Do Sanctions Affect Growth?*

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Abstract

Direct estimates of economic effects of sanctions suffer from endogeneity stemming from contemporaneous adverse shocks (omitted variable bias) and the strategic targeting of high-growth adversaries (selection bias). We address these issues by instrumenting sanctions with sender aggressiveness, proxied by the total number of sanctions imposed by the sender in a given year. Our IV estimates indicate that each additional sanction reduces target-state real income by 0.4 percent, while the OLS estimates exhibit severe attenuation bias. We further document substantial heterogeneity in sanctions' effects across sanction types, their success, and long-run impacts.

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1 Introduction

Since the end of World War II, individual states and international coalitions – like the US, the EU, and the UN, also known as 'senders' – have increasingly utilized sanctions as instruments of coercive diplomacy. While the primary aim of sanctions is to compel changes in a target's behavior or policies deemed objectionable by senders, they often impose substantial economic costs on sanctioned states (targets).[1] Accurate assessment of their impact is therefore a central concern for policy analysis (Felbermayr et al., 2020a, 2021). We revisit this important question by proposing a novel method to address the intrinsic endogeneity associated with the imposition of sanctions.[2]

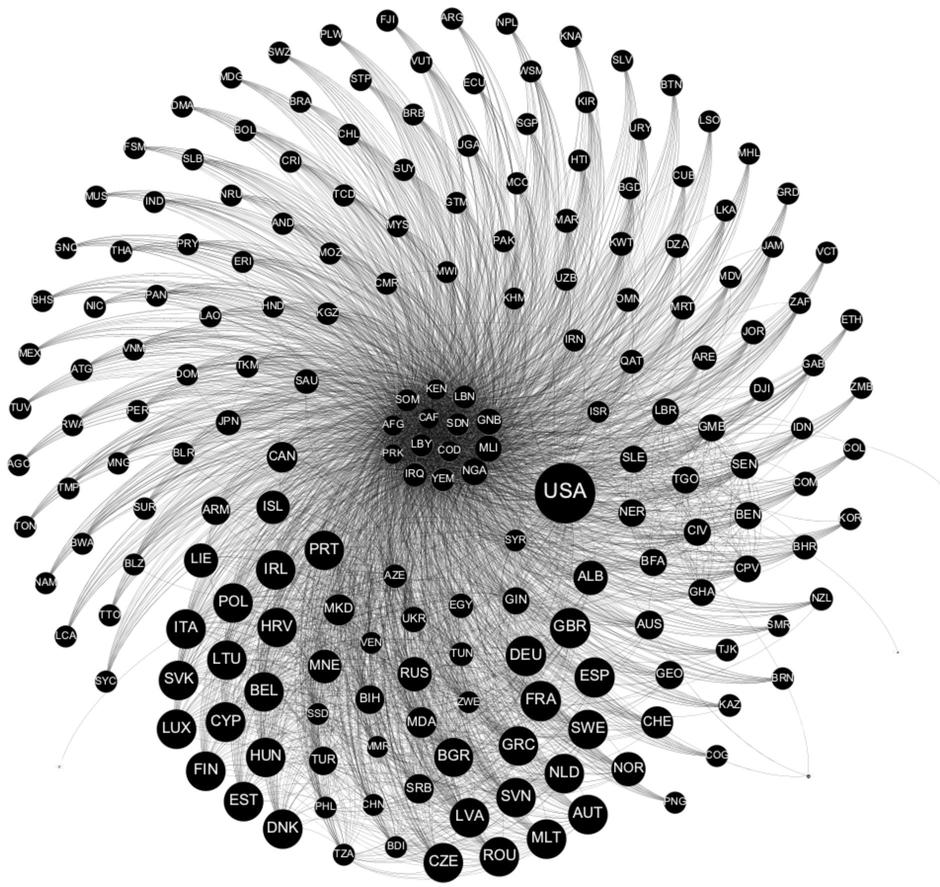
To illustrate the potential relationship between sanctions and economic growth, Figure 1 displays the network of all types of sanctions in 2019. The clusters in the figure represent targets, while the surrounding nodes indicate senders. The size of each node reflects the number of sanctions imposed by the corresponding sender(s). Three notable observations emerge from Figure 1. First, the large number of nodes indicates that the vast majority of countries in the world were involved in sanctions in 2019. Second, prominent senders—indicated by larger node sizes, most notably the United States—are wealthy, developed economies. Third, and most importantly for our purposes, Figure 1 depicts the emergence of a 'black hole' of states that have been heavily targeted by nearly all other countries. Notably, the economies of the targets in the black hole (e.g., the Central African Republic, the Democratic Republic of Congo, Kenya, Mali, Sudan, Yemen, etc.) are among the poorest in the world. This naturally prompts the question "Do sanctions

[1] As emphasized by Eaton and Engers (1992), Eaton and Engers (1999), Morgan and Schwebach (1997), and Drezner (2011), rigorous evaluations of sanction effectiveness must be grounded in careful modeling frameworks and comprehensive cost-benefit analyses that consider the impact on all stakeholders. For overviews of the relevant literature, see Felbermayr et al. (2021), van Bergeijk (2021) and Morgan et al. (2023).

[2] We posit that sanctions can negatively impact a targeted nation's GDP through multiple channels, including: (i) restricting access to critical commodities, resources, expertise, food, medicine, nutrition, and credit essential for investment in physical and human capital; (ii) distorting leadership priorities and increasing the risk of resource misallocation; and (iii) eroding consumer and investor confidence in markets, policy objectives, and the state's capacity to manage the economy effectively.

affect growth?"

Figure 1: The 'Black Hole' of Sanctions



Notes: This figure uses network analysis and the Global Sanctions Data Base (Felbermayr et al., 2020a) to depict the landscape of sanctions in the world in 2019. The clusters in the figure include countries that are sanctioned, while the periphery around each cluster includes sanctioning states. The size of the nodes reflects the number of sanctions sent by each country.

The empirical evidence on the growth effects of sanctions remains inconclusive. Hufbauer et al. (2009) document significant GDP losses driven by trade and investment disruptions, while Neuenkirch and Neumeier (2015) estimate that UN/US sanctions reduce per capita growth by 0.75–2 percent. Similarly, Felbermayr et al. (2020b) project a 4.2 percent income gain from lifting sanctions on Iran. Conversely, other studies find no systematic adverse effects: Rosenberg et al. (2016) report no growth decline, and Shin et al. (2016) conclude that sanctions do not hinder target economies "in a meaningful way" (p.

492).

Estimating the causal impact of sanctions on growth is complicated by well-recognized endogeneity concerns (Gutmann et al., 2020). The extant literature has attempted to address this challenge using various methods, including theoretical interest-group models (Kaempfer and Lowenberg, 2007), careful adjustments to control samples (Neuenkirch and Neumeier, 2015), and nearest-neighbor matching (Neuenkirch and Neumeier, 2016). While some studies employ endogenous treatment models (Gutmann et al., 2020), a robust instrumental variable strategy remains underdeveloped.

We posit that there are two main sources of endogeneity concerns in estimating the impact of sanctions on target country growth. On one hand, sanctions often coincide with domestic disruptions. For example, the comprehensive sanctions imposed on Iraq in 1990 (following its invasion of Kuwait) occurred alongside wartime shocks that may have independently depressed economic growth, potentially overstating the negative impact of sanctions. On the other hand, sanctions may be imposed strategically on fast-growing adversaries, generating a misleading positive correlation. The imposition of US sanctions in response to the emergence of the Soviet Union as a geopolitical rival may reflect such a case.[3] As a result, it remains unclear whether OLS estimates systematically understate or overstate the true effects of sanctions.

To address the endogeneity concerns, our IV strategy proxies sender *aggressiveness* using a time-varying index based on the number of sanctions that the sender imposed each year. This instrument exploits substantial variation in sanctioning frequency over time—particularly by the US and the EU—which can be linked to the adoption of specific laws and regulations that are plausibly exogenous to target countries' economic growth (e.g., the US International Emergency Economic Powers Act of 1977 and the EU Maastricht Treaty of 1992). In this respect, our approach is closely related to a broad literature

[3] According to Drezner (2021), US sanctions imposed on Soviet allies during the initial phase of the Cold War were largely successful as an act of containment, particularly in curbing the Soviet Union's economic influence.

that leverages the pairwise structure of the data in combination with heterogeneous treatment propensities across decision-making units (Rajan and Subramanian, 2008; Maestas et al., 2013; Dobbie and Song, 2015; Aizer and Doyle Jr, 2015).[4]

Our empirical analysis confirms the endogeneity challenges noted above and validates 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 a 0.14 percent reduction in the contemporaneous real GDP per capita due to the imposition of an additional sanction, a magnitude notably smaller than conventional estimates (Neuenkirch and Neumeier, 2015; Felbermayr et al., 2020b). In contrast, our IV estimation results reveal that an additional economic sanction leads to a 0.4 percent reduction in real GDP per capita, which is more than twice as large as the corresponding OLS estimate.[5] We also show that the sender's aggressiveness significantly predicts the bilateral probability of sanctions imposition, yielding a strong first-stage F-statistic.

We then estimate the impact of sanctions along two salient dimensions of Global Sanctions Data Base (GSDB) (Felbermayr et al. (2020a) and Kirikakha et al. (2021)): sanction type and success.[6] 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

[4]Specifically, Rajan and Subramanian (2008) use historical and political ties to predict bilateral aid flows; Maestas et al. (2013) exploit variation in disability examiners' allowance rates against otherwise similar applicants; Dobbie and Song (2015) instrument for bankruptcy protection using judge-specific leniency against comparable filers; and Aizer and Doyle Jr (2015) use judges' incarceration propensities against observationally similar defendants to identify the causal effects of juvenile detention.

[5]The magnitude of our IV estimate is much closer to the findings of Neuenkirch and Neumeier (2015), Felbermayr et al. (2020b) and Laudati and Pesaran (2023). The substantial attenuation bias in OLS estimates helps explain why prior studies often fail to detect a significant effect of sanctions on growth.

