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Matthias Neuenkirch and Florian Neumeier

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Coordination: Bernd Hayo • Philipps-University Marburg
Faculty of Business Administration and Economics • Universitätsstraße 24, D-35032 Marburg
Tel: +49-6421-2823091, Fax: +49-6421-2823088, e-mail: hayo@wiwi.uni-marburg.de

Always Affecting the Wrong People?

The Impact of US Sanctions on Poverty

Matthias Neuenkirch[a] **and Florian Neumeier**[b]

[a]University of Trier

[b]Philipps-University Marburg

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Corresponding author:

Matthias Neuenkirch
Department of Economics
University of Trier
D-54286 Trier
Germany
Tel.: +49 – 651 – 2012629
Fax: +49 – 651 – 2013934
Email: neuenkirch@uni-trier.de

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* The usual disclaimer applies.

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Abstract

In this paper, we analyze the effect of US economic sanctions on the target countries' poverty gap during the period 1978–2011. Econometrically, we employ a nearest neighbor matching approach to account for differences in the countries' economic and political environment and the likelihood of being exposed to US sanctions. Our results indicate that US sanctions are indeed affecting the wrong people as we observe a 2.3–5.1 percentage points (pp) larger poverty gap in sanctioned countries compared to their nearest neighbors. Severe sanctions, such as fuel embargoes, trade restrictions, the freezing of assets, or embargoes on most or all economic activity are particularly detrimental and lead to an increase in the poverty gap by 6.1–7.4 pp.

Keywords: Economic Sanctions, Nearest Neighbor Matching, Poverty, United States.
JEL: F51, F52, F63, I32.

1. Introduction

Over the past several decades, economic sanctions have become a popular tool of statecraft in international politics, and no country in the world has used economic sanctions more often than the US (Hufbauer et al., 2009; Cortright and Lopez, 2000). Designed as a means of compelling governments to comply with the imposing state's interests, these measures aim at changing the target nation's policies by inflicting economic damage. In this regard, they are viewed as a nonviolent, more humane alternative to military intervention. Indeed, the extant economic literature documents that economic sanctions can have a detrimental influence on the target state's economic situation. Neuenkirch and Neumeier (2014) find that the imposition of UN and US sanctions decreases the target state's real GDP per capita growth rate by 2.3–3.5 and 0.5–0.9 percentage points (pp). Hufbauer et al. (2009) report that bi- and multilateral economic sanctions significantly reduce the target state's GNP as well as the volume of bilateral trade between the imposing state and the sanctioned target state.

However, the imposition of economic sanctions is often met with harsh criticism based on the unpleasant reality that the sanctions fail to meet the desired objectives in as many as 65–95% of the cases (Hufbauer et al., 2009; Pape, 1997, 1998), or even worse, prove to be counterproductive. For example, economic sanctions are shown to negatively affect the targeted state's respect for human rights (Peksen, 2009; Wood, 2008) and to deter the development of democracy (Peksen and Drury, 2010). Even more striking, economic sanctions often appear to have devastating consequences on the overall quality of life for the citizens of the target state. Qualitative research based on single-country case studies finds that sanctions negatively affect the availability of food and clean water (Cortright and Lopez, 2000; Weiss et al., 1997), access to medicine and health-care services (e.g., Garfield, 2002; Gibbons and Garfield, 1999), and have an adverse effect on life expectancy and infant mortality (e.g., Ali Mohamed and Shah, 2000; Daponte and Garfield, 2000). These results can be particularly unfair when the regime against which sanctions are directed lacks democratic legitimation.

The paper at hand adds to the literature by providing a quantitative evaluation of the influence economic sanctions exert on a particularly vulnerable group in society, those living in poverty. Economic sanctions typically cause a slump in imports and exports as well as a retraction of foreign investments and aid, which may lead to a shortage in supplies and commodities necessary to secure subsistence (Hufbauer et al., 2009; Heine-

Ellison, 2001; Weiss et al., 1997). In this regard, economic sanctions may aggravate both the *incidence* and the *depth* of economic deprivation.

