Analyzing governmental intervention in curbing crime using Sao Paulo as a case study

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To: **Danilo Freire**

From: **Muhammad Abdurrehman Asif, Gadi Borovich**

Date: **24 April 2020**

Re: **Validity of findings in Evaluating the Effect of Homicide Prevention Strategies in São Paulo, Brazil**

Abstract

Homicide rates in Sao Paulo took a dramatic increase in the 1990's. The Government intervened and policing efforts along with strictness on criminals increased. In this article we recreate findings from Freire (2018) using data and code made publicly available. Then we discuss the use and plausibility of the Synthetic Control methods (Abadie and Gardeazabal, 2003; Abadie, Diamond, and Hainmueller, 2010), as a way to extend and check the validity and robustness of the findings of Freire.

Replication

Freire, in his study, focuses on building a valid counterfactual to the case of interest - Sao Paulo. In the figures down below, we can see how the two dependent variables (homicide rates) follow almost the same exact trend from 1990 to 1998, but in the year when the policies were passed, there is a significant drop in the homicide rate for Sao Paulo compared to its counterfactual. The figure representing the gap in homicide rates between the treatment against the synthetic control reinforces this point by representing an effect of almost -20 deaths per 100,000 habitants in Sao Paulo.

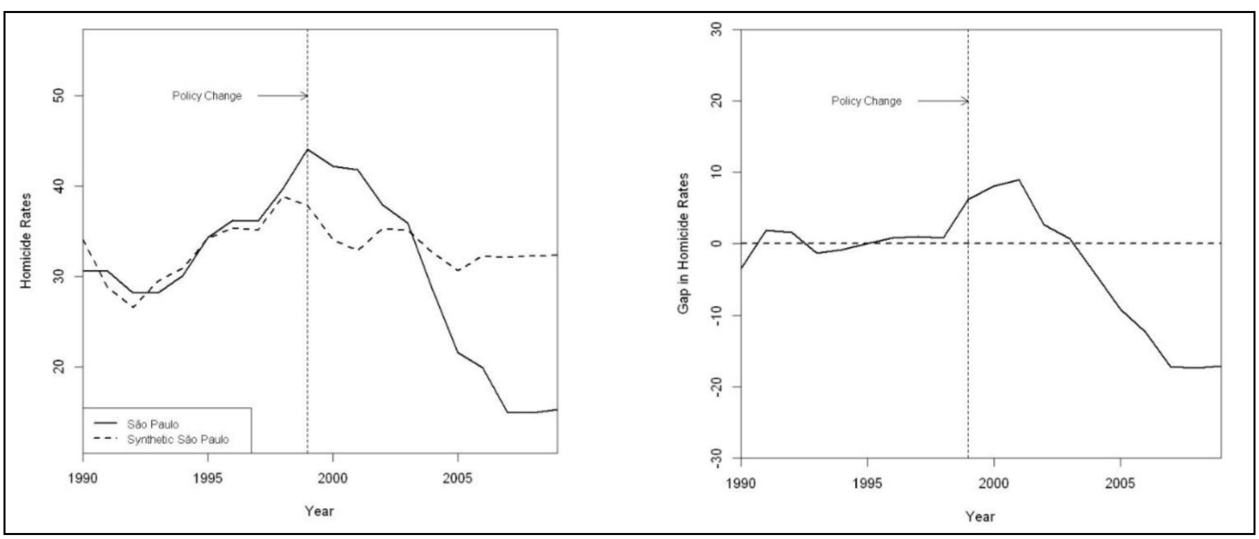

Figure I. Replicated visualizations from Freire's original paper (2018). The left graph represents homicide rates across time for the treatment variable (including policy change) against its synthetic counterfactual. The right graph represents the gap in homicide rate between the two variables, more clearly visualizing the effect size.

Extension

However, we argue that further robustness checking is needed to assess the validity of Freire's results. We will do an extension using principles outlined by Abadie, A., Diamond, A., & Hainmueller, J. (2015) where we analyze the robustness of the results across time, adding to the limited analysis in the original paper of placebo across space.