[6]We abstract from the objective of sanctions in our analysis. Conceptually, we view sanction type as a more consequential determinant of economic outcomes than stated objectives—for example, trade sanctions aimed at ending conflict may generate effects comparable to those targeting regime change. In addition, the sparse number of observations across many objective categories results in weak instruments, rendering this dimension ill-suited for IV estimation. Results based on specifications incorporating sanction objectives are available upon request.

such sanctions induce target countries to divert significant resources to military buildup and defense. With regards to various measures of 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 examine the impact of sanctions across different time horizons and find that sanctions have no long-term effects on growth. Specifically, our estimates of the impact on the change in real GDP per capita over periods of 8 years or longer are not statistically significant. While this result is reassuring, we approach it with caution. Specifically, our IV specifications control for a country's human and physical capital, allowing the coefficient to be interpreted as the effect of sanctions on long-run growth conditional on these observable factors. However, it is plausible that sanctions influence long-run growth through their impact on the accumulation of human and physical capital.

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 and additional estimation results.

2 Identifying the Impact of Sanctions on Growth

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

2.1 The Challenge: Sanction Endogeneity

The endogeneity problem in identifying the impact of sanctions on growth is well understood from an econometric perspective. Typically, researchers estimate the impact of sanctions on economic performance by regressing the change in real GDP per capita, Y_{jt}, on the number of sanctions imposed, S_{jt}:

$$\ln(Y_{jt}) = \beta_S S_{jt} + \mathbf{Z}_{jt}\boldsymbol{\alpha} + \Phi_j + \Psi_t + \varepsilon_{jt}. \tag{1}$$

The vector $\mathbf{Z}_{jt}$ includes a set of country-specific, time-varying controls (e.g., physical capital, human capital). Country and year fixed effects are captured by Φ_j and Ψ_t, respectively, and ε_{jt} denotes the error term.

Despite controlling for $\{\mathbf{Z}_{jt}, \Phi_j, \Psi_t\}$, the coefficient β_S remains potentially biased due to the endogenous nature of sanction imposition. Specifically, sanctions may respond to latent, time-varying characteristics of the target country that are correlated with economic performance but unobserved by the econometrician, which implies that $\mathbb{E}(\varepsilon_{jt}|S_{jt}, \mathbf{Z}_{jt}, \Phi_j, \Psi_t) \neq 0$, thereby violating the exogeneity assumption required for consistent estimation via OLS. The direction of this endogeneity-induced bias is, ex ante, indeterminate.

To motivate this concern, consider two distinct scenarios. One involves cases where sanctions coincide with acute negative shocks driven by domestic instability. For example, during the 2011 Libyan civil war, Libya was subject to sanctions by the United Nations, the European Union, the United States, and others. That year, Libya's GDP contracted by over 50 percent. An OLS regression would likely conflate the negative effects of the civil conflict with those of the sanctions, overstating the magnitude of β_S.[7]

In contrast, sanctions may also be strategically deployed in response to a country's economic ascent. For example, the imposition of US sanctions on the Soviet Union during the 1980s may have been partially motivated by concerns over its growing economic and

[7] Liberia in 1992 offers a parallel example: UN and ECOWAS sanctions coincided with civil war, confounding the isolation of their causal effects from conflict-related economic disruption.

geopolitical influence. If stronger economic performance increases the likelihood of being sanctioned, then OLS estimates will be attenuated, biasing the estimated effect toward zero.[8]

These contrasting scenarios underscore two key econometric implications. First, OLS estimates of the impact of sanctions on economic growth are likely to suffer from substantial bias, arising from omitted variable bias—such as domestic instability—and selection bias, due to the strategic imposition of sanctions on rapidly growing adversaries. Second, the direction of bias is ambiguous: while conflict-related sanctions likely inflate the magnitude of β_S, strategically motivated sanctions linked to continued economic growth may induce attenuation.[9]

2.2 The Solution: An IV Strategy

To address endogeneity, we employ the following IV strategy. Let S_{ijt} be an indicator variable equal to one if sender i imposes a sanction on target j in year t. We model the sanction decision as a function of three sets of determinants: sender-specific time-varying characteristics (s_{it}), time-invariant bilateral attributes (d_{ij}), and target-specific time-varying characteristics (τ_{jt}). Formally,

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

The term s_{it} captures time-varying sender characteristics, such as its political regime or strategic priorities. The vector d_{ij} contains time-invariant dyadic characteristics (e.g., geographic distance, common language, colonial ties). The interaction $s_{it} \cdot d_{ij}$ allows the impact of sender characteristics to be heterogeneous across pairs. Finally, τ_{jt} includes

[8] Comparable patterns arise in cases such as US sanctions on South Korea (1973–1977) for human rights violations amidst rapid growth, and US sanctions on Chinese technology firms during the late 2010s and early 2020s, driven by national security concerns.

[9] We confirm in Section 4 that the OLS estimates tend to attenuate the negative impact of sanctions on growth.

time-varying target characteristics (e.g., domestic instability, economic performance) that may be correlated with the outcome variable, thus inducing endogeneity.

The primary endogeneity concern is that time-varying target-specific characteristics, τ_{jt}, may be correlated with the outcome variable, $\ln(Y_{jt})$. Our identification strategy, therefore, seeks to isolate variation in S_{ijt} driven by sender-specific, time-varying factors, s_{it}, which are plausibly exogenous to the target's economic performance. This approach is inspired by instrumental variable designs that exploit agent-specific propensities within dyadic frameworks (Rajan and Subramanian, 2008; Maestas et al., 2013; Dobbie and Song, 2015; Aizer and Doyle Jr, 2015).

Specifically, we parameterize the exogenous component of the sanction decision:

$$\begin{aligned} S_{ijt} = \;& \beta_A AGGRSS_{it} + \beta_G AGGRSS_{it} \times \mathbf{GEOGR_{ij}} + \beta_H AGGRSS_{it} \times \mathbf{HISTORY_{ij}} \\ & + \gamma \mathbf{GEOGR_{ij}} + \alpha \mathbf{HISTORY_{ij}} + \Psi_t, \end{aligned} \tag{3}$$

where $AGGRSS_{it}$ is our primary instrument, a novel index of sender i's *aggressiveness* in year t, proxied by its total number of imposed sanctions. The underlying identification assumption—the exclusion restriction—is that $AGGRSS_{it}$ is uncorrelated with unobserved, time-varying determinants of the target country's economic performance. The interaction terms between $AGGRSS_{it}$ and bilateral characteristics ($\mathbf{GEOGR_{ij}}$, $\mathbf{HISTORY_{ij}}$) allow the effect of sender aggressiveness on sanction likelihood to vary heterogeneously across country pairs.[10]

To further mitigate endogeneity and avoid mechanical contemporaneous correlation, we employ two definitions of our aggressiveness index. First, following Maestas et al. (2013), Dobbie and Song (2015) and Aizer and Doyle Jr (2015), we use a contemporaneous 'leave-one-out' measure, $AGGRSS_{i,-j,t}$, defined as the total count of sanctions imposed by sender i in year t on all targets, excluding sanctions directed at target j. This specification ensures the instrument is not mechanically correlated with the sanction decision

[10] A similar approach has been used in the context of foreign aid by Rajan and Subramanian (2008), who instrument bilateral aid flows using historical relationships between donor-recipient pairs.

for target j. Second, we use its one-year lag, $AGGRSS_{i,t-1}$. This alternative measure mitigates concerns that global contemporaneous shocks (e.g., business cycles) might jointly determine sender aggressiveness and target characteristics. Our IV regression estimates are robust to either definition. We provide descriptive statistics motivating this index in Section 3.

The vector $\mathbf{GEOGR_{ij}}$ in equation (3) contains bilateral geographic controls: an indicator for contiguity ($CNTG_{ij}$) and the logarithm of bilateral distance ($DIST_{ij}$). These variables proxy for exogenous economic interdependence (Anderson and van Wincoop, 2004; Eicher et al., 2012; Blonigen and Piger, 2014). Following Feyrer (2019, 2021), we allow the elasticity of sanction incidence with respect to distance, γ, to be time-varying.

The vector $\mathbf{HISTORY_{ij}}$ in equation (3) includes time-invariant dyadic controls for common official language ($LANG_{ij}$) and colonial relationships ($CLNY_{ij}$). This follows Rajan and Subramanian (2008), who use historical ties to instrument for bilateral aid. We also include year fixed effects, Ψ_t, to capture the secular rise in sanction use documented in Section 3.[11]

Equation (3) thus models the sanction likelihood using sender aggressiveness, predetermined dyadic characteristics (geography, history), and their interactions. These drivers are plausibly exogenous to target-country growth, conditional on the fixed effects in the second-stage regression, equation (7).[12] This specification allows us to predict the probability of bilateral sanctions, $\hat{S}_{ijt}$, using variation that is independent of the target-specific factors that may trigger sanctions and directly affect growth. Our empirical analysis confirms the instrument's robustness to the exclusion of $\mathbf{GEOGR_{ij}}$, $\mathbf{HISTORY_{ij}}$, or Ψ_t.

Our IV strategy proceeds in three steps. First, we estimate a 'stage-zero' Probit model

[11]The key identifying variation stems from $AGGRSS_{it}$ and its interaction terms. While $\mathbf{GEOGR}_{ij}$, $\mathbf{HISTORY_{ij}}$, and Ψ_t enhance instrument relevance by absorbing bilateral and temporal heterogeneity, they are not essential for identification. First-stage F-statistics exceed conventional thresholds for weak instruments even absent these controls. Results are available upon request.

