# Internet Appendix

# for

# *Choose Your Battles Wisely: The Consequences of Protesting Government Procurement Contracts*

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### *A.1 Bid Protest Dockets at the GAO*

In this section, we provide details on how the GAO categorizes the bid protests it receives from businesses (see, e.g., Figures A.I and A.II). The GAO gathers related protests in the same docket and assigns an index number to each. Each bid protest is assigned a unique 8-digit number referred to as a button number or "B-number" (e.g., B-123456.01). The first six digits of each B-number refer to the docket (e.g., B-123456) and the last two digits refer to the index number (e.g., 01). The first protest in docket B-123456 would thus be assigned a B-number of B-123456.01.

[Figures A.I and A.II about here]

The protesting firm's attorneys are often privy to the protested agency's sensitive procurement documents which often give rise to additional grounds for protest. To increase the likelihood of a sustained protest, protesting firms are incentivized to file protests on multiple legal bases. A subsequent protest filed for the same docket (either by the incumbent protestor or by a new protestor) is assigned B-number B-123456.02, and so forth. Each B-number is linked with a protesting firm's name, protest filing date, GAO decision date, name of the protested government agency, related solicitation, or request for quotation (RFQ) number, the GAO's final decision on the protest, and the GAO's explanation for its decision.

Since the GAO groups bid protests against the same contract in the same 6-digit docket number, we can easily identify firms that protest the same contract or proposed contract award. Button number B-295663.01, for example, maps to a bid protest filed by ELR Consultants, LLC on January 12$^{th}$, 2005 for solicitation number DE-RP24-04OH20179, which was solicited by the Department of Energy. On January 25$^{th}$, 2005, GAO's decision on B-295663.01 was finalized, and ELR Consultants, LLC was informed that its protest was dismissed because the firm was not an interested party. There were 11 subsequent bid protests filed on the same docket number. Four of these (B-295663.03, B-295663.05, B-295663.08, and B-295663.10) yielded corrective actions; others were dismissed or withdrawn. Section A.6 of the Appendix provides our findings on solicitations that incorporate the protested contracts.

### *A.2 Additional Summary Statistics on Power Outages and Bid Protests*

We start this subsection by providing additional summary statistics on power outages. Panel A of Table A.I presents yearly standard deviations of power outage characteristics previously described in Table II. As shown, the time-series averages of *Stdev. of Affected Customers*, *Stdev. of Length of Power Outage*, *Stdev. of Prob. of Protest Day Power Outage*, and *Stdev. of Prob. of Power Outage* are 0.26, 3.78, 3.83%, and 2.50% respectively. Panel B of Table A.I details the drivers of power outages for each year in our sampling period. We categorize drivers of power outages as *Weather Conditions*, *Technical Issues*, and *Natural Disasters*. To do so, we utilize the variable *EventType* from EIA's major electric disturbances and unusual occurrences dataset. Our label *Weather Conditions* refers to, e.g., wind, rain, and snow, that drive major and unusual electric disturbances as defined by EIA.

In line with Federal Emergency Management Agency (FEMA) and presidential disaster declarations, we categorize Tropical Storm Ernesto, Hurricane Sandy, Hurricane Ike, Hurricane Dolly, Winter Storm Nemo, Tropical Storm Fay, and earthquakes as *Natural Disasters*. *Technical Issues* refer to equipment trips and failure, cyberattacks, suspected vandalism, and other mechanical problems. As shown in Panel B, weather conditions, technical failures, and natural disasters drive 73.35%, 20.68%, and 5.98% of power outages on bid protest days on average, and they exhibit time series heterogeneity in addition to the spatial heterogeneity documented earlier in Figure II. For example, in 2014 (2007), 88.64% (100%) of power outages on bid protest days are driven by weather conditions, 6.82% (0%) are driven by technical issues, and 4.55% (0%) are driven by natural disasters.

[Table A.I about here]

Panel C of Table A.I presents summary statistics on the number of lawyers supporting the bid protester firms, GAO, and government agencies along with the number of pages and characters in the GAO response documents. To provide empirical evidence on the relation between power outages and bid protest quality, we conduct a textual analysis of GAO's response documents to bid protests. We scrape all public GAO

response documents for the period 2015–2016 available from GAO's webpage. These documents not only summarize the bid protest reports submitted by the bid protesters but also explain the justifications behind GAO's decisions on the corresponding cases. In an average bid protest, the bid protester firm has 4.34 lawyers, the government agency has 2.47, and the GAO has 2.09. The average GAO response document consists of 10.42 pages, which is close to the representative bid protest document we received from an anonymous bid protester.

The second set of data in this subsection allows us to compare firms that submit bid protests at the GAO against firms that prefer other venues. To obtain non-GAO cases on government contracting, we draw data from Thomson Reuters Westlaw. In doing so, we obtain data on the following Westlaw variables: *Case Title, CourtLine, FiledDate, Court Docket, Citation,* and *Summary*. Going over case summaries, we see that only a small number of Westlaw cases refer to or contain information about GAO protests. Therefore, we match the Westlaw data with our sample using firm names rather than cases. More specifically, we match firm names from the Westlaw dataset with firm names from GAO and USASpending datasets. This allows us to compare characteristics of firms that exist (i) only in the GAO dataset, (ii) only in the Westlaw dataset, and (iii) in both datasets.

[Figure A.III about here]

Figure A.III shows proportional Venn diagrams of the number of cases involving firms that exist in GAO and non-GAO universes such as the Court of Appeals, Court of Claims (CFC), Court of International Trade, District Courts, and the Supreme Court. These courts are labeled in the figure as *Other Venues*. As shown in the figure, there are 10,315 cases in the merged universe. 8,682 of these cases belong to firms that only protest at the GAO, 721 cases belong to firms that only protest at non-GAO venues, and 912 cases belong to firms that protest at both GAO and non-GAO venues. This figure confirms that the GAO is the most prominent venue for bid protesters.

[Figure A.IV about here]

Figure A.IV presents the number of cases per year at the GAO and non-GAO venues. To prevent duplication, we tabulate the time series of the 721 cases belonging to firms that only protest at non-GAO venues against the time series of all GAO cases including the 912 cases belonging to firms that protest at both GAO and other venues. As mentioned in Section 2.2, we identify an average of 91 CFC cases per year before matching the Westlaw data with GAO and USASpending.gov datasets. As shown in Figure A.IV, on average 52.25 of these cases belong to firms that are only in the Westlaw universe and can be matched with the USASpending.gov universe using firm names. As shown in the figure, CFC is consistently the second most popular venue after the GAO. Collectively, these figures provide important information on different venues that hear procurement-related cases or protests under their jurisdictions. As expected, the GAO is consistently the most popular bid protest venue.

Do firms that protest procurement contracts at the GAO differ from firms that protest procurement contracts at other venues? Table A.II presents key summary statistics on firms that exist (i) only in the GAO dataset, (ii) only in the Westlaw dataset, and (iii) in both datasets. Panel C firms are larger in terms of the size of government contracts they receive (along the intensive and extensive margins) but firms from all panels are roughly comparable in terms of most business-type designations. Panel D of Table A.II. shows summary statistics at the case level. To compile these, we manually download 3,404 case summary documents from Westlaw, covering all the cases from Panels A to C in Table A.II. We conduct textual analysis on these documents.

To determine the outcomes of the cases, we first locate the conclusion section in each document, right before the "all citations" part. We search for key terms such as "denied," "rejected," "dismissed," and "remanded" to classify case outcomes. We find that 45.45% of cases are denied, indicating judges do not approve the requests of the protesting firms. Another 3.7% are rejected, 15.92% are dismissed (often due to missing evidence, procedural errors, or jurisdictional issues), and 5.61% are remanded, i.e., sent back for further action or a new trial, often due to procedural errors or issues identified by the appellate court. In total, 70.68% (65.07%) of the cases are not successful, including (excluding) the remanded cases.

Understanding the economic stakes of each case is more challenging. Case documents often do not provide useful financial information, especially when the case is dismissed due to procedural or contractual issues. Furthermore, the complex and evolving details of each case introduce additional complexity. For example, the case of *Q Integrated Companies LLC v. United States* involves several types of financial costs, including bid preparation and labor costs. Initially, Q Integrated seeks $71,384.52 in direct labor costs, $42,354.00 in indirect labor costs, $28,894.99 in consultant costs, and $2,831.41 in other costs related to travel, copying, printing, and shipping with regard to the procurement. However, they later reduce their labor cost claims in response to objections. In general, final costs are documented in later parts of the same document.

To find financial details of each case, we identify paragraphs that contain monetary amounts—specifically, those with '$' and 'cost' or '$' and 'contract'. We examine these paragraphs with two dictionaries in hand. The first dictionary is all about costs, featuring terms like 'total cost', 'damages', and 'labor cost' among others. The second dictionary is on contract amounts, containing phrases like 'contract amount' or 'award'. We focus on cost-related phrases, but if those are not available and we only see contract terms, we assume that the case's financial significance can be estimated by the mentioned contract value.

If we come across several mentions of cost phrases in a given case, we use the latest one. This approach is based on us noticing that the initial financial figures are often higher and then adjust down as the case progresses. Following this method, we arrive at an average logged cost of 12.46 and a median of 12.75. These numbers are winsorized at 2.5% in both tails. When we focus on the first instances instead, those averages come out slightly higher, at 13.07 and 13.54, respectively.

[Table A.II about here]

The last piece of information we present in this subsection is key summary statistics on the merged GAO and FPDS universe. To collect detailed information on the protested contracts, we match the GAO data with procurement data from the FPDS. Our GAO data provide us with solicitation identifiers of

protested contracts. GAO's data on solicitation identifiers are not fully consistent. In particular, in some observations, we see requests for quotes or parts of solicitation numbers (rather than entire solicitation numbers) being reported. Using FPDS API and *ezsearch* portal, we scrape all government contracting information associated with the solicitation identifiers in hand. This provides us with detailed data on all *contracts* and *contractors* associated with the linked solicitations. The data we scrape spans the period between 2005 and 2022 and contains pre-protest and post-protest contracting data on 3,503 matched solicitations. We report summary statistics on these data in Table A.III.

[Table A.III about here]

As shown in Table A.III, 6.41 contractors work on the protested solicitations on average. *Log(Government Expenditure on Protested Solicitation)* measures how much the government spends on a protested solicitation per year after the bid protest. It has an average of 0.36, which is sizable relative to the average contract amount bid protesters receive from protested agencies per year (see, e.g., *Logged Protested Agency Contract Amt.*$_{+1}$ in Table III, which has a mean of 0.73). The remaining variables suggest that the solicitations that are scraped from the FPDS server belong to firms that are comparable to the rest of the sample in terms of business designations but are more active than the rest of the firms in our broader sample, as reflected in the probability of receiving contracts. Section A.6 provides our empirical findings from analyzing the merged GAO and FPDS universe.

### *A.3 Supporting Evidence on the Validity of Our Instrument*

This section provides robustness tests on our instrument and supporting evidence for our modeling assumptions. We start with running the 2SLS regressions reported in Table IV with a different instrument, i.e., the number of electricity customers that experience power outages on the bid protest day divided by the number of residential electricity customers in the protester firm's state. We present our findings in Table A.IV. As shown in column (1), we find a 100% increase in the probability of local residential electricity customers experiencing a power outage on a bid protest deadline decreases the probability of getting a corrective action by 7% (t-stat=-3.73). As shown in column (2), a corrective action decision decreases the

probability of receiving government contracts from the protested agency in the protest year by 42%(t-stat=-3.25). This provides additional support for the *Retaliation* hypothesis by demonstrating the negative influence of corrective action decisions on government contract allocation with a different instrument.

[Table A.IV about here]

As summarized in Panel A of Figure A.VII, our main empirical methodology sheds light on the magnitude of the local and homogenous effect of corrective action decisions on future contracting arrangements. The necessary assumptions of our 2SLS procedure are relevance, independence, and exclusion. We address the instrumental relevance assumption by using F-tests with well-known rules of thumb (Staiger and Stock 1997, Stock and Yogo 2005, Lee et al. 2022). We also show that the 2SLS coefficient attains a t-stat greater than the critical tF value for a 5% test. For the independence assumption, we rely on the as good as random variation in being exposed to an unprecedented power outage at the time of a bid protest submission.

[Figures A.VII and A.VIII about here]

The exclusion restriction assumption states that our instrument, *Prob. of Protest Day Power Outage*, should not affect the outcome variable, future contracts, through channels other than corrective action decisions. Because of our fixed effects structure (i.e., docket, year × month, agency × year, and state × year fixed effects), backdoor paths through which power outages can influence future contracting arrangements are limited. Figure A.IX compares the characteristics of firms that were exposed to power outages during their bid protests against the characteristics of firms that were not. It presents the differences in firm characteristics and local economic activity firms are exposed to, along with 90% confidence intervals. As shown, these two groups of firms are balanced along observable dimensions such as previous contracting activities, logged sales, sales growth, logged employee count, employee growth, business-type designations, contemporaneous local economic growth, and unemployment rates.

[Table A.V about here]

Table A.V presents our findings from regressing our instrument (*Prob. of Protest Day Power Outage*) on state-level (quarterly or monthly) economic outcome variables. As potential economic predictors of power outages, we use logged values of one plus percent change in state-level GDP, percent change in personal income, labor market participation rate, and unemployment rate. We chose these variables because they are likely first-order confounders to the relation between power outages and contracting outcomes. Table A.V presents our findings. As shown, we find that the economic outcome variables above do not predict our instrumental variable.

Another common concern with the 2SLS procedure is compliance with the treatment assignment. More specifically, the effect we are measuring with the second-stage coefficient could be confounded by firm-level characteristics or decisions. For example, firms with fewer resources, firms for whom the outcome of the protest is less important, incompetent firms that do not have a backup generator, or firms that expect their protests to be ineffective could show less effort, have weaker capacity, or write worse bid protests and end up receiving fewer corrective action decisions.

To tackle concerns related to compliance, we take an approach similar to Abadie, Angrist, and Imbens (2002) to compute the characteristics of compliers. Compliers in their setting are employees who are offered a training program and accept it. Our setting is somewhat different in that compliers are firms that file unsuccessful protests and are exposed to power outages.[1] We compare firms that (i) file a successful protest and are not affected by power outages, (ii) file unsuccessful protests and are not affected by power outages, (iii) file a successful protest and are affected by power outages, and (iv) file unsuccessful protests and are affected by power outages. Comparing differences between groups (i) and (ii) relative to differences between groups (iii) and (iv) allows us to assess whether some of these potential compliance issues can contaminate our findings.

[1] If our setting were directly comparable to Abadie et al. (2002), then some firms in our sample would be offered the opportunity to protest and others would not. This would be the assignment condition. Firms that file protests would be treated. Compliers, therefore, would be firms that were allowed the opportunity to protest and accepted the opportunity.

[Figure A.X about here]

Figure A.X presents our results from comparing observable firm characteristics. As shown in the first row of the figure, we do not find a statistically significant difference in last year's contracts between successful and unsuccessful protesters that are *unexposed* to power outages. As shown in the second row of the figure, we also do not find a statistically significant difference in last year's contracts between successful and unsuccessful protesters that are *exposed* to power outages. Importantly, the confidence intervals for the first and second rows of the figure intersect, which suggests that the difference in differences (between groups (i) and (ii) above relative to differences between groups (iii) and (iv) above) is also statistically insignificant. The remaining parts of the figure present similar results on business-type designations. Collectively, Figure A.X provides support for the validity of our instrument by showing evidence that reduces the possibility of firm-level characteristics contaminating our results.

### *A.4 Bid Protest Starters*

In this subsection, we investigate how corrective action decisions impact bid protest starter firms. To the extent that our results reflect bias against protesting firms that create additional work for procurement officers, we predict that any negative reputation effects could accrue primarily to the earliest protesting firms. To test this prediction, we group bid protesters into two subsamples: protest starters and protest followers. We categorize the firms filing the first three protests in each GAO docket as protest starters.

[Table A.VI about here]

We present our results in Table A.VI. As shown in column (1), we find a 100% increase in the probability of a power outage on a bid protest deadline decreases the probability of getting a corrective action by 55% (t-stat = –5.94). As shown in column (2), a corrective action decision decreases the probability of receiving government contracts from the protested agency in the protest year by 46% (t-stat = –3.25) exceeding the 42% reduction reported in Panel A of Table IV. Once again, the t-stat of our 2SLS coefficient exceeds the critical tF value for a 5% test.

We conclude that *starting* a bid protest docket and receiving corrective action from the GAO against a government agency causes economically and statistically significant distortions on future government contracts. This result is important as it provides further evidence for the *Retaliation* hypothesis even after using tighter benchmarks in terms of the speed at which firms protest.

