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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 difference-in-differences estimates showing that financial and oil sanctions led to large oil production losses among firms that 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 USD 6.2bn a year at current oil prices. 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.

Keywords: Economic sanctions, Venezuela, oil production.

JEL Codes: F510, F340, F380, H770, Q340, Q380

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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. Economic sanctions are often imposed through multilateral bodies such as the United Nations Security Council, although unilateral sanctions are also increasingly 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 (Imperiale, 2020).

Despite their growing use, there is considerable controversy regarding the effectiveness as well as the impact of economic sanctions. Hufbauer, Schott, and Elliott (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.[1] Some have argued that sanctions are ineffective at sparking regime change because they generate inadequate incentives to relinquish power (Peksen and Drury, 2010; Oechslin, 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. Afesorgbor and Mahadevan (2016) find that sanctions have a negative effect on the target country's income inequality, while Biglaiser and Lektzian (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 and those that affect only targeted elites, become relevant. Understanding the effect of sanctions can also be pertinent to planning for 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 (Felbermayr, Kirilakha, Syropoulos, Yalcin, and Yotov, 2020; Kavakli, Chatagnier and Hatipoglu, 2019; Ahn and 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.

1 See the survey in section 4 of Kamepfer and Lowenberg (2007).

This paper applies a difference-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 difference-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 the observed capacity to enter into special financing deals – relative to those that lacked it. By doing so, we provide the first estimate of the effects of sanctions on the Venezuelan oil industry based on disaggregated firm-level data. This is also, to the best of our knowledge, the first study to use firm-level data to assess the effect of sanctions on the economy of sanctioned countries. Two previous studies have used firm-level data to assess the effect of sanctions on exporting firms of sanctioning countries (Besedeš, Goldbach and Nitsch, 2021; Crozet, Hinz, Stammann and Wanner, 2021).

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 discuss an alternative interpretation in which the pre-sanctions period is defined as the treatment period and treatment as financial market access, and provide estimates for a shorter pre-treatment period for which there is no systrenatic difference in pre-trends.

The rest of the paper is organized as follows. The following section provides a summary of U.S. sanctions on Venezuela in the period under study and reviews the empirical literature on their effect. We then introduce the data set and discuss the key stylized facts that emerge from looking at its aggregate patterns. The paper then goes on to discuss results of the econometric analysis, and concludes with some closing reflections on implications and future research.

An overview of Venezuela sanctions, 2015-2020

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. 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 (Human Rights Council, 2020).

Although the Obama administration sanctioned seven individuals linked to the repression of protests in 2014, [2] the administration of Donald Trump would come to use sanctions intensively as the basis of its Venezuela policy. The U.S. would take the first step in the direction of

2 The number rises to 18 if one counts Venezuelan entities designated using powers different from those granted by the national emergency declaration.

economic – as opposed to individual - sanctions on August 24, 2017, when President Trump issued an executive order prohibiting the purchase of new debt issued by the Government of Venezuela or PDVSA or 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.

In 2018, the U.S. government issued an additional executive order allowing the Secretary of the Treasury, 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). 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% of exports and 12% 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 (DeYoung, Mufson, and Faiola, 2019; C-SPAN, 2019).

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.[3] 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 gave the executive branch the power to sanction non-U.S. persons for having "materially assisted" the Venezuelan government or its state-owned entities.[4]

The U.S would ultimately use this authority in February 2020. The key secondary sanction decision was to designate two subsidiaries of the Russian energy company Rosneft that had handled business with Venezuela (Mohsin and Millard, 2020). The U.S. also sanctioned two Mexican companies that had signed oil-for-food deals with Venezuela (Kassai, 2020). Rosneft, at the time, carried out 70-80%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 (Argus Media, 2019).

3 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 and Kassai (2019) and Bloomberg (2019). On the US-India negotiations related to Venezuelan oil purchases, see Gordon, Gupte, and Bambino (2019).

4 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 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 (2019).

Economic impact of sanctions decisions

Most studies of the effect of Venezuela sanctions have focused on their impact on the country's oil sector.[5] 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. 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%monthly in the period preceding the 2017 financial sanctions to an average of 3.1% 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 oil prices, Colombian oil output stabilized after oil prices began recovering in 2017, while Venezuelan oil output continued declining.

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.

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 difference-in-differences estimates using the cross-country panel according to 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 on 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.

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

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).[6]

Rodríguez (2019) contends that there are several reasons why financial sanctions could have led to a worse 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.[7] Another reason is that the fact that PDVSA and the 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.

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 fewer than 90 days (Federal Register, 2017), that exception 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.

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 (Ministerio del Poder Popular de Petróleo, 2019). 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

6 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%. Virtually all of this increase is caused by the collapse of the country's GDP valued in foreign currency.

7 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.

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. To the best of our knowledge, this is the first study to use the Orinoco Belt data for the purposes of econometric estimation.

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 (Rodríguez, 2005). 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.

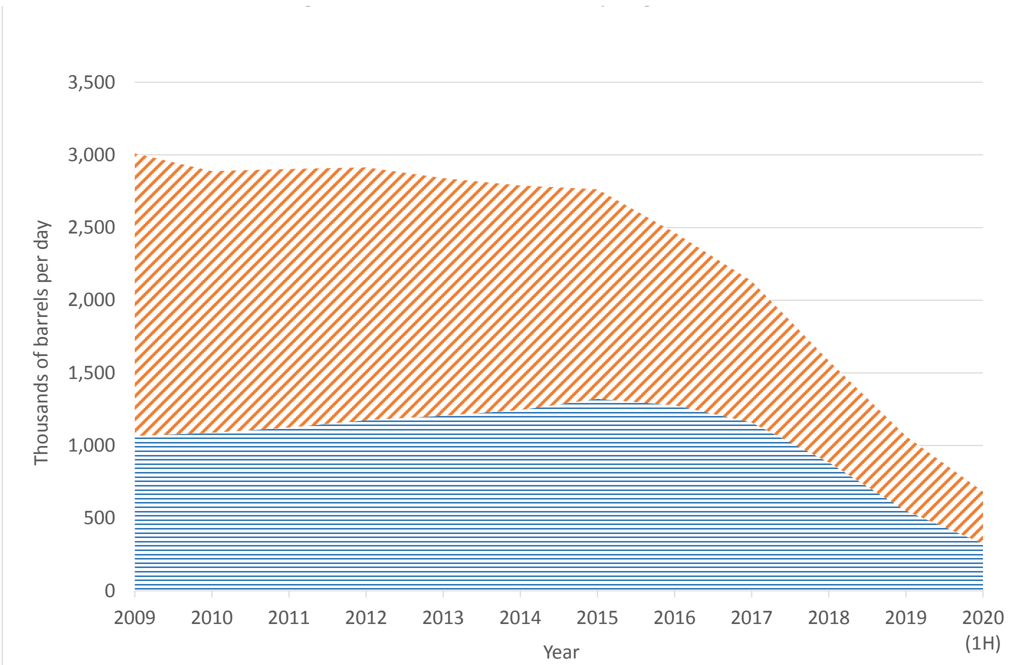
Over time, the Orinoco Belt became one of the main sources of production growth in the Venezuelan oil industry. This was due in part to the greater concentration of joint venture arrangements with national and multinational companies in the area, which allowed FDI inflows into an area of significant potential during a period of high oil prices. As the first panel of Figure 1 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 at 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% 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.

