Gun policy and the steel paradox

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Abstract

Using Measure 114's narrow passage in Oregon as a natural experiment, we study how new gun regulations affect firearm demand. Background checks, a proxy for demand, rose 13.9% in anticipation of the referendum and surged 157% immediately following the election. After judicial intervention halted the law's enactment, demand returned near pre-election levels. Temporal displacement/harvesting does not explain the demand spike: after eighteen months, we still observe a substantial cumulative increase of 63,000 excess firearm-related background checks. Administrative data reveal significant county-level heterogeneity. This evidence underscores the paradoxical effect of gun-control policies, offering a cautionary lesson to policymakers.

Keywords: Guns, Gun Control, Gun Policy, Loss Aversion

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1 Introduction

The United States leads high-income countries in gun deaths (The Global Burden of Disease 2016 Injury Collaborators, 2018). Over the past twenty years, gun fatalities have increased (Centers for Disease Control and Prevention, National Center for Health Statistics, 2021), peaking at 48,830 deaths in 2021 (Gramlich, 2023). Amidst this widespread gun violence, recurrent mass shootings and high-profile gun accidents repeatedly reignite firearm policy debates. In recent decades, these debates translated into few federal gun law changes. However, numerous state-level policy changes affected access to and legal uses of firearms during this time (Cheng and Hoekstra, 2013; McClellan and Tekin, 2017). In 2022, a public referendum in Oregon provided voters the choice to strengthen gun control throughout the state. Measure 114 promised background checks for all gun sales, restrictions on magazine capacities, and a new permit-to-purchase program. The referendum narrowly passed but was never implemented—becoming inextricably tangled in judicial stays. This unique setting allows us to estimate intertemporal dynamics of firearm demand in the presence of salient future restrictions.

Proponents of Measure 114 intended to reduce the number of firearms in Oregon, subsequently reducing firearm-related accidents and violence. On the surface, this reasoning is plausible: restricted access to firearms may well reduce the number of firearms (and firearms-related deaths). Koenig and Schindler (Koenig and Schindler) find that firearm purchase delay laws reduce handgun purchases and, subsequently, homicides. Both Webster et al. (2014) and Williams Jr (2020) find evidence that the repeal of Missouri's permit-to-purchase program led to more gun purchases and additional gun homicides. Likewise, there is evidence that firearm homicides decreased following the passage of a permit-to-purchase restriction in Connecticut (Rudolph et al., 2015). Complementary work by Crifasi et al. (2015) shows permit-to-purchase programs reduced firearm suicides in both Connecticut and Missouri. In short, enacting permit-to-purchase programs in the past led to reductions in gun ownership and violence; expiration increased ownership and deaths.[1]

[1] More broadly, studies show that higher firearm availability leads to more crime: Duggan (2001) and Cook and Ludwig (2006) find several proxies for gun ownership are associated with increases in homicide risk and Billings (2023) shows increases in local violent crime rates and property theft in regions with more firearm background checks following national high-profile mass shootings. Increases in firearm thefts are particularly prominent, suggesting that guns themselves are often targets of

Yet, while this literature would predict that permit-to-purchase provisions reduce gun deaths and accidents *in the long run*, if individuals anticipate these laws—and increase gun purchases—such benefits may be attenuated or delayed. This anticipatory behavior appears in many other contexts. Becker et al. (1994) find that while smokers reduce their smoking behavior when they anticipate future tax hikes, they also stockpile cigarettes to avoid future taxes. Labor economists refer to similar anticipatory behavior in program participation as *Ashenfelter dips* (Ashenfelter and Card, 1985), and environmental economists refer to *Green* and *Blue Paradoxes* in climate change policy (Sinn, 2012; Jensen et al., 2015) and marine reserves (McDermott et al., 2018). A long literature in macroeconomics highlights the intertemporal dynamics of durable good consumption when consumers expect future price or credit changes (Ogaki and Reinhart, 1998; Gowrisankaran and Rysman, 2012; Gavazza and Lanteri, 2021). We find the high-salience threat of Measure 114's implementation induced a 157-percent increase in anticipatory gun sales.

Accordingly, we contribute to a growing body of literature that suggests the existence of a *Steel Paradox*. In the long run, substantive restrictions like permit-to-purchase programs may increase public safety by reducing the number of guns in circulation or preventing "dangerous" purchases. However, the passage of such programs may spawn short-run anticipation effects. Unlike cigarettes, even short-run anticipatory purchases of durable goods like firearms may elevate the stock of the good for decades. Firearm demand may even be particularly prone to anticipatory shifts; both President Obama's 2008 election (Depetris-Chauvin, 2015) and the Sandy Hook mass shooting (Levine and McKnight, 2017) increased firearm demand, despite no corresponding new federal gun control regulations and little change in state-level laws. While firearm policy may reduce long-run sales, short-run effects warrant key consideration for policy in the United States.

Whereas Depetris-Chauvin (2015) and Levine and McKnight (2017) show *perceived* threat of potential new gun restrictions can spur sales, we study the effect of an *actual* looming restriction. Moreover, because our restrictions are limited to a particular geography, we can (1) identify the precise timing of

crime—rather than deterrents.

anticipatory responses using high-frequency administrative data from the affected geography, distinguishing between pre-election and pre-implementation anticipatory effects, and (2) disentangle longer-run responses using unaffected states as credible longer-run counterfactuals.

To estimate short-run and medium-run effects, we use background-check data from the FBI's National Instant Background Check System (NICS) records and from the Oregon State Police (OSP). Using a synthetic difference-in-differences approach (Arkhangelsky et al., 2021) at the month-by-state level, we find significant evidence of strong anticipation effects immediately after the passage of Oregon's Measure 114. We also provide new evidence of the precise temporal and spatial patterns of Measure 114's effects using daily county-level data from OSP. We find that background checks gradually increased in the weeks preceding the election and then immediately and substantially surged after the outcome of the vote was known. This surge subsided after a judge halted the implementation of the new law. In absolute magnitudes, the anticipation we observe for Oregon in the month of the election is over six times larger than we observe for Obama's election and over three times larger than Sandy Hook. We also examine the heterogeneous anticipation effects of Measure 114 across Oregon counties. Roughly equal shares of the increase in firearm sales can be attributed to counties that voted for or against the measure. However, the per capita effect is roughly fifty percent larger for counties that largely did not support the measure. In counties most strongly opposed to Measure 114, we observe multiple weeks in which the number of background checks translates to approximately 1% of the local population purchasing a firearm.

Finally, Measure 114 provides a unique experiment for decomposing elements of firearm demand due to the fact that Oregon never implemented the policy. This failure to implement allows us to differentiate between (a) increased overall firearm demand and (b) *harvesting effects* through which consumers temporally accelerated planned firearm purchases. We do not find strong evidence of an eventual decrease after the initial surge in demand for firearms, suggesting that the demand for firearms differs from other durable goods (Gowrisankaran and Rysman, 2012; Gavazza and Lanteri, 2021; Moshary et al., 2023).

2 Background

2.1 Firearm laws in the United States

Within the United States, firearms are regulated at both the federal and state levels. Since the National Firearms Act of 1934, expansion of federal regulation has typically been rare and modest—and often catalyzed by high-profile firearm-related incidents or rising crime rates. Prior to the 2022 passage of the Bipartisan Safer Communities Act, the most recent national-level firearm regulation policies were the Federal Assault Weapon Ban (1994–2004) and the Brady Handgun Violence Prevention Act (1993) (Vizzard, 2015). The Brady Act requires that a background check be performed through the FBI's National Instant Criminal Background System (NICS) for all purchases from a federal firearms licensee (FFL), manufacturer, or importer.

In contrast to the limited changes in federal regulation, numerous states introduced or modified state-level firearm laws over the past 20 years. Individual states commonly modify requirements around the federal background check mandate. For example, states with Universal Background Check laws extend the national background check requirements to include sales and transfers by private parties and at gun shows. States may also implement additional firearm permitting requirements, restrictions on the types of guns that can be manufactured or sold, and where/how they can be carried. Other states relaxed firearm restrictions during this period. As of 2024, twenty-one states have expanded background check laws beyond the federal mandate; fourteen of these states require a background check at the point of sale for all firearm classes. Fourteen states have banned or restricted possession of high-capacity magazines. While twelve states require a permit to purchase all or some classes of firearms, Missouri (2007) and North Carolina (2023) repealed existing permit-to-purchase laws in recent decades (Everytown Research & Policy, 2024c,b,a).

2.2 Firearm reform in Oregon

Between 2015 and 2021, Oregon gradually strengthened its firearm restrictions—implementing laws intended to enhance safety across a broad set of gun owners[2] and to restrict firearm access for high-risk users.[3] Everytown, a nonprofit organization that evaluates states on their gun laws, ranked Oregon the 11th safest state in the U.S. in Februray 2022 (Everytown Research & Policy, 2022). In November 2022, Oregon voters narrowly passed Measure 114, which aimed to increase barriers to access for potential gun owners. The measure required prospective firearm purchasers to complete a firearms safety course and purchase a permit from local law enforcement; permits required renewal after five years. Measure 114 also granted greater latitude for permit denial due to psychological concerns and criminalized possession of magazines capable of holding more than ten rounds of ammunition.[4]

Measure 114's passage surprised many Oregonians. The initiative originated with a small, grassroots organization comprised of local religious leaders called Lift Every Voice Oregon (LEVO). LEVO—founded in 2018 in response to the Parkland shooting—had little political success before acquiring the signatures needed to include Measure 114 on the November 2022 ballot (Lift Every Voice Oregon, 2024). This, too, appeared unlikely: Oregon law required 112,020 signatures in early July to place the measure on ballots, which volunteers obtained only two weeks before the deadline after the tragedy at Uvalde heightened interest in gun control policies (Tim, 2022).[5]

On the left side of Figure 1, weekly Google Trends data depict the rapid increase in searches related to firearm policy around Uvalde, but this level of interest was fleeting.[6] Search terms such as "2nd amendment", "gun safety", and "gun" peaked during the week in which the Uvalde shooting occurred. However, by July, these terms fell to routine levels. Even as the 2022 election approached—and with it, the opportunity to directly vote on gun policy—Google Trends suggest Oregonians had little interest

[2] E.g., expanding background check data infrastructure and mandating safe firearm storage practices.
[3] Oregon implemented a "red flag" law that extended gun restrictions to those under a restraining order or convicted of stalking.
[4] The passage of Measure 114 led to Everytown re-ranking Oregon as the 9th safest state in 2023 (Everytown Research & Policy, 2023).
[5] In May 2022, twenty-one people were killed in a school shooting at an elementary school in Uvalde, Texas. The shooting and its aftermath were highly publicized and influenced firearm policy discussions (Tim, 2022).
[6] Google Trends data depict relative search interest normalized against the period's peak interest for the given term.

in these topics or "Measure 114" until immediately prior to the election. The right side of Figure 1 illustrates Google Trends results for Measure 114 and several terms related to the measure (gun stores, background checks, and magazines). Search interest in Measure 114 began to rise a month before the 2022 election and peaked during the week of the election—as did interest in the topics that Measure 114 proposed to measure: background checks and magazines.

