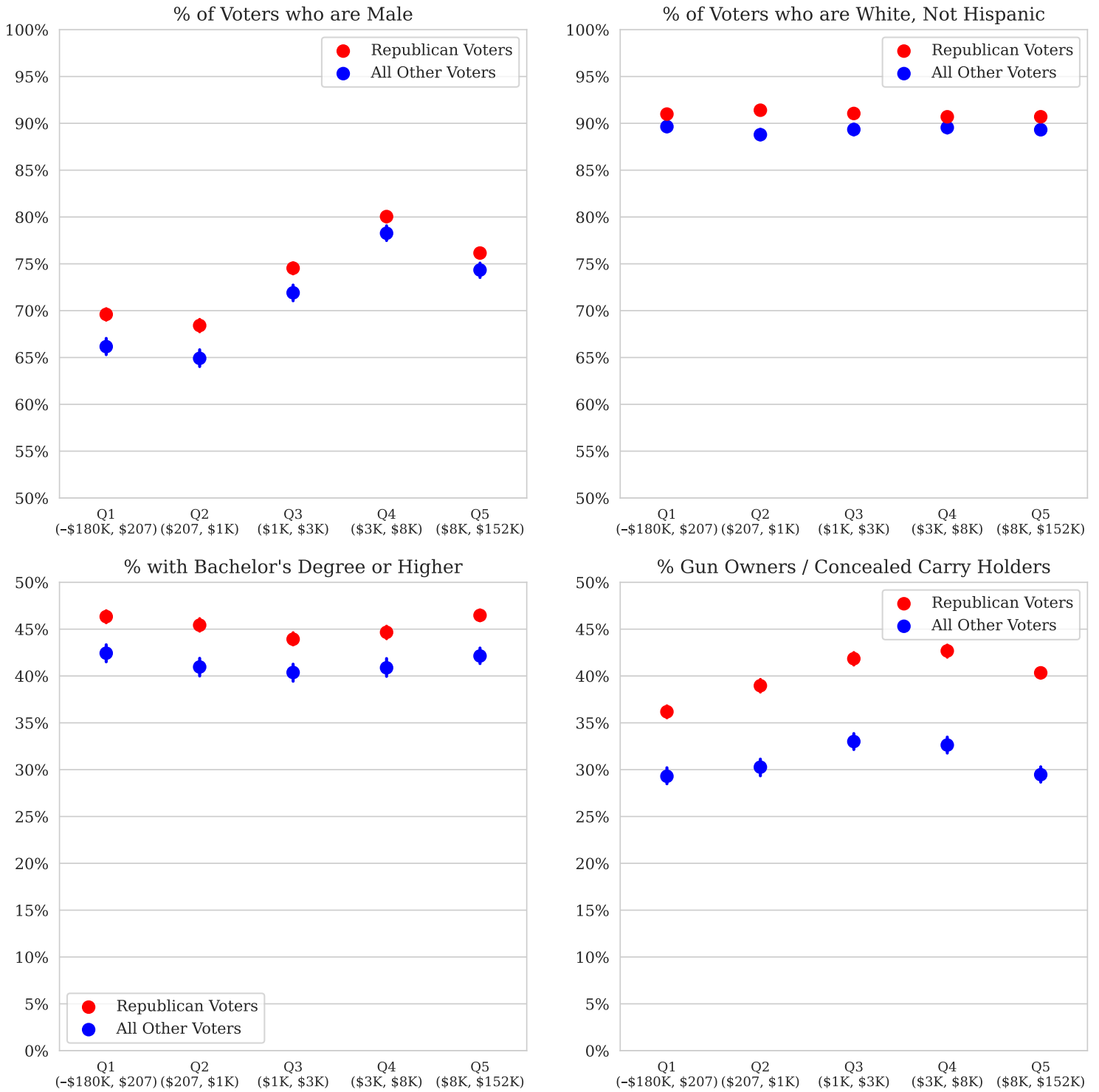


**Figure FSM18:** Variation in Past Turnout by Net Benefit Quintile



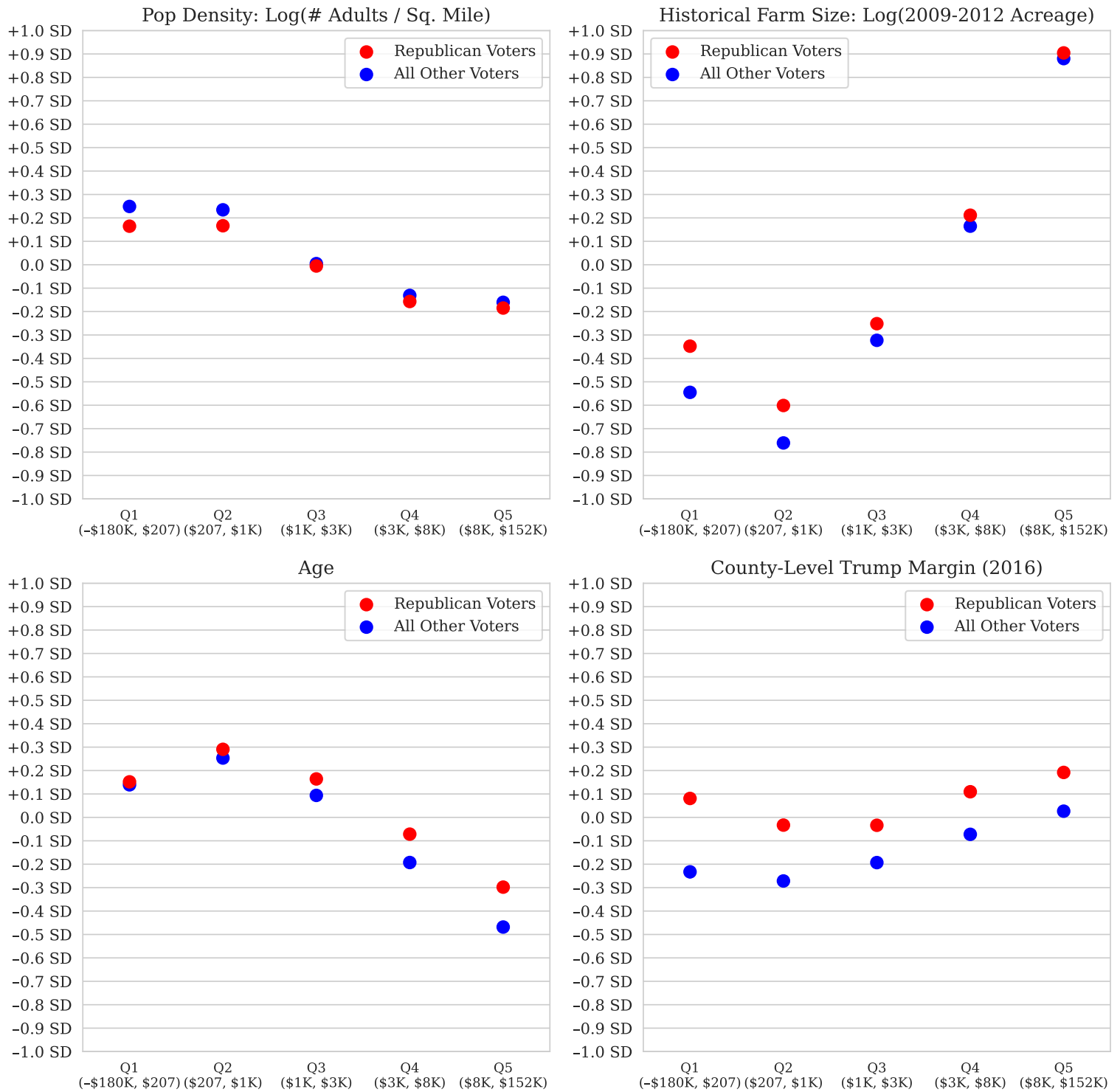
Notes: Each panel depicts sample averages among voters in the main analysis sample, with bootstrapped 95% confidence intervals.

**Figure FSM19: Variation in Categorical Demographics by Net Benefit Quintile**



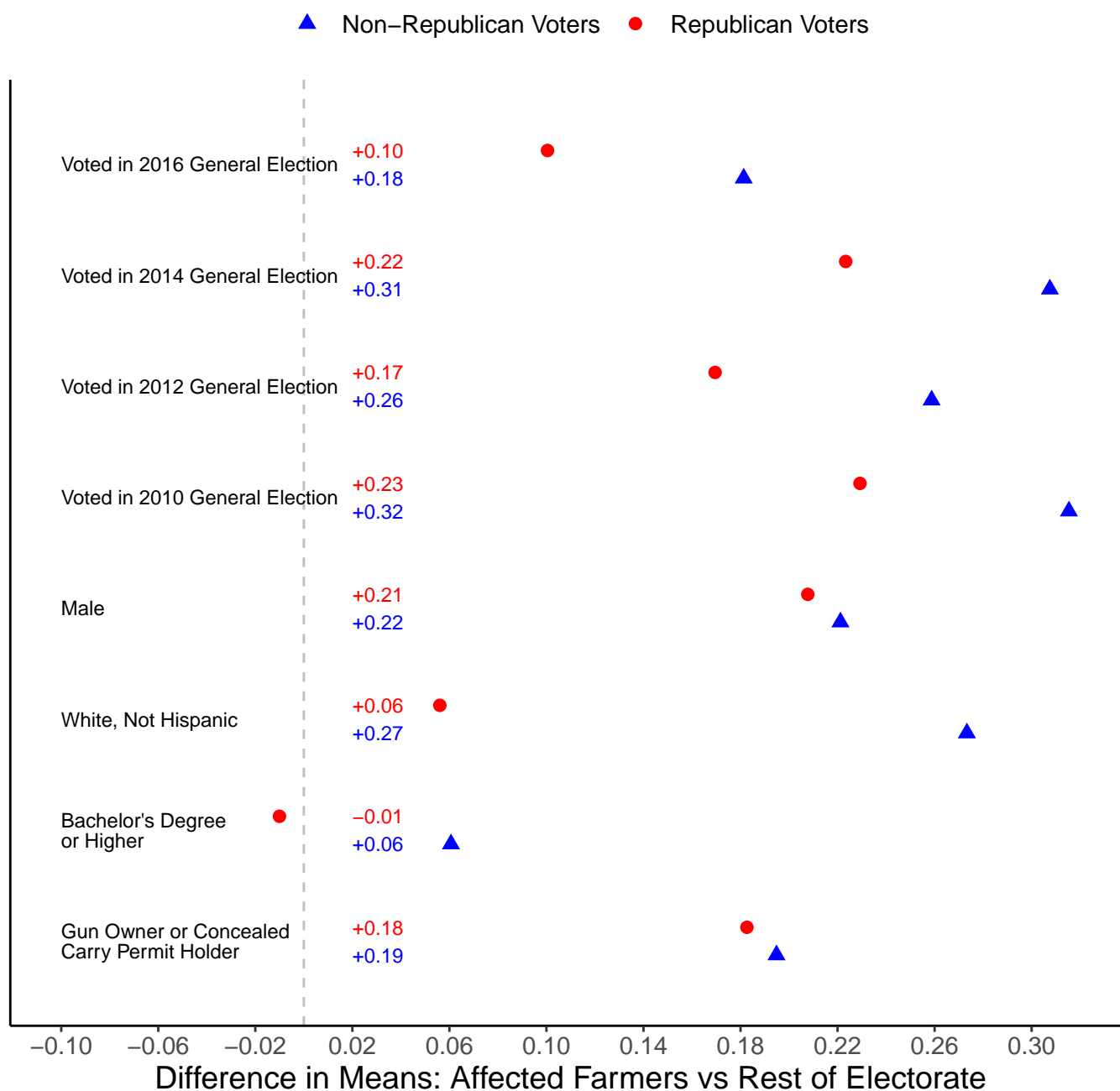
Notes: Each panel depicts sample averages among voters in the main analysis sample, with bootstrapped 95% confidence intervals.

**Figure FSM20:** Variation in Quantitative Demographics by Net Benefit Quintile



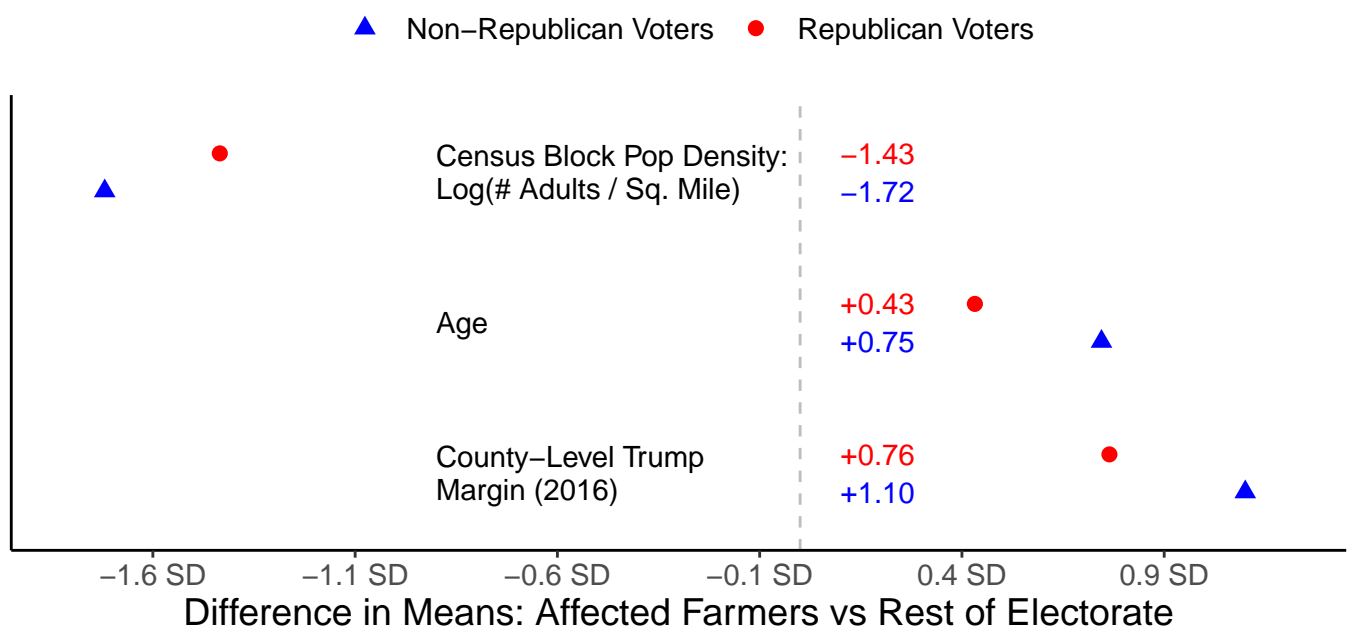
Notes: Each panel depicts sample averages among voters in the main analysis sample. Fields are normalized before taking average to facilitate comparison and interpretation. Bootstrapped 95% confidence intervals are plotted but not visible due to precision of estimates.

**Figure FSM21:** Covariate Balance by Figure 11 Treatment Status: Difference in Means for Binary Fields



Notes: Plot depicts average covariate values among voters flagged as treated in Figure 11 analyses minus the sample average among the rest of the electorate. Voters were considered treated if 2013-2017 USDA program enrollment indicated production of agricultural commodities directly affected by the trade war / MFP; see “Broader Effects of the Trade War and MFP on Farmers’ Political Engagement” in the main text or Further Supplemental Materials Section G for details.

**Figure FSM22:** Covariate Balance by Figure 11 Treatment Status: Difference in Means for Standardized Quantitative Fields



Notes: Plot depicts average covariate values among voters flagged as treated in Figure 11 analyses minus the sample average among the rest of the electorate. Voters were considered treated if 2013-2017 USDA program enrollment indicated production of agricultural commodities directly affected by the trade war / MFP; see “Broader Effects of the Trade War and MFP on Farmers’ Political Engagement” in the main text or Further Supplemental Materials Section G for details. Each of the three fields depicted in this figure has been standardized by dividing by its standard deviation.

## **E Record Linkage and Entity Resolution for USDA Administrative Data**

As noted in Online Appendix Section A.1, we have collected nearly the universe of USDA farm program direct payment transactions for the period 2004–2020. Through multiple FOIA requests, we obtained variously overlapping Farm Service Agency transaction database snapshots in different formats that together provide detailed information on the beneficiaries of US farm programs. While not all farmers receive direct payments from the USDA, the vast majority of row crop acreage (e.g. corn, soybeans, wheat, sorghum, cotton, rice, peanuts) is covered by such by such programs. As such, we consider these payment records to constitute a near-complete directory of row crop producers in the US.

As we discuss below, our data building pipeline for this project was extensive and centered on the objective of linking the largest number of voters possible to farm policy outcomes while minimizing measurement error. We first address a conceptual problem for analysing farm activity over time: our various transaction-level USDA databases are at the recipient level, and lack consistent and unique identifiers for recipients (and their farms) across the full 2004-2020 span. To overcome this, we develop an entity resolution algorithm to cluster transactions (and thereby recipients) into distinct “farming households,” which we refer to as “farms” for brevity.

As noted in Online Appendix A.2, the commercial vendor L2 maintains state voter files with essential data preprocessing (e.g., purging duplicates and linking voter profiles over time) and a number of useful added fields. As noted in Online Appendix A.3, farm-level measures of campaign contributions rely on itemized contribution records in the Database on Ideology, Money in Politics, and Elections (DIME) (Bonica, 2014). We develop a highly customized record linkage algorithm to link USDA farm program recipients to distinct voter profiles for L2’s February 2018, June 2019, and May 2021 voter file snapshots, and then to distinct contributor profiles in DIME. In doing to, we link voters and consumers in L2’s nationwide

databases—and contributors in DIME—to distinct farms in the USDA administrative payment data. In our main analyses, this allows us to estimate changes in individual-level voter behavior stemming from farm-level policy outcomes.

## E.1 Data and Pre-Processing

All transaction-level records in our patchwork of USDA Farm Service Agency database snapshots contain at least a few key fields: names and addresses of payment recipients, key payment details (i.e. date of payment, transaction amount, and program name), and the Farm Service Agency office through which the payment was processed (generally indicative of the physical location of the farm in question). In particular, a nearly comprehensive 2004-2020 dataset we have obtained is limited to these fields. Certain older datasets feature additional useful fields. Prior to calendar year 2019, the USDA employed a (nearly) distinct identifier (the “customer number”) for farm program recipients that is useful for identifying individuals over time. Certain pre-2019 database snapshots we’ve obtained also attribute transactions to particular crops (e.g. corn or soybeans). Critically, the calendar year 2018 database of MFP transactions—on which we base all of our main analyses—features these “customer number” identifiers and attributes transactions to specific crops.

Linking farm payment records to L2 consolidated voter/consumer profiles is inherently difficult due to the lack of standardized and highly informative fields in the USDA payment files. In particular, recipient names are not broken into constituent parts (i.e., first name, middle name, last name, suffix) and in many cases refer to a business entity rather than an individual (e.g., “[FIRST] [LAST] FARMS LLC”, “[CITY] DAIRY FARM INC”). Additionally, the current FSA record system does not provide unique recipient identifiers, and records lack useful identifying characteristics—such as age and gender—that would help narrow down potential comparisons.

To surmount these difficulties and obtain a merge with both high precision and recall, we created a highly customized record-linkage algorithm that leverages extensive pre-processing and auxiliary data to squeeze as much matching-relevant information as possible from the

USDA program data. We start by using text analysis to parse each recipient name into constituent components: first name, first middle name, second middle name (rarely populated), last name, and suffix.<sup>6</sup> We standardize the resulting name fields by uppercasing all letters and stripping out whitespace and punctuation. We standardize suffixes by converting generations to integers, so that (for instance) “SR” and “T” map to the same value. We then merge in nicknames (and formal versions of nicknames) using the proprietary pdNicknames database. We also assign each farm recipient profile a gender based on first name if the SSA name popularity rank for a given gender is 20 times greater than that of the other gender.

Address pre-processing similarly centered on extensive text analysis to extract street and PO Box numbers. In addition, we geocoded addresses in the USDA payment data.<sup>7</sup> As L2 provides geographic coordinates for each address in the voter and commercial files, we are therefore able to calculate straight-line distances for every pair of potential matches we evaluate.

For the purposes of record linkage, we define a distinct farm program recipient “profile” as a unique combination of name and address. In the following section, we describe our method for linking distinct individuals in the L2 databases to such profiles. Afterwards, we discuss how we cluster these profiles into distinct “farms” and ensure that each L2 voter/consumer is matched with at most one distinct farm.

## E.2 Probabilistic Record Linkage Model Specification

After the pre-processing stage, our record-linkage procedure centers around estimating the canonical record-linkage model of Fellegi and Sunter (1969). We direct readers to Enamorado,

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<sup>6</sup>We do this by categorizing each recipient name into one of 92 distinct regular expressions based on the structure and organization of its name components. When a recipient name indicates a couple (e.g., “[FIRST] AND [FIRST] [LAST] FAMILY CORPORATION”) we split the profile in two and separately consider comparisons with L2. Additionally, in cases in which we cannot extract a likely human name from the recipient name box, we are usually able to extract a name from the first or second address boxes (e.g., a business entity has second address field “% [FIRST] [LAST]”).

<sup>7</sup>In the case of PO Box mailing addresses or addresses that otherwise are not amenable to geocoding, we use representative latitude/longitude coordinates for the provided ZIP code.



Fifield, and Imai (2019) for a full theoretical treatment of this model, and instead provide a brief summary of the model’s structure within our setting. In the notation of Enamorado, Fifield, and Imai (2019), consider datasets  $\mathcal{F}$  (farm subsidy recipients) and  $\mathcal{V}$  (voters). Each recipient  $i \in \mathcal{F}$  can be compared to a voter  $j \in \mathcal{V}$  along seven dimensions:

$$K = \{\text{first, middle, last, suffix, location, occupation, age}\}.$$

For a comparison between profiles  $i \in \mathcal{F}$  and  $j \in \mathcal{V}$ , we define an agreement vector  $\gamma_{ij} \equiv (\gamma_{\text{first}}, \gamma_{\text{middle}}, \gamma_{\text{last}}, \gamma_{\text{suffix}}, \gamma_{\text{location}}, \gamma_{\text{occupation}}, \gamma_{\text{age}})$  such that each coordinate  $k \in K$  reflects a discrete-valued similarity along the specified dimension. For example,  $\gamma_{\text{last}}(i, j)$  takes on one of three values (0, 1, or 2) to indicate the string-distance similarity between the last names of  $i$  and  $j$ . In particular,  $\gamma_{\text{last}}(i, j) = 2$  if there is an exact match on last name, whereas  $\gamma_{\text{last}}(i, j) = 0$  indicates that the two surnames are quite dissimilar. The full definitions of the agreement measures  $\gamma_k(\cdot, \cdot)$  are given in Table FSM2; note in particular that  $\gamma_{\text{occupation}}$  and  $\gamma_{\text{age}}$  rely only on the characteristics of the given L2 profile.

Having defined the agreement vector, we can write down our record linkage model as follows. Let  $M_{ij}$  be a latent mixing variable that indicates whether  $i \in \mathcal{F}$  and  $j \in \mathcal{V}$  are actually a match. We assume that

$$M_{ij} \stackrel{\text{i.i.d.}}{\sim} \text{Bernoulli}(\lambda),$$

where  $\lambda \in (0, 1)$  denotes the (unknown) probability of a match across all comparisons under consideration. Additionally, for  $m \in \{0, 1\}$  and  $k \in K$ ,

$$\gamma_k(i, j) \mid M_{ij} = m \stackrel{\text{indep}}{\sim} \text{Discrete}(\pi_{k,m}),$$

where  $\pi_{k,m}$  is a vector containing the probabilities of realizing each agreement level in dimension  $k$  given that the comparison is actually a match ( $m = 1$ ) or not ( $m = 0$ ). In words,  $\pi_{\text{last},1}$  is a triple containing the conditional probabilities that a comparison of records yields

Table FSM2: Match Agreement Measures for USDA-L2 Record Linkage

Dimension	Level	Description
first	5	Exact match on first name (and names are more than initials)
	4	First name of $i$ is a nickname for first name of $j$
	3	First name of $i$ starts with / ends with first name of $j$ , OR there is a Jaro-Winkler string similarity of at least 0.9
	2	Profile $i$ provides only a first initial, and it matches first initial of $j$
	1	Gender inferred from recipient first name does not conflict with L2 gender field
	0	None of the above criteria are satisfied
middle	3	Exact match on both middle names (and names are more than initials)
	2	Either middle name of profile $i$ is an exact match with either middle name of profile $j$ , OR there is a Jaro-Winkler string similarity between first middle names of at least 0.9
	1	Profile $i$ provides only a middle init, and it matches either middle init of $j$
	0	None of the above criteria are satisfied
last	2	Exact match on last name
	1	Jaro-Winkler string similarity between last names of at least 0.94, or one last name is a shortened version of the other (e.g. “Jones” and “Jones-Smith”)
	0	None of the above criteria are satisfied
suffix	1	Profiles $i$ and $j$ both have suffix field populated, and there’s a match
	0	Profile $i$ has suffix field populated, and it conflicts with $j$
location	5	Street number or PO box match, and ZIP code matches (L2 mailing address, voter file residence, or commercial file residence)
	4	Recipient ZIP matches any L2 address ZIP
	3	Recipient city matches any L2 address city, OR haversine distance between profiles less than 10 miles
	2	Haversine distance between profiles less than 50 miles
	1	Haversine distance between profiles less than 100 miles
	0	None of the above criteria are satisfied
occupation	1	Voter file occupation is “Skilled Trades-Farmer” OR commercial file occupation is “Farmer/Dairyman” OR commercial file occupation group is “Farmer”
	0	None of the above criteria are satisfied
age	5	L2 profile age $> 70$
	4	L2 profile age $\in (60,70]$
	3	L2 profile age $\in (50,60]$
	2	L2 profile age $\in (40,50]$
	1	L2 profile age $\in (30,40]$
	0	L2 profile age $\leq 30$

Note: The Fellegi and Sunter (1969) model employs a missing at random (MAR) assumption, and by construction a missing value in one dimension has no bearing on the inferred match probability for a given comparison.

a match on last name of similarity level 0, 1, or 2 given that the two records in question are actually a match. Likewise,  $\pi_{\text{last},0}$  is the triple of probabilities that a surname match level is obtained conditional on a pair of records actually *not* being a match.

Given there are a total of  $6 + 4 + 3 + 2 + 6 + 2 + 6 = 29$  distinct agreement levels across the seven match dimensions, we have 59 parameters to estimate: the overall match probability ( $\lambda$ ), 29 conditional-on-match agreement level probabilities ( $\pi_{k,1}$ ), and 29 conditional-on-nonmatch agreement level probabilities ( $\pi_{k,0}$ ). As noted in Enamorado, Fifield, and Imai (2019), with a couple of technical assumptions,<sup>8</sup> we can write down a likelihood function for this data-generating process and readily estimate these 59 parameters using the Expectation-Maximization (EM) algorithm.

### E.3 Record Linkage Implementation

Linking USDA recipient profiles to L2 voter and consumer file IDs entails evaluating pairwise comparisons for each profile we are interested in matching. Given the data standardization described in the first subsection, we are left with 4,953,072 USDA profiles to match to L2 IDs. Profiles in L2’s 2021 commercial files are uniquely identified by an L2 “consumer ID”; L2 provides a crosswalk that matches most consumer IDs to distinct “voter IDs” within the voter files. After consolidating information between each snapshot of the voter file and the 2021 commercial file, we arrive at the task of comparing each USDA recipient profile with 285,494,364 profiles from the February 2018 L2 snapshot (179,925,726 voter IDs consolidated with 220,652,455 consumer IDs), 290,767,966 profiles from the June 2019 snapshot (185,199,328 voter IDs consolidated with 225,996,623 consumer IDs), and 313,631,026 profiles from the May 2021 snapshot (208,062,388 voter IDs consolidated with 239,816,591 consumer

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<sup>8</sup>These assumptions are not innocuous. In particular, we must assume conditional independence among linkage variables given the match status. In practice, problems associated with violations of this assumption are similar to multicollinearity issues in linear regression modeling. If two match categories measure essentially the same information, parameter estimates can become highly unstable. To mitigate this issue in our setting, we chose our set of seven match dimensions to be maximally disconnected. For example, instead of separately evaluating similarity of street name and ZIP code, we bundled all information relating to location into a single match dimension.

IDs).<sup>9</sup> Even considering each of these snapshots separately, it is not computationally feasible for us to compare each USDA recipient profile with each L2 profile. For example, the Cartesian product of the USDA recipient profiles and 2018 L2 profiles would entail over 1.4 quadrillion comparisons.

It is thus critical to place strict limits on the comparisons made when estimating the Fellegi-Sunter record linkage model. First, we group states into 30 regions, and estimate separate models for each region. However, we still require further refinement to obtain a computationally feasible number of comparisons within each region. Indeed, such large-scale record linkage nearly always necessitates the use of an explicit “blocking” technique, in which the researcher places restrictions on which comparisons are to be evaluated. We devise blocking rules based on the match fields to further winnow down the set of comparisons. We use the PySpark package **splink** to implement a blocking strategy, estimate model parameters via the EM algorithm, adjust for surname frequency, and compute match probabilities. Documentation is available at <https://github.com/moj-analytical-services/splink>.

As noted in the **splink** documentation, it is important that blocking rules limiting comparisons are not based on the same fields that are used for estimating model parameters. However, it is typically critical to block on match fields to reduce the number of comparisons to a manageable level. As such, **splink** allows users to implement a typical workaround from record linkage practice: estimating and combining multiple “smaller” models. Specifically, we estimate six models, each with different tight blocking rules based on a subset of match fields that allow estimation of parameters corresponding to the remaining fields. Table FSM3 lists the six record linkage models we estimate and combine for each of the 30 regions. To make this exposition clear, we note that the first row indicates that we estimate a model with the

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<sup>9</sup>As noted in Online Appendix Section A.2, L2 tracks voters across time. We have found voter IDs to be very consistent across snapshots. The one systematic exception is that L2 voter IDs are state-specific, and when a voter moves across state lines between election snapshots, L2 creates a new voter ID and assigns the voter’s turnout history to the new ID. Given that farming is tied to a particular plot of land, out-of-state moves are extremely rare in our main analysis sample. Nonetheless, in further data processing, we attempt to track the small handful of voters in our sample who appear to have changed voter IDs across snapshot. Details are available from the authors upon request.

**Table FSM3: Record Linkage Blocking Rules**

Parameters Estimated	Blocking Rule
last, suffix, location, occupation, age	Exact match on first name AND exact match on middle name
first, suffix, location, occupation, age	Exact match on last name AND exact match on middle initial
first, middle, last, suffix, occupation, age	Exact match on ZIP AND exact match on street number / PO box number
first, middle, suffix, occupation, age	Exact match on ZIP code and last name
middle, last, suffix, occupation, age	Exact match on ZIP code and first name
middle, suffix, location, occupation, age	Exact match on first name AND exact match on last name

Notes: Each row specifies a separate record linkage model estimated. The left column lists the match dimension from the Fellegi-Sunter model for which parameters are being estimated (see Table FSM2). The right column lists restrictions on the set of comparisons considered. The six models are combined to produce a single set of global parameter estimates; see the **splink** package documentation and Enamorado, Fifield, and Imai (2019) for details on combining models.

match dimensions **last**, **suffix**, **location**, **occupation**, and **age** from Table FSM2. In doing so, we limit profile comparisons to those featuring an exact match on first name and middle name. Of course, requiring comparisons to match on first name and middle name will throw out many true matches, such as cases in which a recipient doesn't report their middle name or only reports a first initial. However, the final set of match evaluations is made using a model aggregated from the six sets of estimates here, applied to all comparisons satisfying *any* of the six blocking rules listed in the right column of Table FSM2. Indeed, it should be extremely uncommon for a true match (or at least, a match that could be identified by a human coder) to not satisfy any of our six blocking rules. In summary, this procedure enables computationally tractable estimation of all parameters of our record linkage model, while allowing us to still vet nearly 100% of remotely plausible match comparisons.

Estimating the model parameters allowed us to compute a match probability for each

comparison, which we further adjusted for the relative frequency of each profile’s surname (see the `splink` documentation or Enamorado, Fifield, and Imai (2019) for details). After manually reviewing several hundred comparisons, we find the match probability to be a very strong indicator of relative match plausibility, with comparisons rated above 90% almost always corresponding to what a human coder would consider a match. Nonetheless, we find that we can most closely implement our preferred concept of a match by utilizing the estimated match probabilities in an ensemble of rules.<sup>10</sup>

To present the final “match rule” we settled on, we define:

- a “weakly consistent match on name” as when there is no definitive clash on first name, last name, or suffix,<sup>11</sup>
- a “strictly consistent match on name” as a weakly consistent match on name that also features no clash in middle name as well as exact matches on first and last name,<sup>12</sup> and
- an “affirmative match on name” as a strictly consistent match on name that also obtain some positive indication of a match from the middle name fields (e.g. middle initials match)<sup>13</sup>.

We then accept matches that satisfy any of the following four conditions:

1. weakly consistent name and match probability above 80%,

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<sup>10</sup>It is worth noting that just because we do not solely rely on the Fellegi-Sunter match probability for determining matches does not mean that the model performs “poorly” or has a high error rate. At its core, our method is an *unsupervised* machine learning algorithm – rather than supply the algorithm with ground truth matches, we simply supply a series of profiles and ask the model to identify clusters within the data. In our setting, there’s not a single objective underlying truth as to what a “true” match is, since farm recipient profiles sometimes indicate family names or LLCs rather than individuals.

<sup>11</sup>Formally, using the agreement levels defined in Table FSM2, a weakly consistent match on name satisfies  $\gamma_{\text{first}} \notin \{0, 1\}$ ,  $\gamma_{\text{last}} > 0$ , and  $\gamma_{\text{suffix}} \neq 0$ , where comparisons with missing values on either sided are coded  $\gamma_k = -1$ .

<sup>12</sup>Formally, using the agreement levels defined in Table FSM2, a strictly consistent match on name satisfies  $\gamma_{\text{first}} = 5$ ,  $\gamma_{\text{middle}} \neq 0$ ,  $\gamma_{\text{last}} = 2$ , and  $\gamma_{\text{suffix}} \neq 0$ .

<sup>13</sup>Formally, using the agreement levels defined in Table FSM2, an affirmative match on name satisfies  $\gamma_{\text{first}} = 5$ ,  $\gamma_{\text{middle}} > 0$ ,  $\gamma_{\text{last}} = 2$ , and  $\gamma_{\text{suffix}} \neq 0$ .

2. weakly consistent name and perfect match on address (i.e.  $\gamma_{\text{location}=5}$ ),
3. strictly consistent match on name, addresses quite close ( $\gamma_{\text{location}>2}$ ), and the L2 ID is the only L2 ID in the set of comparisons constituting a strictly consistent match on name for that USDA profile,
4. affirmative match on name, addresses quite close ( $\gamma_{\text{location}>2}$ ), and the L2 ID is the only L2 ID in the set of comparisons constituting an affirmative match on name for that USDA profile.

In actuality, the vast majority of accepted matches satisfied both of the first two conditions on this list. However, the latter two conditions allowed us to scrape a few more highly credible matches and thereby maximize the accuracy of our record linkage.

## E.4 Entity Resolution for Farm Program Recipients

Were we to stop our record linkage at this point and assign USDA program recipients to the voter(s) matched to each profile, the resulting links would be highly accurate, but would present certain conceptual difficulties for measuring policy outcomes. Some farms have joint ownership, and some individuals use different names over time or in different Farm Service Agency transaction databases (e.g. “John A Smith” vs “Smith Family Farm LLC”). As such, we find it most appropriate to cluster USDA recipient profiles together at the “farming household” level and define all farm attributes and policy outcomes at this level.