To the best of our knowledge, the literature lacks a direct assessment of the impact of economic sanctions on the target states' level of poverty. We aim at filling this gap by analyze the effect of US economic sanctions on the target countries' poverty gap, that is, the average shortfall from the poverty line at 1.25 US dollars purchasing power parity (PPP) a day during the period 1978–2011. Econometrically, we employ a nearest neighbor matching approach to examine the impact of US sanctions on poverty in countries exposed to US sanctions (i.e., the treatment group) as compared to non-sanctioned countries which are as similar as possible along two observable dimensions (i.e., the control group). First, we match each sanctioned country with a country not exposed to sanctions based on macroeconomic characteristics which potentially affect the outcome variable of interest. Second, we also take into account that selection into the treatment group might be endogenous and control for the reasons for being exposed to economic sanctions.

Our results indicate that US sanctions are indeed affecting the wrong people as we observe a 2.3–5.1 pp increase in the poverty gap in sanctioned countries as compared to their nearest neighbors. Severe sanctions, such as fuel embargoes, trade restrictions, the freezing of assets, or embargoes on most or all economic activity are particularly detrimental and lead to an increase in the poverty gap by 6.1–7.4 pp.

The remainder of this paper is organized as follows. Section 2 introduces the empirical methodology and Section 3 the dataset. Section 4 presents the empirical results. Section 5 concludes.

2. Empirical Methodology

The aim of this paper is to study whether US sanctions have a detrimental impact on the level of poverty in the target state. The analysis relies on the following measure of poverty:

$$(1)\ pg = 100\frac{1}{N}\sum_{j=1}^{q}\frac{z-y_j}{z}$$

pg is the poverty gap, N is the total population, q is the total population of poor who are living at or below the poverty line, z is the poverty line, and y_j is the income of the poor individual j. In this calculation, individuals whose income is above the poverty line have a poverty gap of zero. By definition, the poverty gap is a percentage between 0 and 100%. Following the classification of the World Bank, we set z to 1.25 US dollars PPP a day. The key advantage of this measure over a poverty headcount ratio is that it measures both, the *incidence* of poverty *and* its *depth* by taking into account the actual shortfall from the poverty line.

The biggest challenge of the empirical work below is to establish a causal link between the imposition of sanctions and the degree of poverty in a country. For one thing, the reasons for imposing economic sanctions—such as, for instance, engagement in interstate conflict, human rights violations, and political repression (see Section 3)—are likely associated with the sanctioned country's political and economic situation which, in turn, are related to poverty. Another issue is that the number of country-year observations for which an internationally comparable measure of the poverty gap is available from a reliable source is relatively small, as the World Bank database reports only 847 observations for 106 countries during the period 1975–2012. To overcome the—in econometric terms—endogeneity with regard to the imposition of economic sanctions and to address the absence of a balanced panel structure, we employ a nearest neighbor matching approach.

Our analysis is based on the idea that the imposition of US sanctions represents a treatment. The units of analysis are country-year observations; observations with US sanctions in place represent the treatment group, observations without US sanctions a potential control group. Our measure of interest is the so-called average treatment effect on the treated (ATT), which is defined as follows:

$$(2)\ \tau_{ATT} = E[pg(1)|T = 1] - E[pg(0)|T = 1]$$

$pg(\cdot)$ is the outcome variable, that is, the average shortfall from the poverty line at 1.25 US dollars PPP a day. T indicates whether a unit is exposed to treatment ($T = 1$) or not ($T = 0$). Accordingly, $E[pg(1)|T = 1]$ is the expected outcome after treatment and $E[pg(0)|T = 1]$ the counterfactual outcome, that is, the outcome a unit exposed to treatment would have achieved if it had not received treatment. As the counterfactual outcome is not observable, we need a suitable proxy to identify the ATT. If the treatment is randomly assigned, then the average outcome of units not exposed to treatment,

$E[pg(0)|T = 0]$, represents a proper substitute. However, as discussed before, the imposition of US sanctions and, thus, selection into treatment is likely endogenous.

To solve the identification problem, we rely on a nearest neighbor matching approach. The idea of nearest neighbor matching is to mimic randomization with regard to the assignment of the treatment. The unobserved counterfactual outcome is imputed by matching the treated units with untreated units which are as similar as possible with regard to all *pre-treatment* characteristics that (i) are associated with selection into treatment (i.e., the likelihood of being exposed to US economic sanctions) and (ii) influence the outcome of interest. The realizations of the poverty gap measure for these matches are then used as an empirical proxy for the unobserved counterfactual.

