

Missed Targets: A Mixed-Methods Analysis of Targeted Sanctions, Financial Sanctions, and Income Inequality

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Abstract: This research builds upon previous scholarship to investigate the impact that targeted and financial sanctions have on income inequality in the recipient state. Data analysis for 80 countries between 1989 and 2015 demonstrates that targeted sanctions increase income inequality in targets with concentrated trade portfolios, but this effect diminishes as the trade portfolio is diversified. Financial sanctions decrease income inequality when values of elite wealth are low but increase inequality when elite wealth is very high. The implications of these results are explored through two case studies: Iran from 2006 to 2012, and Sudan from 2000 to 2009. These findings suggest that targeted sanctions have far more economic externalities than previously assumed. Additionally, the impact and efficacy of financial sanctions may be weakened due to increased evasion.

Key Words: Targeted sanctions; Financial sanctions; Income inequality; Iran; Sudan

I. Introduction

Economic statecraft is a dominant approach to modern foreign policy. Many countries and international organizations increasingly use economic pressure to exert their influence and demand policy change in recipient states, with the number of sanctions cases reaching an all-time high of 400 in 2022 (Morgan et al. 2003, 5). Though sanctions are regarded as a less costly alternative to war, these economic tools can have broad and disastrous effects, such as increasing poverty (Neuenkirch and Neumeier, 2016, 110) worsening levels of democracy (Peksen and Drury 2010, 240), harming respect for human rights (Peksen 2009, 59), deteriorating health care systems (Yazdi-Feyzabadi et al. 2024, 2), and more.

Due to their popularity as a policy tool, considerable research has emerged around economic sanctions concerning both their efficacy and impacts. Consequently, many states have

begun to recognize the catastrophic effects of sanctions, leading them to opt for “smart” or targeted sanctions placed on individuals, companies, and entities associated with the offending government rather than imposing embargos or sanctions on the entire country. However, although these smart sanctions theoretically limit economic harm to the specific targets (Drezner 2015, 756–7), they are still a clear attempt to change the economic landscape of a given country. By withholding the wealth or assets of certain entities, the imposing state introduces new economic factors and incentives that very well may impact the general population. Thus, it is crucial to learn whether targeted sanctions truly succeed at isolating the impact of the sanction to specific individuals, and if not, to what extent they affect economic conditions in the recipient country. As targeted sanctions are viewed as more refined than other sanction types, I compare targeted sanctions to their comprehensive counterparts to provide a more accurate narrative around the negative externalities associated with various sanction types.

Specifically, I examine the effects of targeted and financial sanctions on income inequality — a single measure that indicates whether the sanctions are hitting the wealthiest citizens, or the poorest. Additionally, income inequality is a critical measure in and of itself. The income distribution of a given country is associated with development and long-term economic growth (Kuznets 1955; Stiglitz 2015), influences the division of political power and rates of political engagement (Solt 2015), and is even related to the health of a given country’s population (Wilkinson 2006). Ultimately, I find that targeted sanctions have a positive impact on income inequality — an effect which increases as the trade portfolio of the recipient state concentrates around fewer industries. Financial sanctions decrease income inequality in the target state, particularly when levels of elite wealth are low.

II. Literature Review

Previous research has found causal links between several sanction types and income inequality. This specific vein of sanctions scholarship begins with J. H. Cooper, who is attributed as the first to research the impact of sanctions on economic inequality. In 1989, by examining sanctions on South Africa through the Stolper-Samuelson theorem, he demonstrated that the position of capital was likely to be strengthened by sanctions at the expense of labor (Cooper 1989, 14).

In 2016, Afesorgbor and Mahadevan found a strong positive effect of sanctions on income inequality — on average, “income inequality is greater by 1.5–1.7 points in countries and years in which economic sanctions were imposed” (Afesorgbor and Mahadevan 2016, 5). However, the relationship between sanctions and income inequality is not consistent across all sanction types. Afesorgbor and Mahadevan observed that financial sanctions increase income inequality while import sanctions reduce income inequality (Afesorgbor and Mahadevan 2016, 7). In 2020, Jin Mun Jeong built upon this work to consider the effect of foreign aid sanctions on income inequality, finding that aid sanctions disproportionately impact upper-income groups in the target country, reducing income inequality (Jeong 2020, 689). Furthermore, this effect increases with the amount of foreign aid the target country receives.

After reviewing the literature, it is clear that sanctions can have drastic effects on the economy of the target state. However, these effects are not consistent across the specific sanction instruments used. Even when just analyzing financial sanctions, there are contradictory results. Broadly, Afesorgbor and Mahadevan found that they increase inequality, whereas Jeong observed that financial aid sanctions — the most common type of financial sanction — decrease inequality (Afesorgbor and Mahadevan 2016, 7; Jeong 2020, 678). This discrepancy emphasizes

how crucial it is to research specific sanction types, as each can have their own discrete impact on the economy.

There is a significant gap in quantitative sanctions literature concerning targeted sanctions. Most studies that examine targeted sanctions either focus on their effectiveness or survey the political impacts of these targeted tools, as with Park and Choi's 2022 exploration of targeted sanctions and human rights (Park and Choi 2022). Studies that do analyze how targeted sanctions impact economic conditions are limited in scope, such as Ahn and Ludema's examination of how targeted sanctions affected Russian firm performance from 2014–2016 (Ahn and Ludema 2020). Furthermore, many studies only consider targeted sanctions issued by international organizations such as the United Nations and the European Union, excluding notable sanctions issued by individual countries (Biersteker and Zuzana Hudáková 2021; Ghodsi and Karamelikli 2022; Giumelli 2015; Portela 2014).

Ultimately, I contribute to the literature by exploring the relationship between targeted and financial sanctions and income inequality, focusing on sanctions issued by the United States, the European Union, and the United Nations from 1989 to 2015. I employ a mixed-methods sequential exploratory design to do so. I begin with a large-N quantitative study estimating the average effect of targeted and financial sanctions on target states' Gini coefficients, incorporating the sanctions data from the EUSANCT database version 2.0 (Weber and Schneider 2020b) and the inequality data from the Standardized World Income Inequality Database version 9.9 (SWIID) (Solt 2025).

I then elaborate on the quantitative findings through two case studies, including one typical case, Sudan, from 1995 to 2009, and one deviant case, Iran, from 2006 to 2012. Comparative case analysis allows me to explicate exact causal mechanisms and to examine how

the complexities of my model apply to real-life scenarios. I am particularly interested in investigating the ability of sanction targets to insulate themselves from economic or financial harm, and how any given policy response to sanctions — or lack thereof — affects the distribution of capital in the target country. Implementing a mixed-methods design is especially crucial given the lack of research on the broad economic effects of targeted sanctions, which are likely to operate differently than blanket sanctions.

III. Hypothesis formation and theoretical foundations

While a variety of sanction types can be targeted, I am only concerned with targeted financial sanctions, including asset freezes and banning financial transactions for the specified target, which have become one of the “preferred instrument[s] of statecraft” of the US and EU (Drezner 2015, 756). These targeted financial sanctions can be placed on individuals, corporations, and other entities such as militaries, terrorist organizations, or governments. To explain how targeted sanctions impact inequality, I rely on Kirshner’s microfoundations approach, which examines how groups within the target country are affected differently by sanctions, rather than assuming that the target country is impacted as an aggregate (Kirshner 1997, 33). Considering this, Kirshner explains how sanctions work best when they both directly weaken the government as well as “core” influential groups, who are incentivized to pressure the government to protect their interests (Kirshner 1997, 50). Consequently, I expect that what differentiates low-cost and high-cost sanctions is not the type of target, but how important that target is to the sanctioned state.

In 2015, Beladi and Oladi constructed a game theory model of an oligarchic economy and theorized that the effectiveness of targeted sanctions depends on the elasticity of

substitutions (Beladi and Oladi 2015, 26). Specifically, they indicate that targeted sanctions are likely to be more effective the higher the income of the target (assuming that there is “high substitutability of non-compliance with other policy objectives”) (Beladi and Oladi 2015, 26). However, this relationship is dependent on high levels of corruption and the oligarchs’ reliance on the leader of the country — if neither of those conditions are present, then targeted sanctions have the opposite effect (Beladi and Oladi 2015, 26). Despite the limitations of this model — including its focus on oligarchical governments and its examination of sanction effectiveness, not sanction costs — it still provides a valuable glimpse into targeted sanction mechanisms.

