Small Dollar Donations and Globalization: How Trade-Related Layoffs Translate to Costly Political Action

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Abstract

The correlation between free trade's negative consequences and political outcomes in the United States has received a lot of scholarly attention over the last decade. A persistent empirical challenge across this work has been the ecological inference challenge, in which empirical evidence aggregated to geographic units is used to infer behaviors of interest theorized at the individual level. Efforts to overcome this limitation that use survey self-reports of public opinion are marred by the preponderant influence of partisanship on these responses. In this project, we exploit a rich new dataset of individual-level small dollar donations to ActBlue to causally identify the true effect of free trade's negative labor market outcomes on costly political behavior. We implement recent methodological innovations in generalized difference-in-differences estimation to compare donor behavior before and after highly salient mass layoffs occur, relative to the change in donation behavior of otherwise similar donors living in otherwise similar areas that did not witness these layoffs. We show that trade-related layoffs stimulate political participation through small dollar donations, but that the main beneficiaries of this increased political participation are conservative groups and candidates. By providing a carefully identified estimate of a costly political behavior using rich data, we contribute more convincing evidence of a political response to free trade's negative consequences in the United States.

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1 Introduction

Three stylized statements about the first quarter of the 21st century motivate this paper. First, there has been a political shift toward the right among less educated Americans, especially whites. Second, there has been an increase in the economic hardships associated with free trade which theoretically disproportionately fall on this group. Third, the political right has grown more anti-globalist over this same period. In aggregate then, a natural explanation presents itself: the negative consequences of free trade lead those who suffer them to rationally support protectionist candidates.

Implicit in this story is a microfoundation which assumes those hurt by trade (1) perceive this hardship and attribute it correctly, and (2) update their policy preferences accordingly. Yet empirical evidence for this microfoundation is mixed at best. While free trade's losers do hold more protectionist policy preferences (Scheve and Slaughter, 2001; O'Rourke et al., 2001; Mayda and Rodrik, 2005), and while exposure to trade "shocks" is associated with shifts toward conservative politics (Autor et al., 2017, 2020; Colantone and Stanig, 2018b,a), these relationships are tiny compared to the preponderant influence of partisanship and associated ideological and culture dispositions (Milner and Tingley, 2010; Mansfield and Mutz, 2009). Furthermore, identifying who is negatively affected by free trade, whether they accurately perceive the connection between their hardship and foreign policy, remain thorny empirical challenges not yet satisfactorily resolved in the literature. Finally is the challenge of causal identification in which strategies for identifying causal relationships at aggregate levels do not map cleanly on to individual-level data. If anything, the causal arrow might be reversed, with much stronger evidence of partisans "following their leaders" on the topic of trade, than updating their views on trade and conditioning their political support accordingly (Naoi and Urata, 2013; Lenz, 2012).

In this paper, we contribute to this puzzle by introducing a new dataset of individual behavior that allows for plausibly causal estimates of the microfoundations of interest. Instead of relying on survey self-reports, we use donations to political candidates, groups, and causes. But unlike existing work which relies on traditionally available campaign contribution data from the Federal Election Commission (FEC) which was restricted to "large" donors (i.e., those who contribute $200 or more to a single recipient in an election cycle), we introduce a new dataset on small dollar donors scraped from the two primary political action committees developed over the last 20 years: ActBlue (liberal) and WinRed (conservative).

We argue that these data overcome many of the empirical issues associated with previous attempts to resolve the microfoundation puzzle at the heart of IPE's explanation for the recent globalist-backlash sweeping across advanced industrial democracies. First, our data is a proper panel of individuals, allowing us to observe the same person's behavior at multiple points in time. These data afford more plausibly causal interpretations of before-after behavior relative to exposure to a trade shock, compared to the typically time-series cross sectional datasets that comprise the bulk of work on public opinion on trade. Second, our data sidesteps many of the problems with self-reported attitudes and behaviors associated with survey data, such as expressive or insincere responding. By focusing on dollar-denominated (i.e., costly) behaviors, we can be more confident that the patterns we observe reflect genuine (and politically consequential) behaviors. Third, we also observe each donor's physical address, self-reported occupation, and self-reported employer, allowing us to construct a much more sophisticated measure of their exposure to free trade's negative consequences along the dimensions of labor market position and geographical proximity. Fourth, we are able to observe all these measures at the unit of the individual, overcoming the ecological inference challenges that have limited previous attempts to speak to the microfoundations undergirding classic IPE theories of political behavior.

We exploit these rich data to compare individuals living closer to sites that laid off significant parts of their workforce due to free trade, to those that live further away. We exploit these data to compare individuals working in occupations more vulnerable to free trade's negative consequences, to those in jobs more insulated. And we exploit these rich data to compare individuals working in industries that compete more with cheap goods produced abroad, to those in industries that don't compete with imports. Our study focuses on two particularly high-salience instances of trade shocks: Boeing's decision to lay off more than 5,000 workers from its Seattle-based plant on August 7th, 2017; and GMC's decision to shutter its Lordstown, PA factory in 2021.

In this draft, we show that those who live closer to the Boeing plant increased their contributions to ActBlue (the left-leaning political action committee) by less than half of the increase observed among donors living further away from the plant. In addition, we show that those working in more vulnerable occupations behaved similarly but even more starkly, reducing contributions to the liberal PAC after the layoffs while those in more secure occupations increased their donations over the same period. Finally, we use a triple-difference specification to show that the largest declines in donations to ActBlue are among those who live closer to the factory and those who work in more exposed occupations. The summary story of our data is that, while those working in fragile occupations reduced contributions to ActBlue overall between 2016 and 2018, those living closer to the Boeing plant did so by more than those living further away. Conversely, those working in insulated occupations increased their contributions to ActBlue over the same period, especially those who lived closer to the Boeing plant.

Our work continues to build the dataset for the Lordstown, PA setting. We have gathered all small dollar contributions to ActBlue and WinRed from January 1st, 2017 to December 31st, 2022 from donors living in either Ohio or Pennsylvania. Despite the greater variation in these data, important challenges remain. First, while the official layoffs did not occur until March 6th, 2019, awareness of them preceded this date by more than six months. Second, WinRed did not fully come online until late in 2019. As such, our identification of the WinRed contributions is purely cross-sectional, comparing donors living closer to or further from the shuttered Lordstown factory.

In addition, we are still in the process of matching each donor with a listed employer to their industry of occupation, which will thus let us further compare those working in industries that do and do not compete with cheaply-made foreign goods. Finally, we plan to exploit the rich information on the recipients of these donations, the majority of which are earmarked for specific federal candidates or interest groups. We hope to devise a method to characterize recipient stances on globalization, and in particular free trade, to add a final level of granularity to our analyses.

2 Theory and Existing Research

The traditional IPE account of the political economy of trade assumes that individuals operate according to a fundamentally rational actor model. Those negatively affected by some policy (in this case, free trade) will oppose the policy, and condition their political behavior accordingly. Much of the initial IPE scholarship took this model for granted and focused on using individual-level data to adjudicate between competing economic models of free trade's winners and losers. This work amounted to relying on humans to behave rationally in order to reveal whether their industry of employment, their occupation of employment, or their "skill" level best predicted variation in support for free trade. In so doing, this work mostly ignored a flourishing literature on public opinion that questioned the validity of the underlying rational actor framework writ large, and as such suffered a bit from the "lonely economist" syndrome (to coin a phrase), in which scholars with a tidy formal model seem to hope that real humans adhere to their theorized comparative statics, and seem almost disappointed when humans behave otherwise.

More recent IPE scholarship has begun to confront the challenges posed by other models of human cognition and behavior. Hainmueller and Hiscox (2006) looked at a bevy of empirical findings that used education as a proxy for skill and questioned whether it was more plausible to think of education as a proxy for – well – education. Instead of better educated respondents supporting free trade because it aligned with economic models of relative factor endowments, mightn't they support free trade because they learned about its benefits in school? A number of other skeptics emerged, questioning whether individuals were so myopic as to only pay attention to their own welfare, or if they instead care about "sociotropic" considerations (Mansfield and Mutz 2009, although whether sociotropic considerations obtain due to altruistic concerns for others, or whether they instead redraw the boundaries of myopic material interests remains to be resolved). And a flurry of research highlighted the strong associations between protectionist views and a bundle of other attitudes such as xenophobia and racial resentment that were not, at least nominally, connected with the tight predictions of economic models of individual welfare (Margalit, 2011, 2012; Mutz and Kim, 2017).

Yet despite these challenges to the microfoundations of IPE's classic rational actor perspective, the aggregate patterns are still well-described by the rational actor model, as summarized above. In the United States and across many western European democracies, free trade's losers are increasingly supporting protectionist parties. Sure there might be some empirical challenges with the microfoundations, but models that are parsimonious and accurate should be celebrated. If the rational actor framework helps us make sense of the world, why challenge it at all?

We present two arguments for doing so. First, the microfoundations are themselves of substantive interest because they help us understand the pathways by which politics operates, and thus identify the loci of power. If attitudes on free trade do not come from the bottom up, as predicted by the rational actor account, but instead are defined and spread by political entrepreneurs and elites, this carries dramatic implications for how we understand representation and political influence across advanced industrial democracies.