We design an entity resolution algorithm that implements this clustering using recipient profiles’ names and locations. We also make maximal use of the Farm Service Agency’s internal system of linking and tracking profiles over time. In particular, as noted in the first subsection, the Farm Service Agency used “customer numbers” to identify and track distinct recipients in operations prior to calendar year 2019. While our most comprehensive 2004–2020 database of FSA transactions lacks this identifier, earlier database releases we have obtained for 2004–2012, 2014–2017, and CY 2018 MFP transactions feature this field. We merge in these customer numbers to our most comprehensive 2004–2020 transaction-level database using an

**Table FSM4: Match Agreement Measures for Farm Entity Resolution**

Dimension	Level	Description
first	5	Exact match on first name (and names are more than initials)
	4	First name of $i$ is a nickname for first name of $j$
	3	First name of $i$ starts with / ends with first name of $j$ , OR there is a Jaro-Winkler string similarity of at least 0.9
	2	Profile $i$ provides only a first initial, and it matches first initial of $j$
	1	Gender inferred from first name $i$ does not conflict with that of $j$
	0	None of the above criteria are satisfied
middle	3	Exact match on both middle names (and names are more than initials)
	2	Either middle name of profile $i$ is an exact match with either middle name of profile $j$ , OR there is a Jaro-Winkler string similarity between first middle names of at least 0.9
	1	Profile $i$ provides only a middle init, and it matches either middle init of $j$
	0	None of the above criteria are satisfied
last	2	Exact match on last name
	1	Jaro-Winkler string similarity between last names of at least 0.94, or one last name is a shortened version of the other (e.g. “Jones” and “Jones-Smith”)
	0	None of the above criteria are satisfied
suffix	1	Profiles $i$ and $j$ both have suffix field populated, and there’s a match
	0	Profile $i$ has suffix field populated, and it conflicts with $j$
entity	3	Names for $i$ and $j$ both have entity text, and there’s an exact match
	2	Jaro-Winkler string similarity between entity text of at least 0.94
	1	Jaro-Winkler string similarity between entity text of at least 0.85 OR one is a shortened version of the other (e.g. “FARMS” and “FARMS LLC”)
	0	Profile $i$ has suffix field populated, and it conflicts with $j$
recipient_type	1	Names for $i$ and $j$ indicate the same recipient type
	0	Names for $i$ and $j$ indicate different recipient types
customer_number	4	Profiles share a customer number
	3	Customer numbers for $i$ and $j$ belong to the same connected component in the business party share graph AND in the payment attribution graph
	2	Customer numbers for $i$ and $j$ belong to the same connected component in the business party share graph
	1	Customer numbers for $i$ and $j$ belong to the same connected component in the payment attribution graph
	0	None of the above criteria are satisfied
location	5	Street number or PO box match, and ZIP code matches
	4	Match on ZIP or ZCTA
	3	Match on city + state OR haversine distance less than 10 miles
	2	Haversine distance between profiles less than 50 miles
	1	Haversine distance between profiles less than 100 miles
	0	None of the above criteria are satisfied

Note: The Fellegi and Sunter (1969) model employs a missing at random (MAR) assumption, and by construction a missing value in one dimension has no bearing on the inferred match probability for a given comparison. The “entity text” for a profile (if any) refers to the text not indicating a person’s name (e.g. “FARMS INC” for “JONES FARMS INC”).



exact match on name and either address or ZIP code. While this criteria for assignment is quite strict, this nonetheless allows us to assign an official USDA identifier to the vast majority of transactions between 2004 and 2020. We generate stand-in “customer numbers” for the remaining transactions in the 2004-2020 database using distinct combinations of name and address. We then define our entity resolution problem as how to appropriately cluster customer numbers together into units that most resemble a single business or family operation.

Similar to our approach to the record linkage problem, we identify a number of distinct dimensions on which the similarity of two USDA recipient profiles can be compared. From parsing recipient names, we obtain all of the name fields considered by the USDA-L2 record linkage algorithm (e.g. first, middle, last, suffix). Additionally, we capture the residual part of the name not captured by these fields (e.g. “REVOCABLE TRUST” for “JANE DOE REVOCABLE TRUST” or “GREEN PASTURES FARMS” for “GREEN PASTURES FARMS”), which we term the “entity” associated with a profile name. Likewise, we assign each profile a “recipient type” based on the structure of the recipient name field, with the most frequent designations being the name of an individual (81% of profiles), a trust in an individual’s name (4% of profiles), and a farm in the name of an individual (3% of profiles).

We also use additional auxiliary information on links between customer numbers to further inform our clustering. Specifically, we leveraged FSA datasets from 2004-2012 and 2014-2017 that link individual FSA IDs to the FSA IDs of businesses that are partly or wholly owned by the individual in question. These links come in two forms. In the 2014-2017 “business party share” files, the FSA records whether certain customer numbers have ownership stakes in other customer numbers. In a separate set of databases, payments are attributed to a customer number indicating a joint operation as well as customer numbers indicating the individuals receiving shares of this payment. We construct directed graphs from these two sets of links and identify connected components within these graphs. We use membership in the same connected components of these graphs as an indication of similarity in our entity resolution algorithm. This is especially useful for the minority of profiles in which the recipient

name indicates an entity but not an individual (“GREEN PASTURES FARMS”), and ensures that in the large majority of such cases we are able to link the business name to an individual’s name.

Having described these new fields, our entity resolution algorithm is easy to describe, since it very closely mirrors the structure of our record linkage algorithm described in the previous sections. Specifically, we estimate a Fellegi-Sunter record linkage model in which we compare each of the 4,953,072 recipient profiles to the remaining profiles. Figure FSM4 presents the agreement vectors that define our model. For the sake of brevity, we omit a detailed discussion of our implementation (including our blocking strategy). After estimating match probabilities among profiles, we construct a directed graph between customer numbers by drawing an edge between two customer numbers if we are able to identify a link between any two of their constituent recipient profiles. We then label the connected components of this graph as distinct “farming households” or “farms.”

**Table FSM5: Farm Groups Among USDA Customer Numbers**

	# of Customer Numbers	# of Farms (Clusters)
All Farm Program Recipients (Full 2004-2020 Database)	3,909,482	2,792,985
Recent Recipients (All Programs, 2013-2020)	1,999,763	1,610,089
2018 MFP Recipients (All Recipients)	581,953	502,352
2018 MFP Recipients (CY 2018 Recipients)	350,301	306,941

Notes: Of the \$8.6 billion distributed through the 2018 MFP, \$5.2 billion (60.0%) was released in calendar year 2018, \$3.4 billion (39.7%) was released in calendar year 2019, and \$21 million (0.2%) was released in calendar year 2020. Our main analysis depends on a crop-level tabulation of CY 2018 MFP transactions.

Table FSM5 presents the results of this clustering algorithm. The full set of 4,953,072 distinct profiles fed into the Fellegi-Sunter model (which together reflect the universe of 2004-2020 Farm Service Agency transactions) are associated with just under four million customer

numbers, which we cluster into 2.8 million farm groups. We identify 1.6 million farm groups among the last eight years of payments, including 502,352 that enrolled in the 2018 MFP. Notably, we find that 858,369 groups received payments in 2017, which is roughly comparable to the 643,145 farms recorded as receiving government payments in the 2017 Census of Agriculture. This suggests that the clusters identified by our entity resolution algorithm are comparable to the USDA’s definition of a distinct farm (if, perhaps, a little more narrow).

## **E.5 USDA-L2 Record Linkage Results**

After linking USDA recipient profiles to individuals in the L2 voter file, and clustering USDA profiles into farm groups, we allocated each linked L2 ID to a unique farm group. In the very rare occasion in which an L2 voter/consumer profile was linked to multiple distinct farm groups, we allocate the profile to the farm that obtained the higher match probability. Table FSM6 presents record linkage results at the farm level, separately for the full database of 2,792,985 farm groups, for the full set of farms that enrolled in the 2018 MFP, and for the set of farms in our database of crop-level MFP transactions for 2018 (which serve as the basis for our main analysis sample).

The top part of this table presents the share of farms that were linked to any individuals featured in one of L2’s databases (the February 2018 release of L2’s voter files, the June 2019 voter files, the May 2021 voter files, or L2’s 2021 commercial files). Across our entire database of farms, we are able to identify at least one L2 profile associated with 69% of farms. The match rate is higher for farms enrolled in programs in more recent years. For example, 92% farms in the calendar year 2018 MFP database used for our main analyses are linked to at least individual in L2’s databases. While our record linkage algorithm is actually fairly conservative in declaring valid matches, this match rate is quite close to the upper bound on what is feasible. For example, 2% of farms in this CY 2018 MFP database are never associated a profile featuring an identifiable surname (e.g. they refer to the name of a foundation or business). We conclude that our record linkage algorithm successfully identifies the vast majority of feasible true matches between these databases, while keeping

false positives to an absolute minimum.

The second part of Table FSM6 presents the distribution of the number of individuals matches with a farm, given that the farm finds a match in the database. A majority of linked farms are linked to only one individual within the L2 databases. This is consistent with our observation that USDA program recipients usually only list the name of one individual, as well as the fact that most farms in the US are operated by a single family. However, in a nontrivial minority of cases, farms list multiple family members for farm program receipts. Correspondingly, we find that a third of farms enrolled in the 2018 MFP were linked to two or more individuals.

Since all of the statistical inferences in our paper concern registered voters, the third part of this table presents the distribution of the number of matched *voters* conditional on a farm being matches to voters. Since the large majority of farmers are registered voters, these figures are quite similar to those presented for matched individuals.

**Table FSM6: Farm-Level L2 Match Rates Among USDA Recipients**

	2018 MFP (CY 2018) <i>N</i> = 306,941	2018 MFP (All Recipients) <i>N</i> = 502,352	All Programs (2004–2020) <i>N</i> = 2,792,985
<hr/>			
% of Farms Linked to...			
Any L2 Profile	92%	90%	69%
Any L2 Voter Profile	87%	85%	62%
Any L2 Consumer Profile	85%	83%	61%
A Voter in the Feb. 2018 Snapshot	85%	82%	59%
A Voter in the June 2019 Snapshot	84%	82%	58%
A Voter in the May 2021 Snapshot	84%	82%	57%
Among Farms Linked to any L2 Profile...			
% Linked to 1 L2 Profile	66%	67%	82%
% Linked to 2 L2 Profiles	22%	21%	14%
% Linked to 3 L2 Profiles	6%	6%	3%
% Linked to 4 L2 Profiles	3%	3%	1%
% Linked to 5+ L2 Profiles	3%	3%	1%
Among Farms Linked to an L2 Voter Profile...			
% Linked to 1 Voter	70%	71%	84%
% Linked to 2 Voters	20%	20%	13%
% Linked to 3 Voters	5%	5%	2%
% Linked to 4 Voters	2%	2%	1%
% Linked to 5+ Voters	2%	2%	1%

Notes: Matching to an L2 voter profile is not exclusive of matching to an L2 consumer profile, since L2 has already linked many individuals between these two datasets. An “L2 profile” refers to a distinct individual, and has been assigned either an L2 voter ID, an L2 consumer ID, or both. The leftmost column refers to farms associated with customer numbers in the crop-level tabulation of calendar year 2018 MFP transactions used in the main analyses. The middle column refers to farms enrolled in the 2018 MFP, regardless of which year they initially enrolled.

## E.6 USDA-DIME Record Linkage

To study campaign contribution behavior, we link farm groups identified within the USDA recipient data to the DIME database of 1979-2020 itemized campaign contributions. Contributors within DIME are identified uniquely by contributor IDs using a bespoke entity resolution algorithm. We link DIME contributor IDs to distinct farm IDs within our database using two distinct approaches, and then assign each farm the union of these two sets of matched IDs.

First, as with our L2-USDA merge, we design a bespoke probabilistic record linkage algo-

rithm to match USDA recipient profiles to DIME profiles. This closely mirrors the approach of our L2-USDA record linkage. Table FSM7 presents the analogous agreement measures we define to estimate the Fellegi-Sunter record linkage model. Second, we use a crosswalk between recent DIME contributors and L2 voter profiles (Bonica and Grumbach, 2022). This crosswalk matches DIME contributor IDs for contributors active between 2011 and 2020 to at most one distinct L2 voter ID in the 2021 L2 voter file snapshot. Using our previously obtained USDA-L2 matches, we take voters matched to a particular farm group, and then assign the farm those contributor IDs which are associated with the respective L2 voter IDs in Bonica and Grumbach’s (2022) crosswalk. Together, these two approaches match 35,401 of the 122,157 farms (29%) to one or more contributor profiles in DIME.

Table FSM7: Match Agreement Measures for USDA-DIME Record Linkage

Dimension	Level	Description
first	5	Exact match on first name (and names are more than initials)
	4	First name of $i$ is a nickname for first name of $j$
	3	First name of $i$ starts with / ends with first name of $j$ , OR there is a Jaro-Winkler string similarity of at least 0.9
	2	Profile $i$ provides only a first initial, and it matches first initial of $j$
	1	Gender inferred from recipient first name does not conflict with L2 gender field
	0	None of the above criteria are satisfied
middle	3	Exact match on both middle names (and names are more than initials)
	2	Either middle name of profile $i$ is an exact match with either middle name of profile $j$ , OR there is a Jaro-Winkler string similarity between first middle names of at least 0.9
	1	Profile $i$ provides only a middle init, and it matches either middle init of $j$
	0	None of the above criteria are satisfied
last	2	Exact match on last name
	1	Jaro-Winkler string similarity between last names of at least 0.94, or one last name is a shortened version of the other (e.g. “Jones” and “Jones-Smith”)
	0	None of the above criteria are satisfied
suffix	1	Profiles $i$ and $j$ both have suffix field populated, and there’s a match
	0	Profile $i$ has suffix field populated, and it conflicts with $j$
location	5	Street number matches and (ZIP or city matches), OR PO box match and ZIP code matches
	4	ZIP is nonmissing and matches
	3	City matches, OR haversine distance between profiles less than 10 miles
	2	Haversine distance between profiles less than 50 miles
	1	Haversine distance between profiles less than 100 miles
	0	None of the above criteria are satisfied
occupation	4	DIME profile indicates contributor has a “farmer” occupation
	3	DIME profile indicates contributor has a “rancher” occupation
	2	DIME profile indicates contributor is self-employed or retired
	1	DIME profile indicates contributor has misc other occupation
	0	DIME profile indicates occupation distinctly not related to agriculture (e.g. attorney, accountant, banker, librarian, artist, judge, pharmacist)

Note: The Fellegi and Sunter (1969) model employs a missing at random (MAR) assumption, and by construction a missing value in one dimension has no bearing on the inferred match probability for a given comparison.

## F Further Details on Empirical Strategy

### F.1 Analysis of Parametric Model Performance: The Value of Non-parametric Covariate Adjustment

In this section, we motivate our use of Double ML estimation of a partially linear regression (PLR) model by directly measuring the improvement in model fit over OLS. Specifically, we demonstrate that, given our rich array of pre-treatment controls, OLS delivers a relatively lackluster performance in approximating the conditional expectation functions of our outcomes and treatments of interest. As we point out in “Empirical Strategy” in the main text, the focal estimand in both OLS estimation of a linear regression model and DML estimation of a PLR model—the constant marginal effect of the treatment exposure—can be thought of in terms of a binary residual-on-residual regression of the residualized outcome variable on the residualized treatment variable. In other words, both of these approaches attempt to eliminate confounding by subtracting out the conditional expectations of the outcome and treatment (respectively) given a set of pre-treatment controls. We show that, in our setting, a nonparametric estimate of these two conditional expectation functions delivers a substantial improvement over an OLS approximation. This suggests that DML may enable a significantly more credible approach to debiasing the relationship between farmers’ political engagement and their policy outcomes in the 2018 trade war.

We compare the out-of-sample prediction performance of OLS with CatBoost, the supervised learning algorithm we employ in all of our DML PLR estimates. We consider the samples we use to estimate effects on Republican farmer turnout ( $N = 111,910$  voters), non-Republican farmer turnout ( $N = 56,233$  voters), and farm contributions ( $N = 122,157$ ). For each of these analyses, we randomly partition the sample into five equally-sized test folds.<sup>14</sup> For each of the five test folds, we train OLS and CatBoost learners on the four remaining

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<sup>14</sup>To prevent data leakage in our analyses of farmer turnout, we ensure that voters matched to the same farm are always allocated to the same fold when splitting the sample.



“training” folds, and obtain out-of-sample model predictions on the respective test fold. After stacking the five sets of test set predictions  $\hat{y}$  for each estimator, we compute out-of-sample  $R^2$  metrics:

$$R^2 = 1 - \frac{\text{RSS}}{\text{TSS}} = 1 - \frac{\sum_i (y_i - \hat{y}_i)^2}{\sum_i (y_i - \bar{y})^2}. \quad (4)$$

Since this metric relies only on out-of-sample model predictions, it provides an apples-to-apples comparison of model fit across estimators using different numbers of controls. Note that, while this measure is bounded above at 1 like conventional  $R^2$  values, it can achieve negative values if a model is overfit so poorly that its predictions are outperformed by the sample mean.

We calculate  $R^2$  values for out-of-sample predictions of our three main treatment measures of interest (MFP net benefit percentile, MFP compensation rate, and MFP made whole status) as well as five political engagement outcomes (turnout in 2018, net Republican contributing, Republican contributing, Democratic contributing, and Trump contributing). We perform this exercise for an OLS estimator using a shorter array of controls, an OLS estimator using our full array of controls, and a CatBoost algorithm using our full array of controls. These “full” and “light” arrays of controls are the sets of fields used to produce our (main) DML estimates as well as our OLS robustness checks, respectively. See Table FSM9 below for complete lists of each respective set of controls.

Table FSM8 below presents the results of this exercise. By comparing the out-of-sample  $R^2$  values between OLS with a shorter array of controls, OLS with a full array of controls, and CatBoost, we arrive at several general conclusions regarding our covariate adjustment strategy. First, there is substantial information in our set of pre-treatment controls regarding farmers’ latent propensity for political engagement and trade war outcomes. However, it is spread out across many fields, and may be embedded in interactions between fields. This is evident in the fact that adding more pre-treatment covariates often significantly improves model fit, even beyond a linear model that controls for recent political engagement. However, the linear model is prone to catastrophic overfitting. Not only does the extension to the full “kitchen

sink” of controls typically hurt the linear model’s performance in these analyses, it causes the out-of-sample  $R^2$  to turn negative in several instances. In those applications (predicting farms’ “net Republican contributing” status, their propensity to contribute to Republicans, their propensity to contribute to the Trump campaign, and their MFP compensation rate), kitchen sink OLS performs worse out-of-sample than simply predicting the sample mean.

In every treatment and outcome variable we consider, CatBoost provides a better approximation of the true conditional expectation function than either a lighter OLS specification or a kitchen sink OLS specification. Moreover, the gains in model fit are large and substantively meaningful. Using CatBoost instead of a simple linear model to predict Republican farmers’ turnout in 2018 increases out-of-sample  $R^2$  from 0.25 to 0.39 (a 54% increase). Likewise, it increases out-of-sample  $R^2$  from 0.09 to 0.28 (a 223% improvement) for predicting farms’ “net Republican contributing” status. As such, relying on nonparametric adjustment for observed controls dramatically increases the plausibility of our claim that our covariate adjustment strategy eliminates systematic, long-standing differences in farmers’ political engagement propensity across different treatment dispositions.

**Table FSM8: Comparison of OLS and CatBoost Performance in Estimating Conditional Expectations of Outcomes and Treatments given Observed Controls**

Field to Predict	Out-of-Sample $R^2$ OLS (Light Controls) (A)	Out-of-Sample $R^2$ OLS (Full Controls) (B)	Out-of-Sample $R^2$ CatBoost (Full Controls) (C)	CatBoost Improvement Over Light OLS (C – A)/A
<u>Analysis of Republican Farmer Turnout (<math>N = 111,910</math>)</u>				
Turnout in 2018	0.253	0.270	0.388	54%
Net Benefits	0.420	0.393	0.495	18%
Compensation Rate	0.247	0.225	0.333	35%
Made Whole	0.072	0.071	0.125	73%
<u>Analysis of Non-Republican Farmer Turnout (<math>N = 56,233</math>)</u>				
Turnout in 2018	0.363	0.374	0.446	23%
Net Benefits	0.458	0.455	0.508	11%
Compensation Rate	0.278	0.269	0.332	20%
Made Whole	0.070	0.064	0.106	51%
<u>Analysis of Farm Contributions (<math>N = 122,157</math>)</u>				
Net Rep Contrib	0.088	-0.559	0.283	223%
Any Rep Contrib	0.124	-1.136	0.229	84%
Any Dem Contrib	0.066	0.128	0.322	391%
Any Trump Contrib	0.032	-0.091	0.035	10%
Net Benefits	0.132	0.415	0.503	280%
Compensation Rate	0.235	-0.017	0.300	28%
Made Whole	0.061	0.047	0.113	86%

Notes: Table presents (five-fold cross-validated) out-of-sample  $R^2$  values for predictions of outcomes and treatments given the specified sets of controls. Each main analysis sample is partitioned into five disjoint train/test set pairs, with 80% of the sample allocated to the training set in each case. In each instance, an OLS or CatBoost model is fitted on the training set, and then applied to generate predictions on the test set. “Net Benefits” refers to the “Net Benefit (Percentile)” treatment measure (ranging from 0 to 1). “Net Rep Contrib” refers to “Net Republican Contributing,” the main outcome measure used in our analysis of farm-level contributions. “Any Rep Contrib,” “Any Dem Contrib”, and “Any Trump Contrib” refer to binary outcomes measures considered in Further Supplemental Materials H.3.

**Table FSM9: Short and Full Control Sets**

Specification	Control Fields
Short array of controls for turnout analysis	turnout_[cycle] for cycle in {2010, 2012, 2014, 2016}, education_5pt_[level] for level in {1, 2, 3, 4, 5}, white_not_hispanic, race_ethnicity_missing, female, age, log_pop_density, log_historical_acreage_09_12, democrat (only for non-Republican turnout regression), consolidated_cong_district_[d] for $d \in \{1, \dots, 200\}$ (see table notes)
Broad array of controls for turnout analysis (used for “Full Controls” OLS specification and CatBoost)	turnout_[cycle] for cycle in {1992, 1994, ..., 2014, 2016}, primary_turnout_[cycle] for cycle in 1992, 1994, ..., 2014, 2016, democrat (only for PLR of non-Republican turnout), consolidated_cong_district_[d] for $d \in \{1, \dots, 200\}$ (see table notes), education_5pt_[level] for level in {1, 2, 3, 4, 5}, white_not_hispanic, race_ethnicity_missing, protestant, catholic, female, age, log_pop_density, military_or_veteran, christian_family, gun_owner_or_concealed_carry, log_historical_acreage_09_12, county_GOP_pres_margin_[cycle] for cycle in {2000, 2004, 2008, 2012, 2016}, precinct_turnout_G[YY] for YY denoting 2010, 2012, 2014, or 2016, precinct_turnout_G[YY]_[party] for YY denoting 2010, 2012, 2014, or 2016 and party denoting Republicans, Democrats, or independents, net_rep_amount_before_2005, net_rep_amount_[quarter] for each quarter between Q1 2005 and Q4 2017, rep_amount_[quarter] for each quarter between Q1 2016 and Q4 2017, dem_amount_[quarter] for each quarter between Q1 2016 and Q4 2017 (see table notes), rep_contribution_history, dem_contribution_history, CIDs_active_before_2018_count, total_amount_pre_2018, pre_2018_cfscore
Short array of controls for contribution analysis	rep_trx_count_before_2005, rep_trx_count_[quarter] for each quarter between Q1 2005 and Q4 2017, dem_trx_count_before_2005, dem_trx_count_[quarter] for each quarter between Q1 2005 and Q4 2017, consolidated_cong_district_[d] for $d \in \{1, \dots, 200\}$ (see table notes)
Broader array of controls for contribution analysis (“Full Controls” OLS specification)	trump_amount_[quarter] for each quarter between Q1 2016 and Q4 2017 (see table notes), trump_trx_count_[quarter] for each quarter between Q1 2016 and Q4 2017 (see table notes), rep_amount_before_2005, rep_amount_[quarter] for each quarter between Q1 2005 and Q4 2017, rep_trx_count_before_2005, rep_trx_count_[quarter] for each quarter between Q1 2005 and Q4 2017, dem_amount_before_2005, dem_amount_[quarter] for each quarter between Q1 2005 and Q4 2017, dem_trx_count_before_2005, dem_trx_count_[quarter] for each quarter between Q1 2005 and Q4 2017, other_amount_before_2005, other_amount_[quarter] for each quarter between Q1 2005 and Q4 2017, other_trx_count_before_2005, other_trx_count_[quarter] for each quarter between Q1 2005 and Q4 2017,

Continued on next page

Table FSM9 – continued from previous page

Specification	Control Fields
	consolidated_cong_district_[d] for $d \in \{1, \dots, 200\}$ (see table notes), log_historical_acreage_09_12, rep_contribution_history, dem_contribution_history, CIDs_active_before_2018_count, total_amount_pre_2018, pre_2018_cf_score, republican_voter_count, democratic_voter_count, turnout_[cycle]_count for cycle in 2008, 2010, 2012, 2014, 2016, primary_turnout_[cycle]_count for cycle in 2008, 2010, 2012, 2014, 2016, military_or_veteran_count, christian_family_count, gun_owner_or_concealed_carry_count, log_pop_density, county_GOP_pres_margin_[cycle] for cycle in $\{2000, 2004, 2008, 2012, 2016\}$ , precinct_turnout_G[YY] for YY denoting 2010, 2012, 2014, or 2016, precinct_turnout_G[YY]_[party] for YY denoting 2010, 2012, 2014, or 2016 and party denoting Republicans, Democrats, or independents
<b>Additional</b> contribution analysis con- trols provided to CatBoost (be- yond those in previous list)	trump_amount_[quarter] for each quarter between Q3 2014 and Q1 2016 (see table notes), trump_trx_count_[quarter] for each quarter between Q3 2014 and Q1 2016 (see table notes), net_rep_amount_before_2005, net_rep_amount_[quarter] for each quarter between Q1 2005 and Q4 2017, net_rep_trx_count_before_2005, net_rep_trx_count_[quarter] for each quarter between Q1 2005 and Q4 2017, voter_[i]_age for $i \in \{1, 2, 3\}$ , voter_[i]_female for $i \in \{1, 2, 3\}$ , voter_[i]_ethnicity for $i \in \{1, 2, 3\}$ , voter_[i]_education, voter_[i]_republican, voter_[i]_democrat, voter_[i]_turnout_ge_[cycle], voter_[i]_turnout_pe_[cycle]

Notes: See Online Appendix B for descriptions of these fields. Congressional district fixed effects are implemented by including indicators for the 200 most common districts within the sample dataset. These top 200 districts include over 97.5% of sample farms and sample voters. This restriction ensures that OLS predictions can be made across cross-validation folds; it also reduces training time for CatBoost. In several instances, we preclude controls in the full list provided to CatBoost from use in OLS training. This prevents OLS from failing or severely degrading due to perfect multicollinearity, near-perfect multicollinearity, or missing values. CatBoost is robust against each of these issues, and requires no such careful curation. We omit quarterly 2016-2017 Democratic contributions from the OLS control set in the turnout analysis. In the contribution analysis, we do not provide OLS with: quarterly net Republican contribution amounts, quarterly net Republican transaction counts, quarterly contribution counts / amounts to Trump between Q1 2014 and Q1 2017, and individual-level demographics of the three oldest voters associated with the specified farms (which often feature missing values). Given that the “full controls” OLS already seems to typically be overfit in the contribution analyses, we consider it implausible that the dominance of CatBoost over OLS is a product of omitting these controls.

## F.2 Implementation of Double Machine Learning Estimation

As a general estimation concept, double machine learning (DML) is a combination of two ingredients: a Neyman orthogonal score function, and sample splitting. Chernozhukov et al. (2018) note that the score function used by the residual-on-residual regression presented in the main text (Equation 3) satisfies Neyman orthogonality, which informally means that

the moment conditions used to identify  $\theta$  are locally insensitive to the values of nuisance parameters  $q(X_i) = \mathbb{E}[Y_i | X_i]$  and  $m(X_i) = \mathbb{E}[B_i | X_i]$ .<sup>15</sup> Thus, we can plug in somewhat noisy estimates of  $q$  and  $m$  without strongly violating the regression moment conditions. In particular, using an orthogonal score function mitigates the effect of bias introduced from using a machine learning estimator with regularization. We direct curious readers to Chernozhukov et al. (2018) for a formal explication of Neyman orthogonality and its contribution to DML estimation.

The second ingredient—sample splitting—is somewhat easier to conceive of in an informal manner. Chernozhukov et al. (2018) show that estimating the nuisance parameters  $q$  and  $m$  on the same sample used to estimate  $\theta$  can lead to severe bias from overfitting. On the other hand, if we first split the sample into “training” and “test” subsets, and then estimate  $q$  and  $m$  on the training set and  $\theta$  on the test set, the resulting estimate would be cleansed of bias from overfitting. However, this would come at a substantial cost to efficiency, since we would only be using part of the sample for each estimation stage. An intuitively appealing solution—termed “cross-fitting”—is to split the sample as just described, and then flip the labels on the training and test sets and estimate  $q$  and  $m$  on the test set followed by  $\theta$  on the training set. If we average the estimates  $\hat{\theta}_1$  and  $\hat{\theta}_2$ , the resulting estimator restores efficiency while retaining the benefits of sample splitting. In practice, Chernozhukov et al. (2018) recommend a  $K$ -fold cross-fitting approach, in which one splits the sample into  $K$  disjoint subsets. For each fold  $k \in \{1, \dots, K\}$ , the nuisance parameters  $q$  and  $m$  are estimated using the remaining  $K - 1$  folds before  $\theta$  is estimated using the  $k$ th fold.

All DML estimation in this paper is conducted using software maintained by Battocchi et al. (2019). We estimate the outcome and treatment nuisance functions using the gradient boosted regression tree algorithm CatBoost. Chernozhukov et al. (2018) note that boosting methods belong to the broad class of machine learning algorithms to which their results apply, and we discuss the attractive properties of this particular method in Subsection F.3 of these

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<sup>15</sup>See Section 4.1 of Chernozhukov et al. (2018) for discussion of the particular score function underpinning Equation 3.

supplementary materials. We follow the recommendation of Chernozhukov et al. (2018) and use five-fold cross-fitting. To prevent data leakage in the first-stage treatment model fitting, we ensure that voters matched to the same farm are always allocated to the same fold when splitting the sample. We conduct inference for  $\theta$  using the asymptotic normal distribution of the estimator.

As noted in the main text, we investigate treatment effect heterogeneity by allowing the causal effect parameter  $\theta$  to be a function of some low dimensional set of covariates  $V_i$ . As formally stated by Battocchi et al. (2019), we can slightly extend the partially linear model of Equations 1 and 2 as follows:

$$Y_i = \theta(V_i) \cdot B_i + g(V_i, X_i) + \varepsilon_i, \quad \mathbb{E}[\varepsilon_i \mid X_i, B_i] = 0, \quad (5)$$

$$B_i = m(V_i, X_i) + \eta_i, \quad \mathbb{E}[\eta_i \mid V_i, X_i] = 0, \quad (6)$$

$$\mathbb{E}[\eta_i \cdot \varepsilon_i \mid V_i, X_i] = 0, \quad (7)$$

where  $V_i$  is a set of covariates through which effect heterogeneity may operate, and  $X_i$  denotes the remaining controls. In the main text, we only consider a very simple form of heterogeneity, an interaction of the treatment with an indicator variable (e.g.  $\theta(V_i) = \alpha + \beta \cdot \text{turnout\_2014}_i$ ). However, in Further Supplemental Materials H.2, we obtain a highly flexible estimate of effect heterogeneity with respect to enrollment date by using a causal forest for the final stage, residual-on-residual regression (as first explicated in Section 6.1.1 of Athey, Tibshirani, and Wager (2019)).

### F.3 Using CatBoost to Estimate Nuisance Parameters

We estimate the partially linear regression model’s nuisance parameters  $q(X_i) = \mathbb{E}[Y_i \mid X_i]$  and  $m(X_i) = \mathbb{E}[B_i \mid X_i]$  using CatBoost, a supervised learning algorithm based on gradient boosted regression trees. Like other tree-based gradient boosting algorithms, CatBoost iteratively trains individual decision trees, aggregating many weak learners in a sequential process to create an ensemble that continually improves on a specified objective (usually classification

or regression). In general, flexible supervised learning algorithms such as CatBoost are an attractive choice in our setting, as we would like to obtain highly flexible estimates of farmers’ turnout, contribution, and treatment propensities, and we have a moderately high number of covariates for which we’d like to permit flexible interactions in a data-driven manner.

Despite recent well-publicized advances in using deep learning for natural language and machine vision applications, tree-based methods still mark the state-of-the-art for supervised learning with tabular data (Grinsztajn, Oyallon, and Varoquaux, 2022). CatBoost—a portmanteau of “categorical boosting”—is attractive for our application because of its advances in handling categorical features (such as congressional district and ethnicity) (Prokhorenkova et al., 2018). Perhaps most importantly, its default hyperparameter choices perform well without additional tuning, a highly practical attribute given our need to train additional learners for each individual regression model we estimate.

In our turnout analyses, we specify the estimation of the conditional expectation of a voter’s 2018 turnout  $q(X_i)$  as a CatBoost classification problem, with a cross-entropy loss objective. In our analyses using the binary treatment (the “made whole” effect estimates and the large-scale comparisons of farmers and non-farmers), we do the same for  $\hat{m}(X_i)$ . For the two continuous treatments (net benefit percentile and compensation rate), we obtain  $\hat{m}(X_i)$  by minimizing mean squared error in a CatBoost regression framework.

In recognition of the somewhat “black-box” nature of gradient boosting algorithms, we examine a measure of feature importance to check the sensibility of our nuisance parameter estimates. For the sake of brevity, we focus here on our farm sample turnout analyses (i.e., those presented in “The Effect of Improved Policy Outcomes on Voter Turnout” section). In Figures FSM23 through FSM30, we plot CatBoost’s default measure, which reflects how much on average the prediction changes as the feature value changes. Note that the measure is normalized to sum to 100 across all features. Together, Figures FSM23 through FSM30 account for all nuisance function estimates that underpin our main turnout results as presented in Figure 6. Figures FSM23 and FSM24 depict the relative importance of covariates for



predicting turnout among sample Republicans and non-Republicans, respectively. Likewise, Figures FSM25, FSM26, FSM27, FSM28, FSM29, and FSM30 rank the covariates that are most predictive of sample members' treatment dispositions.