In the final weeks before the election, Measure 114 rapidly became highly salient and controversial, passing with only 50.7% of the vote (Ballotpedia, 2022). Typically, Oregon referenda become effective 30 days after passage. During this pre-implementation period, gun advocates challenged the constitutionality of Measure 114 in court and some local sheriffs announced they would not enforce the law. Google Trends data show elevated interest in Measure 114 through its scheduled implementation week and then waning when it was suspended pending litigation. As of 2024, Measure 114 has never been substantively implemented.

Thus, in focusing on Oregon's Measure 114, we are able to study the effects of a highly salient threat to firearm access that occurred absent of any complicating direct effects from the actual policy change. The Oregon context may be particularly relevant to understanding these anticipatory effects: the measure appeared in a referendum, so all voters became aware of it. Further, because Oregon votes by mail, all registered voters received information about the proposed regulation leading up to election day. This referendum format for changing gun policy presents meaningful differences from both permit-to-purchase and broader regulation that has been studied previously. Ballotpedia (2024) shows only ten states for which any form of gun law appeared directly on the ballot between 2000 and 2023—mostly as referenda to reinforce the "Right to Bear Arms." Oregon was the only state during this time that directly voted on permit-to-purchase laws. However, other states may follow suit; in 2024, citizens of seven states unsuccessfully attempted to place gun policy directly on the ballot (Chan, 2024).

3 Data and methods

3.1 Data

Measuring firearm ownership To test for the effects of the Measure 114 ballot initiative on firearm demand, we use background checks to proxy for legal firearm purchases. Direct firearm ownership data are scarce and largely inaccessible to researchers. No central database on ownership exists, and alternatives used to quantify purchases of other durable goods—such as the Nielsen Consumer Panel or large-scale surveys—are uniquely ill-suited for use in firearm research due to sensitivity around gun-ownership status.[7] In contrast, the FBI's monthly summaries of state-level background checks for firearms are widely available and frequently used by researchers investigating firearm-purchasing behaviors. We supplement these data with an uncommonly spatially and temporally detailed dataset obtained from the Oregon State Police. We describe each dataset in detail below.

FBI NICS data Federal law requires FFLs to submit background checks before selling firearms (U.S. Congress, 1993). FFLs, or government agencies on their behalf, conduct these background checks through the FBI's National Instant Criminal Background Check System (NICS) (Federal Bureau of Investigation, 2024). The FBI releases state-level monthly aggregates of total firearm-related background checks across numerous categories that summarize the purpose of the check. These purposes may include permit checks, checks for private (non-FFL) sales, or checks across broadly defined weapon categories (e.g., handgun, long gun). We obtained these data for January 2000–March 2024 by scraping the tables in the FBI's monthly NICS PDFs following Singer-Vine (2023).

While federal law mandates background checks for FFLs, states have latitude to extend this requirement to additional sales or to exempt certain transactions—for example, allowing permits to replace transaction-linked background checks. Thus, NICS background check data are most appropriate for testing for relative changes in firearm transactions rather than comparing levels of transactions across

[7] E.g., the Nielsen Consumer Panel censors firearm purchases. Recent research also suggests respondent selection bias for firearms is increasing over time Urbatsch (2019).

states. In states like Oregon, which require background checks for all firearm sales, background check data are more likely to correspond to levels of transactions. Recent work validating the NICS proxy in Massachusetts, another state with universal background checks, shows a close relationship in both levels and changes between firearm transactions and NICS data (Armona and Rosenberg, 2024).

States may also choose whether to centralize NICS background checks through a state office or to have FFLs interface directly with the FBI's systems. States that centralize this process are designated "point of contact" (POC) states. This choice can affect NICS processing: delays within POC agencies can result in duplicate records within the NICS system. Such was the case in Oregon, where the rapid increase in firearm demand caused by the passage of Measure 114 exceeded state capacities to process background checks. For this reason, we supplement NICS data with background check data obtained directly from the Oregon State Police, which we describe below.[8]

OSP data In Oregon, the Oregon State Police (OSP) serve as the POC with NICS. Practically, this designation means that gun sellers interact with OSP, who query NICS in the course of conducting a more detailed state background check. We obtained daily county-level counts of state background checks from OSP for March 2018–March 2024. Counties are defined by the location of the firearm seller.

In Oregon, NICS and OSP data are closely aligned until Measure 114 passed, at which time the two data sets briefly but substantially decouple. This deviation occurred because background checks left pending for more than 24 hours were resubmitted to NICS—but were not duplicated within OSP systems. This decoupling reflects administrative capacity constraint amidst the extreme levels of demand in Oregon from Measure 114. We do not observe such a divergence during the COVID gun buying spike. Using publicly available data released by three other POC states, we confirmed this decoupling was unique to Oregon and not reflective of national delays in NICS processing during this time period.[9] As a result, we use OSP data aggregated by month in place of NICS data for Oregon in our cross-state analyses. We also take advantage of the finer temporal and geographic detail in the OSP data for supplementary

[8] Appendix Figure A2 depicts the time-series of background checks per 100k residents for Oregon and the rest of the US.
[9] Appendix section *NICS appendix* further discusses this issue.

analyses.

Additional data We use several additional data sources for our analysis. First, we use population data from the Census for state populations and Oregon county populations to calculate changes in background checks per 100,000 residents (2011; 2019; 2020; 2023; 2024). We use data from the New York Times for Oregon's county-level 2022 election results The New York Times, 2022 and state-level POC and Brady Exempt status from the Bureau of Alcohol, Tobacco, and Firearms (2024).

Sample Because of the considerable variation in the sizes of state and county populations, which may be correlated with the propensity to own firearms, we measure background checks as a rate per capita. We use data from March 2018–March 2024, the period for which we observe OSP background checks. For Oregon, we all reported background checks in the OSP data. These closely align with the sum of the "handgun", "long gun", "multiple", and "other" NICS aggregates for the state. For all other states, we use the sum of non-private sales in these four categories in the NICS data as our measure for "standard sales" of firearms.

For county-level analyses, we group Oregon counties based on whether the majority of the county's votes opposed or supported Measure 114. Six counties voted in favor of the measure—jointly accounting for 46% of Oregon's population—and thirty opposed (The New York Times, 2022).

While we obtained NICS data for the contiguous United States, we exclude Alabama and North Carolina from all analyses due to changes in background-check requirements contemporaneous with our evaluation period. We also exclude states that border Oregon from our primary analyses to avoid potentially contaminated control units. In supplementary analyses, we demonstrate that our results are robust to instead excluding POC states, Brady-exempt states, or states that at any time implemented permit-to-purchase from the set of potential controls.

3.2 Methods

Our main objective is to identify Measure 114's impact on firearm-purchasing decisions in Oregon. To do so, we primarily use the *synthetic difference-in-differences* estimator[10] developed by Arkhangelsky et al. (2021), which estimates the average treatment effect on the treated (ATT) $\hat{\tau}$ by solving

$$\left(\hat{\tau}, \hat{\mu}, \hat{\alpha}, \hat{\beta}\right) = \underset{\tau,\mu,\alpha,\beta}{\operatorname{argmin}} \sum_{i=1}^{N} \sum_{t=1}^{T} \left(BGC_{it} - \mu - \alpha_i - \beta_t - \tau Trt_{it}\right)^2 \hat{w}_i \hat{\lambda}_t$$

where BGC_{it} denotes the total number of background checks (per 100,000 residents) in state i during month t; α_i and β_t reference unit and month-of-sample fixed effects; and Trt_{it} takes the value of 1 for Oregon starting in October 2022, and 0 in all other states and time periods. Donor-state and month-of-sample weights appear as $\hat{w}_i$ and $\hat{\lambda}_t$. We select October as the beginning of the treatment period as it allows us to capture the effects of changing expectations around a policy. For this reason, we are interested in separately quantifying both pre-election and post-election anticipation effects (anticipating the law and, separately, anticipating the election). Local news appears to have picked up the story of the pending referendum in October 2022, the month prior to the election, and Google Trends data (Figure 1) also suggest the initiative had relatively low salience until the month leading up to the election. Consequently, we consider October to be Oregon's first "treated" month. We estimate standard errors, under an assumption of homoskedasticity across states, using the placebo procedure described in Arkhangelsky et al. (2021).[11]

The synthetic difference-in-differences estimator (SDID) varies from the canonical difference-in-differences (DID) and synthetic control (SC) estimators in ways that advantage our analysis. Whereas DID analyses equally weight control units and time periods regardless of their similarity to the treated unit or time periods, SDID upweights units and periods whose outcomes resemble the treated unit and periods. While both DID and SDID rely on a parallel-trends assumption, the assumption is likely more plausible

[10] In the appendix, we also provide estimates from synthetic control and 'standard' difference-in-differences estimators. Differences across estimators are not meaningful.

[11] While the homoskedasticity assumption may not precisely hold in this context, in Appendix B we show that our inference is robust to alternative specifications including restricted control states (Table B2) and standardizing the outcome variable (Table B3).

with such weights. Further, Arkhangelsky et al. (2021) note that this systematic selection of weights mitigates the bias concerns identified by Roth (2022) that occur when researchers condition analyses on pre-trends tests. Figure 2 provides evidence of a strong pre-treatment match between the trends of Oregon and its SDID-based synthetic—supporting the plausiblity of the parallel-trends assumption in our setting.

In contrast, SC estimators vary unit weights—but not time weights—and typically do not account for level differences across units or periods (i.e., fixed effects). One common concern with SC estimators is that unit weights can be sparse. Appendix Figure A3 shows that a broader base of units contribute to our counterfactual Oregon in the SDID estimation than with SC—reducing concerns that idiosyncracies in a small subset of donor states could drive the results.[12]

We also calculate Measure 114's cumulative effect firearm demand in Oregon. To calculate the measure's accumulated effect, we sum the individual monthly SDID treatment-effect estimates ($\hat{\tau}_t = BGC_{Oregon,t} - \widehat{BGC}_{Oregon,t}$) across the treated months—following Ciccia (2024).

In addition to calculating an overall ATT, we consider heterogeneity in Measure 114's effect along two important dimensions: (1) temporal heterogeneity as a function of high-salience legal events and (2) geographic hetereogeneity related to local opposition to the measure.