In their 2020 paper researching targeted sanctions placed on Russian firms, Ahn and Ludema found that in some cases, the Russian government shielded targets from sanction costs, transferring the burden to taxpayers (Ahn and Ludema 2020, 2). Indeed, governments frequently choose to shield firms that are deemed “strategic,” primarily including “firms producing goods or services with limited substitutability related to national security or social stability” (Ahn and Ludema 2020, 10). Additionally, Ahn and Ludema estimated that the cost of sanction-related bailouts made up around 45% of total sanction costs to the Russian government — demonstrating the great impact that targeted sanctions can have on governments and taxpayers (Ahn and Ludema 2020, 20).

In his 2012 chapter in “Sanctions, Statecraft, and Nuclear Proliferation,” Arthur A. Stein explains how sanctions allow states to accrue a higher level of control over the economy and thus allow them to shield certain groups from the worst sanction effects (Stein 2012, 45). Primarily, sanctions introduce an external threat that the state must respond to, thereby providing a motive and opportunity for the government to increase its relative power (Stein 2012, 45). Further, sanctions incentivize arbitrage and rent-seeking behavior (Stein 2012, 45). If a government can

create monopolies or monopsonies, it can profit significantly under sanctions, increasing its ability to protect political, military, or economic elite from sanctions (Stein 2012, 45).

Additionally, even without deliberate shielding, targeted sanctions may have a multitude of effects on the general population as governments invariably shift their consumption and investment patterns in response to the sanctions (Stein 2012, 45–6).

Together, this research suggests that both the effectiveness and costs of targeted sanctions largely depend on the relative market power and strategic importance of the sanction target as compared to alternative entities. Thus, it appears crucial to include some measure that approximates the government's willingness to shield targets from sanction costs. I ultimately decided to test the HH market concentration index (HHI), which measures the level of diversification within a given country's trade portfolio. A country with a trade portfolio concentrated in a small handful of markets has an index value close to one, and a country with perfectly diversified trade has a value near zero (World Integrated Trade Solution 2024). This index could approximate a country's willingness to shield targets, as a country with a more concentrated trade portfolio has a higher incentive to protect those industries and firms. When the state does not shield targets, targeted sanctions should decrease inequality as those at the top lose access to assets and income sources. However, given Ahna and Ludema's finding that shielding made up around half of total sanction costs to the Russian government, when the conditions for shielding increase, the sanction should exact greater economic costs on the greater population (Ahn and Ludema 2020, 20). Hence, my main hypothesis is as follows:

Hypothesis 1: Targeted sanctions generally decrease income inequality when the recipient country has a diverse trade portfolio but increase inequality when trade is highly concentrated.

It is most useful to examine targeted sanctions in comparison to other sanction mechanisms, especially as targeted sanctions are seen as less destructive alternatives to comprehensive sanctions (Drezner 2015, 756–7). I compare the effects of targeted sanctions to (non-targeted) financial sanctions — a broad category that includes the partial or total termination of foreign aid, asset freezes, investment bans, and restrictions on financial transactions, among others (Weber and Schneider 2020a). As targeted sanctions are generally considered a type of financial sanction, it seems most apt to compare their effects to that of their comprehensive counterparts. Additionally, following the end of the Cold War, financial sanctions have been employed significantly more frequently than trade sanctions — since 1990, there have only been 5 stand-alone cases of trade sanctions (Hufbauer, et al. 2007, 98). Thus, despite omitting trade sanctions from my analysis, I still include the vast majority of sanctions imposed between 1989 and 2015.

Due to the sheer variety of tools and mechanisms that reside within the category of financial sanction, it is difficult to determine possible conditioning factors. However, there is evidence that financial sanctions generally impact the rich rather than the poor. Indeed, wealthier players — including government officials and economic elite — are more likely to be involved in international finance and thus are more likely to be affected by financial freezes (Hufbauer, et al. 2007, 46, 98). Financial sanctions are also more difficult to evade. Evasion is not significantly profitable and comes with a significant risk of actors losing their international credit and capital (Hufbauer, et al. 2007, 46–7).

However, elites have other methods of insulating themselves from negative sanction effects, which may result in an increase in inequality like that observed by Afesorgbor and

Mahadevan (Afesorgbor and Mahadevan 2016, 7). Again, relying upon the microfoundations approach, I argue that the state is most incentivized to protect the elite when elite power and influence are high (Kirshner 1997, 42). Whether elite interests lay in the continuation or the dissolution of the sanction regime is a topic for future research.¹

While financial sanctions disproportionately affect the wealthy, I hypothesize that this effect is strongest when the elite have a smaller proportion of power and wealth. Under these circumstances, financial sanctions will reduce inequality as they reduce the position of those at the top. However, if elites have significant power, they are more likely to utilize the authority of the state to protect their interests. Due to the theoretical mechanisms outlined above, I suggest that financial sanctions are conditioned by a measure of elite power such as the share of wealth held by the top 1%. Accordingly, my second hypothesis is as follows:

Hypothesis 2: Financial sanctions decrease income inequality in the recipient country when the top 1% has a relatively low concentration of wealth. However, when elite power is high, financial sanctions increase inequality.

IV. Quantitative Data and Empirical Strategy:

To test my hypotheses concerning the effect of sanctions on income inequality, I use the widely accepted Gini coefficient obtained by the Standardized World Income Inequality Data (SWIID) (Solt 2025). SWIID provides the most comprehensive and comparable set of Gini

¹ Kirshner (1997) argues that sanctions that hit both the government and core support groups are most effective as it places both direct and indirect pressure on the government to change course (Kirshner 1997, 42). However, there is also research that suggests that sanctions provide valuable opportunities for the sanctioned government and elite to accumulate power (Stein 2012, 45-47). Due to these conflicting narratives, it is difficult to discern whether the elite will pressure the state to resolve the sanctions, or whether the economic power they might have gained under the sanctions outweigh any sanction costs.

coefficients as it includes data for 199 countries over six decades — making it the optimal source for a cross-national study (Solt 2025). I specifically use the Gini coefficient calculated from disposable income as it allows for a better understanding of spending power.

I obtain sanctions data from the EUSANCT Dataset, which includes sanctions issued by three principal senders — US, the UN, and the EU — on 80 target states in the period from 1989 to 2015 (Weber and Schneider 2020b, 97).² This dataset is one of few that includes targeted sanctions against both individuals and companies, which makes it crucial for my study. Though it includes sanction threats as well as imposed sanctions, I ignore threats as they are unlikely to have a significant impact on target countries.

EUSANCT differentiates between 12 types of sanctions. For the purposes of this study, targeted sanctions and targeted sanctions against entities/corporations are counted as targeted sanctions. Embargoes (both partial and total), blockades, foreign aid cuts, and major financial sanctions are all counted as financial sanctions. My primary unit of analysis is country-years, with a dummy variable indicating whether a targeted or financial sanction was imposed on any given target state in any year between 1989 and 2015.

As I hypothesize that the effects of both targeted and financial sanctions are conditioned by other economic factors, I include these factors as additional explanatory variables. First, I add the share of wealth held by the top 1%, which I suggest impacts both targeted and financial sanction mechanisms. This data is gathered from the World Inequality Database (Bajard et al. 2022). A downside of using this data source is that it lacks comprehensive data between 1989–1994. However, the share of wealth in any given country is typically consistent across a five-year period. Thus, to not lose valuable cases, I extrapolated the data from 1989–1994 by taking the

² While the EUSANCT dataset includes more than 80 target states, I have had to remove some from my study due to a lack of data. Examples include Cuba, North Korea, and Yugoslavia.

five-year average from 1995–2000 for countries where all data values from 1995–2000 were within 1 percentage point of one another. This extrapolation did not significantly affect the distribution of the data.