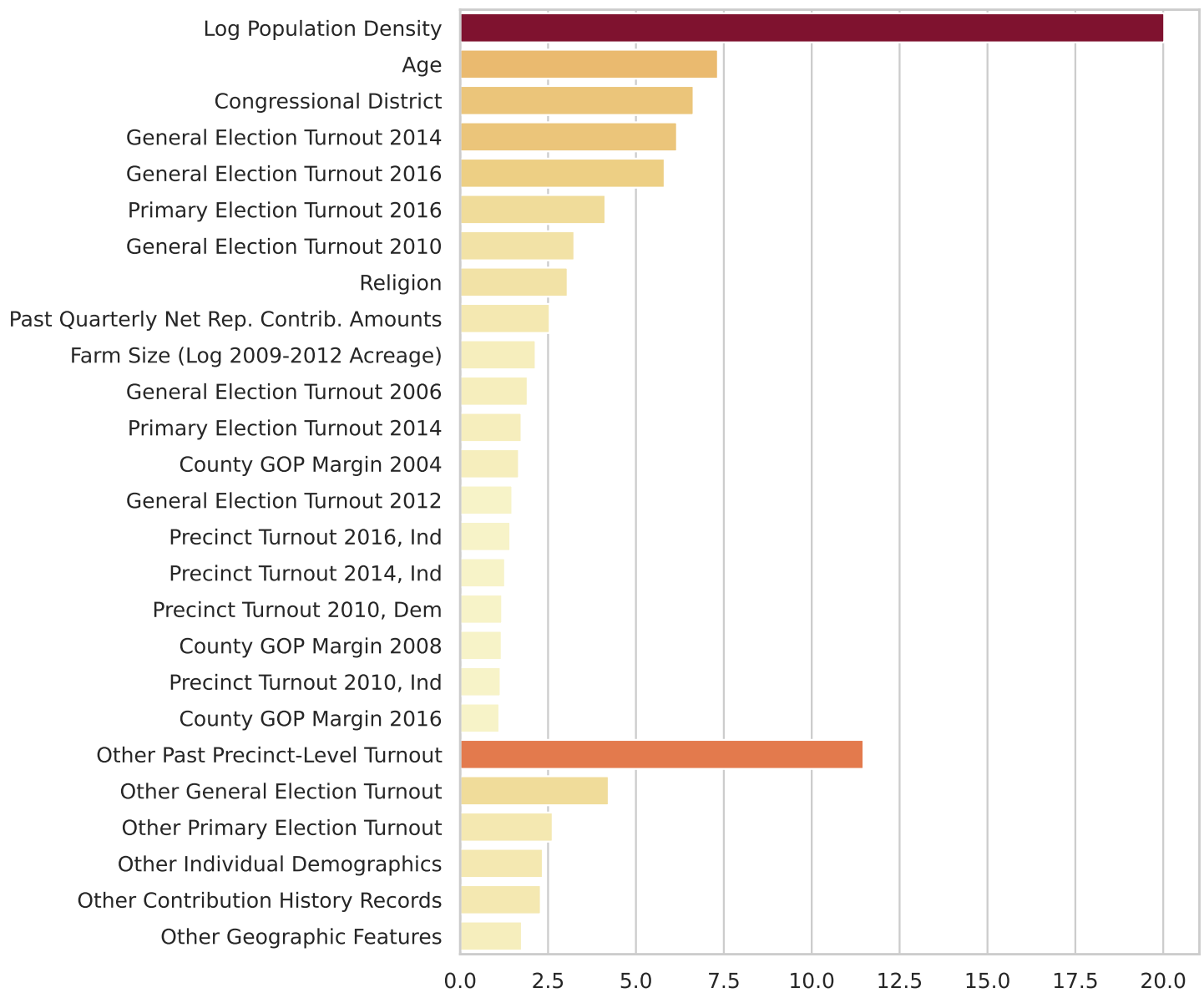
Generally, the fields that one might expect to be highly predictive of turnout and treatment, respectively, score quite well according to these measures. Voters' turnout records in recent elections, as well as their age, are very important predictors of turnout in 2018. This holds for Republicans and non-Republicans alike. As expected, the most important predictors of treatment disposition are voters' locations (congressional district) and historical farm size. This reflects the fact that variation in crop portfolios partially stems from regional variation in the suitability of particular crops, as well as the fact that—holding crop composition constant—MFP benefits are proportional to farm size. On this latter count, it is worth noting that farm size is the single most important predictor of net benefits (Figures FSM25 and FSM26) but not compensation rate (Figures FSM27 and FSM28). This corroborates our assertion in Further Supplemental Materials Section D that the MFP compensation rate measure isolates variation from idiosyncratic crop-specific errors in program design, while variation in our net benefit measure is driven by a combination of such errors as well as overall farm size.

We were surprised by at least one result from Figures FSM23–FSM30. As shown in Figures FSM23 and FSM24, we find that census block population density is the single most important predictor of 2018 turnout for both Republican and non-Republican members of our sample—at least, according to the default CatBoost measure of feature importance. We can imagine (at least) a couple reasons for this. First of all, this is our only explicit covariate capturing the urban/rural divide in political geography, and it is extremely granular, perhaps allowing CatBoost to identify a relatively large number of profitable divisions in the data in conjunction with other key covariates. In contrast, turnout in the 2016 general election (which might be expected *ex ante* to rank first on this list) is a binary field, and is highly correlated with the 25 other individual-level turnout records we control for. Second, population density might in-and-of-itself be an important determinant of turnout. While we are unaware of

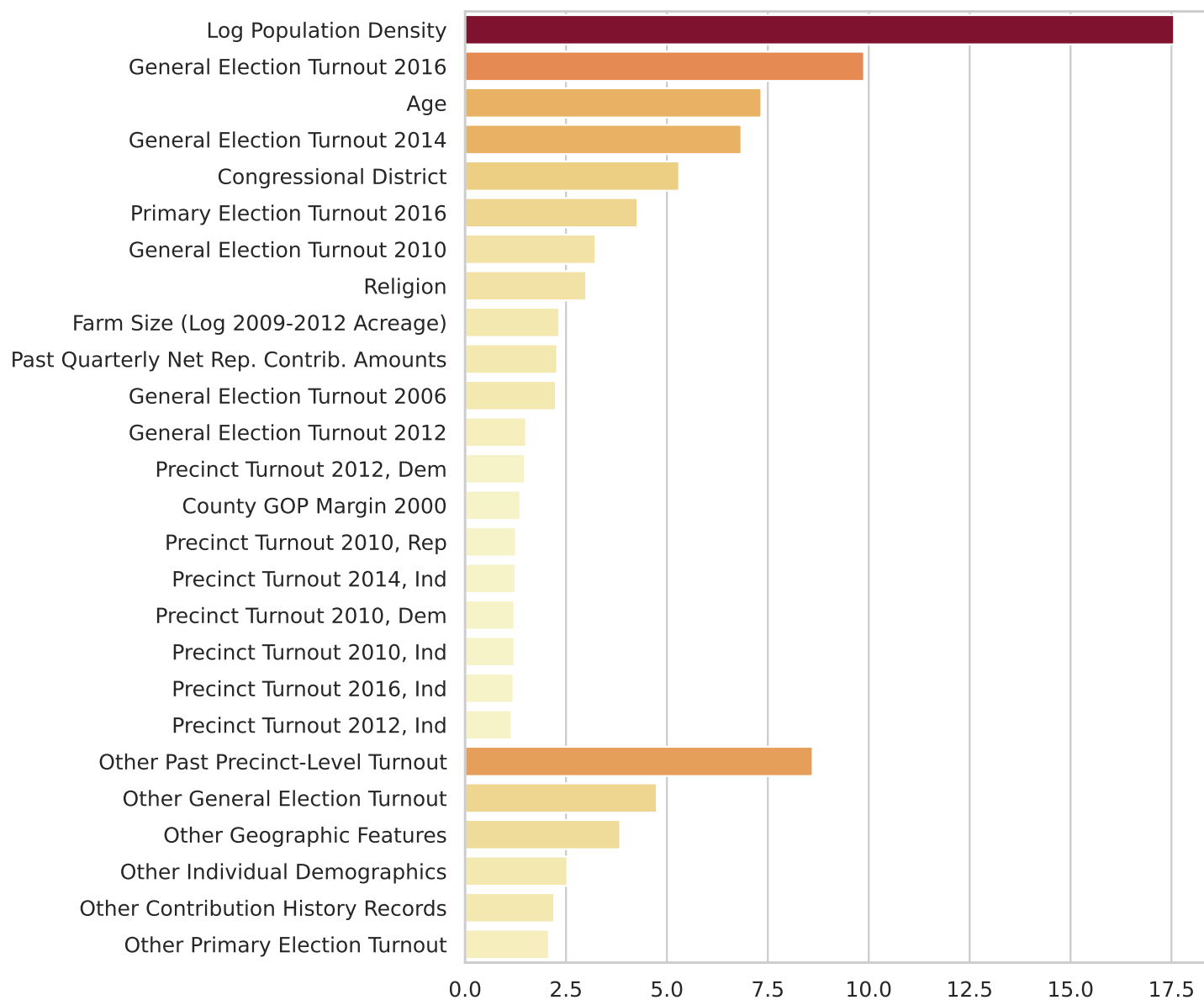
any recent studies whose objective is to estimate urban-rural differences in US voter turnout, Gimpel et al. (2020) argue that such differences are highly salient for other forms of political behavior. In particular, they find that population density can account for 10-16 percentage points of differences in partisan affiliation after controlling for race, religion, age, education level, income, sex, marital status, and reported level of religious commitment.

On a final note, a less striking—but perhaps still surprising—result from Figures FSM23 through FSM30 is just how many different fields are predictive of turnout and policy outcomes. If the true signal really is spread out across this many covariates, these feature importance results may be an implicit endorsement of using a data-driven covariate adjustment strategy in our setting (as we have), as opposed to a more bespoke approach of trying to manually identify four or five features that *ex ante* appear to be important controls.

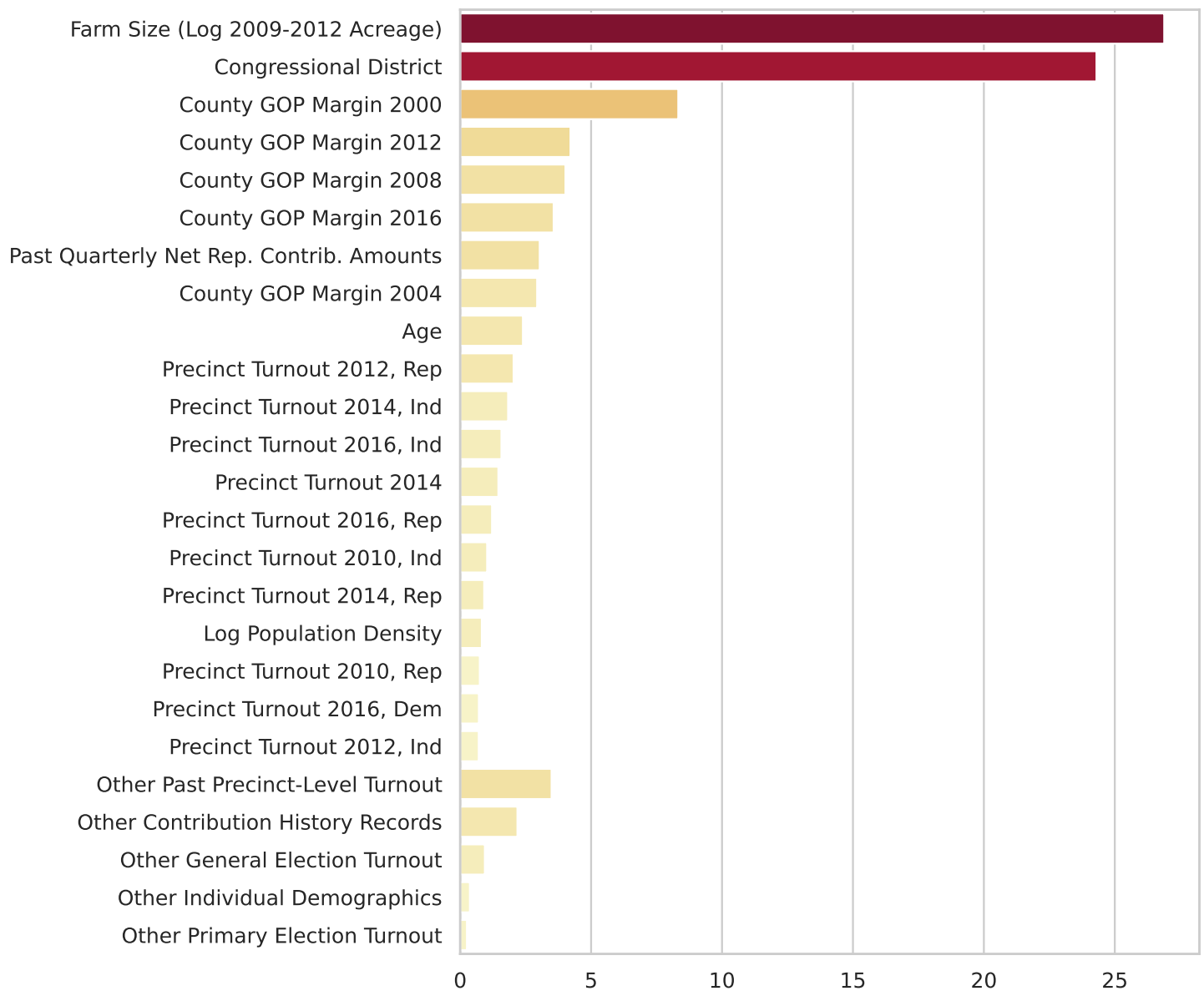
**Figure FSM23:** Feature Importance for Voter-Level Analyses: Predicting 2018 Turnout Among Republicans



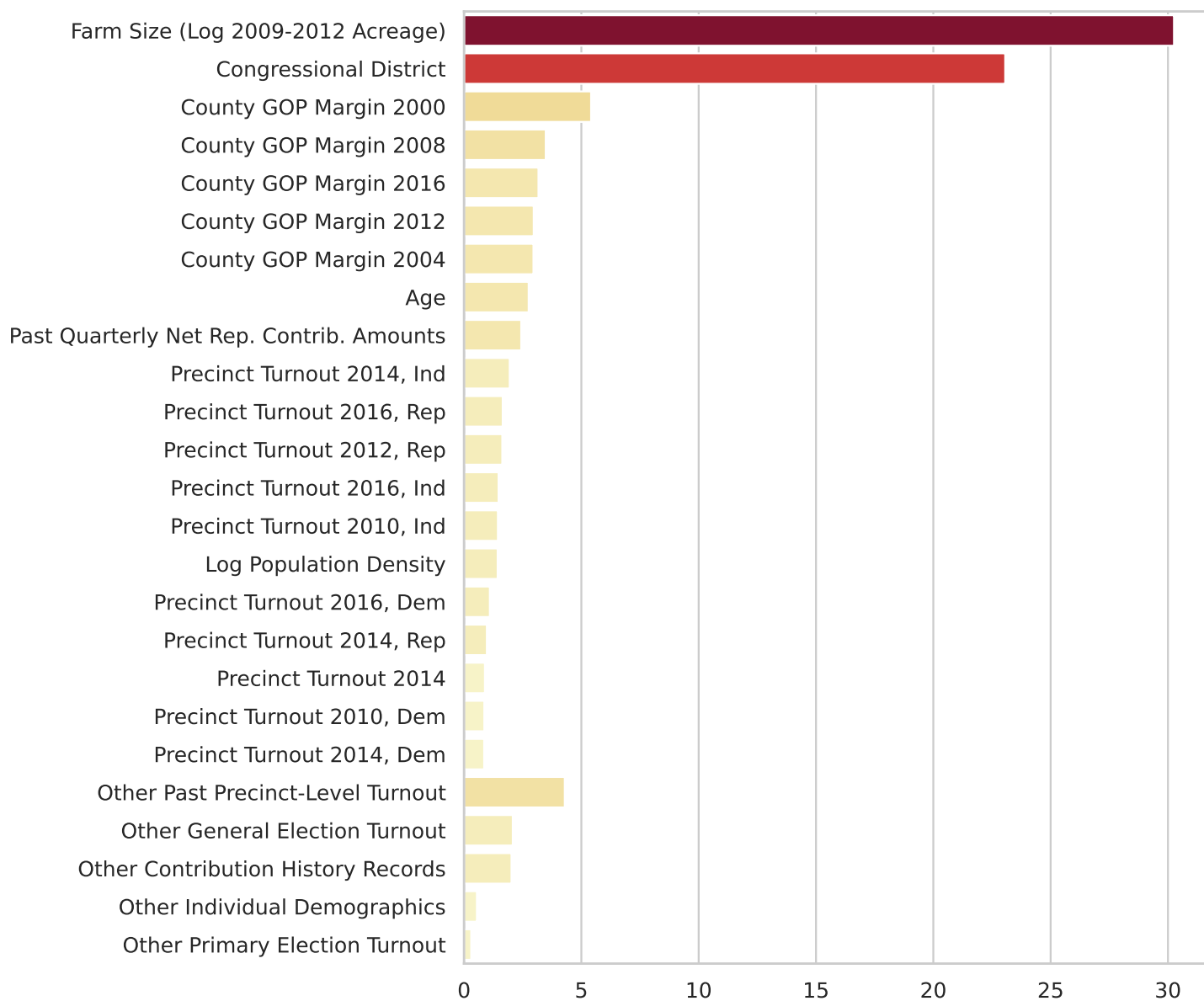
**Figure FSM24:** Feature Importance for Voter-Level Analyses: Predicting 2018 Turnout Among Non-Republicans



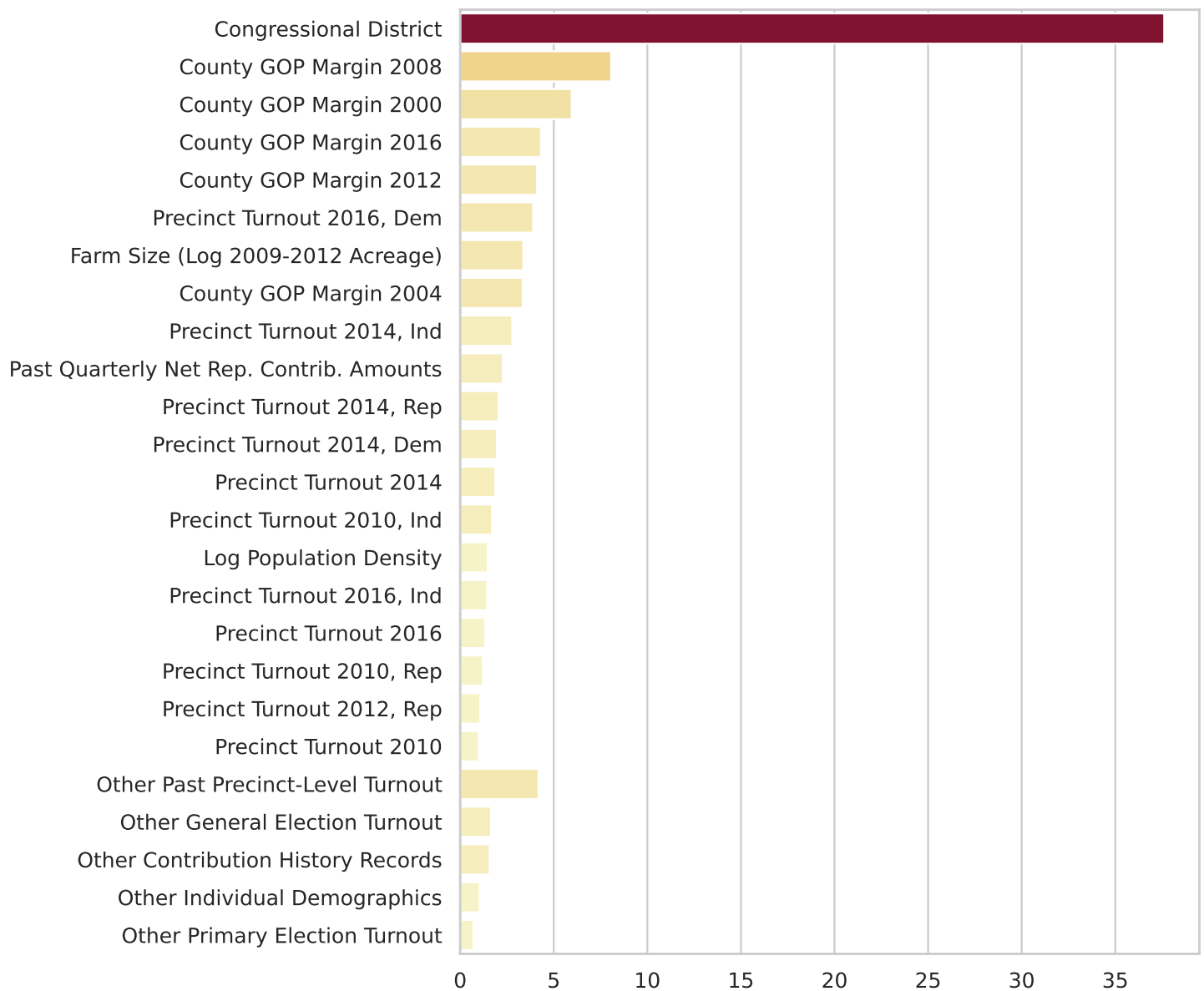
**Figure FSM25:** Feature Importance for Voter-Level Analyses: Predicting Net MFP Benefit Percentile Among Republicans



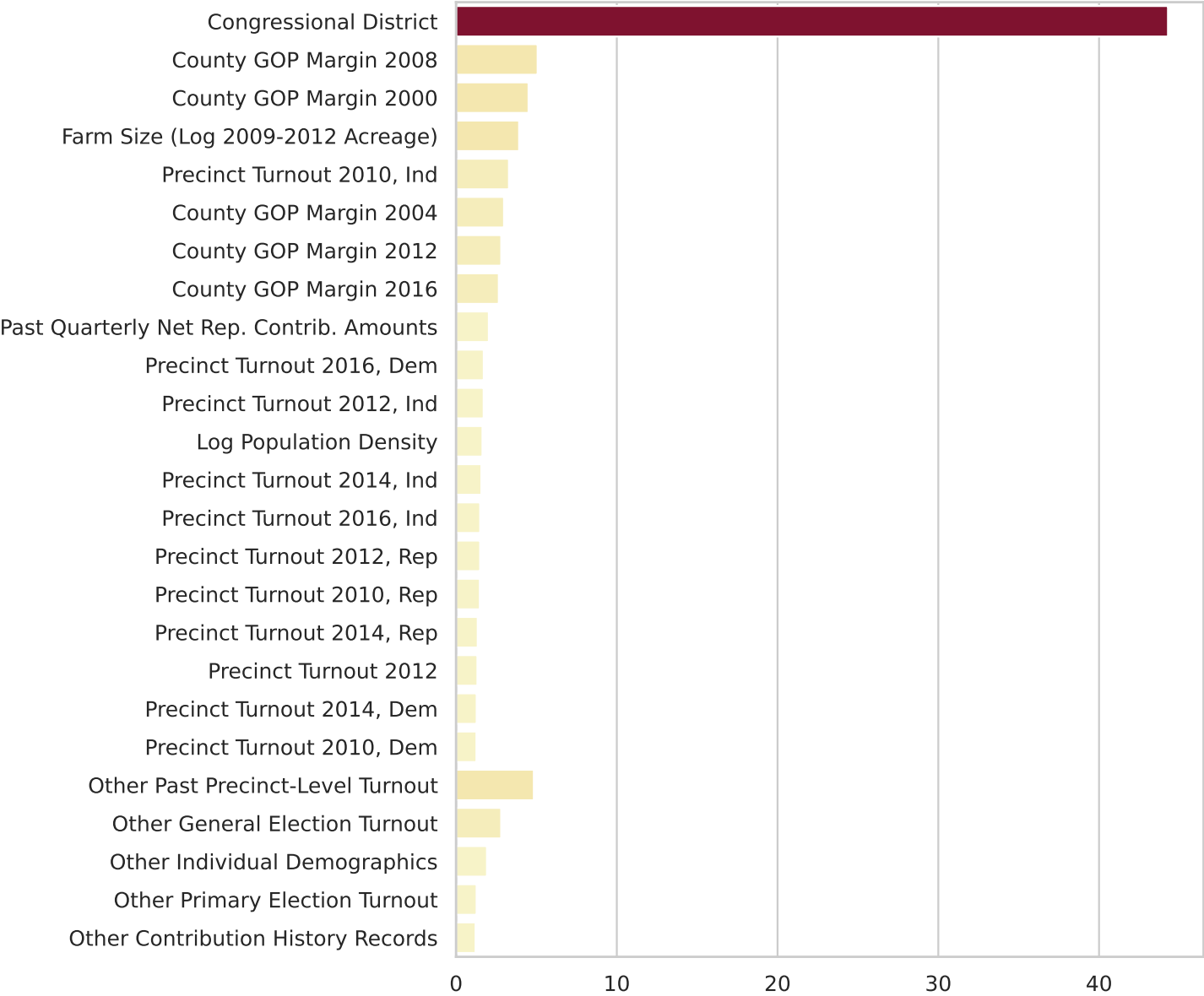
**Figure FSM26:** Feature Importance for Voter-Level Analyses: Predicting Net MFP Benefit Percentile Among Non-Republicans



**Figure FSM27:** Feature Importance for Voter-Level Analyses: Predicting Compensation Rate Among Republicans

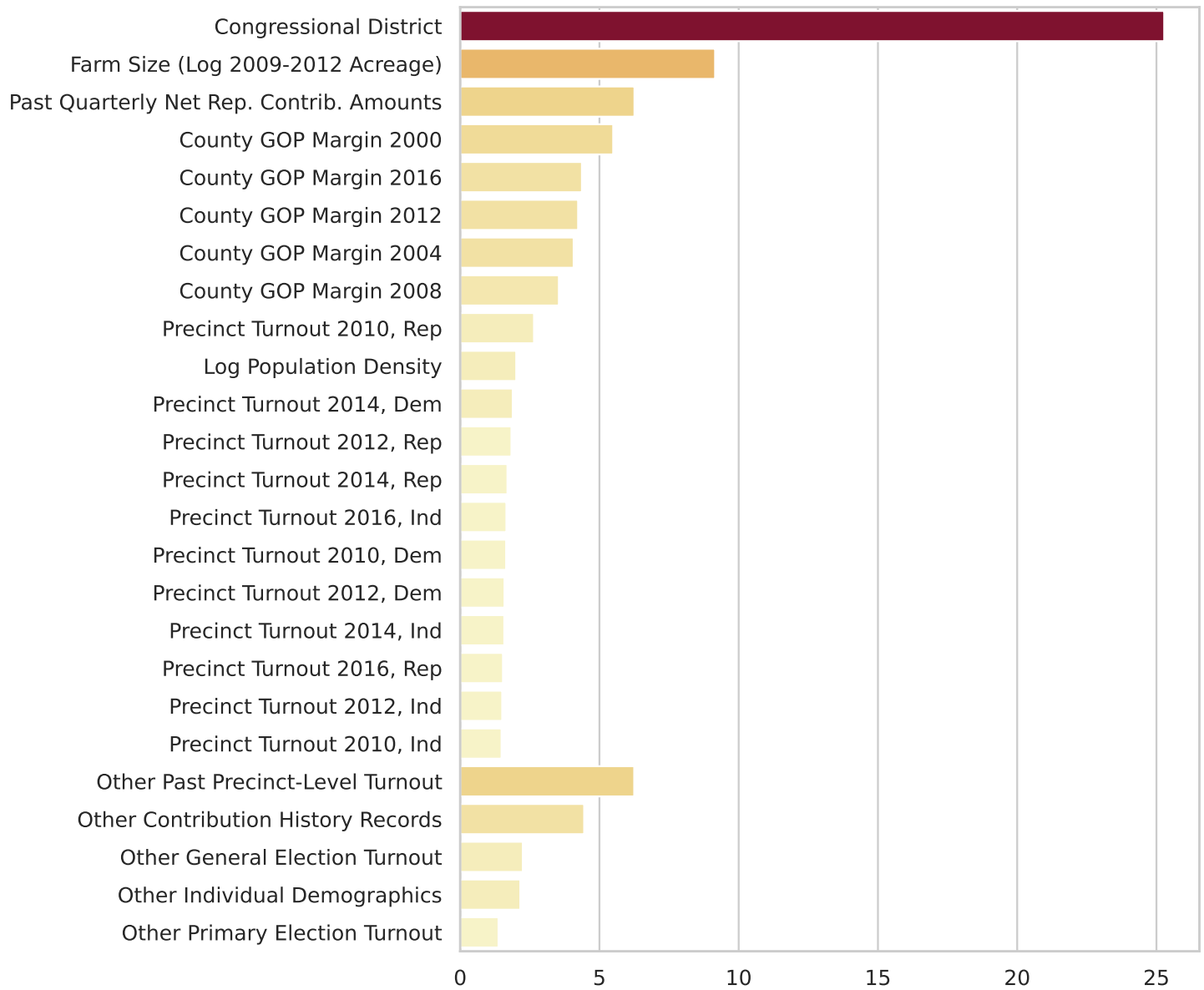


**Figure FSM28:** Feature Importance for Voter-Level Analyses: Predicting Compensation Rate Among Non-Republicans

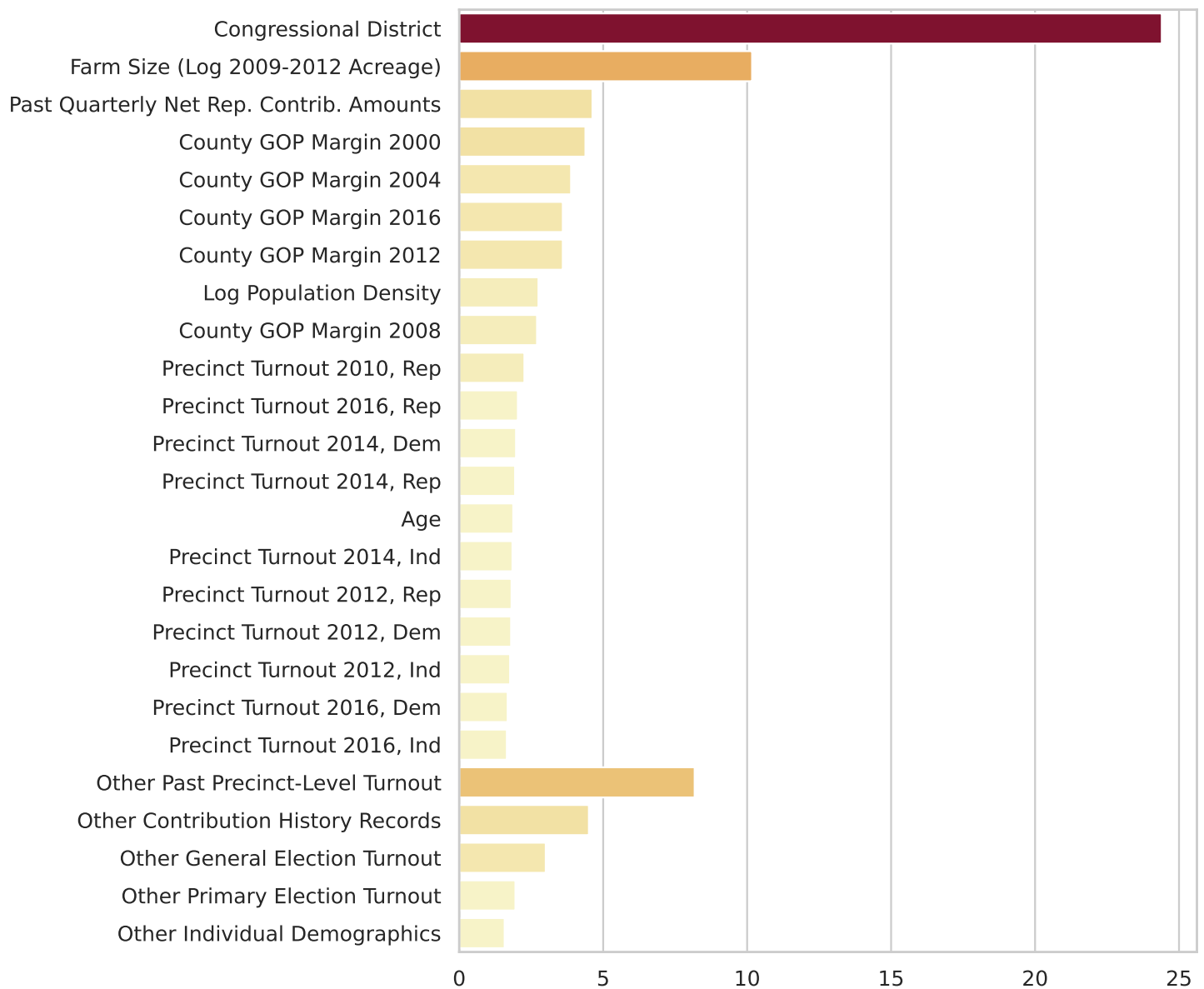




**Figure FSM29:** Feature Importance for Voter-Level Analyses: Predicting Made Whole Status Among Republicans



**Figure FSM30:** Feature Importance for Voter-Level Analyses: Predicting Made Whole Status Among Non-Republicans



## G Further Details on Broader Effects Analyses

In the main text (“Broader Effects of the Trade War and MFP on Farmers’ Political Engagement”), we complement our main analyses of farmers’ political engagement by assessing differences in turnout and contribution trends between farmers affected by the trade war and the rest of the electorate. Agriculture was the industry most exposed to retaliatory tariffs in 2018 (Parilla and Bouchet, 2018), and was the only industry that received a significant compensation package. As such, farmers producing affected commodities experienced a sharp increase in the salience of economic policy in 2018 that was well beyond what the average member of the electorate experienced. As such, we interpret differential changes in turnout and contributions among these two groups as suggestive evidence regarding the effect of increased policy salience on political engagement. The main results of these analyses are presented in Figures 11 and 12.

In this section, we briefly discuss how we categorize registered voters as directly affected by the trade war and MFP, our binary “treated” disposition for the purposes of this analysis. In 2017, US agricultural exports to China totaled \$19.5 billion, of which \$15.1 billion (78%) came from soybeans, cotton, corn, coarse grains (e.g. barley, sorghum, oats), wheat, and dairy products.<sup>16</sup> These commodities garnered over 97% of MFP disbursements in CY 2018. The crops in this list have also incurred the overwhelming majority of commodity support payments authorized through traditional farm bill USDA programs, while dairy producers have been the greatest recipients of commodity-specific support payments in the farm bill aside from row crops. As such, one way to identify farms with a very high likelihood of being on the front lines of the 2018 trade war—and a high likelihood of enrolling in the MFP—is by examining receipt of traditional flagship farm safety net programs for row crops, as well as receipt of safety net programs for milk producers.

We flag a farm as having been directly affected by the trade war if it received payments from one of these programs in the five years preceding the trade war (2013-2017); we enumerate and

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<sup>16</sup>See <https://www.fas.usda.gov/china-2020-export-highlights> for details.

