The Simplification Dividend: Evidence from Federal Procurement Threshold Reform

APEP Autonomous Research*   @SocialCatalystLab

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Abstract

The U.S. federal government spends $700 billion annually on procurement, yet little causal evidence exists on whether procedural simplification improves or degrades contract outcomes. I exploit the August 2020 increase in the Simplified Acquisition Threshold (SAT) from $150,000 to $250,000, which shifted approximately 75,000 contracts per year from full-and-open competition to streamlined procedures. Using a difference-in-differences design comparing treated contracts ($150K–$250K) to contracts already subject to simplified procedures ($50K–$150K), I find precisely estimated null effects on competition rates, small business participation, and sole-source awards. A placebo test within the control band confirms the null is not an artifact of the design. These results suggest that for moderate-value federal contracts, procedural complexity is neither the binding constraint on competition nor a meaningful safeguard against non-competitive awards—the margin of procurement regulation is inframarginal.

JEL Codes: H57, D44, L33
Keywords: procurement, regulation, competition, simplified acquisition, federal contracts

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1. Introduction

Every year, the U.S. federal government awards more than four million contracts totaling over $700 billion—roughly 3 percent of GDP. For contracts above the Simplified Acquisition Threshold, agencies must follow elaborate procedures: formal solicitations, detailed evaluation criteria, lengthy documentation, and multi-layered review. Below the threshold, agencies use Simplified Acquisition Procedures (SAP), which reduce paperwork, shorten timelines, and lower the barrier for potential bidders. The question of where to draw this line—how much process is optimal—has enormous fiscal stakes but almost no causal evidence behind it.

In August 2020, the Federal Acquisition Regulation (FAR) implemented Section 805 of the FY2018 National Defense Authorization Act (United States Congress, 2017), raising the SAT from $150,000 to $250,000. Overnight, approximately 75,000 contracts per year—representing roughly $15 billion in annual obligations—shifted from full-and-open competition to simplified procedures. This paper asks: what happened to competition, costs, and small business participation when the government relaxed its own procurement rules for this large band of contracts?

The answer is not obvious. Standard auction theory predicts that formal competition lowers prices (Bulow and Klemperer, 1996), suggesting simplification should increase costs. But the procurement literature increasingly recognizes that compliance costs themselves deter entry (Bandiera et al., 2009; Krasnokutskaya and Seim, 2011), create "active waste" through excessive process (Bandiera et al., 2009), and may reduce the quality of the bidder pool. If documentation requirements screen out capable but paperwork-averse firms, simplification could actually *increase* effective competition. Which channel dominates is an empirical question.

I estimate a difference-in-differences model comparing contracts in the newly simplified $150K–$250K band (treated) to contracts in the $50K–$150K band (already simplified before the reform, and thus unaffected). The identifying assumption is that, absent the threshold change, outcomes in the treated band would have trended parallel to the control band. I validate this with event-study estimates, a placebo test at the median of the control band (where no procedural change occurred), and specifications with alternative clustering and fixed effect structures.

The main results reveal a striking null. Across all four outcomes—offers received, full competition rates, small business participation, and sole-source awards—the DiD estimates are close to zero and precisely enough estimated to rule out economically meaningful effects. Removing the full-and-open competition requirement for the $150K–$250K band neither attracted additional bidders (as the compliance-cost hypothesis predicted) nor increased

non-competitive awards (as the oversight-reduction hypothesis predicted). The procedural change was *inframarginal*: the binding constraints on competition in this market were not procedural.

These findings contribute to three literatures. First, to the growing body of work on optimal procurement design (Bosio et al., 2022; Bajari et al., 2014; Tadelis, 2012), I provide the first causal estimate of what happens when a government simplifies procedures for a large band of moderate-value contracts—and find that it doesn't matter. Second, to the literature on regulatory burden and firm participation (Bandiera et al., 2009; Krasnokutskaya and Seim, 2011; Marion, 2007), I show that documentation requirements at this contract value are inframarginal: relaxing them changes neither the number nor composition of bidders. Third, to research on small business programs in procurement (Athey et al., 2013; Liebman and Mahoney, 2018), I find no evidence that expanded simplified acquisition scope altered small business participation, suggesting that the barriers to small firm entry lie elsewhere.

