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Sanctioned to Death?
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Abstract

We empirically analyze the effect of UN and US economic sanctions on life expectancy and its gender gap in target countries. Our sample covers 98 less developed and newly industrialized countries over the period 1977–2012. We employ a matching approach to account for the endogeneity of sanctions. Our results indicate that an average episode of UN sanctions reduces life expectancy by about 1.2–1.4 years. The corresponding decrease of 0.4–0.5 years under an average episode of US sanctions is significantly smaller. In addition, we find evidence that women are affected more severely by the imposition of sanctions. Sanctions not being "gender-blind" indicates that they disproportionately affect (the life expectancy of) the more vulnerable members of society. We also detect effect heterogeneity, as the reduction in life expectancy accumulates over time and countries with a better political environment are less heavily affected by economic sanctions. Finally, we provide some evidence that an increase in child mortality and Cholera deaths as well as a decrease in public spending on health care are transmission channels through which UN sanctions adversely affect life expectancy in the targeted countries.

Keywords: Gender Gap, Human Development, Life Expectancy, Sanctions, United Nations, United States.

JEL: F51, F53, I15, J16, K33.

1. Introduction

Economic sanctions are increasingly important in international politics where they frequently serve as a substitute for military confrontation. While sanctions might seem to be a civilized and rather innocuous way of punishing states for violating international law, human rights, or simply the national interests of some other state, their effects can be dramatic. For example, Neuenkirch and Neumeier (2015) find that sanctions imposed by the United Nations and those imposed by the United States reduce the targeted country's GDP by 25% and 13%, respectively. This is hardly surprising in that economic sanctions are intended to force the target country into compliance with the sanctioning countries' demands by inflicting economic harm. Sanctions may have painful consequences for a country's economic elites, as they can, for example, trigger financial crises (Hatipoglu and Peksen 2018), but it seems to be primarily the income of the weak members of society that is hurt by sanctions. Recent studies consistently show that economic sanctions increase both poverty and income inequality in the target state (Afesorgbor and Mahadevan 2016; Choi and Luo 2013; Neuenkirch and Neumeier 2016).

Estimates of economic damage are clearly imperfect proxies for the overall social costs of sanctions.[1] Here, we are interested in the impact of economic sanctions on another dimension of human well-being. Since 1990, the United Nations (2015) has promoted a concept of human development that is broader than mere economic growth. Its "Human Development Index" uses a population's income, life expectancy, and level of education as the ultimate criteria for assessing development. Accordingly, we argue that reductions in life expectancy are an important and yet largely neglected dimension of the social costs of using sanctions.[2] Moreover, studying the effects of sanctions on life expectancy allows us to evaluate their differential effect on the population at large and on a more vulnerable subgroup of the population, that is, women.

To date, only a few empirical studies have started to systematically analyze the health effects of economic sanctions. Peksen (2011) is the first cross-country studies in this field

1 Studies on the human rights consequences of sanctions also show only very specific effects that are in no way reflective of the total negative impact on the population. See Gutmann et al. (2017) for empirical results and a survey of the literature.

2 The third dimension of the Human Development Index, education, is not well-suited for identifying a causal effect of sanctions on human well-being. Education is a highly persistent quantity that does not react quickly to adverse shocks due to sanctions. In addition, there is no broad and reliable database with annual education data that also covers countries subject to sanctions (see Barro and Lee 2013). The same problem arises with the use of happiness data (see DiTella and MacCulluch 2006).

and focuses on child mortality rates as the dependent variable. However, Peksen does not address concerns about endogeneity. Allen and Lektzian (2013) is the only study that estimates the effect of sanctions on life expectancy. They find no statistically significant effect. The major problem of Allen and Lektzian's study is that although they claim to address endogeneity by using a Heckman selection model, their instrumental variables cannot fulfill the exclusion restriction, as sanctions are instrumented with an indicator for military conflict. Yet, the authors themselves argue and even empirically demonstrate that military conflict has a direct effect on life expectancy. Failure to address the endogeneity of sanctions should induce a bias in the estimates, as we illustrate in Section 4.

A more recent single country-study by Parker et al. (2016) analyzes sanctions against firms operating in specific parts of the Democratic Republic of the Congo under section 1502 of the United States' Dodd-Frank Act. Parker et al. find that the boycott on mineral purchases to disrupt the finances of local warlords increased infant deaths in villages near the targeted mines by over 140 percent. They further report evidence that the boycott reduced mothers' consumption of infant health care goods and services. While this study is appealing in light of its identification strategy, these results can hardly be generalized to other countries and types of sanctions.

This paper contributes to the literature on the effects of sanctions by evaluating the association between UN and US sanctions on the one hand and changes in life expectancy as well as its gender gap, in targeted states on the other hand. Garfield (1999) argues that public data on health conditions can be of particularly poor quality during sanction episodes, as reported data are often incomplete, improperly processed, and sometimes even manipulated. Life expectancy, which is based on mortality rates, is a more reliable statistical indicator in such situations and we hence focus on this single measure of health outcomes. Plümper and Neumayer (2006) also discuss the use of health-adjusted life expectancy data based on information from the World Health Organization, but dismiss its use as not practicable and of limited attractiveness.

