Qualified Immunity Reform’s Effects on Crime: A Preliminary Statistical Analysis [ROUGH DRAFT]

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## Introduction and Background

Qualified immunity is a court-established doctrine that shields government officials from personal liability for constitutional violations unless the officials violated clearly established laws. After the killing of George Floyd sparked movements against police violence around the country, many activists directed their attention towards qualified immunity as a subject of reform. Activists argued that qualified immunity prevents police officers from being held accountable for excessive uses of force within civil court. Supporters of qualified immunity argued that efforts to limit qualified immunity would prevent police officers from effectively performing their job out of fear of frivolous lawsuits. In this statistical report, we aim to provide preliminary data-driven insights on the effects of recently passed qualified immunity reform on violent and property crime rates in major urban jurisdictions.

Four states have passed measures to limit qualified immunity: Colorado, Connecticut, New Mexico, and New York. Of those states, Colorado passed its reform the earliest, with its measure taking effect June 19, 2020. As a result, we decided to analyze Colorado crime data to determine the effects of qualified immunity on crime rates. In particular, our research question was as follows: Was the passage of qualified immunity reform in Colorado in June of 2020 correlated with significantly larger proportional increases in average daily violent and property crime incidents compared to increases in control jurisdictions? Because statewide incident-level data for 2020 and 2021 YTD from Colorado was not publicly available, we further narrowed the scope of our analysis to Denver and Colorado Springs, the two largest jurisdictions within Colorado.

Although we attempt to establish some level of causation in this study through the use of a synthetic control method, we lack the volume of observational data needed to successfully establish causation. In particular, we are missing observations on several key lurking variables, including the effects of COVID on poverty rates in each jurisdiction, 2020 and 2021 census data, community attitudes towards policing as a result of the George Floyd protests, amidst several other control variables. Much of this data will only be released a few years from now, severely limiting the contours of the present analysis. However, due to the prescience of the qualified immunity question and the need for data within the debate, we decided to produce this preliminary report to at least illustrate the plausible effects of qualified immunity on crime rates in Colorado. None of the findings in this report should be interpreted as demonstrating a conclusive causal relationship between qualified immunity reform and crime.

## Methodology

In this study, we utilized a synthetic control method following Abadie’s guidelines from his article “Using Synthetic Controls: Feasibility, Data Requirements, and Methodological Aspects.” In a synthetic control method, researchers create a weighted average of jurisdictions with the goal of minimizing the distance between the weighted average and the true jurisdiction’s pre-treatment predictor and response values. The synthetic control method has the advantage of systematically identifying the strongest control jurisdictions based on predictor numbers over time, as opposed to manually identifying a geographically close jurisdiction and claiming the existence of sufficient connection to isolate the effects of the policy intervention, thus removing the effects of researcher bias from the analysis. Generally, the synthetic control method is then followed by a series of placebo synthetic control constructions to determine if the post-treatment to pre-treatment RMSPE (root mean squared prediction error) ratio for the treated jurisdiction is extreme compared to those of placebo synthetic controls. If employed successfully, the synthetic control method can successfully prove the existence of a causal connection between different variables.

In practice, however, we faced critical data limitations that made it impossible to run the most statistically robust version of the synthetic control method. Because we lacked widely available 2020-21 post-treatment crime and census data (we only had that data for very specific jurisdictions), we could not create a series of placebo synthetic controls with post-treatment RMSPE values to compare the RMSPE ratio of the treated jurisdiction with. In addition, we only had access to a very small number of time periods (2011-19) and included an enormous sample of jurisdictions within the donor pool (many of which were substantially different from the treated jurisdictions) due to the rarity by which we could obtain 2020-21 data from police departments, significantly increasing the potential for bias. To continue the data analysis under these limitations, we primarily utilized the synthetic control methodology to identify similar jurisdictions to Denver or Colorado Springs and to provide weights for some of those jurisdictions.