Formally, the estimate of the ATT based on nearest neighbor matching is defined as follows:

$$(3)\ \hat{\tau}_{ATT}(x) = E[pg(1)|T = 1, X = x] - E[pg(0)|T = 0, X = x]$$

x is a vector of relevant pre-treatment characteristics which are described in Section 3, $E[pg(1)|T = 1, X = x]$ is the expected outcome for the units that received treatment, and $E[pg(0)|T = 0, X = x]$ is the expected outcome for the treated units' nearest neighbors. The nearest neighbors are determined using a distance measure which is a weighted function of the covariates contained in the vector x. The distance between any two units i and j is calculated as follows:

$$(4)\ \|x_i - x_j\| = [(x_i - x_j)' S^{-1}(x_i - x_j)]^{1/2}$$

S is a Mahalanobis scaling matrix used to standardize the realizations of the covariates.

In this paper's context, the intuition behind nearest neighbor matching is to compare the poverty gap of countries which are exposed to US sanctions to that of non-sanctioned countries which are as similar as possible to sanctioned countries. The average difference in poverty between sanctioned countries and the 'nearest' non-sanctioned countries must then be due to treatment, that is, the imposition of economic sanctions by the US. In this sense, the empirical approach mimics a randomized experiment by balancing the treatment and the control group according to observable characteristics.

An advantage of the nearest neighbor matching approach is that it is non-parametric, insofar as no empirical model for either the outcome variable or the selection into treatment needs to be specified. Thus, potential types of misspecification, for instance, those regarding the functional form of the empirical model, which likely lead to biased estimates, are ruled out. The price of this flexibility is that, if more than one continuous covariate is used for matching, the estimate of the ATT is $\sqrt{n}$-consistent only if a bias adjustment is applied (Abadie and Imbens, 2006, 2011). We apply nearest neighbor matching with replacement, meaning that an untreated unit can be used multiple times as a match, which improves the quality of the matching (Caliendo and Kopeinig, 2008).

3. Data

As previously mentioned, we match the treated units with untreated units which are as similar as possible with regard to relevant pre-treatment characteristics. The first group of matching variables captures factors which influence the likelihood of being selected into treatment. Hufbauer et al. (2009) name three primary reasons for the imposition of sanctions: (i) to coerce states (or militant groups within states) to terminate acts that threaten or infringe the sovereignty of another state, that is, by resorting to violence against another state or destabilizing the incumbent government; (ii) to foster democratic change in a country, protect democracy, or destabilize an autocratic regime; (iii) to protect the citizens of a state from political repression and enforce human rights. Consequently, we include the Political Terror Scale indicator, which measures physical integrity rights violations into the vector x. Additionally, we take into account the level of democracy or autocracy in a country. Finally, we control for three different types of conflicts (interstate armed conflicts, internal armed conflicts without intervention from other states, and internationalized internal armed conflicts with intervention from other states) by including six separate dummy variables for minor conflicts and wars.

The second group of variables controls for factors related to the overall level of economic development: (i) the log of real GDP per capita in 2005 US dollars, (ii) trade openness (imports plus exports divided by GDP), (iii) the log of population, (iv) average years of total schooling for people of age 15 and older, (v) the dependency ratio, that is, the ratio of people younger than 15 or older than 64 as percentage of working-age population, (vi) the ratio of people living in rural areas as percentage of the total population, and (vii) the under 5 mortality rate, that is, the probability that a new-born baby will die before reaching age five. We employ the first lag of these variables to circumvent problems of reverse causality. Finally, we add year dummies to control for time-specific effects such as global business cycle movements or changes in the global political environment that affect our sample states (e.g., the fall of the Iron Curtain or the adoption of the Millennium Development Goals). Technically, the inclusion of year dummies makes it more likely that sanctioned countries are matched with non-sanctioned countries in the same year.[1]