Another important conclusion from this analysis pertains to the role of the control group in generating 2SLS estimates in our main analysis. It is possible that the control group is treated, in the sense that *not* being subject to a power outage improves the probability of winning a corrective action. This effect potentially violates the SUTVA assumption, making the interpretation of the treatment "winning a corrective action" hard to understand since it combines the effect for the treated and control. Table A.VI circumvents this concern, as all firms in this subsample rapidly protest the same solicitation. Therefore…

### *A.5 Revisiting the Table VII Sample*

In this section, we run our 2SLS procedure on the subsample of firms we utilize in Table VII. As shown in column (1) of Table A.VII, we find an economically and statistically significant result in the first-stage regression. In particular, we obtain a reduction of 55% in the probability of receiving a corrective action due to a 100% exposure to a power outage. As shown in column (2), we find that a corrective action decision reduces the probability of receiving government contracts from the protested agency in the protest year by 42%, and the t-stat of our 2SLS coefficient exceeds the critical tF value of 2.51 for a 5% test.

[Table A.VII about here]

### *A.6 Analyses of Protested Solicitations*

In this subsection, we delve deeper into analyzing the contracts that get protested at the GAO. To do so, we use the merged GAO-FPDS-USASPending.gov dataset previously described in Table A.III and explained in Appendix Section A.2. We start by running our 2SLS procedure from Section 4 on the subsample of solicitations that exist in the merged GAO-FPDS-USASPending.gov universe. This allows us to test whether our findings from the broader GAO sample hold within the GAO-FPDS subsample. We

then examine whether receiving a corrective action decision increases or decreases the chances of receiving more contract awards *under the protested solicitations* in addition to the overall effects we previously identified on (broader) agency-level contracting decisions.

[Table A.VIII about here]

We present our findings in Table A.VIII. Column (1) presents our results from running first-stage regressions. The estimated coefficient of interest on *Prob. of Protest Day Power Outage* is equal to -0.39 (t = –4.49). Column (2) presents results from our second-stage regressions. As shown, the estimated coefficient of interest on *Instrumented corrective action* is equal to –0.50 (t = –3.23). This provides additional support for our *Retaliation* hypothesis and suggests that receiving a corrective action from the GAO against a government agency drives economically and statistically significant distortions in next year's government contracts from the protested agency in the GAO-FPDS-USASpending subsample.

The third column of Table A.VIII presents results from using *Received Future Contracts From Protested Solicitation* as a dependent variable. This variable indicates whether a protester received future government contracts under the solicitation it protests. We control for *Previous work on Protested Solicitation*. This variable is equal to one if a bid protester received a government contract within the protested solicitation in the year of the protest, and it aims to control for whether existing work is being done under the solicitation. Therefore, it helps us with potential concerns related to treatment compliance or capacity constraints. As shown in columns (4) and (5), we identify second-stage coefficients of –0.35 and –0.26 under different specifications. Collectively, our findings from the merged GAO-FPDS-USASpending dataset suggest that corrective action decisions distort contracting arrangements at the solicitation level, as well.

***A.7 Dockets with a Single Protest***

The main findings of our paper are based on the comparison of firms that protest the same solicitation. This strategy allows us to use power outages to estimate the local treatment effect of corrective action decisions on future contracting arrangements. Nonetheless, many bid protests in our sample are single-bid protests that get dropped from our regressions due to docket fixed effects, which our identification strategy

relies on. In this section, we provide our findings from estimating a local treatment effect of corrective action decisions on future contracting arrangements of single protesters.

To do so, we match single protesters with observably comparable government contractors from the USASpending universe. Our purpose is to come up with synthetic counterfactual values for the dependent and control variables of single protesters. In doing so, we also make sure that the synthetic counterfactuals have the same probability of getting corrective actions and being exposed to power outages on average. To obtain counterfactual values, we first identify government contractors that receive government contracts from the protested agencies in the year of bid protests. We then keep those contractors that have the same key business-type designations (i.e., Small Business, Women-owned Business, Veteran-owned Business, and Minority-owned Business) as the single protester firm. This approach provides us with comparable contractors to single protesters that do not submit bid protests.

For the observable counterpart of the counterfactual dependent variable, i.e., Y(0), we calculate matched contractors' average *Received Protested Agency Contracts*$_{+1}$ value so we can compare it to the single protester's *Received Protested Agency Contracts*$_{+1}$ value. Since differences in firm-level characteristics between single protesters and matched contractors can contribute to differences in *Received Protested Agency Contracts*$_{+1}$, we also calculate the average *Received Government Contract* and *Received Government Contract*$_{-1}$ values of the matched contractors and use them as control variables in our 2SLS procedure.

To obtain counterfactuals for the instrument and corrective action decisions, we randomly assign power outages and corrective actions so that the mean *Prob. of Protest Day Power Outage* and C*orrective Action* values for the counterfactual protesters are equal to the average values in our sample. These provide us with factual outcomes from single protesters and counterfactual outcomes from observably comparable contractors. We compare these outcomes using the 2SLS procedure explained in Section 4.

[Table A.IX about here]

We present our findings in Table A.IX. Column 1 of the table presents our findings from instrumenting corrective action decisions with the probability of being exposed to a protest day power outage. As shown, we estimate a coefficient of -0.25 that suggests a reduction in the possibility of getting a corrective action decision of 25%. As shown in column 2, the estimated coefficient of interest on *Instrumented corrective action* is equal to –0.18 and is statistically significant. This result is robust to controlling for docket, year × month, state × year, and government-agency × year fixed effects. In untabulated results, we once again find the OLS estimate to be more positive than our 2SLS estimate.

Column 3 of Table A.IX presents our findings from employing a matching estimator as an alternative to the 2SLS method for analyzing single protestors. In order to assess the impact on this group, we initially pair single bid protestors with other contracting firms that also challenge government contracts but have not been granted any corrective actions. We refine this match by ensuring that these firms share the same business classification and that the contracts they protest are within 95% to 105% in value of those protested by the single protestor firms. This stricter matching strategy leads to a reduction in the number of observations from 14,832 to 11,634. To derive a counterfactual for the dependent variable, we calculate the average value of the *Received Protested Agency Contracts*$_{+1}$ for these matched firms. The variable for *Corrective Action* is set to zero for these firms, indicating that they did not receive any corrective action decisions.

As shown, the matching estimator indicates a 14% decrease in future government contracts, whereas our 2SLS analysis shows a slightly higher reduction of 18%. Overall, these findings support our *Retaliation* hypothesis and highlight that receiving a corrective action decision from the GAO against a government agency drives economically and statistically significant distortions in single protesters' subsequent government contracts from protested agencies.

In light of this result, we next examine the heterogeneity of the government agency retaliation across different docket sizes. Our 2SLS procedure provides an estimate for the local and homogenous effect of corrective action decisions under relevance, independence (within dockets), and exclusion restriction

assumptions. The estimation of heterogenous effects relies on the premise of monotonicity, which suggests that for each protester, the probability of getting a corrective action (treatment) should be a monotonically decreasing function of the level of the power outage intensity (instrument). What can violate this assumption? A protester would need to write more (less) effective bid protests when there are more (less) power outages, which sounds unlikely.

Under the *Retaliation* hypothesis, we can expect the effect to grow relative to docket size if the contracting officers want to send a strong signal to future protesters or if they think punishing certain successful protesters will be less salient if many firms are protesting and attention on a small subset of protesters may be comparably low. On the other hand, we can also expect more distortionary effects in smaller dockets if the public does not pay too much attention to smaller dockets. It is not clear which of these hypotheses will be supported by the data.

[Figure A.XI about here]

Figure A.XI presents our findings on effect heterogeneity. It plots our 2SLS coefficient estimates on the y-axis. To avoid running 2SLS regressions on small subsamples, we report our findings using samples of growing docket sizes, e.g., single protests, two or three protests in a docket, at most four protests in a docket, etc., on the x-axis. As shown in the figure, we find that the distortionary effect of corrective action decisions on future contracting activity intensifies economically and statistically as the number of protests within a docket grows. Firms that get corrective actions in larger protests experience greater reductions in future contracts.

The last piece of evidence we present in this section is on government agency discretion. If our findings are driven by *Retaliation*, we should expect the distortions in future contracting arrangements to be higher when the protested government agency has more discretion on the allocation of government contracts. We tackle this question by examining the allocation of agency contracts with varying degrees of agency discretion. We start by categorizing all government contract types under the merged GAO-USASpending

universe. As contracts with low government agency discretion, we examine Government-wide Acquisition Contracts (GWACs) referring to contracts, in which multiple government agencies align their interests to purchase goods or services.

Under contracts with high government agency discretion, we study (i) delivery orders or blanket purchase agreements, which are orders for anticipated supplies and are placed under established government solicitations, (ii) definitized contracts, i.e., contracts with an agreement on, or determination of, contract terms, specifications, and price, and (iii) purchase orders, indefinite-delivery contracts, and basic ordering agreements, i.e., contracts, terms of which will be definitized in the future, or contracts that are not established, e.g., one-off contracts. Collectively, these are contracts that the protested agency can find easier to manipulate or show discretion to avoid certain firms, with which it does not want to get into advanced (definitized) contractual relationships. As mentioned, we expect under the *Retaliation* hypothesis the distortions in contracting arrangements to be higher (lower) when the protested government agency has more (less) discretion. For example, the protested agency may find it difficult to justify to other agencies the unreasonable exclusion of a successful protester from the allocation of a contract.

[Figure A.XII about here]

We summarize our findings on agency discretion in Figure A.XII. As shown, we identify reductions in future government contracts only when we examine contracts with higher agency discretion. We do not find statistically or economically significant changes in future agency contracts that are allocated in coordination with other government agencies. Our findings from this section provide additional support for the *Retaliation* hypothesis.

In Section 2.1, we argued that the *Retaliation* hypothesis could manifest itself by (i) agencies not allocating contracts to successful protesters and/or (ii) successful protesters not wanting to work with agencies. Our results on contract cancellations provide evidence for argument (i) because they reveal that protested agencies do not want to work with successful protesters anymore. This of course does not refute

argument (ii). Although it's difficult or perhaps not necessary to try to separate these arguments. Our findings from this section do partially contradict argument (ii) because they show that protesters do continue to work for GWAC contracts with protested agencies.

### A.8 *Power Outages and Bid Protest Effectiveness*

To provide empirical evidence on whether power outages distort bid protest quality, we conduct a textual analysis of GAO's response documents to bid protests. We scrape all public GAO response documents in 2015 and 2016 available from GAO's webpage. These documents summarize the bid protest reports submitted by the bid protesters and explain the justifications behind GAO's decisions. Importantly, when summarizing the bid protests, GAO provides detailed information on the identities of lawyers that represent the bid protesters, the government agencies, and the GAO. We scrape the GAO data to test whether power outages make it more difficult for bid protesters to support their cases with the use of lawyers.

[Table A.XIII about here]

We first study the impact of power outages on the number of lawyers that represent protesters. We then confirm that the number of lawyers has strong positive relations with the probability of getting a corrective action decision. As shown in column (1) of Table A.XIII, we find that a 100% increase in *Prob. of Protest Day Power Outage* decreases the number of protester lawyers by 6.49. Meanwhile, the number of lawyers that represent the protested agency and the GAO remains unaffected by the same power outage. We complement these findings by running Poisson pseudo-maximum likelihood regressions (PPML) with multi-way fixed effects. Our coefficient estimates are shown in brackets. As shown, we find a reduction of 71% ($= e^{-1.24} - 1$) in the number of protester firm lawyers, yet we don't find similar reductions in agencies' and the GAO's lawyers. This result supports the hypothesis that power outages disrupt the litigation power of the firm, but not that of the agency and GAO.

We next ask whether additional bid protest lawyers matter for the probability of getting a corrective action and, if so, how much they matter. As shown in column (4), we find that an additional protester lawyer increases the chances of getting a corrective action decision by around 4%. Collectively, these results highlight the distortionary effects of power outages on bid protest quality and the importance of lawyers for bid protest outcomes.

### *A.9 Neighboring Non-protester Firms*

As another robustness test, we study whether power outages disrupt the activities of neighboring non-protesting firms. To do so, we identify firms that receive comparable amounts of government contracts to those received by bid protesters, face the same power outages, but do not submit bid protests to the GAO. If power outages do not satisfy the exclusion restriction, then they should disrupt the contracting activities of these neighboring non-protesters, as well.

[Table A.XIV about here]

We present our findings in Table A. XIV. As shown, we find no relation between power outages and government contracts allocated to the neighboring non-protesting firms. In columns (1) and (2) of Panel A, we an insignificant association between power outages and contract allocation to neighboring non-protester firms from the protested and non-protested agencies. Similarly, in columns (3) and (4) we demonstrate insignificant associations between corrective action decisions to protester firms and contract allocation to neighboring non-protester firms from the protested and non-protested agencies. Panel B shows similar results on a subset of firms with similar agency-level contracts. In summary, we find no relation between power outages and government contracts allocated to the neighboring non-protesting firms.

### *A.10 Using Computer Vision Techniques to Analyze Nearby Geographies After Power Outages*

To further validate that power outages do not disrupt bid protester firms through channels other than bid protests, we analyze satellite images of firms' surroundings around bid protest days. To that end, we concentrate on bid protesters that experience significant power outages close to bid protest filing days. A potential driver of power outages is natural disasters, which may disturb bid protester firms' productive

capacities and economic activities around them. Such disturbance to productive capabilities potentially violated the exclusion restriction in our baseline analysis.

To motivate, Figure A.XIII shows satellite images of tornado aftermath around a randomly chosen firm that is at the center of all images. We provide images of the region before and after the event, and we highlight the visual differences between the two. Panel C underlines the path of the tornado along with the establishments it disturbs (circled in bold). Understandably, if a power outage in our sample was caused by such a tornado, the exclusion restriction could be violated. To confirm that our exclusion restriction is not violated due to natural disasters, we borrow a popular technique pioneered by Wang, Bovik, Sheikh, and Simoncelli (2004) that can be used to detect visual differences between satellite images.[2]

[Figure A.XIII about here]

We first generate satellite images from Planet.com for 93 randomly chosen bid protests.[3] We use daily images from all available satellites: RapidEye, PlanetScope, Landsat 8, SkySat, and Sentinel-2. We filter these satellite images so that the minimum area coverage is 100%, cloud cover is 0%, the off-nadir angle is -60 to 60, ground sample distance (i.e., a radius around the bid protester firm) is 10 km, sun azimuth is 0 to 360, and sun elevation is -90 to 90. Planet.com allows us to download videos of up to 120 randomly chosen images, and we examine the -10 days to +10 days event window between bid protests.[4]

For each bid protest, we first create a video of corresponding satellite images. These videos (named "stories" by the data vendor) contain satellite images of firms' surroundings with the above filters along with the dates of these satellite images. We first use computational techniques to split these videos into different frames, i.e., photographs of firms' surroundings with dates on the upper right corner. We then use

[2] See Mukherjee, Panayotov, and Shon (2019) on the use of satellite image data in asset pricing.
[3] We found similar results when we included 42 additional bid protests. We thank Planet.com for allowing to use their data.
[4] See https://on.doi.gov/2I1Fpef for information on nadir angles and azimuth.

machine learning and natural language processing techniques to determine which photograph corresponds to which date.

Once we have all satellite images in hand, we compare satellite images taken on bid protest days with satellite images taken from -10 days to +10 days relative to bid protest filing days. To do so, we use a simple statistical measure. We first convert images into vector form. In other words, we compute $\mathbf{x} = \{x_i \mid i = 1,2, \ldots, N\}$ and $\mathbf{y} = \{y_i \mid i = 1,2, \ldots, N\}$, where each item in vectors **x** and **y** shows the intensity of color in each pixel of the corresponding images and N denotes the number of pixels. Similar to Wang, Bovik, Sheikh, and Simoncelli (2004), our visual similarity measure is calculated as

$$\text{Visual Similarity } = l(x,y) \cdot c(x,y) \cdot s(x,y) = \left(\frac{2\mu_x\mu_y}{\mu_x^2+\mu_x^2}\right) \cdot \left(\frac{2\sigma_x\sigma_y}{\sigma_x^2+\sigma_x^2}\right) \cdot \left(\frac{\sigma_{xy}}{\sigma_x\sigma_y}\right)$$

where $\mu_x$ and $\mu_y$ are the local sample means of **x** and **y**, $\sigma_x$ and $\sigma_y$ are the local sample standard deviations of **x** and **y**, and $\sigma_{xy}$ is the sample cross-correlation of **x** and **y** after removing the means. This measure is also known as structural visual similarity.[5] We compare images taken on the bid protest filing days with images taken within the -180 days to +180 days event window.

The visual similarity index measures the similarities of three elements: the similarity of brightness (luminance), i.e., l(**x**, **y**), the similarity of contrasts, i.e., c(**x**, **y**), and the similarity of structures, i.e., s(**x**, **y**). The first component equals 1 if and only if $\mu_x = \mu_y$. This holds when the average brightness of images **x** and **y** are equal. Since $\sigma_x$ and $\sigma_y$ can be viewed as estimates of contrast, the second component measures how similar the contrasts in **x** and **y** are. The range of values is [0,1], where the highest value of 1 is achieved if and only if $\sigma_x = \sigma_y$. Lastly, the third component denotes the correlation between **x** and **y**, and its range is [-1,1]. The maximum is attained when $y_i = ax_i + b$ for i=1,2,…,N, where a>0.