**Table FSM10: Programs Indicating Predisposition for Trade War Exposure**

Program(s)	Disbursements	Farms	Description
DCP/ACRE	\$4,911,382,207	759,961	The Direct and Countercyclical Programs (DCP) and the Average Crop Revenue Election (ACRE) option were the USDA's flagship safety net programs for row crop farmers under the 2008 farm bill (in effect for 2009-2013). Farmers enrolled in either DCP or ACRE, and in either case received an unconditional, fixed "direct" payment each year, in addition to a conditional "countercyclical" payment if market conditions deteriorated. Eligible commodities included wheat, corn, sorghum, barley, oats, upland cotton, rice, soybean, peanuts, and various minor oilseeds.
ARC/PLC	\$20,191,674,953	731,239	The 2014 farm bill replaced DCP/ACRE with a combination of the Agriculture Risk Coverage (ARC) and Price Loss Coverage (PLC) programs. Row crop farmers enrolled in either ARC or PLC (but not both) for the duration of the 2014 farm bill (2015-2018). All DCP/ACRE-eligible commodities save cotton were eligible for ARC/PLC.
CTAP	\$489,271,165	81,531	In lieu of receiving ARC/PLC payments under the 2014 farm bill, cotton farmers were allowed to enroll in a special subsidized insurance product ("STAX"). However, the rollout was delayed, and so Congress authorized the Cotton Transition Assistance Program (CTAP) to make interim payments to cotton producers in 2014 and 2015.
MILC	\$489,271,165	81,531	The Milk Income Loss Contract Program (MILC) compensated dairy producers when domestic milk prices fell below a specified level.
CGCS	\$328,046,136	28,180	The Cotton Ginning Cost Share (CGCS) program provided a one-time cost share assistance payment to cotton producers with a share in the 2015 cotton crop.
MPP for Dairy	\$11,147,716	4,701	The Margin Protection Program for Dairy replaced MILC for the duration of the 2014 farm bill. Like its predecessor, it provided payments to dairy producers when milk prices relative to input costs fell below a specified threshold.
Dairy Indemnity Program	\$2,905,284	236	The Dairy Indemnity Payment Program made compensated milk producers when raw milk was recalled from the commercial market due to contamination concerns.

Note: Disbursement totals and farm counts reflect calendar year 2013-2017 receipts.

describe the particular programs in Table FSM10. We choose 2013-2017 as our reference period as it precedes the 2018 trade war and contains the last year that the 2008 farm bill was in effect (2013). This is important because the flagship program supporting row crop farmers under the 2008 farm bill (DCP/ACRE) included a universal fixed annual payment, and roughly 90% of these payments were directed to the five row crops that were the central focus of the MFP and trade war (corn, soybeans, wheat, cotton, sorghum). As such, DCP/ACRE recipients in 2013 included nearly the universe of row crop farms bearing the brunt of the trade war (and eligible for the MFP). The programs succeeding DCP/ACRE under the 2014 farm bill (ARC/PLC) also primarily benefited growers of these crops, but did not issue unconditional payments each year to all enrolled farms. Instead, ARC/PLC only issued payments when market conditions deteriorated for particular crops, and as such our payment databases do not reveal the full set of enrolled farms for these programs. As we note in Table FSM10, we also consider programs that specifically supported dairy and cotton farmers nationwide through this period.

For this analysis, we flagged a farm as directly exposed to the trade war / MFP if it received a payment from any of these programs between 2013 and 2017. 61% of the voters linked to such farms ultimately enrolled in the 2018 MFP, and this group of voters captures 94% of the voters linked to farms participating in the 2018 MFP. We conclude that this set of programs represents a strong indicator of predisposition to direct trade war exposure. In our analysis, we interpret the difference in turnout between voters linked to such farms and turnout among the rest of the electorate (after adjusting for covariates) as the intent-to-treat effect of increased policy salience.

We note that using participation in these 2013-2017 programs as the treatment indicator comes with a couple disadvantages relative to directly using participation in the 2018 MFP. First, we are constrained to estimating an intent-to-treat effect instead of a direct effect. Second, several commodities covered by the MFP (pork, tree nuts, sweet cherries) were not covered explicitly by any 2013-2017 farm programs, and thus cannot be counted by our treatment indicators. However, we view these concerns as minor relative to the two key advantages

of our treatment indicator: (1) it better captures the part of the electorate directly affected by 2018 agricultural policy shock, and (2) it mitigates selection bias. On the latter count, we note that enrollment in the 2018 MFP implies that an individual was alive and active in late 2018 or early 2019. This might be particularly noteworthy in our Figure 11 analysis, as our available turnout controls only run through 2016, and in the interim two years voters may have become inactive or deceased. If latent activity levels drive both propensity to enroll in a novel program (the 2018 MFP) as well as turnout, then it seems plausible that directly comparing MFP enrollees to the rest of the electorate may bias the effect upwards (even with our rich set of controls).

On a closing note, we provide a complete list of controls employed in our large-scale contribution analyses (Figures 12, FSM50, FSM51, FSM52, FSM53) in Table FSM11.

**Table FSM11: Controls used in Large-Scale Analysis of Overall Impact of Increased Policy Salience on Contributions**

Field(s)	Description
<code>rep.contribution.history</code>	Indicates voter had a distinctly Republican pre-2018 contribution history. Takes a value of 1 if three conditions satisfied: (i) voter is associated with some contribution to a Republican candidate or PAC prior to 2018, (ii) voter is associated with a greater dollar amount of pre-2018 contributions to Republicans than Democrats, and (iii) if voter is assigned a pre-2018 CFscore, this CFscore is strictly greater than zero.
<code>dem.contribution.history</code>	Indicates voter had a distinctly Democratic pre-2018 contribution history. Takes a value of 1 if three conditions satisfied: (i) voter is associated with some contribution to a Democratic candidate or PAC prior to 2018, (ii) voter is associated with a greater dollar amount of pre-2018 contributions to Democrats than Republicans, and (iii) if voter is assigned a pre-2018 CFscore, this CFscore is strictly less than zero.
<code>CIDs.active.before.2018.count</code>	Number of distinct contributor IDs in Bonica’s DIME 4.0 database that made itemized contributions prior to 2018 and were linked to the voter.
<code>pre.2018.cfscore</code>	Pre-2018 analogue of Bonica’s (2014) common-space campaign finance score (“CFscore”) measure of donor ideology. For each voter, we take the average of pre-2018 contribution recipients’ CFscores, weighted by the dollar amounts of the respective donations. For voters not linked to pre-2018 contributions associated with recipient CFscores, we impute a value of 0.
<code>total.amount.pre.2018</code>	Total dollar amount of 1979-2017 itemized contributions made by DIME contributor profiles linked to the voter.
<code>total.rep.amount.pre.2018</code>	Total dollar amount of 1979-2017 itemized contributions to Republicans made by DIME contributor profiles linked to the voter.

Continued on next page

Table FSM11 – continued from previous page

Field(s)	Description
total_dem_amount_pre_2018	Total dollar amount of 1979-2017 itemized contributions to Democrats made by DIME contributor profiles linked to the voter.
total_other_amount_pre_2018	Total dollar amount of 1979-2017 itemized contributions to candidates and PACs not affiliated with Republican or Democratic parties.
total_trump_amount_pre_2018	Total dollar amount of 1979-2017 itemized contributions to Trump and closely affiliated PACs.
total_net_rep_amount_pre_2018	Total dollar amount of 1979-2017 itemized contributions to Republicans made by DIME contributor profiles linked to the voter, minus total amount contributed to Democrats.
net_rep_amount_before_2005	Total dollar amount of voter's 1979-2004 political contributions to Republican candidates and PACs, minus total dollar amount to Democratic candidates and PACs.
net_rep_amount_[quarter] for quarter in 2005-Q1, 2005-Q2, ..., 2017-Q3, 2017-Q4	Total dollar amount of voter's political contributions to Republican candidates and PACs in the specified quarter, minus total dollar amount to Democratic candidates and PACs.
net_rep_trx_count_before_2005	Number of itemized contributions made to Republican candidates and PACs 1979-2004, minus number made to Democratic candidates and PACs.
net_rep_trx_count_[quarter] for quarter in 2005-Q1, 2005-Q2, ..., 2017-Q3, 2017-Q4	Number of itemized contributions made to Republican candidates and PACs in the specified quarter, minus number made to Democratic candidates and PACs.
[party]_amount_before_2005 for each party in "Republican", "Democrat", "Other"	Total dollar amount of voter's 1979-2004 political contributions to party candidates and PACs.
[party]_amount_[quarter] for quarter in 2005-Q1, 2005-Q2, ..., 2017-Q3, 2017-Q4 and for each party in "Republican", "Democrat"	Total dollar amount of voter's political contributions to party candidates and PACs in the specified quarter.
trump_amount_[quarter] for quarter in 2014-Q3, 2014-Q4, ..., 2017-Q3, 2017-Q4	Total dollar amount of voter's political contributions made to Trump campaign and affiliated PACs in the specified quarter.
congressional_district_[d]	Indicators for each of the 435 House districts (L2).
republican	Indicator for Republican Party affiliation (L2 voter file)
democrat	Indicator for Democratic Party affiliation (L2 voter file)
independent	Indicator for voter identifying as independent (L2 voter file)
third_party	Indicator for third party affiliation (L2 voter file)
education	Five-point scale indicating highest level of education obtained: "less than HS diploma", "HS diploma", "some college or vocational/technical degree", "bachelor's degree", "graduate degree" (L2). In the instance in which the L2 voter and consumer profiles disagree, we take the greater of the two.
race_ethnicity	Categorical variable taking a value of "White, Not Hispanic", "Black or African American", "Hispanic", "Asian", "American Indian or Alaskan Native", "Native Hawaiian or Other Pacific Islander", "Other / Multi-Ethnic", or "Missing". In the instance in which the L2 voter and consumer profiles disagree, we use the value from the voter file.
religion	Categorical variable taking a value of "Protestant", "Catholic", "Jewish", "Buddhist", "Mormon", "Muslim", "Lutheran", "Other" or "Missing".

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**Table FSM11 – continued from previous page**

Field(s)	Description
ISPSA	Experian’s “Index of Social Position for Small Areas”; denotes neighborhood’s relative socioeconomic status based on a combination of census occupation and education measures.
mosaic_code_zip4	Experian Mosaic customer segmentation measure, applied to the voter’s ZIP+4 code. Bins consumers (and their neighborhoods) into groups such as “Settled in Suburbia” and “Work Hard Pray Hard”.
female	Indicator for voter’s gender.
age	Integer reflecting voter’s age on Election Day 2018.
pop.density	Estimated number of adults per square mile calculated for all adults living within the voter’s census block.
military_or_veteran	Indicator taking a value of 1 if the L2 voter file labels the individual as “military,veteran”, the L2 commercial file labels them a “veteran”, or the commercial file indicates that there is a veteran in the household.
christian_family	Indicator taking a value of 1 if the L2 commercial file considers the individual to be in the “Christian families” demographic.
gun_owner_or_concealed_carry	Indicator taking a value of 1 if either the L2 voter or commercial file notes that the individual is a gun owner or holds a concealed carry permit.
county_GOP_pres_margin_[cycle] for cycle in 2000, 2004, 2008, 2012, 2016	County-level Republican two-party vote share margin in the specified presidential election.
precinct_turnout_G[YY] for YY denoting 2010, 2012, 2014, or 2016	General election turnout within the voter’s precinct in the specified general election.
precinct_turnout_G[YY]_[party] for YY denoting 2010, 2012, 2014, or 2016 and party denoting Republicans, Democrats, or independents	General election turnout among voters affiliated with the specified party within the voter’s precinct in the specified general election.
turnout_[cycle] for cycle in 2008, 2010, 2012, 2014, 2016	Indicator for whether the individual voted in the specified general election.
turnout_[cycle] for cycle in 1992, 1994, . . . , 2004, 2006	Categorical variable specifying turnout records for the given general election. This variable takes a value of 4 if a vote was recorded in the individual’s L2 turnout history; 3 if the individual was not yet 18 on Election Day; 2 if L2 does not have records for that state-cycle combination; 1 if turnout in the election was less than 20% among all individuals in the L2 voter file currently residing in said state; 0 if there is no record of a vote cast in said election, and none of the previous factors apply.
primary_turnout_[cycle] for cycle in 1992, 1994, . . . , 2014, 2016	Indicator variable specifying turnout records for the given primary election. Takes a value of 1 if a vote was recorded in the individual’s L2 turnout history; 0 otherwise.



## H Additional Results and Robustness Checks

### H.1 Robustness Checks and Placebo Tests for Farm Sample Turnout Analyses

We present a number of supplemental analyses to demonstrate that our main conclusions are unlikely to be driven by the particular implementation of our empirical strategy or unobserved confounding factors. Inferences drawn using alternative statistical methods, analyses of effect heterogeneity, and a placebo test all corroborate our main conclusion: large differences in policy outcomes had at most negligible impacts on partisan turnout rates. For readers who prefer to peruse statistical results in table form, we direct attention to Section I of these Further Supplemental Materials, which consolidates all Double Machine Learning (DML) estimates from our article (including most of the results from this section) into a single table. We also note that we devote much of the next section (H.2) to discussing an additional important robustness check: effect heterogeneity by date of MFP enrollment.

First, since the DML approach of Chernozhukov et al. (2018) is a relatively new methodology, we replicate our main results using a simple OLS specification. We present the results in Table FSM12. We emphasize that the DML estimator we employ in all of our main analyses holds several key advantages over OLS: we can control for a variety of pre-treatment covariates nonparametrically while also explicitly allowing for differential selection into treatment dispositions according to observed covariates. Moreover, model selection is data-driven, limiting researcher degrees of freedom. However, as noted, DML is a fairly new causal inference paradigm, and DML covariate adjustment is a fairly black-box process.<sup>17</sup> As such, it is reassuring to see that our main results depicted in Figure 6 are very consistent with the estimates presented in Table FSM12. We nonetheless prefer our main estimates as they rely on the less restrictive assumptions of the partially linear model, and as such we utilize DML estimators

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<sup>17</sup>To render our DML covariate adjustment somewhat more interpretable, in Further Supplemental Materials Section F.3 we plot a measure of feature importance for each of our controls with respect to our CatBoost estimation of the first-stage nuisance functions  $q(X_i) = \mathbb{E}[Y_i | X_i]$  and  $m(X_i) = \mathbb{E}[B_i | X_i]$ .

**Table FSM12: Estimated Effects of Improved Policy Outcomes on 2018 Turnout by Party (Ordinary Least Squares Estimates)**

	Republicans			Non-Republicans		
	(1)	(2)	(3)	(4)	(5)	(6)
Net MFP Benefit Percentile	0.006 (0.004)	—	—	0.007 (0.007)	—	—
MFP as % of Damage	—	0.004* (0.002)	—	—	-0.002 (0.004)	—
MFP Made Whole	—	—	0.004 (0.003)	—	—	0.008 (0.005)
Democrat	—	—	—	0.030*** (0.003)	0.030*** (0.003)	0.030*** (0.003)
Voted in 2016	0.392*** (0.006)	0.392*** (0.006)	0.392*** (0.006)	0.398*** (0.006)	0.398*** (0.006)	0.398*** (0.006)
Voted in 2014	0.159*** (0.004)	0.159*** (0.004)	0.159*** (0.004)	0.181*** (0.005)	0.181*** (0.005)	0.181*** (0.005)
Voted in 2012	0.104*** (0.006)	0.104*** (0.006)	0.104*** (0.006)	0.094*** (0.006)	0.095*** (0.006)	0.094*** (0.006)
Voted in 2010	0.095*** (0.004)	0.095*** (0.004)	0.095*** (0.004)	0.109*** (0.005)	0.109*** (0.005)	0.109*** (0.005)
Historical Farm Size: Log(Base Acres 2009-2012)	0.001 (0.001)	0.001* (0.001)	0.001* (0.001)	-0.004*** (0.002)	-0.003*** (0.001)	-0.003*** (0.001)
Female	-0.016*** (0.002)	-0.016*** (0.002)	-0.016*** (0.002)	-0.016*** (0.003)	-0.016*** (0.003)	-0.016*** (0.003)
Age	-0.001*** (0.000)	-0.001*** (0.000)	-0.001*** (0.000)	-0.001*** (0.000)	-0.001*** (0.000)	-0.001*** (0.000)
Census Block Pop. Density: Log( Adults / Sq. Mile)	-0.007*** (0.000)	-0.007*** (0.000)	-0.007*** (0.000)	-0.006*** (0.001)	-0.006*** (0.001)	-0.006*** (0.001)
Intercept	0.300*** (0.015)	0.295*** (0.015)	0.296*** (0.015)	0.256*** (0.019)	0.257*** (0.019)	0.249*** (0.019)
Education Controls	Yes	Yes	Yes	Yes	Yes	Yes
Race/Ethnicity Controls	Yes	Yes	Yes	Yes	Yes	Yes
House District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	111,892	111,892	111,892	56,211	56,211	56,211
R-squared	0.258	0.258	0.258	0.372	0.372	0.372

Notes: Robust standard errors clustered at the farm level in parentheses. Dependent variable is turnout in the 2018 general election. 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). We control for education using five indicators corresponding to six categories: missing education (6% of sample), less than high school (2%), high school (31%), some college or vocational/technical degree (17%), bachelor’s degree (33%), and graduate degree (11%). We control for race/ethnicity with indicators for “white, not Hispanic” (90%) and “missing race/ethnicity” (8%). \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$  (two-tailed).

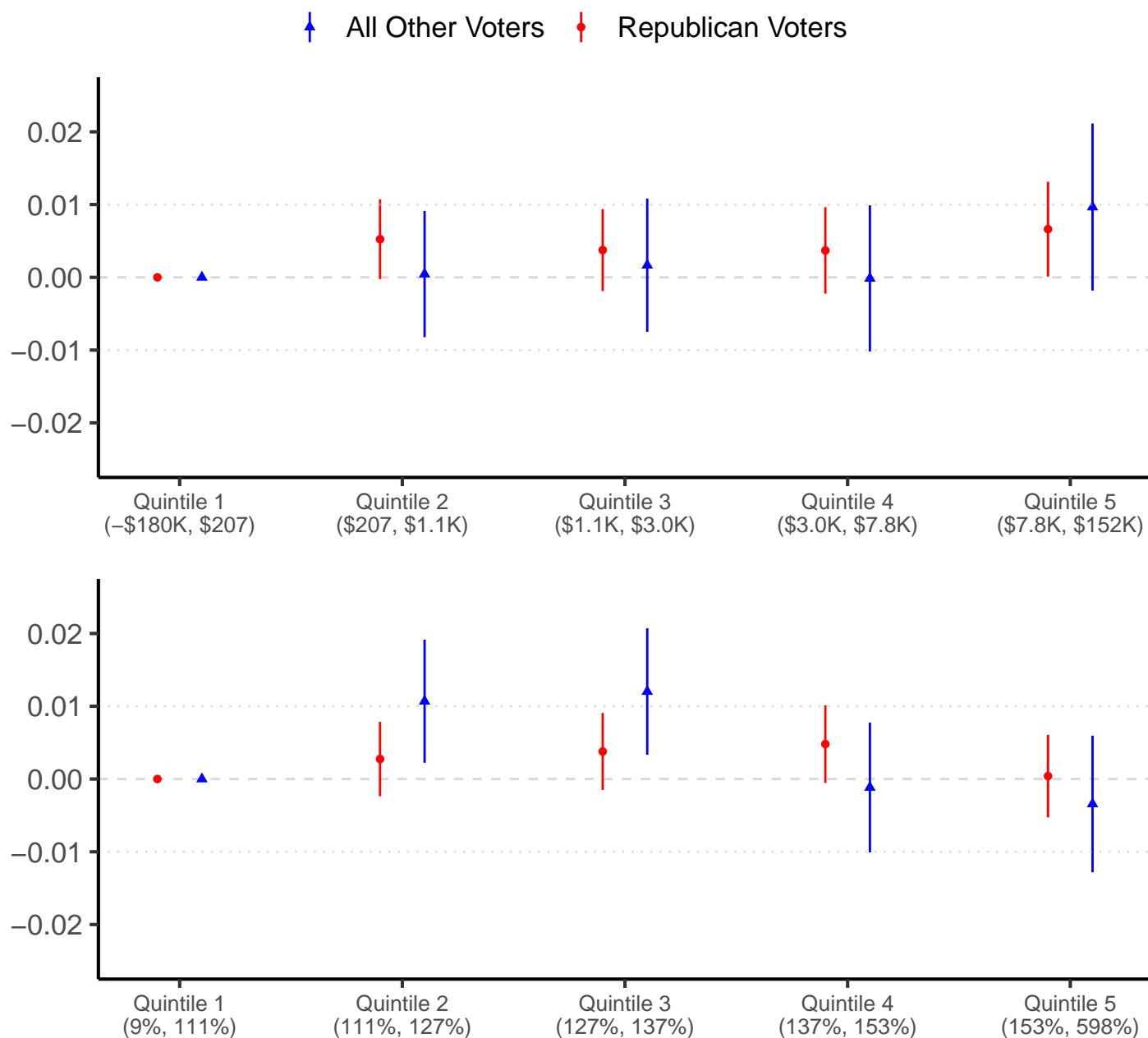
in all of our remaining turnout analyses.

Second, to verify that the linear effect specification in the partially linear model is not masking a more substantial (but nonlinear) effect, we estimate a specification in which treatment status is captured by four policy outcome quintile indicators. We bin voters into quintiles by MFP net benefits and compensation rates, and then use DML to regress turnout in the 2018 midterms on indicators for quintiles 2-5. Each coefficient plotted in Figure FSM31 reflects the estimated increase in turnout from a voter being placed in quintile  $q \in \{2, \dots, 5\}$  instead of the first quintile, which spans -\$180,169 to -\$134 in net MFP benefits and 9.1% to 111% in MFP compensation as a share of tariff-induced losses. As this divides the effective sample size for each estimate by 5, it is unsurprising that the resulting estimates are noisier than the main estimates depicted in Figure 6. Nonetheless, these analyses provide no evidence that our linear effect specification is disguising large nonlinear effects on turnout.

In our third robustness check, we consider whether turnout effects might be heterogeneous by the level of electoral competition in a voter’s district. Farmers tend to live in highly rural areas, and given high levels of geographic partisan sorting in recent decades, one might wonder whether members of our sample have the opportunity to turn out in a competitive election. This could be theoretically relevant if the mechanism by which incumbent-party voters are mobilized is an increased desire to keep the incumbent party in power as a reward for good policy outcomes. Given such a theoretical hypothesis, one might be concerned that low electoral competition might render moot large changes in policy outcomes, as voters perceive limited ability to reward or punish the incumbent party.

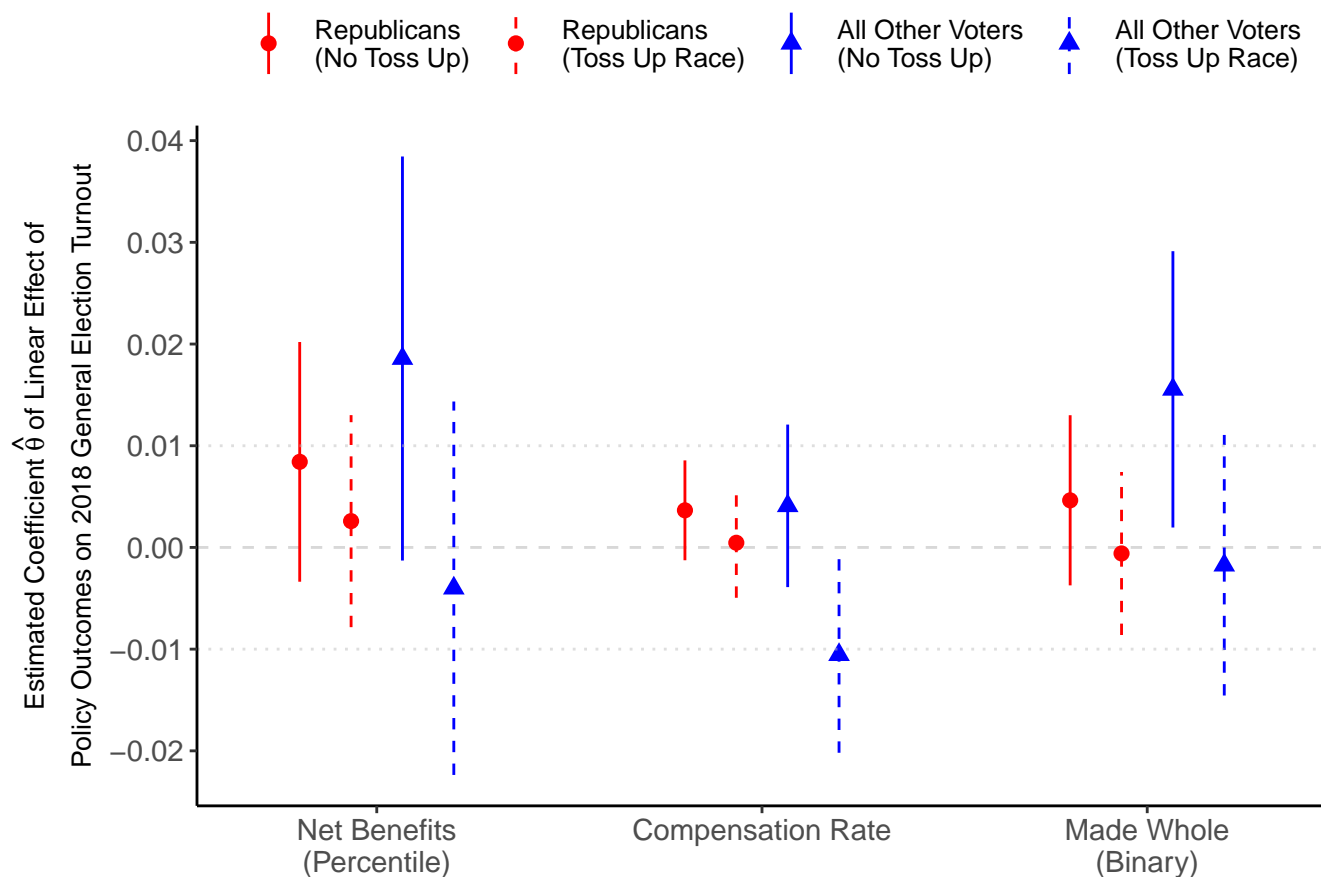
To investigate this concern, we collect Cook Political Report ratings ahead of the August 27 announcement of the MFP commodity-specific rates. Using ratings for US House and governor’s races from August 15 and US Senate ratings from August 3, we construct an indicator variable for each member of our sample that takes a value of 1 if the voter faced at least one House, Senate, or Governor’s race labeled a “toss up,” and a value of 0 otherwise. We find that 53% of voters in our main sample did actually face one or more such “toss up”

**Figure FSM31:** Estimated Effects of Net Benefits and Compensation Rates on 2018 Turnout (Quintile-Binned Effect Specifications)



Notes: Treatment in upper panel is MFP benefits minus estimated 2018 tariff-induced losses, specified in terms of quintile indicators. Treatment in lower panel is MFP benefits as a share of tariff-induced losses, also specified in terms of quintiles. Effects are estimated separately for Republicans and non-Republicans. First quintile serves as the left-out category in each specification. To view these results in table form, see Further Supplemental Materials Section I.

**Figure FSM32:** Effect of Policy Outcomes on 2018 Turnout (Heterogeneity by Level of Electoral Competition)



Notes: 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 Further Supplemental Materials Section I.

rates, and we re-estimate our main specifications with this indicator added as an interaction term.

We present the resulting estimates in Figure FSM32. Perhaps surprisingly, the interaction term on the competitive election indicator is actually negative in all specifications, though we do not observe any meaningful heterogeneity among Republican voters (our primary subsample of interest). As a side note, non-Republican estimates presented in this figure constitute a rare example in this study in which we observe nontrivial differences in estimates obtained using different treatment specifications. Specifically, we find that a 100 percentage point in-

crease the MFP compensation rate reduced non-Republican turnout by 1.1 percentage points in competitive races (but only competitive races), whereas making a farmer whole in a non-competitive race (and only a non-competitive race) increased turnout by 1.6 percentage points. We consider the compensation rate estimate to be the more credible of the two; as discussed in detail in Further Supplemental Materials Section D, compensation rate is the most uncorrelated with pre-treatment covariates among our three treatment measures, and is thus likely the most statistically robust. As such, one appropriate conclusion from these results might be that non-Republicans were more likely to turn out in highly competitive races if they obtained very bad compensation outcomes. However, taking any of these effect sizes at face value still corroborates our overall conclusion: especially large differences in compensation outcomes failed to have a large impact on turnout.

In our fourth robustness check, we demonstrate that our results are not artifacts of the particular method we chose to estimate individual-level trade war damage. We re-run our main estimates (Figure 6) using the four alternative methods we propose for aggregating commodity-specific price impact estimates from the agricultural economics literature. For details on these measures, see Online Appendix A.4 (and Table OA2 in particular). Figure FSM33 compares these alternative results to those obtained using our main specification; our estimates are very consistent across the five aggregation methods. Note that the first column of estimates in Figure FSM33 expands the sample of voters to include those running dairies or raising hogs, the second column adds in hog farmers (but not dairy owners), and the fifth column is based only on the subset of our main sample that grew only corn and/or soybeans in 2018.

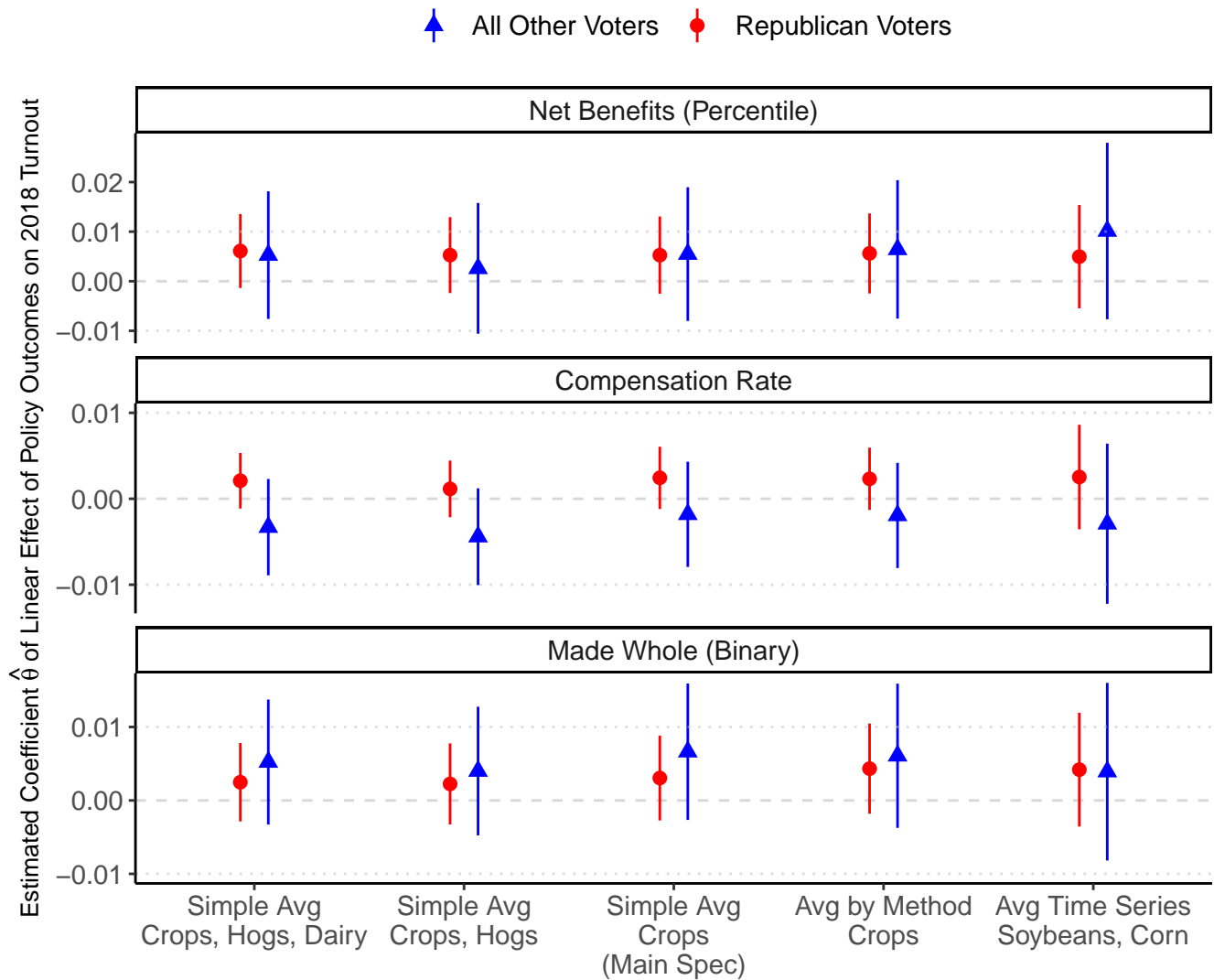
Finally, in our fifth supplemental analysis, we assess the validity of our estimation strategy by constructing a set of placebo estimators that are analogous to tests of “parallel trends” in the differences-in-differences framework. We estimate the “effect” of net MFP benefits in 2018 on 2016 turnout for each of our policy outcome treatments. To the extent our research design is valid, each of these placebo estimates should be close to zero. On the other hand,

large and systematic deviations from zero may be indicative of the sign and magnitude of bias in our main estimate.

We note that our ability to construct a valid placebo test is somewhat limited by the nature of our panel data on farmer turnout. Since state voter list administrators remove voters who move or become inactive for a certain duration, attrition in voter files causes well-known issues of survivorship bias that can only be fully mitigated by collecting snapshots before and after an event or treatment of interest. Since the earliest voter file snapshots we have for our sample are from February 2018, we have reason to believe that such attrition is minimal for the 2016 general election, and thus find it to be a useful candidate for a placebo test. To make each of these estimates maximally comparable to our actual estimates, we restrict our sample to voters already registered before the 2016 election, and limit to covariates observed before the 2016 election.

The estimates in Figure FSM34 show no systematic, large deviations from zero. Estimated effects on Republican turnout were less than one percentage point in magnitude for each policy outcome considered, and yield no evidence that our main effect in 2018 is substantially biased downwards. Likely by chance (given the large number of hypotheses tested in our robustness checks), the estimated effect of making a Republican farmer whole on 2016 turnout is positive and statistically significant. However, even taking this estimate at face value would not imply that our DML estimator is providing an attenuated estimate of the true effect of making a farmer whole; subtracting the putative “bias” of 0.6 percentage points (i.e. the placebo estimate) from our main estimate depicted in Figure 6 would imply a revised turnout effect of -0.3 percentage points. We similarly note that none of the placebo estimates for non-Republicans suggest that our actual null result is spurious.