The second panel of Figure 1 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 in August 2017 and the end of our sample in 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 1. Oil Production by Region and Ownership Structure, 2009-2020



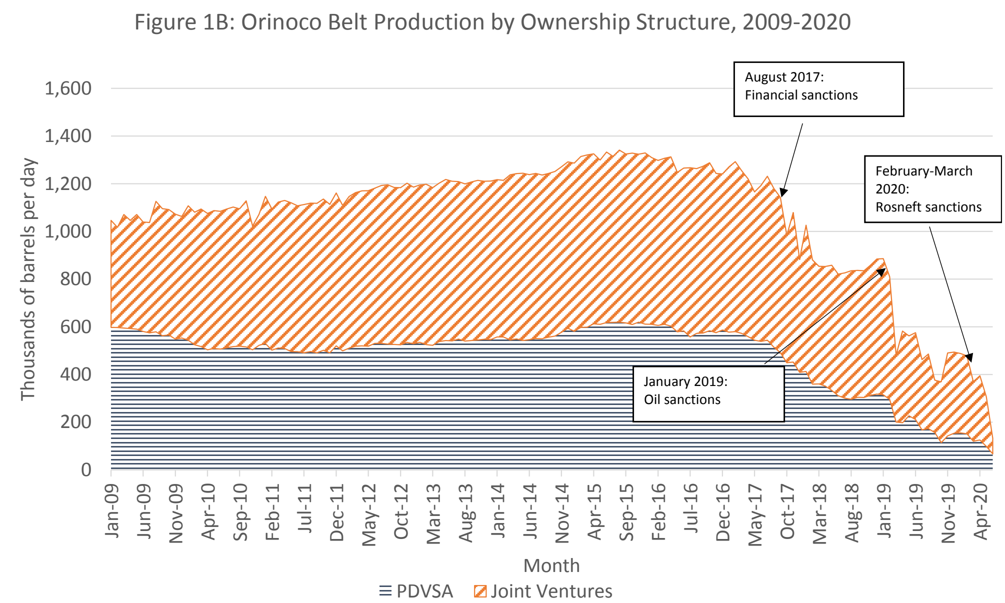


Figure 1A shows yearly data on Venezuela's production subdivided between the Orinoco Belt region and the rest of the world between 2009 and 1H20. The Belt holds approximately half of the country's production. As can be seen, production in the region showed moderate growth on the first 6 years (2009 to 2015), only to fall in the remaining years depicted in the figure. This decrease was consistent with an overall collapse in output, as the Orinoco Belt still held 47.5% of national production by 1H21. Figure 1B depicts detailed monthly crude production data distinguishing between fields wholly owned by PDVSA and those operated by joint ventures. Production in both types of fields collapsed between August 2017 and the end of the sample, June 2020. Joint ventures' production collapsed by 90.1%, a slightly faster rate than that of PDVSA fields, which fell by 86.9%. Source: PDVSA.

Source: Own calculations, 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 the previous section 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% (see Appendix 1). The fact that these deals were signed as late as December of 2016 suggests 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 the 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 in 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 (Aizhu and Párraga, 2019). 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.[8] 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.[9] 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 in 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 (OFAC, 2020).

8 See Cohen and Párraga (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.

9 See General Licenses 8-8F in Venezuela-Related Sanctions. U.S. Department of Treasury.

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 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.

Econometric Analysis

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 ten blocs that are wholly operated by PDVSA and 23 that are operated by JVs, in all of which PDVSA has a majority stake.[10] Of these, ten have more than one partner, and four have three partners. The most frequent nationality of the largest minority partner is Venezuelan and Chinese, each with four 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. [11] 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 ten blocs operated completely by PDVSA. 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.

10 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.

11 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	-	-	10	559,709
Belarus	1	4,657	-	-	1	4,657
Brazil	3	5,362	-	-	3	5,362
China	4	157,254	1	143,351	3	13,903
Cuba	2	5,639	-	-	2	5,639
France	1	103,096	1	103,096	-	-
India	1	17,797	1	17,797	-	-
Italy	1	496	-	-	1	496
Russia	3	132,135	1	118,763	2	13,373
Spain	1	24,542	-	-	1	24,542
US	2	191,938	1	154,778	1	37,159
Venezuela (private)	4	30,102	1	28,888	3	1,215
Total	33	1,232,727	6	566,672	27	666,054

Table 1 shows daily average crude output for all JVs according to the nationality of their second-largest partner (by law, the largest partner is always PDVSA). It also shows how output is divided between SFV (firms receiving Special Financing Vehicles) and non-SFV firms. Sources: Own calculations, PDVSA.

