

## Sanctions and Oil Production: Evidence from Venezuela's Orinoco Basin

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We use the differential access to credit of oil firms in Venezuela's Orinoco Basin to identify the economic effects of financial and oil sanctions on firm output. Using a panel of monthly firm-level oil production from 2008-2020, we provide differences-in-differences estimates showing that financial and oil sanctions led to large losses in oil production among firms which had access to international credit prior to sanctions. The estimated effects explain around half of the output drop experienced in those firms since the adoption of sanctions, for a total loss of around 235 thousand barrels per day. We also argue that by impeding the government from extending special financing arrangements to other firms in the area, sanctions precluded the adoption of policy decisions that could have stabilized production at pre-sanctions levels.

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<sup>1</sup> Visiting Fellow, Kellogg Institute for International Studies, University of Notre Dame. I thank Dany Bahar, María Eugenia Boza, Dorothy Kronick, Geoff Ramsey, Gustavo Rojas, Marianna Párraga, David Smilde and participants at the Notre Dame Development Workshop for comments and Adolfo De Lima, Arnaldo Espinoza and Juan Vera for excellent research assistance. All errors remain my own.

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### 1. Non-technical summary

This paper uses new evidence from one of Venezuela’s largest oil-producing regions to assess the effect of financial and oil sanctions on the nation’s oil industry. The collapse over the past five years of oil output, which previously generated more than nine-tenths of the country’s export earnings, plays a central role in the country’s broader economic crisis. Lower export revenues from oil have led to a dearth of foreign exchange, causing the government to sharply cut back imports and triggering one of the largest economic contractions in contemporary world history.

Starting in 2017, the United States imposed increasingly tight restrictions on financial and trade-related transactions involving the government of Venezuela. The role of these sanctions in the country’s oil and economic collapse is a controversial issue. Using diverse quantitative methods, several research papers have identified large and significant effects of successive waves of sanctions. Other scholars have warned that there are multiple competing explanations, including prior lack of investment and mismanagement of the state-owned oil company, which could equally well explain the collapse.

To this date, all quantitative studies of the economic effect of sanctions on the Venezuelan oil industry have focused on the analysis of aggregate national oil production data. While the national data does show an acceleration of the rate of decline of oil production after each round of sanctions, this evidence is at best suggestive, given the multiple other potential determinants of industry performance. A key issue of discussion is how much of the effect can be attributed to the 2017 financial sanctions, which barred the state-owned oil company from borrowing or refinancing its debt, and how much to the 2019 oil sanctions, which impeded it from accessing specific export markets.

This paper's contribution is to address these questions using a microeconomic data set which contains information on monthly oil production of specific firms in a region of Venezuela's oil industry since 2008. This detailed data allows us to control for other potential factors that could also affect production at the national level. Concretely, it allows us to separate out the effect of any other variables that affect the whole oil sector at a given moment of time, as well as those that affect only specific firms. Doing so allows us to more precisely estimate how different firms vary in their reaction to economic sanctions.

Our statistical approach is based on the observation that there are significant differences in the degree to which firms in the country's oil sector were exposed to international financial markets prior to sanctions. A large part of investment in the country's Orinoco Basin – the area in which our study focuses - is carried out by joint venture arrangements in which private sector companies partner with PDVSA. Prior to sanctions, some of these joint ventures had entered special financing deals which allowed them to borrow from their foreign partners to fund ongoing investment projects. Our hypothesis is that in response to sanctions, the behavior of these firms with access to international finance was different from that of the rest of the sector, which lacked that access.

If the 2017 financial sanctions impacted oil production, we would expect the most-affected firms to be those that had access to financial markets prior to sanctions. For those firms, sanctions meant losing that access. In contrast, there is no reason for firms that lacked financial market access prior to sanctions to have been affected by sanctions. Therefore, we expect to see a faster drop in output, relative to their prior performance, in firms with prior financial market access than in the rest of the sample.

Indeed, we find such an effect. The effect is quantitatively and statistically significant and robust to alternative specifications. Firms that had entered special financing deals prior to sanctions suffered a much more rapid drop in growth in the post-sanctions period than those that had no such access. In our baseline estimates, sanctions explain between 45 and 54% of the observed drop in production in firms with financial market access. Somewhat counterintuitively, we find that firms with U.S.-based partners were more protected than firms where the partners came from other countries. One explanation of this may be the willingness of U.S. authorities to grant specific licenses exempting U.S.-based firms from sanctions.

Given that firms with financial market access accounted for around half of production in the Orinoco Basin prior to sanctions, the effect that we identify can account for around one-fourth of the observed drop in output in the region. This should be interpreted as a lower bound estimate of the effect of sanctions, as it captures the effect that works through one specific channel – that of access to credit markets via special financing vehicle arrangements.

In a different yet complementary interpretation of our estimates, sanctions barred the oil industry from a specific form of financing arrangements – namely loans from joint-venture partners with payment secured through control of export flows – that had proven successful between 2013 and 2017 and which the government would likely have chosen to continue extending to the rest of the sector. In that alternative scenario, we estimate that sanctions can account for around three-fifths of the decline observed in the region.

We make no attempt to extrapolate our estimates outside of the Orinoco Basin. However, our estimates indicate that Orinoco Basin production would be between three to five times as high as its current level in the absence of sanctions. Only considering the additional Orinoco Basin production, our estimates indicate that Venezuela's export revenues in the absence of sanctions would have been two to three times as high as they were in 2020.

## 2. Introduction

Economic sanctions are a foreign policy tool commonly used to attempt to induce changes in the conduct of targeted nations or entities. Sanctions have become an increasingly important policy instrument as an alternative to full-fledged armed conflict over the past few decades. Although they are often imposed through multilateral bodies such as the United Nations Security Council, unilateral sanctions are also common. The frequency with which the U.S. government imposes unilateral sanctions as well as the breadth of their coverage have risen markedly in the recent past, with the absolute number of U.S. sanctions designations more than doubling over the past decade.<sup>2</sup>

Despite their growing use, there is considerable controversy regarding the effectiveness as well as the impact of economic sanctions. Hufbauer et al. (1990) documented 116 sanctions episodes since 1914 and spawned a literature of empirical studies analyzing the determinants of the success of sanctions. Sanctions have been found to be more effective in politically unstable countries, countries with a weak economy and those with closer ties with the sanctioning country.<sup>3</sup> Some have argued that sanctions are ineffective at sparking regime change because they generate inadequate incentives to relinquish power (Peksen and Drury, 2010; Oechsling, 2014; Cohen and Weinberg, 2019); others contend that sanctions specifically aimed at fostering democracy increase the probability of rulers losing power (Soest and Wahman, 2014).

The effectiveness of economic sanctions in achieving their intended goals is also significantly related to the magnitude of their overall economic effect as well as the distribution of that effect among agents in the sanctioned country. Neuenkirch and Neumier (2015) find that UN sanctions on average decrease the sanctioned country's per capita growth rate by 2.3-3.5 percentage points, but that the effect of unilateral U.S. economic sanctions is smaller and less distinct. Afesorbor and Mahadevan (2016) find that sanctions have a negative effect on the target country's income inequality, while Biglaiser and Lektzlan (2020) find that import sanctions cause losses in the sanctioned country's stock market only when the target country is not already affected by multiple previous sanctions.

Estimating the economic effects of sanctions is relevant for other reasons than gauging their effectiveness. Some legal scholars have argued that collective punishment of civilians is a violation of international law, akin to the use of siege warfare currently considered a war crime (Shagabutdinova and Berejikian, 2007; United Nations, 2019). To assess the strength of this argument, quantitative measures of the impact of sanctions on the general population, as well as estimates that allow us to distinguish between the costs borne by the economy as opposed to those that affect only targeted elites, become relevant. Understanding the effect of sanctions can also be pertinent to planning for

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<sup>2</sup> Imperiale, J. (2020)

<sup>3</sup> See the survey in section 4 of Kamepfer and Lowenberg (2007)

post-conflict recovery, as large sanctions impacts could imply a more rapid pace of growth once sanctions are lifted even in the absence of large levels of aid.

Most studies of economic sanctions use cross-national panel data sets to attempt to identify the effect of the adoption of sanctions on several outcome variables (Febelmayer et al., 2020; Kavakli, Chatagnier & Hatipoglu, 2019; Ahn & Ludema, 2020). In the absence of adequate sources of exogenous variation, these studies are plagued by the problems frequently associated with cross-country econometrics. Precisely because sanctions are designed to respond to political developments in the targeted country, it is hard to tease out cause from effect in the correlations observed in the data. Sanctions, for example, often target emerging authoritarian regimes in cases where the international community is trying to halt a process of democratic backsliding. To the extent that the political conflict sparked by an incumbent's power grab has negative economic effects, it is not surprising to observe acute deteriorations in economic indicators occur after sanctions. For the same reasons, evidence of growth recoveries after sanctions are eased could be simple reflections of the economic effects of the changes in government conduct that led to the lifting of sanctions.

This paper applies a differences-in-differences specification to firm-level data from Venezuela's Orinoco Basin for the 2008-2020 period to identify the economic effect of financial and trade sanctions on the country's oil sector. Between 2017 and 2020, the U.S. imposed sectoral economic sanctions on Venezuela restricting the access of the country's state-owned oil firms to international financial and oil markets. We use the fact that some joint-venture firms between the government and private sector partners had greater access to credit than others prior to the sanctions to identify the effect of limiting access to financial markets on oil production. The existence of differential levels of access to credit between firms allows us to control for time- as well as firm-specific factors that could affect production, focusing on the differential impact of credit market limitations on firms with different levels of pre-sanctions credit access.

Our paper uses a differences-in-differences specification to estimate whether there is a significant change in the rate of growth of production of firms that had financial market access – as proxied by observed capacity to enter into special financing deals – relative to those that lacked it. One drawback of our data is that firms in our treatment group (firms with market access) on average grow more rapidly than those in our control group (firms without it) prior to the adoption of sanctions. Our tests should thus be interpreted as tests of deviations of the “parallel growth” hypothesis – i.e., of a stable difference in growth rates between the groups. In implementing and assessing this specification, we make use of recent advances in dealing with non-parallel trends specifications. We also present an alternative interpretation in which the pre-sanctions period is defined as the treatment period and treatment as financial market access, and show that under this interpretation the parallel trends hypothesis holds in the non-treatment period.

The rest of the paper is organized as follows. In section 3 we provide a summary of U.S. sanctions on Venezuela in the period under study and review the empirical literature on their effect. Section 4 introduces our data set and discusses the key stylized facts that emerge from looking at its aggregate patterns. Section 5 presents our econometric analysis, and section 6 offers some concluding reflections.

### 3. An overview of Venezuela sanctions, 2015-2020

#### 3.1 A chronology of sanctions decisions

Nicolás Maduro was elected to the Presidency of Venezuela in March 2013, shortly after the death of his predecessor and mentor Hugo Chávez. In contrast to Chávez, Maduro had a much harder time maintaining political stability and ensuring electoral support. Partly due to declining oil prices and overspending during boom years, Venezuela plunged into recession in 2014 and the government lost control of the National Assembly in parliamentary elections held in December 2015. As major protests racked the country, the Maduro government increasingly appealed to more repressive and authoritarian methods, using its control of courts to stifle dissent and reduce the power of elected opposition leaders.<sup>4</sup>

In March 2015, the administration of Barack Obama issued an executive order declaring a national emergency with respect to the “unusual and extraordinary threat to the national security posed by the situation in Venezuela.”<sup>5</sup> This declaration served to create the current framework for Venezuela sanctions, as it allowed the president to restrict trade and financial interactions with actors associated with that threat under the 1917 International Emergency Economic Powers Act.