Second, I included the HH market concentration index, which I hypothesize may impact targeted sanctions. I gathered this data from the World Integrated Trade Solution (WITS) — a dataset created by the World Bank alongside other international organizations to compare trade statistics across countries (World Integrated Trade Solution 2024). This dataset is one of very few that compiles global HHI data, making it extremely valuable for my analysis. However, there are many significant holes in the data. I interpolate to fill gaps of up to 5 years, taking the average of the two data values on either end of the missing value(s). This interpolation did not significantly affect the distribution of the data. Additionally, this data is highly skewed, as most data points fall between 0 and 0.25. Accordingly, I take the natural log of this variable to standardize the distribution.

There are several confounding variables that I control for. First are the political institutions and conditions present in each country, which can impact both the likelihood and type of sanctions that are issued as well as the level of inequality present. Indeed, in 2003, Reuveny and Li found that a higher level of democracy lowers income inequality (Reuveny and Li 2003, 593). Additionally, democratic countries are less likely to issue sanction threats against one another (Drury et al., 2014, 46–7). I use the Polity5 dataset to measure the level of democracy present in each country, allowing me to de-confound the impact of government type on both inequality and sanctions (Marshall and Gurr, 2020).

Likewise, I control for the level of human rights and political freedom using the Freedom in the World (FIW) dataset, which gives each country a score based on its political rights and

civil liberties (Freedom House, 2025). Indeed, human rights often serve as an impetus for sanctions (Ari and Sonmez 2024, 68–9), and political freedom(s) have been found to impact income inequality as well (Simpson 1990, 687; Uzar 2023, 16). Notably, FIW’s scores are easily categorized into three groups: free (F), partly free (PF), and not free (NF) (Freedom House, 2025). Consequently, the scores are incorporated into the model as dummy variables with three levels.

I also include data from the Correlates of War’s Militarized Interstate Disputes dataset to control for interstate conflict (Palmer et al. 2020). Militarized conflict has a two-way relationship with economic sanctions, where sanctions can cause conflict, and conflict can cause sanctions (Kim 2018). War can cause impermanent shifts in income inequality as well (Bircan et al. 2016). Thus, it is a crucial confounder to control. This data is included as a dummy variable, with a value of one indicating that a country was at war in any given year, and a value of 0 if there is no recorded interstate conflict.

I also account for a number of pre-existing economic conditions and relationships. Theoretically, economic integration decreases the likelihood of sanctions, as countries become increasingly invested in each other's economies. Accordingly, I include several measures that serve as indicators of economic engagement, including foreign trade, foreign direct investment, and foreign aid dependence. Indeed, foreign trade can impact both economic inequality and the likelihood of becoming a target of sanctions (Reuveny and Li 2003, 593; Drury et al., 2014, 47). Similarly, foreign direct investment (FDI) has been found to increase inequality (Reuveny and Li 2003, 593) and decrease the imposition of sanctions (Lektzian and Biglaiser 2013, 85). I include both measures as a percentage of total GDP, obtaining the data from the World Development Indicators database (World Bank Group, 2025). Lastly, I calculate aid dependence as total

foreign aid as a percentage of GDP, gathering both aid and GDP data from the World Development Indicators database (World Bank Group, 2025). Like the two prior variables, foreign aid influences both inequality and sanctions. Specifically, it can decrease inequality and decrease the duration of sanction cases (Kim and Kim, 2022, 74; Jeong 2019, 242).

I also control for developmental differences, as national GDP and other economic factors often influence inequality. The most popularly accepted relationship between economic growth and income inequality is the inverted U-shaped curve hypothesized by Kuznets (Kuznets 1955). As found by Acemoglu and Robinson, this relationship is not universal as it is dependent on social and political factors (Acemoglu and Robinson 2002, 199). Still, it is necessary to take GDP per capita and its squared term into account, which I measure by including data from the World Development Indicators database (World Bank Group, 2025).

In terms of empirical methods, I estimate the models using an ordinary least squares model with Newey–West standard errors with a lag length of 2 to correct for autocorrelation and panel heteroskedasticity. The lag length is based on the number of observed time periods (26 in this case), as $L = 0.75 * T^{\frac{1}{3}}$ where L is the lag length and T is the number of observed time periods. Additionally, my model includes a full set of country and year fixed effects dummies to account for unobserved unit-specific factors such as cultural norms or year-specific global events. It is crucial to include these fixed effects dummies to preserve exogeneity and thus, to ensure my results are unbiased.³

V. Quantitative Analysis and Results

³ As a result of the fixed effects model, the constant term is redundant as it is the sum of all the fixed effects. The specific software that I used to create my model does not report the constant term, and thus it is absent from my regression tables. However, it has been accounted for through the model.

Table 1 reports the models testing the effects of both targeted and financial sanctions on income inequality. Model 1 compares targeted and financial sanctions without any conditioning factors, whereas Model 2 tests the conditioning effects of the HHI and wealth holdings of the top 1% on the respective sanction types.

Table 1: Targeted and financial sanctions and Gini-coefficient

	Model 1	Model 2
Targeted sanction	0.411 (0.275)	2.466** (0.765)
Targeted sanction x Log HHI		0.863* (0.320)
Financial sanction	-0.098 (0.173)	-1.935* (0.758)
Financial sanction x share of wealth held by top 1%		0.061* (0.025)
Log HHI	-0.663*** (0.177)	-0.702*** (0.182)
Share of wealth held by top 1% (% of total wealth)	0.275*** (0.031)	0.276*** (0.031)
Polity	0.041 (0.029)	0.043 (0.029)
War dummy	0.138 (0.104)	0.128 (0.103)
Freedom status NF	-0.027 (0.527)	0.002 (0.520)
Freedom status PF	0.108 (0.446)	0.190 (0.439)
Trade (% of GDP)	0.003 (0.006)	0.002 (0.006)
FDI (% of GDP)	0.000 (0.003)	-0.001 (0.003)
Aid dependence (% of GDP)	0.053* (0.021)	0.046* (0.020)

Log GDP per capita	4.212***	4.016***
	(1.010)	(1.000)
(Log GDP per capita) ²	-0.201**	-0.186**
	(0.061)	(0.060)
Num.Obs.	1374	1374
R2	0.973	0.973
R2 Adj.	0.970	0.970
AIC	5099.0	5080.8
RMSE	1.42	1.41

Newey–West standard errors in parentheses. All models include country and year fixed effects (not shown).

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Model 1 returns insignificant results for both sanction types. However, after introducing conditioning effects in Model 2, both targeted and financial sanctions have a significant effect on the sanctioned state's Gini coefficient. Specifically, due to the signs of their respective coefficients, the results of Model 2 suggest that targeted sanctions increase income inequality and financial sanctions decrease income inequality. Additionally, the model returns statistically significant terms for both interaction terms. The negative sign of the interaction term between targeted sanctions and log HHI suggests that the increasing effect of targeted sanctions on Gini is maximized on targets with highly concentrated trade portfolios. However, this conditioning effect deteriorates as log HHI decreases. Turning to financial sanctions, its interaction term with the share of wealth held by the top 1% is significant and positive, indicating that the decreasing effects of financial sanctions on income inequality are strongest when elite power is low.

When conducting interaction models, it is crucial to analyze the marginal effects of the primary explanatory variable at different values of the conditioning variable (See Brambor et al.

2006). Thus, Figure 1 and Figure 2 respectively display the marginal effects of targeted and financial sanctions with each of their conditioning variables.