Second, accuracy and parsimony should not be the only two qualities by which we separate good theory from bad. Microfoundations are – as is written on the tin – the foundations of good theory. An accurate and parsimonious model whose assumptions are invalid is not necessarily a "bad" model, but it is an incomplete one. By the same logic, a model with rock solid microfoundations but poor predictive accuracy is also not necessarily "bad". We argue that highlighting disconnects between microfoundations and aggregate outcomes highlights blind spots in our understanding that, when filled in, advance the literature.

With this motivation in mind, we position this paper as a contribution that overcomes several of the empirical challenges associated with causally testing these microfoundations using observational data. First, by using campaign contributions instead of self-reported attitudes and behaviors, we remove the bias generated by expressive responding to surveys which can inflate the appearance of partisan or ideological predictors of trade attitudes. Second, by incorporating three dimensions of how individuals are exposed to free trade's negative consequences, we provide a more holistic account of who wins and who loses due to free trade that doesn't constrain us to a particular economic model. Third, by exploiting a particularly high salience shock at a specific moment in time, and using a proper panel of the same individuals measure over time, we can rely on difference-in-differences methods to provide a more plausibly causal interpretation of the empirical patterns we document.

In so doing, our results provide evidence in support of the microfoundations for the rational actor model: individuals exposed to free trade's negative consequences adjust their political behaviors in ways that pull them away from the less protectionist political party during our period of analysis. Much work remains to be done in this paper, of course. We plan to further bolster our multi-dimensional measure of exposure by incorporating the industry in which our individuals are employed at the time they donate. We plan to more carefully describe the recipients of the donations themselves, disaggregating the more protectionist from the more pro-free trade. And we plan to expand our analysis to include a more recent localized trade shock in Lordstown, Ohio, where we can measure not just contributions to the left-leaning ActBlue organization, but also those to the right-leaning WinRed organization; and where we can explore the generalizability of our conclusions from a cosmopolitan Seattle in 2017 to the suburban and rural parts of the rust belt in 2019.

3 Data

We combine several sources of data to construct the tabular dataset used in our final analysis, in which rows index individual donors by time, and columns include the amount they contribute; their geographic proximity to the site of trade-related layoffs; a measure of their occupational fragility, operationalized as either the change in total jobs in their occupation between the year in which the layoffs occurred and ten year prior, or the change in real wages over the same period; and (aspirationally but not yet completed) measures of their industry-based exposure to import competition from cheap Chinese goods.

3.1 Treatment

Our main predictors of interest are the loss of 5,275 jobs at Boeing's Seattle factory on August 7th, 2017, and the shuttering of the Lordstown, OH General Motors factor on March 6th, 2019. These layoffs were explicitly thought to be the result of free trade by those actually affected, since they were included in applications for Trade Adjustment Assistance by affected parties. In the Boeing example, the application was filed by the president of the International Association of Machinists and Aerospace Workers (IAMAW) whose provided reason for the impending layoffs was "unfair subsidies provided to Boeing chief competitor, Airbus", which led to cost-saving measures by Boeing, including "offshoring production of aircraft parts and engineering to countries like China, Japan, India, Russia, etc.". While the layoffs affected workers throughout the pacific northwest, the vast majority were located at Boeing's factory at 2925 112th Street, Tukwila WA 98168.[1] In the Lordstown example, the application was filed by Ohio's state workforce office whose provided reason for the impending layoffs was "falling demand for cars", and the certification states "increased aggregate United States imports of articles like or directly competitive with the article produced by Lordstown Complex contributed importantly to the worker group separations and sales/production declines at Lordstown Complex.".[2]

3.2 Outcome

Our outcomes of interest are political donations. In both cases, we collect data for all campaign contributions to ActBlue from January 1, 2016 until December 31, 2018 in Washington, and contributions to both ActBlue and WinRed from January 1, 2017 until December 31, 2022 in Ohio and Pennsylvania, using the OpenFEC API, the official API for the Federal Election Commission. Data employed from these donations reflects the most up-do-date report version histories as amended. Because ActBlue and WinRed are conduit committees where donations are intended to pass through to other committees, our data reflects the full universe of donations to ActBlue and WinRed during the time period analyzed. Thus, our data does not suffer from missingness due to the FEC's rules for reporting itemized receipts which mandate that campaigns must only report an individual's donations if their donations exceed $200 over the course of the election cycle. Donation information includes the amount donated, donation date, the donor's employer, the donor's occupation, and the donor's name and address. For

[1] https://www.dol.gov/agencies/eta/tradeact/petitioners/petitions/taw?num=92903

[2] https://www.dol.gov/agencies/eta/tradeact/petitioners/petitions/taw?num=94427

donations to conduit committees, the FEC also reports earmarks, where donors can donate to a given conduit committee for the purposes of being passed to a *specific* committee. We retain the earmark memos which report the campaign committee that the ActBlue or WinRed donation is intended for (if specified by the donor).

3.3 Mapping Donors to Treatment

For the cases of interest in this study we restrict the data to donors whose self-reported residence is in Washington state in the case of the Boeing layoffs, and to donors whose self-reported residence is in either Ohio or Pennsylvania in the case of the GMC layoffs.[3] We characterize exposure to the layoffs using two aspects of the data: distance to the site of the layoffs and labor market "fragility", defined below. For the former, we leverage the information on each donor's physical address. For each donor, we geocode their address using the Census Batch Geocoding Service, which uses a street, city, state, and zip code to identify the latitude and longitude of their place of residence. We use these latitude and longitude variables to determine the distance a donor lives from the layoff analyzed.

For the latter measure, we use the donor's self-reported occupation to link them to labor market information from the Bureau of Labor Statistics (BLS). Specifically, we measure the cosine similarity between the ChatGPT vector representations of these self-reported responses, and the ChatGPT vector representations of the descriptions of occupations found in the Department of Labor's Occupation Outlook Handbook (OOH) (we use the method developed in https://joeornstein.github.io/publications/fuzzylink.pdf). We then measure the ten year change in the total number of jobs and the average annual wages for each occupation between either 2006 and 2016 (in the case of the Boeing layoffs), or 2009 and 2019 (in the case of the Lordstown layoffs). For donors working in occupations that experienced a net decline in total jobs over this period, or those who saw a decline in real wages (i.e., wages that grew by 19% or less, where 19% reflects the cumulative inflation over this period), we define them as "fragile". As illustrated in the first facet of Figure 1 which relies on the Boeing case, there is evidence of a U-shaped relationship between labor market position and campaign contributions, with the largest contributions made by those whose occupations are either growing or shrinking.

There are additional opportunities to exploit these rich data to further describe each donor's ex-

[3] We look at both Ohio and Pennsylvania for the latter case, since the Lordstown factory sits close to the Pennsylvania border.

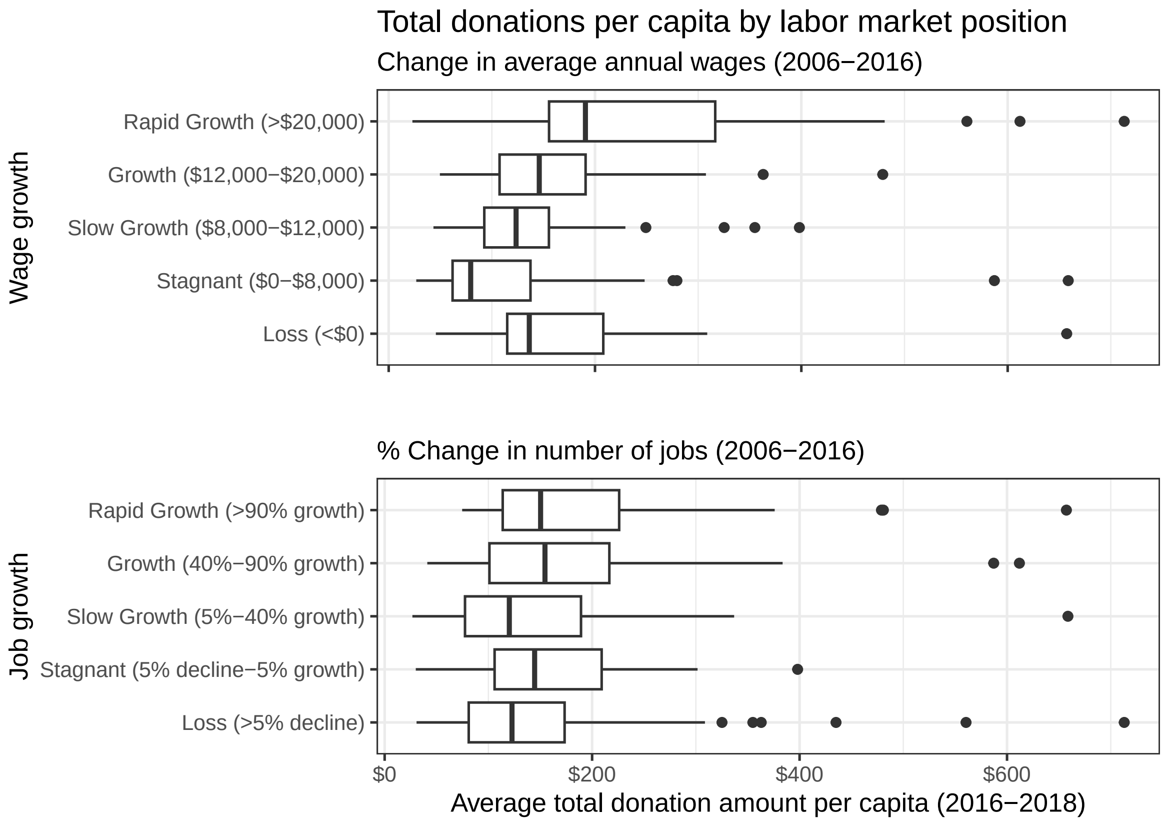


Figure 1 Descriptive relationship between total small dollar donations per individual (x-axes) and different measures of their labor market position (y-axes), divided into changes in the average annual ways between 2006 and 2016 (top facet) and the percent change in the number of jobs between 2006 and 2016 (bottom facet).