To explore temporal heterogeneity, we use the SDID estimator to construct a plausible counterfactual for Oregon—again following Ciccia (2024). We then consider the implied treatment effect separately for each of four distinct time periods of interest. First, we calculate a pre-election October anticipation effect—restricting post-treatment data to only October 2022. Second, we calculate a post-election, "short-run" ATT using post-treatment data from November–December 2022.[13] Third, we calculate a "post-stay" effect using the first three months after the judicial stay (January–March 2023). Finally, our "long-run" ATT reflects only outcomes observed more than six months after treatment (April 2023 through the end of the sample in March 2024). Figure A2 supplements these SDID results with daily OSP

[12] Appendix Figure B1 compares results from SDID, DID, and SC estimators.
[13] Because the election was held on November 8th, and the judicial stay occurred on December 6 (and because NICS data follow calendar months), this short-run period includes one week before the election in November and several weeks post-stay.

data that demonstrate the precise timing of gun demand changes relative to election day, the intended implementation day, the judicial order that paused implementation, and major religous/commercial holidays. Together, these causal and descriptive approaches illustrate key temporal dynamics of gun demand induced by proposition-driven gun regulation.

We also decompose our initial pooled estimates to better understand the spatial dispersion of Measure 114's demand spike. For this task, we return to our initial SDID estimator but intead of considering Oregon as a single treated unit, we separate it into several individual treated units. We aggregate the county-level OSP data into two groups based on whether a majority of the votes cast within the county were in favor of or opposition to Measure 114.[14] Using SDID, we then use estimate the ATT for each of these two partitions of Oregon—illustrating how Meausure 114's demand shocks varied by local support for the proposition.

4 Results

4.1 State-level analyses

We begin with our results for Measure 114's effects—its appearance on the ballot and subsequent passage—on firearm demand, estimated via SDID. Panel a of Figure 2 plots monthly background checks (per 100k residents) in Oregon against the SDID-based synthetic Oregon. Background checks spiked during the month of the election—suggesting a large shift in firearm demand—and were elevated in prior and successive months (October and December). Table 1 presents the resulting estimates. Column 0 contains the average effect pooled across the entire treatment period (October 2022 to March 2024). Starting in October 2022, Measure 114 significantly increased the average number of monthly background checks by 82 per 100k residents—a 14-percent increase from Oregon's monthly average rate of 592 per 100k residents.[15] The cumulative effect of this surge in firearm demand is 1,481.9 additional background checks per 100k resident. This increase corresponds to more than 62,000

[14] In the appendix, we also separate counties into quartiles of support.
[15] This value (592) comes from the twelve month prior to treatment (Oct. 2021–Sept. 2022).

additional background checks.[16]

4.2 Temporal and cumulative effects

Columns 1–4 of Table 1 temporally decompose the pooled effect in column 0 by mutually exclusive time periods. Column 1 presents the pre-election anticipation effect: In the month before the election, background checks surged by 82 per 100k (a 14-percent increase). In November and December 2022 (column 2)—approximately spanning the election and judicial stay—Measure 114 increased monthly background checks by 930 per 100k, 157 percent over Oregon's baseline rate. Column 3 shows that in the three months after the judicial stay (January–March 2023), firearm demand in Oregon no longer significantly exceeded Oregon's expected trajectory: the point estimate (44 background checks per 100k) does not significantly differ from zero. Notably, this 'null' result rules out short-term harvesting effects as the main explanation underlying the immense surge in firearm demand estimated in columns 1–2.

The long-run effect in column 4 (−48.3 per 100k) rules out enduring large increases or decreases in firearm demand due to Measure 114—ruling out long-run increases above than 2% and reductions beyond 19% with 95% confidence. The point estimate and interval do not entirely rule out small harvesting effects—i.e., gun purchases moved forward in time due to Measure 114. However, the magnitude of the estimates suggests harvesting was not the main catalyst in Measure 114's surge in firearm demand: the negative long-run cumulative effect in column 4 accounts for only a quarter of Measure 114's accumulated demand estimated in columns 1–3.

Panel b of Figure 2 bears additional evidence of Measure 114's cumulative effects—plotting the cumulative sum of estimated treatment effects in post-treatment months. The figure highlights the clear surge in firearm demand around the election and prior to the judicial intervention. At its peak, Measure 114 had induced an excess of 2,130 background checks per 100k residents (enough for 2% of the Oreogn population to receive a firearm-related background check). After accounting potential harvesting 18 months after the election, this cumulative effect is approximately 1,482 per 100k. Again, these cumula-

[16] Oregon's population is approximately 4.2 million.

tive estimates are consistent with partial—but incomplete—harvesting: approximately 70 percent of the initial increase persisted eighteen months after Measure 114.

Finally, Figure 3 illustrates these firearm demand dynamics with higher temporal resolution. Specifically, we plot the daily number of firearm background checks in Oregon during five months surrounding the election. Vertical lines mark the beginning of our treatment period, election day, Black Friday, and the day of the judicial stay. In the weeks prior to the election, a clear upward trend in demand emerged, followed by an enormous jump the day after the election. Demand remained elevated for weeks—peaking on Black Friday—until the judicial stay prevented Measure 114 from being implemented. After the judicial order, firearm demand returned relatively quickly to its original trend.

4.3 County-level analyses

Local support of Measure 114 provides further insight into Measure 114's induced firearm demand—i.e., *who* drove these effects? To shed light on this question, we separately estimate ATTs (again via SDID) for two subsets of Oregon: (1) counties where the majority of the residents voted in favor of Measure 114 (six counties) and (2) counties where the majority voted against the new restrictions (30 counties). Figure A1 maps these subsets along with counties' support of the measure, populations, and firearm-related background checks. The results, in Figure 4, show that background checks increased in both sets of counties. However, in counties that opposed Measure 114, the spike in firearm demand is substantially larger—the effect (per 100k residents) is approximately 50% larger, peaking around 1,680 additional monthly background checks per 100k residents. In terms of cumulative effects: While counties that opposed the measure account for 54% of Oregonians, they accounted for 70% of the per-capita increase in firearm purchases.[17]

[17] We estimate a cumulative increase of 2,110 background checks per 100k Oregonians for counties against Measure 114 and 1,070 for counties in support of Measure 114. We separately estimate SDID models for the two groups of counties and then scale the effects by the groups' population shares.

4.4 Robustness

Appendix Tables B1 and B2 demonstrate our results are robust across alternative empirical strategies (DID and SC) and various sets of potential control states. Table B1 reveal our SDID-based estimates are quite similar to synthetic-control and difference-in-differences estimates and generally sit between the SC and DID estimates. Our main specification excludes states that border Oregon—and two states that changed firearm background policy around Measure 114 (Alabama and North Carolina). Table B2 shows changing the set of potential control state does not meaningfully affect our results (all columns exclude Alabama and North Carolina). Column 1 provides our main specification (i.e., excluding states that border Oregon); column 2 excludes "point-of-contact" states; column 3 excludes states with permit-to-purchase laws; and column 4 excludes states with Brady-Act exemptions. Across these four different sets of potential controls, the estimates of Measure 114's impact on firearm demand are nearly identically—despite the number of potential donors ranging 24–41 and the implied changing donor weights. Finally, in Appendix Figure A4, we conduct a placebo test in which the placebo treatment starts one year early and ends at the start of the actual treatment period (running October 2021–September 2022). Our empirical strategy yields a null result—Oregon and its synthetic follow nearly identical paths. Together, these exercises suggest our effects are quite robust to alternative researcher decisions and any idiosyncracies specific to Oregon's seasonality.

5 Conclusion

Our results document that Oregon's gun-control public referendum, Measure 114, induced substantial increases in firearms background checks throughout the state. This effect was most pronounced between the election—when voters narrowly approved the measure—and the judicial stay that indefinitely paused the measure. During this period, we estimate Measure 114 induced 78,000 additional firearm-related background checks—sufficient for 1.9% of Oregon's residents to have purchased a gun in a two-month period—in a state where an estimated 40% of residents live in a household with a firearm (Schell

et al., 2020). After the judicial stay, monthly background-check rates subsided substantially in the following months. We document some evidence consistent with a small degree of harvesting in the final 12 months of observation: intertemporal consumption dynamics explain less than 20 percent of the induced demand. Eighteen months after Measure 114 effects' began, we estimate a persistent cumulative increase of approximately 63,000 background checks throughout Oregon. Accordingly, Measure 114's threat of gun control induced substantial firearm purchases unexplained by temporal harvesting.

Measure 114's impacts varied throughout Oregon. The measure was a highly contested and passed with less than 51% of the popular vote—largely due to support from Oregon's urban areas. We find considerably stronger demand effects in counties where the majority of voters opposed Measure 114. This dynamic is consistent with work outside of economics that explores the relationship between political ideology and firearm ownership—(e.g., Burton et al., 2021; García-Montoya et al., 2022).

Measure 114 yielded a singularly large shock to firearm demand—both in absolute terms and relative to previous demand-inducing events (e.g., President Obama's election and recent mass shootings).[18] Earlier demand spikes resulted from a combination of (a) raised expectations for firearm policy change and (b) heightened concerns about personal security/defense. Measure 114's unprecedented spike notably resulted from key factors related to firearm access—without coinciding safety concerns. First, Measure 114 was highly salient. As a public referendum, all registered Oregon voters (88% of Oregonians above 18) received voter pamphlets and thus were (in theory) informed (Oregon Secretary of State Elections Division, 2022). Second, Measure 114 passed. This passage signaled an approaching enactment, unlike previous more speculative events, such as President Obama's election. Third, Measure 114 passed with support from less than 51% of voters—in a sense, maximizing the potential for anticipatory effects. Finally, Oregon law requires 30 days before approved referenda take effect. This implementation period provided ample time for anticipatory effects—e.g., the sustained increases in firearm demand we observe. The coincidence of Measure 114's high salience, probable enactment, strong opposition, and

[18] Appendix Figure A2 contextualizes Measure 114's effect, providing Oregon's and other states' demand (per 100k) time-series during Measure 114 to earlier shocks to national firearm demand (Depetris-Chauvin, 2015; Levine and McKnight, 2017). The non-Oregon time-series aggregates NICS data from the contiguous US (excluding Alabama and North Carolina). Oregon's series use NICS data from January 2005–February 2018 and OSP data thereafter.

long implementation lag likely explain the effect's extraordinary magnitude.