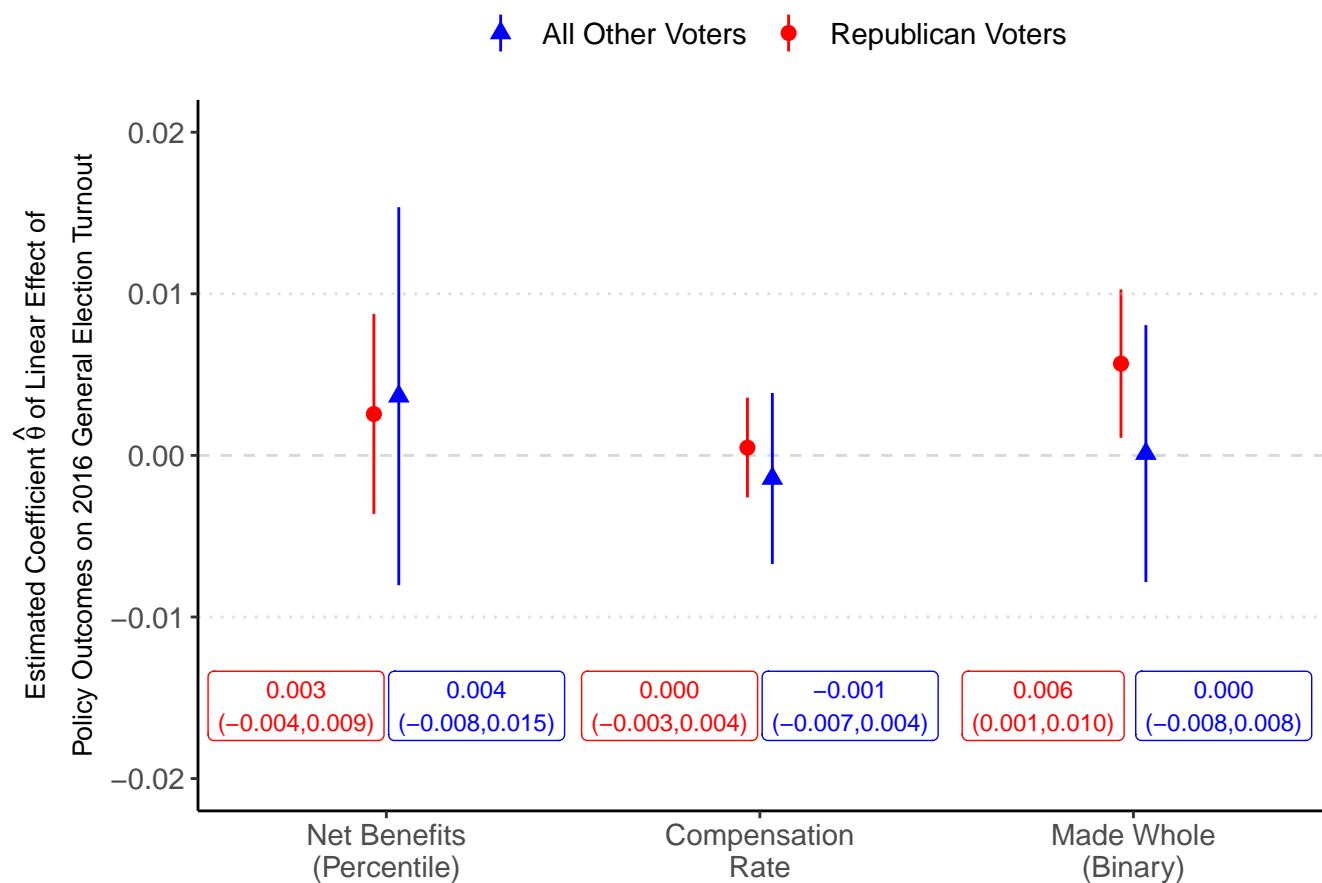
**Figure FSM33:** Turnout Effect Estimates Using Alternative Damage Measure Specifications



Notes: 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 Further Supplemental Materials Section I.



**Figure FSM34:** Placebo Estimates of the Effect of 2018 Policy Outcomes on 2016 Turnout



Notes: 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).

## H.2 Enrollment Timing and Heterogeneous Turnout Effects

In this section, we consider the possibility that payment timing contributes to our turnout results. While the FSA began accepting MFP applications on September 4th, a full two months before Election Day, the program’s payment basis (certified farm-level harvested production) caused a significant delay in payment uptake. MFP regulations stipulated that farmers wait until their harvest was complete before applying for benefits. As shown in Figure FSM35, only the wheat crop was largely in by the time MFP applications opened up. As a result, the first several weeks of MFP enrollment were dedicated almost exclusively to the wheat harvest (see Figure FSM36),<sup>18</sup> and the majority of the soybean and corn harvests that drive our identifying variation in policy outcomes were not enrolled until after Election Day.

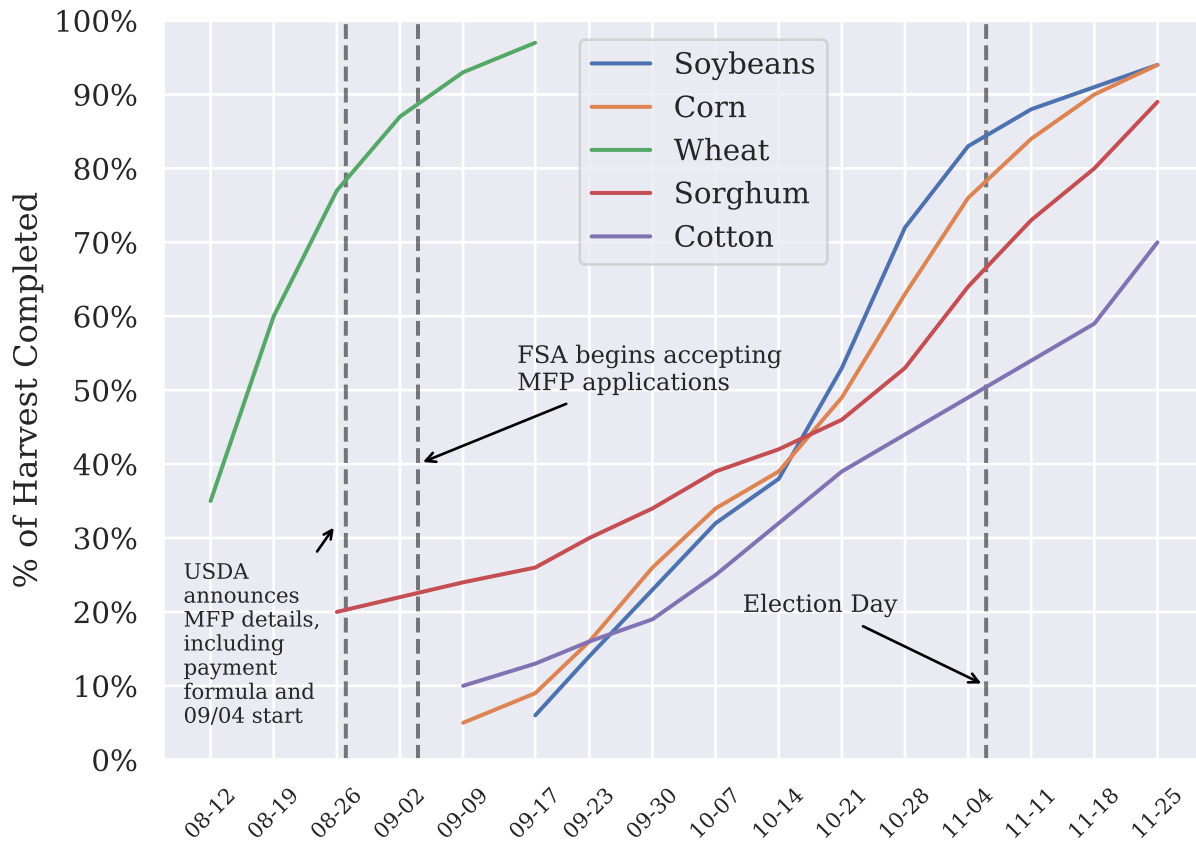
In our main specifications, we include all 2018 enrollments, including those completed in November and December, as farmers were almost certainly aware of their eligibility—and how much money they would be receiving—well before Election Day. Indeed, all members of our sample (by construction) were USDA farm program veterans, and thus were used to regular interactions with their FSA county offices to maintain enrollment in the broader (frequently changing) farm safety net.<sup>19</sup> While we therefore find it likely that farmers within our sample accurately anticipated MFP benefits based on their crop portfolios, we nonetheless examine the possibility that the timing of checks—and associated salience in producers’ minds—mattered. In particular, it is unclear *ex ante* whether one might expect MFP benefits to be significantly more salient if enrollment occurs before Election Day. Previous studies have found that benefits received immediately prior to an election may have a greater effect (Huber, Hill, and Lenz, 2012). This is because voters could be myopic and subject to psychological recency

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<sup>18</sup>At the opposite end of the spectrum, the cotton harvest wrapped up noticeably later than that of the other four crops we study. It is likely for this reason that very few cotton harvests were fully enrolled by the end of CY 2018, and therefore why our sample—which is limited to producers who made all 2018 MFP enrollments in CY 2018—only features 2,960 cotton producers.

<sup>19</sup>As referenced in “Data on Policy Outcomes and Voter Turnout” and discussed further in Online Appendix A.5, we only admit farms into our sample that were enrolled in the flagship USDA safety net programs between 2004 and 2012.

**Figure FSM35: National 2018 Harvest Progress by Major Field Crop**

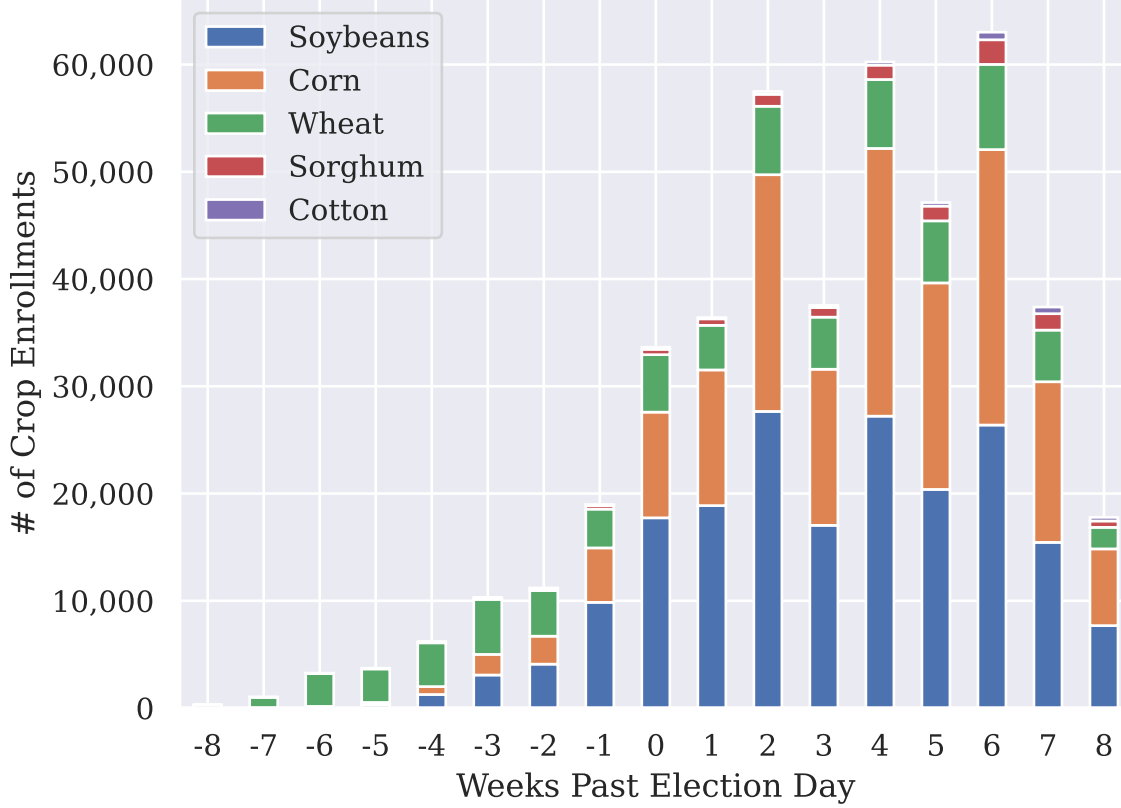


Notes: Data comes from USDA NASS weekly Crop Progress Reports, 08/20/2018–11/26/2018. “Wheat” refers to spring wheat; winter wheat is harvested in early summer.

biases, inconsistent with pure rational voting.

To engage with this concern, we estimate specifications of our partially linear model in which we allow the effect of policy outcomes to vary according to the time of first MFP registration. For a simple first cut, we create an indicator variable for whether a producer’s first MFP record in the FSA system was created before Election Day, and we interact this variable with policy outcomes in the final DML estimation stage. The results, plotted in Figure FSM37, provide no evidence that late MFP registration is driving our results. For Republican voters, the coefficients on the interaction terms estimated across all three treatment measures are both substantively small and far from statistically significant. The estimated coefficients on the interaction terms for non-Republicans are quite noisy, but nonetheless are not statistically significant in any of the three specifications.

**Figure FSM36:** MFP Enrollments by Week

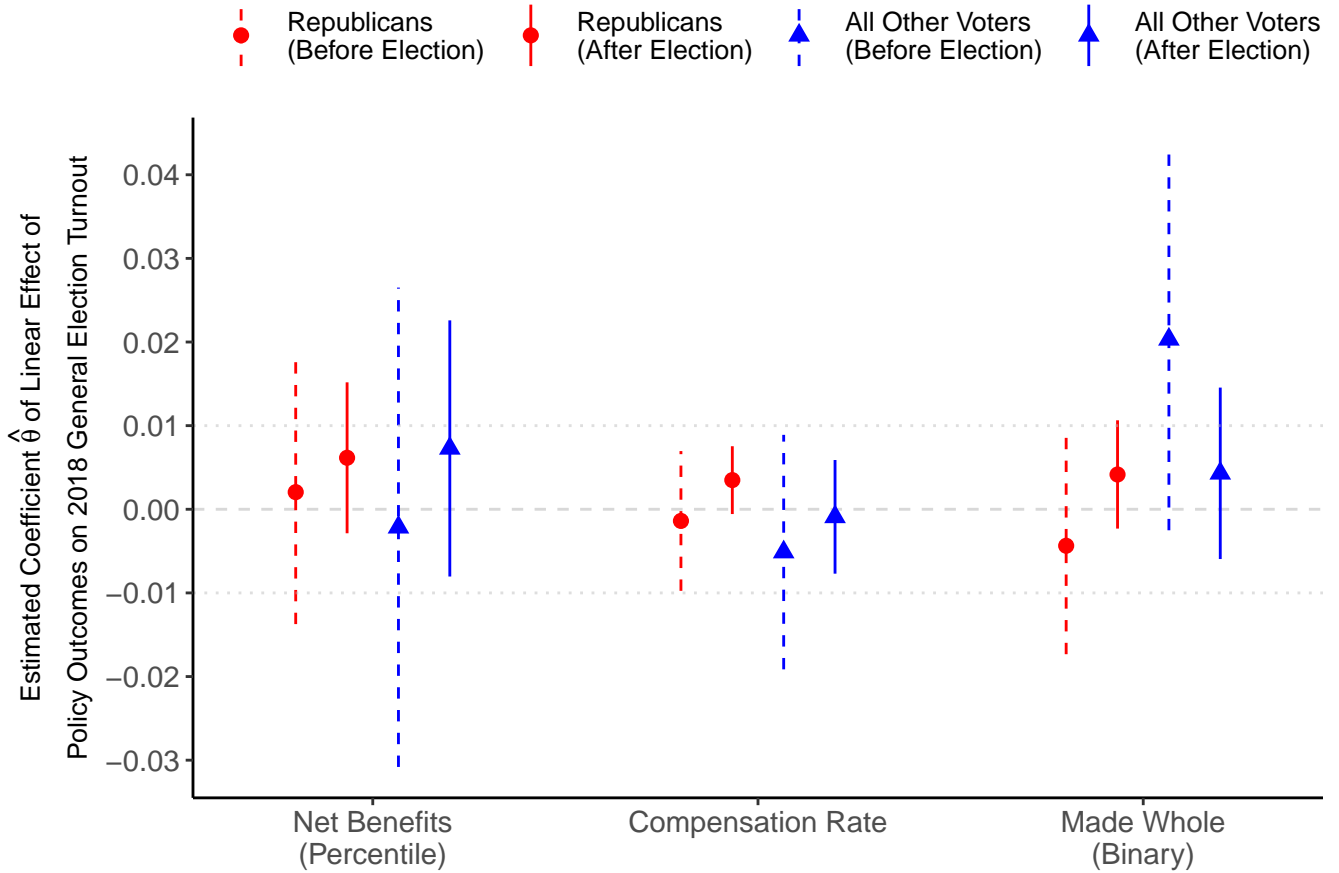


As the two most prominent field crops (corn and soybeans) were not enrolled in large quantities until closer to Election Day, we consider an alternative approach that allows for considerably more general heterogeneity by time. Specifically, we use a causal forest for the final stage, residual-on-residual regression. This method was initially presented in Section 6.1.1 of Athey, Tibshirani, and Wager (2019), though we use the slightly modified implementation (and the EconML software) of Battocchi et al. (2019). Letting  $d$  denote the number of days past Election Day that a producer may have enrolled in the MFP, the causal forest DML estimator solves a local residual-on-residual moment condition:

$$\hat{\theta}(d) = \operatorname{argmin}_{\theta} \sum_{i=1}^N K_d(d_i) \cdot (Y_i - \hat{q}(d_i, X_i) - \theta \cdot (B_i - \hat{m}(d_i, X_i)))^2,$$

where  $\hat{q}(d_i, X_i) = \hat{\mathbb{E}}[Y_i | d_i, X_i]$  and  $\hat{m}(d_i, X_i) = \hat{\mathbb{E}}[B_i | d_i, X_i]$  are machine learning predictions for producer  $i$ 's turnout likelihood and MFP policy outcome (respectively) given their

**Figure FSM37:** Effect of Policy Outcomes on 2018 Turnout (Heterogeneity by Enrollment Date)



Notes: 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 Further Supplemental Materials Section I.

MFP enrollment date  $d_i$  and observed covariate values  $X_i$ . The kernel  $K_d(d_i)$  is a distance metric that reflects how frequently  $d$  and  $d_i$  fall in the same leaf of a subsampled honest random forest trained with a causal criterion. We direct readers to the EconML documentation (Battocchi et al., 2019) for further details on this implementation.<sup>20</sup>

We obtain causal forest estimates of effect heterogeneity by time for four separate treatment/subsample specifications: (i) the effect of net benefit percentile among Republican pro-

<sup>20</sup>Battocchi et al. (2019) discuss this implementation in the page on “Forest Based Estimators”: <https://econml.azurewebsites.net/spec/estimation/forest.html>.

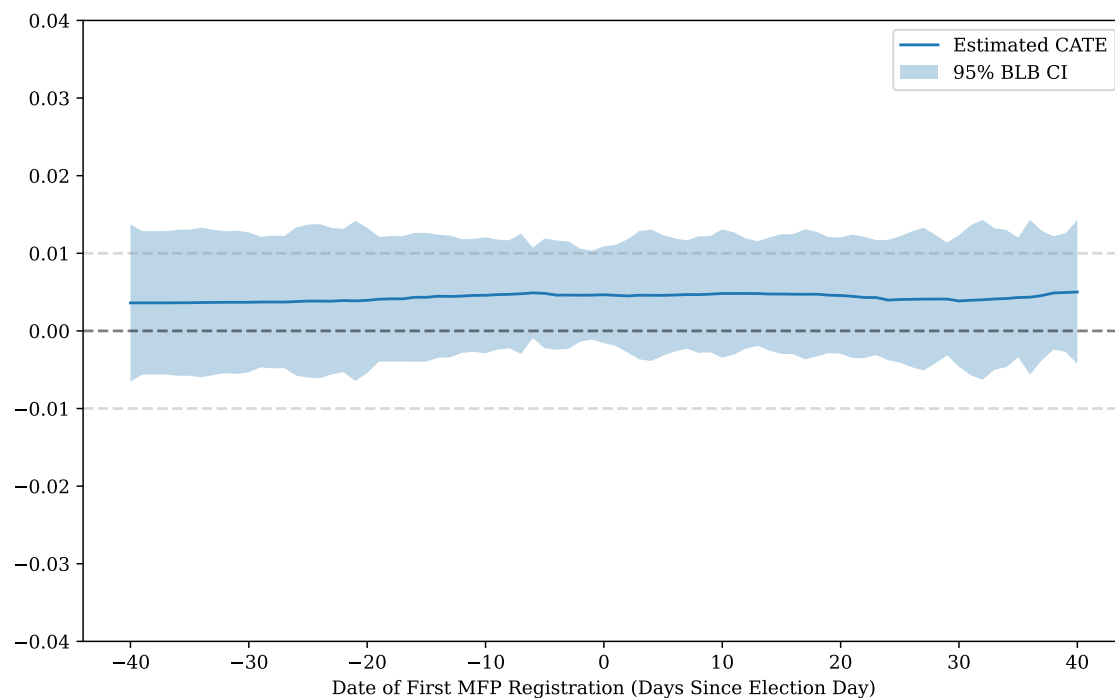
ducers, (ii) the effect of net benefit percentile among non-Republican producers, (iii) the effect of compensation rate among Republicans, and (iv) the effect of compensation rate among non-Republicans. The estimator has a number of hyperparameters relating to the random forest trained for the distance metric  $K_d$ . For each of the four treatment/subsample specifications, we tune eight of the most important parameters by iteratively evaluating 8,000 sets of parameters using a tree-structured Parzen estimator (TPE) sampler. In each iteration, we evaluate the model specification using the out-of-sample R-scorer  $\tau$ -risk metric that was advocated by Schuler et al. (2018) in their simulation study comparing methods for causal model selection.<sup>21</sup> We select the hyperparameters that performed best according to this metric, and plot the resulting effects in Figures FSM38 through FSM41.

None of these four sets of estimates provides evidence that our main results mask an increase in treatment salience just before Election Day. While the highly flexible estimation procedure necessarily entails wide point-wise confidence intervals at times, the Republican sample estimates are still precise enough to rule out any large increase in salience ahead of Election Day. Moreover, there appears to be scarce enough effect heterogeneity that the out-of-sample R-scorer  $\tau$ -risk metric actually selects for almost completely constant treatment effect specifications for the two compensation rate models. Altogether, we conclude that the null results in our main analyses are not an artifact of benefit timing.

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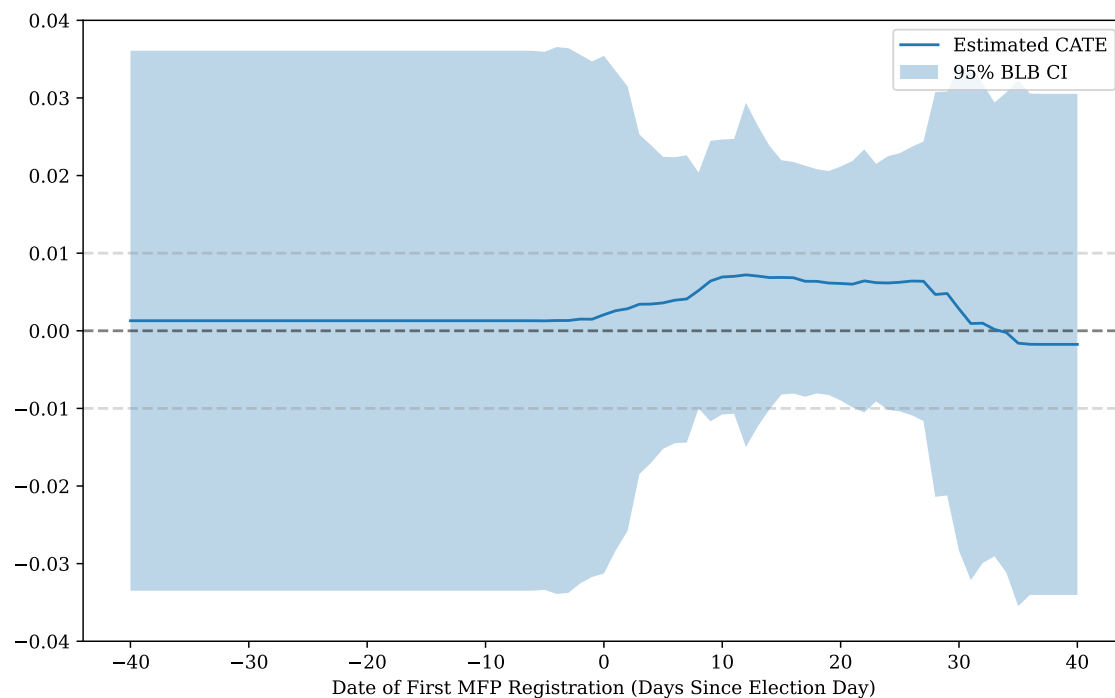
<sup>21</sup>We note that this loss metric was first proposed by Nie and Wager (2021).

**Figure FSM38:** Heterogeneous Turnout Effects by Date of MFP Enrollment: Causal Forest Double ML Estimates of Net Benefit Percentile CATE among Republican Voters



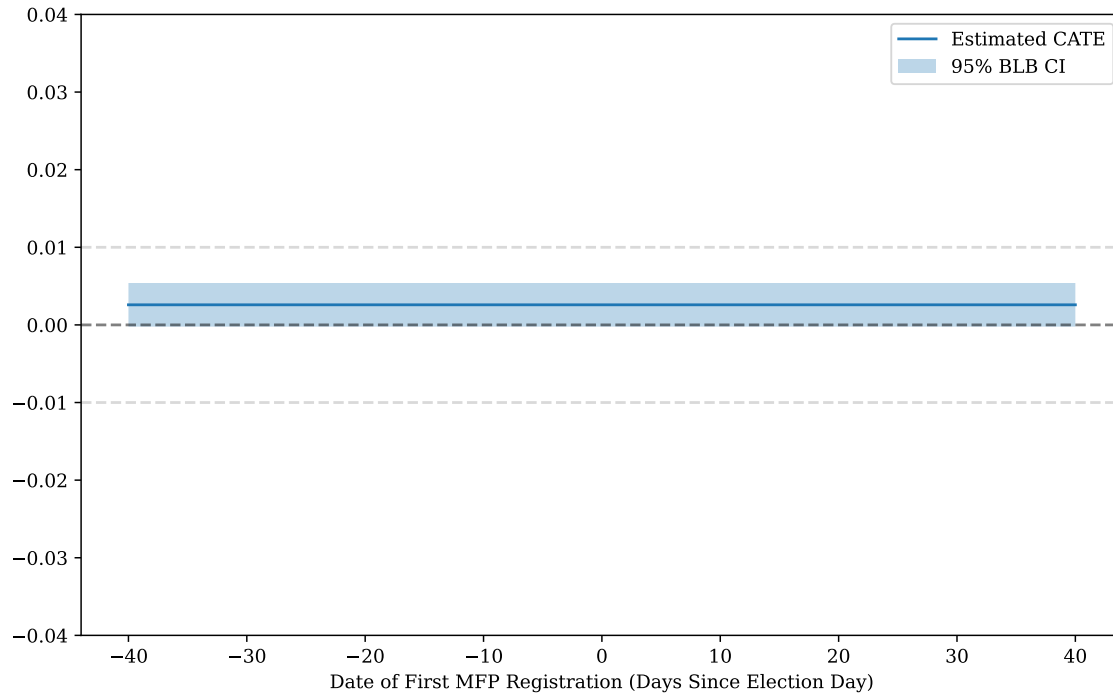
Note: 95% confidence intervals were calculated using Bootstrap of Little Bags.

**Figure FSM39:** Heterogeneous Turnout Effects by Date of MFP Enrollment: Causal Forest Double ML Estimates of Net Benefit Percentile CATE among Non-Republican Voters



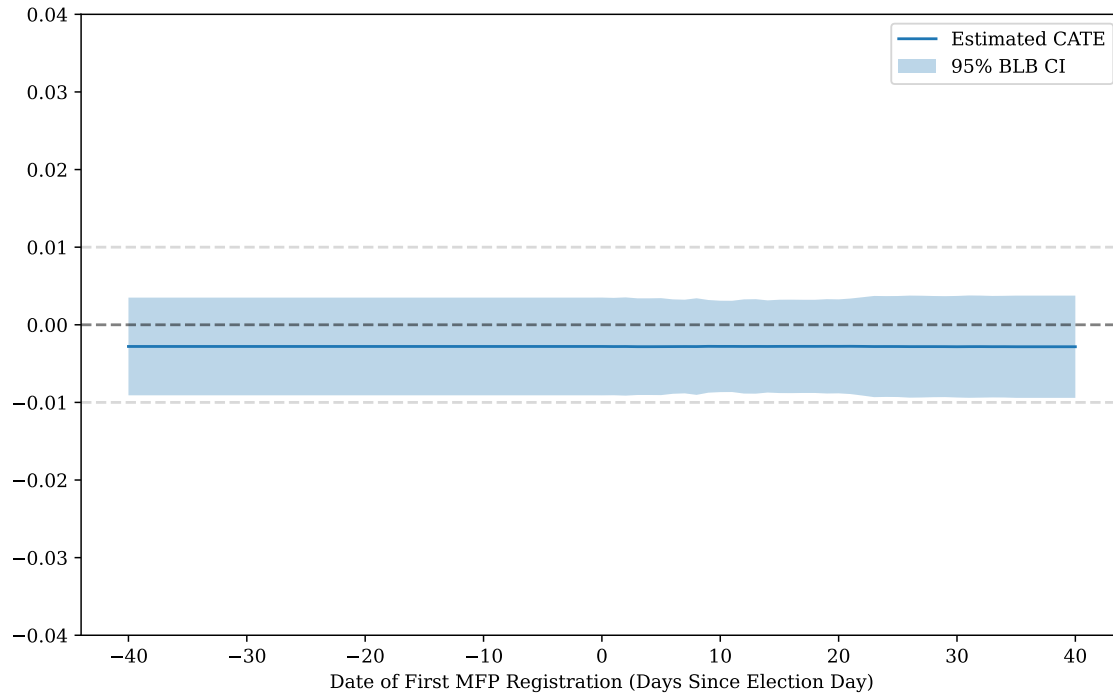
Note: 95% confidence intervals were calculated using Bootstrap of Little Bags.

**Figure FSM40:** Heterogeneous Turnout Effects by Date of MFP Enrollment: Causal Forest Double ML Estimates of Compensation Rate CATE among Republican Voters



Note: 95% confidence intervals were calculated using Bootstrap of Little Bags.

**Figure FSM41:** Heterogeneous Turnout Effects by Date of MFP Enrollment: Causal Forest Double ML Estimates of Compensation Rate CATE among Non-Republican Voters



Note: 95% confidence intervals were calculated using Bootstrap of Little Bags.



### H.3 Robustness Checks and Placebo Tests for Farm Sample Contribution Analyses

In this section, we present (a) estimates of the effects of variation in policy outcomes on alternative contribution outcomes, (b) the results of a placebo test for our main contribution results, and (c) robustness checks concerning our method of damage estimation and our DML approach to estimating the partially linear model.

In the main text (“The Effect of Improved Policy Outcomes on Campaign Contributions”), we analyze the effect of variation in compensation outcomes on farms’ political contributions. Unlike voter turnout, political contributions are not a binary or unidimensional behavior. Contribution decisions vary by timing, frequency, amount, and recipient, and thus specifying contribution behavior as the outcome is nontrivial. For the sake of concision, the estimates we present in the main text concern effects on a maximally parsimonious measure: a farm’s three-point “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 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. Substantively, we can interpret our estimates of effects on this measure as reflecting changes in farms’ engagement in favor of the incumbent Republican Party. This reflects our study’s theoretical motivation towards gauging the extent to which the incumbent party can improve their electoral standing by affecting political engagement through economic policies.

Our main findings concerning farm-level contribution behavior indicate that variation in compensation outcomes had no meaningful impact on net Republican contributing status. We demonstrate here that, likewise, there were no meaningful impacts on key aspects of farms’ contribution behavior. To start, in Figures FSM42 and FSM43, we estimate the effect of variation in our three treatments of interest on farms’ propensity to make any itemized

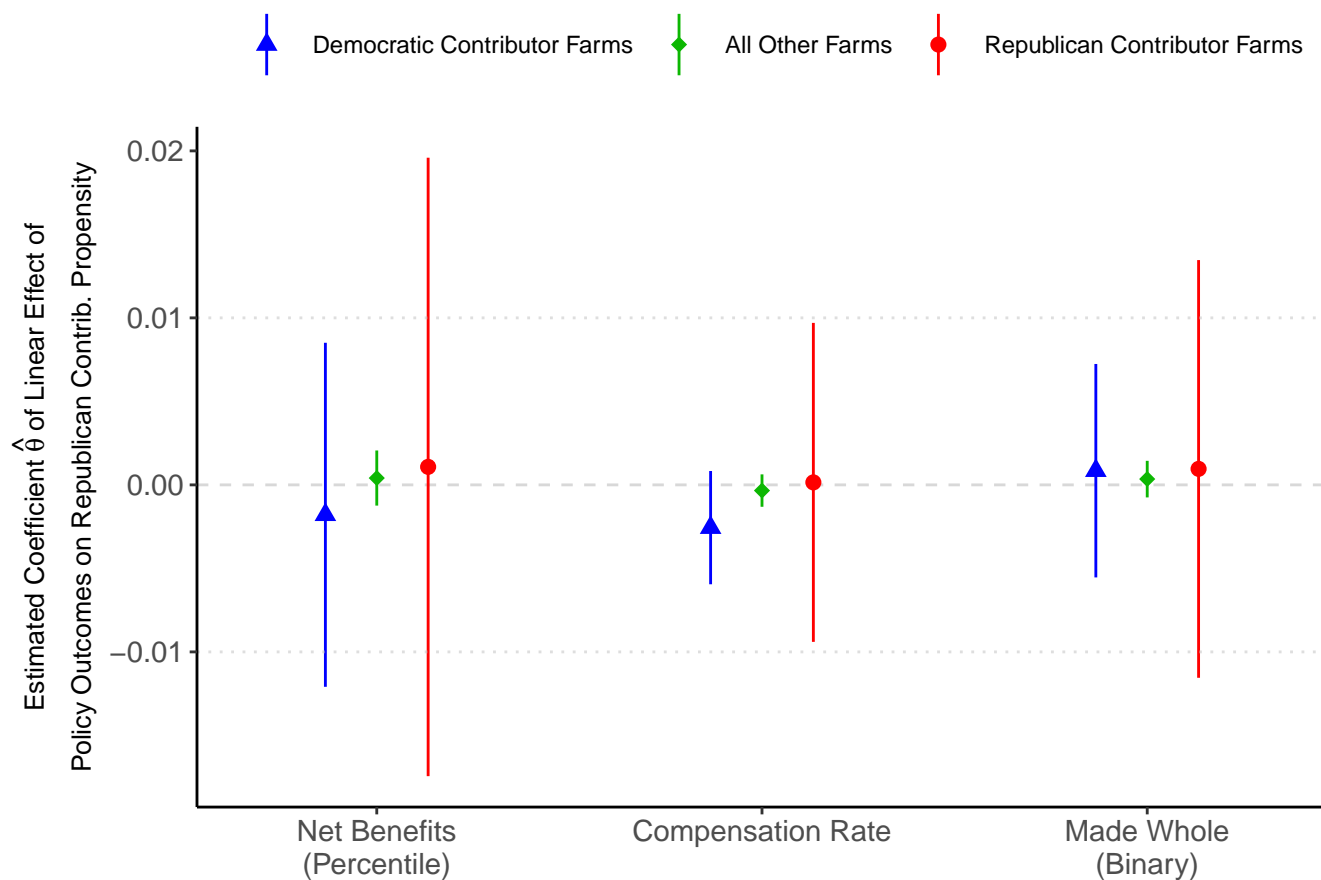
contribution to Republicans or Democrats, respectively. To establish baselines from which to interpret these estimates, we note that among the 15,192 sample farms with a distinctly Republican pre-2018 contribution history, average net Republican contributing in the analysis period (08/27/2018-05/23/2019) was 0.073, with 8.2% contributing to Republicans and 0.9% contributing to Democrats. Among the 5,620 sample farms with a distinctly Democratic pre-2018 contribution history, average net Republican contributing was -0.207, with 0.8% contributing to Republicans and 21.5% contributing to Democrats. Among the remaining 101,345 sample farms, average net Republican contributing was -0.002, with 0.3% contributing to Republicans and 0.5% contributing to Democrats.

