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### **Abstract**

We study the effects of newly passed gun legislation on the demand for firearms. We focus on Oregon, where voters passed Measure 114 in a referendum in November of 2022. After weeks of public debate and news coverage, the measure narrowly passed with 50.7 percent of the votes. We study the effects of this result on the demand for guns proxied by background checks. We find background checks surged by 400 percent for six weeks following the vote. After a judge's decision prevented the law from taking immediate effect, background checks fell from their all-time high, but persisted at roughly 100 percent above their original levels, even five months after the original passage of the measure. Additionally, we conduct the first analysis of background check data at the county-daily level, using Oregon State Police administrative data. We find significant heterogeneity in Oregonians' firearm purchasing behavior across counties. The surge in firearm demand resulting from Oregon's Measure 114 exemplifies the gun-control paradox and provides a cautionary tale to policymakers.

**JEL Codes:** I18, I21, I28

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

# 1 Introduction

The United States leads the developed world in gun deaths and has seen a steady increase in annual deaths in the last two decades. Indeed, 2021 marked an unfortunate high point, with nationwide gun deaths totaling 48,830 (?). Mass shootings and other high-profile gun accidents continue to make headlines, while federal changes to gun laws remain essentially non-existent since the expiration of the Assault Weapons Ban in 2004.

While federal policies stagnate, some states have begun to limit access to firearms. Even Texas is debating an increase in the minimum legal age to purchase high-capacity rifles to twenty-one in response to the highly publicized tragedy at Uvalde in May of 2022. Oregon also recently joined a growing set of states moving to pass gun control measures intended to improve firearm safety. Oregon's recent legislation came about through a public referendum process, appearing on the November 2022 ballot as Measure 114. If passed, this referendum promised background checks for all gun sales, restrictions on magazines, and a new permit-to-purchase program. The last provision attracted considerable debate in the final weeks leading up to the election.

On the surface, one may expect the provisions specified in Measure 114 would be effective in reducing long-run gun deaths and accidents. Indeed, 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 homicides. Likewise, the passage of a permit-to-purchase restriction in Connecticut was followed by decreases in homicides ([Rudolph et al., 2015](#)). [Crifasi et al. \(2015\)](#) study both Missouri's and Connecticut's permit-to-purchase programs and finds evidence that these restrictions serve to reduce suicide risk as well ([Rudolph et al., 2015](#)). However, if individuals anticipate the laws, the laws' potential benefits may be attenuated (or even entirely offset).

Anticipatory behavior has been found in many other contexts. [Becker et al. \(1994\)](#) find that while smokers cut their smoking behavior anticipating future tax hikes, they also stockpile cigarettes

to avoid future taxes. In labor economics, similar anticipation effects are seen in program participation, referred to as an “Ashenfelter Dip,” and in environmental economics, this has been noted for climate change policy, referred to as a “Green Paradox” ([Ashenfelter and Card, 1985](#); [Sinn, 2015](#)). We find evidence that the salient threat of the implementation of Measure 114 led to a roughly 400 percent increase in gun sales.

We contribute to a growing body of literature that suggests there is a “steel paradox.” While gun control policy may have benefits in the long-term, it may be met with short-term increases in firearm demand. [Depetris-Chauvin \(2015\)](#) finds President Obama’s election saw increases in background checks despite the Obama administration never leading any federal gun control policies. [Levine and McKnight \(2017\)](#) find that the Sandy Hook mass shooting led to increases in firearm purchases and gun accidents, again amidst no movement in federal legislation (and very little change in state-level laws). So while policies like a permit-to-purchase program and increased background checks may reduce gun sales in the long run, the short-run effects also warrant consideration for gun policy in the United States.

The effects of gun availability on local crime patterns have long been debated ([Kleck, 2004](#)). Theoretically, gun ownership could create a deterrent effect against many types of crime. However, the bulk of the literature suggests that more guns lead to more crime, rather than less. [Duggan \(2001\)](#) finds several proxies for gun ownership are associated with increases in homicide risk and increases in the risks for other violent crimes as well. [Cook and Ludwig \(2006\)](#) find that gun ownership, proxied by the fraction of suicides involving guns, corresponds to higher rates of violent crime. Likewise, [Billings \(Billings\)](#) finds that regions which exhibit more firearm background checks after national high-profile mass shootings see increases in their local violent crime rates, and increases in property theft (particularly those which involve firearms). Thus, rather than deterring crimes, guns themselves are often the targets of crime.

We add to this growing literature with evidence that suggests that there is a “steel paradox” for firearms policy. In the long run, the passage of a substantive restriction like a permit-to-purchase program may increase public safety by reducing the number of guns in circulation—or preventing “dangerous” purchases. However, the passage of a permit-to-purchase program will likely spawn anticipation effects. To estimate short-run and medium-run effects, we use the National Instant Background Check System (NICS) data from the FBI. Using national data at the month-state level and a synthetic difference-in-differences approach ([Arkhangelsky et al., 2021](#)), we find evidence of strong anticipation effects immediately after the passage of Oregon’s Measure 114. We also use NICS data at the county-day level from Oregon’s state police, focusing on the temporal patterns of anticipation effects and their geographic heterogeneity. We find that background checks actually began to increase in the weeks before the election and then immediately jumped after the outcome of the vote was known. We also find that background checks subsided after a judge halted the implementation of the new law. Still these checks remain elevated relative to their earlier baseline level, before the measure was first proposed. We also examine the heterogeneous impact 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 weeks in which the number of background checks translates to nearly 1% of the local population purchasing a firearm. It’s noteworthy that in absolute magnitudes, the anticipation effects we estimate for Oregon are much larger than the Obama effect, the Sandy Hook effect, or the surge in gun purchases observed early during the COVID19 pandemic.

## 2 Background

### 2.1 Gun laws in the United States

Conversations surrounding firearms and firearm regulation play an increasingly important role in American politics. The firearm-regulation debate, and research concerning firearms, have both evolved over time in focus and framing ([Steidley and Yamane, 2022](#)). However, the central tenets of proponents and opponents of firearm regulation remain relatively unchanged. Those in favor of regulation often cite the large number of injuries and deaths related to firearms each year (the CDC reported over 48,000 firearm-related deaths in 2021, and that firearms were the leading cause of death for children as of 2020). On the other hand, opponents of regulation frequently argue that access to firearms is a constitutional right and that firearms are an important tool for citizens to keep themselves safe. [?](#) highlights that both sides argue that “evidence matters” and the other side “ignore[s] the facts.”

Currently, firearms are regulated at both the national and state levels. Since the National Firearms Act of 1934, expansion of federal regulation has been modest.<sup>1</sup> Until the passage of the Bipartisan Safer Communities Act (2022) under the Biden administration, the most recent national-level firearm regulation policies were the Brady Handgun Prevention Act (1993) and the Federal Assault Weapon Ban (1994–2004). In the latter half of the 20th century and the early 21st century, federal-level firearm regulation has often been catalyzed by either high-profile firearm-related incidents or by rising crime rates.

In contrast to the few changes in federal regulation, there have been significant changes to state-level firearm laws in the United States over the last 20 years. Some states have enacted more-stringent regulations on firearm ownership, while others have loosened their laws. A thorough discussion of firearm laws is beyond the scope of this paper. However, we briefly discuss

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<sup>1</sup>[Vizzard \(2015\)](#) provides a good overview of firearm policy in the United States.

three firearm-related laws relevant to this paper: background check, permit-to-purchase, and high-capacity magazine laws.

**Background Checks:** Federal law requires 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. There are several modifications states have made to this federal mandate. First, depending on state law, FFLs either communicate with the NICS directly or alert a state-designated point of contact that then communicates with the NICS. Second, states have adopted laws that expanded background check requirements beyond FFL sales. For example, states with Universal Background Check laws (UBC) require background checks for all firearm sales and transfers (e.g., sales between private parties, sales at gun shows, or gifts). Third, states may run their own background checks that access state records that are not included in a NICS background check. Finally, state laws may allow for the substitution of an ATF-qualified alternate permit that can act in place of an NICS background check. However, an individual must undergo a background check to obtain the permit.

**Permit-to-Purchase:** A permit-to-purchase (P2P) law requires individuals a permit or license before purchasing a firearm. In contrast to background checks, P2P laws have been implemented only at the state level. To obtain a permit, an individual must apply for a permit at a local agency. Typically, the local agency is a law enforcement agency, and the application must be completed in person. Depending on state law, successfully obtaining a permit may require in addition to a background check (e.g., a gun-safety course). Like UBC laws, P2P laws ensure that all gunowners are subject to a background check. Some states have adopted both UBC laws and P2P laws, meaning that a background check is performed for individual at the time of the permit application and an additional time at the point of sale.

**High-capacity magazine restrictions:** The Federal Assault Weapon Ban of 1994 banned the manufacture, sale, and possession of new high-capacity magazines (more than 10 rounds) for civilian use. However, it allowed individuals who already possessed high-capacity magazines before the ban to keep them. The ban had a sunset provision and expired in 2004. In response, several states have imposed limits on magazine capacity. Due to the frequent use of high-capacity magazines in mass shooting events (MSE), high-capacity magazines have come under renewed scrutiny.

Table ?? reports the status of background check, permit-to-purchase, and high-capacity magazine laws in the United States. 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. Nine states require a permit (or license) to purchase all or some classes of firearms. Missouri (2007), Nebraska (2023), and North Carolina (2023) repealed their existing permit-to-purchase laws. 14 states have banned or restricted possession of high-capacity magazines.

## **2.2 Gun laws in Oregon**

In November 2022, nearly two million Oregon voters narrowly passed Measure 114, an initiative aimed to curb access to firearms and high-capacity magazines. Measure 114 would require that buyers receive a permit from local law enforcement prior to purchasing a firearm. In contrast to previous background check requirements, Measure 114 would result in permits being denied more easily on the basis of concerns over an individual's psychological state. Additionally, permits are contingent on demonstrated completion of a firearm safety course. Permits under Measure 114 are valid for five years. Beyond the gun-permitting requirements, Measure 114 would make it a criminal offense to possess magazines capable of holding ten or more rounds.

The passage of Measure 114 was controversial throughout Oregon and followed a decade of gradually strengthening gun legislation in the state. Between 2015 and 2021, Oregon expanded

its background check data infrastructure, implemented a “red flag” law enabling judges to order the removal of firearms from at-risk individuals upon a petition from a household member or law enforcement agency, extended gun restrictions for those under a restraining order or convicted of stalking, and mandated safe firearm storage practices. With the added provisions of Measure 114, Oregon rose in [Everytown’s gun law strength](#) rankings from 11th in the country to 9th in January 2023.

Although Oregon referenda typically become effective 30 days after passage, uncertainty over how and when Measure 114’s implementation spread rapidly after the election. Some sheriffs announced that they would not enforce provisions of the law, and gun advocates challenged the constitutionality of Measure 114 in court. As of May 2023, litigation was ongoing and Measure 114 had not been implemented.

In response to this uncertainty, in January 2023 Oregon lawmakers introduced Senate Bill 348, which would implement the primary provisions of Measure 114. Additionally, Senate Bill 348 would raise the minimum age for firearm purchasers. In April 2023, the bill successfully passed out of committee but awaits a vote by the Senate.

## 3 Data and methods

### 3.1 Data

**Measuring gun ownership** Despite the prevalence of firearms in policy debates and in the national dialogue, firearm-related data are scarce or inaccessible. Rather than being a shortsighted blunder, this lack of data is often by design. For instance, despite including thousands of detailed product categories, the Nielsen Consumer Panel censors information about firearm purchases. One approach to filling this data gap has been to conduct national firearm-ownership



surveys (e.g., the NRA’s National Gun Owners Survey). However, this approach has several disadvantages. First, large-scale surveys are conducted infrequently, and granular temporal analysis using this type of data is not feasible. Second, surveys are always subject to sample selection bias, and the representativeness of any voluntary survey is often a concern. Finally, regarding selection bias, [Urbatsch \(2019\)](#) finds an increasing level of nonresponse over time for firearm-ownership surveys—especially among Republicans and those who do not trust the government.

