The Market for Immigration Detention*

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Abstract

We study the consequences of private immigration detention contracting, using presidential elections as shocks to enforcement expectations. ICE contractors earned cumulative abnormal returns of 20 percentage points after the 2024 election, the largest effect across seven agencies and five electoral cycles. This effect was concentrated entirely among detention contractors, with our results implying that GEO Group and CoreCivic in particular saw $1.8 billion in combined equity gains. A shift-share IV design reveals that a one-standard-deviation increase in procurement raised the county–month detainee rate by 2.7 per 100,000 population, roughly 32% of the pre-2016 mean. The marginal detainee had no criminal record: The procurement expansion increased detentions of workers whose detention removed them from the tax base, suppressed earnings, and generated no measurable public safety benefit.

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1 Introduction

A foundational question in public economics and political economy is where the boundary between the state and the market should lie (Hart et al., 1997; Shleifer, 1998). The US immigration enforcement setting offers a unique opportunity to answer it. The federal government holds tens of thousands of individuals in civil administrative custody across facilities operated predominantly by private contractors under per-diem arrangements (Transactional Records Access Clearinghouse (TRAC), 2025), and Project 2025 calls for Congress to mandate and fund a significant increase in detention capacity to bring the number of beds available daily to 100,000 (The Heritage Foundation, 2023), such that the politics of enforcement translate directly into private cash flows. This paper asks three questions: Do financial markets price enforcement expectations into contractor equity values? Does procurement expansion causally increase detention? What are the fiscal consequences for taxpayers?

This paper exploits presidential elections as shocks to immigration enforcement expectations. Elections compress policy uncertainty into a single event, and equity markets respond in real time (Fisman, 2001): By immediately repricing contractor shares, investors reveal what they expect for future procurement. After the 2024 election, ICE contractors experienced the largest post-election revaluation across all federal law enforcement agencies since 2008; this effect was concentrated entirely among detention contractors. A shift-share design reveals a large causal effect of procurement on detention operating entirely through detentions of individuals with no criminal record. These marginal detentions generated no measurable public safety benefit. The implied fiscal incidence is stark: For every dollar of net government cost, shareholders of the two dominant publicly traded contractors captured $3–4 in private returns.

Answering these questions requires use to solve three problems. First, contractor profit margins are not directly observed, so recovering them requires us to link equity market valuations to the procurement expansions they anticipate. Second, procurement and detention are simultaneously determined, so naive comparisons would confound supply and demand. Third, the fiscal incidence of procurement expansions depends on who is detained at the margin: Detentions of individuals with criminal records generates a public safety benefit that offsets the fiscal cost, while detentions of workers without criminal records does not. Instead, it removes them from the tax base and suppresses earnings. The two cases thus have opposite implications for the net cost of enforcement procurement.

We address each problem in turn. The immigration enforcement setting is well suited for the task. Immigration detention has largely been privatized: Immigration and Customs Enforcement (ICE) contracts extensively with private firms under per-diem arrangements (Ryo and Peacock, 2018), and contractor revenues scale linearly with custody volume. Funding of this system exhibits pronounced swings tracking the electoral cycle, as shown in Figure 1. Under the first Trump administration, the dedicated facility population rose from roughly 18,000 to a peak of 35,000; the Biden administration oversaw a decline to nearly 30,000; and the post-2024 surge had surpassed all prior peaks within months of the start of the second Trump administration. Because revenues scale with the detained population, electoral outcomes are the primary determinant of contractor firm value, making presidential elections the natural source of variation we exploit.

We draw on three data sources. We link the universe of federal law enforcement procurement contracts from USASpending.gov from 2008 to 2026 to daily equity returns from Center for Research in Security Prices (CRSP) and Compustat and to individual-level ICE detention records compiled by Transactional Records Access Clearinghouse (TRAC) (2025) and Deportation Data Project (2025) through Freedom of Information Act (FOIA) requests spanning 2003 through early 2025. These data allow us to study enforcement expansion through two complementary lenses: its capitalization into equity prices and its causal effect on the detained population.

We begin by investigating how presidential elections affect the stock returns of federal law enforcement contractors. Equity prices aggregate investors' expectations about future policy (Knight, 2006; Jayachandran, 2006; Snowberg et al., 2007; Wolfers and Zitzewitz, 2009), which makes them an informative signal of anticipated procurement changes. Presidential elections provide a sharp setting for this analysis: Trump's 2016 campaign centered on building a border wall and mass deportations (Bursztyn et al., 2023), and his 2024 campaign escalated those commitments, promising the largest domestic deportation operation in US history (The Heritage Foundation, 2023). Both campaigns signaled large-scale expansions of detention capacity that would directly benefit private contractors. We study all publicly traded contractors who hold contracts with seven federal law enforcement agencies—ICE, Customs and Border Patrol (CBP), the Federal Bureau of Investigation (FBI), the Federal Bureau of Prisons (BOP), the United States Marshals Service (USMS), the Drug Enforcement Agency (DEA), and the Bureau of Alcohol, Tobacco, and Firearms (ATF)—in the five presidential elections from 2008 to 2024.

Our first design uses synthetic difference-in-differences (SDID; Arkhangelsky et al.,

2021) and synthetic control methods (SC; Abadie et al., 2010; Acemoglu et al., 2016) to estimate the effect of each election on contractor stock returns based on the event-study approach to valuing political connections pioneered by Fisman (2001) and applied to law enforcement contractors by Ba et al. (2023). Each treated firm is compared to a weighted combination of publicly traded firms in the same industry, matched on pre-period return trajectories and firm characteristics. ICE contractors earned cumulative abnormal returns (CARs) of 20 percentage points (pp) in the month following the 2024 election, the largest effect across all seven agencies and all presidential elections since 2008. The response was concentrated entirely among detention contractors; information technology (IT), health, personnel, and weapons contractors showed zero effect. The specificity of this market response points to an anticipated expansion in detention capacity rather than a generic signal of wider enforcement and is consistent with investors pricing the per-diem revenue stream that scales directly with the detained population.

We then examine GEO Group and CoreCivic, the two dominant publicly traded ICE detention contractors, which together accounted for 28% of all ICE detention contracts in 2025. GEO Group alone holds 42% of all privately contracted detention beds, the exclusive electronic monitoring contract, and the exclusive air transportation subcontract for deportation flights (The GEO Group, Inc., 2025b,a). These firms' abnormal returns diverged by 63 and 39 pp from the returns of their synthetic counterfactuals in the first three weeks after the 2024 election, implying $1.8 billion in combined equity gains. The market's signal proved accurate: In 2025, GEO Group expanded its ICE detention capacity from 20,000 to 26,000 beds, and CoreCivic reported a 28% increase in its population of ICE detainees (Halper, 2025).

In the second part of the paper, our design tests whether the procurement expansions implied by financial markets translated into increased detention. The central identification challenge is that procurement and detention are determined simultaneously: Counties with greater enforcement activity attract more federal contracts and detain more individuals at the same time. Ordinary least squares (OLS) thus yields estimates near zero not because procurement has no effect but because the two are jointly driven by unobserved enforcement conditions. We address this issue with a shift-share instrument (Goldsmith-Pinkham et al., 2020; Borusyak et al., 2022) that isolates variation in local procurement growth driven by national vendor-level shocks, scaled by each county's pre-period exposure to those vendors. Firms whose obligations expanded after 2016 were not on differential pre-period trajectories. Counties with greater baseline contractor exposure were not on

differential pre-period detention trends.

The instrument reveals a large and precisely estimated effect. A one-standard-deviation increase in procurement spending raised the county–month detainee rate by 2.7 per 100,000 population, roughly 32% of the pre-2016 mean. The effect is stable across specifications, whether we add county-level controls or absorb state-level enforcement trends with state–year–month fixed effects. The increase in procurement-induced detention operated predominantly through detentions of individuals with no criminal record: 80% of the effect corresponds to detentions of individuals with no recorded conviction. The increase was also concentrated among men, especially those aged 18–34, and among nationals from deportation-cooperative countries. Custody durations range from short spells resolved within a year to prolonged stays exceeding three years—the types of detentions that generate the most continuous per-diem revenue for private detention operators.

Beyond the aggregate effect, we examine what the detention expansion did—and did are consistent with a growing body of evidence that interior enforcement disproportionately not—accomplish. The detention expansion generated no measurable public safety benefit. Violent and property crime rates were unaffected, ruling out a deterrence mechanism. Immigration rates of Hispanic and Mexican-born noncitizens showed no response, ruling out internal migration as a margin of adjustment through which enforcement might have reduced the local noncitizen population. These null results are consistent with the growing body of evidence showing that interior enforcement disproportionately targets individuals without criminal records (East et al., 2026), imposes substantial costs on immigrant communities (East et al., 2023; Alsan and Yang, 2024), and generates no measurable public safety gains (Gonçalves et al., 2024). The composition of the detention increase—detentions of working-age men with no criminal history, from cooperative countries, held in prolonged custody—directly informs the incidence accounting that follows.

The two designs in combination identify the fiscal incidence of detention procurement expansions. The market response design recovers shareholders' valuation of the enforcement expansion; the procurement design identifies the net cost to government, including direct outlays and forgone tax revenue from removals of workers from the labor force. Together, they allow us to point-identify the rent extraction rate—that is, the share of each procurement dollar retained as profit—without directly observing contractor costs. We embed the estimates in the marginal value of public funds (MVPF) framework of Hendren and Sprung-Keyser (2020). Because the marginal detainee has no criminal history, the public safety offset is zero, and the MVPF reduces to the ratio of shareholder gains to net gov-

ernment cost. The rent extraction rate is 6.14 for GEO Group and 3.70 for CoreCivic; the aggregate MVPF is 3.58. This ratio measures fiscal incidence, not social welfare: The headline number should be read as the rate at which public enforcement dollars are converted into private rents, consistent with the prediction of Hart et al. (1997) that privatizing coercive services generates rents when quality is noncontractible (Mukherjee, 2021; Yegen, 2021).

Literature Review Our paper bridges literatures only rarely brought into dialogue with one another. The first shows that financing and administrative incentives shape coercive practices, here applied entirely outside the criminal legal system to immigrants facing no criminal charges. Fiscal design determines who is punished: Asset forfeiture turns punishment into revenue (Norris and Rose, 2024); cost-sharing reduces incarceration (Ouss, 2020); and per-diem contracts lengthen time served without reducing recidivism (Mukherjee, 2021). This fits the longer pattern of confinement expanding to serve economic interests, with persistent effects on legal trust (Gilmore, 2007; Davis, 2011; Archibong and Obikili, 2023). Coercion also extends beyond detention to electronic monitoring and surveillance, with contractor profits rising after anti-Black violence (Di Tella and Schargrodsky, 2013; Ba et al., 2023; Rivera, 2025). Conditions inside facilities further shape outcomes among those confined (Alsan et al., 2025; Alsan and Yang, 2025). Across these settings, confinement is determined as much by who bears the costs and who captures the benefits as by crime or legal doctrine.

Second, we speak to the political economy of procurement and politically contingent rents. A large literature documents that political connections and partisan control shift rent allocation through procurement (Khwaja and Mian, 2005; Faccio, 2006; Goldman et al., 2013), operating through formal and informal organizational channels (Bertrand et al., 2020, 2026), institutional design (Bosio et al., 2022), contract renegotiation (Brogaard et al., 2021), endogenous enforcement (Fisman et al., 2018; Chu et al., 2021), and bureaucratic incentives and selection (Colonnelli et al., 2020; Spenkuch et al., 2023). We complement this work by tracing rents through a complete policy pipeline—from elections to asset valuations, from procurement to detention, and from detention to fiscal incidence. Our setting adds a distinct margin: because detention revenues scale mechanically with detainee-days, procurement is not only a governance choice but a direct channel through which enforcement spending becomes a claim on private cash flows.

Plan The rest of the paper is organized as follows. Section 2 describes the institutional background. Section 3 presents the data. Section 4 estimates the effect of presidential elections on contractor stock returns. Section 5 identifies the causal effect of procurement on local detention and characterizes the marginal detainee. Section 6 combines the two designs to recover the rent extraction rate and compute the MVPF of immigration enforcement procurement. Section 7 concludes.

2 Background: Detention, Contracts, and Electoral Cycles

Federal Immigration Detention System Immigration detention in the United States is a form of civil, administrative custody authorized under the Immigration and Nationality Act to secure the presence of noncitizens during removal proceedings or pending deportation. Detainees have not been convicted of any crime and are held under a parallel administrative legal system with fewer procedural protections than criminal defendants. The system has grown into one of the largest carceral enterprises in the country, drawing on the same physical infrastructure, private operators, and financing mechanisms as the criminal justice system (Ryo and Peacock, 2018; Murakawa, 2014).

The system expanded rapidly following the creation of the Department of Homeland Security (DHS) in 2003, which consolidated immigration enforcement functions previously dispersed across multiple agencies. Congress institutionalized the expansion through the "bed mandate," first introduced in the FY 2010 DHS Appropriations Act, which required ICE to maintain a minimum of 33,400 detention beds per day—a floor that rose to 34,000 in 2012. By transforming detention capacity into a statutory minimum rather than an operational variable, the mandate provided private operators with a guaranteed demand signal, effectively underwriting the growth of the private detention industry (Sinha, 2015).

Facility Types and Private Contractors The immigrant detention network is a heterogeneous assemblage of facility types operating under distinct contractual arrangements. Figure 1, constructed from administrative data released by the Vera Institute of Justice (Vera Institute of Justice, 2025), plots the weekly average daily detention population from 2010 through early 2025, disaggregated across four categories.

"Dedicated facilities"—contract detention facilities (CDFs), dedicated intergovernmental service agreement facilities (DIGSAs), and service processing centers (SPCs)—are operated primarily by private prison corporations under direct contracts with ICE. As Figure 1

shows, dedicated facilities consistently house the largest share of the detained population and exhibit the sharpest responsiveness to enforcement expansions. Capacity in the "private" category surged from roughly 18,000 beds during the Obama years to over 34,000 at the height of Trump's first term, collapsed during the COVID-19-era enforcement pause under Biden, and then exploded to unprecedented levels exceeding 40,000 by early 2025 following Trump's return to office—a trajectory that dwarfs every prior peak in the 17-year series. GEO Group and CoreCivic are the dominant operators in this segment; as Figure 2 illustrates, both firms received substantial ICE contract obligations in FY 2025 relative to their 2008–2016 historical averages (expressed in 2025 dollars), with GEO Group receiving approximately $800 million—well above the 45-degree line of equal spending—and CoreCivic approximately $600 million in FY 2025 obligations alone.

"Nondedicated facilities" operate under intergovernmental service agreements (IGSAs), whereby ICE pays local governments a per-diem rate to house detainees in existing jails, allowing rapid capacity expansion with fewer procurement requirements than direct contracts (U.S. Government Accountability Office, 2021). Because local governments receive per-detainee incentive fees and may subcontract operations to private firms, the arrangement creates financial incentives to maintain or expand jail capacity rather than reduce it (U.S. Government Accountability Office, 2021; Ryo and Peacock, 2018). "Federal facilities," operated by BOP, USMS, and the Department of Defense (DOD), play a more limited role, typically housing individuals subject to criminal rather than civil immigration proceedings.

2024 Election and the Enforcement Surge Figure 1 makes the sensitivity of detentions to the electoral cycle strikingly visible: Republican victories correspond to sharp increases in the privately detained population, while Democratic administrations have overseen stabilization or decline. In no other cycle have the fluctuations in detentions approached their magnitude in the post-2024 surge. On inauguration day, the administration declared a national emergency, directed DHS to detain all apprehended individuals to the fullest extent permitted by law, and ended the practice of releasing individuals pending removal proceedings (The White House, 2025a,b). Capacity expanded rapidly through no-bid private contracts and conversion of military installations into detention sites, and armed forces were deployed to support ICE operations (Migration Policy Institute, 2025; American Immigration Council, 2025). The detained population rose by nearly 75% in the first eleven months of the new administration, surpassing every prior peak in the 17-year series (Amer-

ican Immigration Council, 2025).

The contractual architecture of the immigration detention system provides the mechanism connecting electoral outcomes to firm revenues. Private operators earn per-diem fees for each individual held in their facilities, so sustained increases in the detained population generate predictable, recurring revenue streams for not only GEO Group and CoreCivic but also the broader network of firms providing ancillary services, including food and commissary, medical care, electronic monitoring, transportation, and IT. Figure 2 reveals that the post-2024 expansion extended well beyond the two major private prison operators: A wide range of publicly traded contractors—spanning IT firms such as Leidos and CACI and professional services firms—expanded their law enforcement procurement obligations substantially in FY 2025 relative to their 2008–2016 historical averages, with many sitting well above the 45-degree line of equal spending. All obligations are expressed in 2025 dollars.

3 Data Sources

Government Contractors We collect the universe of prime contract transactions awarded by DHS and the Department of Justice (DOJ) from USAspending.gov for fiscal years 2008–2026.[1] These two departments encompass the principal federal law enforcement agencies and include ICE, CBP, the FBI, the BOP, the USMS, the DEA, and the ATF, among others.

For each contract transaction, we observe the awarding agency and subagency, the federal action obligation amount, the recipient's name and unique entity identifier (UEI), the place of performance, and detailed product and service codes (PSC and NAICS). We aggregate transactions to the recipient–fiscal year–subagency level, which yields approximately 562,000 observations across roughly 105,000 unique contractors. Total obligations over this period amounted to approximately $467 billion, of which ICE accounted for $39 billion with about 7,500 unique contractors and CBP accounted for $70 billion and close to 13,000 contractors.

Entity Registration Data. We supplement the procurement records with contractor characteristics from the System for Award Management (SAM.gov), the federal government's

[1] USAspending.gov is the official source for spending data for the US government, maintained by the Department of the Treasury as mandated by the Federal Funding Accountability and Transparency Act of 2006.

primary database of entities authorized to do business with the government. We compile 24 monthly SAM.gov entity extracts spanning November 2014–November 2025 and covering approximately 16.7 million entity–time observations. SAM.gov provides each contractor's legal business name, physical address, organizational structure (e.g., sole proprietorship, corporation, nonprofit), primary industry codes (Standard Industrial Classification [SIC] and North American Industry Classification System [NAICS]), and registration status. We merge the SAM.gov records with the contract data using the recipients' UEIs, achieving near-universal match rates.

Finance Data We collect financial data from Wharton Research Data Services (WRDS) for each publicly traded firm in our sample. We use Beta Suite to compute the firms' abnormal returns over event windows centered on the five most recent US presidential elections (2008, 2012, 2016, 2020, 2024). In addition, we use daily stock information from CRSP. We link firms to their Compustat identifiers using the standard CRSP–Compustat Merged database, restricting to primary link types and to ordinary common shares traded on major US exchanges. The firms' geographical information and SIC and NAICS codes are collected from Compustat.