Baseline specification

We use a standard difference-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 in August of 2017 listed in Tables 1 and 2 as our treatment group and the remaining twenty-six firms[12] that did not have SFV deals at that time as our control group. Therefore, our baseline specification is:

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

where p_{it} is an indicator of production in firm i at time t, η_i denotes a firm-specific fixed effect, λ_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 ε_{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

12 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)

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.[13] In fact, there is a weak negative correlation between the series in the period prior to the adoption of sanctions in August 2017(ρ=-.33). Remarkably, however, there is a very strong correlation in the period beginning in August 2017 (ρ=.89). This suggests that these groups may have become more similar, rather than different, as a result of the intervention. I return to this issue and its implications for model specification 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 for 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 100 barrels per day per bloc (approximately 0.2% of average daily bloc production). Appendix 2 discusses in greater detail the choice of imputation value and its effect on the estimators

Figure 2. Output per Firm by Financing Access, 2008-2020

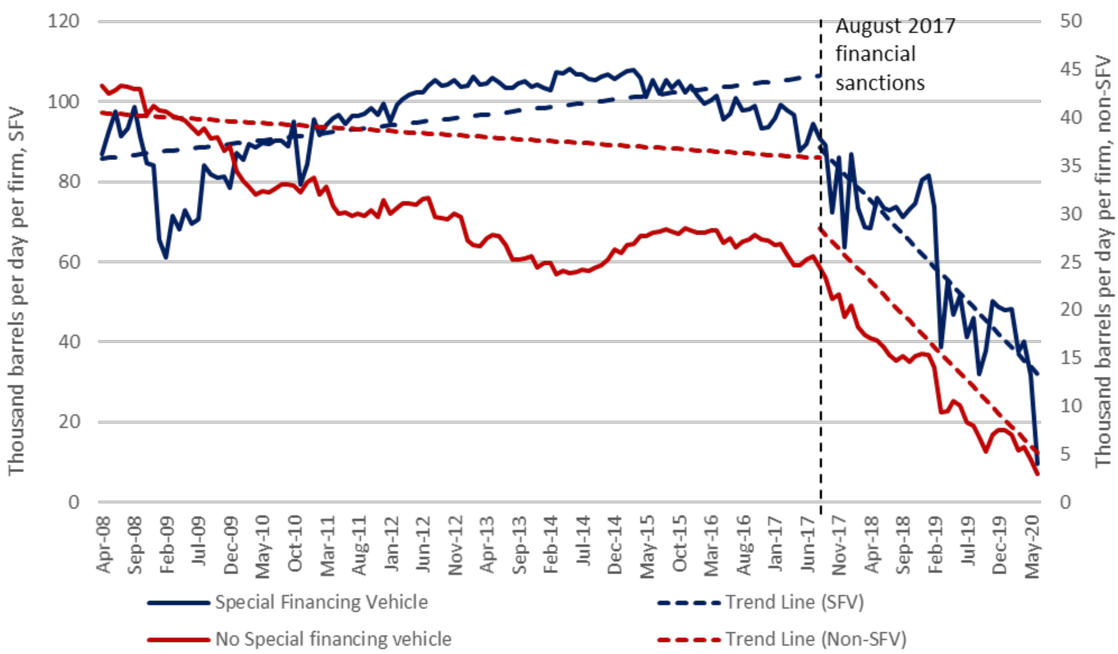


Figure 2 shows the evolution of monthly crude production for SFV (blue) and non-SFV (red) firms in the Orinoco Basin. Trendlines show a break on the pre-sanctions and post-sanctions period. Sources: Own calculations, PDVSA.

13 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.

Table 2 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 average treatment effect (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 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 in 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 somewhat higher (66.0 log points) when we impute the minimum values; statistical significance is slightly stronger (p=.053). 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). [14] In all four specifications, the ATE for the oil sanctions period is larger than for the period with only financial sanctions.

Table 2. Panel Regression Results, Baseline Specification, Full Sample (January 2008-December 2020)

	Level of Production	Log of Production (zeros dropped)	Log of Production (imputed minima)	Standardized
SFV*Trend	243.9	0.013***	0.014***	0.022***
	(199.6)	(0.003)	(0.004)	(0.007)
Average Treatment Effects				
SFV*Financial Sanctions	-39314***	-0.52*	-0.66*	-1.736***
	(13215)	(0.304)	(0.329)	(0.395)
SFV*Financial and Oil Sanctions	-51416***	-0.739	-0.939*	-2.106***
	(16177)	(0.443)	(0.541)	(0.453)
N	4188	3832	4188	4188
Adjusted R-Squared	0.2528	0.3873	0.3998	0.4801
Mean dependent variable	0	0	0	0

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

An alternative approach is to eschew the issue of non-parallel trends altogether and focus on a more recent time period for which the data shows similar pre-intervention trends in both the control and treatment groups. Table 3 shows the results of focusing on the period beginning

14 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.

in January 2015, 31 months before the sanctions. The results continue to yield significant coefficients for the levels and standardized specifications but not for the logarithmic specifications.

There are several reasons why the broader sample is preferable, even though the shorter sample provides us with the appearance of parallel trends. The first one is that the appearance of parallel trends in a small subsample does not actually mean that the trends are parallel; it may simply mean that we have insufficient data to establish that they are non-parallel (as the broader sample suggests). Furthermore, restricting to the post-2015 sample would give us a treatment window only 1.2 times as large as the post-treatment window, well below the average level of 2.6 found in the most commonly cited landmark difference-in-differences studies (Rodríguez, 2019, Table 1). Excessively short pre-treatment windows are known to bias difference-in-differences estimates (see Jaeger, Joyce, and Kaestner, 2016 for an illustration). Restricting the pre-treatment window also discards an important part of our data (58% of observations) and increases the share of zero-production observations (from 8.5 to 15.1%), whose treatment is sensitive to imputation. We discuss these issues further in Appendix 5, which presents specification diagnostic plots. We return to the full sample in the estimates reported in the remainder of the paper.

Table 3. Panel Regression Results, Restricted Sample (January 2015-June 2020)

Average Treatment Effects	Level of Production	Log of Production (zeros dropped)	Log of Production (zeros dropped)	Log of Production (imputed minima)	Standardized
SFV*Financial Sanctions	-25610**	0.084	-0.045	-0.045	-0.717***
	(10549)	(0.267)	(0.313)	(0.313)	(0.205)
SFV*Financial and Oil Sanctions	-35646**	-0.012	-0.204	-0.204	-0.9***
	(14015)	(0.416)	(0.545)	(0.545)	(0.234)
N	2113	1832	2113	2113	2113
Adjusted R-Squared	0.3184	0.4254	0.4027	0.4027	0.5154

Standard errors in parentheses. Asterisks denote statistical significance: *10%, **5%, ***1%. All specifications include month-specific dummies, firm fixed effects, and post-treatment month*SFV interactions. Average Treatment Effect is the average of estimates of βk 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 omitted category is firms operated completely by PDVSA, which 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 .027, 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 2.[15]

