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SANCTIONS AND THE VENEZUELAN ECONOMY: WHAT THE DATA SAY

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Abstract: This paper considers the evidence on the effect of financial and oil sanctions in the 2017-19 period on Venezuelan oil production and broader socio-economic indicators. Using a panel of countries covering 95% of the word's oil production, we show that Venezuela's acceleration in the rate of decline in oil output after the imposition of financial sanctions in 2017 was more rapid than that of all other oil-producing economies in the world except for those undergoing armed conflict at the time. Using synthetic control methods, we estimate that financial sanctions were associated with a decline in production of 797tbd, which at today's oil prices would represent USD 16.9bn a year in foregone oil revenues. We also show that the alternative hypothesis that the decline in oil production was a result of the oil industry's militarization is inconsistent with longer-run patterns in the data. The facts that production rose or stabilized in Chinese and Russian joint ventures and that it fell in offshore subsidiaries that were sanctions-affected while rising in those that were sanctions-exempt lends further support to the hypothesis that sanctions drove the decline in oil production. However, we argue that while higher oil revenues in the absence of sanctions would have likely produced improvements in socio-economic indicators, it is incorrect to infer any effect of sanctions from the increase in mortality rates in 2018 given that increased oil prices offset the effect of lower oil output on that year's revenues.

[1] Rodríguez is Chief Economist at Torino Economics in New York. The author thanks Dany Bahar, María Eugenia Boza, Adolfo De Lima, Mónica García, Guillermo Guerrero, Ricardo Hausmann, Dorothy Kronick, Francisco Monaldi, Frank Muci, Michael Penfold, Jeffrey Sachs, Victor Sierra, Mark Weisbrot, Nicolle Yapur, Eduardo Zambrano and all the members of the Torino Economics staff for their comments and suggestions. All errors and shortcomings remain the author's sole responsibility. The views expressed in this paper are those of the author and do not necessarily represent the position of Torino Economics or of Torino Capital, LLC.

TABLE OF CONTENTS

1 Introduction

Have U.S. economic sanctions contributed to the decline in Venezuela's living standards? This question has become the focus of an intense recent debate. Weisbrot and Sachs (2019a, hence WS) argue that both financial sanctions imposed in 2017 and oil sanctions imposed in 2019 caused a decline in oil production and thus contributed to ensuing declines in several socio-economic indicators. Hausmann and Muci (2019) question the counterfactual assumption that oil production would not have declined in the absence of sanctions and claim that the 2019 drops in oil output were caused by electrical blackouts. Morales (2019) proposes the alternative of militarization as an explanation for the decline in oil production. Bahar, Bustos, Morales and Santos (2019) argue that social indicators show strong pre-existing trends before the sanctions and thus likely reflect the effect of past policies. (Because these last three papers make broadly similar arguments, I will henceforth refer to them as HMB when referring to arguments that are made by the three sets of authors.)

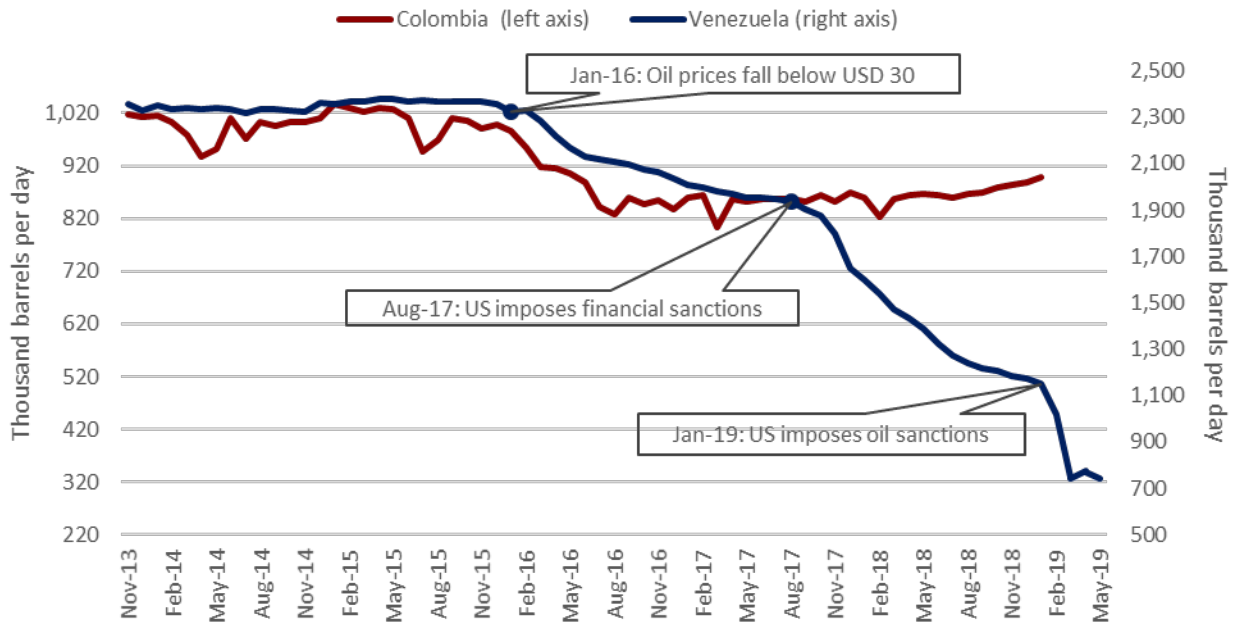
At issue in this debate is how to interpret the sizable decline in oil production that began in the second half of 2017. Venezuela's oil production had been declining through 2016 and the first half of 2017, but the speed of that decline accelerated markedly in the second half of 2017, coinciding with the adoption of financial sanctions, which barred new borrowing by the government and the state-owned oil company PDVSA firm. The rate of decline accelerated even more in the first months of 2019, after the U.S. banned all oil trade in Venezuela. The key question is whether we can attribute those accelerations to financial and oil sanctions, or whether they would have occurred in their absence.

Much attention in this debate has been focused on the choice of counterfactual. Weisbrot and Sachs (2019a) borrow a graph from Rodríguez (2018) which compares the evolution of oil production in Venezuela and Colombia (see **Chart 1**). Production is stable in both countries between 2013 and 2015 and declines by similar magnitudes after oil prices plunge at the start of 2016. But in the second half of 2017, when oil prices begin to recover, Colombia's oil production stabilizes, while Venezuela's oil production collapses.

The comparison made by Weisbrot and Sachs here is essentially a "differences-in-differences" analysis: a comparison of the changes before and after an intervention between a group that receives the intervention (called the treatment group) and one that does not (called the control group). Since the control group is meant to help replicate the unobserved counterfactual of what would have happened in the treatment group in the absence of an intervention, the control group should show that it is able to replicate well the evolution of the treatment group before receiving the treatment. This is known as the "parallel trends" assumption: the control and treatment group must show similar pre-treatment trends. The assumption clearly holds in the case of Colombia and Venezuela in the period shown by the authors, with the oil production series for both countries showing a

correlation of .92 in the four years prior to the adoption of sanctions on August of 2017 and the growth trends being nearly identical.[2]

Chart 1: Colombia and Venezuela oil production, 2013-2019



Sources: Torino Economics, OPEC, National Hydrocarbon Agency

Hausmann and Muci (2019) contend that Colombia is not a good control group because the series are uncorrelated in longer-run data going back to 1999. Their arguments are echoed by Morales (2019) and Bahar et al. (2019), although the latter restrict the data to a shorter time interval (since 2010) to make the argument of lack of correlation. These authors also point to structural dissimilarities between the two countries that would lead us to expect their oil productions to behave differently to different shocks. In contrast, WS account for the similarities in trend previous to 2H17 as the expectable response of high marginal cost producers to the price fluctuations observed in those periods.

Similarity, of course, is in the eye of the beholder. If we only allowed comparisons between identical cases, then the social sciences would cease to exist. No two cases are ever identical; the relevant question is whether they are alike enough for one to serve as a counterfactual for the other. Although the parallel trends hypothesis is intended to function as a test of that similarity, both sets of researchers point to more substantive reasons why

[2] Some of the initial critiques of WS focused on their graphical representation in a figure similar to our **Chart 1**. Santos (2019) argued that both countries should be represented in the same absolute scale, while Hausmann and Muci (2019) stated without much explanation that WS had "conveniently re-scaled" their graphs. Rodríguez (2019a) argues that the WS proportional scaling was appropriate given that the variable of interest is proportional oil output growth. See also Weisbrot and Sachs (2019b) This point appears to have been conceded by Bahar et al. (2019), who use proportional scaling for their graphical Colombia-Venezuela comparison (see their Figure 2).

they should or should not be viewed as similar. For WS and Rodríguez (2018), marginal cost is what is relevant because it summarizes the multiple factors that determine an industry's output reaction to price fluctuations, which is what we need to know to build the counterfactual response of the sanctioned country in the absence of sanctions. In this reasoning, it is irrelevant whether an industry has high marginal cost because of poor management or because of high costs of extraction: they will both react similarly to exogenous price fluctuations. HMB have a more restrictive vision of what is an acceptable similarity: they demand not only that industries in both countries have the same cost function, but that they do so for the same underlying reasons. Ultimately, there is no way to test these substantive evaluations of similarity against each other, and researchers may and typically do hold different views as to what makes one case a good counterfactual for another.

In order to go beyond this impasse, this paper broadens the space of potential counterfactuals. We analyze the evolution of Venezuela's oil production as compared with that of 36 other oil producing nations in a data set that accounts for 95% of the world's oil production. We estimate the magnitude of the sanctions effect on Venezuela using every possible alternative counterfactual, as well as a synthetic group built from a combination of alternative counterfactuals. We find that the result of a negative effect of sanctions on oil production is robust and holds across a wide set of possible counterfactuals.

We also consider the evidence on the effect of more recent oil sanctions. Since Venezuela is not the only country to have faced this type of sanctions, we can exploit the cross-national variation to make inferences on the effect of oil sanctions on oil output. We find that oil sanctions have been associated with large drops of production in all the countries on which they were imposed, including Venezuela, and identify in the data a strong and statistically significant negative effect of these sanctions on oil production.

We then explore the possibility that alternative interventions, such as militarization of the oil industry, are the source of the decline in output. We argue that some of these alternative hypotheses have problems in explaining other patterns observed in the data or require particularly restrictive assumptions. In contrast, the sanctions hypothesis is consistent with other patterns observed in the data – such as the resilience of production in Chinese and Russian joint ventures and sanctions exempt entities like CITGO.

Last, we consider the possibility of extracting conclusions on the effect of sanctions on broader measures of living standards, as done by both sets of authors. We argue that the simple time trend of social indicators is unlikely to be informative about the effect of sanctions without an account of the underlying drivers of these trends. While the existence of pre-trends in social indicators as pointed to by HMB is not proof of a lack of effect of sanctions, neither is a before-and-after comparison of these indicators as presented by WS. In fact, we argue that WS's attribution of mortality increases to sanctions is inconsistent with their thesis that the effect of sanctions operates through lower oil production, given that during 2018 oil price rises compensated for declining oil output.

2 In search of a counterfactual: pairwise comparisons

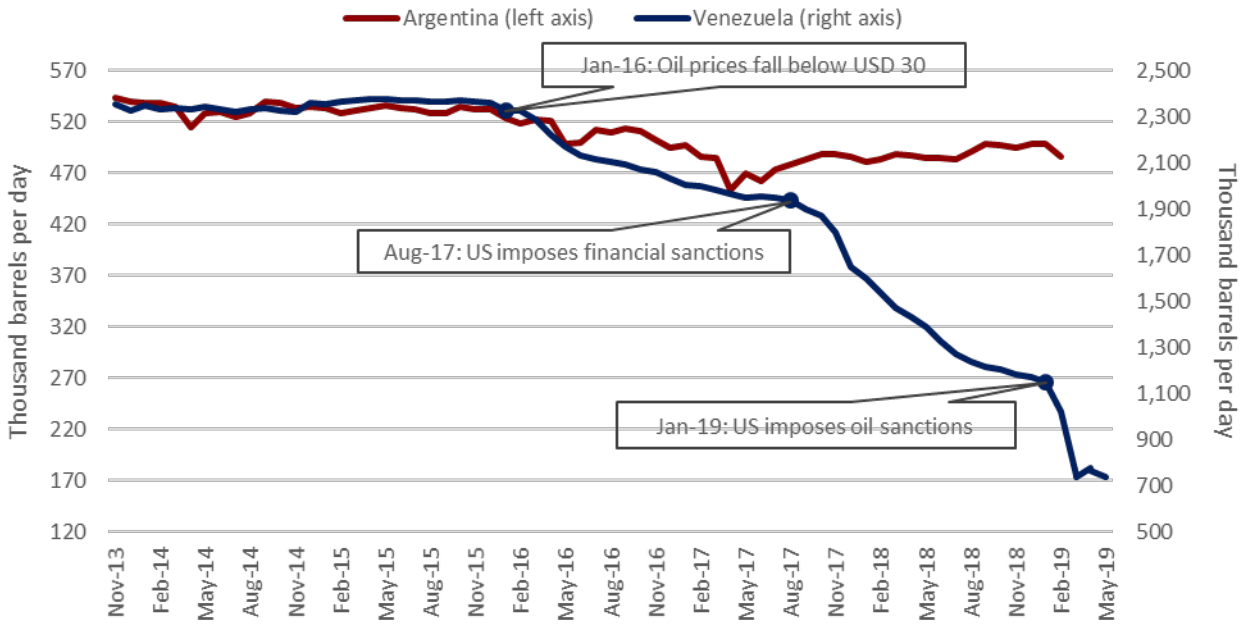
The fact that a sizable part of the recent discussion on the impact of sanctions on Venezuelan oil production has been centered on the appropriateness of Colombia as a counterfactual would suggest that WS's results are sensitive to the choice of counterfactual. In fact, that is not the case. There are several other possible counterfactuals that have the same similarity of trends with Venezuela prior to sanctions – and the same divergence after sanctions – as Colombia. **Charts 2 and 3** present two such comparisons: Argentina and Mexico.

Argentina is an interesting point for comparison because during the 2003-2015 period it was ruled by a government of a similar ideological orientation and policy approach to that of Venezuela. In 2012, Cristina Kirchner's administration nationalized YPF, the country's largest oil firm; similarly, in 2007 Hugo Chávez forced foreign operators to accept minority stakes in joint ventures with the government. Argentina's and Venezuela's exchange controls also served as taxes on the oil industry during this period by forcing the state-owned companies to sell foreign exchange at the overvalued official rate. While a more free-market administration came into power in Argentina at the end of 2015, it has made only relatively minor changes in oil sector policies. Similarly, the inefficiencies of Mexico's bloated and politicized national oil company PEMEX make it perhaps an ideal comparison case for PDVSA. In the words of *The Economist*, "each has access to enviable hydrocarbon resources, but is prevented from exploiting them efficiently by state interference."[3]

Both Argentina and Mexico show a strong correlation with Venezuela in terms of oil production trends in the four years preceding Venezuela sanction, with correlations of .92 and .84, respectively. But in both of them we find strong divergence in trends after the second half of 2017, with output stabilizing in Argentina while continuing its slow descent in Mexico but plunging in Venezuela.

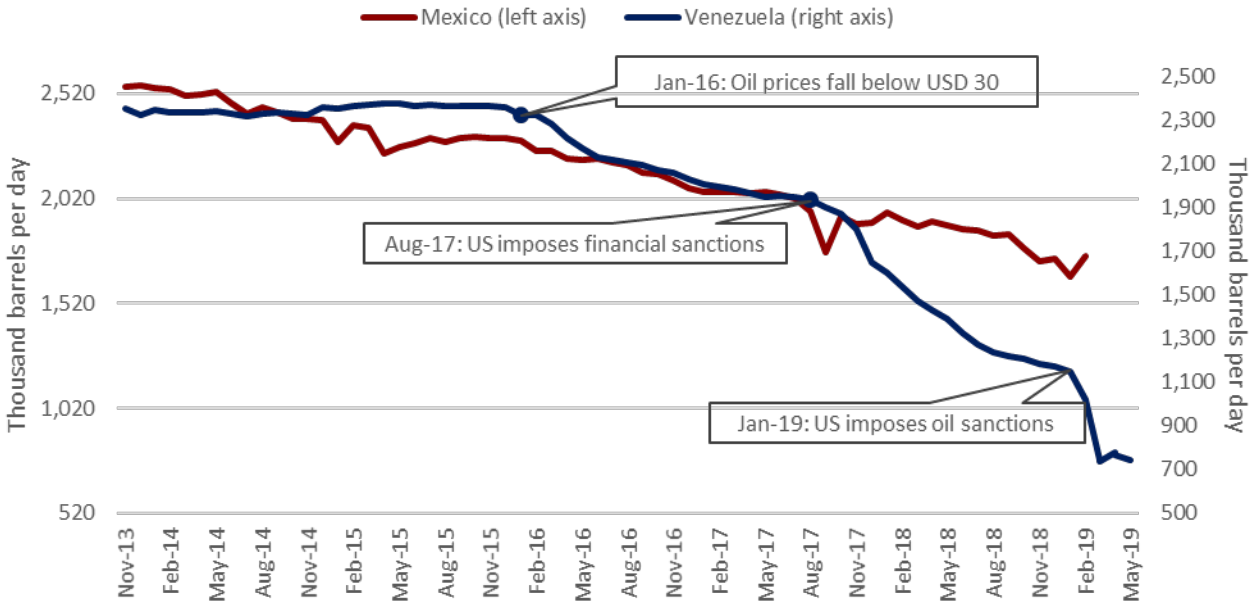
[3] The Economist, The Economist Intelligence Unit (2014).