[12]Our identification assumption is one of *conditional* exogeneity. While $\mathbf{GEOGR_{ij}}$ or $\mathbf{HISTORY_{ij}}$ may correlate with target growth *levels*, these level effects are absorbed by target fixed effects (Φ_j) in the second-stage regression. The maintained assumption is that these variables do not correlate with the *marginal effect* of sanctions on growth.

for the bilateral sanction probability, S_{ijt}, based on equation (3):[13]

$$\textbf{Stage zero}: \quad \begin{aligned} S_{ijt} = \;& \beta_A AGGRSS_{it} + \beta_G AGGRSS_{it} \times \mathbf{GEOGR_{ij}} + \beta_H AGGRSS_{it} \times \mathbf{HISTORY_{ij}} \\ & + \gamma \mathbf{GEOGR_{ij}} + \alpha \mathbf{HISTORY_{ij}} + \Psi_t + \nu_{ijt}, \end{aligned} \tag{4}$$

where ν_{ijt} is an error term. Second, we aggregate the predicted probabilities, $\hat{S}_{ijt}$, across all senders i to construct the instrument for the endogenous regressor at the target-year level:

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

Third, we implement a conventional two-stage least squares (2SLS) estimation. The first stage regresses the observed sanction count, S_{jt}, on the instrument, $\hat{S}_{jt}$, and all 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 Φ_{jt}^{2nd} denotes the vector of all exogenous covariates from the second-stage IV regression.[14] The second-stage regression is:

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

Equation (7) differs from the OLS specification (1) in two respects. First, the endogenous regressor $\sum_i \omega_{it} S_{ijt}$ explicitly allows sanction counts to be weighted by sender characteristics, ω_{it}.[15] Second, equation (7) employs a more demanding fixed effects structure,

[13] We demonstrate the robustness of our results to the use of the Logit and Poisson Pseudo-Maximum Likelihood (PPML) estimators.

[14] We use only the point estimates from the stage-zero regression in the subsequent IV estimations, omitting the associated standard errors. This approach is valid since the point estimates meet the dual requirements of exogeneity and relevance, which are necessary conditions for the validity of an IV strategy.

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

including target fixed effects (Φ_j) and region-by-year fixed effects (Ψ_{tR}).[16]

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. We compile them using 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 Appendix Table A.1. Summary statistics for all the variables we employ appear in Appendix Table A.2, where panel A describes our bilateral data and panel B describes our country-specific variables.

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 Global Sanctions Data Base (GSDB). 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

[16] Broader regions include Africa, Caribbean, Central America, Central Asia, East Asia, Europe, North America, Pacific, South America, and Southeast Asia.

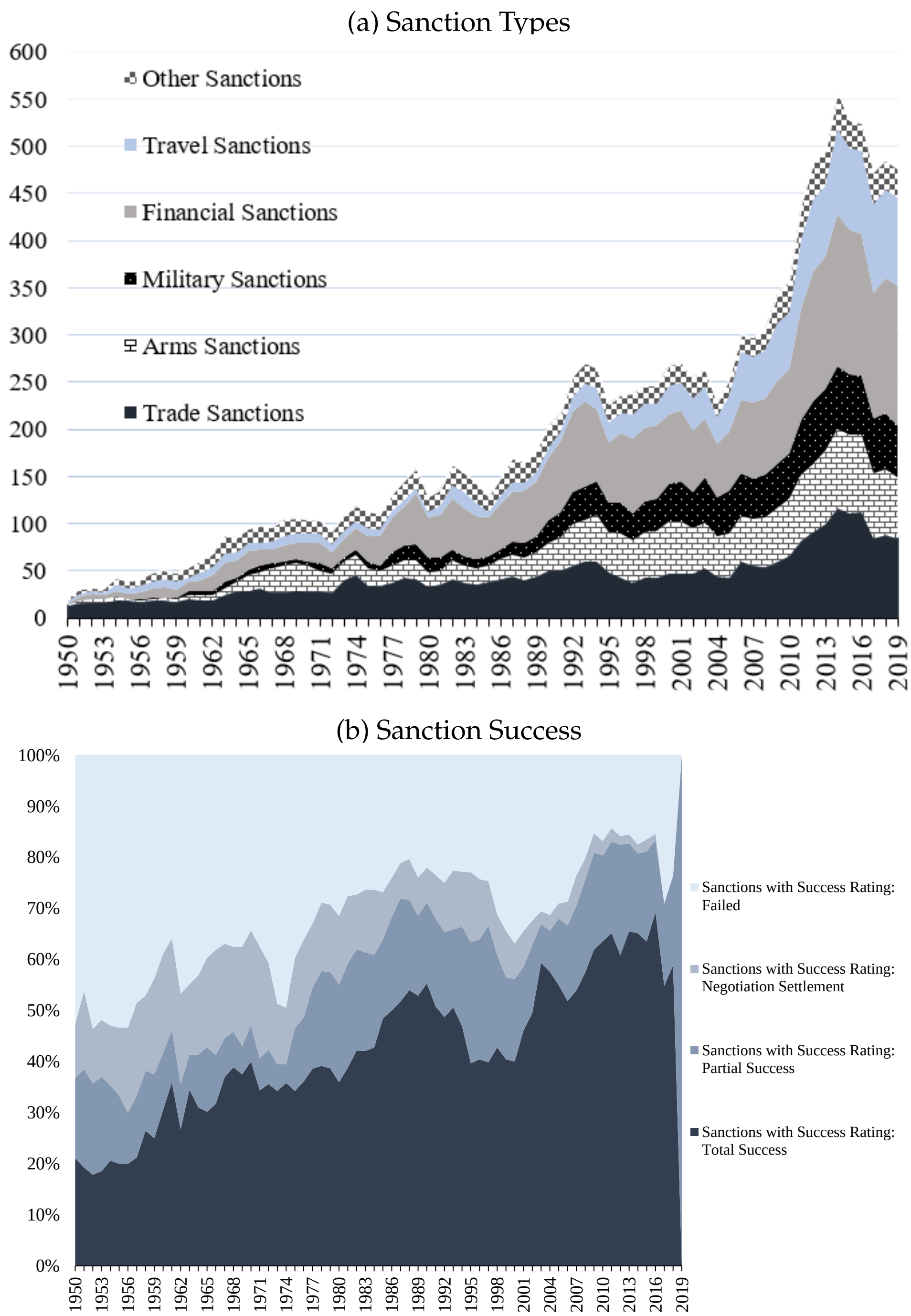
be very beneficial to our country-specific analysis. Finally, we will capitalize on the fact that the GSDB classifies sanctions across types and whether they were successful in order to study the heterogeneous effects of sanctions.

Figure 2, which is reproduced from Felbermayr et al. (2020a), depicts the evolution of sanctions across the two 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).[17] 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. Following the convention, we sometimes refer to financial and travel sanctions as 'smart' sanctions (Cortright and Lopez, 2002; Drezner, 2011; Rosenberg et al., 2016).

Panel (b) of Figure 2 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, and failure. The findings in panel (b) 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 (Felbermayr et al., 2020a). Second, the figure omits the category of ongoing sanctions (Kirikakha et al., 2021). Despite these limitations, two clear patterns stand out in panel (b). First, consistent with the existing literature (Pape (1997), Hufbauer et al. (2009), 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 sanc-

[17]We refer readers to Felbermayr et al. (2020a) for the definition of sanctions types as well as the success score. A short description is also provided in Appendix Tables A.3 and A.4.

Figure 2: Evolution of Sanctions by Type and Whether They Were Successful



Notes: The two panels of this figure are replicated from Felbermayr et al. (2020a). The original source used to construct each graph is the second edition of the Global Sanctions Data Base (GSDB) (Kirikakha et al., 2021). The figure describes the evolution of sanctions between 1950 and 2019 by type, in panel (a) and by whether they were successful, in panel (b).

tions) have been successful in achieving their objectives.[18] Second, panel (b) 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 *aggressiveness* index. The objective of this section is 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 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 desirable for identification. Second, the majority of sanctions are imposed by the US (close to 37%) and the EU (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 senders.

Motivated by the results in Table 1, panel (a) of Figure 3 plots the evolution of the number of sanctions that were unilaterally imposed by the US. The figure reveals that

[18]The observed rate of sanction success aligns closely with the findings reported by Hufbauer et al. (2009) and Morgan et al. (2014).

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: Global Sanctions Data Base (GSDB).

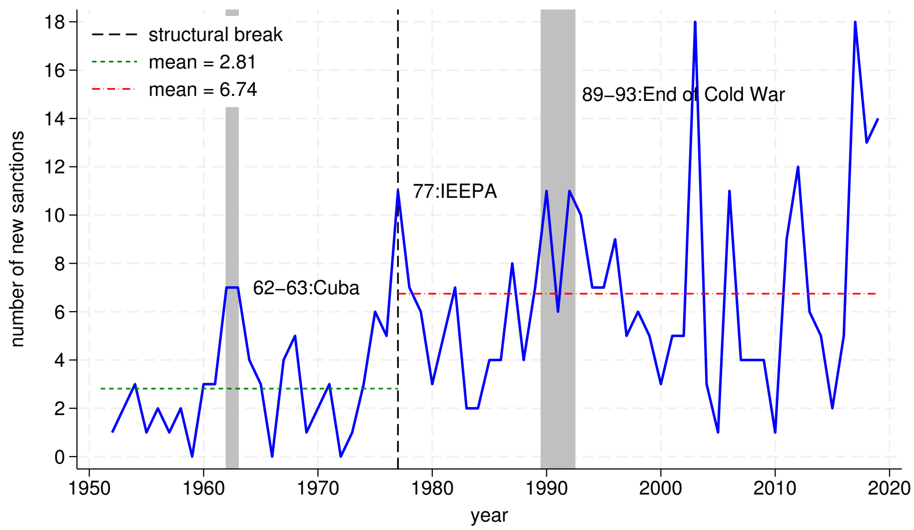
this number varies significantly during the period of investigation. Most important for our purposes, we detect the appearance of a structural break in 1977 that is captured by the green vs. red dashed lines depicting the average number of sanctions before and after 1977, respectively.[19] As documented in the literature (Hufbauer et al., 2009), the reason 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.[20]

[19]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.