ICE Detention Microdata The primary source is individual-level ICE administrative detention records obtained through FOIA requests and litigation. The main series, spanning January 2003–December 2023, is compiled by the Transactional Records Access Clearinghouse (TRAC) (2025); we extend the coverage through early 2025 using records released by the Deportation Data Project (2025), obtained through FOIA, as well. Each record corresponds to a distinct detention episode and reports the county of the detaining facility, dates of entry into and departure from custody, gender, country of origin, and, where applicable, the most serious criminal conviction on record. We assign each episode to the county of the detaining facility, the geographic unit at which both detention capacity and enforcement activity are localized. For each detention spell, stay duration is measured as the elapsed time between the entry date and either the recorded departure date or the end of the observation window; individuals with no departure record are treated as remaining in active custody, so stay duration captures the full distribution of custody lengths, not only completed spells. We aggregate these records to a balanced county–month panel by counting all individuals associated with a given county facility in a given calendar month.

County-Level Covariates The third source is the five-year estimates from the American Community Survey (ACS), from which we draw four county-level variables: the unemployment rate, total population recorded, poverty rate and median household income. These data are available at county level. We assign each annual county observation to all twelve calendar months within that year and merge into the main panel on county and year, which gives us a balanced county–month series of time-varying local economic conditions.

We supplement these with county-level crime data from the FBI's Uniform Crime Reporting (UCR) program, using the concatenated files compiled by Kaplan (2025), which harmonize agency-level offense reports into consistent county–year panels for 2003–2025. We extract violent and property crime rates per 100,000 residents to analyze the impact on public safety.

4 Market Responses to Election Cycles

4.1 Sample Construction and Descriptives

Sample Construction We restrict the sample to publicly traded firms incorporated in the United States and listed at any point between fiscal years 2008 and 2026. We identify firms connected to federal law enforcement by matching the universe of DHS and DOJ contractors from USASpending.gov to Compustat using a fuzzy name matching algorithm applied to the SAM.gov entity roster, which we supplement by manual verification. A firm is classified as treated with respect to a given agency if it holds at least one contract with that agency during the four fiscal years preceding each election, which ensures that treatment assignment is determined ex ante. Because treatment status is defined at the firm–election level, firms can enter or exit the treated group throughout election cycles as their contract portfolios evolve.

The treated group consists of firms holding contracts with the focal agency during the pre-election window. We study seven federal law enforcement subagencies, ICE and CBP within DHS and the FBI, USMS, DEA, ATF, and BOP within DOJ, but focus primarily on ICE contractors, which range from private prison operators such as GEO Group and CoreCivic to technology vendors and professional services firms. For the 2024 election, we identify 58 ICE contractors and 332 non-ICE law enforcement contractors; pooling across all five elections yields 150 and 738 unique firms, respectively. The donor pool, publicly traded firms with no law enforcement contracts, comprises 3,828 firms for 2024 and 6,476 unique

firms across all elections. To avoid contamination, we exclude firms contracting with other law enforcement agencies but not the focal agency. We require a balanced panel of 84 trading days per firm–election, 63 pre-election and 21 post-election, dropping firms with missing returns within this window.

We conduct agency-level analyses separately for the seven federal law enforcement subagencies. For ICE, we further disaggregate by contract category to examine effect heterogeneity across six procurement areas: detention and custody services, IT and data systems, professional services, personnel and administration, weapons and tactical equipment, and health, food, and labor services. Each firm is assigned to its primary ICE category on the basis of the category representing its largest share of obligations in the pre-election window. This decomposition allows us to distinguish, for example, between the market response for firms operating private detention facilities and that for firms providing IT or surveillance infrastructure.

Summary Statistics Table 1 reports summary statistics across two panels: Panel A focuses on our main specification around the 2024 presidential election, and Panel B pools all firm–election observations from the 2008–2020 elections. In both panels, columns (1)–(2) cover ICE contractors, columns (3)–(4) non-ICE law enforcement contractors, and columns (5)–(6) the donor pool of noncontractor firms.

First, law enforcement contractors are systematically larger and more leveraged than the firms in the donor pool: In Panel A, both ICE and non-ICE contractors have mean log assets roughly two points higher than donor pool firms and carry meaningfully higher leverage, which reflects the fact that federal procurement tends to favor established, well-capitalized firms. Profitability is broadly similar across groups, though the donor pool firms exhibit slightly negative mean returns on equity and greater dispersion. Second, ICE and non-ICE law enforcement contractors are closely matched on observable financial characteristics, size, profitability, and leverage in both panels. On the contracting side, ICE contractors are notably more concentrated than their non-ICE counterparts: In Panel A, they hold on average three times as many contracts at more than twice the average contract size, with the large standard deviations indicating substantial heterogeneity within the ICE contractor group. The 2024 cycle includes 58 ICE contractors, 332 non-ICE law enforcement contractors, and 3,828 donor pool firms; the pooled 2008–2020 panel is roughly two to three times larger, reflecting turnover as firms enter, exit, or lose their public listing across electoral cycles.

4.2 Empirical Strategy

4.2.1 Computing Excess Returns

We start our analysis by focusing on firms' abnormal returns, which measure gains or losses beyond general market movements. Using the Carhart four-factor model (Fama and French, 1993; Carhart, 1997; Fisman and Wang, 2015)[2]. For each firm i on date t, the abnormal return is

$$AR_{it} = R_{it} - [\hat{\alpha}_i + \hat{\beta}_{MKT,i} RM_t + \hat{\beta}_{SMB,i} SMB_t + \hat{\beta}_{HML,i} HML_t + \hat{\beta}_{UMD,i} UMD_t] \quad (1)$$

where R_{it} is the excess return on asset i over the T-bill rate at t, $\hat{\alpha}_i$ is the estimated four-factor alpha, RM_t is the market excess return, SMB_t is the size factor, HML_t is the value factor, and UMD_t is the momentum factor. For each firm i, the cumulative abnormal returns (CARs) from day n to m are $CAR_i[n, m] = \sum_{t=n}^{m} AR_{it}$.

4.2.2 Estimation Strategy

Synthetic Difference-in-Differences To identify the effect of presidential elections on contractor stock performance, we use the SDID estimator (Arkhangelsky et al., 2021). In SDID, a weighted control group is constructed from donor pool firms so that their pre-election return trends match those of treated firms as closely as possible and pre-election periods are reweighted to better resemble the post-election window. Under the assumption that these weighted trends would have remained parallel post-election in the absence of the treatment, SDID recovers the average treatment effect on the treated (ATT). For each firm i at date t, the outcome y_{it} is given by

$$y_{it} = \mu + \alpha_i + \gamma_t + LE_{it}\beta + \varepsilon_{it} \quad (2)$$

where α_i is a fixed effect for a firm, γ_t is a fixed effect for time, μ is a constant, and ϵ_{it} is the error term. The dummy variable LE_{it} denotes exposure to binary treatment, i.e., whether a firm contracts with the focal law enforcement agency and whether t is after the election. In our main application, $LE_{it} = ICE_{it}$, indicating firms with ICE contracts in the four fiscal years preceding the election. We also estimate specifications for each of the remaining six agencies separately. The SDID estimator captures the average effect of election exposure,

[2]For each firm–date, with an estimation window of 252 trading days ending 30 days before the relevant event date (Li and Lie, 2006; Jayachandran, 2006)

$\hat{\beta}^{sdid}$, written as

$$(\hat{\beta}^{sdid}, \hat{\mu}, \hat{\alpha}, \hat{\gamma}) = argmin_{\mu,\gamma,\alpha,\beta} \sum_i \sum_t (y_{it} - \alpha_i - \mu - \gamma_t - LE_{it}\beta)^2 \hat{\omega} i \hat{\lambda} t \tag{3}$$

where $\hat{\omega}_i$ corresponds to the weights for each firm that align the pre-election trends in the outcome of the control firms with those of the treated group. The SDID estimator includes firm fixed effects αi and time weights λ_t, which balance the pre-election period with the post-election periods. Finally, we follow Arkhangelsky et al. (2021) to compute the standard errors using a placebo analysis by applying the SDID estimator to firms not exposed to treatment.

Synthetic Control As an alternative to SDID for constructing the counterfactual of firms exposed to election outcomes, we use SC (Abadie and Gardeazabal, 2003; Abadie et al., 2010; Acemoglu et al., 2016). The SC estimator captures the average effect of election exposure $\hat{\beta}^{sc}$, which can be written as

$$(\hat{\beta}^{sc}, \hat{\mu}, \hat{\gamma}) = argmin_{\mu,\gamma,\beta} \sum_i \sum_t (y_{it} - \mu - \gamma_t - LE_{it}\beta)^2 \hat{\omega}_i \tag{4}$$

where $\hat{\omega}_i$ represents the weights for each firm that align the pre-election trends in the outcomes of control firms with those of the treatment group. The SC approach imposes $\alpha_i = 0$ for all firms. In contrast, the SDID estimator includes firm fixed effects, α_i, and time weights, λ_t, that balance pre-election periods with post-election ones. While in the SDID method weights for control units are selected so that the pre-exposure trends are as parallel as possible, in the SC method, the control unit weights are selected so that the pre-exposure trends are as close as possible. Similarly to the SDID procedure, standard errors are computed on the basis of a placebo analysis (Arkhangelsky et al., 2021).

4.3 Results

4.3.1 ICE Contractor Returns Around the 2024 Election

Enforcement Windfall Figure 3 presents our main result. The pre-election CARs of ICE contractors track their synthetic controls closely, supporting the parallel trends assumption. After the election, they diverged sharply. The SDID estimate of 0.202 ($SE = 0.059$) and the SC estimate of 0.225 ($SE = 0.072$) indicate that ICE contractors earned approximately

20 pp in abnormal returns relative to their counterfactuals over the month following the election. The dynamics are informative: Returns did not spike on election day itself but instead built gradually over the first week, consistent with a period of price discovery, then accelerated through trading day 11—reaching nearly 40 pp above the SC outcome—before settling at around 30 pp higher through the end of the event window. This pattern is consistent with investors progressively updating their expectations as the incoming administration signaled the scope of its deportation agenda through executive orders, public statements, and early contract announcements.

Detention Contractors vs. Other Contractors Figure 4 decomposes the aggregate result by contract category. The effect is concentrated entirely among detention contractors—guard services, custodial operations and facility construction, with SDID and SC estimates of 0.258 ($SE = 0.077$) and 0.255 ($SE = 0.075$), closely matching the magnitude of the aggregate estimate of 0.202. Every other category—health and food, IT, personnel, and weapons—shows estimates indistinguishable from zero. Professional services firms show a delayed upward movement after trading day 12, but the estimates are noisy and an order of magnitude smaller (0.036, $SE = 0.060$). The concentration of the effect among detention contractors suggests that markets did not reprice the broader set of ICE service providers after the 2024 election, but instead directed the revaluation narrowly toward the firms whose revenues are most directly tied to the physical custody of detainees and the per-diem fee structure that governs private detention contracts. The result points to a specific mechanism—anticipated expansion of detention bed capacity—rather than a diffuse response to a generic enforcement signal.

GEO Group and CoreCivic We next closely examine GEO Group and CoreCivic, the two dominant private prison operators, which together account for just 2 of the 1,022 ICE contractors in our sample yet captured approximately 28% of total ICE contract obligations in FY 2025. Figure 5 shows that for these two firms, in contrast to the broader ICE contractor aggregate, the market response was immediate: Their CARs diverged from the SC outcome on the first trading day after the election and rose without interruption through the end of the event window. The SDID estimates of 0.634 ($SE = 0.175$) for GEO Group and 0.389 ($SE = 0.085$) for CoreCivic are both statistically significant, consistent across methods, and substantially larger than the aggregate ICE estimate of 0.202. The concentration of the largest and most immediate responses among the two firms whose revenues scale directly

with the detained population is consistent with markets pricing anticipated expansion of private detention capacity.

4.3.2 Election Effects Across Agencies and Electoral Cycles

Figure 6 places the 2024 ICE result in a broader context by reporting the SDID and SC estimates for all seven law enforcement agencies after all five presidential elections from 2008 to 2024. Two patterns stand out. First, the 2024 ICE estimate, highlighted in red, is by a wide margin the largest coefficient in the entire figure, across all agencies and all electoral cycles. ICE contractors also show positive and statistically significant effects for prior elections, consistent with markets pricing enforcement expectations under Republican administrations more generally, but the 2024 estimate dwarfs those for all preceding years. For no other agency in any other cycle does the estimate come close to replicating the magnitude of the 2024 ICE estimate.

Second, beyond ICE, evidence of systematic electoral effects is limited. The CBP and BOP contractors show positive point estimates for several cycles, particularly 2016 and 2024, but the confidence intervals are wide, and the patterns are not consistent across methods. DEA, FBI, ATF, and USMS contractors show estimates centered near zero for nearly every election, with no discernible electoral-cycle effects. The specificity of the market response to ICE contractors—and within ICE to detention contractors and the two dominant private prison operators—is not a feature of federal law enforcement contracting broadly, but a distinct characteristic of the immigration detention industry and its unique sensitivity to the enforcement priorities of incoming administrations.

4.3.3 Robustness

In our main specification, we compute the abnormal returns using the Carhart four-factor model (Fama and French, 1993; Carhart, 1997), which controls for market risk, size, value, and momentum and is the standard benchmark in political event studies (Jayachandran, 2006; Fisman and Wang, 2015; Acemoglu et al., 2016). The choice of benchmark could, in principle, affect the estimated CARs if the treated firms systematically differ from the donor pool firms along these dimensions. We therefore reestimate against two alternative benchmarks under which the factor structure is gradually relaxed: the capital asset pricing model (CAPM) benchmark, which conditions only on the market risk (Sharpe, 1964; Lintner, 1965), and the raw returns benchmark, where we impose no factor model at all

(Lyon et al., 1999). If the results reflect genuine anticipatory repricing of enforcement expectations rather than artifacts of the benchmark, they should be stable across all three specifications.

Main Results Figure A.1 confirms that the aggregate ICE result is insensitive to the choice of benchmark return model. The pre-election CARs track closely their SC counterfactuals under all three specifications, and the post-election divergence is large, persistent, and virtually identical in magnitude and timing. The consistency across both the SDID and SC estimators within each model further reinforces this conclusion, ruling out benchmark misspecification as a driver of the main finding.

Detention Contractors vs. Other Contractors Figure A.2 confirms that the concentration of the effect among detention contractors is not sensitive to the choice of benchmark. Across all three specifications, detention contractors show large, positive, and precisely estimated effects, with confidence intervals that exclude zero by a wide margin under the Carhart four-factor model, CAPM, and raw returns benchmarks alike. Every other category—health/food/labor, IT and data, personnel, professional services, and weapons—shows estimates clustered near zero across all three models and both estimators, with wide confidence intervals that provide no evidence of a systematic post-election revaluation. The previously documented pattern of a market response concentrated entirely among the firms whose revenues scale directly with the physical custody of detainees is robust to how the abnormal returns are constructed.

GEO Group and CoreCivic Figure A.3 shows that the large post-election revaluation of GEO Group and CoreCivic is equally robust. The SDID and SC estimates are large, precisely estimated, and tightly clustered across all three return models, with confidence intervals that exclude zero by a wide margin in every case. The equity gains documented previously are not an artifact of the choice of benchmark.

Election Effects Across Agencies and Electoral Cycles Finally, Figure A.4 replicates the full cross-agency, cross-cycle coefficient plot under all three return models. Both conclusions from Section 4.3.2 hold: The 2024 ICE estimate remains by a wide margin the largest in the figure across all agencies and elections, and the near-zero estimates for DEA, FBI,

ATF, and USMS contractors persist regardless of the benchmark, as do the imprecise positive point estimates for CBP and BOP in select cycles. The specificity of the market response to ICE detention contractors is not sensitive to how abnormal returns are constructed.

5 Impact of Procurement on Local Immigration Detention

Presidential elections shift enforcement expectations, and equity markets immediately capitalize those shifts into contractor valuations. But market prices measure what investors anticipate, not what governments ultimately deliver—and the composition of any detention increase matters as much as its magnitude. Advocates of detention expansions argue that the new detentions will be targeted at individuals who pose a public safety risk (The Heritage Foundation, 2023), but a growing body of evidence suggests that capacity expansions affect predominantly individuals with no criminal history (East et al., 2026). Who is detained at the margin determines both the public safety value of enforcement expansions and their net fiscal cost and so directly shapes how we interpret the private returns documented in the previous section. This section addresses both questions, asking how much procurement-driven capacity expansions causally increase detentions and who, at the margin, bears the burden of the new detentions.

5.1 Sample Construction and Descriptives

Sample Construction The analysis uses a balanced county–month panel spanning January 2003–early 2025. The three main data sources are ICE administrative detention records, federal procurement obligations from USASpending.gov, and county-level covariates from the ACS. The detention outcomes count all individuals appearing in custody at any point during the month, and stay length is measured from entry to either recorded release or the end of the observation window.

The treatment variable aggregates ICE and BOP immigration procurement obligations to the county–month level. The raw procurement obligations vary substantially across counties because of differences in detention infrastructure and facility size, so we normalize by each county's 2011–2015 average monthly detainee count to ensure comparability across counties with different detention footprints. Five-year ACS estimates are merged into the panel as time-varying controls for unemployment, log population, poverty rate, and median household income, capturing secular changes in local economic conditions

that may independently affect detention patterns.

All disaggregated detainee counts, by criminal record, gender, age group, country of origin, deportation-compliance status, departure status, and stay length, are constructed at the county–month level in the cleaning pipeline before estimation. To limit the influence of outliers, county obilgation is trimmed at the 1st and 99th percentiles. The final estimation sample comprises 145,745 county–month observations, and all tables in the procurement section use this common sample.

Descriptives Table 2 reports summary statistics for the common estimation sample of 145,745 county–month observations. The raw procurement measure averages $19.6 thousand per baseline detainee–month, with a standard deviation of $180.9 thousand, reflecting the considerable right skew in procurement obligations across counties and time. The standardized treatment is mean zero and unit standard deviation by construction. For the average county–month, we observe 8.4 total detainees per 100,000 residents, though detention exposure is highly heterogeneous across counties. The detained population is mostly men (7.97 men vs. 0.44 women per 100,000) and concentrated in the prime working-age groups: Detainees aged 18–34 and 35–49 together account for roughly three-quarters of the total. By country of origin, detainees from Mexico (5.6 per 100,000) and other Central and South American countries (2.2 per 100,000) together comprise the large majority of the detained population, while detainees from countries designated uncooperative with deportation or at risk of noncooperation represent a small share (0.27 and 0.17 per 100,000, respectively). The majority of detainees in any given county–month remain in custody beyond three years (5.9 per 100,000), reflecting the prevalence of long-term or unresolved cases in administrative detention records.