In the absence of direct measures, researchers have employed a wide range of firearm proxies varying from NRA membership to outdoor magazine subscriptions. Firearm Suicides divided by total Suicides (FSS) and NICS FBI background checks seem to have emerged as the two most popular proxies for firearm ownership.

Each proxy presents unique advantages and disadvantages for researchers ([Cook and Ludwig, 2019](#)). For instance, FSS ensures that a gun is observed in the community, thus avoiding challenges presented by issues like illegal firearm markets or inherited firearms.<sup>2</sup> However, due to the relative rarity of these events, using FSS as a proxy for firearm ownership is challenging in areas with small populations or across time ([Kleck, 2004](#); [Cerqueira et al., 2018](#)).

[Lang \(2013\)](#), who was one of the first researchers to implement the FBI background checks proxy, points out several drawbacks to using these background checks as a proxy. In some states, if a buyer possesses an up-to-date CCW permit, they may be excused from a background check when purchasing a firearm. On the other hand, some states periodically run “permit-check” background checks on all permit holders, regardless of a permit holder’s decision to buy a new firearm—compromising the reliability of background checks as a proxy for gun purchases. Additionally, while the Brady Act requires background checks for all purchases from Federal Firearm licensees (FFL), the federal government does not require background checks for private sales or for sales at gun

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<sup>2</sup>Because medical records indicate whether a firearm was used in the suicide there is no ambiguity about the presence of a firearm.

shows. While some states have enacted laws that expand background checks for private and gun-show sales, many states have not. Finally, there is evidence that compliance with federal law is not universal and that compliance levels may vary systematically by geography (e.g., [Castillo-Carniglia et al., 2018, 2019](#); ?). Despite these drawbacks, [Kim and Wilbur \(2022\)](#) demonstrate that background checks outperform other commonly used proxies.

**OSP data** This paper uses FBI background checks to proxy for firearm purchases. Since the passage of the Brady Gun Act in 1993, the federal government has required that all gun purchases from FFLs be conditioned on the results of a background check on the buyer through the FBI's National Instant Criminal Background Check System (NICS).

Our strategy to use FBI background checks as a proxy for firearm purchases is common in firearm research. However, our background-check data are uniquely spatially and temporally detailed. To date, research that uses background-check data relies on the NICS FBI background check data aggregated to the month and state level. All FFLs are required to run a background check through the NICS, but the particular entity that contacts the NICS depends on state law. In some states, local law enforcement agencies are appointed as intermediaries between sellers and the NICS. For other states, state laws require FFLs and other covered sellers to contact the NICS directly. In Oregon, firearm sellers contact the Oregon State Police (OSP), who then contact the NICS. Consequently, the OSP maintains detailed records of all NICS background checks requested in Oregon. The Oregon State Police (OSP) provided us with daily county-level background check data—significantly improving the level of detail possible in our analysis. The data begin in February of 2018 and are appended every month. Currently, the overall period of observation for the OSP data runs through March of 2023. The data also indicate the status of each background check (i.e., Approved, Canceled, Denied, and Pending).

**FBI NICS data** We complement the OSP data with the publicly available FBI NICS data. The FBI aggregates background data to the state-month level. As mentioned before, federal law requires gun sales from FFLs to be conditioned on background checks through the FBI's NICS. Numerous states (e.g., California) have implemented laws that expand background checks to other types of purchases (e.g., gun shows). These laws are frequently referred to as Universal Background Checks (UBCs). However, because background-check laws vary across states, comparing total background checks across states is not always accurate. Fortunately, the FBI also distinguishes between the different types of background check. For instance, the FBI data indicate whether the background check was on behalf of an FFL (as opposed to a private sale) and also note the type of firearm for which the background check was being run (e.g., handgun or long gun).

The observation period for our FBI data runs from January 2000 through April 2023. The data include Oregon's background check data. The OSP data offer greater granularity than the publicly available FBI data, but the OSP records do not indicate the type of background check. A comparison the time-series of OSP data versus the time-series of FBI data for Oregon in Figure 1 suggests that the OSP data likely do not include permit checks. In the FBI data, Oregon agencies also requested very few background checks for private gun sales. Consequently, we use the set of *Non-private gun sales* among the FBI background checks for our analysis.

**Additional data** We use several additional data sources for our analysis. First, we use the population data from the Census for state populations (2000-2022) and Oregon county populations (2018-2022).<sup>3</sup> We use the data to calculate changes in per-capita background checks. Given that there is considerable variation in the sizes of state and county populations that may be correlated with the propensity to own firearms, measuring background checks as a rate per capita is an essential adjustment to this measure.

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<sup>3</sup>The different time periods reflect county-level background data from the OSP beginning in February of 2018.

We also use data from the New York Times for Oregon’s 2022 election results. We look at both the referendum election results for Measure 114 and votes in the contemporaneous Oregon governor race.

### 3.2 Methodology

The primary objective of our analysis is to examine how the firearm-purchasing decisions of Oregonians respond to the proposal and passage of Measure 114. Due to the granularity of the OSP data, we can track background checks at the daily level for individual counties in Oregon. However, we do not have daily county-level data for counties in other states. Thus we cannot simply compare the treated (Oregon counties) to the control (non-Oregon counties). Our estimation strategy therefore uses two steps to address this issue. Our first step is a proof of concept that Measure 114 has a causal effect on background checks in Oregon. For this step, we use a synthetic difference-in-difference (DiD) approach to compare month-state-level background checks in Oregon to a synthetic Oregon comprised of a weighted mix of other states. We explain background checks in State  $i$  at time  $t$  the following equation:

$$bgc_{it} = \beta_1 \mathbf{1}(\text{Oregon}_i) + \beta_2 \mathbf{1}(\text{Post}_t) + \beta_3 (\mathbf{1}(\text{Oregon}_i) \times \mathbf{1}(\text{Post}_t)) + \varepsilon_{it}$$

After establishing a causal effect of Measure 114 on background checks in Oregon, we use the OSP data to examine responses over time to Measure 114 in more temporal and geographic detail. More specifically, we measure changes in background checks at the daily and weekly levels and examine heterogeneity in these changes across Oregon’s thirty-six counties. Our first approach is to examine the time-series of background checks in Oregon. Given that we have already established a causal relationship, a time-series model is perhaps the most informative method to employ as we examine the behavior of Oregonians. We also use a time-series model to examine the effect of a

county's support for Measure 114 on its response behavior.

To complement our time-series model, we implement an event study to measure the average treatment effect across counties in Oregon. We estimate this event study using the following specification:

$$bgc_{it} = \beta_0 + \beta_t \mathbf{1}(Treated) + \varepsilon_{it} \quad (1)$$

We use the event study to pin down the date of when anticipatory firearm sales begin in Oregon at a finer temporal level than is permitted by the FBI background-check data.

Next, we return to our initial synthetic-control DiD strategy to further examine county-level heterogeneity. However, instead of comparing all of Oregon to a synthetic Oregon, we now compare individual counties to synthetic counties. As mentioned, county-level data are unavailable for other states. Instead, our synthetic counties are comprised of a weighted mix of other entire states, excluding Oregon.

Finally, we explore heterogeneity in background checks across counties in Oregon. We do so by splitting our OSP data by county-level election results for Measure 114. First, we split the data into two groups: counties where the majority of voters (50%>) supported Measure 114 and counties where the majority of voters did not support Measure 114. We use a time-series model to visualize the difference in behaviors. Second, we split the data into quartiles—dictated by voting results for Measure 114 for each county. Each quartile has nine Oregon counties. Again, use a time-series model to visualize the difference in behaviors.

## 4 Results

### 4.1 Treatment period

Before we estimate the effects of Measure 114 on background checks, we explore the timing of when Oregonians first began to respond to the bill. An obvious choice is when Measure 114 passed on November 8th, 2022. However, Oregonians were likely aware of the bill ahead of election day and may have responded in anticipation of its passage. To get a sense of Oregonians' awareness of Measure 114 prior to election day, we use Google Trends. We use the search "Measure 114" from January 2022 through April 2023. Figure 2 displays the results from Google Trends. While we see a spike in searches during the week of the election, we also notice that Google searches for "Measure 114" increase several weeks ahead of election day.

### 4.2 State-level synthetic control difference-in-difference

We use a synthetic control difference-in-difference strategy to establish a causal relationship between Measure 114 and background checks. First, we create a synthetic control for Oregon as a whole, based on other states. We then use a difference-in-difference specification to compare background checks in Oregon to a simulated counterfactual for Oregon had Measure 114 not passed. Figure 3 illustrates the effect of Measure 114 with background checks per capita as the outcome variable.<sup>4</sup> The top panel compares Oregon and synthetic Oregon over time, and the bottom panel plots the difference between the two (Oregon minus synthetic Oregon). Figure 3 assumes that treatment begins only as of November 2023. However, the results from Figure 2 suggest that treatment—namely, anticipation of Measure 114 and its impact on Oregonians—may begin earlier than November. Given that the data are aggregated at the monthly level for this part of the analysis, we

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<sup>4</sup>Results for the effect of Measure 114 with the raw count of background checks as the outcome variable can be seen in Appendix ??.

must choose between October and November 2022.

Figure 3 illustrate an enormous surge in background checks in anticipation of Measure 114. The top panels of the figure confirm the findings in previous studies that firearm purchases respond to mass shooting events (MSE) and other events—like President Obama’s 2008 election and the onset of the COVID-19 pandemic. However, it is striking how much larger was the response of Oregonians to Measure 114 compared to any other event over the last 20 years. It is also worth noting that the bottom panels of the figures suggest that Oregonians were more responsive than other similar states to the effects of COVID-19.

We report additional results in Table 2, where we distinguish background checks by type. We can observe that the lion’s share of the increase in background checks concerned handgun sales. Table 3 reports the cumulative increase in background checks in the five months since the passage of Measure 114. During our observation period (November 2022 through March 2023), Measure 114 induced a cumulative increase of about 150,000 background checks in Oregon. In per-capita terms, this translates to approximately 3.5 background checks for every one hundred Oregonians.

### 4.3 Time-series models

As mentioned, in contrast with the FBI data, the background check data provided by the Oregon State Police has much higher resolution, being reported at the day and county level. This enables us to observe decisions about purchasing firearms in greater detail. However, we are constrained in that the data do not allow for an obvious counterfactual. Relying on the causal relationship established via our state-level synthetic control results, we first examine the OSP data as a time-series from February 2018 through February 2023 aggregated, to the weekly level across all Oregon counties—shown in Figure 4. Our decision to aggregate to the weekly level is motivated by weekly patterns in the data. More specifically, there are sharp declines in background checks on Sunday



and Monday of each week. Figure 4 demonstrates a large but brief spike in background checks during the week of 3/15/20, when the COVID-19 Pandemic unofficially “began.” The other distinct spike in background checks occurs in conjunction with the passage of Measure 114. In comparison to the spike related to COVID-19, this surge in background checks is substantially larger in magnitude and lasts considerably longer.

To get a better understanding of responses to Measure 114, we use same the weekly aggregated timeseries, but focus on the period between January 2022 and the end of the OSP data in February 2023—as seen in Figure 5. Figure 5 shows that there is anticipatory behavior beginning early in October 2022 (indicated in red), followed by a prolonged surge in background checks that continues through the end 2022 (indicated in blue).