posure to free trade's negative consequences. Work is currently underway to link each donor to an industry, thereby allowing us to calculate the change in imports to the United States that compete with this industry's production (a la Autor et al. (2016), albeit calculated per individual instead of per commuting zone). In addition, we also have historical data from the O*NET database which includes the 10 year projection for each occupation from 2016.

3.4 Moderators

In addition to geocoding all addresses, we use a donor's name and self-reported address to estimate race and gender for the donors. To estimate race, we employ Bayesian Improved Surname Geocoding (BISG) (Imai et al., 2022), which uses surname and geolocation information to predict an individual's racial or ethnic group. It uses Bayesian statistical methods to update the probability of an individual's race or ethnicity based on their surname and geographic location, leveraging existing demographic data about the distribution of surnames and races within specific areas. This method improves the accuracy of race and ethnicity classification compared to using surname or geography alone. We estimate gender using the `gendeR` package in R, which determines an individual's gender based on their first name. This estimation relies on Social Security Administration data, using birth records to calculate the proportion of people with a specific first name who belong to a particular gender category.

4 Methods

Our estimand of theoretical interest is the change in donation behavior induced by a discontinuous shock in one's exposure to free trade's negative consequences. As discussed above, exposure to these negative consequences causes an individual to update negatively on the value of free trade, and condition their political behaviors accordingly. In our data, these behaviors are observed as small dollar donations to one of myriad recipients, which we code as either protectionist or pro-free trade, allowing us to measure a proportion of donations toward protectionist interests per day.

Our main approach is to rely on a difference-in-differences specification to calculate the statistical estimand: the average treatment effect on the treated (ATT). To establish a running example that will continue to the analysis, consider our Boeing plant that laid off 5,275 workers on August 7th, 2017. The plant is located just south of Seattle, Washington, as illustrated by the dark red diamond in Figure 2. Nearby this location are 1,256 small dollar donations made by 766 donors totalling $36,182 that were made on August 7th, 2017 in the surrounding area, defined as within a 2 hour drive to the Boeing plant, and illustrated as hollow circles in Figure 2. 967 of these donations were earmarked to a variety of recipients beyond the general ActBlue organization, the most popular recipients being the DCCC, the End Citizens United PAC, and a variety of progressive candidates and PACs.

This geographic visualization highlights one of three differences we exploit to estimate the effect of plant closure on political behavior: distance. All else equal, we assume that living closer to the location of trade-related layoffs increases one's exposure, and can thus compare the behaviors of individuals living closer by the plant to those living further away.

The second dimension by which we calculate differences is time. For a discrete event like mass layoffs, we know three dates that are of interest from the TAA data. First, we know the impact date, which is when the authors of the TAA petition claim that the layoffs occurred. Second, we know the petition date, which is when the company, the union, the state official, or a group of workers filed the

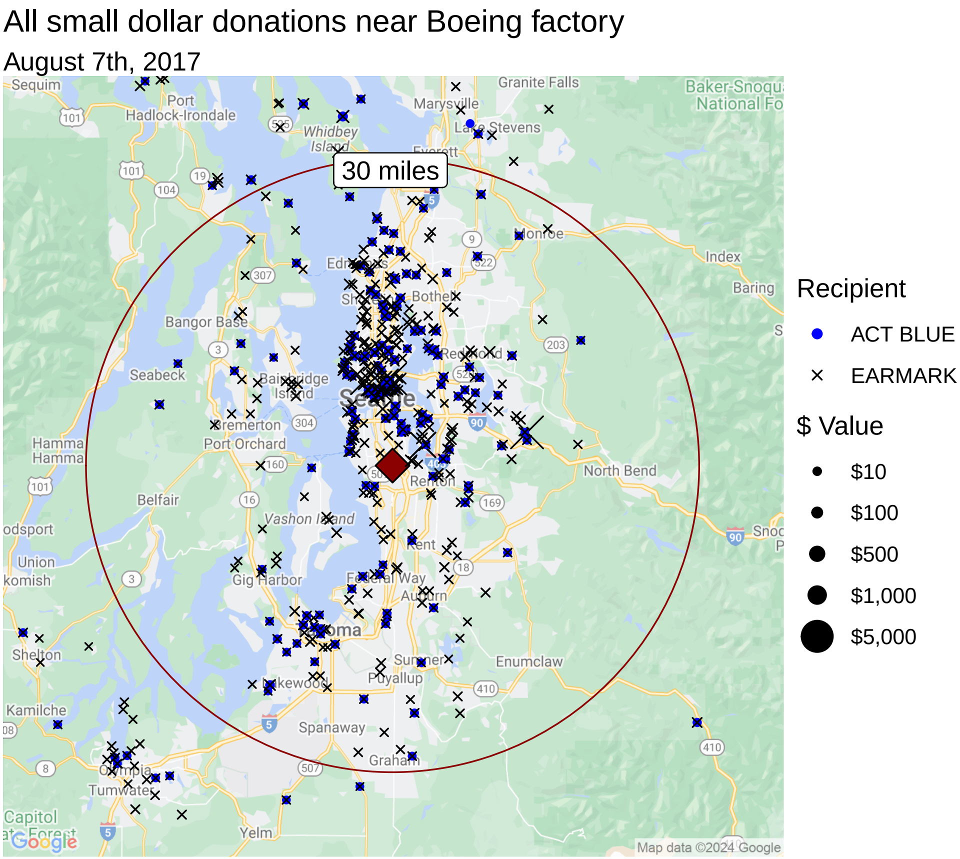


Figure 2 Example of a high-salience trade shock and the surrounding small dollar donation data. On August 7th, 2017 Boeing laid off 5,275 workers from it's factory south of Seattle (red diamond). The same day, within a two hour drive of the factory, 766 donors contributed $36,182 to various political groups, candidates, and campaigns.

petition with the Department of Labor. Third, we know the determination date, which is when the DoL investigator ruled on the petition, deciding either to certify or deny. Each of these dates arguably contains information about the impact of free trade on welfare in the United States. The date when workers were laid off (the impact date) is arguably the most sensible date to examine, assuming that the workers and those in the surrounding area understood the reason for the layoffs. However, if the cause for the layoffs is not widely known, the institution date might also be a reasonable candidate to examine the effect of exposure on behaviors since we know that by this point at least some of the affected individuals had connected the jobs lost with free trade by virtue of deciding to apply for TAA. Finally, the determination date might also be useful in cases where the Department of Labor investigator certified the petition. In so doing, the investigator serves as a mouthpiece for authority, confirming that the cause of the layoffs was indeed due to free trade.

We visualize the daily donations between January 1st, 2016 and December 31st, 2018 in Figure 3, highlighting the three dates of interest with vertical black lines. We illustrate the overtime variation that constitutes the second difference in our difference-in-differences estimator by looking at three measures: total donors by day (top panel), total amount donated by day (middle panel), and total number of contributions (bottom panel).