[Figure A.XIV about here]

[5] Our findings are similar when we use a simple mean squared error (MSE) measure for visual similarity.

Panel A of Figure A.XIV shows how the surroundings of bid protester firms visually change in the event time of bid protests. We concentrate on bid protester firms that experience significant power outages on bid protest filing days and plot visual similarities between their satellite images taken on the bid protest days and their satellite images taken within the -10 days to +10 days event window.[6] As shown in the figure, the visual similarities between days -t and +t are within each other's confidence intervals (we only report one set of confidence intervals). In other words, a day after the bid protest looks like a day before the bid protest. This suggests that there is no significant disruption in nearby geographies. Our findings are robust to controlling for bid protest and year-month interactive fixed effects that control for fixed firm and protest docket characteristics along with seasonality.

Panel B of Figure A.XIV shows a placebo test, in which we examine how the surroundings of bid protester firms visually change one year before bid protest filing dates. More specifically, if a bid protest was filed on 01/01/2016 and if there was a power outage on that day, we study the surroundings of the bid protester firm between 10 days before 01/01/2015 and 180 days after 01/01/2015. As seen in the figure, the visual similarities between days -t and +t are within each other's confidence intervals.

Finally, we also show how the surroundings of 10 randomly chosen establishments visually change in the event time of four actual natural disasters.[7] The purpose of this exercise is to demonstrate that our visual similarity measure can quickly detect large visual changes that are driven by natural disasters. As shown in Panel C of Figure A.XIV, there is a large and immediate decline in visual similarities in the first days after the natural disasters. As shown in the figure, visual similarities between day -t and +t are not within each other's confidence intervals, particularly in the first 10 days after the disasters. This suggests that there are significant disruptions in these geographies.

[6] If we cannot find satellite images for bid protest filing dates, we use satellite images from the closest previous date available.

[7] These include cases like the tornado in Texas from Figure A.III; a hurricane in Port Arthur, FL; a fire in Paradise, CA; and a hurricane in El Reno, OK. The locations are 10725 Villager Rd, Dallas, TX 75230; 221 Houston Ave, Port Arthur, TX 77640; 6286 Mountain Meadow Ct, Paradise, CA; 1644 OK-66, El Reno, OK 73036, and the disaster dates are 10/21/2019; 8/30/2017;11/5/2018; and 5/23/2019 respectively.

### *A.11 Natural disasters*

As a final robustness test, we exclude from our analysis days with natural disasters in the state. Based on our categorization of the drivers of power outages (see, e.g., Table A.I), *Natural Disasters* refers to Tropical Storm Ernesto, Hurricane Sandy, Hurricane Ike, Hurricane Dolly, Winter Storm Nemo, Tropical Storm Fay, and earthquakes. We rerun the 2SLS procedure after dropping these events from our sample. We report our findings in Table A.XV and find economically similar results.

**Table A.I**

**Additional Summary Statistics**

This table reports additional summary statistics on power outages (Panels A and B) and GAO's bid protest response documents (Panel B). In Panel A, we present yearly standard deviations of power outage characteristics. Panel B reports additional information on drivers of power outages that coincide with bid protests. Our power outage data is drawn from unusual occurrences in Appendices B1 and B2 of U.S. Energy Information Administration (EIA)'s Electric Power Monthly reports. To study drivers of power outages, we utilize EIA's designated variable *EventType* and classify drivers of power outages as *Weather Conditions*, *Technical Issues*, and *Natural Disasters*. *Weather Conditions* refers to weather conditions, e.g., wind, rain, and snow. *Natural Disasters* refers to Tropical Storm Ernesto, Hurricane Sandy, Hurricane Ike, Hurricane Dolly, Winter Storm Nemo, Tropical Storm Fay, and earthquakes. *Technical Issues* refers to equipment trip & failure, cyberattack, suspected vandalism or physical attacks, and other technical problems. In Panel C, *Protester Lawyers* denotes the number of lawyers defending a protester in a bid protest. *Agency Lawyers* and *GAO Lawyers* denote the number of lawyers representing the agency and GAO in a bid protest. *Nr. Pages* denotes the number of pages GAO's response document to a bid protest has. *Nr. Characters* denotes the number of characters (divided by 1,000) the GAO's response document to a bid protest has. GAO's response documents are scraped from the official GAO webpage and span the period 2015–2016.

**Panel A. Standard Deviations**

| Year | Stdev. of Affected Customers | Stdev. of Length of Power Outage | Stdev. of Prob. of Protest Day Power Outage | Stdev. of Prob. of Power Outage |
|---|---|---|---|---|
| 2005 | 0.09 | 2.63 | 1.88% | 1.15% |
| 2006 | 0.34 | 3.43 | 3.65% | 1.91% |
| 2007 | 0.10 | 3.20 | 3.62% | 3.72% |
| 2008 | 0.43 | 2.97 | 2.93% | 1.82% |
| 2009 | 0.20 | 3.74 | 3.51% | 3.61% |
| 2010 | 0.38 | 3.32 | 3.21% | 1.43% |
| 2011 | 0.31 | 2.16 | 5.14% | 6.29% |
| 2012 | 0.73 | 3.48 | 15.23% | 4.73% |
| 2013 | 0.19 | 1.46 | 1.93% | 1.76% |
| 2014 | 0.22 | 1.52 | 2.31% | 1.19% |
| 2015 | 0.04 | 1.00 | 0.98% | 1.24% |
| 2016 | 0.07 | 16.44 | 1.60% | 1.16% |
| **Avg. Stdev** | **0.26** | **3.78** | **3.83%** | **2.50%** |

**Panel B. Drivers of Power Outages in Our Sample**

| Year | Weather Conditions | Technical Issues | Natural Disasters |
|---|---|---|---|
| 2005 | 50.00% | 50.00% | 0.00% |
| 2006 | 70.59% | 5.88% | 23.53% |
| 2007 | 100.00% | 0.00% | 0.00% |
| 2008 | 66.67% | 16.67% | 16.67% |
| 2009 | 86.96% | 8.70% | 4.35% |
| 2010 | 94.23% | 5.77% | 0.00% |
| 2011 | 75.76% | 24.24% | 0.00% |
| 2012 | 51.76% | 28.24% | 20.00% |
| 2013 | 63.16% | 34.21% | 2.63% |
| 2014 | 88.64% | 6.82% | 4.55% |
| 2015 | 87.50% | 12.50% | 0.00% |
| 2016 | 44.90% | 55.10% | 0.00% |
| **Mean** | **73.35%** | **20.68%** | **5.98%** |

**Panel C. Bid Protest Characteristics**

| | N | Mean | Median | Stdev | Min | Max |
|---|---|---|---|---|---|---|
| Protester lawyers | 1,194 | 4.34 | 3.00 | 6.11 | 0.00 | 46.00 |
| Agency lawyers | 1,194 | 2.47 | 2.00 | 2.54 | 0.00 | 15.00 |
| GAO lawyers | 1,194 | 2.09 | 2.00 | 0.45 | 1.00 | 8.00 |
| Nr. Pages | 1,194 | 10.42 | 9.00 | 5.54 | 2.00 | 31.00 |
| Nr. Characters | 1,194 | 27.40 | 23.53 | 15.93 | 2.84 | 86.03 |

## Table A.II

### GAO Protesters vs. Protesters in Other Venues

This table reports summary statistics on bid-protester firms that exist only in the GAO dataset (Panel A) or Westlaw dataset (Panel B) along with those firms that exist in both GAO and Westlaw datasets (Panel C). We present the number of observations, mean, median, and standard deviation. The sampling period is 2005–2016. Detailed variable descriptions are presented in the Table III caption.

| | **Panel A: Firms that exist only in the GAO sample** | | | |
|---|---|---|---|---|
| | **N** | **Mean** | **Median** | **Stdev** |
| Received Government Contracts$_{-1}$ | 8,682 | 0.69 | 1.00 | 0.46 |
| Received Government Contracts | 8,682 | 0.87 | 1.00 | 0.33 |
| Received Government Contracts$_{+1}$ | 8,682 | 0.76 | 1.00 | 0.43 |
| Logged Government Contract Amt.$_{-1}$ | 8,682 | 19.94 | 0.33 | 92.35 |
| Small Business | 8,682 | 0.28 | 0.00 | 0.45 |
| Women-owned Business | 8,682 | 0.17 | 0.00 | 0.38 |
| Veteran-owned Business | 8,682 | 0.22 | 0.00 | 0.41 |
| Minority-owned Business | 8,682 | 0.29 | 0.00 | 0.45 |
| | **Panel B: Firms that exist only in the Westlaw sample** | | | |
| | **N** | **Mean** | **Median** | **Stdev** |
| Received Government Contracts$_{-1}$ | 721 | 0.65 | 1.00 | 0.48 |
| Received Government Contracts | 721 | 0.70 | 1.00 | 0.46 |
| Received Government Contracts$_{+1}$ | 721 | 0.58 | 1.00 | 0.49 |
| Logged Government Contract Amt.$_{-1}$ | 721 | 3.50 | 0.04 | 21.77 |
| Small Business | 721 | 0.18 | 0.00 | 0.38 |
| Women-owned Business | 721 | 0.15 | 0.00 | 0.36 |
| Veteran-owned Business | 721 | 0.21 | 0.00 | 0.41 |
| Minority-owned Business | 721 | 0.19 | 0.00 | 0.39 |
| | **Panel C: Firms that exist in both samples** | | | |
| | **N** | **Mean** | **Median** | **Stdev** |
| Received Government Contracts$_{-1}$ | 912 | 0.83 | 1.00 | 0.38 |
| Received Government Contracts | 912 | 0.95 | 1.00 | 0.21 |
| Received Government Contracts$_{+1}$ | 912 | 0.89 | 1.00 | 0.32 |
| Logged Government Contract Amt.$_{-1}$ | 912 | 69.67 | 1.77 | 293.80 |
| Small Business | 912 | 0.30 | 0.00 | 0.46 |
| Women-owned Business | 912 | 0.23 | 0.00 | 0.42 |
| Veteran-owned Business | 912 | 0.24 | 0.00 | 0.42 |
| Minority-owned Business | 912 | 0.27 | 0.00 | 0.45 |

| | Panel D: Summary statistics on Westlaw cases | | | |
|---|---|---|---|---|
| | **N** | **Mean** | **Median** | **Stdev** |
| Unsuccessful Case | 3,404 | 0.71 | 1.00 | 0.68 |
| Denied Case | 3,404 | 0.45 | 0.00 | 0.50 |
| Rejected Case | 3,404 | 0.04 | 0.00 | 0.19 |
| Dismissed Case | 3,404 | 0.16 | 0.00 | 0.37 |
| Remanded Case | 3,404 | 0.06 | 0.00 | 0.23 |
| Settled Case | 3,404 | 0.02 | 0.00 | 0.13 |
| Logged Contract Value Estimate | 1,206 | 12.46 | 12.75 | 4.48 |

**Table A.III**

**Summary Statistics on Protested Solicitations on FPDS**

This table reports summary statistics on firms appearing in both the GAO data and USASpending data that also exist in the U.S. government's Federal Procurement Data System (FPDS). To identify these firms, we scrape all FPDS data associated with solicitation identifiers available in the GAO sample. The scraped data contains information on all government contracts under solicitations that are protested at the GAO. We present the number of observations, mean, median, and standard deviation. The sampling period is 2005–2016. *Nr. of Contractors Working on Protested Solicitation* denotes the number of contractors that receive a government contract under a given solicitation before or after a bid protest. *Log(Government Expenditure on Protested Solicitation)* denotes the log of the total contract amount allocated under a solicitation per year after the bid protest. *Received Government Contracts* is equal to one if a bid protester received a government contract during the year of the bid protest. Detailed descriptions of the remaining firm-level variables are presented in the Table III caption.

| | N | Mean | Median | Stdev |
|---|---|---|---|---|
| Nr. of Contractors Working on Protested Solicitation | 3,503 | 6.41 | 1.00 | 22.20 |
| Log (Government Expenditure on Protested Solicitation) | 3,503 | 0.36 | 0.00 | 2.44 |
| Received Government Contracts | 3,503 | 0.89 | 1.00 | 0.31 |
| Small Business | 3,503 | 0.31 | 0.00 | 0.46 |
| Women-owned Business | 3,503 | 0.18 | 0.00 | 0.38 |
| Veteran-owned Business | 3,503 | 0.22 | 0.00 | 0.42 |
| Minority-owned Business | 3,503 | 0.31 | 0.00 | 0.46 |

**Table A.IV**

**Additional Instrumental Variables**

This table reruns our 2SLS procedure with a different instrumental variable. To instrument corrective actions, we use the number of electricity customers that experience power outages on the bid protest day divided by the number of residential customers in the bid protester firm's state. Detailed information on model specifications and variable descriptions can be found in Table IV's caption. We report relevant *tF Critical values* for 10% and 5% significance levels ($\sqrt{c_{0.1}(F)}$ and $\sqrt{c_{0.05}(F)}$) based on the first stage F-statistic for the excluded instrument following Lee, McCrary, Moreira, and Porter (2022). ***, **, and * indicate that the coefficient estimate is significantly different from zero at the 1%, 5%, and 10% levels, respectively.

| | **Corrective Action** | **Received Protested Agency Contracts$_{+1}$** | **Received Protested Agency Contracts$_{+1}$** |
|---|---|---|---|
| | (1) | (2) | (3) |
| Affected Electricity Customers / Residential Customers | -0.07*** | .. | .. |
| | (-3.73) | | |
| Instrumented Corrective Action | .. | -0.54*** | .. |
| | | (-3.25) | |
| Corrective Action | .. | .. | 0.03** |
| | | | (2.64) |
| Active Contractor | 0.01 | 0.22*** | 0.21*** |
| | (0.57) | (5.94) | (7.27) |
| Business-type x Year FE | Yes | Yes | Yes |
| Docket FE | Yes | Yes | Yes |
| Year x Month FE | Yes | Yes | Yes |
| Agency x Year FE | Yes | Yes | Yes |
| State x Year FE | Yes | Yes | Yes |
| Observations | 9,455 | 9,455 | 9,455 |
| F-stat of the excluded instrument | 13.47 | .. | .. |
| $\left\lvert\sqrt{c_{0.1}(F = 13.47)}\right\rvert$ | .. | 1.970 | .. |
| $\left\lvert\sqrt{c_{0.05}(F = 13.47}\right\rvert$ | .. | 2.981 | .. |

**Table A.V**

**Predicting Power Outages Using Local Economic Activity**

This table reports results from regressing our instrument on local economic variables. *Prob. of Protest Day Power Outage* is equal to the number of electricity customers in the bid protester firm state that experienced a major and unexpected power outage on the day when protest *p* within docket *d* was filed, divided by the total number of electricity customers in the bid protester firm state. *Logged Local GDP Growth, Logged Local Income Growth, Logged Local Labor Market Participation,* and *Logged Local Unemployment Rate* refer to log of one plus the percent change in state-level GDP, percent change in personal income, labor market participation rate, and unemployment rate. These quarterly and monthly data are drawn from U.S Census Bureau (see files SQGDP and SQINC for state-quarter-level data GDP and Income, respectively) and the U.S. Bureau of Labor Statistics (see file CES for state-month-level data on employment outcomes). The control variables are as in Table IV. ***, **, and * indicate that the coefficient estimate is significantly different from zero at the 1%, 5%, and 10% levels, respectively.

| | **Prob. of Protest Day Power Outage** | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Logged Local GDP Growth | 0.0093 | 0.0090 | 0.0063 | 0.0059 | 0.0059 |
| | (0.36) | (0.36) | (0.28) | (0.27) | (0.27) |
| Logged Local Income Growth | .. | .. | 0.0068 | 0.0077 | 0.0077 |
| | | | (0.22) | (0.24) | (0.24) |
| Logged Local Labor Market Participation | .. | .. | .. | -0.0301 | -0.0301 |
| | | | | (-0.15) | (-0.15) |
| Logged Local Unemployment Rate | .. | .. | .. | -0.0463 | -0.0463 |
| | | | | (-0.51) | (-0.51) |
| Active Contractor | .. | 0.0009 | .. | .. | .. |
| | | (1.56) | | | |
| Business-type FE | Yes | Yes | Yes | Yes | Yes |
| Docket FE | Yes | Yes | Yes | Yes | Yes |
| Year x Month FE | Yes | Yes | Yes | Yes | Yes |
| Agency x Year FE | Yes | Yes | Yes | Yes | Yes |
| State x Year FE | Yes | Yes | Yes | Yes | Yes |
| Observations | 9,455 | 9,455 | 9,455 | 9,455 | 9,455 |
| Adj. R-squared | 0.011 | 0.011 | 0.010 | 0.010 | 0.010 |