Accordingly, the specifics of Measure 114's context contrast its effect with other studies that show a *Steel Paradox* effect—e.g., Balakrishna and Wilbur (2022)'s evaluation of Massachusetts's assault-weapons ban. Although both studies examine responses to substantive gun regulation, Massachusetts's ban allowed little potential for lasting anticipatory effects. Massachusetts's ban went into effect within 24 hours of the state's announcement, and the "announcement was widely seen as a surprise. It was not preceded by public comment or debate" (Balakrishna and Wilbur, 2022). While Balakrishna and Wilbur observe a sharp spike in gun demand in the 24 hours between the announcement and enactment, the cumulative increase in gun sales was much smaller than Measure 114's effect—both per capita and in total.

Measure 114's demand effects and eventual fate provide important insights and considerations for future policy and research. To date, public-referenda research typically focuses on property taxes and public schooling,[19] contexts that differ substantially from firearms regulation on several key dimensions—especially anticipatory effects. Within gun policy in the US, firearm violence is now considered a public health crisis (Braga, 2022), and support for firearms control has increased, but legislative changes have been limited (Crifasi et al., 2019). In such environments, referenda like Measure 114 may appear to present the best path forward for regulation advocates. However, we find this public referendum, effectively decided by the median voter, led to massive increases in gun sales—the opposite effect intended by the measure's authors. Had Measure 114 gone into effect at the end of our sample (April 2024), we estimate it would take more than a year for the restrictions to negate the measure's anticipatory effects.[20] Paradoxically, while relaxing firearm controls may increase firearm sales, so may tightening controls—at least in the short run—due to anticipatory effects. Measure 114 thus stands out as a cautionary tale for policymakers: firearm-control laws may actually increase firearm access when passage and enforcement are sufficiently salient and slow.

[19] See Brunner and Ross (2010); Glomm et al. (2011); Brunner et al. (2015).
[20] Appendix section *Counterfactual calculation* explains this calculation and provides alternative counterfactuals.

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6 Tables

	Pooled	*Decomposed*			
		Pre-114	*Immediate*	*Post-stay*	*Long-run*
	(0)	(1)	(2)	(3)	(4)
Monthly effect	82.3	82.4	929.7	44.3	−48.3
	(28.5)	(23.3)	(36.1)	(36.7)	(31.9)
Accum. effect	1,481.9	82.4	1,859.5	133.0	−580.0
	(512.6)	(23.3)	(72.2)	(110.0)	(383.3)
Trt. period	Oct. '22–Mar. '24	Oct. '22	Nov. '22–Dec. '22	Jan. '23–Mar. '23	Apr. '23–Mar. '24

Table 1: Measure 114's increased firearm background checks beyond temporal harvesting. This table compares the *Pooled* treatment effect (column 0)—i.e., averaging across all post-treatment months (Oct. 2022–Mar. 2024)—to treatment effects in mutually exclusive temporal subsets. The temporal subsets are (1) the month prior to Measure 114 (Sept. 2022); (2) the month of the election and the following month (Nov.–Dec. 2022); (3) the three months following the judicial stay (Jan.–Mar. 2023); and (4) the remainder of the treatment period (Apr. 2023–Mar. 2024). In Jan.- Sep. 2022, prior to Measure 114, Oregon averaged 592 monthly background checks (per 100k residents). In addition to the estimated treatment effect on monthly background checks and the (placebo-based) standard errors, we provide the implied accumulated effect—the excess firearm background checks due to Measure 114.

7 Figures

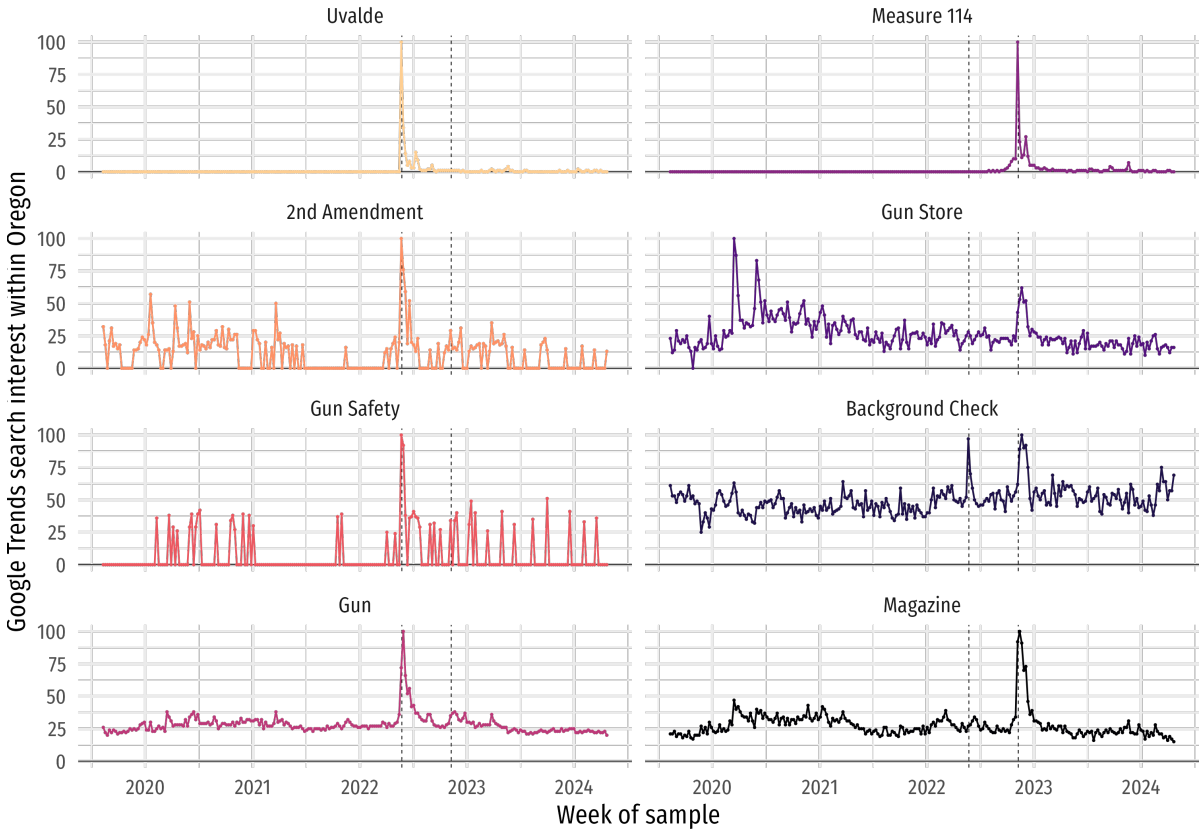
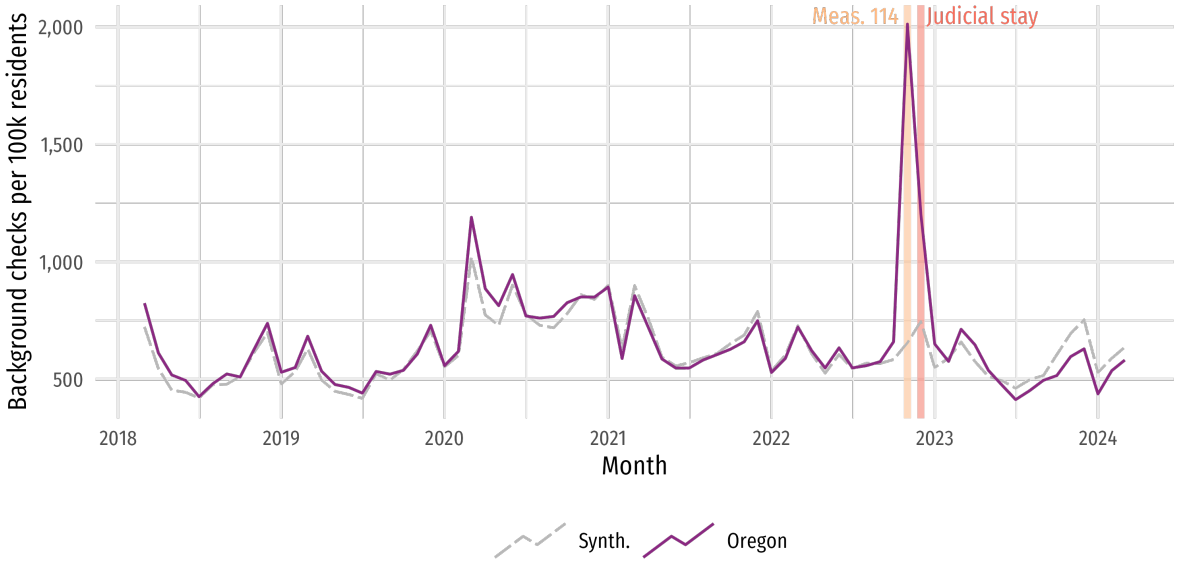


Figure 1: Firearms-related Google searches in Oregon spiked around the Uvalde schooting and again around Measure 114. Each subfigure presents a time-series of weekly interest (0–100; via *Google Trends*) in the stated search topic. The first dashed vertical line marks the Uvalde school shooting (24 May 2022); the second dashed line denotes the election in which Measure 114 was on Oregon's ballot (08 Nov. 2022). The left column provides topics related to the Uvalde shooting and subsequent discussions around gun safety. The column on the right presents topics related regulations in Measure 114.

(a) Effect of Measure 114 on firearm background checks



(b) Cumulative effect of Measure 114 on firearm background checks

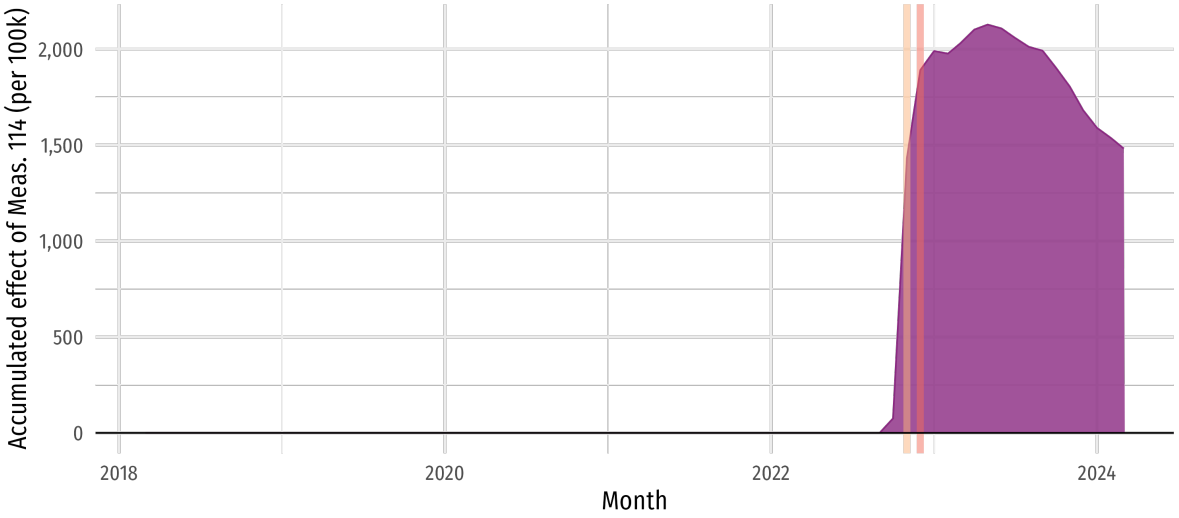


Figure 2: Measure 114 induced substantial gun demand Oregon beyond short-run harvesting. This figure visualizes the estimated effect of Measure 114 on Oregon firearms background checks (shown in Table 1). The two time-series in **Panel a** present monthly background checks per 100k for Oregon (purple) and *synthetic* Oregon (based upon the SDID projection). The vertical orange line marks the date of Measure 114 (08 Nov. 2022), while the red line shows the judicial stay (06 Dec. 2022). Treatment begins October 2022. **Panel b** plots the accumulated background checks induced by Measure 114—i.e., summing the monthly difference between Oregon and synthetic Oregon, beginning in October 2022.