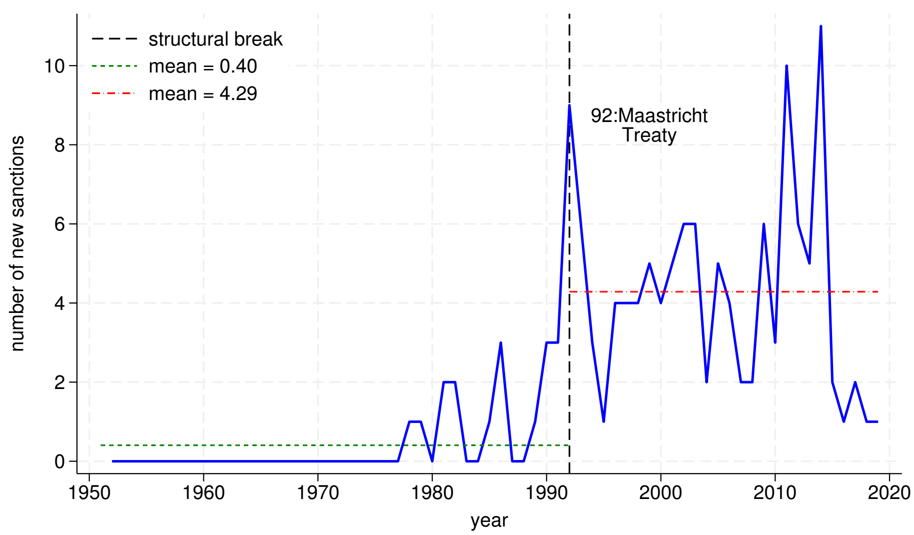
[20]The key role played by IEEPA in US sanctions is also evidenced by US executive order documents of economic sanctions, where we find that documents in recent times share the following paragraph: *"By the authority vested in me as President by the Constitution and the laws of the United States of America, 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).

Figure 3: 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 Data Base (GSDB).

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 (Pape, 1997).[21] Indeed, Rosenberg et al. (2016) contend that a "powerful motivation behind the 2003 invasion of Iraq was the widespread, albeit mistaken, belief that the UN sanctions regime had failed."[22]

Table 1 identifies the European Union as the second most active sender. Panel (b) of Figure 3 plots the evolution of EU sanctions over the sample period. 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),[23] 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) signed in 1992.[24] Since then, the imposition of EU sanctions fell under the

[21]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.

[22]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, 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.

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

[24]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.

domain of CFSP, and sanctions have been part of regular EU foreign policy (Giumelli and Ivan, 2013). As illustrated in panel (b) of Figure 3, the number of EU sanctions increased significantly since the early 90s.

The key takeaway is that sanctioning decisions are correlated across targets for a given sender, partly due to institutional shifts such as the International Emergency Economic Powers Act and the Maastricht Treaty. This motivates our use of sender aggressiveness—measured by the sender's total sanctions against all other targets—as an instrument. We establish the econometric validity of this approach in Section 4.

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 Dataset (DGD) (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 (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 two parts. Section 4.1 reports the main estimates of the impact of sanctions on growth, provides evidence for our IV strategy, and conducts a series of sensitivity analyses. Section 4.2 explores heterogeneous effects across various dimensions and examines the short- versus long-run sanctions impacts.

4.1 Main Results: Sanctions Hurt Growth

Table 2 reports the stage-zero Probit estimates. Column (1), which omits interaction terms, shows the baseline effects of $AGGRSS_{it}$ and bilateral covariates. The likelihood of sanctions imposition increases with sender aggressiveness, is lower for pairs with historical ties ($LANG_{ij}$, $CLNY_{ij}$) or greater distance ($DIST_{ij}$), and is higher for contiguous pairs ($CNTG_{ij}$). Column (2) introduces the interaction terms, representing our preferred specification. The results are intuitive: increased sender aggressiveness disproportionately raises the probability of sanctioning for targets that are geographically closer or share a common language.[25] Column (3) uses the lagged instrument ($AGGRSS_{i,t-1}$), while columns (4)–(6) test alternative estimators. Overall, Table 2 demonstrates that our instrument is relevant and the stage-zero specification is robust.[26]

Table 3 presents our main first-stage and 2SLS estimation results. Column (1) reports the benchmark OLS estimate of equation (7), which yields a coefficient of -0.141. Our preferred IV specifications are presented in columns (2) and (3). Columns (4)–(6) report sensitivity analyses corresponding to the alternative stage-zero estimators from Table 2, while columns (7) and (8) present 2SLS results with alternative covariates.

[25]The fact that sender aggressiveness has a disproportionate effect on sanction likelihood for countries that are geographically closer suggests that economic and political considerations may be intertwined in sanction decisions. For instance, countries that are geographically closer may have stronger economic ties, and thus sanctions from aggressive senders may be more likely to be imposed on such countries. In contrast, countries that share a common language may result from political influence through ethnicity or colonial history, making them more susceptible to sanctions from aggressive senders.

[26]For brevity, we do not report the coefficient estimates for the time-varying distance elasticities and year fixed effects. These are plotted in Appendix Figure A.1 (all sanctions), Figure A.2 (by type), and Figure A.3 (by success).

Table 2: Stage Zero Results: Sanctions and Growth, 1950-2019

VARIABLES	(1) W/O Int.	(2) Main IV	(3) LAG IV	(4) Logit Zero	(5) Poisson Zero	(6) OLS Zero
AGGRSV	0.813***	5.696***	7.854***	8.873***	4.300***	1.849***
	(0.063)	(0.927)	(0.953)	(1.533)	(0.725)	(0.314)
AGGRSV#LANG		0.246**	0.225*	0.415**	0.212**	0.033
		(0.117)	(0.121)	(0.193)	(0.092)	(0.036)
AGGRSV#CLNY		0.186	0.006	0.424	0.305*	0.034
		(0.257)	(0.269)	(0.419)	(0.185)	(0.073)
AGGRSV#CNTG		0.125	0.257	0.344	-0.127	0.093
		(0.558)	(0.640)	(1.015)	(0.356)	(0.161)
AGGRSV#DIST		-0.567***	-0.775***	-0.882***	-0.419***	-0.181***
		(0.106)	(0.109)	(0.175)	(0.083)	(0.036)
LANG	-0.090***	-0.121***	-0.111***	-0.197***	-0.131***	-0.026***
	(0.016)	(0.026)	(0.025)	(0.044)	(0.028)	(0.006)
CLNY	-0.134**	-0.200**	-0.187**	-0.376**	-0.244**	-0.052***
	(0.055)	(0.087)	(0.085)	(0.150)	(0.096)	(0.018)
CNTG	0.102**	0.102	0.096	0.144	0.138	0.022
	(0.050)	(0.099)	(0.104)	(0.182)	(0.094)	(0.023)
DIST	-0.033***					
	(0.012)					
Observations	442,578	442,578	442,578	442,578	442,578	442,578
DIST#Year	NO	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES

Notes: This table reports the stage zero regression results of the impact of sender aggressiveness on the probability of sanction imposition. The dependent variable is a binary indicator equal to one if country j is sanctioned by any sender i in year t. The key independent variable is the sender aggressiveness index (AGGRSV) and its interaction terms with bilateral characteristics including common language (LANG), colonial relationship (CLNY), contiguity (CNTG), and log distance (DIST). Standard errors clustered at the sender-target-pair level are reported in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 3: Main Results: Sanctions and Growth, 1950-2019

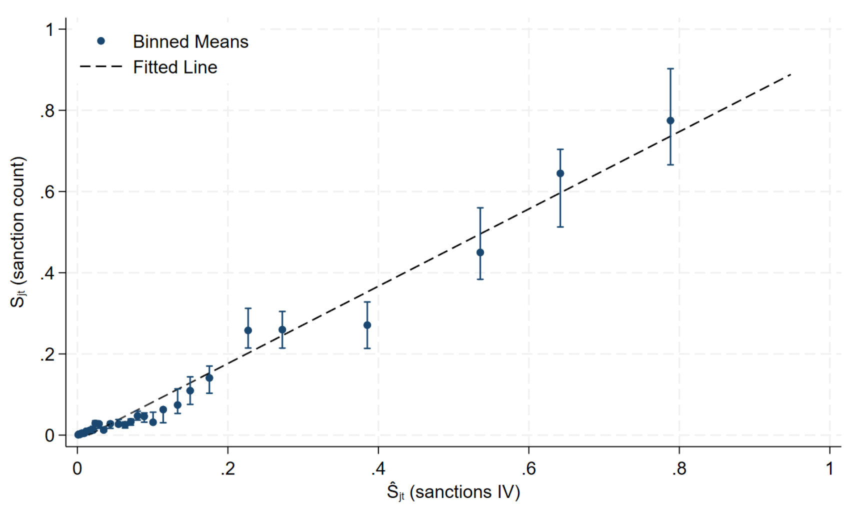
VARIABLES	(1) OLS	(2) Main IV	(3) LAG IV	(4) Logit Zero	(5) Poi. Zero	(6) OLS Zero	(7) No Ctrl.	(8) Full Ctrl.
Panel A. First Stage. Dependent Variable: SANCT								
$\hat{S}_{jt}$		1.102***	1.146***	1.105***	1.103***	0.383**	1.136***	1.144***
		(0.317)	(0.317)	(0.317)	(0.318)	(0.151)	(0.279)	(0.368)
Observations		6,480	6,480	6,480	6,480	6,480	7,765	5,531
R^2		0.460	0.463	0.460	0.459	0.415	0.488	0.488
First Stage F		12.07	13.06	12.14	12.01	6.45	16.61	9.67
p-value		0.00	0.00	0.00	0.00	0.01	0.00	0.00
Panel B. 2SLS Regression. Dependent Variable: log GDP per capita								
SANCT	-0.141***	-0.396**	-0.386**	-0.396**	-0.398**	-0.197	-0.540**	-0.392*
	(0.0358)	(0.196)	(0.190)	(0.196)	(0.197)	(0.202)	(0.240)	(0.213)
ln(capital)	0.258***	0.266***	0.265***	0.266***	0.266***	0.260***		0.264***
	(0.0364)	(0.037)	(0.037)	(0.037)	(0.037)	(0.037)		(0.039)
human capital	0.322**	0.289**	0.290**	0.289**	0.289**	0.315**		0.375**
	(0.138)	(0.138)	(0.138)	(0.138)	(0.138)	(0.144)		(0.161)
Observations	6,480	6,480	6,480	6,480	6,480	6,480	7,765	5,531

Notes: This table reports the main IV regression results of the impact of sanctions on real GDP per capita. Panel A presents the first-stage regression results, where the dependent variable is the sanction count S_{jt} and the key independent variable is the instrumented sanction count $\hat{S}_{jt}$. Panel B presents the second-stage regression results, where the dependent variable is the log of real GDP per capita and the key independent variable is the sanction count S_{jt}. All regressions include region-year and country fixed effects. Columns (1) presents the OLS results for comparison. Column (2) presents our preferred IV specification using a 'leave-one-out' measure of sender aggressiveness and column (3) presents an alternative specification where we use a one-year lag of sender aggressiveness. Columns (4)-(6) present robustness checks based on alternative stage-zero estimators. Columns (7) and (8) present robustness checks based on alternative covariates. In column (7), we exclude all control variables except for fixed effects, while in column (8), we include additional control variables for trade openness, WTO membership, EU membership, and an index for average hostility against other countries. Standard errors clustered at the target country level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Panel A of Table 3 reports the first-stage estimates specified in equation (6). The results confirm the instrument's relevance. In our preferred specification (column 2), the instrument $\hat{S}_{jt}$ is a strong, positive, and highly significant predictor of the actual sanction count S_{jt}. The estimate of the coefficient, $\beta_S^{1st} = 1.102$, is not statistically distinguishable from unity, implying a near one-to-one relationship between predicted and actual sanctions. The Kleibergen-Paap F-statistic of 12.07 comfortably exceeds conventional thresholds, mitigating concerns of a weak instrument. Most other specifications feature similarly strong results. The notable exception is column (6), derived from a linear stage-zero model, which yields a low F-statistic (6.45) and is thus unreliable.