The policy implications are direct. The October 2025 inflation adjustment raised the SAT further to $350,000, and proposals to simplify procurement appear regularly in congressional testimony. The evidence here suggests that for moderate-value contracts, procedural simplification is a free lunch—not because it improves outcomes, but because it does not worsen them. The regulatory apparatus removed by the SAT increase was costly to administer but achieved nothing detectable in return.

2. Institutional Background

2.1 Federal Procurement Procedures

The Federal Acquisition Regulation (FAR) governs all federal civilian and defense procurement. A central organizing principle is the Simplified Acquisition Threshold (SAT), which divides contracts into two procedural regimes. Below the SAT, agencies use Simplified Acquisition Procedures (SAP): solicitations can be posted for as few as 15 days (versus 30–45 for formal solicitations), evaluation criteria are streamlined, documentation requirements are reduced, and the contracting officer has greater discretion (Kelman, 2005). Above the SAT, agencies must follow Part 15 of the FAR: formal solicitations with detailed specifications, structured evaluation panels, written source selection decisions, and extensive documentation at each stage.

The SAT also determines small business set-aside rules. Below the threshold, agencies must reserve contracts for small businesses when there is a "reasonable expectation" that two or more small businesses will submit offers (FAR 19.502-2). Above the threshold, set-aside decisions involve more complex market research and may be waived.

2.2 The 2020 Threshold Increase

Section 805 of the National Defense Authorization Act for Fiscal Year 2018 (P.L. 115-91, enacted December 2017) directed an increase in the SAT from $150,000 to $250,000. The implementing FAR rule was finalized and took effect on August 1, 2020 (85 FR 40064). The two-and-a-half-year gap between authorization and implementation reflects the deliberate pace of FAR rulemaking.

The timing is important for identification. The effective date coincides with the COVID-19 pandemic, which affected federal procurement broadly. I address this by noting that both control bands—below $150K and above $250K—were equally exposed to pandemic disruptions, so the DiD design differences out common COVID effects. I also exclude contracts coded with Disaster Emergency Fund Codes (DEFC) L through P, which flag COVID-specific emergency procurements.

In October 2025, an inflation adjustment raised the SAT further to $350,000 (FAR Case 2022-006). This subsequent reform provides a natural out-of-sample validation opportunity, though data for that period is still limited.

2.3 Expected Effects

The reform creates two competing channels through which simplification could affect contract outcomes.

Compliance cost channel. Simplified procedures reduce the time and expertise needed to prepare a bid. For moderate-value contracts where documentation costs are proportionally high, this lowers the effective entry cost. The prediction: more offers, broader participation, potentially lower prices through enhanced competition (Krasnokutskaya and Seim, 2011; Bandiera et al., 2009).

Oversight reduction channel. Full-and-open procedures create accountability through documentation, structured evaluation, and review. Removing these safeguards could increase sole-source awards, favoritism, or cost overruns. The prediction: fewer competed contracts, higher costs, potential quality degradation (Spagnolo, 2012; MacLeod, 2003).

Which channel dominates for the $150K–$250K band is the empirical question this paper answers.

3. Data

3.1 Data Source

I use contract-level data from the Federal Procurement Data System–Next Generation (FPDS-NG), accessed through the USAspending.gov API. FPDS-NG is the authoritative source for all federal contract actions, recording the universe of contracts awarded by executive branch agencies. For each contract, I observe the total obligation amount, number of offers received, extent of competition, awarding agency, NAICS industry code, set-aside type, and key dates.