We study a sample of 98 less developed and newly industrialized countries over the period 1977–2012. A matching approach is employed to account for the potential endogeneity of sanctions. This is the first study to estimate the health and mortality consequences of economic sanctions in a large sample of countries, while taking concerns about the endogeneity of sanction seriously. First, we create a control group comprised of countries not exposed to sanctions that are otherwise as similar as possible to the treatment group (i.e., those countries exposed to sanctions) regarding the observable social, political, and economic conditions that potentially affect the population's life expectancy. We, thus, create credible counterfactuals for countries exposed to sanctions and thereby account for the endogenous selection into the treatment group. Second, we compare the life expectancy in the treatment group to its counterfactual to obtain estimates for the average treatment effect of sanctions. Beyond identifying average treatment effects, we inquire into various forms of effect heterogeneity and analyze several transmission channels through which sanctions could affect life expectancy in the targeted countries.

Our analysis also contributes to a small literature that studies the effects of severe shocks (caused, e.g., by violent conflict or natural disasters) on life expectancy and its gender gap. Here we build on two well-established studies. Plümper and Neumayer (2006) analyze the effect of armed conflict on the gender gap in life expectancy in a sample of 106 countries. Both civil war and interstate war are found to reduce the gender gap in life expectancy, which means that although women generally live longer than men, this difference in life expectancy is significantly reduced by violent conflict. Neumayer and Plümper (2007) study the effect of natural disasters on the gender gap in life expectancy based on a sample of 141 countries. Their results confirm that, analogous to their previous study, natural disasters narrow the gender gap in life expectancy. This effect is larger for disasters with a higher number of casualties and in countries with limited women's rights. Just like conflict and natural disasters, economic sanctions can generate sudden and sizable adverse economic shocks. They undermine the functioning of national health services and the availability of vital goods, including food and medicine, which might be particularly threatening to the most vulnerable social groups.

Our results show that, on average, sanctions are associated with a decrease in life expectancy by about 1.2–1.4 years during an episode of UN sanctions. The corresponding average decrease of 0.4–0.5 years under US sanctions is significantly smaller. In addition, we find evidence that women are affected more severely by the imposition of sanctions; sanctions are not "gender-blind." This finding confirms claims in the qualitative literature on the effects of sanctions. Hence, sanctions are adverse shocks on a society comparable to both violent conflict and natural disasters, which have been shown to affect women more than men. We also detect some effect heterogeneity, as the reduction in life expectancy accumulates over time and countries with a better political environment are less severely affected by economic sanctions. Finally, we provide some evidence that an increase in child mortality and Cholera deaths as well as a decrease in public spending on health care are transmission channels through which UN sanctions adversely affect life expectancy in the targeted countries.

We document the qualitative robustness of our results with two additional exercises. First, we utilize exogenous variation in the effectiveness of US sanctions and show that their adverse effect on life expectancy decreases with the target country's distance from the United States. Second, we rely on the threat of sanctions as a placebo treatment and show that the estimated decrease in life expectancy is indeed caused by the imposition of UN and US sanctions, not by the threat of sanctions or by a particularly bad social, political, and economic situation in target countries. These additional tests support the interpretation of our results as estimates of causal treatment effects.

The remainder of this paper is organized as follows. Section 2 provides our theoretical arguments for why sanctions may have adverse effects on life expectancy and in particular on the life expectancy of women in target countries. Section 3 introduces the dataset and our empirical strategy. Section 4 shows some descriptive statistics. Section 5 presents and discusses the empirical results. Section 6 concludes.

2. Theoretical Considerations and Hypotheses

There are many reasons why sanctions may have an adverse effect on the life expectancy of the target country's population. First, sanctions could damage the country's health infrastructure by limiting the import and production not only of medical supplies, but also of various goods and services that are important for maintaining ambulances, hospitals, and the like. For example, when studying sanctions against Haiti and Iraq, Garfield (1999) reports that adverse consequences for health infrastructure may prevail even when humanitarian goods are explicitly exempted from sanction measures. The limited effectiveness of such exemption clauses is due to the fact that their implementation tends to be imperfect and the general costs of trading goods increase anyway. Second, due to the detrimental economic effects of sanctions and reduced income from tariffs, governments will be more resource constrained and forced to cut public health expenditures. Third, private health services may continue to be available, but increased prices due to higher costs and demand will put these services out of the reach of a large share of the population. This could, in part, explain why more vulnerable members of society—according to Garfield (1997), particularly women and children—will be the most negatively affected by sanctions. Fourth, the decay of public infrastructure, especially a collapse of the sanitation system, can lead to the spreading of infectious diseases. Fifth, harsh economic conditions that force workers to take up any available job, along with a lack of adequate work material and tools, may undermine occupational safety. More generally, economic shocks might be associated with income-smoothing behavior that entails substantial health risks. Burke et al. (2015) demonstrate that adverse income shocks explain up to 20% of variation in HIV prevalence across African countries. Sixth, a shortage of food and clean water also has direct adverse health effects. Taken together, these arguments suggest that the civilian population will suffer from poor health under economic sanctions and that this will disproportionally affect the more vulnerable members of society: the poor, women, children, and the elderly (see also Peksen 2011; Allen and Lektzian 2013).

Our empirical study tests two hypotheses. In line with the above arguments on the health effects of sanctions, we expect that:

H1: Sanctions have a negative effect on the population's life expectancy.