Figure 4 provides a visualization of the first-stage relationship from column (2), excluding fixed effects. The figure plots binned means of the observed sanction count S_{jt} against the instrumented values $\hat{S}_{jt}$ (Cattaneo et al., 2024). The relationship is markedly linear, with a fitted slope near one and an intercept near zero, confirming the instrument's strong predictive power across the entire distribution.

Figure 4: First-Stage Relationship Between Instrumented and Actual Sanctions



Notes: The figure plots binned means of the actual sanction count S_{jt} against the instrumented values $\hat{S}_{jt}$, along with a fitted line from the first-stage regression. The instrument $\hat{S}_{jt}$ is constructed from sender-specific aggressiveness measures. Binned means and corresponding 95% confidence intervals are estimated using the `binsreg` package (Cattaneo et al., 2024).

Panel B of Table 3 reports the 2SLS estimates. Our preferred specification (column 2) yields a coefficient on $SANCT$ of -0.396, which is highly significant. This implies that an additional sanction reduces real GDP per capita by approximately 0.4 percent. This effect is more than twice the magnitude of the OLS coefficient (-0.141) reported in column (1). This discrepancy confirms that OLS estimates are biased, severely understating the adverse impact of sanctions. The coefficients on control variables (physical and human capital) remain stable relative to the OLS specification.

The attenuation in the OLS estimates likely reflects endogenous selection bias. Senders often strategically target adversaries that are experiencing rapid economic growth or growing in influence (Becko, 2024; Mattoo et al., 2024; Clayton et al., 2025). Examples include US sanctions against the Soviet Union during the Cold War (Drezner, 2021) and on Chinese firms since 2018 (Hufbauer and Jung, 2020). This strategic selection induces a positive correlation between the error term and the sanction variable, biasing the OLS coefficient toward zero. Our IV strategy, by design, isolates exogenous variation in sanctions uncorrelated with such target-specific growth dynamics.

The remaining columns of Table 3 confirm the robustness of our results. Column (3) instruments $SANCT$ using the lagged aggressiveness index, $AGGRSS_{i,t-1}$. The resulting coefficient (-0.386) is statistically indistinguishable from our main estimate (-0.396), mitigating concerns about global contemporaneous shocks. We therefore rely on the $AGGRSS_{i,-j,t}$ instrument for the remainder of the analysis. Columns (4)–(6) test alternative stage-zero estimators. The 2SLS estimates are robust to using a Logit (column 4) or PPML (column 5) model in stage zero. In contrast, a linear probability model (column 6) produces an insignificant second-stage coefficient.

Finally, columns (7) and (8) confirm the robustness of our IV specification to alternative sets of covariates. Column (7) omits all time-varying controls, while column (8) includes additional controls (trade openness, WTO membership, EU membership, and a hostility index). The resulting coefficients on $SANCT$ (-0.540 and -0.392, respectively) are not statistically distinguishable from our baseline estimate in column (2).

The analysis in this section yields three conclusions. First, from a policy perspective, economic sanctions have a large and statistically significant negative impact on contemporaneous economic growth. Our preferred estimate suggests a 0.4 percent reduction in real GDP per capita per sanction, a magnitude much more comparable to previous findings (Neuenkirch and Neumeier, 2016; Felbermayr et al., 2020b; Laudati and Pesaran, 2023). Second, from a methodological perspective, our IV strategy, which exploits sender-side aggressiveness, is shown to be both relevant and robust. Third, from an econometric perspective, we demonstrate that OLS estimates are severely attenuated, significantly under-predicting the true economic cost of sanctions.

4.2 On the Heterogeneous Effects of Sanctions

We now explore heterogeneous effects, demonstrating the flexibility of our IV framework. We adapt the baseline specification from Table 3 (column 2) by constructing dimension-specific instruments. To assess the impact of trade sanctions, for example, we define the $AGGRSS_{it}$ index using only senders' trade sanctions. This approach leverages the fact that different sanction types exhibit distinct temporal patterns.

4.2.1 Heterogeneous Effects by Sanction Type

We first examine heterogeneity by sanction type. Table 4 presents the stage-zero Probit estimates. Column (1) reproduces the baseline results for comparison; subsequent columns report estimates for each specific sanction type. The coefficients, particularly on $AGGRSS_{it}$ and its interactions, exhibit considerable heterogeneity. For instance, the main effect of $AGGRSS_{it}$ is strongly positive for trade sanctions (column 2) but negative for military assistance sanctions (column 6).

While some individual coefficients in Table 4 appear counterintuitive, this is the result of the complex specification, which includes numerous interaction terms, high-dimensional

Table 4: Stage Zero by Sanction Type, 1950-2019

VARIABLES	(1) ALL	(2) TRADE	(3) FINCL	(4) TRAVL	(5) ARMS	(6) MLTRY	(7) OTHER
AGGRSV	5.696***	22.567***	-0.760	18.931***	29.627***	-13.301***	-31.387***
	(0.927)	(2.494)	(1.832)	(3.411)	(2.956)	(2.939)	(11.135)
AGGRSV#LANG	0.246**	-1.349***	-0.128	-2.434***	0.203	0.012	-3.436***
	(0.117)	(0.314)	(0.249)	(0.397)	(0.347)	(0.195)	(0.778)
AGGRSV#CLNY	0.186	0.948	-2.086***	-0.061	0.529	0.933**	6.346*
	(0.257)	(0.801)	(0.672)	(1.190)	(1.259)	(0.441)	(3.487)
AGGRSV#CNTG	0.125	-1.173	-1.178	-1.039	-1.414	0.392	-1.184
	(0.558)	(0.895)	(0.799)	(1.069)	(1.167)	(1.270)	(2.280)
AGGRSV#DIST	-0.567***	-2.291***	0.192	-2.547***	-3.644***	1.528***	1.406
	(0.106)	(0.289)	(0.214)	(0.407)	(0.350)	(0.321)	(1.285)
LANG	-0.121***	-0.066***	0.011	0.258***	-0.075*	0.052**	0.187***
	(0.026)	(0.025)	(0.029)	(0.039)	(0.045)	(0.026)	(0.047)
CLNY	-0.200**	-0.165*	0.328***	0.136	-0.031	-0.341**	-0.108
	(0.087)	(0.098)	(0.121)	(0.171)	(0.187)	(0.143)	(0.148)
CNTG	0.102	0.127	0.171*	0.030	0.342**	0.053	0.011
	(0.099)	(0.078)	(0.094)	(0.114)	(0.168)	(0.140)	(0.112)
Observations	442 578	252 393	304 201	261 749	307 124	243 978	165 609
DIST#Year	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES

Notes: This table reports the stage zero Probit regression results of the impact of sender aggressiveness on the probability of sanction imposition, differentiated by sanction type. To ease comparison, column (1) reproduces the stage zero results for all sanction types. Columns (2) through (7) report estimates for each success category: trade (column 2), financial (column 3), travel (column 4), arms (column 5), military assistance (column 6), and other sanctions (column 7). The dependent variable is a binary indicator equal to one if country j is sanctioned by any sender i in year t for a specific sanction type. The key independent variable is the sender aggressiveness index (AGGRSV) and its interaction terms with bilateral characteristics. Standard errors clustered at the sender-target-pair level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

fixed effects, and high correlation between the covariates. Simpler specifications, presented in Appendix Tables A.5 (omitting interactions) and A.6 (single regressor), confirm that the underlying correlations are intuitive. For instance, $AGGRSS_{it}$ is positively correlated with all sanction types, while $LANG_{ij}$, $CLNY_{ij}$, and $DIST_{ij}$ are generally negatively correlated. Thus, we conclude that the type-specific stage-zero models capture the intended exogenous variation through main effects, interaction terms, and high-dimensional fixed effects.