Because no database of city predictors and violent/property crime rates over 2011-2019 previously existed, we created a new database from scratch. Using American Community Survey (ACS) census data from 2011 to 2019, we recorded each jurisdiction’s name, single female-led family household percentage, high school graduate or higher percentage, percentage of the population who lived in the same house they lived in a year ago, percentage of the population who were over 18, percentage of the population who were white, percentage of the population who were self-employed, unemployment rate, median income, child poverty rate, and the percentage of housing units occupied by their owners. Each of these predictors were identified as possible or significant predictors of violent and property crime within metropolitan and nonmetropolitan counties in a study published by researchers Wells and Weishelt (Explaining Crime in Metropolitan and Non-Metropolitan Counties). We then combined the city predictor data with violent crime, property crime, and population statistics from Uniform Crime Reporting (UCR) Crime in the United States fact tables. Our final full database included over 83000 observations and 20 variables, each observation representing a jurisdiction at a particular year. We then filtered the database to only include cities above 50000 in population to remove small rural jurisdictions that would likely not match the dynamics of more urban areas like Denver. Additionally, we removed jurisdictions with missing data on violent/property crime rates or missing yearly data.

Since neither the UCR nor the ACS published data for 2020-2021, we utilized the Synth package to create a synthetic control model for Denver and Colorado Springs from 2011-2019. We optimized the synthetic control model for 2016 to 2019 to obtain jurisdictions that could follow the most recent trends in both Denver and Colorado Springs. We then identified the top 5 jurisdictions with the highest weights and reran the synthetic control model with only those jurisdictions to recalculate the weights. With those identified control jurisdictions, we submitted requests for incident-level crime data from those departments for 2019-2021. When those requests were either unanswered or denied (as in the case of Ann Arbor Police Department), we removed the city from the synthetic control model and reran the model until we obtained at least four police departments with accessible incident level crime data whose plot looked at least somewhat similar to the plots of the treated jurisdictions (Denver and Colorado Springs).

We subdivided all 2019-2021 incident-level crime data into property and violent crimes based on UCR definitions. In particular, we categorized murder and nonnegligent homicide, aggravated assault, robbery, and forcible rape (including sexual assault with an object, fondling, and forcible sodomy) as violent crimes. We categorized larceny charges, burglary, damage/destruction of property, arson, shoplifting, pocket-picking, and motor vehicle theft as property crimes. We calculated the daily numbers of violent and property offenses for June of 2019 to June of 2020 (before qualified immunity reform) and June of 2020 to June of 2021 (after qualified immunity reform) in control and treated jurisdictions. We then subtracted the daily numbers of violent and property offenses in the 2019-20 time period from the 2020-21 time period and divided by the total number of violent or property offenses in the 2019-20 time period to make the daily numbers of violent and property crimes proportionate to each jurisdiction’s respective crime numbers. Finally, we created a bootstrapped null distribution assuming no true difference between the daily increases of the synthetic jurisdiction compared to Denver or Colorado Springs and calculated a p-value based on the probability of observing the real difference or greater between Denver/Colorado Springs and the respective synthetic control difference based on the null distribution.