We have argued that women's health is more severely affected by sanctions than that of men. This is for two reasons. During sanction episodes, (i) women might be subject to additional health risks and (ii) at the same time, they are less likely to receive needed medical treatment. The first reason is partially explained by women being forced to enter the labor market and/or to work more hours to secure the subsistence of their household (the so-called added-worker effect; see Sabarwal et al. 2011 for a survey on economic shocks and female labor force participation). Labor market participation may expose women to hazardous working conditions with significant health consequences (Lim et al. 2012; WHO 2009). Duryea et al. (2007) find a 50% increase in the probability of 16-year-old girls in Brazil becoming employed if the male head of the household becomes unemployed, leading most of them to drop out of or not advance in school. Similar effects have been reported for wives and daughters during the Peso crisis in Mexico (Skoufias and Parker 2006; Parker and Skoufias 2006), in the Latin American economic crisis in Buenos Aires (Cerrutti 2000), and in the East Asian crisis in Indonesia (Smith et al. 2002) and the Philippines (Lim 2000). These effects can be large and persistent (see, e.g., Stephens 2002). The added-worker effect manifests primarily among low-income households, women with low education, older women, and in low- and middle-income countries without effective social security systems. At the same time, highly educated women from high-income households may be discouraged from participation in the labor market (Sabarwal et al. 2011). Hence, it is expected that under sanctions, not only will women participate more in the labor market, but also that their participation will more likely occur in occupations prone to health risks. Another explanation why women are more likely to experience additional health risks than men is that women are for physiological reasons more likely harmed by food scarcity and a damaged health infrastructure (Plümper and Neumayer 2006). Women are more susceptible to iron and vitamin deficiencies; moreover, lack of obstetrical care can be a serious health risk for them (see, e.g., WHO 2009).

The second reason given above for women's health being more affected by sanctions is based on the expectation that women with health problems are less likely to benefit from medical care than are men. Rose (1999) studies rainfall shocks in rural India and finds that adverse rainfall shocks pressure households to sacrifice the survival of their daughters. Maccini and Yang (2009) find that Indonesian women who experienced 20% higher rainfall in their year and location of birth are 3.8 percentage points less likely to self-report poor health. There is no such effect on men, which Maccini and Yang explain by gender biases in household resource allocation in favor of men. Baird et al. (2011) and Friedman and Schady (2013) report a robust effect of GDP shocks on infant mortality in developing countries, which is larger for poorer countries and more severe shocks. This adverse effect is twice as large for girls as it is for boys. Although this gender difference is found throughout the world, the effect in the MENA region is particularly pronounced. In this region, the infant mortality of girls increases four times as much as that of boys in the event of a GDP shock. Baird et al. (2011) and Friedman and Schady (2013) conclude from their results that families protect boys more than girls during economic downturns. Barcellos et al. (2014) show that in rural India boys are generally treated better, which results in boys being more resilient, health-wise, to shocks. Cultural reasons are only one explanation for why scarce resources are less likely spent on medical care for females than for males. Another reason is that in most countries, men are the primary income earners. The medical treatment of a male household member can thus be considered a necessary investment, whereas the health of wives and daughters might have to take a backseat in the face of resource constraints (Dercon and Krishnan 2000). Another financial incentive to sacrifice female rather than male offspring is found in sometimes-required dowry that parents have to pay when a girl is married (see Duflo 2012). For these reasons, we expect that:

H2: Sanctions have a larger negative effect on the life expectancy of women than on that of men.

3. Empirical Method and Data

3.1. Matching Using Entropy Balancing

This paper investigates whether UN and US sanctions are significantly related to a deterioration in life expectancy in the target state. The biggest challenge is to establish a *causal* link between the imposition of sanctions and life expectancy. This challenge arises because the reasons for imposing economic sanctions are associated with the social, political, and economic situation in the target country and these conditions are, in turn, directly related to a country's health outcomes.

We employ a matching approach to mitigate this endogeneity problem. We consider the imposition of sanctions by the United Nations or the United States a treatment. Consequently, country-years in which UN or US sanctions were in place comprise the treatment group, observations without UN and US sanctions constitute a potential control group. Our measure of interest is the average treatment effect on the treated (ATT), which is defined as follows:

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

$\Delta le(\cdot)$ is the outcome variable, that is, the absolute change in life expectancy. T indicates whether a unit is exposed to treatment ($T = 1$), or not ($T = 0$). Accordingly, $E[\Delta le(1)|T = 1]$ is the expected outcome after treatment and $E[\Delta le(0)|T = 1]$ is the counterfactual outcome, that is, the outcome a treated unit would have experienced if it had not received the treatment. As this counterfactual outcome is not observable, we need a suitable proxy to be able to identify the ATT. The average outcome of units not exposed to treatment would represent a proper counterfactual only if the treatment is randomly assigned. However, as discussed before, the imposition of sanctions and, thus, selection into treatment is likely endogenous. Employing a matching estimator addresses this problem.

In general, the idea of matching is to mimic randomization of treatment assignment. The unobserved counterfactual outcome is imputed by matching the treated units with untreated units that are as similar as possible regarding all observable *pre-treatment* characteristics that (i) are associated with selection into treatment (i.e., the likelihood of being exposed to economic sanctions) and, at the same time, (ii) influence the outcome of interest. The realizations of change in life expectancy for these matches are then used as an empirical proxy for the unobservable counterfactual. The estimate of the ATT based on matching, that is, the conditional difference in means for the outcome variable between the treatment and control group, is defined as follows:

$$(2)\ \hat{\tau}_{ATT}(x) = E[\Delta le(1)|T = 1, X = x] - E[\Delta le(0)|T = 0, X = x]$$

x is a vector of relevant pre-treatment characteristics, which we describe in more detail below, $E[\Delta le(1)|T = 1, X = x]$ is the expected outcome for the treated units, and $E[\Delta le(0)|T = 0, X = x]$ is the expected outcome for the best matches to those units.