Turning to the treatment variable, we rely on a dataset comprising all US sanction episodes based on Wood (2008) and Neuenkirch and Neumeier (2014). As mentioned before, one major issue of the present analysis is the low frequency of internationally comparable data on the poverty gap from a reliable source. After checking for the availability of all control variables, we have 60 country-year observations in which US sanctions were in place.[2] The potential control group consists of 659 observations. This implies that the control group is 11 times larger than the treatment group which allows us to obtain an appropriate match for the sanctioned country-year observation in a comfortable way.[3]

Because the US employs a variety of different sanction measures, the depth of the impact these various economic sanctions might have on the target state's poverty gap depend on the severity of the sanctions. Previous sanction measures employed by the US range from freezing private and public funds and assets to banning grants and credits to imposing embargoes on certain or all economic activities. Consequently, we categorized each sanction as either 'mild,' 'moderate,' or 'severe,' based on the definitions found in Wood (2008) (see Table 1).

Using these definitions, we also test if the detrimental effect of sanctions increases with their severity. For that purpose, we re-do the nearest neighbor matching procedure and match (i) the 37 country-year observations with sanctions of level 1 in place with non-sanctioned country-year observations and (ii) the 23 country-years observations with sanctions of either level 2 or 3 in place with non-sanctioned country-year observations.

[1] A list of the control variables along with their definitions and sources can be found in Table A1 in the Appendix.

[2] We have only four country-year observations for UN sanctions for which data for the poverty gap and all control variables is available. As a consequence, the present analysis focuses on US sanctions only. We feel confident that the imposition of UN sanctions does not confound our empirical results as only one of the UN sanction country-year observations coincides with a US sanction country-year observation.

[3] The number of (non-)sanctioned years per country can be found in Table A2 in the Appendix.

Table 1: Definition of sanction categories

Level	Obs.	Definition
1: mild	37	Retractions of foreign aid, bans on grants, loans, or credits, or restrictions on the sale of specific products or technologies; not including primary commodities embargoes
2: moderate	16	Import or export restrictions, bans on US investment, and other moderate restrictions on trade, finance, and investment between the US and target nation
3: severe	7	Comprehensive economic sanctions such as embargoes on all or most economic activity between the US and the target nation

Source: Wood (2008: 500).

4. Empirical Results

4.1 Descriptive Statistics

Table 2 shows the sample means of the outcome variable and all continuous covariates, split into four groups: country-year observations where sanctions were in place (Sanc. = Yes), country-year observations where no sanctions were in place (Sanc. = No), country-year observations where sanctions of level 1 were in place (Sanc. = 1), and country-year observations where sanctions of level 2 or 3 were in place (Sanc. = 2, 3). The column t-test shows t-test statistics for differences in means between sanctioned and non-sanctioned country-year observations alongside their p-values in square brackets. Table 3 presents the frequency of conflict events, also split into these four groups.

Table 2: Sample Means

	Sanc. = Yes	**Sanc. = No**	**t-test**	**Sanc. = 1**	**Sanc. = 2, 3**
poverty gap 1.25_t	11.0	6.0	−3.9 [0.00]	10.3	12.2
log(real GDP/capita)$_{t-1}$	7.2	7.7	3.9 [0.00]	7.4	6.8
openness$_{t-1}$	49.0	75.1	6.5 [0.00]	52.1	43.9
log(population)$_{t-1}$	17.6	16.5	−4.4 [0.00]	16.7	19.1
schooling$_{t-1}$	5.6	7.5	7.3 [0.00]	5.5	5.6
dependency ratio$_{t-1}$	72.7	64.5	−3.6 [0.00]	76.4	66.7
rural population$_{t-1}$	53.5	44.8	−3.6 [0.00]	48.1	62.0
mortality rate$_{t-1}$	68.9	46.3	−3.8 [0.00]	76.9	55.9
political terror$_t$	3.3	2.6	−5.3 [0.00]	3.1	3.6
polity score$_t$	−0.8	5.2	7.4 [0.00]	1.7	−4.8
observations	60	659		37	23

Notes: Sanc. = Yes: sanctioned country-year observations; Sanc. = No: non-sanctioned country-year observations; t-test: t-test statistics for differences in means between sanctioned and non-sanctioned country-year observations alongside their p-values in square brackets; Sanc. = 1: country-year observations with sanction of level 1; Sanc. = 2, 3: country-year observations with sanction of levels 2 or 3.

