# Do Sanctions Affect Growth?*

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**Abstract**

Direct measures of the economic impact of sanctions are contaminated by the endogeneity that arises when other events in target countries (e.g., civil or interstate conflicts, political independence, etc.) instigate the imposition of sanctions. To address this issue, we propose a novel instrument, sender's aggressiveness, captured by the number of sanctions imposed in a given year. After establishing the validity of this instrument, we quantify the impact of sanctions on growth in sanctioned states and show that, on average, an additional sanction decreases contemporaneous real GDP per capita in target states by 0.39 percent. We also substantiate the presence of a significant (in magnitude) downward bias in the corresponding OLS estimates and demonstrate that the effects of sanctions on growth vary widely depending on the types of sanctions considered, their purported objectives, measures of their success, and the duration of their effects.

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# 1 Introduction

History is replete with examples of economic sanctions (e.g., trade embargoes and economic blockades). Since World War II, however, individual states and international alliances (e.g., the US, the EU, and the UN, a.k.a. the 'senders') have amplified their reliance on sanctions as instruments of coercive diplomacy. They have also shown a preference for 'smart sanctions' (e.g., financial restrictions and travel bans) and a proclivity to impose them on powerful entities and influential elites whose involvement in policymaking is salient. (The recent imposition of sanctions on Russia is a notable case in point.) What's more, there is evidence suggesting that senders have steadily increased their frequency of use of these and other types of sanctions over time.[1]

By their very definition, economic sanctions are potentially punitive measures aiming to alter the course of policymaking in current and/or future 'targets'. A challenging issue in this context – and one that has attracted the attention of numerous scholars, policy analysts, government representatives, and intellectuals – is whether these 'weapons' of choice succeed in achieving their professed objectives.[2] Many analysts share the view that an important criterion for the likelihood of success of (imposing or threatening to impose) sanctions should be on whether they engender compliance.[3] Others contend that assessments on this front will likely bear no fruit if our understanding of the benefits and costs to all sides is incomplete or unquantified.[4] Although we are sympathetic to the

[1]Subsection 3.1 briefly discusses the evolution of sanctions since 1950s. We refer readers to Felbermayr et al. (2020a) and Felbermayr et al. (2021) for detailed discussions.

[2]A burgeoning empirical literature explores the consequences of interstate economic sanctions. Ahn and Ludema (2020) investigate the impact of smart sanctions on targeted firms, and Gutmann et al. (2021) study their consequence on life expectancy and gender gap. See Hufbauer and Jung (2020) and van Bergeijk (2021) for recent characteristics and studies on economic sanctions.

[3]Some researchers (e.g., Pape (1997); Kaempfer and Lowenberg (1988); Haass (1997)) contend that sanctions are likely to fail because of severe enforcement problems, because often they hurt innocent civilians and create incentives to rally behind the flag and, perhaps most notably, because the target's rulers perceive the senders' demands as existential threats to their leadership.

[4]A number of researchers (e.g., Eaton and Engers (1992), Eaton and Engers (1999), Morgan and Schwebach (1997), and Drezner et al. (1999)) emphasized the importance of modeling theoretically and empirically the cost and benefit aspects of sanctions to senders and targets. Numerous other scholars have participated in a lively – and occasionally contentious – exchange of ideas.

former view, in this paper we explore a component of the latter. Specifically, we contribute to the ongoing debate on the efficacy of economic sanctions by studying empirically their impact on targeted countries' growth.

Expectedly, the impact of economic sanctions on GDP growth has attracted the attention of the media, researchers, and policy practitioners. However, while the standard prediction is that sanctions damage economic growth in targets, the extant empirical evidence is mixed. For example, Shin et al. (2016), who set out to study the extent to which sanctions "impair" targeted countries' economies, find that "[a]ll of these variables (US, US case, unilateral, and multilateral sanctions) fail to achieve significance, indicating that none of these sanctions, regardless of the economic indicator, hinders the performance of target economies in a meaningful way" (p. 492). Similarly, focusing on smart sanctions, Rosenberg et al. (2016) report that these sanctions "...are correlated with stronger growth relative to the target's peer economies, though this result is not statistically significant" (p. 18). They then proceed to conclude that "...sanctioned countries do not suffer significant costs as measured by lost economic growth" (p. 15).

In prior research, Hufbauer et al. (2008) also provided assessments of the impact of bilateral and multilateral sanctions on the target countries' GDP. Emphasizing the resultant contraction of their foreign trade and investment flows, they report sizable reductions in these countries' GDPs. More recently, Felbermayr et al. (2020b) study the effects of terminating the complete trade US, EU and UN sanctions on Iran and show, among other things, that Iran's per capita income would rise by about 4.2%. Neuenkirch and Neumeier (2015), also explore econometrically the effects of multilateral UN and unilateral US sanctions on the rate of economic growth in target states. Their findings suggest that, on average, the UN sanctions reduce a targeted country's per capita GDP growth rate by 2.3-3.5%, whereas the corresponding effects of US sanctions are smaller in magnitude and less lasting. As noted above, we, too, contribute to this literature. A key component of our work is that it recognizes and treats an under-appreciated problem: the inherent endogeneity between the imposition of sanctions and their effects on economic growth in target countries.

The notion that the imposition, selection of instrument(s), breath of coverage and duration of sanctions may be endogenous is not unfamiliar to students of political economy. In their survey of the literature on economic sanctions Kaempfer and Lowenberg (2007) recognize this problem and address it theoretically with the help of an interest group model. They also underscore some of the empirical challenges faced in this context. Gutmann et al. (2019) bring the empirical dimension of this problem into sharper focus. In their work on the effects of US sanctions on human rights, they argue poignantly that an important drawback of empirical works is that they "ignore" the "potential endogeneity of economic sanctions" (p. 2).[5]

One may describe the endogeneity problem as follows. Although some sanctions are imposed to change a target country's behavior (as noted above), other sanctions are imposed in response to certain events in targeted countries. Often, some of these events generate macro-economic uncertainty as well as disrupt economic activity. For example, the UN arms embargo on South Africa in 1963 was imposed in response to the country's apartheid regime. Similarly, the comprehensive sanctions on Iraq in 1990 were triggered by that country's invasion of Kuwait. The important problem is that the apartheid regime and the war against Kuwait may have caused a negative spurious correlation between the target country's GDP per capita and the imposition of sanctions. On the other hand, it is possible that some events in targets lead to a positive spurious correlation between the target country's growth and the imposition of sanctions. The independence of Singapore from Malaysia in 1965, and the contemporaneous imposition of sanctions by Indonesia on Singapore might be viewed as an example of this case. From a quantitative perspec-

[5] Gutmann et al. (2019) also deserve credit for addressing this issue by employing an endogenous treatment model. Specifically, they use as instruments the potential target country's geographical and generic distance from the US, as well as its voting alignment with the US in the UN General Assembly (UNGA). Neuenkirch and Neumeier (2015), too, are aware of this problem and address it by adjusting the size of the control sample. In their exploration of the effects of sanctions on poverty, Neuenkirch and Neumeier (2016) consider a strategy that relies on the 'nearest neighbor matching approach."

tive, a related question in this context is whether OLS estimates understate or overstate the effects of sanctions.

We study the effects of economic sanctions on real GDP per capita in target countries with the help of the new edition of the Global Sanctions Database (GSDB), cf. Felbermayr et al. (2020a) and Kirilakha et al. (2021), which contains a comprehensive coverage of sanctions during 1950-2019. We contribute to the empirical literature on the subject in the following ways. First, and foremost, we recognize the inherent endogeneity problem associated with unobserved variables (that confound the outcomes of interest) in targeted countries. To tackle the endogeneity problem, we propose a novel instrumental variable (IV) strategy that capitalizes on sender countries' aggressiveness. Second, we demonstrate that, without addressing endogeneity issue, ordinary least square (OLS) estimates understate the negative impact of sanctions.[6] Finally, we study the importance of various sanction dimensions (e.g., the type of sanctions imposed, their objectives, likelihood of success, and duration) and demonstrate that their effects on growth are very heterogeneous.

We view our proposed instrument and its application to the endogeneity of sanctions problem as our primary methodological contribution to the related literature. For these reasons, a succinct description of our instrument is warranted. We construct and put into use senders' time-varying sanctions count to proxy their aggressiveness. The motivation behind this index is that it is based on systematic variations in the pattern of sanctions which are driven by time-varying sender-specific characteristics and are in no way influenced by the actions of target countries. That is, our instrument relies on some of the most important patterns in the evolution (variation) over time of the frequency of sanctions imposition in our sample (e.g., by the US and the EU) which can be traced to the independent promulgation of certain laws and regulations (e.g., the US International Emergency Economic Powers Act of 1977, and the EU Maastricht Treaty of 1992, respec-

[6] This provides a tentative explanation as to why previous studies, e.g., Shin et al. (2016) and Rosenberg et al. (2016), fail to find the negative effects of sanctions on target nations' growth.

tively). This, we contend, is a valid instrument for the imposition of sanctions in our empirical IV analysis.[7]

Our empirical analysis confirms the above endogeneity challenges and the validity of our IV strategy. Starting with an OLS estimation of the aggregated effect of sanctions on economic growth, we find that this method generates an estimate of 0.14 percent reduction in the contemporaneous level of real GDP per capita due to the imposition of an additional sanction. In contrast, our IV estimation results reveal that an additional economic sanction leads to a 0.39 percent reduction in the contemporaneous level of real GDP per capita, which is more than twice as large in magnitude than the corresponding OLS estimate.[8] We also demonstrate that the sender's aggressiveness is a significant predictor of the bilateral probability of sanctions imposition. Our instrument passes all econometric specification tests.

After establishing that our instrument can be adjusted to match various features of economic sanctions, we quantify the impact of sanctions across the three salient dimensions of the GSDB: sanction type, objective, and success. We find that the effects of sanctions on growth are fairly heterogeneous across types. As one might expect, trade sanctions have the largest negative impact on growth; in contrast, 'smart' sanctions (e.g., financial and travel sanctions) have smaller effects on growth. We also obtain large and significant estimates for arms sanctions and sanctions on military assistance. A possible explanation for these results may be that such sanctions induce target countries to divert significant resources to military buildup and defense. Our analysis of the relevance of sanction objectives suggests that those sanctions that were designed to stop or prevent wars turned out to be most harmful to growth. With regards to various measures of

[7] We also develop a broader IV strategy that capitalizes on prominent approaches from the existing literature – such as the historical and cultural ties instruments of Rajan and Subramanian (2008) and the time-varying distance instrument of Feyrer (2019, 2021). To establish the robustness of our main findings and conclusions, in our empirical analysis we experiment with alternative combinations of our instrument and the instruments used in the extant literature.

[8] In comparison, Neuenkirch and Neumeier (2015) find that US sanctions decrease target states' GDP *growth* by a comparable magnitude (0.75 to 1 percentage point).

success, we find: first, that sanctions that were declared 'total success' had the strongest negative impact on growth in target countries; and, second, that even failed sanctions hurt growth in targets.

Finally, we study the impact of sanctions across different time horizons and find that sanctions have no long-run effects on growth. Specifically, our estimates of the impact on the change in real GDP per capita over time spans of 8 years or longer are not statistically significant. While encouraging, we interpret our finding of no long-run sanction effects on growth with caution. We discuss possible explanations of this finding as well as possible channels through which sanctions may affect long-run growth – which we control for in our econometric specification but do not model explicitly (e.g., through physical capital accumulation) – and conclude by pointing to possible avenues for future research.

The remainder of the paper is organized as follows. Section 2 elaborates on the endogeneity problem associated with the conventional OLS estimation approach and our IV strategy. Section 3 describes our data and sources with a focus on sanctions. Section 4 presents and discusses our estimation results. Section 5 concludes. A supplementary appendix includes descriptive statistics, estimation results, and robustness analysis.

## 2 Identifying the Impact of Sanctions on Growth

As in other international policies (e.g., regional trade agreements) endogeneity may be a serious challenge in identifying the impact of economic sanctions on growth. In Subsection 2.1, we describe this issue and its implications. We also discuss possible sources of endogeneity. Then, in Subsection 2.2, we propose and defend an instrumental variables strategy to address this issue.