5.2 Estimation Strategy

Setup We seek to identify the causal effect of federal procurement obligations on local immigration detention outcomes. For each county c in month t, we estimate

$$Y_{ct} = D_{ct}\,\beta + X'_{ct}\Gamma + \mu_c + \delta_t + \varepsilon_{ct} \tag{5}$$

where Y_{ct} is the detention outcome of interest and D_{ct} is the treatment variable of interest: county-level ICE and BOP procurement obligations for detention services, assigned to counties by place of performance, divided by the county's 2011–2015 average monthly de-

tainee count. Normalizing by the pre-period enforcement baseline scales the procurement flows by the size of the local detention footprint, ensuring comparability across counties with very different enforcement histories. The resulting variable is then standardized to have mean zero and unit standard deviation, so that β is interpretable as the effect of a one-standard-deviation increase over the mean procurement obligations per baseline detainee. The vector X_{ct} includes time-varying county-level controls, μ_c denotes county fixed effects accounting for time-invariant differences across counties, and δ_t are year–month fixed effects capturing common aggregate shocks. Standard errors are clustered at the county level.

Endogeneity Estimating equation (5) by OLS does not recover the causal effect of procurement on detention. The central challenge is that procurement is endogenous to local enforcement demand: Counties under sustained enforcement pressure simultaneously attract more federal contracts and detain more individuals, while ICE may strategically direct procurement toward counties where it anticipates high enforcement activity. Unobserved local factors, political support for enforcement, existing detention infrastructure, and border proximity further confound the relationship. The county and time fixed effects absorb time-invariant heterogeneity and common aggregate shocks but cannot resolve this time-varying simultaneity.

Shift-Share Instrument Design We address this identification problem with a shift-share instrument (Goldsmith-Pinkham et al., 2020; Borusyak et al., 2022). The instrument isolates variation in local procurement growth driven by *national* vendor-level shocks, shifts in firm-level contract obligations across all counties, scaled by each county's *pre-existing* exposure to those firms:

$$Z_{ct} = \sum_{f} \mathrm{s}_{cf}^{\mathrm{pre}} \mathrm{g}_{ft} \tag{6}$$

where $\mathrm{s}_{cf}^{\mathrm{pre}}$ is firm f's share of county c's total procurement obligations during the 2011–2015 baseline and g_{ft} is the leave-one-out growth rate of firm f's national obligations in month t, computed without county c to prevent mechanical correlation with local outcomes. Intuitively, the instrument gauges how much local procurement would have grown, given a county's preexisting contractor mix, had those vendors experienced only the demand growth observed elsewhere in the country. The variation between counties in Z_{ct} therefore reflects historical vendor relationships rather than contemporaneous local en-

forcement conditions.

We use Z_{ct} to instrument for D_{ct} in a two-stage least squares (2SLS) framework. The first stage is

$$D_{ct} = Z_{ct}\,\pi + X'_{ct}\Gamma + \mu_c + \delta_t + u_{ct} \tag{7}$$

where π captures the effect of the shift-share instrument on local procurement obligations. Under the assumption that national vendor-level shocks are exogenous to county-specific detention trends, conditional on county and time fixed effects, the resulting 2SLS estimator recovers the causal effect of procurement-driven capacity expansion on detention outcomes.

5.3 Identifying Assumptions and Their Plausibility

The instrument combines two sources of variation: baseline exposure shares s_{cf}^{pre}, which govern how sensitive each county is to national procurement shocks, and national shifters g_{ft}, which supply within-county variation over time. Valid identification requires that at least one component satisfy the exogeneity condition conditional on the fixed effects in equation (7).

Following Borusyak et al. (2022), identification can come from quasi-random assignment of the underlying shocks: In the Bartik setting, the national shifters should be interpretable as exogenous or as noisy estimates of latent exogenous shocks, instead of being systematically related to shock-level unobservables. Following Goldsmith-Pinkham et al. (2020), we instead interpret identification as coming from the exogeneity of the baseline shares s_{cf}^{pre}: Counties with different historical exposure should not differ in outcome changes for reasons unrelated to the endogenous regressor. When pre-period data are available, parallel pretrends in more- vs. less-exposed counties are a useful diagnostic of this shares-based design.

We assess both conditions with four tests: (i) *Relevance*: testing whether the instrument predicts local procurement obligations; (ii) *Shift Exogeneity:* testing that national shocks are orthogonal to pre-existing county enforcement trends; (iii) *Share Exogeneity*: confirming that pre-period exposure is unrelated to prior trends in detention outcomes and local economic conditions; and (iv) *Exposure Concentration:* ruling out that identification disproportionately leverages observations of a small number of influential contractors.

Relevance We first verify the presence of a first-stage correlation—that is, that the instrument is a strong predictor of local procurement. Figure 7 plots the residualized treatment against the residualized instrument, after conditioning on county fixed effects, year–month fixed effects, and time-varying county controls. The relationship is monotonically increasing: Counties with greater baseline exposure to firms that expanded nationally saw progressively larger increases in local procurement obligations.

The corresponding first-stage coefficient is 0.055 ($SE = 0.01$), with a Kleibergen–Paap Wald F-statistic of 30.80, well above conventional weak-instrument thresholds. In economic magnitude, a one-standard-deviation increase in the shift-share instrument value over the mean raises the standardized treatment by about 0.055 standard deviations. The first-stage evidence indicates a strong positive link between the Bartik shifter and local procurement intensity in the exact benchmark specification used for the baseline 2SLS estimates. Appendix Table A.1 reports first-stage estimates across the three main 2SLS specifications; the Kleibergen–Paap Wald F-statistic exceeds 30 in all columns and reaches 41 in the state-by-year-month specification, confirming that the strong first stage is robust to the inclusion of local controls and alternative fixed-effect structures.

Shift Exogeneity: Shock-Level Pretrend Balance The national shifters g_{ft} must be as good as randomly assigned conditional on controls. We assess this condition with a firm-level regression of pre-period procurement growth on the post-period shock. Let $\bar{g}_f^{\text{post}}$ denote firm f's average procurement obligations after the policy change and Δg_f^{pre} the log change in its average obligations between 2009–2011 and 2013–2015. If the shifters satisfy the identifying assumption, post-period shocks should be orthogonal to pre-period growth conditional on firm characteristics:

$$\Delta g_f^{\text{pre}} \;=\; \rho\, \bar{g}_f^{\text{post}} \;+\; Z_f'\eta \;+\; u_f \tag{8}$$

Table 3 reports estimates of $\hat{\rho}$ from different specifications. The estimates are small and statistically indistinguishable from zero throughout. Firms that received large procurement obligations after 2016 were not on differential growth paths beforehand. The data are consistent with as-good-as-random assignment of the national shifters.

Share Exogeneity Valid identification also requires that the baseline exposure shares be uncorrelated with preexisting county-level differences in detention trends or in the socioeconomic conditions that proxy for unobserved enforcement demand. We assess this

through event-study regressions, complemented by a formal cross-sectional pretrend test.

Event-Study Balance We estimate pre-period event-study regressions of the form

$$Y_{ct} = \sum_{t=2009}^{2014} \beta_t \cdot \widetilde{Z}_c \cdot D_t + \alpha_c + \gamma_t + \varepsilon_{ct} \tag{9}$$

where $\widetilde{Z}_{ct}$ is the county's annual shift-share exposure, α_c is county fixed effects, γ_t is year fixed effects, and D_t denotes year dummies with 2015 as reference year. Figure 8 reports $\hat{\beta}_r$ for six outcomes: detention per 100,000 residents, Hispanic share of the population, poverty rate, unemployment rate, median household income and county-level crimes per 100,000. All outcomes are standardized within our sample. The pre-period coefficients are close to zero with no systematic trend across all six panels. Counties with higher baseline contractor exposure were not in differential detention or socioeconomic trajectories prior to the post-2016 procurement expansion.

Cross-Sectional Pretrend Test As a complement, we follow Goldsmith-Pinkham et al. (2020) and regress county pre-period changes in detention outcomes, the difference in average monthly detention rates between 2013–2015 and 2009–2011, on baseline shift-share exposure, conditioning on county-level controls. Table 4 shows that the estimated coefficient is small and statistically indistinguishable from zero, confirming that counties with greater baseline contractor exposure were not on differential pre-period detention trajectories. Together, these tests indicate that the baseline exposure shares satisfy the parallel trends condition. Combined with the above shock-level balance, the evidence supports the exogeneity of both components of the shift-share design.

Exposure Concentration Shift-share estimators can be sensitive to settings in which a small number of firms contribute disproportionately to the identifying variation (Goldsmith-Pinkham et al., 2020; Adao et al., 2019). We assess this using Rotemberg-style influence weights, the normalized sum of squared county baseline shares for each firm, which measure each firm's contribution to the cross-county variance of the instrument. Table 5 reports results for the top firms by this measure. Concentration is moderate: The effective number of firms, computed as the reciprocal of the Herfindahl–Hirschman index over influence weights, averages 2.2 across counties with a median of 1.4. The most influential firms span a range of service categories—facility management, food service, healthcare, and

technology—rather than being concentrated solely among private prison operators. GEO Group and CoreCivic rank second and third, with influence weights of 0.009 and 0.005, respectively. The shift-share design therefore captures procurement-driven detention expansion across the full contractor network; our earlier focus on GEO Group and CoreCivic reflects their outsized share of detention bed capacity and equity market visibility, not their dominance of the identifying variation. Identification does not rest on a single dominant firm or service category.

5.4 Results

5.4.1 Scale and Composition of Detentions

A key question on the political economy of immigration enforcement is whether procurement-driven capacity expansions translate into actual custody outcomes and, if so, for whom. We address both questions in turn, first estimating the aggregate effect on detention stocks and then characterizing the population among whom the effect materializes.

Main Effect on Detention Stocks Table 6 reports our main estimates. Columns (1)–(3) report the OLS and columns (4)–(7) the 2SLS estimates. The OLS estimates are near zero across all specifications not because procurement has no effect on detention, but because the two are simultaneously determined. Counties under enforcement pressure attract more contracts and detain more people at the same time; OLS cannot separate cause from effect.

The shift-share instrument breaks this simultaneity by isolating the procurement variation driven by national vendor shocks interacted with preexisting county exposure. The 2SLS estimates remain large and precisely estimated: A one-standard-deviation increase in procurement obligations per baseline detainee over the mean raises the county–month detainee count by 2.655 ($SE = 0.723$), against a pre-2016 mean of 8.417 detainees per county–month, an effect equivalent to roughly 32% of the baseline mean. The effect is stable across specifications, ranging from 2.620 to 2.695, whether we add county-level controls or absorb state-level enforcement trends with state–year–month fixed effects. Column (7) provides an additional robustness check using an alternative share-construction window of 2011–2014, excluding the year immediately preceding the 2016 election to guard against pre-electoral sorting in procurement relationships; the estimate of 2.620 ($SE = 0.790$) remains close to the baseline.

One concern that may arise is that our baseline treatment variable combines procurement obligations from both ICE and the Bureau of Prisons (BOP), which may reflect distinct enforcement motivations: ICE contracts are directed specifically at immigration detention, whereas BOP contracts support federal criminal incarceration more broadly. To address this, we replicate the three baseline 2SLS specifications using ICE-only procurement obligations—restricting both the treatment variable and the shift-share instrument to ICE contracts—as a direct robustness check on whether the main results are driven by the inclusion of BOP spending.

The results in Appendix Table A.3 confirm that the estimates are robust to this restriction. The 2SLS coefficient on ICE-only procurement obligations ranges from 2.199 to 2.229 across the three specifications, compared to 2.655–2.695 in the baseline, and remains statistically significant at the 1% level throughout. The Kleibergen–Paap F-statistics range from 17.67 to 20.88, comfortably above the weak-instrument threshold. The modest attenuation relative to the baseline reflects the exclusion of BOP variation rather than any fundamental change in the relationship, suggesting that the documented effect of procurement capacity on detention outcomes is not an artifact of combining ICE and BOP contracts in the treatment measure.

A final concern is that our full sample spanning 2003–2025 incorporates variation from earlier enforcement regimes, including the Secure Communities program active from 2008 through 2014, which could confound the interpretation of the shift-share estimates as capturing procurement-driven expansion. Table **??** addresses this by restricting the estimation sample to 2014 onward, limiting identification to the post-Secure Communities period. The 2SLS estimates range from 2.115 to 2.297 across specifications, remaining close to the baseline estimates of 2.655–2.695 and statistically significant throughout. The results are not driven by the earlier enforcement regime.

Criminal Record Status A central justification offered by proponents of immigration enforcement expansion is that the new detentions will target individuals who pose a public safety risk. If this were true, procurement-driven increases in enforcement capacity should raise the count of detainees with, not without, criminal records. Table 7 tests this directly by decomposing the total detainee effect by criminal-record status. The results show that the overwhelming majority of the effect is concentrated among individuals with no recorded criminal conviction: A one-standard-deviation increase in procurement obligations raises the noncriminal detainee count by 2.169 ($SE = 0.897$), against a pre-2016

mean of 8.628. For detainees with any criminal record, we observe a much smaller and statistically nonsignificant increase (0.527, $SE = 0.462$), from a near-zero pre-2016 baseline of 0.037.

The disaggregation by offense type in columns (3) and (4) shows a marginally significant increase for detainees with violent convictions (0.103, $SE = 0.088$) and a positive but imprecise estimate for those with non-violent convictions (0.424, $SE = 0.417$). Noncriminal detainees account for approximately 87% of the total effect, indicating that procurement-driven enforcement expansions operate predominantly through detentions of individuals whose immigration status alone, rather than any criminal history, triggers contact with immigration enforcement. We interpret this as indicative of an enforcement model that prioritizes the filling of detention beds over the targeting of individuals who pose a public safety risk.

Heterogeneity by Gender and Age The criminal-record results establish that procurement expansions predominantly target individuals with no criminal history. In Table 8, we examine who among this population bears the burden of the rise in enforcement. The gender result shows that the entire effect is driven by detentions of men (2.699, $SE = 0.732$), while the coefficient for female detainees is small and negative (-0.003, $SE = 0.074$). The expansion in procurement does not change the gender composition of the detained population; it increases the detention of men.

The age decomposition shows the strongest result for the 18–34 cohort, the category with the most detainees at baseline, for whom the coefficient of increase is 2.400 ($SE = 0.617$). The coefficients for ages 35–49, 50–64, and 65 and older are smaller and imprecisely estimated. For detainees under 18, we observe a negative coefficient of increase (-0.027, $SE = 0.007$), albeit from a very small pre-2016 baseline of 0.036. Together with the criminal-record findings, the revised estimates indicate that the dominant margin of procurement-driven enforcement expansions remains detentions of adult men with no criminal record.

5.4.2 Country of Origin, Compliance, and Custody Duration

Having established that procurement-driven enforcement expansions operate through detentions of working-age adult men with no criminal record, we now examine three dimensions that speak to the international reach of the expansion and the nature of the custody

it generates: where detainees come from, whether their removal is operationally feasible, and how long they remain in custody once detained.

Concentration by Country of Origin: Mexico and Latin America Table 9 disaggregates the total effect by country and continent of origin. The point estimates continue to suggest concentration on these dimensions, though the most precisely estimated increase now appears to be in detentions of South and Central American nationals: This group shows a coefficient of 1.735 ($SE = 0.456$), against a pre-2016 mean of 1.891. For Mexican nationals, we see a positive but imprecise coefficient of 0.475 ($SE = 0.425$), against a pre-2016 mean of 6.273. For nationals from Asia, Africa, Europe, North America outside of Mexico, and Oceania, the coefficients are negligible, ranging from -0.021 to 0.017. The pattern of the point estimates aligns with enforcement operations targeting migration corridors from Latin America rather than a broad-based expansion of interior enforcement across all nationalities.

Removability and Deportation Compliance A natural concern is that the detention effects documented above reflect mechanical retention: Some detainees cannot be removed because their home governments refuse to accept deportations, in which case procurement expansions would generate longer custody stocks regardless of enforcement intent. Table 10 tests this by disaggregating the total effect by the DHS's classification of detainees' origin countries by their degree of deportation compliance. The results rule out this interpretation. The increase in detentions from the procurement expansion falls almost entirely on detainees from cooperative countries: A one-standard-deviation increase in procurement obligations raises their count by 2.723 ($SE = 0.692$) over a pre-2016 mean of 8.149, an impact accounting for approximately 80% of the main effect. For nationals from fully uncooperative countries, the increase in detentions is small and statistically nonsignificant (-0.058, $SE = 0.093$), and for nationals of countries at risk of noncompliance with deportations, we observe a similarly small and nonsignificant response (-0.027, $SE = 0.079$). The increase in detentions driven by procurement occurs predominantly among nationals whose deportation is operationally feasible. The nondeparture pattern documented below therefore reflects other mechanisms—prolongation of legal proceedings, case backlogs, or operator incentives to sustain occupancy—rather than forced retention attributable to noncooperation among origin states.

Custody Duration and the Stock of Unresolved Cases The compliance results establish that the detention effects are not mechanically driven by retention of nationals whose origin states do not cooperate with deportations. In Table 11, we examine where in the custody cycle the effect materializes, decomposing the total number of detainees by departure status and stay duration. Two findings stand out. First, the effect is roughly equally split between departed and nondeparted detainees: The estimate for the latter group—those in active, unresolved custody—is 2.019 ($SE = 0.641$), compared to a pre-2016 mean of 5.668, while the coefficient for the former group is 0.676 ($SE = 0.287$), indicating that procurement expansions generate both a flow of resolved cases and a persistent stock of unresolved ones. Second, the decomposition by stay duration shows effects across the full distribution: short spells of at most one year (0.813, $SE = 0.368$), stays of an intermediate range of one to three years (-0.096, $SE = 0.134$), and prolonged stays exceeding three years (1.978, $SE = 0.670$), with the longest stays accounting for the largest share. Procurement expansions generate effects at both tails of the custody duration distribution, a flow of short-term detainees whose cases are resolved within a year and a group held in prolonged custody exceeding three years—the population who generates the most continuous per-diem revenue for private detention operators.

5.4.3 Ruling Out Other Mechanisms

Two alternative channels could confound our interpretation: Procurement may alter the local immigrant population rather than enforcement intensity, or detention may reduce crime. Table 12 tests both using county-level immigration and crime rates per 100,000 residents as placebo outcomes.

Immigration Flow If procurement expansion deterred noncitizens from settling in high-enforcement counties, the detention estimates would partly reflect compositional changes in the population at risk of detention rather than enforcement intensity. Panel A of Table 12 shows no such effect: Neither the Hispanic noncitizen nor the Mexican-born noncitizen immigration rate responds significantly to procurement in any specification, including those with state–year fixed effects. If anything, the point estimates are positive, inconsistent with a deterrence effect. The detention effects documented above therefore operate through intensified detentions among the existing population rather than through changes in its composition.