This methodology had a few other critical limitations. First, because of the lack of 2020 and 2021 census data, we could only identify jurisdictions with similar recorded attributes to either Denver or Colorado Springs until 2019 which does not entail that those similarities in control predictors continued until 2020 and 2021. In addition, since we did not have access to UCR data for 2020 and 2021, we had to use 2011-2019 weights in 2020 and 2021 calculations, which may extrapolate beyond the capabilities of the synthetic control, since the trends between the predictor variables and the responsive crime rates may not continue into 2020 and 2021. Second, because of the several denied requests (especially from Ann Arbor and Clarksville Police Departments), we were forced to rely partially on convenience sampling in order to successfully carry out the study. Despite this, the plots comparing the synthetic control property/violent crime rates with the Denver property/violent crime rates look strong enough to indicate that the synthetic controls at least partially match the crime trends of the treated jurisdictions even when certain jurisdictions were removed due to lack of data. Third, because we needed to determine if the increases between the 2019-20 time period and 2020-21 time period were significantly greater than increases in control jurisdictions, we were forced to employ a test where we subtracted daily crimes in one time period from daily crimes in another time period. This method may have substantially exaggerated the standard deviation of violent and property crimes, since daily fluctuations in crime do not remain constant over the course of a year. The test may have been more successful on a monthly level, but we did not have enough monthly difference data to successfully arrive at statistical conclusions through simulation. Fourth, because we had to standardize the daily crime numbers by dividing crime numbers from some relative figure for each jurisdiction (in this case, the total number of offenses in the 2019-20 time period), smaller jurisdictions disproportionately influenced the variance of the synthetic control, since standardized daily fluctuations of 1-2 offenses were much greater In small jurisdictions compared to larger jurisdictions. See Appendix 1 for further details.

## Exploratory Data Analysis

We began by visualizing monthly violent crime and property crime numbers in Denver from 2016 to 2021.

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**Fig. 1: Monthly Violent and Property Crime Numbers in Denver 2016-2021:** Each bar represents the number of reported incidents in a single month. Red bars represent the months following the passage of the police accountability legislation in June 19, 2020. Data from June 20-30 is included in the month immediately preceding the red bars (June 2020). The black dotted line represents 10 reported violent incidents or 50 reported property incidents above the previous maximum number of offenses in a single month in the four years prior to legislation.

After the passage of the police accountability legislation in June 19, 2020, Denver experienced some increase in violent crimes. In particular, both July and August of 2020 had more violent crimes in a single month than the previous years’ record for violent crimes in a single month. Denver’s violent crimes then decreased over the fall and winter before increasing again next summer, following seasonal patterns. We could interpret Denver’s violent crime increase as part of Denver’s steady yearly increases in violent crime since 2016.

On the other hand, Denver’s property crime incidents increased far more dramatically than its violent crimes did. In **every month** following the passage of the police accountability legislation, Denver experienced more property crimes than the city had in any single month in the previous 4-5 years. Denver’s property crimes also did not decrease to normal levels as Denver’s violent crimes did. Importantly, however, Denver’s property crime increase seems to have begun around March or April of 2020 instead of June, possibly implying that other factors (such as COVID) may have partly fueled the rise in property crime.

To determine if Denver trends reached other jurisdictions, we also included the following two graphs exploring crime trends in Colorado Springs. Although our preliminary analysis has not gotten far enough to successfully compare Colorado Springs with a synthetic control, we believe that the incorporation of Colorado Springs may still provide important insights on the replicability of Denver trends.

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**Fig. 2: Monthly Violent and Property Crime Numbers in Colorado Springs 2016-2021:** See Fig. 1 for details on how to interpret the above 2 graphs.

Similarly to Denver, Colorado Springs may have also experienced some increase in violent crime, although only one month after the passage of the police accountability bill surpassed the previous record for the highest number of violent crimes in a single month.

Diverging from Denver trends, however, Colorado Springs lacked the large rise in property crime numbers that Denver saw after the passage of qualified immunity. In fact, Colorado Springs property crimes may even have decreased over the same time period. Figure 2 demonstrates the possibility that factors specific to Denver (such as worse recessionary effects from COVID or greater distrust of police officers) may have led to (or at least exacerbated) the major increase in property crimes, as opposed to the statewide police accountability bill causing the increase.

We then directly compared Denver’s violent and property crime monthly numbers with synthetic control numbers from 2019 to 2021 (the only publicly available data) to determine if Denver’s trends diverged from similar jurisdictions’ crime trends.

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**Fig. 3: Monthly Violent and Property Crime Numbers in Denver Compared to Control 2019-2021:** The red dashed line represents the passage of the police accountability legislation in June 19, 2020. Although the control continues to mostly track Denver violent crimes even after the passage of the police accountability law, Denver property crimes far surpass the control after April of 2020.