## 2.1 The Challenge: Sanction Endogeneity

The challenge with potential endogeneity in identifying the impact of sanctions on growth is fairly standard from an econometric perspective. In addition to summarizing the principal components of this challenge and its implications in the context of sanctions, we use this subsection to introduce some notation and to offer several motivational examples that apply to our current setting. We aim to estimate the impact of the number of sanctions ($S_{jt}$) imposed on target country $j$ in year $t$ on this country's growth, as measured by the change in its real GDP per capita ($Y_{jt}$). To this end, consider the following benchmark OLS specification:

$$\ln(Y_{jt}) = \beta_S S_{jt} + \mathbf{Z_{jt}}\alpha + \Phi_j + \Psi_t + \epsilon_{jt}, \tag{1}$$

In addition to the key variable of interest ($S_{jt}$) in (1), $\mathbf{Z_{jt}}$ is a vector of target country-specific control variables (e.g., physical capital, human capital, etc.), $\Phi_j$ is a set of country fixed effects, $\Psi_t$ is a set of year fixed effects, and $\epsilon_{jt}$ is the error term.

Conditional on $\mathbf{Z_{jt}}$, $\Phi_j$, and $\Psi_t$, estimates of the coefficient of interest $\beta_S$ are likely to be biased because the imposition of sanctions itself may be an endogenous outcome determined by target countries' time-varying characteristics that are not captured in our model; that is,

$$\mathbb{E}(\epsilon_{jt}|S_{jt}, \mathbf{Z_{jt}}, \Phi_j, \Psi_t) \neq 0.$$

While the possibility that $\beta_S$ may be biased is clear and real, it is difficult to predict the direction of the potential bias. To demonstrate this, we offer two sets of opposing examples. The first one is related to the Libyan civil war that lasted from February to October of 2011. During the same period, Libya was targeted with sanctions from the United Nations, the European Union, and the United Stated, among others. In that year, Libya's GDP dropped by over 50%. Based on this, it is conceivable that an OLS regression identifies a spurious correlation between sanctions and the decline of Libya's output that might have been due to the devastating effect of the civil war. As a result, the OLS estimate of the negative impact of sanctions on growth would be biased upward and OLS would predict stronger negative sanction effects than the true effects.[9]

The second example we could consider is related to the formation of Malaysia (including Singapore), due to the Malaysia Agreement (signed in 1963) and the subsequent independence of Singapore from Malaysia. As a result of this agreement, Indonesia sanctioned Malaysia, and its sanction was subsequently extended to include Singapore. Singapore, which became independent from Malaysia in 1965, experienced significant growth, despite the existing sanction from Indonesia. In this example, an OLS regression may under-predict the negative impact of the sanction on Singapore. In fact, a simple OLS regression with year fixed effects implies that Indonesia's sanction actually had a positive effect (+47%) on Singapore's GDP. In this example, sanctions and GDP growth are both correlated with an unobserved event (SGP's political independence from Malaysia), causing a positive bias in the OLS estimates.[10]

The examples described above have two implications for the identification of the impact of sanctions on growth. First, they demonstrate that OLS estimates of the relevant sanction effects may be severely biased. To overcome this issue, in Subsection 2.2, we propose an appropriate instrumental variable strategy. Second, they indicate that the bias could go in either direction, thus leaving the parsing of effects to empirical analysis. In Section 4, we show that, on average, the OLS estimates understates the negative impact of sanctions on growth.

[9] Another example when OLS may over-predict the negative impact of sanctions is the case of the UN sanctions on Liberia in 1992 due to its lingering civil conflict (UNSC Resolution 788). These sanctions took the form of an arms embargo that aimed to end civil conflict. Interestingly, the ECOWAS (Economic Community of West African States) also imposed comprehensive sanctions (specifically, sanctions in the form of trade, financial, travel, and an arms embargo) on Liberia in that same year. Due to the endogneity issue, an OLS regression would be unable to distinguish whether the change in output is due to UN/ECOWAS sanctions or due to civil conflict.

[10] Another example when OLS may over-predict the impact of sanctions is the case of the US sanctions on South Korea during 1973 – 1977 due to human rights violation. South Korea achieved rapid economic growth during the Park Chung-hee administration, which was notorious for violating human rights. As such, sanctions and growth will suffer positive endogeneity bias through the incidence of human rights violation that led to the imposition of sanctions.

## 2.2 The Solution: An IV Strategy

To address the endogeneity problem, we propose an IV strategy that includes a new instrument and capitalizes on notable developments from the existing literature. To fix ideas, let $S_{ijt}$ denote an indicator variable that equals one when a sender country $i$ imposes a sanction on target country $j$ in year $t$. Conceptually, $S_{ijt}$ is a function of sender-specific time-varying characteristics ($s_{it}$), target-specific time-varying characteristics ($\tau_{it}$), and bilateral time-varying characteristics ($b_{ijt}$); that is,

$$S_{ijt}(\cdot) = S_{ijt}(s_{it}, \tau_{jt}, b_{ijt}). \tag{2}$$

The primary concern about endogeneity is that the time-varying country-specific characteristics ($\tau_{jt}$) will affect growth in the target country ($\ln(Y_{jt})$) directly, beyond their correlation with $S_{ijt}$. Therefore, the task that confronts us is to construct an IV that captures the component of $S_{ijt}(\cdot)$ that can be deemed exogenous to a target country's GDP per capita. To this end, we parameterize $S_{ijt}(\cdot)$ as follows:

$$S_{ijt} = \beta_A AGGRSS_{it} + \mathbf{GEOGR_{ijt}}\gamma + \mathbf{HISTORY_{ij}}\alpha + \psi_t. \tag{3}$$

Here, $AGGRSS_{it}$ is a novel index of sender's *aggressiveness* and our main instrument. It is constructed by summing the sanction cases that were imposed by sender $i$ in each year $t$. The idea behind this component of our instrument is that while the sender's aggressiveness may influence the probability that a bilateral sanction will be imposed, it is unlikely that the sender's overall aggressiveness will have a direct effect on the target country's growth. To further mitigate endogeneity concerns with this instrument and at the same time avoid any mechanical contemporaneous correlation, we use two alternatives of our aggressiveness index: (i) its lag – $AGGRSS_{i,t-1}$, and (ii) its contemporaneous values but without the sanction on the specific target – $AGGRSS_{i,-j,t}$. We offer some descriptive statistics related to our aggressiveness index in Section 3.

The second term in equation (3) is the vector $GEOGR_{ijt}$, which includes an indicator variable for contiguity ($CNTG_{ij}$) and a continuous variable for (the log of) bilateral distance ($DIST_{ij}$) between $i$ and $j$. The motivation for the inclusion of these variables is that (exogenous) geography is expected to capture, at least in part, the bilateral economic ties and interdependence between targets and senders. For example, contiguity and distance are two of the most important determinants of international trade (cf. Anderson and van Wincoop (2004)) and foreign direct investment (cf. Eicher et al. (2012) and Blonigen and Piger (2014)). Capitalizing on the recent analysis and intuition of Feyrer (2019, 2021) in the case of international trade, we refine our geography covariates by allowing for time-varying distance effects, $\sum_{i \neq j} \beta_{dt} \mathbf{DIST}_{ijt}$, in order to capture the relative changes in distance-based bilateral costs of economic interaction and interdependence between senders and targets.

The third term in equation (3) is the vector $HISTORY_{ij}$, and it is motivated by the work of Rajan and Subramanian (2008), who implement an IV procedure in which the historical relationships between two countries are an important determinant of bilateral foreign-aid decisions. Following Rajan and Subramanian (2008), we proxy for the historical relationships between the countries in our sample with an indicator variable for common official language ($LANG_{ij}$) and an indicator variable for the presence of colonial relationships ($CLNY_{ij}$) between $i$ and $j$. Finally, motivated by the remarkable increase in the overall number of sanctions over time and during the period of investigation, we also include in (3) a set of year fixed effects $\psi_t$. We offer detailed descriptive statistics on the evolution of sanctions across several dimensions (e.g., by type and objective) in Section 3.

In sum, through (3), we posit that the likelihood of imposing sanctions is driven by exogenous variables including the sender's aggressiveness, a set of pre-determined (historical) country-pair characteristics, and a set of proxies for bilateral geography. Thus, by design, the econometric version of equation (3) should enable us to predict the probability of bilateral sanctions *without* relying on any target-specific factors/traits that may have triggered sanctions and could have influenced the target-country's growth directly. In the empirical analysis, we demonstrate the effectiveness of alternative instruments that only rely on certain components of (3), e.g., without including $GEOGR_{ijt}$ and $HISTORY_{ij}$.

Armed with this analysis and intuition, we implement our IV strategy in three steps. First, at 'stage zero' of our empirical analysis, we estimate the following econometric version of equation (3) using the Probit estimator.[11]

$$\textbf{Stage zero}: \quad S_{ijt} = \beta_A AGGRSS_{it} + \mathbf{GEOGR_{ijt}}\gamma + \mathbf{HISTORY_{ij}}\alpha + \psi_t + \nu_{ijt}, \tag{4}$$

where $\nu_{ijt}$ is an error term.

Second, we sum the predicted bilateral probabilities from the Probit regression to construct our sender-specific instrument $\hat{S}_{jt}$:

$$\textbf{The instrument}: \quad \hat{S}_{jt} \equiv \sum_i \hat{S}_{ijt}. \tag{5}$$

Third, we use the instrument in the implementation of a conventional 2SLS (two staged least squares) IV estimation. In the first-stage, we regress the endogenous variable $S_{jt}$ on our instrument $\hat{S}_{jt}$, along with all other exogenous covariates from the second stage.

$$\textbf{First stage}: \quad S_{jt} = \beta_S^{1st}\hat{S}_{jt} + \Phi_{jt}^{2nd} + \epsilon_{jt}, \tag{6}$$

where $\Phi_{jt}^{2nd}$ is loosely defined to include all exogenous covariates from the following second-stage IV regression:

$$\textbf{Second stage}: \quad \ln(Y_{jt}) = \beta_S \sum_i \omega_{it} S_{ijt} + \mathbf{Z_{jt}}\alpha + \Phi_j + \Psi_{tR} + \epsilon_{jt}. \tag{7}$$

[11] In the sensitivity analysis, we demonstrate the robustness of our results to the use of the Logit estimator and we also demonstrate the inappropriateness of the OLS estimator.

There are several differences between (7) and the benchmark OLS specification (1) described at the beginning of this section. First, we note explicitly that the total number of sanctions ($S_{jt}$) imposed on target $j$ could be constructed as a weighted sum of the sanction cases ($S_{ijt}$) that are levied on $j$ by all individual senders $i$ at time $t$. $\omega_{it}$ is a flexible weight that we use to capture the fact that, for example, depending on the size of the sender, some sanctions could be more 'painful' than others.[12] In addition, (7) is based on a more demanding fixed effects structure, where, in our main specifications, we will allow the time fixed effects $\Psi_{tR}$ to also vary by broader region $R$.

# 3 Data and Sources

To perform the empirical analysis we construct two unbalanced panel datasets, one bilateral (i.e., at the country-pair level) and one country-specific. Both datasets cover the period 1950-2019. To compile them, we relied on several standard data sources, which we describe in detail below. Our bilateral dataset includes 207 sanctioning states (senders) and 182 sanctioned states (targets), and we employ it for the Probit regressions at stage zero of the empirical analysis, whose estimates are used to construct our instrument. The country-specific dataset includes only the target states that received sanctions during the period of investigation, and we rely on it to obtain our main estimates of the impact of sanctions on growth. A list of all sanctioned countries in our sample, along with corresponding sanction-year observations, appears in Table 7. Summary statistics for all the variables we employ appear in Table 8, where Panel A describes our bilateral data and Panel B describes our country-specific variables.

[12] In the empirical analysis, we experiment with alternative weights, including senders' GDPs and a simple unweighted count of the number of sanctions.

### 3.1 Data on Sanctions

Common across both datasets, and most important for our purposes, are the data on sanctions which are extracted from the latest edition of the Global Sanctions DataBase (GSBD), cf. Felbermayr et al. (2020a) and Kirilakha et al. (2021). The GSDB provides case-by-case information on 1,101 publicly traceable sanctions over the period 1950-2019. Several features render this database attractive for our purposes. First, it is the most comprehensive sanctions dataset in that it attempts to cover the universe of sanction cases. Second, the GSDB includes information on senders, targets, the year of sanction imposition, and the year of sanction removal/lifting. We will utilize all of this information in our analysis. Third, the GSDB has a very long time coverage, which can be very beneficial to our country-specific analysis. Finally, we will capitalize on the fact that the GSDB classifies sanctions across three main dimensions (including type, objective, and success) in order to study the heterogeneous effects of sanctions.