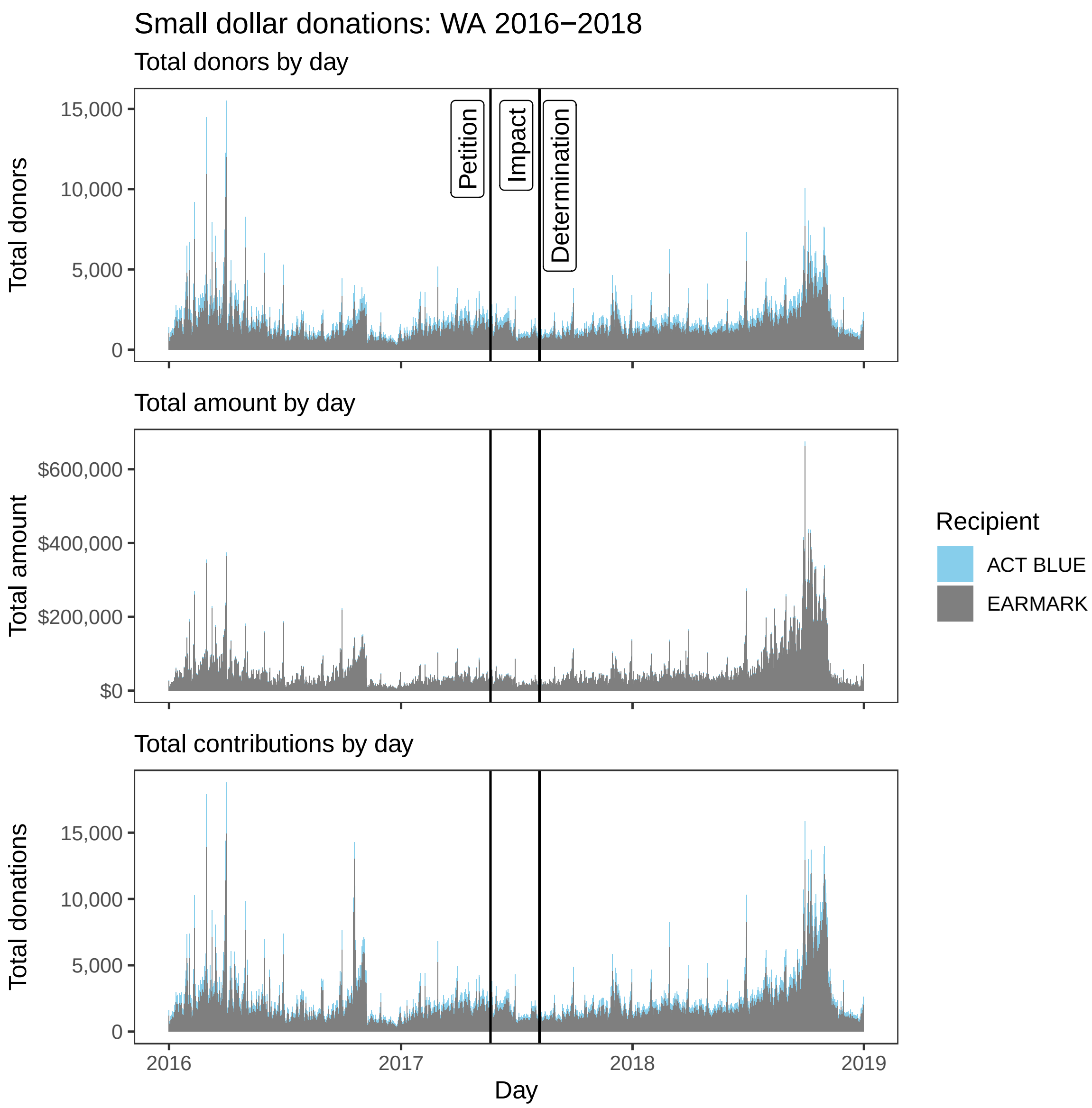


Figure 3 Donations by day, broken out by total donors (top panel), total amount donated (middle panel), and total number of contributions (bottom panel). Each variable is disaggregated by recipient type, coarsened to donations to the ActBlue charity itself, versus donations earmarked for specific recipients.

The figure also highlights several interesting qualities of the data that are worth discussion. First, note that the Boeing layoffs in Seattle occurred after the TAA petition was filed. Specifically, the petition was filed on May 22nd, 2017 and the jobs were not actually terminated until August 7th, 2017. This is consistent with the Worker Adjustment and Retraining Notification (WARN) Act of 1988 which required companies laying off 100 or more workers to provide at least 60 days notice prior to when the layoffs occur, in order to allow those losing their jobs time to find new employment or – as it may happen – file for additional government support through programs like the TAA. Indeed, this is what happened, with the Department of Labor certifying the Boeing layoffs under the TAA on August 8th, 2017, just one day after the layoffs.

The preceding discussion has implications for our identification strategy, since it suggests that our empirical proxy for the trade-related layoff "shock" is potentially lagged relative to when the public actually perceives it. Indeed, analysis of news coverage around this time reveals an initial report on planned layoffs as early as January 11th, 2017 (https://uk.news.yahoo.com/finance/news/boeing-internal-memo-warns-involuntary-002751116.html), and a flurry of coverage between late-March and the end of April 2017 (a search for "boeing layoffs seattle" restricted to news content and between January 1st 2017 and September 1st 2017 produces hundreds of results on Google with a preponderance found between March and June; https://tinyurl.com/mwx25x5c). A Google trends search for the term "boeing layoffs" reveals a spike in search traffic in April of 2017, as illustrated in Figure 4.

As such, a standard difference-in-differences estimator that compares the donation behavior of those living closer to and further from the layoffs, before and after the impact date, risks misidentifying when the shock was actually experienced if the recorded impact date is later than when the public was aware of the layoffs. In this case, we risk defining treated respondents who were already exposed to the layoffs as control. Conversely, setting the date too early (such as January 11th, 2017) and defining the post-treatment period as starting immediately after that date risks defining control respondents who were not yet aware of the impending layoffs as treated. In either case, we risk attenuating the effect estimate by either inflating the difference in the pre-period or compressing it in the post-period, as illustrated in Figure 5.

To accommodate this fuzziness around the timing of the shock, we augment our standard difference-in-differences estimator in several ways. First, we drop 2017 entirely and compare donation behavior in 2018 to that of the same donors in 2016 for Boeing, and 2018 versus 2020 for GMC. Second, we also focus only on 2017 and aggregate the data to weeks, choosing different possible dates for exposure to the layoffs between January and August. Third, we use our measures of labor market fragility to

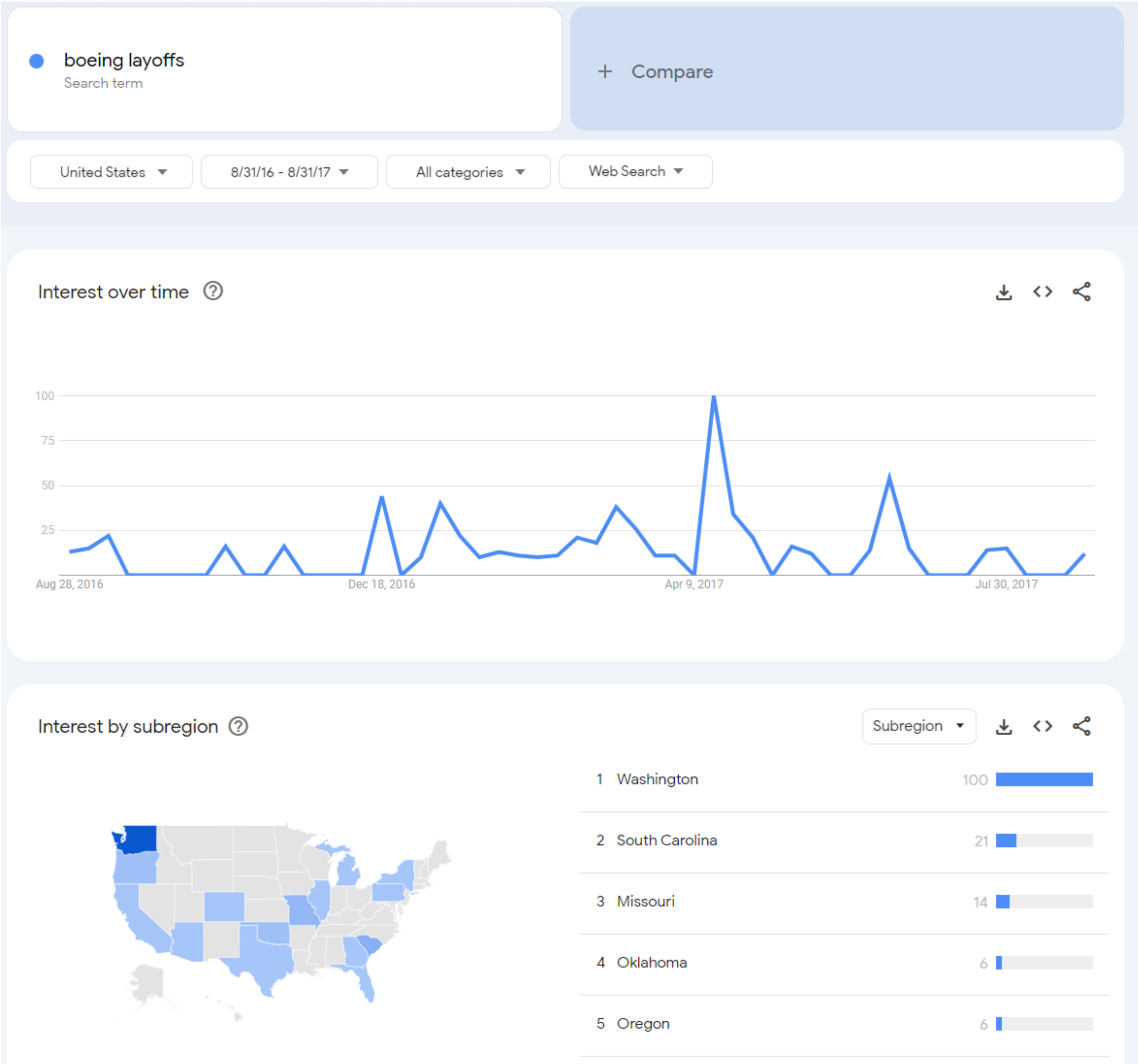
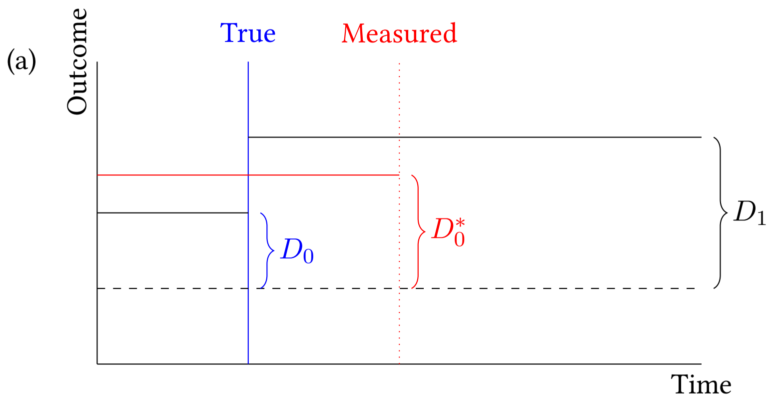