**Table A.VI**

**Protest Starter Firms**

This table studies government contracts allocated to protest starter firms by the government agencies they protest. Protest starter firms are the firms that submit the first three bid protests in each docket. Column 1 presents results from first-stage regressions, column 2 presents results from second-stage regressions, and column 3 presents results from OLS regressions. Detailed information on model specifications and variable descriptions can be found in Table IV's caption. ***, **, and * indicate that the coefficient estimate is significantly different from zero at the 1%, 5%, and 10% levels, respectively. We report relevant *tF Critical values* for 10% and 5% significance levels ($\sqrt{c_{0.1}(F)}$ and $\sqrt{c_{0.05}(F)}$) based on the first stage F-statistic for the excluded instrument following Lee, McCrary, Moreira, and Porter (2022).

| | **Corrective Action** | **Received Protested Agency Contracts$_{+1}$** | **Received Protested Agency Contracts$_{+1}$** |
|---|---|---|---|
| | (1) | (2) | (3) |
| Prob. of Protest Day Power Outage | -0.55*** | .. | .. |
| | (-5.94) | | |
| Instrumented Corrective Action | .. | -0.61** | .. |
| | | (-3.00) | |
| Corrective Action | .. | .. | 0.02 |
| | | | (1.43) |
| Active Contractor | 0.00 | 0.17*** | 0.16*** |
| | (0.22) | (4.77) | (5.43) |
| Business-type x Year FE | Yes | Yes | Yes |
| Docket FE | Yes | Yes | Yes |
| Year x Month FE | Yes | Yes | Yes |
| Agency x Year FE | Yes | Yes | Yes |
| State x Year FE | Yes | Yes | Yes |
| Observations | 6,446 | 6,446 | 6,446 |
| F-stat of the excluded instrument | 35.23 | .. | .. |
| $\left\lvert\sqrt{c_{0.1}(F = 35.23)}\right\rvert$ | .. | 1.634 | .. |
| $\left\lvert\sqrt{c_{0.05}(F = 35.23)}\right\rvert$ | .. | 2.275 | .. |

**Table A.VII**

**Subsample 2SLS Results**

This table reruns our 2SLS procedure for the subsample of firms we analyze in Table VII. Column 1 presents results from first-stage regressions, column 2 presents results from second-stage regressions, and column 3 presents results from OLS regressions. Detailed information on model specifications and variable descriptions can be found in Table IV's caption. ***, **, and * indicate that the coefficient estimate is significantly different from zero at the 1%, 5%, and 10% levels, respectively. We report relevant *tF Critical values* for 10% and 5% significance levels ($\sqrt{c_{0.1}(F)}$ and $\sqrt{c_{0.05}(F)}$) based on the first stage F-statistic for the excluded instrument (F=4.771$^2$=22.76) following Lee, McCrary, Moreira, and Porter (2022).

| | **Corrective Action** | **Received Protested Agency Contracts$_{+1}$** | **Received Protested Agency Contracts$_{+1}$** |
|---|---|---|---|
| | (1) | (2) | (3) |
| Prob. of Protest Day Power Outage | -0.55*** | .. | .. |
| | (-4.77) | | |
| Instrumented Corrective Action | .. | -0.54** | .. |
| | | (-2.60) | |
| Corrective Action | .. | .. | 0.01 |
| | | | (0.68) |
| Active Contractor | 0.01 | 0.14*** | 0.14*** |
| | (0.29) | (4.20) | (4.56) |
| Business-type x Year FE | Yes | Yes | Yes |
| Docket FE | Yes | Yes | Yes |
| Year x Month FE | Yes | Yes | Yes |
| Agency x Year FE | Yes | Yes | Yes |
| State x Year FE | Yes | Yes | Yes |
| Observations | 6,074 | 6,074 | 6,074 |
| F-stat of the excluded instrument | 22.76 | .. | .. |
| $\lvert\sqrt{c_{0.1}(F=22.76)}\rvert$ | .. | 1.752 | .. |
| $\lvert\sqrt{c_{0.05}(F=22.76)}\rvert$ | .. | 2.510 | .. |

**Table A.VIII**

**Analyzing the Protested Solicitations**

This table reruns our 2SLS regressions from Section 4 on the merged GAO-FPDS-USASpending universe. The first two columns present our findings from the first- and second-stage regressions as in Table IV. The third and fourth columns present results from OLS regressions. The fourth and fifth columns present results from using *Received Future Contracts From Protested Solicitation* as a dependent variable. This variable is equal to one if a protester received government contracts (starting the year after submitting the bid protest) under the solicitation it protests. *Previous work on Protested Solicitation* is equal to one if a bid protester received a government contract within the protested solicitation in the year of the protest. The remaining control variables are as in Table IV. ***, **, and * indicate that the coefficient estimate is significantly different from zero at the 1%, 5%, and 10% levels, respectively. We report relevant *tF Critical values* for 10% and 5% significance levels ($\sqrt{c_{0.1}(F)}$ and $\sqrt{c_{0.05}(F)}$) based on the first stage F-statistic for the excluded instrument following Lee, McCrary, Moreira, and Porter (2022).

*(The table is on the following page)*

| | Corrective Action | Received Protested Agency Contracts$_{+1}$ | Received Protested Agency Contracts$_{+1}$ | Received Future Contracts From Protested Solicitation | Received Future Contracts From Protested Solicitation |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Prob. of Protest Day Power Outage | -0.39*** | .. | .. | .. | .. |
| | (-4.49) | | | | |
| Instrumented Corrective Action | .. | -0.50*** | .. | -0.37** | -0.26** |
| | | (-3.23) | | (0.14) | (0.09) |
| Corrective Action | .. | .. | 0.03* | .. | .. |
| | | | (1.96) | | |
| Active Contractor | 0.00 | 0.20*** | 0.20*** | -0.01 | -0.00 |
| | (0.13) | (4.24) | (5.59) | (0.01) | (0.01) |
| Previous work on Protested Solicitation | .. | .. | .. | .. | 0.61*** |
| | | | | | (0.07) |
| Business-type x Year FE | Yes | Yes | Yes | Yes | Yes |
| Docket FE | Yes | Yes | Yes | Yes | Yes |
| Year x Month FE | Yes | Yes | Yes | Yes | Yes |
| Agency x Year FE | Yes | Yes | Yes | Yes | Yes |
| State x Year FE | Yes | Yes | Yes | Yes | Yes |
| Observations | 3,302 | 3,302 | 3,302 | 3,302 | 3,302 |
| F-stat of the excluded instrument | 20.20 | .. | .. | .. | [First-stage F = 21.46] |
| $\left\lvert\sqrt{c_{0.1}(F)}\right\rvert$ | .. | 1.793 | .. | 1.793 | 1.772 |
| $\left\lvert\sqrt{c_{0.05}(F)}\right\rvert$ | .. | 2.594 | .. | 2.594 | 2.550 |

**Table A.IX**

**Dockets with a Single Protest**

This table presents our findings from analyzing GAO dockets with single bid protests. We study the effect of corrective action decisions on single bid protesters' contracting outcomes. The first two columns present the local average treatment effect of corrective action decisions based on 2SLS regressions, using comparable government contractors, and the third column presents the local average treatment effect of corrective action decisions based on matching with comparable bid-protesting firms. To obtain counterfactuals in the first two columns, we first identify government contractors that receive government contracts from the protested agencies in the year of the bid protests. We then keep those contractors that have the same business-type designation (i.e., *Small Business, Women-owned Business, Veteran-owned Business,* and *Minority-owned Business*) as the single protester and receive protested agency contracts of 95% to 105% of the amount received by the single protester firm. This provides us with comparable contractors to single protesters that do not submit bid protests and are not exposed to power outages. To obtain a counterfactual dependent variable, we calculate the mean *Received Protested Agency Contracts*$_{+1}$ of these matched contractors. Since differences in firm-level characteristics between single protesters and matched contractors can contribute to differences in *Received Protested Agency Contracts*$_{+1}$, we also compute the average *Received Government Contract* and *Received Government Contract*$_{-1}$ values of the matched contractors and use them as control variables in our 2SLS procedure. To obtain counterfactuals for the instrument and corrective action decisions, we randomly assign power outages and corrective actions so that the mean *Prob. of Protest Day Power Outage* and *Corrective Action* of the counterfactual protesters are equal to the average values in our sample. These assignments provide us with factual outcomes from single protesters and counterfactual outcomes from observably comparable government contractors. We compare these outcomes using the 2SLS procedure explained in Section 4. In column 3, we match single bid protesters with other contractors that also protest government contracts from government agencies but have not received any corrective action decisions. Among the matched firms, we again retain those contractors that have the same business-type designation (i.e., *Small Business, Women-owned Business, Veteran-owned Business,* and *Minority-owned Business*) as the single protester and receive protested agency contracts of 95% to 105% of the amount received by the single protester firm. To obtain a counterfactual dependent variable, we calculate the mean *Received Protested Agency Contracts*$_{+1}$ of these matched contractors. *Corrective Action* is equal to zero since these contractors, as they do not receive corrective action decisions. The first column presents the first-stage regression results, and the second column presents the second-stage regression results of the 2SLS procedure. The third column presents regression results based on the matching exercise. ***, **, and * indicate that the coefficient estimate is significantly different from zero at the 1%, 5%, and 10% levels, respectively. We report relevant *tF Critical values* for 10% and 5% significance levels ($\sqrt{c_{0.1}(F)}$ and $\sqrt{c_{0.05}(F)}$) based on the first stage F-statistic for the excluded instrument following Lee, McCrary, Moreira, and Porter (2022).

*(The table is on the following page)*

| | **Corrective Action** | **Received Protested Agency Contracts$_{+1}$** | **Received Protested Agency Contracts$_{+1}$** |
|---|---|---|---|
| | (1) | (2) | (3) |
| Prob. of Protest Day Power Outage | -0.25*** | .. | .. |
| | (-7.47) | | |
| Corrective Action (2SLS) | .. | -0.18*** | .. |
| | | (-3.87) | |
| Corrective Action (Matching) | .. | .. | -0.14*** |
| | | | (-7.90) |
| Firm Controls | Yes | Yes | Yes |
| Docket FE | Yes | Yes | Yes |
| Year x Month FE | Yes | Yes | Yes |
| Agency x Year FE | Yes | Yes | Yes |
| State x Year FE | Yes | Yes | Yes |
| Observations | 14,832 | 14,832 | 11,634 |
| F-stat of the excluded instrument | 55.78 | .. | .. |
| $\lvert\sqrt{c_{0.1}(F)}\rvert$ | .. | 1.547 | .. |
| $\lvert\sqrt{c_{0.05}(F)}\rvert$ | .. | 2.109 | .. |
| Adj. R-squared | | | 0.223 |

**Table A.X**

**Additional Robustness Tests on the 2SLS Procedure**

This table reruns our 2SLS procedure with a different set of control variables. Column 1 presents results from first-stage regressions, column 2 presents results from second-stage regressions, and column 3 presents results from OLS regressions. *Log(Recent Government Contract)* is log of total contracts a protester received 60 to 30 days before a protest. information on model specifications and variable descriptions can be found in Table IV's and A.V's captions. ***, **, and * indicate that the coefficient estimate is significantly different from zero at the 1%, 5%, and 10% levels, respectively. We report relevant *tF Critical values* for 10% and 5% significance levels ($\sqrt{c_{0.1}(F)}$ and $\sqrt{c_{0.05}(F)}$) based on the first stage F-statistic for the excluded instrument following Lee, McCrary, Moreira, and Porter (2022).

| | **Corrective Action** | **Received Protested Agency Contracts$_{+1}$** | **Corrective Action** | **Received Protested Agency Contracts$_{+1}$** |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Prob. of Protest Day Power Outage | -0.60*** | .. | -0.61*** | .. |
| | (-3.66) | | (-3.64) | |
| Instrumented Corrective Action | .. | -0.53** | .. | -0.54*** |
| | | (-3.04) | | (-3.20) |
| Log(Recent Government Contract) | 0.00 | 0.02*** | .. | .. |
| | (0.99) | (5.85) | | |
| Active Contractor | .. | .. | 0.01 | 0.22*** |
| | | | (0.71) | (5.92) |
| Local GDP Growth | .. | .. | 3.73** | 3.36* |
| | | | (2.82) | (1.96) |
| Local Income Growth | .. | .. | -4.07* | -2.68 |
| | | | (-2.12) | (-1.29) |
| Business-type x Year FE | Yes | Yes | Yes | Yes |
| Docket FE | Yes | Yes | Yes | Yes |
| Year x Month FE | Yes | Yes | Yes | Yes |
| Agency x Year FE | Yes | Yes | Yes | Yes |
| State x Year FE | Yes | Yes | Yes | Yes |
| Observations | 9,455 | 9,455 | 9,455 | 9,455 |
| F-stat of the excluded instrument | 13.37 | .. | 13.23 | .. |
| $\lvert\sqrt{c_{0.1}(F)}\rvert$ | .. | 1.972 | .. | 1.978 |
| $\lvert\sqrt{c_{0.05}(F)}\rvert$ | .. | 2.989 | .. | 3.002 |

## Table A.XI

## Details on Contract Cancellation

Panel A (Panel B) describes contract modifications we (do not) consider as contract cancellations along with initiating actors and related regulations. Modifications initiated by government, firm, and both are labelled under Actor as "Gov", "Firm", and "Both", respectively.

**Panel A: Actions we consider as Contract Cancellation**

| Action | Description | Actor | Regulation |
|---|---|---|---|
| ***Terminate For Default*** | *Termination for default* is the exercise of the Government's contractual right to completely or partially terminate a contract because of the contractor's actual or anticipated failure to perform its contractual obligations. | Gov | FAR 49.4 |
| ***Terminate For Convenience*** | *Termination for convenience* is the exercise of the Government's contractual right to terminate the performance of work under this contract in whole or, from time to time, in part if the contracting officer determines that a termination is in the Government's interest without being required to pay damages, despite full contractor compliance with its contractual obligations. See https://bit.ly/3LPcLdp for more information on no-cost termination. | Gov | FAR 49.502 |
| ***Terminate For Cause*** | A contracting officer may terminate individual orders for *cause*, e.g., due to a material breach of the contract. *Termination for cause* shall comply with FAR 12.403 and may include charging the contractor with excess costs resulting from repurchase. | Gov | FAR 8.406-4, FAR 12.403 |
| ***Cancellation*** | *Cancellation* is often used interchangeably with *Terminate For Default* and *Terminate For Convenience*. Nevertheless, a "*canceled contract*" can have three different meanings. The first is a contract terminated for *Default*, which under FAR 49.401 refers to the exercise of the Government's contractual right to completely or partially terminate a contract because of the contractor's actual or anticipated failure to perform its contractual obligations. The second meaning is related to multi-year contracting. FAR clause 52.217-2, states in part (a) "Cancellation," as used in this clause, means that the Government is canceling its requirements for all supplies or services in the program years after that in which notice of cancellation is provided, (b) except for cancellation under this clause or termination under the Default clause, any reduction by the Contracting Officer in the requirements of this contract shall be considered a termination under the *Termination for Convenience* of the Government clause, and (c) if cancellation under this clause occurs, the Contractor will be paid a cancellation charge, not over the cancellation ceiling specified in the Schedule as applicable at the time of cancellation. The third meaning is related to the acquisition of commercial items. FAR 12.403 is often used as the process for a no-cost cancellation, with a modification to that effect. In sum, the three instances cited above can be justification for the contracting officer to equate "cancellation" with termination for default or convenience. See, e.g., https://bit.ly/3ft10gA. | Gov | FAR 49.401, FAR 52.217-2, FAR 12.403 |