Figure 1, which is replicated from Kirilakha et al. (2021), depicts the evolution of sanctions across the three dimensions noted above. Panel (a) traces the overall evolution of sanctions as well as their evolution across the six sanction types that are available in the GSDB (i.e., trade, financial, travel, arms, military assistance, and other). Two clear patterns stand out. First, Panel (a) captures the remarkable increase in the overall use of sanctions between 1950 and 2019. This may explain the ever increasing interest in the effects of sanctions, including our interest in studying the impact of sanctions on growth. Second, we notice a shift in the relative shares of sanctions over time. Specifically, Panel (a) captures an increase in the share of financial and travel sanctions since the 90s. Consistent with some related literature (e.g., Cortright and Lopez (2002); Drezner (2011); Rosenberg et al. (2016)) we sometimes refer to financial and travel sanctions in one category of 'smart' sanctions.

Panel (b) of Figure 1 decomposes the overall evolution of sanctions by sanction objective. Specifically, the GSDB distinguishes between nine categories of sanction objectives including policy change, destabilize regime, resolve territorial conflict, prevent war, fight terrorism, end war, improve human rights, promote and restore democracy, and other objectives. Similar to the evolution of sanctions by type, we see from Panel (b) that the relative shares of sanctions with different objectives have changed over time as well. Specifically, the graph reveals an increase in the share of sanctions with objectives to improve human rights, to restore and promote of democracy, and to fight terrorism, while the share of sanctions that pursue regime destabilization has steadily fallen over time. This motivates our analysis of the heterogeneous effects of sanctions on growth by objective.

Finally, Panel (c) of Figure 1 depicts the evolution of sanctions depending on whether they were successful in achieving their political objectives. The GSDB distinguishes between four types of sanctions depending on their success including: total success, partial success, settlement by negotiations, failure. The findings in Panel (c) should be interpreted with caution, subject to two caveats. First, the classification of sanctions according to their success is somewhat subjective, even though it is based on data from official and publicly available documents, cf. Felbermayr et al. (2020a). Second, the figure omits the category of ongoing sanctions, cf. Kirilakha et al. (2021). Despite these limitations, two clear patterns stand out in Panel (c). First, consistent with the existing literature (cf. Pape (1997), Hufbauer et al. (2007), Bapat and Morgan (2009), and Felbermayr et al. (2021)), the figure reveals that relatively few sanctions (i.e., on average, only about 30% to 40% of all sanctions) have been successful in achieving their objectives. Second, Panel (c) shows that the share of successful sanctions has increased over time, thus motivating our empirical analysis of the heterogeneous effects of sanctions on growth depending on the degree of sanction success.

## 3.2 Motivating Our Instrument

In addition to capitalizing on the rich dimensionality of the GSDB in order to study the heterogeneous effects of sanctions, we use this database to construct the key component of our instrument, namely our time-varying sender-specific *aggressiveness* index. The objective of this section it to motivate and rationalize our IV strategy. To this end, we first offer some summary statistics and then supplement these statistics with a discussion of the institutional background behind the rise in the use of sanctions by two key sanction senders: the United Sates (US) and the European Union (EU).

Table 1 reports statistics on the variation in the number of sanctions imposed by the top 10 senders in the world during 1950-2019. Specifically, the absolute numbers and corresponding shares shown there are constructed on the basis of the total number of sanctions imposed by each country in our sample during the period of investigation. The two main messages from this table may be described as follows. First, there is significant variation in the number of sanctions imposed by individual countries, even among the top-10 most 'aggressive' senders. Naturally, such cross-section variation is welcome for our identification purposes. Second, the majority of sanctions are imposed by the United States (close to 37%) and the European Union (close to 14%). We capitalize on this observation next, when we zoom in on some specific reasons behind the prominent use of sanctions by these specific countries.

Motivated by the results in Table 1, Panel (a) of Figure 2 plots the evolution of the number of sanctions that were unilaterally imposed by the US. The figure reveals that this number varies significantly during the period of investigation. Most important for our purposes, we detect the appearance of a structural break in 1977 that is captured by the green vs. red dashed lines depicting the average number of sanctions before and after 1977, respectively.[13] As documented in the literature, cf. Hufbauer et al. (2008), the reason

[13]Before 1977, a small peak in sanctions emerged around 1962-63 that was driven by the crisis in Cuba. During that period, the US initiated three sanction cases against Cuba.

for the significant increase in the number of US sanctions post 1977 is the passing of the International Emergency Economic Powers Act (IEEPA) during that year, which provided the US president with broad authority to regulate a variety of economic transactions following a declaration of national emergency (Casey et al., 2019). Hufbauer (1998) notes that IEEPA became the 'all-purpose' statute for US sanctions and that, when IEEPA was enacted, the frequent use of economic sanctions by the US fell at odds with customary international law at the time.[14]

In the post-IEEPA era, there is still some year-to-year variation driven by geopolitical changes. For instance, in the final years of Cold War, we find a significant increase in the number of US sanctions. Not surprisingly, target countries from 1989 to 1993 include Azerbaijan, the Soviet Union and Yugoslavia. This is accompanied by two separate cases related to China (1989 and 1993) following the Tiananmen Square incident. Around the turn of the 21st century, we observe a decline in the number of US sanctions. A possible explanation for this is that toward the end of the 1990s, scholars and policymakers were frustrated by the (lack of) effectiveness of past sanctions, cf. Pape (1997).[15] Some would even argue that *"(a) a powerful motivation behind the 2003 (US) invasion of Iraq was the widespread, albeit mistaken, belief that the UN sanctions regime had failed."* (Rosenberg et al., 2016).[16]

[14]The key role played by IEEPA in US sanctions is also evidenced by US executive order documents of economic sanctions, where we find that documents in recent times share the following paragraph: *"By the authority vested in me as President by the Constitution and the laws of the United States of America, including the International Emergency Economic Powers Act (50 U.S.C. 1701 et seq.) (IEEPA), the National Emergencies Act (50 U.S.C. 1601 et seq.), section 212(f) of the Immigration and Nationality Act of 1952, as amended (8 U.S.C. 1182(f)), and section 301 of title 3, United States Code, I, BARACK OBAMA, President of the United States of America, hereby find that ..."* (Executive Order 13608).

[15]Widely cited examples include the failed attempts to: force Iraq out of Kuwait, to topple the Haitian military, and to punish China for human rights abuses, among others.

[16]We note that large year-to-year variations still exist after 2001. There was a peak in 2006 that is not well explained by institutional reasons. In 2006, the US imposed sanctions against: Fiji (twice) for the military coup there, Belarus for undermining democratic institutions, and Venezuela for terrorism, among others. There was another peak in 2011-12 against Libya, Mali and Yemen associated with the Arab Spring movement. However, there was also a contemporaneous increase in sanctions during 2011-12 against countries not associated with the Arab Spring (such as Belize, Indonesia, Guinea-Bissau, Guatemala and Moldova), perhaps reflecting a political climate at the time that displayed a high propensity to use economics sanctions.

According to Table 1, the European Union is the second most active sender of economic sanctions. Against this backdrop, Panel (b) of Figure 2 displays the evolution of the number of EU sanctions during the period of investigation. We discern a structural break in the data in the early 90s. Specifically, prior to 1992, the EU issued only a limited number of sanctions through the European Economic Community (EEC),[17] while post 1992 the number of EU sanctions increased significantly. The observation of a discrete increase in the number of EU sanctions post 1992 is reinforced by the green vs. red dashed lines that depict the average number of sanctions before and after 1992, respectively. The primary reason for the sharp change is the Maastricht Treaty (a.k.a. Treaty on European Union), which led to the creation of EU's Common Foreign and Security Policy (CFSP) singed in 1992.[18] Since then, the imposition of EU sanctions fell under the domain of CFSP, and sanctions have been part of regular EU foreign policy (Giumelli and Ivan, 2013). As illustrated in Panel (b) of Figure 2, the number of EU sanctions increased significantly since the early 90s.

The key takeaway from the analysis in this section is that some of the most important patterns in the evolution (variation) over time in the frequency of sanction use by the main sanctioning states in our sample (e.g., the US and the EU) can be traced to the promulgation of regulations and laws (e.g., the International Emergency Economic Powers Act and the Maastricht Treaty, respectively). In other words, this discussion demonstrates that there are systematic variations in the pattern of sanctions driven by time-varying sender-specific characteristics that can be deemed orthogonal to the actions of (generally small) target countries. This motivates and rationalizes the use of the proposed aggressiveness index as a valid instrument for the imposition of sanctions in our empirical IV analysis in Section 4, where we also demonstrate its effectiveness and validity from an econometric

[17] These include two sanctions against South Africa (in 1985 and 1986 for Apartheid) and one sanction against China (1989, Tiananmen).

[18] According to Article J.1 of title V of the Maastricht Treaty, the primary goal of CFSP is to safeguard the common values and fundamental interests under the principles of the UN Charter: to preserve peace, strengthen international security and consolidate democracy, the rule of law and respect for human rights.

perspective.

### 3.3 Other Data and Sources

In addition to data on sanctions, we rely on data from several other sources. To obtain our Probit estimates of the probability of sanction impositions at stage zero, we rely on data from the USITC's Dynamic Gravity Database (DGD), cf. Gurevich and Herman (2018). We use DGD to obtain the variables on bilateral distance, contiguous borders, regions, hostility, and colonial relationships. The dataset used to complete our country-specific data for the main analysis is the Penn World Tables, cf. Feenstra et al. (2015). The variables that we use from the Penn World Tables include real GDP per capita, physical capital, population, trade-openness, TFP, and human capital.

## 4 Estimation Results and Analysis

We present our findings in three steps, which correspond to the subsections of this section. First, in Subsection 4.1, we discuss our main results regarding the impact of sanctions on growth, We also offer evidence for the validity of our instrument, and we present our findings in a series of sensitivity experiments. Then, in Subsection 4.2, we explore the heterogeneous effects of sanctions on growth across various dimensions. Finally, also in Subsection 4.2, we evaluate and compare the short- vs. long-run impact of sanctions and their effects across different periods of time.

### 4.1 Main Results: Sanctions Hurt Growth

Our main results appear in Table 2, which includes 3 panels and 11 columns. Panel A reports our stage-zero regression results, where we model the bilateral probability of sanction imposition. Panel B reports our first-stage IV estimates, where we instrument for the total number of sanctions imposed. Panel C presents our main findings on the impact of sanctions on growth. The estimates in column (1) are obtained with the OLS estimator. Our preferred IV results appear in column (2). The estimates in columns (3)-(5) are based on three alternatives sets of instruments. The results in columns (6)-(8) are based on three alternative estimators for the stage-zero analysis. In columns (9)-(11) we experiment with alternative sets of control variables. The standard errors in panel A are heteroskedasticity robust, while the standard errors in panels B and C are heteroskedasticity robust and clustered at the country level. Finally, we note that, to facilitate exposition and ease interpretation, the coefficient estimate (and its standard error) for $SANCT$ in Table 2 is multiplied by 100.

We start the empirical analysis with the OLS estimates from column (1) of Table 2, where, in addition to the country fixed effects ($\Phi_j$) and the year-region fixed effects ($\Psi_{tR}$), we control for physical capital and human capital. The main message from the estimates in column (1) is that economic sanctions have a negative impact on contemporaneous real income. This is captured by the negative and highly statistically significant estimate (-0.141 std.err. 0.036) on the coefficient of $S_{jt}$, suggesting that an additional sanction is associated with 0.14 percent reduction in the target's real GDP per capita. We find this result to be intuitive and consistent with corresponding findings from the existing literature which, in combination with the positive, sizable and significant estimates on physical and human capital, is encouraging for the representativeness of our estimating sample. While we find the OLS estimate of the impact of sanctions on growth from column (1) plausible, we believe that it may be biased due to the endogeneity concerns we discussed earlier. Therefore, next we implement our IV strategy.

Our preferred results are presented in column (2) of Table 2. Following our identification strategy, we present three sets of results. In Panel A we report the estimates from stage zero of our analysis, where we model the bilateral probability for imposition of sanctions. Most importantly, we find that the estimate on our sender's aggressiveness index is positive and highly statistically significant, thus confirming our conjecture that sender's aggressiveness is an important determinant of the probability for sanctions. The estimates on the rest of the covariates in from Panel A are also intuitive. Thus, for example, we obtain negative estimates on the impact of language and colonial relationships, which suggest that countries that have stronger cultural and historical ties are less likely to sanction each other. This result is consistent with the findings from Rajan and Subramanian (2008), regarding the importance of historical relationships for bilateral foreign-aid decisions. The estimate on the dummy variable for common borders is positive and significant, reflecting the fact that very often (e.g., due to territorial arguments) neighboring countries are more likely to sanction each other.