Chart 2: Argentina and Venezuela oil production, 2013-2019



Sources: Torino Economics, OPEC, EIA

Chart 3: Mexico and Venezuela oil production, 2013-2019



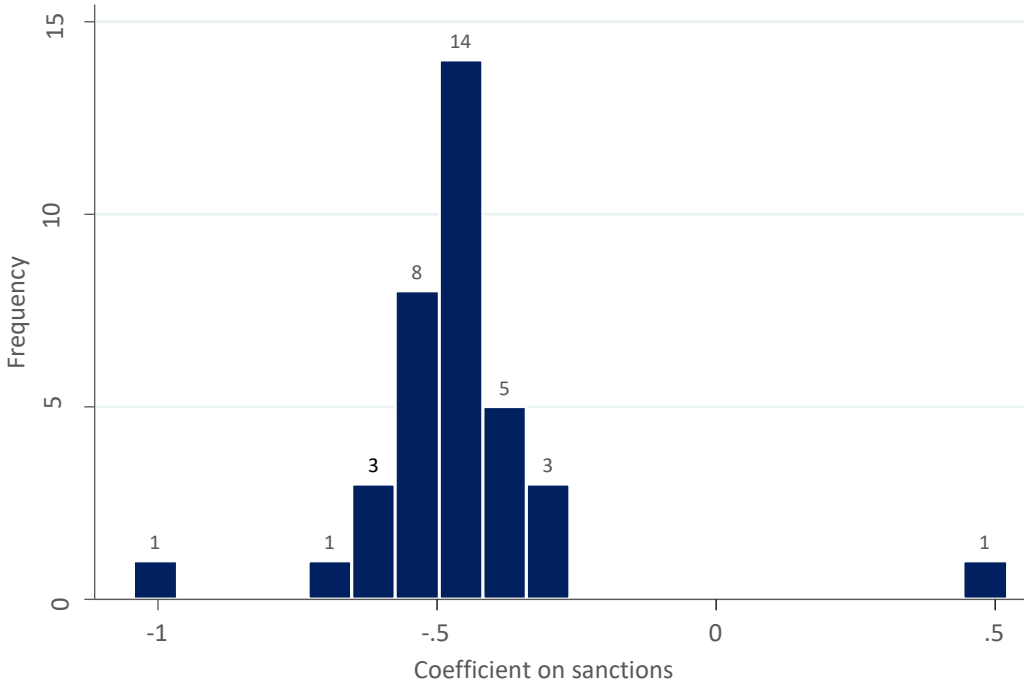
Sources: Torino Economics, OPEC, EIA

The results shown in these graphs are not special to this particular comparison group. In order to test their robustness, we estimate a classic differences-in-differences fixed effects regression on all pairwise comparisons of Venezuela with 36 other oil-producing countries. Data sources and characteristics are described in the appendix. The regression is:

$$o_{it} = \alpha_i + \beta_1 S_{it} + \beta_2 t + \varepsilon_{it} \quad (1)$$

where o_{it} is the logarithm of oil output, α_i is a country-specific effect, t is a time trend, ε_{it} is a country and time-specific disturbance, and S_{it} is an indicator for the existence of financial sanctions.[4]

Chart 4: Distribution of sanctions coefficients, pairwise regressions



The histogram plots the distribution of coefficients on β_1 on specification (1) run on data from Venezuela and each one of the rest of the countries in the data set separately, for a total of 36 regressions. S_i is set to 1 for Venezuela from September of 2017 onwards, and to zero for all other countries. Sample starts on September of 2013 - which implies a 4-year pre-treatment window – and ends on May of 2019. All regressions include country fixed effects and a time trend.

Sources: Torino Economics

Run on a four-year pre-trend window (2013-2017), all but 1 of the 36 regressions produce a negative estimate of β_1. The exception is the pairwise comparison with Yemen, to which we return below. **Chart 4** shows the distribution of these estimates. The median coefficient estimate is -.46, which indicates that sanctions lead to a

[4] We set S_i=0 for all countries except Venezuela in these regressions as the idea is to compare the evolution of Venezuela against other countries in this period and no other country shares the same treatment period as Venezuela. Setting S_i=1 for sanctioned countries in (1) has no visible effect on the results reported in this section.

46 log point decline in oil output. Taking the August 2017 level of oil production as the starting point, this estimate would imply that sanctions are associated with a 37.1% decline in Venezuela's oil production, or of 689 thousand barrels per day or USD15.2bn in export revenue at current prices.[5]

If we exclude two outliers (Yemen, which has the only positive coefficient, and Libya, which has the largest negative one), the remaining 34 coefficients (94.4% of the sample) fall within the range (-.66,-.28) and their standard deviation is .09. The minimum absolute value coefficient of -.27 in this range would imply a loss of 472tbd or USD 10.0bn, whereas the maximum of -.63 would imply a loss of 933tbd or USD 19.8bn.

One way to think about this distribution of coefficients is in the context of Bayesian econometric analysis. When faced with model uncertainty, a traditional Bayesian approach is to consider all possible models and focus on the characteristics of the distribution of estimates. If a coefficient estimate is relatively insensitive to model specification, it also means that it is insensitive to the prior probabilities that a researcher may have on potential models. A particularly demanding criterion is to require that a coefficient estimate have the same sign across all possible model specifications; this is known as "extreme bounds analysis" (Leamer, 1983).

Since most econometric estimates would not pass such a stringent test, other authors have suggested considering the distribution of coefficient estimates out of a set of candidate models and classifying as robust the variables for which a large share of coefficients have the same sign (Sala-i-Martin, 1997, Doppehloffer et al., 2004). The regressions underlying **Chart 4** essentially embody this approach. 35 of the 36 coefficients in regression (1) share the same sign, indicating that the result is relatively insensitive to the choice of counterfactual.[6]

A more classical statistical approach would demand that we place greater weight on models that we have good grounds to consider as more plausible than others. One way to assess the plausibility of models is by considering the similarity of their pre-treatment trends. A drawback of such an approach is that it could lead us to lose valuable information and possibly restrict the comparison set excessively.

Fortunately, for our 4-year window of pre-treatment comparison, there are several cases that satisfy this criterion. 3 of our 36 economies have pre-treatment correlations in excess of .9 with Venezuela (Argentina, China and Colombia) while two more (Mexico and Nigeria) have pre-treatment correlations between .75 and .9.[7] The average sanctions coefficient for those two groups of counterfactuals are respectively -.40 and -.39, similar to the -.46 average for the whole set of comparators.

Our 4-year pre-treatment window implies a ratio of 2.4 months of pre-treatment to treatment period. This is a relatively standard length of a pre-treatment period in differences-in-differences specifications. Table 1

[5] We use the Torino Economics estimated average price for Venezuela's oil basket of USD 58.2 per barrel for this and all other counterfactual revenue calculations presented in this paper.
[6] See Granger and Uhlig (1990), Sala-i-Martin (1997), Doppelhofer et al. (2004)
[7] A high correlation may be compatible with divergent pre-trends if the series are non-stationary. However, in all of these cases, the growth trends are similar in magnitude to those of Venezuela, with the difference in monthly growth rates over the 4-year window averaging 0.13 percentage points with a maximum of 0.23 percentage points (China).

summarizes some landmark studies from the literature and shows that the ratio of pre-treatment to treatment period oscillates between 1.7 and 4.7, with a mean of 2.6.

The danger of excessively lengthening the pre-treatment period is that we may contaminate our data with variations that are irrelevant to the comparison that we want to make. In this sense, it is worth recalling that in the absence of an experimental setup, it is essentially impossible for the control and treatment group to be identical; what we seek is that they be sufficiently similar. A group of 35-year old males may be an adequate control for a group of 40-year old males, but if we stretch their pre-treatment window to include their childhoods, we will find widely divergent behaviors at the start of the series due to age differences. Similarly, if we were to extend the pre-treatment period for the analysis of 1980s tobacco control legislation back into the 1920s, we would find that the pre-trend correlations are affected by Prohibition-era controls which are largely irrelevant today.

TABLE 1: LENGTH OF PRE-TREATMENT AND TREATMENT PERIOD IN LANDMARK STUDIES

Study	Topic	Ratio of pre-treatment to treatment period
Abadie, A., Diamond, A., and Hainmueller, J. (2010)	California smoking initiative	1.7
Card, D., and Krueger, A. B. (2000)	Minimum wages in New Jersey and Pennsylvania	4.7
Pischke, J. S. (1999)	School year and repetition in Bavaria	2.0
Besley, T., and Burgess, R. (2004)	Labor regulations in India	1.8
Kearney, M. S., and Levine, P. B. (2015)	Impact of media on pregnancy	3.0
Average		**2.6**

Sources: Torino Economics, cited authors

In this sense, Hausmann and Muci's suggestion of lengthening the pre-treatment window back to 1990 (28 years) implies a ratio of pre-treatment to treatment period of 16.6, which is unusually high for the literature. For example, extending the pre-treatment period of the Abadie et al. (2010) California smoking initiative comparison by a similar length would have required taking the sample back to 1806, more than four decades before California became a state.

Nevertheless, it is instructive to analyze the effects on our results of lengthening the treatment window. In **Table 2**, we show the results for 4-, 8- and 12-year windows – respectively starting in 2013, 2009 and 2005 - as well as for our maximum sample of monthly data which goes back to October 2000 (16.9 years). Even as we lengthen the comparison window significantly more than is normal in the literature, coefficient estimates remain consistently negative. The magnitude of our median estimated coefficient in fact rises as we lengthen the treatment window, reaching -.53 in the 12-year window. 92-94% of the coefficients (33-34 out of 36) are negative in the longer treatment windows, with only 2-3 cases (depending on the length of the pre-treatment window) showing positive coefficients.[8]

[8] 99.3% of the negative coefficients and 77.8% of the positive coefficients are significant at the 10% level using OLS standard errors. If we cluster standard errors by country, those percentages fall to 47.4% of positive and 0% of negative coefficients. Although over-rejection of the null is a well-known problem for OLS differences-in-differences analysis, the common solution of clustering by cross-sectional unit is not necessarily an improvement for a small number of cross-sectional observations (N), and some of the other solutions for small N in the literature (e.g., using the means of the before-and-after samples)

TABLE 2: SUMMARY OF PAIRWISE REGRESSION RESULTS

Specification	1	2	3	4
Start of pre-treatment period	Sep-13	Sep-09	Sep-05	Oct-00
Start of treatment period	Sep-17	Sep-17	Sep-17	Sep-17
End of treatment period	May-19	May-19	May-19	May-19
Length of treatment Window	4.0	8.0	12.0	16.9
Ratio of pre-treatment to treatment periods	2.4	4.8	7.2	10.2
Median	-0.463	-0.515	-0.534	-0.537
5th percentile	-0.657	-0.807	-0.847	-0.873
95th percentile	-0.279	0.639	0.600	0.451
Standard deviation	0.210	0.309	0.329	0.321
Negative	35	33	33	34
Positive	1	3	3	2
Percent negative	97.2%	91.7%	91.7%	94.4%
Percent positive	2.8%	8.3%	8.3%	5.6%

Table shows summary statistics of pairwise regressions on specification (1) run on data from Venezuela and each one of the other countries in the data set separately, for a total of 36 regressions per treatment window. S_i is set to 1 for Venezuela from September of 2017 onwards, and to zero for all other countries. Dates of start of sample vary according to pre-treatment window. Coefficient statistics refer to estimates of β_1. All regressions include country fixed effects and a time trend.

Sources: Torino Economics

The three countries that show positive coefficients in at least one of the specifications are Sudan, Syria and Yemen. These three countries have been engulfed in bloody armed conflicts in the period under study. In all these cases, the post-2017 period coincided with an intensification of armed conflict that led to severe losses in oil infrastructure.

Yemen's civil war began in 2015 in the aftermath of the Arab Spring, pitting Saudi and Iranian-backed factions against each other, and has led to an estimated loss of 233 thousand lives. Yemen's oil production had been on a downward trend before the civil war, but there is no doubt that the war caused production to plummet. A Saudi bombing campaign initiated in February of 2015 led production to fall by 80.4% in 8 months, from 112tbd to 22tbd; by the end of 2018, as the war raged on, production had fallen to just 16tbd, 86% less than at the start of the war. Similarly, Syria's output was 391tbd in February 2011, just before the beginning of the country's civil war. By the end of 2018, it had fallen by 93.3%. In the case of Sudan, output on April of 2011, before the start of the conflict, was 440tbd. Over the following year, it fell precipitously, stabilizing at around 100tbd from 2012 on.

cannot be applied for N=2. In any case, the focus of Bayesian analysis is not the standard error of estimates but the distribution of coefficients. See Bertrand, M., Duflo, E., and Mullianathan S. (2004).

In other words, what the data is telling us is that the only possible counterfactuals against which Venezuela does not do worse after the 2017 sanctions are countries undergoing protracted armed conflicts. **Table 3** underscores this point further by describing the largest proportional collapses in production over a short period of time (less than two years) seen in the data.[9] In the list of 10 largest collapses, Venezuela appears twice: at #7 (the 2002/3 oil strike) and at #10 (the period between June 2017 and May 2019). There are five other countries on the list: Libya, Iraq, Sudan, Syria and Yemen, with Libya and Yemen appearing more than once on. Almost all of these countries saw armed conflicts at the time of the collapse. In fact, Venezuela is exceptional in making it into this list without having a war take place in its territory during the period of the collapse. (The other exception is Libya in 2012-13, where the collapse was caused by an oil strike after the end of the first Libyan Civil War)[10] Put more bluntly, the data tells us that the output collapse that we have seen after financial sanctions were imposed in Venezuela are of a magnitude that we have only seen when armies devote themselves to blowing up oil fields or when oil workers decide to voluntarily bring production to a halt.

TABLE 3: 10 LARGEST PROPORTIONAL DECLINES IN OIL PRODUCTION

Rank	Country	Initial month	Production at initial month (tbd)	Final month	Production at final month (tbd)	Number of months	Decline (tbd)	Decline (%)	Key events
1	Libya	Jan-10	1,710	Aug-11	20	19	(1,690)	-98.8%	Intrastate war
2	Iraq	Apr-01	2,879	Apr-03	53	24	(2,826)	-98.2%	Interstate war
3	Libya	Jul-12	1,600	Nov-13	210	16	(1,390)	-86.9%	Oil strike
4	Syria	Aug-11	397	Aug-13	53	24	(344)	-86.6%	Intrastate war
5	Sudan	Jun-11	460	Apr-12	64	10	(396)	-86.1%	Interstate war
6	Yemen	Jul-13	135	Jul-15	22	24	(113)	-83.7%	Interstate war
7	**Venezuela**	**Oct-02**	**2,995**	**Jan-03**	**594**	**3**	**(2,401)**	**-80.2%**	**Oil and national strikes**
8	Libya	Oct-14	850	Feb-15	250	4	(600)	-70.6%	Intrastate war
9	Yemen	Jan-11	308	Mar-12	102	14	(206)	-66.9%	Intrastate war
10	**Venezuela**	**Jun-17**	**1,955**	**May-19**	**741**	**23**	**(1,214)**	**-62.1%**	**US sanctions and political crisis**

Table displays key characteristics of the ten largest contractions in oil production lasting 24 months or less. A contraction is defined as the period between a peak and a trough in the series, provided that the period is less than 24 months. Wars listed in the last column follow the classification of the Correlates of War Project's Militarized Interstate Dispute Locations (v2.0) data set.

Sources: Torino Economics, OPEC, EIA, U.S. Energy Administration, Bloomberg

However, an important caveat on the results shown in **Table 2** is that as we raise the pre-treatment window, it is more difficult to find cases that show a strong pre-treatment correlation to Venezuela. In contrast to the 4-year window, where there are five economies with pre-treatment correlations above 0.75, in the 8- and 12- year window there are only two (Mexico and Nigeria), and none in the 19-year window.[11] Therefore, one could argue that there are no adequate control groups for Venezuela in these longer treatment windows. In the next section, we implement the strategy of a building a synthetic control group to establish a counterfactual that is appropriate for longer treatment windows.

[9] Some countries experience large declines over longer periods of time due to depletion of reserves; thus, we restrict the comparison to include countries that, like Venezuela, experience intense declines over relatively short periods.
[10] Krauss, C. (2013).
[11] For these two countries, the difference in monthly growth rates with Venezuela is small, averaging 0.14 percentage points in the 8-year and 0.11 percentage points in the 12-year window.

3 A synthetic control group approach

As Abadie and Gardezeabal (2003) and Abadie et al. (2010) have argued, it is often the case that a combination of control units designed to replicate as closely as possible the evolution of the treatment group before an intervention can serve as a much better comparison for the unit exposed to the intervention than any single unit on its own. This is the essence of the synthetic control group method, which creates a counterfactual as a linear combination of potential control units that approximates the values of the predictors of the outcome variable of interest in the pre-treatment period.

Using this method, we build a synthetic Venezuela that approximates the values of a set of control variables that serve as predictors for oil production. Our set of covariates includes domestic oil consumption, refining capacity, oil reserves, political institutions, and lagged oil production 4, 8 and 12 years before treatment. We work with the 12-year window, which begins in 2005 because longer-run windows are affected by the 2002-03 oil strike, which caused a 79.8% decline in oil production in a two-month period. Although production never returned to its pre-strike levels, the data suggest that most of the effect of the strike was gone by 2005. Because of how atypical – and idiosyncratic – the oil strike plunge and subsequent recovery is, it is hard for any other countries to replicate the Venezuelan data (which also explains the low pairwise correlations in the 17-year windows). While using the 17-year window does not qualitatively change our results, it does diminish the capacity of the synthetic control group to replicate the pre-treatment data.

In applying the synthetic control method, it is very important to choose the donor pool from economies that are not also being treated, and that share some basic similarities with the treatment group. We therefore restrict our control group to non-sanctioned economies with an average GDP per capita less than 40 thousand dollars in 2014[12] (Venezuela's was 18 thousand). This leaves us with a donor pool of 13 countries.[13]

[12] 2014 is the last year of Venezuelan GDP data in the World Bank's World Development Indicators Database.
[13] Algeria, Argentina, Azerbaijan, Brazil, China, Colombia, Egypt, India, Indonesia, Kazakhstan, Malaysia, Mexico and Vietnam. Eleven countries are excluded from the donor group by the income criterion (Australia, Canada, Denmark, Kuwait, Norway, Oman, Qatar, Saudi Arabia, UAE, the United Kingdom and the United States), three by the sanctions criterion (Iran, Iraq and Russia) and eight because of insufficient data in either the dependent variable or the covariates (Angola, Ecuador, Equatorial Guinea, Gabon, Libya, Nigeria, Sudan and Syria).