Public Safety If detentions incapacitate individuals who pose a public safety risk, procurement expansions should reduce local crime rates. Panel B shows no evidence of this. The violent crime rate is effectively unchanged, with point estimates near zero and a 95% confidence interval that rules out any effects on crime larger than 3% of the pre-2016 mean. Property crime is similarly unaffected. The null is consistent with the decomposition by criminal record status in Table 7, which showed that 87% of the detention expansion fell on individuals with no criminal record. We thus conclude that procurement-driven detention expansions produce no measurable public safety benefit.

6 Private Returns to Immigration Enforcement

The federal government pays private contractors a per-diem fee for each person held in immigration detention, and the profit margin on that fee is the rate at which taxpayer spending becomes private rent. Interpreting the incidence of this spending requires that we take a stand on the public value of the output produced at the margin. We point-identify the rent extraction rate by linking abnormal equity returns around political events (Fisman, 2001) to the procurement-driven detention expansion documented in the previous section and embed the result in the MVPF framework of Hendren and Sprung-Keyser (2020).

6.1 Incidence Accounting for Immigration Enforcement Procurement

6.1.1 From Procurement to Profit

Consider contractor i operating a detention facility under a per-diem contract with ICE. The government pays a fixed rate p_i for each detainee–day. Let d_i denote the number of detainees held by contractor i and τ_i their average stay length. Total detainee–days are therefore $\mathcal{D}_i = d_i \cdot \tau_i$, and total procurement obligations are

$$G_i = p_i \cdot \mathcal{D}_i = p_i \, d_i \, \tau_i \tag{10}$$

The contractor incurs a marginal cost c_i per detainee–day, covering staffing, housing, and other operating expenses. Total costs are $c_i \cdot \mathcal{D}_i$, and profits are revenue minus costs:

$$\Pi_i = (p_i - c_i) \cdot \mathcal{D}_i \tag{11}$$

To see how profits relate to procurement obligations, note that $(p_i - c_i) = \mu_i \cdot p_i$, where

$$\mu_i \equiv \frac{p_i - c_i}{p_i} \in [0, 1] \tag{12}$$

is the profit margin, the fraction of the per-diem rate not absorbed by operating costs. Substituting into equation (11):

$$\Pi_i = \mu_i \cdot p_i \cdot \mathcal{D}_i = \mu_i \cdot G_i \tag{13}$$

Equation (13) has a simple interpretation: Profits are a fixed fraction μ_i of procurement obligations. A one-dollar increase in G_i raises Π_i by exactly μ_i, regardless of whether the expansion comes from more detainees, longer stays, or a higher per-diem rate. The profit margin μ_i therefore summarizes the private return to public spending on detention. Our goal in this section is to recover μ_i from the data without our having to independently estimate c_i, and to characterize its welfare interpretation.

6.1.2 Measuring the Net Government Cost

Cost Per Detainee The shift-share IV identifies the government's marginal cost of generating detention. A one-standard-deviation increase in the instrument value over its mean raises the county–month detainee count by $\hat{\pi}$. The government's marginal procurement outlay per additional detainee is

$$\frac{\partial G}{\partial d} = p \cdot \bar{\tau} \tag{14}$$

where p is the per-diem rate and $\bar{\tau}$ is the average stay length. When we scale by the instrument-induced detention increase, the total fiscal cost is

$$\Delta G = p \cdot \hat{\pi} \cdot \bar{\tau} \cdot \bar{N} \tag{15}$$

where $\bar{N}$ is the baseline county–month detainee count. Each term is directly observed or estimated from the data: p from procurement contract records, $\hat{\pi}$ from the shift-share IV, $\bar{\tau}$ from the duration decomposition, and $\bar{N}$ from the Vera detention panel.

Net Government Cost The net cost of procurement expansion has three components. The first is the direct fiscal outlay ΔG from equation (15). The second is a fiscal externality: Detention of an individual for $\bar{\tau}$ days removes him from the labor force, suppressing

earnings by $\bar{w} \cdot \bar{\tau}$, where $\bar{w}$ is the average daily wage. At an effective tax rate t, this generates forgone tax revenue of $t \cdot \bar{w} \cdot \bar{\tau}$ per detainee. The third is any public safety benefit B per detainee, avoided victimization costs or reduced criminal justice expenditures, that offsets the first two. After we sum over the $\hat{\pi}$ instrument-induced detainees, the net government cost is

$$C = \underbrace{\Delta G}_{\text{fiscal outlay}} + \underbrace{t \cdot \bar{w} \cdot \bar{\tau} \cdot \hat{\pi}}_{\text{forgone tax revenue}} - \underbrace{B \cdot \hat{\pi}}_{\text{public safety benefit}} . \tag{16}$$

The magnitude of B is an empirical input, not an assumption. The procurement-induced detention increase falls entirely on individuals with no criminal record and no final order of removal, which informs the calibration of B.

6.1.3 Measuring Private Returns to Procurement

The profit margin μ_i is not directly observed. Equation (13), however, implies that we can recover μ_i from the joint variation in equity returns and procurement obligations without ever observing c_i.

Under rational expectations, the post-election equity appreciation of contractor i reflects the present discounted value of all future profit gains implied by the procurement expansion:

$$\widehat{CAR}_i \cdot MktCap_{i,0} = \Delta\Pi_i \tag{17}$$

where $\widehat{CAR}_i$ is the SDID abnormal return and $MktCap_{i,0}$ is pre-election market capitalization. The left-hand side is the dollar gain in firm value; the right-hand side is the profit increase the market assigns to the procurement expansion.

From equation (13), $\Delta\Pi_i = \mu_i \cdot \Delta G_i$. Substituting into equation (17):

$$\widehat{CAR}_i \cdot MktCap_{i,0} = \mu_i \cdot \Delta G_i \tag{18}$$

The rent extraction rate is therefore

$$\phi_i \equiv \frac{\Delta\Pi_i}{\Delta G_i} = \frac{\widehat{CAR}_i \cdot MktCap_{i,0}}{\Delta G_i} = \mu_i \tag{19}$$

Equation (19) is the central identification result: ϕ_i is point-identified from the ratio of the equity gain to the procurement expansion. The SDID estimator provides the numerator via equation (17); the shift-share IV provides the denominator via equation (15). Whether $\phi_i = \mu_i$ should be interpreted as a rent extraction rate depends on whether the marginal

detention generated by procurement produces any measurable public benefit.

6.1.4 MVPF of Immigration Enforcement Procurement

The MVPF of a policy is the ratio of beneficiaries' willingness to pay to the net cost to the government (Hendren and Sprung-Keyser, 2020):

$$MVPF = \frac{WTP}{\text{Net Government Cost}} \tag{20}$$

In the canonical MVPF framework, beneficiaries are households, and the resulting statistic is interpreted as a welfare object. Here, however, the beneficiaries are private contractors rather than households. Accordingly, we adopt the MVPF framework as an incidence-accounting device: The same numerator–denominator logic applies, but the resulting ratio is interpreted as a shareholder incidence rather than social welfare metric. In our setting, the willingness of contractors to pay is revealed by the response of the equity market to the anticipated expansion in procurement. Aggregating across contractors, the numerator is

$$WTP = \sum_i \Delta\Pi_i = \sum_i \widehat{CAR}_i \cdot MktCap_{i,0} \tag{21}$$

The denominator is the net government cost from equation (16). The MVPF of immigration detention is therefore

$$MVPF = \frac{\sum_i \widehat{CAR}_i \cdot MktCap_{i,0}}{\Delta G + t \cdot \bar{w} \cdot \bar{\tau} \cdot \hat{\pi} - B \cdot \hat{\pi}} \tag{22}$$

Dividing numerator and denominator by ΔG:

$$MVPF = \frac{\phi}{1 + \frac{t \cdot \bar{w}}{p} - \frac{B}{p \cdot \bar{\tau}}} \tag{23}$$

where $\phi = WTP/\Delta G$ is the aggregate rent extraction rate. If detention generates no public safety benefit ($B = 0$), the forgone tax revenue inflates the denominator above one, so $MVPF < \phi$. Every dollar of procurement costs the government more than a dollar once the fiscal externality from the removal of workers from the tax base is internalized.

6.2 Shareholder Returns per Dollar of ICE Spending

We now combine the two empirical designs to estimate the rent extraction rate ϕ_i for GEO Group and CoreCivic, the two largest private detention contractors. Both firms account for a sizable share of immigration detention, although the available investor reports do not permit a directly comparable combined estimate. GEO reports that it alone accounts for approximately 42% of private-sector ICE detention beds (The GEO Group, Inc., 2025b). CoreCivic, in turn, reports that private operators manage about 72% of the ICE detention population and that CoreCivic alone manages roughly 23% of that population (CoreCivic, Inc., 2026).[3] The calculation proceeds in three steps: We recover the dollar value of equity gains from the SDID event study, measure the incremental procurement cost from federal contracting data, and take the ratio.

Step 1: Equity Market Gains The SDID estimator identifies the cumulative abnormal return of each ICE detention contractor around the 2024 presidential election, netting out common market movements and pre-election trends using a donor pool of non-ICE federal contractors. Figure 5 plots the SDID abnormal return paths over a 21-trading-day window following the election. GEO Group's cumulative abnormal return is $\widehat{CAR}_{GEO} = 0.634$, which implies a 63% post-election appreciation relative to the synthetic control. With a pre-election market capitalization of $1.98 billion, the dollar gain in firm value, the market's revealed present discounted value of all future profit increases, is

$$\widehat{CAR}_{GEO} \times MktCap_{GEO,0} = 0.634 \times 1{,}980 = \$1{,}255 \text{ million}$$

CoreCivic's SDID abnormal return is $\widehat{CAR}_{CXW} = 0.389$, with a pre-election market capitalization of $1.45 billion, producing a dollar gain of

$$\widehat{CAR}_{CXW} \times MktCap_{CXW,0} = 0.389 \times 1{,}454 = \$566 \text{ million}$$

The SC estimator yields similar CARs of 0.600 and 0.337, respectively, confirming that the results are not sensitive to the choice of counterfactual construction (Figure 5).

[3] Taken together, these figures indicate that GEO Group and CoreCivic play a central role in privately operated immigration detention, while also underscoring that the two reports measure different margins, beds in GEO's case and detainee population in CoreCivic's.

Step 2: Government Procurement Cost The denominator of the rent extraction rate requires the increment in ICE procurement for each firm. We measure ΔG_i as the difference between FY 2025 ICE obligations and the FY 2021–2024 baseline average, using contract-level data from USASpending. GEO Group's FY 2025 ICE obligations are $621 million, up from a baseline of $417 million, yielding $\Delta G_{GEO} = \$205$ million. CoreCivic's FY 2025 obligations are $306 million, up from $153 million, yielding $\Delta G_{CXW} = \$153$ million. Together, the two firms account for $358 million in incremental ICE procurement relative to the pre-election baseline.

Step 3: Rent Extraction Rate Applying equation (19), we calculate the rent extraction rate for each firm as the ratio of equity gains to incremental procurement:

$$\phi_i = \frac{\widehat{CAR}_i \times MktCap_{i,0}}{\Delta G_i}$$

The numerator, from Step 1, is the market's valuation of the profit increase implied by the enforcement expansion. The denominator, from Step 2, is the incremental procurement spending. For GEO Group, $\phi_{GEO} = 1{,}255/205 = 6.14$; for CoreCivic, $\phi_{CXW} = 566/153 = 3.70$.

The rent extraction rate ϕ_i measures how many dollars of shareholder value are created per additional dollar the government spends on detention at firm i. A value of $\phi_{GEO} = 6.14$ means that each dollar of incremental ICE procurement directed to GEO Group generates $6.14 in equity value for its shareholders. The ratio exceeds one because the market capitalizes not just the immediate profit on the new spending but the present value of the entire stream of future profits the expansion implies, higher baseline capacity, longer contract horizons, and the expectation that elevated enforcement will persist. In this sense, ϕ_i captures the total private return to a marginal dollar of public procurement, inclusive of the option value embedded in the government's revealed commitment to expanded detention.

From Rent Extraction to the MVPF To move from ϕ to the MVPF, we adjust the denominator for the fiscal externalities identified in equation (16). The MVPF formula from equation (23) normalizes by the cost multiplier $C/\Delta G$. Under the baseline calibration, a per-diem rate of $p = \$142$, average stay length of $\bar{\tau} = 194$ days, effective tax rate of

$t = 0.186$ (Hendren and Sprung-Keyser, 2020), and average daily wage of $\bar{w} = \$322$,[4] the cost multiplier is

$$\frac{C}{\Delta G} = 1 + \frac{t \cdot \bar{w}}{p} - \frac{B}{p \cdot \bar{\tau}} = 1 + \frac{0.186 \times 322}{142} - 0 = 1.422$$

The public safety benefit B is set to zero on the basis of the composition results: The marginal detainee has no criminal record and originates from a deportation-cooperative country, and the procurement expansion generates no measurable reduction in county-level crime rates.

The cost multiplier measures the full social cost of each dollar the government spends on enforcement procurement. A value of 1.422 means that every dollar of direct ICE spending costs the government $1.42 once fiscal externalities are internalized. The wedge arises because detention of an individual removes her from the labor force for the duration of her stay: At a daily wage of $322 and a tax rate of 18.6%, each detainee–day suppresses $322 in earnings and $59.89 in tax revenue, which amounts to 42.2 cents per dollar of per-diem spending. With no offsetting public safety benefit, the cost multiplier exceeds one, and the MVPF is therefore strictly below ϕ:

$$MVPF_i = \frac{\phi_i}{1.422}$$

In the baseline calibration, the MVPF is 4.32 for GEO Group and 2.60 for CoreCivic; aggregating between both firms yields a combined MVPF of 3.58 (Table 13). MVPF measures the dollar value that beneficiaries receive per dollar of net government cost (Hendren and Sprung-Keyser, 2020), where the net cost includes both direct fiscal expenditures and forgone tax revenue from the removal of detainees from the labor force. Here, the beneficiaries are corporate shareholders, so the statistic quantifies the rate at which public enforcement dollars are converted into private rents rather than social value. A high MVPF therefore does not indicate social efficiency—it measures rent extraction. This is the outcome Hart et al. (1997) predict when governments contract out incarceration to profit-maximizing firms and is consistent with evidence that confinement systems can generate private returns with limited offsetting social benefits (Mukherjee, 2021; Yegen, 2021).

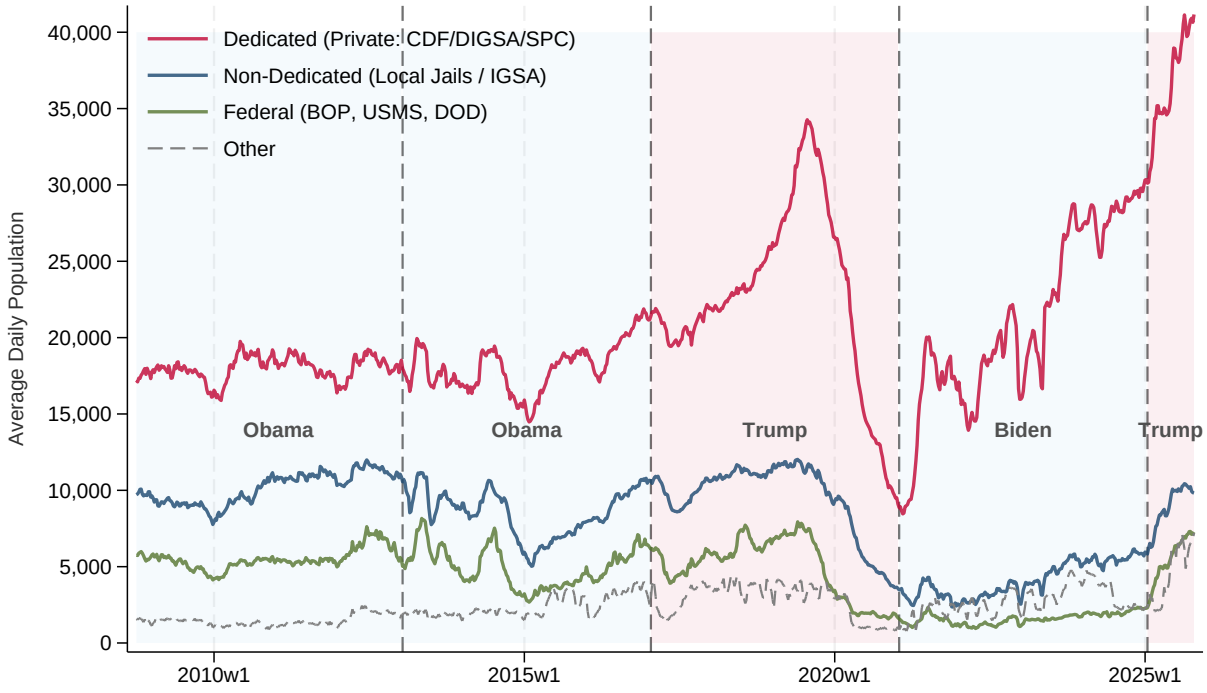
[4]We calibrate the average daily wage with reference to Edwards and Ortega (2017), who estimate the average weekly earnings of undocumented workers at $581 vs. $1,039 for US-born workers in 2013 dollars. Inflating to 2025 dollars using the Bureau of Labor Statistics's (BLS's) CPI-U yields weekly earnings of $803 and annual earnings of $41,753 (U.S. Bureau of Labor Statistics, 2025, 2026), implying an average daily wage of approximately $322 over 130 workdays per year.

7 Conclusion

This paper uses presidential elections as shocks to immigration enforcement expectations to study how detention procurement translates into private shareholder gains and confinement outcomes. Equity markets immediately repriced the shares of ICE detention contractors following the 2024 election, generating billions in combined gains for GEO Group and CoreCivic. A shift-share instrument confirms that the procurement expansions anticipated by markets translated into detention increases, which were concentrated among individuals with no criminal record and generated no measurable public safety benefit. Linking the two designs through a welfare accounting framework, we find that shareholders gain roughly $3–4 for every dollar of net government cost. This ratio does not indicate that enforcement procurement is socially efficient—it quantifies the rate at which public funds are converted into private rents, the outcome Hart et al. (1997) predict when governments contract out incarceration to profit-maximizing firms, and consistent with evidence that carceral systems can generate private returns with limited offsetting social benefits.