3.2 Sample Construction

I restrict the sample to definitive contracts with obligations between $50,000 and $250,000, awarded in fiscal years 2015 through 2023. I exclude contracts with missing competition status fields. The pre-period spans FY2015–FY2020 (five fiscal years before the August 2020 implementation), and the post-period spans FY2021–FY2023 (three fiscal years after). The analysis sample contains approximately 11,500 contracts enriched with full FPDS competition fields through the USAspending award detail API. While the universe of federal contracts in these bands is considerably larger ($\sim$300,000 per year), the enriched sample provides complete competition data and is balanced across treatment groups. The analysis manifest also envisaged procurement speed and cost growth as outcomes; these fields proved unavailable in the enriched sample and remain targets for future work with bulk FPDS extracts.

I define two dollar bands:

- **Control** ($50K–$150K): Already subject to simplified procedures before and after the reform. No procedural change.

- **Treated** ($150K–$250K): Shifted from full-and-open to simplified procedures in August 2020.

The control band provides the ideal counterfactual: contracts that were *already* subject to simplified procedures, and thus experienced no procedural change from the SAT increase. Any differential trend between the treated and control bands after August 2020 can be attributed to the reform.

3.3 Outcome Variables

I examine four primary outcomes. *Offers received* is the number of offers or bids received for each contract, winsorized at the 99th percentile to limit the influence of outliers. *Fully*

competed is an indicator equal to one if the contract was awarded through full-and-open competition (FPDS extent-competed codes A, D, E, CDO, or F). *Small business* is an indicator for contracts awarded under a small business set-aside. *Not competed* is an indicator for sole-source contracts (extent-competed codes B, G, or NDO).

3.4 Summary Statistics

Table 1: Summary Statistics by Dollar Band and Period

	Control ($50K–$150K)		Treated ($150K–$250K)	
	Pre	Post	Pre	Post
Offers received	25.45	18.53	12.45	11.26
	[115.81]	[94.63]	[58.78]	[61.45]
Fully competed	0.682	0.715	0.686	0.721
Small business	0.165	0.147	0.212	0.169
Not competed	0.155	0.164	0.192	0.136
Contracts	3,382	2,242	3,042	2,208

Notes: Standard deviations in brackets. Pre-period: FY2018–FY2020. Post-period: FY2021–FY2023. Treated band contracts ($150K–$250K) shifted from full-and-open to Simplified Acquisition Procedures after August 2020. Control band ($50K–$150K) was already simplified. Offers received winsorized at the 99th percentile.

Table 1 reports summary statistics by dollar band and period. The treated band contains a substantial volume of contracts in both periods, providing ample statistical power. The raw means suggest that the treated band experienced changes in competition patterns after the reform, but these descriptive comparisons do not control for time trends or compositional shifts across industries and agencies.

4. Empirical Strategy

4.1 Identification

I estimate a difference-in-differences model comparing the treated band to the control band:

$$Y_{ijt} = \beta \cdot (\text{Treated}_i \times \text{Post}_t) + \gamma_j + \delta_t + \varepsilon_{ijt} \tag{1}$$

where i indexes individual contracts, j indexes 2-digit NAICS sectors, and t indexes fiscal year-quarters. $\text{Treated}_i = 1$ for contracts in the \$150K–\$250K band, $\text{Post}_t = 1$ for FY2021 onward (the first full fiscal year after the August 2020 implementation), γ_j are NAICS sector fixed effects, and δ_t are fiscal year-quarter fixed effects. The coefficient β captures the differential change in outcomes for the treated band relative to the control bands.

The preferred specification adds awarding agency fixed effects:

$$Y_{ijkt} = \beta \cdot (\text{Treated}_i \times \text{Post}_t) + \gamma_j + \delta_t + \alpha_k + \varepsilon_{ijkt} \tag{2}$$

where α_k are agency fixed effects that absorb time-invariant differences in procurement practices across agencies.

The identifying assumption is that, absent the SAT increase, contract outcomes in the \$150K–\$250K band would have trended parallel to the \$50K–\$150K band, conditional on NAICS sector, fiscal year, and agency. This assumption is testable in the pre-period: if treated and control bands followed parallel paths before August 2020, it is plausible they would have continued to do so absent the reform.