In addition to the estimates we report in Table 2, the underlying specification we use to obtain the results in column (2) includes two other sets of covariates (the estimates of the time-varying distance variables and the year fixed effects), whose traits can be visualized in Figure 3. Specifically, Panel (a) of Figure 3 plots the estimates of the time-varying distance variables from our model which, due to perfect collinearity, are obtained relative to 1950. Two key findings emerge. First, we notice significant variation over time, which is important for our purposes from an econometric perspective. Second, from an economic/policy perspective, our estimates reveal that the incidence of sanctions decays with bilateral distance.[19] In Panel (b) of Figure 3 we plot the estimates of the year fixed effects in our model. Consistent with our expectations (and with our descriptive analysis in Section 3.1), these estimates clearly capture the point that the probability of using sanctions as policy tools has been trending upwards.

Next, we turn to the baseline first-stage estimates in column (2) of Panel B in Table 2, where we establish the validity of our instrument, as defined in equation (6). Three principal findings stand out from Panel B. First, the estimate on the coefficient of our instrument is large in magnitude and highly statistically significant. Second, our estimate is

[19]We also note that, without the interaction with the year fixed effects, the coefficient on the log of distance is negative and statistically significant (-0.263, std.err. 0.063), suggesting that, on average, countries that are located far away from each other sanction each other less. We find this result intuitive.

not statistically distinguishable from one. This suggests that a unit increase in our exogenous instrument variable is associated with a count increase in the number of sanctions. Third, the Kleibergen-Paap F-statistic reported at the bottom of Panel B is higher than the conventional levels suggested by Stock and Yogo (2002).

Since our first-stage estimates in Panel B of Table 2 are obtained with fixed effects, we could not interpret meaningfully the estimate of the constant term. Therefore, to address this problem, we obtained corresponding estimates without the fixed effects. The results (not provided in the table) offered further support for our instrument because: (i) the constant term was economically small and not statistically different from zero (0.006 std.err. 0.042); and (ii) the estimate on our instrument was not statistically different from one (0.966 std.err. 0.142). This further confirms the one-for-one relation between our instrument variable and the endogenous regressor without recourse to a constant term. In sum, the results in Panel B of Table 2 and the additional analysis we performed suggest that our instrument can predict the incidence of sanctions on target countries very well.

Our main IV estimates are reported in Panel C of Table 2. Three main findings can be discerned upon inspection of this panel. First, the estimates on the two main control variables (physical and human capital) remain unchanged. Second, and more important for our purposes, the estimate of the effect of sanctions on growth remains negative and highly statistically significant, thereby reinforcing our earlier conclusion that economic sanctions hurt contemporaneous economic growth. Third, the estimated IV coefficient of the impact of sanctions is more than twice as large in absolute value than the corresponding OLS estimate in column (1). Specifically, our IV estimate suggests that an additional sanction is associated with 0.39 percent reduction in the target's real GDP per capita. This result has two concrete implications: (i) it confirms our suspicion that the OLS estimate was biased and reinforces the need for an IV treatment; and (ii) it reveals that the OLS estimate under-predicts the negative impact of sanctions on growth in target states, thus suggesting that, on average, the imposition of sanctions may have coincided with factors that have stimulated growth in such states.

We conclude the analysis in this section with a series of robustness checks, which we group in three broad sets of results. Columns (3)-(5) offer estimates that are obtained on the basis of three alternatives for the instrument we use. Most important for us from a methodological perspective, the estimates in column (3) are obtained with an instrument that only relies on our aggressiveness index and a set of year fixed effects. A comparison between the corresponding results of columns (2) and (3) from each panel reveals that they are not statistically different from each other. This confirms the validity of our novel instrument. To obtain the estimates in columns (4) and (5), we combine our aggressiveness index with the proxies for historical ties (i.e., *a la* Rajan and Subramanian (2008)) and with the time-varying distance instruments (i.e., *a la* Feyrer (2019, 2021)), respectively. In each case, the estimates of the impact of sanctions on growth remain very similar to our main estimate from column (2).[20]

The results in columns (6)-(8) are based on three alternative estimators for the stage-zero analysis. Two main findings stand out. First, we note that the estimates that rely on the Logit estimator, in column (6), and those that use the Poisson Pseudo Maximum Likelihood (PPML) estimator, in column (7), deliver estimates that are very similar to our main results. However, the results from column (8), which rely on OLS to capture the probability of bilateral sanctions, are not reliable – as expected. This is reflected in the insignificant estimates both in Panels B and C of Table 2.

Finally, the last set of robustness experiments are obtained with the use of alternative covariates for our main IV specification. Specifically, the estimates in column (9) are obtained with our main regressor but without any additional covariates except the two sets

[20]We do note, however, that the results in each panel of column (4), where we rely on the combination of the aggressiveness index and historical proxies but do not include the proxies for time-varying geography, are generally inflated. This may be due to the fact that the only time-varying regressor in stage zero of column (4) is sender aggressiveness, which absorbs most of the variation in the dependent variable (as suggested by an inflated coefficient for sender aggressiveness). This, in turn, is the cause for reduced variation in the instrumental variable, an inflated first-stage coefficient, and a slightly inflated IV regression coefficient. However, it is noteworthy that the first-stage F-statistic improves in this case.

of (year-region and country) fixed effects. In addition to the main sets of fixed effects and the covariates for human and physical capital, the results in column (10) are obtained after adding control variables for trade openness, WTO membership, EU membership and an index for hostility. Finally, the estimates in column (11) are obtained after replacing the country and the year-region fixed effects with country and year fixed effects. While the results from Panel C are not statistically different from our main estimate in column (2), we note that the estimates are larger when we include fewer covariates, i.e., in columns (9) and (11), and only slightly smaller when we add more covariates, i.e., in column (10). We view this as an indication that our main set of covariates performs well.

We draw two conclusions based on our analysis in this section. First, from a policy perspective, our analysis demonstrates that, on average, sanctions are an important impediment to contemporaneous economic growth. Specifically, our main estimates suggest that an additional sanction would lead to a 0.39 percent reduction in the target's real GDP per capita. Second, from a methodological perspective, the analysis demonstrates that the OLS estimates of the effects of sanctions on growth may be significantly biased, i.e., by more than 100 percent. Moreover, on a related note, our results reveal that, on average, OLS tends to under-predict the impact of sanctions on growth.

## 4.2 On the Heterogeneous Effects of Sanctions

Motivated by some of the patterns we discovered in the descriptive analysis in Subection 3.1 and utilizing the various dimensions of the sanctions data from the GSDB, in this subsection we study the heterogeneous effects of sanctions. Each of the specifications we employ is based on our main econometric model from column (2) of Table 2. The primary difference is that, for each of the specifications in this section, we construct a separate instrument that is based on information specific to the particular dimension we investigate. For example, to examine the impact of trade sanctions we construct an aggressiveness index that is based only on information related to trade sanctions. Intuitively, the idea is for our IV construction procedure to account for the fact that different types of sanctions have experienced heterogeneous trends that vary by senders and time.

We start by exploring the differential impact of sanctions on real GDP per capita depending on the type of sanctions considered. Our findings are presented in Table 3 where, for ease of comparison, in column (1) we reproduce our main estimates from column (2) of Table 2. Columns (2) through (7) of Table 3 replicate our main specification for each type of sanction, namely trade sanctions in column (2), financial sanctions in column (3), travel sanctions in column (4), arms sanctions in column (5), military assistance sanctions in column (6), and other sanctions in column (7). The specifications used to obtain the results in each column preserve all features from our main specification, and the panel structure of Table 3 is the same as the structure of Table 2.

The stage-zero estimates from Panel A of Table 3 reveal that aggressiveness by type is a very strong predictor of the bilateral probability of imposing each type of sanctions. Without exception, all estimates of the coefficients on $AGGRSV$ are positive, sizable, and statistically significant.[21] Our first-stage estimates confirm the validity of our instruments for each type of sanction. Specifically, the estimates on $\hat{S}_{j,t}$ are positive, economically close to one and, in all cases, not statistically different from one. Moreover, all Kleibergen-Paap F-statistics reported at the bottom of Panel B are significantly higher than the conventional levels suggested by Stock and Yogo (2002).

Turning to the main estimates of the impact of sanction on growth from Panel C, we see that they are quite heterogeneous. The estimate of the impact of trade sanctions is among the largest in absolute value. We find this result intuitive and expected because trade sanctions affect adversely both producers and consumes in target countries. The estimate on financial sanctions is also negative and statistically significant but smaller than the one for trade sanctions. A possible explanation for this result is that, by design,

[21] Overall, while heterogeneous, the estimates on the rest of the covariates in Panel A are also consistent with our main findings. Figures with the estimates of the year fixed effects for each type of sanction are included in the Appendix.

financial sanctions are 'smart' precisely because they target specific entities and aim to inflict on them rather than the economy as a whole. This intuition is reinforced by the economically small and statistically insignificant estimates on travel sanctions and other sanctions. Finally, we obtain large and significant estimates for arms sanctions and military assistance. A possible explanation for these results could be that, in the presence of such sanctions, significant domestic resources may need to be diverted toward the military sector for national defense. Overall, the figures in Table 3 suggest that the average estimates of the impact of sanctions may hide significant heterogeneity depending on the type of sanction considered.

The results in Panel D of Table 3 are obtained with our IV procedure, however, after assigning weights to the key covariate. Specifically, instead of using a simple count of sanctions, we now define this variable as the product between the number of sanctions imposed and the share of world GDP of the senders that impose these sanctions. The idea behind this specification is to allow for the possibility that the same number of sanctions can be more damaging if imposed by large senders. For brevity, we omit the stage-zero and first-stage results and we only report our main IV estimates for each sanction type. Comparison between the results in Panel D and Panel C reveals that, consistent with our expectations, the weighted estimates are slightly larger in magnitude; that is, the negative impact of sanctions is increasing in the combined size of the senders that impose them. However, they are not statistically different from those based on a simple count.

Next, we look for heterogeneous effects of sanctions on growth depending on the declared sanction objective. Our findings are presented in Table 4 where, once again, in column (1) we include our main estimates from column (2) of Table 2. Columns (2) through (10), reproduce our main specification for sanctions depending on their objectives; that is, policy change in column (2), destabilize regime in column (3), resolve territorial conflict in column (4), prevent war in column (5), fight terrorism in column (6), end war in column (7), improve human rights in column (8), promote and restore democracy in column

(9), and other objectives in column (10). The specifications used to obtain the results in each column of Table 4 preserve all features from our main specification, and the panel structure of the table is the same as the structure of Table 2.

Similar to our main findings and to the results on the effects of sanctions by type, the estimates from panels A and B of Table 4 reveal that: (i) aggressiveness is a significant predictor of the bilateral probability of imposing sanctions with specific objectives; and (ii) that our instrument performs well for sanctions depending on their objectives. The only exception involves sanctions with objectives to destabilize regime. Our explanation for the peculiar results in this category is the small number of observations in combination with the very small and decreasing share of such sanctions in the post-1990 period. (See Figure 1.) Overall, we notice that the estimates in Table 4 are less significant as compared to their counterparts from Tables 2 and 3. The two significant estimates are for sanctions with objectives to stop or prevent wars. These are also two of the more sizable estimates. The other sizable (but not statistically significant) estimate we obtain is for sanctions aiming to address human rights abuses and the resolution of territorial conflict. Interestingly, we obtain very small estimates on sanctions targeting policy change and terrorism.

Table 5 reports four estimates of the impact of sanctions on growth depending on the declared sanction objective. As before, in column (1) we include our main estimates from column (2) of Table 2. Columns (2) through (5), reproduce our main specification for sanctions depending on their success, namely failed sanctions in column (2), sanctions with settlement by negotiations in column (3), total success in column (4), and partial success in column (5). We draw three main conclusions from the estimates in Table 5. First, sanctions that were deemed total success had the strongest negative impact on growth. We find this result intuitive. Second, we find that failed sanctions, too, hurt growth in target countries. There could be a number of explanations for this result, including political economy arguments. Finally, we do not obtain significant estimates for the effects for sanctions in settlement and negotiation and for sanctions with partial success.