Robustness across time

This extension will show whether the gap in homicide rate is due to the difference between the treatment effect of Sao Paulo, and its natural change. Can the gap be attributed to the policy change, or is there other confounding variables or noise that are leading to the effect between the treatment and its counterfactual? To answer this question, we can do placebo across time. The effect can only be attributed to the policy if there is no gap assuming that the policy actually happened in years prior to 1999. Using statistical software, we ran simulations to evaluate the gap between the treatment and natural effects by introducing the placebo treatment in years prior to the real policy change. See Figure II below.

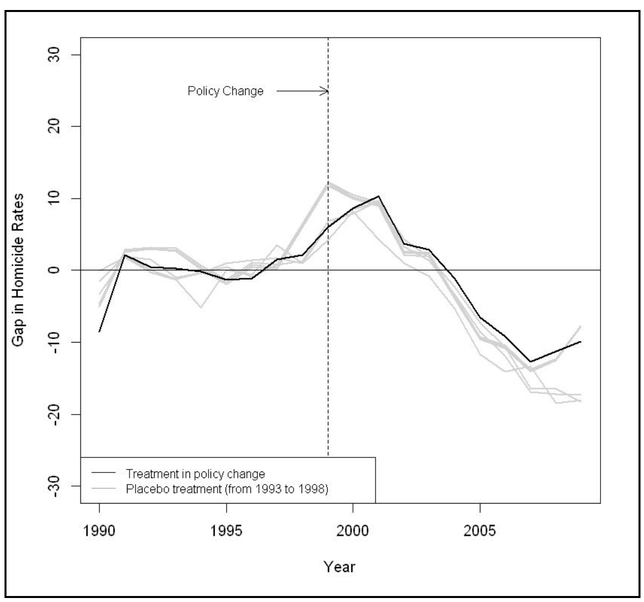

Figure II. This visualization represents the gap in homicide rates across time. Its purpose is to help analyze placebo across time. The black line is when the real treatment (policy change) occurred, and the grey lines represent the placebo treatments assuming that the treatment happened in years prior.

How should Figure II look like if the synthetic control would not be robust?

In Figure I, we saw how there is a significant effect size between the natural effect and the treatment effect after the policies were passed in 1999. However, we cannot conclude the validity of those results until we compare it with placebos across time. These allow us to test what happens if we assume that the treatment effect was not the actual policies but other factors that may have happened in years prior. If the synthetic control would not be robust, then there would be a drop in the gap of homicide rates before the actual policy, suggesting that there is noise in our control, and we would not be able to attribute the change to the policies.

However, Figure II shows that there is little variance between the actual treatment variable against the placebos until the date of the policy change in 1999. This shows that the effect is due to the policies, because otherwise we would see a drop in the gap in years prior. The fact that all variables overlap with little variance uptil 1999 further strengthens the claim in the original paper.

One reason why this graph is not sufficient on itself is the fact that we are only interested in the gaps until the time placebo we set, and since the time placebo varies for each individual plotted line, we cannot observe individually the appropriate year uptil which we should analyze each line. The line whose gaps are relevant til 1994 is muddled up with the one who is relevant til 1996, and therefore some sort of individual analysis is required.

Although there is a placebo treatment across multiple years plotted above, it would be valuable to individually look at certain years to visualize the difference between synthetic control and Sao Paulo. Time placebo's are valuable in looking at how closely the control and treated unit match up until the placebo year is set. Therefore, 2 years have been selected for this analysis. Since the dataset starts from 1990, and the actual treatment year is 1998, 1993 and 1996 are deemed as appropriate years due to the marginality between them - one's further away and one's closer to the treatment year.

