Estimating the Effect of Speed Cameras on Traffic Collisions

Seamus Long

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1 Introduction

The use of speed cameras is readily studied, and has been shown to decrease both the number of accidents, and their severity [Wilson and Bellamy, 2010]. Much of the existing literature focuses on RCTs, or manned speed cameras (as opposed to automated). For example, Chen et al 2005 find that an RCT program in British Columbia led to a drastic reduction in fatalities from crashes [Chen et al., 2000]. However, the major shortcoming of this paper, and others like it, is that the study period is only a year, which does not allow for examination of the long-run effects of such a program.

This pilot study adds to the existing literature by examining the short- and long-run effects of automated speed cameras through a difference in difference research design. I find that the 2019 automated speed camera program by the Province of British Columbia lead to a significant reduction in the number of all types of crashes (both those causing injury, or merely property damage), but that the treatment effect declined over time, and in some cases, became insignificant after 3 years. However, the confounding effect of Covid may be responsible for the change in collisions.

2 Data Sources

In 2019, the province of British Columbia passed a law that would convert 35 existing red light cameras to speed cameras when the light is green. The cameras were added to existing high traffic corridors in the following locations:

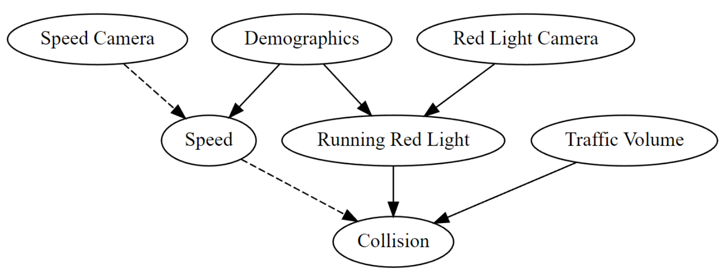
- 12 locations in Vancouver
- 7 locations in Surrey
- 3 locations in Burnaby
- 2 locations in Langley and Kelowna

- 1 location in Abbotsford, Coquitlam, Delta, Maple Ridge, Nanaimo, North Vancouver, Pitt Meadows, Port Coquitlam and Richmond

Using publicly available collision statistics from the Insurance Company of British Columbia (ICBC), this paper examines the effect of the program on collisions in the lower mainland [ICBC, 2023a]. I excluded Kelowna and Nanaimo from the sample, as their locations are significantly different, and I expect that regional weather and traffic pattern variations mean they should not be treated as part of the same sample. After dropping these observations, 32 treated intersections were included in the study.

The data includes the number of collisions in most of the intersections in the province, and provides a count of both collisions where a casualty occurred (injury or death), and those with property damage only (PDO). This data is available from 2018-2022, giving 2 years pre-treatment, and 3 years post. In addition to crash numbers and intersections, the data set indicates the municipality the intersection lies in. I have reached out to ICBC to acquire micro level data on specific accidents, or at least on a shorter time frame, but have not yet received a response.

In addition to the primary data set, I used a map of traffic cameras to manually add an indicator to those that had either a red light camera or a speed camera [ICBC, 2023b]. Finally, ICBC also reports the total number of collisions by region, which was used in the construction of a control for the effect of Covid-19 on traffic patterns. The data collected so far provides the minimum required to satisfiy identification of the effect of speed cameras on colisions, as shown in the DAG below.



This model represents a reduced form of the actual casual model, that includes things like weather, visibility, and road conditions. While those factors are relevant, we have assumed they are relatively constant across years, and across the sample. Since all observations are from the same region, the intra-year weather effects are likely minimal, and can be ignored. Additionally, as road conditions are not known we have assumed they are constant, as well as demographic factors, such as age and sex. Given this description of the model, with speed cameras only affecting collisions through speed, we have satisfied identification, if we hold demographics constant.

3 Pre Processing and Matching

The principle issue in this research design was the construction of a suitable control group. Because the selection into treatment was not random, using the set of all other intersections provided a poor description of the counterfactual trend.

Data was initial filtered to include only intersections that recorded at least 20 collisions over the entire 5 year study period. This excluded minor intersections that experienced very few regular accidents, as they would not be suitable comparisons for the treatment group, leaving 2071 total intersections in the data set. Intersections that were included were scanned to see if every year was included, and in places that they were absent, a 0 was imputed. Because entries only existed for locations and years that had at least one accident reported, this practice has no risk of erroneously imputing values.