Figure 4 Google trends search results for "boeing layoffs" between August 7, 2016 and August 7, 2017.

examine whether the relationship between donation behavior and distance is stronger among those in more precarious labor market positions, for whom learning about mass layoffs is more threatening.[4]

Our estimation strategy thus relies on a triple-differences specification, in which we combine the before/after difference in exposure to the layoffs that constitute the shock with two measures of exposure to these layoffs: distance from the plant that laid off workers and labor market fragility. In theory,

[4]Going forward, we plan to implement a series of robustness checks in which we set the exposure date to different times in 2017. In addition, we plan to implement weighting strategies to match "exposed" donors to "insulated" on the basis of pre-treatment covariates as well as pre-treatment outcomes, using either the trajectory balancing methods described in Hazlett and Xu (2018) or the matrix completion approach described in Liu et al. (2022). Finally, we will also implement covariate balancing propensity score methods (Imai and Ratkovic, 2014) at the geographic unit (i.e., the county or commuting zone) to ensure comparisons are constrained to donors living in similar types of areas who differ only in their proximity to the layoffs and their pre-treatment labor market fragility.



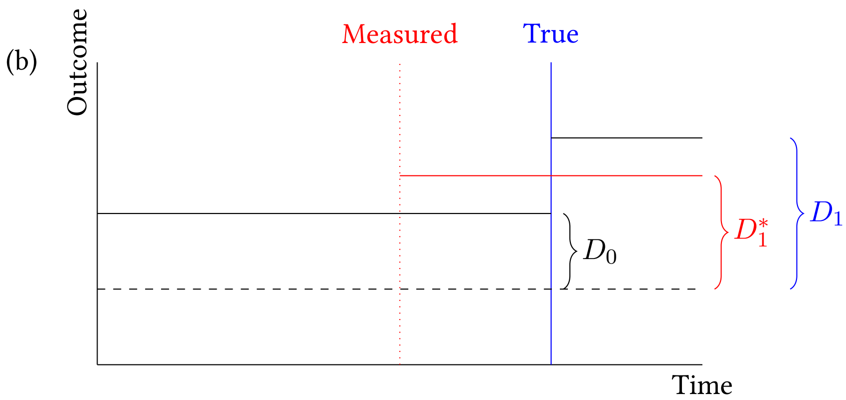


Figure 5 Visualization of identification challenges associated with uncertainty over timing of exposure. In panel (a) if we rely on the impact date as the measured time of the exposure to trade-related layoffs, while the true exposure occurred earlier, our pre-treatment difference between exposed and insulated groups will be biased upward to D_0^*, attenuating our diff-in-diff effect estimate of $\tau^* = D_1 - D_0^*$ toward zero, since $D_1 - D_0^* < D_1 - D_0$. In panel (b), the situation is reversed. If we set the exposure date too early relative to the true exposure, we bias our estimate of D_1 down to D_1^*, again attenuating our diff-in-diff estimator by $\tau^* = D_1^* - D_0 < \tau = D_1 - D_0$.

both distance and labor market fragility identify the donors in the data who are particularly attuned to trade-related layoffs, and are thus more likely to notice the shock, and interpret it as a reflection of the negative consequences of free trade. Although different donors appear different numbers of times in the data, we ensure a proper panel by measuring the total number of contributions made in 2016 and 2018, as well as the total and average amounts contributed by donor in both periods. For donors who don't appear in either the 2016 or 2018 data, we record their outcomes as zero. Thus, with our dataset comprising a panel of donors-by-year, we run the following specification:

$$\begin{aligned} amount_{i,d,o,t} = {} & \alpha_i + \beta_1 close_{i,d} + \beta_2 fragile_{i,o} + \beta_3 post_t \\ & + \beta_4 close * fragile + \beta_5 close * post + \beta_6 fragile * post \\ & + \beta_7 close * fragile * post + \varepsilon_{i,d,o,t} \end{aligned} \tag{1}$$

where i indexes the donor, d indexes their distance from the layoffs, o indexes their occupation, and t indexes the year. The β_7 coefficient thus captures the extent to which donation behavior differs among individuals living within 30 miles from the site of the layoffs ($close$), among those for whom median wages / annual employed fell between 2006 and 2016 ($fragile$), before and after the layoffs ($post$). We test the robustness of our results to different choices of proximity cutoffs as well as different definitions of labor market fragility.

In the Lordstown data, we adopt a similar approach but caveat that our preliminary results compare 2020 to 2018, the latter period being prior to the widespread adoption of WinRed as a donation conduit among Republican and conservative donors. We build the panel in the same fashion as before, imputing zeros for all donors who do not appear in either the pre or the post period. However, for the WinRed donations, all these outcomes are imputed to be zero. Thus the estimate reduces to a cross-sectional comparison in donations to WinRed in 2020 between those living closer to, and further from, the Lordstown layoffs.

5 Results

We start with a series of simpler regression results that build toward the triple differences specification described above, focusing primarily on the Boeing results. First, we predict variation in donation behavior as a function of each component of our triple differences terms independently. Second, we run the set of difference-in-differences interactions, subsetting the data by the hold-out variable (i.e., $post * close$ among fragile and non-fragile workers). Finally, we run the triple differences specification. We iteratively build up to the final specification to elucidate the sources of identifying variation to which we appeal to make our causal claim.

In each of these specifications, we avoid the thorny issues of timing by comparing 2016 to 2018 donation behavior, dropping 2017 entirely. Thus we compare the average total contributions made in 2016 against those made in 2018. In addition, we coarsen geographic proximity to a binary measure of whether the donor lives within 20 miles of the Boeing layoffs, and similarly coarsen labor market fragility to a binary indicator for whether the donor's occupation experienced a decline in average annual wages between 2006 and 2016.

Although these decisions facilitate the ease of interpretation, they also complicate our ability to implement fixed effects. In column 1 of Table 1, we cannot include year fixed effects since these are collinear with the $post$ indicator, but we can include donor fixed effects, thus capturing the change in total donation behavior within donors between 2016 and 2018. In columns 2 and 3, we cannot include donor fixed effects but can demean by year, providing a descriptive snapshot of the cross sectional variation across the two measures of interest: proximity to the layoffs and labor market fragility.[5] In columns 4 through 6 where we run the interacted specifications, we include donor fixed effects when interacting with the $post$ variable, and year fixed effects when interacting the $close$ and $fragile$ variables. All specifications cluster standard errors by the donor.

The coefficients here are directly interpretable, representing total dollars donated relative to the omitted intercept value. For example, column 1 suggests that donors increased their donation amount by $63.76 in 2018 relative to 2016. Substantively, columns 1 through 3 indicate that donations went up over time, are higher among those who live closer to the Boeing plant, and are lower among those working in occupations that had seen a decline in wages between 2006 and 2016.

Of course, these coefficients tell us nothing about how donation behavior might have changed as a function of proximity to the layoffs. The secular overtime increase can be explained by liberals' redoubled efforts to win the House in the 2018 midterm elections after the shock of Donald Trump's 2016 victory. Similarly the positive coefficient on proximity is likely capturing more politically active individuals who live closer to downtown Seattle. And the negative coefficient on fragile labor market positions is consistent with the descriptive patterns discussed above and visualized in Figure 1, suggesting that those in these occupations simply donate less in general.

The interaction terms in columns 4 and 5 provide a more plausibly causal story, although one that requires us to believe that the parallel trends assumptions holds. Nevertheless, column 4 suggests that donors living more than 30 miles from the layoffs increased their donations by $30.10 in 2018 compared to 2016, while those living closer to the layoffs increased donations by an additional $51.51. (Note that, due to the collinearity of the donor fixed effects with our measure of proximity and fragility, the constitutive interaction terms are absorbed into the intercepts.) Conversely, those who worked in relatively

[5] We define both proximity and labor market fragility on the basis of addresses and occupations measured in 2016. As such, there is no over-time variation in these measures.