The control appears to closely track Denver’s monthly violent crime numbers both before and after the passage of the police accountability law. Although Denver experienced some increases in the summer immediately following the passage of the police accountability bill that were not fully matched by the synthetic control, Denver’s numbers soon fell comfortably into the control model’s range.

On the other hand, Denver’s increase in property crime numbers was significantly greater than increases in other jurisdictions. From roughly February 2020 to July 2020, Denver property crimes steadily increased, while synthetic control numbers remained stagnant. Denver property crimes also remained high even after the summer, maintaining its much higher position compared to the synthetic control even as late as June of 2021. However, the graph comparing property crime numbers between Denver and the synthetic control unit is also much weaker than the violent crime graph, with the two lines significantly deviating from each other even before the passage of qualified immunity reform. The weaknesses in the graph may speak to possible dissimilarities between the synthetic control for Denver property crimes and true Denver.

Finally, we took the daily differences between the synthetic control and Denver between the two periods (the differences that we would later simulate on) and graphed the daily proportional increases in violent and property crimes.

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**Fig. 4: Daily Violent and Property Crime Increases in Denver Compared to Control:** Each point represents the increase from corresponding days in the year prior to the passage of the police accountability law to the year following the passage of the police accountability law. For instance, the number of violent crimes in August 8, 2019 subtracted from the number of violent crimes in August 8, 2020 and divided by the total number of violent crimes from June 19, 2019 to June 18, 2020 represents one point at August 8, 2020 on the left graph. The trendlines represent the smoothed averages for each jurisdiction. Points on the first graph are made more transparent due to increased clutter.

The left graph does not appear to depict any significant daily increase in violent crimes in either Denver or the synthetic control unit. The points are relatively randomly scattered and the trendlines are both roughly at 0 except for some divergence as the summer of 2021 approached. The graph also indicates the existence of a possible outlier on June 2, 2021. Upon further examination, the point at June 2, 2021 is mostly explainable by an extremely large amount of crimes in Fort Smith in June 1, 2020 (the dates don’t exactly match due to the extra leap day in 2019-2020) followed by 0 crimes in June 2, 2021. Since the difference was much below any other difference values, we decided to remove the June 2, 2021/June 1, 2020 data from the analysis to prevent means from being skewed.

On the other hand, daily property crimes in Denver significantly increased, while the synthetic control trendline remained roughly at 0. Thus, even when comparing synthetic control differences with Denver, Denver still experienced a much larger increase in property crimes.

After analyzing the raw numbers and figures, we ran a series of models and tests to determine if Denver experienced a significant increase in property and violent crimes after the passage of the police accountability law. Although we do have population-level data from the attempted censuses of reported crimes, we still use statistical inference methods to account for possible yearly/daily variations based solely on chance.

## Analysis

### Synthetic Control Modelling

We began by creating the database of all census cities/towns within America from 2011-2019 that had both UCR and ACS data. From there, we filtered the database to only include cities greater than 50000 in population to identify roughly 500 similar jurisdictions to Colorado Springs and Denver. Using each jurisdiction’s violent and property crime rates, we created synthetic jurisdictions based on the weighted averages of all 500 jurisdictions (weights determined through proximity to predictor and response values of the treated jurisdictions). The resulting graphs comparing the true treated units with their synthetic control models pre-treatment over 2011-19 are shown below.

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**Fig. 5: Synthetic Control Graph of All Jurisdictions for Denver Violent Crimes:** The line tracks the yearly violent crime rates of Denver and the weighted average of the > 500 jurisdictions based on UCR numbers. The violent crime rate is obtained by dividing the number of violent crimes by the UCR population and multiplying by 100,000.

The first graph comparing Denver’s annual violent crime rates with the synthetic control model’s is relatively strong. The synthetic control model’s annual violent crime rates roughly match most of the trends and the level of Denver’s violent crime rate prior to treatment in 2020. There are some imperfections; for instance, Denver’s random yearly fluctuation isn’t fully matched each year, although Denver’s overall trend is roughly captured in the model.