However, the Obama administration used those powers sparingly, designating only seven individuals linked to repression of protests in 2014 using these powers.<sup>6</sup> It would be the administration of Donald Trump that would come to use sanctions intensively as the basis of their Venezuela policy. These began with the designation of Vice-President Tareck El Aissami in February of 2017 and were subsequently increased to cover several cabinet members, supreme court justices, high-ranking officials, President Maduro and First Lady Cilia Flores.<sup>7</sup>

The U.S. would take the first step in the direction of economic – as opposed to individual - sanctions on August 24, 2017. On that day, President Trump issued an executive order prohibiting the purchase of new debt issued by the Government of Venezuela or PDVSA or of previously issued debt held by the government or entities under its control. It also barred dividend payments to Venezuela, impeding the government from using the profits from its offshore subsidiaries to fund its budget. Exceptions were built in for short-term commercial debt, winding down of existing contracts, and transactions related to the financing of purchases of agricultural commodities or medical goods from the United States. The 2017 sanctions were specifically motivated by the government’s decision to carry out elections for a National Constitutional Convention (*Asamblea Nacional Constituyente*), designed to strip away all powers from the opposition-controlled National Assembly, on July 30<sup>th</sup> of that year.

During 2018, the U.S. government issued three additional executive orders with limited immediate economic effects. The first one, published in March, blocked all transactions in cryptocurrencies issued by the Venezuelan government. A second order in May of the same year – issued on the day after a presidential election that the U.S. and the mainstream opposition considered rigged - blocked

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<sup>4</sup> Human Rights Council (2020).

<sup>5</sup> Federal Register (2015)

<sup>6</sup> The number rises to 18 if one counts Venezuelan entities designated using powers different from those granted by the national emergency declaration.

<sup>7</sup> While El Aissami’s 2017 designation was done in the context of the Kingpin Act related to drug-trafficking activities, most other designations were done within the framework of Obama’s national emergency declaration.

Venezuela from selling debt owed to the government – for example, accounts receivables of the state-owned oil company – or pledging existing assets as collateral in order to obtain financing. A third executive order allowed the Treasury secretary, in consultation with the State Department, to determine that actors in that particular sector of the economy were contributing to the national emergency generated by the Venezuelan situation and to single out those sectors for restrictions.

Eventually, the Trump administration would determine that four broad economic sectors were contributing to the national emergency: gold (November 2018), oil (January 2019), finance (March 2019) and defense and security (May 2019). Appealing to these determinations, it subsequently added several private and public sector entities belonging to these sectors to the SDN list. The designations were broad enough to essentially preclude U.S. actors from doing business with anyone in these sectors of the Venezuelan economy.

Oil, accounting for 95 percent of exports and 12 percent of GDP at the time of the sanctions, was by and large the most relevant sector of those targeted. The U.S. announced the decision to designate the state-owned monopoly of oil production and distribution, *Petróleos de Venezuela, S.A. (PDVSA)* as part of a major ratcheting up of pressure on the Venezuelan regime, shortly after its decision to recognize National Assembly President Juan Guaidó as the country's interim president.<sup>8</sup> Guaidó's claim to the presidency arose from an interpretation of Article 233 of the Constitution, which allowed the head of the Legislature to assume interim powers in case of a vacancy at the start of a presidential term. One implication of recognition was the transfer of control of Venezuela's assets abroad to the Guaidó administration, including the bank accounts of the central government, central bank, and PDVSA.<sup>9</sup>

Ultimately, the net effect of sanctions would depend on Venezuela's capacity to route that oil to other markets. Initially, the U.S. focused on blocking Venezuela from channeling the oil it could not export to the U.S. to other destinations, pressuring some of PDVSA's other clients so that they would not increase imports from Venezuela.<sup>10</sup> As the country's political crisis dragged on without clear resolution, the U.S. started increasing pressure on non-U.S. firms to cut (rather than just maintain) their purchases from Venezuela. In August 2019, it sent a strong signal that it was willing to do so by adopting a new executive order that blocked any transactions with the government of Venezuela and that also gave the executive branch the power to sanction non-U.S. persons for having "materially assisted" the Venezuelan government or its state-owned entities.<sup>11</sup>

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<sup>8</sup> DeYoung, K., Mufson, S. and Faiola, A. (2019); C-SPAN (2019).

<sup>9</sup> See Wong, E (2019) for the transfer of the Republic and central bank's accounts. The management of the accounts of PDVSA and its subsidiaries would take place more gradually as courts began to recognize Guaidó a[ppointees to the boards of these companies.

<sup>10</sup> While the U.S. has no jurisdiction to restrict trade between Venezuela and other countries, it can restrict trade between the U.S. and non-U.S. actors that do business with Venezuela. This threat of secondary sanctions has been effective at altering conduct of non-U.S. firms. For example, Reliance Industries, Venezuela's largest customer in India, announced in March 2019 that its U.S. subsidiary had stopped all business with Venezuela (as required by sanctions) and that its global parent "has not increased crude purchases." This happened after consultations with U.S. authorities and a direct warning from the Indian government. See Chakraborty, D., Kassai, L (2019) and Bloomberg (2019). On the US-India negotiations related to Venezuelan oil purchases, see Gordon, M., Gupte, E., Bambino, J. (2019)

<sup>11</sup> See Federal Register (2019). In fact, the August 2019 order was redundant. By that time, most of the entities of the Venezuelan public sector, including all its oil industry, had been blocked. U.S. authorities made a point nevertheless of

The U.S. would ultimately use this authority in February 2020, on the heels of an international tour designed to showcase Guaidó's global support that culminated in a visit to the White House and an appearance at President Trump's annual State of the Union address. Administration officials said at the time that the U.S. government's "maximum pressure" campaign against Venezuela was only "50-60 %" implemented and would continue to be ramped up as long as Maduro refused to give up power.<sup>12</sup>

The key secondary sanction decision was to designate two subsidiaries of the Russian energy company Rosneft that had handled business with Venezuela.<sup>13</sup> The U.S. also sanctioned two Mexican companies that had signed oil-for-food deals with Venezuela.<sup>14</sup> Rosneft at the time carried out 70-80 percent of Venezuela's oil sales – a predominance that had been spurred by other partners' caution at doing direct business with the country in the wake of the U.S.'s prior warnings. It had also supplied almost all the gasoline imported by the country during the previous year, as Venezuela's refining infrastructure remained beset by operational problems and the effect of sanctions.<sup>15</sup> Predictably, the country began suffering severe gasoline shortages shortly after Rosneft halted all trade with Venezuela and divested from its Venezuela operations.

### 3.2 Economic impact of sanctions decisions

Most studies of the effect of Venezuela sanctions have focused on their impact on the country's oil sector.<sup>16</sup> Since oil accounted for 95% of Venezuelan exports prior to sanctions, it would be natural to expect any first-order effects to impact the economy through the oil sector. Prior research has documented the high level of import-dependence of the Venezuelan economy, establishing a clear channel through which oil activity can impact growth: lower oil rents lead to import contraction, which drives the reduction in non-tradables output.<sup>17</sup> These studies have emphasized the relevance of complete specialization in driving the effect: since the economy is completely specialized in oil, even large real exchange rate depreciations do not lead to an expansion in alternative export or import-competing goods production, which act as cushions on the effect of term-of-trade changes in incompletely specialized economies.

Rodríguez (2018) first pointed out that the adoption of financial sanctions coincided with the acceleration of the rate of decline in Venezuela's oil production, which went from 1.0 per cent monthly in the period preceding the 2017 financial sanctions to an average of 3.1 percent per month in the subsequent 16 months. He also suggested the use of neighboring Colombia, which had similar pre-sanctions trends in oil production to Venezuela, as a potential counterfactual. While Colombia saw a similar decline in output in 2016 and early 2017, possibly a common reaction to plummeting global

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highlighting their new powers to sanction non-U.S. firms, to the extent that many analysts characterized the new order as the adoption of secondary sanctions. See, for example, De Alba, M. (2019)

<sup>12</sup> Lawler, D. (2020)

<sup>13</sup> Mohsin, S. and Millard, P. (2020)

<sup>14</sup> Kassai, L (2020)

<sup>15</sup> Argus Media (2019)

<sup>16</sup> An exception is Bull and Rosales (2020) which focus on the incentives for informalization and criminalization of the Venezuelan economy created by sanctions.

<sup>17</sup> Rodríguez, F. and Sachs, J. (1999), Hausmann, R. and Rigobon, R. (2002), Hausmann, R. and Rodríguez, F. (2012).



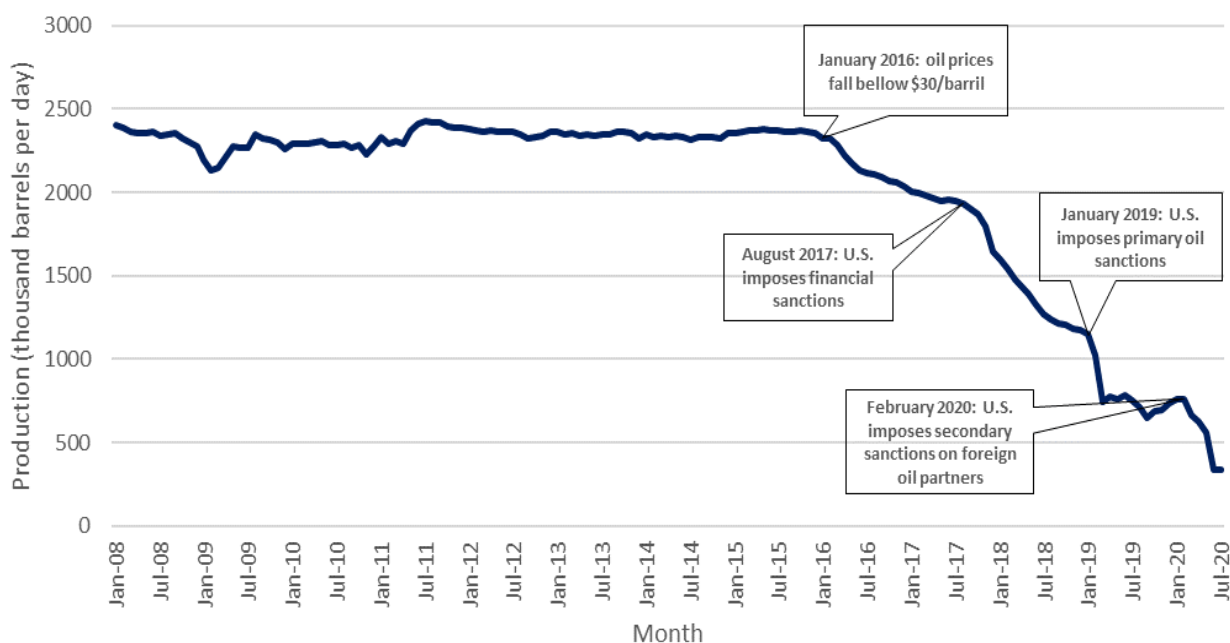
oil prices, Colombian oil output stabilized after oil prices began recovering in 2017, while Venezuelan oil output continued declining.

Figure 1 shows these inflection points on the updated data. The series shows remarkable stability up until 2016. Production begins falling in 2016, and the rate of decline accelerates markedly after the first financial sanctions in August of 2017. Output then suffers additional drops following the adoption of oil sanctions in January of 2019 and secondary sanctions on foreign oil partners in February of 2020. Venezuela’s current production level, at around 500tbd, is around one-fifth of its pre-2015 production.