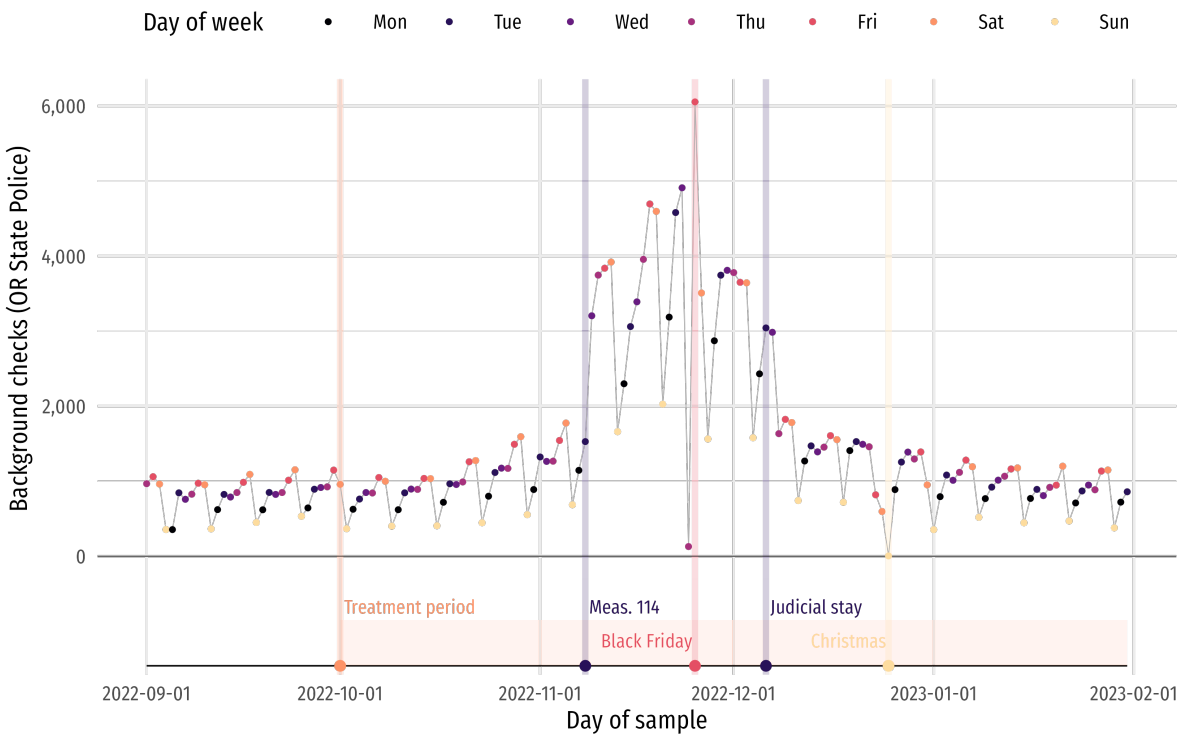
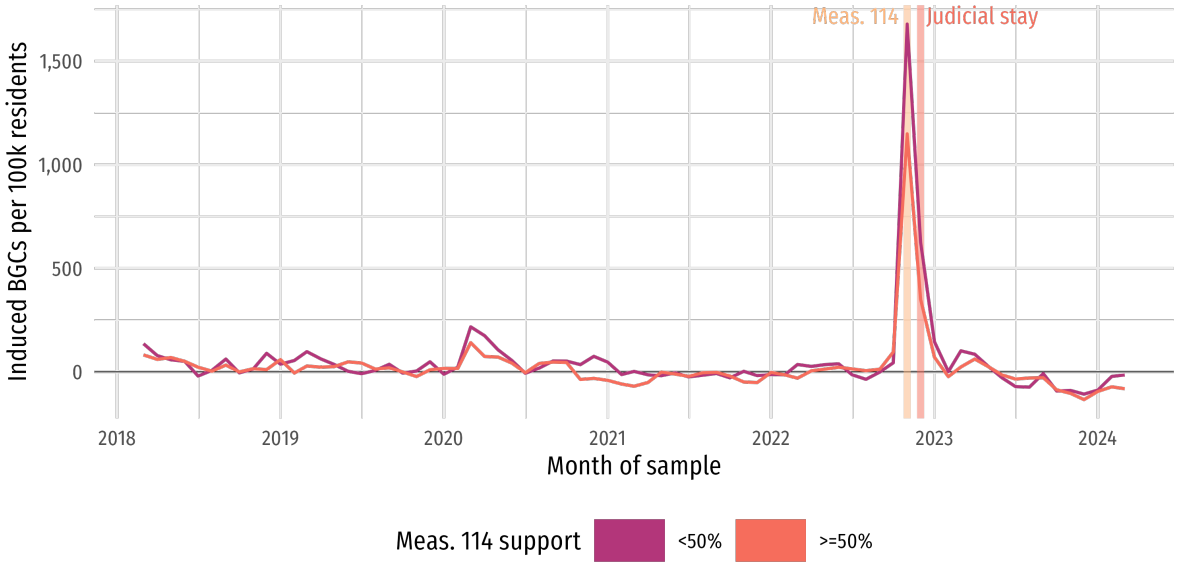


Figure 3: Daily time-series of OSP background checks in months preceding and following Measure 114 (2022-09-01 through 2023-01-31), colored by day of week. Larger dots on the lower axis (connected to vertical lines) denote the start of our treatment period (01 Oct. 2022), the date of the election that included Measure 114 (08 Nov. 2022), Black Friday (25 Nov. 2022), the date of the judicial stay (06 Dec. 2022), and Christmas (25 Dec. 2022). The lightly shaded orange box marks the treatment period (which continues beyond the end of the figure).

(a) Effect of Measure 114 on firearm background checks by Measure 114 support



(b) Cumulative effect of Measure 114 on firearm background checks by Measure 114 support

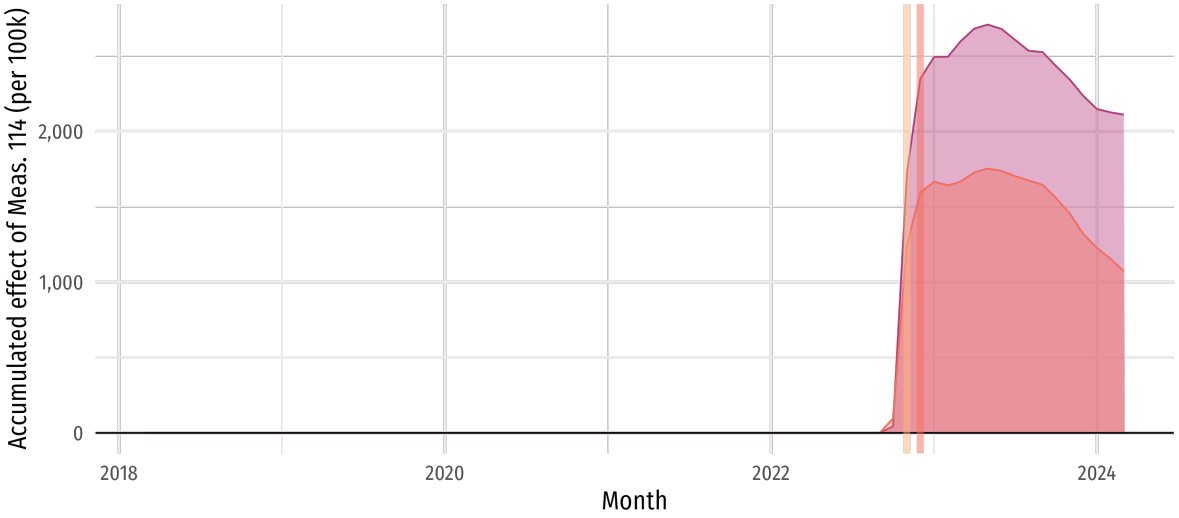


Figure 4: Measure 114's impact on firearm demand varied considerably with local support. This figure reproduces the time-series and accumulated effect shown in Figure 2 but with counties split into two groups by their support for Measure 114 in the election: majority opposed (darker purple) and majority supported (orange). In **Panel a**, the Measure-114 spike in demand among majority-opposition counties is nearly 50% larger than the spike in majority-support counties (per 100k). In **Panel b**, this difference translates in substantially higher accumulated effects in majority-opposed counties. Note that we omit the time-series of the synthetic counterparts for clarity of presentation. Appendix Figure A5 extends this figure two quartiles of support for Measure 114.