Table 1 Descriptive regressions of dimensions

Dependent Variable:	Total amount donated					
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
Post	63.76***			30.10***	74.83***	37.13***
	(2.729)			(2.506)	(2.865)	(2.623)
Close		43.80***				
		(2.846)				
Fragile			-6.455			
			(5.061)			
Post × Close				51.51***		57.82***
				(4.685)		(4.922)
Post × Fragile					-199.4***	-136.9***
					(6.760)	(5.886)
Post × Fragile × Close						-94.41***
						(11.27)
Fixed-effects						
Donor	Yes			Yes	Yes	Yes
Year		Yes	Yes			
Fit statistics						
Observations	380,712	380,712	380,712	380,712	380,712	380,712
R^2	0.59511	0.00166	0.00116	0.59528	0.59570	0.59591

Clustered (Donor) standard-errors in parentheses
*Signif. Codes: ***: 0.001, **: 0.01, *: 0.05, .: 0.1*

secure occupations in 2016 increased their total donations by $74.83, while those in fragile occupations actually reduced their donations by almost three times as much over the same two-year period, a decline of $199.40 relative to those in secure occupations. We visualize these latter two difference-in-difference results with marginal effects plots in Figure 6 to aid interpretation.

Finally, column 6 in Table 1 captures the three-way interaction which constitutes our triple difference specification, comparing how those living closer to and further from the layoffs, before and after the layoffs occurred, differed among those in secure and fragile occupations. As illustrated, there is a negative association at the intersection of these three dimensions of exposure to Boeing's layoffs, suggesting that those working in more fragile occupations, living closer to the layoffs, significantly reduced their contributions to ActBlue following the layoffs. Substantively, column 6 indicates that donors living further away and working in secure occupations increased their average annual contri-

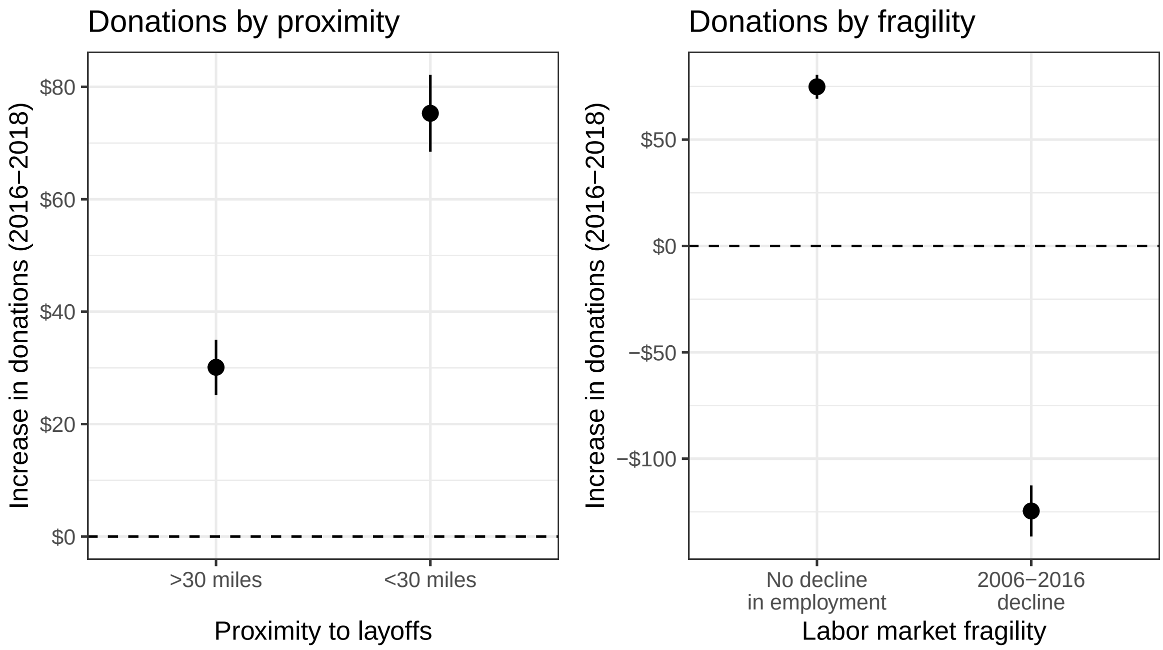


Figure 6 Marginal effects plots visualizing results of columns 4 and 5 from Table 1.

butions to ActBlue by $37.13 between 2016 and 2018, while those living within 30 miles of the layoffs and working in secure occupations increased by an additional $57.82. Conversely, those working in fragile occupations reduced their contributions between 2016 and 2018 by $99.77 ($37.13 - $136.90), and those working in fragile occupations living nearby the layoffs reduced their contributions by an additional $94.41. The net result is a gap in donations to progressive recipients via ActBlue of roughly $260 between those more and less exposed to the layoffs. Figure 7 visualizes these interpretations. In the subsequent robustness checks, we focus on the three-way interaction coefficient which captures the estimand of interest.

5.1 Robustness Checks

The preceding results offer suggestive evidence that exposure to Boeing's layoffs due to free trade corresponded to a decline in support for progressive causes, as proxied with donations to ActBlue. Here, we subject this general pattern to a variety of robustness tests. First, we test the sensitivity of our conclusions to how we measured our outcome variable. Our main results use the sum of the total amount contributed by donor in 2016 and 2018, aggregating over all types of recipients. While work is ongoing to carefully classify each recipient by their policy position vis-a-vis free trade, we can at least

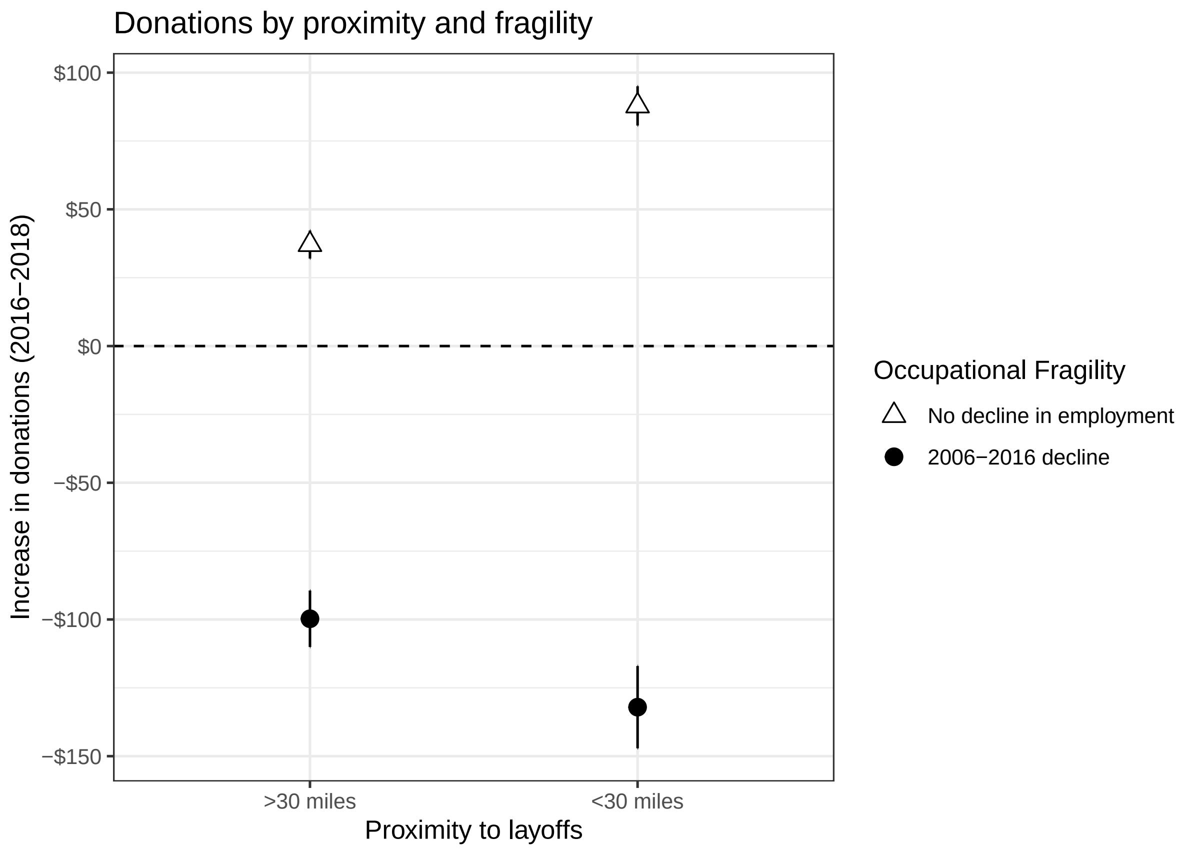


Figure 7 Marginal effects plots visualizing results of column 6 from Table 1.

separate contributions out by whether they are earmarked for a specific recipient or are just given to ActBlue itself. In addition, we can instead calculate the total number of contributions instead of their dollar value. Similarly, we replace the summed outcome measures with averages, capturing the average amount donated and number of donations per day in 2016 to 2018 by donor. Finally, we test robustness to using logged measures of the contributions instead of their raw value.