Finally, Figure 6 shows daily background checks around the time of the passage of Measure 114 on November 8th, 2022. To minimize day-of-week variation, we exclude Monday and Sunday from the data.<sup>5</sup> However, a weekly spike on Saturday is still evident followed by fewer background checks on Tuesday. We can see an increase in background checks beginning in early October (highlighted in red) that then surge with the passage of Measure 114. “Black Friday,” the shopping event the day after the U.S. Thanksgiving holiday, 17 days after the vote on Measure 114, is the high-water mark for background checks. We omit Thanksgiving Day from the data to improve clarity of the trend of background-check trend.<sup>6</sup> For, roughly 2,000 background checks on 12/13/2022 and 12/14/2022 in the OSP data, it is not possible to attribute the background check to a specific county. These checks were dropped, accounting for a sudden drop in background checks (highlighted in yellow). We also see that background checks dipped around Christmas Day (highlighted in green).

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<sup>5</sup>Sharp dips in background checks occur on Monday and Sunday each week, and excluding them from the figure paints a clearer picture than with their inclusion.

<sup>6</sup>The sharp decline in background checks on Thanksgiving Day reflect the closure of stores rather than a change in the firearm purchasing behavior for Oregonians

## 4.4 Event Study

We also use the OSP daily data by county to perform a selection of event studies. Each county is a group in our event studies, but because Measure 114 is a statewide law, each group has the same event date. We aggregate the data for each county to the weekly level and begin our observation period in January 2022. We are not especially interested in the average treatment effect—the average increase in background checks per county. However, these treatment studies allow us to identify anticipatory behavior more accurately. We use two event dates for our event studies: the week Measure 114 passed (the week starting 11/07/2022) and the week when we see anticipatory behavior in our timeseries (starting 10/10/2022).

By comparing Figures 7 and 8 it is apparent that there is indeed anticipatory behavior. When we designate the event date as the election week, we see that the weeks preceding Measure 114 are consistently statistically negative—instead of the white noise we would expect to see. In addition, we see that predictions are consistently statistically negative starting seven weeks after our reference point, which again contradicts the timeseries figures 4, 5, and 6. In contrast, when we designate our event date as the week of 10/10/2022, we no longer see the consistently statistically significant negative observations preceding Measure 114. We also observe that background checks do not return to pre-Measure 114 levels in the immediately following weeks. Instead, they remain positive and statistically significant.

## 4.5 County Heterogeneity

The temporal resolution of the OSP data allows us to analyze heterogeneity in background checks across counties. We combine the FBI and OSP data to perform a synthetic DiD for each county in Oregon. To create our synthetic control for each Oregon county, however, we use non-Oregon state-level (rather than county-level) background checks. Given that the FBI data are available only

at the monthly level, our analysis of county treatment effects must be constrained to the monthly level. Table 4 reports the estimated treatment effect for each county, where the outcome variable is background checks per 100,000 county residents. Each row in Table 4 reflects the results of a distinct synthetic control model, where the counterfactual predictions for each Oregon county stem from a different set of weights on a different subset of Non-Oregon states. There is significant variation across counties in the magnitudes of these estimates—most estimates are statistically significant and positive, apart from those for Malheur and Wheeler Counties. Counties containing the largest urban centers (e.g., Multnomah County, which contains much of the metropolitan area associated with the city of Portland) see relatively modest increases in background checks. In contrast, the state’s more-rural counties (e.g., Harney County and Douglas County) featured much larger per-capita increases in background checks.

We dig further into heterogeneity in county-level background checks by differentiating county background checks by county voting results for Measure 114. We do this in two ways. First, we separate counties into just two groups: counties that voted >50% in support of Measure 114 and those that voted >50% in opposition to Measure 114. Figure 9 shows the timeseries of background checks for these two groups from February 2018 through February 2023. The dependent variable in the top panel is total background checks, and the dependent variable in the bottom panel is background checks per 100,000 county residents. Figure 9 shows that while the total background checks between the two groups of counties are comparable, the per-resident background checks were roughly 300% larger for counties that voted in opposition to Measure 114. Figure 10 uses the same specifications as Figure 9 but focuses our attention on just the months of 2022. Focusing on the weeks surrounding Measure 114, the bottom panel of Figure 10 indicates that the rates of per-capita background checks were over 50% higher for the counties that voted against the law.

Next, instead of grouping counties simply by majority support for or against Measure 114, we

bin counties into quartiles of the voting percentage distribution—determined by an equal number of counties for each quartile. Despite its passage overall, most counties did not individually vote in favor of Measure 114. Across counties, the range for the first quartile for support for Measure 114 is [11%, 20.8%]. For the second quartile, it is (20.8%, 31%); for the third quartile, it is (31%, 43.5%]; and for the fourth quartile, it is (43.5%, 74%]. Figure 11 tells an even more striking story about which Oregon counties supported Measure 114 and how they responded to its passage. The top and bottom panels of Figure 11 again summarize the total background checks and background checks per county resident, respectively. The small rural counties, with the strongest opposition to Measure 114, are responsible for a small share of total checks, but they saw massive increases in per-capita background checks. Comparing per-capita rates of background checks between quartiles, we see that increases for counties in the first and second quartile were approximately *twice* as large as for counties in the fourth quartile. Weekly per-capita rates for counties in the first and second quartile indicate that there were multiple weeks where the number of firearm background checks was close to 1% of the population.

## 5 Conclusion

We document large increases in Oregon’s gun-purchasing background checks in response to the state’s restrictive firearm control law, Measure 114. We estimate a cumulative increase of approximately 150,000 firearm sales resulting from Measure 114—equivalent to 3% of the state’s population purchasing firearms. We find that the increase in background checks begins weeks ahead of Election Day—suggesting anticipatory behavior even before the outcome of the vote is known. The increase in background checks surges after the passage of Measure 114 and persists above baseline levels for months afterward. The extant literature documents other cases where citizens have responded in anticipation of restrictive firearm laws (e.g., [Balakrishna and Wilbur, 2022](#); [Depetris-](#)

[Chauvin, 2015](#); [Iwama et al., 2021](#)). The surge in firearm sales following the passage of Oregon's Measure 114 dwarfs estimates from similar studies in duration and magnitude. [Balakrishna and Wilbur \(2022\)](#) documented increased firearm sales when a Massachusetts law was reinterpreted, leading to a ban on certain types of firearms. [Iwama et al. \(2021\)](#) find that Massachusetts' Gun Violence Reduction Act (MGVR) of 2014 led to an uptick in firearms sales ahead of the law. While [Balakrishna and Wilbur \(2022\)](#) and [Iwama et al. \(2021\)](#) find large increases in firearm sales in response to stricter laws, the duration of their periods of the increase are much shorter than in the case of Oregon's Measure 114. [Depetris-Chauvin \(2015\)](#) documents an increase in firearms sales following President Obama's election in 2008. While [Depetris-Chauvin \(2015\)](#) finds a long-term effect, Figures 4 and 8 suggest that the increase due to Oregon's Measure 114 is considerably larger than the "Obama Effect" for Oregonians.

We offer several possible explanations for Measure 114's unmatched effect on firearm sales. First, in the cases of [Balakrishna and Wilbur \(2022\)](#) and [Iwama et al. \(2021\)](#), they do not proxy for firearm sales. Given that Massachusetts law, the authors can take advantage of a direct measure of all firearm sales in the state. In contrast, we must resort to background checks as a proxy for firearms sales. Consequently, our measure of firearms sales could also be counting firearms-related background checks that are not new firearms sales (e.g., background checks may be run for "concealed-carry" permits). However, in our state-level analysis, we subset the FBI data to include just non-private firearms sales, and in our OSP-FBI data comparison exercise (see Figure 1), we find that the OSP data do not appear to include permit-related checks. It is also worth noting that not every background check run made to support a possible firearms actually sale results in a purchase, and some background checks can be shared across multiple purchases by the same individual. An argument thus can be made that our proxy is just as likely to undercount firearm sales as it is to overcount them.

Another possible explanation for Measure 114's effect is that it was more salient to individuals than the specific gun laws studied in other circumstances. [Depetris-Chauvin](#) finds evidence for two competing explanations for increased firearms sales: fear of firearms control, and racial prejudice. Importantly, an actual firearm control law motivated neither of these mechanisms. Unsurprisingly, the actual passage of a specific firearms-related law is more salient to citizens than the fear of the election of a perceived anti-firearm politician. In contrast to [Depetris-Chauvin \(2015\)](#), [Balakrishna and Wilbur \(2022\)](#) and [Iwama et al. \(2021\)](#) measure the impacts of actual firearm control laws. However, again, an argument can be made that Measure 114 is more salient to the affected citizens. Citizens did not have an opportunity to personally vote on the firearm laws that [Balakrishna and Wilbur](#) and [Iwama et al.](#) studied. [Balakrishna and Wilbur](#) study an existing law that was unpredictably reinterpreted, and [Iwama et al.](#) study a law introduced and passed by the state legislature, not by statewide referendum. Measure 114 is distinct from these laws because it began as a petition that gained enough support to warrant its inclusion on Oregon's 2022 election ballot and then was voted on by Oregonians. Measure 114 was in the public eye for an extended period, required citizen participation, and was sufficiently controversial to be only narrowly approved by voters.

The difficulty in implementing Measure 114 after its passage might explain the sustained surge in background checks. The ban studied by [Balakrishna and Wilbur](#) went into effect within 24 hours of the state government's announcement of the ban, and "[T]he announcement was widely seen as a surprise. It was not preceded by public comment or debate." This element of surprise may account for why [Balakrishna and Wilbur](#) found no evidence of any anticipatory buying before the announcement, and following the ban, firearm sales decreased well below the pre-ban rate. [Iwama et al.](#) find that Massachusetts' Gun Violence Reduction Act (MGVR) of 2014 led to an uptick in firearm sales ahead of the law. However, the authors find, "*As anticipated, [the association between the legislation passing and handgun sales] dissipated quickly over time.*" We find no such quick dissipation. During

the time since its passage in November 2022, Measure 114 has been stuck in Oregon’s court system—challenged as violating the 2nd Amendment. As a result, Measure 114 inspired anticipatory firearms purchases but is, as of this writing, still unable to increase firearms controls in the long run.

As a thought exercise, we estimate how long it would take for Measure 114’s implementation to reverse the increase in firearms that it inadvertently induced. To do this, first, we calculate the weekly rate of background checks for Oregon before the “Measure 114” effect. We exclude the surge in background checks in March 2020 due to the onset of the CoViD-19 pandemic. In the end, we calculate the mean for weekly background checks in Oregon from February 2018 through September 2022 to be roughly 6,100 statewide checks. We then assume that the cumulative increase in firearm sales resulting from Measure 114 ends in March of 2023—which is unlikely, given the trend in the data. As indicated in Table 3, the cumulative increase is approximately 147,000 background checks. We then assume a reduction level in the case that Measure 114 was implemented. For the sake of argument, we choose an ambitious 20% reduction in firearm sales. In this scenario, it would take approximately 120 weeks—over two years—to achieve a cumulative reduction in firearms equivalent to the increase induced by Measure 114. Of course, if we choose a more realistic reduction in firearm sales, the timeline increases considerably.

After documenting the large surge in background checks in Oregon, we turned to the task of disentangling this increase by examining heterogeneity across Oregon counties. Measure 114 was a highly contested bill—winning less than 51% of the popular vote. Most counties individually opposed the bill, which passed statewide due only to support from those counties containing the large urban centers. We explore the relationship between support for the bill and changes in background checks. In general, smaller, rural, and right-leaning counties were responsible for about half of the increase in background checks, despite making up only a small share of the state’s population. This

dynamic is consistent with work exploring the relationship between political ideology and firearm ownership (e.g., [Burton et al., 2021](#); [García-Montoya et al., 2022](#); [Joslyn et al., 2017](#); [Tatalovich and Haider-Markel, 2022](#); [Warner and Ratcliff, 2021](#)). [Joslyn et al. \(2017\)](#) argues that firearms ownership is becoming one of the strongest correlates for Republican ideologies. Table 4 indicates that the greatest per-capita increases in county-level background checks occurs in more-rural counties that oppose Measure 114 (e.g., Harney, Douglas, Union, and Tillamook Counties). However, there are several exceptions. For example, despite broadly supporting Measure 114 relative to other counties, Hood River saw one of the largest increases in background checks. Lake County voted strongly against the law but saw only a relatively modest increase in background checks. Part of the explanation for these contradictions could be attributed to the very small populations in some Oregon counties, and using whole states other than Oregon to synthetically control for such small counties could lead to perplexing results. It is also possible that the relationship between firearm ownership, support for firearm control, and responses to imminent control is nuanced. While it is reasonably straightforward to determine who owns a firearm, the psychology and social attitude of firearm owners are complex ([Burton et al., 2021](#); [Schleimer et al., 2020](#)). Panel 2 of Figures 9 and 11 demonstrate that background checks increased the most in counties with historically higher shares of background checks per capita. This result suggests that, to some degree, a larger share of the increase in firearms sales can be attributed to people or people with neighbors who already owned firearms before Measure 114.