Chart 5: Venezuela and synthetic control production, 2005-2018

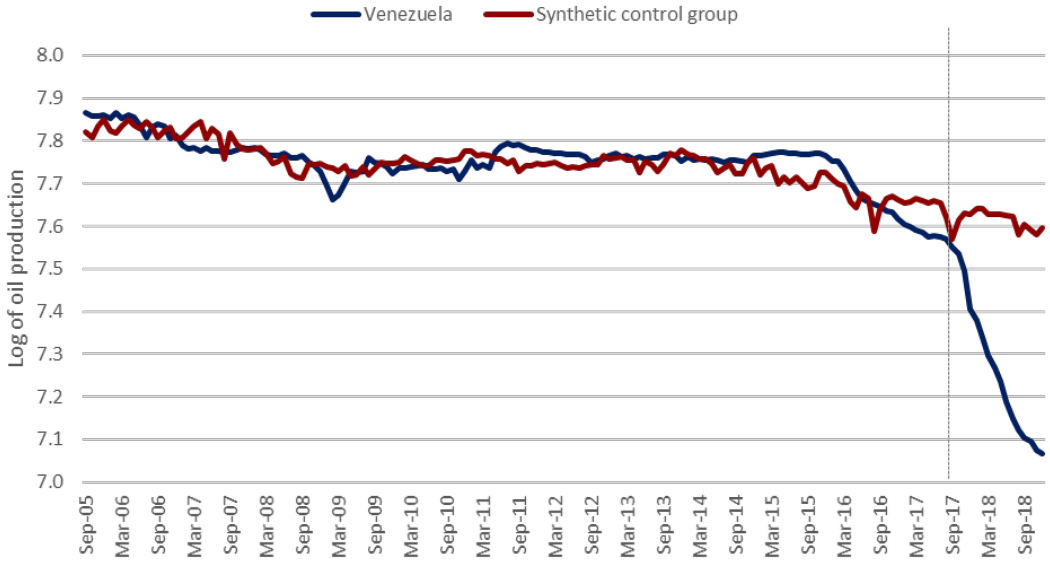


Chart displays the evolution of oil production in Venezuela and a synthetic control group. The synthetic control group is created by the method of Abadie, Diamond and Hainmueller (2010). Pre-treatment period begins in September of 2005 and thus has a 12-year length. Controls include domestic oil consumption, per capita GDP, refining capacity, oil reserves and the Polity Index. All variables except for the polity index are represented in logs. Donor pool includes all non-sanctioned countries with a per capita GDP of less than USD 40,000 in 2014.

Sources: Torino Economics, OPEC

Chart 5 plots the evolution of the logarithm of oil production in Venezuela and the synthetic group. The method chooses as synthetic control group for Venezuela a combination of Mexico (55.5%), Kazakhstan (31.9%) and China (12.6%). The synthetic control reproduces the treated country very well throughout the whole pre-2017 period. Venezuela somewhat outperforms the synthetic group in 2015 and underperforms it in 2016, but in both cases the difference is within two standard deviations of the mean-squared error (MSE). By August 2017, the month at the end of which financial sanctions were imposed, the difference between the synthetic and treatment groups was of 5 log points, as opposed to an MSE of 4 log points (**Chart 6**).

This difference will grow markedly after the adoption of U.S. financial sanctions. On the month after treatment[14] the difference between synthetic and control group production rises to 8 log points, and then in November it rises again to 13 log points, more than three standard deviations of the pre-trend difference. By 2018, when our sample comes to an end – and before the adoption of U.S. oil sanctions, the difference had grown to 53 log points, or more than 14 times the pre-treatment standard deviation of the series. Taking the August 2017 level of production as the baseline, this would imply a loss of 797tbd in production, or USD 16.9bn at today's oil prices.

[14] I.e., October of 2017

Chart 6: Difference between synthetic and control group

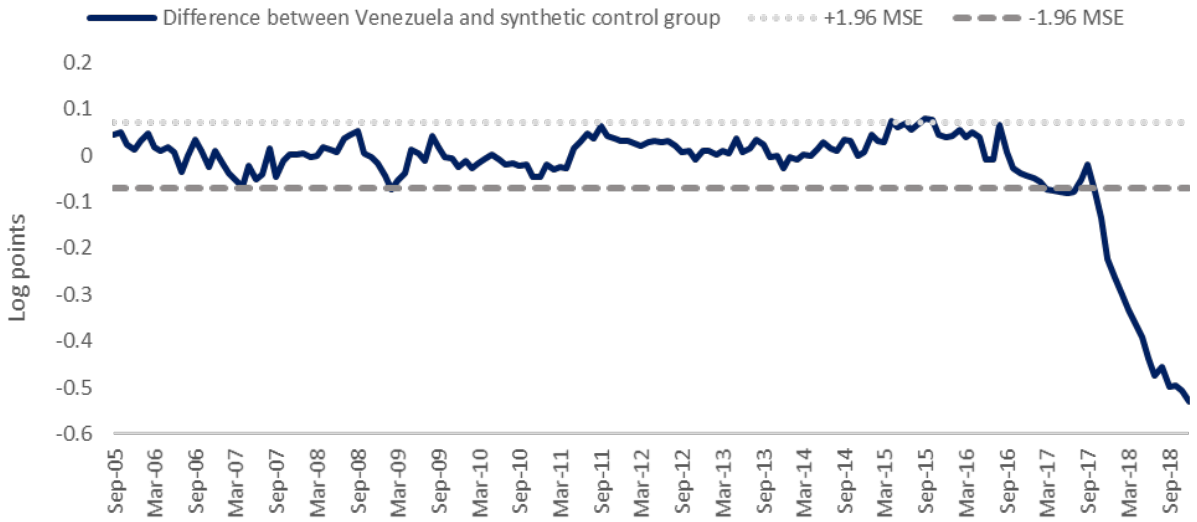


Chart displays the difference between the logs of oil production in Venezuela and the synthetic control group represented in Chart 5. Confidence intervals are given by the range ±1.96 times the mean-squared error between both series in the pre-treatment window.

Source: Torino Economics

In order to understand whether our results could have been caused by chance, we carry out placebo tests on the 13 members of the control group, arbitrarily taking them out of the treatment and the control group. **Chart 7** shows that, while the Venezuela series is abnormal in terms of the discontinuous break after the intervention period, there are several other series that produce similar differentials between the treatment and the synthetic group. This is due mainly to the fact that the method does poorly in fitting the oil production series of several countries.

Chart 7: Placebo tests, all countries

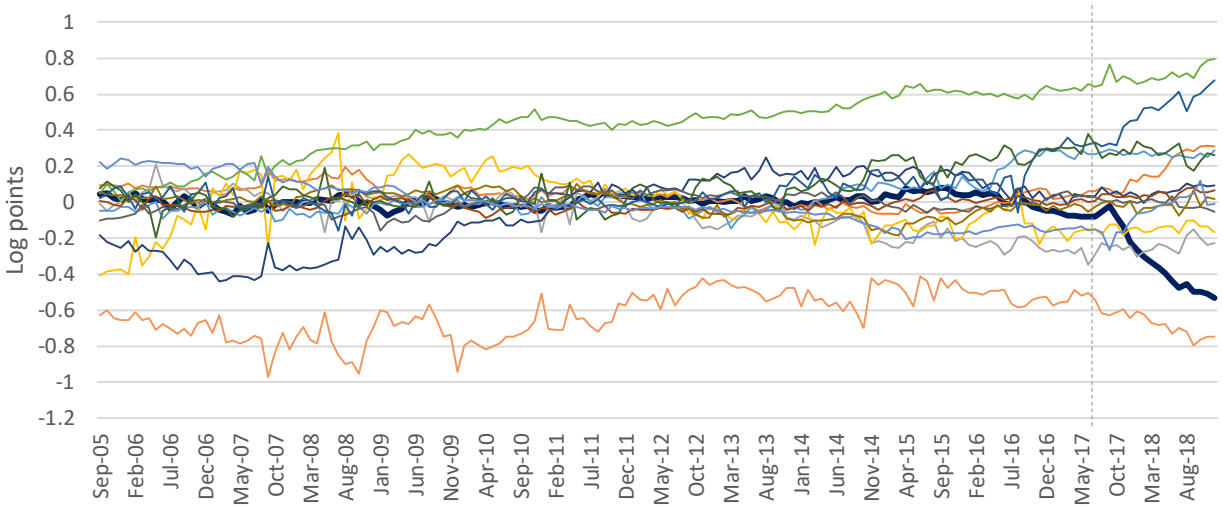


Chart displays the difference between the logs of oil production in every country and the synthetic control group constructed by applying the Abadie, Diamond and Hainmueller (2010) method to each country in our donor pool using the same treatment window as in Chart 5.

Source: Torino Economics

Chart 8: Placebo tests, low MSE countries

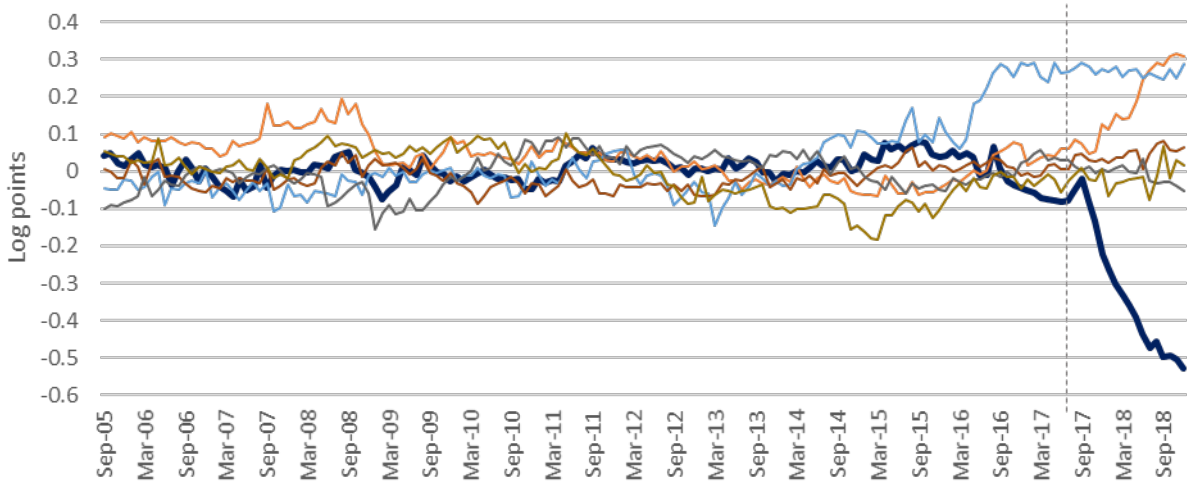


Chart displays the difference between the logs of oil production in every country and the synthetic control group constructed by applying the Abadie, Diamond and Hainmueller (2010) method to each country in our donor pool using the same treatment window as in Chart 5, only for the countries in which the mean-squared error of prediction is less than 3 times that observed when applying the method to Venezuela. This restriction ensures that the placebo tests are considered only in cases in which the method performs adequately in constructing a counterfactual for the placebo-treated country.

Source: Torino Economics

Because of this, in **Chart 8** we restrict to countries that have a mean-squared prediction error in the pre-treatment window less than three times that observed for Venezuela. These are in fact the countries for which the synthetic control method produces an adequate counterfactual. Here we find that the magnitude of the estimated treatment effect is much larger than that observed in any of the low-MSE placebo tests. In other words, we find that the test does not produce estimated effects of intervention such as that obtained in Venezuela in cases in which an intervention is not present.

4 Effect of oil sanctions

The recent discussion on the effect of sanctions on the Venezuelan economy has focused not only on the effect of the 2017 financial sanctions but also on the more recent oil sanctions imposed by the United States in January of this year. WS argue that "the sanctions implemented in 2019… cut off Venezuela from most of the international payments system, thus ending much of the country's access to these essential imports including medicine and food — even those that could normally be bought with available dollars." They point to the very strong decline of oil production (also visible in **Chart 1**) after January 2019 as evidence of the large economic cost of these sanctions.

Hausmann and Muci, in contrast, argue that the oil sanctions are not the main cause of the decline in oil production. They show a daily graph of oil production estimates from a private consultancy (IPD) showing very large declines in oil production on the dates of the electricity blackouts suffered by Venezuela earlier this year, with production then recovering most of the losses after the end of the blackouts. The authors summarize their case thus: "Can we straightforwardly attribute the decline to the sanctions? No."

There are now two more additional month of data since the publication of the Hausmann-Muci article showing that oil production has continued to decline despite the end of the blackouts. Production recovered moderately in April (4.2%) but fell again in May (-4.5%) and now stands at 741tbd, 410tbd below the level in January, the month during which sanctions were imposed. This is despite the fact that the intensity of nation-wide blackouts fell from 8.3 days in March to 0.8 days in April and no nationwide blackouts were reported in May.[15] The data thus sheds strong doubts on the hypothesis that the decline in production was caused by the blackouts.

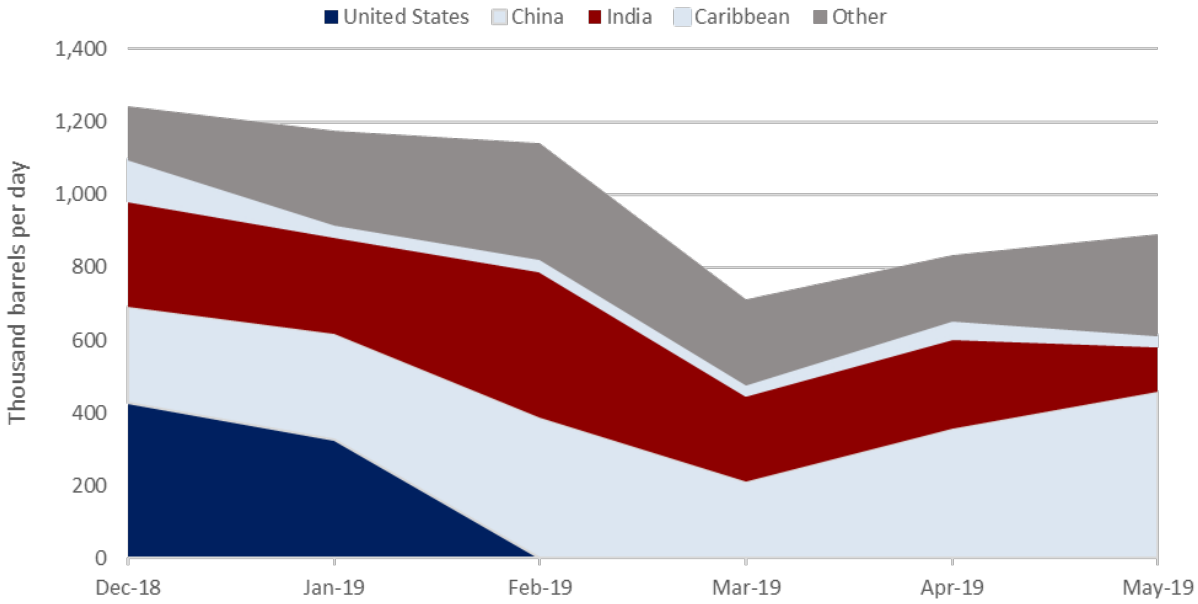
Hausmann and Muci also argue that "most sanctions were to take effect on April 15 and yet oil production declined precipitously before that date. After all, Venezuela was able to skirt the sanctions by redirecting oil sales from the U.S. to India, China, and Russia." They therefore suggest that the sanctions were not yet binding at the time of the latest data and thus could not be blamed for the collapse of oil output seen in the first months of 2019.

Both of these statements are incorrect. Sanctions barring imports of oil products from the United States to Venezuela, which provide inputs for approximately one-quarter of Venezuela's pre-sanctions oil production, were barred immediately from the date of publication of the sanctions on January 28. And while there formally was a wind-down period until April 28 (not, as the authors state, April 15[16]) for Venezuela's oil exports to the U.S., purchasers of this oil were required to deposit the payment in blocked accounts that would not be available to the Nicolás Maduro government; it thus made no sense for Maduro to ship to them any oil that had not been paid for previous to the sanctions.

[15] See Torino Economics' April 8 LatAm this Week report ("*Venezuela: Darkness Falls")* for a more detailed description of the construction of our estimates of intensity of nationwide blackouts.
[16] See US Department of the Treasury, Office of Foreign Assets Control (2019).

It is also incorrect to state that Venezuela could redirect oil sales to India, China and Russia. First of all, Venezuela does not sell oil to Russia – it sells it to refineries in other countries such as India that are owned by Russian companies. Regarding sales to other destinations, **Chart 9** shows that they have not picked up the slack generated by the loss of the U.S. market. Sales to destinations other than the United States rose only moderately between January (847tbd) and May (892tbd), by 45tbd, implying that the bulk of the loss of the U.S. market (326tbd in January) was not compensated by increased sales to other countries. A likely reason for this is that U.S. officials have put substantial pressure on third countries – including open threats of secondary sanctions – not to increase oil purchases from Venezuela after sanctions were imposed.