Once the data set was complete, propensity score matching in the pre-treatment period using a cross-validated logistic regression function from sklearn to assign intersections to the control group. To target matches based on parallel pre-treatment trends, as is required for dif-in-dif, the difference between the 2019 and 2018 crash observations were added to the variable set. This is because in a 2-period sample, the difference of the two observations is directly proportion to the slope. The final set of data used in matching uses only the time-trend difference in both PDO and Casualty crashes, a dummy variable for red light cameras, as well as dummies for the municipality. The flexibility of the matching program was enhanced by the inclusion of 3rd order polynomial and interaction term via the polynomialfeatures function, also from sklearn. Critically, the 'treatment' indicator was omitted from the matching program.

The matching program unfortunately provided relatively low propensity scores, with the highest being 0.80, however, the drop off in sample size was rapid. Ultimately, a cut-off propensity score of 0.3 was selected, as it resulted in a control group of 270 units, making the treatment group roughly 10% of the total sample. When increasing the propensity score cutoff to 0.35, only 124 units were included, and at 0.4 only 3 remained.

4 Main Results

Once the data was separated into treatment and control groups, a simple ordinary least squares regression could be performed to estimate the effect of the speed camera program on the number of collisions. The basic OLS equation was as follows:

$$Colissions_i = \beta_0 + \beta_1 Treatmenti + \beta_{2:5} Year_{2019:2022}) + \beta_{6:8}(Treated_i * Year_{2020:2022}) + \epsilon_i$$

In the above equation, Collisions represents the absolute count of colissions, either PDO, Casualty or Total (the sum of PDO and Casualty), depending on the regression. β_0 estimates the pre-treatment level of the control group for the first year, 2018, while β_1 is the marginal effect of being in the treatment group. This estimates the pre-treatment parallel difference between the treatment and control group. β_2 through β_5 measure the year-by-year trends. Of key interest are β_6 through β_8, which measure the treatment effect on the treated in 2020, 2021 and 2022. We expect to see a negative result here, as the additional of speed cameras should reduce the number of collisions.

Below, the results for each regression are presented in regression summary tables, as well in figures illustrating the trends, counterfactual, and confidence intervals. In the PDO regression, I find that the treatment resulted in a highly significant decrease in the number of accidents in 2020, that rapidly decayed, and became insignificant by 2022. When considering Casualty crashes only, the treatment is significant at the 5% level throughout the entire study period. Finally total crashes, the sum of both PDO and Casualty, experiences a decline in treatment effect over time, and loses significance in 2022, likely driven by the PDO result, which makes up the bulk of total crashes recorded.

In the first year of treatment, the program lead to approximately 30% fewer crashes in each category. This treatment effect is significantly higher than other studies on the topic, but may be driven by different sampling. This study used all reported accident statistics, while similar studies used coroners and police reports, which would only include the most severe accidents.