Weisbrot and Sachs (2019) used this evidence to contend that both financial and oil sanctions had led to significant declines in oil revenues and thus caused the import contraction that led to major deteriorations in socio-economic indicators. They argue that it is “virtually certain that the US economic sanctions made a substantial contribution” to the increase in mortality observed in 2018, associated with an additional 40 thousand deaths.

Other authors have offered alternative interpretations of the oil output decline. 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. They contend that Colombia is not a good control group because the Venezuela and Colombia series are uncorrelated in longer-run data going back to 1999. Morales (2019) proposes the alternative of militarization of the oil industry 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.

**Figure 1: Venezuela’s Oil Production, 2008-2020**



Source: Own calculations, OPEC.

Rodríguez (2019) uses a data set of monthly oil production from 37 oil exporting countries covering 95% of the world's oil production to tackle some of these issues. He shows that the 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. He proposes a synthetic control estimator to proxy the counterfactual Venezuela scenario. The synthetic control estimate attributes a decline in production of 797tbd to financial sanctions. He also presents differences-in-differences estimates using the cross-country panel according in which oil sanctions are associated with a decline of between 41 to 44% in oil production.

Oliveros (2020) surveys the qualitative evidence of the impact of sanctions of the economy. He finds significant evidence of overcompliance and inability to use the humanitarian exceptions approved by the U.S. government to its sanctions regime. He cites several examples of humanitarian agencies that have had payments for medical supplies blocked by financial institutions alleging sanctions-related restrictions. He also quotes business leaders claiming that the harm caused by sanctions to their productive capacity is similar to that of the 2007-08 wave of expropriations. He presents several counterfactual exercises based on extrapolations of prior trends. Even in the most conservative of these scenarios, he estimates that sanctions can be associated with a cumulative decline in oil production of 502 thousand barrels per day. Equipo Anova (2021) uses a regression discontinuity design approach to estimate the break in trend in oil output at the time of sanctions and finds that they are associated with a decline of 698 tbd in oil production, or 33.1% of pre-sanctions oil output.

### 3.3 The financial channel

Some authors have questioned whether financial sanctions could have had any additional effect on the country's oil industry, given that the government had already lost access to international capital markets on the eve of their adoption (Hausmann and Muci, 2019, Bahar et al., 2019.) Their contention is that the sanctions were essentially non-binding and therefore any acceleration of the rate of decline of oil output after their adoption likely reflects the effect of other changes in policy or the broader environment that took place at the same time, such as the increase in military presence in the firm's top management (Morales, 2019).<sup>18</sup>

Rodríguez (2019) contends that there are several reasons why financial sanctions could have led to a worser trajectory of the country's oil industry than would have been observed in a scenario without sanctions. One is that they had the effect of impeding the debt restructuring that would have ultimately been carried out if PDVSA had been unable to recover solvency. In other words, even if one could argue that a PDVSA default was inevitable, it is improbable that it would have been nearly as traumatic as the one that actually occurred.<sup>19</sup> Another reason is that the fact that PDVSA and the

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<sup>18</sup> The issue is complicated by the fact that capacity to pay depends on oil revenues. Declining oil production can easily make a debt unsustainable that would not be so under other conditions. Venezuela's external debt to GDP ratio stood at 37% in 2012, a ratio at which sustainability concerns are typically absent. By 2019, it had risen to 284 percent. Virtually all of this increase is caused by the collapse of the country's GDP valued in foreign currency.

<sup>19</sup> The Venezuelan government announced in November 2017 the creation of a commission to restructure Venezuela's debt, but that commission produced no results, largely because there was no legal way in which U.S. investors could negotiate with it. Although there is no legal impediment for institutions in other countries to participate in such a restructuring, non-U.S. creditor groups have shied away from any action that would impose restrictions on their capacity to do business in the U.S. and that would leave them with bonds that would not be tradable in U.S. markets. Furthermore, any changes to existing bonds would have to be approved by the Guaidó administration to be valid under U.S. laws.

government had lost - or could have been expected to lose - access to unsecured financing in international bond markets does not mean that the whole Venezuelan oil industry had lost access to all relevant credit.

There were at least two lending channels that were open to the oil industry on the eve of sanctions: loans to joint ventures between PDVSA and multinational companies, and direct financing from suppliers. Financing agreements through which foreign partners would lend to finance investment in a joint venture (JV) agreement as long as they could pay the loan from the JV's production, also known as Special Financing Vehicle (SFV) deals, became one of the most effective mechanisms for PDVSA to raise production at the time. Likewise, before sanctions were imposed, PDVSA had begun to refinance a significant part of its arrears with service providers through the issuance of New York law promissory notes. The August 2017 Executive Order put an end to both types of arrangements. These are the financial arrangements that our paper focuses on.

Other modalities also allowed the country's oil industry to maintain some financial market access at the time of sanctions and which thus imply that the sanctions were not completely redundant. 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. U.S. financial sanctions had two effects on these arrangements: they stopped the company from 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.

This type of financial arrangements is not unusual among distressed debtors. Both are examples of the way in which credit is often interconnected with the production process. External finance is costly in the presence of informational asymmetries because creditors cannot easily verify whether a default occurs as a result of a project's inherent risk or the debtor's decision not to pay.<sup>20</sup> One way in which borrowers can resolve these informational asymmetries is by entering into joint-ventures which not only give external investors an equity stake but also concede some level of operational control and access to information.<sup>21</sup> This can be done either by jointly operating production processes or by outsourcing some production processes to external suppliers. Suppliers can extend trade credit at rates that would not be agreed to by financial intermediaries because of their informational advantage and capacity to control assets that are required for production.<sup>22</sup> An extreme way to resolve the informational asymmetry is by fully collateralizing loans, yet this is often difficult if not impossible to do for sovereign or quasi-sovereign debtors.

Sanctions brought these mechanisms to a halt. Joint venture partners were precluded by the 2017 sanctions from entering into financing agreements with entities in which PDVSA had a majority stake – a condition that applied to all joint ventures since PDVSA control was a condition of Venezuelan law. And while an exception was carved out for short-term debt of less than 90 days,<sup>23</sup> that exception

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<sup>20</sup> Smith, J. (1987)

<sup>21</sup> Balakrishnan, S., & Koza, M. (1993).

<sup>22</sup> Bernanke, B. and Gertler, M. (1989)

<sup>23</sup> Federal Register (2017)

was insufficient to cover part of PDVSA trade-credit, and completely ruled out the conversion of trade credit arrears into financial debt that was being carried out by PDVSA at the time. It also became moot after the January 2019 designation of PDVSA which impeded any type of dealings with the firm.

#### 4. Data and Stylized Facts

This paper uses a firm-level panel data set of monthly production levels in Venezuela's Orinoco Belt region. The Orinoco Belt is home to 262 billion barrels of oil reserves, the bulk of the country's proven reserves of 304 billion.<sup>24</sup> The Belt is located around the Orinoco River Basin which divides the country's southern tropical forest areas from its northern, more urbanized regions. Its main deposits of crude petroleum are located in three key eastern states (Anzoátegui, Guárico and Monagas) and consist largely of heavy crude with a higher production cost than Venezuela's western fields.

The Orinoco Belt generates around half of the country's production and more than two-thirds of its production by joint ventures. This is largely a result of the opening of Orinoco Belt Investment to foreign investment in the late 90s.<sup>25</sup> During that period of low oil prices, authorities deemed the investment cost of developing these fields as too high for then cash-strapped PDVSA. The contracts initially assigned in the 90s were renegotiated by the Chávez administration in 2006. At that moment, PDVSA negotiated their conversion from operating contracts with private sector firms into joint ventures in which PDVSA held a majority stake.

Over time, the Orinoco Belt became one of the main sources of production growth in the Venezuelan oil industry. This was due in part to the greater concentration of joint venture arrangements with national and multinational companies in the area, which allowed FDI inflows into an area of significant potential during a period of high oil prices. As Figure 2 shows, Orinoco Belt production showed moderate production growth during the 2009-2015 period, which partially offset a trend of declining production in other areas of the country.

Venezuela's relatively stable oil production up until 2015 thus combines two distinct trends: a gradual growth of Orinoco Belt production, which increased by 24.0% between 2009 and 2015, and a steady reduction of production in the rest of the country, where it fell by 25.9% in the same period (Figure 8.8). Starting in 2016 - and accelerating after 2017 - production in both areas falls by comparable rates. Therefore, the share of the Orinoco Belt in total domestic production, which had grown steadily prior to 2015, remained stable at 48% in the 2015-2020 period.

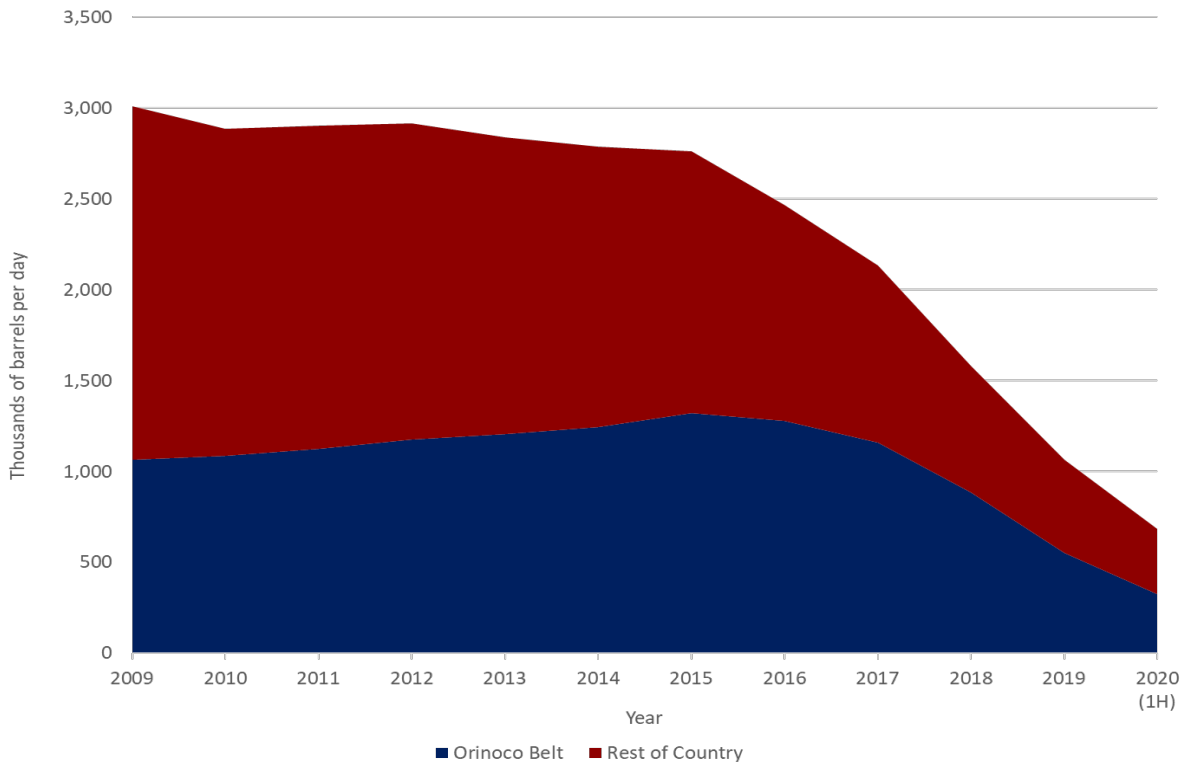
In other words, while there were factors leading to the decline of oil production before 2015 in the nation's western fields, the Orinoco basin area seemed impervious to them. Even as production turned the corner in 2016 with the collapse of oil prices, Orinoco Belt production remained relatively resilient: in the first seven months of 2017 (the period prior to the adoption of the first financial sanctions), Orinoco Belt production was only 7 percent lower than its average 2015 levels. Understanding the subsequent turnaround in Orinoco Belt production is therefore essential to figuring out why Venezuelan oil production collapsed from 2017 on.