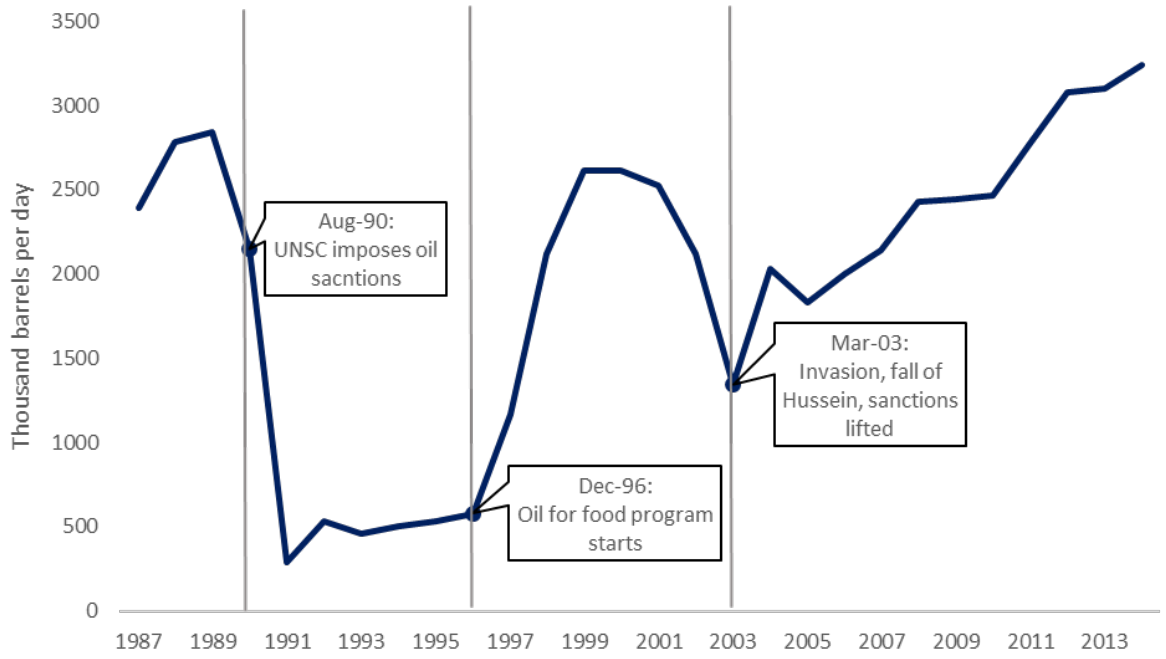
Chart 9: Venezuelan oil exports by destination



Sources: Torino Economics, Bloomberg

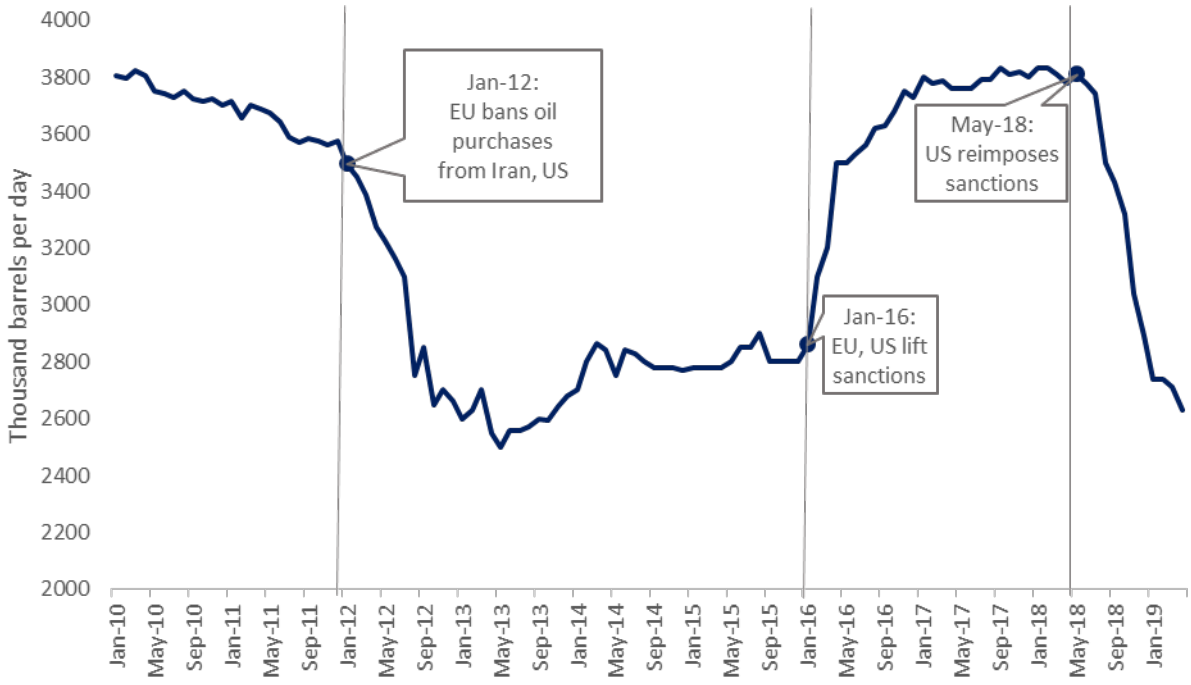
Because the oil sanctions are similar to those imposed on other countries in the past, we can get a sense of their expected impact by looking at how they correlate with oil production in those countries. **Charts 10 and 11** show two examples: Iraq and Iran. In the case of Iraq, we see a precipitous drop in output of 87% on the year of sanctions (also the year of the first Gulf War). Output remains low for several years after the war and then recovers almost all the lost ground when Iraq is permitted to begin exporting oil again through the U.N. sponsored oil-for-food program. Sanctions are lifted after invasion in 2003, and production begins to recover.

Chart 10: Iraq oil production, 1987-2014



Sources: Torino Economics, BP Statistical Review

Chart 11: Iran oil production, 2010-2019



Sources: Torino Economics, Bloomberg

Chart 11 shows a similar story for the case of Iran. Output falls by approximately 20% after the EU bans oil imports from Iran and the U.S. implements secondary sanctions on countries trading with Iran. These restrictions are lifted on January 2016, and again output recovers most of the lost ground. On May 2018, the U.S. again imposes secondary sanctions on Iranian oil sales, leading to a decline of a similar proportion. It's worth noting that most of Iran's oil exports go to Asia, making the threat of secondary sanctions very relevant. During the periods covered in this figure, the U.S. exempted several of Iran's trading partners from these sanctions, as long as they made a commitment to significantly reduce oil imports from Iran.

In order to explore these results more systematically, we run a set of panel regressions on data from 38 oil exporting countries.[17] The regression is an extension of equation (1) to incorporate covariates, and is now run on a panel of countries rather than pairwise comparisons:

$$o_{it} = \alpha_i + \gamma_t + \pi_i t + \beta_0 X_{it} + \beta_1 S_{it} + \varepsilon_{it} \quad (2)$$

Where X_{it} is now a *jx1* vector of covariates and S_{it} is an indicator variable capturing whether the country was subject to oil sanctions. α_i and γ_t respectively denote country and time fixed effects, while *t* is a time trend, which is allowed to have country-specific effects.[18]

Results are described in **Table 4**. Column (1) displays a simple initial differences-in-differences specification with no additional control except for the country fixed effects and the sanctions indicator (so γ_t and all π_i are equal to zero). This shows a statistically significant estimated negative effect of 53 log points. Once we control for time effects using monthly dummies and country-specific time trends, the effect remains significantly negative and rises to 58 log points. As noted by Besley and Burgess (2004), the inclusion of country-specific trends in differences-in-differences specifications can serve to identify whether the intervention leads to changes from pre-existing trends. Including these controls also significantly raises the R-squared of the regression, reassuring us that we are capturing the bulk of the determinants of oil performance even without yet including additional controls.

[17] There is one additional country in our data set, the Democratic Republic of Congo, that is not included in the regressions summarized in **Table 2**. DRC data begins in 2018 and thus does not allow for a before-and-after comparison but can nevertheless be used as an additional observation in the panel differences-in-differences regressions.

[18] We will make no attempt in these regressions to identify a separate effect for financial sanctions. Venezuela's case is atypical in the presence of financial sanctions without oil sanctions. Typically, both financial and oil sanctions are imposed and removed simultaneously, impeding us from empirically estimating the effects separate from each other. Therefore, the coefficient estimates in this section should be interpreted as the joint effect of oil and financial sanctions.

TABLE 4: PANEL FIXED EFFECTS REGRESSION RESULTS

Model	1	2	3	4	5
Oil sanctions	-0.5284*	-0.5843*		-0.2060***	-0.2986***
	(0.2830)	(0.3454)		(0.0364)	(0.0187)
Domestic oil consumption (ln)				0.0087	
				(0.1440)	
Per capita GDP (ln)				0.8537***	
				(0.2632)	
Refining capacity (ln)				-0.0575	
				(0.0419)	
Oil reserves (ln)				0.1242	
				(0.1134)	
Polity Index				0.0055	
				(0.0111)	
Oil sanctions adopted			-0.4938*		
			(0.2820)		
Oil sanctions adopted (t-1)			-0.5530		
			(0.4452)		
Oil sanctions adopted (t-2)			-0.6872		
			(0.5443)		
Oil sanctions adopted (t-3)			-0.8102		
			(0.6286)		
Oil sanctions adopted (t-4 and earlier)			-0.5448**		
			(0.1027)		
Oil sanctions adopted (t+1)			-0.1525		
			(0.1218)		
Oil sanctions adopted (t+2)			-0.1198		
			(0.1194)		
Oil sanctions adopted (t+3)			-0.1173		
			0.5448		
F-test on all lags and contemporaneous effect equal to zero			19.09***		
F-test on all leads equal to zero			2.53*		
Time variables	None	Month effects and country trends	Month effects and country trends	Month effects and country trends	Month effects and country trends; sample restricted to same observations as column 4
N	8,666	8,666	8,494	6,459	6,459
R^2	0.87	0.96	0.96	0.99	0.99
Countries	38	38	38	29	29

Table displays coefficient estimates from estimating variants of equation (2) on a panel of 38 countries. Regressions are estimated by panel fixed effects. Si is set to one for all countries facing oil sanctions as described in the appendix. Robust standard errors in parentheses. Asterisks denote significance levels as follows: * 10%, ** 5%, *** 1%.

Source: Torino Economics

In column (3), we pass to another set of robustness tests which rely on considering the time pattern of the variation in the intervention and dependent variables. We follow the idea of Autor (2003) of using Granger causality tests to study whether sanctions effects precede or are preceded by changes in oil production. We therefore include a set of controls indicating whether sanctions were adopted in the current period, in past, or in future periods. We include three leads and three lags as well as the contemporaneous effect and a variable capturing all cases in which the sanctions were adopted four or more periods prior to the current one. Note that what this does is allow us to distinguish the sanctions effect estimates in the prior specifications according to the moment at which sanctions happened, rather than forcing all periods after sanctions adoption to share the same effect.

The results find insignificant effects on any of the leads, indicating that changes in output do not precede changes in sanctions. On the other hand, it finds significant effects for the contemporaneous adoption of sanctions as well as for sanctions imposed more than three months previously. All lags are jointly statistically significant at a 1% level.[19] The coefficient on sanctions imposed four or more months previously of -.54 is similar to the coefficients in specifications 1 and 2.

Column (5) undertakes an additional robustness test in which we control for the additional covariates. The estimated effect of oil sanctions falls strongly in magnitude, though it is statistically significant at the one-percent level. Its magnitude of -0.21 is less than half of the estimated coefficients in other specifications in this table. However, the main reason for the drop in the coefficient is the loss of around a quarter of the observations. In column 5 we show the results of re-estimating the specification in column 2 with the same observations as in column 4, where we obtain a coefficient of -.30, closer in magnitude to the -.21 estimate with controls.

Omitting these last two estimates, we are left with a range of estimates of -52 to -58 log points. Applied to the Venezuelan economy, these effects would imply a decline of between 445tbd and 506tbd if we take the January 2019 production level as our starting point. Valued at today's market prices, this would amount to USD 9.5-10.8bn in foregone oil revenues. It would also represent 32-36% of the country's 2018 oil exports and 43-49% of its 2018 imports of goods and services.

Note that because financial sanctions almost always accompany oil sanctions (Venezuela being an atypical case), our coefficient estimates could be interpreted as proxying for the joint effect of financial and oil sanctions.[20] Our results thus suggest that the financial sanctions imposed on Venezuela in 2017 had similar effects on the economy than those that could have been expected from full-fledged oil sanctions.

Nevertheless, this does not imply that Venezuela will not suffer additionally from oil sanctions; in fact, the data available since January of this year strongly suggest that it has. Coefficients in linear regressions should typically

[19] Lags on months 1-3 are jointly statistically significant at the 10% level (p=.06). While leads are also jointly significant at a 10% level (p=.07), leads greater than one month are neither individually nor jointly significant (p=.62 for the joint test). Recall that we time the intervention as starting on the first month in which more than half of the days of the month were under sanctions, implying that the first lead month can include a partial sanctions effect.

[20] Attempts to separately estimate a financial sanctions effect in these regressions yielded very unstable coefficient estimates, which makes sense given the high level of collinearity with the oil sanctions variable. However, the coefficient on oil sanctions remained robust to the inclusion of a control for financial sanctions.

be interpreted as average effects from heterogeneous effects across different units.[21] For example, we can see from **Charts 10 and 11** that Iran's output decline during sanctions was proportionately much lower than that of Iraq, a difference that is explained by the fact that most of Iran's trade before sanctions was with Asian countries that were not directly affected by sanctions. Therefore, we will likely have to wait for more data to be available to produce a more robust estimate of the effect of oil sanctions on Venezuelan oil output. What this section has done is to show that the experience of a panel of oil-producing countries shows that oil sanctions do generally have a negative effect on oil output and given an indication of the magnitude of the potential effect

[21] For a discussion and application to cross-country growth empirics, see: Rodriguez and Shelton (2013).

5 Testing an alternative hypothesis: PDVSA's "military disruption"

HMB argue that the decline in oil production in late 2017 should not be attributed to sanctions given that there are several other potential determinants of oil production that are varying at around the same time. All three set of authors offer the management changes undertaken in the oil industry towards the end of 2017, which gave considerably more power to the military, as a potential alternative explanation for the decline in production.

The argument is developed in greatest detail by Morales (2019) who contends that the acceleration of the drop in oil output in December of 2018, the month immediately following the appointment of General Manuel Quevedo as President of PDVSA, together with anecdotal evidence regarding the effects on oil industry management caused by this "military disruption" lend support to the idea that "the principal cause of any acceleration in the decline of production is associated with military management of the oil industry."

Ever since the rise to power of Lieutenant Coronel Hugo Chávez in 1999, the military have played a significant role in the management of Venezuela's government and economy. Chávez and later Maduro appointed former or active military personnel to key government positions including the vice-presidency (Diosdado Cabello, Ramón Carrizales), finance ministry (Francisco Usón, Rodolfo Marco Torres) and interior ministry (Ramón Rodríguez Chacín, Jesse Chacón, Miguel Rodríguez Torres), among others. As of mid-2018, 26% of cabinet positions were occupied by mostly active military, similar to the 25% in 2014 or the 32% in 2004.[22]

The same pattern is present in the board of PDVSA. Maduro's appointment of Manuel Quevedo is not the first time that an active general with no prior industry experience is tapped to head the country's oil industry. Chávez appointed General Guaicaipuro Lameda to lead the oil industry in 2000, a position in which he lasted for two years (until he participated in the failed 2002 coup attempt). Even before Chávez's time, PDVSA's first President was General Rafael Alfonzo Ravard, who ran the firm from 1976 to 1983.

Venezuela observers will be quick to point out that Ravard and Lameda were seen as being immensely more competent than Quevedo. That may well be true. But this would be invoking a different hypothesis: that of competence, not militarization. Is it possible that the competence of the government's choice of key management figures deteriorated markedly in late 2017 relative to the previous 17 years in power of *chavismo*? Perhaps. What we do know is that competence-based explanations are remarkably difficult to test, given our inability to directly measure the competence of persons in key government positions. Economists tend to be skeptical about untestable explanations that rely on unobservable variables because they are not amenable to being evaluated through the scientific method.

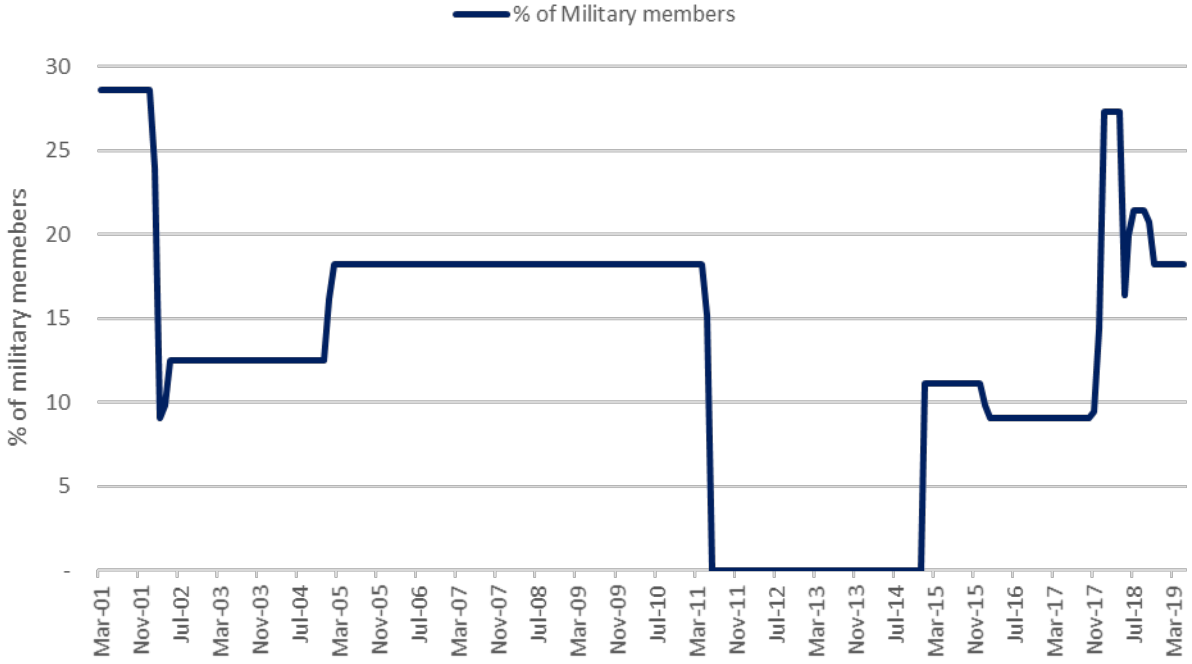
A full examination of the competence hypothesis is outside the scope of this section. One possible extension of our work would be to expand the analysis to account for observable proxies for competence such as prior industry

[22] San Miguel, R. (2018). Also see: Von Bergen Granell, F. (2016)

or management experience of PDVSA appointees. A brief examination of the patterns in PDVSA appointments, however, suggests that such an approach may have similar problems in accounting for the longer-term trends. For most of the period since 1999, the presidency of PDVSA was held by persons who, similarly to Quevedo, had no prior industry experience. These included Guaicaipuro Lameda (2000-2002), an active general who had briefly occupied middle level public sector management positions in an armaments manufacturer, Alí Rodríguez Araque (2002-2004), a former leftist guerrilla leader, and Rafael Ramírez (2004-2014), an electrical engineer with only a brief experience managing the gas state-owned firm.

The main exception was Eulogio Del Pino (2014-17), who did come from PDVSA's technical cadres. However, Del Pino's tenure does not seem to have brought about an improvement in the levels of professionalism of the PDVSA board as a whole. For example, PDVSA's CFO during most of Del Pino's tenure was Erick Malpica Flores, a nephew of first lady Cilia Flores who would be later tied to drug trafficking and included in OFAC's list of Specially Designated Nationals. According to some accounts, Malpica was said to wield as much power in PDVSA as Del Pino himself.[23] Del Pino's tenure was also the period during which exclusively political appointees such as Delcy Rodríguez (who had served previously as Communications Minister and is now Maduro's Vice-President) would be tapped for PDVSA board positions.

Chart 12: Military as % of PDVSA board



Sources: Torino Economics, PDVSA Management reports, Official Gazette

[23] US v. Campo Flores et al.

More in-depth examination of the competence hypothesis in accounting for the decline of Venezuela's oil production is a potentially productive area of research if it can successfully tackle the complex and even seemingly intractable measurement problems we have alluded to. For the remainder of this section, we focus on the more concrete militarization hypothesis.