We use entropy balancing, a method proposed by Hainmueller (2012), to select matches for the units exposed to treatment. Entropy balancing is implemented in two steps. First, weights are computed that are assigned to units not subject to treatment. These weights are chosen to satisfy pre-specified balance constraints involving sample moments of pre-treatment characteristics while remaining, at the same time, as close as possible to uniform base weights. In our analysis, the balance constraints require equal covariate means across the treatment and the control group, which ensures that the control group contains units not subject to treatment that are, on average, as similar as possible to the units that received treatment. In the second step, the weights obtained in the first step are used in a weighted regression analysis which includes the treatment indicator as an explanatory variable. This yields an estimate for the ATT. The corresponding regression equation is:

$$(3)\ \Delta le_{it} = \alpha_i + \tau T_{it} + \beta x_{it} + \mu_t + \varepsilon_{it}$$

The index *i* refers to the country and *t* to the year. τ represents the ATT as defined above. α_i is a country-fixed effect, μ_t a year-fixed effect, and x_{it} is the set of pre-treatment characteristics employed in the matching procedure. Including the vector x_{it} in the regression analysis is equivalent to including control variables in a randomized experiment and enhances estimation efficiency. Equation (3) is estimated using weighted least squares; observations in the treatment group have a weight of 1 and observations in the control group have a positive weight obtained from the first step of the matching approach (see also Table A2 in the Appendix).

Its combination of matching and regression analysis gives entropy balancing some advantages over other treatment effect estimators. A particularly important advantage over standard regression-based approaches (including differences-in-differences estimation) as well as matching methods based on propensity scores is that entropy balancing is nonparametric in the sense that no empirical model for either the outcome or the selection into treatment needs to be specified. Hence, it rules out misspecification regarding the functional form of the empirical model, which would yield biased estimates. Also, in contrast to standard regression-based analysis, treatment effect estimates based on entropy balancing do not suffer from multicollinearity, as the reweighting scheme orthogonalizes the covariates with respect to the treatment indicator.

In contrast to other matching methods, entropy balancing ensures a high covariate balance between the treatment and control group, even in small samples. With "conventional" matching methods, such as nearest neighbor matching or propensity score matching, each treated unit is —in the simplest case—matched with the one untreated unit that is closest in terms of a metric balancing score. Accordingly, the control group is comprised of only a subset of the units that are not subject to treatment (Diamond and Sekhon, 2013; Hainmueller, 2012). Put differently, with conventional matching methods, untreated units receive a weight equal to either 0, in the event it is not a best match for a treated unit, or 1, in the event it is a best match for one treated unit. However, this procedure does not guarantee a sufficient balance of pre-treatment characteristics across the treatment and control groups, if the number of untreated units is limited and the number of relevant pre-treatment characteristics is large. This is a serious problem, as a low covariate balance may lead to biased treatment effect estimates. In contrast, in entropy balancing the vector of weights assigned to the units not exposed to treatment is allowed to contain any non-negative values (and not only 0 and 1). Entropy balancing thus can be interpreted as a generalization of conventional matching approaches.[3] By applying weights that indicate the similarity of the untreated to the treated units and that may take on any non-negative values, a synthetic control group is created that represents a virtually perfect image of the treatment group with respect to observable pre-treatment characteristics.[4]

By combining a reweighting scheme with a regression analysis, entropy balancing also allows us to properly address the panel structure of our data. Specifically, we control for both country- and time-fixed effects in the second step of the matching approach. The inclusion of country-fixed effects is particularly helpful in accounting for potential time-invariant unobserved heterogeneity between countries that have never been exposed to sanctions and those that have during the sample period. Otherwise, it could be argued that the social, political, and economic environment of these two groups differs in terms of time invariant characteristics beyond those captured by the set of covariates employed in the entropy balancing approach. The inclusion of year dummies helps to control for time-specific effects, such as technical progress, global business cycles, or changes in the global political environment, that affect all states in our sample.

A word of caution is required concerning the merits of the matching procedure. Identification of a *causal* treatment effect is based on the assumption that adjusting the sample for differences in observable pre-treatment characteristics will remove any bias from comparison between the treatment and control groups. Similar to other treatment effect estimators for non-experimental data, matching approaches may yield biased (and inconsistent) treatment effect estimates in case treatment assignment is related to unobservable or omitted variables. To check for selection bias and to explore the sensitivity of our estimates, we conduct several robustness tests, involving the use of exogenous variation in treatment intensity as well as placebo tests (see Section 5.3.).

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3 A thorough discussion of the advantages of entropy balancing over other matching approaches can be found in Hainmueller (2012). He demonstrates, using Monte Carlo simulations as well as empirical applications, that entropy balancing outperforms other matching techniques, such as propensity score matching, nearest neighbor matching, and genetic matching, in terms of estimation bias and mean square error.

4 In that, entropy balancing is very similar to the synthetic control method pioneered in Abadie and Gardeazabal (2003) and Abadie et al. (2010). The main difference is that the latter method is applied when only one unit is subject to treatment. In such a case, the synthetic control method is used to construct a synthetic control group that resembles the sole treated unit. In contrast, entropy balancing applies to cases where the number of treated units is larger than one. The synthetic control group constructed using entropy balancing resembles the average unit exposed to treatment.

In the subsequent empirical analysis, we estimate sanction effects on the life expectancy of both men and women. To account for correlated error terms across both regression equations, we apply seemingly unrelated regression (SUR) analysis (see Zellner 1962). In particular, SUR regression analysis facilitates testing for differences in the impact of sanctions on life expectancy across both sexes.