Table 3: Frequency of Conflict Events

	Sanc. = Yes			Sanc. = No			Sanc. = 1			Sanc. = 2, 3		
Type of Conflict	**0**	**1**	**2**	**0**	**1**	**2**	**0**	**1**	**2**	**0**	**1**	**2**
interstate_t	52	6	2	654	5	0	31	6	0	21	0	2
internal w/o interv._t	48	12	0	548	92	19	30	7	0	18	5	0
internal w/ interv._t	60	0	0	656	3	0	37	0	0	23	0	0
observations	60			659			37			23		

Notes: 0: no conflict; 1: minor conflicts, defined as between 25 and 999 battle-related deaths in a given year; 2: wars, defined as at least 1,000 battle-related deaths in a given year.

The figures reveal that the poverty gap is larger for country-year observations where sanctions were in place as compared to the observations belonging to the potential control group: the difference is as large as 5 pp. We also have some descriptive evidence that the severity of sanctions matters as countries exposed to moderate or severe sanctions have a larger poverty gap than countries which face mild sanctions.

However, the economic and political environment is generally worse in countries which face US sanctions as these are characterized by (i) a lower real GDP per capita, (ii) less exports and imports, (iii) a larger population, (iv) a lower schooling level, (v) a higher dependency ratio, (vi) a higher share of people living in rural areas, (vii) a higher infant mortality rate, (viii) a higher degree of physical integrity rights violations, (ix) a lower degree of democracy, and (x) a higher likelihood of being involved in an interstate conflict compared to the observations in the potential control group.

These descriptive findings illustrate why it is important to create an appropriate control group using the nearest neighbor matching approach before calculating treatment effects, otherwise, the effect of sanctions on poverty could be incorrectly estimated.

4.2 Treatment Effects

Table 4 shows the ATTs for the poverty gap at 1.25 US dollars PPP.

Table 4: The Impact of Sanctions on the Poverty Gap

	Coef.	S.E.	p-value	Obs.
Sanctions vs no sanctions	5.13	1.19	0.00	719
Sanctions level 1 vs no sanctions	3.07	1.45	0.03	696
Sanctions level 2 or 3 vs no sanctions	6.35	2.01	0.00	682

Notes: Results of Abadie-Imbens (2006, 2011) estimation of average treatment effects on the treated with bias correction for all continuous covariates. Abadie-Imbens (2012) robust standard errors are used.

US economic sanctions have a detrimental impact on the target states' level of poverty. The poverty gap is 5.1 pp larger in sanctioned countries as compared to non-sanctioned countries which were as close as possible in terms of observable political and economic characteristics. The estimate of the treatment effect is significant at the 1 percent level. In addition, the adverse effect of US sanctions clearly increases with their severity. Mild sanctions, which include retractions of foreign aid and credit bans, lead to an increase in the poverty gap by 3.1 pp as compared to the nearest neighbors which are obtained from all non-sanctioned country-year observations. The impact of moderate or severe sanctions, such as fuel embargoes, trade restrictions, the freezing of assets, or embargoes on most or all economic activity are more than twice as large; they lead to an increase in the poverty gap by 6.4 pp. Besides the striking difference in the coefficients' size we also observe differences in the degree of significance. The treatment effect for the levels 2 and 3 is significant at the 1 percent level, whereas the estimate for level 1 is significant at the 5 percent level only.

4.3 Robustness Tests

We explore the robustness of our findings by applying two modifications to the set of covariates employed in the nearest neighbor matching approach. Thus far, our matching approach does not differentiate between countries which never have been exposed to US sanctions and those that have been subject to US sanctions at some point during the sample period. It might be argued that the economic and political environment differs across these two groups of countries beyond the set of covariates employed for the estimation of the ATTs in Section 4.2. Consequently, in a first step, we add a dummy variable to the set of matching covariates which takes the value 1 for countries which have been exposed to US sanctions and zero otherwise. Roughly speaking, the inclusion of this covariate makes it more likely that a sanctioned country-year observation is matched with another observation of a country which has been exposed to US sanctions during the sample period but not in the specific year under consideration.