Chart 12 displays the evolution of the participation of the military in PDVSA management, as measured by the share of military (both active and retired) in the PDVSA board of directors. Several facts are striking. The first one is that at no moment in the series do the military make up more than one-third of PDVSA management. The majority of PDVSA's board over the past two decades, as well as today, is civilian. The second one is that the series is largely trendless, with alternating episodes of decline and increase. The third one is that while there is a rise in the share of military personnel under Maduro, the level of militarization as measured by this indicator in late 2017 or 2018 was not too different from that in the 2005-11 period, and was actually lower than in 2001, near the beginning of Chávez's presidency.

Interestingly, the series does become much more volatile in late 2017. Between 2001 and 2016, there was on average a change in board every 1.2 years. In contrast, between August of 2017 and October 2018, there were seven changes in board in a 14-month period. While it is possible that this greater volatility in management affected oil production, this pattern of frequent shake-ups is also what one would expect to see in a firm whose production is plummeting and whose owners are trying different strategies, including management changes, to halt that decline.

In any case, the data suggests it would be very hard to prove a link between militarization and oil output. Even though the deterioration in oil production in 2014-17 coincided with a period of re-militarization, the country's decline in oil production between 2001 and 2008 coincided with a period of de-militarization of the oil industry. The time-series data suggest, if anything, a lack of correlation.

This point is developed more systematically in **Table 5**. Regressing the log of oil production on militarization of the PDVSA board in a time-series regression produces a negative, insignificant coefficient. Once we control for the effect of sanctions, the militarization effect becomes positive (more military board members lead to higher production) albeit insignificant, while the sanctions variable is negative and significant at the 10% level. Adding a time trend and controls produces a small, negative and statistically insignificant militarization coefficient (p=.73), while the coefficient on financial sanctions remain negative and significant.

It is also worth noting that the point estimates of the militarization hypothesis are not only statistically insignificant, they are also economically not all that relevant. Even if we take the highest absolute value coefficient in **Table 5** (specification 3, -.30), it would imply that an 18 percentage point increase in the militarization rate of PDVSA, as has taken place under the Maduro administration, would lead to a 5 log point decline in oil production, or (taking the August 2017 number as a baseline) a reduction in 102tbd in the country's oil production, which valued today would amount to USD 2.2bn. In contrast, the sanctions coefficient in the same regression would imply that sanctions led to a 37 log point decline in production, or 599tbd, valued today at USD 12.7bn. In other words, even using the specification that is most favorable to the militarization hypothesis and ignoring its lack of

statistical significance, we would attribute to militarization an effect that is around one-sixth the size of that of sanctions.

TABLE 5: TIME SERIES REGRESSIONS FOR MILITARIZATION HYPOTHESIS

Model	1	2	3	4
Militarization	-0.1044	0.1366	-0.2999	-0.0614
	(0.1523)	(0.2890)	(0.3139)	(0.1800)
Financial sanctions		-0.5087*	-0.3703*	-0.2071**
		(0.2669)	(0.2196)	(0.0966)
Time trend			-0.0014*	0.0001
			(0.0008)	(0.0012)
Domestic consumption (ln)				(0.4677)
				(0.5348)
Per capita GDP (ln)				0.9373*
				(0.5040)
Refining capacity (ln)				-6.9273
				(5.4850)
Oil reserves (ln)				-0.0163
				(0.0726)
Polity Index				0.0000
				(0.0052)
Constant	7.7071***	7.7399***	8.6446***	51.4107
	(0.0716)	(0.0229)	(0.5270)	(37.3274)
AR terms	2	2	2	2
N	219	219	219	219
Wald Test	96.78***	93.71***	124.39***	343.65***

Table displays the results from estimating time-series regressions on the monthly Venezuelan data. All regressions follow an AR specification with two autoregressive lags. Sample begins on October 2000 and ends on May 2019. Control variables in column 4 are updated for the more recent time periods for which they are unavailable assuming GDP evolves as in Central Bank data up to 3Q18 and IMF forecasts for the remainder of the sample.

Source: Torino Economics

A full-fledged testing of the time-series model linking oil production and its determinants is outside the scope of the paper. It is possible that the results shown in **Table 5** could be reverted under more complex specifications. There are severe limitations in evaluating the patterns of this data, including the fact that we only count with annual data for all our explanatory variables. What **Table 5** aims to do is not to provide us with a definitive model estimate for Venezuelan oil production in time-series data, but to show that both the unconditional and conditional correlations present in the data do not appear to lend support to the idea that militarization is a driver of the production decline.

6 Evaluating the plausibility of competing hypotheses

Evaluation of competing hypotheses in event-study analysis typically also requires some examination of the plausibility of the causal links implicit in each hypothesis. In this section, we take a step back and discuss the plausibility of competing hypotheses, including sanctions and militarization as well as other alternative explanations.

The presence of confounding factors is almost always a problem for event-study analysis in the social sciences. Generally speaking, in non-experimental settings it is always possible to find other factors that change at around the time of the treatment and which could be put forward as alternative causal hypotheses. Sometimes it is possible to test these alternative hypotheses using the data, and sometimes it is not. When it is not, researchers must often rely on a subjective assessment of the more general plausibility of the assumptions underlying each of the hypotheses put forward.

To take an extreme and perhaps trivial example, there are many other events that took place in 1989, the year in which California's anti-smoking Proposition 99 studied by Abadie et al. (2010) was passed. Ronald Reagan left office, the AIDS epidemic hit San Francisco, and the 49ers won the Super Bowl for a second consecutive year. Can we tell a story whereby these factors influenced smoking consumption in California? Probably. Would it be a very plausible story? Probably not. Should we conclude that Proposition 99 did not cause the decline in cigarette consumption in California because of the presence of these alternative hypotheses? Not unless we can tell a reasonably convincing story about the mechanism for their effect.

In other words, concluding that an intervention had no effect simply because other potential treatment interventions were present at the same time is not generally the best practice in event-study or differences-in-differences analyses. The best practice is to try to find ways in which you can test one hypothesis against the other one – as we have attempted to do in section 5 - and, if that is lacking, to analyze the general plausibility of the competing hypotheses.

a. Financial sanctions

The hypothesis that financial sanctions are the explanation for the acceleration in the rate of decline of PDVSA's oil output in the second half of 2017 is based on the premise that there are strong links between finance and real activity. In other words, this hypothesis posits that the oil industry's lack of access to credit made it increasingly difficult for the firm and its partners, including suppliers and clients, to carry out activities that were necessary to maintain the firm's level of oil production.

Lack of access to external finance can lead a firm that has no access to alternative sources of funding to cut back on operating expenses, with immediate effects on production. But there is another myriad of ways in which sanctions barring lending can curtail a firm's ability to carry out its day-to-day activities. This is because modern

finance is commingled with a set of other activities that are essential to the productive process. Most obviously, trade credit is often a necessary by-product of relations with vendors, and while the August 2017 sanctions carved out exceptions for trade-credit of under less than 90 days, there is substantial evidence that these exceptions were far from sufficient to protect all trade-related loans.[24]

In this sense, it is important to highlight that the financial sanctions hypothesis is part of a broader "financial toxification" hypothesis that contends that the decline in PDVSA's oil production was impacted by the increase in the regulatory and reputational costs for financial institutions and intermediaries of interacting with any entities owned by the Venezuelan government that occurred in the context of the worsening of the country's political crisis in 2017 (Rodríguez, 2018). The decision by the National Assembly in early 2017 to warn international banks that if they lent money to Venezuela they would be illegally financing a dictatorship after the government decided that it would not request parliamentary authorization for debt issuances, was a key inflection point in this worsening, as was the opposition's vocal denunciation of Goldman Sachs' purchase of USD 2.8bn of bonds in May of that year from Venezuela's central bank through an intermediary. These actions should be seen as steps in the implementation of a strategy to use what the opposition saw as one of its only effective weapons at the time in its fight against Maduro's moves to curtail Venezuelan liberties: its ability to restrict the Venezuelan government's access to funds. The decision by U.S. authorities to formally bar financing to the Venezuelan government on August of 2017 should be seen as an additional step in the escalation of that strategy.[25]

The financial toxification hypotheses thus tell us that it was the country's worsening political crisis that led to the loss of access to international financing, with costly spillovers into oil production and economic growth. The opposition's use of their ability to curtail the government's access to funds has to be understood within the context of the government's prior decisions to undermine key elements of Venezuelan democracy by invalidating the request for a recall referendum in October of 2016, stripping away the National Assembly's legal authority through the Supreme Court decisions of March 2017 and convening elections for a Constitutional Convention in July of 2017 without having held the required prior referendum. At a very basic level, the financial toxification hypothesis should be understood as the thesis that politics was a key driver of the country's economic crisis.

Recognizing that the onset of financial toxification was a continuous rather than a discrete process does imply that timing the sanctions intervention at the end of August of 2017 does not capture the full breadth of this hypothesis. That said, a good case can be made that efforts to impair the Venezuelan government's access to credit gained force in the spring and summer of 2017, culminating in the August 2017 sanctions and September 2017 issuance of a letter of guidance by the Financial Crimes Enforcement Network (FinCen) warning that "all Venezuelan government agencies and bodies, including SOEs [state-owned enterprises] appear vulnerable to public corruption and money laundering" and recommending that several transactions originating from Venezuela

[24] For example, even Citgo, the PDVSA-owned U.S. refining arm, began having trouble obtaining routine trade credit after sanctions were imposed. See Parraga, M. and Ngai, C. (2017).
[25] On May, Venezuelan opposition economist Ricardo Hausmann (2017) had penned what would become a highly influential op-ed arguing that Wall Street was earning profits at the expense of increased hunger of Venezuelans, thus setting the stage for the promotion of efforts to curtail these investments.

be immediately flagged as potentially criminal. Timing the intervention earlier in 2017 does not alter the statistical results presented in sections 2-5.

As the Venezuelan government became financially more toxic – in the precise sense of being more prone to generating regulatory and reputational risk for its partners – firms that had previously been willing to do business with it decided that the benefit was no longer worth the risk. Even prior to the adoption of sanctions in August of 2017, tankers carrying Venezuelan oil shipments were stranded off U.S. coasts, as purchasers were unable to obtain letters of credit from financial institutions.[26] One important casualty of financial toxification was the loss by the Venezuelan government of correspondent banking relationships enabling the execution of wire transfers and trade finance. Following the decision of large financial institutions such as Citibank to close Venezuela's correspondent bank accounts, the Maduro government started shifting its correspondent banking activity – which is necessary to carry out wire transfers and trade-related credit operations in the United States – to small and less known financial institutions. At the end of 2017, after the sanctions announcement, these banks ceased providing correspondent services to the Venezuelan government, often citing increased reputational risk.[27]

However, there are also two direct ways in which the August 2017 sanctions affected the state-owned firm's ability to carry out production and investment activities. In 2013, PDVSA had begun signing financing agreements whereby foreign partners would lend funds into joint ventures with the state-owned oil company as long as the loan could be repaid directly from the joint venture's production. Given that Venezuelan law requires the government to be the majority shareholder in joint ventures (JV), this design allowed foreign partners to fund most of the investment in the JVs, without being formally granted a higher stake in the company. In return, partners would be paid directly from offshore accounts into which the proceeds from oil sales would flow directly before being transferred to the PDVSA-controlled entity.

Between 2013 and early 2017, PDVSA had signed 7 of these financing agreements with foreign joint-venture companies for a total USD 10.9bn in lending. Loan terms ranged from 3 to 13 years and loan rates fell in the range of Libor plus 5-7%, well below the costs at the time of PDVSA's unsecured financing. Production in these joint ventures rose by 9.0% between the date of execution and early 2017, according to data collected by independent oil consultancy IPD Latin America, during a period in which, as we have seen, production fell for Venezuela as a whole. U.S. financial sanctions brought these financing arrangements to a halt, and thus impeded PDVSA from continuing to use one of the most effective mechanisms that it had found to sustain production.

Similarly, beginning in 2015 PDVSA had begun refinancing its arrears with service providers such as General Electric, Halliburton and Schlumberger through the issuance of New York law promissory notes at 6.5% interest. By mid-2017, PDVSA had issued and remained current on at least USD 3.2bn of these promissory notes. Like many other oil companies, PDVSA had begun accumulating arrears after oil prices plunged in 2016, and foreign suppliers were willing to continue providing some level of services as long as they found a way to ensure that their arrears would be paid. U.S. financial sanctions had two effects on these arrangements: they stopped the company from

[26] Ulmer, A. and Parraga, M. (2017).
[27] Pons, C. (2017).

issuing new notes, and they also barred holders of these notes from trading them – unless they obtained an OFAC license to do so – thus generating large losses for the firms that had accepted to refinance PDVSA debt through these mechanisms.

If financial sanctions were indeed behind the acceleration in the rate of decline in oil production, we could expect to see different evolutions in projects that were less dependent on these financing agreements. Regrettably, data on production by field or even by subsidiaries for PDVSA is not available, and it would be extremely difficult to characterize their credit exposure even if it were. But one interesting piece of evidence comes from the evolution of production in entities in which the financing was being provided by the nation's Chinese and Russian partners, for which production data has been made public. As non-U.S. entities, Chinese and Russian firms were not subject to U.S. financial sanctions. Perhaps even more importantly, we can also safely assume that they were not constrained by reputational considerations, given the strong stance of the governments that control them in support of the Maduro regime.

In December of 2018, output at Sinovensa, the joint venture between PDVSA and China's National Petroleum Company accounting for around a tenth of Venezuela's oil production, stood at 130tbd.[28] This is essentially unchanged from the average 2017 production level of 128tbd estimated by the U.S. Energy Information Administration.[29] Output at joint ventures with Russia's Rosneft grew 7% y-o-y in 2018, according to a statement by Rosneft CEO Igor Sechin, who also added that the company does not expect significant declines in output of its Venezuelan JVs in 2019. These figures contrast strongly with the 30% drop observed in Venezuela's oil production as a whole in 2018.[30] In other words, output at Russian and Chinese joint ventures, which were much less affected by sanctions and toxification, remained stable and even grew as the remainder of Venezuelan oil production was collapsing.

These examples also shed some light on the redundancy hypothesis put forward by Zambrano (2018) and Bahar et al (2019) according to which sanctions were not binding because Venezuela was already shut out of capital markets. In order to carefully consider this hypothesis, it is important to draw a distinction between different types of debt, and in particular between sovereign unsecured debt and PDVSA commercial and secured debt. While it is true that the government of Venezuela was unable to obtain financing for unsecured transactions before the adoption of sanctions, the evidence just discussed clearly shows that PDVSA was able to obtain financing at the time both for the rescheduling of PDVSA commercial debt and for transactions secured by external assets or revenue flows. These transactions are inherently different from sovereign unsecured debt because there is an additional incentive for creditors to participate in them, be it the ability to renew profitable service contracts or the value of the assets or flows used as collateral.[31]

[28] Cohen, L. (2018).
[29] U.S. Energy Information Administration (2018).
[30] Both CNPC and Rosneft provided funds to Venezuela that would have been barred for U.S. firms by financial sanctions during this period, with CNPC buying an additional 9.9% stake in Sinovensa in 2018 and Rosneft receiving a 30-year license to operate offshore gas fields in late 2017, as well as supplying PDVSA with oil products after the 2019 sanctions. See Rosneft, Rosneft Information Division. (2017). Also see Kurmanaev, A. and Krauss, C. (2019).
[31] In this context, the collateral of the JV finance agreements is the access to revenue flows from JV sales before they reach the Venezuelan government.

It may still be the case, of course, that even these obligations are being acquired at an unsustainably high financing cost; this is the argument made by Bahar et al. (2019) when referencing the high yields implicit in PDVSA's collateralized exchange of bonds due 2017, which they estimate at 22%. This implies that PDVSA would have sooner or later proved unable to service these obligations and would have been forced to default on them, suffering the same consequences on output as would have been produced by the sanctions. This is not so much an argument that the sanctions were irrelevant but rather that they precipitated what would have anyway happened in their absence. This is a more sophisticated form of the redundancy hypothesis, in that it argues that even if the sanctions were not redundant at the time of their adoption (immediate redundancy), they would eventually have become redundant over time (eventual redundancy).

The eventual redundancy variant invites several reactions. First, as we have seen, there is very broad variation in financing costs faced by PDVSA in commercial and secured transactions in the 2016-17 period, with JV loans and refinancing of commercial arrears occurring at rates of around 6-7 percent. This suggests that there were some mechanisms under which PDVSA was still able to obtain financing at a reasonable cost due to the interest of some creditors (JV partners and suppliers) in continuing to carry out activities from which they derived profit. While it may be true that PDVSA was unable to refinance its bond debt at a reasonable cost, the evidence suggests that this was not the case for other types of financing directly linked to productive activities.

A second point is that even if the nation would have eventually lost access to financing, bringing forward that moment in time could have caused significant revenue losses for the country. Relatedly, companies that enter into default typically have the option of restructuring their debt, which PDVSA was impeded from doing because of the financial sanctions. These two points reinforce each other in that bringing forward the moment of default is costlier for the nation when the nation is at the same time impeded from restructuring.

This leads us into a contentious issue, which is whether the Maduro administration would have been capable of restructuring its debt in the absence of sanctions. It is possible to argue that the poor quality of macroeconomic management under Maduro would have made a restructuring impossible, while it is also possible to make the contrary argument: that Maduro, as many of his leftist counterparts such as Nicaragua's Daniel Ortega, could have shifted to more orthodox policies if that became the only way in which he had access to resources.

An additional problem with the eventual redundancy hypothesis is that it fails to take into account the fact that the recovery in oil prices that took place through 2017 and 2018 would have been expected, in the absence of sanctions and toxification, to lower the cost of PDVSA financing. **Chart 13** lays out this case by presenting the evolution of oil prices and the cost of ensuring against a default on Venezuelan debt, as measured by prices on 5-year Credit Default Swaps. As we can see, investors were willing to offer insurance on Venezuelan debt for around 1000 basis points (10% of the nominal value of the insured debt) during the period up to late 2014 when oil prices were at around USD 100. As oil prices fall to around USD 40/bl in early 2015, the cost of insuring against default rises sharply to around 5000bp. CDS costs spike to their highest level (9316 bp) precisely at the moment at which oil prices hit their minimum (USD 21.6/bl) at the start of 2016. As oil prices recover into the mid-40s – and the government defeats predictions of default despite the oil price stress – CDS costs fall to around 4000bp.