3.2. Dependent and Independent Variables

Our dependent variable measures how long someone born in a specific year is expected to live if mortality rates at each age stay as they are at the time of birth.[5] It, thus, reflects present-day mortality rates of each age group in society and not necessarily how mortality rates will evolve in the future. If an aggregate shock, like the imposition of economic sanctions, increases mortality rates in the present, life expectancy at birth instantly adjusts, independent of the permanence of the changes in mortality rates. Naturally, this aggregate measure reacts more strongly to additional deaths among young members of society. Data on life expectancy at birth are from the US Census Bureau (2013), which are believed to be more accurate than similar data from the World Bank (see Plümper and Neumayer 2006).[6] Theoretically, health-adjusted life expectancy (HALE) would be a preferable measure of human well-being, as it adjusts life expectancy for expected years with disability and could, thus, give a more comprehensive picture of the health effects of sanctions. However, HALE data are available for only a few years and, therefore, cannot be used in our analysis. Fortunately, HALE and standard life expectancy tend to be highly correlated. We test whether sanctions affect the life expectancy of both sexes differently by using gender-specific life expectancy data. The combined data on life expectancy and our set of independent variables cover the period 1977–2012 and the sample consists of 2,483 observations in 98 less developed and newly industrialized countries.[7]

Our treatment indicators are built from data on UN and US sanction episodes. Here, we draw on datasets created by Wood (2008), Hufbauer et al. (2009), and Neuenkirch and Neumeier (2015). In total, our sample includes 30 countries that have been targeted by either UN or US sanctions during the period we study, yielding 266 country-year

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[5] All variable definitions and data sources can be found in Table A1 in the Appendix.

[6] The Census Bureau argues that "as a result of single-year age and calendar-year accounting, IDB data capture the timing and demographic impact of important events such as wars, famine, and natural disasters, with a precision exceeding that of other online resources for international demographic data."

[7] The list of countries in our sample can be found in Table A2 in the Appendix.

observations in the treatment group.[8] Based on definitions by Wood (2008), summarized in Table 1, we categorize each sanction as either "mild," "moderate," or "severe."

Table 1: Definition of Sanction Categories

Level	UN Sanctions	Obs.	US Sanctions	Obs.
1: Mild	Restrictions on arms and other military hardware; typically include travel restrictions on a nation's leadership or other diplomatic sanctions	46	Retractions of foreign aid, bans on grants, loans, or credits, or restrictions on the sale of products or technologies; not including primary commodities embargoes	119
2: Moderate	Fuel embargoes, restrictions on trade in primary commodities, or the freezing of public and/or private assets	17	Import or export restrictions, bans on US investment, and other moderate restrictions on trade, finance, and investment between US and target nation	87
3: Severe	Comprehensive economic sanctions, such as embargoes, on all or most economic activity between UN member states and the target	1	Comprehensive economic sanctions, such as embargoes, on all or most economic activity between the US and the target nation	9
Sum		64		215

Notes: The "Obs." columns show the number of sanctioned country-years for which life expectancy data and data for all control variables are available. Definitions are based on Wood (2008:500).

Table 1 shows that the total number of country-year observations in which UN sanctions are in place (64 or 2.6% of all observations) is much lower than that for US sanctions (215 or 8.7%). In addition, US sanctions, on average, fall into a harsher category than those imposed by the United Nations, as 44.7% of US sanctions are moderate or severe (compared to 28.1% for the United Nations). These findings are not surprising, since UN sanctions must be imposed by the UN Security Council, which requires the unanimous consent of five veto powers, whereas US sanctions only have to pass the US Congress. In the subsequent empirical analysis, we utilize this dataset to construct different treatment indicators. In a first step, we employ separate binary treatment indicators for UN and US

8 Information on the sanctioned countries can be found in Table A2 in the Appendix.

sanction episodes. In a second step, we construct binary indicators for mild sanctions on the one hand and moderate/severe sanctions on the other hand.

Our first group of control variables in the vector x of Equations (2) and (3) includes factors relevant for the likelihood of being sanctioned by the United Nations or the United States. Hufbauer et al. (2009) state that sanctions have been imposed mostly for three reasons: (i) to coerce states (or militant groups within states) to stop threatening or infringing the sovereignty of another state; (ii) to foster democratic change in a country, protect democracy, or destabilize an autocratic regime; or (iii) to protect the citizens of a state from political repression and enforce human rights. Consequently, we use the Political Terror Scale indicator to measure physical integrity rights violations and we take into account a country's level of democracy as measured by the Polity 2 indicator. Moreover, we control for the occurrence of minor conflicts (defined as any intrastate or interstate armed conflict resulting in between 25 and 999 battle-related deaths in that year) and major conflicts (defined as conflicts resulting in at least 1,000 battle-related deaths in that year).

The second group of control variables includes factors related to economic development: (i) average years of schooling and the gender gap of this variable,[9] (ii) level of globalization, (iii) log of real GDP per capita, (iv) growth rate of real GDP per capita, (v) log-population size, (vi) growth rate of the population, (vii) share of people living in rural areas, and (viii) log of received official development assistance per capita. We employ the first lag of all these variables to mitigate problems of reverse causality.