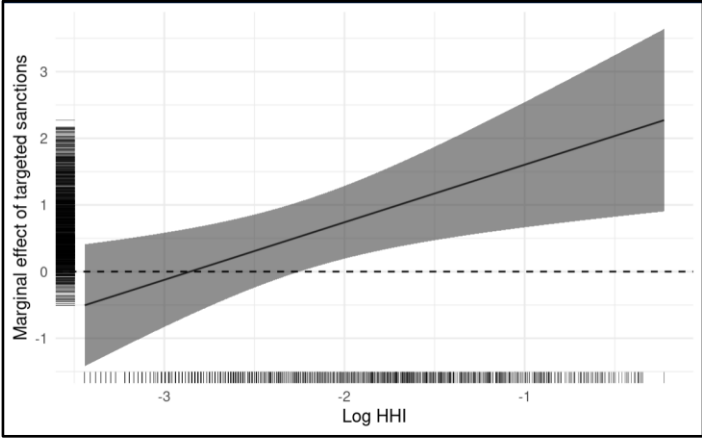


Figure 1: Graph of the marginal effects of targeted sanctions on the Gini coefficient across Log HHI

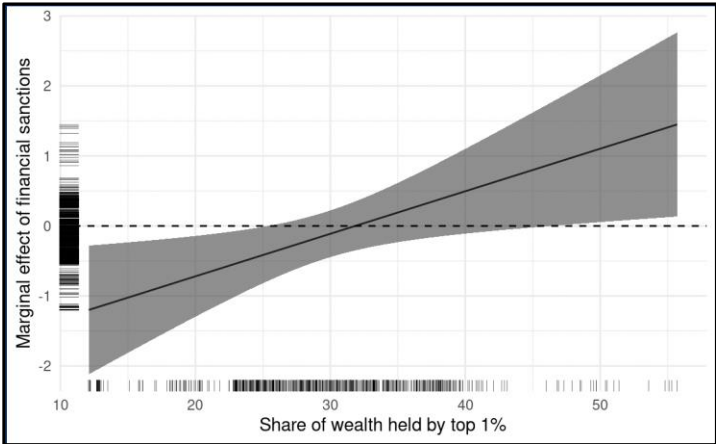


Figure 2: Graph of marginal effects of financial sanctions on Gini coefficient across the share of wealth held by the top 1%

As shown in Figure 1, targeted sanctions generally have a positive effect on income inequality, and that effect increases as the HH market concentration index increases. Notably, the conditioning effect is only statistically significant when both the upper and lower confidence intervals (marked by the gray shading) are either above or below zero. Consequently, the HHI only has a statistically significant effect on targeted sanctions when its logged value is greater than -2.254, and the un-logged value is greater than 0.105. 50.2% of values fall above this point. As a result, targeted sanctions have no statistically significant effect for the most diversified economies but do have an increasing positive effect for all economies with an HHI above the 50th percentile. The increasing marginal effect of targeted sanctions on income inequality ranges from 0.5203594 (when log HHI is equal to -2.254) to 2.27 (when log HHI reaches its maximum value of -0.226).

As shown in Figure 2, the negative effects of financial sanctions diminish as the share of wealth held by the top 1% increases. Indeed, financial sanctions only have a statistically significant negative effect when the share of wealth is below 25.4, which only includes 29% of values. Additionally, at extremely high values of elite wealth — when the top 1% holds at least 46% of all wealth — financial sanctions have an increasing effect. However, only 1.5% of observations have such a high proportion of elite wealth.⁴ As a result, for countries with low levels of elite wealth, financial sanctions successfully target that wealth and decrease income inequality. However, this effect diminishes as the share of wealth held by the top 1% increases to 25.4. The decreasing marginal effect of financial sanctions ranges from -1.19971 (when the share of wealth reaches its minimum value of 12.1) to -0.39 (when the share of wealth is equal to 25.4).

⁴ All observations that do meet this standard are from South Africa between the years 1995 and 2014. No other country has such a high proportion of elite wealth.

Overall, the results support the targeted sanction hypothesis. Targeted sanctions increase inequality at an increasing rate: the more concentrated the trade portfolio is, the more sanction costs are passed on to the citizenry in the form of heightened inequality. However, for all values of log HHI below the 50th percentile, there is little to no significant conditional relationship between targeted sanctions and HHI. This means that contrary to the first hypothesis, targeted sanctions do not decrease inequality when HHI is low. This may indicate that when there is a diverse trade portfolio, there is enough competition that targeted sanctions have no systematic effect on inequality.

These findings also support the financial sanction hypothesis: financial sanctions decrease inequality, particularly at low levels of elite wealth. Additionally, there is some evidence that when the elite have accumulated a high level of wealth, financial sanctions may even increase inequality. However, all evidence for this increasing relationship comes from South Africa — an outlier state due to its extremely high levels of inequality and wealth held by the top 1%.

The results for financial sanctions differ from those of Afesorbor and Mahadevan (2016), who showed that financial sanctions have a strong positive effect on income inequality. The differences between findings could be owed to the use of different sanction datasets. Afesorbor and Mahadevan use the Economic Sanctions Reconsidered data set by Hufbauer et al. (2007), which covers 116 sanction cases from 1960–2008 (Afesorbor and Mahadevan 2016, 3). The EUSANCT dataset accounts for more sanction cases (209, when excluding sanction threats) imposed in a much shorter period (1989–2015) (Weber and Schneider 2020, 102–3). Additionally, due to the more restricted time scope of EUSANCT, most sanction cases examined in my study occur following the conclusion of the Cold War. It is well documented that

economic sanctions became more popular after the end of the Cold War (Cortwright and Lopez 1999, 735; Garfield 2002, 94; Morgan et al. 2023, 5). The unique economic and political conditions of the Cold War could also be an influencing factor, as well as the increase in global economic and financial integration that followed it (Chingono et al 2012; Jentleson 2000; Berger 2001, 1079).

In addition to the sanction variables and their respective interaction terms, several control variables display statistically significant relationships with income inequality. The share of wealth held by the top 1% consistently produces a positive and significant coefficient, indicating that as wealth inequality increases, so does income inequality. Though wealth inequality is not always associated with income inequality (Skopek et al. 2014), there is an increasing correlation between the two measures (Stiglitz 2015; Piketty 2014). Log HHI has a consistently negative coefficient, suggesting that trade diversification is associated with a decrease in inequality. The positive and statistically significant coefficient of the aid dependence variable aligns with previous findings that foreign aid increases income inequality (Jeong 2020, 678–81). Lastly, log GDP per capita and its squared term produce a robust positive and negative coefficient respectively, confirming the presence of an inverted U-shaped relationship between GDP and inequality.

VI. Robustness Checks

To substantiate the results of my quantitative study, I tested several additional models as robustness checks. First, I estimated Model 2 using alternative dependent variables from the World Income Inequality Database (WIID) (UNU-WIDER 2025). While SWIID is the preferred source of Gini-coefficients due to its size and comparability, WIID data is regularly used to

cross-check results calculated using SWIID. Additionally, WIID calculates income quintiles and deciles, allowing for a closer examination of who is most affected by changes in the Gini coefficient. I specifically ran Model 2 using the WIID Gini coefficient, and the first, fifth, and tenth income deciles. As the WIID models have much less data than the SWIID model (566 observations vs. 1374), I also eliminated all country-years that did not have WIID values and ran a model by the SWIID Gini with the sample size as the WIID variables. Models 1–5 in Table A1 (see Appendix in Supplemental Materials) estimate the effects of targeted and financial sanctions on these alternate variables.

Notably, the results of Models 1–4 differ from my own, as the effect of targeted sanctions is both statistically insignificant and greatly reduced. Additionally, the interaction term between targeted sanctions and log HHI is insignificant in Models 1 and 2 (which are calculated using the WIID Gini and decile 1, respectively). The interaction term is significant and negative for Model 3 (decile 5) and significant and positive for Model 4 (decile 10). This suggests that the effect of targeted sanctions on the middle-income decile is strongest in a more diverse economy, while the effect on the highest income decile is strongest in a concentrated economy. Despite the insignificance of targeted sanctions in Models 1–4, the coefficients are positive for all but Model 3, supporting my conclusion that targeted sanctions generally increase income inequality. While the negative sign of the term in Model 3 may suggest that the middle-income decile loses income while the bottom and top deciles increase their shares, it is difficult to extrapolate further due to the insignificance of the results.

Model 5, which is calculated based on the limited sample of SWIID Gini coefficients, reports positive and significant results for both targeted sanctions and the interaction term with log HHI. As SWIID still retains a higher level of significance over WIID despite a restricted

sample size, these results indicate that there is some difference between the two Gini coefficients that leads to this change in statistical significance. Future research is necessary to determine how and why these results differ based on the source of the Gini coefficient.