PDO Crash Regression

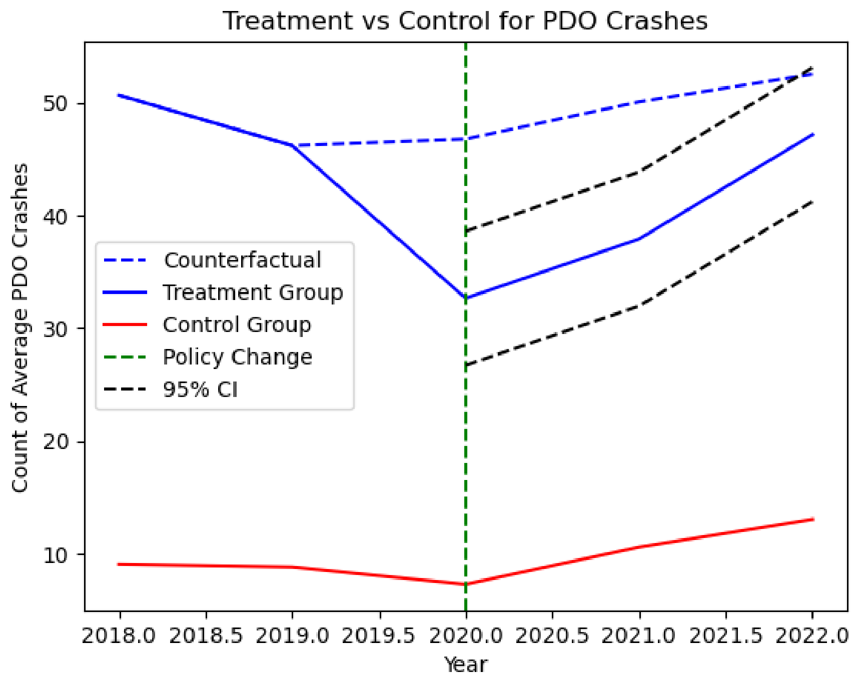
	coef	**std err**	**t**	**P$>|$t$|$**	**[0.025**	**0.975]**
Intercept	15.0269	0.307	49.012	0.000	14.426	15.628
Treatment	33.7299	1.751	19.265	0.000	30.298	37.162
Y2019	-0.6697	0.432	-1.551	0.121	-1.516	0.177
Y2020	-5.1843	0.434	-11.933	0.000	-6.036	-4.333
Y2021	-2.9317	0.434	-6.748	0.000	-3.783	-2.080
Y2022	-0.5673	0.434	-1.306	0.192	-1.419	0.284
Y2020:Treated	-10.9162	3.033	-3.600	0.000	-16.861	-4.972
Y2021:Treated	-7.9188	3.033	-2.611	0.009	-13.863	-1.974
Y2022:Treated	-1.0332	3.033	-0.341	0.733	-6.978	4.911