Panel A of Table 5 reports the first-stage estimates. The results confirm the relevance of our type-specific instruments. In all specifications, the Kleibergen-Paap F-statistic deci-

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

VARIABLES	(1) ALL	(2) TRADE	(3) FINCL	(4) TRAVL	(5) ARMS	(6) MLTRY	(7) OTHER
Panel A. First Stage. Dependent Variable: SANCT							
$\hat{S}_{jt}$	1.102***	1.289***	1.133***	1.050***	1.046***	0.853***	1.765***
	(0.317)	(0.242)	(0.239)	(0.286)	(0.255)	(0.219)	(0.258)
Observations	6 480	4 604	4 486	1 945	2 829	5 721	3 468
R^2	0.460	0.327	0.482	0.500	0.471	0.417	0.513
First Stage F	12.07	28.42	22.39	13.49	16.81	15.17	46.87
p-value	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Panel B. 2SLS Regression. Dependent Variable: log GDP per capita							
SANCT	-0.396**	-0.583***	-0.338**	-0.224	-0.459**	-0.541***	-0.055
	(0.196)	(0.220)	(0.148)	(0.146)	(0.178)	(0.181)	(0.155)
Observations	6 480	4 604	4 486	1 945	2 829	5 721	3 468
Panel C. 2SLS Regression (GDP Weighted)							
SANCT	-0.410**	-0.599**	-0.339**	-0.223	-0.442**	-0.546***	-0.055
	(0.203)	(0.230)	(0.149)	(0.146)	(0.170)	(0.183)	(0.158)
Observations	6 480	4 604	4 486	1 945	2 829	5 721	3 468

Notes: This table presents a series of estimates on the impact of sanctions on growth, differentiated by sanction type. To ease comparison, our main results for any types of sanctions from column (2) of Table 3 are reproduced in column (1). Columns (2) through (7) reproduce our main specification for each type of sanction: 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). Panel A presents the first-stage regression results. Panel B presents the second-stage regression results. Panel C reports estimates based on GDP-weighted sanctions. All regressions include region-year and country fixed effects, as well as controls for physical and human capital. Standard errors clustered at the target country level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

sively rejects the null of weak instruments. Furthermore, the coefficient on the instrument $\hat{S}_{jt}$ is not statistically distinguishable from unity for nearly all sanction types (the exception being 'other' in column 7), indicating strong predictive power.

Panel B of Table 5 presents the 2SLS estimates, which reveal significant heterogeneity. Trade sanctions (column 2) exert the largest negative impact (-0.583), an intuitive result given their broad economic scope. Financial sanctions (column 3) are also significant and negative (-0.338), but smaller in magnitude. This is consistent with the 'smart' sanction hypothesis, which posits that such measures are designed to target specific entities rather than the entire economy (Drezner, 2011; Ahn and Ludema, 2020). This interpretation is further supported by the small and statistically insignificant coefficients for travel (column 4) and other (column 7) sanctions. Notably, sanctions on arms (column 5) and military assistance (column 6) yield large, significant negative estimates (-0.459 and -0.541, respectively), possibly reflecting the diversion of domestic resources toward military buildup.

Finally, panel C of Table 5 reports a robustness check where the endogenous variable, S_{jt}, is a weighted sum of sanctions based on the sender's share of world GDP. This specification allows sanctions imposed by larger economies to have a greater impact.[27] The resulting estimates are qualitatively and quantitatively similar to those in panel B. This is unsurprising, as the principal senders in our sample are also the largest economies (see Table 1), implying a high correlation between the weighted and unweighted sanction counts.

4.2.2 Heterogeneous Effects by Sanction Success Score

We next examine heterogeneous effects based on the political success of sanctions. The relationship between the economic impact of sanctions and their political efficacy remains a central question in the literature (Galtung, 1967; Pape, 1997; Hufbauer et al., 2009).[28] We contribute to this debate by testing whether politically successful sanctions impose a larger negative impact on target-country growth.[29]

Table 6 reports the stage-zero estimates, and Table 7 presents the first-stage as well as the 2SLS results. As in the analysis by type, the stage-zero coefficients in Table 6 exhibit significant heterogeneity, and interpretation of individual coefficients is challenging. However, simpler specifications, reported in Appendix Tables A.7 and A.8, confirm that the underlying correlations remain intuitive. For instance, sender aggressiveness increases the likelihood of sanctions across all success categories, while common language,

[27]Stage-zero results for the GDP-weighted regressions are the same as panel A. The first-stage regression results are very similar to panel B and are available upon request.

[28]Galtung (1967) critiqued what he termed the "naive theory of sanctions," which posits a direct causal chain: economic pressure from sanctions leads to popular discontent, which in turn compels the target government to concede to the sender's demands. However, Galtung and subsequent studies, such as Pape (1997), argued that this theory often fails because it overlooks the target's internal political dynamics. Instead of fostering opposition, sanctions can trigger a "rally around the flag" effect, unifying the population behind the leadership and allowing the regime to blame external forces for economic hardships. This suggests that the relationship between the economic impact of sanctions and their political success is not straightforward. Our analysis sheds light on this issue by examining whether sanctions that are deemed successful are also those that impose the greatest economic costs.

[29]Sanction success is defined as the achievement of stated policy objectives. See Section 3.1 for details.

colonial ties, and distance are generally associated with a lower probability. Thus, we conclude that the stage-zero specification captures the intended exogenous variation.

Table 6: Stage Zero by Success, 1950-2019

VARIABLES	(1) ALL	(2) FAIL	(3) NEGTN	(4) TSCCSS	(5) PSCCSS
AGGRSV	5.696***	17.710***	-46.107	21.923***	-24.473**
	(0.927)	(2.714)	(50.892)	(2.210)	(12.220)
AGGRSV#LANG	0.246**	-0.256	-3.903	-0.393	-5.565**
	(0.117)	(0.238)	(6.320)	(0.266)	(2.166)
AGGRSV#CLNY	0.186	0.251	7.933	-0.382	1.554
	(0.257)	(0.380)	(9.581)	(0.755)	(3.955)
AGGRSV#CNTG	0.125	-0.422	2.414	-0.197	5.107
	(0.558)	(1.665)	(16.074)	(0.970)	(6.564)
AGGRSV#DIST	-0.567***	-1.717***	4.771	-2.429***	1.537
	(0.106)	(0.299)	(5.948)	(0.260)	(1.477)
LANG	-0.121***	-0.299***	0.153	-0.081***	0.187***
	(0.026)	(0.025)	(0.135)	(0.025)	(0.047)
CLNY	-0.200**	-0.140	-0.461***	0.012	-0.069
	(0.087)	(0.099)	(0.178)	(0.113)	(0.181)
CNTG	0.102	0.040	-0.718***	-0.087	-0.205
	(0.099)	(0.071)	(0.269)	(0.080)	(0.167)
Observations	442 578	166 787	24 528	259 182	76 578
DIST#Year	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES

Notes: This table reports the stage zero Probit regression results of the impact of sender aggressiveness on the probability of sanction imposition, differentiated by sanction success score. To ease comparison, column (1) reproduces the stage zero results for all sanction types. Columns (2) through (5) report estimates for each success category: failed sanctions (column 2), sanctions ending in a negotiated settlement (column 3), total success (column 4), and partial success (column 5). The dependent variable is a binary indicator equal to one if country j is sanctioned by any sender i in year t for a specific sanction success score. The key independent variable is the sender aggressiveness index (AGGRSV) and its interaction terms with bilateral characteristics. Standard errors clustered at the sender-target-pair level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Panel A of Table 7 reports the first-stage estimates. The results confirm the relevance of our success-specific instruments. The Kleibergen-Paap F-statistics are well above conventional thresholds in all specifications, mitigating concerns of weak instruments. The coefficient on $\hat{S}_{jt}$ is not statistically distinguishable from unity in most columns (the exception being column 3), indicating strong predictive power.

Panel B reports the 2SLS estimates. The coefficients are negative across all success categories, suggesting an adverse growth impact regardless of the political outcome. How-

Table 7: Sanctions and Growth by Sanction Success Score, 1950-2019

VARIABLES	(1) ALL	(2) FAIL	(3) NEGTN	(4) TSCCSS	(5) PSCCSS
Panel A. First Stage. Dependent Variable: SANCT					
$\hat{S}_{jt}$	1.102***	1.766***	1.347***	1.564***	1.559***
	(0.317)	(0.389)	(0.094)	(0.395)	(0.347)
Observations	6 480	5 844	1 441	4 941	2 170
R^2	0.460	0.425	0.722	0.453	0.654
First Stage F	12.07	20.64	206.39	15.68	20.22
p-value	0.00	0.00	0.00	0.00	0.00
Panel B. 2SLS Regression. Dependent Variable: log GDP per capita					
SANCT	-0.396**	-0.257*	-0.099	-0.334*	-0.155
	(0.196)	(0.142)	(0.098)	(0.174)	(0.144)
Observations	6 480	5 844	1 441	4 941	2 170

Notes: This table presents a series of estimates on the impact of sanctions on growth, differentiated by sanction success score. To ease comparison, our main results for any types of sanctions from column (2) of Table 3 are reproduced in column (1). Columns (2) through (5) reproduce our main specification for each sanction success score: failed sanctions in column (2), sanctions that ended in negotiation settlement in column (3), sanctions that were a total success in column (4), and sanctions that were a partial success in column (5). Panel A presents the first-stage regression results. Panel B presents the second-stage regression results. All regressions include region-year and country fixed effects, as well as controls for physical and human capital. Standard errors clustered at the target country level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

ever, the estimates vary in magnitude and statistical significance. Specifically, the coefficients for sanctions ending in 'negotiation settlement' (column 3) and 'partial success' (column 5) are small and not statistically significant. In contrast, sanctions classified as 'failed' (column 2, -0.257) and 'total success' (column 4, -0.334) both have significant negative effects on growth. These results suggest no clear correlation between the magnitude of the economic impact and the degree of political success.

4.2.3 Medium and Long-Term Effects of Sanctions

We conclude the analysis in this section by investigating the medium and long-term effects of sanctions on growth. As discussed in Dai et al. (2021) and Gutmann et al. (2023), the short-term estimates of the impact of various shocks on growth may differ significantly from their medium and long-term effects. Therefore, it is important to assess whether our main results on the negative impact of sanctions on growth persist over longer horizons.

To this end, we amend our main econometric model to adopt the following log-difference regression specification:

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

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,[30] $\mathbf{\Delta_T Z_{jt}}$ is the vector of changes of the control variables (i.e., physical and human capital) during the same period, Ψ_{tR} denotes the region-year fixed effects, and ϵ_{jt} is the error term.[31] We choose ts 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 leverage the flexibility of our instrument, which we construct as the cumulative number of predicted sanctions from stage zero, $\sum_{n=t-T+1}^{t} \hat{S}_{jn}$.

Our findings are presented in Table 8, where we include our main results in column (1) for comparison purposes. The results in columns (2), (3), and (4) correspond to 4-year, 8-year, and 12-year intervals, respectively. We omit the stage-zero results as they are the same as those in column (2) of Table 2.