Though targeted sanctions lose statistical significance in the WIID models, financial sanctions produce strong significant results across all five models. The same applies to the interaction term between financial sanctions and the share of wealth held by the top 1%. In terms of direction, financial sanctions report a negative coefficient and a positive interaction term in Models 1, 4, and 5. Financial sanctions have a positive coefficient and a negative interaction term in Models 2 and 3. The income decile regressions confirm that at low values of elite wealth, financial sanctions negatively hit those in the top decile while increasing the income shares of those in the bottom and middle.

To further investigate the causal mechanisms at hand, I isolated the two types of targeted sanctions (those placed on individuals versus those placed on corporations/entities) and ran two models with each. The results of these models are reported in Table A2 (see Appendix in Supplemental Materials) and indicate that the main findings do not significantly change regardless of whether the targeted sanction is placed on an individual or a corporation or entity.

I conducted an additional robustness check lagging all right-hand covariates by 1 to address concerns of simultaneity bias. The results are reported in Table A3 (see Appendix in Supplemental Materials). Notably, all main findings concerning targeted and financial sanctions and their respective interaction terms remain robust to lagged covariates. Additionally, Model 1 — which measures the impact of targeted and financial sanctions without any conditioning effects — reports a positive and statistically significant ($p < 0.1$) coefficient for targeted sanctions, confirming that targeted sanctions can increase inequality regardless of other

economic conditions. The increased significance may indicate that the effects of sanctions are lagged in some way.

Lastly, I tested models using alternate interaction terms to check whether targeted and financial sanctions are conditioned by any other variables. I ran targeted sanctions by the share of wealth held by the top 1%, which, as a measure of elite economic power, could influence the government's willingness to shield targets. I also ran targeted sanctions by the Polity score variable; higher levels of democracy could limit the government's ability to shield targets as shielding may be viewed as an act of favoritism or corruption. While democracy does not have a linear relationship with corruption, the strength of various democratic components does impact corruption as found by McMann et al. (McMann et al. 2019, 903). Additionally, as democracies are more responsible to their citizens than autocracies, there are more disincentives to passing sanction costs off to the populace. In terms of financial sanctions, I ran models using foreign aid dependence (total foreign aid as a percentage of GDP) and remittances (as a percentage of GDP) as conditioning variables. As financial sanctions primarily take the form of foreign aid sanctions, their effects may be conditioned by foreign aid (Jeong 2020). Additionally, financial sanctions could shut off cash flows like remittances, which would directly negatively affect lower and middle classes (Afesorgbor and Mahadevan 2016, 7).

Table A4 (see Appendix in Supplemental Materials) reports the results of all models with alternate conditioning variables. Financial sanctions and the interaction terms produced no significant coefficients in either the aid dependence or remittance models. Targeted sanctions, conversely, produced a negative and statistically significant ($p < 0.05$) interaction term when conditioned by the Polity score. The negative sign of the coefficient suggests that the impact of targeted sanctions is exacerbated in countries with more autocratic governments. However, this

relationship may be complicated by the fact that autocracies are more likely to be recipients of sanctions (Cox and Drury 2006; Drury et al. 2014).

Targeted sanctions also produce significant terms when conditioned with the share of wealth of the top 1%. Indeed, targeted sanctions have a strong positive coefficient, and the interaction term is statistically significant and negative. While the positive effect of targeted sanctions aligns with the main findings, the interaction term suggests an inverse conditional relationship, where the negative impact of targeted sanctions is strongest when the elite have less wealth. While the interaction term works contrary to the main findings, there are plausible explanations. For example, targeted sanctions may worsen inequality by allowing opportunities for previously weak elite to consolidate their wealth while their rivals are sanctioned. It could also be the result of shielding — the negative effects of shielding may be exacerbated when the elite are weak, as government shielding can dramatically increase the holdings of key sanctioned elite more than it could when the elite already hold immense power and authority. As there may be other mechanisms at hand, future research is required to fully understand the relationship between sanctions and elite wealth.

VII. Case Studies

While the large-N portion of this study provided empirical evidence of a relationship between targeted and financial sanctions with income inequality, there are still many theoretical and practical gaps that must be bridged. For example, the questions of 1) which population is impacted most by these sanctions, 2) how might states shield targets from targeted sanctions, and 3) what leads states to deviate from the empirical model, all remain to be answered. To investigate these questions, as well as other causal links in my model, I provide narrative

evidence from two case studies, including one typical case, and one atypical case. I chose these two cases through analyzing the residuals of all observations included in my quantitative study. Typical cases had residuals less than or equal to 1, while atypical cases had residuals larger than 2. Additionally, I eliminated observations without a sustained sanction episode (defined as 3 years or more). From the remaining observations, I chose two cases that I thought would provide an interesting glimpse at internal causal mechanisms and that had a wealth of sources and prior study to rely on.

The first of these cases is Iran from 2006–2012. During this period, Iran was subject to an increasingly destructive sanction regime due to its pursuit of a nuclear program and enriched uranium. Notably, Iran is a typical case from 2006–2010 with a residual of less than 1 throughout the entire period. However, in 2012, Iran becomes an atypical case with a residual greater than 2. This dramatic and quick shift proved interesting and allowed me to investigate exactly what prevents cases from aligning with the model at hand. The second case is Sudan from 2000–2009. At this point in time, Sudan faced several sanctions associated with terrorism, human rights violations, and ongoing war and violence. Additionally, due to increasing HHI and a low level of wealth concentrated by the top 1%, both targeted and financial sanctions impacted Sudan's inequality during this period. Thus, I am able to compare the effects of both sanction types and determine if there is any unique combined effect.

I begin my analysis with the case of Iran, specifically focusing on changes between 2010 and 2012 that allowed for the country to deviate from the model. Then, I investigate the case of Sudan, explaining the economic conditions that allowed the sanctions to take effect. Lastly, I compare the two cases to one another.

From 2006–2012, Iran was subject to increasingly harsh sanctions from the US, EU, and UN regarding its developing nuclear program. Before 2010, most of these sanctions were targeted to curtail nuclear activity through banning trade of materials and technology used for nuclear development, banning Iranian weapons exports, and freezing the assets and activity of individuals and corporations involved in the nuclear program (“International Iran Sanctions Database” 2023, 21–5). The most far-reaching sanction was a United States revocation of Iran’s “U-turn” license, preventing American banks from processing any payments indirectly connected to Iranian individuals or banks (“International Iran Sanctions Database” 2023, 21). During and following the summer of 2010 however, the US and EU dramatically escalated their sanctions regime. By 2012, the EU had instituted a total oil embargo, banned all transactions between EU and Iranian banks, and had frozen the assets of the Iranian Central Bank in the EU, on top of increasing the number of individuals and entities exposed to targeted sanctions (“International Iran Sanctions Database” 2023, 26–7). In the same two years, the United States instituted a total import ban on Iran, sanctioned all foreign banks that facilitated transactions to buy oil products from Iran, froze all US-based assets owned by the Iranian government or the Central Bank of Iran, and placed targeted sanctions on individuals and entities associated with the petroleum trade, nuclear development, human rights abuses, the Iranian Revolutionary Guard Corps, and more (“International Iran Sanctions Database” 2023, 13–21). Due to the immense economic harm caused by these sanctions, Iran and the P5 entered nuclear talks in October of 2013.

Amid intensifying sanctions, Iran privatized many formerly government-owned properties and services, with the number of privatizations accelerating between 2007 and 2014 (Doutaghi 2024, 188). However, this growing private sector was still closely tied to the state, as

“whatever industries or companies that have privatized have been taken over by parties affiliated with the state, and more specifically Khamenei and the Revolutionary Guards” (Nader 2012, 223). Indeed, much of Iran’s economy was controlled by charitable foundations called “bonyads,” which became massive entities designed to serve the needs of those close to the regime (Nader 2012, 215). Additionally, privatization often led to monopolization, as individuals close to the regime were granted control over whole key sectors of the economy (Doutaghi 2024, 210). Likely because of these private monopolies as well as trade restrictions on certain industries, Iran’s HH market concentration index slowly increased from 2006 to 2011 (from 0.075 to 0.107). Then, in 2012, the HHI jumped from 0.093 to 0.159 — far beyond the point at which targeted sanctions should increase inequality (see Figure 3).