None of the estimated effect sizes illustrated in Figures FSM42 and FSM43 are statistically different from zero. Since most farms did not make itemized political contributions prior to 2018, all estimates of contribution effects among Republican-contributing and Democratic-contributing farms are somewhat noisy relative to those depicted for voter turnout. Nonetheless, even if taken at face value, the point estimates would not entail especially large effects on contributions to Republicans or Democrats. In particular, our PLR estimates indicate that a 100 percentage point increase in the compensation rate would have been expected to cause a 0.01 percentage point increase in a Republican contributing farm's likelihood to contribute to Republicans, and a 0.8 percentage point decrease in a Democratic contributing farm's likelihood to contribute to Democrats. Farms without a partisan contribution history were not mobilized (or demobilized) to contribute to a party, either. Among such farms, a 100 percentage point increase in the compensation rate would have been expected to yield a 0.03 percentage point decrease in the propensity to contribute to Republicans, and a 0.01 percentage point increase in the propensity to contribute to Democrats. Altogether, these results confirm our main findings presented in Figure 9: while we do not have the statistical precision to rule out a modest partisan advantage, farm-level economic policy outcomes were clearly not a major determinant of farmers' decisions to contribute to the Republican or Democratic parties during the 2018-2019 trade war.

We next consider whether better (or worse) policy outcomes could have affected the likelihood that farmers' contributed to Trump's reelection campaign (or closely allied PACs) during the 2018-2019 trade war. In Figure FSM44, we reproduce our main contribution analysis (Figure 9) with another outcome: an indicator variable for a farm's decision to make any contribution to Trump's reelection in the months after the MFP payment rates were announced. We find minuscule effect sizes among Republican contributing farms, Democratic contributing farms, and other farms. These results corroborate our claim in the main text that, to the extent voters held Trump accountable by electoral means in 2018, they had ample ability to do so by supporting or opposing downstream Republican Party candidates. Indeed, the lack of meaningful effects documented in Figure FSM44 confirms that our null results in our main analyses are unlikely to be a product of Trump's personal absence from the 2018 ballot.

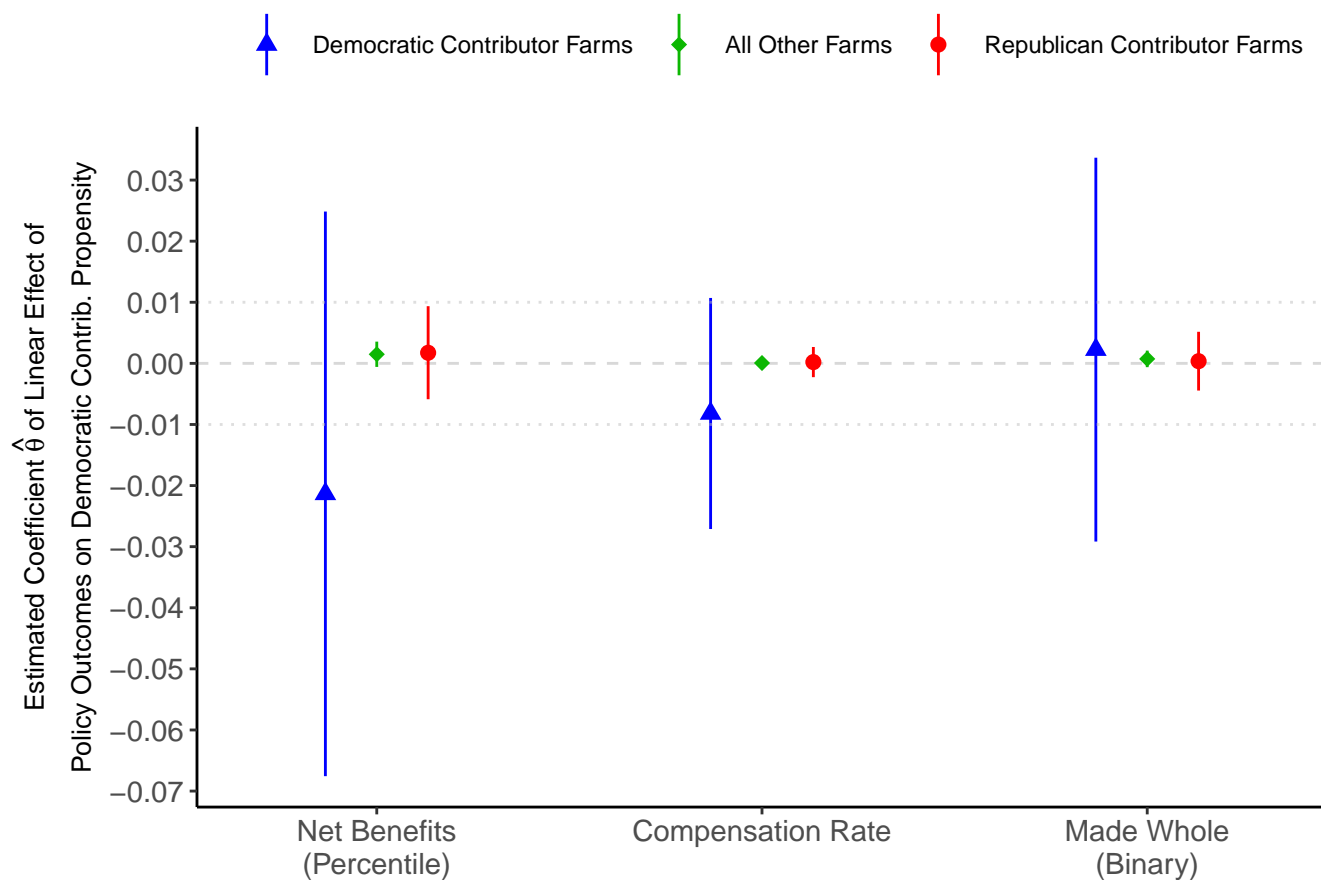
Next, we provide evidence that the null results in our main contribution analyses are not artifacts of the particular outcome or treatment measures we have constructed. In Figure FSM45, we demonstrate that we obtain similar results if we specify our outcome as the number of contributions to Republicans minus the number of contributions to Democrats (instead of our -1/0/1 "net Republican contributing" scale). In Figure FSM46, we re-run our main estimates (Figure 9) using the four alternative methods we propose for aggregating commodity-specific price impact estimates from the agricultural economics literature. For details on these measures, see Online Appendix A.4 (and Table OA2 in particular). Figure FSM33 compares these alternative results to those obtained using our main specification; our estimates are very consistent across the five aggregation methods. Note that the first column of estimates in Figure FSM33 expands the sample of farms to include those raising dairy cattle or hogs, the second column adds in hog farmers (but not dairy owners), and the fifth column is based only on the subset of our main sample that grew only corn and/or soybeans in 2018.

**Figure FSM42:** Estimated Effects of Improved Policy Outcomes on Propensity to Contribute to Republicans by Prior Contribution Behavior



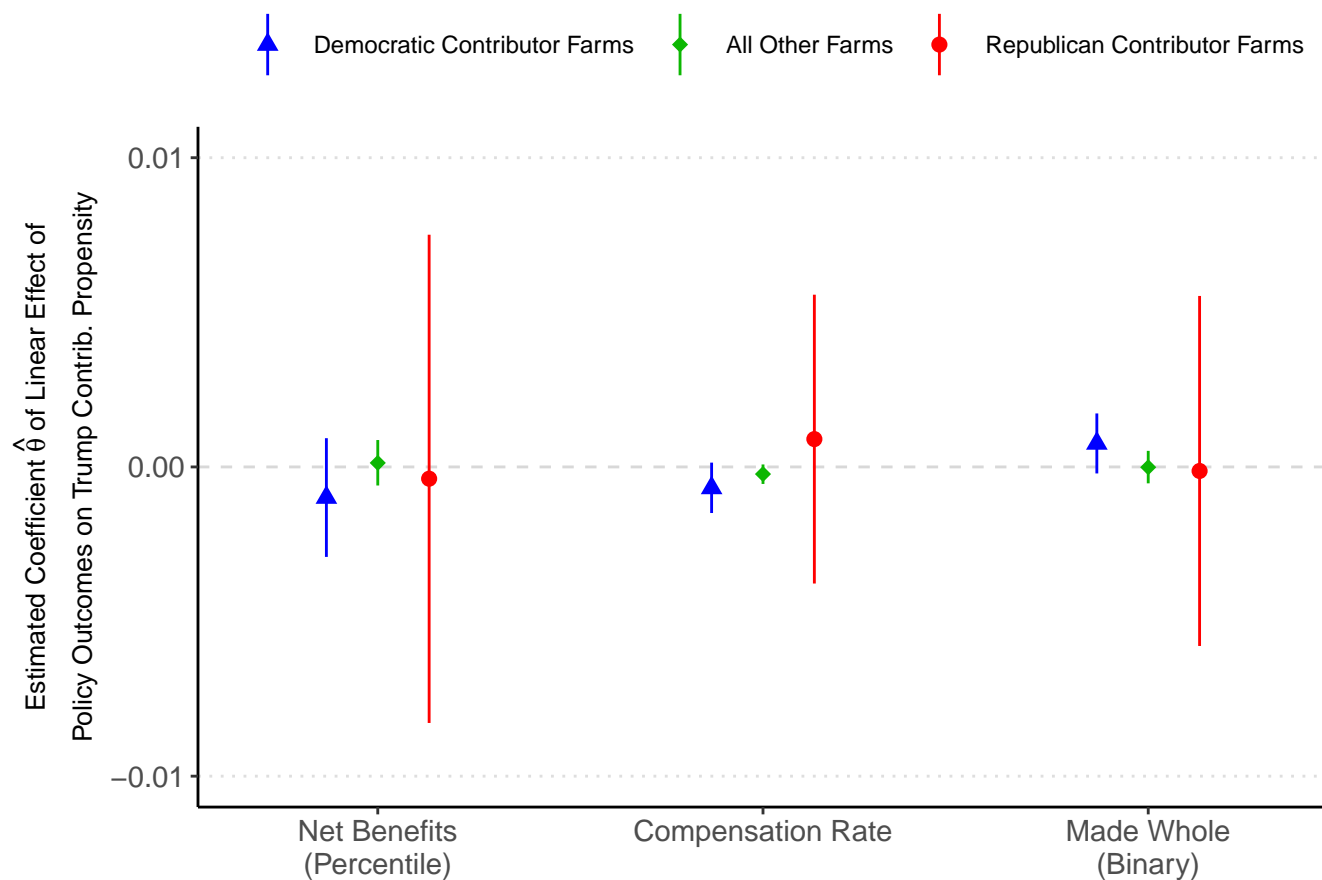
Notes: Effects are estimated jointly among sample of 122,157 farms, with treatment interactions allowing for separate slope estimates among (a) farms with distinctly Republican contribution histories before 2018, (b) farms with distinctly Democratic contribution histories, and (c) all other farms. Point 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 Further Supplemental Materials Section I.

**Figure FSM43:** Estimated Effects of Improved Policy Outcomes on Propensity to Contribute to Democrats by Prior Contribution Behavior



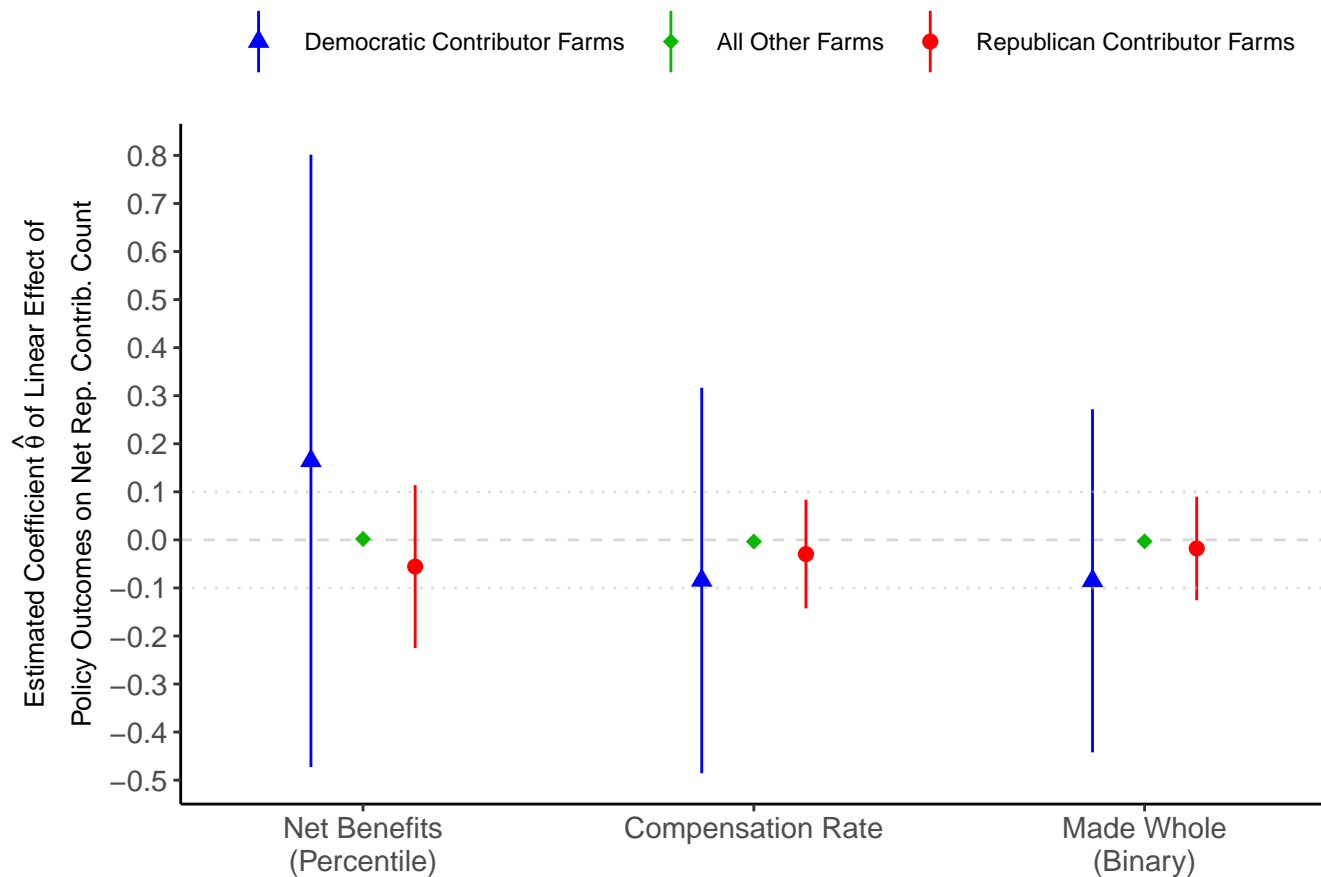
Notes: Effects are estimated jointly among sample of 122,157 farms, with treatment interactions allowing for separate slope estimates among (a) farms with distinctly Republican contribution histories before 2018, (b) farms with distinctly Democratic contribution histories, and (c) all other farms. Point 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 Further Supplemental Materials Section I.

**Figure FSM44:** Estimated Effects of Improved Policy Outcomes on Propensity to Contribute to Trump by Prior Contribution Behavior



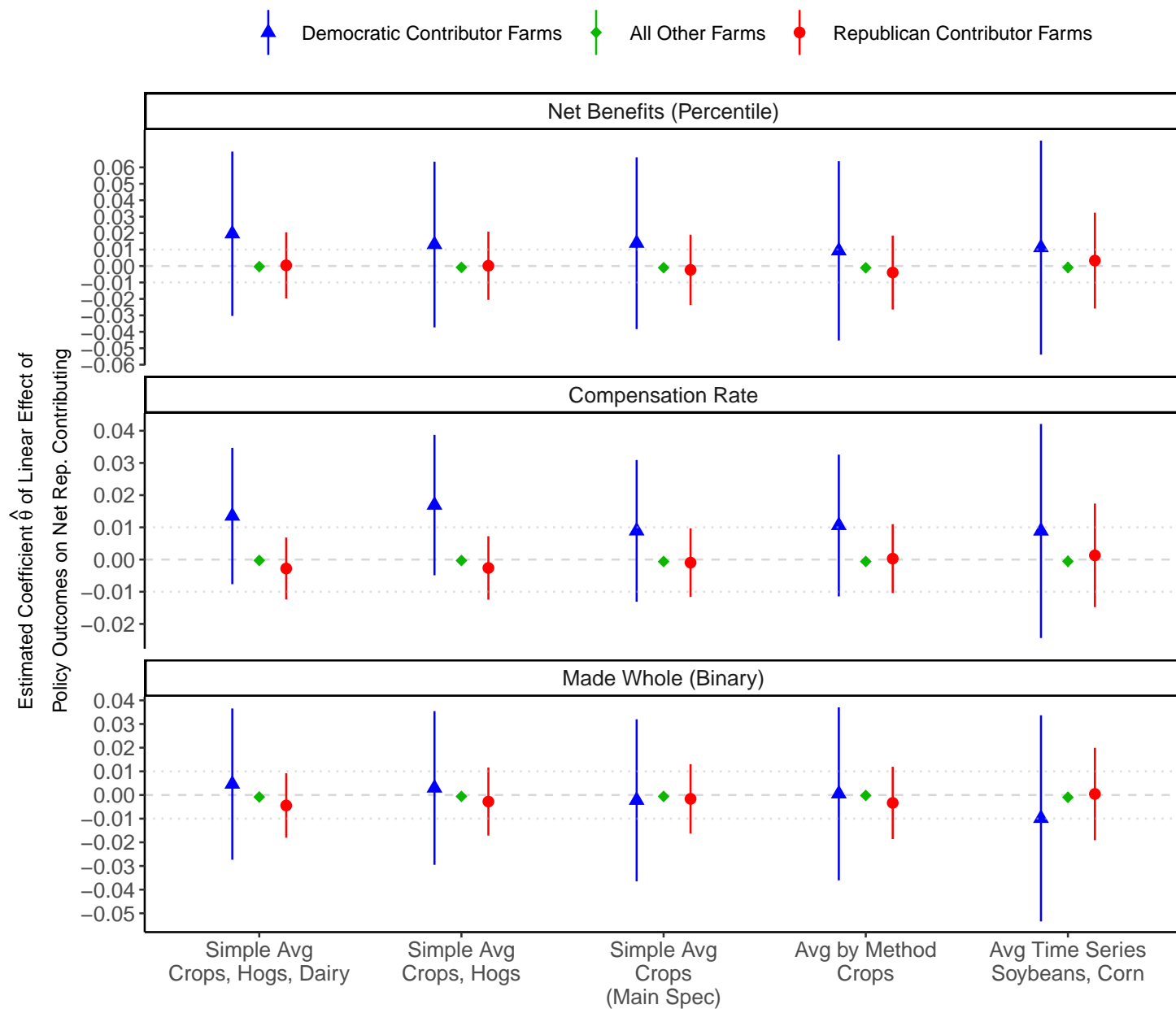
Notes: Effects are estimated jointly among sample of 122,157 farms, with treatment interactions allowing for separate slope estimates among (a) farms with distinctly Republican contribution histories before 2018, (b) farms with distinctly Democratic contribution histories, and (c) all other farms. Point 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 Further Supplemental Materials Section I.

**Figure FSM45:** Estimated Effects of Improved Policy Outcomes on Net Republican Contribution Count



Notes: 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 Further Supplemental Materials Section I.

**Figure FSM46:** Comparison of Estimated Effects of Improved Policy Outcomes on Net Republican Contributing Using Different Trade War Damage Measures



Notes: 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 Further Supplemental Materials Section I.

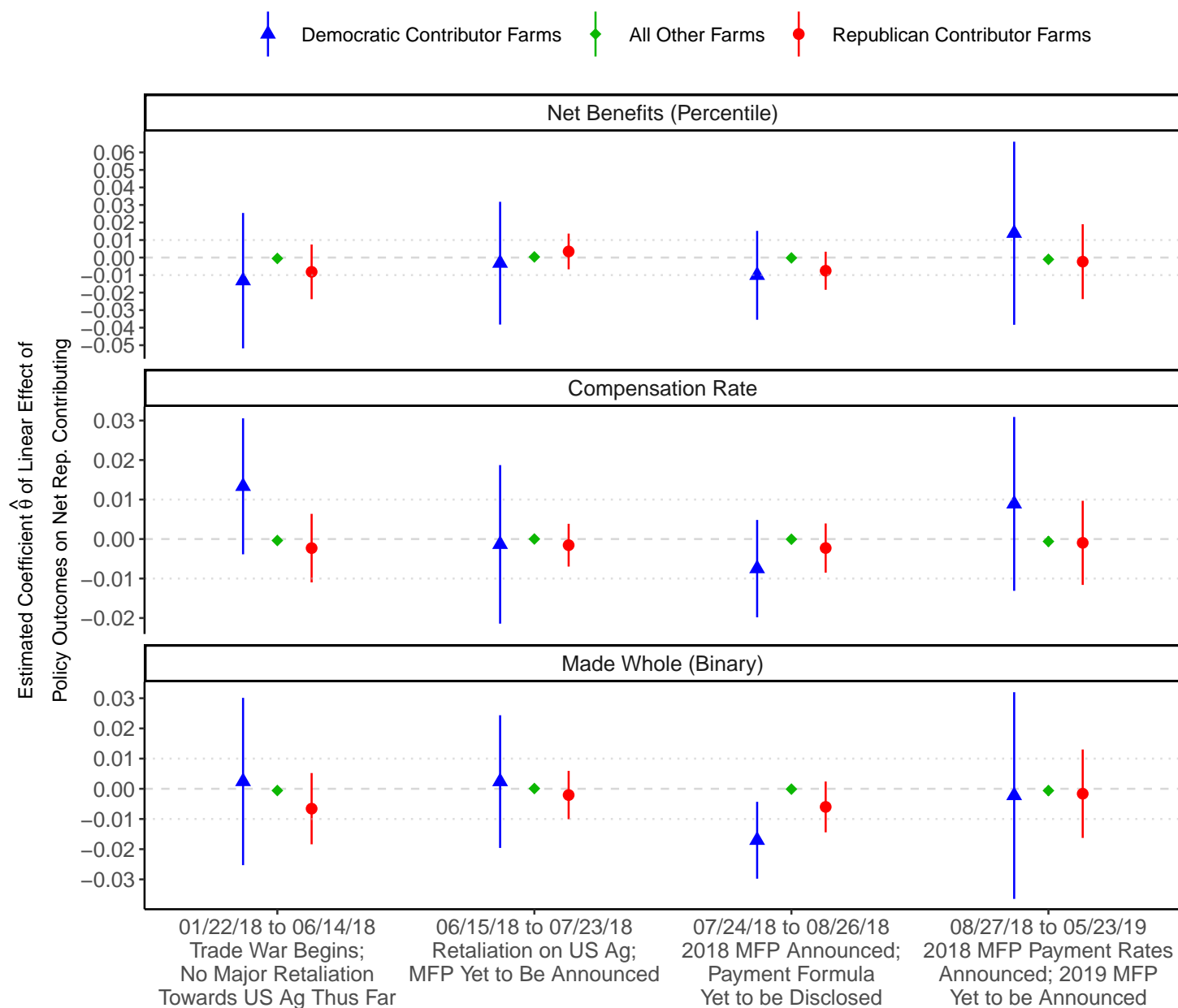


In Figure FSM47, we conduct a placebo analysis to evaluate the extent to which our covariate adjustment strategy has successfully eliminated the influence of confounding factors that simultaneously drive political contributions and correlate with our treatment measures. By examining a set of placebo estimators, this exercise is analogous to a test of “parallel trends” in the differences-in-differences framework. We estimate the “effect” of improved policy outcomes on farm-level contributions in early 2018, but prior to the announcement of retaliatory tariffs on US agriculture. To the extent our research design is valid, each estimated effect of policy outcomes on placebo-period (01/22/18–06/14/18) net Republican contributing should be close to zero. On the other hand, large and systematic deviations from zero may suggest our treatment effect estimates are capturing something more than just farmers’ experienced policy outcomes, as neither farm-level variation in MFP benefits nor trade war losses should be anticipated prior to the 06/15/2018 announcement of retaliatory tariffs on US agriculture.

While this analysis mirrors the placebo tests for our turnout analysis that we present in Figure FSM34, we are actually able to conduct a much more proper placebo analysis for campaign contributions than we were for turnout. This is because turnout analyses based on panel data must grapple with the spectre of survivorship bias arising from voter file attrition (as we discuss in Further Supplemental Materials H.1). Since campaign contributions are made continuously throughout the electoral cycle, it is much easier to ensure that we can articulate a valid placebo estimand for our contribution analyses. Notably, admission into our sample requires that farms are linked to registered voters in the February 2018 L2 voter file; in practice, linked voters in this sample virtually all registered to vote by the beginning of 2018. While this sample admission criteria might plausibly pose issues with utilizing turnout in previous elections as a placebo estimand, it provides no barrier to using early 2018 contributions as a placebo estimand.

In Figure FSM47, we compare estimated impacts of improved policy outcomes on net Republican contributing in our main placebo period (01/22/18–06/14/18) and our main anal-

**Figure FSM47:** Estimated Effects of Improved Policy Outcomes on Net Republican Contributing Across Distinct Trade War Policy Regimes



Notes: 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 Further Supplemental Materials Section I.

ysis period (08/27/18–05/23/19). For the sake of transparency, we also present estimated effects in the two intervening phases of trade war policy developments (06/15/18–07/23/18 and 07/24/18–08/26/18), though the interpretation of such estimates is somewhat ambiguous, and so we refrain from further comments on those counts. None of our nine placebo-period estimates are especially large, and each is statistically insignificant at the 10% level. Taken at face value, our PLR estimates suggest that a 100 percentage point increase in the compensation rate decreased net Republican contributing by 0.4 percentage points among Republican contributing farms, increased net Republican contributing by 0.9 percentage points among Democratic contributing farms, and decreased net Republican contributing by 0.02 percentage points among all other farms. We obtain similarly mild placebo effect sizes for our “net benefit” and “made whole” treatments. This corroborates our claim that our empirical strategy successfully purges the influence of first-order confounding variables.

Finally, in Table FSM13, we replicate our main contribution results from Figure 9 using the OLS estimator. As noted in the main text, this is not the preferred approach because it requires making stringent functional form assumptions without strong *a priori* theory. Nonetheless, we estimate these regressions for the sake of transparency. In three separate regressions, we regress net Republican contributing on a policy outcome treatment measure (net MFP benefit percentile, compensation rate, or made whole status) in addition to a series of controls: quarterly counts of contributions to Republicans (2005–2017), total number of contributions to Republicans prior to 2005 (1979–2004), quarterly counts of contributions to Democrats (2005–2017), total number of contributions to Democrats prior to 2005 (1979–2004), congressional district fixed effects, and indicators for whether the farm had a “distinctly Republican contribution history” or a “distinctly Democratic contribution history” (see Online Appendix A.3 for formal definitions of these categories). Mirroring our PLR model contribution analyses, we add in interaction terms between our treatments and these latter two indicators, so that we allow treatment effect slopes to differ for Republican contributor farms, Democratic contributor farms, and all other farms.

We first note that our results for our subgroup of greatest interest—farms with a distinctly Republican contribution history—are somewhat similar to those obtained via DML estimation of the PLR model. Our OLS estimates imply that, among Republican contributing farms, moving a farm across the IQR of net MFP benefits yielded a 0.02 percentage point increase in net Republican contributing, increasing the compensation rate by 100 percentage points increased net Republican contributing by 0.6 percentage points, and making a farm whole increased net Republican contributing by 0.6 percentage points. These compare to PLR effect sizes of -0.001, -0.001, and -0.001, respectively. However, all six of these estimates are statistically insignificant at the 10% level, and PLR and OLS confidence intervals substantially overlap.

Analyzing farms without a partisan contribution history, we find effect sizes that—as with our PLR results—are generally negligible. Our PLR and OLS estimates diverge to a somewhat greater degree when considering subgroup effects among Democratic contributing farms. However, we find little reason to put stock into the positive results we find here, as we find several reasons to view our PLR null results as far more credible. First, the OLS estimates among Democratic contributor farms are internally inconsistent: the one statistically significant result we uncover (from the “net MFP benefit percentile”) has the opposite sign as obtained with a different treatment specification (“made whole” status). Likewise, the statistically significant results for net MFP benefit percentile fails the type of placebo test that we subject our PLR estimates to in Figure FSM47 above.

Indeed, we find the performance of OLS in depicting (in)credible effect sizes in this analysis to directly mirror our analysis in Further Supplemental Materials F.1. As presented in Table FSM8, we assess the out-of-sample performance of OLS and nonparametric covariate adjustment in a cross validation exercise. We find that nonparametric adjustment for controls consistently outperforms OLS adjustment in all of our main analyses. Moreover, the contrast is especially dramatic in the context of our campaign contribution analyses. Using CatBoost over the OLS specification reported in Table FSM13 below increased out-of-sample  $R^2$  by

223% for predicting net Republican contributing, by 280% for predicting net MFP benefit percentile, by 28% for predicting the MFP compensation rate, and by 86% for predicting MFP made whole status. Perhaps because the information from which to infer latent propensity to make political contributions is so dispersed among the contribution history controls, linear models appear especially bad at eliminating confounding in our contribution analyses. It might thus be unsurprising that our PLR estimates hold a particularly strong advantage over OLS in this setting.

To conclude the discussion of our OLS results, we find that in a couple important contexts (Republican contributing farms, farms lacking a partisan contribution history), our estimates are fairly consistent with the PLR estimates presented in Figure 9. To the extent we find any disagreement, we interpret this analysis as demonstrating the tangible benefits of leveraging a data-driven semiparametric estimator, rather than relying on a restrictive parametric model.

**Table FSM13: Estimated Effects of Improved Policy Outcomes on Net Republican Contributing by Prior Contribution Behavior (Ordinary Least Squares Estimates)**

	(1)	(2)	(3)
Net MFP Benefit Percentile	-0.002* (0.001)	—	—
Net MFP Benefit Percentile × Distinctly Rep Contrib History	0.007 (0.007)	—	—
Net MFP Benefit Percentile × Distinctly Dem Contrib History	0.067*** (0.018)	—	—
MFP as % of Damage	—	-0.002** (0.001)	—
MFP as % of Damage × Distinctly Rep Contrib History	—	0.008** (0.004)	—
MFP as % of Damage × Distinctly Dem Contrib History	—	0.010 (0.009)	—
MFP Made Whole	—	—	-0.002* (0.001)
MFP Made Whole × Distinctly Rep Contrib History	—	—	0.007 (0.007)
MFP Made Whole × Distinctly Dem Contrib History	—	—	-0.005 (0.017)
Distinctly Rep Contrib History	0.035*** (0.005)	0.028*** (0.006)	0.032*** (0.007)
Distinctly Dem Contrib History	-0.178*** (0.010)	-0.160*** (0.014)	-0.142*** (0.016)
Intercept	-0.002*** (0.001)	-0.000 (0.001)	-0.001 (0.001)
House District Fixed Effects	Yes	Yes	Yes
Observations	122,142	122,142	122,142
R-squared	0.199	0.199	0.199

Notes: Robust standard errors in parentheses. Dependent variable is a farm's "net Republican contributing" status, which takes a value of 1 if the farm gives to a Republican candidate or PAC (but not to a Democratic candidate or PAC) between 08/27/2018 and 05/23/2019, -1 if the farm gives to a Democratic candidate or PAC (but not to a Republican candidate or PAC), and 0 otherwise. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$  (two-tailed).

## H.4 Additional Findings and Robustness Checks for MFP Perception Analyses

In the main text (“Farmer Perceptions of the 2018 MFP”), we use Qu et al.’s (2019) survey of Midwestern corn and soybean farmers to argue that farmers’ attitudes toward the MFP strongly reflected their net policy outcomes, as determined by the policy’s treatment of their 2018 crop portfolios. We conduct several analyses to demonstrate that our results are not artifacts of our four-point measure of MFP approval, our three standard policy outcome treatment measures, or systematic longstanding differences between corn-heavy farms and soybean-heavy farms.

We first examine an alternative measure of policy outcomes: crop portfolios. Since only 4% of respondents reported planting something other than corn or soybeans in 2018 (and “other crops” accounted for less than 2% of total reported acreage), a very simple yet reasonable measure of these farmers’ portfolio is their soybean share of their corn/soybean planted acreage. As soybean losses were compensated much more generously than corn losses, we would expect soybean share of 2018 acreage to positively impact respondents’ views of MFP helpfulness.

As reported in Table FSM14, we regress attitudes toward the MFP (on the four-point response scale) on (a) soybean share of planted 2018 acreage; and (b) expected 2018/2019 marketing year revenue for corn and soybeans. Since the revenue measures are highly skewed but also feature zeroes, we follow the typical approach in applied econometrics of applying an inverse hyperbolic sine (i.e.,  $\text{arcsinh}$ ) transformation (Bellemare and Wichman, 2020). An more-traditional approach would obviate the creation of missing data via normalization by adding one to all observations and then taking the natural logarithm. This technique is problematic because the choice of the added constant (e.g., 1) is arbitrary but can significantly affect inferences. In contrast, if we apply the  $\text{arcsinh}$  function to all observations and use the procedure recommended by Aihounton and Henningsen (2020) to pick an appropriate scale, we can obtain a functional form in a data-driven manner. Using this method, we render revenue

**Table FSM14: Farmers with Soybean-Heavy 2018 Portfolios Viewed MFP as More Helpful**

	Outcome: 4-Point Scale of MFP Helpfulness			
	(1)	(2)	(3)	(4)
Soybean Share of 2018 Acreage	1.312*** (0.251)	1.246*** (0.244)	—	—
arcsinh(100K of Corn Revenue) (2018 Harvest at Forecast Price)	—	—	-0.199*** (0.063)	-0.274*** (0.089)
arcsinh(100K of Soy Revenue) (2018 Harvest at Forecast Price)	—	—	0.447*** (0.067)	0.358*** (0.095)
Log(Total Acres 2013-2017)	—	0.163*** (0.060)	—	0.112 (0.133)
Female	—	-0.281 (0.222)	—	-0.301 (0.221)
Education (5-Point Scale)	—	0.012 (0.042)	—	0.013 (0.042)
Age	—	-0.013*** (0.004)	—	-0.013*** (0.004)
Off-Farm Income	—	0.022 (0.085)	—	0.031 (0.085)
Raised Hogs	—	0.089 (0.139)	—	0.118 (0.138)
Dairy Cattle	—	-0.494** (0.249)	—	-0.448** (0.221)
Beef Cattle	—	-0.076 (0.086)	—	-0.068 (0.086)
Raised Poultry	—	0.344 (0.233)	—	0.353 (0.231)
Other Livestock	—	-0.070 (0.185)	—	-0.086 (0.184)
Intercept	1.105*** (0.120)	0.679 (0.526)	1.366*** (0.116)	1.615** (0.795)
State Fixed-Effects	No	Yes	No	Yes
Observations	575	575	575	575
R-squared	0.045	0.117	0.077	0.126

Notes: 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) and (4) include indicator variables denoting the missing status of each of these fields. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$  (two-tailed).



in units of \$100,000 and then apply the arcsinh transformation.<sup>22</sup> In columns (1) and (3), we report regression coefficients without any controls. In columns (2) and (4), we add in controls for longstanding farm size, demographics, whether farms raised particular types of livestock, and each respondent’s state. We find large and statistically significant differences attributable to variation in crop portfolios. Taking the bivariate regression in column (1) at face value, a farmer who planted a 75-25 soybean-corn split in 2018 found the 2018 MFP to be 0.7 points more helpful (on a four-point scale) than a farmer who planted a 25-75 split ( $p < .01$ ). The coefficients obtained for corn and soybean revenue rendered separately, presented in columns (3) and (4), suggest that farmers’ evaluations of the helpfulness of the MFP were intrinsically linked to damages sustained in the trade war. While farmers were paid a (nominally) positive rate on their corn harvest, consistent with the negative *net* compensation levels noted in Table 1, higher levels of corn production were associated with strictly worse perceptions of MFP helpfulness.