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<sup>24</sup> Ministerio del Poder Popular de Petróleo (2019)

<sup>25</sup> Rodríguez, J. (2005)

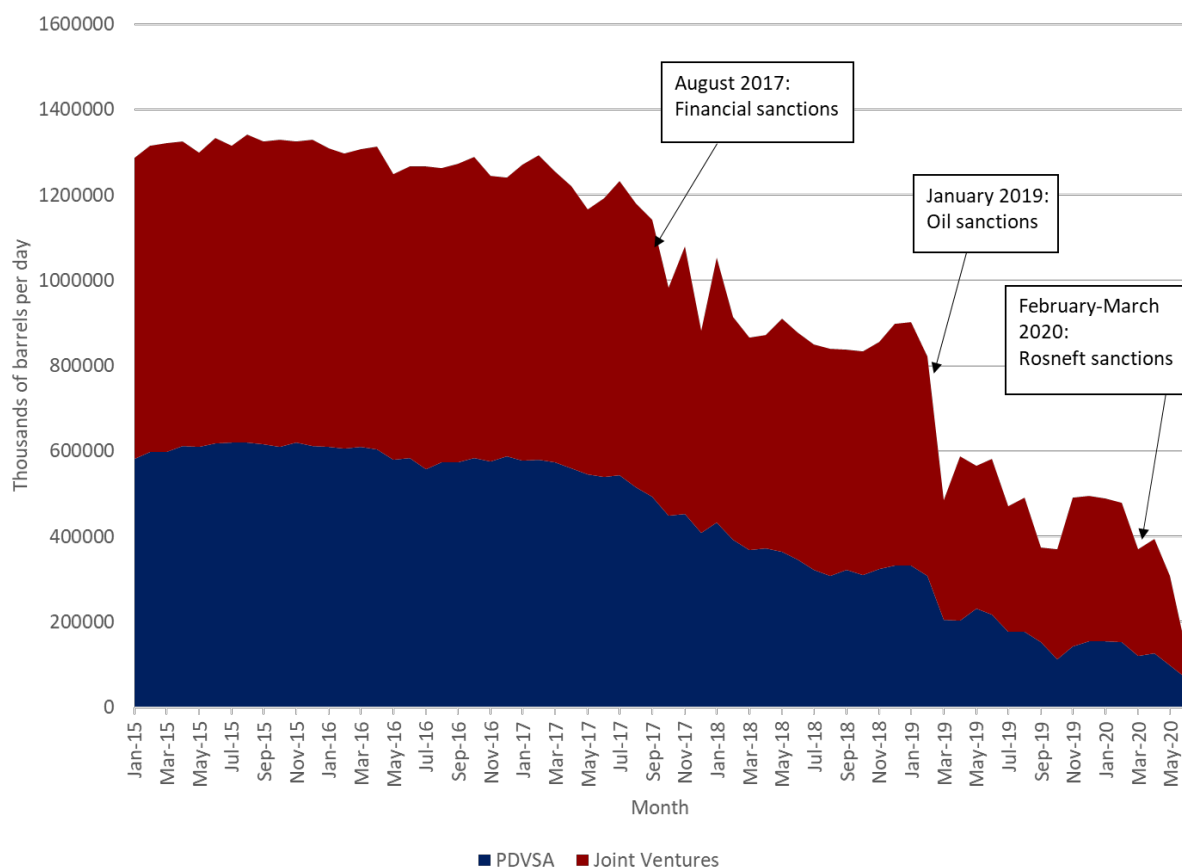
**Figure 2: PDVSA Production by Region, 2009-2020**



**Source: Own calculations, PDVSA.**

Figure 3 looks at our more detailed monthly data for the Orinoco Belt for the most recent five-year period that includes the adoption of the different variants of U.S. sanctions policy. The data shows a continued decline in production both in fields operated completely by PDVSA as well as those controlled by joint ventures with private sector partners. Between the month of adoption of the first U.S. financial sanctions on August 2017 and the end of our sample on June of 2020, joint venture production declined by 90.1%, a slightly higher rate than that by which production fell in fields that were completely owned by PDVSA (86.9%).

**Figure 3: Orinoco Belt Production, 2015-2020**



Source: PDVSA

The data also seems to show steeper declines in JV production in the periods immediately following sanctions adoption, even if in some episodes this production is then able to bounce back in subsequent months. For example, in the four-month period following the adoption of each of the different types of sanctions – financial, oil and secondary – JV production shrank respectively by 29.0 %, 41.3% and 79.9%, higher than the contractions observed by wholly-owned PDVSA subsidiaries of respectively 20.5%, 30.4% and 55.6%.

Our discussion in section 2 underscored the potential sensitivity of joint venture financing to the 2017 financial sanctions. Between 2013 and 2016, 12 Special Financing Vehicle agreements were concluded between PDVSA and nine different foreign firms for a total of USD 11.1bn. Seven of these agreements were located in the Orinoco Belt and were aimed at financing investment in production blocs that accounted for 46% of the area’s production.

As we have noted, unsecured lending by PDVSA was clearly too expensive – if at all available - on the eve of the sanctions to make it economically viable for funding the oil sector’s investment projects. This was not the case with the Orinoco Basin JV agreements, for most of which interest rates oscillated between Libor + 4.5% and Libor +6.9%. The fact that these deals were signed as late as December of 2016 suggest that the industry retained access to capital markets under this modality of financing,

even if not under other modalities. When financial sanctions arrived in 2017, they effectively barred a modality of financing which was available and economically viable.

Our data set contains monthly production for each of these fields, but no other time-varying information. Nevertheless, we do have data on the nationality and stake of foreign partners, which allows us to evaluate how the sensitivity of firms to financial and oil sanctions varies according to some characteristics of the JV partners.

However, these links are complex. Even though U.S. economic sanctions in principle only restrict transactions of U.S. firms, large multinational oil firms of other nationalities commonly have significant economic interaction with the United States, making it just as costly for them to run afoul of sanctions restrictions. The fact that the U.S. government may be more willing to accommodate policy in response to lobbying by American firms through the issuance of general or specific licenses complicates interpretation of country effects and interactions.

As a case in point, consider the reaction of China's National Petroleum Company CNPC to the U.S.'s oil sanctions in 2019. Although CNPC continued trading with Venezuela after the January 2019 oil trading ban, it decided to suspend all loadings of Venezuelan oil on August of 2019, immediately after the U.S. imposed additional sanctions barring transactions with the Venezuelan government and directly threatened foreign partners with secondary sanctions.<sup>26</sup> As of the date of writing, CNPC has not resumed direct purchases. There is some evidence that in late 2019 it may have shifted to indirect purchases through Russia's Rosneft and ship-to-ship transfers designed to be less detectable by U.S. authorities, yet the amounts purchased through this mechanism were small relative to purchases before the suspension.<sup>27</sup> Rosneft, in turn, suspended trading with Venezuela and divested from its Venezuela investments after it was hit with secondary sanctions in the first quarter of 2020, thereby also effectively putting an end to indirect CNPC purchases.

On the other hand, the only U.S. company in Venezuela's oil sector, Chevron, was granted licenses together with other U.S. companies to continue operating in Venezuela from the outset of the oil sanctions.<sup>28</sup> These licenses, which were periodically renewed for periods of 3-6 months, allowed Chevron to maintain operations even as some European firms were subject to the threat of secondary sanctions if they increased output. Although U.S. authorities modified the general license applying to Chevron and other U.S. companies on April 2020 to ban all drilling, lifting, processing, purchase or sale of Venezuelan oil or oil products as of July 2019, Chevron's Petropiar JV in Venezuela remains operative, producing 115 thousand barrels per day as of October 2020.<sup>29</sup>

That Chevron continues to produce oil despite an explicit prohibition on U.S. firms doing so implies that it must have a specific license permitting it to do so. Since specific licenses are not public, amending the general license may have served the public relations aim of generating the impression that restrictions were being tightened on the Maduro regime, possibly with the purpose of scoring political points with the Venezuelan diaspora ahead of the U.S.'s November 2020 presidential

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<sup>26</sup> Aizhu, C. and Parraga, M. (2019)

<sup>27</sup> See Cohen, L. and Parraga, M. (2020). CNPC had purchased 350 thousand barrels per day in the first six months of 2019 from Venezuela, while indirect purchases identified by Reuters amounted to 109 thousand barrels per day during the last six months of 2019.

<sup>28</sup> See General Licenses 8-8F in Venezuela-Related Sanctions. U.S. Department of Treasury.

<sup>29</sup> OFAC (2020)

elections, while the granting of a non-public specific license could have been intended to allow Chevron to continue its operations unhindered out of the public eye.

These examples suggest that while it may be reasonable to expect differences in the responsiveness of JV output to sanctions conditional on the nationality of the partner, it may be difficult to *ex ante* predict the sign of those effects, given that they essentially reflect both the ability of firms of different nationalities to accommodate to the new restrictions and the willingness or ability of policymakers to carve out exceptional treatment for those firms. In any case, the anecdotal evidence suggests that the U.S.'s Chevron may have been more insulated from sanctions than other firms as a result of American authorities' actions to protect it.

## 5. Econometric Analysis

### 5.1 The data

Our data corresponds to observations of monthly production in 33 production blocs in the Orinoco Belt spanning the twelve-and-a-half-year period from January 2008 to June 2020. A bloc is a geographic subdivision formed by one or more fields. The subdivisions were created to allocate areas of the region to oil-producing firms, which may or may not be joint ventures. The sample contains 10 blocs that are wholly operated by PDVSA and 23 that are operated by JVs, in all of which PDVSA has a majority stake.<sup>30</sup> Of these, 10 have more than one partner and 4 have three partners. The most frequent nationality of the largest minority partner is Venezuelan and Chinese, each with 4 firms. The panel is unbalanced as some of the blocs begin production after the start of our sample.

In our sample, six firms from six different countries were involved in Special Financing Vehicle (SFV) deals before the first sanctions.<sup>31</sup> All but one of the firms was involved in one deal; the exception was India's ONGC, which was involved in two. At the same time, 16 joint ventures in the Basin were not involved in SFV deals, nor were any of the 10 blocs operated completely by PDVSA. This implies that there is considerable variation in SFV exposure both in the sample as a whole as well as within nationalities of foreign partners. To take one example, there are two JVs in the area with a U.S. partner (both of them with Chevron). One of them, Petropiar, functions under an SFV deal, while the other one, Petroindependencia, does not.

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<sup>30</sup> The requirement of a majority PDVSA stake was imposed explicitly in the 2001 Hydrocarbons Law (2001); however, most scholars consider that it is implicit in article 12 of the Constitution.

<sup>31</sup> A seventh firm concluded a financing deal in 2018 with China's CNPC for the Petrozumano bloc. Since this deal is after the imposition of the sanctions, we maintain this firm as a non-SFV firm for our baseline specification. Including it in the treatment group does not significantly alter our results. Inclusion in the treatment group could be justified with the argument that what SFV intends to measure is access to SFV financing, for which having obtained such financing, regardless of when it happened, is a good proxy.