Year	HHI	Wealth share of top 1%	Gini coefficient
2006	0.075	0.301	40.7
2007	0.084	0.313	40.2
2008	0.084	0.277	39.6
2009	0.084	0.269	39
2010	0.093	0.279	38.3
2011	0.107	0.279	37.5
2012	0.1585	0.279	36.9

Figure 3: Table of Iran’s HHI, wealth share of top 1%, and Gini coefficient from 2006–2012.

However, between 2006 and 2012 Iran’s Gini coefficient slowly decreased. Prior to 2010, this decline was likely the result of changes in oil revenue, as previous research links inequality trends in Iran to changes in oil prices due to the high prevalence of oil rents (Salehi-Isfahani 2009, 24–5).⁵ This downward trend in inequality between 2006 and 2010 is also accurately

⁵ Indeed, decreasing inequality between 2006 and 2010 cannot be attributed to the decreasing effects of financial sanctions, as Iran’s top 1% consistently holds more than 25.4% of total wealth. Nor can it be attributed to public

represented in my model, as the absolute value of all residuals for Iran during this period remain equal to or less than 1. When Iran's HHI increased to 0.159 in 2012 however, my model inaccurately predicts that inequality should rise in kind. Instead, Iran's Gini continued to decrease. So, why was Iran getting more equal despite increasing economic concentration and overwhelming sanctions?

It is not because sanctions significantly target elite economic interests. While some studies show that politically connected firms were impacted by sanctions at higher rates than ordinary firms (Ghasseminejad and Jahan-Parvar 2021; Draca et al. 2023), this generally did not negatively affect elite wealth. The elite offset any costs of sanctions through privatization, filling market vacuums that emerged following the departure of American and European firms, creating new trade networks with Asia, and monopolizing consumer goods (Borszik 2015, 27–28). Consequently, the wealth of the top 1% remains constant from 2010 to 2012 (see Figure 3).

Rather, the decrease in inequality is likely associated with a slight growth in the incomes of lower- and middle-class Iranians. Indeed, in December 2010, the Ahmadinejad government implemented a universal cash transfer program (UCTs) to compensate for rising energy and food prices. As found by many studies, these UCTs drastically reduced poverty, lowering the headcount ratio from 22.5 percent to 10.6 percent within one year of implementation⁶(Enami and Lustig 2018, 2).⁷ These decreasing effects carried over into 2012 as well, though the rate of reduction was much smaller (Enami and Lustig 2018, 3–4). Thus, despite known sanction effects and increased market concentration, Iran's poorest citizens experienced a notable income increase during this period, resulting in a more equal income distribution.

policy, as there is no policy measure introduced over this period that systematically lowered inequality (Salehi-Isfahani 2009, 23).

⁶ The headcount index was measured with respect to the 2005 US\$4 PPP poverty line.

Though sanctions in Iran never impacted inequality — either due to insufficient values of the conditioning variables, or external factors such as UCTs — sanctions placed on Sudan from 2000 to 2009 resulted in a notable increase in inequality. Sudan, like Iran, has been sanctioned extensively by the United States and its European allies. This sanction regime first began in 1990 when USAID cut all non-humanitarian aid to Sudan due to a military coup the year prior (Center for Global Development 2011). In response to confirmation that the government of Sudan had been sponsoring terrorist activities, in 1997 the United States imposed a trade embargo against Sudan and blocked all government assets and foreign investment (Aluoch 2015, 74–5). Between 2000 and 2009, further targeted sanctions were imposed on the Sudanese government and militias fighting in the country, as well as companies associated with the government (Aluoch 2015, 74–6).

However, despite the substantial number of sanctions targeting the Sudanese government, the elite did not suffer significantly. Beginning in the early 90s, the new Sudanese regime adopted an economic agenda defined by austerity and privatization, supposedly to revitalize the economy (Suliman 2007, 2; Thomas 2017, 24). The Sudanese ruling party — the National Congress Party (NCP) — and political elites associated with the regime used this privatization scheme to increase their holdings, and thus their power (Suliman 2007, 4–5). This neoliberal economic agenda deepened inequality, isolated rural regions, increased unemployment, and increased the cost of living, to the detriment of the general Sudanese population (Suliman 2007, 5; Thomas 2017, 24–5).

Sudanese elite did not only profit from privatization. In 1999, the discovery of oil in what is now South Sudan quickly transformed the Sudanese economy into one almost entirely based on oil (Ali et al. 2023, 78). Between 1999 and 2011, the Sudanese economy grew at an annual

rate of 5.8% due to an oil boom and a massive increase in oil exports (Ali et al. 2023, 78). Most of this oil revenue benefited the public sector, “revealing a budget allocation scheme based on patronage and bargaining instead of one that would strategically support private enterprise and economic diversification” (Ali et al. 2023, 80). From 2005–2011, there was a substantial increase in the number of civil servants, soldiers, and paramilitaries on the government payroll, the result of oil rents and cronyism (Adam 2022, 4–5).

The combination of privatization and oil rents shielded prominent elite from damaging sanction effects. As described by Wang et al., “During the 20 years of comprehensive US sanctions enforcement episodes, Sudanese political elites took advantage of extant resources in their favor so that to divert sanctions’ cost to the general population” (Wang et al. 2023, 1476). Additionally, even though financial sanctions should have damaged the position of the elite — the wealth share of the top 1% remains under the 25.4 threshold the whole period (see Figure 4) — many were able to circumvent comprehensive financial sanctions through transferring their assets from American financial institutions to the Middle East and Asia (Malik and Malik 2015, 8). The potency of the Western sanction regime was further diluted by Sudan’s increased reliance on other allies, such as China (Malik and Malik 2015, 8).

While Sudan's government and elite were insulated from economic harm, the general population suffered from a severe lack of necessities such as grain, medicines, and electronic devices (Wang et al. 2023, 1476). The Sudanese healthcare and food systems were rendered “essentially dysfunctional” (Wang et al. 2023, 1476). Due to a lack of widely accessible goods, black markets sprung up, extorting the population while enriching the elite and other businesspeople (Wang et al. 2023, 1477).

Unsurprisingly, Sudan experiences a substantial increase in market concentration during this period. Prior to the discovery of oil, Sudan’s HHI remained under the 0.105 threshold needed for targeted sanctions to have increasing effects on inequality. However, in 2000, the HHI increased to 0.194 and only continued to grow, reaching a level of 0.353 in 2009 (see Figure 4). Higher levels of concentration led to increased inequality, as Sudan’s Gini coefficient grew from 48.4 to 49.9 between 2000 and 2009. Ultimately, as indicated by this rising inequality, Sudan’s elite benefitted from the sanctions, maintaining their wealth and power while the general population suffered.

Year	HHI	Wealth share of top 1%	Gini Coefficient
2000	0.194	0.245	48.4
2001	0.262	0.245	48.5
2002	0.287	0.245	48.7
2003	0.299	0.245	48.9
2004	0.273	0.245	49
2005	0.352	0.245	49.2
2006	0.338	0.245	49.4
2007	0.325	0.245	49.5
2008	0.329	0.245	49.7
2009	0.353	0.245	49.9

Figure 4: Table of Sudan’s HHI, wealth share of top 1%, and Gini coefficient from 2000–2009.

Iran and Sudan share several economic and political characteristics. Since 1989, both have had an Islamic government engaged in patronage and corruption. During the relevant periods, the economies of both countries were heavily reliant on oil. They had each been subject to extensive sanctions prior to the periods of observation — US sanctions on Iran began in 1979 and US sanctions on Sudan were first imposed in 1990. The two states also underwent similar economic transformations during the observed periods, as both pursued a policy of neo-liberal

privatization to the benefit of political and economic elite, which resulted in increasing market concentration.

However, despite many similarities, inequality in Iran fell throughout the observed period while Sudan's Gini coefficient climbed higher. Prior to 2012, Iran's decreasing inequality was associated with changes in oil revenue as noted by Salehi-Isfahani (Salehi-Isfahani 2009, 24–5). Additionally, according to my model, sanctions did not impact inequality in Iran at this time due to insufficient values of both HHI and the share of wealth held by the top 1%. While Iran's HHI crossed the conditioning effect threshold in 2012, Iran staved off the predicted increasing effect of targeted sanctions through an extensive cash transfer program that gave each household \$45 per month (\$90 in PPP USD) (Salehi-Isfahani 2017, 11–2). UCTs were associated with a massive fall in poverty rates and increased the incomes of the poor at a faster rate than that of the non-poor (Salehi-Isfahani 2017, 9, 11–2).