It is unclear what will be the long-term impact of Measure 114 and related Senate Bill 348 on firearms prevalence and overall welfare in Oregon. Measure 114 undoubtedly induced a substantial increase in firearm sales. Moreover, neither law has been successfully implemented. A robust body of work demonstrates that firearm ownership rates are often positively correlated with suicide and homicide rates (e.g., [Braga et al., 2021](#); [Cook and Donohue, 2017](#); [Depetris-Chauvin, 2015](#);



Lang, 2013; Siegel and Rothman, 2016). Additionally, research finds examples of stricter laws reducing suicide and homicide rates.<sup>7</sup> A complementary body of work suggests that laxer laws lead to increased rates of homicide and suicide.<sup>8</sup> However, reviews of the extant research find that the impact of firearms control laws is far from conclusive (Santaella-Tenorio et al., 2016; Wintemute, 2015, 2019).

The apparently variable impacts of firearms laws could be attributed to the implementation and enforcement of these different laws (Wintemute, 2019). For instance, there is evidence that individuals in states with Universal Background Check (UBC) laws do not always get background checks (e.g., ???). Comparing the results of (Castillo-Carniglia et al., 2018, 2019) Castillo-Carniglia (2018, 2019) and (Kagawa et al., 2023) provides a good example of how non-compliance can obscure measurement of the efficacy of laws. Castillo-Carniglia et al. found that while the states of Colorado, Delaware, Oregon, and Washington passed UBC laws, background checks increased only in Delaware and Oregon—suggesting non-compliance. Kagawa et al. found that UBC laws in Colorado, Delaware, Oregon, and Washington do not unambiguously reduce suicide rates. Considering the apparent non-compliance in Colorado and Washington, it is difficult to know how to interpret Kagawa’s results. Balakrishna and Wilbur (2022) provide another example of non-compliance. They find that the sale of banned firearms decreased substantially after the ban, but did not decrease to zero.

In addition to non-compliance, the heterogeneity in state laws may undermine the efficacy of state-level firearm control laws. Evidence shows that firearms tend to flow from states with laxer laws into states with stricter laws (e.g., Andrade et al., 2020; Knight, 2013; Takada et al., 2021). Concerningly, but perhaps not unsurprisingly, firearm homicides are more often connected

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<sup>7</sup>(e.g., Anestis et al., 2015; Conner and Zhong, 2003; Cook and Ludwig, 2006; Crifasi et al., 2018; ?; Irvin et al., 2014; Klarevas et al., 2019; Knopov et al., 2019; Liu and Wiebe, 2019; ?; McCourt et al., 2020; Raissian, 2016; Rudolph et al., 2015; Siegel et al., 2019; Smith and Spiegler, 2020; Tashiro et al., 2016; Webster et al., 2020).

<sup>8</sup>(e.g., Alban et al., 2018; ?; Donohue, 2017; Doucette et al., 2019; Reeping et al., 2022; Siegel et al., 2017; Williams, Williams).

to illegally acquired firearms (e.g., [Braga et al., 2021](#); [Cook et al., 2015](#); ?; [Semenza et al., 2023](#)).

Firearm violence is a "public health crisis" ([Braga, 2022](#)). Despite scattered minimal increases in firearms control, support for firearms control continues to increase ([Crifasi et al., 2020](#)). However, an effective way to reduce firearms violence through state-level laws remains elusive. Relaxing firearm controls leads to increased firearms sales, and tightening firearms controls—at least in the short run—also leads to increased firearms sales. Measure 114 is thus a cautionary tale for policymakers: A firearm control law will lead to an increase in firearms if it is not quickly and effectively passed and meticulously enforced.

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## 6 Tables



Table 1: Firearm laws in 2023

## State-level background check, permit-to-purchase, and high capacity magazine laws in 2023.

Background Check Laws				
State	What type of guns require BGC?	When is the background check performed?	High capacity magazines prohibited?	Permit-to-purchase law?
Alabama	—	—	No	—
Alaska	—	—	No	—
Arizona	—	—	No	—
Arkansas	—	—	No	—
California	All firearms (handguns, rifles, and shotguns)	Point-of-sale	Yes	—
Colorado	All firearms (handguns, rifles, and shotguns)	Point-of-sale	Yes	—
Connecticut	All firearms (handguns, rifles, and shotguns)	Permit to purchase and point-of-sale	Yes	Yes
Delaware	All firearms (handguns, rifles, and shotguns)	Point-of-sale	Yes	—
Florida	—	—	No	—
Georgia	—	—	No	—
Hawaii	All firearms (handguns, rifles, and shotguns)	Permit to purchase	Yes	Yes
Idaho	—	—	No	—
Illinois	All firearms (handguns, rifles, and shotguns)	Permit to purchase and point-of-sale	Yes	Yes
Indiana	—	—	No	—
Iowa	—	—	No	—
Kansas	—	—	No	—
Kentucky	—	—	No	—
Louisiana	—	—	No	—
Maine	—	—	No	—
Maryland	All firearms (handguns, rifles, and shotguns)	Permit to purchase (for handguns) and point-of-sale (for all guns)	Yes	Yes
Massachusetts	All firearms (handguns, rifles, and shotguns)	Permit to purchase	Yes	Yes
Michigan	All firearms (handguns, rifles, and shotguns)	Permit to purchase	No	Yes
Minnesota	Handguns and semiautomatic military-style assault weapons	Permit to purchase or point-of-sale	No	Optional
Mississippi	—	—	No	—
Missouri	—	—	No	Repealed
Montana	—	—	No	—
Nebraska	Handguns only	Permit to purchase	No	Repealed
Nevada	All firearms (handguns, rifles, and shotguns)	Point-of-sale	No	—
New Hampshire	—	—	No	—
New Jersey	All firearms (handguns, rifles, and shotguns)	Permit to purchase and point-of-sale	Yes	Yes
New Mexico	All firearms (handguns, rifles, and shotguns)	Point-of-sale	No	—
New York	All firearms (handguns, rifles, and shotguns)	Permit to purchase (for handguns and semiautomatic rifles) and point-of-sale (for all guns)	Yes	Yes
North Carolina	—	—	No	Repealed
North Dakota	—	—	No	—
Ohio	—	—	No	—
Oklahoma	—	—	No	—
Oregon	All firearms (handguns, rifles, and shotguns)	Permit to purchase and point-of-sale	Yes	Yes

Continued on next page

Table 1 – continued from previous page

Pennsylvania	Handguns only	Point-of-sale	No	—
Rhode Island	All firearms (handguns, rifles, and shotguns)	Point-of-sale	Yes	Yes
South Carolina	—	—	No	—
South Dakota	—	—	No	—
Tennessee	—	—	No	—
Texas	—	—	No	—
Utah	—	—	No	—
Vermont	All firearms (handguns, rifles, and shotguns)	Point-of-sale	Yes	—
Virginia	All firearms (handguns, rifles, and shotguns)	Point-of-sale	No	—
Washington	All firearms (handguns, rifles, and shotguns)	Point-of-sale	Yes	—
West Virginia	—	—	No	—
Wisconsin	—	—	No	—
Wyoming	—	—	No	—

Table 2: Change in Oregon Background Checks after Measure 114

Type of Check	Estimate	S.E.	CI.lower	CI.upper	P.value
<b>Raw Count</b>					
Handgun	21,500	1,800	18,000	25,000	<0.001
Long gun	7,700	1,600	4,600	10,800	<0.001
All non-private gun sales	28,700	2,800	23,300	34,100	<0.001
<b>Per 100,000 residents</b>					
Handgun	516	25	466	566	<0.001
Long gun	178	26	127	229	<0.001
Non-private gun sales	679	45	591	767	<0.001

Notes: Results from synthetic control difference-in-difference. Estimated using state-level FBI background checks from January, 2000 through March, 2023. Treatment spans from Nov. 2022 through Feb. 2023.

Table 3: Cumulative change in Oregon Background Checks after Measure 114

Type of Check	Months after election	Estimate	S.E.	CI.lower	CI.upper	P.value
<b>Raw Count</b>						
Handgun	0 (Oct. 2022)	3,500	1,700	300	7,900	0.048
	1 (Nov. 2022)	45,300	3,300	39,400	52,500	<0.001
	2 (Dec. 2022)	72,800	5,400	61,400	84,600	<0.001
	3 (Jan. 2023)	89,400	6,800	75,600	104,100	<0.001
	4 (Feb. 2023)	97,800	8,600	79,300	115,700	<0.001
	5 (Mar. 2023)	111,100	10,200	92,500	134,900	<0.001
Long gun	0	300	16,00	-3,100	3,000	0.628
	1	16,000	3,100	8,700	20,900	0.012
	2	24,700	5,100	12,500	33,000	0.012
	3	30,400	6,500	13,800	41,300	0.014
	4	32,900	7,700	13,900	45,500	0.016
	5	38,700	9,100	14,700	54,300	0.016
Non-private gun sales	0	3,700	2,200	-1,300	8,300	0.098
	1	60,900	4,300	51,300	68,800	<0.001
	2	96,600	7,100	81,100	110,900	<0.001
	3	118,400	9,200	99,500	138,000	<0.001
	4	129,200	11,200	107,100	154,000	<0.001
	5	147,300	14,600	113,200	177,400	<0.001
<b>Per 100,000 residents</b>						
Handgun	0	93	21	55	140	<0.001
	1	1,079	48	990	1,192	<0.001
	2	1,731	81	1,575	1,912	<0.001
	3	2,143	101	1,941	2,376	<0.001
	4	2,349	124	2,095	2,635	<0.001
	5	26,73	145	2,392	3,013	<0.001
Long gun	0	-5	23	-51	46	0.906
	1	362	49	253	468	<0.001
	2	555	83	389	764	<0.001
	3	695	106	488	961	<0.001
	4	749	129	485	1,054	<0.001
	5	883	149	580	1,206	<0.001
Non-private gun sales	0	84	38	14	166	0.020
	1	1,427	81	1,259	1,610	<0.001
	2	2,258	135	1,972	2,561	<0.001
	3	2,792	172	2,437	3,180	<0.001
	4	3,039	213	2,591	3,515	<0.001
	5	3,479	252	2,960	4,036	<0.001

Notes: Results from synthetic control difference-in-difference. Estimated using state-level FBI background checks from January, 2000 through March, 2023. Treatment spans from Nov. 2022 through Feb. 2023.