**Table 1: Orinoco Belt blocs by nationality of main JV partner and Special Financing Vehicle (SFV) arrangement**

Main partner	Total		SFV		Non-SFV	
	Firms	Output	Firms	Output	Firms	Output
100% PDVSA	10	559,709	0	-	10	559,709
Belarus	1	4,657	0	-	1	4,657
Brazil	3	5,362	0	-	3	5,362
China	4	157,254	1	143,351	3	13,903
Cuba	2	5,639	0	-	2	5,639
France	1	103,096	1	103,096	0	-
India	1	17,797	1	17,797	0	-
Italy	1	496	0	-	1	496
Russia	3	132,135	1	118,763	2	13,373
Spain	1	24,542	0	-	1	24,542
US	2	191,938	1	154,778	1	37,159
Venezuela (private)	4	30,102	1	28,888	3	1,215
<b>Total</b>	<b>33</b>	<b>1,232,727</b>	<b>6</b>	<b>566,672</b>	<b>27</b>	<b>666,054</b>

Source: PDVSA, own calculations.

Table 2 describes the financial details of the eight financing deals signed by joint ventures in the Orinoco Basin region.<sup>32</sup> Note that these deals were concluded as late as December of 2016, eight months before the adoption of financial sanctions and at a time at which PDVSA was only able to issue bonds that were backed by collateral in international financial markets. Deals include short-term revolver loans, prepaid export deals and loans with terms as long as 13 years. With one exception, interest rates are at or below Libor + 5.8% (approximately 6.5% at the time) and are well below levels typically associated with high expectations of default.

<sup>32</sup> This table includes Petrozumano (see previous footnote). The reason why there are more deals than firms is that there are two different financing deals for Petrolera Indovenezolana.

**Table 2: Financial Conditions for Special Financing Vehicles in the Orinoco Basin**

Name	Main partner	Main partner nationality	Other partner	Facility Type	Amount (USD mn)	Date	Maturity	Term	Rate
Petropiar	Chevron	United States		Revolver	10	Jan-15	2017	1	Libor+4.5%
Petrocedeño	Total	France	Statoil	Revolver	60	Oct-14	2017	1	1.32%
Petrolera Sinovensa	CNPC	China		Loan	4,015	Jun-13	2023	10	Libor+5.8%
Petrodelta	HNR/CT Energía	Venezuela		Revolver	6	Dec-15	2017	1	12%
Petrolera Indovenezolan	ONGC	India		Revolver	60	Nov-16	2017	1	libor + 5.5%
Petrolera Indovenezolan	ONGC	India		Loan	318	Nov-16	2019	3	libor + 5.5%
Petromonagas	Rosneft	Russia		Prepaid expor	1,985	Dec-16	2021	13	Libor+5%
Petrozumano	CNPC	China		Loan	184	Sep-18	N/A	N/A	N/A

Source: PDVSA financial statements, own calculations and estimates.

## 5.2 Baseline specification

We use a standard differences-in-differences specification with time and firm dummies, a treatment group trend and time-specific treatment effects. We treat the six firms with SFV agreements as of the time of the announcement of financial sanctions on August of 2017 listed in Tables 1 and 2 as our treatment group and the remaining twenty-six firms<sup>33</sup> that did not have SFV deals at that time as our control group. Therefore, our baseline specification is:

$$p_{it} = \eta_i + \lambda_t + \delta(SFV_i \cdot t) + \sum_{k=T_0}^T \beta_k SFV_i S_k + \varepsilon_{it}, \quad (1)$$

where  $p_{it}$  is an indicator of production in firm  $i$  at time  $t$ ,  $\eta_i$  denotes a firm-specific fixed effect,  $\lambda_t$  a month-specific fixed effect,  $t$  a time trend,  $S_k$  an indicator variable that takes the value 1 on month  $k$  and 0 on all other months,  $SFV_i$  an indicator variable that takes the value 1 for firms for which there is a special financing vehicle arrangement during our sample and 0 for those for which there is not,  $T_0$  is the first month of sanctions (August 2017) and  $\varepsilon_{it}$  an iid error term.

The inclusion of a treatment group trend term in the specification controls for pre-intervention differences in the behavior of the treatment relative to the control group. If these differences are important, then  $\beta = \{\beta_{T_0} \dots \beta_T\}$  should be seen as capturing deviations from the hypothesis of *parallel growth* (i.e., that the difference between the growth rates of the treatment and control group would have persisted in the absence of treatment); if they are not, controlling for them still reduces bias and maintains reasonable power to detect a treatment effect (Bilinski and Hatfield, 2019). Time-specific treatment effects are necessary so that the treatment group trend does not pick up variation over time in the treatment effect (Wolfers, 2006).

Inclusion of a treatment group trend term, while recommendable even in the presence of parallel trends, is particularly important given the visual evidence of important deviations from the parallel trends hypothesis. As Figure 4 shows, production per firm in SFV and non-SFV firms exhibits different patterns of variations in the pre-sanctions period.<sup>34</sup> In fact, there is a weak negative

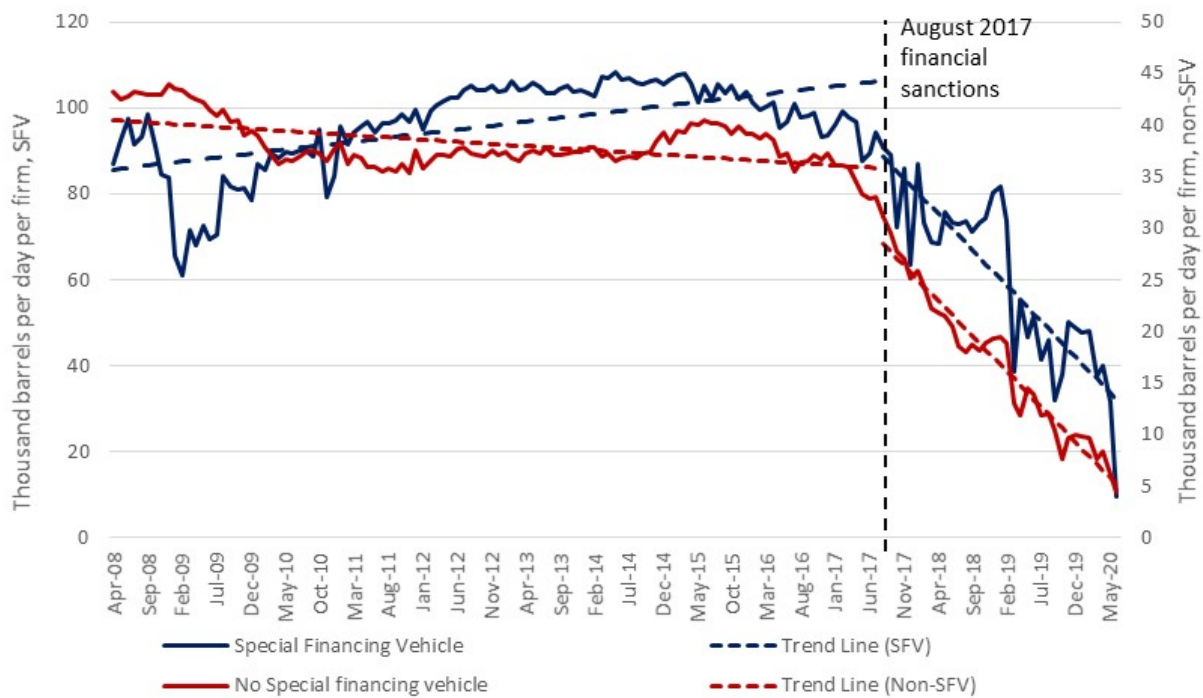
<sup>33</sup> We refer indistinctly to firms and blocs in our discussion for ease of exposition. While each firm operated by a JV is operated by a distinct firm (even if the foreign partner is the same), the 10 blocs wholly operated by PDVSA are operated by a single firm, a PDVSA subsidiary known as the Corporación Venezolana de Petróleo (CVP)

<sup>34</sup> Given that some firms begin production after the start of our sample, Figure 4 restricts to comparing the 15 non-SFV firms that were producing as of April of 2008, the first month on which the 6 SFV firms in our sample were also producing. Taking averages over all firms leads to similar results, yet biases towards even lower pre-sanctions growth of non-SFV firms, as entrants tend to have lower production levels than previously established producers.

correlation between the series in the period prior to the adoption of sanctions on August 2017 ( $\rho = -.33$ ). Remarkably, however, there is a very strong correlation in the period beginning in August 2017 ( $\rho = .89$ ). This suggests that these groups may have become more similar, rather than different, as a result of the intervention. We return to this issue and its implications for model specification in section 5.3 below.

Another relevant issue regards the choice of production indicator. Many firm-level studies use the logarithm of production as a dependent variable both because they involve the estimation of multiplicative production function specifications (which is not our case) and as a scaling device to ensure comparability of firms of different sizes. However, the use of logarithms leads us to lose the information contained in observations in which the firm's output falls to zero. These observations not only account for an important 8.5% of our sample, but also of a much larger share of observations (19.1%) in the post-sanctions period. Deleting these observations could lead us to inadvertently omit data that is particularly informative about the effect of sanctions on production. Therefore, alongside the logarithmic specification for  $p_{it}$ , we also provide estimates for several alternative specifications of the dependent variable: the absolute level of production (in barrels per day), production standardized using the firm-level sample mean and standard deviation, and the logarithm of production with the minimum imputed to be equal to one-half of the average sample minimum as a proportion of the firm's sample range of variation of production.

**Figure 4: Output per Firm by Financing Access, 2008-2020**



Source: Own calculations, PDVSA.

Table 3 summarizes our results. The table reports the average treatment effects ( $ATE = \sum_{k=T_0}^T \beta_k$ ) as well as the trend coefficient and associated standard errors for the four specifications of the dependent variable. The row labeled “SFV\*Financial Sanctions” captures the ATE for the period starting with the adoption of financial sanctions (from August 2017 to the end of the sample), while the “SFV\*Financial and Oil Sanctions” shows the ATE for the period during which both financial and oil sanctions were in place (from January 2019 to the end of the sample). When the dependent variable is the level of production (column (1)), we find an ATE of 39.3 thousand barrels per day, which is strongly significant ( $p=.006$ ). Note that the average level of pre-sanctions production in our SFV firms is 94.9 thousand barrels per day (tbd), so that this effect entails a loss of 41.4% of initial production is attributable to sanctions. If we just calculate the ATE over the period of oil sanctions (from January 2019 until the end of our sample on June 2020), we get a somewhat stronger effect of 51.4 thousand barrels per day, or 54.2% of pre-sanctions production ( $p=.003$ ).

We obtain similar results with alternative specifications of the dependent variable, although the degree of statistical significance varies. The simple logarithmic specification delivers an ATE of 52.0 log points, equivalent to a decline of 40.5% from the initial baseline, yet is only borderline significant ( $p=.097$ ). Recall that this specification leads to the loss of 8.5% of the sample, so some loss of statistical confidence is to be expected. The coefficient is very similar (52.7 log points) when we impute the minimum values; statistical significance is slightly stronger ( $p=.061$ ). When we use standardized levels as the dependent variable, the effect is slightly larger as a share of initial output: sanctions are associated with a 1.7 standard deviation drop in production, which evaluated at the SFV averages yields a 48.5% output drop, and statistical significance is much stronger ( $p=.000$ ).<sup>35</sup> In all four specifications, the ATE for the oil sanctions period is larger than for the whole financial sanctions period.