In other words, prior to the adoption of U.S. sanctions, there was a very strong negative correlation between oil prices and perceived default probabilities (-.93). This breaks down completely after sanctions are imposed: between August and November 2017, oil prices recover strongly, but CDS prices continue to rise (the correlation in this period is +.79). This suggests that the market began to see oil prices as irrelevant to the occurrence of a default after the adoption of the sanctions, as they understood that the sanctions were likely to drive the country into a default regardless of the evolution of the oil market.[32]

Chart 13: Venezuelan oil prices and Credit Default Swap prices

Sources: Torino Economic, Bloomberg

What the data is telling us is that in the absence of sanctions, one should have expected Venezuelan default probabilities and the cost of financing to the government to have declined as oil prices improved. To fix ideas, note that between the first half of 2017 and the second half of 2018, Venezuelan oil prices rose from USD 38.5/bl in September of 2016 (the moment of the PDVSA bond swap referred to by Bahar et al.) to USD 64.9/bl in the second half of 2018. Taking the August 2017 production level as a reference, this implies that Venezuela would have been able to reap USD 18.7bn in additional oil revenues from increased oil prices, more than double the USD 9.0bn of service on its bonds that it was scheduled to pay in 2018. Those additional oil revenues should have

[32] Bahar et al.'s (2019) Figure 1 shows the JP Morgan EMBI measure of sovereign risk but make no mention of oil price fluctuations. As is the case for the CDS data, the EMBI measure shows a strong negative correlation between oil prices and Venezuelan spreads up until August of 2017, with the correlation similarly breaking down after 2017. Bahar et al. argue that the lack of increase in the sovereign spread after sanctions suggests that the market did not perceive the default as relevant. However, they fail to consider that the relevant counterfactual in the absence of sanctions would be that spreads would have fallen, given the strong recovery experienced by oil prices in the second half of 2017.

significantly improved the country's access to financing in 2018 in the absence of sanctions, making a default less likely as well as improving the conditions under which the government could have refinanced or restructured its debt. If the government managed to avert default with oil prices in the 30s, one should not jump to conclude that a prolonged default (i.e., one that was not followed by a successful restructuring) would have become inevitable with oil prices in the 60s.

Ultimately, an even more serious problem of both variants of the redundancy hypothesis is that it is hard to explain why the U.S. would adopt a set of policies that had no effect on the Venezuelan government's access to resources. That is, if we believe Venezuela would have had lacked access to any type of financing even in the absence of sanctions, then we must conclude that Donald Trump's August 2017 Executive Order, which then barred transactions which would not have taken place, was essentially irrelevant. It is hard to understand why the Trump administration would have chosen to respond to Maduro's decision to conduct the illegal 2017 elections for the Constitutional Convention through a decision that had no effect on the choice set of the Venezuelan government.

The redundancy hypothesis runs counter to the statements of high-ranking Trump administration officials at the moment of the adoption of sanctions, which suggest that U.S. authorities believed that the actions significantly curtailed the Maduro regime's access to resources. At a press briefing held immediately after the publication of Executive Order 13808, National Security Adviser H. R. McMaster stated that the ban "strongly punishes the Venezuelan regime…Through the President's strong action, the United States will target the means with which the Maduro dictatorship can accumulate debt to enrich its corrupt regime insiders and perpetuate its repressive behavior."[33] Treasury Secretary Steven Mnuchin added that "Maduro may no longer take advantage of the American financial system to facilitate the wholesale looting of the Venezuelan economy at the expense of the Venezuelan people." According to the redundancy hypothesis, these statements must reflect either ignorance or deliberate misrepresentation of the consequences of their actions by high-ranking U.S. officials.

Interestingly, the redundancy hypothesis also runs counter to statements by at least one high-ranking official of the Guaidó administration. On April of 2019, Jon Bilbao, a former PDVSA official that had been fired by Chávez in 2003, was appointed President of PDVSA-owned Monómeros, Colombia's largest producer of fertilizers, by interim president Juan Guaidó. On June 6, Bilbao told the *Financial Times* that his most pressing concern was getting U.S. sanctions lifted. The company had swung from net profits of USD 20mn in 2016 to net losses of USD 23mn in 2018, after financial sanctions were imposed. According to Bilbao, if sanctions were lifted, Monómeros could turn to produce a profit this year. Bilbao referred to the difficulties in obtaining routine trade credit, noting that the 30-day sanctions trade credit exemption was too stringent given that under normal conditions suppliers allowed 60-120 days for payment.[34] Because Monómeros' production process lasted at a minimum three months from the moment of purchase of raw materials to the sale of finished products at markets, the company had to pay for raw materials up front without any cash flow, Bilbao said, adding that banks had also cut their access to credit for fears of running afoul of U.S. sanctions.[35] Bilbao's statements are consistent with those often made by

[33] Gearan, A. and Faiola, A. (2017).
[34] Despite being a PDVSA subsidiary, Monómeros is subject to the more stringent 30-day trade credit exemption.
[35] Long, G. (2019).

Maduro government officials; what is remarkable is that they come from an official of the new interim administration.

In sum, there are good reasons to consider the sanctions hypothesis as a plausible explanation for the decline in oil production. Financial sanctions as well as the more general toxification of Venezuela's finances made it increasingly costly for its partners, suppliers and service providers to carry out activities that were necessary to sustain production. Sanctions also prohibited two types of arrangements which were being intensively used by PDVSA into 2017 in order to maintain production: financing of JVs by partners and refinancing of commercial debt through New York law promissory notes. Output at JVs with Chinese and Russian partners, which were largely unaffected by sanctions, has remained stable or grown even as production at other subsidiaries was collapsing. And ultimately, U.S. authorities made it abundantly clear that the purpose of financial sanctions was to deprive Venezuela's government of funds. The data suggests that they achieved their objective.

b. Militarization

As explained in section 5, the militarization hypothesis contends that the decision by the Maduro government to put General Manuel Quevedo at the helm of the country's oil industry, alongside the increasing involvement of military personnel in the firm's operations, was the cause of the acceleration of the decline in production. We have already discussed the empirical evidence in favor of this hypothesis in that section; we now focus on the broader plausibility of it as an explanation.

One way to think about the militarization explanation is that it is telling us that in November of 2018, President Maduro took a decision to replace PDVSA's management which had an immediate and large, negative effect on oil production. In fact, one of the pieces of evidence that Morales (2019) puts forward for the hypothesis is that of immediacy, highlighting that right after the appointment of Quevedo on November 26 of 2017, we see oil output falling by 8.4% in December, much higher than the average 2.4% decline observed in the preceding three months after sanctions.

For starters, the presence of a large and immediate adverse effect of a new PDVSA president on oil production – so rapid that it can be rapidly observed just days after his appointment – should raise some alarm flags. First and foremost is the question of whether new management can affect production so rapidly. (Note that, by contrast, oil production decelerates only gradually after the August 2017 sanctions.[36]) The pattern is particularly odd since Quevedo inherited the same board of his predecessor Nelson Martínez, a new board was only appointed on December 22 of 2017, and because Quevedo would only be granted broad-ranging powers over the state-owned oil company's contracts in April of 2018.[37] Thus, it appears more probable that we would see the effects of this change in management set on more gradually rather than occurring immediately upon his appointment.

[36] The rate of decline in oil production, which averaged 1.0% from January 2016 to August 2017, fell to 1.8% in September and 1.5% in October, to then accelerate markedly from November onwards.
[37] Buitrago, D. (2019).

If this hypothesis were true, it would be unclear why Maduro took a decision that led his government to suffer such massive losses in revenues. Admittedly, we have ample evidence that Maduro has, for most of his administration, taken policy decisions which have been economically suboptimal. A case in point is the decision to run six years of large fiscal deficits financed by monetization, ultimately driving the economy into hyperinflation. However, the immediacy of the output effect postulated by the militarization hypothesis makes it somewhat different from other apparently myopic policy decisions. Inflationary financing or currency overvaluation are policies that have short-term benefits but long-term costs and are thus rational for policy makers with high discount rates as are typically seen in political systems with high stakes of power.[38] But, if a policy leads to no short-term gains but rather imposes large and immediate revenue losses on the government, then even very short-sighted policymakers will have no reason to adopt them.

Immediacy of adverse effects is also problematic because it significantly reduces the model uncertainty faced by policymakers. It is one thing to monetize budget deficits on the belief that they don't cause inflation and to face the consequences of a mistaken monetary theory several years later; it is quite another to have immediate verification that you were wrong. To borrow a medical analogy, individuals often take decisions which have adverse long-term effects, such as smoking or consuming high amounts of sugar. But most individuals suffering from severe allergies will do their best to avoid contact with the allergen after suffering their first episode of anaphylactic shock. In this analogy, the shock is the USD 3.2bn a year in foregone oil revenues caused just by the December loss in production, which the militarization hypothesis attributes to Quevedo's appointment. The paradox is that immediately after suffering it, Maduro seems to have asked for another helping of the allergen.

Put differently, the militarization hypothesis tells us that Maduro took a decision which had an immediate and verifiable effect of subtracting around USD 20bn a year from Venezuelan state coffers, in a context where those additional oil revenues would have likely stopped the country's economy from suffering a massive contraction and sliding into hyperinflation. Is it possible to come up with explanations for why this could have been optimal from Maduro's standpoint? Sure. One can say that he needed to do it in order to buy off the military so that he could stay in power. Or that he wasn't aware that production was declining (maybe because Quevedo hid the information from him). Or one could simply explain it away as a consequence of Maduro's irrationality (i.e., the incompetence hypothesis redux). But the fact that we need to come up with these auxiliary hypotheses means that we have to make the militarization hypothesis increasingly convoluted and thus inherently less plausible.

It is, for example, difficult to square this explanation with other government decisions in very similar contexts. In March of this year, Venezuela experienced massive nation-wide blackouts that left the country without energy for eight days of a single month. On April 1, Maduro fired one - and only one - member of his cabinet, Electricity Minister General Luis Motta Domínguez, and replaced him with a civilian engineer, Igor Gavidia. So, at what is possibly the greatest moment of threat to his hold on power and when he relied the most on the military to stay

[38] See Rodríguez (2019b), chapter 6, for a development of this hypothesis to explain the policies of the 2013-17 period.

in power, [39] Maduro was free to choose to fire a general and replace him by a civilian in response to a crisis that cost the country an estimated USD 1.6bn in direct losses. If so, why is it that two years ago, at a moment of much greater political stability, Maduro couldn't do the same to an incompetent general who – according to the militarization hypothesis – was responsible for losses more than 10 times as large than those of the electricity crisis?

Or think about the idea that the only way that Maduro could keep the military leadership sufficiently happy was to give them direct control of PDVSA even though this would cost state coffers around USD 20bn a year. In order to sustain this thesis, we would have to assume that it was impossible for Maduro to maintain PDVSA at pre-Quevedo levels of efficiency and to make side payments to the military that would have kept them equally happy. With USD 20bn in potential side payments, it is hard to come up with a satisfactory explanation of why the military would have demanded direct control of the industry rather than simply receive a hefty transfer of funds.

It is not that one cannot come up with answers to these questions. There are always ways to modify an explanatory hypothesis with more auxiliary hypotheses to make it fit the data, as we know well since at least the times of Ptolemaic astronomy. Yet there are good reasons why scientists prefer simple, straightforward answers anchored on parsimonious theories rather than complicated and convoluted ones, if both do equally well at explaining reality.[40] It is because we know that it is easy to manipulate theories by increasing their complexity to make them conveniently fit the data. If you torture the theory enough, it will confess.

And in this sense, the sanctions hypothesis is parsimonious, simple, and straightforward. While the militarization hypothesis requires us to believe that Maduro took a reversible decision that had the immediate effect of leaving his government with around USD 20bn less a year in export revenues, the sanctions hypothesis only requires us to believe that Donald Trump decided to leave Maduro with around USD 20bn less a year to fund his regime.

c. Corruption investigations

The 2017 changes in the leadership of the oil industry coincided with the jailing of several high-ranking industry executives on corruption allegations. Most prominently, on November 30, 2017, the country's Prosecutor General announced the arrests of two former PDVSA presidents, Nelson Martínez and Eulogio Del Pino. Martínez had presided CITGO from 2013 to 2017, before being tapped by Maduro to become oil minister. Del Pino had presided PDVSA between 2014 and 2017, moving to the oil ministry precisely as Martínez was shifted to PDVSA in August. Only days before their arrests, they had both been replaced by General Manuel Quevedo. The arrests were part of a broad-ranging corruption investigation in which warrants were issued for 95 government officials, including 12 high-ranking executives as well as the two former ministers.

[39] On January 23, National Assembly President Juan Guaidó claimed the Presidency and was subsequently recognized by 53 countries. On April 30 there would be a failed coup attempt led by Guaidó. Therefore, early April was one of the moments in his presidency when Maduro most counted on military support for his survival.

[40] There is an extensive literature in epistemology associated with parsimony as a criterion for choosing among competing scientific theories. See Sober (2015).

One plausible hypothesis is that it was this investigation, rather than the militarization of the oil industry *per se*, that caused a paralysis in the firm's professional cadres and precipitated the collapse in the oil industry. It is certainly not hard to tell a story whereby public officials become excessively cautious in the midst of corruption investigations and prefer to suspend decisions which could fall under the scrutiny of investigators. Fears of policy paralysis are commonly cited as a concern in wide-ranging corruption investigations.[41]

This variant faces some of the same problems of the militarization hypothesis pointed out above: why would Maduro have ordered such a wide-ranging corruption probe in the country's oil industry if he knew that probe would cost him around USD 20bn in revenues? And, if he didn't know initially that the cost would be so high, why didn't he revert the decision as soon as he understood the implications? How plausible is it to believe that Maduro had in his power a decision that could lead to the recovery of billions of dollars of lost revenues – to suspend the investigation and bring old managers back into the oil industry leadership – yet decided (and continues to decide) not to do so?

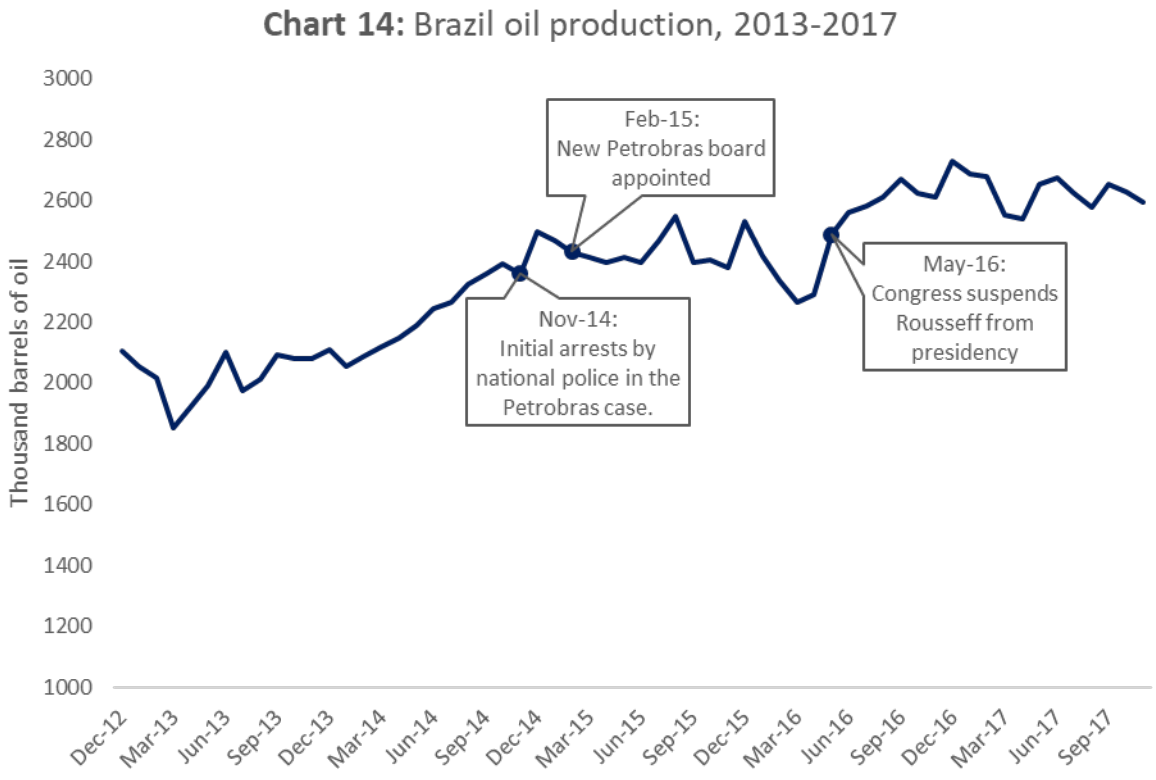
Were Venezuela a nation with an independent judiciary and prosecutor's office, one could claim that once corruption investigations are started it is hard to put a stop to them. But that is certainly not the case in a country where these institutions are in practice completely subordinated to the power of the executive branch. One would have to stretch credulity too much to believe that there is any semblance of commitment to public transparency in the investigations of a regime whose links to organized crime and drug trafficking are exhaustively documented. Given the complete subordination of the Prosecutor General's Office to the regime – the current prosecutor was appointed by the rubber-stamp National Constitutional Convention after the previous one broke with the government – it is evident that reverting the investigation and most of its effects is an action that was and continues to be in Maduro's choice set.

Nevertheless, it is still worth asking whether it is plausible to expect that a wide-ranging corruption investigation could lead to a complete paralysis in a state-owned oil company of the magnitude that we have seen in Venezuela's oil production. One interesting point of comparison could be the Petrobras corruption investigation, which had similar consequences for the management of Brazil's state-owned oil firm.

Though every two cases are different, the Petrobras investigation has several elements in common with Venezuela's. First, it involved many seemingly untouchable figures of Brazilian politics, including former President Dilma Rousseff, who chaired the board of the company between 2003 and 2010. Second, it involved, as did Venezuela's, alleged kickbacks in contracts between the firm and private sector providers. The scale of both investigations was similar with 110 persons formally charged with corruption, money laundering, and other financial crimes in Brazil.

[41] See, for example, Huang, Y. (2015). Also see: Vaishnav, M. and Smogard, D. (2014).

Chart 14 displays the evolution of Brazil's oil production through the onset of the Petrobras investigation. The initial set of arrests came in mid-November of 2014, and a new Petrobras board was appointed in March of 2015.