Table 4: County-level changes in Oregon Background Checks after Measure 114

County	Estimate	S.E.	CI.lower	CI.upper	p.value
Baker	604.11	77.85	451.54	756.69	<0.001
Benton	293.05	46.07	202.76	383.34	<0.001
Clackamas	492.04	53.13	387.91	596.17	<0.001
Clatsop	290.70	45.87	200.79	380.60	<0.001
Columbia	644.16	53.13	540.04	748.29	<0.001
Coos	542.44	49.20	446.01	638.88	<0.001
Crook	567.42	56.92	455.86	678.98	<0.001
Curry	164.10	55.49	55.35	272.85	0.003
Deschutes	516.09	55.10	408.09	624.09	<0.001
Douglas	872.43	57.85	759.05	985.81	<0.001
Gilliam	286.34	56.27	176.05	396.63	<0.001
Grant	583.12	53.13	478.99	687.25	<0.001
Harney	1194.93	74.14	1049.61	1340.24	<0.001
Hood River	901.86	47.56	808.64	995.08	<0.001
Jackson	519.09	55.84	409.65	628.53	<0.001
Jefferson	238.37	53.19	134.13	342.62	<0.001
Josephine	464.66	54.97	356.91	572.40	<0.001
Klamath	539.01	50.77	439.50	638.52	<0.001
Lake	152.73	48.21	58.24	247.21	0.002
Lane	473.86	53.13	369.73	577.99	<0.001
Lincoln	578.81	48.00	484.73	672.88	<0.001
Linn	725.99	50.73	626.57	825.41	<0.001
Malheur	-4.99	64.11	-130.65	120.66	0.938
Marion	620.44	54.65	513.33	727.56	<0.001
Morrow	174.87	52.16	72.65	277.09	0.001
Multnomah	164.59	55.31	56.19	273.00	0.003
Polk	264.62	42.28	181.75	347.49	<0.001
Sherman	581.36	55.78	472.04	690.69	<0.001
Tillamook	754.47	50.41	655.66	853.27	<0.001
Umatilla	545.84	49.04	449.71	641.96	<0.001
Union	759.19	56.22	649.00	869.38	<0.001
Wallowa	655.54	68.11	522.05	789.03	<0.001
Wasco	397.37	57.28	285.09	509.64	<0.001
Washington	368.40	53.13	264.27	472.53	<0.001
Wheeler	-58.01	45.64	-147.46	31.43	0.204
Yamhill	735.33	54.49	628.53	842.12	<0.001

Notes: Results from synthetic control difference-in-difference. Treatment is for individual Oregon counties from Feb. 2018 through Feb. 2023. Controls estimated using state-level FBI background checks.

## 7 Figures

## Background Checks by month Oregon State Police data vs. FBI data (2018 - 2023)

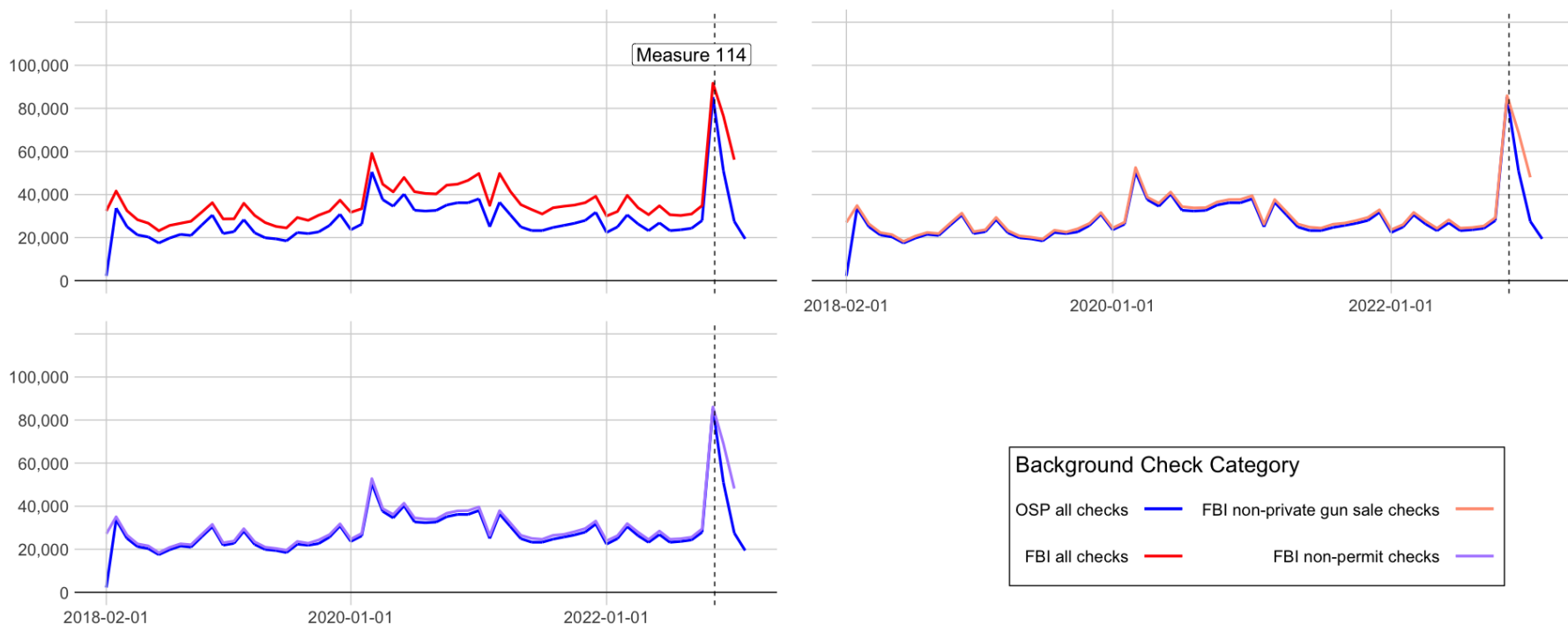


Figure 1: FBI vs. OSP data

Comparing OSP data to different subsets of FBI data suggests that excluding permit-related background checks from the FBI data gives the closest match between datasets. Non-private gun sales is compromised of handgun, long gun, other, and multiple from the FBI data. Non-permit excludes permit and permit-recheck from the FBI data.

## Google Trends for "Measure 114" in Oregon

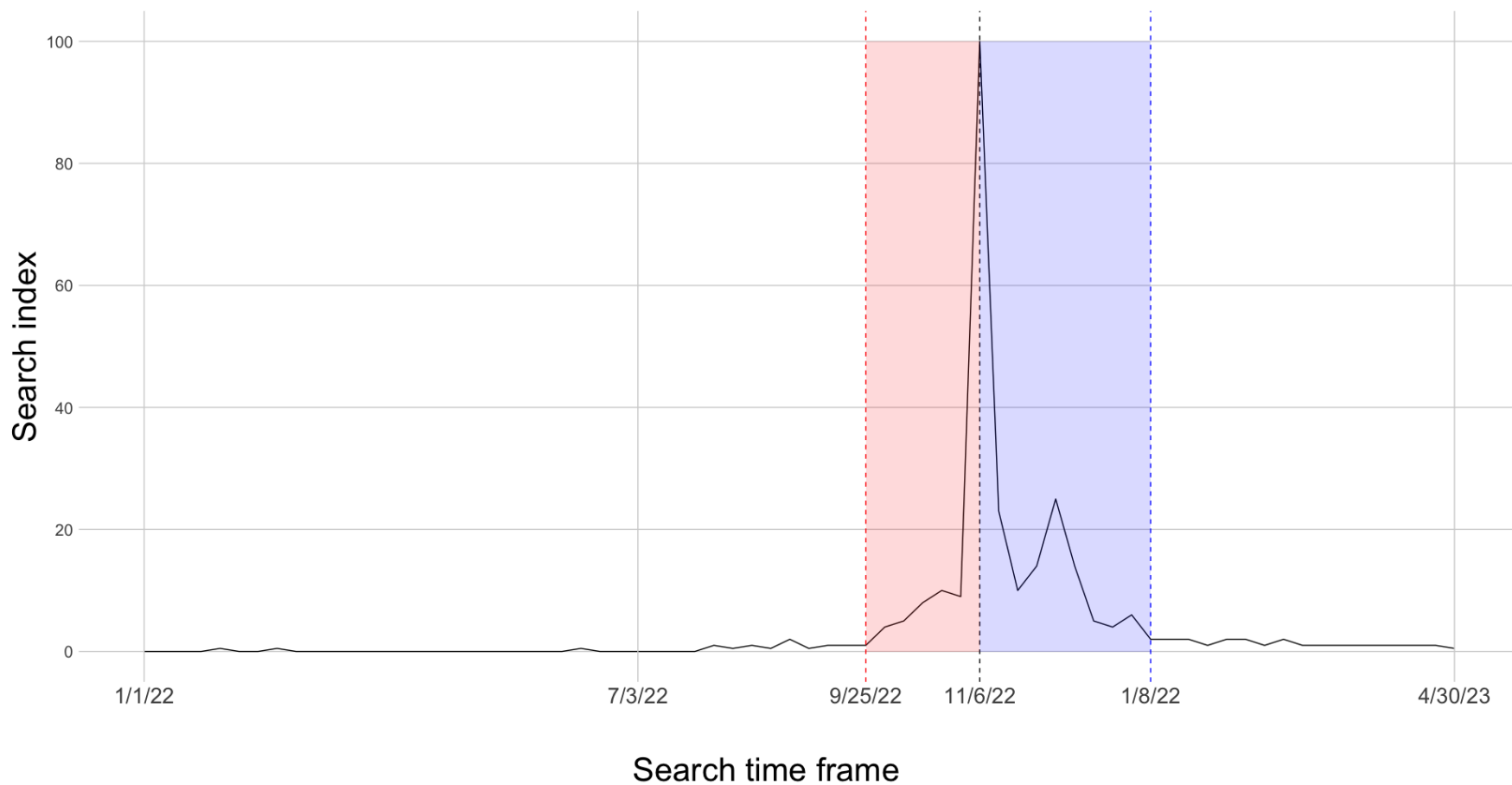


Figure 2: Google Trends search for Measure 114

We use Google Trends to examine Oregonians' awareness of Measure 114. The observation period is from January of 2022 until April 2023. Google Trends observations are at the week level. The search index is calculated that the week with the highest number of searches in the observation period is 100, and all other weeks are normalized to that week.



## Oregon vs. Synthetic Oregon

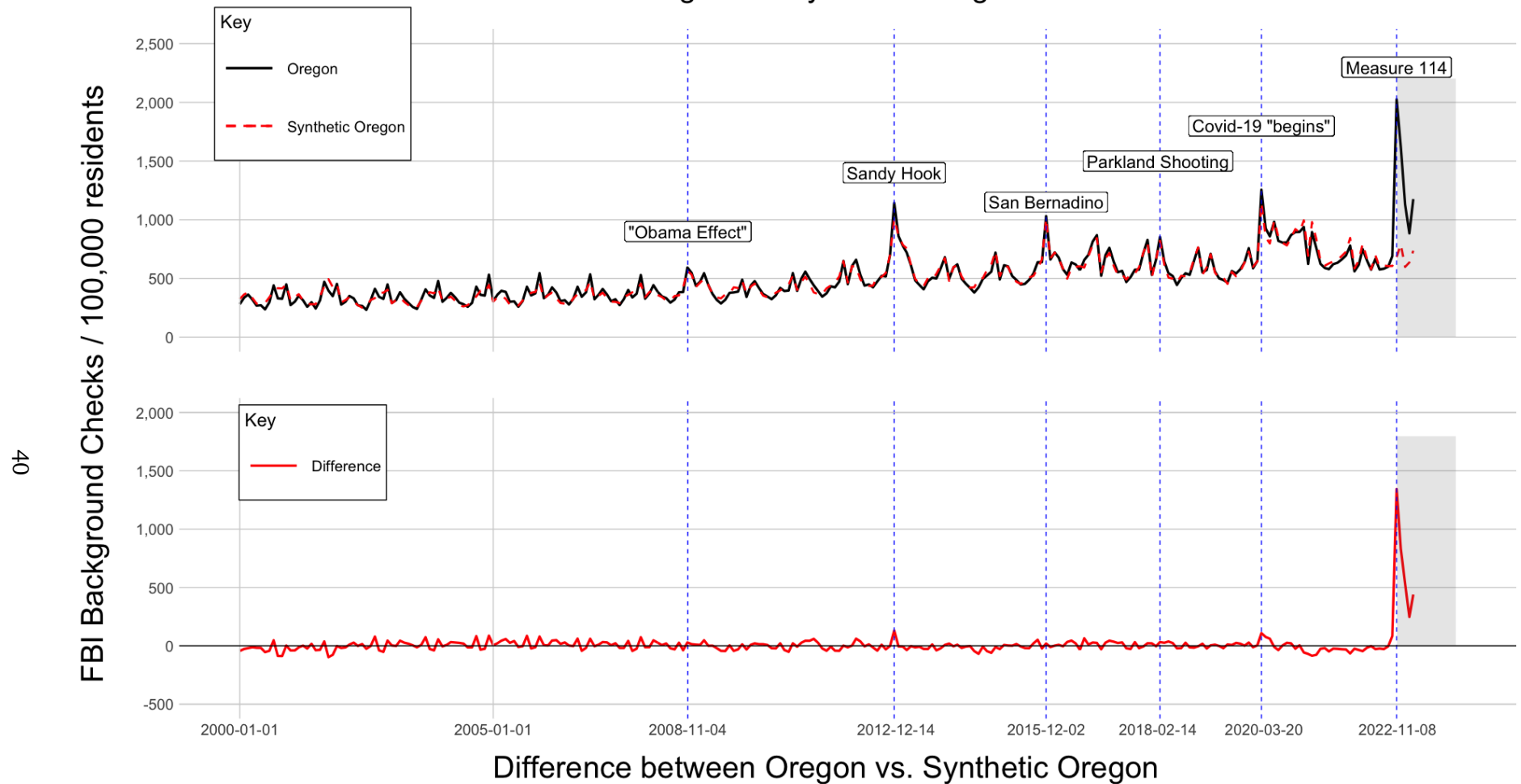


Figure 3: Synthetic control difference-in-difference for Oregon background checks (per capita)