A Appendix figures

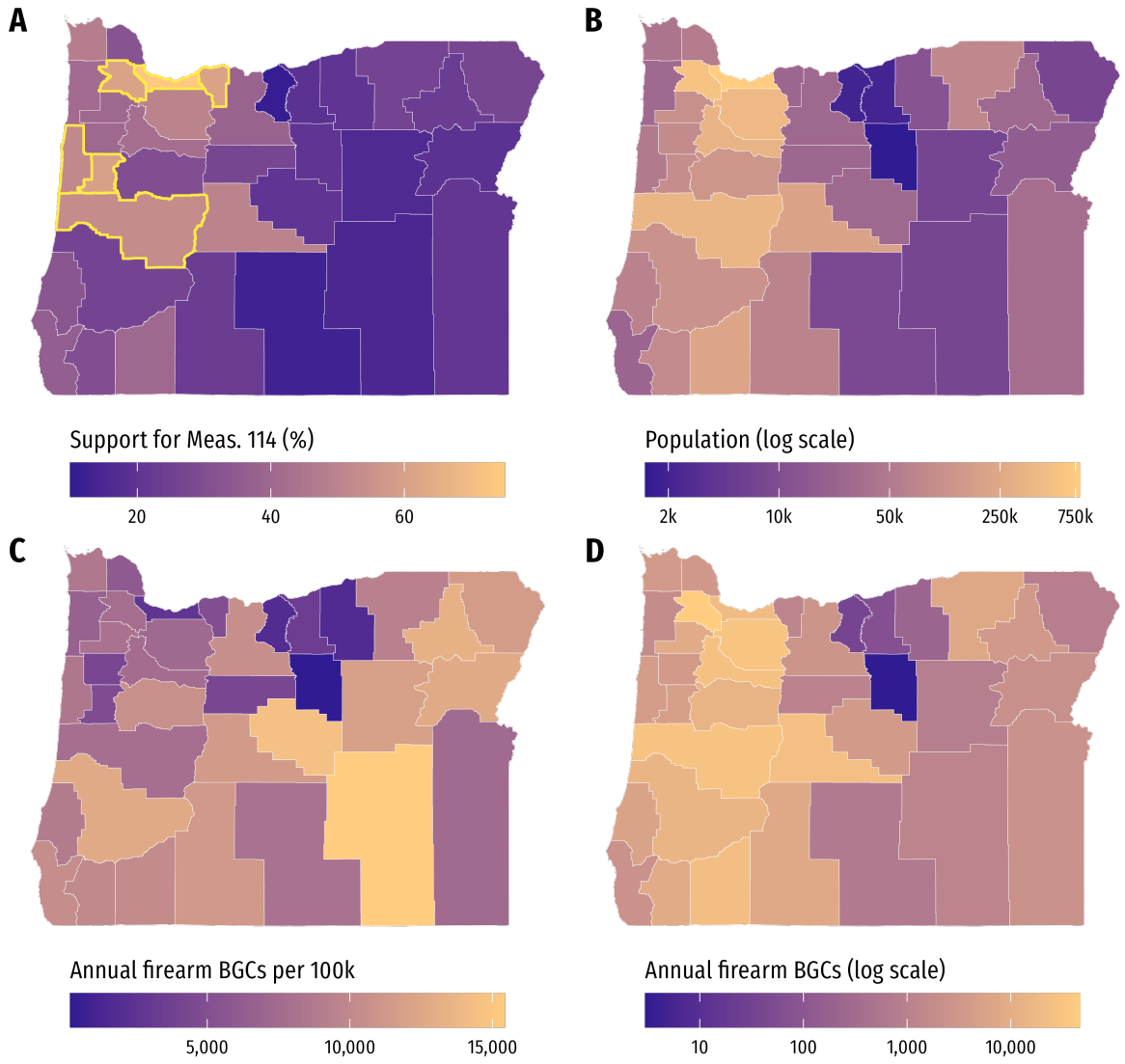


Figure A1: County-level support for Measure 114, population, and firearm background checks
Subfigure **A** maps county-level support for Measure 114 in the 2022 election. In the six counties traced in yellow, a majority of voters supported the measure. Subfigure **B** provides county-level population (filled in log-scale due to skew). Subfigures **C** and **D** show annual firearm-related background checks in the 12 months preceding Measure 114 (Oct. 2021–Sept. 2022)—first, per 100k residents (**C**) and then in absolute level (in **D**, log-scale).

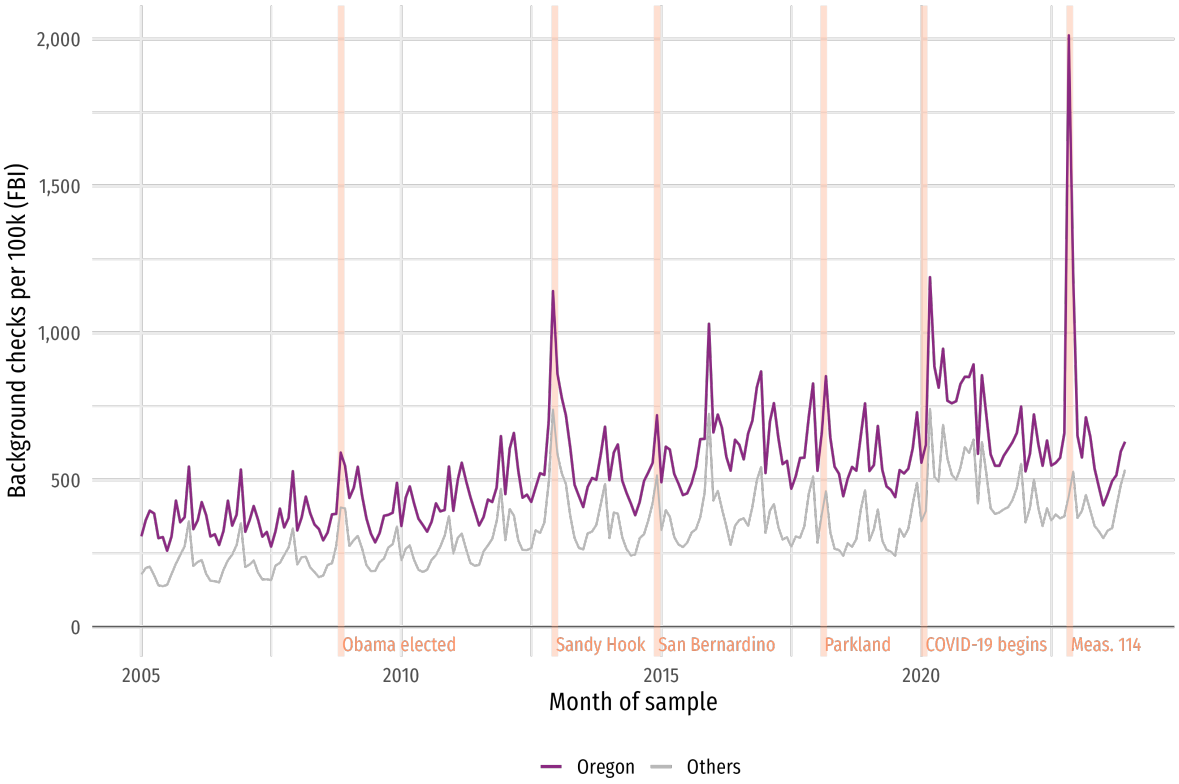


Figure A2: Oregon and the rest of the US followed similar trajectories for firearms background checks (per 100k) 2005–2024, except during Measure 114. The magnitude of Oregon's Measure 114 spike exceeds all preceding spikes in Oregon and among the *Others*. While the "Obama effect", the "COVID effect", and the effects of several high-profile shootings induced background-check spikes, none compare to Measure-114's effects in Oregon. As in the rest of the paper, we omit data Alabama, California, Idaho, Nevada, North Carolina, and Washington from *Others*. Starting in 2019, we replace Oregon's data from the FBI's NICS data with the OSP's data to avoid the capacity-related issues with the NICS discussed in *NICS appendix*.

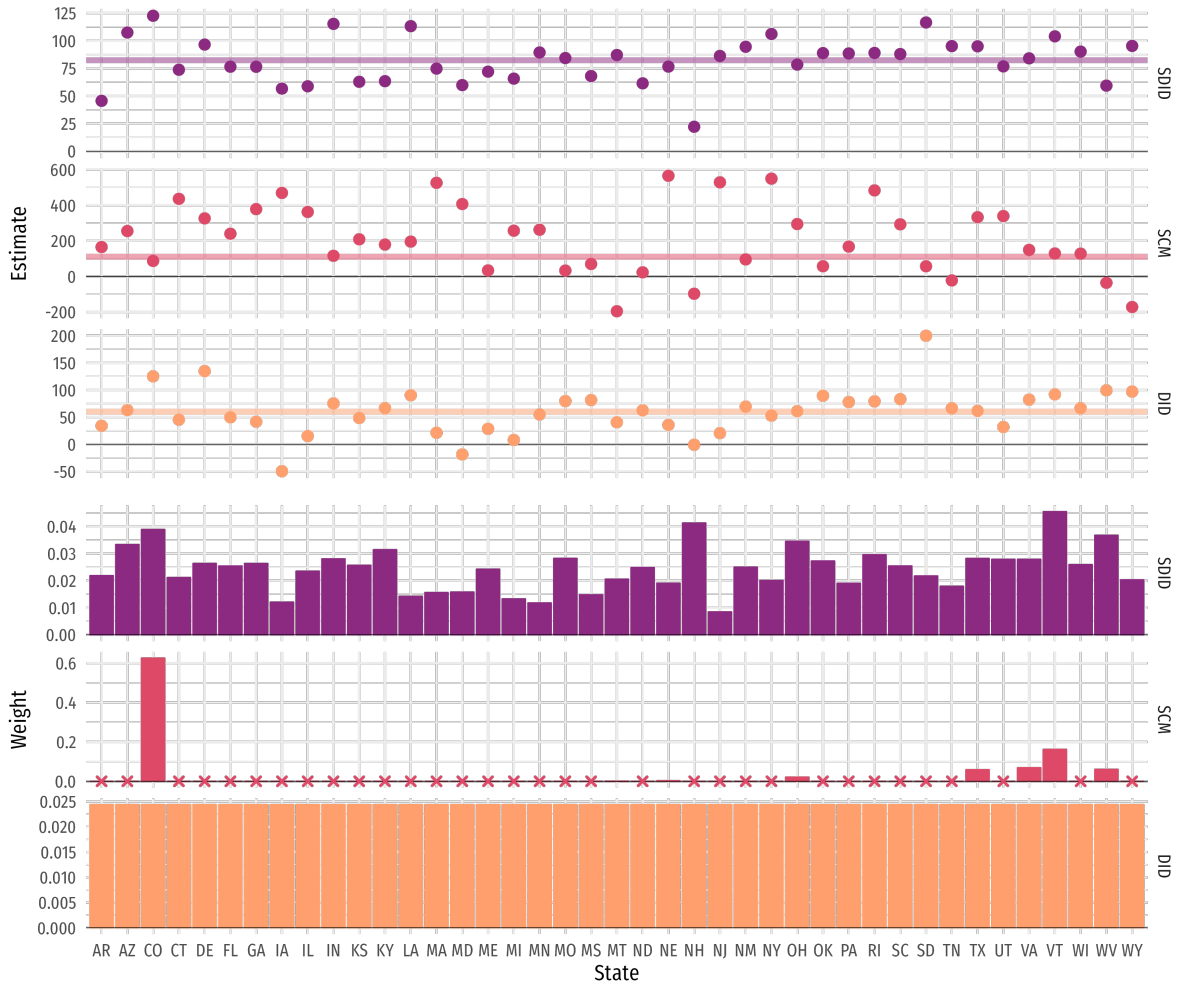


Figure A3: Comparing estimates and weights across the three estimators. The first three rows depict state-by-state estimates from three different estimators (color also denotes estimator)—SDID (synthetic difference in differences), SCM (synthetic control method), and DID (difference in differences). Each point represents a treatment-effect estimate from comparing Oregon to the given state (x axis) using the given method (color). The second panel (bottom three rows) depicts the weights applied to each state by each of the estimators. The $\times$'s in the SCM subfigure denote states that received zero weight. DID methods apply equal weight to all potential control units. Note that y axes differ across methods, as their scales vary substantially. Finally, the bold horizontal lines in the top three rows given the eventual estimate from each of the methods—i.e., weighting the top panel's estimates with the bottom panel's weights.