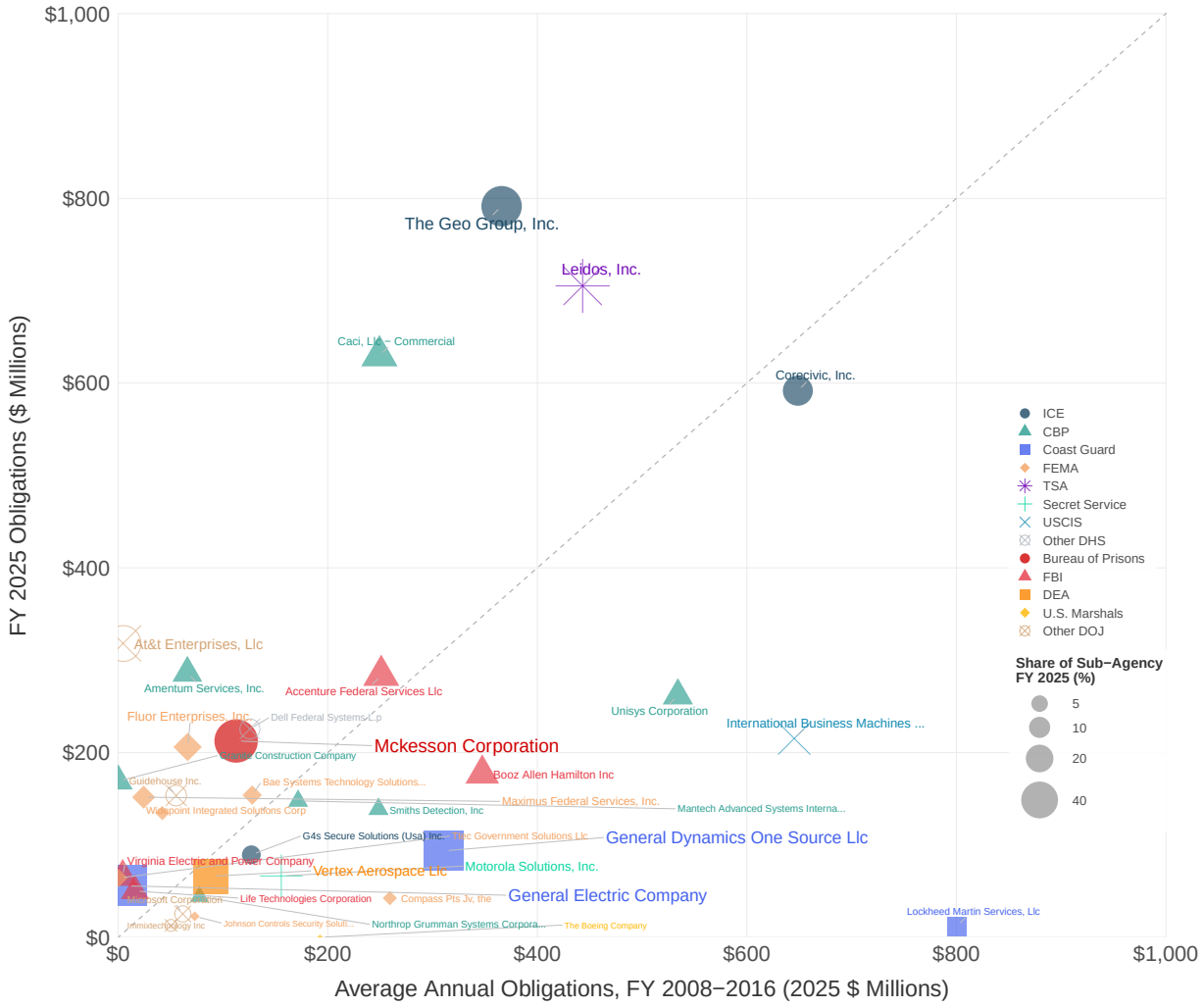
The findings reflect a broader feature of the carceral state (Gilmore, 2007; Davis, 2011; Murakawa, 2014). Immigration detention is one node in a larger network of privately operated facilities in which contractor revenues scale with the number of people confined. When the state's authority to detain is contracted out to private firms operating under per-diem arrangements, the financial returns to confinement accrue to shareholders rather than the public. When electoral cycles provide demand shocks that drive procurement, and when the marginal individual detained has no criminal record, the equilibrium level of confinement reflects the incentives of the contracting system as much as the requirements of public safety. Quantifying the rate at which public procurement is converted into private returns, and identifying who bears the fiscal and social cost, is a necessary step toward evaluating the distributional and fiscal consequences of this system.

Figure 1: ICE Detention Population by Facility Type



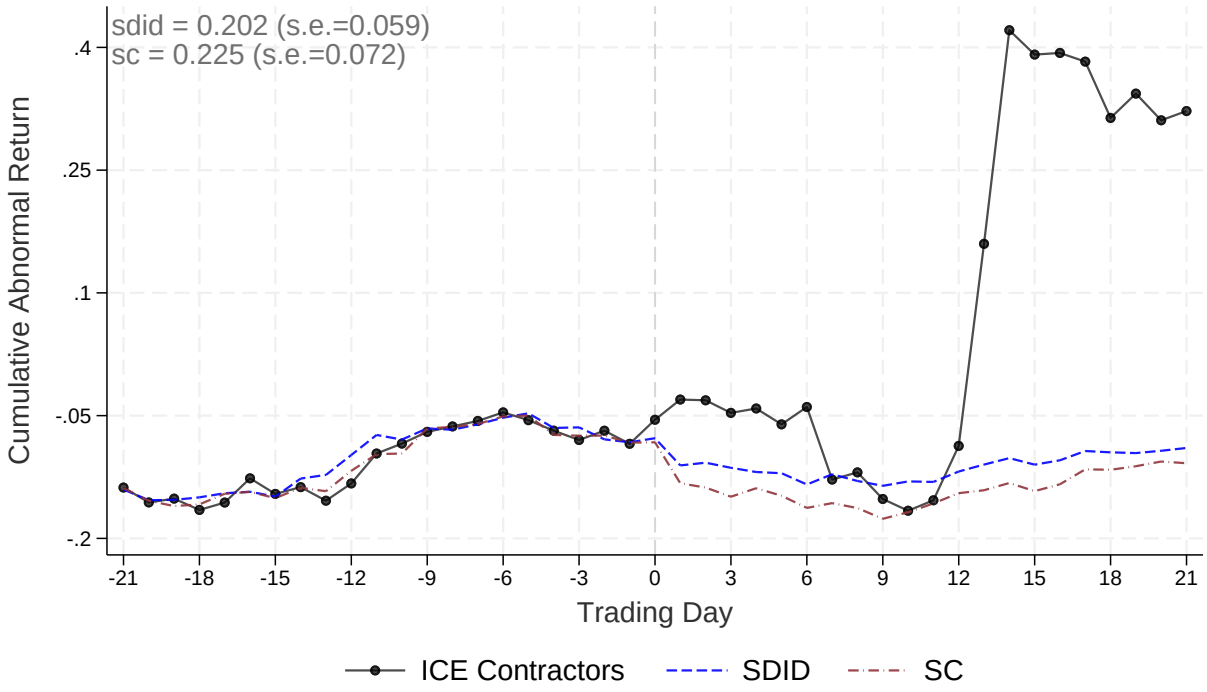
Notes: This figure plots the weekly average daily detention population held by ICE across four facility types using data from the Vera Institute of Justice. Dedicated facilities include contract detention facilities (CDFs), dedicated intergovernmental service agreement (DIGSA) facilities, and service processing centers (SPCs), which are privately operated by firms such as GEO Group and CoreCivic. Nondedicated facilities are local government jails contracting with ICE under intergovernmental service agreements (IGSAs). Federal facilities include those operated by the Bureau of Prisons (BOP), US Marshals Service (USMS), and Department of Defense (DOD). The "Other" category includes hospitals, hotels, holding facilities, and family residential centers. Vertical dashed lines indicate US presidential inaugurations (2013, 2017, 2021, 2025). Shaded regions denote Democratic (blue) and Republican (red) administrations.

Figure 2: 2025 Contract Obligations vs. 2008–2016 Historical Average



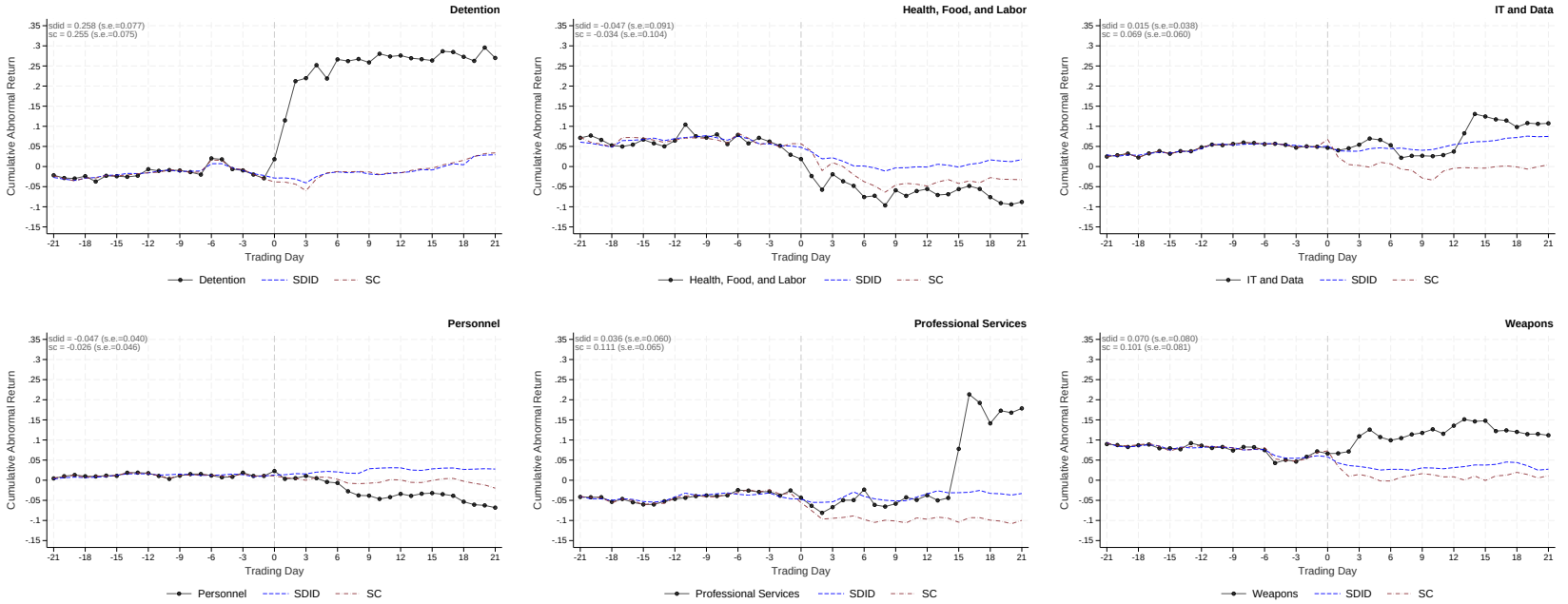
Notes: This figure presents a scatter plot of fiscal year (FY) 2025 contract obligations against the FY 2008–2016 historical average for publicly traded law enforcement contractors, both expressed in 2025 dollars. Each dot represents a firm with at least $50 million in obligations in either period. Color and shape indicate the subagency awarding the largest share of the firm's FY 2025 contracts. Dot size and label size are proportional to the firm's share of its primary subagency's total FY 2025 obligations among publicly traded contractors. The dashed line marks equal spending across periods; firms above the line expanded their contracting relative to the CPI-adjusted historical average.

Figure 3: Impact of 2024 Election on ICE Contractors' Stock Returns



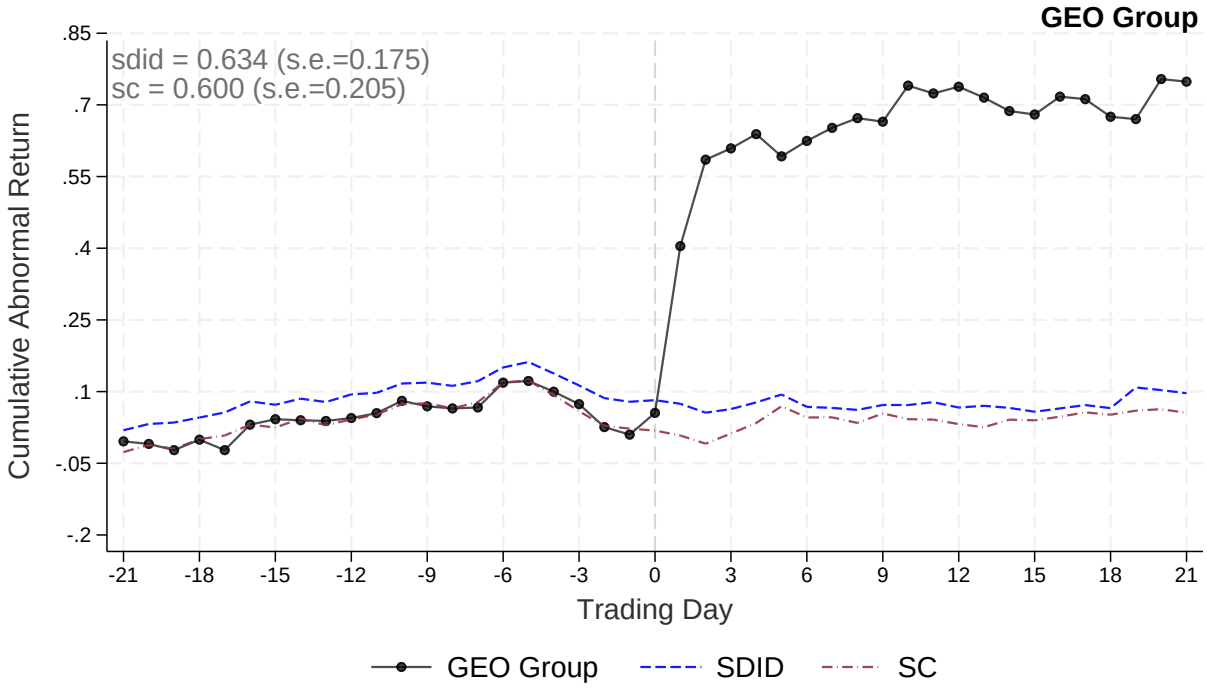
Notes: This figure shows the daily effect of the 2024 US presidential election on cumulative abnormal returns (CARs) for ICE contractors. The solid line plots observed CARs; the dashed lines plot counterfactual CAR paths constructed by synthetic difference-in-differences (SDID) and synthetic control (SC). The vertical dashed line marks the first trading day after the election. The SDID and SC post-election estimates are reported in the figure, with standard errors in parentheses. The CARs are computed as the cumulative sum of abnormal returns from one quarter before the election. Abnormal returns are estimated with the Carhart four-factor model over a 252-trading-day estimation window ending 30 trading days prior to the election.

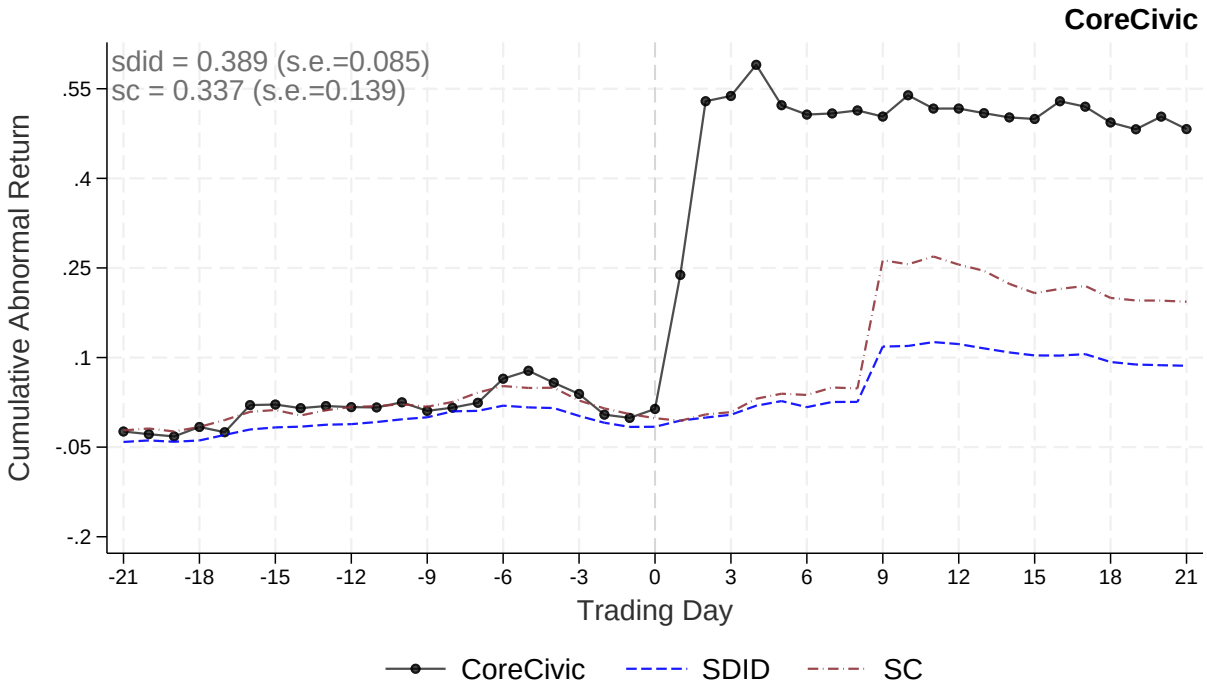
Figure 4: Impact of 2024 Election on Stock Returns by ICE Contract Category



Notes: This figure shows the daily effect of the 2024 US presidential election on cumulative abnormal returns (CARs) for firms with ICE contracts, separately by contract category: detention, health/food/labor, information technology (IT) and data, personnel, professional services, and weapons. Categories are defined by product/service code (PSC) classification. Each panel plots the observed CARs for treated firms (solid line) alongside counterfactual CAR paths constructed by synthetic difference-in-differences (SDID) and synthetic control (SC). The vertical dashed line marks the first trading day after the election. The SDID and SC post-election estimates are reported in the figure, with standard errors in parentheses. The CARs are computed as the cumulative sum of abnormal returns from one quarter before the election. Abnormal returns are estimated with the Carhart four-factor model over a 252-trading-day estimation window ending 30 trading days prior to the election.