We also include the first lag of overall life expectancy and the first lag of the gender gap in life expectancy (i.e., the life expectancy of women minus that of men).[10] Thereby, we take into account that social, political, and economic development differ across country-year observations beyond what is measured by the other covariates. In the case of sanctioned countries, we replace the first lag by the last observed life expectancy before

[9] By controlling for the gender gap in schooling, that is, the total years of schooling for women of age 15 and older minus that for men of age 15 and older, we implicitly take into account *de facto* women's rights. This is important because Neumayer and Plümper (2007) only find an effect on the gender gap in life expectancy that is conditional on women's rights. Knowles et al. (2002) link the gender gap in education directly to adverse health and development outcomes. An advantage of using this indicator as a proxy for women's rights in general is its far superior country and time coverage compared to other indicators.

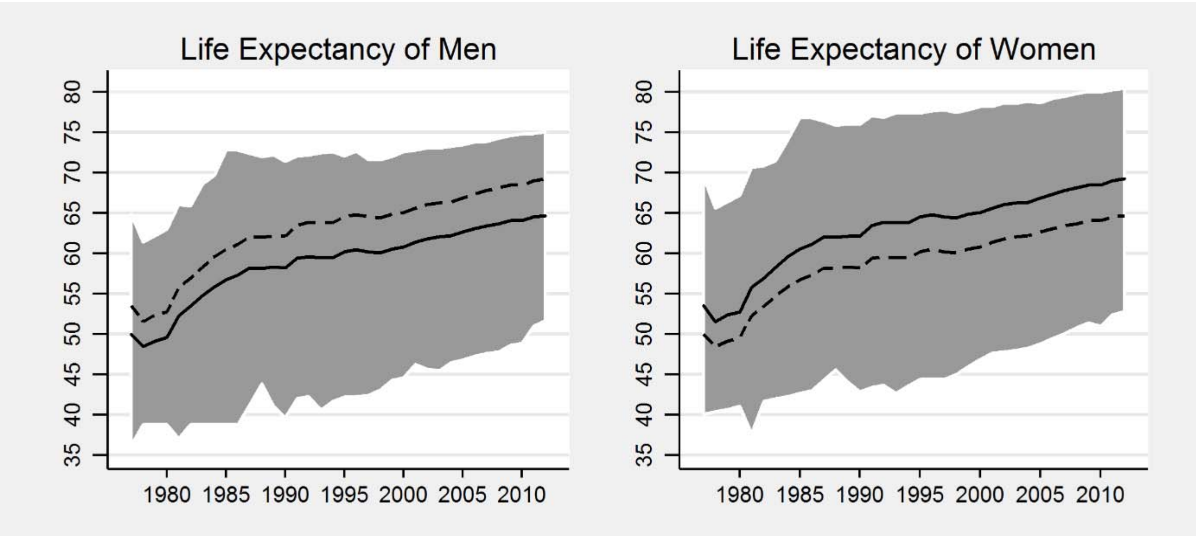
[10] Note that by controlling for overall life expectancy and its gender gap we implicitly control for life expectancy of men and women. If both genders constitute roughly 50% of the total population, life expectancy of men (women) can be obtained by subtracting (adding) 50% of the gender gap from (to) overall life expectancy. The same considerations apply to overall schooling and its gender gap.

the imposition of sanctions to ensure that we compare non-sanctioned observations across groups. Finally, we add year dummies to the control variables.[11]

4. Descriptive Statistics and Covariate Balance

For a first impression of the data and the sample, we begin with some descriptive statistics. Figure 1 shows the time trend in average life expectancy in our sample separately for men and women (solid lines). The shaded areas represent the range between the 5% and the 95% quantile of the distribution. The dashed lines indicate the mean for the opposite sex in order to facilitate comparison.

Figure 1: Life Expectancy Over Time



Notes: Average life expectancy per year (solid lines). Shaded areas represent the range between the 5% and the 95% quantile. Dashed lines indicate, respectively, the mean for the opposite sex.

Figure 1 suggests that there is an upward trend in life expectancy for both genders, as reflected by the mean, the 5th percentile, and the 95th percentile. The average life expectancy of men increased from 50.0 years in 1977 to 64.7 years in 2012. Over the same period, the average life expectancy of women increased from 53.6 years to 69.3 years. The difference between the life expectancy of women and that of men, that is, the gender gap

[11] Note that adding country-fixed effects in the first step of the matching algorithm is not feasible as using these to compute the vector of weights would imply that all countries that were never subject to sanctions would receive a weight of zero and thus be discarded.

in life expectancy, fluctuates between 3.1 years and 4.6 years over the sample period. Overall, the gap widened from 3.6 years in 1977 to 4.6 years in 2012.

For a first impression of the association between sanctions and life expectancy, we check whether average life expectancy differs across our treatment and control groups. Table 2 shows the average life expectancy for men and women when no sanctions are in place (row (1)), when UN sanctions are in place (row (2)), and when US sanctions are in place (row (3)).[12] Rows (4) and (5) report the differences between countries subject to either UN or US sanctions and the non-sanctioned countries, respectively.

Table 2: Differences Across Subgroups

	Life Expectancy Men	Life Expectancy Women
(1) No Sanctions	60.76	64.98
(2) UN Sanctions	46.06	49.35
(3) US Sanctions	57.29	61.05
(4) Difference (2) – (1)	–14.70***	–15.63***
(5) Difference (3) – (1)	–3.47***	–3.93***

Notes: Row (1) shows the average life expectancy in country-year observations without sanctions in place; rows (2) and (3) show the average life expectancy in country-year observations with UN and US sanctions. Rows (4) and (5) report the difference in average life expectancy between countries subject to either UN or US sanctions and non-sanctioned countries, respectively. ***/**/* indicates significance of the corresponding t-statistic at the 1%/5%/10% level.