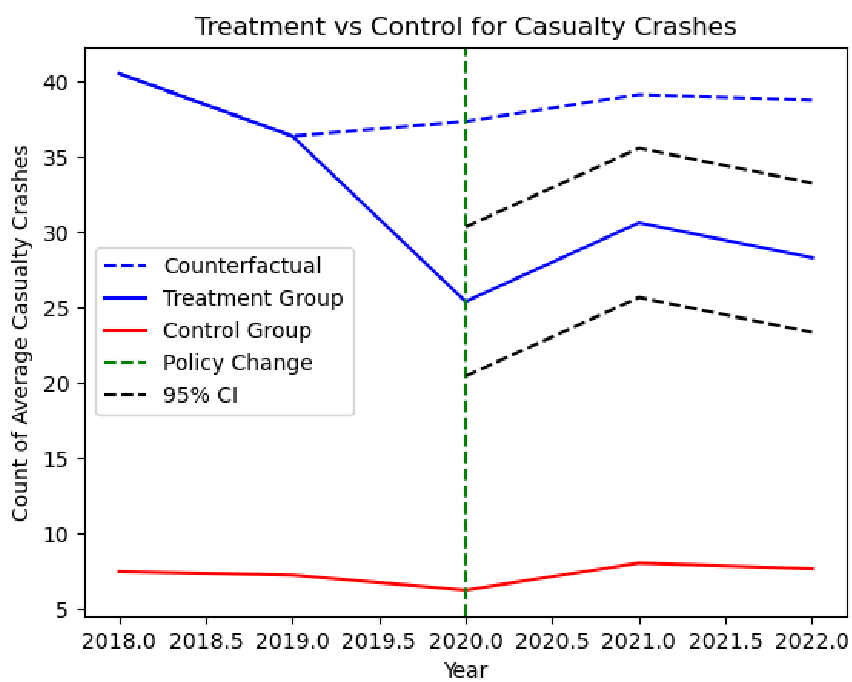
Casualty Crash Regression

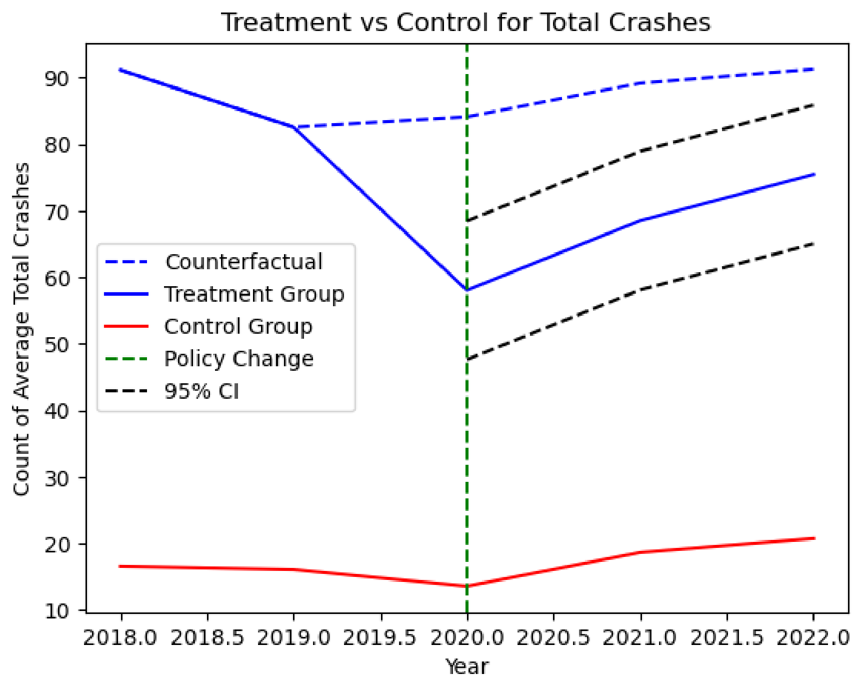
	coef	**std err**	**t**	**P> |t|**	**[0.025**	**0.975]**
Intercept	12.3346	0.255	48.348	0.000	11.834	12.835
Treatment	26.4084	1.457	18.126	0.000	23.553	29.264
Y2019	-0.6422	0.359	-1.787	0.074	-1.347	0.062
Y2020	-4.5803	0.361	-12.670	0.000	-5.289	-3.872
Y2021	-3.4641	0.361	-9.582	0.000	-4.173	-2.755
Y2022	-3.7873	0.361	-10.477	0.000	-4.496	-3.079
Y2020:Treated	-8.7877	2.523	-3.482	0.000	-13.734	-3.841
Y2021:Treated	-4.6852	2.523	-1.857	0.063	-9.632	0.261
Y2022:Treated	-6.6745	2.523	-2.645	0.008	-11.621	-1.728

Total Crash Regression

	coef	**std err**	**t**	**P> |t|**	**[0.025**	**0.975]**
Intercept	27.3615	0.538	50.899	0.000	26.308	28.415
Treatment	60.1383	3.070	19.590	0.000	54.121	66.156
Y2019	-1.3119	0.757	-1.732	0.083	-2.796	0.172
Y2020	-9.7646	0.762	-12.819	0.000	-11.258	-8.272
Y2021	-6.3958	0.762	-8.397	0.000	-7.889	-4.903
Y2022	-4.3546	0.762	-5.717	0.000	-5.848	-2.862
Y2020:Treated	-19.7039	5.317	-3.706	0.000	-30.126	-9.281
Y2021:Treated	-12.6039	5.317	-2.370	0.018	-23.026	-2.181
Y2022:Treated	-7.7076	5.317	-1.450	0.147	-18.130	2.715


Treatment vs Control for PDO Crashes
Counterfactual
Treatment Group
Control Group
Policy Change
95% CI
Count of Average PDO Crashes
Year
10
20
30
40
50
2018.0
2018.5
2019.0
2019.5
2020.0
2020.5
2021.0
2021.5
2022.0


Treatment vs Control for Casualty Crashes
Counterfactual
Treatment Group
Control Group
Policy Change
95% CI
Count of Average Casualty Crashes
Year
5
10
15
20
25
30
35
40
2018.0
2018.5
2019.0
2019.5
2020.0
2020.5
2021.0
2021.5
2022.0



The results in these graphs and regressions suggest that the speed camera program had a lasting, significant effect on collisions resulting in injuries, but did not affect the overall rate of accidents significantly. The initial drop in PDO crashes could be entirely attributed to Covid affecting traffic patterns, and it's climb to the return of commuter traffic. The return of commuter traffic to near pre-pandemic levels has been observed in California [Bhagat-Conway, 2023]. This suggests that while speed cameras may not be effective at reducing the total number of collisions, they are likely reducing the proportion of severe collisions occurring.

One other observation of note in the graphs is the weakness of the parallel trend in the pre-treatment period. While the control group has a downward sloping curve like the treatment groupm, the magnitude of these slopes are drastically different. This calls into question the results found, as difference-in-difference relies heavily on a persistent parallel counter factual. More work is required in matching to produce a better suited sample.