**Panel B: Actions we do not consider as Contract Cancellation**

| Action | Description | Actors | Regulation |
|---|---|---|---|
| ***Additional Work, Supplemental Agreement For Work Within Scope*** | Supplemental agreement and additional work are contract modifications signed by the contractor and the contracting officer. They reflect equitable adjustments in contract terms. | Both | FAR 43.103 |
| ***Change Order*** | Government contracts contain a changes clause that permits the contracting officer to make unilateral changes, in designated areas, within the general scope of the contract. The contractor must continue the performance of the contract as changed, except that in cost-reimbursement or incrementally funded contracts the contractor is not obligated to continue performance or incur costs beyond the limits established in the Limitation of Cost or Limitation of Funds clause. | Gov | FAR 43.2, 32.706-2 |
| ***Exercise An Option*** | The contracting officer may include options in contracts when it is in the Government's interest. When using sealed bidding, the contracting officer shall make a written determination that there is a reasonable likelihood that the options will be exercised before including the provision at FAR 52.217-5, Evaluation of Options, in the solicitation. | Both | FAR 17.2, 17.207, FAR 52.217 |
| ***Definitize Letter Contract*** | Definitization means the agreement on, or determination of, contract terms, specifications, and price, which converts the undefinitized contract action to a definitive contract. A definitive contract refers to any contract that must be reported to FPDS other than an indefinite delivery vehicle. | Both | N/A |
| ***Novation Agreement, Rerepresentation, M&A*** | A novation is an agreement made between two contracting parties to allow for the substitution of a new party for an existing one. If Rerepresentation or Non-novated acquisition or M&A is chosen as the reason for modification then the vendor's socio-economic information is refreshed from SAM and the Contracting Officer's Determination of Size field is opened. | Both | FAR 42.12 |
| ***Close Out*** | The contract administration office is responsible for initiating administrative closeout of the contract after receiving evidence of its physical completion. | Gov | FAR 4.804-5 |
| ***Change PIID*** | Change PIID is a new user privilege only supplied to the Department System Administrator. If an agency has created a modification that changes the contract PIID they would use this reason for modification when entering data into FPDS. The change PIID reason for modification allows the user to change the PIID, the PIID agency code, or both. Change PIID can only be used once through the duration of the contract. | Gov | N/A |
| ***Transfer Action*** | Administrative Transfer Action is a reason for modification that is meant to be used when one Department or Agency transfers a contract to another. It is not intended for transfers between offices. Admin Transfer privileges are distributed via the Agency System Administrator. | Gov | N/A |

| | | | |
|---|---|---|---|
| ***Vendor Name, Duns, Address Change*** | If a change of the contractor's name, address, or DUNS is involved and the Government's and contractor's rights and obligations remain unaffected, the parties shall execute an agreement to reflect the name change. The contractor shall forward to the responsible contracting officer three signed copies of the Change-of-Name Agreement. | Firm | FAR 42.1205 |
| ***Funding Only Action*** | Funding-only action refers to additional funds being allotted for the continued performance of the contract. | Both | FAR 43.103 |
| ***Other Administrative Action*** | Other Administration Action is used only when another value does not apply. | Gov | FAR 43.103 |

**Table A.XII**

**Additional Robustness Tests on the 2SLS Procedure**

This table runs our 2SLS regressions from Section 4 with two-way standard error clustering within (i) years and (ii) agency & business-types. ***, **, and * indicate that the coefficient estimate is significantly different from zero at the 1%, 5%, and 10% levels, respectively. We report relevant *tF Critical values* for 10% and 5% significance levels ($\sqrt{c_{0.1}(F)}$ and $\sqrt{c_{0.05}(F)}$) based on the first stage F-statistic for the excluded instrument (F) following Lee, McCrary, Moreira, and Porter (2022).

| | **Corrective Action** | **Received Protested Agency Contracts$_{+1}$** | **Received Protested Agency Contracts$_{+1}$** |
|---|---|---|---|
| | (1) | (2) | (3) |
| Prob. of Protest Day Power Outage | -0.60*** | .. | .. |
| | (-3.86) | | |
| Instrumented Corrective Action | .. | -0.55** | .. |
| | | (-2.93) | |
| Corrective Action | .. | .. | 0.03** |
| | | | (2.84) |
| Active Contractor | 0.01 | 0.22*** | 0.21*** |
| | (0.60) | (4.89) | (5.87) |
| Business-type x Year FE | Yes | Yes | Yes |
| Docket FE | Yes | Yes | Yes |
| Year x Month FE | Yes | Yes | Yes |
| Agency x Year FE | Yes | Yes | Yes |
| State x Year FE | Yes | Yes | Yes |
| Observations | 9,455 | 9,455 | 9,455 |
| F-stat of the excluded instrument | 14.862 | .. | .. |
| $\lvert\sqrt{c_{0.1}(F = 14.862)}\rvert$ | .. | 1.920 | .. |
| $\lvert\sqrt{c_{0.05}(F = 14.862)}\rvert$ | .. | 2.867 | .. |

**Table A.XIII**

**Power Outages and Bid Protest Quality**

This table reports the distortionary effects of power outages on the quality of bid protests. We present estimates from:

$$y_{pd} = \alpha + \pi \times \text{Prob. of Protest Day Power Outage}_{pd} + \gamma' \text{ X+ Fixed effects} + \varepsilon_{pd},$$

where *p* indexes the bid protest number, and *d* indexes the GAO docket number. $y_{pd}$ is the dependent variable of interest and is one of *Protester lawyers*, *Agency lawyers*, *GAO lawyers*, and *Corrective action*. *Protester lawyers* denotes the number of lawyers defending bid protest *p*. *Agency (GAO) lawyers* denote the number of lawyers representing the agency (GAO) in bid protest *p*. *Nr. Characters* denotes the number of characters (in thousands) GAO's response document to bid protest *p* has. The remaining variables are explained in detail in Table III's caption. For comparison, we present coefficient estimates from running Poisson pseudo-maximum likelihood regressions (PPML) with multi-way fixed effects for the first three columns. The sampling period is 2015-2016. ***, **, and * indicate that the coefficient estimate is significantly different from zero at the 1%, 5%, and 10% levels, respectively.

| | **Protester lawyers** | **Agency lawyers** | **GAO lawyers** | **Corrective action** |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Prob. of Protest Day Power Outage | -6.49** | 0.03 | -0.04 | .. |
| | (-5.56) | (0.05) | (-1.03) | |
| | [-1.24**] | [0.39*] | [-0.02] | |
| Protester lawyers | .. | .. | .. | 0.04*** |
| | | | | (8.48) |
| Active Contractor | -0.01 | -0.04 | 0.01 | -0.02 |
| | (-0.06) | (-0.83) | (1.74) | (-0.99) |
| Nr. characters | .. | .. | .. | -0.00 |
| | | | | (-0.17) |
| Business-type x Year FE | Yes | Yes | Yes | Yes |
| Docket FE | Yes | Yes | Yes | Yes |
| Year x Month FE | Yes | Yes | Yes | Yes |
| Agency x Year FE | Yes | Yes | Yes | Yes |
| State x Year FE | Yes | Yes | Yes | Yes |
| Observations | 1,016 | 1,016 | 1,016 | 1,016 |
| R-squared | 0.992 | 0.995 | 0.998 | 0.956 |

## Table A.XIV

## Robustness Tests on Neighboring Non-protester Firms

This table reports the effects of power outages on government contract allocations to firms that neighbor protester firms and do not submit bid protests. It presents results from estimating:

$$z_{pdn} = \alpha + \delta \times \text{Shock}_{pdn} + \gamma' \text{ X} + \text{Fixed effects} + \varepsilon_{pd},$$

where *p* indexes the bid protest number, *d* indexes the GAO docket number, and *n* indexes the non-protester firms from protester firms' states. In Panel A, the non-protester firms are from the same states as bid-protester firms and the total government contract amount non-protester firms receive must be between 90% and 110% of the amount received by a protester firm during the year before the bid protest. In Panel B, the non-protester firms (i) are from the same states as bid-protester firms, (ii) the total government contract amount non-protester firms receive is between 90% and 110% of the amount received by a protester firm during the year before the bid protest, and (iii) the agency-level government contract amount non-protester firms receive (from the protested agency) is between 90% and 110% of the amount received by a protester firm during the year before the bid protest. ***, **, and * indicate that the coefficient estimate is significantly different from zero at the 1%, 5%, and 10% levels, respectively.

### Panel A. Similar Non-Protester Firms in Terms of Government Contracts

| | **Received Protested Agency Contracts$_{+1}$** | **Received Non-protested Agency Contracts$_{+1}$** | **Received Protested Agency Contracts$_{+1}$** | **Received Non-protested Agency Contracts$_{+1}$** |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Prob. of Protest Day Power Outage | 0.55 | 0.90 | .. | .. |
| | (0.37) | (0.83) | | |
| Corrective Action | .. | .. | 0.04 | -0.06 |
| | | | (0.04) | (0.08) |
| Firm controls | Yes | Yes | Yes | Yes |
| Business-type Dummies | Yes | Yes | Yes | Yes |
| Firm FE | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes |
| Observations | 24,675 | 24,675 | 24,675 | 24,675 |
| R-squared | 0.599 | 0.507 | 0.599 | 0.507 |

**Panel B. Similar Non-Protester Firms in Terms of Agency Contracts**

| | Received Protested Agency Contracts$_{+1}$ | Received Non-protested Agency Contracts$_{+1}$ | Received Protested Agency Contracts$_{+1}$ | Received Non-protested Agency Contracts$_{+1}$ |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Prob. of Protest Day Power Outage | -0.31 | 1.58 | .. | .. |
| | (0.34) | (1.73) | | |
| Corrective Action | .. | .. | 0.01 | -0.04 |
| | | | (0.01) | (0.05) |
| Firm controls | Yes | Yes | Yes | Yes |
| Business-type Dummies | Yes | Yes | Yes | Yes |
| Firm FE | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes |
| Observations | 18,015 | 18,015 | 18,015 | 18,015 |
| R-squared | 0.683 | 0.340 | 0.683 | 0.340 |

**Table A.XV**

**Excluding Natural Disasters**

This table reruns our 2SLS procedure from Section 4 after excluding power outages due to natural disasters (as defined in Table A.I). Standard errors are clustered two-way at docket and year levels. ***, **, and * indicate that the coefficient estimate is significantly different from zero at the 1%, 5%, and 10% levels, respectively.

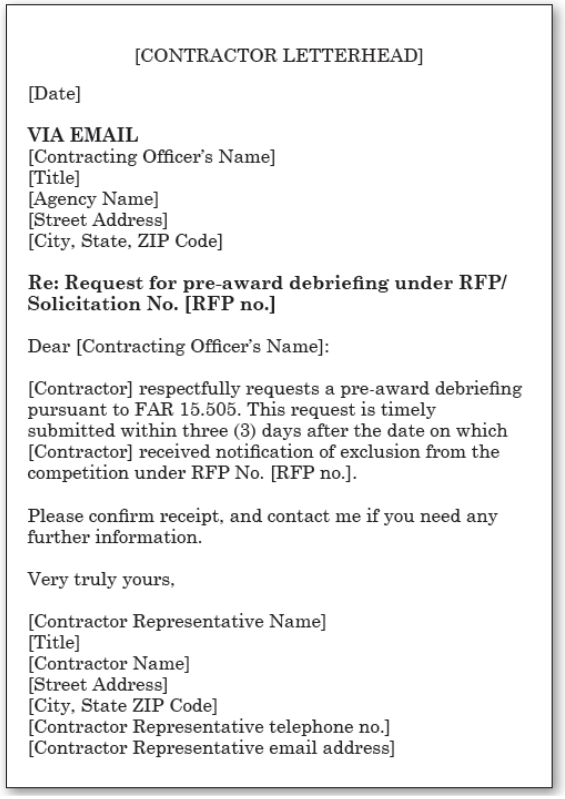
| | **Corrective Action** | **Received Protested Agency Contracts$_{+1}$** | **Received Protested Agency Contracts$_{+1}$** |
|---|---|---|---|
| | (1) | (2) | (3) |
| Prob. of Protest Day Power Outage | -0.69*** | .. | .. |
| | (-3.58) | | |
| Instrumented Corrective Action | .. | -0.43** | .. |
| | | (-2.77) | |
| Corrective Action | .. | .. | 0.03** |
| | | | (2.66) |
| Active Contractor | 0.22*** | 0.01 | 0.21*** |
| | (6.17) | (0.67) | (7.29) |
| Business-type x Year FE | Yes | Yes | Yes |
| Docket FE | Yes | Yes | Yes |
| Year x Month FE | Yes | Yes | Yes |
| Agency x Year FE | Yes | Yes | Yes |
| State x Year FE | Yes | Yes | Yes |
| Observations | 9,439 | 9,439 | 9,439 |

## Figure A.I

## Request for Debrief Letters

This figure shows a sample request for debrief letter that can be submitted to government agencies to start a bid protest process. The letter on the left is for a pre-award debriefing and the letter on the right is for a post-award debriefing. Source: https://www.hollandhart.com.

(a) Request for a pre-award debriefing



[CONTRACTOR LETTERHEAD]

[Date]

**VIA EMAIL**
[Contracting Officer's Name]
[Title]
[Agency Name]
[Street Address]
[City, State, ZIP Code]

**Re: Request for pre-award debriefing under RFP/ Solicitation No. [RFP no.]**

Dear [Contracting Officer's Name]:

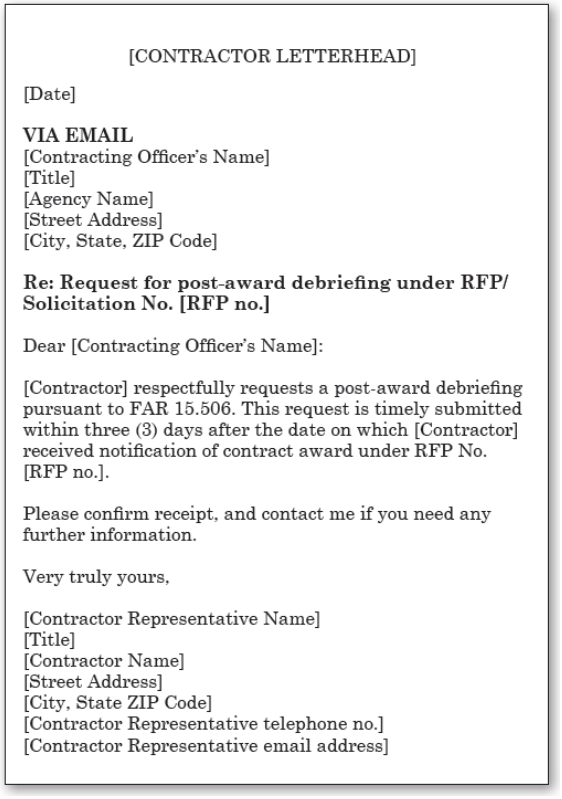
[Contractor] respectfully requests a pre-award debriefing pursuant to FAR 15.505. This request is timely submitted within three (3) days after the date on which [Contractor] received notification of exclusion from the competition under RFP No. [RFP no.].

Please confirm receipt, and contact me if you need any further information.

Very truly yours,

[Contractor Representative Name]
[Title]
[Contractor Name]
[Street Address]
[City, State ZIP Code]
[Contractor Representative telephone no.]
[Contractor Representative email address]

(b) Request for a post-award debriefing



[CONTRACTOR LETTERHEAD]

[Date]

**VIA EMAIL**
[Contracting Officer's Name]
[Title]
[Agency Name]
[Street Address]
[City, State, ZIP Code]

**Re: Request for post-award debriefing under RFP/ Solicitation No. [RFP no.]**

Dear [Contracting Officer's Name]:

[Contractor] respectfully requests a post-award debriefing pursuant to FAR 15.506. This request is timely submitted within three (3) days after the date on which [Contractor] received notification of contract award under RFP No. [RFP no.].

Please confirm receipt, and contact me if you need any further information.

Very truly yours,

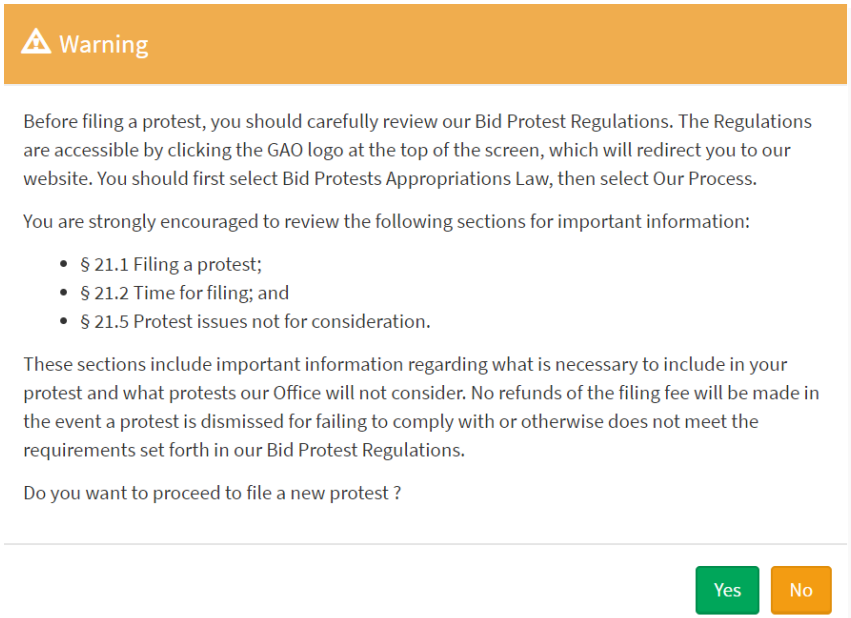
[Contractor Representative Name]
[Title]
[Contractor Name]
[Street Address]
[City, State ZIP Code]
[Contractor Representative telephone no.]
[Contractor Representative email address]

## Figure A.II

## GAO Bid Protest Process

This figure shows the bid protesting process at the GAO. The image on the left shows the pop-up message that reminds potential bid protesters of the bid protest regulations and the timeline. The image on the right shows the page that bid protesters use for bid protest submissions. Source: https://epds.gao.gov/protest-request.