We conclude the analysis in this subsection by investigating the medium and long-term effects of sanctions on growth. To this end, we amend our main econometric model to adopt the following log-difference regression specification:

$$\Delta_T \ln(\mathrm{Y}_{jt}) = \sum_{n=t-T+1}^{t} S_{jn} + \mathbf{\Delta_T Z_{jt}}\alpha + \Psi_{tR} + \epsilon_{jt}, \tag{8}$$

where $\Delta_T \ln(\mathrm{Y}_{jt})$ is the change in real GDP per capita in country $j$ from year $t-T$ to $t$ $(= \ln \mathrm{Y}_{jt} - \ln \mathrm{Y}_{j(t-T)})$, the key regressor of interest $\sum_{n=t-T+1}^{t} S_{jn}$ denotes the total number of sanctions that the country received during the period from $t-T+1$ to $t$,[22] $\mathbf{\Delta_T Z_{jt}}$ is the vector of changes of the control variables (i.e., physical and human capital) during the same period, $\Psi_{tR}$ denotes the region-time fixed effects, and $\epsilon_{jt}$ is the error term.[23] We choose $t$s that are divisible by $T$ to avoid representing data from a given year for multiple times in the regression. Finally, to address the endogeneity problem, we capitalize on the flexibility of our instrument, which we construct as the cumulative number of sanctions with $\sum_{n=t-T+1}^{t} \hat{S}_{jn}$ that correspond to each period of interest.

Our findings are presented in Table 6 where, as before, we include our main results in column (1) for comparison purposes. The results in columns (2), (3), and (4) are for 4-year, 8-year, and 12-year intervals respectively. Without going into detail, we note that the estimates from panels B and C are consistent with our previous findings. The main conclusion from Table 6 is that the negative impact of sanctions on growth vanishes over time. This is captured by the fact that the IV estimates on $SANCT$ in Panel C decrease in magnitude and lose statistical significance with the increase in the time horizon under investigation. However, we caution the reader that our result of no long-run effects of sanctions must be interpreted with care. Thus, for example, it is possible that sanctions may still impact long-term growth through physical capital, which we control for explic-

[22] Thus, for example, when we set $T = 10$, the regression specification investigates how changes in the total number of sanctions in a decade (e.g., from 1991 to 2000) affect the 10-year change in GDP per capita (i.e., the difference between 2000 and 1990).

[23] We note that, due to the differencing, the country fixed effects from our main specification drop out.

itly in our regressions in Table 6. In addition, it is possible that different types of sanctions (e.g., trade vs. smart vs. arms sanctions) may affect long-run growth differentially. We leave these policy-relevant questions for further research.

# 5 Concluding Remarks

To study the impact of sanctions on economic growth of target countries, in this paper we propose a novel IV strategy that addresses the endogeneity problem that arises from target-country specific characteristics (e.g., civil war or political independence). Our key methodological contribution is a new instrument that captures the sanction aggressiveness of sender countries. The idea behind our instrument is that some of the most important patterns in the evolution (variation) over time of the frequency of sanctions imposition by the main sanctioning states in our sample (e.g., the US and the EU) can be traced to the promulgation of regulations and laws (e.g., the US International Emergency Economic Powers Act of 1977 and the EU Maastricht Treaty of 1992, respectively). In other words, we identify systematic variations in the pattern of sanctions driven by time-varying sender-specific characteristics that are exogenous from the perspective of each target country.

We implement our econometric strategy with the help of the latest edition of the Global Sanctions Database (GSDB), cf. Kirilakha et al. (2021), which enables us to identify the average effect of sanctions on growth and, additionally, to capitalize on the flexibility of our instrument to distinguish between the effects of sanctions by type, objective, and success. Our empirical analysis confirms the bias in standard OLS estimates and the validity of the proposed instrument. Our estimates reveal that an additional economic sanction leads to 0.39 percent reduction in the contemporaneous level of GDP per capita, which is more than double the size of the corresponding OLS estimate.

We also obtain very heterogeneous effects of sanctions across various dimensions.

Specifically, we find that trade sanctions have the largest negative impact on growth, while 'smart' sanctions, too, have negative but smaller effects. Based on their objectives, the sanctions that hurt growth the most are the ones that are designed to stop or prevent wars. We also find that sanctions that declared total success had the strongest negative impact on growth, but also that failed sanctions hurt growth in target countries. Finally, we do not find significant sanction effects on long-term growth on average. However, we do not rule out the possibility that sanctions may impact growth through channels that are outside our model. In combination with our findings, the flexibility of our IV strategy opens new avenues and points to possibilities for further research.

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# Tables and Figures

Table 1: Top Senders of Sanctions

| Sender | Sanctions | Percentage |
|---|---|---|
| United States | 366 | 36.59% |
| European Union | 137 | 13.69% |
| United Nation | 82 | 8.19% |
| Canada | 47 | 4.69% |
| Japan | 37 | 3.70% |
| Australia | 34 | 3.40% |
| Switzerland | 33 | 3.29% |
| Russia | 30 | 3.00% |
| League of Arab States | 17 | 1.70% |
| India | 14 | 1.39% |

**Notes:** This table shows the number and share of sanctions imposed by the top ten senders during the period between 1950 and 2019. Data: the Global Sanctions DataBase (GSDB).

Table 2: Main Results: Sanctions and Growth, 1950-2019

| VARIABLES | (1) OLS | (2) Main IV | (3) Ag. Year | (4) Ag. His. | (5) Ag. Geo. | (6) Logit Zero | (7) Poi. Zero | (8) OLS Zero | (9) No Ctrl. | (10) Full Ctrl. | (11) Year FE |
|---|---|---|---|---|---|---|---|---|---|---|---|
| *Panel A. Stage Zero: Sanction Probability* | | | | | | | | | | | |
| AGGRSV | | 1.106*** | 1.120*** | 2.521*** | 1.164*** | 1.768*** | 0.880*** | 0.369*** | | *same as column (2)* | |
| | | (0.020) | (0.020) | (0.022) | (0.020) | (0.033) | (0.012) | (0.006) | | | |
| LANG | | -0.087*** | | -0.068*** | | -0.135*** | -0.097*** | -0.023*** | | | |
| | | (0.004) | | (0.004) | | (0.008) | (0.005) | (0.001) | | | |
| CLNY | | -0.160*** | | -0.261*** | | -0.274*** | -0.160*** | -0.048*** | | | |
| | | (0.018) | | (0.017) | | (0.030) | (0.021) | (0.005) | | | |
| CNTG | | 0.111*** | | | 0.061*** | 0.180*** | 0.106*** | 0.031*** | | | |
| | | (0.013) | | | (0.013) | (0.023) | (0.014) | (0.004) | | | |
| Observations | | 442,578 | 442,578 | 442,578 | 442,578 | 442,578 | 442,578 | 442,578 | | | |
| *Panel B. First Stage* | | | | | | | | | | | |
| $\hat{S}_{jt}$ | | 1.143*** | 1.094*** | 2.054*** | 1.097*** | 1.146*** | 1.141*** | -0.157 | 1.180*** | 1.190*** | 1.191*** |
| | | (0.316) | (0.324) | (0.552) | (0.329) | (0.317) | (0.317) | (0.213) | (0.278) | (0.369) | (0.304) |
| Observations | | 6,480 | 6,480 | 6,480 | 6,480 | 6,480 | 6,480 | 6,480 | 7,765 | 5,531 | 6,490 |
| $R^2$ | | 0.463 | 0.460 | 0.461 | 0.459 | 0.463 | 0.462 | 0.410 | 0.492 | 0.491 | 0.441 |
| First Stage F | | 13.10 | 11.40 | 13.85 | 11.09 | 13.12 | 12.94 | 0.54 | 18.07 | 10.40 | 15.32 |
| $p$-value | | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.46 | 0.00 | 0.00 | 0.00 |
| *Panel C. Main Regression* | | | | | | | | | | | |
| SANCT | -0.141*** | -0.390** | -0.400** | -0.461** | -0.401** | -0.391** | -0.392** | -0.689 | -0.529** | -0.383* | -0.672*** |
| | (0.0358) | (0.190) | (0.197) | (0.203) | (0.198) | (0.190) | (0.192) | (0.957) | (0.231) | (0.206) | (0.192) |
| ln(capital) | 0.258*** | 0.265*** | 0.266*** | 0.268*** | 0.266*** | 0.265*** | 0.265*** | 0.274*** | | 0.263*** | 0.239*** |
| | (0.0364) | (0.037) | (0.037) | (0.038) | (0.037) | (0.037) | (0.037) | (0.055) | | (0.039) | (0.045) |
| human capital | 0.322** | 0.290** | 0.289** | 0.281** | 0.288** | 0.290** | 0.290** | 0.252 | | 0.376** | 0.554*** |
| | (0.138) | (0.138) | (0.139) | (0.140) | (0.139) | (0.138) | (0.138) | (0.192) | | (0.161) | (0.173) |
| Observations | 6,480 | 6,480 | 6,480 | 6,480 | 6,480 | 6,480 | 6,480 | 6,480 | 7,765 | 5,531 | 6,490 |
| $R^2$ | 0.985 | | | | | | | | | | |

**Notes:** This table reports a series of estimates of the impact of sanctions on growth. Panel A reports our stage-zero regression results, where the dependent variable is a bilateral indicator variable for sanctions. All estimates in Panel A except for column (4) are obtained with year fixed effects. Panel B tests the validity of our instruments from specifications where the dependent variable is the total number of sanctions imposed on a country. It also reports Kleibergen-Paap F-statistics for the instrument variable and, in parentheses, corresponding $p$-value under the null hypothesis that the coefficient of our instrument is zero. Panel C reports our main estimates of the impact of sanctions on growth, where the dependent variable is real GDP per capita. Column (1) reports OLS results. Our preferred IV specification is in column (2). In columns (3)-(5), we experiment with alternative instruments. Columns (6)-(8) use different estimators (logit, poisson, and OLS, respectively) at stage zero. Finally, in columns (9)-(11) we experiment with alternative covariates in the second-stage IV specifications. Specifically, column (9) only includes the number of sanctions, column (10) additionally includes trade openness, WTO membership, EU membership and a proxy for hostility, and column (10) includes country and year fixed effects. The estimates in all other columns are obtained with country and region-year two-way fixed effects. The standard errors in panel A are heteroskedasticity robust, and the standard errors in panels B and C are heteroskedasticity robust and clustered at the country level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. See text for further details.

Table 3: Sanctions and Growth by Type of Sanction, 1950-2019

| VARIABLES | (1) ANY | (2) TRADE | (3) FINCL | (4) TRAVL | (5) ARMS | (6) MLTRY | (7) OTHER |
|---|---|---|---|---|---|---|---|
| *Panel A. Stage Zero: Sanction Probability (Probit)* | | | | | | | |
| AGGRSV | 1.106*** | 1.129*** | 0.791*** | 0.528*** | 0.618*** | 0.305*** | 0.307*** |
| | (0.020) | (0.028) | (0.027) | (0.032) | (0.029) | (0.023) | (0.040) |
| LANG | -0.087*** | -0.182*** | -0.005 | 0.084*** | -0.045*** | 0.058*** | 0.003 |
| | (0.004) | (0.007) | (0.007) | (0.007) | (0.005) | (0.007) | (0.008) |
| CLNY | -0.160*** | -0.030 | -0.054* | -0.058 | -0.045 | -0.239*** | 0.010 |
| | (0.018) | (0.025) | (0.029) | (0.036) | (0.027) | (0.036) | (0.035) |
| CNTG | 0.111*** | -0.001 | 0.077*** | -0.011 | 0.236*** | 0.108*** | -0.027 |
| | (0.013) | (0.021) | (0.021) | (0.021) | (0.017) | (0.024) | (0.022) |
| Observations | 442 578 | 252 393 | 304 201 | 261 749 | 307 124 | 243 978 | 165 609 |
| *Panel B. First Stage* | | | | | | | |
| $\hat{S}_{jt}$ | 1.143*** | 1.351*** | 1.151*** | 1.030*** | 0.964*** | 0.855*** | 1.562*** |
| | (0.316) | (0.241) | (0.240) | (0.288) | (0.270) | (0.220) | (0.310) |
| Observations | 6 480 | 4 604 | 4 486 | 1 945 | 2 829 | 5 721 | 3 468 |
| $R^2$ | 0.463 | 0.333 | 0.485 | 0.491 | 0.462 | 0.416 | 0.426 |
| First Stage F | 13.10 | 31.53 | 22.96 | 12.82 | 12.71 | 15.09 | 25.43 |
| $p$-value | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 |
| *Panel C. IV Regression* | | | | | | | |
| SANCT | -0.390** | -0.543*** | -0.334** | -0.225 | -0.485** | -0.543*** | -0.076 |
| | (0.190) | (0.204) | (0.146) | (0.154) | (0.197) | (0.182) | (0.224) |
| Observations | 6 480 | 4 604 | 4 486 | 1 945 | 2 829 | 5 721 | 3 468 |
| *Panel D. IV Weighted* | | | | | | | |
| SANCT | -0.507** | -0.708*** | -0.417** | -0.280 | -0.584** | -0.682*** | -0.099 |
| | (0.246) | (0.267) | (0.184) | (0.193) | (0.235) | (0.228) | (0.291) |
| Observations | 6 480 | 4 604 | 4 486 | 1 945 | 2 829 | 5 721 | 3 468 |

**Notes:** This table reports a series of estimates of the impact of sanctions on growth depending on the type of sanction. To ease the comparison, our main results for any types of sanctions from column (2) of Table 2 are reproduced in column (1). Columns (2) through (7), reproduce our main specification for each type of sanction, namely trade sanctions, in column (2), financial sanctions, in column (3), travel sanctions, in column (4), arms sanctions, in column (5), military assistance sanctions, in column (6), and other sanctions, in column (7). The specifications used to obtain the results in each column in Panels A through C preserve all features from our main specification. In addition, however, this table includes weighted IV estimates, which are obtained with a GDP weighted sum of the count of sanctions that appear in Panel D. The standard errors in panel A are heteroskedasticity robust, and the standard errors in panels B, C and D are heteroskedasticity robust and clustered at the country level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. See text for further details.