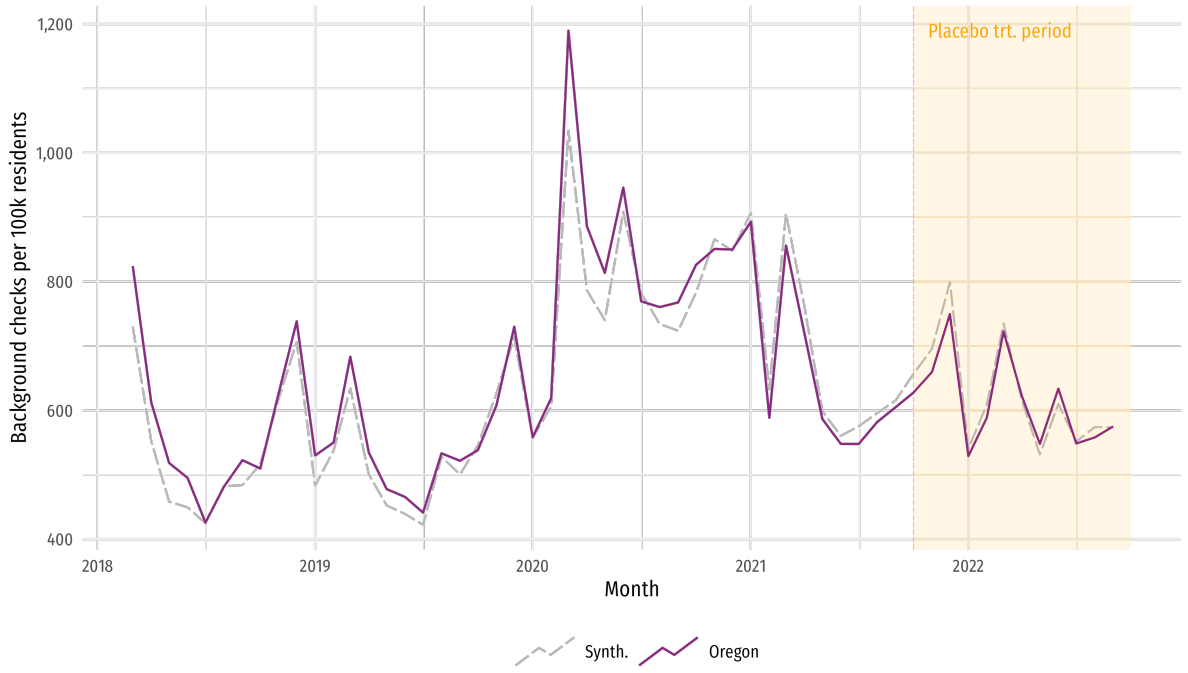
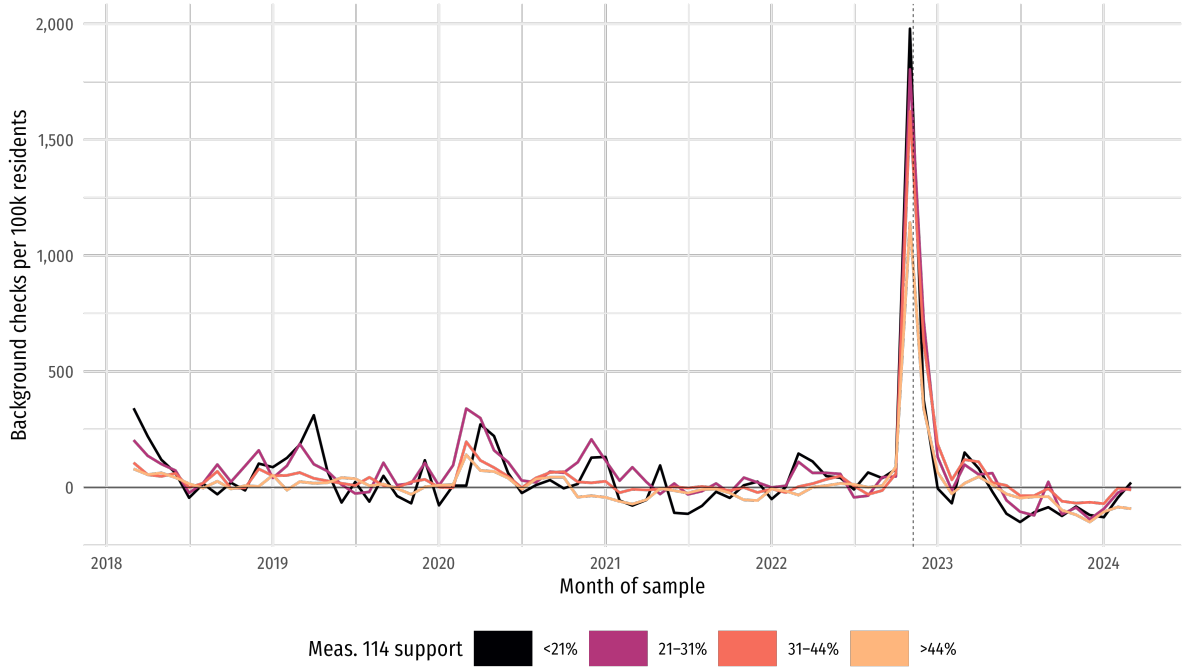


Figure A4: Placebo one-year prior to treatment shows no effect. This figure compares the time-series for the number of background checks in SDID-based synthetic Oregon (dashed grey) to actual Oregon (solid dark purple) but with a placebo treatment period that begins exactly one year early (October 2021) and ends before treatment (ending in September 2022). The resulting estimates bear to evidence at or during the placebo treatment period.

(a) Effect of Measure 114 on firearm background checks by quartile of Measure 114 support



(b) Cumulative effect of Measure 114 on firearm background checks by quartile of Measure 114 support

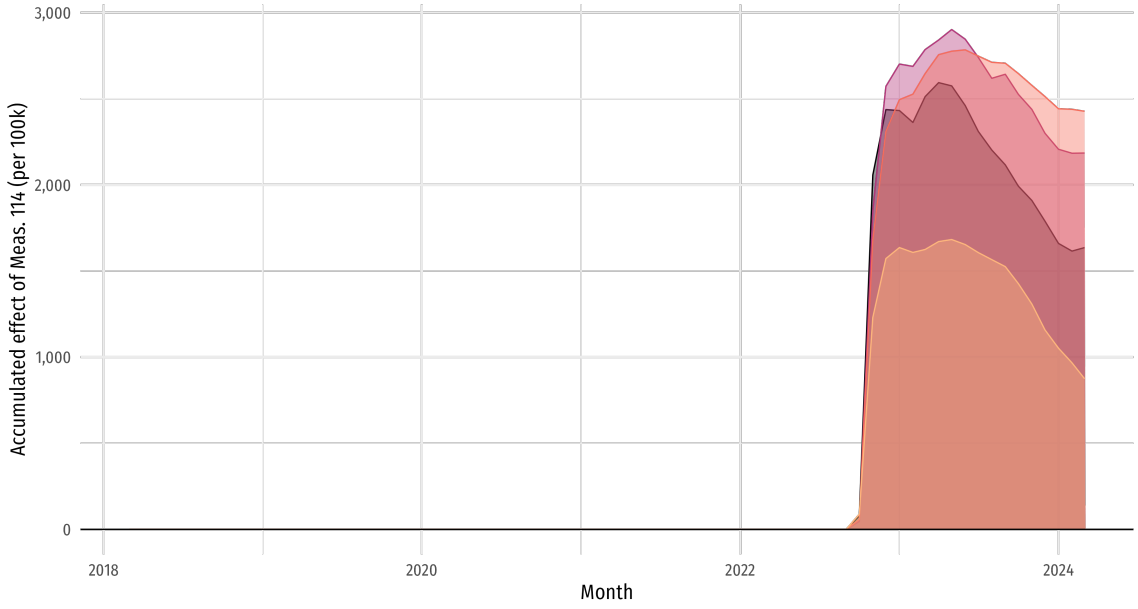


Figure A5: Measure 114's impact on firearm demand varied considerably with local support. This figure further decomposes the heterogeneity results from Figure 4 by quartile of support—grouping counties into (unweighted) quartiles based upon their support of Measure 114 in the election. In **Panel a**, higher opposition translates into larger demand spikes around Measure 114. In **Panel b**, demand variation after Measure 114 drives somewhat different accumulation volumes across the quartiles. The most supportive quartile by far accumulated the lowest level of excess firearm background checks per 100k residents.

B Appendix tables

	Estimator		
	SDID	*SCM*	*DID*
Time frame	(1)	(2)	(3)
Panel A: *Monthly effect (per 100k)*			
All	82.3	111.0	60.0
	(28.5)	(39.0)	(45.0)
October	82.4	95.6	49.8
	(23.3)	(42.9)	(61.8)
Post-election; pre-stay	929.7	941.4	887.6
	(36.1)	(45.6)	(66.4)
Early post-stay	44.3	35.8	25.4
	(36.7)	(40.4)	(54.0)
Long run	−48.3	−7.3	−68.5
	(31.9)	(44.2)	(52.7)
Panel B: *Accumulated effect (per 100k)*			
All	1,481.9	1,998.2	1,079.1
October	82.4	95.6	49.8
Post-election; pre-stay	1,859.5	1,882.7	1,775.2
Early post-stay	133.0	107.3	76.3
Long run	−580.0	−87.4	−822.2

Table B1: Estimated effects are robust to choice of estimator.
This table reports treatment effect estimates using alternative estimation models. All models use our main sample. In Column 1, we re-report estimates from the synthetic difference-in-differences model. In Column 2, we show results using standard difference-in-differences. In Column 3, we show results of a synthetic control specification.