**Table 3: Panel Regression Results, Baseline Specification**

Dependent variable	Level of Production	Log of Production (zeros dropped)	Log of Production (imputed minimum)	Standardized
SFV*Trend	243.9 (199.6)	0.013*** (0.003)	0.014*** (0.003)	0.022*** (0.007)
<b>Average Treatment Effects</b>				
SFV*Financial Sanctions	-39312*** (13215)	-0.52* (0.304)	-0.527* (0.272)	-1.736*** (0.395)
SFV*Financial and Oil Sanctions	-51413*** (16177)	-0.739 (0.443)	-0.719* (0.393)	-2.106*** (0.453)
N	4188	3832	4188	4188
Adjusted R-Squared	0.2528	0.3873	0.4623	0.4801

Standard errors in parentheses. Asterisks denote statistical significance: \*10%, \*\*5%, \*\*\*1%. All specifications include month dummies, firm fixed effects, and post-treatment month\*SFV interactions. Average Treatment Effect is the average of estimates of  $\beta_k$  in equation (2)

Our prior discussion has suggested the possibility that the effect of financial and oil sanctions may depend on the JV partner firm’s nationality. To assess that possibility, table 4 reproduces our analysis adding post-sanctions effects for four categories of firms according to the nationality of their largest minority partner: China, Russia, the United States and other countries. In this specification, the

<sup>35</sup> Alternative specifications of the scaled variable, such as those using production as a percentage of average pre-sanctions or of August 2017 production, deliver similarly strong results.

omitted category are firms operated completely by PDVSA, who have no minority partners. In order to maintain consistency with our specification (1), we also introduce pre-treatment trends for each of the country groups. That is, we estimate:

$$p_{it} = \eta_i + \lambda_t + \delta(SFV_i \cdot t) + \sum_{j=1}^C \gamma_j(C_{ji} \cdot t) + \sum_{k=T_0}^T \beta_k SFV_i S_k + \sum_{j=1}^C \sum_{k=T_0}^T \gamma_{kj}(C_{ji} \cdot S_k) + \varepsilon_{it}, \quad (2)$$

where  $C_{ji}$  is an indicator variable that takes the value 1 if firm  $i$  belongs to country group  $j$ .

Our treatment effects estimates are robust to the inclusion of country group effects and trends. In fact, the magnitude of the estimated sanctions effects increases across the board, with statistical significance also strengthening. Notably, the sanctions coefficient for the two logarithmic specifications is significant at conventional levels ( $p=.017$  and  $.016$ , respectively), as opposed to the borderline significance in Table 3. The stronger effects also imply that a larger share of the decline in SFV production can be attributed to sanctions. For example, in specification (1) of table 4 the estimated sanctions effect is equivalent to 50.4% of pre-sanctions production, as opposed to 41.4% in the same specification of Table 3.<sup>36</sup>

**Table 4: Panel Regression Results, Country Group Effects and Trends.**

Dependent variable	Level	Logarithm (zeros dropped)	Logarithm (imputed minimums)	Standardized
SFV*Trend	155.7 (221.8)	0.014*** (0.005)	0.015*** (0.005)	0.022** (0.008)
<b>Average Treatment Effects</b>				
SFV*Financial Sanctions	-47788*** (11249)	-0.771** (0.306)	-0.737** (0.290)	-2.051*** (0.454)
SFV*Financial and Oil Sanctions	-61802*** (13662)	-1.053** (0.439)	-0.968** (0.411)	-2.396*** (0.536)
China*Financial Sanctions	12721 (20229)	0.203 (0.406)	-0.051 (0.292)	0.456 (0.513)
Russia*Financial Sanctions	19955 (18178)	0.624 (0.419)	0.427 (0.410)	0.849 (0.626)
United States*Financial Sanctions	32600* (18099)	1.163*** (0.256)	1.209*** (0.304)	1.17** (0.553)
Other*Financial Sanctions	33359** (38073)	-0.047 (1.329)	0.231 (1.470)	0.834 (1.076)
<b>N</b>	<b>4188</b>	<b>3832</b>	<b>4188</b>	<b>4188</b>
<b>Adjusted R-Squared</b>	<b>0.3814</b>	<b>0.4588</b>	<b>0.5193</b>	<b>0.5143</b>
Standard errors in parentheses. Asterisks denote statistical significance: *10%, **5%, ***1%. All specifications include month dummies, firm fixed effects, post-treatment month*SFV and post-treatment month*country group interactions. Average Treatment Effect is the average of estimates of $\beta_k$ and $\gamma_{kj}$ in equation (2)				

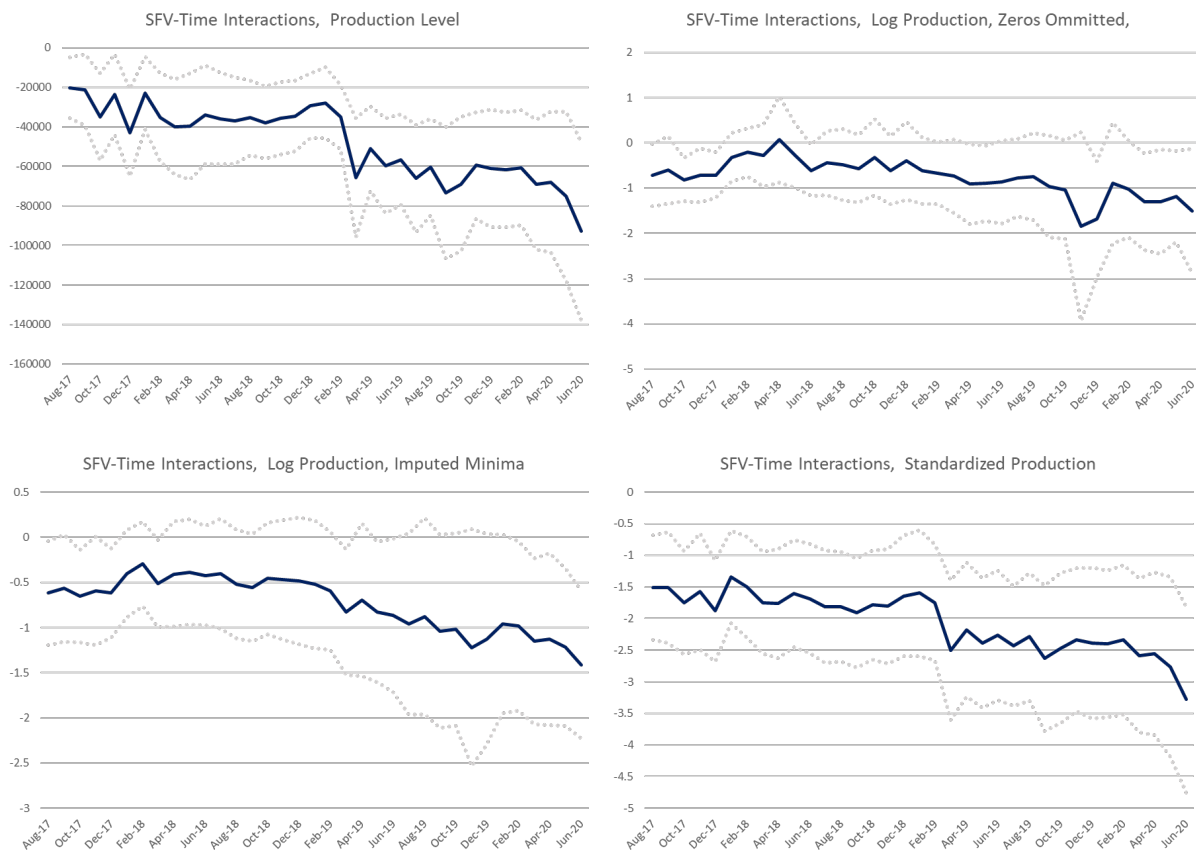
The ATE for the oil sanctions period is here again stronger than for the whole financial sanctions period. Greater detail on the variation of treatment effects over time is shown in Figure 5, which plots the individual  $\beta_k$  estimates and associated 95% confidence intervals over time. All four estimates

<sup>36</sup> One may be tempted to use the sum of total derivatives with respect to the  $S_k$  terms across nationalities for the average SFV firm rather than the sum of their partial derivatives as an indicator of the effect of sanctions. This would, in our view, be incorrect. Because being an SFV firm is an indicator of pre-sanctions JV access to financial markets, only the interaction effect between sanctions and SFVs captures the effect on firms of closing off access to financial markets. The fact that firms of some nationalities may have outperformed others in the post-sanctions period captures some elements of differential resilience yet cannot be adequately conceived of as a treatment effect.

show increasingly large effects over time. While the levels specification does show an important discrete drop just after the oil sanctions, the pattern of variation over time is more continuous in the other specifications. It is, of course plausible to expect that the production effects of losing access to international markets increase over time. Of course, the incremental effects could also be due to the incremental tightening of sanctions.

The country effects estimated in Table 4 also provide some interesting evidence on the resilience of certain groups to sanctions. Although the patterns vary somewhat across specification, the one robust effect that emerges is that JVs with U.S partners outperformed others in the post-financial sanctions period. This is consistent with the hypothesis that U.S. firms were relatively protected by OFAC granting of general and specific licenses and may have thus managed to become more insulated from the effect of sanctions. Note that in both the levels and standardized specifications (but not the logarithmic) this protection offsets only part of the negative sanctions effect.

**Figure 5: SFV-Time Interactions for post-sanctions period, alternative specifications.**



### 5.3 Dealing with non-parallel trends

As noted above, our specification allows for a treatment group trend as well as country group trends.<sup>37</sup> This corresponds to a *parallel growth* specification which tests whether there is a change in the

<sup>37</sup> Control group and omitted country group trends are subsumed in the time effects by construction.

difference in growth rates between the control and treatment groups. Therefore, our results should not be interpreted as stating that SFV firms grow more slowly than non-SFV firms in the post-sanctions period (they do not) but rather that they grow more slowly than they would have grown had they maintained the same difference in growth rates with non-SFV firms that they had prior to sanctions.

The trend coefficient estimates in both Tables 3 and 4 are consistent with the pre-sanctions parallel growth thesis, as they indicate that SFV firms consistently saw higher pre-sanctions growth rates than non-SFV firms. This difference is statistically significant in the logarithmic and standardized specification, though not in the levels specification (where a parallel trends specification may thus be defensible). This begs the question of whether the linear difference in growth rates specification is correct, or whether higher-order non-linear trends may be present. Table 5 explores this possibility, controlling for a non-linear cubic spline trend in each specification. We find evidence that the non-linear term is significant in the levels and standardized specifications (though not in the logarithmic specifications). The ATE estimate in these two specifications – where the tests suggest that omitting the non-linearity could be important - continues to be statistically significant. While the ATE is smaller and not significant in the logarithmic specifications after controlling for the cubic spline terms, this may simply be due to the loss in power caused by increasing the trend’s complexity, and is less of a concern given the lack of significance of the non-linear trend terms in the logarithmic specifications.