Background check data is Non-private gun sales subset of FBI data. Outcome variable, background checks, calculated as a per capita (100,000 state residents). The top panel confirms previous findings that Mass-shooting Events and fear of gun control that led to changes in gun purchasing behavior (e.g., Obama's election in 2008).

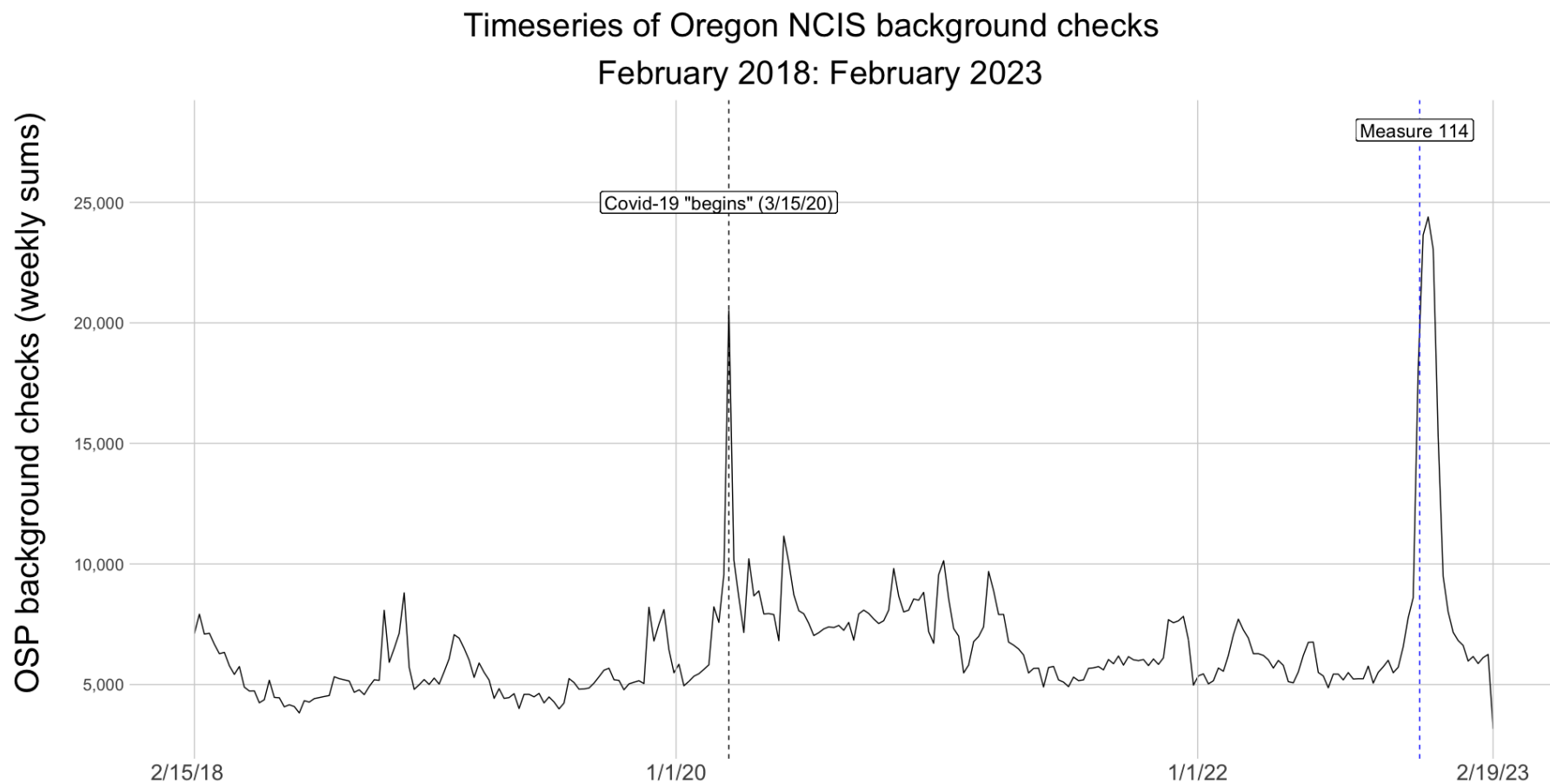


Figure 4: Weekly timeseries (2018-2022)

*Background checks provided by Oregon State Police from Feb. 2018 through Feb. 2023. Data has been aggregated from the daily level to weekly checks. There are two significant spikes in checks: Covid lockdown begins and Measure 114 passes.*

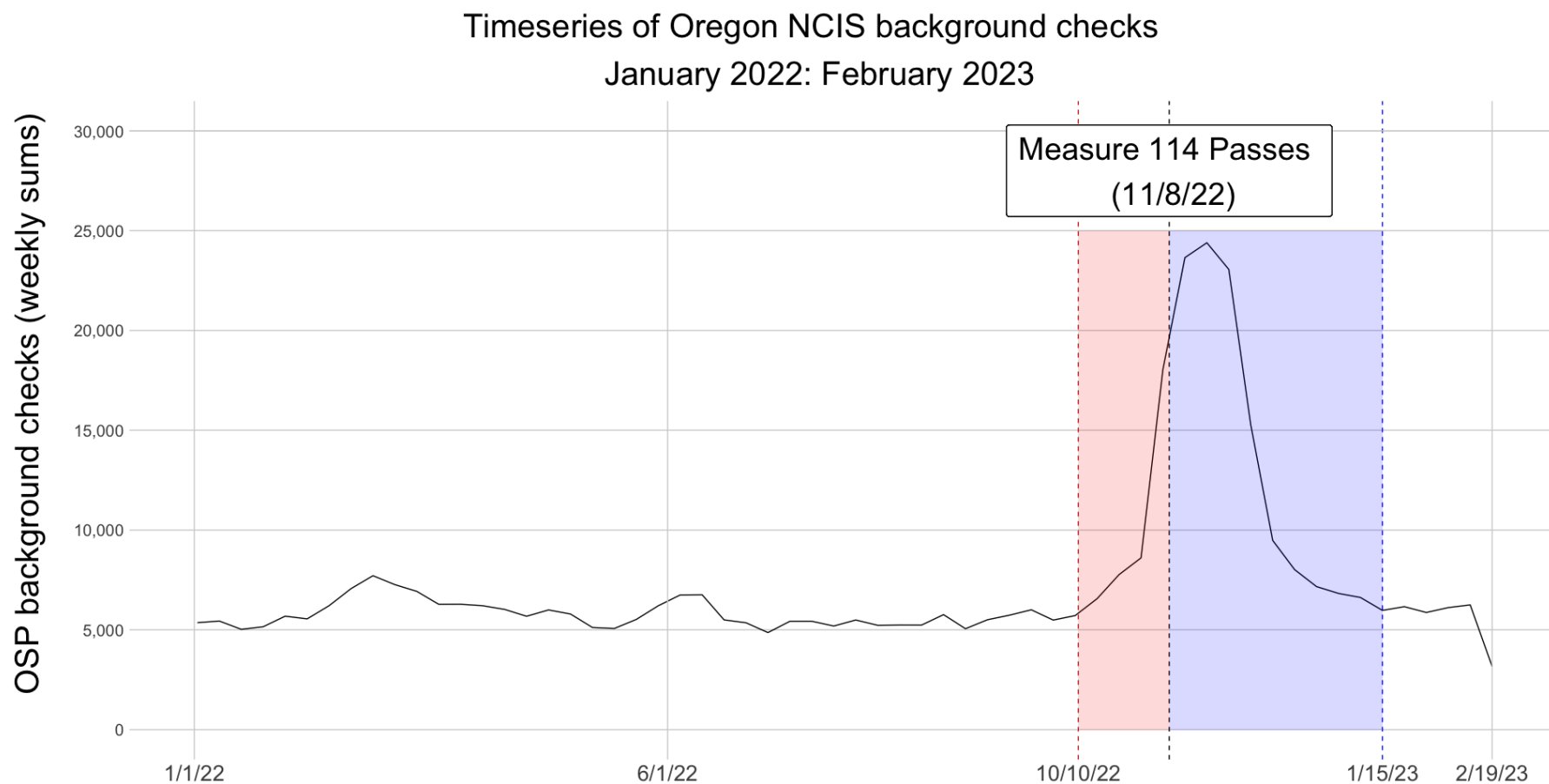


Figure 5: Weekly timeseries (2022)

*Background checks provided by Oregon State Police from Jan. 2022 through Feb. 2023. Data has been aggregated from the daily level to weekly checks. Anticipatory behavior appears to begin in October (marked in red). Increased sales persist through mid-January (marked in blue).*