Table 2 presents the results of the triple difference specification used in column 6 of Table 1. As illustrated, our conclusions are largely robust to these choices of outcome measure, with the exception of donations made directly to ActBlue, which exhibit no correlation with exposure to the layoffs as defined by our triple difference specification. Additional work to code the specific recipients of each donation is underway.

Second, we test the sensitivity to alternative measures of labor market exposure. For example, instead of measuring labor market fragility using the 2006 to 2016 decline in the number of jobs, we measure it using the decline in real wages by occupation. Alternatively, we define labor market expo-

Model:	Total Amount ($) (1)	(2)	Avg. $ (3)	Avg. conts (4)	Tot. Ear (5)	Tot. Act (6)
Variables						
Post	37.13***	0.3895***	0.3010***	0.0589***	0.3841***	0.0566***
	(2.623)	(0.0159)	(0.0114)	(0.0027)	(0.0158)	(0.0051)
Post × Fragile	-136.9***	-3.773***	-2.697***	-0.6734***	-3.739***	-0.7181***
	(5.886)	(0.0340)	(0.0246)	(0.0050)	(0.0338)	(0.0192)
Post × Close	57.82***	0.2255***	0.1557***	0.0334***	0.2250***	0.0403***
	(4.922)	(0.0200)	(0.0144)	(0.0033)	(0.0198)	(0.0065)
Post × Fragile × Close	-94.41***	-0.2707***	-0.3084***	-0.0141*	-0.2734***	0.0107
	(11.27)	(0.0429)	(0.0313)	(0.0062)	(0.0427)	(0.0238)
Log Outcome	N	Y	Y	Y	Y	Y
Fixed-effects						
Donor	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics						
Observations	380,712	380,712	380,712	380,712	380,712	380,712
R^2	0.59591	0.26214	0.23010	0.13938	0.26338	0.52045

Clustered (Donor) standard-errors in parentheses
*Signif. Codes: ***: 0.001, **: 0.01, *: 0.05, .: 0.1*

Table 2 Robustness of results to different measures of the outcome, including logged measures (columns 2 - 6), the average donation amount (column 3), the number of total contributions (column 4), the amount earmarked for a specific recipient (column 5), and the amount donated to the ActBlue PAC (column 6).

sure to Boeing's layoffs on the basis of whether the donor worked for Boeing or one of the major US airlines (United, American, and Delta), or worked in aerospace, broadly defined. This categorization requires donors to describe their occupation and / or their employer using the keywords on which we base our regular expression search. Nevertheless, we expect that workers in these occupations or for these employers are more likely to have been exposed to the Boeing layoffs. Our main conclusions persist with these alternative measures of labor market fragility, as illustrated in Table 3, with the exception of the aerospace workers whose negative interaction term is not statistically significant (column 3). While the noisier estimate may be due to the fewer treated observations we have, it is also plausible that geographic proximity should not matter as much for these workers, since they are likely to learn about the layoffs through professional networks regardless of where they live. We provide descriptive evidence in support of this interpretation by re-running the specification from column 1 on the data subset to the 790 donors who work in aerospace (column 4) versus those that do not (column 5). As illustrated, the triple interaction term in column 4 is positive and noisily estimated, suggesting that geographic proximity doesn't matter for these donors, although we again caution against an over-interpretation of patterns based on this small subset. Conversely, among those for whom knowledge of the layoffs is less likely to flow through social networks, the triple interaction term is slightly more negative.

Dependent Variable:	Total amount donated				
	Job loss	Wage loss	Aerospace	Aero subset	Non-Aero
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
Post	37.13***	37.73***	30.45***	-96.62	37.34***
	(2.623)	(2.627)	(2.509)	(66.89)	(2.625)
Post × Fragile (emp)	-136.9***			-90.60	-135.7***
	(5.886)			(80.55)	(5.917)
Post × Close	57.82***	57.92***	52.24***	-18.72	58.14***
	(4.922)	(4.965)	(4.704)	(70.52)	(4.931)
Post × Fragile (emp) × Close	-94.41***			71.28	-96.15***
	(11.27)			(85.58)	(11.60)
Post × Fragile (wage)		-138.9***			
		(6.355)			
Post × Fragile (wage) × Close		-57.26***			
		(11.24)			
Post × Fragile (aero)			-158.5***		
			(46.49)		
Post × Fragile (aero) × Close			-50.13		
			(48.78)		
Fixed-effects					
Donor	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	380,712	380,712	380,712	1,580	379,132
R^2	0.59591	0.59586	0.59533	0.70960	0.59586

Clustered (Donor) standard-errors in parentheses
*Signif. Codes: ***: 0.001, **: 0.01, *: 0.05, .: 0.1*

Table 3 Robustness of results to different measures of the measure of labor market fragility, including occupations with a net decline in jobs between 2006 and 2016 (column 1); occupations in aerospace or those who work for Boeing, Delta, United, and American Airlines (column 2); and occupations that saw a decline in real wages between 2006 and 2016 (column 3).

Taken together, our broad conclusions are robust to myriad different choices for measuring both the outcome and labor market fragility. Nevertheless, the preceding results should still be met with a fair degree of skepticism, especially when it comes to a causal interpretation attributable to layoffs. In particular is our decision to coarsen both time and geographic distance. The former decision was motivated by the aforementioned uncertainty over when, exactly, news of the impending layoffs became widely known. To probe the sensitivity of our conclusions with respect to this decision, we subset our data to only 2017 and aggregate to weeks, allowing us to compare different choices of the date by which the pre/post binary treatment is constructed. Given the multiple observations for each donor in the pre and post layoff periods, we include quadratic time trends for both the full period as well as the post period, resulting in an interrupted time series specification.[6] We visualize the results in Figure 8, confirming the conclusions drawn above, with estimates that are more noisily measured, but the strongest evidence of a decline occurring around April of 2017.

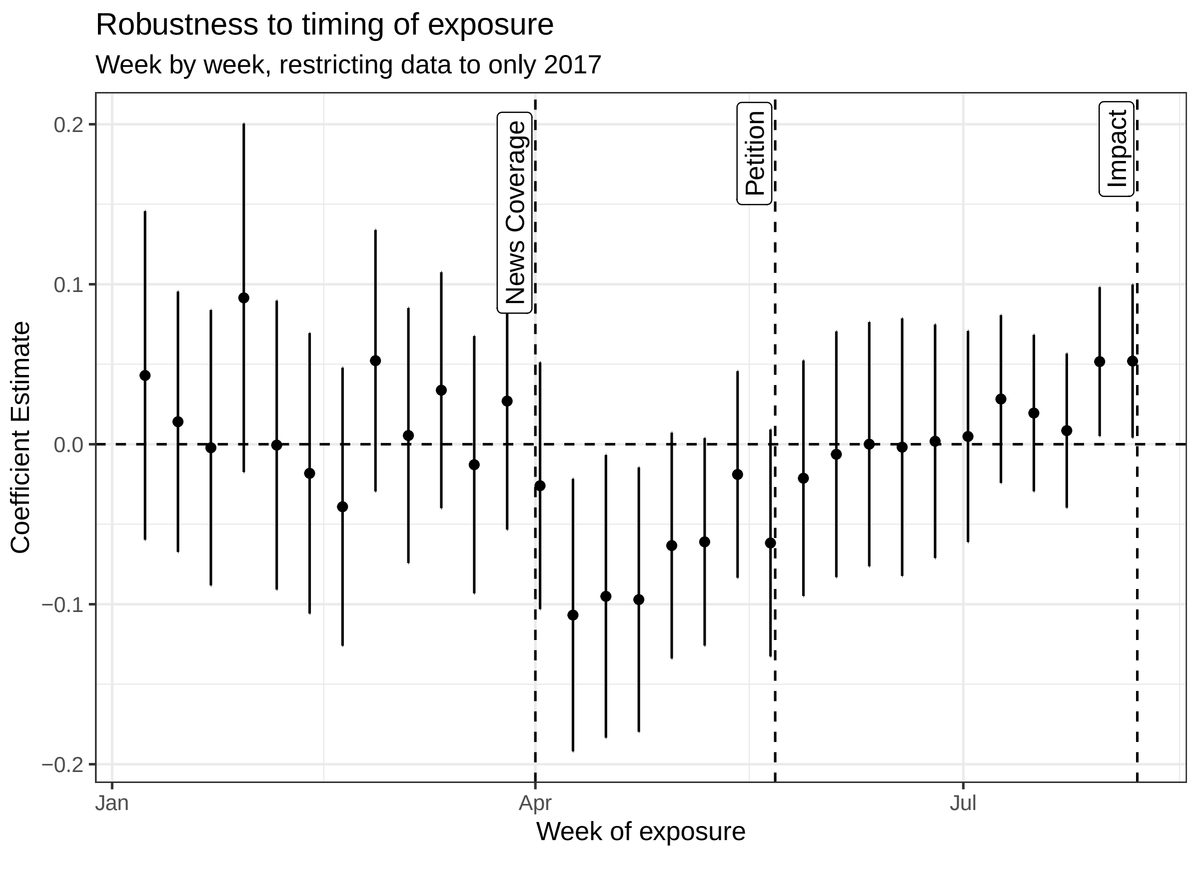


Figure 8 Strength of results to different choices of timing threshold to define when the public became aware of the impending layoffs.