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

	Level	Logarithm (zeros dropped)	Logarithm (imputed minima)	Standardized
SFV*Trend	155.7	0.014***	0.014***	0.022**
	(221.8)	(0.005)	(0.005)	(0.008)
Average Treatment Effects				
SVF*Financial Sanctions	-47788***	-0.771**	-0.834**	-2.051***
	(11249)	(0.306)	(0.360)	(0.454)
SFV*Financial and Oil Sanctions	-61803***	-1.053**	-1.188**	-2.396***
	(13662)	(0.439)	(0.574)	(0.536)
China*Financial Sanctions	12717	0.203	-0.211	0.456
	(20229)	(0.406)	(0.365)	(0.513)
Russia*Financial Sanctions	19952	0.624	0.141	0.849
	(18179)	(0.419)	(0.490)	(0.626)
United States*Financial Sanctions	32597*	1.163***	1.37***	1.17**
	(18100)	(0.256)	(0.325)	(0.553)
Other*Financial Sanctions	33355**	-0.047	0.244	0.834
	(38069)	(1.329)	(1.815)	(1.076)
N	4188	3832	4188	4188
Adjusted R-Squared	0.3814	0.4588	0.4621	0.5143

Standard errors in parentheses. Asterisks denote statistical significance: *10%, **5%, ***1%. All specifications include month-specific dummies, firm fixed effects, post-treatment month*SFV and post-treatment month*country group interactions. Average Treatment Effect is the average of estimates of βk and γkj 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 β_k estimates and associated 95% confidence intervals over time. All four estimates 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 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 specifications, 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

15 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.

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 3. SFV-Time Interactions for post-sanctions period, alternative specifications.

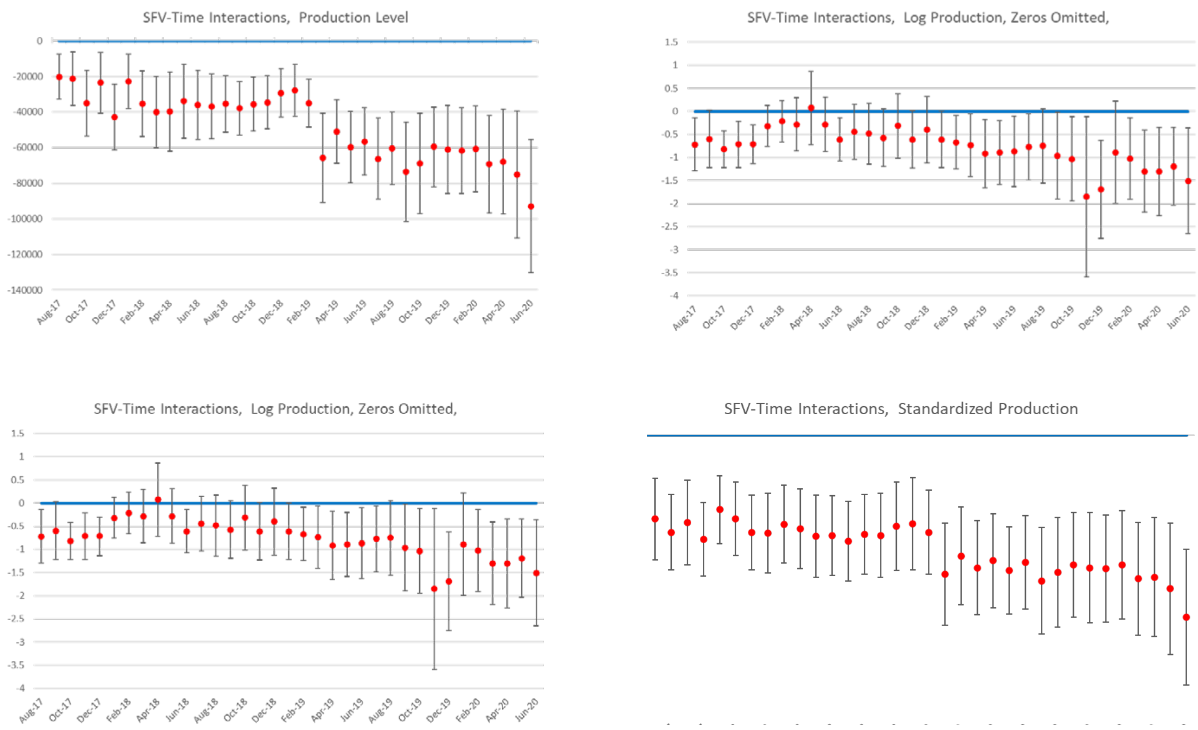


Figure 5 plots estimates of monthly treatment effects (β_k) for alternate specifications with an associated 95% confidence interval over time. All plots show increasingly larger effects on firm production.

Dealing with non-parallel trends

As noted above, our specification allows for a treatment group trend as well as country group trends.[16] This corresponds to a *parallel growth* specification which tests whether there is a change in the 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 tables 2 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. We explore this possibility in greater detail in Appendix 3, where we show that controlling for higher-order trends preserves the main results regarding the magnitude and significance of ATEs.

In the working paper version of this paper, we discuss an alternative 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. We suggest it may be more appropriate to think of access to finance as the treatment and the imposition of

16 Control group and omitted country group trends are subsumed in the time effects by construction.

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. Estimates from regressions of our production indicator on country and treatment group-specific trends and country and group-specific month dummies for all periods prior to (instead of after) the start of sanctions on T_0 yields estimates that unambiguously show SFV firms with higher production levels previous to sanctions (see Rodríguez, 2020 for a fuller discussion).[17]

Magnitude of estimated effects

We now turn to a discussion of the magnitude of the estimated effects. As we have already pointed out, the estimates presented in Tables 2-4 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 ask how much of the implosion of the Venezuelan oil sector these effects can account for.

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 5 presents the estimates, based on our baseline models presented in Table 2. We assume that in the absence of sanctions, firms with SFV deals as of August 2017 would have maintained financial market access, while firms that did not have these deals in place would have continued to lack access. Under that assumption, 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 297-347 tbd (52.2-60.9% 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 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 (58.1-67.8% 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. One 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

17 There is a well-established literature on dealing with time-varying treatment effects which can accommodate withdrawal of treatment (see section 5.2.4 of Lee, 2016). We are unfamiliar, however, with any literature dealing with the estimation of time-varying treatment effects under non-parallel trends, and a broader development of such an estimation framework for these is out of the scope of our paper.

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.

In Appendix 4, we develop an alternative scenario in which the government is assumed to extend SFV arrangements to all production blocs in the absence of sanctions. We term this the active scenario, in contrast to the baseline passive scenario discussed in this section, in which only firms with these arrangements at the time of sanctions are assumed to maintain access to credit. The active scenario presents much larger effects which range from 499 tbd to 1,297 tbd, or between 45.4% and 118.1% of the post-sanctions production loss in the whole region.