Sources: Torino Economics, EIA

The investigation proceeded through 2015 and March 2016, eventually bringing about the suspension and impeachment of Rousseff in 2016 and the conviction of former president Luis Inácio Lula da Silva in 2018.[42] The chart shows little to no effect of the investigation on Brazilian oil output.[43] Brazilian oil output grew by an annual average of 4.6% in 2015 and 2016, as opposed to an average of 3.5% in the previous four years.

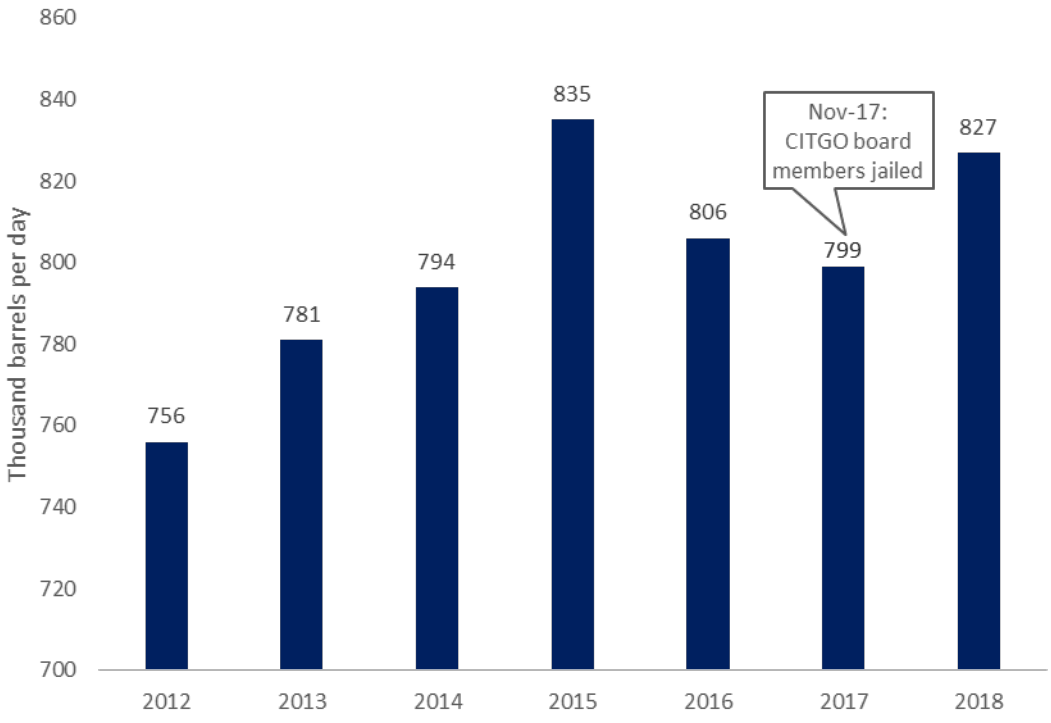
Perhaps it is too much of a stretch to compare the policy paralysis effects of a corruption investigation in Dilma's Brazil to Maduro's Venezuela. While the *Lava Jato* investigation was seen by many as a genuine attempt to root out corruption from the Brazilian system, the PDVSA investigations were generally perceived as a way to settle accounts among different power groups within *Chavismo*, with little semblance of being a genuine attempt to root out graft. If that's the way in which oil firm managers saw them in each country, then one could explain why Venezuelan managers could have been thrown into paralysis much more deeply than Brazilian ones. Ideally, we

[42] Although Rousseff was impeached on charges of misrepresenting budget figures, the calls for her ouster were clearly impacted by the results of and her perceived involvement in the *Lava Jato* scandal.
[43] Petrobras accounted for 90% of Brazilian oil production in 2014. Monthly data is available up to April 2015, and yearly data afterwards, showing similar trends to those displayed in Chart 15.

would like to be able to compare the effect of similar corruption investigations on firms with similar corporate governance structures and subject to similar political incentives and constraints. Otherwise we could be capturing the responses of completely different reaction functions.

Fortunately, we do have one such source of comparison in the Venezuelan oil industry. PDVSA's U.S. subsidiary, CITGO, was the focus of essentially the same corruption investigation as all the rest of the state-owned oil industry. In fact, the PDVSA corruption investigation began with the arrest of six top CITGO executives – including the subsidiary's acting head – on November 22, on accusations that they had tried to defraud the nation through an unauthorized restructuring of CITGO's financial liabilities. One would have expected that the arrest of these executives would have sent shock waves through CITGO's management, many of whom were drawn from the same political groups of the whole PDVSA leadership and could be brought back to Caracas on the government's orders.[44] If the hypothesis is that the 2017 corruption investigation paralyzed industry managers who were afraid to take even routine decisions for fears of being accused of corruption, one would expect to see a similar paralysis to have ensued in CITGO after the arrest of most of the company's board.

Chart 15: CITGO refining production, 2012-18



Sources: Torino Economics, CITGO financial statements

[44] To take one example, CITGO's CFO at the moment of the scandal – yet unaffected by it given his recent appointment at the time – was Calixto Ortega, a family member of a Constitutional Court justice who now serves as Central Bank President.

The data shows the contrary. In 2018, CITGO's refining output rose by 3.5%, while net income jumped 22.3%. CITGO operations, therefore, seem to have been unfazed by the 2017 corruption investigations. This is at the same time as PDVSA's production was collapsing, and other PDVSA-owned subsidiaries like Monómeros (see the discussion in section a) were thrown into disarray by the sanctions. CITGO, in contrast, appears to have been largely unaffected by whatever it was that caused the industry's collapse.

There is one crucial difference between CITGO and PDVSA and its other subsidiaries. One day after the issuance of Executive Order 13805 on August 24, 2017, banning financing of PDVSA and its subsidiaries, the Treasury Department issued a General License authorizing all transactions by CITGO Holding or its subsidiaries that would otherwise have been banned by the new sanctions. Because of its relevance to the U.S. market for refined products, CITGO was the only part of Venezuela's oil industry that was fully exempted from sanctions. And it appears to be the only part of Venezuela's oil industry that saw its production and revenue grow in 2017, despite facing a corruption investigation that landed most of its board in jail.

d. Investment cuts

Perhaps a more promising alternative explanatory hypothesis would link the collapse of Venezuela's oil output in 2017 to the cuts in investment that resulted from the cash crunch suffered by the nation's oil company after oil prices declines sharply towards the end of 2014. As oil prices declined and Venezuela slashed imports, investment in the oil industry also suffered. PDVSA fixed investment fell by 54% between 2014 and 2016, the last year for which PDVSA has published consolidated financial statements. Other measures of investment, available for more recent time periods, also show sizable declines: from 2014 to 2017, completed wells fell by 29% and active rigs fell by 33% (31% if we consider only the pre-sanctions period of 2017).

It is quite plausible that these investment cuts led to a decline in the firm's productive capacity that lies behind the output drop that began in 2016 and accelerated in late 2017. The limited data available does not show an acceleration in the rate of investment decline, but given nonlinearities in the production function, it is certainly possible to account for an accelerating decline in productive capacity even with a relatively steady rate of investment decline.

In order to know whether the investment drops can account for the patterns in the output data, we would need to count with a fuller specification of the oil industry's production function, including estimates of the rate of decline per fields and a better sense of the time lags between investment and production. The construction of such an estimate is beyond the scope of this paper. Until then, the investment cuts hypothesis remains a reasonably plausible contending account of part – or all – of the post-2016 production decline.

There are, however, a number of complications that impede us from drawing clear conclusions from the evolution of the investment data. To start with, the PDVSA data on gross fixed investment is aggregate, low frequency, and unavailable for more recent time periods. A particularly problematic aspect of this data is that it is in nominal dollars, with the local currency values converted at the accounting rate used by PDVSA for its financial statements.

This exchange rate depreciated rapidly between 2014 and 2016, falling much more rapidly than the real value of the currency according to the general price level.[45] Therefore, part of the drop in investment is an artifact of real exchange depreciation, and it is hard to know how much without a more concrete breakdown of costs. Furthermore, even the real value of investments carried out in dollars may not have dropped as rapidly as the nominal dollar values, given that the price of investment goods related to the oil sector was also falling alongside crude oil prices. Perhaps even more importantly, there are huge variations in the nominal series over time, with a seven-fold rise between 2004 and 2014 apparently having little effect on productive capacity, suggesting that variations over time in the efficiency of investment dwarf any changes in the series with concrete effects on productive capacity.

It may thus seem more promising to consider non-financial proxies for investment such as completed wells or active rigs. While both of these fell between 2014 and 2017, neither of the declines was unprecedented in the historical data. Active rigs fell from 76 to 52 between 1H14 and 1H17, similar to the decline from 78 to 54 seen between 2H07 and 2H09, another period of falling prices. Completed wells – for which we only have annual data – actually rose between 2014 and 2016, dropping sharply only in 2017. Even the low average level observed in 2017 (which could be impacted by sanctions, as it is a yearly average) is above the level observed in 2013.

Predicting the evolution of oil production and linking it to the evolution of aggregate observable proxies of investment is an extremely complex task which goes well beyond what we have set out to do in this paper. Nevertheless, an interesting datum comes from the production forecasts of industry experts just before the sanctions. By mid-2017, industry experts were fully aware of the extent of the decline in investment, based both on existing sector and financial data. Therefore, any decline in production caused by investment declines should have been incorporated into their forecasts. By contrast, the adoption of financial sanctions at the time was largely unexpected and thus unlikely to have been considered in these forecasts.[46] The assessment of these industry specialists can thus give us a reasonable counterfactual of the way in which we would have expected oil production to evolve in the absence of sanctions.

In May of 2017, IPD Latin America – perhaps the most prominent oil consultancy covering Venezuela – published a report outlining three possible scenarios for Venezuela's oil production. Up until that moment, IPD could boast a spotless record in predicting the evolution of Venezuelan oil output. In that report, they presented a "worst case" scenario which predicted an average rate of decline of oil production of 7.6% a year over the 2016-20 period (a more moderate scenario forecast a 2.8% decline and an optimistic one predicted a 3.0% recovery). The IPD report emphasized the decline of investment as the driver of this decline, stating that "low oil prices had led to a

[45] Between 2014 and 2016, the VEF/USD exchange rate used by PDVSA in its financial statements rose by 1,297%, much greater than the 687% rise in average prices according to the Central Bank's CPI.

[46] Through the summer of 2017, there was some speculation that the U.S. would impose oil sanctions. A story by The New York Times published on August 25 said that the administration had decided not to implement these given fears of their effect on the U.S. refining sector. See Krauss, C. (2017). It is important to remember that the onset of Venezuela's political crisis was quite rapid, beginning with the Supreme Court's decisions 155 and 156 on March to declare the National Assembly in contempt and the July 30th elections to the Constitutional Convention – which was the actual trigger of sanctions. Many political forecasters were indeed predicting up into late July that Maduro would not go through with the Constitutional Convention which prompted the financial sanctions. See Toro (2017).

lack of resources for drilling operations, diluent imports and paying oil service providers in a timely fashion." It did not mention any risk of sanctions – which, as we have argued, were unexpected at the time – but did refer to the risk of PDVSA reorganization and corruption investigations as additional reasons for its pessimistic outlook. Nevertheless, even in that pessimistic scenario which accounted for investment cuts and the risks of corruption investigations, it predicted that active rigs would rise to 47 on average for the 2018-20 period from 40 in 2017.[47]

Even these bearish forecasts were unable to come close to the magnitude of the ensuing contraction. Since 2017, production has fallen at an annual rate of 35.9%, almost five times as rapidly than in IPD's most pessimistic scenario. Instead of recovering from their 2017 lows as expected, active rigs have dropped to 22, less than half their 2017 value.[48] The magnitude of the post-August '17 drop in production took even the best forecasters in the field by surprise.

It is almost certain that the decline in investment following the 2014 plunge in oil prices had an effect on Venezuela's oil production. And in fact, it is the only apparent explanation for the decline of 18% (422tbd) in output seen between December of 2015 and August of 2017. As we highlighted in sections 1 and 2, declines of this magnitude took place in countries with similar trajectories such as Argentina, Colombia, Mexico, or our synthetic group before the onset of sanctions. Whether they can account for the post-2017 evolution of production appears more doubtful, given the fact that investment drops of similar magnitudes had taken place in the past without generating even remotely similar effects on production. This was precisely what led forecasters at the time to expect at worst a moderate continued decline in production.

In order to account for the post-2H17 decline in production, it thus appears that the investment hypothesis would require the presence of an economically significant interaction between investment drops and another variable that was not present during past investment declines. For example, it could be argued that the drop in investment was likely to cause a collapse in production only if it interacted with a political crisis or the relative price uncertainty generated by hyperinflation. The further development of this hypothesis remains an open area for future research.

[47] IPD Latin America (2017).
[48] Rigs ended up averaging 49 in 2017 according to Baker-Hughes data.

7 Assessing impacts on social indicators

Perhaps the most controversial assertion of WS is their claim that sanctions contributed to an estimated 40 thousand deaths from 2017-18. It is worth noting that in the text of the paper, the authors only claim that sanctions have contributed to this figure, but their Executive Summary and broader dissemination have made the stronger and more controversial claim that sanctions have "inflicted" 40 thousand deaths.[49] It is worth noting that, in private communications, Jeffrey Sachs highlighted that he subscribes to the language in the body of the paper, which emphasizes the intrinsic uncertainty concerning the magnitude (though not the sign) of the mortality effects, rather than the language of the executive summary and press release, which left out the expression of uncertainty.

The 40 thousand figure is essentially taken from applying the 31 percent estimated increase in mortality from the National Survey on Living Conditions (ENCOVI) to the country's population figures. The calculation thus attributes all the increase in mortality observed in 2018 to U.S. financial sanctions. Put differently, the WS Executive Summary implicitly uses a counterfactual of zero increase in mortality in the absence of sanctions.

HMB are correct in their criticism of this calculation as problematic given the absence of any attempt to identify the relative contribution of other factors that could have impacted on health. As they point out, the country's public health system was undergoing severe problems from before the adoption of sanctions, suggesting that other factors could be at play in driving the observed deterioration both before and after the sanctions. Sanctions may have had an effect, but there are so many other potential causes that it is hard to justify attributing the totality of this variation to sanctions without more systematic analysis.

Although WS do not make explicit their reasoning behind this calculation, they would appear to be relying strongly on two ideas: (i) that variations in mortality are primarily determined by variations in income, and (ii) that variations in income are primarily determined by variation in oil exports. There is some support in the literature for both claims.

First, there is an extensive literature on the links between GDP per capita and a vast array of health and broader living standards indicators, including mortality.[50] In particular, a case has been made that nearly all the cross-national variation in health outcomes can be explained as a result of per capita income and other socioeconomic variables that do not vary over short periods of time (e.g., inequality, female education, ethnic and religious factors).[51] It thus makes sense to suspect that a large increase in mortality that coincides with a large decrease in

[49] In the text of the paper, the authors write that "it is virtually certain that the US economic sanctions made a substantial contribution to these deaths." However, the Executive Summary makes a much stronger claim, stating that "sanctions have inflicted, and increasingly inflict, very serious harm to human life and health, including an estimated more than 40,000 deaths from 2017–2018". This phrasing was also used in press releases and is widely cited in the press. See, for example: Buncombe, A. (2019).
[50] See, Pritchett, L. and Summers, L. (1996) and the Human Development Report (2010).
[51] Filmer, D. and Pritchett, L. (1999).

income (Venezuela's GDP dropped by 18% in 2018, according to our estimates) is likely a result of that decrease in income.

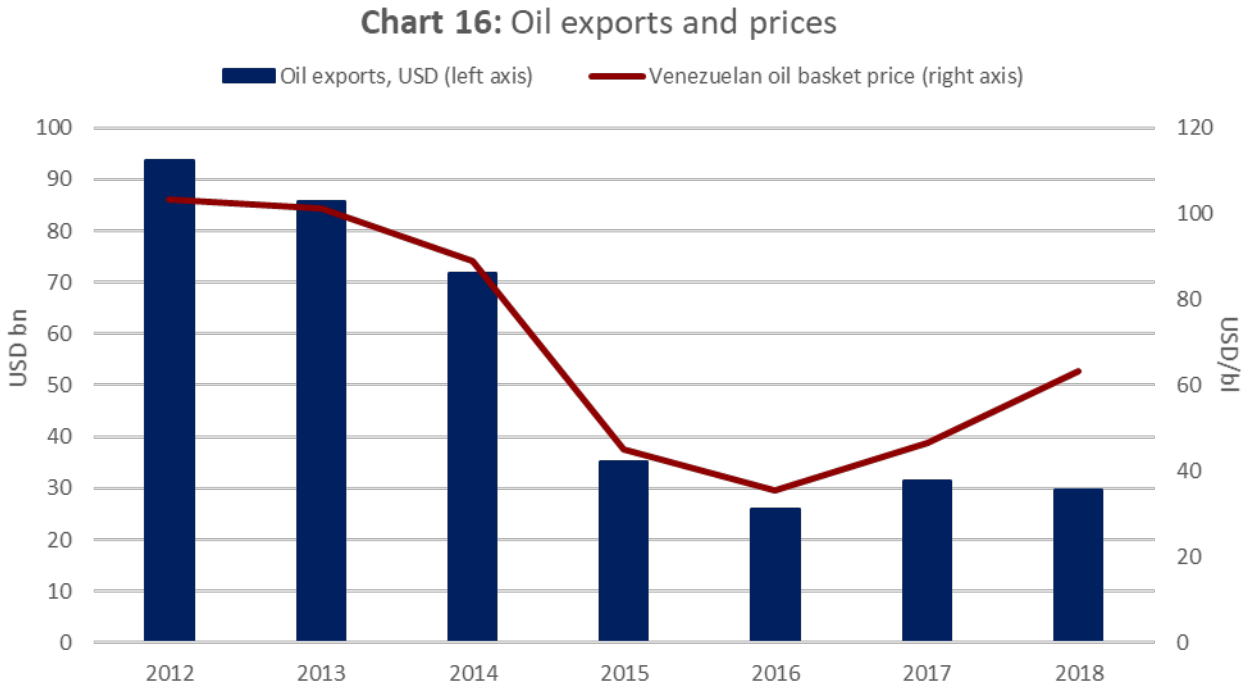
On the link between export revenues and growth in the Venezuelan case, there is also a broad literature. Empirically, Venezuelan growth is highly import-dependent, with imports (which are, of course, paid for with export revenue) accounting for 79% of the variation in growth rates since 1997 (Rodríguez and Guerrero, 2019). Furthermore, we know that the collapse in income over the 2012-18 period has been accompanied by a huge income contraction (imports fell by 88% and GDP by 48% over this period). That changes in oil revenues lead to changes in GDP by altering the economy's capacity to provide for imports is an expectable feature of an economy that is completely specialized in oil (Hausmann and Rodríguez, 2012).