(a) Bid-protest warning




(b) Bid protest submission

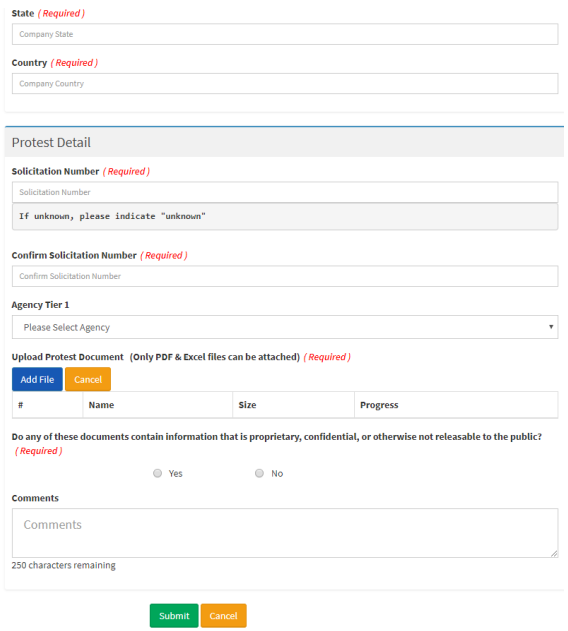


**Figure A.III**

**Protesters at the GAO vs. Other Venues**

This figure presents a Venn diagram of GAO and non-GAO cases related to government procurement contracts. It shows the number of cases involving firms that exist in GAO and non-GAO datasets. The areas of the sets are proportional to the number of cases they contain. Non-GAO cases consist of cases at the Court of Appeals, Court of Claims, Court of International Trade, District Courts, and the Supreme Court, which are labeled collectively as *Other Venues*. The data on *Other Venues* is drawn from the Thomson Reuters Westlaw dataset. It spans all cases against the U.S. government concerning the allocation of government contracts. We manually merge Thomson Reuters Westlaw data with bid protest data from GAO and government contracting data from USASpending.gov  contractor firm name.

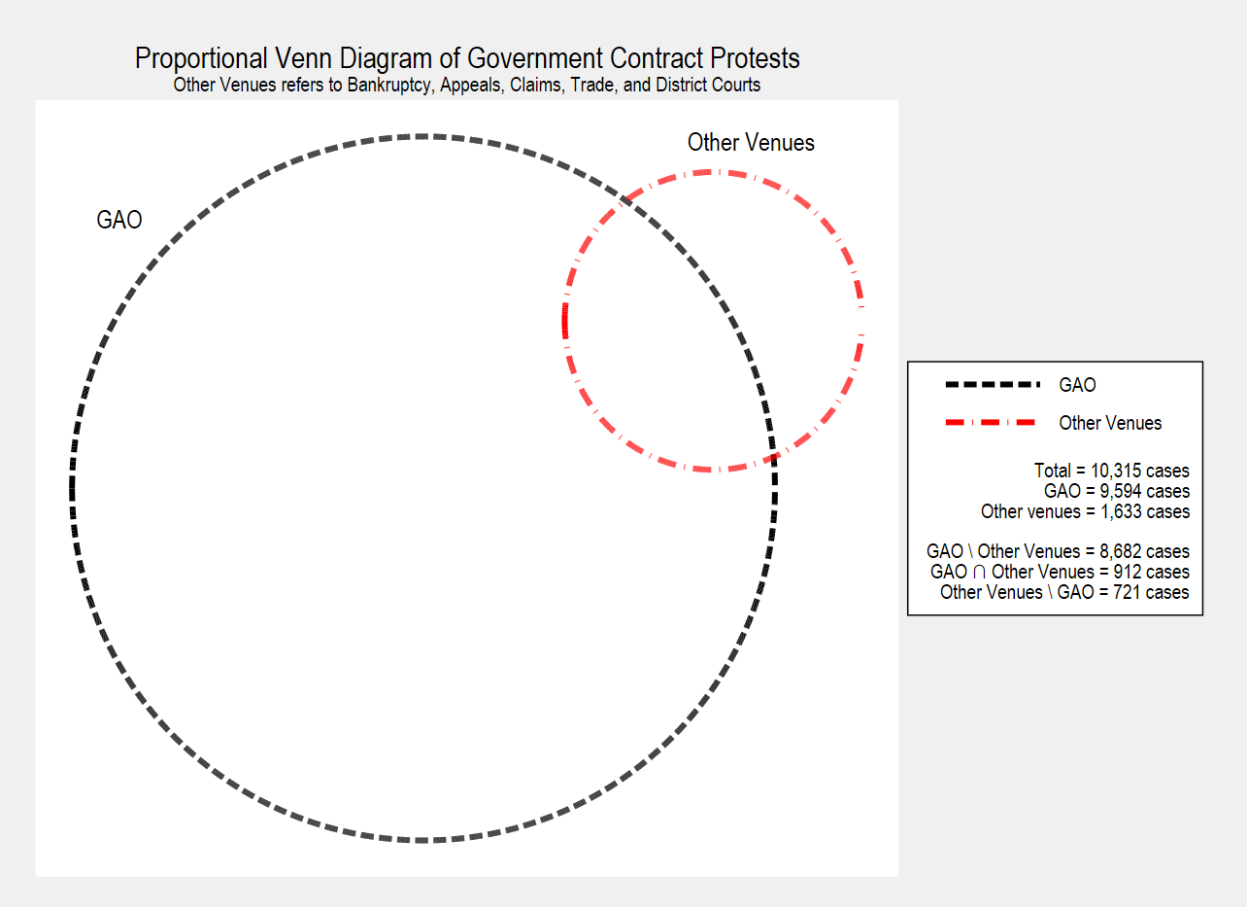


**Figure A.IV**

**GAO and non-GAO Cases over Time**

This figure shows the number of procurement cases filed with the GAO, Court of Appeals, Court of Claims, Court of International Trade, District Courts, and the Supreme Court, by year during the period 2005–2016. Non-GAO cases are drawn from the Thomson Reuters Westlaw dataset.

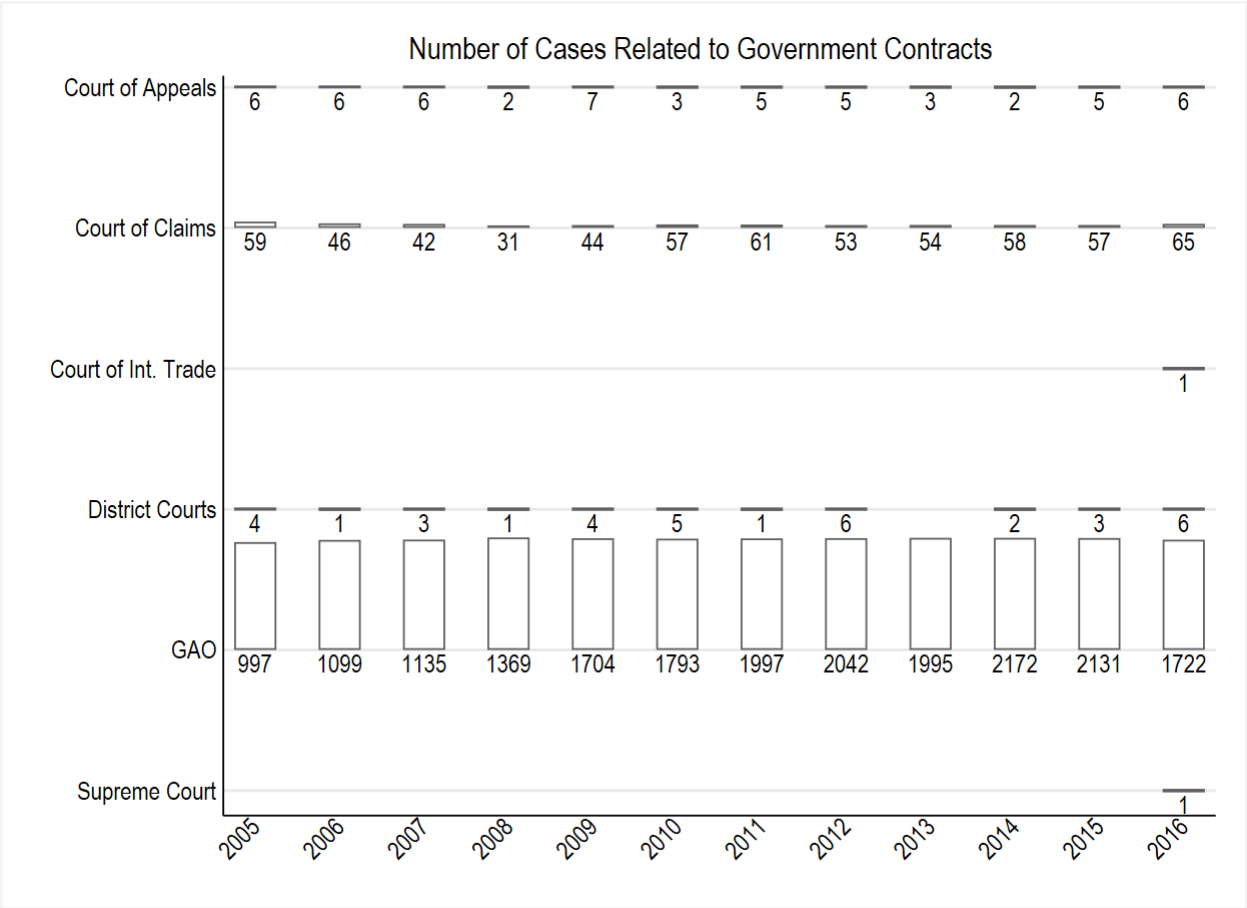
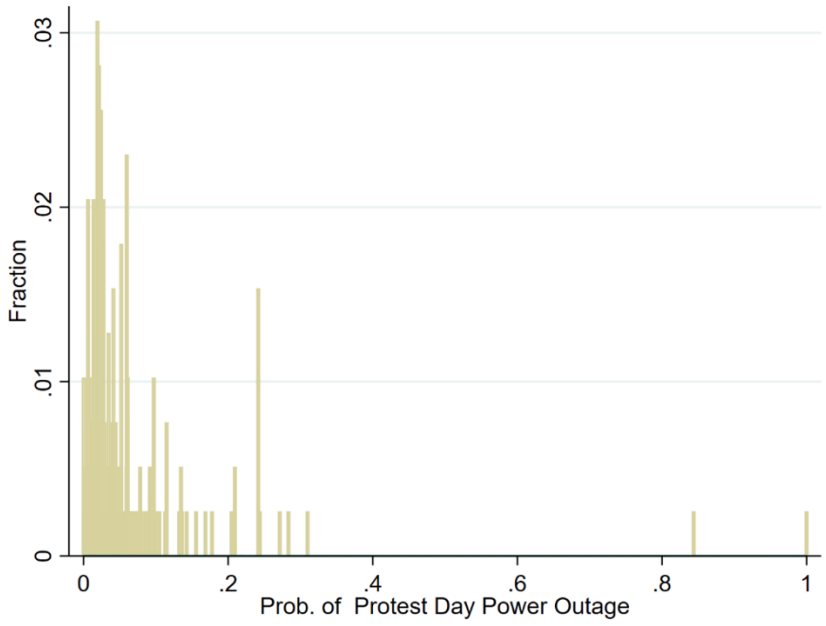


**Figure A.V**

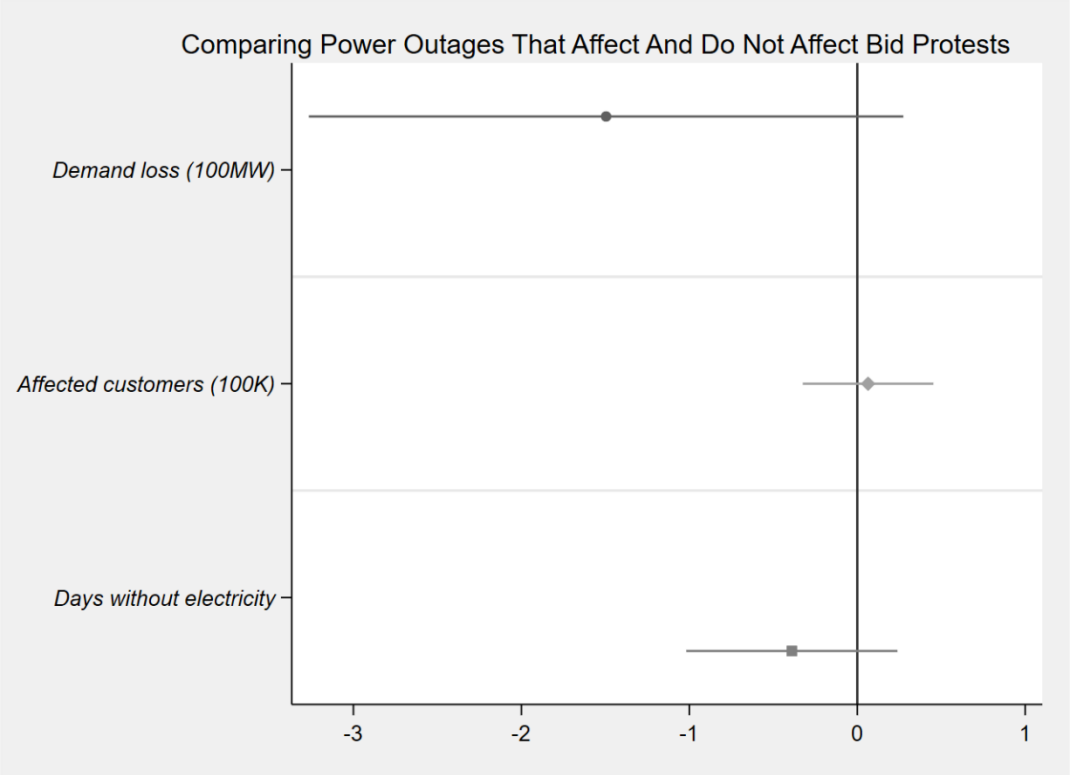
**Histogram for Prob. of Protest Day Power Outage**

Panel A shows the histogram for our main instrument, *Prob. of Protest Day Power Outage*. The X-axis shows *Prob. of Protest Day Power Outage* and the Y-axis shows fractions. Panel B compares power outages based on demand loss, affected number of customers, and days without electricity. We present differences in means (Outages that do not coincide with bid protests – Outages that coincide with bid protests) along with their 90% confidence intervals.

**Panel A. Histogram for Prob. of Protest Day Power Outage**



**Panel B. Power Outages Outside vs. Inside Bid Protest Window**



## Figure A.VI

### Protesting the Same Contract After Being Exposed to the Same Power Outage

This histogram shows how firms that got impacted by the same power outage compare in terms of the speed in protesting the same contract. The X-axis shows the number of days between the latest and the earliest bid protests against the same solicitation by multiple protesters facing the same power outages, and the Y-axis shows fractions.

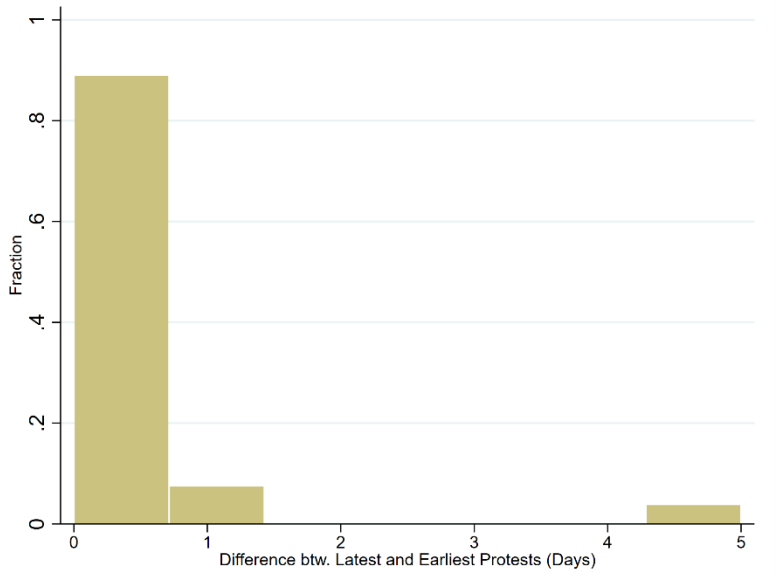
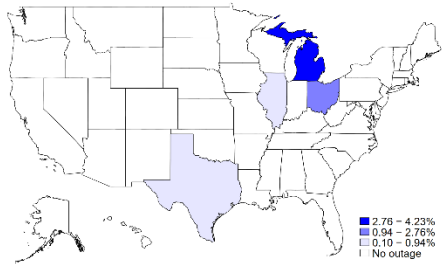


**Figure A.VII**

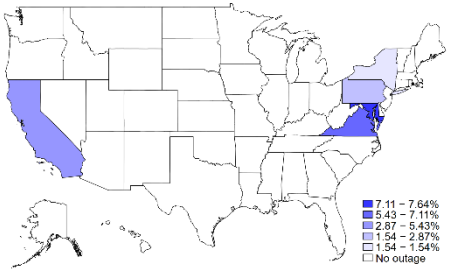
**The Probability of Protest Day Power Outage Across Time and Geographies**

The map shows the geographic and times series distributions of *Prob. of Protest Day Power Outage*, which is equal to the number of electricity customers in a bid protester firm's state that experienced a major and unexpected power outage on the day of protest filing, divided by the total number of electricity customers in bid protester firm's state. We use data from the merged GAO-USASpending-EIA universe, spanning the period 2005–2016. To identify locations of bid-protester firms, we manually merge firm names in the GAO bid protest dataset and the USASpending dataset. To identify bid protests that coincide with unanticipated power outages, we collect power outage data from U.S. Energy Information Administration (EIA)'s Electric Power Monthly reports. EIA lists major electric disturbances and unusual occurrences in Appendix B1 and Appendix B2 of each Electric Power Monthly report. Using the merged sample, we count the number of bid protests filed during unanticipated power outage days in each state. States with higher power outage pervasiveness are reported in darker blue.