The life expectancies of men and women are much lower in country-years with UN sanctions in place (relative to no sanctions): 14.7 years for men and 15.6 years for women. There are also lower life expectancies in country-years with US sanctions in effect, although the differences are only 3.5 years for men and 3.9 years for women.[13] Thus, descriptive evidence suggests that women are more strongly affected by UN and US sanctions than are men. But can we take these differences at face value?

Next, we want to gain insight into the conditions under which sanctions are imposed. Table 3 shows the mean values of our control variables when the sample is split into non-sanctioned (column (1)) and sanctioned country-year observations (column (2)). Column (3) reports the differences across the groups and their statistical significance.

12 Our sample contains 13 observations with UN and US sanctions in place in the same country and year.

13 Note that the life expectancy of both men and women is increasing by roughly one year over the five years before a sanction episode starts. This implies that countries that are about to be sanctioned are experiencing an upward trend in life expectancy that is similar to their non-sanctioned counterparts.

Table 3: Covariate Mean Values by Subsample

	(1) No Sanctions	(2) Sanctions	(3) = (2) – (1) Diff.
Lag Life Expectancy Total	62.54	53.92	–8.62***
Lag Life Expectancy Gap	4.20	3.67	–0.53***
Lag Schooling Total	5.84	3.97	–1.87***
Lag Schooling Gap	–0.82	–1.54	–0.72***
Lag Globalization	44.68	34.11	–10.57***
Polity 2	1.57	–1.82	–3.39***
Human Rights Violations	2.66	3.62	0.96***
Minor Conflicts	0.14	0.25	0.11***
Major Conflicts	0.05	0.10	0.05***
Lag Log Real GDP/Capita	7.27	6.28	–0.99***
Lag Real GDP/Capita Growth	1.67	0.86	–0.81**
Lag Log Population	15.97	16.30	0.33***
Lag Population Growth	1.96	2.38	0.42***
Lag Rural Population	54.65	61.32	6.67***
Lag Log Off. Dev. Ass./Capita	3.17	3.12	–0.05
Observations	2,217	266	

Notes: Column (1) shows the mean value for country-year observations without sanctions and column (2) the mean value for country-year observations with sanctions. Column (3) shows the difference between the groups; ***/**/* indicates significance of the corresponding t-statistic at the 1%/5%/10% level.

The figures confirm that country-years during which sanctions are in place differ markedly and with respect to all pre-treatment characteristics from times during which there are no sanctions. Lagged life expectancy is lower for country-year observations with sanctions in place and the difference of 8.6 years is substantial. In addition, the social, political, and economic environment is generally worse in countries that face UN or US sanctions, as these are characterized by (i) less education, (ii) a lower degree of democracy, (iii) a higher level of physical integrity rights violations, (iv) a higher likelihood of being engaged in minor or major conflicts, (v) a lower level of globalization, (vi) a lower real GDP per capita (and its growth rate), (vii) a larger population size (and its growth rate), and (viii) a higher share of people living in rural areas compared to country-years without sanctions in place. The gender gaps in life expectancy and education are significantly smaller in the treatment group, which implies that women face relatively harsher conditions in countries targeted by sanctions. These descriptive statistics illustrate why it is important to use an appropriate control group when estimating sanction effects. Otherwise, the effect of sanctions on life expectancy and its gender gap can be dramatically overestimated, as illustrated by the mean-comparison tests in Table 2. As the existing research on the health effects of sanctions has not dealt with this problem (see Peksen 2011 and Allen and Lektzian 2013), the reliability of their results has to be questioned.

Table 4 reports the descriptive statistics for our sample after the application of the matching algorithm. Column (4) shows the mean values for country-year observations in the synthetic control sample (Control), which is created by entropy balancing. Column (2) shows, as in Table 3, the average conditions for country-year observations with sanctions in place (Sanctions). Column (5) displays the difference in the average conditions between the treated and the synthetic control groups.

Table 4: Covariate Balancing

	(4) Control	(2) Sanctions	(5) = (2) – (4) Diff.
Lag Life Expectancy Total	53.92	53.92	0.00
Lag Life Expectancy Gap	3.67	3.67	0.00
Lag Schooling Total	3.97	3.97	0.00
Lag Schooling Gap	–1.54	–1.54	0.00
Lag Globalization	34.12	34.11	–0.01
Polity 2	–1.82	–1.82	0.00
Human Rights Violations	3.62	3.62	0.00
Minor Conflicts	0.25	0.25	0.00
Major Conflicts	0.10	0.10	0.00
Lag Log Real GDP/Capita	6.28	6.28	0.00
Lag Real GDP/Capita Growth	0.86	0.86	0.00
Lag Log Population	16.30	16.30	0.00
Lag Population Growth	2.38	2.38	0.00
Lag Rural Population	61.31	61.32	0.01
Lag Log Off. Dev. Ass./Capita	3.12	3.12	0.00
Weighted Observations	266	266	

Notes: Column (4) shows the average conditions in country-year observations in the synthetic control sample, which is created by entropy balancing, and column (2) the average conditions in country-year observations with sanctions. Column (5) shows the difference in the average conditions between the groups.

Comparing the average pre-treatment characteristics of the treatment group to those of the synthetic control group reveals the efficacy of entropy balancing. All covariates are virtually perfectly balanced and no statistically significant difference in the mean values remains. Thus, we are confident that the control group in the subsequent empirical analysis is comprised of credible counterfactuals for the sample of country-year observations subject to UN or US sanctions.[14] This allows us to estimate coefficients that should be more reflective of the true causal effect of sanctions than the results of earlier studies.