Following Sudan's regime change in 1989, its new government pursued a “radical and far-reaching program of economic reforms” involving austerity and privatization (Thomas 2017, 24). This contributed to a consistent pattern of increasing inequality Thomas 2017, 24–5). However, the rate of increase accelerated following the discovery of oil in 1999 and the associated dramatic growth in Sudan's market concentration index. Notably, Sudan's HHI grew both faster and higher than Iran's, which is likely the primary factor contributing to positive sanction impacts on Sudan's Gini coefficient.

VIII. Discussion

In the quantitative portion of this study, I ran and analyzed a model that tested the empirical relationship between targeted and financial sanctions and the Gini coefficient. Additionally, I analyzed a model with interaction terms to test possible conditioning factors that may impact how and by how much sanctions impact inequality. The results of this model suggest that targeted sanctions have a positive relationship with income inequality — an effect that increases as the market concentration index (HHI) increases above a logged value of -2.254. The model also returned statistically significant results for financial sanctions, which reduce income inequality at decreasing values of elite wealth below 25.4.

To further investigate causal mechanisms and factors not considered in my model, I conducted two case studies. These case studies provide an invaluable glimpse into how and why sanctions impact inequality. In both Iran and Sudan, the elite were not greatly affected by either targeted or financial sanctions, likely due to other economic measures — such as the privatization of significant industries — that increased income or wealth sources. While states more often shield themselves from sanctions through nationalization rather than privatization (see Peksen 2017), in both Iran and Sudan, privatization has insulated elite from negative sanction effects through increasing their income and assets enough to offset any losses. However, in Sudan there were other shielding tactics at play as well — Sudanese elite also benefitted from an oil boom, increased patronage and rent-seeking, and the expansion of government payrolls.

Regardless of the specific shielding mechanisms, if sanction-caused changes in inequality are not associated with the elite, it indicates that 1) targeted sanctions are likely much less targeted than previously thought, and 2) rather than being due to reductions or gains in elite income, changes in inequality may be associated with income losses suffered by the middle and

lower classes. This second point is extremely consequential, as it suggests that targeted sanctions actively harm the most vulnerable members of society. However, as the case of Iran demonstrates, robust policy such as cash transfer programs can prevent sanctions from hitting the poor. This is vital information, as it provides actionable steps for policy makers looking to address any hurt caused by these sanctions.

Notably, financial sanctions did not have a clear effect in either country even though Sudan consistently had a wealth index low enough for these sanctions to decrease inequality. Sudan may have experienced some decreasing effect; however, it was certainly overshadowed by the more significant increases in inequality associated with targeted sanctions. Generally, financial sanctions affect a smaller proportion of cases — the conditioning effects are only statistically significant for less than a third of all quantitative observations — and when financial sanctions do influence inequality, their decreasing effects are limited. This does not necessarily mean that financial sanctions are less consequential than other sanction types. Rather, it may indicate more complicated internal mechanisms — perhaps financial aid sanctions, which have been found to have a decreasing impact on inequality, are competing with other types of financial sanctions such as restrictions on financial transactions. These results might also suggest that financial sanctions are easier to evade than previously thought. Like in Sudan, sanctioned elites and governments may address sanctions from the US, EU, and UN through turning to other sources of foreign aid and financial institutions (Malik and Malik 2015, 8).

IX. Conclusion

This research examines the impact of targeted and comprehensive financial sanctions on the income inequality of target states. The findings of this study suggest that targeted sanctions

increase inequality in states with highly concentrated trade portfolios, but this effect diminishes as the market concentration index (HHI) decreases. Additionally, the increasing effects of these sanctions can be temporarily avoided through public policy, as was the case with Iran's Universal Cash Transfer program. In terms of comprehensive financial sanctions, the results show that financial sanctions decrease income inequality in states with low elite power as measured by the share of wealth held by the top 1%. However, this effect deteriorates as elite power increases and can be avoided even at low enough values of elite wealth, as demonstrated through the case of Sudan.

This study adds several valuable contributions to the literature. It is the first analysis of the economic impacts of targeted sanctions broadly. This is extremely significant as targeted sanctions have become one of the most popular sanction mechanisms — specifically because they theoretically limit the negative externalities associated with more comprehensive sanctions (Drezner 2015, 756). This research counters that assumption by suggesting that targeted sanctions can be just as harmful, if not more, than other sanction types. Second, this study examines possible conditioning variables that may influence the extent to which both targeted and financial sanctions impact inequality. This may allow policy makers to better understand how these two types of sanctions work and the conditions necessary for sanctions to meet policy goals.

Third, due to its mixed-methods format, this research also provides valuable insights into when and where sanctions have worked according to the model — and where they have not and why. This real-world context provides further understanding of the internal mechanisms at hand, benefiting both policy makers and future researchers. Finally, this research adds to growing scholarship on the re-distributional effects of various economic sanctions. While there is

significant research analyzing the costs of sanctions, only a handful of studies specifically analyze the impacts on income inequality. I suggest that future researchers elaborate on this work by studying how other types of sanction impact inequality. Additionally, future research could study possible alternative conditioning variables such as level of development or type of economy.

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Appendix:

Table A1: Robustness check with WIID Gini-coefficients and deciles

	WIID				SWIID
	Model 1 (Gini)	Model 2 (Decile 1)	Model 3 (Decile 5)	Model 4 (Decile 10)	Model 5 (Gini)
Targeted sanction	0.041 (2.060)	0.496 (0.300)	-0.192 (0.235)	2.113 (1.628)	2.167+ (1.169)
Targeted sanction x Log HHI	1.523 (1.072)	0.052 (0.147)	-0.251* (0.116)	2.132* (0.892)	1.261* (0.506)
Financial sanction	-5.855*** (1.452)	0.643*** (0.169)	0.685** (0.188)	-4.563** (1.312)	-3.361** (1.081)
Financial sanction x share of wealth held by top 1%	0.176*** (0.046)	-0.019** (0.005)	-0.021** (0.006)	0.142** (0.043)	0.102** (0.035)
Log HHI	-0.998+ (0.534)	0.037 (0.066)	0.129+ (0.063)	-1.010* (0.453)	-0.995* (0.373)
Share of wealth held by top 1% (% of total wealth)	0.402*** (0.083)	-0.038*** (0.010)	-0.050*** (0.010)	0.361*** (0.076)	0.264*** (0.049)
Polity	0.025 (0.077)	-0.006 (0.009)	-0.004 (0.009)	-0.005 (0.065)	0.137* (0.054)
War dummy	-0.008 (0.278)	-0.003 (0.035)	0.003 (0.036)	0.019 (0.231)	0.032 (0.193)
Freedom status NF	-1.932+ (1.001)	0.280+ (0.136)	0.140 (0.116)	-1.362 (0.810)	0.355 (0.624)
Freedom status PF	0.062 (0.633)	0.029 (0.071)	-0.063 (0.080)	0.258 (0.509)	0.483 (0.483)
Trade (% of GDP)	-0.001 (0.012)	-0.004* (0.001)	0.002 (0.002)	-0.013 (0.010)	-0.000 (0.010)
FDI (% of GDP)	0.003 (0.004)	0.000 (0.001)	-0.001 (0.001)	0.004 (0.003)	-0.001 (0.003)
Aid dependence (% of GDP)	-0.009 (0.088)	0.006 (0.011)	-0.004 (0.011)	-0.035 (0.081)	0.101+ (0.058)
Log GDP per capita	3.359 (2.565)	-0.267 (0.320)	-0.513 (0.326)	2.834 (1.975)	2.519 (1.830)
(Log GDP per capita) ²	-0.119 (0.147)	0.001 (0.018)	0.022 (0.019)	-0.138 (0.112)	-0.066 (0.106)
Num.Obs.	566	566	566	566	566

R2	0.953	0.909	0.955	0.952	0.971
R2 Adj.	0.942	0.887	0.944	0.941	0.963
AIC	2660.0	316.0	323.9	2469.0	2295.4
RMSE	2.08	0.26	0.26	1.76	1.51

Newey–West standard errors in parentheses. All models include country and year fixed effects (not shown).