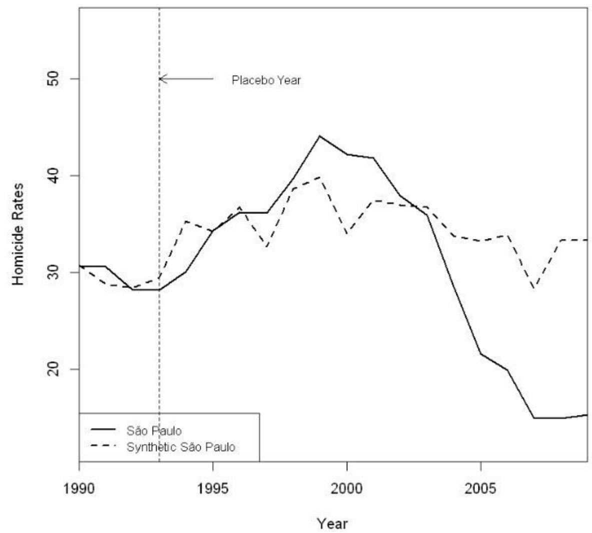

Figure III. Time Placebo for 1993. This graph is an extension from Figure II, where it only represents the placebo treatment assuming that the policies were passed in 1993.

As shown in the figure above, the synthetic control casts doubts on the findings due to the lack of proper matching. With just 3 years to align on, and many controls, there is still a large gap up until the treatment introduction. This can be attributed to a few years to match on and therefore a longer placebo might be more appropriate.

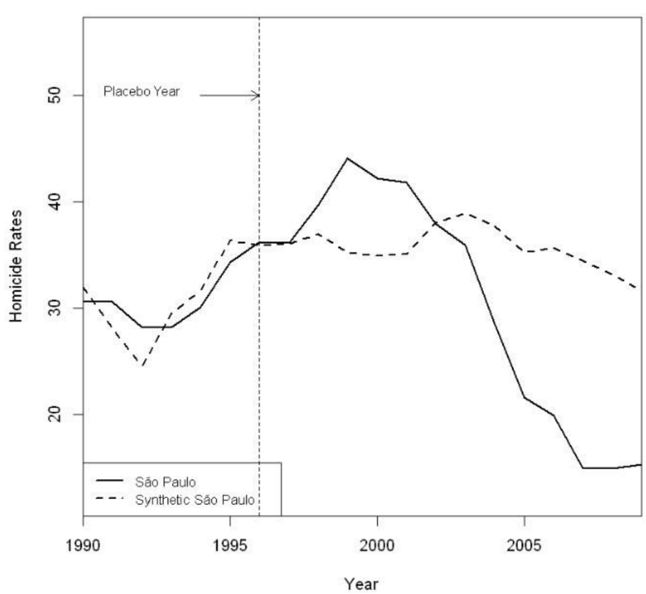

Figure IV. Time Placebo for 1996. This graph is an extension from Figure II, where it only represents the placebo treatment assuming that the policies were passed in 1996. It adds to Figure III as the placebo in 1996 allows us to analyze more data up to that date.

This has more interesting results. Up until 1992 the synthetic control does a terrible job of matching. Beyond this, it still never really performs well. This casts doubt on the entire synthetic control study and is alarming for the original findings. Although small variances may be expected, the data is very miniscule. There are a few years and there are very low numbers of homicide rates, therefore, even the smallest variance can seem large. It would therefore be better to calculate confidence intervals for each year and observe whether the confidence interval for the synthetic control overlaps with Sao Paulo.

Robustness Test

For the Robustness Test, the variables that make-up the synthetic control will be manipulated to see whether there is overfitting or bias introduced to the weighting of the controls.