5 Robustness - Controlling for Covid

Because of the co-occurrence of the Covid-19 pandemic and the treatment, it is possible that the change in traffic volumes accounted for most, or all of the observed treatment effect. I attempt to control for possible uniform differences in traffic volumes by considering the number of collisions in an intersection as a fraction of the total number in the Lower Mainland. If Covid lockdown did lead to uniform differences in driving behavior, then this adjustment should control for the difference, and isolate the true treatment effect of the program.

As with the absolute difference model, a matching program was run to generate a suitable control group. Instead of using the difference in absolute collisions between 2018 and 2019, this model used the difference in relative collisions. Otherwise, the model was identical.

Unfortunately, this matching program failed to produce usable results, with the highest propensity score attained being less than 2%. It is possible that a better matching program could have been devised, but due to time constraints I opted to use the same control group computed for the first test.

The OLS regression equation is identical to the absolute trend, however, as stated previously, this model uses fractional collisions. The betas can be interpreted the same way, but this time representing absolute differences in fractions.

Below, the results for each regression are presented in regression summary tables, as well in figures illustrating the trends, counterfactual, and confidence intervals. In all three of the regressions the results were entirely insignificant at the 5% level across all treatment years.

PDO Fractional Crash Regression

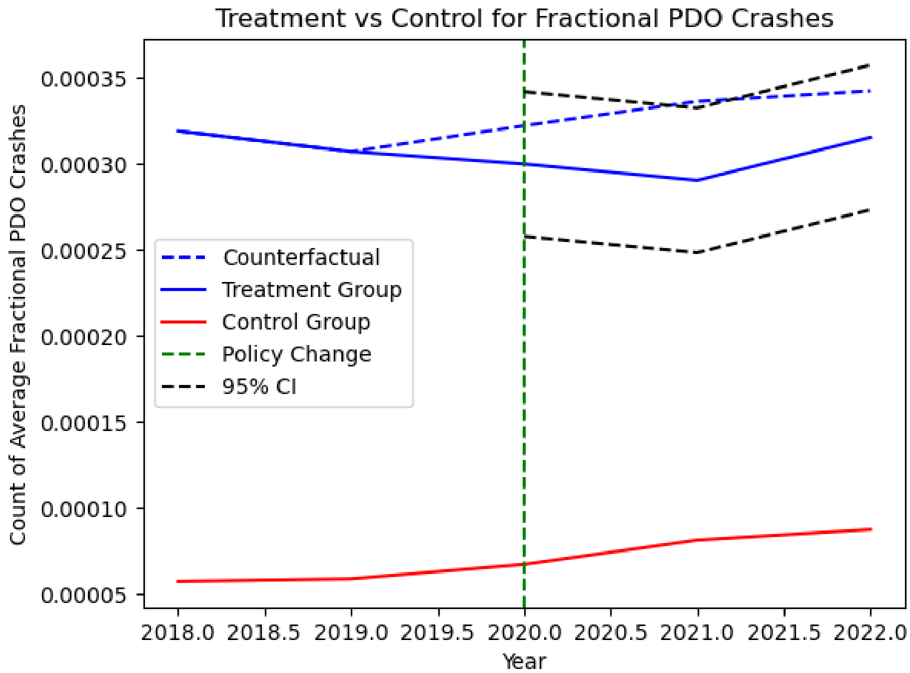
	coef	std err	t	P> |t|	[0.025	0.975]
Intercept	9.447e-05	2.17e-06	43.564	0.000	9.02e-05	9.87e-05
Treatment	0.0002	1.24e-05	17.589	0.000	0.000	0.000
Y2019	9.945e-07	3.05e-06	0.326	0.745	-4.99e-06	6.98e-06
Y2020	-4.17e-06	3.07e-06	-1.357	0.175	-1.02e-05	1.85e-06
Y2021	-1.898e-06	3.07e-06	-0.618	0.537	-7.92e-06	4.12e-06
Y2022	2.124e-06	3.07e-06	0.691	0.490	-3.9e-06	8.15e-06
Y2020:Treated	-8.508e-06	2.14e-05	-0.397	0.692	-5.06e-05	3.35e-05
Y2021:Treated	-2.026e-05	2.14e-05	-0.945	0.345	-6.23e-05	2.18e-05
Y2022:Treated	6.124e-07	2.14e-05	0.029	0.977	-4.14e-05	4.27e-05