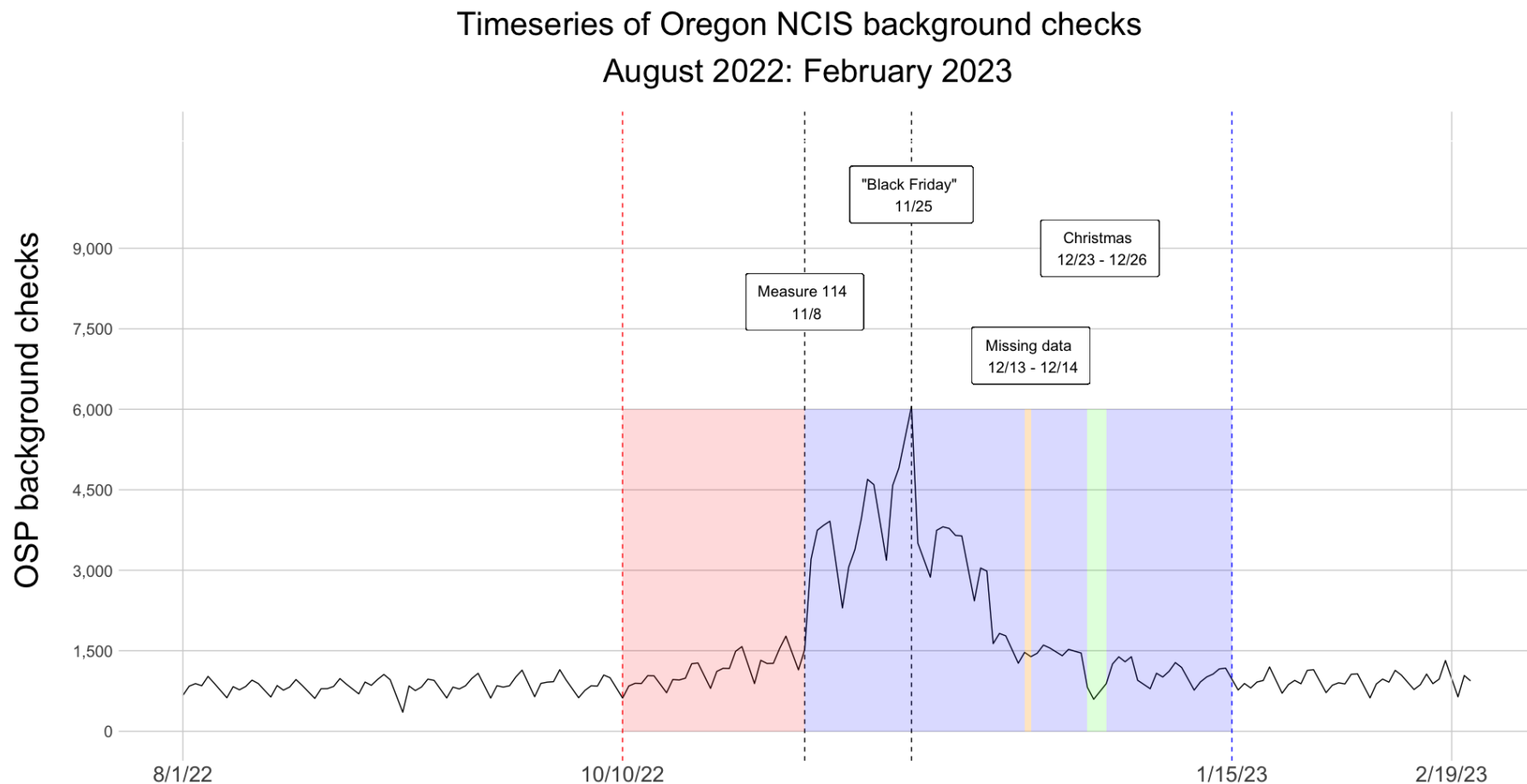


Figure 6: Daily timeseries

*Background checks provided by Oregon State Police from Aug. 2022 through Feb. 2023. Sunday and Monday of each week were dropped to improve interpretability. Anticipatory behavior appears to begin in October (marked in red). Increased sales persist through mid-January (marked in blue). The highest number of background checks are recorded on “Black Friday.” There were a significant number of background checks ( 2000) recorded on 12/13 and 12/14 but were attributed to an “Unknown” county and were dropped. Background checks decreased significantly near and on Christmas.*

Effect of Measure 114 on background checks (2022)  
Reference point week of 10/30/22: 11/06/22

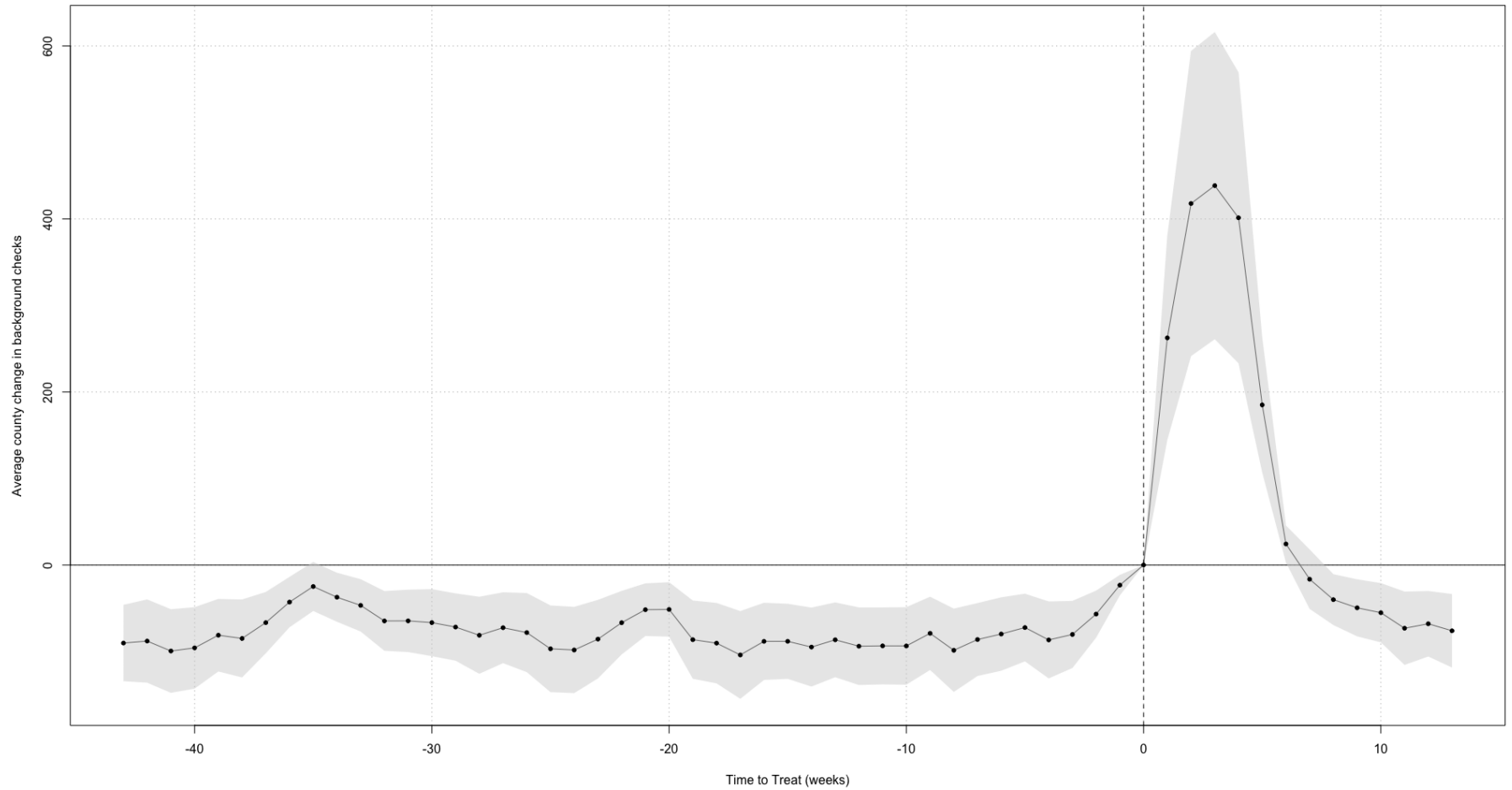


Figure 7: Event Study for OSP background checks (11-08)

*Background checks aggregated to the week. Event date is the week of Election Day (11/08). Point of reference: 11-08-22. We see consistently statistically significant negative observations preceding our event date, suggesting that background checks increased before our event date.*

Effect of Measure 114 on background checks (2022)  
Reference point week of 10/2/22: 10/8/22

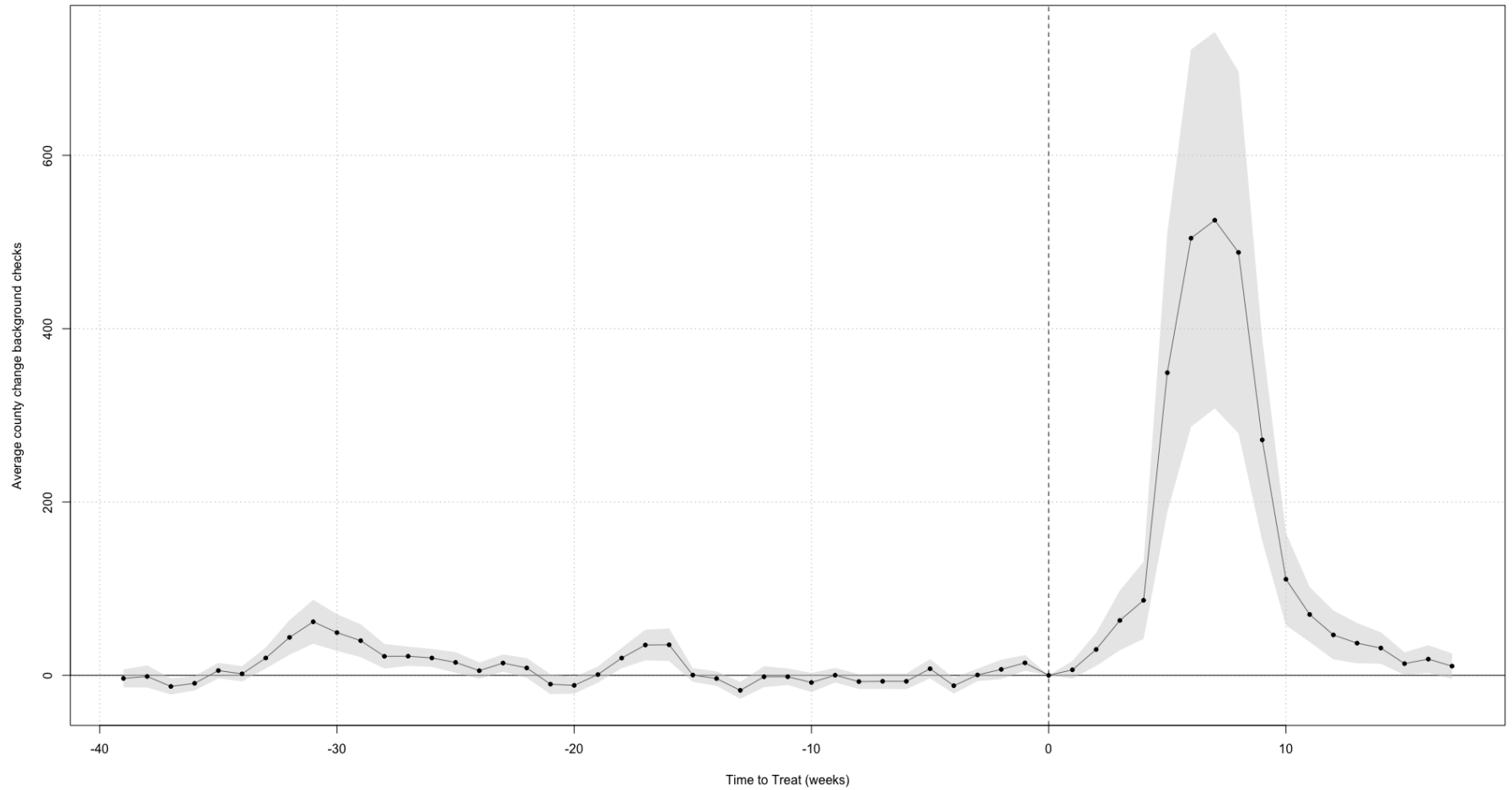


Figure 8: Event Study for OSP background checks (10-2)

Background checks aggregated to the week. Event date is the week of (10/10). Point of reference: 10-02-22. We see consistently statistically significant negative observations preceding our event date, suggesting that background checks increased before our event date.