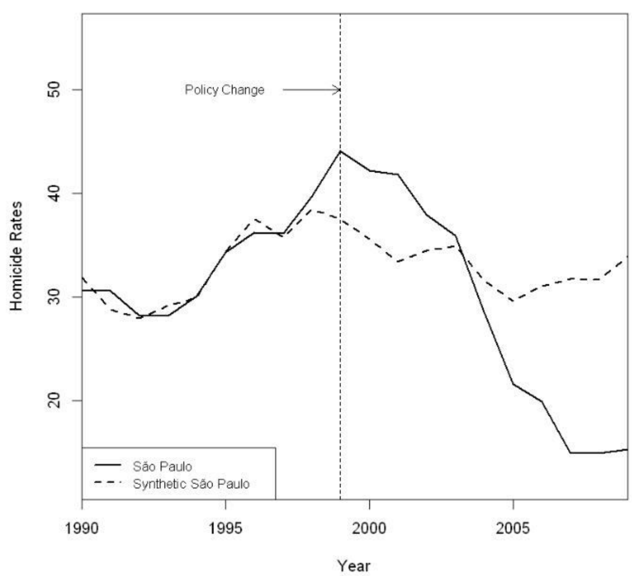

Figure V. In this graph, we are replicating Figure II's synthetic control but removing the state in the control group with the highest weight (Santa Catarina).

First we remove the highest weighted control, Santa Catarina, which makes up 26.2% of the control. This synthetic control arguably performs better than the original. It is interesting that the exclusion of one unit of control from a total of N=21 sample can lead to such a better match. The variance of predictors such as GDP and population vary so greatly between Brazilian cities can provide some sort of explanation for this. Appendix B contains the weights after matching.

Perhaps it would be better for the entire study to exclude Santa Catarina.

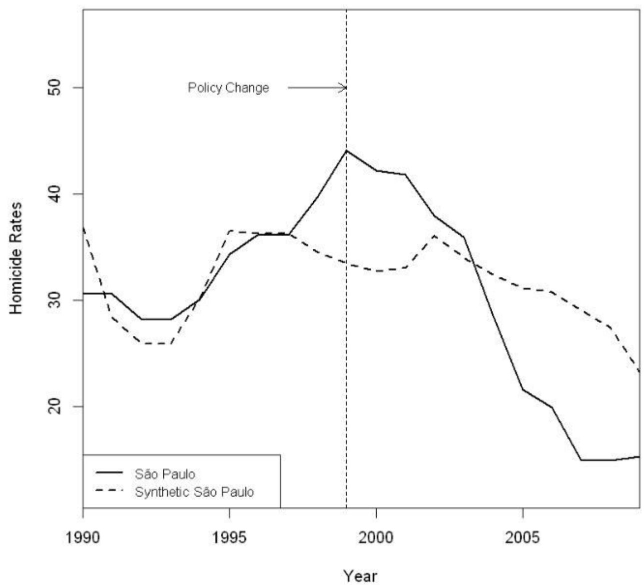

Figure VI. We do the same approach as the one used for Figure V but also removing the state with the second highest weight from the synthetic control.(Distrito Federal)

It is important to clarify that controls were removed iteratively. The top weighted cities were not removed from the original synthetic control, rather as new Synthetic variants of Sao Paulo were created, the cities which became the biggest component were removed. The figure above has lost the promise Figure V showed. Distrito Federal was removed for this. This reinforces the argument that (N - 2) for the control is not going to benefit the study, and therefore Distrito Federal is an important city to map out a synthetic Sao Paulo. The post treatment behavior is also noteworthy as it takes a downturn similar to that of Sao Paulo. Does this cast doubt on the findings and will the inclusion of more states and predictors map out the behavior of Sao Paulo and show perhaps a smaller treatment effect? There is an argument for it.