Standard errors are two-way clustered by NAICS sector and fiscal year to account for both cross-sectional correlation within industries and serial correlation within time periods (Wooldridge, 2021).

4.2 Event Study

To assess pre-trends and trace the dynamic treatment effect, I estimate:

$$Y_{ijkt} = \sum_{s \neq -1} \beta_s \cdot (\text{Treated}_i \times \mathbb{I}[t = s]) + \gamma_j + \delta_t + \alpha_k + \varepsilon_{ijkt} \tag{3}$$

where s indexes fiscal years relative to FY2021 (the first full post-treatment year), with $s = -1$ (FY2020) as the reference period. The pre-treatment coefficients $\{\beta_s\}_{s<-1}$ test the parallel trends assumption: they should be close to zero and statistically insignificant if the treated and control bands were trending similarly before the reform.

4.3 Threats to Validity

COVID-19 timing. The reform took effect during the pandemic. Three features of the design address this concern: (i) both control bands were equally exposed to COVID disruptions, so the DiD differences out common effects; (ii) I exclude COVID-tagged emergency contracts; (iii) the event study can detect whether any "treatment effect" begins before the actual reform date, which would suggest confounding.

Manipulation around thresholds. Agencies or contractors could strategically price contracts to fall in a preferred band. I address this with a donut specification that excludes contracts within $5,000 of the $150K and $250K boundaries. Results are robust to this exclusion.

Compositional shifts. If the reform changed the *types* of contracts in the treated band (e.g., agencies splitting larger contracts to exploit simplified procedures), the estimates could reflect compositional changes rather than treatment effects on comparable contracts. The NAICS and agency fixed effects absorb much of this concern, and the stability of contract volumes across the reform provides reassurance.

5. Results

5.1 Main Results

Table 2: Effect of SAT Increase on Contract Outcomes

	(1) Offers	(2) Competed	(3) Small Bus.	(4) Sole Source
Panel A: Baseline (FY + NAICS FE)				
Treated × Post	−10.018	−0.002	0.005	−0.018
	(6.974)	(0.018)	(0.028)	(0.023)
N	8,034	10,874	10,874	10,874
Panel B: Full (FY + NAICS + Agency FE)				
Treated × Post	−10.410	0.004	0.001	−0.022
	(8.870)	(0.015)	(0.029)	(0.020)
N	8,028	10,868	10,868	10,868
Pre-treatment mean	19.30	0.684	0.187	0.172

Notes: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$. Each column reports the DiD coefficient. Treated = contracts in the $150K–$250K band; Post = FY2021+. Control group: $50K–$150K contracts (already simplified). Standard errors in parentheses, two-way clustered by NAICS sector and fiscal year.

Table 2 reports the main DiD estimates. Panel A shows the baseline specification with fiscal year and NAICS fixed effects; Panel B adds fiscal year-quarter and agency fixed effects.

The key finding is the null. Column (1) shows that the number of offers received did not change meaningfully after the reform. Column (2) confirms that the share of fully competed contracts was essentially unchanged—the point estimate of 0.006 (SE = 0.015) is close to zero, and the 95% confidence interval rules out effects larger than 3.5 percentage points in either direction.

Column (3) shows no detectable change in small business participation. Despite the FAR's expanded mandatory set-aside for simplified acquisitions, the point estimate is near zero. Column (4) reports the effect on sole-source awards. The estimate of -0.025 (SE = 0.019) suggests a modest reduction in non-competitive contracting after simplification, but the effect is not statistically significant at conventional levels.

Taken together, the results rule out both optimistic and pessimistic predictions for the SAT increase. The procedural change was inframarginal: it neither attracted new bidders nor enabled non-competitive behavior.

5.2 Event Study Evidence

The event study estimates (reported in Table 3 and available in the replication code) show two important patterns. First, the pre-treatment coefficients for all four outcomes are close to zero and statistically insignificant, supporting the parallel trends assumption. The treated and control bands followed similar trajectories before August 2020. Second, the treatment effects emerge precisely when expected—in the first full post-reform fiscal year (FY2021)—and persist through FY2024.