Our decision to define "closeness" as within 30 miles affords a similar sensitivity test. The choice of 30 miles in the main results is motivated by a back-of-the-envelope calculation of common commute times in the Seattle area, for which 30 miles is an outer bound capturing driving times of 1 to 2 hours,

[6]The patterns are robust to a linear implementation of the time trends.

depending on traffic. Obviously, this is a reductive operationalization of geographic proximity, especially for donors living north of downtown Seattle for whom even 30 miles is a longer trip than those living south of the Boeing plant. We evaluate the robustness of our results to different choices of this threshold, visualized in Figure 9, finding that the strongest evidence of a pull away from progressive donations is between 10 and 50 miles away from the layoffs. Defining the proximity cutoff as either further than 50 miles or closer than 10 miles likely either mis-assigns unexposed donors to the exposed group, or mis-assigns exposed donors to the unexposed group.

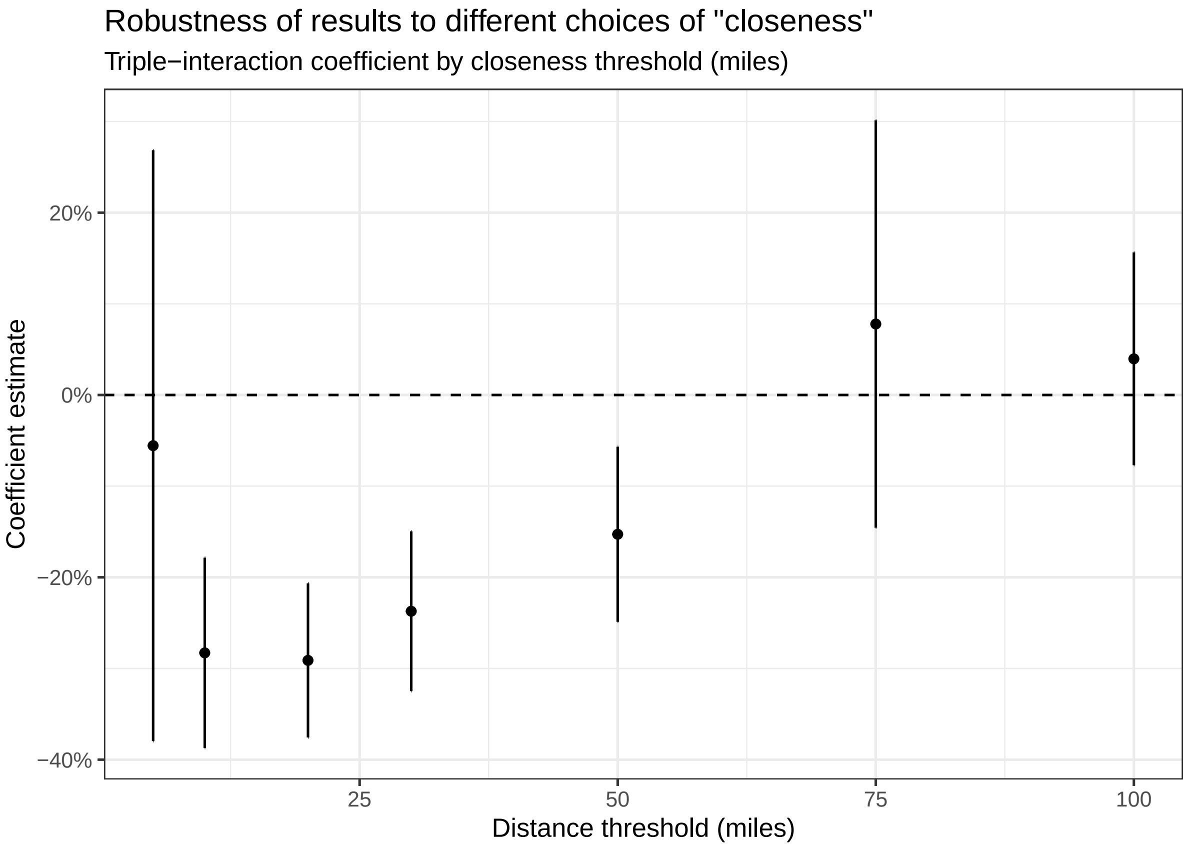


Figure 9 Strength of results to different choices of distance threshold to define proximity to Boeing layoffs.

5.2 Lordstown

We now turn to our analysis of the Lordstown layoffs. As before we use a distance of 30 miles to define exposure, and again implement donor fixed effects. For donations to ActBlue, the estimation strategy is identical to the above. However, for donations to WinRed, the pre-layoffs donations are all zero since WinRed hadn't been widely adopted in 2018.

Figure 10 visualizes our preliminary results, starting first with all donations before disaggregating by recipient. As illustrated, the overall relationship between exposure and donations is negative, consistent with the results summarized above. Furthermore, the strength of the negative association increases dramatically when we subset the donations to only those made to ActBlue. Finally, we find an opposite estimate for donations made to WinRed. However, the interpretation of this coefficient can only be understood as a comparison between those living closer to, and further from, the site of the layoffs.

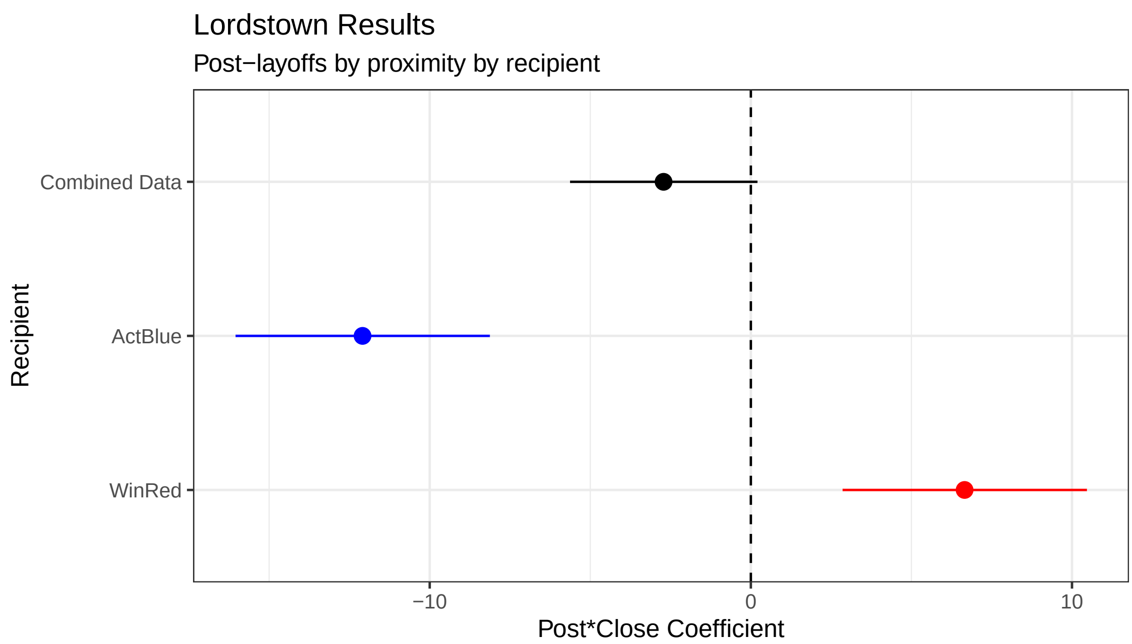


Figure 10 Interaction coefficient estimates between post-layoffs at Lordstown and proximity to the layoffs of donors, disaggregated by recipient (ActBlue in blue, WinRed in red).

6 Conclusion

While more work remains on this project, our initial analysis suggests that small dollar donors moved away from progressive political groups when exposed to the negative consequences of free trade, as operationalized by Boeing's layoffs of more than 5,000 workers at its Seattle-based plant, and General Motors Corporation's decision to shutter its factory in Lordstown, OH. Going forward, we intend to further improve our causal interpretation of these empirical patterns through additional methodological tests, improved measures of labor market position and exposure.

In addition, we plan to implement two additional extensions. First, we will expand our sample for both Boeing and GMC to encompass the full country. Among these donors we will additionally estimate the effect of "exposure" defined as working for either Boeing or GMC, regardless of where the donor lives. Second, we plan to identify one final set of layoffs that occurred on or after 2020, such that we can fully implement the diff-in-diff strategy applied to the Boeing layoffs, and to the Lordstown layoffs with respect to the ActBlue donations. This final case study will afford a confirming test of the conclusion that it is not simply a net decline in donations that is associated with trade-related layoffs, but a politically bias shift away from Democratic recipients and towards Republicans.

For now, we are open to and grateful for any and all comments and suggestions.

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