The main conclusion from Table 8 is that the negative impact of sanctions on growth diminishes over time. This is evidenced by the IV estimates on $SANCT$ in panel B, which decrease in magnitude and lose statistical significance as the time horizon under investigation increases. However, we caution that the interpretation of no long-run effects of sanctions should be approached with care. Specifically, sanctions may still impact long-term growth through physical capital, which we control for explicitly in our regressions in Table 8. Additionally, different types of sanctions (e.g., trade, smart and arms sanctions) may have varying effects on long-run growth. These policy-relevant questions warrant

[30] To be specific, when we set $T = 4$, the regression specification investigates how changes in the total number of sanctions in between 1981 to 1984, for instance, affect the 4-year change in GDP per capita from 1980 to 1984.

[31] Due to the differencing, the country fixed effects from our main specification drop out.

Table 8: 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. First Stage				
$\hat{S}_{jt}$	1.102***	0.930***	0.931***	0.930***
	(0.317)	(0.099)	(0.129)	(0.155)
Observations	6 480	1 572	766	454
R^2	0.460	0.341	0.371	0.395
First Stage F	12.07	88.79	51.85	36.04
p-value	0.00	0.00	0.00	0.00
Panel B. IV Regression				
SANCT	-0.396**	-0.010*	-0.010	-0.009
	(0.196)	(0.005)	(0.007)	(0.007)
ln(capital)	0.266***	0.160***	0.199***	0.209***
	(0.037)	(0.026)	(0.036)	(0.034)
human capital	0.289**	-0.024	-0.007	0.011
	(0.138)	(0.062)	(0.080)	(0.098)
Observations	6 480	1 572	766	454

Notes: This table presents estimates of the medium- and long-term effects of sanctions on growth. To ease comparison, our main contemporaneous results from column (2) of Table 3 are reproduced in column (1). Columns (2) through (4) present results from long-difference IV regressions for 4-year, 8-year, and 12-year intervals, respectively. Panel A presents the first-stage regression results. Panel B presents the second-stage IV regression results. Heteroskedasticity robust standard errors are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

further research. Methodologically, we note that the IV strategy that we propose in this paper is flexible enough to accommodate such extensions in future work.

5 Concluding Remarks

This paper estimates the impact of sanctions on target-country economic growth using a novel instrumental variable (IV) strategy. We address endogeneity arising from omitted variable bias (e.g., unobserved domestic instability) and selection bias (e.g., the strategic targeting of high-growth adversaries). Our instrument leverages variation in sender-country aggressiveness, which is driven by institutional and political factors plausibly exogenous to target-country economic outcomes.

Our 2SLS estimates reveal that OLS substantially understates the adverse effects of sanctions. We find that each additional sanction reduces a target's real GDP per capita by 0.4 percent—an effect more than double the magnitude implied by the corresponding OLS estimate.

We demonstrate the flexibility of this IV strategy by investigating heterogeneous effects across sanction types and over different time horizons, uncovering considerable variation. Our IV design provides a broadly applicable framework for future empirical research on the economic consequences of sanctions.

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A Supplementary Appendix

This appendix provides supplementary descriptive statistics and estimation results. Table A.1 lists the target countries and their respective sanction counts. Tables A.3 and A.4 define the sanction types and success scores, while Table A.2 reports summary statistics. Tables A.5 through A.8 present stage-zero estimates from specifications excluding interaction terms or using single regressors. Finally, Figures A.1–A.3 visualize the time-varying effects and year fixed effects from the stage-zero model.

Table A.1: Sanctioned Countries

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

Table A.2: Summary Statistics

Variable	Mean	Median	Std. Dev.	Min	Max
Panel A. Variables in Stage Zero					
AGGRSV	14.93	14	13.3	0	148
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 B. Variables in First Stage and IV Regression					
year	1987.4	1989	19.79	1948	2019
ln(GDPPC)	8.36	8.25	1.54	4.89	12.19
ln(capital)	11.14	11.12	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)	11.03	2.5	18.69	.03	96.18
trade sanction	6.18	0	26.72	0	191
$\hat{S}$ (trade)	6.18	1.08	11.54	.02	79.19
financial sanction	8.47	0	34.35	0	192
$\hat{S}$ (financial)	8.46	.57	19.54	0	105.06
travel sanction	12.89	0	44.24	0	192
$\hat{S}$ (travel)	12.89	.55	25.96	0	110.88

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

Table A.3: Types of Sanctions

Sanction Type and Explanation
Trade Sanctions: Measures that aim to restrain economic interactions with a target country by limiting international trade.
Financial Sanctions: Restrictions on financial transactions, such as freezing the exchange of financial assets and investments, or limiting the availability of credit.
Travel Restrictions: Limits on the freedom of geographical movement of individuals. This includes restrictions on entry into the sender country, or journeys from the sanctioning to the sanctioned country.
Arms Sanctions: Restrictions on arms sales, specifically banning arms exports to and/or arms imports from a sanctioned country.
Military Assistance: Bans covering either monetary or personal assistance related to military activities.
Other Types: A residual category for less frequently occurring sanctions, primarily entailing diplomatic measures (e.g., exclusion from international bodies, interruption of diplomatic relations), as well as flight and harbor restrictions.

Source: Felbermayr et al. (2020a).

Table A.4: Sanction Success Scores

Success Score and Explanation
Partial Success/Achievement: The GSDB classifies a sanction as partially successful if the sanctioned state partially accepts the requests from the sanctioning state(s). For example, US sanctions against Ecuador in 1995 were partially lifted following the deployment of an international observer group.
Full Success/Achievement: The GSDB classifies a sanction as fully successful when a sanctioned state fully accepts the requests of the sanctioning state(s). A notable example is the US sanction against Haiti in 1991, which led to the return of President Jean-Bertrand Aristide.
Settlement by Negotiations: This category indicates whether the sanctioning and sanctioned parties agree to settle a conflict by negotiations. This can apply to war-related, human rights, democracy, and territorial conflict objectives. For instance, the EU's arms sanction on Ethiopia and Eritrea in 1999 aimed for a peace agreement.
Enhancement/Failure: Sanctions are classified here when the reason for the policy persists or even strengthens, or when the imposing countries' priorities change, leading to a lifting of sanctions even if the original objective was not met. An example is the failed US military sanction on Indonesia, where human rights objectives were superseded by counter-terrorism priorities.

Source: Felbermayr et al. (2020a).

Table A.5: Stage Zero by Type, 1950-2019 without Interaction Terms

VARIABLES	(1) ALL	(2) TRADE	(3) FINCL	(4) TRAVL	(5) ARMS	(6) MLTRY	(7) OTHER
AGGRSV	2.337***	4.065***	4.916***	9.985***	6.507***	13.270***	9.583***
	(0.078)	(0.123)	(0.129)	(0.139)	(0.147)	(0.462)	(0.285)
LANG	-0.082***	-0.171***	-0.043**	0.124***	-0.018	0.019	-0.065**
	(0.016)	(0.018)	(0.018)	(0.018)	(0.017)	(0.028)	(0.025)
CLNY	-0.284***	-0.160**	-0.317***	-0.333***	-0.164**	-0.881***	-0.062
	(0.056)	(0.067)	(0.066)	(0.111)	(0.076)	(0.190)	(0.106)
CNTG	0.110**	0.043	0.139***	0.136**	0.248***	0.147*	-0.026
	(0.047)	(0.060)	(0.045)	(0.057)	(0.051)	(0.084)	(0.064)
DIST	-0.041***	-0.066***	-0.000	0.059***	0.092***	0.018	0.031*
	(0.011)	(0.013)	(0.011)	(0.011)	(0.012)	(0.017)	(0.016)
Observations	442 578	252 393	304 201	261 749	307 124	243 978	165 609
DIST#Year	NO	NO	NO	NO	NO	NO	NO
Year FE	NO	NO	NO	NO	NO	NO	NO

Notes: This table reproduces the stage zero regression by sanction type in Table 4 without the interaction terms of sender aggressiveness as well as distance and year fixed effects and year fixed effects. Standard errors clustered at the sender-target-pair level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.6: Stage Zero by Type, 1950-2019, Single Regressor

VARIABLES	(1) ALL	(2) TRADE	(3) FINCL	(4) TRAVL	(5) ARMS	(6) MLTRY	(7) OTHER
AGGRSV	2.340***	4.149***	4.894***	9.757***	6.351***	13.155***	9.622***
	(0.078)	(0.126)	(0.127)	(0.137)	(0.145)	(0.449)	(0.284)
LANG	-0.112***	-0.194***	-0.089***	-0.033**	-0.081***	-0.064***	-0.095***
	(0.014)	(0.018)	(0.014)	(0.015)	(0.015)	(0.020)	(0.023)
CLNY	-0.108**	-0.034	-0.096*	-0.064	-0.078	-0.408***	-0.097
	(0.049)	(0.070)	(0.052)	(0.081)	(0.066)	(0.112)	(0.095)
CNTG	0.075*	0.049	-0.005	0.008	0.065	-0.022	-0.139***
	(0.039)	(0.056)	(0.038)	(0.044)	(0.041)	(0.060)	(0.050)
DIST	-0.050***	-0.079***	-0.026***	-0.048***	-0.013	-0.025*	0.042***
	(0.010)	(0.012)	(0.009)	(0.010)	(0.010)	(0.013)	(0.013)
Observations	442 578	252 393	304 201	261 749	307 124	243 978	165 609

Notes: This table presents the stage zero regression by sanction types where we regress only one regressor at a time to show the correlation with sanctions imposition. Standard errors clustered at the sender-target-pair level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.7: Stage Zero by Success, 1950-2019 without Interaction Terms

VARIABLES	(1) ALL	(2) FAIL	(3) NEGTN	(4) TSCCSS	(5) PSCCSS
AGGRSV	2.337***	3.020***	13.866***	3.744***	9.068***
	(0.078)	(0.191)	(2.766)	(0.110)	(0.557)
LANG	-0.082***	-0.277***	0.080	-0.113***	0.121***
	(0.016)	(0.023)	(0.091)	(0.016)	(0.027)
CLNY	-0.284***	-0.131	-0.501***	-0.211***	-0.447***
	(0.056)	(0.092)	(0.112)	(0.071)	(0.104)
CNTG	0.110**	-0.011	-0.572***	-0.085	0.023
	(0.047)	(0.084)	(0.205)	(0.055)	(0.084)
DIST	-0.041***	-0.091***	-0.278***	-0.062***	0.005
	(0.011)	(0.021)	(0.074)	(0.012)	(0.018)
Observations	442 578	166 787	24 528	259 182	76 578
DIST#Year	NO	NO	NO	NO	NO
Year FE	NO	NO	NO	NO	NO