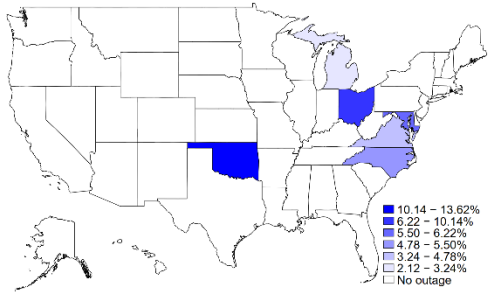
Prob. of Protest Day Power Outage in 2005



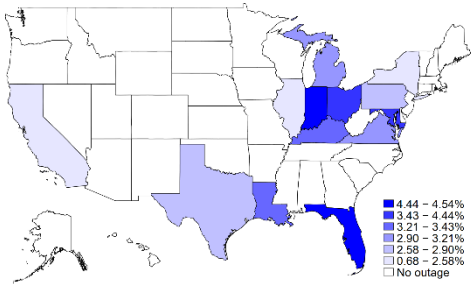

Prob. of Protest Day Power Outage in 2006



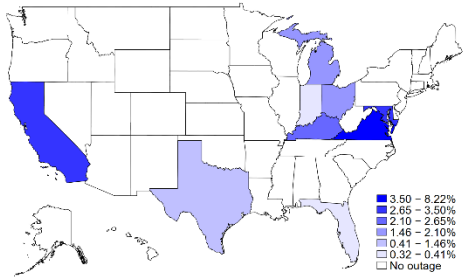

Prob. of Protest Day Power Outage in 2007



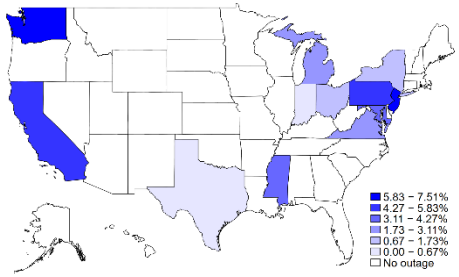

Prob. of Protest Day Power Outage in 2008



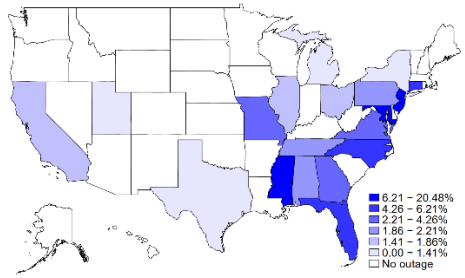

Prob. of Protest Day Power Outage in 2009



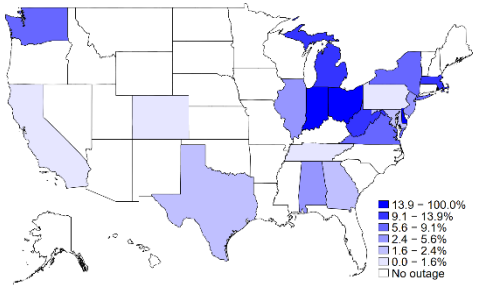

Prob. of Protest Day Power Outage in 2010



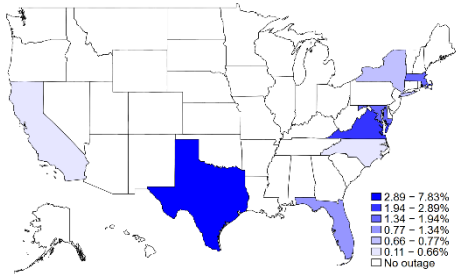

Prob. of Protest Day Power Outage in 2011



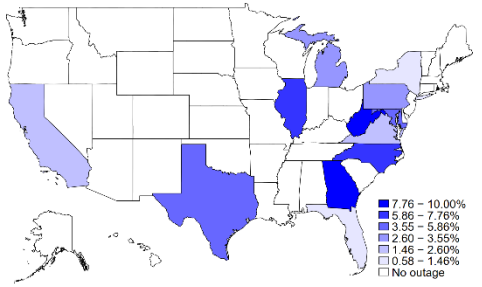

Prob. of Protest Day Power Outage in 2012



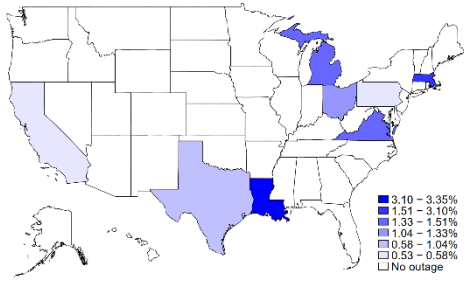

Prob. of Protest Day Power Outage in 2013




Prob. of Protest Day Power Outage in 2014




Prob. of Protest Day Power Outage in 2015




Prob. of Protest Day Power Outage in 2016

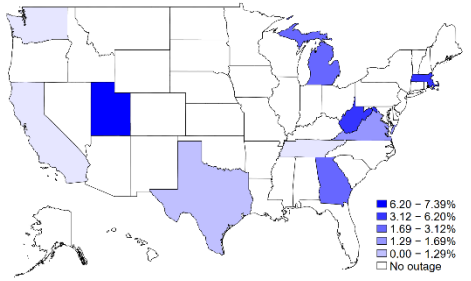
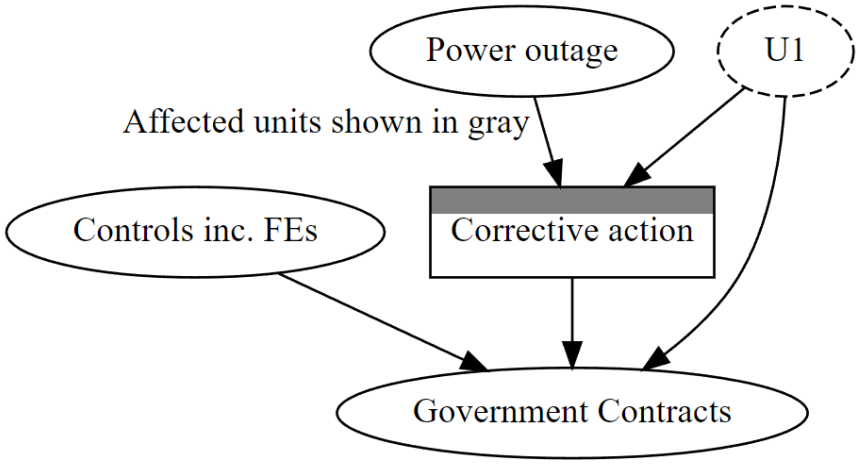


**Figure A.VIII**

**Causal Diagrams**

Panel A presents a simplified causal diagram that summarizes our empirical methodology described in Section 4. Panel B presents an alternative causal diagram that highlights another potential confounder, *U2*, between the instrument (Power outage) and the outcome variable (Government contracts), in addition to potential confounder U1 between Corrective action and Government contracts.

**Panel A: Our Model**




**Panel B: An Alternative Model**

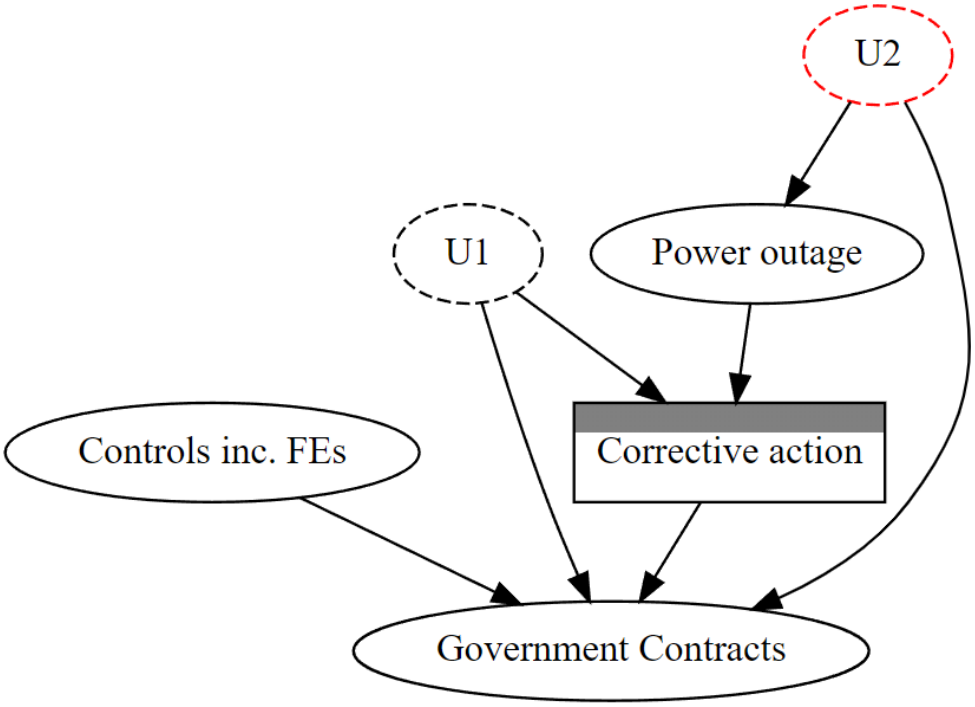


**Figure A.IX**

**Comparing Exposed vs. Unexposed Firms to Power Outages**

This figure compares protesting firms that are and are not exposed to power outages during their protest window along several firm characteristics potentially correlated with the likelihood of experiencing a power outage. We compare firms based on firm-level characteristics and local economic outcomes. We present differences in means (Exposed firm mean – Unexposed firm mean) along with their 90% confidence intervals. Coefficient estimates for differences in *Logged Revenue, Logged Employees, Logged Local GDP, Logged Income growth, Logged Labor Market participation,* and *Logged Unemployment Rate* are rescaled for the figure's readability. Variables are defined in Table III and Table A.V captions.

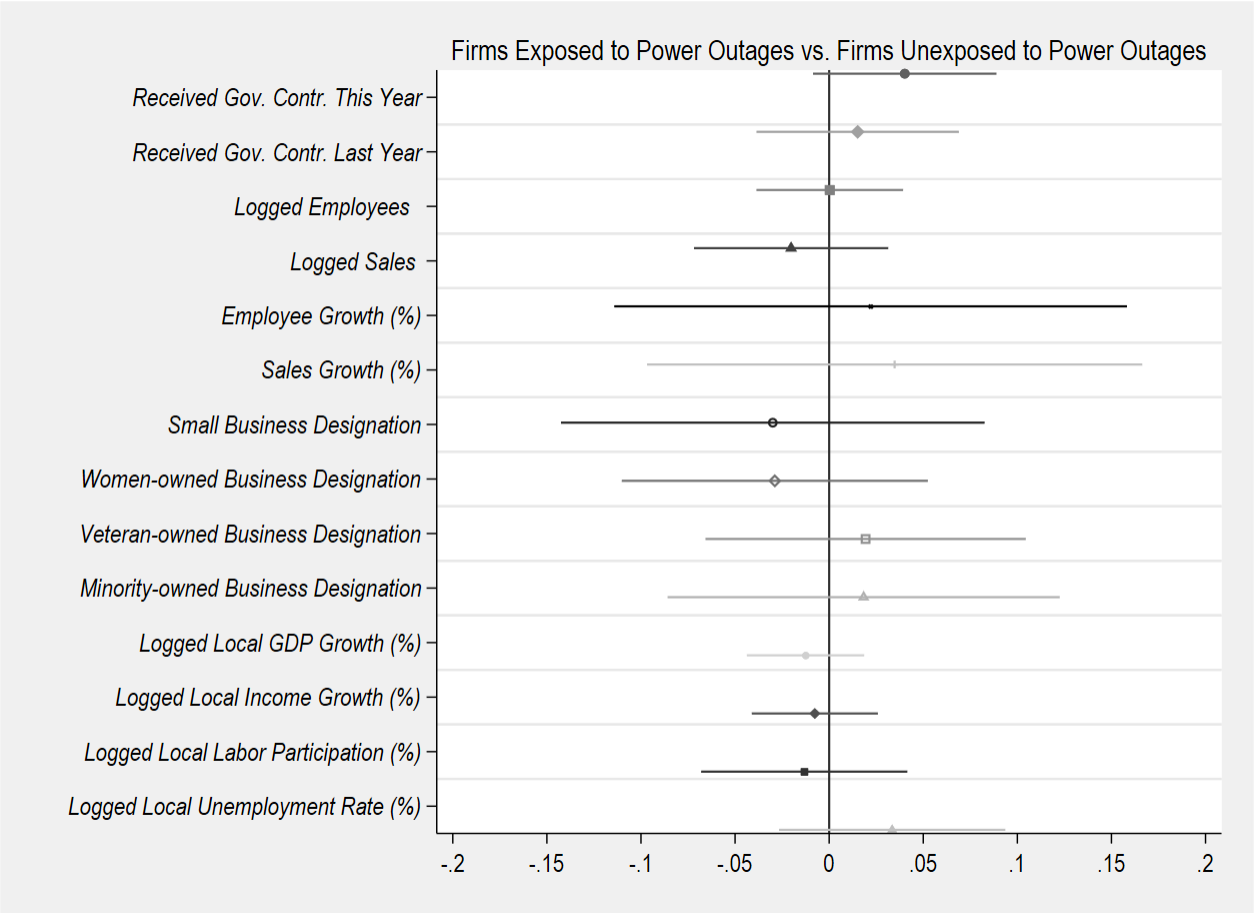


**Figure A.X**

**Compliance and Power Outage**

This figure presents findings from comparing characteristics of firms that were (i) successful or unsuccessful in their bid protests and (ii) exposed to power outages during their bid protests or not. We present differences in means along with their 90% confidence intervals. Detailed variable descriptions are presented in Table III's caption.