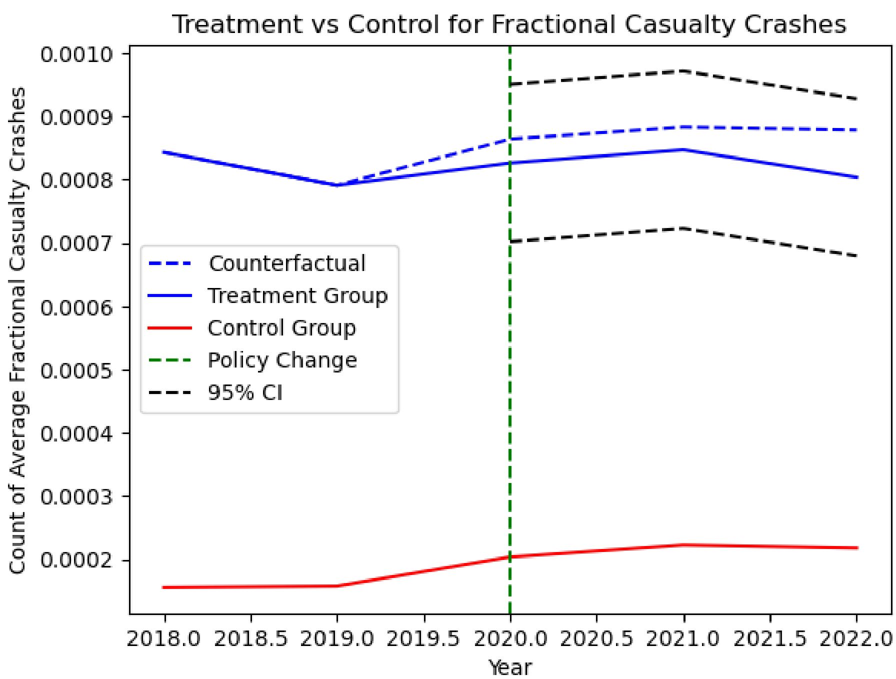
Casualty Fractional Crash Regression

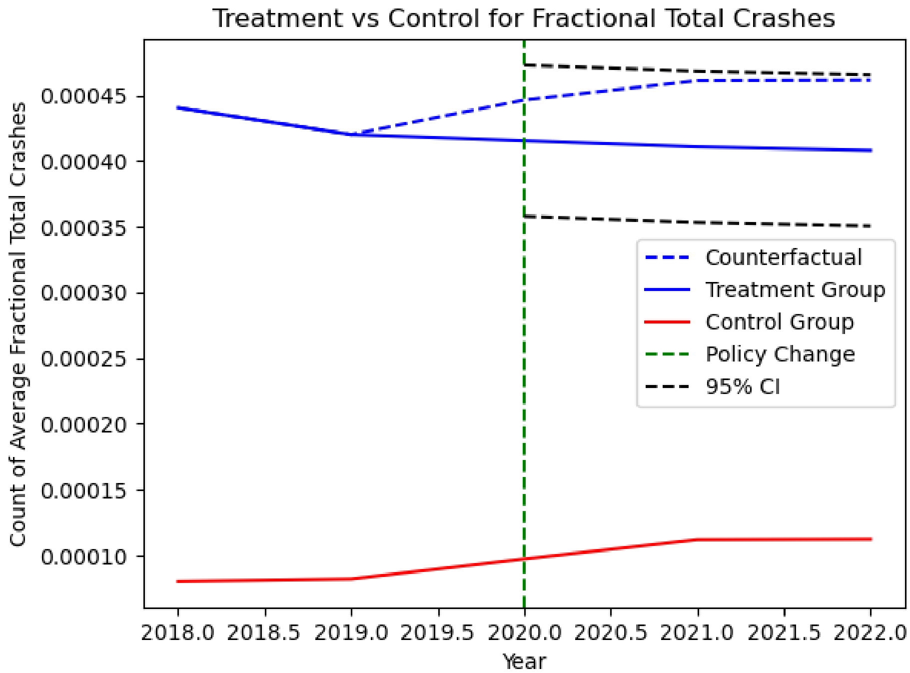
	coef	std err	t	P> |t|	[0.025	0.975]
Intercept	0.0003	6.41e-06	40.031	0.000	0.000	0.000
Treatment	0.0006	3.66e-05	15.338	0.000	0.000	0.001
Y2019	-1.883e-06	9.03e-06	-0.209	0.835	-1.96e-05	1.58e-05
Y2020	-4.114e-06	9.08e-06	-0.453	0.651	-2.19e-05	1.37e-05
Y2021	-1.095e-05	9.08e-06	-1.206	0.228	-2.88e-05	6.85e-06
Y2022	-1.371e-05	9.08e-06	-1.510	0.131	-3.15e-05	4.09e-06
Y2020:Treated	1.229e-05	6.34e-05	0.194	0.846	-0.000	0.000
Y2021:Treated	4.011e-05	6.34e-05	0.633	0.527	-8.42e-05	0.000
Y2022:Treated	-6.764e-07	6.34e-05	-0.011	0.991	-0.000	0.000