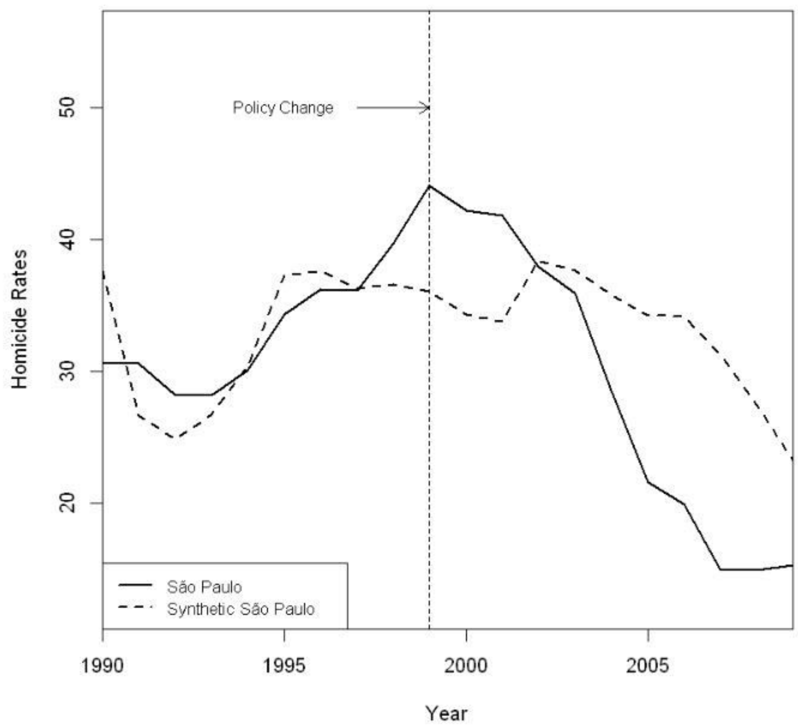

Figure VII. To continue analyzing the robustness, we want to assess the SCM if we remove a third state from the control, and analyze the variance between the two variables if this happens. (Rio Grande de Sul)

For this, a third city, Rio Grande de Sul, was also removed. Although the control does not match nearly as well as any prior one, the post-treatment behavior of the Synthetic is similar to Figure VII. With only 3 control cities removed, already the post-treatment synthetic control resembles Sao Paulo more closely. It is important to note that the control as a whole performs worse, but post-treatment it behaves similarly, albeit a treatment effect still being observable.

Conclusion

The robustness tests done make a case that the original studies validity is doubtful. The extension of the replication did not uncover any findings that give firm ground to validify the existing claims and discoveries. Therefore, there are 3 proposed steps to move forward in replicating the study for complete validity.

1. Gather data for more years and more cities. Perhaps data from 1985 upwards would be good enough to test efficiency of time placebos for the synthetic control, and greater sample size can lead to a better combination for a control.
2. Match on more relevant predictors, such as general crime rate, arrests per capita etc.
3. Find confidence intervals for the synthetic control and use that to map out and gain an extra level of analysis for the performance of the policy implementation.

References

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Appendix A

Replicated Weights output

	w.weights	unit.names	unit.numbers
11	0.000	RondÃ´nia	11
12	0.000	Acre	12
13	0.000	Amazonas	13
14	0.140	Roraima	14
15	0.000	ParÃ¡	15
16	0.000	AmapÃ¡	16
17	0.000	Tocantins	17
21	0.000	MaranhÃ£o	21
22	0.000	PiauÃ	22
23	0.000	CearÃ¡	23
24	0.000	Rio Grande do Norte	24
25	0.000	ParaÃba	25
26	0.000	Pernambuco	26
27	0.000	Alagoas	27
31	0.000	Minas Gerais	31
32	0.222	EspÃrito Santo	32
33	0.154	Rio de Janeiro	33
41	0.000	ParanÃ¡	41
42	0.262	Santa Catarina	42
43	0.000	Rio Grande do Sul	43
50	0.001	Mato Grosso do Sul	50
51	0.001	Mato Grosso	51
52	0.000	GoiÃ¡s	52
53	0.217	Distrito Federal	53

	Treated	Synthetic	Sample Mean
state.gdp.capita	23.285	23.105	11.830
state.gdp.growth.percent	1.330	2.632	3.528
population.projection.ln	17.335	14.791	14.867
years.schooling.imp	6.089	6.100	4.963
special.homicide.rates.1990.1998	32.672	32.557	21.843
Special.proportion.extreme.poverty.1990.1998	0.054	0.083	0.185
special.gini.imp.1990.1998	0.536	0.562	0.578