Due to limitations in obtaining 2020 and 2021 data, I limited the calculation of the weighted average to the top 4 jurisdictions and recalculated the synthetic model to obtain new weights (the 5th jurisdiction, Ann Arbor, denied our FOIA request). The true fit of the model to pre-treatment Denver is depicted below:

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The synthetic of the 4 cities is slightly higher in terms of level but still follows the trend of true pre-treatment Denver roughly correctly. Thus, we determined the synthetic control to be similar enough to the real Denver to safely proceed with statistical testing.

We then created graphs to compare the synthetic control for Denver property crimes with true Denver.

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**Fig. 7: Synthetic Control Graph of All Jurisdictions for Denver Property Crimes:** The line tracks the yearly property crime rates of Denver and the weighted average of the > 500 jurisdictions based on UCR numbers. The property crime rate is obtained by dividing the number of property crimes by the UCR population and multiplying by 100,000.

The synthetic control for Denver property crimes does not successfully track Denver property crimes prior to 2017. From 2017 to 2019, the plot is roughly correct in comparing property crime rates, but the significant drop in the synthetic model from 2016 to 2017 indicates that the model may be somewhat limited in its explanatory potential and ability to cast a counterfactual for Denver. Excluding Ann Arbor, we refitted the synthetic control model with the 5 most highly weighted jurisdictions.

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The synthetic of the top 5 cities still does not successfully track Denver property crimes very closely. This may reflect crucial limitations in the ability of the synthetic control model to account for Denver property crime trends. Regardless, we will proceed with caution until we are able to create a stronger synthetic control.

### Statistical Bootstrapping Simulations

After collecting data from each of the 4-5 jurisdictions identified in each test along with Denver, we calculated the daily differences in violent and property crime between the June 19, 2019 to June 18, 2020 time period compared to the June 19, 2020 to June 19, 2021 time period (the first time period also had an extra day from the leap year). In particular, we corresponded the dates so that the number of violent crimes on June 19, 2019 was subtracted from the number of violent crimes on June 19, 2020 and created a dataset of these differences in violent and property crime numbers. These differences were then divided by the total number of violent or property crimes in the first period of time. We divided by the total number of crimes in the previous period as opposed to the population in order to account for jurisdictions which began from already-high crime rates and the proportionately smaller increase in crime rate that the same absolute increase in crime would entail.

To calculate the synthetic control differences for comparison, we used the weights in the previous section and multiplied them by the proportional daily differences in crime between the two periods. We then summed up the proportional daily differences and joined the two datasets together. We used bootstrapping to create a null distribution of 10000 differences in mean centered at 0 and determined if the probability of observing the difference between the mean proportional average daily increase in Denver or Colorado Springs with the mean proportional average daily increase in the synthetic control or greater was low enough to justify concluding that Denver or Colorado Springs’ increase in violent crime was significantly greater than control jurisdictions.

#### Denver Violent Crime Tests

A table of the synthetic control jurisdictions for Denver violent crimes with weights is shown below:

**Table 1: Synthetic Control Weights for Denver Violent Crimes**

|  |  |
| --- | --- |
| Name | Weight |
| Seattle, Washington | 0.496466442 |
| Fort Smith, Arkansas | 0.487715311 |
| Champaign, Illinois | 0.011537542 |
| Houston, Texas | 0.004280705 |

We generated the following two hypotheses:

. The true mean daily proportional difference in number of violent offenses between the June 2020 to June 2021 time period compared to the June 2019 to June 2020 time period in Denver, CO is equal to the true mean daily proportional difference in number of violent offenses between the two time periods in the synthetic control.

. The true mean daily proportional difference in number of violent offenses between the June 2020 to June 2021 time period compared to the June 2019 to June 2020 time period in Denver, CO is greater than the true mean daily proportional difference in number of violent offenses between the two time periods in the synthetic control.