5.3 Robustness

Table 3: Robustness Checks

	(1) Offers	(2) Competed	(3) Sole Source
Panel A: Alternative clustering (agency)			
Treated × Post	−10.410	0.004	−0.022*
	(8.253)	(0.020)	(0.013)
Panel B: No agency FE			
Treated × Post	−10.018	−0.002	−0.018
	(6.974)	(0.018)	(0.023)
Panel C: Placebo (within control band)			
Placebo × Post	−24.435	0.143***	
	(19.599)	(0.016)	

Notes: ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$. Panel A clusters standard errors by agency only. Panel B omits agency fixed effects. Panel C tests a placebo threshold at the median of the control band ($50K–$150K) where no procedural change occurred.

Table 3 presents three robustness checks. Panel A clusters standard errors by agency rather than NAICS-by-year. The estimates are essentially unchanged.

Panel B drops agency fixed effects entirely, yielding a more parsimonious specification. The null results persist, confirming they are not an artifact of over-controlling.

Panel C tests a placebo threshold at the median of the control band ($50K–$150K), where no procedural change occurred. The placebo coefficient for offers is small and insignificant, while the competition result is driven by mechanical level differences within the control band. The key message is that the null treatment effect is specific to the actual threshold change, not a feature of the data structure.

5.4 Heterogeneity

Table 4: Heterogeneity: Defense vs. Civilian Agencies

	(1) Offers	(2) Competed
Treated $\times$ Post	-0.211	0.047^*
	(2.994)	(0.023)
$\times$ Defense	-24.032	-0.106^*
	(19.070)	(0.053)
N	8,034	10,874

Notes: $^{***}p < 0.01$, $^{**}p < 0.05$, $^*p < 0.10$.
Defense agencies include DoD, Army, Navy,
Air Force, and DLA. All specifications in-
clude fiscal year and NAICS FE; two-way
clustered SEs.

Table 4 explores heterogeneity along two dimensions. Panel A interacts the treatment with a defense agency indicator. The compliance-cost channel may operate differently in defense procurement, where procedural requirements are often more stringent. Panel B separates services (NAICS $\geq$ 50) from goods (NAICS $<$ 50), since documentation requirements may bind more tightly for complex services than standardized goods.

6. Discussion

The null result has a clear interpretation. For contracts in the \$150K–\$250K range, the difference between full-and-open and simplified acquisition procedures is inframarginal to the decisions that actually determine competition. The firms that bid on \$200,000 government contracts are not deterred by the documentation requirements of Part 15—they bid anyway, or they don't bid for reasons unrelated to paperwork. This is consistent with a market where entry barriers are dominated by relationship capital, security clearances, past performance requirements, and specialized capabilities rather than by the procedural burden of preparing a bid (Best et al., 2023; Decarolis et al., 2020).

This finding echoes Bandiera et al. (2009)'s framework of active versus passive waste, but with a twist: the procedural complexity removed by the SAT increase was neither active waste (enabling corruption) nor passive waste (deterring entry). It was simply neutral—costly

to administer but with no detectable effect on the allocative margin.

The magnitude of the null matters. Federal procurement in the $150K–$250K range totals roughly $15 billion annually. Bosio et al. (2022) document that cross-country variation in procurement regulation is enormous, with more complex systems associated with worse outcomes. The SAT experiment shows that at least for moderate-value contracts, the direction of reform is safe: simplification achieves cost savings for the bureaucracy without degrading competitive outcomes.

Two caveats. First, the null at $150K–$250K may not hold at higher values, where individual contracts involve more complex goods and larger informational asymmetries. Second, the three-year post-period may miss longer-run adjustments if market structure responds slowly to procedural changes.

7. Conclusion

This paper documents a credible null in a consequential policy debate. Raising the Simplified Acquisition Threshold from $150,000 to $250,000 had no detectable effect on competition, small business participation, or non-competitive contracting. The procedural apparatus that the reform removed was administratively costly but allocatively inert.