In Tables FSM15 and FSM16, we replicate the analyses from Tables FSM14 and 2 using an alternative, binary measure of MFP attitudes that takes a value of 1 if the respondent found the MFP to be “quite helpful” or “very helpful” and a value of 0 if they selected “not at all helpful”, “somewhat helpful”, or “not sure.” Qualitatively, all results resemble their analogues from the main text based on a four-point outcome scale. In particular, the regressions depicted in columns (1) and (2) of Table FSM15 imply that farmers who planted a 75-25 soybean-corn split were roughly 30 percentage points more likely to show enthusiasm for the MFP than those who planted a 25-75 split.

In Table FSM17, we replicate analyses from Tables FSM14 and 2 with added controls for lagged versions of the regressors of interest using 2013–2017 planting decisions instead of 2018 planting decisions. For example, in the estimates we present in column (1), we estimate

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<sup>22</sup>The inverse hyperbolic sine transformation is not invariant to the unit of measurement of the transformed variable. Guided by the results of Monte Carlo simulations, Aihounton and Henningsen (2020) recommend repeating the regression analysis with multiple units of measurement and selecting the unit that maximizes  $R^2$ . We estimate the model in column (3) of Table FSM14 denoting revenue in units of \$1, \$100, \$1,000, \$10,000, \$100,000, and \$1 million, and find \$100,000 to be optimal according to this criterion.

**Table FSM15: Farmers with Soybean-Heavy 2018 Portfolios Viewed MFP as More Helpful (Binary Measure of MFP Helpfulness)**

	Outcome: MFP “Quite”/“Very” Helpful			
	(1)	(2)	(3)	(4)
Soybean Share of 2018 Acreage	0.622*** (0.114)	0.572*** (0.116)	—	—
arcsinh(100K of Corn Revenue) (2018 Harvest at Forecast Price)	—	—	-0.096*** (0.030)	-0.110** (0.051)
arcsinh(100K of Soy Revenue) (2018 Harvest at Forecast Price)	—	—	0.193*** (0.029)	0.164*** (0.042)
Log(Total Acres 2013-2017)	—	0.053* (0.029)	—	0.022 (0.067)
Female	—	-0.143 (0.105)	—	-0.151 (0.105)
Education (5-Point Scale)	—	-0.005 (0.022)	—	-0.006 (0.022)
Age	—	-0.005*** (0.002)	—	-0.005*** (0.002)
Off-Farm Income	—	0.054 (0.042)	—	0.058 (0.042)
Raised Hogs	—	0.046 (0.076)	—	0.055 (0.075)
Dairy Cattle	—	-0.133 (0.190)	—	-0.108 (0.182)
Beef Cattle	—	-0.011 (0.044)	—	-0.006 (0.044)
Raised Poultry	—	0.169 (0.132)	—	0.178 (0.131)
Other Livestock	—	-0.029 (0.104)	—	-0.036 (0.103)
Intercept	0.167*** (0.053)	0.102 (0.263)	0.326*** (0.062)	0.553 (0.393)
State Fixed-Effects	No	Yes	No	Yes
Observations	621	621	621	621
R-squared	0.037	0.086	0.051	0.090

Notes: Robust standard errors in parentheses. Dependent variable is an indicator that takes a value of 1 if the respondent found the MFP to be “quite helpful” or “very helpful” and a value of 0 if they selected “not at all helpful”, “somewhat helpful”, or “not sure.” Of the 621 respondents in this sample, 21 did not report gender, education, age, or whether they earned off-farm income. We imputed the 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).

**Table FSM16: Farmers with Better Policy Outcomes Viewed MFP as More Helpful (Binary Measure of MFP Helpfulness)**

	Outcome: MFP Perceived as “Quite” or “Very” Helpful					
	(1)	(2)	(3)	(4)	(5)	(6)
Net MFP Benefit Percentile	0.322*** (0.067)	0.268*** (0.073)	—	—	—	—
MFP as % of Damage	—	—	0.281*** (0.055)	0.271*** (0.054)	—	—
MFP Made Whole	—	—	—	—	0.143*** (0.053)	0.158*** (0.052)
Log(Total Acres 2013-2017)	—	0.028 (0.031)	—	0.071** (0.029)	—	0.070** (0.029)
Female	—	-0.146 (0.104)	—	-0.151 (0.105)	—	-0.156 (0.106)
Education (5-Point Scale)	—	-0.004 (0.022)	—	-0.001 (0.022)	—	0.002 (0.022)
Age	—	-0.005*** (0.002)	—	-0.005*** (0.002)	—	-0.005*** (0.002)
Off-Farm Income	—	0.057 (0.043)	—	0.053 (0.043)	—	0.055 (0.043)
Raised Hogs	—	0.036 (0.078)	—	0.039 (0.077)	—	0.024 (0.077)
Dairy Cattle	—	-0.115 (0.194)	—	-0.106 (0.184)	—	-0.109 (0.190)
Beef Cattle	—	-0.003 (0.044)	—	-0.006 (0.044)	—	-0.008 (0.045)
Raised Poultry	—	0.195 (0.130)	—	0.173 (0.131)	—	0.197 (0.128)
Other Livestock	—	-0.061 (0.103)	—	-0.042 (0.103)	—	-0.040 (0.103)
Intercept	0.289*** (0.039)	0.435 (0.269)	0.118* (0.066)	-0.086 (0.270)	0.330*** (0.048)	0.125 (0.270)
State Fixed-Effects	No	Yes	No	Yes	No	Yes
Observations	621	621	621	621	621	621
R-squared	0.035	0.077	0.030	0.083	0.011	0.069

Notes: Robust standard errors in parentheses. Dependent variable is an indicator that takes a value of 1 if the respondent found the MFP to be “quite helpful” or “very helpful” and a value of 0 if they selected “not at all helpful”, “somewhat helpful”, or “not sure.” Of the 621 respondents in this sample, 21 did not report gender, education, age, or whether they earned off-farm income. We imputed the 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).

**Table FSM17: Effect of More Favorable MFP Treatment on MFP Perceptions is Robust to Controlling for Past Crop Portfolios**

	Outcome: Four-Point Scale of MFP Helpfulness				
	(1)	(2)	(3)	(4)	(5)
Soybean Share of 2018 Acreage	1.185*** (0.345)	—	—	—	—
Soybean Share of 2013-2017 Acreage	0.093 (0.391)	—	—	—	—
arcsinh(100K of Corn Revenue) (2018 Harvest at Forecast Price)	—	-0.260* (0.136)	—	—	—
arcsinh(100K of Soy Revenue) (2018 Harvest at Forecast Price)	—	0.361** (0.177)	—	—	—
arcsinh(100K of Corn Revenue) (2013-2017 Harvest at Forecast Price)	—	-0.023 (0.178)	—	—	—
arcsinh(100K of Soy Revenue) (2013-2017 Harvest at Forecast Price)	—	-0.004 (0.198)	—	—	—
Net MFP Benefit Percentile (2018 Production)	—	—	0.829*** (0.229)	—	—
Hypothetical NB Percentile (2013-2017 Production)	—	—	-0.160 (0.234)	—	—
MFP as % of Damage (2018 Production)	—	—	—	0.541*** (0.190)	—
Hypothetical % of Damage (2013-2017 Production)	—	—	—	0.173 (0.206)	—
MFP Made Whole (2018 Production)	—	—	—	—	0.296* (0.178)
Hypothetical Made Whole (2013-2017 Production)	—	—	—	—	0.203 (0.194)
Log(Total Acres 2013-2017)	0.164*** (0.060)	0.121 (0.162)	0.095 (0.064)	0.215*** (0.060)	0.212*** (0.061)
Intercept	0.662 (0.531)	1.573* (0.907)	1.462*** (0.538)	0.037 (0.552)	0.484 (0.545)
Demographic Controls	Yes	Yes	Yes	Yes	Yes
Farm Attribute Controls	Yes	Yes	Yes	Yes	Yes
State Fixed-Effects	Yes	Yes	Yes	Yes	Yes
Observations	575	575	575	575	575
R-squared	0.117	0.126	0.119	0.121	0.109

Notes: 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. Of the 575 respondents in this sample, 19 did not report gender, education, age, or whether they earned off-farm income. We imputed the missing values using sample means. Accordingly, each model includes indicator variables denoting the missing status of each of these fields. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$  (two-tailed).

the association between 2018 soybean share of acreage while controlling for 2013–2017 share of acreage. If the positive association between 2018 acreage and MFP support documented in columns (1) and (2) of Table FSM14 was actually attributable to systematic differences between corn-heavy and soy-heavy farms, we would expect including this control to substantially diminish the coefficient on 2018 soybean share. However, the resulting coefficient (1.19) is nearly identical to the initial coefficient (1.25) and remains statistically significant at the 1% level, while the coefficient on 2013–2017 acreage (0.09) is both substantively small and statistically insignificant.

Such an analysis is feasible because, while planting decisions are positively correlated over time, farmers typically rotate crop acreage from year-to-year and have a variety of idiosyncratic reasons to tweak planting ratios in a given year. For example, inclement weather at the beginning of the growing season often delays planting, at which point it can become profitable to shift planned corn acreage to soybeans. Likewise, shifts in fertilizer costs, increased need for livestock grazing, and outbreaks of weeds can also cause large but transitory shifts in planting decisions.<sup>23</sup> Given such year-to-year fluctuations, we use 2013–2017 acreage to construct lagged “treatments” corresponding to our measures of soybean share of acreage, corn and soybean revenue, net MFP benefits, MFP compensation rates, and farms’ “made whole” status.

We likewise replicate model (4) from Table FSM14 and models (2), (4), and (6) from Table 2 with analogous “lagged treatments” added in as controls in each case. These remaining four treatment specifications fare similarly with added lagged regressors. The magnitudes on corn/soy revenue are very similar with the added controls, while the coefficient on net MFP benefits grows a bit larger, and the coefficients on MFP benefits as a share of losses and the indicator for whether farmers are made whole each fall a bit. In each case, the coefficient on 2018 disposition is statistically significant at the 10% level, while in no case is the coefficient on simulated 2013–2017 disposition significant at this level. Hence, our results are not due

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<sup>23</sup>In particular, farmers may shift from corn to soybeans when the price of nitrogen spikes if they have low stocks of fertilizer. Conversely, there have emerged high-profile cases of weed resistance to soybean-safe herbicides in recent years, leading some Midwestern farmers to increase corn acreage for a year while they treat their fields. Likewise, leftover corn stalks provide an advantage in terms of post-harvest grazing for cattle.

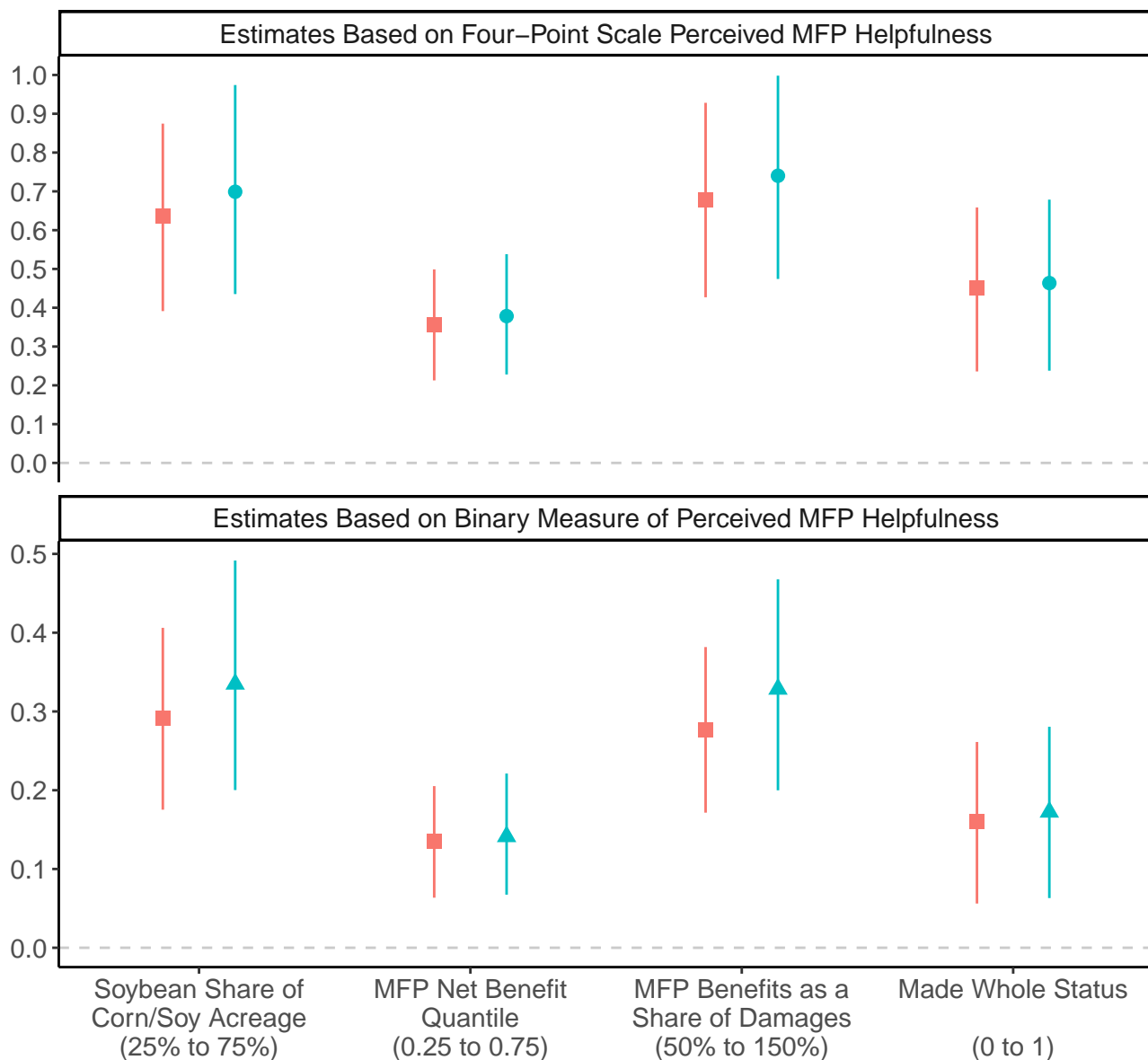
to systematic differences between corn and soybean growers. We conclude that, while the variation we study is cross-sectional, our analyses nonetheless credibly document a strong causal connection between policy outcomes and policy attitudes.

Finally, we demonstrate that the substantive inferences that we draw in this section are not artifacts of estimating a linear model with an outcome that is either a discrete ordinal variable (the four-point scale of perceived MFP helpfulness) or a binary variable (the binary measure of perceived MFP helpfulness). We replicate our analyses presented in Tables FSM14, 2, FSM15 and FSM16 using ordered logistic regression to model the four-point outcome and logistic regression to model the binary outcome. Since logistic regression coefficients are not directly comparable to OLS coefficients, we instead replicate the counterfactual estimates from these prior analyses using predictions from our new models. That is, we compare OLS and logistic regression predictions for the difference in MFP perceptions one would expect to be associated with (a) a 50% shift in soybean share of corn/soybean acreage, (b) moving across the IQR of net MFP benefits, (c) a 100 percentage point increase in the MFP compensation rate, and (d) making a farmers' losses whole through the MFP. We construct confidence intervals for each of these differences by evaluating the distribution of these estimates across 10,000 bootstrap samples of the survey dataset. As shown in Figure FSM48, the (ordered) logistic regression effect sizes are strikingly similar to the OLS effect sizes in all cases. In fact, the point estimates are slightly larger in each of the eight comparisons. We conclude that the more interpretable OLS specifications we present are fully sufficient for drawing accurate inferences regarding the scale of policy outcomes' effects of farmers' perceptions of MFP helpfulness.

**Figure FSM48:** OLS and Logistic Regression Estimates Yield Similar Substantive Inferences Regarding the Effect of Improved MFP Dispositions on Perceived MFP Helpfulness

Implied increase in perceived MFP helpfulness across specified range of MFP dispositions, according to estimates obtained via:

■ OLS Regression    ● Ordered Logistic Regression    ▲ Logistic Regression



Notes: Formally, given an MFP perception  $Y_i$ , an MFP disposition “treatment”  $B_i$ , a vector of controls  $X_i$ , an estimated OLS or (ordered) logistic conditional expectation function  $\hat{E}[Y_i | B_i, X_i]$ , and specified treatment dispositions  $B_{\text{low}}$  and  $B_{\text{high}}$ , we estimate  $\theta \equiv \hat{E}[Y_i | B_i = B_{\text{high}}, X_i = \bar{X}] - \hat{E}[Y_i | B_i = B_{\text{low}}, X_i = \bar{X}]$ . We obtain 95% confidence intervals for  $\hat{\theta}$  via bootstrapping. To mitigate convergence issues during bootstrapping, we use a slightly abbreviated list of controls compared to those presented in Tables FSM14, 2, FSM15, and FSM16: controls for animal production are specified using indicators for hogs, beef, and any other animal products. We also omit indicators for the small handful of farms for which we imputed gender, age, or off-farm income status.

## H.5 Survey Evidence Regarding Tariff Approval

We complement our analysis of farmers’ perceptions of MFP helpfulness with an investigation of their attitudes towards Trump’s decision to increase tariffs on imports from China. In Qu et al.’s (2019) survey of Midwestern farmers, the authors ask respondents about their views on the 2018 US-China trade war in a survey section that begins with the following preamble:

This section seeks your opinion specifically about the trade disruption between the US and China. By “trade disruption,” we mean the changes in trade policy between the two countries that resulted in increased tariffs on products imported to the US and products exported from the US to China.

The authors then ask respondents “To what extent do you support or oppose raising tariffs on products imported from China?” Among the 602 members of our analysis sample who responded to this item, opinions appear to be mixed: 18% said they “strongly oppose” the tariffs, 12% “somewhat” opposed them, 10% replied “neither oppose nor support,” 38% replied “somewhat support,” and 21% replied “strongly support.” However, this variation in responses appeared to be almost completely orthogonal to farmers’ crop portfolios and, by extension, their net policy outcomes within 2018. As presented in Tables FSM18 and FSM19, we gauged associations between this five-point scale and farmers policy outcomes (as measured in the previous sections). In no case did we obtain a statistically significant association between inferred policy outcomes (or crop portfolios) and support for tariff escalation. As noted in the main text, this suggests that farmers’ recognition of beneficial policy outcomes may have not translated to changes in policy preferences, and may therefore suggest a possible explanation by which we obtain null results for our analyses of farmers’ political behavior.



**Table FSM18: Soybean Content of Portfolios Unrelated to Support for Tariff Escalation**

	Outcome: 5-Point Scale of Tariff Approval			
	(1)	(2)	(3)	(4)
Soybean Share of 2018 Acreage	-0.131 (0.373)	-0.059 (0.381)	—	—
arcsinh(100K of Corn Revenue) (2018 Prod at Forecast Price)	—	—	0.026 (0.091)	0.243 (0.159)
arcsinh(100K of Soy Revenue) (2018 Prod at Forecast Price)	—	—	-0.054 (0.098)	0.142 (0.150)
Log(Total Acres 2013-2017)	—	-0.129 (0.091)	—	-0.461** (0.232)
Female	—	-0.247 (0.390)	—	-0.272 (0.391)
Education (5-Point Scale)	—	0.024 (0.064)	—	0.028 (0.064)
Age	—	-0.014*** (0.005)	—	-0.014*** (0.005)
Off-Farm Income	—	0.147 (0.127)	—	0.157 (0.127)
Raised Hogs	—	0.096 (0.215)	—	0.095 (0.215)
Dairy Cattle	—	0.157 (0.511)	—	0.149 (0.525)
Beef Cattle	—	-0.036 (0.128)	—	-0.029 (0.129)
Raised Poultry	—	0.491 (0.358)	—	0.477 (0.360)
Other Livestock	—	-0.226 (0.337)	—	-0.229 (0.335)
Intercept	2.383*** (0.181)	3.752*** (0.827)	2.361*** (0.185)	5.344*** (1.296)
Observations	602	602	602	602
R-squared	0.000	0.034	0.001	0.038

Notes: Robust standard errors in parentheses. Dependent variable is a five-point scale indicating respondents' level of support for raising tariffs on products imported from China ("strongly oppose", "somewhat oppose", "neither", "somewhat support", or "strongly support"). Of the 602 respondents in this sample, 26 did not report gender, education, age, or whether they earned off-farm income. We imputed the 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).

**Table FSM19: Farmers' MFP Policy Outcomes were Unrelated to Support for Tariff Escalation**

	Outcome: 5-Point Scale of Tariff Approval					
	(1)	(2)	(3)	(4)	(5)	(6)
Net MFP Benefit Percentile (2018 Production)	0.127 (0.201)	0.183 (0.222)	—	—	—	—
MFP as % of Damage (2018 Production)	—	—	0.003 (0.195)	-0.001 (0.198)	—	—
MFP Made Whole (2018 Production)	—	—	—	—	-0.011 (0.162)	-0.002 (0.164)
Log(Total Acres 2013-2017)	—	-0.151 (0.095)	—	-0.130 (0.092)	—	-0.130 (0.092)
Female	—	-0.249 (0.384)	—	-0.247 (0.390)	—	-0.247 (0.389)
Education (5-Point Scale)	—	0.025 (0.064)	—	0.024 (0.064)	—	0.024 (0.064)
Age	—	-0.014*** (0.005)	—	-0.014*** (0.005)	—	-0.014*** (0.005)
Off-Farm Income	—	0.149 (0.126)	—	0.147 (0.127)	—	0.147 (0.126)
Raised Hogs	—	0.123 (0.218)	—	0.100 (0.216)	—	0.100 (0.216)
Dairy Cattle	—	0.156 (0.518)	—	0.155 (0.513)	—	0.155 (0.512)
Beef Cattle	—	-0.023 (0.129)	—	-0.036 (0.129)	—	-0.036 (0.129)
Raised Poultry	—	0.474 (0.364)	—	0.486 (0.359)	—	0.486 (0.359)
Other Livestock	—	-0.230 (0.337)	—	-0.224 (0.337)	—	-0.224 (0.337)
Intercept	2.260*** (0.116)	3.790*** (0.812)	2.320*** (0.241)	3.730*** (0.872)	2.333*** (0.150)	3.730*** (0.834)
Observations	602	602	602	602	602	602
R-squared	0.001	0.035	0.000	0.034	0.000	0.034

Notes: Robust standard errors in parentheses. Dependent variable is a five-point scale indicating respondents' level of support for raising tariffs on products imported from China ("strongly oppose", "somewhat oppose", "neither", "somewhat support", or "strongly support"). Of the 602 respondents in this sample, 26 did not report gender, education, age, or whether they earned off-farm income. We imputed the 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).

## H.6 Marginal Effect Heterogeneity by Made Whole Status

We consider the prospect that the effects on political behavior that we estimate may be shaped by voters’ sense of loss aversion towards policy outcomes. It is theoretically possible that voters may have viewed a marginal increase in MFP benefits differently if they were already at a point of being made whole. That is, under loss aversion, a policy improvement that decreases net losses might be significantly more salient than a policy improvement increasing net benefits. Since the vast majority of our sample was made whole by the 2018 MFP, such a conjectured relationship might *ex-ante* sound like a plausible contributor to the null results we observe with respect to voter behavior.

To gauge the plausibility of this explanation, we re-estimate the effects of improved policy outcomes on MFP attitudes and voter turnout with a piecewise linear specification that allows for separate lines to be fit on each side of the “made whole” thresholds in our main continuous policy outcome measures (net MFP benefit percentile and MFP compensation rate). For example, instead of regressing our four-point scale of perceived MFP helpfulness on net MFP benefit percentile (as we do in Table 2), in Table FSM20 we present a regression of this four-point scale on net MFP benefit percentile, an indicator for whether a farm is made whole, and an interaction between these two variables. We find no statistically significant difference in the marginal effects of MFP net benefits or a farm’s MFP compensation rate across the two sides of the made whole threshold. In part, this may be because the coefficients on the interaction terms are very noisily estimated. However, three of the four estimated interaction term coefficients in Table FSM20 are actually positive, which suggests that our survey data cannot provide any corroboration of the conjecture that farmers’ loss aversion drives our null results.

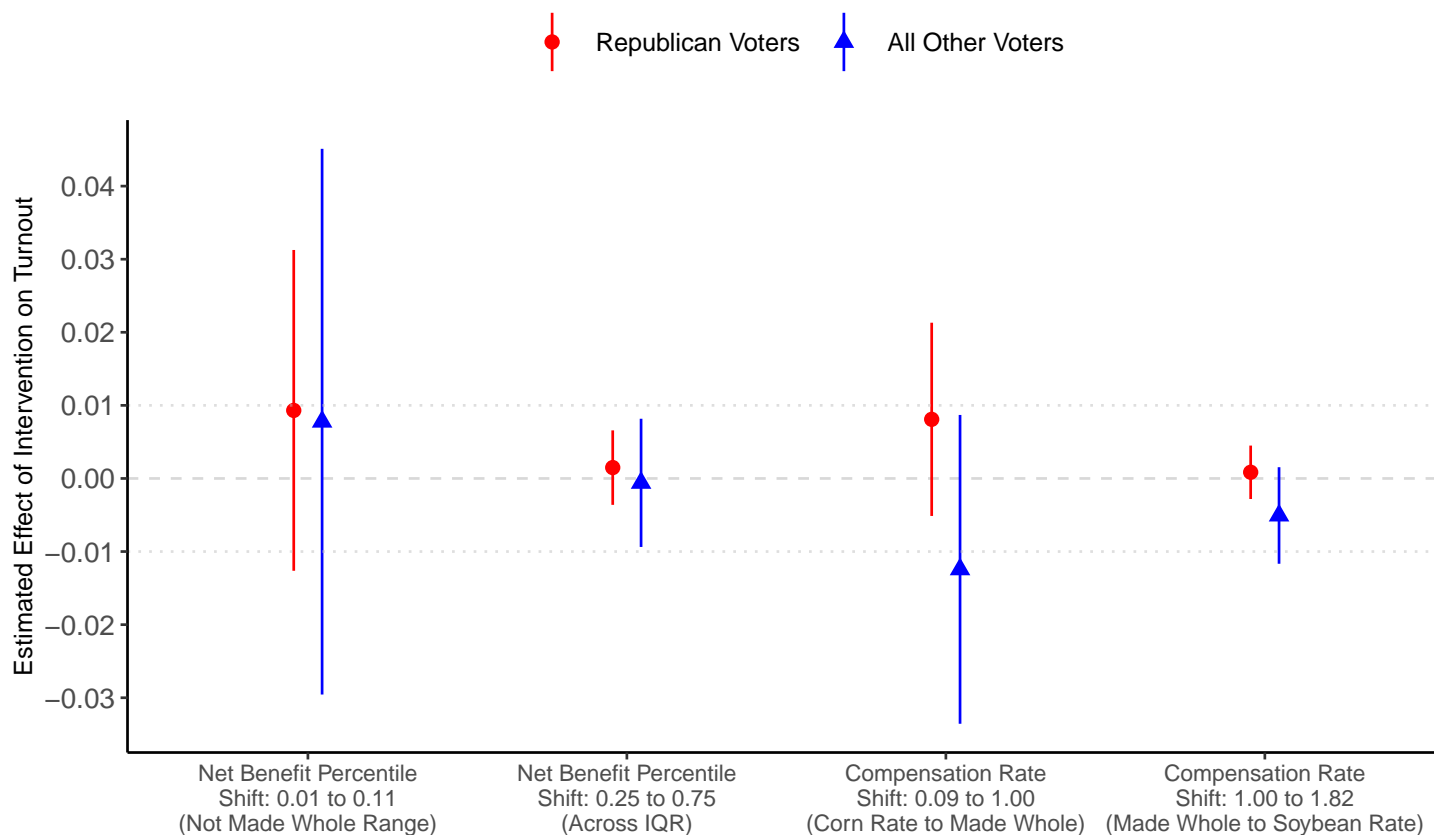
We conduct an analogous exercise with our administrative data on farmers’ 2018 turnout. Fitting separate lines on each side of the made whole boundary, we present these heterogeneous marginal effect estimates in Figure FSM49. To facilitate substantive interpretation, we calculate and present four substantive policy counterfactuals: given our piecewise linear effect

**Table FSM20: Heterogeneity by Made Whole Status in Association between Policy Outcomes and Perceived MFP Helpfulness**

	Outcome: 4-Point Scale of MFP Helpfulness			
	(1)	(2)	(3)	(4)
MFP Made Whole (Indicator)	-0.211 (0.241)	0.217 (0.292)	-0.237 (0.458)	-0.313 (0.445)
Net MFP Benefit Percentile	-0.983 (2.306)	1.343 (2.599)	—	—
Net MFP Benefit Percentile $\times$ Made Whole	1.869 (2.313)	-0.793 (2.662)	—	—
MFP as % of Damage	—	—	0.688** (0.296)	0.493* (0.291)
MFP as % of Damage $\times$ Made Whole	—	—	0.120 (0.431)	0.338 (0.423)
Log(Total Acres 2013-2017)	—	0.129* (0.076)	—	0.220*** (0.062)
Female	—	-0.285 (0.214)	—	-0.284 (0.219)
Education (5-Point Scale)	—	0.023 (0.042)	—	0.020 (0.043)
Age	—	-0.013*** (0.004)	—	-0.012*** (0.004)
Off-Farm Income	—	0.024 (0.086)	—	0.017 (0.085)
Raised Hogs	—	0.086 (0.139)	—	0.085 (0.139)
Dairy Cattle	—	-0.443* (0.266)	—	-0.452* (0.233)
Beef Cattle	—	-0.052 (0.086)	—	-0.059 (0.086)
Raised Poultry	—	0.397* (0.237)	—	0.343 (0.236)
Other Livestock	—	-0.136 (0.189)	—	-0.100 (0.185)
Intercept	1.465*** (0.216)	1.074 (0.758)	0.958*** (0.210)	0.155 (0.554)
Observations	575	575	575	575
R-squared	0.065	0.120	0.041	0.121

Notes: 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. Of the 575 respondents in this sample, 19 did not report gender, education, age, or whether they earned off-farm income. We imputed the missing values using sample means. Accordingly, each model includes indicator variables denoting the missing status of each of these fields. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$  (two-tailed).

**Figure FSM49:** Substantive Turnout Effect Sizes for Policy Counterfactuals Based on a Piecewise Linear Specification with Heterogeneous Marginal Effects by Made Whole Status



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 Further Supplemental Materials Section I

estimates, how much would turnout be increased by (a) moving a farmer from the 1st percentile of net benefits (-\$7,340) to the 11th percentile of net benefits (+\$11), versus (b) moving a farmer across the interquartile range of net benefits (\$391 to \$6,110)? Likewise, how much more turnout could be induced by (c) increasing a farm’s compensation rate from 9% of trade war losses (the corn-only-portfolio rate) to 100% of trade war losses, than by (d) increasing a farm’s compensation rate from 100% of trade war losses to 182% (the soybean-only-portfolio rate)? The estimates of these effects presented in Figure FSM49 provide no conclusive evidence for the loss aversion hypothesis. While Republican marginal effects are estimated to be slightly larger on the “not made whole” side of the treatment distribution, even among Republicans

not made whole by the MFP, the estimated marginal impacts of improved policy outcomes are not statistically significant. We conclude that, while we do not have the statistical power to conclusively rule out loss aversion as a contributor to our null result, our findings are certainly not simply an artifact of such a mechanism.

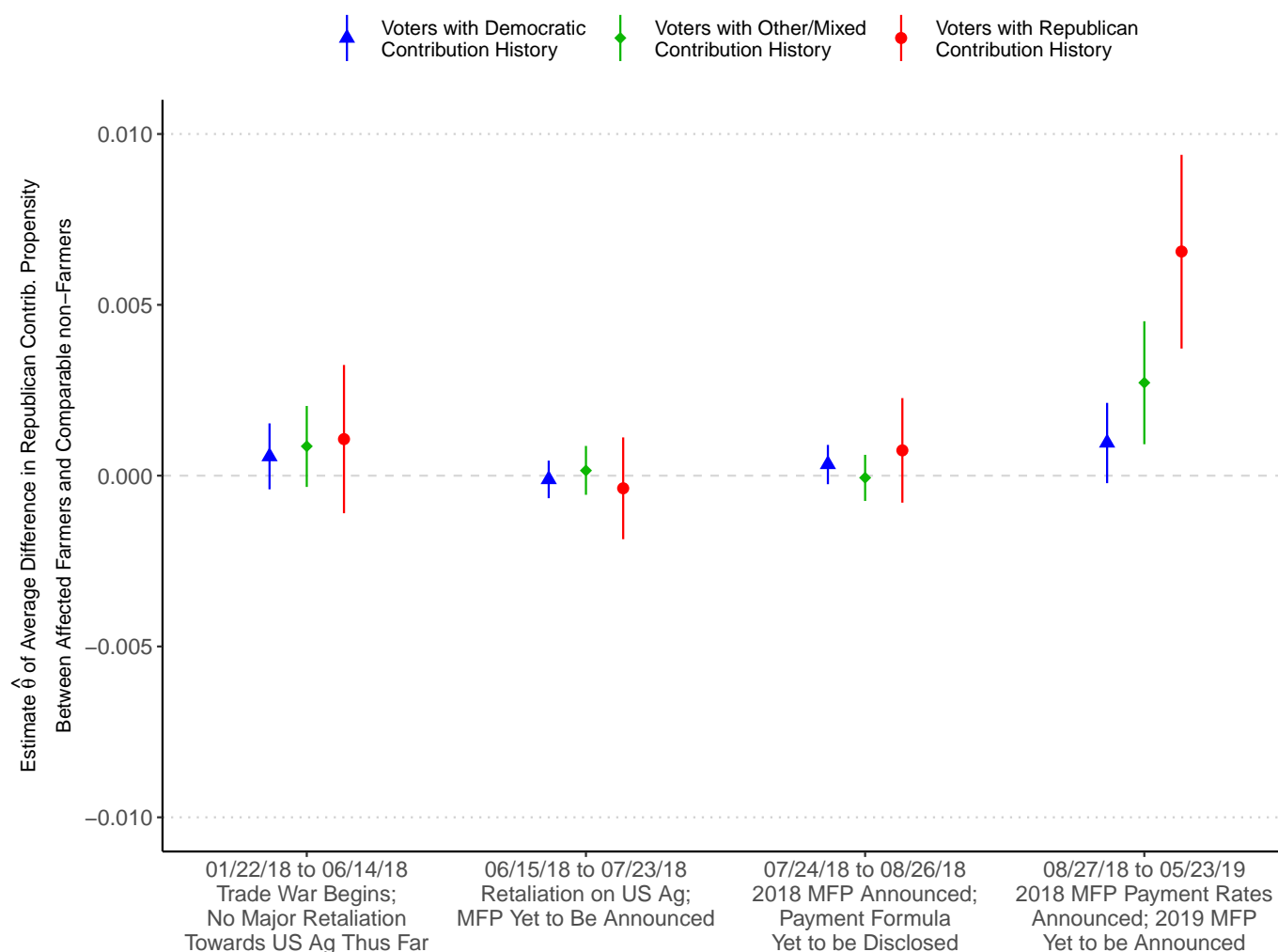
## **H.7 Robustness Checks for Broader Sample Contribution Analyses**

We conclude our series of robustness checks and supplemental analyses by supplementing our large-scale analysis of farmer contribution behavior in the main text. In Figure 12 (within “Broader Effects of the Trade War and MFP on Farmers’ Political Engagement”), we zoom out from individual farms’ policy outcomes to inquire what effects the broader political salience of the trade war and MFP might have had on farmers’ political contributions. Analyzing nearly 8 million voters in L2’s February 2018 voter file snapshot that have been linked to pre-2018 contributions in DIME, we compare the net Republican contributing of 85,012 voters linked to farms producing affected commodities with that of 7,850,104 voter-contributors not linked to any farm in our USDA database of farm program records. Using a PLR model to adjust for pre-treatment covariates, we find that Republican contributors increased their net Republican contributing by 0.8 percentage points relative to comparable non-farmers after the MFP payment formula was announced and enrollments began. However, we do not find similarly strong immediate effects from the announcement of retaliatory tariffs on US agriculture several months earlier.