= 0.01

Although we had population-level data, we utilized a bootstrapped simulation and hypothesis testing to determine if the difference between the Denver increases and the synthetic control increases could have resulted purely from chance. We bootstrapped 10000 differences in mean assuming no true difference in mean between Denver increases and synthetic control increases and graphically depicted the null distribution below.

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**Fig. 9: Null Distribution of Denver Violent Crime Differences in Mean with Control:** Each observation in the histogram represents a single simulated difference in mean between Denver and the synthetic control. The red dotted line refers to the observed difference in mean. We took all observations at the observed value or greater and divided by the total number of simulated values to arrive at the p-value.

Because the p-value of 0.0592 is greater than a reasonable alpha level of 0.01, we fail to reject the null hypothesis. The data does not provide sufficient evidence at the 1% or 5% level that Denver’s average daily increase in violent crimes from the 2019-20 time period to the 2020-21 time period is significantly greater than the synthetic control’s average daily increase in violent crimes. However, the data does provide sufficient evidence at the 10% level that Denver’s average daily increase in violent crime after the passage of the police accountability bill is significantly greater than the synthetic control’s average daily increase in violent crime.

We also conducted a monthly difference in difference test using the synthetic control model as the “control” jurisdiction, since the graph modeling the trends of the synthetic control graph with true Denver trends indicated the possibility of parallel yearly violent crime trends between the synthetic control model and Denver, although the levels of the two models did not exactly match. By utilizing monthly data and linear modeling for 2019-2021, we decreased the influence of daily crime fluctuations on the results while simultaneously retaining sufficient data points to draw some statistical conclusions.

We created the dummy variables of “time” and “treated” for this end. “Time” takes the value of 1 after June 19, 2020 in both the synthetic control and Denver (with June 20-30 falling under the June 1 value due to monthly numbers), representing the passage of the police accountability legislation. “Treated” takes the value of 1 for Denver and 0 for the synthetic control, representing the jurisdiction designations. The linear model is shown below:

**Table 2: Difference in Difference Test for Denver Violent Crimes**

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
| Term | Estimate | Std. Error | Statistic | P-value |
| (Intercept) | 390.732 | 13.913 | 28.084 | 0.000 |
| time1 | 18.503 | 21.998 | 0.841 | 0.404 |
| treated1 | -15.177 | 19.676 | -0.771 | 0.444 |
| time1:treated1 | 37.942 | 31.110 | 1.220 | 0.228 |

The interaction variable for variables “time” and “treated” reflects the difference in difference estimate. Because the p-value of 0.277 far exceeds a reasonable alpha level of 0.01, we fail to reject the null hypothesis. The data does not provide sufficient evidence on the monthly level that the passage of qualified immunity reform in June 19, 2020 in Denver corresponded to an increase in violent crime that outpaced other control jurisdictions.

#### Denver Property Crime Tests

The following jurisdictions and weights were utilized to construct the synthetic control for Denver property crimes:

**Table 3: Synthetic Control Weights for Denver Property Crimes**

|  |  |
| --- | --- |
| NAME | weight |
| Austin, Texas | 6.935988e-01 |
| Champaign, Illinois | 2.188792e-01 |
| Seattle, Washington | 8.747524e-02 |
| Fort Smith, Arkansas | 4.654831e-05 |
| Houston, Texas | 1.805219e-07 |

Since Houston had a negligible weight, we decided to exclude Houston from the analysis and run the bootstrapping with data from the other 4 jurisdictions.

. The true mean daily proportional difference in number of property offenses between the June 2020 to June 2021 time period compared to the June 2019 to June 2020 time period in Denver, CO is equal to the true mean daily proportional difference in number of violent offenses between the two time periods in the synthetic control model.

. The true mean daily proportional difference in number of violent offenses between the June 2020 to June 2021 time period compared to the June 2019 to June 2020 time period in Denver, CO is greater than the true mean daily proportional difference in number of violent offenses between the two time periods in the synthetic control model.