In a second step, we include the lagged poverty gap for each country in the set of matching covariates to take into account that the stage of economic and political development might differ across country-year observations beyond the set of covariates employed for the estimation of the ATTs in Section 4.2. Roughly speaking, the inclusion of this variable makes it more likely that countries with similar realizations of the past poverty gap are matched. Since we are missing some values for the poverty gap in our dataset we employ the latest available lagged observation for each country and include another variable to the set of covariates which indicates the distance (in years) between the two observations of the poverty gap. Table 5 sets out the results for both modifications.

Table 5: The Impact of Sanctions on the Poverty Gap: Robustness Tests

	Coef.	S.E.	p-value	Obs.
Robustness test 1: Dummy variable for ever-sanctioned countries				
Sanctions vs no sanctions	4.30	1.32	0.00	719
Sanctions level 1 vs no sanctions	2.56	1.54	0.10	696
Sanctions level 2 or 3 vs no sanctions	7.41	2.18	0.00	682
Robustness test 2: Lagged poverty gap				
Sanctions vs no sanctions	2.25	0.91	0.01	642
Sanctions level 1 vs no sanctions	0.98	1.27	0.44	624
Sanctions level 2 or 3 vs no sanctions	6.06	1.43	0.00	617

Notes: Results of Abadie-Imbens (2006, 2011) estimation of average treatment effects on the treated with bias correction for all continuous covariates. Abadie-Imbens (2012) robust standard errors are used.

The inclusion of the dummy variable for ever-sanctioned countries (upper panel of Table 5) and the lagged poverty gap[4] (lower panel of Table 5) to the set of covariates leads to smaller treatment effect estimates for the overall effect of US sanctions on poverty, which is 2.3–4.3 pp instead of 5.1 pp as in Section 4.2. However, both estimates remain highly significant (at the 1 percent level). Similarly, the impact of mild sanctions is lower when including a dummy variable for countries that have ever been exposed to US sanctions (2.6 pp, significant at the 10 percent level) and even insignificant when controlling for the lagged poverty gap. Finally, the treatment effects for moderate or severe sanctions remain roughly the same when modifying the set of matching covariates (6.1–7.4 pp) compared to Section 4.2 (6.4 pp).

To summarize, the estimates of the overall treatment effect and the treatment effect for moderate and severe sanctions are highly significant throughout all three specifications. Consequently, our analysis documents a robust detrimental impact of US sanctions on the target country's level of poverty.

[4] Note that the number of observations is smaller in the lower panel of Table 5 compared to Table 4 and the upper panel of Table 5. Due to the inclusion of the lagged poverty gap we lose the first available observation for each country.

5. Conclusions

In this paper, we analyze the effect of US economic sanctions on the target countries' poverty gap, that is, the average shortfall from the poverty line at 1.25 US dollars PPP a day during the period 1978–2011. Econometrically, we employ a nearest neighbor matching approach to account for differences in the countries' economic and political environment and the likelihood of being exposed to US sanctions.

Our results indicate that US economic sanctions have a detrimental impact on the target states' level of poverty as we observe a 2.3–5.1 pp larger poverty gap in sanctioned countries as compared to their nearest neighbors. Severe sanctions, such as fuel embargoes, trade restrictions, the freezing of assets, or embargoes on most or all economic activity are particularly harmful in terms of their impact on poverty (6.1–7.4 pp).

Our findings indicate that sanctions indeed are affecting the wrong people. The substantial increase in the poverty gap is particularly dreadful since sanctions fail to achieve their aims in 65–95% of the cases in which they are imposed (Hufbauer et al., 2009; Pape, 1997, 1998). Hence, it appears that it is the general population of the sanctioned state who bear the burden of US economic sanctions. And among the general population, one group that is particularly harshly affected is those living in poverty. This appears to be particularly unfair given that the regimes against which sanctions are directed typically lack democratic legitimation.