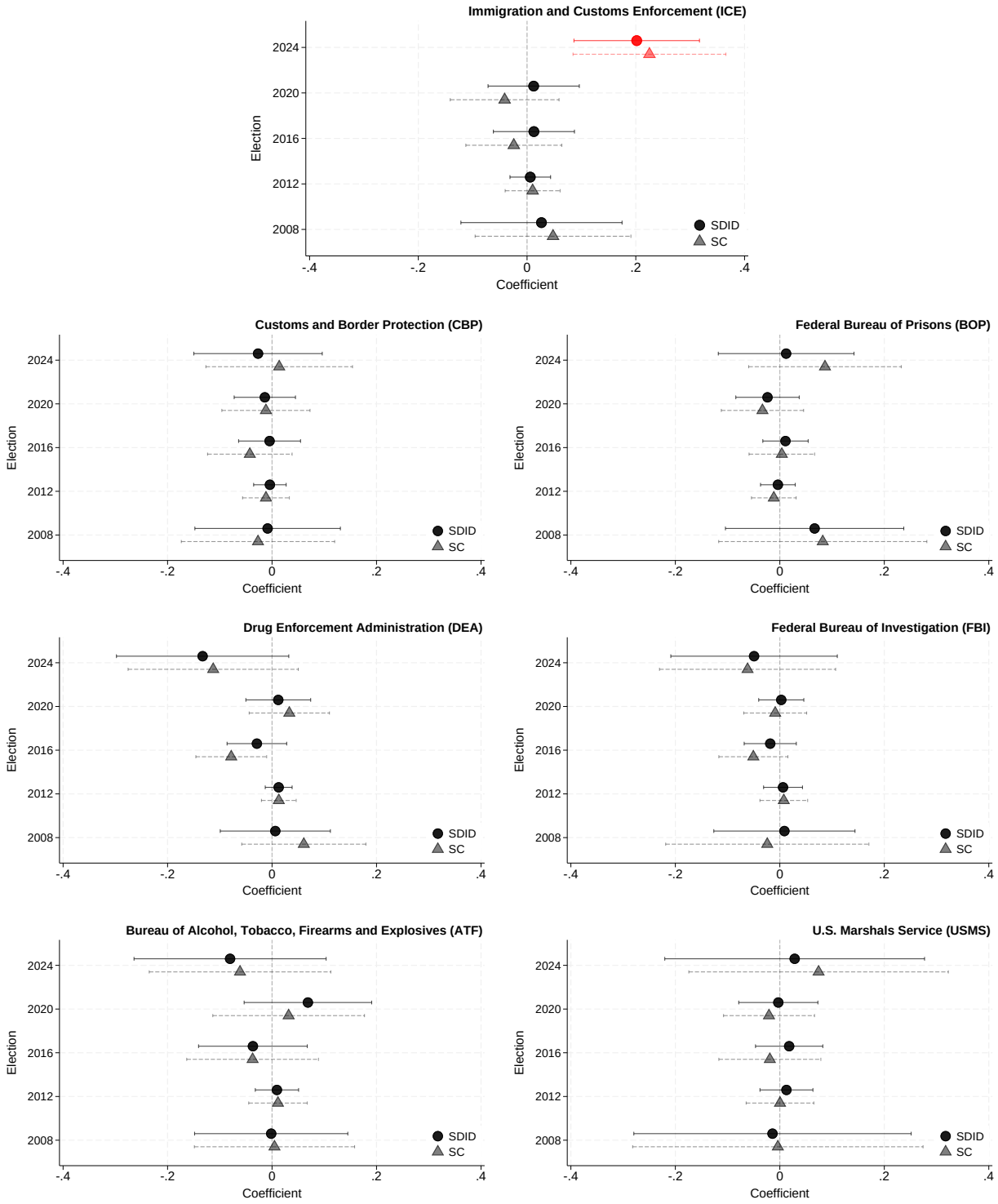
Figure 5: Impact of 2024 Election on GEO Group and CoreCivic Stock Returns





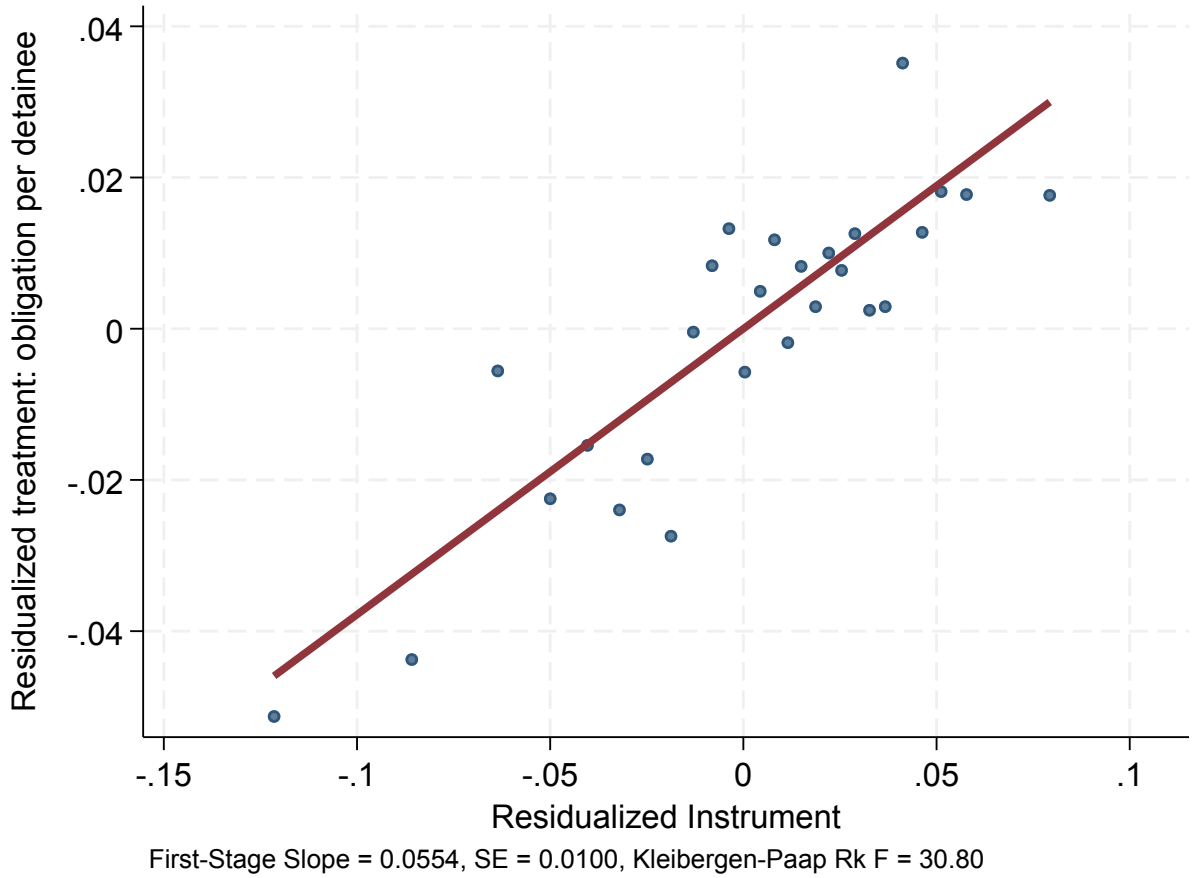
Notes: This figure shows the daily effect of the 2024 US presidential election on cumulative abnormal returns (CARs) for GEO Group and CoreCivic. The solid line plots observed CARs; the dashed lines plot counterfactual CAR paths constructed by synthetic difference-in-differences (SDID) and synthetic control (SC). The vertical dashed line marks the first trading day after the election. The SDID and SC post-election estimates are reported in the figure, with standard errors in parentheses. The CARs are computed as the cumulative sum of abnormal returns from one quarter before the election. Abnormal returns are estimated with the Carhart four-factor model over a 252-trading-day estimation window ending 30 trading days prior to the election.

Figure 6: Estimates of Election Effects on Cumulative Abnormal Returns by Agency



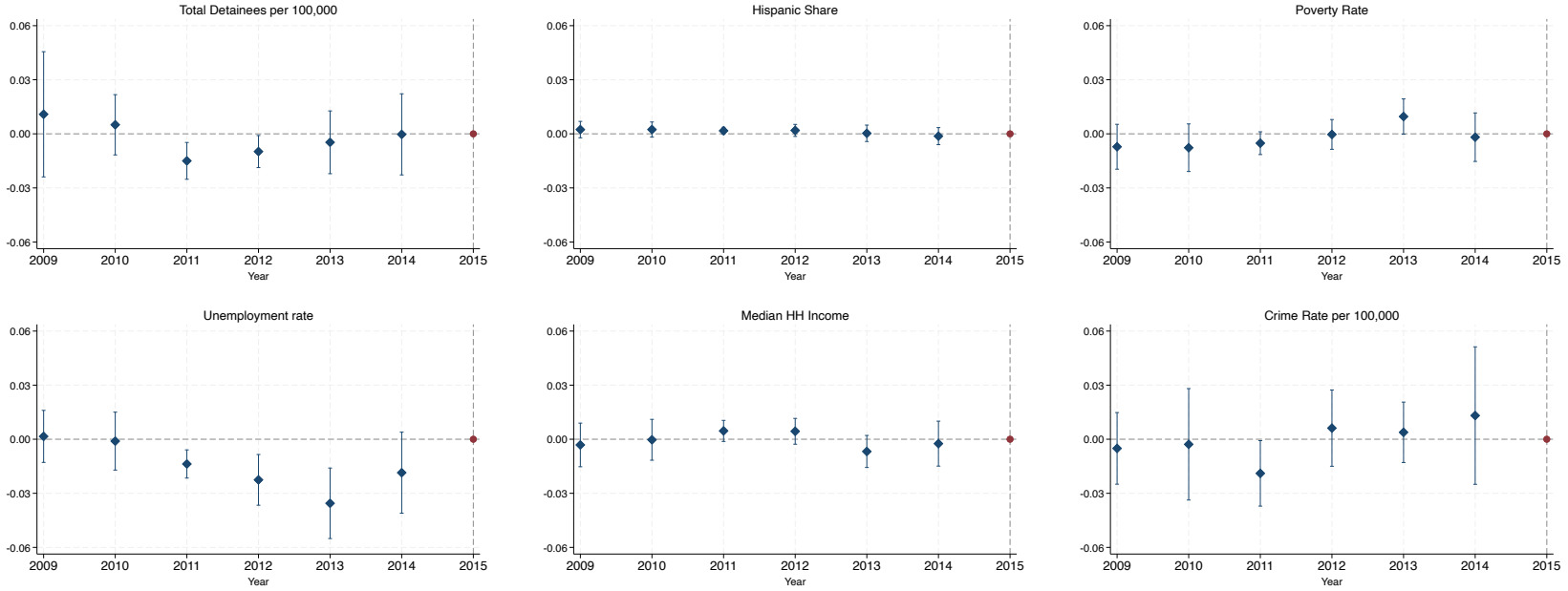
Notes: This figure reports the synthetic difference-in-differences (SDID) and synthetic control (SC) treatment effect estimates on cumulative abnormal returns (CARs) of publicly traded firms contracting with each federal agency, separately for each presidential election (2008–2024). Circles denote the SDID estimates, and triangles denote the SC estimates. Horizontal lines represent 95% confidence intervals based on placebo standard errors. The CARs are computed as the sum of daily abnormal returns over 63 trading days from one quarter before the election. Abnormal returns are calculated with the Carhart four-factor model with an estimation window of 252 trading days ending 30 days before the event. The ICE 2024 estimate, in red, is highlighted to denote our main specification.

Figure 7: First Stage: Shift-Share Instrument and Local Procurement Intensity



Notes: Binned scatter plot of the treatment variable (vertical axis) against the shift-share instrument (horizontal axis), after residualization of both on county fixed effects, year–month fixed effects, and time-varying county controls. Each bin contains an equal number of observations. The treatment is county-level enforcement procurement obligations divided by the 2011–2015 average monthly detainee count standardized to have mean zero and unit standard deviation.

Figure 8: Pre-period Balance: Detention Outcomes and County Characteristics



Notes: Each panel reports coefficients on the interaction between the standardized shift-share exposure measure and pre-period year indicators (2009–2014), with 2015 as the omitted reference year. The detention rate panel is estimated on the county–month panel with county and month fixed effects; the other panels are estimated at county–year frequency. All outcomes are standardized to have mean zero and unit standard deviation within each estimation sample. Blue dots are coefficient estimates; vertical bars are 95% confidence intervals with standard errors clustered at the county level.

Table 1: Summary Statistics for Law Enforcement Contractors and Donor Pool Firms

	ICE		Non-ICE LE		Donor Pool	
	(1) Mean	(2) SD	(3) Mean	(4) SD	(5) Mean	(6) SD
Panel A: 2024 Election						
Size	8.54	2.27	8.54	2.13	6.73	2.49
Profitability	0.01	0.20	0.02	0.22	-0.05	0.36
Leverage	0.50	0.34	0.53	0.35	0.40	0.40
Avg. Contract Size	0.46	0.80	0.20	0.71		
Number of Contracts	130.3	262.6	44.0	314.1		
Number of Firms	58		332		3,828	
Observations	58		332		3,828	
Panel B: Elections 2008–2020						
Size	8.32	2.05	7.77	2.09	6.57	2.16
Profitability	0.04	0.21	0.01	0.24	-0.01	0.28
Leverage	0.42	0.30	0.42	0.32	0.37	0.36
Avg. Contract Size	0.22	0.55	0.17	1.44		
Number of Contracts	133.4	226.2	39.2	404.8		
Number of Firms	150		738		6,476	
Observations	323		1,656		15,052	

Notes: This table reports summary statistics for firms in US presidential election years from 2008 to 2024. Panel A focuses on the 2024 election, and Panel B pools all firm–election year observations from 2008 through 2020. Columns (1)–(2) report statistics for ICE contractors, columns (3)–(4) for non-ICE law enforcement (LE) contractors, and columns (5)–(6) for the donor pool of noncontractor firms used as the comparison group in the estimation. Size is the log of total assets. Profitability is return on equity (net income divided by common equity). Leverage is total debt divided by total capital. Average contract size (in millions of dollars) is total law enforcement obligations divided by the number of contracts. All financial variables are measured at their values in the fiscal quarter preceding each election. Contract variables are at their values in the federal fiscal year of each election. Profitability and leverage are winsorized at the 1st and 99th percentiles.

Table 2: Summary Statistics for the Procurement-and-Detention Analysis Sample

Variable	Mean	SD	Observations
Panel A: Treatment and Instrument			
County Procurement Obligations per Baseline Detainee (1,000 USD)	19.628	180.893	145,745
Shift-Share Instrument	49.481	613.901	145,745
Panel B: Main Outcome and Criminal Record			
Total Detainees per Capita	8.417	13.446	145,745
Noncriminal Detainees per Capita	7.184	11.845	145,745
Any Criminal Detainees per Capita	1.233	5.035	145,745
Violent-Crime Detainees per Capita	0.381	2.125	145,745
Nonviolent-Crime Detainees per Capita	0.852	3.589	145,745
Panel C: Gender And Age			
Female Detainees per Capita	0.439	2.676	145,745
Male Detainees per Capita	7.969	12.945	145,745
Detainees Under 18 per Capita	0.022	0.318	145,745
Detainees Aged 18–34 per Capita	4.009	7.132	145,745
Detainees Aged 35–49 per Capita	2.180	4.752	145,745
Detainees Aged 50–64 per Capita	0.386	1.499	145,745
Detainees Aged 65+ per Capita	0.028	0.301	145,745
Panel D: Country of Origin and Compliance			
Mexican-Origin Detainees per Capita	5.621	10.242	145,745
Other Central And South American–Origin Detainees per Capita	2.223	5.122	145,745
Asian-Origin Detainees per Capita	0.195	0.998	145,745
African-Origin Detainees per Capita	0.113	0.778	145,745
European-Origin Detainees per Capita	0.080	0.663	145,745
North America Excluding Mexico Detainees per Capita	0.026	0.377	145,745
Oceania-Origin Detainees per Capita	0.014	0.228	145,745
Uncooperative-Country Detainees per Capita	0.265	1.347	145,745
At-Risk-Country Detainees per Capita	0.170	0.934	145,745
Cooperative-Country Detainees per Capita	7.904	12.755	145,745
Panel E: Departure Status and Stay Duration			
Departed Detainees per Capita	2.559	6.255	145,745
Nondeparted Detainees per Capita	5.858	9.753	145,745
Stay At Most 1 Year per Capita	2.126	5.405	145,745
Stay 1–3 Years per Capita	0.349	1.647	145,745
Stay More Than 3 Years per Capita	5.942	9.903	145,745
Panel F: County Controls			
Log Population	11.729	1.265	145,745
Unemployment Rate	7.190	2.964	145,745
Poverty Rate	14.855	5.802	145,745
Median Household Income	56937.550	17417.672	145,745

Notes: The table reports summary statistics for the common estimation sample used throughout the detention procurement analysis. The sample comprises 145,745 county–month observations spanning January 2003–early 2025. Treatment is county-level ICE and BOP enforcement procurement obligations by place of performance, normalized by each county's 2011–2015 average monthly detainee count. The standardized treatment is recentered and rescaled to have mean zero and unit standard deviation in this sample. County controls are drawn from five-year American Community Survey (ACS) estimates merged to the monthly panel. Panel A reports treatment measures. Panel B reports total detainees and detainees by criminal-record status. Panel C reports the results by detainee gender and age group. Panel D reports the results by detainee region of origin and origin countries' deportation compliance designation. Panel E reports the results by detainee departure status and stay length. Panel F reports county-level controls. All detention outcomes are expressed per capita (per 100,000 county residents).

Table 3: Shift Exogeneity: Shock-Level Pretrend Balance

	Coefficient	SE	p-value	N
Panel A: Obligation-based shifter				
Without pre-level controls	2.44×10^{-8}	(1.43×10^{-8})	0.087	3,071
With pre-level controls	9.13×10^{-9}	(1.29×10^{-8})	0.480	3,071
Panel B: Detention-count shifter (cross-check)				
Without pre-level controls	1.08×10^{-4}	(2.08×10^{-3})	0.958	3,071
With pre-level controls	-1.03×10^{-3}	(1.58×10^{-3})	0.512	3,071

Notes: Each row reports the coefficient $\hat{\rho}$ from a firm-level regression of pre-period procurement growth on the post-period procurement shock, following the approach of **?**. Pre-period growth is the log change in the firm's average procurement between 2009–2011 and 2013–2015. The post-period shock is the firm's average procurement level after the policy change. A small, statistically nonsignificant $\hat{\rho}$ indicates that firms with larger post-period obligations were not on systematically different pre-period growth paths, supporting the exogeneity of the national shifters. Panel A uses procurement obligations as the primary measure of the shifter; Panel B uses detention transaction counts as an independent cross-check. Pre-level controls (where indicated) include the log of the firm's baseline procurement, log baseline county population, log baseline median household income, baseline unemployment rate, and baseline poverty rate. Standard errors are robust to heteroskedasticity.