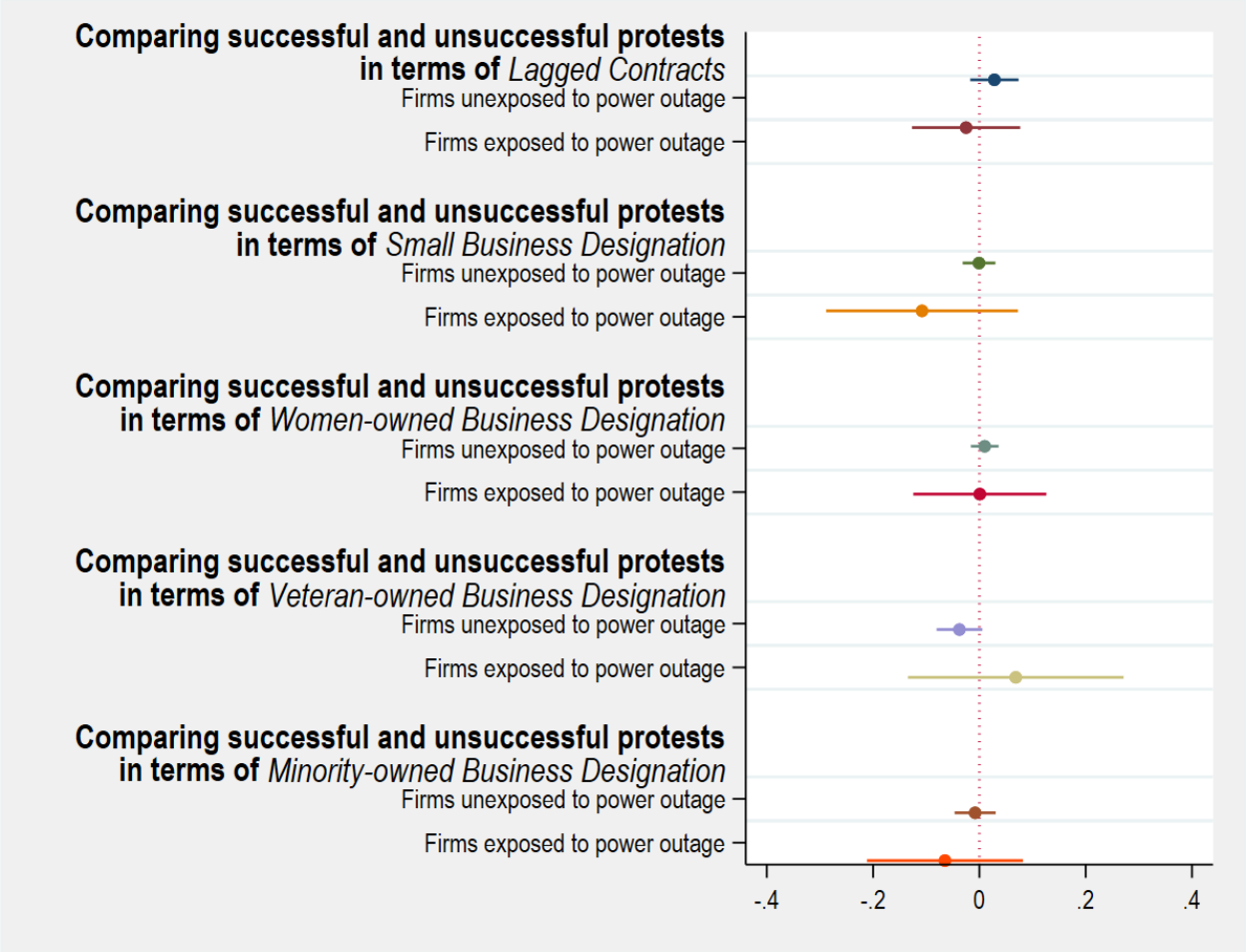


**Figure A.XI**

**2SLS Results Across Different Docket Sizes**

This figure presents results from the 2SLS procedure described in Section 4 on GAO dockets with varying numbers of bid protests. The y-axis shows second-stage regression coefficients and the x-axis shows the number of protests within dockets. The bars in dark denote a statistical significance of 10% or better. To estimate the 2SLS coefficient for dockets with *single* bid protests, we match protesters with government contractors that receive government contracts from protested agencies in the year of the bid protest. Our matching criteria are detailed in the Appendix section.

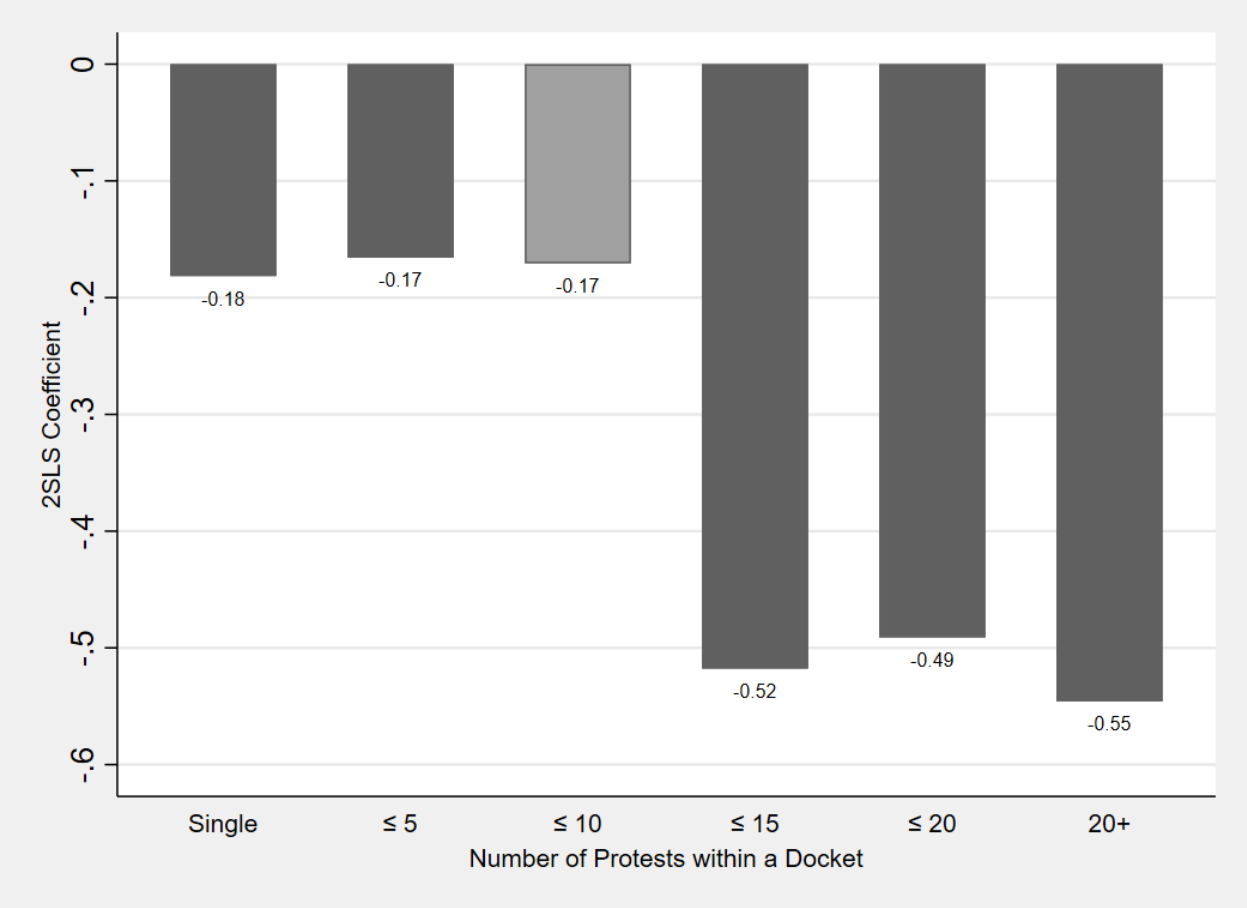


**Figure A.XII**

**Agency Discretion in Contract Allocation**

This figure presents how corrective actions influence the probabilities of receiving government contracts with varying degrees of government-agency discretion. We study all contract types under the merged GAO-USASpending universe. As contracts with low government agency discretion, we examine *Government-wide Acquisition Contracts* referring to contracts, in which multiple government agencies align their interests to purchase goods or services. Under contracts with high government agency discretion, *Established Contracts* refers to delivery orders or blanket purchase agreements, which are orders for anticipated supplies and are placed under established government contracts. *Definitized Contracts* refers to definitized contracts, i.e., contracts with an agreement on, or determination of, contract terms, specifications, and price. Under the FAR, most contracts are definitized within 180 days of the initial (letter) contracts that authorize the contractor to begin immediately manufacturing supplies or performing services, or before the completion of 40 percent of the work to be performed. *Other Discretionary Contracts* refers to purchase orders, indefinite-delivery contracts, and basic ordering agreements. These refer to contracts, terms of which will be definitized in the future, or contracts that are not established, e.g., one-off contracts. *Discretionary Contracts Combined* refers to *Established Contracts*, *Harder-to-cancel Contracts*, or *Other Discretionary Contracts*. We show results from the 2SLS procedure described in Section 4. The x-axis shows second-stage regression coefficients for *Corrective Action* and their 90% confidence intervals, and the y-axis lists contract types.

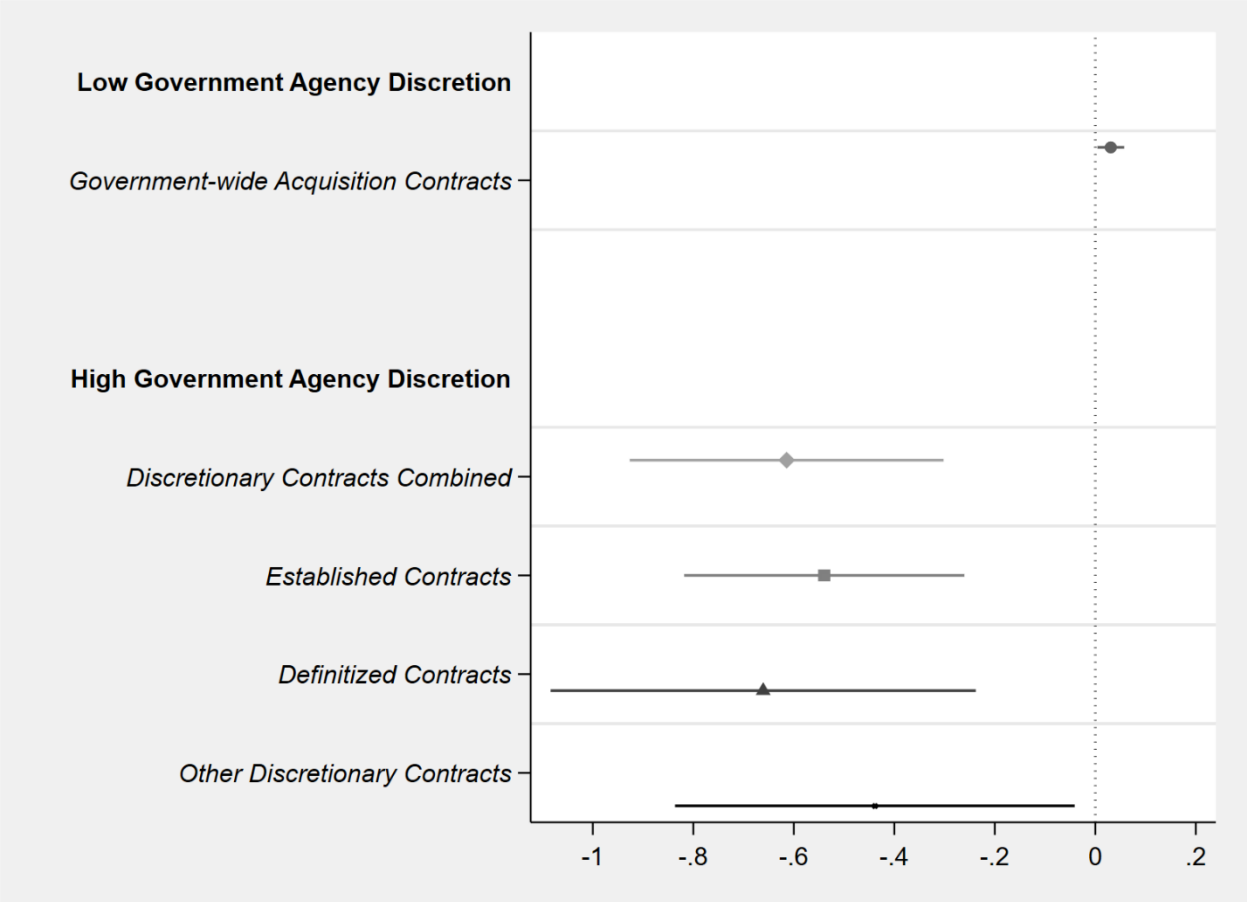


**Figure A.XIII**

**Computer Vision and Effects of Natural Disasters**

The satellite images show how a tornado disrupted local businesses on Villager Rd, Dallas, TX 75230. The first image in Panel A is from 10/19/2019, the second image in Panel B is from 10/22/2019, and the third image in Panel C shows the visual difference between these two satellite images, highlighting the route of the tornado and the affected establishments (circled with bold colors). The tornado hit on 10/21/2019.

**Panel A. Before the tornado**


**Panel B. After the tornado**


**Panel C. Detecting differences between Panel A and Panel B using computer vision**

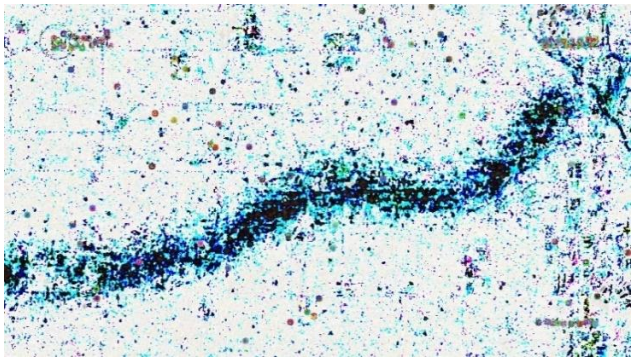
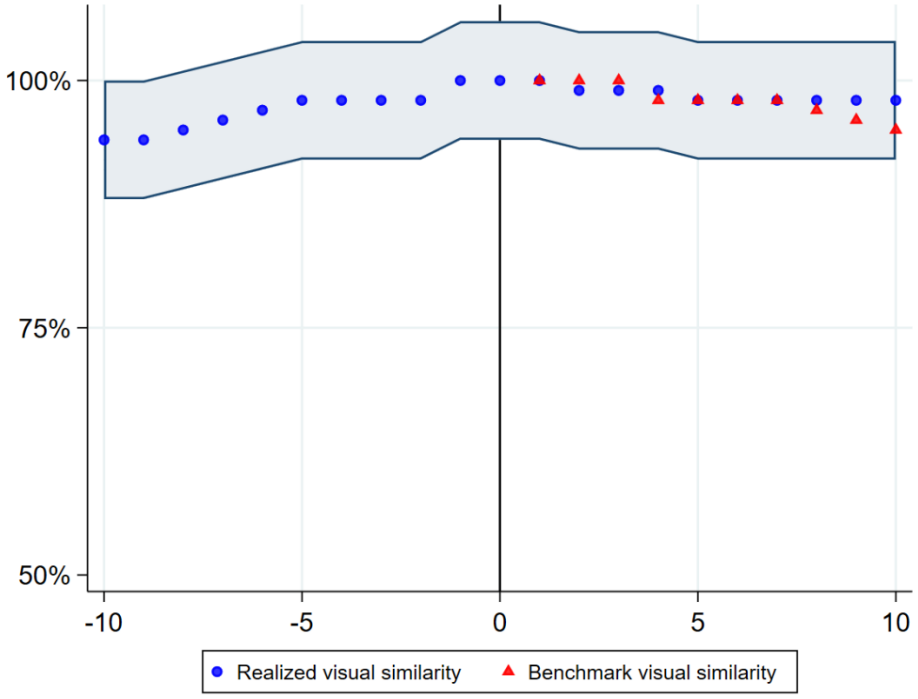


**Figure A.XIV**

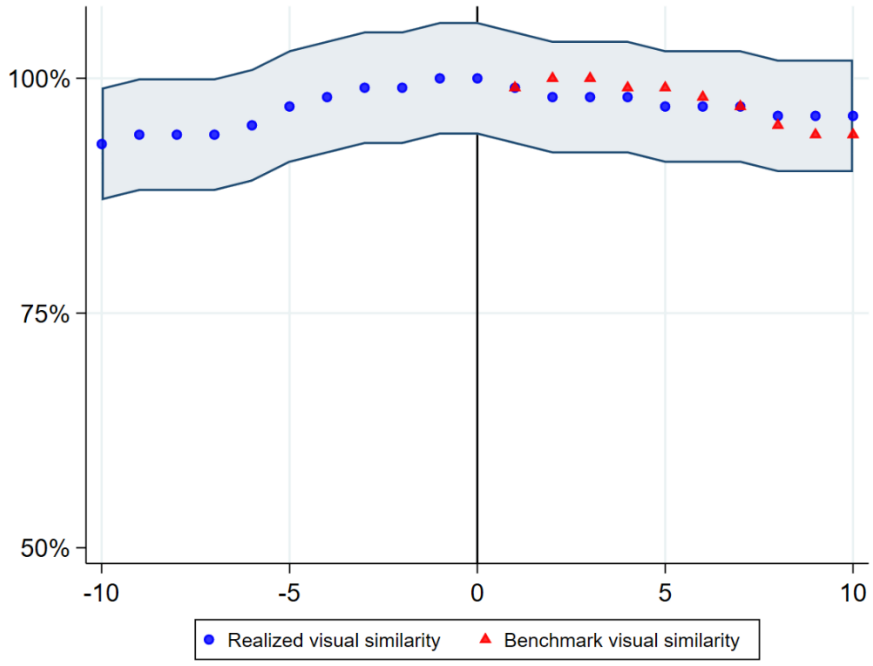
**Using Computer Vision to Analyze Bid-protester Firms' Surroundings**

Panel A presents our results from comparing satellite images of bid protester firms' surroundings in the event time of bid protests. These bid protesters experience significant power outages on bid protest filing days. The event window contains 10 days before and after bid protest filing dates. The confidence intervals indicate significance at the 5% level, and the red triangle on day t demonstrates visual similarity from day -t, i.e., the day symmetric across the y-axis. Panel B presents results from a placebo test that shows visual changes from a year ago (relative to bid protest filing dates). Panel C shows visual changes after ten natural disasters. The placebo test and the selected natural disasters are explained in detail in Part C of Section IV.

**Panel A. Comparing satellite images that were taken on bid protest filing days with satellite images that were taken within the -10 days to +10 days event window.**



**Panel B. Placebo test on visual changes from a year ago**




**Panel C. Visual changes after natural disasters**