Appendix B

Santa Catarina removed from weights

	w.weights	unit.names	unit.numbers
11	0.001	RondÃ´nia	11
12	0.001	Acre	12
13	0.001	Amazonas	13
14	0.178	Roraima	14
15	0.001	ParÃ¡	15
16	0.001	AmapÃ¡	16
17	0.002	Tocantins	17
21	0.002	MaranhÃ£o	21
22	0.001	PiauÃ	22
23	0.001	CearÃ¡	23
24	0.001	Rio Grande do Norte	24
25	0.001	ParaÃba	25
26	0.002	Pernambuco	26
27	0.002	Alagoas	27
31	0.010	Minas Gerais	31
32	0.044	EspÃrito Santo	32
33	0.005	Rio de Janeiro	33
41	0.005	ParanÃ¡	41
43	0.002	Rio Grande do Sul	43
50	0.315	Mato Grosso do Sul	50
51	0.077	Mato Grosso	51
52	0.012	GoiÃ¡s	52
53	0.337	Distrito Federal	53

	Treated	Synthetic	Sample Mean
state.gdp.capita	23.285	23.280	11.525
state.gdp.growth.percent	1.330	3.516	3.552
population.projection.ln	17.335	14.159	14.844
years.schooling.imp	6.089	6.099	4.943
special.homicide.rates.1990.1998	32.672	32.668	22.447
special.proportion.extreme.poverty.1990.1998	0.054	0.082	0.190
special.gini.imp.1990.1998	0.536	0.568	0.580

Appendix C

Removed 2 controls

	w.weights	unit.names	unit.numbers
11	0.000	RondÃ´nia	11
12	0.000	Acre	12
13	0.000	Amazonas	13
14	0.034	Roraima	14
15	0.000	ParÃ¡	15
16	0.000	AmapÃ¡	16
17	0.000	Tocantins	17
21	0.000	MaranhÃ£o	21
22	0.000	PiauÃ	22
23	0.000	CearÃ¡	23
24	0.000	Rio Grande do Norte	24
25	0.000	ParaÃba	25
26	0.000	Pernambuco	26
27	0.000	Alagoas	27
31	0.000	Minas Gerais	31
32	0.000	EspÃrito Santo	32
33	0.449	Rio de Janeiro	33
41	0.000	ParanÃ¡	41
43	0.515	Rio Grande do Sul	43
51	0.000	Mato Grosso	51
52	0.000	GoiÃ¡s	52

	Treated	Synthetic	Sample Mean
state.gdp.capita	23.285	18.843	9.860
state.gdp.growth.percent	1.330	2.586	3.574
population.projection.ln	17.335	16.104	14.884
years.schooling.imp	6.089	6.158	4.807
special.homicide.rates.1990.1998	32.672	32.334	21.546
special.proportion.extreme.poverty.1990.1998	0.054	0.087	0.200
special.gini.imp.1990.1998	0.536	0.562	0.579

Appendix D

3 controls removed

	w.weights	unit.names	unit.numbers
11	0.000	RondÃ´nia	11
12	0.000	Acre	12
13	0.000	Amazonas	13
14	0.125	Roraima	14
16	0.000	AmapÃ¡	16
17	0.000	Tocantins	17
21	0.000	MaranhÃ£o	21
22	0.000	PiauÃ	22
23	0.000	CearÃ¡	23
24	0.000	Rio Grande do Norte	24
25	0.000	ParaÃba	25
26	0.000	Pernambuco	26
27	0.000	Alagoas	27
31	0.000	Minas Gerais	31
32	0.000	EspÃrito Santo	32
33	0.417	Rio de Janeiro	33
41	0.458	ParanÃ¡	41
51	0.000	Mato Grosso	51
52	0.000	GoiÃ¡s	52

	Treated	Synthetic	Sample Mean
state.gdp.capita	23.285	16.692	9.561
state.gdp.growth.percent	1.330	1.991	3.617
population.projection.ln	17.335	15.733	14.789
years.schooling.imp	6.089	5.830	4.738
special.homicide.rates.1990.1998	32.672	33.081	22.254
special.proportion.extreme.poverty.1990.1998	0.054	0.102	0.210
special.gini.imp.1990.1998	0.536	0.563	0.581