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Appendix

Table A1: Variable Description and Data Sources

poverty gap 1.25. Average shortfall from the poverty line at 1.25 US dollars PPP a day (counting the non-poor as having zero shortfall), expressed as a percentage of the poverty line. *Source*: World Bank.

log(real GDP/capita). Natural logarithm of real GDP per capita in 2005 US dollars. *Source*: UN.

openness. Sum of exports and imports, expressed as percentage of GDP. *Source*: UN.

log(population). Natural logarithm of total population. *Source*: UN.

schooling. Average years of total schooling for people of age 15 and older. Missing country-year observations are linearly interpolated. *Source*: Barro and Lee (2013).

dependency ratio. Ratio of people younger than 15 or older than 64 as percentage of working-age population between 15 and 64 years. *Source*: World Bank.

rural population. Ratio of people living in rural areas as percentage of total population. *Source*: World Bank.

mortality rate. Probability per 1,000 that a new-born baby will die before reaching age five. *Source*: World Bank.

political terror. Terror scale measuring physical integrity rights violations based on US State Department ratings; ranges from 1 (lowest value) to 5 (highest value). *Source*: Political Terror Scale.

polity score. Polity scale variable; ranges from strongly democratic (+10) to strongly autocratic (–10). *Source*: Polity IV Database.

Table A1: Variable Description and Data Sources (continued)

interstate conflict. Interstate armed conflict between two or more states; indicator variables for minor conflicts (between 25 and 999 battle-related deaths in a given year) and wars (at least 1,000 battle-related deaths in a given year). *Source*: UCDP/PRIO Armed Conflict Dataset.

internal conflict w/o intervention. Internal armed conflict between the government of a state and one or more internal opposition group(s) without intervention from other states; indicator variables for minor conflicts and wars. *Source*: UCDP/PRIO Armed Conflict Dataset.

internal conflict w/ intervention. Internationalized internal armed conflict between the government of a state and one or more internal opposition group(s) with intervention from other states on one or both sides; indicator variables for minor conflicts and wars. *Source*: UCDP/PRIO Armed Conflict Dataset.

US sanctions. As defined in Table 1. *Source*: Hufbauer et al. (2009) and Neuenkirch and Neumeier (2014).

Table A2: List of Sample Countries

Country-year observations belonging to the treatment group and the potential control group (60/247).

Brazil (4/22), Cambodia (1/4), Cameroon (1/2), Central African Republic (1/2), Chile (2/8), China (9/7), Colombia (1/17), Ecuador (3/10), El Salvador (1/14), Fiji (1/1), Gambia (1/1), Guatemala (4/4), Honduras (1/20), India (1/5), Indonesia (3/11), Iran (5/0), Jordan (1/6), Kenya (1/3), Nicaragua (1/3), Pakistan (4/4), Panama (1/13), Peru (1/15), Poland (2/17), Romania (2/14), South Africa (1/4), Syria (1/0), Thailand (1/13), Uruguay (1/17), Vietnam (2/4), Zambia (2/6).

Notes: First figure in brackets indicates country-year observations where US sanctions have been in place (treatment group). Second figure denotes observations without sanctions (potential control group).

Country-year observations belonging to the potential control group only (412).

Albania (5), Algeria (2), Argentina (22), Armenia (11), Bangladesh (8), Benin (1), Bolivia (11), Botswana (2), Bulgaria (8), Burundi (3), Congo (Republic) (1), Costa Rica (23), Croatia (6), Czech Republic (2), Dominican Republic (16), Egypt (5), Estonia (8), Ghana (5), Guyana (2), Hungary (10), Jamaica (8), Kazakhstan (10), Kyrgyzstan (11), Laos (4), Latvia (11), Lesotho (4), Lithuania (8), Malawi (3), Malaysia (9), Mali (4), Mauritania (6), Mexico (13), Moldova (14), Morocco (5), Mozambique (3), Namibia (2), Nepal (4), Niger (4), Papua New Guinea (1), Paraguay (14), Philippines (9), Russia (12), Rwanda (4), Senegal (5), Serbia (9), Sierra Leone (3), Slovak Republic (7), Slovenia (4), Sri Lanka (6), Swaziland (3), Tajikistan (5), Togo (2), Trinidad and Tobago (2), Tunisia (6), Turkey (11), Uganda (7), Ukraine (13), Venezuela (13), Yemen (2).

Notes: Figure in brackets indicates number of country-year observations in the potential control group.