Therefore, if one assumes that large variations in mortality (particularly increases, which are rare) are driven by changes in income and one assumes that Venezuelan GDP would have stabilized in the absence of the decline in production caused by the 2017 sanctions, then one can conclude that the main cause of the increase in mortality that year was the adoption of sanctions.

Of course, the fact that such a back-of-the-envelope calculation is justifiable does not mean that it should be given the veneer of certainty that it received in the WS Executive Summary and some of the ensuing discussion. In our view, the presentation of such a conclusion should be approached with utmost caution and accompanied by multiple and extensive caveats, given the uncertainty about the choice of model to infer the exports-mortality link.

However, there is an even more basic problem with this line of thinking. If the channel through which sanctions affect standards of living is through declining oil revenues and per capita income, then sanctions cannot be the cause of the increase in mortality observed in 2018. This is because oil export revenues did not fall significantly in 2018. **Chart 16** shows that, despite the decline in oil production observed in 2018, oil prices recovered strongly that year, rising 35% (from USD47 to USD63 per barrel of Venezuelan oil). This oil price rise was sufficient to offset most of the effect on exports of the 30% decline in oil production seen in 2018. Oil export revenue in 2018, at USD 29.8bn, was only slightly lower than 2017 revenue of USD 31.5bn and higher than that of 2016 (USD 25.9bn); furthermore, the country's total goods imports actually rose in 2018 (from USD 12.0bn to 14.9bn), partly because the default on the country's external debt allowed the economy to run a lower current account surplus. But then it is hard to argue that the government had less money for imports in 2018 as a result of sanctions. In other words,

sanctions could not have led to the deterioration of social indicators by causing lower oil incomes because – even despite the effect of sanctions – oil revenues were not significantly lower in 2018 than in prior years.

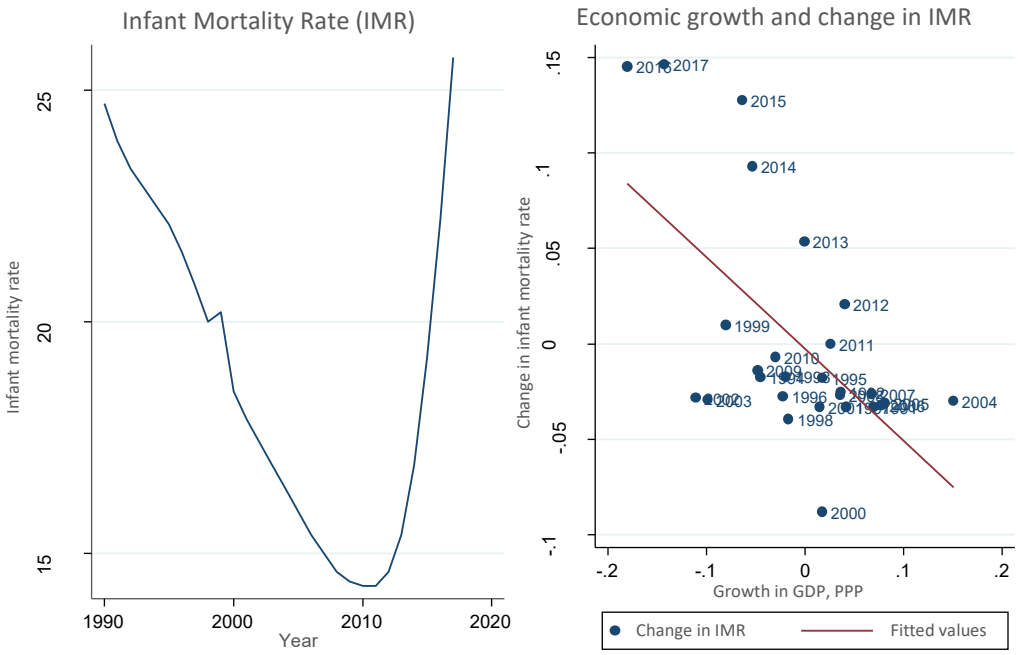


Sources: Torino Economics

This does not mean that sanctions did not *contribute* to the deterioration in living standards in 2018 and 2019; it just means that they cannot *account for* it. In fact, if one finds the existence of an export-growth link and a growth-mortality link plausible and believes that sanctions negatively affected oil production, then one would conclude that living standards would have been higher in the absence of sanctions. This is because higher oil production and export revenues would have permitted economic growth (or at least the avoidance of such a deep economic contraction) likely leading to across-the-board improvements in living standards in comparison with a scenario under sanctions. The increase in oil revenues in 2018 does not prove that sanctions did not have an effect on social indicators; it simply tells us that the increase in mortality in 2018 used by WS is uninformative about the magnitude of that effect.

For the same reason, we find that the evidence presented by Bahar et al. (2019) of strong pre-trends in social indicators such as mortality, real wages and food and medicine imports is also uninformative regarding the effect that sanctions could have had on these indicators from 2017 onwards. This is because their data is strongly consistent with the existence of a link between income and social indicators (one of the conditions for there to be an effect of sanctions on these variables) and because there are good reasons to believe that the reduction in output observed between 2012 and 2016 was driven by different factors than those that drove the contraction from 2017 onwards.

Chart 17: Infant mortality rate and economic growth, 1990-2017



The left panel reproduces the time series of infant mortality represented in Figure 8 of Bahar et al. (2019). The right panel plots the annual change in infant mortality rate in the same data against the growth rate of GDP in constant purchasing power from the World Bank's World Development Indicators Data Bank.

Sources: Torino Economics, Bahar et al. (2019)

Chart 17 illustrates the first point using data from Bahar et al.'s Figure 8. The left panel reproduces their plot (with a few more years of data), showing a strong reversion in the trend of decline in infant mortality at around 2012, with the post-2012 increase offsetting the pre-2012 gains. The right panel shows the same data, now plotted against the economy's growth rate. The data display a strong, negative association between growth and increases in mortality. The years from 2013-2017 are all in the upper left quadrant of the figure, showing negative economic growth and infant mortality increases. In other words, **Chart 17** suggests that Bahar et al. are correct in their intuition that the same underlying process is driving the increase in IMR for the 2012-17 period. That process is the massive decline in the economy's GDP.

Now, while it is true that the Venezuelan economy has been contracting since 2013, this does not mean that it contracted for the same reasons during all of this period. Put differently, different factors may have driven economic contraction in the 2012-16 period than in the period than from 2017 on.

We have a pretty good sense of what drove the economic contraction between 2012 and 2016. We know that in this period, the economy suffered a massive negative terms of trade shock, with oil prices falling by 66% and oil

exports declining by 72%. The economy was ill-prepared for this shock, as it had accumulated next to no net savings from the oil price boom of the preceding 13 years. Lack of security of property rights and poor macroeconomic policies compounded the problem, making the government lose access to international capital markets as soon as oil revenues dried up. Therefore, the economy could not borrow nor deplete enough savings during this period, leading to the import cuts which drove the economy's implosion.

We know that the story from 2017 on is different because oil prices began to recover in 2017. From 2017 on, terms of trade cannot explain the contraction because the economy was benefiting from a positive terms of trade shock. For the same reason that we expect a credit-constrained economy to cut back imports in presence of a negative terms-of-trade shock, we would also expect it to expand imports when it benefits from a positive terms-of-trade shock. This did not happen in Venezuela. The main reason for this is that oil production began declining.[52]

In other words, whether sanctions contributed to the worsening in socioeconomic indicators in 2017 and beyond depends on the role that one assigns to sanctions in the decline in oil production. If one believes – and, as we have argued in the preceding sections, we see good reason to believe – that sanctions contributed to the decline in oil production, then it follows that export revenue would have been higher, and the deterioration of socio-economic indicators lower, in the absence of sanctions.

[52] Another important reason is that the country went from running a current account deficit in 2016 to running large current account surpluses in 2017 and 2018. This is due to a combination of the depletion of existing assets and the loss of access to international capital markets discussed in section 6a.

8 Concluding comments

This paper has provided evidence that the 2017 financial sanctions and the 2019 oil sanctions adversely affected the Venezuelan economy and contributed to the decline in the country's income and living standards. We have provided several models and alternative estimation techniques to approximate that effect. In our preferred specification, we estimate that financial sanctions were associated with economic losses of USD 16.9bn per year. Additional cross-national estimates suggest that oil sanctions, imposed earlier this year on an already decimated oil industry, will likely lead to a further decline in oil output.

We have attempted to address the methodological concerns raised by other authors in this debate. In particular, we have shown that the identification of a negative effect associated with sanctions in the post-2017 data is not dependent on the use of Colombia as a control, as claimed by HMB, but is instead robust to the choice of counterfactual. We applied synthetic control methods to produce an adequate control group, estimating a large negative post-treatment effect. We also used cross-national data to estimate the effect of oil sanctions such as those imposed on Venezuela earlier this year. And we have shown that the alternative militarization hypothesis has problems accounting for variations in the time-series data: even using the most generous specification to this hypothesis, we find that it can explain at most one-seventh of the decline in output that the 2017 sanctions account for. In contrast to the militarization, corruption or investment hypotheses, which must be modified in *ad hoc* ways to fit the data, the sanctions hypothesis yields additional predictions – that production should have stabilized or grown in Chinese and Russian joint ventures, or that it would not have been affected in sanctions-exempt subsidiaries – which are confirmed by the evidence.

The results of the positive analysis in this paper do not necessarily imply a normative position with respect to sanctions. Deciding whether one supports or opposes sanctions will depend on several additional factors that we have not touched upon. Among them are whether one adopts a consequentialist or a deontological approach to ethics. From a deontological perspective, information about the effects of sanctions is irrelevant; what matters is whether imposing them violates the rights of Venezuelans or whether doing so is within the legitimate sphere of the rights of the United States.

In a consequentialist perspective, one needs to weigh the benefits and the costs of sanctions. The costs of sanctions will of course include the economic and social costs, which we have attempted to estimate in this paper, and in this sense the results of our analysis would certainly inform a consequentialist approach. But, even having identified high costs of sanctions, it is impossible to stake a position on them from a consequentialist perspective without assessing their potential benefits, such as their effect on the probability of producing a reestablishment of the country's democracy.

Furthermore, even if we conclude that sanctions generate net costs for Venezuelan society, this does not necessarily mean that we should attribute blame to the United States government for these effects. Assigning

responsibility for outcomes requires a prior discussion as to which political actors overstepped the bounds of their legitimate sphere of decisions when sanctions were imposed: was it the Trump administration, by imposing measures that, as WS argue, could violate international law? Or was it Nicolás Maduro, by suppressing Venezuelans' right to elect their government in free and fair elections? Thus, one can conclude that sanctions have harmed Venezuelans and even that the costs that they have imposed on the country outweigh their expected benefits, yet still believe that the blame for them lies squarely on Maduro's side.

In addition, even if we find that the costs of sanctions outweigh their benefits, that does not mean that sanctions should be lifted. This is because there may be other policy choices in which sanctions are maintained but are complemented by other initiatives that help offset their negative effects. One example is the creation of an oil-for-food program that would allow oil production to recover but impose strong conditions on the use of the resources generated by the additional oil exports.[53]

Although the positive task of identifying the effect of sanctions on oil production should be kept separate from the normative task of deciding whether they should be kept in place, there are ways in which our normative position could inform our policy choices. One of them is through consideration of the welfare losses associated with making different types of errors. One type of error would be to fail to identify an effect of sanctions that exists, while another would be to identify an effect that does not exist. The large possible adverse human consequences of making the first type of error should point us in the direction of extreme caution when evaluating the adoption of sanctions under uncertainty about their effects. This is the position that I have taken in Rodríguez (2018), where I argued that even suggestive and tentative evidence that sanctions may have lowered Venezuelan revenues should lead us to approach the design of international policy initiatives that may further worsen the lot of Venezuelans with extreme caution.

Lastly, the question of what the effect of the 2017 and 2019 sanctions on the Venezuelan economy was is different from that of what the drivers of the country's economic performance were before 2017. The answers may of course be related, because the factors explaining the economy's contraction in the prior period may continue to have an effect. But they are two distinct lines of inquiry and academic analysis does not benefit from conflating and much less from confusing them. Arguing that sanctions could not have impacted the Venezuelan economy because the crisis began before the sanctions is similar to arguing that a terminally ill patient cannot be killed. Both are *non-sequiturs* that are rapidly identified as such by scholars; the former, however, has become puzzlingly prevalent in Venezuelan political circles.

In my view, it is a settled question that the economy's collapse up until 2017 was due to the poor policy choices and mismanagement of the governments in power since 1999. The economy went into a tailspin in 2014 after suffering a negative terms of trade shock because it was completely unprepared to handle such a shock as a result of the government's decision not to save and to increase indebtedness during the largest positive oil boom in the nation's history. Furthermore, there is extensive evidence that the use of resources by the *Chavista* governments

[53] See Rodríguez, F. (2019c) and Kronick, D. (2019).

was highly inefficient and unproductive, often yielding zero benefits, as in the case of the country's massive literacy program which had no visible effect on literacy trends seen in official statistics.[54] To borrow a useful American trope, Venezuelans could well say "my government went through an oil boom and all we got was this lousy crisis."

But claiming that Chávez and Maduro's economic policies caused a deterioration of living standards in Venezuela is not at odds with accepting the possibility that economic sanctions may have made things even worse. Most phenomena in social sciences have multiple causes. There is no logical reason why Chávez's irresponsibility, Maduro's incompetence and U.S. economic sanctions cannot all have contributed to worsening the plight of Venezuelans.

[54] Ortega, D. and Rodriguez, F. (2008). See also Rodríguez, F. (2003) and Rodríguez, F. (2008) for discussions of social and economic policies during the Chávez administration.

9 Data appendix

Our oil production variable is taken from several sources: OPEC's Monthly Oil Market Report, the U.S. Energy Department's International Energy Statistics, the U.S. Energy Information Administration's International Energy Statistics, Bloomberg's Crude oil production data (OPEC<GO> on the Bloomberg Terminal) and Colombia's National Hydrocarbons Agency's *ANH en Datos* dataset. We choose the series with the highest coverage for each country.

Annual data on oil consumption, refining capacity and proven reserves are taken from British Petroleum's Statistical Review of World Energy. The Polity Index of political institutions is from the Center of Systemic Peace's Polity Project. GDP per capita in constant PPP prices is from the World Bank's World Development Indicators Databank.

Data on the composition of PDVSA's board is constructed directly from the country's official gazettes.

We define oil sanctions as restrictions banning the purchases of oil from the sanctioned country's national oil company and/or privately-owned oil producers. We set the oil sanctions dummy to 1 in each month in which more than half of the days were spent under sanctions and to 0 in each month in which the country spent less than half of the days of the month under sanctions.

In the case of Iran, on January 24, 2012, the EU prohibited import of crude oil and products from the country, as well as financing operations for its oil sector. On January 16, 2016, the UN suspended oil sanctions against Iran under the Joint Plan of Action, a suspension which the EU also assumed. On May 8, 2018, the US government withdrew from Joint Plan of Action and re-imposed sanctions on Iran's energy sector but granted waivers to a number of countries. On April 22, 2019, it ended exemptions from sanctions for countries still buying Iranian oil. We thus set the oil sanctions dummy to 1 between February 2012 and February 2016, and from May 2018 to present.

For Iraq, UN Security Council resolution 661, adopted on August 6, 1990, prohibited all imports from Iraq for signatory countries. After the First Gulf War, UN Security Council Resolution 687 of April 8, 1991, reaffirmed these sanctions by linking them to the proliferation of weapons of mass destruction. Resolution 706 of August 15, 1991, created the first food-for-oil program, though it was not effectively implemented due to the Iraq government refusing them until 1996. Sanctions were lifted on May 22, 2003, after the end of the Iraq war. Because the oil-for-food program allowed Iraq to export oil in the period comprised by our study, we maintain the oil sanctions dummy at zero in our panel data set which begins in October of 2000. However, we exclude Iraq from the donor pool for the synthetic group analysis, because financial restrictions were in place after the launch of the oil-for-food program until 2003.

UN Security Council resolution 1970, adopted on February 26, 2011, prohibited the provision of financing and froze assets belonging to a number of entities related to Libya's Muammar Gaddafi. UN Security Council

Resolution 1973, adopted on March 17, 2011, expanded the subject of the asset freeze in resolution 1970 to all entities linked to the Libyan authorities (including the central bank and national oil company). While the sanctions do not explicitly forbid imports of Libyan goods, they froze the country and national oil company's accounts, which had the same effect given export proceeds could no longer be received by the authorities. UN Security Council resolution 2009, adopted on September 17, 2011, lifted economic sanctions against the Libyan National Oil Company and Zueitina Oil Company[55] and against the central bank of the country. We set the oil sanctions dummy to 1 between February and September 2011.

On September 12, 2014, the European Union enacted financial sanctions on Russia's Rosneft, Transneft and Gazprom Neft (which, together, account for over 50% of Russia's oil production), as well as major financial entities. The sanctions also ban involvement in new oil projects and the provision of oil services by European companies. The U.S. also issued financial sanctions against a number of Russian entities during 2014 and 2015. The sanctions, nonetheless, do not prohibit the oil imports from Russia, and we thus maintain the oil sanctions at zero. As in the case of Iraq, we exclude Russia from our donor pool for the synthetic control group for Venezuela given the presence of partial financial sanctions during the sample period.

On August 24, 2017, the Trump administration issued executive order 13808, which banned U.S. persons from providing financing with maturities longer than 90 days for PDVSA and 30 days for the government of Venezuela. The order also restricts dividend payments from entities controlled by the Venezuelan government, and which reside in the U.S. (which, affects Citgo specifically). On January 28, 2019, U.S. persons were banned from engaging in commercial transactions with PDVSA. While the January sanctions allow for a wind-down period, U.S. persons buying oil from the company are required to deposit proceeds in blocked accounts, which effectively means that the Maduro administration is unable to receive payments for oil exported to the U.S. We set financial sanctions to begin on September 2017 and oil sanctions to begin on February 2019.

[55] Decides that the Libyan National Oil Corporation (LNOC) and Zueitina Oil Company shall no longer be subject to the asset freeze and other measures imposed in paragraphs 17, 19, 20 and 21 of resolution 1970 (2011) and paragraph 19 of resolution 1973 (2011).

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