**Table 5: Panel Regression Results, Trend Break Specifications**

Dependent variable	Level	Logarithm dropped	(zeros	Logarithm (imputed minimums)	Standardized
SFV*Linear Trend	555 (391.6)	0.032*** (0.012)		0.03** (0.011)	0.054*** (0.018)
SFV*Cubic Trend	-442.7** (210.0)	-0.019 (0.012)		-0.015 (0.011)	-0.034** (0.016)
<b>Average Treatment Effects</b>					
SFV*Financial Sanctions	-35399*** (11,518.5)	-0.341 (0.348)		-0.37 (0.316)	-1.182** (0.443)
SFV*Financial and Oil Sanctions	-47163*** (15,159.6)	-0.54 (0.554)		-0.53 (0.498)	-1.366** (0.588)
China*Financial Sanctions	21968 (21,428.6)	0.281 (0.549)		0.166 (0.245)	0.839 (0.560)
Russia*Financial Sanctions	36650* (19,929.2)	-0.066 (0.792)		0.19 (0.728)	0.702 (0.898)
United States*Financial Sanctions	32158 (20,675.5)	0.239 (0.747)		0.568 (0.619)	0.731 (0.656)
Other*Financial Sanctions	38846** (37,435.8)	-0.028 (0.217)		0.474 (0.696)	1.202*** (0.541)
<b>N</b>	<b>4188</b>	<b>3832</b>		<b>4188</b>	<b>4188</b>
<b>Adjusted R-Squared</b>	<b>0.4003</b>	<b>0.4709</b>		<b>0.5300</b>	<b>0.5376</b>

Standard errors in parentheses. Asterisks denote statistical significance: \*10%, \*\*5%, \*\*\*1%. All specifications include month dummies, firm fixed effects, post-treatment month\*SFV and post-treatment month\*country group interactions. Average Treatment Effect is the average of estimates of  $\beta_k$  and  $\gamma_{kj}$  in equation (2) with an additional non-linear cubic spline term.

Faced with the possibility of higher order trends than in the baseline, Bilinski and Hatfield (2019) suggest building confidence intervals around the change in ATEs when we move from the simpler to the more complex trend specification, an approach also known as non-inferiority testing. As they

point out, the issue is not so much whether parallel trends or parallel growth assumptions are violated, but whether their violation makes a material difference to the estimates. Small deviations from parallel trends/growth may not be enough to justify ditching the baseline specification if they are not large enough to change the result of interest.

Table 6 lays out the results of the non-inferiority tests. We find that we can rule out the hypothesis that the change in the ATE is higher than half of the ATE estimate for the levels specification, and higher than the totality of the ATE estimate for the levels and standardized specification. Confidence intervals are wider for the logarithmic specifications; however, misspecification is less of a concern in that specification given the lack of evidence in Table 5 of non-linear effects.

**Table 6: Non-Inferiority Tests, Linear vs. Cubic Spline Specifications.**

Dependent variable	Level	Logarithm (zeros dropped)	Logarithm (imputed minimums)	Standardized
Difference in coefficients		-12389	-0.430	-0.367
<b>Confidence Interval</b>				
Lower Bound		-22085	-0.924	-0.813
Upper Bound		-2693	0.064	0.080
Reject Ho: Diff>One-half of ATE	Yes	No	No	No
Reject Ho: Diff>ATE	Yes	No	No	Yes
See Bilinski and Hatfield (2019) for description of non-inferiority tests. Standard errors built using a cluster-adjusted sandwich estimator of the joint covariance matrix through the <code>suest</code> command in stata (see Weesie,2000)				

There is another, simpler approach to dealing with the non-parallel trends issue that comes from the observation that trends between the treatment and control groups appear to be very similar **after** the adoption of sanctions. This suggests that we may need to rethink our conceptualization of treatment and non-treatment periods. In our underlying argument, SFV firms were not similar to non-SFV firms previous to sanctions. Rather, they were different from non-SFV firms because they had access to credit (which non-SFV firms lacked). Sanctions ensured that neither of them had access to credit.

In other words, it may be more appropriate to think of access to finance as the treatment, and the imposition of sanctions as the end of the treatment. In that case, we would expect trends between both groups of firms to differ significantly during the period of access to credit, and to become similar – as they do - when access to credit is barred. This line of reasoning suggests an alternative approach to estimation, in which we treat the treatment period as the period preceding the imposition of sanctions. In other words, it would imply estimating the equation:

$$p_{it} = \eta_i + \lambda_t + \delta(SFV_i \cdot t) + \sum_{j=1}^C \gamma_j (C_{ji} \cdot t) + \sum_{k=1}^{T_0-1} \beta_k SFV_i S_k + \sum_{j=1}^C \sum_{k=1}^{T_0-1} \gamma_{kj} (C_{ji} \cdot S_k) + \varepsilon_{it}, \quad (3)$$

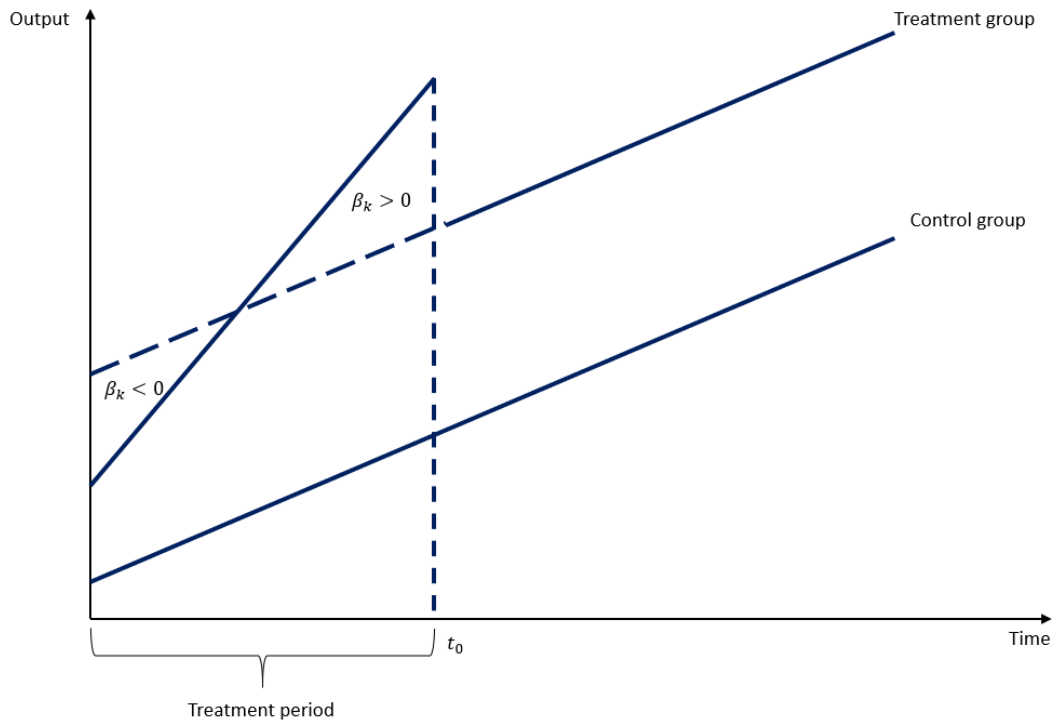
That is, we now regress our production indicator on country and treatment group-specific trends and on country and group-specific month dummies for all periods prior to the start of sanctions on  $T_0$ . Econometrically, the only distinction between equation (2) and (3) relates to the time periods corresponding to the group interactions.

Conceptually, however, the interpretation of the ATE may be quite different. When the treatment effect is positive and increases over time, it follows that  $\sum_k \beta_k > 0$  for any set of  $k$  after the start of



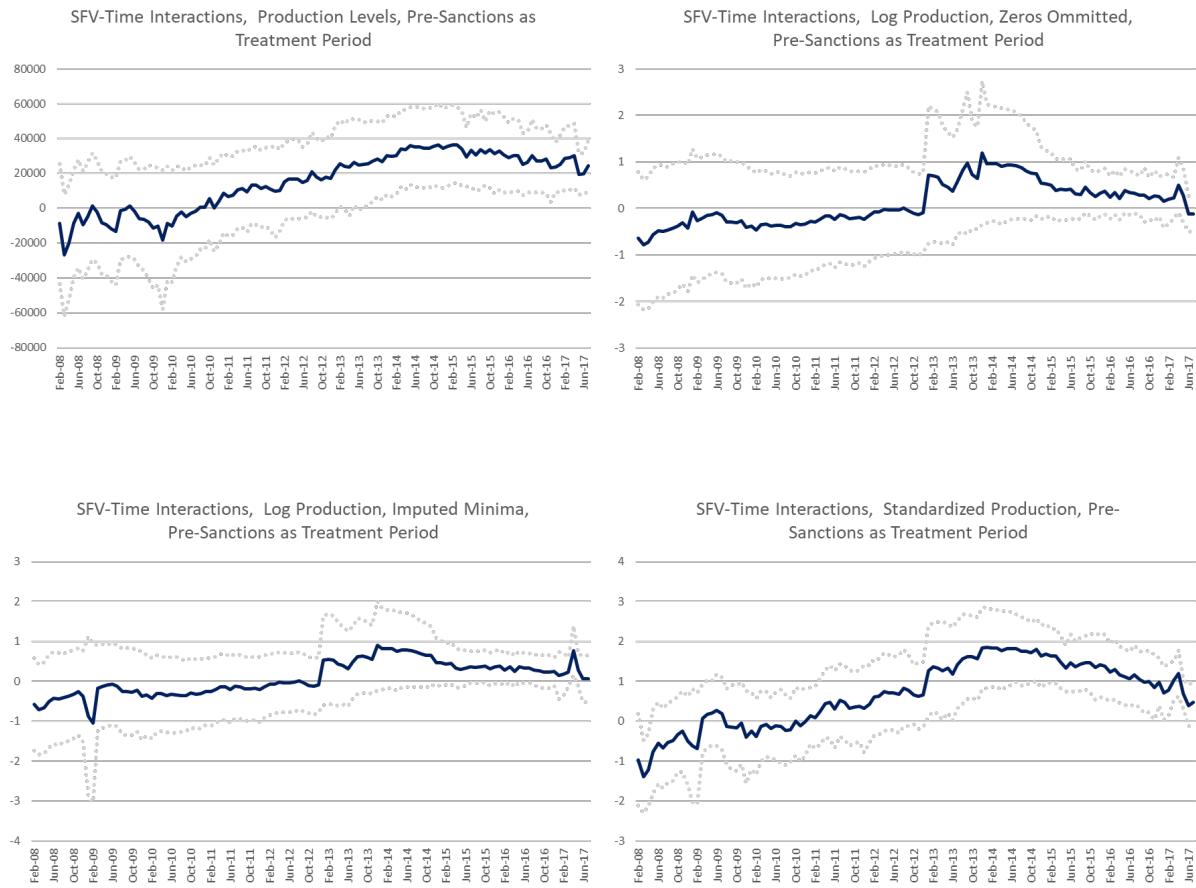
the intervention. This may not hold if the non-intervention period follows the intervention period. The logic is illustrated in Figure 6. If production were increasing more rapidly in the treatment group than in the control group until the end of the treatment, and the end of treatment brings about a discrete drop in production as well as a deceleration in growth, then it follows that there must have been a moment in time in which production must have been lower in the treatment group than in the counterfactual scenario in which that group had never received treatment.

**Figure 6: Interpretation of treatment group effects when treatment precedes non-treatment period.**



In other words, we can't precisely estimate a treatment effect if all we have is data on post-treatment outcomes. Instead, estimates of  $\beta_k$  should be interpreted as lower bounds on treatment effects. For this reason, we focus on estimating  $\beta_k$  in the vicinity of the intervention. Figure 6 shows graphically the full set of estimates for the four regressions while Table 6 displays statistical tests for the average effect estimated over the 24-month period preceding the imposition of sanctions.

**Figure 7: SFV-Time Interactions, Pre-Sanctions as Treatment Period, Alternative Specifications**



The estimates are consistent with the hypothesis that SFV firms had higher production levels previous to sanctions. For example, in the 24 months previous to sanctions, the typical SFV firm produced around 28.0 thousand barrels per day more than it did immediately after sanctions. The log specification is borderline insignificant when zeros are omitted ( $p=.144$ ) and borderline significant when they are imputed ( $p=.064$ ), while the levels and standardized specifications are strongly significant ( $p=.003$  and  $p=.001$ , respectively). All four specifications display effects that are increasing over time, consistent with the hypothesis that growth in  $p$ , and not just its level, was higher in SFV firms than in non-SFV firms.