It is worth underscoring that, by construction, DID estimates can tell us nothing about the effect of other causes distinct from the intervention that is studied. There is certainly no shortage of alternative explanations for the decline in Venezuela's oil production, including underinvestment, mismanagement, corruption and militarization of the oil industry (Monaldi, Hernández and La Rosa, 2020; Hausmann and Muci, 2019; Rodríguez, 2019). Yet even an estimate of sanctions effect accounting for more than 100% of the observed decline does not negate the effect of these other factors; it simply implies that in the absence of sanctions and other constraints, production would have grown strongly. At the same time, DID estimates omit potential indirect effects of the sanctions on the control group. These facts suggest caution when using the estimates of Table 8 to reach conclusions regarding the relative weight of sanctions vis-à-vis other factors in explaining the country's decline.

Table 5. Sanctions Effect Estimates, Passive Scenario

Output	Level of Production	Log of Production (zeros dropped)	Log of Production (imputed minima)	Standardized
Pre-Sanctions Output				
Orinoco Basin		1,231,944		
Orinoco Basin SFVs		569,466		
Sanctions Effect (Barrels/Day)	-235,882	-230,775	-275,143	-276,028
As % of total loss	-41.4%	-40.5%	-48.3%	-48.5%
	-46.1%	-45.1%	-53.8%	-53.9%

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 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, at least in a scenario of policy reforms aimed at recovering access to capital markets, non-Orinoco production regions would also have been able to avoid the observed production decline. [18]

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 255 tbd in oil production in the passive scenario and 637 tbd in the active scenario. Measured at current oil prices of USD 72.0 for the Venezuelan oil basket,

18 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.

these levels would represent foregone export earnings of USD 6.7-USD 16.7 bn per year. By contrast, Venezuela's oil sector is estimated to generate USD 12.1bn in oil revenues in 2020. In other words, our estimates suggest that in the absence of sanctions, Venezuela's oil exports could be more than twice as large as their current levels.

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 the 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.

Appendix

Financial conditions of Orinoco Basin Special Financing Vehicle arrangements

Table A.1 describes the financial details of the eight financing deals signed by joint ventures in the Orinoco Basin region.[19] 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.

19 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 A.1. 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%
Petrowarao	Perenco	UK/ France		Loan	420	Aug-14	2021	7	Libor+4.5%
Petrocedeño	Total	France	Statoil	Revolver	60	Oct-14	2017	1	0.0132
Petrozamora	Gazprom Bank	Russia		Loan	1,000	Dec-13	2019	6	Libor+6.9%
Petrolera Sinovensa	CNPC	China		Loan	4,015	Jun-13	2023	10	Libor+5.8%
Petroboscán	Chevron	United States		Revolver	10	Jul-15	2017	1	Libor+4.5%
Petroboscán	Chevron	United States		Loan	2,000	May-13	2025	12	Libor+4.5%
Petrodelta	HNR/ CT Energía	Venezuela		Revolver	6	Dec-15	2017	1	0.12
Petroquiriquire	Repsol	Spain		Loan	1,200	Oct-16	2026	10	Libor+4.5%
Petrolera Indovenezolana	ONGC	India		Revolver	60	Nov-16	2017	1	Libor + 5.5%
Petrolera Indovenezolana	ONGC	India		Loan	318	Nov-16	2019	3	Libor + 5.5%
Petromonagas	Rosneft	Russia		Prepaid exports	1,985	Dec-16	2021	13	Libor+5%
Petrozumano	CNPC	China		Loan	184	Sep-18	N/A	N/A	N/A

Table 2 lists JVs subject to SFV arrangements, detailing financing conditions as well as main partners and their nationalities. Sources: Own calculations and estimates, PDVSA financial statements.

Imputation for zero production values

There is nothing resembling consensus regarding the appropriate approach regarding zero values in logarithmic specifications. A recent review of articles published in the *American Economic Review* between 2016 and 2020 found that 31% of papers using a logarithmic specification and a data set with zero values opted for dropping the observations, while 48% opted for adding a discretionary value to the variable (Bellego, Benatia and Pape, 2021).

A first question that researchers must ask about zero values is why the data contain zeros. The problem could be due to measurement issues (i.e., small values being rounded down) or to actual economic decisions (i.e., a decision to produce zero output). Strictly speaking, a zero value with a logarithmic specification is an outcome that is not feasible under the theory represented by the logarithmic specification. Therefore, it is often necessary to rethink the theoretical specification to understand how it needs to be modified to incorporate the possibility of zero outcomes.

Consider the problem of a representative firm that sets production of a single good q to maximize profits subject to an exogenous product price p and a concave and increasing cost function *C(q)* with *C' (q)>0* and *C" (q)>0*. Let $a \geq 1$ be a multiplicative factor representing the effect of sanctions on costs. A profit-maximizing firm will choose q to maximize:

$$\Pi = Max[0, pq - C(q)a] \tag{A.1}$$

Let the cost function equal the sum of an exponential term plus a fixed cost F that is only incurred if production values are positive, $C = q^{\gamma} + F \cdot I[q > 0]$. Assuming an interior solution, the first order condition yields:

$$q = \left(\frac{p}{a\gamma}\right)^{\frac{1}{\gamma-1}} \tag{A.2}$$

which can be written in logarithms as:

$$lnq = A_0 + \frac{1}{\gamma-1} lna \tag{A.3}$$

with $A_0 = \frac{1}{\gamma-1}(\ln p - ln\gamma)$. It is specifications like (A.3) that underlie estimation of equations like those reported in columns (2) and (3) of Tables 3-8. Yet zero production values are inconsistent with an equation like (A.4); if we assume that the equation fully represents the predictions of the theory, then zero production would refute the theory.