Time frame	(1)	(2)	(3)	(4)
Panel A: *Monthly effect (per 100k)*				
All	82.3	80.7	84.3	92.7
	(28.5)	(26.5)	(37.6)	(33.2)
October	82.4	82.2	83.0	76.9
	(23.3)	(20.0)	(25.1)	(16.7)
Post-election; pre-stay	929.7	927.8	933.9	935.2
	(36.1)	(37.2)	(38.4)	(36.3)
Early post-stay	44.3	40.7	42.3	41.1
	(36.7)	(32.6)	(39.3)	(43.9)
Long run	−48.3	−47.7	−46.0	−36.1
	(31.9)	(31.1)	(52.2)	(46.0)
Panel B: *Accumulated effect (per 100k)*				
All	1,481.9	1,452.2	1,517.2	1,668.4
October	82.4	82.2	83.0	76.9
Post-election; pre-stay	1,859.5	1,855.5	1,867.9	1,870.4
Early post-stay	133.0	122.0	126.9	123.3
Long run	−580.0	−572.6	−552.0	−433.5
Potential control states				
N. states	41	28	33	24
Set definition	Non-border	Non-POC	Non-P2P	Non-Brady-exempt

Table B2: Estimated effects are robust to choice of control states.
This table reports treatment effect estimates from the SDID model with alternative subsets of donor states and lengths of post-periods evaluated. All specifications omit Alabama and North Carolina due to contemporaneous background law changes and omit non-contiguous U.S. states. Column 1 further excludes states that border Oregon. Column 2 restricts donor states to only those that are not point-of-contact states for any firearm type. Column 3 restricts donor states to only those that have never had a permit-to-purchase law. Column 4 excludes states with Brady-exempt permits. For all specifications, we use pre-intervention data from Mar. 2018–Sept. 2022. We vary the time period over which treatment effects are estimated as in previous tables. Cumulative estimates are calculated as the sum of monthly event study SDID estimates.

	Pooled	*Decomposed*			
		Pre	*Immediate*	*Post-stay*	*Long-run*
	(1)	(2)	(3)	(4)	(5)
Monthly effect	0.62	0.54	6.24	0.35	−0.26
	(0.23)	(0.24)	(0.31)	(0.34)	(0.22)
Accum. effect	11.13	0.54	12.47	1.05	−3.11
	(4.09)	(0.24)	(0.63)	(1.03)	(2.67)
Trt. period	Oct. '22–Mar. '24	Oct. '22	Nov. '22–Dec. '23	Jan. '23–Mar. '23	Apr. '23–Mar. '24

Table B3: Results are robust to standardizing the outcome variable.
This table reproduces Table 1 but with the outcome variable (monthly, state-level background checks per 100,000 residents) standardized using each state's mean and standard deviation prior to Measure 114. One standard deviation of the monthly background checks in Oregon, prior to Measure 114, was approximately 150.6 background per 100,000 residents.

C NICS appendix

We observe a close relationship between the total monthly background checks reported in OSP data and the set of background checks we define as "standard sales" in FBI data from March 2018 through November 2022. However, beginning in December 2022, Figure A6 shows a substantive decoupling of the two time series. Upon discussion with OSP analysts, we confirmed that this reflects duplicate processing in the NICS system. This duplicate processing results from the Measure 114 demand spike overwhelming OSP administrative capacities to conduct background checks. Notably, we do not observe this decoupling during the COVID firearm demand shock.

To confirm that the increased measurement error following Measure 114 is contained to Oregon's NICS records, we obtained background check data from state-level agencies in three other POC states. We observe generally stable correlations between the two data sets within each state, with some variation in various time periods.

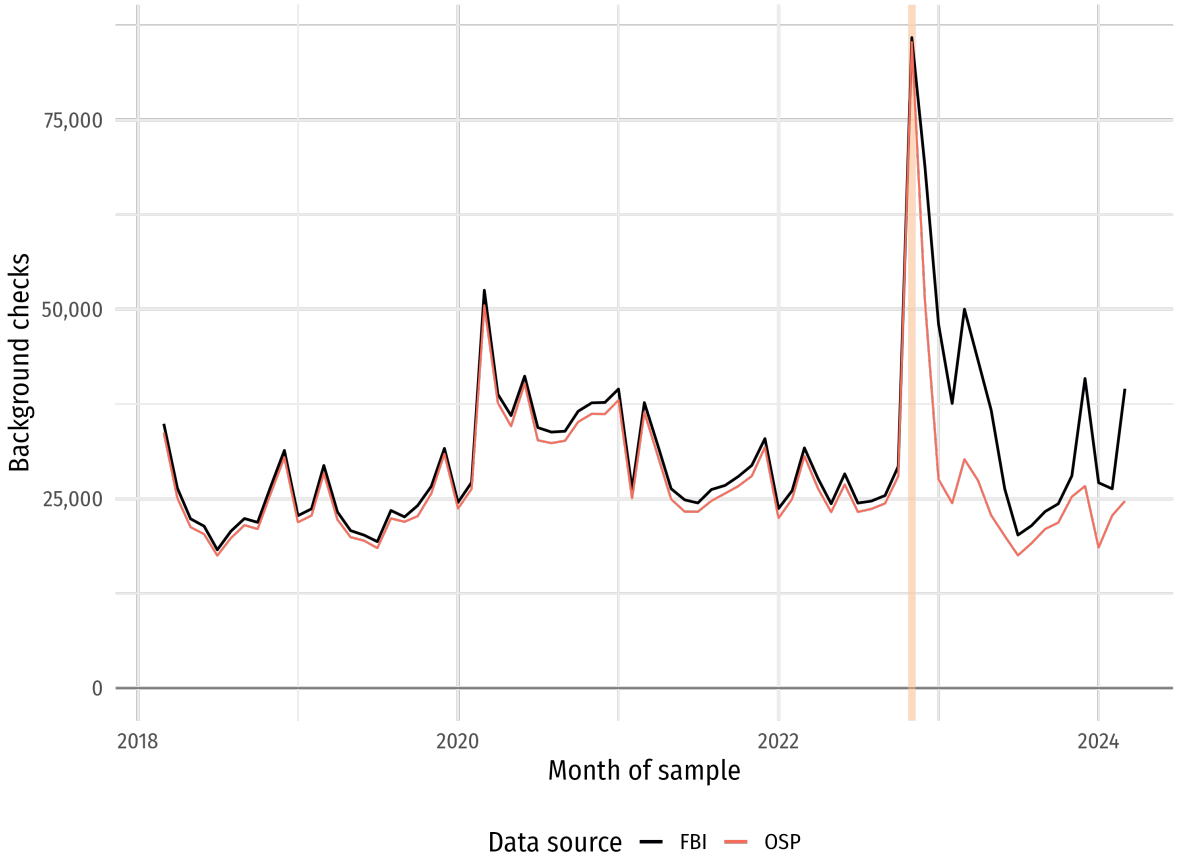


Figure A6: The number of background checks recorded in Oregon by the FBI and OSP diverge sharply after Measure 114. The figure depicts the number of monthly background checks recorded by the FBI (black) and OSP (orange) during March 2018–March 2024. The time-series match very closely until the months following Measure 114, when background-check systems likely hit capacity constraints due to the spike in firearm demand in Oregon.

D Counterfactual calculation

Measure 114's increase in gun sales largely concentrated in the weeks following the election and substantially diminished after the judicial stay. Following the judicial stay in December 2022, the monthly rate of background checks returned near its baseline (Panel A of Figure 2. However, an economically meaningful cumulative increase in background checks continued through the end of our observation period (Panel B of Figure 2). In March 2024, the final month in our data, we estimate a cumulative increase of 1,480 background checks per 100,000 Oregonians—equivalent to 63,000 excess background checks attributable to Meausure 114.

As a thought exercise, we consider an alternative counterfactual: the eventual successful implementation of Measure 114. In this exercise, we suppose Measure 114 went into effect in April 2024 and estimate the time required for Measure 114 to negate the cumulative increase in Oregon's firearm supply caused by the election. We assume Oregon's monthly firearm purchases remain at the levels observed in the final twelve months of our data and that Measure 114 (when finally imposed) strongly binds—reducing the monthly rate for new firearm purchases by 20 percent.[21]

To perform this calculation, we first average the monthly rate of background checks ($\tilde{5}30$ per 100k residents) in Oregon for the final 12 months of our data. We then apply the assumed 20% reduction to calculate a monthly decrease in new gun sales due to the policy (i.e., $0.2 \times 530 \approx 105$). Finally, we divide the accumulated excess increase in background checks from Measure 114 (1,480) by the monthly reduction from the implemented policy (105) to find the number of months required for this policy to eliminate the excess. In this scenario, it would take 14 months to achieve a cumulative reduction in Oregon's stock of firearms equivalent to the increase induced by Measure 114. Adding these 14 months to the 18 months that had already passed, it would take over two and a half years to reduce Oregon's firearm stock below what we expect it to have been if the Measure 114 election never took place.

In the above thought exercise presented, we assume that Oregon's monthly firearm purchases remain at the levels observed in the final twelve months of our data (April 2023 to March 2024). As a sensitivity analysis, we vary this assumption and present the results in Table C1. Model 2 in the table assumes that Oregon's monthly firearm purchases return to levels observed in the twelve months before Measure 114 (October 2021 to September 2022). Model 3 assumes the firearm purchase rates observed in 2021 (January to December). The number of months required for reversal varies slightly across models but remains qualitatively similar.

	Counterfactual scenarios		
Baseline period:	*Apr. 23–Mar. 24*	*Oct. 21–Sep. 22*	*Jan. 21–Dec. 21*
	(1)	(2)	(3)
Time required (months)	14.1	12.1	11.2

Table C1: Months required to offset Measure 114's anticipatory effects This table reports the results of a thought experiment in which we estimate the time required for a successfully implemented Measure 114 to negate the cumulative increase in gun sales induced by Measure 114. In each counterfactual, we assume Masure 114 goes into effect in April 2024 and reduces monthly gun sales by 20%. To estimate the time needed to undo the cumulative increase in Oregon's firearm stock induced by the anticipation of Measure 114, we must assume future monthly firearm purchases. Model 1 assumes monthly purchases in Oregon follow the average rate observed in the final twelve months of our data (April 2023–March 2024). Model 2 assumes monthly purchases return to levels observed in the twelve months before Measure 114 (October 2021–September 2022). Mldel 3 assumes monthly purchases observed in 2021 (January–December).

[21] We use a 20% reduction in monthly sales to reflect a bullish estimate of the effects of a PTP policy guided by outcomes observed in related studies (Balakrishna and Wilbur, 2022; Crifasi et al., 2015; Iwama and McDevitt, 2021; Williams Jr, 2020).