Table 4: Sanctions and Growth by Sanction Objective, 1950-2019

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| VARIABLES | ANY | DEMCR | HRIGTS | REGIM | EWAR | POLCY | PWAR | TRRSM | CONFL | OTHER |
| *Panel A. Stage Zero: Sanction Probability (Probit)* | | | | | | | | | | |
| AGGRSV | 1.106*** | 0.636*** | 0.475*** | -0.514*** | 0.316*** | 1.671*** | 0.181*** | 0.562*** | 1.818*** | 1.224*** |
| | (0.020) | (0.034) | (0.031) | (0.096) | (0.034) | (0.029) | (0.038) | (0.058) | (0.113) | (0.063) |
| LANG | -0.087*** | 0.071*** | 0.238*** | 0.067** | -0.163*** | -0.159*** | -0.105*** | -0.537*** | 0.108*** | -0.009 |
| | (0.004) | (0.009) | (0.008) | (0.028) | (0.007) | (0.010) | (0.009) | (0.014) | (0.022) | (0.027) |
| CLNY | -0.160*** | -0.060 | -0.059 | -0.650*** | -0.060 | -0.277*** | 0.107*** | 0.421*** | -0.086 | 0.221*** |
| | (0.018) | (0.038) | (0.041) | (0.203) | (0.038) | (0.031) | (0.038) | (0.072) | (0.071) | (0.070) |
| CNTG | 0.111*** | -0.168*** | -0.003 | -1.210*** | 0.227*** | -0.095*** | 0.038* | 0.570*** | -0.174*** | -0.485*** |
| | (0.013) | (0.036) | (0.029) | (0.094) | (0.024) | (0.029) | (0.022) | (0.046) | (0.054) | (0.094) |
| Observations | 442 578 | 140 547 | 197 140 | 13 332 | 217 057 | 113 994 | 112 733 | 68 275 | 32 752 | 24 611 |
| *Panel B. First Stage* | | | | | | | | | | |
| $\hat{S}_{jt}$ | 1.143*** | 0.911*** | 1.063*** | 0.055 | 1.115*** | 2.093*** | 1.112*** | 1.161*** | 1.068*** | 1.855*** |
| | (0.316) | (0.265) | (0.306) | (0.136) | (0.213) | (0.693) | (0.386) | (0.155) | (0.195) | (0.234) |
| Observations | 6 480 | 2 637 | 2 877 | 1 094 | 1 511 | 5 853 | 1 911 | 971 | 905 | 3 246 |
| $R^2$ | 0.463 | 0.394 | 0.392 | 0.357 | 0.456 | 0.580 | 0.586 | 0.519 | 0.753 | 0.685 |
| First Stage F | 13.10 | 11.80 | 12.08 | 0.16 | 27.29 | 9.12 | 8.29 | 56.15 | 29.88 | 62.89 |
| $p$-value | 0.00 | 0.00 | 0.00 | 0.69 | 0.00 | 0.00 | 0.01 | 0.00 | 0.00 | 0.00 |
| *Panel C. IV Regression* | | | | | | | | | | |
| SANCT | -0.390** | -0.026 | -0.330 | 202.736 | -0.365* | -0.060 | -0.274* | -0.072 | -0.224 | -0.080 |
| | (0.190) | (0.468) | (0.257) | (588.283) | (0.200) | (0.162) | (0.146) | (0.175) | (0.166) | (0.092) |
| Observations | 6 480 | 2 637 | 2 877 | 1 094 | 1 511 | 5 853 | 1 911 | 971 | 905 | 3 246 |

**Notes:** This table reports a series of estimates of the impact of sanctions on growth depending on the declared sanction objective. To ease comparison, our main results for any types of sanctions from column (2) of Table 2 are reproduced in column (1). Columns (2) through (10), reproduce our main specification for sanctions depending on their objectives, namely policy change, in column (2), to destabilize regime, in column (3), to resolve territorial conflict, in column (4), to prevent war, in column (5), to fight terrorism, in column (6), to end war, in column (7), to improve human rights, in column (8), to promote and restore democracy, in column (9), and other objectives, in column (10). The specifications used to obtain the results in each column preserve all features from our main specification, and the panel structure of this table is the same as the structure of Table 2. The standard errors in panel A are heteroskedasticity robust, and the standard errors in panels B and C are heteroskedasticity robust and clustered at the country level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 5: Sanctions and Growth by Sanction Success Score, 1950-2019

| VARIABLES | (1)<br>ANY | (2)<br>FAIL | (3)<br>NEGTN | (4)<br>TSCCSS | (5)<br>PSCCSS |
|---|---|---|---|---|---|
| *Panel A. Stage Zero: Sanction Probability (Probit)* | | | | | |
| AGGRSV | 1.106*** | 1.572*** | 0.689*** | 0.393*** | -0.392*** |
| | (0.020) | (0.026) | (0.085) | (0.024) | (0.045) |
| LANG | -0.087*** | -0.344*** | 0.124*** | -0.137*** | 0.131*** |
| | (0.004) | (0.009) | (0.022) | (0.006) | (0.013) |
| CLNY | -0.160*** | -0.095*** | -0.474*** | -0.017 | -0.265*** |
| | (0.018) | (0.030) | (0.062) | (0.024) | (0.053) |
| CNTG | 0.111*** | -0.055** | -0.734*** | -0.159*** | -0.032 |
| | (0.013) | (0.027) | (0.075) | (0.019) | (0.047) |
| Observations | 442 578 | 166 787 | 24 528 | 259 182 | 76 578 |
| *Panel B. First Stage* | | | | | |
| $\hat{S}_{jt}$ | 1.143*** | 1.791*** | 1.403*** | 1.606*** | 1.547*** |
| | (0.316) | (0.405) | (0.115) | (0.387) | (0.426) |
| Observations | 6 480 | 5 844 | 1 441 | 4 941 | 2 170 |
| $R^2$ | 0.463 | 0.435 | 0.724 | 0.457 | 0.605 |
| First Stage F | 13.10 | 19.58 | 147.81 | 17.19 | 13.21 |
| $p$-value | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 |
| *Panel C. IV Regression* | | | | | |
| SANCT | -0.390** | -0.245* | -0.097 | -0.325* | -0.160 |
| | (0.190) | (0.131) | (0.094) | (0.168) | (0.164) |
| Observations | 6 480 | 5 844 | 1 441 | 4 941 | 2 170 |

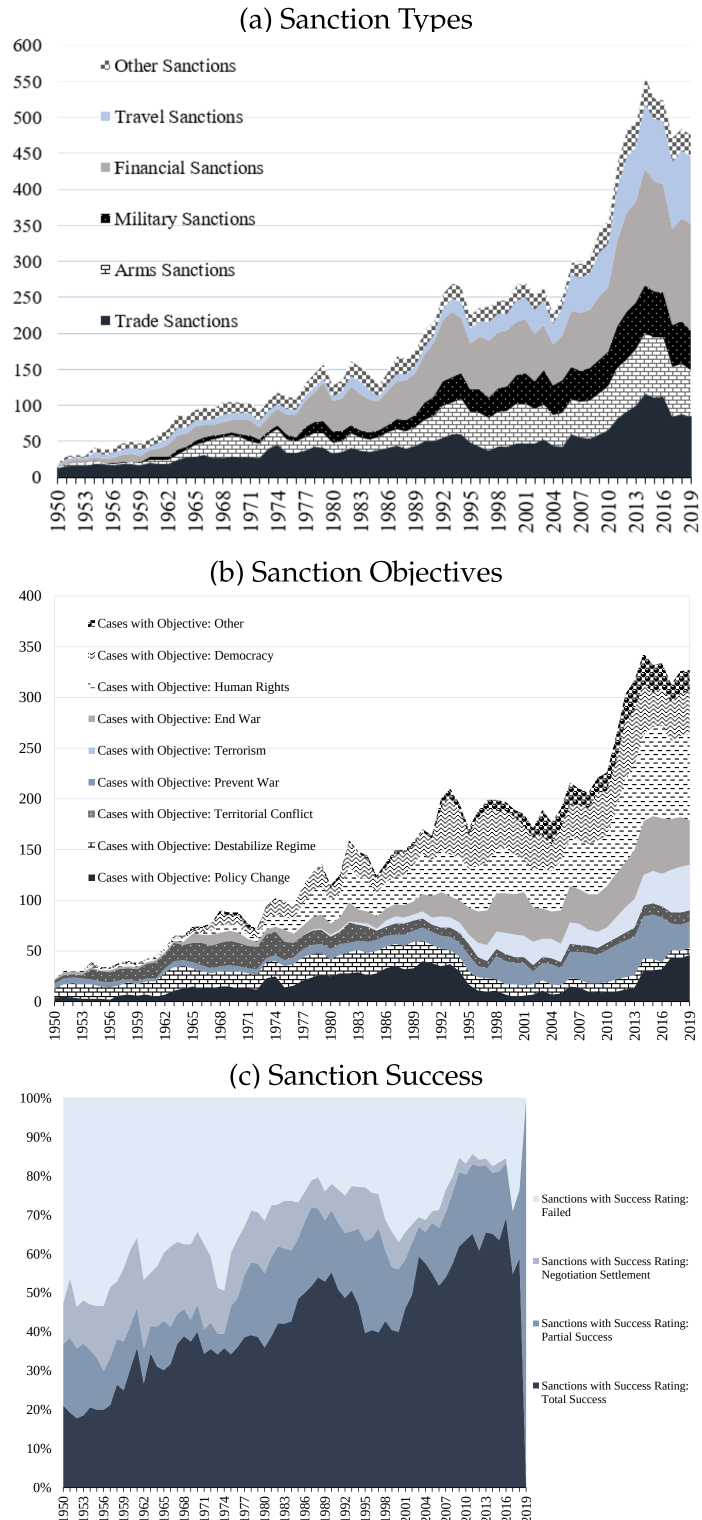
**Notes:** This table reports a series of estimates of the impact of sanctions on growth depending on the declared sanction objective. To ease comparison, our main results for any types of sanctions from column (2) of Table 2 are reproduced in column (1). Columns (2) through (5), reproduce our main specification for sanction depending on their success, namely failed sanctions, in column (2), sanctions with settlement by negotiations, in column (3), total success, in column (4), and partial success, in column (5). The specifications used to obtain the results in each column preserve all features from our main specification, and the panel structure of this table is the same as the structure of Table 2. The standard errors in panel A are heteroskedasticity robust, and the standard errors in panels B and C are heteroskedasticity robust and clustered at the country level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 6: On the Medium and Long Term Effect of Sanctions

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | | long difference | | |
| VARIABLES | Main IV | $T = 4$ | $T = 8$ | $T = 12$ |
| *Panel A. Stage Zero: Sanction Probability (Probit)* | | | | |
| AGGRSV | 1.106*** | *same as column (1)* | | |
| | (0.020) | | | |
| LANG | -0.087*** | | | |
| | (0.004) | | | |
| CLNY | -0.160*** | | | |
| | (0.018) | | | |
| CNTG | 0.111*** | | | |
| | (0.013) | | | |
| Observations | 442 578 | | | |
| *Panel B. First Stage* | | | | |
| $\hat{S}_{jt}$ | 1.143*** | 0.947*** | 0.947*** | 0.945*** |
| | (0.316) | (0.099) | (0.130) | (0.156) |
| Observations | 6 480 | 1 572 | 766 | 454 |
| $R^2$ | 0.463 | 0.344 | 0.375 | 0.398 |
| First Stage F | 13.10 | 90.52 | 52.73 | 36.59 |
| *p*-value | 0.00 | 0.00 | 0.00 | 0.00 |
| *Panel C. IV Regression* | | | | |
| SANCT | -0.390** | -0.010* | -0.010 | -0.009 |
| | (0.190) | (0.005) | (0.007) | (0.007) |
| ln(capital) | 0.265*** | 0.160*** | 0.199*** | 0.209*** |
| | (0.037) | (0.026) | (0.036) | (0.034) |
| human capital | 0.290** | -0.024 | -0.007 | 0.011 |
| | (0.138) | (0.062) | (0.080) | (0.098) |
| Observations | 6 480 | 1 572 | 766 | 454 |

**Notes:** This table shows the effects of economic sanctions on target countries' GDP per capita over various horizons. To ease comparison, our main results for any types of sanctions from column (2) of Table 2 are reproduced in column (1). Columns (2) through (4) report estimates for 4-year, 8-year, and 12-year intervals respectively. The specifications used to obtain the results in each column are based on equation (8), and the panel structure of this table is the same as the structure of Table 2. Heteroskedasticity robust standard errors are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. See text for further details.