This anomaly is the result of assuming an interior solution. (A.3) is not valid for all values of a; it is valid only for values of a that are consistent with non-negative profits. In other words, the full solution to (A.1) implies:

$$q = q = \left(\frac{p}{a\gamma}\right)^{\frac{1}{\gamma-1}} \text{ if } a \leq a^* \\ 0 \quad \text{ if } a > a^* \tag{A.4}$$

$$\text{for } a^* = \left(\frac{p^{\frac{\gamma}{\gamma-1}}\gamma^{-\frac{1}{\gamma-1}}(1-\gamma^{-1})}{F}\right)^{\frac{\gamma-1}{\gamma}} \tag{A.5}$$

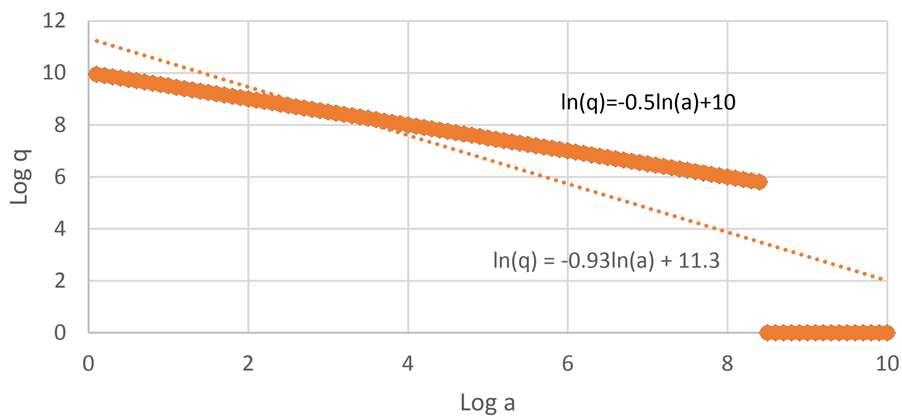
Figure A.1 represents this relationship graphically in logarithmic form. Theory does <u>not</u> predict a continuous relationship between q and a, but rather a discontinuous piecemeal function. If what we are attempting to do is to estimate the relationship between q and a for the complete range of variation of a, then a logarithmic specification is inherently misspecified. This is the problem that imputation tries to solve.

This discussion brings us back to the question of what is it that we want to estimate. If we want to estimate the average relationship between q and a over the whole range of variation of a, including those values that lead to zero production levels, then it may well be a reasonable choice to approximate (A.6) through a linear function in levels. If the regressors are normally distributed, then the OLS estimates will return the average derivatives of the non-linear function (Rodríguez and Shelton 2013, Proposition 1). This is the justification for using absolute production levels as a dependent variable in our estimates in column (1) of tables 3-5 in the text. Alternatively, if we are interested only in the slope of the relationship when $a \leq a^*$ (i.e., assuming an interior solution) then it may make sense to concentrate only on observations with non-zero values.

However, there are various problems associated with using only non-zero values in the log-log specification. One is the standard truncated regression issue: the fact that our sample is restricted to non-zero production levels suggests that the error terms are not uncorrelated with the regressors, violating a key assumption of the classical regression model. Even if they are, it will not necessarily be an efficient solution to discard the useful information that certain observations correspond to values of a that exceed a^*.[20] The problem of imputation becomes one of finding a rule to replace the value observed in the data for these observations ($q=0$) with values such that a regression run on the whole data will produce the best possible estimate of $\frac{1}{\gamma-1}$.

Figure A2 shows an example of the biases that may emerge – and why it is not necessarily a good idea to use the conventional quick fix of adding an arbitrarily small value to the dependent variable. If the imputed value that replaces zero observations is far from the value that would have been generated by the true equation in the absence of the non-negativity constraint on profits, then the quick fix may produce a severe estimation bias. This is precisely the problem in our data, where a value of 1 would indicate an unrealistically low value (one barrel per day) in comparison with the average level of non-zero observations (42,146 barrels per day).

Figure A.1. Misspecification Bias when Replacing In(q) with In(q+1) in Log-Log Specification



An alternative solution is to substitute a constant y^* for $ln(q)$ when $q=0$, where y^* is set to ensure that the OLS estimate of the slope of the log-piecemeal function is equal to b_0. In other words, let $x = \ln(\alpha)$ and let $\tilde{a}, \tilde{b}$ be the solutions to:

$$Min_{\tilde{a},\tilde{b}} S = \int_0^{\overline{x}} \left(\tilde{a} + \tilde{b}x - a - bx\right)^2 f(x)dx + \int_{\overline{x}}^1 \left(\tilde{a} + \tilde{b}x - y^*\right)^2 f(x)dx \quad \text{(A.6)}$$

where $a = \frac{1}{\gamma-1} \ln\left(\frac{p}{\gamma}\right)$ and $b = -\frac{1}{\gamma-1}$. Assuming that x~U[0,1], then it is straightforward to show that $\tilde{b} = b$ if and only if: [21]

$$y^* = a - \frac{-b-3b\overline{x}^2+4b\overline{x}^3}{6\left(\overline{x}-\overline{x}^2\right)}. \quad \text{(A.7).}$$

If we know y^*, we can retrieve an estimate of b by running OLS on the transformed dependent variable:

$$\begin{aligned} y &= \ln(q) \text{ if } q > 0 \\ &= y^* \text{ if } q = 0 \end{aligned} \quad \text{(A.8)}$$

Of course, we do not know a, b nor $\overline{x}$ so we will need an estimate of these to construct y^*. One proposal is to use OLS on the data with zeros dropped to obtain the estimates of a and b used to calculate (A.7) while approximating $\overline{x}$ by the share of non-missing observations on each sample.

20 Whether estimation of OLS on non-zero observations in our model is affected by truncation bias depends on whether a firm observes the shock to production before or after it can decide to stop production. For simplicity, we assume in the rest of the discussion that it observes it after it has set production, so it sets $q=0$ only if expected profits are less than zero.

21 This is established by deriving the first-order conditions for maximization of (A.8) and setting $\tilde{b} = b$. Details of algebraic derivation are available upon request.

Appendix Table A.2 shows the results of using different imputation methods on our baseline specification. The first column shows the results with the zero observations dropped, while the second through fourth columns show respectively the effect of using the quick-fix estimator adding respectively 1, 100, and 1000 barrels per day to production of each bloc. To understand the relative magnitudes of these numbers, note that they are respectively 0.0024%, 0.24%, and 2.4% of average non-zero bloc production in the sample. While all specifications yield negative coefficient estimates, that for x=1 is not significant and is much lower in absolute value than the rest of the estimates. In contrast, both for x=100 and x=1000, the estimates are negative, strongly significant, and higher in absolute value than the specification with zeros dropped. The last column of table A.1 shows our theory-derived imputation estimate, which replaces zero production values with that derived in equation (A.7). This gives us an estimate that is very close to the x=1000 estimate, both in absolute values and in terms of statistical significance.