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A2: Robustness check with different types of targeted sanctions

	Individuals	Entities/Companies
	Model 2	Model 4
Targeted sanction (individuals)	1.778* (0.688)	
Targeted sanction (entities/companies)		2.311** (0.830)
Targeted sanction (individuals) x Log HHI	0.619* (0.275)	
Targeted sanction (entities/companies) x Log HHI		0.748+ (0.373)
Financial sanction	-1.950* (0.762)	-1.877* (0.760)
Financial sanction x share of wealth held by top 1%	0.061* (0.025)	0.059* (0.025)
HHI	-0.674*** (0.180)	-0.684*** (0.180)
Share of wealth held by top 1% (% of total wealth)	0.272*** (0.033)	0.273*** (0.031)
Polity	0.042 (0.029)	0.042 (0.029)
War dummy	0.133 (0.103)	0.133 (0.103)
Freedom status NF	0.022 (0.521)	-0.055 (0.525)
Freedom status PF	0.200	0.172

	(0.438)	(0.440)
Trade (% of GDP)	0.002	0.003
	(0.006)	(0.006)
FDI (% of GDP)	-0.001	-0.001
	(0.003)	(0.003)
Aid dependence (% of GDP)	0.049*	0.044*
	(0.020)	(0.021)
Log GDP per capita	4.037***	3.969***
	(1.023)	(1.000)
(Log GDP per capita) ²	-0.186**	-0.186**
	(0.061)	(0.060)
Num.Obs.	1374	1374
R2	0.973	0.973
R2 Adj.	0.970	0.970
AIC	5086.3	5081.2
RMSE	1.41	1.41

Newey–West standard errors in parentheses. All models include country and year fixed effects (not shown).

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A3: Robustness check with lagged right-hand variables

	Model 1	Model 2
Targeted sanction (t-1)	0.507+	2.921**
	(0.294)	(0.925)
Targeted sanction (t-1) x log HHI		0.988*
		(0.361)
Financial sanction (t-1)	-0.136	-1.683*
	(0.174)	(0.734)
Financial sanction (t-1) x share of wealth held by top 1% (t-1)		0.052*
		(0.024)
Log HHI (t-1)	-0.694***	-0.756***
	(0.173)	(0.180)
Share of wealth held by top 1% (%) (t-1)	0.265***	0.268***
	(0.032)	(0.032)
Polity (t-1)	0.043	0.047

	(0.029)	(0.029)
War dummy (t-1)	0.094	0.092
	(0.103)	(0.102)
Freedom status NF (t-1)	-0.185	-0.155
	(0.526)	(0.518)
Freedom status PF (t-1)	-0.036	0.036
	(0.437)	(0.429)
Trade (% of GDP) (t-1)	0.003	0.003
	(0.006)	(0.006)
FDI (% of GDP) (t-1)	0.003	0.002
	(0.003)	(0.003)
Aid dependence (% of GDP) (t-1)	0.058+	0.054+
	(0.029)	(0.028)
Log GDP per capita (t-1)	4.053***	3.866***
	(1.036)	(1.023)
(Log GDP per capita) ² (t-1)	-0.191**	-0.177**
	(0.063)	(0.062)
Num.Obs.	1294	1294
R2	0.975	0.975
R2 Adj.	0.972	0.973
AIC	4702.6	4686.2
RMSE	1.36	1.35

Newey–West standard errors in parentheses. All models include country and year fixed effects (not shown).

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A4: Robustness check with different interaction terms

	Targeted Sanction		Financial Sanction	
	Share of wealth	Polity	Aid dependence (% of GDP)	Remittances (% of GDP)
Targeted sanction	3.340**	0.180	2.022*	2.835**
	(0.960)	(0.263)	(0.768)	(0.826)
Targeted sanction x Log HHI			0.740*	1.085**
			(0.327)	(0.345)

Targeted sanction x share of wealth held by top 1%	-0.107**			
	(0.031)			
Targeted sanction x polity		-0.117*		
		(0.048)		
Financial sanction	-1.163+	-1.129	-0.018	-0.518
	(0.666)	(0.680)	(0.196)	(0.472)
Financial sanction x share of wealth held by top 1%	0.042+	0.040+		
	(0.023)	(0.023)		
Financial sanction x aid dependence (% of GDP)			0.031	
			(0.031)	
Financial sanction x log remittances (% of GDP)				-0.121
				(0.101)
Log HHI	-0.543**	-0.509*	-0.632**	-0.594**
	(0.182)	(0.185)	(0.189)	(0.200)
Share of wealth held by top 1% (% of total wealth)	0.292***	0.273***	0.296***	0.295***
	(0.030)	(0.031)	(0.031)	(0.031)
Polity	0.051	0.068+	0.050	0.042
	(0.031)	(0.034)	(0.031)	(0.034)
Aid dependence (% of GDP)	0.056*	0.050*	0.028	0.041
	(0.025)	(0.024)	(0.028)	(0.035)
Log remittances (% of GDP)				0.193*
				(0.077)
War dummy	0.136	0.152	0.130	0.164
	(0.109)	(0.109)	(0.109)	(0.112)
Freedom status NF	0.343	0.435	0.316	0.234
	(0.482)	(0.490)	(0.485)	(0.504)
Freedom status PF	0.420	0.516	0.429	0.406
	(0.406)	(0.412)	(0.402)	(0.404)
Trade (% of GDP)	0.003	0.003	0.002	-0.000
	(0.006)	(0.007)	(0.007)	(0.007)
FDI (% of GDP)		0.001	0.001	0.001
		(0.003)	(0.003)	(0.002)
Log GDP per capita	4.132***	3.890***	4.098***	3.899***
	(1.009)	(1.033)	(1.042)	(1.082)
(Log GDP per capita) ²	-0.186**	-0.174**	-0.185**	-0.169*

	(0.060)	(0.061)	(0.061)	(0.063)
Num.Obs.	1298	1298	1298	1250
R2	0.974	0.974	0.974	0.974
R2 Adj.	0.972	0.972	0.972	0.972
AIC	4745.3	4749.8	4756.7	4598.8
RMSE	1.37	1.37	1.38	1.38

Newey–West standard errors in parentheses. All models include country and year fixed effects (not shown).

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

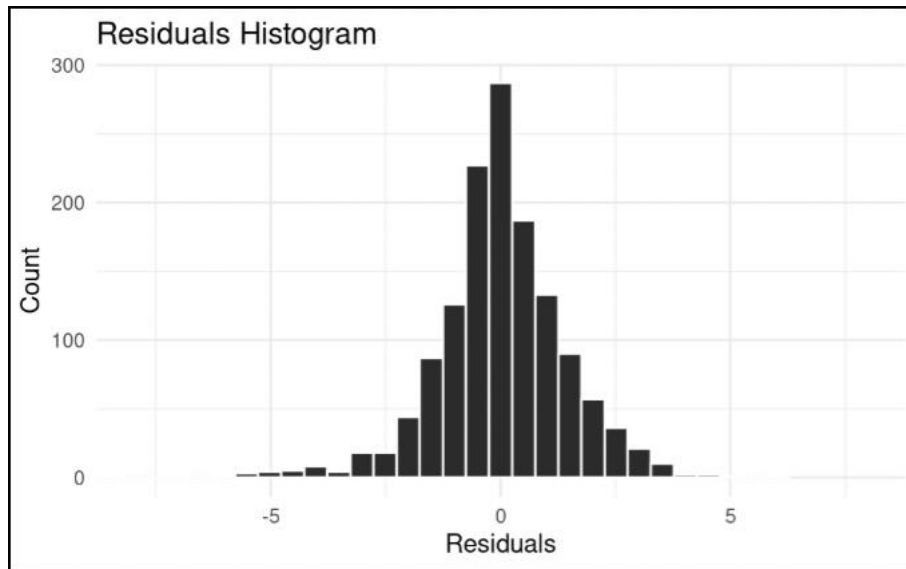


Figure A1: Histogram of residuals from Model 2 in Table 1.