Table 4: Share Exogeneity: County-Level Cross-Sectional Pretrend Balance

Pre-treatment detention outcome	Coefficient	SE	p-value	N
Total detainees rate	-6.05×10^{-9}	(9.64×10^{-8})	0.950	1,537
Female detainees rate	-7.75×10^{-10}	(1.50×10^{-9})	0.605	1,539
Male detainees rate	6.74×10^{-9}	(9.46×10^{-8})	0.943	1,537
Mexico-origin detainees rate	-2.61×10^{-8}	(4.16×10^{-8})	0.531	1,536
Departed detainees rate	3.67×10^{-8}	(3.49×10^{-8})	0.292	1,541
Non-departed detainees rate	-5.26×10^{-8}	(1.00×10^{-7})	0.599	1,540

Notes: Each row reports the ordinary least squares (OLS) coefficient from a county-level cross-sectional regression of pre-period detention changes on baseline shift-share exposure, following Goldsmith-Pinkham et al. (2020). The dependent variable is the county's average monthly detention rate per 100,000 residents in 2013–2015 minus its average in 2009–2011. The regressor is the county's baseline shift-share exposure, $\widetilde{Z}_c = \sum_f s_{cf}^{\text{pre}} \, \bar{g}_f^{\text{post}}$, where $\bar{g}_f^{\text{post}}$ is firm f's average post-period national procurement obligation. Specifications include log baseline population, log baseline median household income, baseline unemployment rate, and baseline poverty rate as controls. A statistically nonsignificant coefficient indicates that counties with greater baseline contractor exposure were not on differential pre-period detention trends. Standard errors are robust to heteroskedasticity.

Table 5: Rotemberg-Style Influence Weights: Top 20 Firms

Rank	Firm	Influence weight
1	Government Energy Management LLC	0.0182
2	The GEO Group, Inc.	0.0087
3	CoreCivic, Inc.	0.0054
4	McKesson Corporation	0.0053
5	Avaya Holdings Corp.	0.0050
6	Alaska, State of	0.0047
7	Dismas Charities Inc	0.0034
8	Sharp Corporation	0.0033
9	Northrop Grumman Innovation Systems LLC	0.0032
10	NaphCare Inc	0.0032
11	Dean Foods Company	0.0031
12	Government of the United States	0.0030
13	Prairie Farms Dairy Inc	0.0028
14	Community Extended Nuclear Transition Residence for Ex-Offenders, Inc.	0.0028
15	Management & Training Corporation	0.0028
16	Fran/San Meats, L.L.C.	0.0028
17	Keeton Corrections Inc	0.0026
18	Krueger International, Inc.	0.0025
19	Net Direct Systems, LLC	0.0024
20	Volunteers of America Inc	0.0023

Notes: The influence weight for each firm equals its normalized sum of squared county baseline shares: $\alpha_f = \sum_c (s_{cf}^{\text{pre}})^2 \,/\, \sum_{f'} \sum_c (s_{cf'}^{\text{pre}})^2$, where s_{cf}^{pre} is firm f's share of county c's enforcement procurement obligations during 2011–2015. Higher values indicate a greater contribution to the cross-county variance of the shift-share instrument. The effective number of firms is the reciprocal of the Herfindahl–Hirschman index computed over these weights. Firm names appear as recorded in federal procurement files.

Table 6: Effect of Procurement on Total Number of Detainees

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Obligation Per Detainee (Std.)	0.107	0.105	0.075	2.655***	2.695***	2.677***	2.620***
	(0.070)	(0.070)	(0.057)	(0.723)	(0.719)	(0.660)	(0.790)
Pre-2016 Mean	8.417	8.417	8.417	8.417	8.417	8.417	8.417
F-stat	—	—	—	30.80	30.46	41.38	27.60
Observations	145,745	145,745	145,745	145,745	145,745	145,745	145,745
Estimator	OLS	OLS	OLS	2SLS	2SLS	2SLS	2SLS
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year–Month Fixed Effects	Yes	Yes	No	Yes	Yes	No	Yes
State–Year–Month Fixed Effects	No	No	Yes	No	No	Yes	No
Local Controls	No	Yes	Yes	No	Yes	Yes	Yes
Alternative IV Shares	No	No	No	No	No	No	Yes

Notes: Columns (1)–(3) report ordinary least squares (OLS) estimates, and columns (4)–(7) report two-stage least squares (2SLS) estimates for the county–month detention rate per 100,000 residents. The treatment is county-level ICE and BOP procurement obligations by place of performance divided by the county's 2011–2015 average monthly detainee count and standardized to unit variance. The instrument is defined in equation (6). Columns (1)/(4) include county and year–month fixed effects. Columns (2)/(5) add county unemployment rate, log population, poverty rate, and median household income. Columns (3)/(6) replace the year–month fixed effects with state–year–month fixed effects. Column (7) uses 2011–2014 baseline shares. All columns use the same county–month analysis sample of 145,745 observations. Standard errors are clustered at the county level. $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.

Table 7: Effect of Procurement on Number of Detainees by Criminal-Record Status (2SLS)

	Criminal Status		Among Criminal Detainees	
	(1) Noncriminal	(2) Any Criminal	(3) Violent	(4) Nonviolent
Obligations Per Detainee (Std.)	2.169**	0.527	0.103	0.424
	(0.897)	(0.462)	(0.088)	(0.417)
Pre-2016 Mean	8.628	0.037	0.012	0.026
F-stat	30.46	30.46	30.46	30.46
Observations	145,745	145,745	145,745	145,745
County Fixed Effects	Yes	Yes	Yes	Yes
Year–Month Fixed Effects	Yes	Yes	Yes	Yes
Local Controls	Yes	Yes	Yes	Yes

Notes: Each column reports results from a separate two-stage least squares (2SLS) regression for a component of the county–month detention rate per 100,000 residents defined by detainee criminal-record status. All specifications include county and year–month fixed effects and county controls for unemployment rate, log population, poverty rate, and median household income. Column (1) reports results for detainees with no recorded criminal conviction. Column (2) reports results for detainees with any recorded conviction. Columns (3) and (4) split the results from column (2) for detainess with violent and nonviolent offenses. All columns use the same county–month analysis sample of 145,745 observations. See Table 6 for treatment and instrument definitions. Standard errors are clustered at the county level. $^{*}\ p < 0.10,\ ^{**}\ p < 0.05,\ ^{***}\ p < 0.01$.

Table 8: Effect of Procurement on Number of Detainees by Gender and Age (2SLS)

	Gender		Age At Detainment				
	(1) Female	(2) Male	(3) < 18	(4) 18–34	(5) 35–49	(6) 50–64	(7) 65+
Obligation Per Detainee (Std.)	-0.003	2.699^{***}	-0.027^{***}	2.400^{***}	0.277	0.084	-0.011
	(0.074)	(0.732)	(0.007)	(0.617)	(0.181)	(0.149)	(0.010)
Pre-2016 Mean	0.422	8.242	0.036	5.066	2.307	0.363	0.020
F-stat	30.46	30.46	30.46	30.46	30.46	30.46	30.46
Observations	$145{,}745$	$145{,}745$	$145{,}745$	$145{,}745$	$145{,}745$	$145{,}745$	$145{,}745$
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Local Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Each column reports results from a separate two-stage least squares (2SLS) regression for a component of the county–month detention rate per 100,000 residents defined by detainee gender or age at detainment. All specifications include county and year–month fixed effects and county controls for unemployment rate, log population, poverty rate, and median household income. Columns (1) and (2) report the results for female and male detainees. Columns (3)–(7) report the results for detainees under 18, aged 18–34, aged 35–49, aged 50–64, and aged 65 and older. The age-group rates do not sum to the total because some records have missing age information. All columns use the same county–month analysis sample of 145,745 observations. See Table 6 for treatment and instrument definitions. Standard errors are clustered at the county level. $^{*}\ p < 0.10,\ ^{**}\ p < 0.05,\ ^{***}\ p < 0.01$.

Table 9: Effect of Procurement on Number of Detainees by Country and Continent of Origin (2SLS)

	(1) Mexico	(2) C./S. Am.	(3) Asia	(4) Africa	(5) Europe	(6) N. Am.	(7) Oceania
Obligation Per Detainee (Std.)	0.475	1.735***	−0.021	0.017	−0.015	−0.015	−0.002
	(0.425)	(0.456)	(0.052)	(0.042)	(0.022)	(0.020)	(0.005)
Pre-2016 Mean	6.273	1.891	0.182	0.102	0.070	0.028	0.012
F-stat	30.46	30.46	30.46	30.46	30.46	30.46	30.46
Observations	145,745	145,745	145,745	145,745	145,745	145,745	145,745
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Local Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Each column reports results from a separate two-stage least squares (2SLS) regression for a component of the county–month detention rate per 100,000 residents defined by detainee country or continent of birth. All specifications include county and year–month fixed effects and county controls for unemployment rate, log population, poverty rate, and median household income. Column (1) reports the results for detainees born in Mexico. Column (2) reports the results for detainees from Central and South America. Columns (3)–(7) report the results for detainees from Asia, Africa, Europe, North America excluding Mexico, and Oceania. The columns do not exhaust all categories of detainees because some records have missing or unclassified country of birth. All columns use the same county–month analysis sample of 145,745 observations. See Table 6 for treatment and instrument definitions. Standard errors are clustered at the county level. $^{*}\ p < 0.10,\ ^{**}\ p < 0.05,\ ^{***}\ p < 0.01$.

Table 10: Effect of Procurement on Number of Detainees by Country Compliance Status (2SLS)

	(1) Uncooperative	(2) At-Risk	(3) Cooperative
Obligation Per Detainee (Std.)	-0.058	-0.027	2.237^{***}
	(0.093)	(0.079)	(0.607)
Pre-2016 Mean	0.218	0.162	8.238
F-stat	30.46	30.46	30.46
Observations	$145{,}745$	$145{,}745$	$145{,}745$
County Fixed Effects	Yes	Yes	Yes
Year–Month Fixed Effects	Yes	Yes	Yes
Local Controls	Yes	Yes	Yes

Notes: Each column reports results from a separate two-stage least squares (2SLS) regression for a component of the county–month detention rate per 100,000 residents defined by detainee origin countries' DHS deportation-compliance status as of December 2024. All specifications include county and year–month fixed effects and county controls for unemployment rate, log population, poverty rate, and median household income. Column (1) reports the results for detainees from fully uncooperative countries. Column (2) reports the results for detainees from at-risk countries. Column (3) reports the results for detainees from all cooperative countries. All columns use the same county–month analysis sample of 145,745 observations. See Table 6 for treatment and instrument definitions. Standard errors are clustered at the county level. $^{*}\ p < 0.10,\ ^{**}\ p < 0.05,\ ^{***}\ p < 0.01$.

Table 11: Effect of Procurement on Number of Detainees by Departure Status and Stay Duration (2SLS)

	Departure Status		Stay Duration		
	(1) Departed	(2) Non-Departed	(3) ≤ 1 Year	(4) 1–3 Years	(5) > 3 Years
Obligation Per Detainee (Std.)	0.676^{**}	2.019^{***}	0.813^{**}	-0.096	1.978^{***}
	(0.287)	(0.641)	(0.368)	(0.134)	(0.670)
Pre-2016 Mean	2.997	5.668	2.528	0.383	5.754
F-stat	30.46	30.46	30.46	30.46	30.46
Observations	145,745	145,745	145,745	145,745	145,745
County Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year-Month Fixed Effects	Yes	Yes	Yes	Yes	Yes
Local Controls	Yes	Yes	Yes	Yes	Yes

Notes: Each column reports results from a separate two-stage least squares (2SLS) regression for a component of the county–month detention rate per 100,000 residents defined by detainee departure status or stay duration. All specifications include county and year–month fixed effects and county controls for unemployment rate, log population, poverty rate, and median household income. Columns (1) and (2) report the results for detainees with and without a recorded departure date. Columns (3)–(5) report the results for detainees with stays of at most one year, one to three years, and more than three years. The more-than-three-year category includes detainees with completed spells exceeding three years and detainees with no recorded departure date. All columns use the same county–month analysis sample of 145,745 observations. See Table 6 for treatment and instrument definitions. Standard errors are clustered at the county level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 12: Immigration and Crime Placebo Outcomes (2SLS)

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Immigration Rates (per 100,000)						
	Hispanic Noncitizen Rate			Mexican-born Noncitizen Rate		
Obligation Per Detainee (std.)	295.537	293.659	346.047	315.335	320.931	262.707
	(256.495)	(253.595)	(280.625)	(248.136)	(252.160)	(209.089)
Pre-2016 Mean	2704.849	2704.849	2704.849	357.849	357.849	357.849
Observations	21,834	21,834	21,764	22,131	22,131	22,059
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	No	Yes	Yes	No
State–Year Fixed Effects	No	No	Yes	No	No	Yes
Local Controls	No	Yes	Yes	No	Yes	Yes
Panel B: Crime Rates (Per 100,000)						
	Violent Crime Rate			Property Crime Rate		
Obligation Per Detainee (Std.)	-0.057	0.527	-1.205	18.174	14.991	26.067
	(3.998)	(4.109)	(3.638)	(28.379)	(30.139)	(29.055)
Pre-2016 Mean	301.657	301.657	301.657	2455.304	2455.304	2455.304
Observations	23,745	23,745	23,691	23,745	23,745	23,691
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	No	Yes	Yes	No
State–Year Fixed Effects	No	No	Yes	No	No	Yes
Local Controls	No	Yes	Yes	No	Yes	Yes

Notes: Panel A outcomes are county–year immigration rates from the American Community Survey (Hispanic noncitizens and Mexican-born noncitizens), each expressed per 100,000 county residents. Panel B outcomes are county–year Uniform Crime Reporting (UCR) violent and property crime rates per 100,000 residents. The treatment is county–year procurement obligations per baseline detainee (standardized), instrumented by the county-year Bartik shift-share IV. Columns (1)–(2)/(4)–(5) include county and year fixed effects; columns (3) and (6) replace the year fixed effects with state–year fixed effects. Column (1)/(4) has no local controls; columns (2)–(3)/(5)–(6) add county unemployment, log population, poverty, and median household income. Standard errors are clustered at the county level. $^{*}\ p < 0.10,\ ^{**}\ p < 0.05,\ ^{***}\ p < 0.01$.

Table 13: MVPF of Immigration Enforcement Procurement

	GEO Group	CoreCivic	Combined
Panel A: Equity Market Gains			
Pre-election market cap (M)	1,980	1,454	3,434
CAR, SDID	0.634	0.389	
CAR, Synthetic Control	0.600	0.337	
$\Delta\Pi_i$ = CAR $\times$ MktCap, SDID (M)	1,255	566	1,821
$\Delta\Pi_i$ = CAR $\times$ MktCap, SC (M)	1,188	490	1,678
Panel B: ICE Procurement			
FY2025 ICE obligations (M)	621	306	927
FY2021–2024 baseline (M)	417	153	570
ΔG_i (M)	205	153	358
Panel C: Rent Extraction and MVPF			
ϕ_i (SDID) = $\Delta\Pi_i/\Delta G_i$	6.14	3.70	5.09
ϕ_i (SC)	5.81	3.20	4.69
Cost multiplier ($C/\Delta G$)	1.422	1.422	1.422
MVPF (SDID)	4.32	2.60	3.58
MVPF (SC)	4.08	2.25	3.30

Notes: This table reports the marginal value of public funds (MVPF) for immigration enforcement procurement directed to GEO Group and CoreCivic, the two largest private immigration detention contractors. Panel A reports pre-election market capitalization and the dollar equity gain $\Delta\Pi_i = \widehat{CAR}_i \times MktCap_{i,0}$ under the synthetic difference-in-differences (SDID) and synthetic control (SC) estimators. Panel B reports FY 2025 ICE obligations, the FY 2021–2024 baseline average, and the incremental procurement ΔG_i. Panel C reports the rent extraction rate $\phi_i = \Delta\Pi_i/\Delta G_i$, the cost multiplier $C/\Delta G = 1 + t\bar{w}/p - B/(p\bar{\tau})$, and the MVPF $= \phi_i/(C/\Delta G)$. Baseline calibration: per-diem rate $p = \$142$/day, average stay length $\bar{\tau} = 194$ days, effective tax rate $t = 0.186$, average daily wage $\bar{w} = \$322$/day, public safety benefit $B = 0$. The "Combined" column sums the dollar amounts across both firms and computes aggregate ratios from the summed numerators and denominators.

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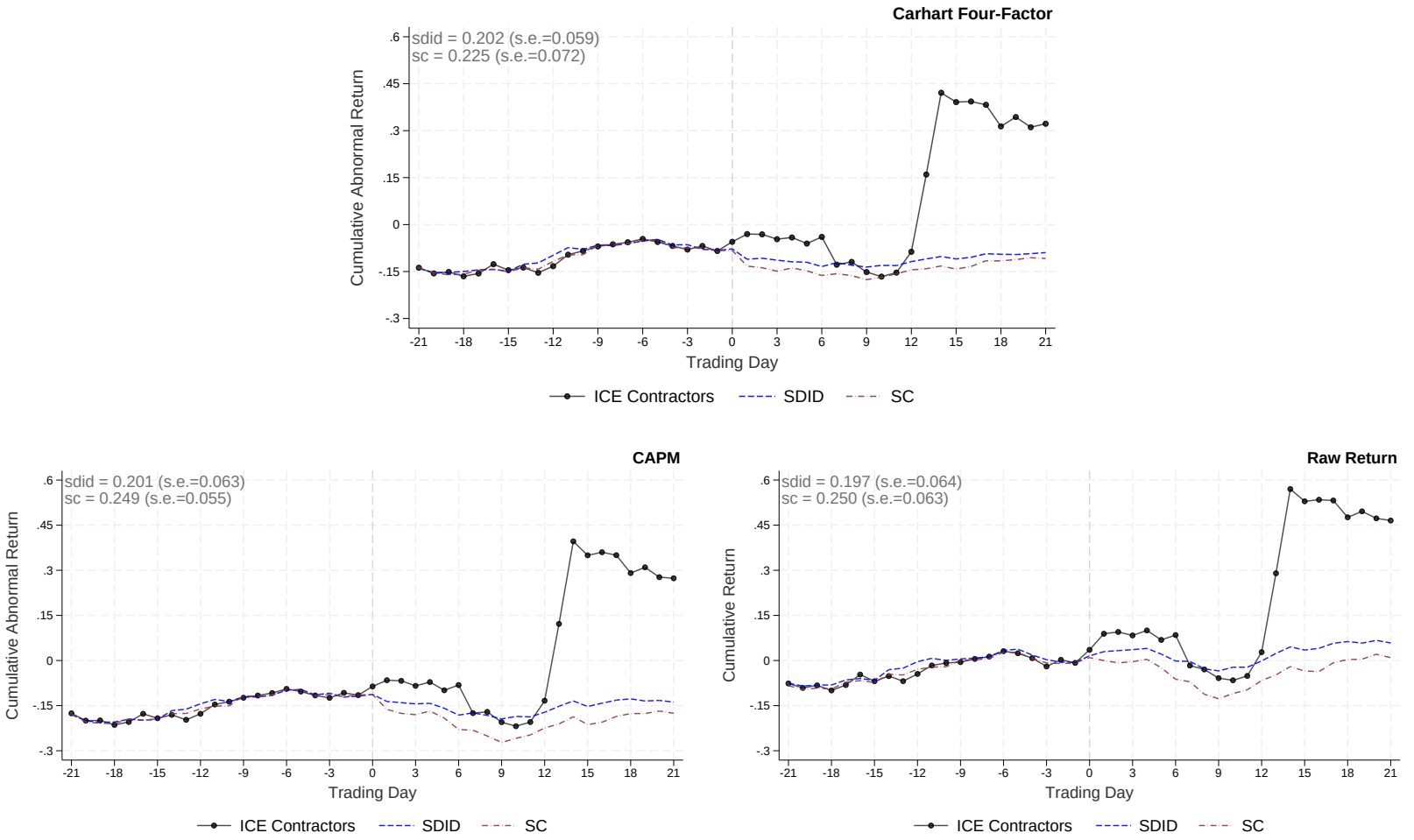
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Supplementary Materials