In the text, we adopt x=100. This is because the results in Table A.2 show that it is an intermediate solution among quick-fix estimates, as well as relatively conservative in comparison to the y* estimate. x=100 also has the virtue of being very close to the median minimum production across blocs in the data (x=107.1), another intuitive replacement for zero production levels.

Table A.2 Sensitivity to alternative imputation methods

Method	Zeros Dropped	Replace q with q+x			Replace ln(q) for q>0
		x=1	x=100	x=1000	ln(q)=y*
SFV*Trend	0.013***	0.02***	0.014***	0.009***	0.012***
	(0.003)	(0.006)	(0.004)	(0.003)	(0.003)
Average Treatment Effects					
SFV*Financial Sanctions	-0.52*	-0.163	-0.66*	-0.81***	-0.796**
	(0.304)	(0.501)	(0.329)	(0.256)	(0.294)
SFV*Financial and Oil Sanctions	-0.739	-0.315	-0.939*	-1.109**	-1.143**
	(0.443)	(0.780)	(0.541)	(0.415)	(0.424)
N	3832	4188	4188	4188	4188
Adjusted R-Squared	0.3873	0.3338	0.3998	0.3707	0.2871

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

Sensitivity to non-linear trends

Table A.3 explores the effect of non-linear trends by introducing a cubic spline trend approximation in each specification.[22] 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 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.

22 On the use of spline functions as a flexible approach to modelling non-linear effects, see Newson (2012) and section 3.5 of Burden, Faires and Burden (2015)

Table A.3. Panel Regression Results, Non-linear Trend Specifications

	Level	Logarithm (zeros dropped)	Logarithm (imputed minima)	Standardized
SFV*Linear Trend	555	0.032***	0.032**	0.054***
	(391.6)	(0.012)	(0.012)	(0.018)
SFV*Spline Trend	-442.8**	-0.019	-0.019	-0.034**
	(210.0)	(0.012)	(0.011)	(0.016)
Average Treatment Effects				
SFV*Financial Sanctions	-35399***	-0.341	-0.362	-1.182**
	(11,518.5)	(0.348)	(0.420)	(0.443)
SFV*Financial and Oil Sanctions	-47162***	-0.54	-0.625	-1.366**
	(15,159.7)	(0.554)	(0.697)	(0.588)
China*Financial Sanctions	21969	0.281	-0.062	0.839
	(21,428.3)	(0.549)	(0.538)	(0.560)
Russia*Financial Sanctions	36651*	-0.066	0.143	0.702
	(19,929.0)	(0.792)	(0.867)	(0.898)
United States*Financial Sanctions	32159	0.239	0.573	0.731
	(20,675.3)	(0.747)	(0.757)	(0.656)
Other*Financial Sanctions	38847**	-0.028	0.453	1.202***
	(37,436.9)	(0.217)	(0.855)	(0.541)
N	4188	3832	4188	4188
Adjusted R-Squared	0.4003	0.4709	0.4719	0.5376

Standard errors in parentheses. Asterisks denote statistical significance: *10%, **5%, ***1%. All specifications include month-specific dummies, firm fixed effects, post-treatment month*SFV and post-treatment month*country group interactions. Average Treatment Effect is the average of estimates of βk and γkj 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 A.4 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 A.2 of non-linear effects.

Table A.4. Non-Inferiority Tests, Linear vs. Cubic Spline Specifications.

	Level	Logarithm (zeros dropped)	Logarithm (imputed minima)	Standardized
Difference in coefficients	-12389	-0.430	-0.472	-0.87
Confidence Interval				
Lower Bound	-22085	-0.924	-0.959	-1.57
Upper Bound	-2694	0.064	0.015	-0.17
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 suest command in stata (see Weesie,2000)

Active counterfactual scenario

This subsection develops an alternative counterfactual scenario in which, in the absence of sanctions, SFV arrangements – or, more generally, access to capital markets – is assumed to extend to all 33 firms in the sample and not just to the ones that had the arrangement at the start of sanctions. The rationale for this scenario is that, given the success of these mechanisms in allowing Orinoco Basin JVs to have access to finance, it would have been reasonable to expect the government to continue extending the program to other potential participants, had sanctions not impeded them from doing so.

Note that the active scenario does not attempt to estimate the effect of full-fledged economic reforms, which 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.[23]

Table A.5 presents the results. The estimated effects are now much larger, ranging 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.

Table A.5. Estimated Effect of Sanctions on Oil Production, Active Scenario

Output	Level of Production	Log of Production (zeros dropped)	Log of Production (imputed minima)	Standardized
Pre-Sanctions Output				
Orinoco Basin		1,231,944		
Orinoco Basin SFVs		569,466		
Sanctions Effect (Barrels/Day)	-1,297,352	-499,244	-595,226	-677,879
As % of pre-sanctions output	-105.3%	-40.5%	-48.3%	-55.0%
As % of total loss	-118.1%	-45.4%	-54.2%	-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.

23 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.

The fact that some of these estimates exceed 100% simply means that the estimated sanctions effect exceeds the magnitude of the observed decline. While this may seem surprising at first sight – especially if one has a strong prior that other factors contributed to the decline – it reflects the fact that in the absence of sanctions, extending the SFV arrangements from the six firms that had them to all 33 firms in the sample would have been expected to produce significant output growth.

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 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 would conclude that in an active scenario, production would be between 632 and 811 tbd, or 4.8-6.1 times current production.

Specification Diagnostic Plots

This subsection presents event-study plots of treatment leads and lags (e.g., Angrist and Pischke, 2009; Miller, Johnson, and Wherry, 2019; Cunningham, 2021). Since all firms receive treatment at the same time, this is simply a plot of time-specific SFV effects. That is, we estimate:

$$p_{it} = \eta_i + \lambda_t + \sum_{k=0}^{T} \beta_k SFV_i + \varepsilon_{it}, \quad \text{(A.9)}$$

and plot the β_k estimates for the whole sample, including the pre-sanctions period $\beta_k = \{0, T_0\text{-}1\}$ and the sanctions period $\beta_k = \{T_0, T\}$.

Note that these time plots, common in difference-in-differences specification searches, do not have a straightforward counterpart in specifications where there is a pre-intervention time-trend. When the parallel trends hypothesis holds, we can assess the strength of the intervention effects by seeing whether pre and post-intervention treatment group effects differ from zero. When pre-treatment effects are not significantly different from zero, but post-treatment effects are, there is strong *prima facie* evidence for a treatment effect. In the absence of parallel trends, however, we need to compare post-intervention treatment effects with the level of the dependent variable that we would have expected as a result of a continuation of the pre-intervention trends. Yet these trends cannot be estimated independently of the pre-intervention treatment group effects that they are collinear with.