Notes: This table reproduces the stage zero regression by sanction success score in Table 6 without the interaction terms of sender aggressiveness as well as distance and year fixed effects and year fixed effects. Standard errors clustered at the sender-target-pair level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

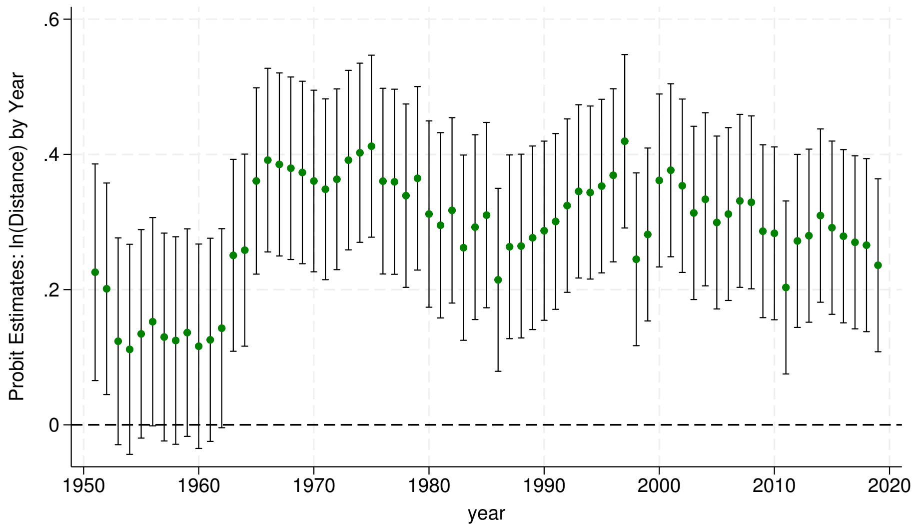
Table A.8: Stage Zero by Success, 1950-2019, Single Regressor

VARIABLES	(1) ALL	(2) FAIL	(3) NEGTN	(4) TSCCSS	(5) PSCCSS
AGGRSV	2.340***	3.104***	9.486***	3.823***	8.651***
	(0.078)	(0.210)	(2.640)	(0.111)	(0.529)
LANG	-0.112***	-0.299***	0.052	-0.175***	0.068***
	(0.014)	(0.022)	(0.085)	(0.016)	(0.023)
CLNY	-0.108**	-0.061	-0.348***	-0.093	-0.291***
	(0.049)	(0.082)	(0.092)	(0.067)	(0.098)
CNTG	0.075*	-0.022	-0.212	-0.095*	-0.034
	(0.039)	(0.079)	(0.134)	(0.052)	(0.070)
DIST	-0.050***	-0.079***	-0.160***	-0.060***	0.009
	(0.010)	(0.019)	(0.060)	(0.012)	(0.014)
Observations	442 578	166 787	24 528	259 182	76 578

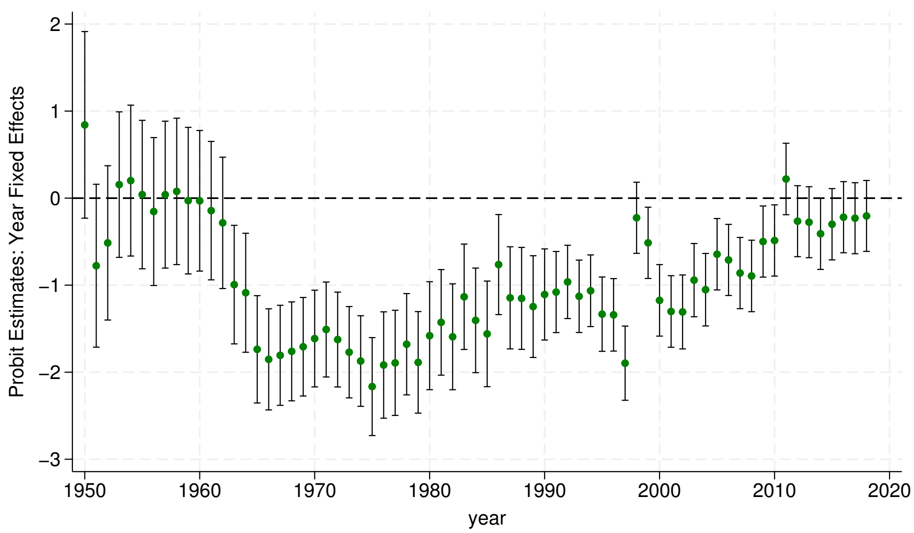
Notes: This table presents the stage zero regression by sanction success score where we regress only one regressor at a time to show the correlation with sanctions imposition. Standard errors clustered at the sender-target-pair level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figure A.1: Sanctions: Evolution and Geography

(a) Time-varying effects of distance on sanctions



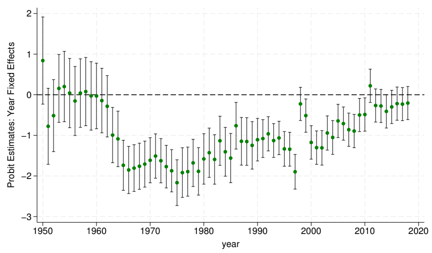
(b) Evolution of the bilateral probability of sanctions



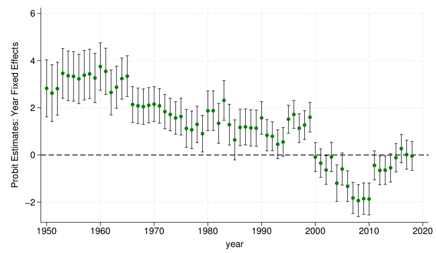
Notes: This figure visualizes the estimates of two sets of covariates corresponding to the our baseline results from column (2) of Table 3. Panel (a) shows the estimates of the time-varying effects of the distance variable on sanctions and their confidence intervals. Panel (b) includes the estimates and confidence intervals of the year fixed effects.

Figure A.2: Incidence of Sanctions by Types

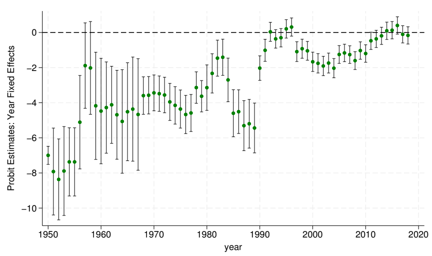
(a) Any



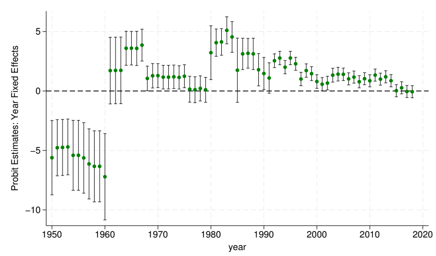
(b) Trade



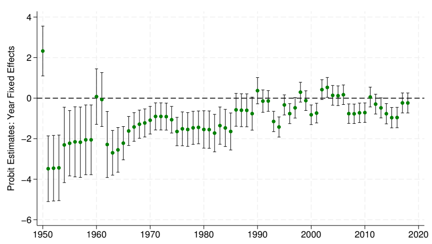
(c) Financial



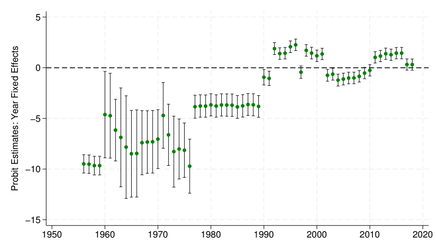
(d) Travel



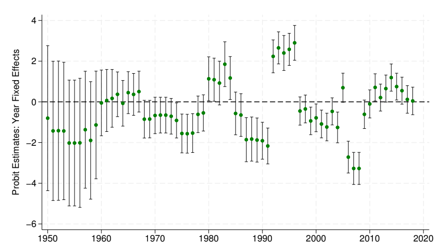
(e) Arms



(f) Military

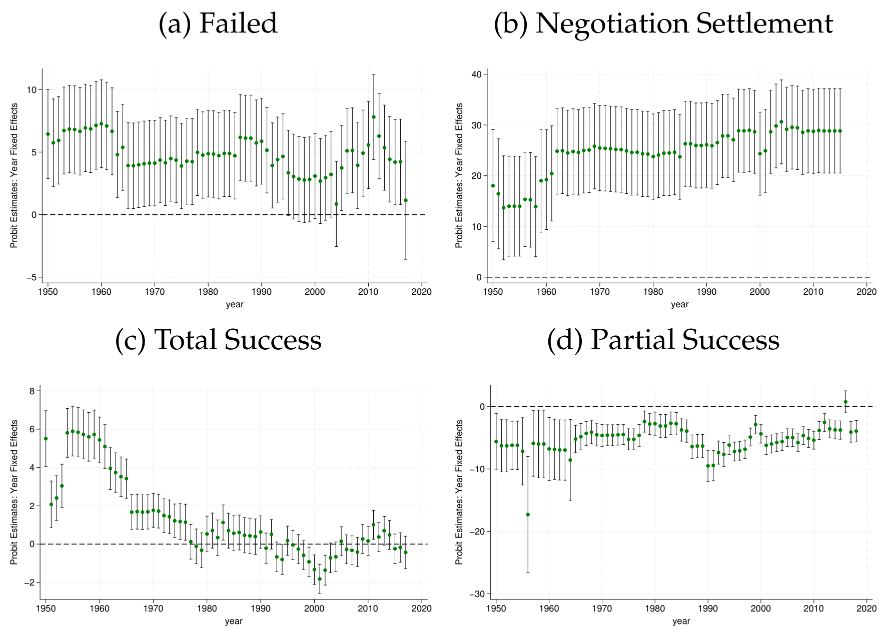


(g) Other



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 A.3: 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.