= 0.01

Once again, we bootstrapped 10000 differences in mean, assuming that the null hypothesis is true. We graphically depicted the null distribution below:

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**Fig. 10: Null Distribution of Denver Property Crime Differences in Mean with Control:** Each observation in the histogram represents a single simulated difference in mean between Denver and the synthetic control. The red dotted line refers to the observed difference in mean. We took all observations at the observed value or greater and divided by the total number of simulated values to arrive at the p-value. This time, the observed value far surpasses any of the values of the null distribution.

Because the p-value of 0 is far less than a reasonable alpha level of 0.01, we reject the null hypothesis. The data provides sufficient evidence to indicate that Denver’s average daily proportional increase in property crime after the passage of qualified immunity was significantly greater than control cities’ increase in property crime over the same time period.

We did not employ a monthly difference in difference test because the parallel trends assumption is clearly violated. The graph comparing the synthetic control trends with the true Denver trends is not parallel, especially from 2016-2017 (see Figure 8).

## Results and Discussion

The data does not provide evidence to indicate that Denver’s increase in violent crime after the passage of police accountability legislation significantly exceeded the violent crime increase in control cities. However, the data does suggest that Denver’s increase in property crime did exceed the property crime increase in similar cities without qualified immunity reform. Unfortunately, because of the critical limitations in our data (see Methodology), we are unable to state that the police accountability legislation *caused* an increase in property crime in Denver. In fact, our exploratory data analysis may even provide evidence to the contrary; although Denver experienced a large rise in property crimes, Colorado Springs (the second largest jurisdiction in Colorado) did not experience the same rise, substantiating the claim that Denver-specific dynamics propelled the increase in property crime. Once data from the census is released, we urge the conducting of more research to explore possible factors that may explain the rise in property crime in Denver that did not seem to be experienced at the same level elsewhere, even in the same state.

Our analysis is not over. In the future, we plan to take several steps to arrive at a stronger conclusion about whether qualified immunity reform had any clear effects on crime rates or police conduct. More immediately, we are creating a synthetic control for Colorado Springs to determine if Colorado Springs’ increase in property crimes also exceeded control cities’ increase. In addition, we also wish to eventually incorporate other large jurisdictions within Colorado, such as Boulder or Aurora, into the analysis to determine what crime trends can best explain crimes within the rest of Colorado. For instance, it is fully possible that Colorado Springs’ lack of a property crime increase could be the outlier, while the rest of Colorado experienced an increase in property crime. In that case, the data would provide more support for the conclusion that qualified immunity reform could cause increases in property crime. In the long-term, we also expect to incorporate 2020 census data (as well as data on the COVID recession, police trust, and police employment) into the analysis as predictor variables to help statistically control for alternative explanations.

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## Appendix 1: Inflated Standard Deviations of Smaller Jurisdictions

The following graph is a density plot of the four jurisdictions that comprise the synthetic control for Denver’s violent crimes and their daily standardized crime numbers.

Chart

Description automatically generated

**Fig. 11: Density Plot of Synthetic Control Standardized Differences in Violent Crime:** Each of the numbers on the x-axis represents the difference between the number of violent crimes on a 2020-21 day and a 2019-20 day divided by the total number of violent crimes in the 2019-20 period. Noticeably, the spreads of each jurisdiction are correlated with their respective populations.

The density plot illustrates that jurisdictions with higher populations (such as Houston and Seattle) tend to have daily numbers that concentrate much more strongly at 0, while jurisdictions with lower populations (such as Fort Smith and Champaign) tend to have daily proportionate numbers that fluctuate far more widely, thus influencing the variation of the synthetic control. This effect is due to the significantly lower daily numbers of crimes that happen in smaller jurisdictions, causing the 1-2 crime number fluctuations that happen from day to day to comprise a larger proportion of the total number of violent crimes in the jurisdiction. The effect cannot be corrected by dividing by population instead.