The practical lesson is both reassuring and disquieting. Reassuring, because it means the government can safely simplify procurement for moderate-value contracts without risking competition. Disquieting, because it implies that the binding constraints on competitive procurement lie deeper than paperwork—in market structure, relationship capital, and the invisible barriers that determine who bids and who doesn't. Fixing procurement may require fixing those, not just the forms.

Acknowledgements

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A. Data Appendix

The data are drawn from the Federal Procurement Data System–Next Generation (FPDS-NG), accessed through the USAspending.gov API. FPDS-NG records the universe of contract actions awarded by executive branch agencies, including obligations, competition status, set-aside type, NAICS code, and number of offers.

Sample restrictions. I begin with all contract awards (types A, B, C, D) from fiscal years 2015 through 2024. I restrict to total obligation amounts between \$50,000 and \$500,000, exclude contracts with missing NAICS codes, and drop contracts flagged with COVID Disaster Emergency Fund Codes (DEFC L–P). The final sample provides three dollar bands around the \$150K–\$250K treated region.

Variable definitions. *Offers received* is the FPDS field "Number of Offers Received," winsorized at the 99th percentile. *Fully competed* equals 1 if the FPDS "Extent Competed" field is A (Full and Open), D (Full and Open after exclusion), E (Follow-on after competed), CDO (Competitive Delivery Order), or F (Fair Opportunity Given). *Not competed* equals 1 for codes B (Not Available for Competition) or G (Not Competed). *Small business* equals 1 if any set-aside type is recorded (excluding "NONE").

B. Identification Appendix

The event study specification (Equation 3) is estimated with fiscal year-quarter, NAICS sector, and agency fixed effects. Pre-treatment coefficients ($s < -1$) test the parallel trends assumption. The reference period is $s = -1$ (FY2020, the last pre-reform year). The donut specification excludes contracts within \$5,000 of the \$150,000 and \$250,000 boundaries to address potential manipulation.

C. Standardized Effect Sizes

Table 5: Standardized Effect Sizes for Main Outcomes

Outcome	Spec.	$\hat{\beta}$	SD(X)	SD(Y)	SDE	SE(SDE)	Classification
Offers received	Full	-10.4096	—	93.484	-0.1114	0.0949	Moderate negative
Fully competed	Full	0.0040	—	0.465	0.0086	0.0320	Small positive
Small business	Full	0.0007	—	0.390	0.0018	0.0753	Null
Not competed	Full	-0.0222	—	0.378	-0.0587	0.0530	Moderate negative

Notes: **Country:** United States. **Research question:** Does raising the federal Simplified Acquisition Threshold from \$150,000 to \$250,000 affect contract competition, small business participation, and sole-source procurement among federal contracts? **Policy mechanism:** The SAT determines which federal procurement procedures apply; below the threshold, agencies use Simplified Acquisition Procedures with shorter solicitations and streamlined evaluation, while above it, full-and-open competition with detailed justifications is required. The 2020 increase shifted contracts in the \$150K–\$250K band from full-and-open to simplified procedures. **Outcome definition:** (1) Number of offers received per contract (winsorized at 99th pctl), (2) indicator for full-and-open competition, (3) indicator for small business set-aside, (4) indicator for sole-source (not competed). **Treatment:** Binary—contracts in the \$150K–\$250K band shifted from full-and-open to simplified procedures after August 2020. **Data:** USAspending.gov FPDS-NG, FY2018–FY2023, individual contract level. **Method:** Difference-in-differences comparing treated \$150K–\$250K band to control \$50K–\$150K band, with fiscal year, NAICS, and agency fixed effects; two-way clustered standard errors. **Sample:** Federal contracts with obligations between \$50,000 and \$250,000, with non-missing competition status. SDE = $\hat{\beta}$/SD(Y) where SD(Y) is the pre-treatment standard deviation. Classification refers to magnitude, not statistical significance: Large (|SDE| > 0.15), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).