Total Fractional Crash Regression

	coef	std err	t	P> |t|	[0.025	0.975]
Intercept	0.0001	2.97e-06	44.485	0.000	0.000	0.000
Treatment	0.0003	1.7e-05	17.564	0.000	0.000	0.000
Y2019	6.075e-07	4.18e-06	0.145	0.885	-7.59e-06	8.81e-06
Y2020	-6.144e-06	4.21e-06	-1.460	0.144	-1.44e-05	2.1e-06
Y2021	-6.379e-06	4.21e-06	-1.516	0.130	-1.46e-05	1.87e-06
Y2022	-7.657e-06	4.21e-06	-1.820	0.069	-1.59e-05	5.9e-07
Y2020:Treated	-8.433e-06	2.94e-05	-0.287	0.774	-6.6e-05	4.91e-05
Y2021:Treated	-1.282e-05	2.94e-05	-0.436	0.663	-7.04e-05	4.48e-05
Y2022:Treated	-1.426e-05	2.94e-05	-0.486	0.627	-7.18e-05	4.33e-05


Treatment vs Control for Fractional PDO Crashes
Count of Average Fractional PDO Crashes
Year
0.00035
0.00030
0.00025
0.00020
0.00015
0.00010
0.00005
2018.0
2018.5
2019.0
2019.5
2020.0
2020.5
2021.0
2021.5
2022.0
Counterfactual
Treatment Group
Control Group
Policy Change
95% CI


Treatment vs Control for Fractional Casualty Crashes
Count of Average Fractional Casualty Crashes
Year
0.0010
0.0009
0.0008
0.0007
0.0006
0.0005
0.0004
0.0003
0.0002
2018.0
2018.5
2019.0
2019.5
2020.0
2020.5
2021.0
2021.5
2022.0
Counterfactual
Treatment Group
Control Group
Policy Change
95% CI



The insignificance of the treatment suggests that the speed camera program had no effect on the proportion of collisions occurring in each intersection, however, as the pre-treatment trends of the control and treatment group and of opposite slope, it is difficult to make the parallel trend argument required for difference in difference. Because of this, these preliminary results should be seen only as reason for performing further investigation into the subject.

6 Conclusions and Room for Further Research

This pilot study considered the effect of speed cameras on the number of collisions in major intersection in British Columbia, and found promising initial results. Through a difference-in-difference frame work, I find that the introduction of speed cameras lead to a lasting reduction in collisions resulting in injury, but did not significantly reduce long-run minor collisions. The parallel trend assumption required for difference in difference is not fully satisfied, and requires refinement. If a micro data set can be acquired from ICBC, it may be possible to employ more sophisticated methods, such as synthetic control, to establish a suitable control sample.

The effect of Covid on traffic patterns was controlled for by completing the same analysis with collisions a fraction of the total amount in the lower mainland per year as the outcome variable. This regression suffered from a failed matching program, and highly non-parallel pre-treatment trends, so no clear conclusions can be drawn from it. In a more robust micro data set, it will be possible to pair observations with actual traffic data to complete remove the effect of Covid by directly controlling for traffic patterns.

Beyond refining this research, there is also room for adjacent study, such as exploring the potential for negative spillovers from this policy caused by risky drivers choosing alternative roots. Such behaviour would lead to a reduction in collisions in the treated intersections at the cost of increased collisions in adjacent untreated ones, and may lead to a bias in the measurement of the treatment effect.

References

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