Weekly background checks by county support for Measure 114  
Jan. 2022 through Feb. 2023

County voted for Measure 114 — No — Yes

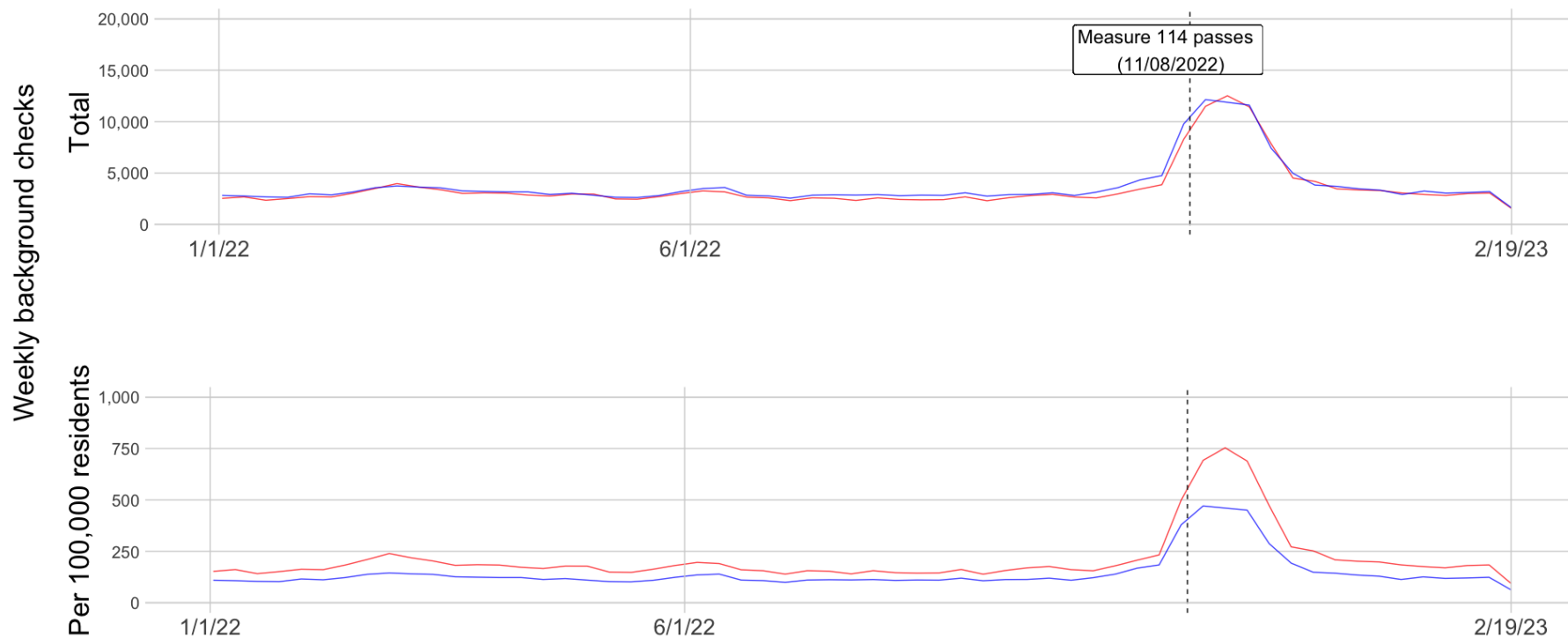


Figure 9: Timeseries for by majority vote share (2018-2022)

Background checks aggregated to the week. Background checks are separated into two groups: counties that voted in majority for Measure 114 (blue) and counties that voted in majority against Measure 114 (red). The top panel measure total checks and the bottom panel measures checks per capita (100,000 residents).

# Weekly background checks by county support for Measure 114 Feb. 2018 through Feb. 2023

County voted for Measure 114 — No — Yes



Figure 10: Timeseries by majority vote share (2022)

Background checks aggregated to the week. Background checks are separated into two groups: counties that voted in majority for Measure 114 (blue) and counties that voted in majority against Measure 114 (red). The top panel measure total checks and the bottom panel measures checks per capita (100,000 residents).



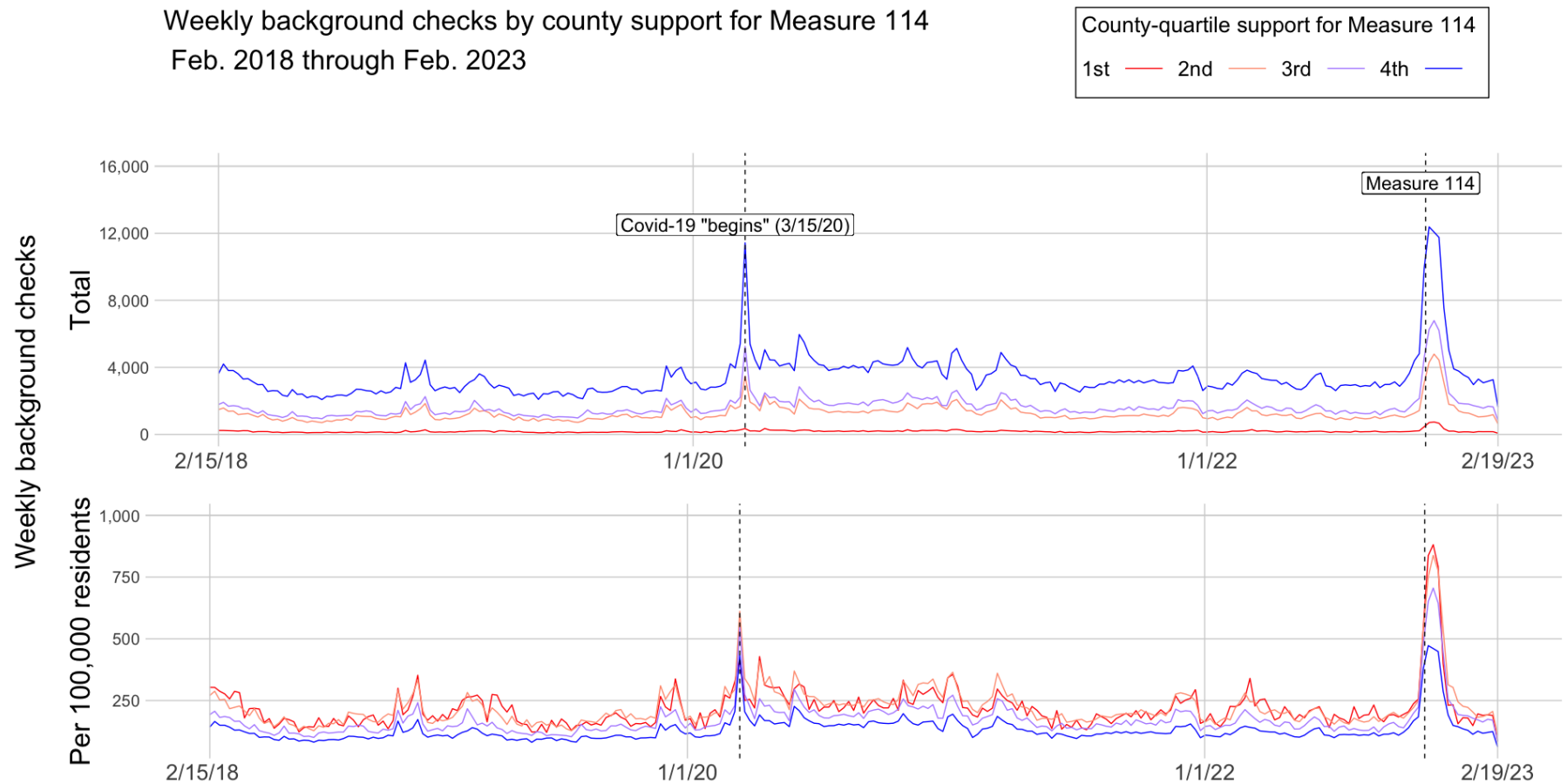


Figure 11: Timeseries by quartile vote share (2018-2022)

*Background checks aggregated to the week. Background checks are separated into quartiles (9 counties per quartile). Quartile is decided by votes for Measure 114. The first quartile for support for Measure 114 is [11%, 20.8%], the second quartile is (20.8%, 31%), the third quartile is (31%, 43.5%], and the fourth quartile is (43.5%, 74%]. The top panel measure total checks and the bottom panel measures checks per capita (100,000 residents).*

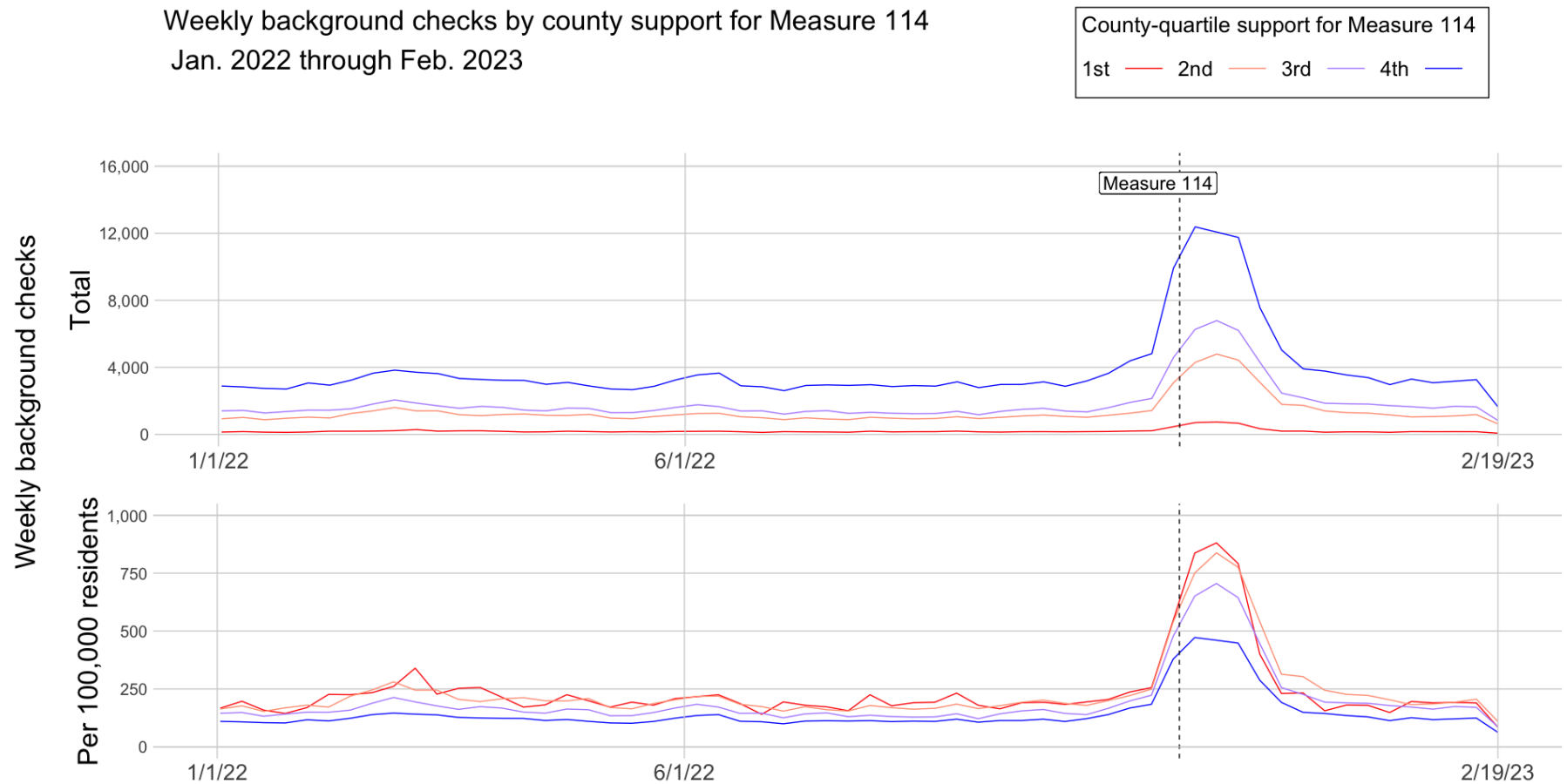


Figure 12: Timeseries by quartile vote share (2022)

Background checks aggregated to the week. Background checks are separated into quartiles (9 counties per quartile). Quartile is decided by votes for Measure 114. The first quartile for support for Measure 114 is [11%, 20.8%], the second quartile is (20.8%, 31%), the third quartile is (31%, 43.5%], and the fourth quartile is (43.5%, 74%]. The top panel measure total checks and the bottom panel measures checks per capita (100,000 residents).