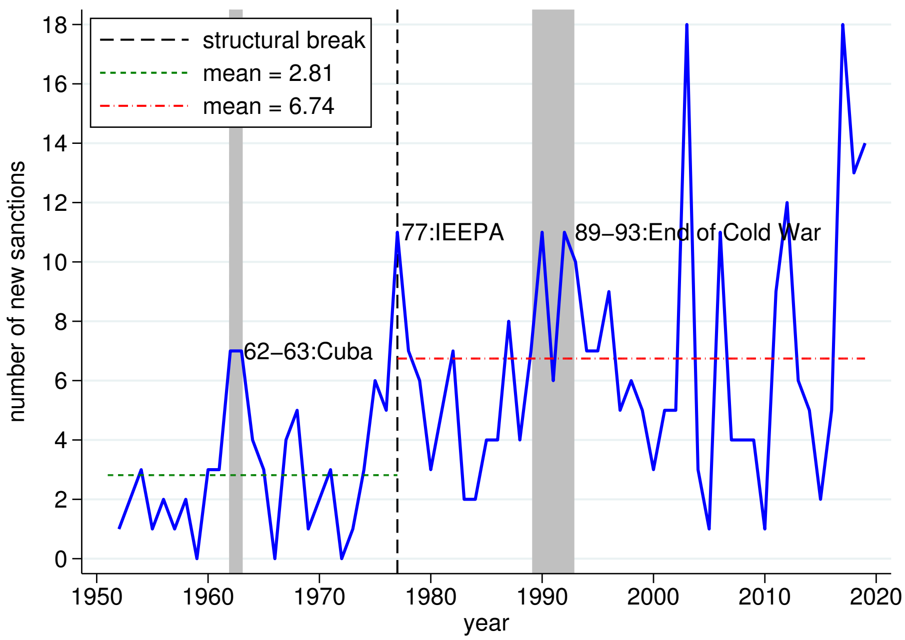
Figure 1: Evolution of sanctions by type, objective, and success



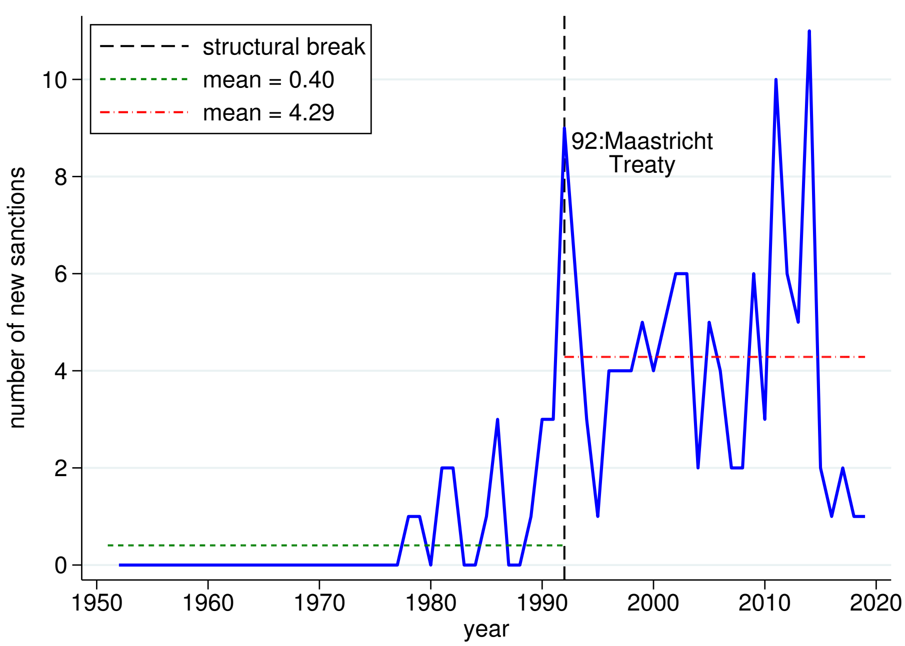

**Notes:** The three panels of this figure are replicated from Kirilakha et al. (2021). The original source used to construct each graph is the second edition of the Global Sanctions DataBase (GSDB), cf. Felbermayr et al. (2020a). The figure describes the evolution of sanctions between 1950 and 2019 by type, in Panel (a), by objective, in Panel (b), and by success, in Panel (c).

Figure 2: Evolution of US and EU Sanctions over Time

(a) US Sanctions



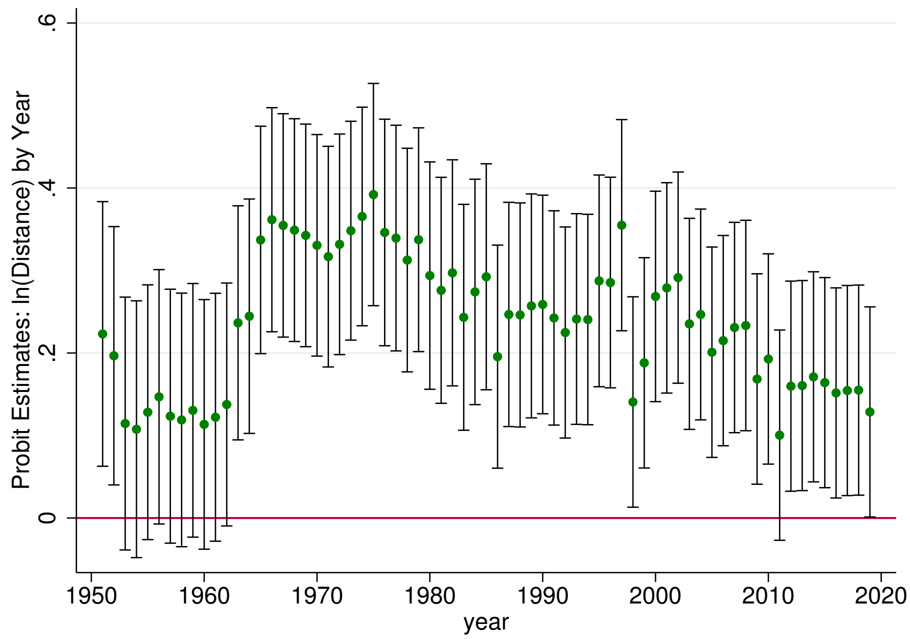

(b) EU Sanctions



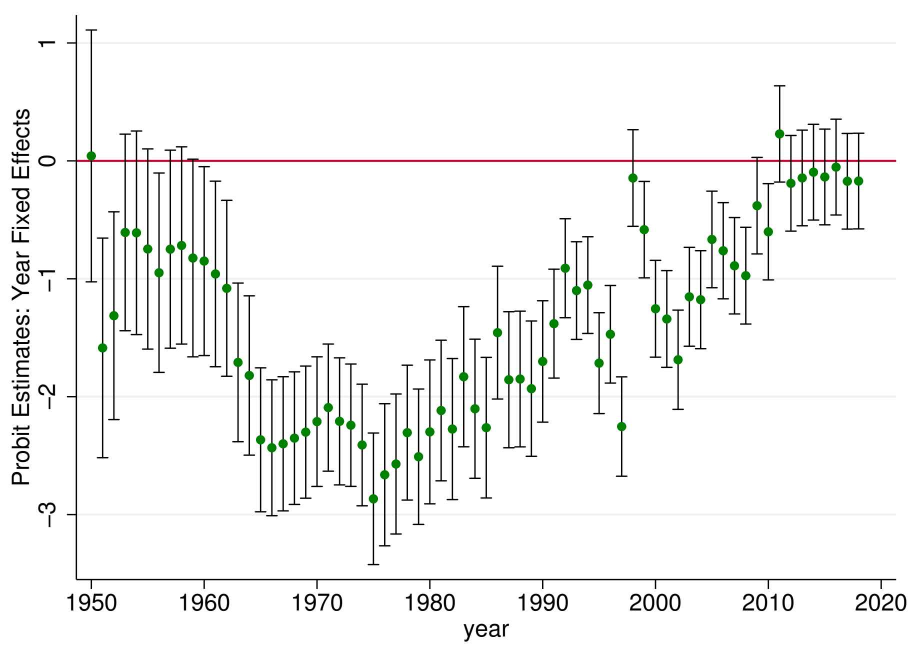

**Notes:** These figures show the evolution of the number of sanctions imposed by the US and the EU, respectively, during the period between 1950 and 2019. Data: Global Sanctions DataBase (GSDB).

Figure 3: Sanctions: Evolution and Geography

(a) Time-varying geography and sanctions




(b) Evolution of the bilateral probability of sanctions




**Notes:** This figure visualizes the estimates of two sets of covariates from the specification that we used to obtain the results from column (2) of Table 2. Panel (a) shows the estimates of the time-varying distance variables and their confidence intervals. Panel (b) includes the estimates and confidence intervals of the year fixed effects. See text for further details.

# Supplementary Appendix

This appendix includes the following tables and figures with summary statistics and additional results. Table 7 lists the countries in our sample along with the number of sanctions that they have imposed or have been targeted with. Table 8 provides summary statistics for the key variables in our empirical analysis. Finally, Figures 4-6 plot the estimates of the first-stage year fixed effects, along with their confidence intervals.

Table 7: Sanctioned Countries

| ISO | Sanction-Year Observation | ISO | Sanction-Year Observation | ISO | Sanction-Year Observation | ISO | Sanction-Year Observation |
|---|---|---|---|---|---|---|---|
| IRQ | 4866 | PHL | 457 | KAZ | 65 | AUS | 13 |
| ZAF | 4664 | BDI | 434 | DOM | 57 | URY | 12 |
| SDN | 4082 | MRT | 410 | LVA | 50 | BOL | 12 |
| LBR | 3249 | ETH | 406 | AUT | 49 | PAN | 11 |
| AFG | 3196 | COL | 397 | IRL | 47 | NPL | 11 |
| COD | 3194 | TUN | 365 | GTM | 47 | ISL | 11 |
| RWA | 2846 | ALB | 341 | GRC | 47 | LSO | 11 |
| SLE | 2745 | GHA | 304 | ARG | 47 | LKA | 10 |
| CIV | 2671 | BFA | 304 | SAU | 46 | TCD | 10 |
| LBN | 2652 | SEN | 303 | QAT | 46 | CMR | 10 |
| LBY | 2486 | CPV | 303 | MNE | 45 | CHE | 9 |
| IRN | 2123 | MNG | 301 | JOR | 45 | NZL | 8 |
| CUB | 1880 | RUS | 271 | ARE | 43 | ATG | 8 |
| MKD | 1868 | UKR | 269 | OMN | 43 | JAM | 8 |
| AGO | 1844 | VNM | 262 | MAR | 43 | MUS | 7 |
| PAK | 1828 | CHL | 215 | SRB | 41 | SMR | 7 |
| CHN | 1729 | UZB | 208 | BHR | 39 | MHL | 7 |
| NGA | 1560 | DEU | 200 | JPN | 39 | LCA | 7 |
| GNB | 1540 | HND | 137 | ITA | 38 | WSM | 7 |
| IND | 1479 | KHM | 135 | PER | 37 | GAB | 7 |
| BIH | 1459 | GNQ | 132 | SWE | 37 | BWA | 7 |
| FJI | 1431 | POL | 129 | LUX | 37 | BGD | 7 |
| CAF | 1382 | THA | 121 | BEL | 37 | TTO | 7 |
| KEN | 1374 | BLZ | 117 | FIN | 34 | MEX | 7 |
| PRT | 1267 | HUN | 108 | ESP | 34 | GUY | 7 |
| AZE | 1103 | CAN | 106 | PRY | 34 | NAM | 7 |
| SVN | 1087 | GBR | 100 | ZMB | 28 | SYC | 7 |
| YEM | 984 | TZA | 98 | COM | 26 | AND | 7 |
| ERI | 961 | ARM | 87 | LTU | 25 | BRB | 7 |
| GIN | 936 | DZA | 87 | MLT | 25 | GRD | 6 |
| MMR | 888 | COG | 82 | SVK | 24 | SSD | 4 |
| ZWE | 795 | MWI | 82 | CZE | 24 | MYS | 4 |
| HRV | 762 | FRA | 82 | KWT | 24 | NRU | 2 |
| MLI | 753 | TUR | 81 | UGA | 23 | SGP | 2 |
| EGY | 725 | CYP | 81 | SUR | 21 | DMA | 2 |
| BGR | 639 | USA | 79 | ROU | 21 | | |
| TGO | 632 | NLD | 78 | NOR | 21 | | |
| MDA | 596 | GEO | 78 | NIC | 17 | | |
| BLR | 575 | IDN | 75 | BRA | 17 | | |
| HTI | 558 | DNK | 74 | ECU | 17 | | |
| BEN | 551 | PSE | 73 | CRI | 16 | | |
| ISR | 516 | TJK | 73 | CSK | 15 | | |
| GMB | 502 | KGZ | 72 | SLV | 15 | | |
| NER | 490 | EST | 72 | LAO | 13 | | |
| MDG | 460 | TKM | 67 | VEN | 13 | | |