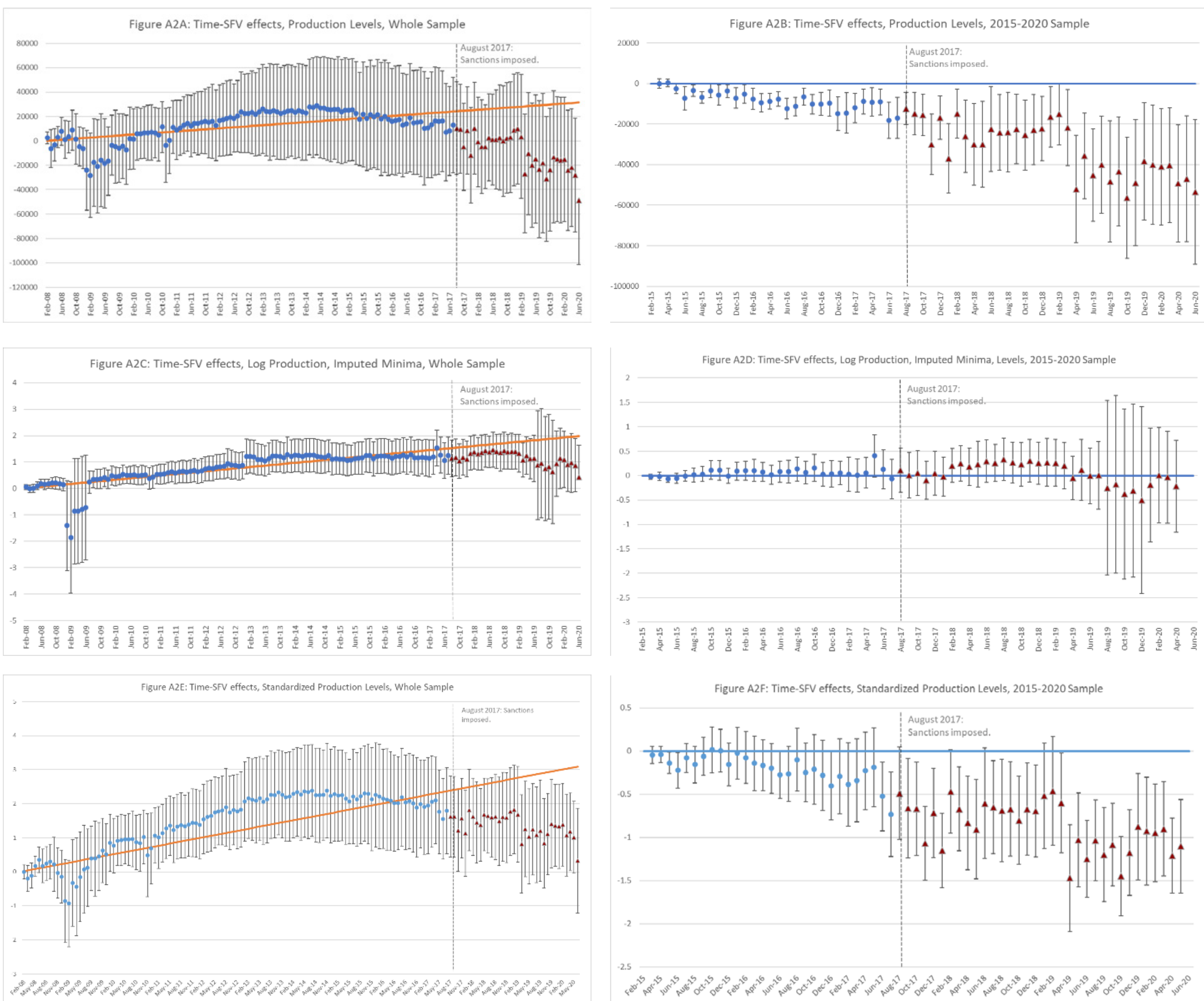
The results of estimating (A.9) are plotted in Figure A2 for the two samples discussed in the text: the full sample (January 2008-June 2020) and the restricted sample (January 2015-January 2020). For the case of the full sample, in which we have shown evidence of pre-treatment trend differences, we report the fitted values of a time-specific SFV trend estimated only over the pre-sanctions period after controlling for firm and time-specific effects. This allows us to assess the extent to which our treatment effects differ significantly from those that would be expected under the parallel growth hypothesis captured by the SFV trend in equation (1).

The panels on the left side of Figure A2 (A, C, and D) show that the time-specific SFV effects begin to differ systematically from the pre-sanctions SFV trend after the time of treatment. No pre-sanctions time effects are significantly different from the trend in any of the specifications, while the sanctions time-effects are significantly higher in 11-54% of cases, depending on specification of the dependent variable. The right-side panels illustrate the results on the restricted sample. Here the results differ between specifications, with post-sanctions SFV effects consistently lower than the pre-sanctions effects in the levels and standardized specification, but not in the logarithmic specification.

The figures clearly show that focusing only on the restricted sample beginning in 2015 does not solve the problem of the need to control for pre-treatment trends. Panels B and F show clear pre-sanctions declining trends, shedding doubts on the validity of the parallel trends hypothesis even in the restricted sample. On the other hand, the plots for the whole sample also capture a pre-treatment change in trend, motivating the need to test for non-linear trends (which we have dealt with in Appendix 3). Comparison of panels C and D suggest that the pre-2015 information plays an important role in helping us identify the treatment effect in the logarithmic specification, cautioning us as to the loss of information from focusing on the restricted sample.

There are at least two important caveats in interpreting these plots. One is that standard errors are high for many of the post-treatment SFV effects and are higher in the 2008-20 than in the 2015-20 sample. This is a consequence of firm-level clustering over a longer period that exhibits greater variation across the dependent variable. The estimation of standard errors and appropriate clustering choices in difference-in-differences designs with autocorrelation is a complex issue that is often best addressed by aggregating over the treatment period, as we do in the main text (see Bertrand, Duflo, and Mullanaithan, 2004). The other caveat is that the logarithmic specification shown below, which gives the weakest results in these plots, is strengthened considerably when we control for home country effects (see Table 4).

Figure A.2. Time-specific treatment group effects, alternative specifications.



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