Importantly, none of the post-intervention trends are statistically significant when we treat the pre-sanctions period as the treatment period. Point estimates for the trend terms are also very small,

between -0.1% and 2.5% of the estimated 24-month average of  $\beta_k$ .<sup>38</sup> The results thus support the hypothesis that sanctioned SFV firms exhibit similar behavior to non-SFV firms, making the pre-sanctions evolution of non-SFV firms an adequate counterfactual for what would have happened with SFV firms on the pre-sanctions period if their access to credit had been impeded

**Table 7: Pre-Sanctions and Post-Sanctions Representation Estimates**

Dependent variable	Level	Logarithm (zeros dropped)	Logarithm (imputed minimums)	Standardized
SFV*Trend	-277.4 (244.8)	0.007 (0.006)	0.007 (0.005)	0.005 (0.005)
<b>Average Treatment Effects</b>				
SFV*Pre-Financial Sanctions	27963*** (8832)	0.265 (0.177)	0.292* (0.152)	1.065*** (0.284)
<b>N</b>	<b>4188</b>	<b>3832</b>	<b>4188</b>	<b>4188</b>
<b>Adjusted R-Squared</b>	<b>0.3590</b>	<b>0.4217</b>	<b>0.5144</b>	<b>0.5326</b>
Standard errors in parentheses. Asterisks denote statistical significance: *10%, **5%, ***1%. All specifications include month dummies, firm fixed effects, pre-treatment month*SFV and pre-treatment month*country group interactions. Average Treatment Effect is the average of estimates of $\beta_k$ and $\gamma_{kj}$ in equation (3) over the 24-month period preceding sanctions.				

We now turn to discussion of the magnitude of the estimated effects. As we have already pointed out, the estimates presented in Tables 3-6 imply economically large and statistically significant effects of the closure of access to financial markets enacted through the 2017 financial sanctions on SFV firms. We now turn to the question of whether these effects can help us account for the implosion of the Venezuelan oil sector.

We start by noting that on the eve of sanctions, SFV firms accounted for 569 tbd, or 46.2% of oil production in the Orinoco Basin. Therefore, even if our estimates were able to account for the totality of the decline in SFV production, they would only be able to explain around one-half of the region's observed production collapse.

Table 8 presents these estimates, based on our baseline models presented in Table 3. Sanctions explain a decline of between 231 and 276 tbd, or 40.5% and 48.5% of pre-sanctions SFV production. We note that these estimates are built on the ATEs estimated over the post-sanctions period, though we did find some evidence that these effects increase with time. If we used the post-oil sanctions ATEs, the estimate range would rise to 297-334 tbd (51.3-58.8% of pre-sanctions production).

How much of the *decline* in production can these estimates explain? When we compare the ATE estimates with the observed declines over the sanction period, the ranges are similar to when the

<sup>38</sup> In other words, even if we put aside concerns on statistical significance, it would take between 81 and 453 months for production to recover from the effect of sanctions in the specifications with positive post-sanctions trends. In the specification with a negative post-sanctions trend, SFV firms continue to grow more slowly than non-SFV firms after sanctions, so the production loss is never recovered. However, none of the effects are statistically different from zero.

denominator is the initial level of production simply because production saw a near-total collapse over this period. Therefore, if we use the average ATE over the sanctions period, the estimated loss accounts for 45.1-53.9% of the observed loss (57.1-65.5% with the post-oil sanctions ATEs). These effects also account for up to around one-quarter of total Orinoco Basin production.

According to our data, Orinoco Basin production at the end of the sample was 133 thousand barrels per day. In other words, our estimates indicate that in the absence of sanctions, production in the Orinoco Basin would sum to 364-409 tbd, or 2.7-3.1 times current production levels.

On the assumption that non-SFV firms had no access to capital markets, these are the correct estimates implied by the model of the effect of closing off their (perhaps limited) access in August of 2017. However, there are reasons why this may be too conservative an estimate. One is that the assumption that the rest of the oil sector had absolutely no access to international capital markets could be too extreme. As we have noted, PDVSA was able to conclude other financial deals outside of SFV agreements in the period immediately before sanctions, namely through collateralized loans and conversion of arrears with providers into New York Law obligations. If the non-SFV sector had some level of capital market access, then the estimates of Table 8 must be treated as lower bounds for the total effect.

Another reason why it may make sense to treat the foregoing estimates as a lower bound is that in the absence of sanctions, nothing would have stopped the government from extending SFV arrangements to the firms that had not yet entered them. In fact, given that these arrangements appear to have been successful at allowing joint ventures to access capital markets under reasonable conditions and that they had been extended to a growing number of firms between 2013 and 2017, it is only natural to expect that the Maduro administration would have welcomed the extension of the model to the rest of the sector in a counterfactual scenario in which there were no sanctions but PDVSA was unable to obtain unsecured financing.

For the purposes of considering these effects, it is useful to distinguish between a passive and an active counterfactual scenario. In the passive counterfactual scenario – captured in the preceding discussion - only the six firms that actually subscribed SFV deals are assumed to maintain capital market access in the absence of sanctions. In an active counterfactual scenario, we assume that the remaining 26 firms in the Orinoco Basin are able to undertake measures that allow them to regain access to capital markets. Under this assumption, we can estimate the counterfactual effects by applying the estimated coefficients to all 33 firms instead of just the ones that had signed SFV deals.

We emphasize that the active scenario is not one of full-fledged economic reforms. Estimating the effect of full-fledged reforms is beyond the scope of this paper. We focus rather on the specific effects of the very limited reforms that we have identified in this paper: those that allow joint venture partners to protect their revenue flows and thus to sever the quality of the credit of their firm from that of their owner.<sup>39</sup>

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<sup>39</sup> In modern market economies, the principle of limited liability insulates the credit of a firm from that of its owner. For this reason, modern corporate law allows the corporate veil to be pierced only when a firm can be shown to be acting as an instrumentality of its owner (see Kirkland, 2015). SFV arrangements can be conceived as agreements that allow the severing of a firm's credit risk from that of its owner by protecting the revenue stream of the firm from attempts by its owner to seize it.

Table 8 also presents the results of the active scenarios. As expected, the estimated effects are now much larger, and range from 499 tbd to 1,297 tbd. These effects could account for between 40.5% and 105.3% of the observed production decline in the Orinoco Basin, and between 45.4 and 118.1% of the observed production decline in the region.

The upper bound of this range merits discussion. Conceptually, it is not hard to understand that the estimates of production loss attributable to sanctions can exceed the magnitude of the observed decline. The active scenario compares a counterfactual in which all firms have access to credit with one in which no firms have access to credit. Non-SFV firms had no credit access prior to sanctions, so it would be natural to expect their output to have grown had they been able to obtain credit access.

However, the wide range of variation in the active estimators, in contrast to the relatively narrow range of the passive scenarios, raises some concerns. The main driver of this difference is the high counterfactual output estimates for the levels specification. This is a direct result of the fact that non-SFV firms are generally smaller than SFV firms, so that attributing the same absolute growth to them (as opposed to proportional growth, as in the log and standardized specifications) significantly raises counterfactual production.

This is not necessarily a problem from a conceptual standpoint: if the main difference between SFV and non-SFV firms is credit market access prior to sanctions, we would expect the latter to become much larger in a scenario in which they gain access to credit. Nevertheless, even if we exclude the levels specification, we estimate that the lack of credit market access caused by sanctions can explain between 45.4 and 61.7% of the observed output decline in the region. Limiting ourselves to that range, we conclude that in an active scenario, production would be between 632 and 811 tbd, or 4.8-6.1 times current production.

**Table 8: Sanctions Effect Estimates, Active and Passive Scenarios**

Dependent Variable	Level of Production	Log of Production (zeros dropped)	Log of Production (imputed minimum)	Standardized
<b>Pre-Sanctions Output</b>				
Orinoco Basin		1,231,944		
Orinoco Basin SFVs		569,466		
<b>Sanctions Effect (Barrels/Day)</b>				
Passive Scenario	-235,872	-230,775	-233,268	-276,028
Active Scenario	-1,297,295	-499,244	-504,636	-677,879
<b>As % of pre-sanctions output</b>				
Passive Scenario	-41.4%	-40.5%	-41.0%	-48.5%
Active Scenario	-105.3%	-40.5%	-41.0%	-55.0%
<b>As % of total loss</b>				
Passive Scenario	-46.1%	-45.1%	-45.6%	-53.9%
Active Scenario	-118.1%	-45.4%	-45.9%	-61.7%

Estimates are based on coefficients in table 3 applied to six SFV and 27 non-SFV firms. Pre-sanctions output is measured in July 2017. Loss is measured as the change in output between July of 2017 and June of 2020.

We are somewhat more hesitant to extend these estimates to regions outside of the Orinoco Basin, given differences in production costs, corporate governance arrangements and private sector participation. Nevertheless, the results are suggestive that in a scenario of policy reforms aimed at

recovering access to capital markets, it is likely that the non-Orinoco production regions would also have been able to avoid the observed production decline.<sup>40</sup>

Even limiting the estimates just to the Orinoco Basin, the estimated effects are macroeconomically significant. Focusing for simplicity on the median estimates of each scenario, sanctions are associated with a loss of 235 tbd in oil production in the passive scenario and 591 tbd in the active scenario. Measured at current oil prices of USD 32.0 for the Venezuelan oil basket, these levels would represent foregone export earnings of USD 2.7-USD 6.9 bn per year. If we extend the estimated proportionate declines for the active scenario in production to the rest of the country's oil sector, we would attribute a total decline of 1,142 tbd, or USD 13.3bn at current oil prices to sanctions. By contrast, Venezuela's oil sector is estimated to generate USD 6.7bn in oil revenues in 2020. In other words, our estimates suggest that in the absence of sanctions, Venezuela's oil exports could be roughly two to three times as large as their current levels.

## 6. Discussion

This paper has used the differential access to credit of oil firms in Venezuela's Orinoco Basin to identify the economic effects of financial and oil sanctions on firm output. We find evidence that financial and oil sanctions led to large losses in oil production among firms that had in place special financing arrangements enabling access to credit relative to those that lacked that access. The effects explain around half of the output drop experienced in those firms since the adoption of sanctions. By barring other firms from the possibility of having access to similar deals, we argue that sanctions impeded the adoption of policies that would, if implemented, have ensured stability of the Orinoco Basin production.

Our results are consistent with those of other studies that find economically large and significant effects of economic sanctions. However, most of the literature has found that it is multilateral sanctions that are associated with large economic effects. Our case, in contrast, provides an example of unilateral economic sanctions with large output effects. One possible explanation for this is the fact that, given the importance of New York credit markets in international finance, United States financial sanctions can effectively act as global restrictions on access to finance, replicating the effects of multilateral sanctions. Furthermore, the willingness of the U.S. to aggressively use secondary sanctions threats dissuaded many non-U.S. actors from interacting with Venezuela, also effectively allowing unilateral sanctions to replicate the effects of multilateral ones.

Our results also suggest that financial sanctions by and of themselves can have large economic effects. Of course, we expect this effect to be conditional on the overall characteristics of the industry and targeted firms. But for a highly leveraged and financially exposed sector such as the Venezuelan oil industry, the evidence suggests that financial sanctions can and did act as an economic surgical strike capable of replicating the effects of a full-fledged trade embargo.

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<sup>40</sup> Firms with SFVs accounted for a smaller fraction of oil output in the rest of the country (21.4% in 2016). However, our monthly data set does not extend to firms outside of the Orinoco Basin.

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