**Notes:** This table lists the target countries in our main regression sample along with the cumulative number of 'any' sanctions they received during the entire sample period.

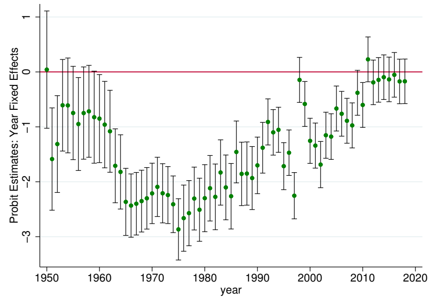
Table 8: Summary Statistics

| Variable | Mean | Median | Std. Dev. | Min | Max |
|---|---|---|---|---|---|
| *Panel A. Variables in Stage Zero* | | | | | |
| AGGRSV | 15.61 | 15 | 13.76 | 0 | 149 |
| CLNY | .02 | 0 | .13 | 0 | 1 |
| CNTG | .03 | 0 | .18 | 0 | 1 |
| LANG | .38 | 0 | .49 | 0 | 1 |
| DIST | 8.54 | 8.65 | .79 | 4.89 | 9.88 |
| *Panel A. Variables in First Stage and IV Regression* | | | | | |
| year | 1987.34 | 1989 | 19.76 | 1948 | 2019 |
| ln(GDPPC) | 8.36 | 8.25 | 1.54 | 4.89 | 12.19 |
| ln(capital) | 11.13 | 11.1 | 2.5 | 2.57 | 18.48 |
| human capital | 2.09 | 2 | .72 | 1.01 | 3.97 |
| any sanction | 11.04 | 0 | 35.78 | 0 | 192 |
| $\hat{S}$ (any) | 11.02 | 2.53 | 18.55 | .03 | 95.83 |
| trade sanction | 6.18 | 0 | 26.72 | 0 | 191 |
| $\hat{S}$ (trade) | 6.18 | 1.07 | 11.6 | .02 | 81.3 |
| financial sanction | 8.47 | 0 | 34.35 | 0 | 192 |
| $\hat{S}$ (financial) | 8.46 | .62 | 19.45 | 0 | 105.17 |
| travel sanction | 12.89 | 0 | 44.24 | 0 | 192 |
| $\hat{S}$ (travel) | 12.89 | .65 | 25.55 | 0 | 109.3 |

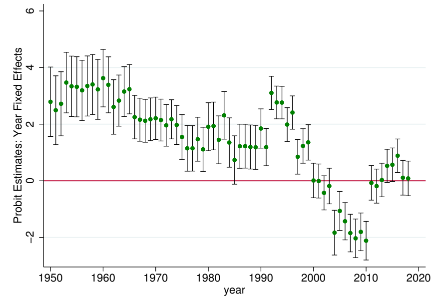
**Notes:** This table shows the summary statistics for the key variables in our regression tables.

Figure 4: Incidence of Sanctions by Types

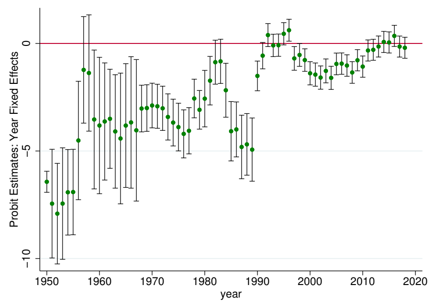
(a) Any



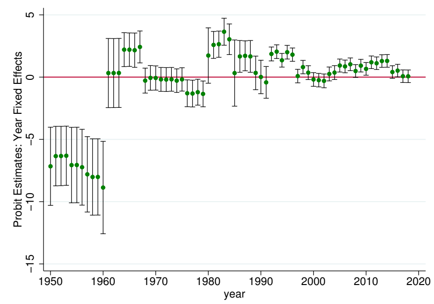

(b) Trade



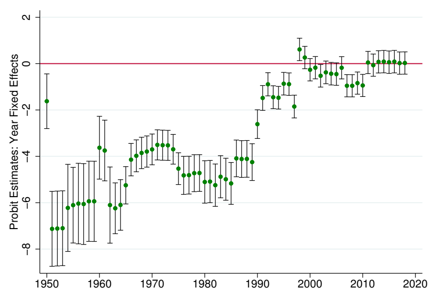

(c) Financial



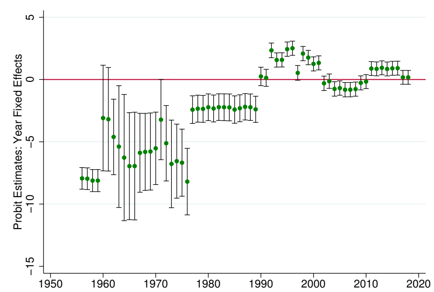

(d) Travel



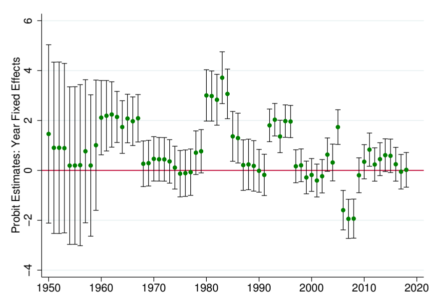

(e) Arms




(f) Military



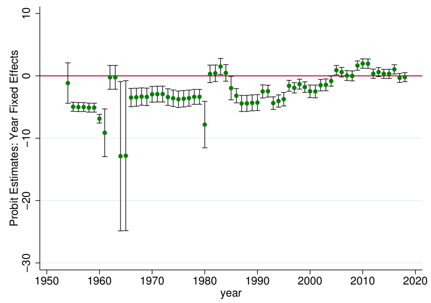

(g) Other



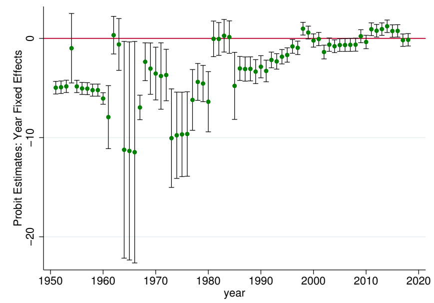

**Notes:** These figures show the point estimates (and their 95% confidence interval) of the year dummies in the stage zero regression. We present one figure for each type of sanctions.

Figure 5: Incidence of Sanctions by Objectives

(a) Democracy

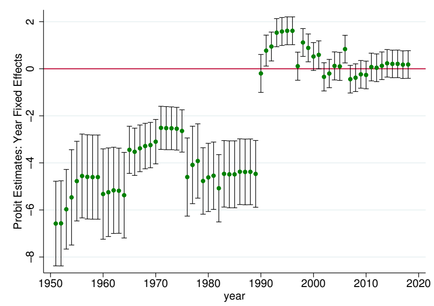



(b) Human Rights

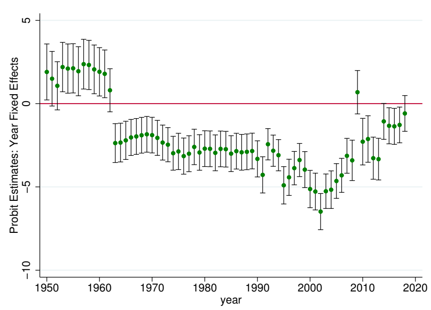



(c) Destabilize Regime


(d) End War

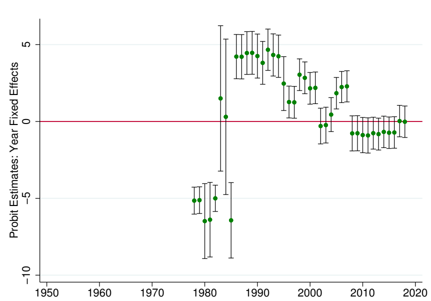



(e) Policy Change

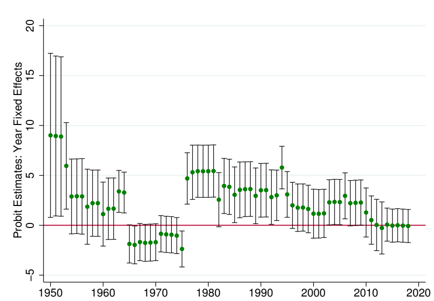



(f) Prevent War


(g) Terrorism

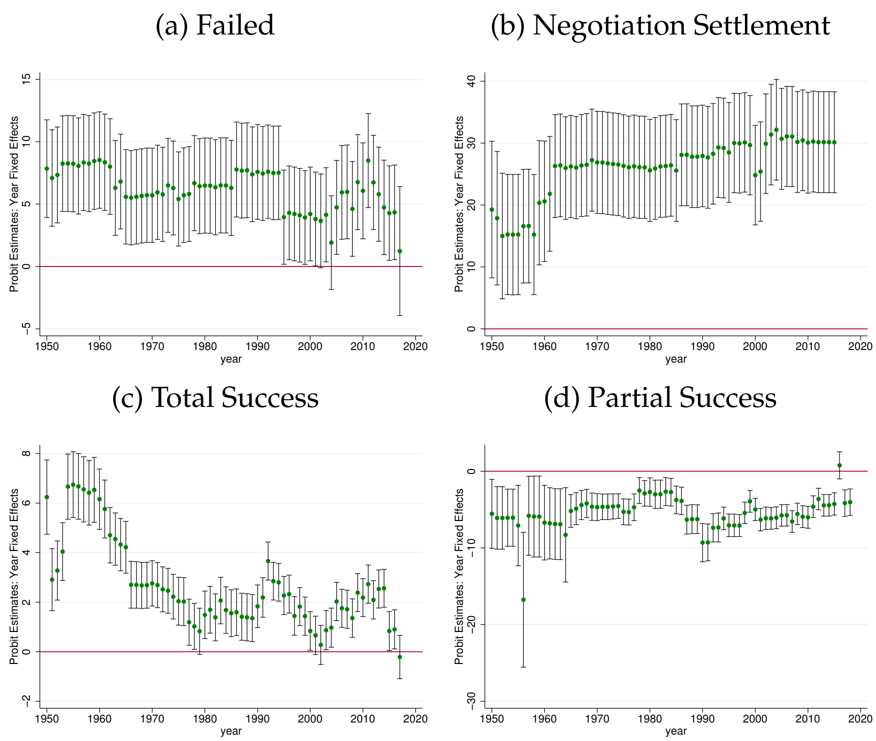



(h) Territorial Conflict




**Notes:** These figures show the point estimates (and their 95% confidence interval) of the year dummies in the stage zero regression. We present one figure for each objective of sanctions.

Figure 6: Incidence of Sanctions by Successfulness




**Notes:** These figures show the point estimates (and their 95% confidence interval) of the year dummies in the stage zero regression. We present one figure for each level of success of sanctions.