In this section, we provide context to these “net Republican contributing” effects by estimating analogous effects for alternative contribution outcomes. In Figures FSM50 and FSM51, we separately estimate effects on voter-contributors’ propensity to contribute to Republicans and Democrats, respectively. These results largely mirror those presented in Figure 12. In particular, we find that the 0.8 percentage point increase in net Republican contributing among Republican contributors can be decomposed into a 0.7 percentage point increase in likelihood to contribute to Republican candidates, and a 0.1 percentage point decrease in the likelihood to contribute to Democratic candidates. Likewise, the 0.4 percentage point effect on net Re-

publican contributing we find for voter-contributors without a distinct partisan contribution history mirrors a 0.3 percentage point increase in the likelihood to contribute to Republicans alongside a 0.2 percentage point decrease in the likelihood of contributing to Democrats. Altogether, the results in Figures FSM50 and FSM51 corroborate our main findings in Figure 12.

**Figure FSM50:** Overall Impact of Increased Policy Salience on Contributions: DML Estimates of Difference in Propensity to Contribute to Republicans between Affected Farmers and Other Contributors



Notes: Point estimates are depicted with 95% confidence intervals. Outcome is a binary variable that takes a value of 1 if the voter made an itemized contribution to any Republican Party candidate or PAC in the specified period. To view these results in table form, see Further Supplemental Materials Section I.

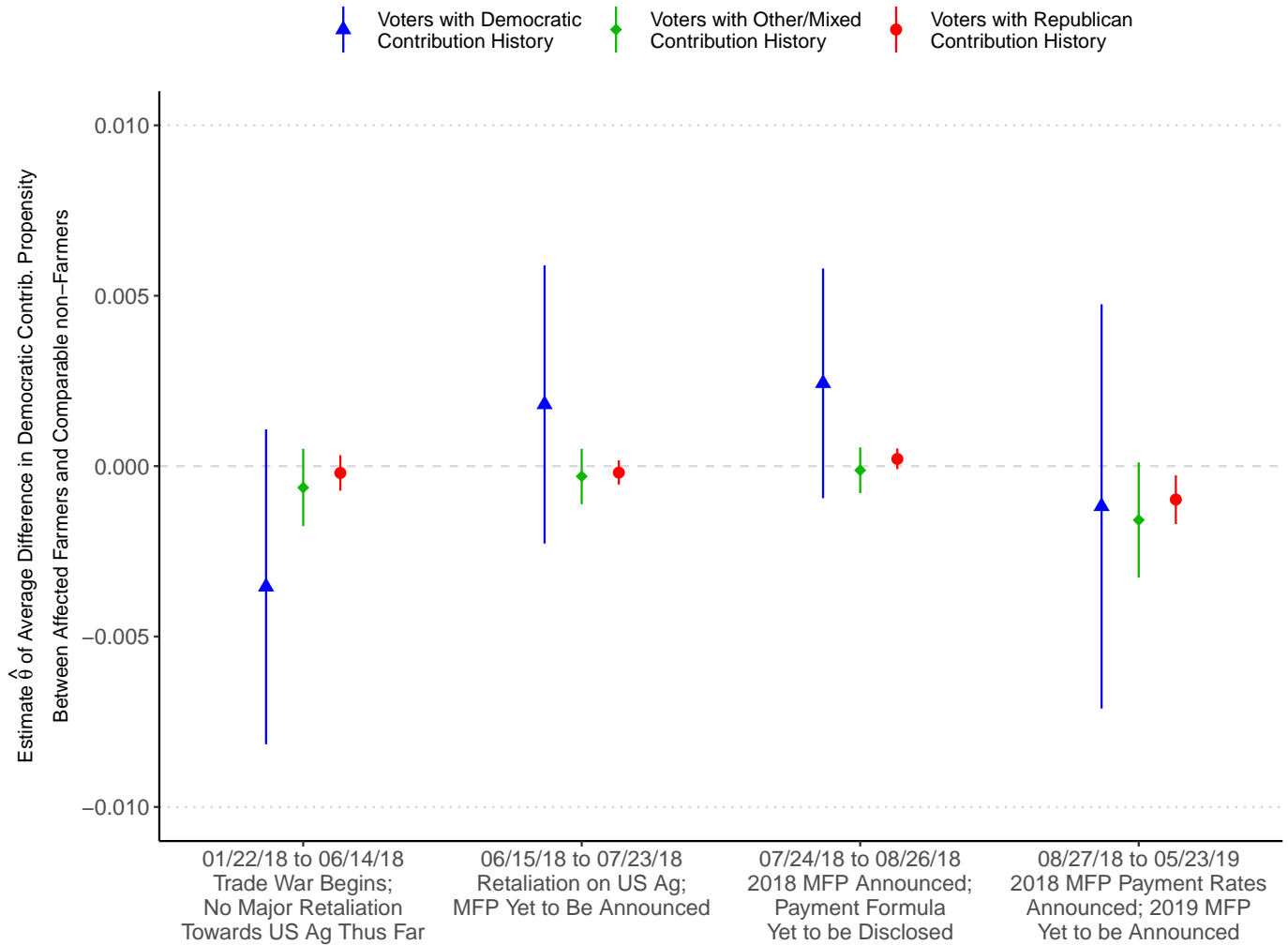
In the analysis presented in Figure FSM52, we inquire what share of this boost in Re-

publican Party contributions made it to Trump’s reelection campaign or closely affiliated PACs. The answer appears to be “none of it.” Though not statistically significant, we find that Republican contributors were 0.1 percentage points less likely to contribute to Trump in the months following the rollout of the MFP. Indeed, estimated effects on the likelihood of contributing to Trump are negative for voters with Republican, Democratic, and nonpartisan contribution histories alike. This is consistent with our previous claim that, since 2018 was not a Presidential election year, farmers likely found it reasonable to reward or punish Trump’s copartisans for their evaluations of his performance.

Finally, we estimate the effect of the MFP rollout on farmers’ net Republican contribution count (that is, the *number* of contributions to Republicans minus the number of contributions to Democrats). Unlike our analysis using our three-point “net Republican contributing” outcome, we find no positive effects on contribution counts. This suggests that the boost in Republican contributing activity occurred mostly on the extensive margin rather than the intensive margin. That is, Republicans who otherwise would have not contributed in the focal period were activated, but active contributors who would have donated regardless do not appear to have been spurred to increase the number of contributions they made. This result raises an interesting question about how policy mobilizes a base of partisan contributors, and may be fertile ground for future research on political contribution behavior.

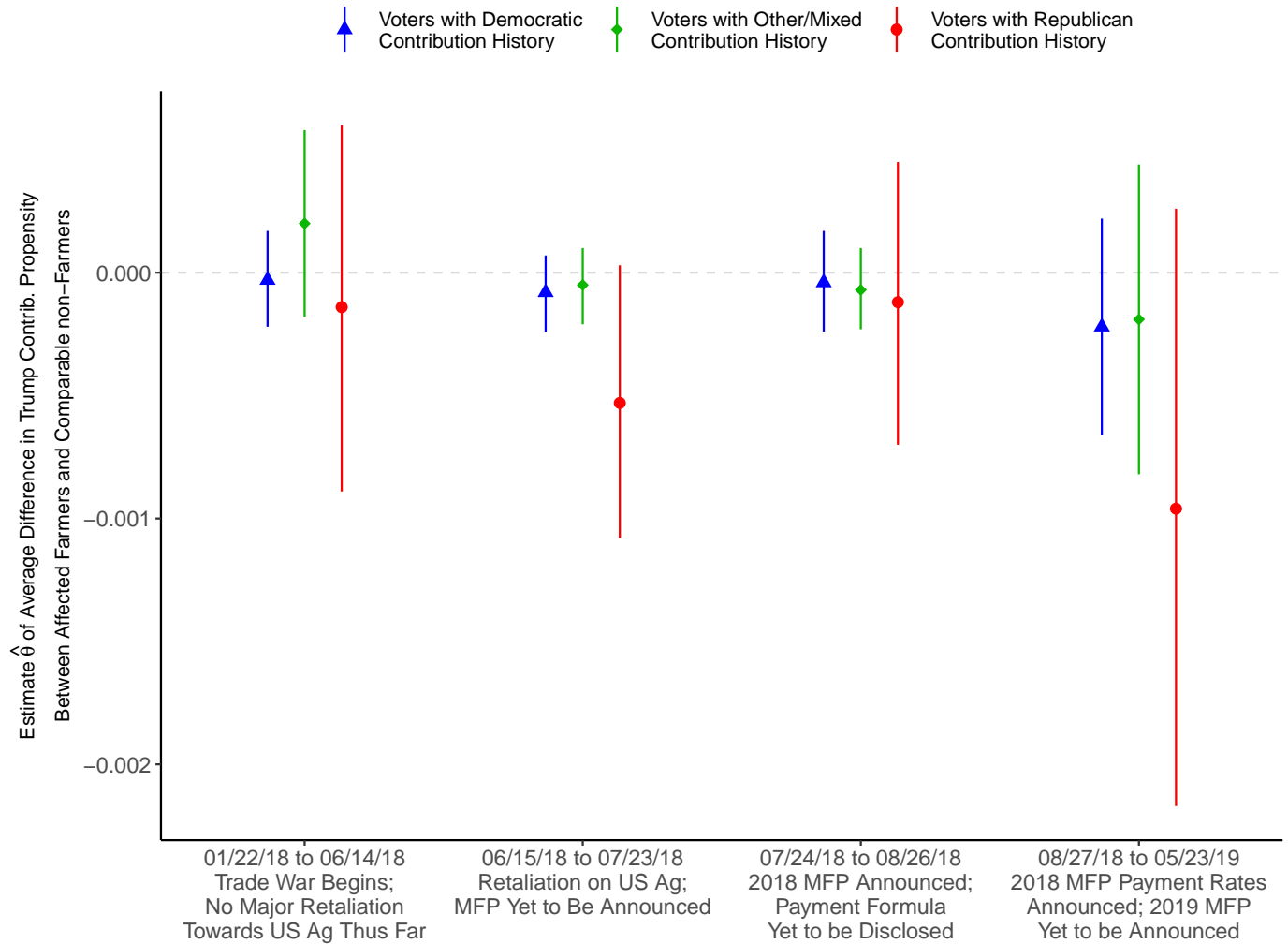


**Figure FSM51:** Overall Impact of Increased Policy Salience on Contributions: DML Estimates of Difference in Propensity to Contribute to Democrats between Affected Farmers and Other Contributors



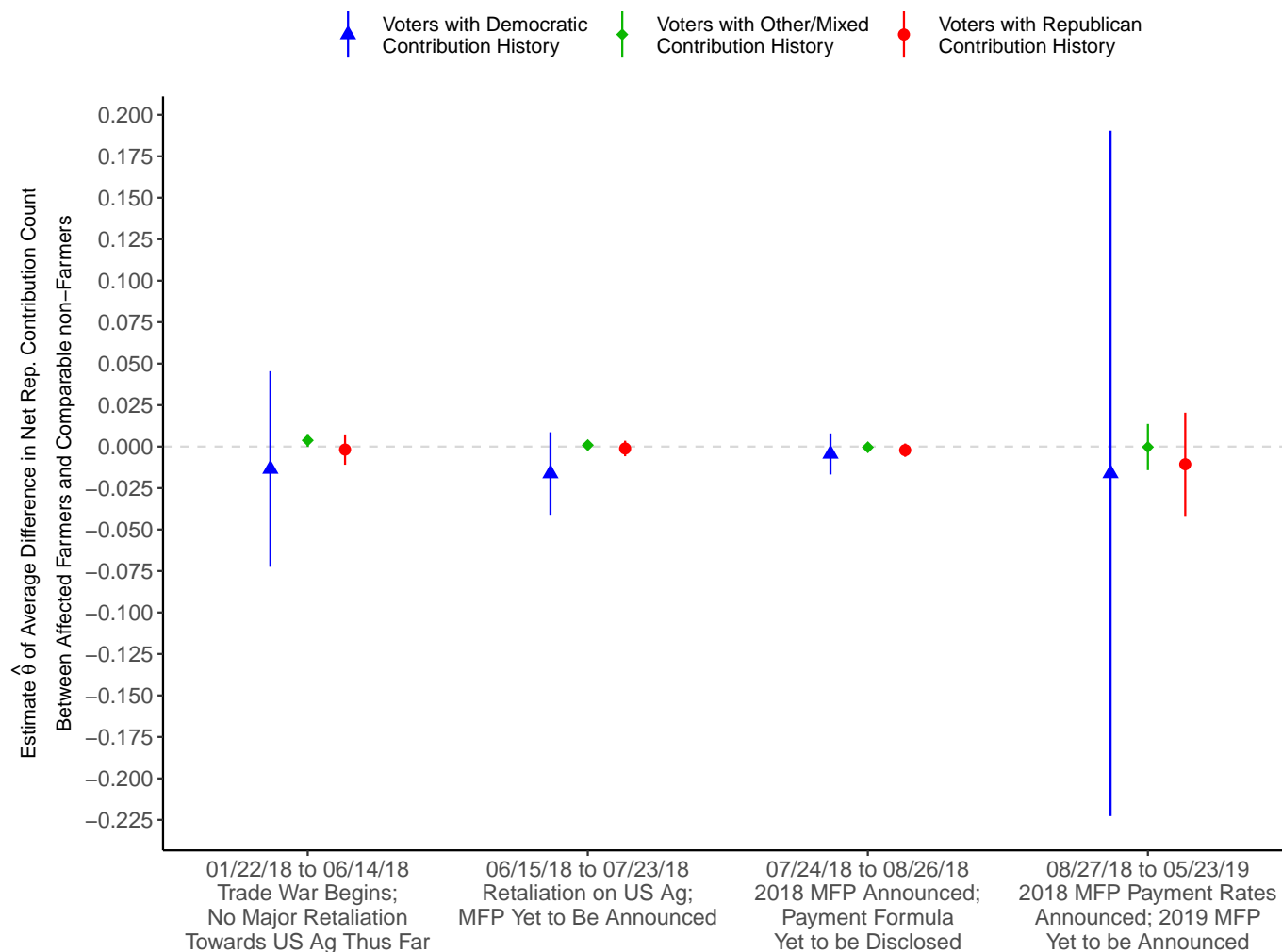
Notes: Point estimates are depicted with 95% confidence intervals. Outcome is a binary variable that takes a value of 1 if the voter made an itemized contribution to any Democratic Party candidate or PAC in the specified period. To view these results in table form, see Further Supplemental Materials Section I.

**Figure FSM52:** Overall Impact of Increased Policy Salience on Contributions: DML Estimates of Difference in Propensity to Contribute to Trump between Affected Farmers and Other Contributors



Notes: Point estimates are depicted with 95% confidence intervals. Outcome is a binary variable that takes a value of 1 if the voter made an itemized contribution to the Trump campaign or an affiliated PAC in the specified period. To view these results in table form, see Further Supplemental Materials Section I.

**Figure FSM53:** Overall Impact of Increased Policy Salience on Contributions: DML Estimates of Difference in Net Republican Contribution Count between Affected Farmers and Other Contributors



Notes: Point estimates are depicted with 95% confidence intervals. Outcome is equal to the number of itemized contributions made to any Republican Party candidate or PAC in the specified period, minus the number of contributions to Democratic Party candidates and PACs. To view these results in table form, see Further Supplemental Materials Section I.

# I Estimates in Table Form

Tables FSM21 and FSM22 present all Double Machine Learning PLR estimates from the main manuscript and Further Supplemental Materials in table form. Specifically, Table FSM21 documents all turnout effect estimates and Table FSM22 documents all contribution effect estimates.

**Table FSM21: DML PLR Turnout Effect Estimates in Table Form**

	Republican Voters			Non-Republican Voters		
	Estimate	CI Lower	CI Upper	Estimate	CI Lower	CI Upper

**Figure 6: Effects of Improved Policy Outcomes on 2018 Turnout –  
Constant Marginal Effects**

Net Benefits (Percentile)	0.005	-0.003	0.013	0.005	-0.008	0.019
Compensation Rate	0.002	-0.001	0.006	-0.002	-0.008	0.004
Made Whole (Binary)	0.003	-0.003	0.009	0.007	-0.003	0.016

**Figure 7: Effects of Improved Policy Outcomes on 2018 Turnout –  
Heterogeneity by Past Turnout**

Net Benefits (Percentile)						
Abstained 2014	-0.002	-0.026	0.023	0.003	-0.025	0.031
Voted 2014	0.007	-0.000	0.014	0.007	-0.006	0.021
Compensation Rate						
Abstained 2014	0.001	-0.010	0.013	0.002	-0.013	0.016
Voted 2014	0.003	-0.001	0.006	-0.003	-0.009	0.003
Made Whole (Binary)						
Abstained 2014	-0.000	-0.019	0.019	0.014	-0.007	0.035
Voted 2014	0.003	-0.002	0.009	0.003	-0.006	0.012

**Figure 11: Overall Impact of Increased Policy Salience on 2018 Turnout  
(Affected Farmers vs. Rest of Electorate)**

Abstained 2014	0.020	0.018	0.023	0.026	0.023	0.029
Overall	0.012	0.012	0.013	0.017	0.016	0.019
Voted 2014	0.010	0.009	0.011	0.012	0.010	0.013

**Figure FSM31: Effects of Improved Policy Outcomes on 2018 Turnout –  
Quintile-Binned Effects**

Net Benefits						
Quintile 2	0.005	-0.000	0.011	0.000	-0.008	0.009
Quintile 3	0.004	-0.002	0.009	0.002	-0.007	0.011

Continued on next page

**Table FSM21 – continued from previous page**

	Republican Voters			Non-Republican Voters		
	Estimate	CI Lower	CI Upper	Estimate	CI Lower	CI Upper
Quintile 4	0.004	-0.002	0.010	-0.000	-0.010	0.010
Quintile 5	0.007	0.000	0.013	0.010	-0.002	0.021
Compensation Rate						
Quintile 2	0.003	-0.002	0.008	0.011	0.002	0.019
Quintile 3	0.004	-0.002	0.009	0.012	0.003	0.021
Quintile 4	0.005	-0.001	0.010	-0.001	-0.010	0.008
Quintile 5	0.000	-0.005	0.006	-0.003	-0.013	0.006

**Figure FSM32: Effects of Improved Policy Outcomes on 2018 Turnout – Heterogeneity by Level of Electoral Competition**

Net Benefits (Percentile)						
No Toss Up	0.008	-0.003	0.020	0.019	-0.001	0.038
Toss Up Race	0.003	-0.008	0.013	-0.004	-0.022	0.014
Compensation Rate						
No Toss Up	0.004	-0.001	0.009	0.004	-0.004	0.012
Toss Up Race	0.000	-0.005	0.006	-0.011	-0.020	-0.001
Made Whole (Binary)						
No Toss Up	0.005	-0.004	0.013	0.016	0.002	0.029
Toss Up Race	-0.001	-0.009	0.007	-0.002	-0.015	0.011

**Figure FSM33: Effects of Improved Policy Outcomes on 2018 Turnout – Alternative Damage Measure Specifications**

Net Benefits (Percentile)						
Avg: Crops, Hogs, Dairy	0.006	-0.001	0.014	0.005	-0.008	0.018
Avg: Crops, Hogs	0.005	-0.002	0.013	0.003	-0.011	0.016
Avg: Crops	0.005	-0.003	0.013	0.005	-0.008	0.019
Avg by Method: Crops	0.006	-0.002	0.014	0.006	-0.008	0.020
Avg TS: Soybeans, Corn	0.005	-0.005	0.015	0.010	-0.008	0.028
Compensation Rate						
Avg: Crops, Hogs, Dairy	0.002	-0.001	0.005	-0.003	-0.009	0.002
Avg: Crops, Hogs	0.001	-0.002	0.004	-0.004	-0.010	0.001
Avg: Crops	0.002	-0.001	0.006	-0.002	-0.008	0.004
Avg by Method: Crops	0.002	-0.001	0.006	-0.002	-0.008	0.004
Avg TS: Soybeans, Corn	0.003	-0.004	0.009	-0.003	-0.012	0.006
Made Whole (Binary)						
Avg: Crops, Hogs, Dairy	0.002	-0.003	0.008	0.005	-0.003	0.014
Avg: Crops, Hogs	0.002	-0.003	0.008	0.004	-0.005	0.013
Avg: Crops	0.003	-0.003	0.009	0.007	-0.003	0.016
Avg by Method: Crops	0.004	-0.002	0.010	0.006	-0.004	0.016
Avg TS: Soybeans, Corn	0.004	-0.004	0.012	0.004	-0.008	0.016

**Figure FSM34: Effects of Improved Policy Outcomes on 2016 Turnout**

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**Table FSM21 – continued from previous page**

	Republican Voters			Non-Republican Voters		
	Estimate	CI Lower	CI Upper	Estimate	CI Lower	CI Upper
<b>(Placebo Estimates)</b>						
Net Benefits (Percentile)	0.003	-0.004	0.009	0.004	-0.008	0.015
Compensation Rate	0.000	-0.003	0.004	-0.001	-0.007	0.004
Made Whole (Binary)	0.006	0.001	0.010	0.000	-0.008	0.008

**Figure FSM37: Effects of Improved Policy Outcomes on 2018 Turnout – Heterogeneity by MFP Enrollment Date**

Net Benefits (Percentile)						
Before Election Day	0.002	-0.014	0.018	-0.002	-0.031	0.027
After Election Day	0.006	-0.003	0.015	0.007	-0.008	0.023
Compensation Rate						
Before Election Day	-0.001	-0.010	0.007	-0.005	-0.019	0.009
After Election Day	0.003	-0.001	0.008	-0.001	-0.008	0.006
Made Whole (Binary)						
Before Election Day	-0.004	-0.017	0.009	0.020	-0.003	0.043
After Election Day	0.004	-0.002	0.011	0.004	-0.006	0.015

**Figure FSM49: Substantive Turnout Effect Sizes for Policy Counterfactuals Based on a Piecewise Linear Specification with Heterogeneous Marginal Effects by Made Whole Status**

Net Benefits (Percentile)						
Not Made Whole Range	0.009	-0.013	0.031	0.008	-0.030	0.045
Across IQR	0.001	-0.004	0.007	-0.001	-0.009	0.008
Compensation Rate						
Corn Rate to Made Whole	0.008	-0.005	0.021	-0.012	-0.034	0.009
Made Whole to Soy Rate	0.001	-0.003	0.004	-0.005	-0.012	0.002

**Table FSM22: DML PLR Contribution Effect Estimates in Table Form**

	Distinctly Democratic Contrib. History	Other/Nonpartisan Contrib. History	Distinctly Republican Contrib. History
<b>Figure 9: Effects of Improved Policy Outcomes on Net Republican Contributing</b>			
Net Benefits (Percentile)	0.011 (-0.037, 0.058)	-0.001 (-0.004, 0.002)	-0.002 (-0.022, 0.017)
Compensation Rate	0.005 (-0.015, 0.025)	-0.000 (-0.002, 0.001)	-0.001 (-0.011, 0.009)
Made Whole (Binary)	-0.000	-0.000	-0.001

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**Table FSM22 – continued from previous page**

	Distinctly Democratic Contrib. History	Other/Nonpartisan Contrib. History	Distinctly Republican Contrib. History
	(-0.033, 0.032)	(-0.002, 0.001)	(-0.014, 0.013)

**Figure 12: Overall Impact of Increased Policy Salience on Net Republican Contributing**

Trade War Begins (01/22/18–06/14/18)	0.004 (-0.001, 0.008)	0.002 (-0.000, 0.003)	0.001 (-0.001, 0.004)
Retaliation on US Ag (06/15/18–07/23/18)	-0.002 (-0.006, 0.002)	0.001 (-0.001, 0.002)	0.000 (-0.001, 0.002)
2018 MFP Announced (07/24/18–08/26/18)	-0.003 (-0.006, 0.001)	0.000 (-0.001, 0.001)	0.001 (-0.001, 0.002)
MFP Rates Announced (08/27/18–05/23/19)	0.002 (-0.004, 0.008)	0.004 (0.002, 0.007)	0.008 (0.005, 0.011)

**Figure FSM42: Effects of Improved Policy Outcomes on Propensity to Contribute to Republicans**

Net Benefits (Percentile)	-0.002 (-0.012, 0.009)	0.000 (-0.001, 0.002)	0.001 (-0.017, 0.020)
Compensation Rate	-0.003 (-0.006, 0.001)	-0.000 (-0.001, 0.001)	0.000 (-0.009, 0.010)
Made Whole (Binary)	0.001 (-0.006, 0.007)	0.000 (-0.001, 0.001)	0.001 (-0.012, 0.013)

**Figure FSM43: Effects of Improved Policy Outcomes on Propensity to Contribute to Democrats**

Net Benefits (Percentile)	-0.021 (-0.068, 0.025)	0.001 (-0.001, 0.004)	0.002 (-0.006, 0.009)
Compensation Rate	-0.008 (-0.027, 0.011)	0.000 (-0.001, 0.001)	0.000 (-0.002, 0.003)
Made Whole (Binary)	0.002 (-0.029, 0.034)	0.001 (-0.001, 0.002)	0.000 (-0.004, 0.005)

**Figure FSM44: Effects of Improved Policy Outcomes on Propensity to Contribute to Trump**

Net Benefits (Percentile)	-0.001 (-0.003, 0.001)	0.000 (-0.001, 0.001)	-0.000 (-0.008, 0.008)
Compensation Rate	-0.001 (-0.001, 0.000)	-0.000 (-0.001, 0.000)	0.001 (-0.004, 0.006)
Made Whole (Binary)	0.001 (-0.000, 0.002)	-0.000 (-0.001, 0.001)	-0.000 (-0.006, 0.006)

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**Table FSM22 – continued from previous page**

	Distinctly Democratic Contrib. History	Other/Nonpartisan Contrib. History	Distinctly Republican Contrib. History
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**Figure FSM45: Effects of Improved Policy Outcomes on Net Republican Contribution Count**

Net Benefits (Percentile)	0.164 (-0.473, 0.801)	0.002 (-0.013, 0.017)	-0.056 (-0.225, 0.114)
Compensation Rate	-0.084 (-0.485, 0.317)	-0.003 (-0.010, 0.003)	-0.029 (-0.143, 0.084)
Made Whole (Binary)	-0.085 (-0.442, 0.272)	-0.003 (-0.014, 0.008)	-0.018 (-0.126, 0.090)

**Figure FSM46: Comparison of Estimated Effects of Improved Policy Outcomes on Net Republican Contributing Using Different Trade War Damage Measures**

Net Benefits (Percentile)			
Avg: Crops, Hogs, Dairy	0.021 (-0.024, 0.065)	-0.000 (-0.003, 0.002)	0.002 (-0.016, 0.021)
Avg: Crops, Hogs	0.010 (-0.036, 0.056)	-0.001 (-0.003, 0.002)	0.002 (-0.017, 0.021)
Avg: Crops	0.011 (-0.037, 0.058)	-0.001 (-0.004, 0.002)	-0.002 (-0.022, 0.017)
Avg by Method: Crops	0.009 (-0.041, 0.059)	-0.001 (-0.004, 0.002)	-0.005 (-0.026, 0.015)
Avg TS: Soybeans, Corn	0.012 (-0.048, 0.071)	-0.001 (-0.004, 0.003)	-0.001 (-0.028, 0.026)
Compensation Rate			
Avg: Crops, Hogs, Dairy	0.014 (-0.004, 0.032)	-0.000 (-0.001, 0.001)	-0.001 (-0.010, 0.008)
Avg: Crops, Hogs	0.013 (-0.005, 0.032)	-0.001 (-0.002, 0.001)	-0.003 (-0.012, 0.006)
Avg: Crops	0.005 (-0.015, 0.025)	-0.000 (-0.002, 0.001)	-0.001 (-0.011, 0.009)
Avg by Method: Crops	0.004 (-0.016, 0.024)	-0.000 (-0.002, 0.001)	-0.001 (-0.012, 0.009)
Avg TS: Soybeans, Corn	0.007 (-0.023, 0.037)	-0.000 (-0.002, 0.002)	-0.001 (-0.016, 0.013)
Made Whole (Binary)			
Avg: Crops, Hogs, Dairy	0.003 (-0.026, 0.032)	-0.001 (-0.002, 0.001)	-0.001 (-0.014, 0.011)
Avg: Crops, Hogs	-0.002	-0.001	-0.004

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**Table FSM22 – continued from previous page**

	Distinctly Democratic Contrib. History	Other/Nonpartisan Contrib. History	Distinctly Republican Contrib. History
	(-0.031, 0.028)	(-0.002, 0.001)	(-0.017, 0.009)
Avg: Crops	-0.000	-0.000	-0.001
	(-0.033, 0.032)	(-0.002, 0.001)	(-0.014, 0.013)
Avg by Method: Crops	-0.007	-0.000	-0.003
	(-0.042, 0.027)	(-0.002, 0.002)	(-0.016, 0.011)
Avg TS: Soybeans, Corn	-0.008	-0.001	-0.003
	(-0.048, 0.031)	(-0.003, 0.002)	(-0.021, 0.015)

**Figure FSM47: Effects of Improved Policy Outcomes on Net Republican Contributing Across Distinct Trade War Policy Regimes**

Net Benefits (Percentile)			
Trade War Begins	-0.013	-0.000	-0.011
(01/22/18–06/14/18)	(-0.048, 0.021)	(-0.002, 0.001)	(-0.025, 0.004)
Retaliation on US Ag	-0.002	0.000	0.002
(06/15/18–07/23/18)	(-0.034, 0.030)	(-0.001, 0.001)	(-0.007, 0.012)
2018 MFP Announced	-0.009	-0.000	-0.004
(07/24/18–08/26/18)	(-0.032, 0.014)	(-0.001, 0.000)	(-0.014, 0.007)
MFP Rates Announced	0.011	-0.001	-0.002
(08/27/18–05/23/19)	(-0.037, 0.058)	(-0.004, 0.002)	(-0.022, 0.017)
Compensation Rate			
Trade War Begins	0.009	-0.000	-0.004
(01/22/18–06/14/18)	(-0.006, 0.024)	(-0.001, 0.000)	(-0.012, 0.004)
Retaliation on US Ag	0.000	0.000	-0.002
(06/15/18–07/23/18)	(-0.017, 0.017)	(-0.000, 0.000)	(-0.007, 0.004)
2018 MFP Announced	-0.005	-0.000	-0.000
(07/24/18–08/26/18)	(-0.016, 0.006)	(-0.000, 0.000)	(-0.006, 0.005)
MFP Rates Announced	0.005	-0.000	-0.001
(08/27/18–05/23/19)	(-0.015, 0.025)	(-0.002, 0.001)	(-0.011, 0.009)
Made Whole (Binary)			
Trade War Begins	-0.001	-0.001	-0.008
(01/22/18–06/14/18)	(-0.025, 0.024)	(-0.002, 0.001)	(-0.019, 0.003)
Retaliation on US Ag	0.003	0.000	-0.003
(06/15/18–07/23/18)	(-0.017, 0.023)	(-0.000, 0.001)	(-0.010, 0.005)
2018 MFP Announced	-0.015	-0.000	-0.004
(07/24/18–08/26/18)	(-0.027, -0.003)	(-0.001, 0.000)	(-0.012, 0.004)
MFP Rates Announced	-0.000	-0.000	-0.001
(08/27/18–05/23/19)	(-0.033, 0.032)	(-0.002, 0.001)	(-0.014, 0.013)

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**Table FSM22 – continued from previous page**

	Distinctly Democratic Contrib. History	Other/Nonpartisan Contrib. History	Distinctly Republican Contrib. History
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**Figure FSM50: Overall Impact of Increased Policy Salience on Propensity to Contribute to Republicans**

Trade War Begins (01/22/18–06/14/18)	0.001 (-0.000, 0.002)	0.001 (-0.000, 0.002)	0.001 (-0.001, 0.003)
Retaliation on US Ag (06/15/18–07/23/18)	-0.000 (-0.001, 0.000)	0.000 (-0.001, 0.001)	-0.000 (-0.002, 0.001)
2018 MFP Announced (07/24/18–08/26/18)	0.000 (-0.000, 0.001)	-0.000 (-0.001, 0.001)	0.001 (-0.001, 0.002)
MFP Rates Announced (08/27/18–05/23/19)	0.001 (-0.000, 0.002)	0.003 (0.001, 0.005)	0.007 (0.004, 0.009)

**Figure FSM51: Overall Impact of Increased Policy Salience on Propensity to Contribute to Democrats**

Trade War Begins (01/22/18–06/14/18)	-0.004 (-0.008, 0.001)	-0.001 (-0.002, 0.001)	-0.000 (-0.001, 0.000)
Retaliation on US Ag (06/15/18–07/23/18)	0.002 (-0.002, 0.006)	-0.000 (-0.001, 0.001)	-0.000 (-0.001, 0.000)
2018 MFP Announced (07/24/18–08/26/18)	0.002 (-0.001, 0.006)	-0.000 (-0.001, 0.001)	0.000 (-0.000, 0.001)
MFP Rates Announced (08/27/18–05/23/19)	-0.001 (-0.007, 0.005)	-0.002 (-0.003, 0.000)	-0.001 (-0.002, -0.000)

**Figure FSM52: Overall Impact of Increased Policy Salience on Propensity to Contribute to Trump**

Trade War Begins (01/22/18–06/14/18)	-0.000 (-0.000, 0.000)	0.000 (-0.000, 0.001)	-0.000 (-0.001, 0.001)
Retaliation on US Ag (06/15/18–07/23/18)	-0.000 (-0.000, 0.000)	-0.000 (-0.000, 0.000)	-0.001 (-0.001, 0.000)
2018 MFP Announced (07/24/18–08/26/18)	-0.000 (-0.000, 0.000)	-0.000 (-0.000, 0.000)	-0.000 (-0.001, 0.000)
MFP Rates Announced (08/27/18–05/23/19)	-0.000 (-0.001, 0.000)	-0.000 (-0.001, 0.000)	-0.001 (-0.002, 0.000)

**Figure FSM53: Overall Impact of Increased Policy Salience on Net Republican Contribution Count**

Trade War Begins (01/22/18–06/14/18)	-0.014 (-0.072, 0.045)	0.004 (-0.000, 0.008)	-0.002 (-0.011, 0.007)
Retaliation on US Ag	-0.016	0.001	-0.001

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**Table FSM22 – continued from previous page**

	Distinctly Democratic Contrib. History	Other/Nonpartisan Contrib. History	Distinctly Republican Contrib. History
(06/15/18–07/23/18)	(-0.041, 0.009)	(-0.001, 0.003)	(-0.006, 0.004)
2018 MFP Announced	-0.004	-0.000	-0.002
(07/24/18–08/26/18)	(-0.017, 0.008)	(-0.002, 0.002)	(-0.006, 0.002)
MFP Rates Announced	-0.016	-0.000	-0.011
(08/27/18–05/23/19)	(-0.223, 0.190)	(-0.014, 0.014)	(-0.042, 0.020)

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