A Additional Specifications

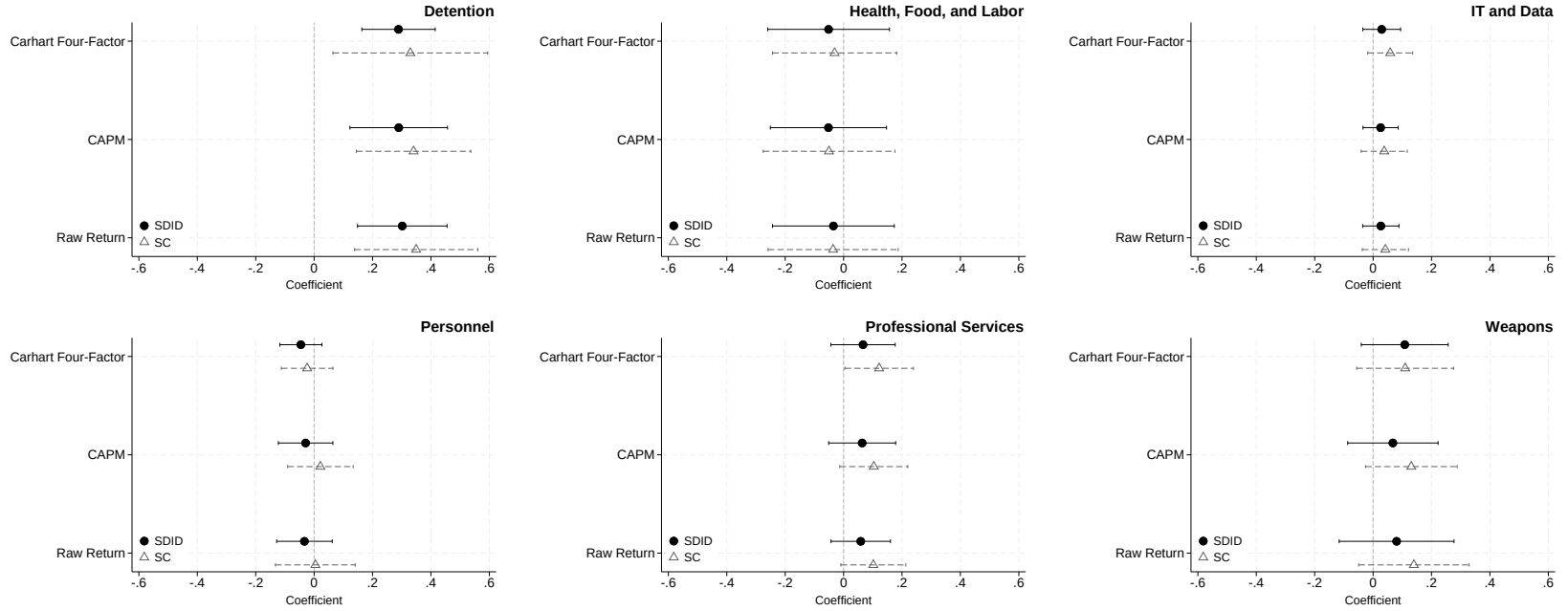
B Additional Figures and Tables

Figure A.1: Impact of 2024 Election on ICE Contractors' Stock Returns
Carhart Four-Factor Model vs. Raw Returns and CAPM



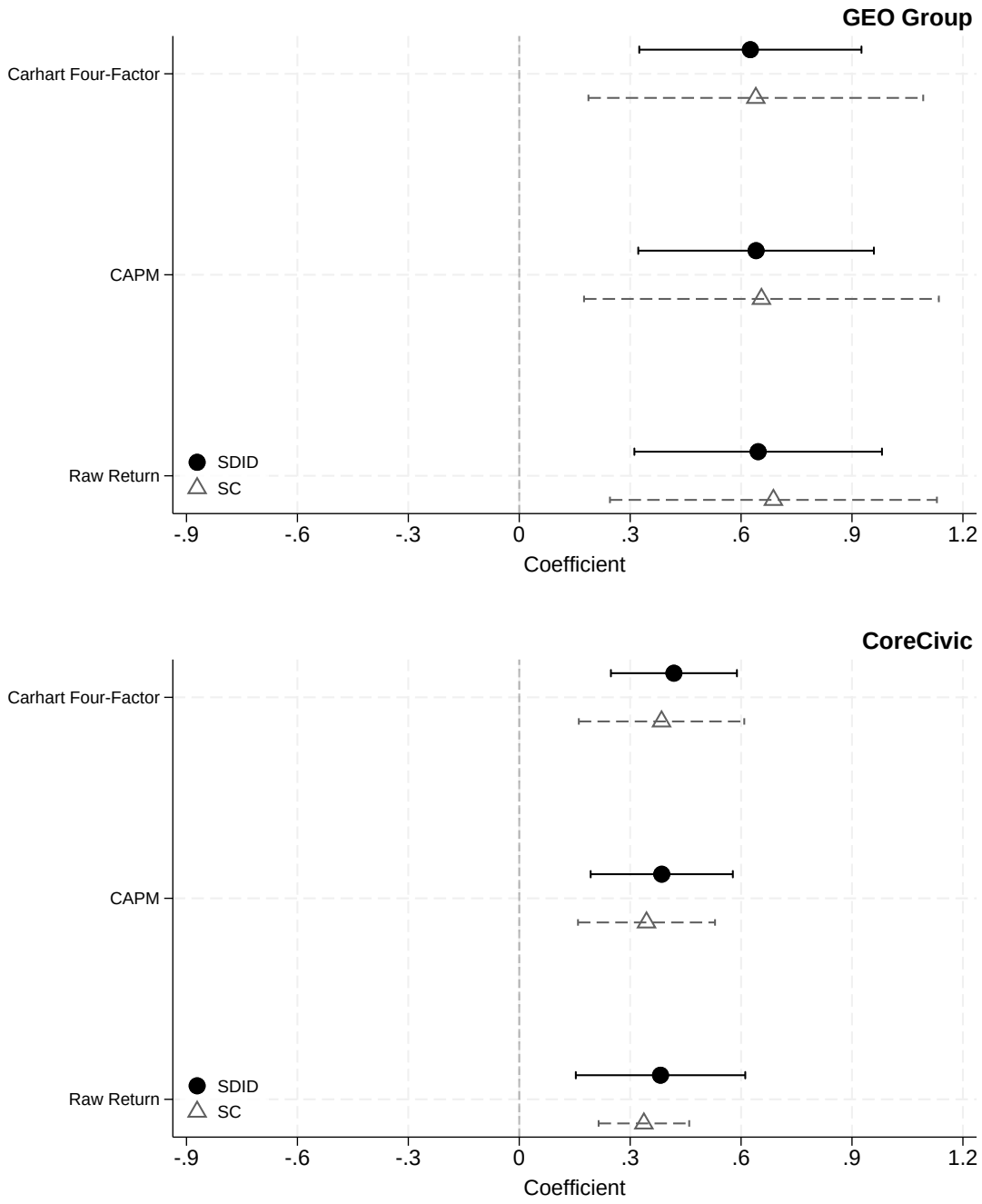
Notes: This figure shows the daily effect of the 2024 US presidential election on cumulative abnormal returns (CARs) for ICE contractors. The solid line plots observed CARs; the dashed lines plot counterfactual CAR paths constructed by synthetic difference-in-differences (SDID) and synthetic control (SC). The vertical dashed line marks the first trading day after the election. The SDID and SC post-election estimates are reported in the figure, with standard errors in parentheses. The CARs are computed as the cumulative sum of daily abnormal returns from one quarter before the election. Results are shown for three return models: the Carhart four-factor model (main specification), the capital asset pricing model (CAPM), and the raw returns benchmark. For the Carhart and CAPM models, abnormal returns are estimated over a 252-trading-day estimation window ending 30 trading days prior to the election.

Figure A.2: Impact of 2024 Election on Stock Returns by ICE Contract Category
Carhart Four-Factor Model vs. Raw Returns and CAPM



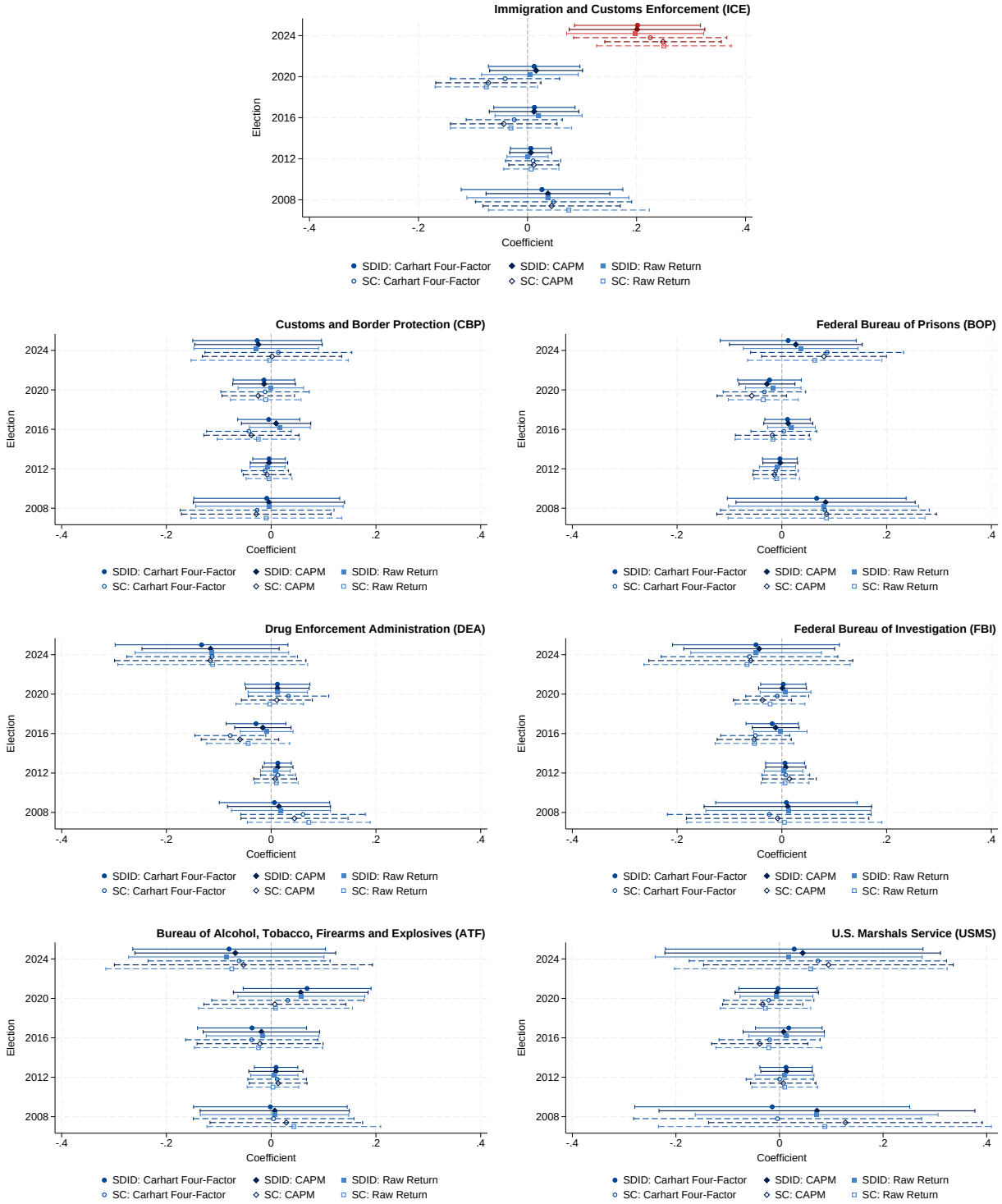
Notes: This figure reports synthetic difference-in-differences (SDID) and synthetic control (SC) estimates of the effect of the 2024 US presidential election on cumulative abnormal returns (CARs) for firms with ICE contracts, separately by contract category: detention, health/food/labor, information technology (IT) and data, personnel, professional services, and weapons. Categories are defined by product/service code (PSC) classification. Circles denote the SDID estimates, and triangles denote the SC estimates. Horizontal lines represent 95% confidence intervals based on placebo standard errors. Results are shown for three return models: the Carhart four-factor model (main specification), capital asset pricing model (CAPM), and raw returns benchmark. The CARs are computed as the cumulative sum of daily abnormal returns over 63 trading days from one quarter before the election. For the Carhart and CAPM models, abnormal returns are estimated over a 252-trading-day window ending 30 days prior to the election.

Figure A.3: Impact of 2024 Election on GEO Group and CoreCivic Stock Returns
Carhart Four-Factor Model vs Raw Returns and CAPM



Notes: This figure reports synthetic difference-in-differences (SDID) and synthetic control (SC) estimates of the effect of the 2024 US presidential election on cumulative abnormal returns (CARs) for GEO Group and CoreCivic. Circles denote the SDID estimates, and triangles denote the SC estimates. Horizontal lines represent 95% confidence intervals based on placebo standard errors. Results are shown for three return models: the Carhart four-factor model (main specification), capital asset pricing model (CAPM), and raw returns benchmark. The CARs are computed as the cumulative sum of daily abnormal returns over 63 trading days from one quarter before the election. For the Carhart and CAPM models, abnormal returns are estimated over a 252-trading-day window ending 30 days prior to the election.

Figure A.4: Estimates of Election Effects on CARs
Carhart Four-Factor Model vs. Raw Returns and CAPM



Notes: This figure reports the synthetic difference-in-differences (SDID) and synthetic control (SC) treatment effect estimates on cumulative abnormal returns (CARs) of publicly traded firms contracting with each federal agency, separately for each presidential election (2008–2024). Circles denote the SDID estimates, and triangles denote the SC estimates. Horizontal lines represent 95% confidence intervals based on placebo standard errors. The CARs are computed as the sum of daily abnormal returns over 63 trading days from one quarter before the election. Results are shown for three return models: the Carhart four-factor model (main specification), captial asset pricing model (CAPM), and raw returns benchmark. The estimation window is 252 trading days ending 30 days before the event for the Carhart and CAPM models. The ICE 2024 estimate is highlighted in red to denote the main specification.

Table A.1: First-Stage Estimates Across Baseline 2SLS Specifications

	(1)	(2)	(3)
Shift-Share Instrument (Std.)	0.0554***	0.0554***	0.0570***
	(0.0100)	(0.0100)	(0.0089)
F-stat	30.80	30.46	41.38
Observations	145,745	145,745	145,745
County Fixed Effects	Yes	Yes	Yes
Year–Month Fixed Effects	Yes	Yes	No
State–Year–Month Fixed Effects	No	No	Yes
Local Controls	No	Yes	Yes

Notes: Each column reports the OLS first-stage regression of the standardized procurement obligation per baseline detainee on the standardized shift-share instrument, corresponding to the first stage of the 2SLS specifications in columns (4)–(6) of Table 6. Column (1) includes county and year–month fixed effects without local controls. Column (2) adds county unemployment rate, log population, poverty rate, and median household income. Column (3) replaces year–month fixed effects with state-by-year–month fixed effects and includes local controls. All columns use the same county–month analysis sample of 145,745 observations. Standard errors are clustered at the county level. $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.

Table A.2: Effect of ICE-Only Procurement on Total Detainees (2SLS)

	(1)	(2)	(3)
Obligation Per Detainee (Std.)	2.208***	2.229***	2.199***
	(0.621)	(0.618)	(0.597)
Pre-2016 Mean	8.417	8.417	8.417
F-stat	17.78	17.67	20.88
Observations	145,745	145,745	145,745
Estimator	2SLS	2SLS	2SLS
County Fixed Effects	Yes	Yes	Yes
Year–Month Fixed Effects	Yes	Yes	No
State–Year–Month Fixed Effects	No	No	Yes
Local Controls	No	Yes	Yes

Notes: This table replicates columns (4)–(6) of Table 6 restricting the treatment and instrument to ICE-only procurement obligations, excluding all Bureau of Prisons (BOP) detention-related contracts. The treatment is county-level ICE procurement obligations by place of performance divided by the county's 2011–2015 average monthly detainee count, standardized to unit variance. The shift-share instrument is constructed using ICE-only baseline exposure shares and ICE-only national shifters. Column (1) includes county and year–month fixed effects without local controls. Column (2) adds county unemployment rate, log population, poverty rate, and median household income. Column (3) replaces year–month fixed effects with state-by-year–month fixed effects and includes local controls. All columns use the same county–month analysis sample of 145,745 observations. Standard errors are clustered at the county level. $^{*}\ p < 0.10,\ ^{**}\ p < 0.05,\ ^{***}\ p < 0.01$.

Table A.3: Effect of Procurement on Total Detainees: Post-2014 Sample (2SLS)

	(4)	(5)	(6)
Obligation Per Detainee (Std.)	2.115***	2.136***	2.297***
	(0.456)	(0.449)	(0.451)
Pre-2016 Mean	6.628	6.628	6.628
Observations	81,412	81,412	81,412
Estimator	2SLS	2SLS	2SLS
County Fixed Effects	Yes	Yes	Yes
Year–Month Fixed Effects	Yes	Yes	No
State–Year–Month Fixed Effects	No	No	Yes
Local Controls	No	Yes	Yes

Notes: This table replicates columns (4)–(6) of Table 6 restricting the treatment and instrument to ICE-only procurement obligations, excluding all Bureau of Prisons (BOP) detention-related contracts. The treatment is county-level ICE procurement obligations by place of performance divided by the county's 2011–2015 average monthly detainee count, standardized to unit variance. The shift-share instrument is constructed using ICE-only baseline exposure shares and ICE-only national shifters. Column (1) includes county and year–month fixed effects without local controls. Column (2) adds county unemployment rate, log population, poverty rate, and median household income. Column (3) replaces year–month fixed effects with state-by-year–month fixed effects and includes local controls. All columns use the same county–month analysis sample of 145,745 observations. Standard errors are clustered at the county level. $^{*}\ p < 0.10,\ ^{**}\ p < 0.05,\ ^{***}\ p < 0.01$.

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