5. Empirical Results

5.1. Main Specification

Table 5 sets out the results of our baseline specification, where we employ binary indicators that identify country-years in which UN or US economic sanctions, respectively, were in place. Columns (1) and (2) of Table 5 contain the estimated treatment effects of UN and US sanctions on the life expectancy of men and women. Column (3) shows the differences between the coefficients for women and men alongside the corresponding standard errors. The figures in the middle panel of Table 5 represent the difference between the estimated effects of UN and US sanctions. The bottom panel provides additional model statistics.

Table 5: The Impact of Sanctions on Life Expectancy

	(1) Δ(LE Men)	(2) Δ(LE Women)	(3) = (2) – (1) Difference
UN Sanctions	–1.16***	–1.44***	–0.28***
	(0.35)	(0.37)	(0.06)
US Sanctions	–0.37*	–0.46**	–0.09**
	(0.21)	(0.22)	(0.04)
Difference UN – US Sanctions	–0.79*	–0.98**	
	(0.43)	(0.45)	
R^2	0.29	0.28	
Weighted Observations	532	532	
Error Term Correlation (1) and (2)	0.99		
Test for Independence (1) and (2)	2,418.9***		

Notes: Average treatment effect on the treated obtained by seemingly unrelated weighted least squares regression with standard errors in parentheses. Models include country- and year-fixed effects and the full set of matching covariates as control variables. ***/**/* indicates significance at the 1%/5%/10% level.

Our findings suggest that economic sanctions imposed by the United Nations and the United States are associated with a significant decline in life expectancy in the targeted country. The estimated effects are notable. On average, the imposition of UN sanctions is

[14] Information on the countries in the weighted control group can be found in Table A2 in the Appendix.

associated with a decrease in life expectancy of about 1.2–1.4 years over the course of a sanction episode. Thus, our first hypothesis (H1) is supported by the data. This contradicts the empirical results of Allen and Lektzian (2013), who find no effect of sanctions on life expectancy. The effect of US economic sanctions is considerably smaller than that of UN sanctions; life expectancy is reduced by "only" 0.4–0.5 years during an episode of US sanctions. This results does not come at a surprise as multilateral UN sanctions ought to have stronger adverse effects than unilateral US sanctions simply because of the larger number of countries involved in the imposition of the former. In the case of US sanctions, target countries are more likely able to avoid losing access to goods or markets by switching to alternative trading partners.

To put these figures into perspective, we compare their size against two of the control variables in the estimations, minor conflicts and major conflicts. The detrimental effect of minor (major) conflicts on life expectancy amounts to –0.56 (–2.51) years for men and –0.57 (–2.21) years for women, respectively. Hence, the adverse effect of UN sanctions is roughly 45%–65% compared to that of major conflicts, whereas the reduction in life expectancy due to US sanctions is roughly 65%–80% of the effect of minor conflicts.

In addition, we find evidence that women are affected more severely by sanctions. The differences of –0.3 years for UN sanctions and –0.1 years for US sanctions between the effects estimated for men and women, which is equivalent to a 24% difference in the effect size, are statistically significant and confirm our second hypothesis (H2).[15] With regard to life expectancy, women appear to suffer more from sanctions than men. In short, sanctions are not "gender-blind," which is in line with the qualitative literature on the humanitarian costs of sanctions. Here, also our observation in Section 4 should be taken into account that sanctioned countries have, even before they are hit by sanctions, smaller gender gaps in education and life expectancy than other countries. This situation is then deteriorating even further once a country is sanctioned.

5.2. Effect Heterogeneity

In this section, we investigate whether the adverse effect of economic sanctions on life expectancy is related to the length and severity of a sanction episode as well as

[15] Note that the high level of significance of the tests for the gender gap in Column (3) is due to the SUR framework, which takes into account the covariance of the point estimates for men and women.

characteristics depicting the target country's political environment. First, we study the effect UN and US sanctions have on life expectancy over the duration of a sanction episode. To this end, we interact our binary sanction indicator with a variable that counts the number of years sanctions are in place.[16] To facilitate interpretation of our results, we refrain from showing a table with the coefficient estimates. Instead, we graphically illustrate the marginal sanction effect for realized sample values of the variable that indicates the number of years since the imposition of sanctions.[17] The upper part of Figure 1 refers to the case of UN sanctions, the lower part to US sanctions. The figures in the left panel illustrate the development of the sanction effect over time for men, the figures in the middle panel show the effect on women, and the figures in the right panel display the gender gap along with 90% confidence bands.

In general, the sanction effect becomes significant during the second year of the sanction episode, with the effect of UN sanctions on the life expectancy of women being an exception. In this case, the adverse consequences of sanctions are significant already in the year of imposition. Initially, women are affected more severely than men by UN and US sanctions, as indicated by the decrease in the gender gap. We find that the adverse effect of both UN and US sanctions increases over time. In the case of UN sanctions, this increase is larger for men (0.31 years) than for women (0.28 years), implying that it is only during the first eight years of a UN sanction episode that women's life expectancy is affected more than that of men. The corresponding decline per year in life expectancy due to US sanctions is 0.20 years for men and 0.16 years for women. Here, women are affected more than men for the first three years; after seven years, we find the opposite, that is, men are affected more strongly by the imposition of US sanctions than women. Consequently, UN and US sanctions differ in their impact on the gender gap in life expectancy. Compared to men, UN sanctions affect women more strongly. In contrast, long-lasting US sanctions have a stronger detrimental effect on men's life expectancy.

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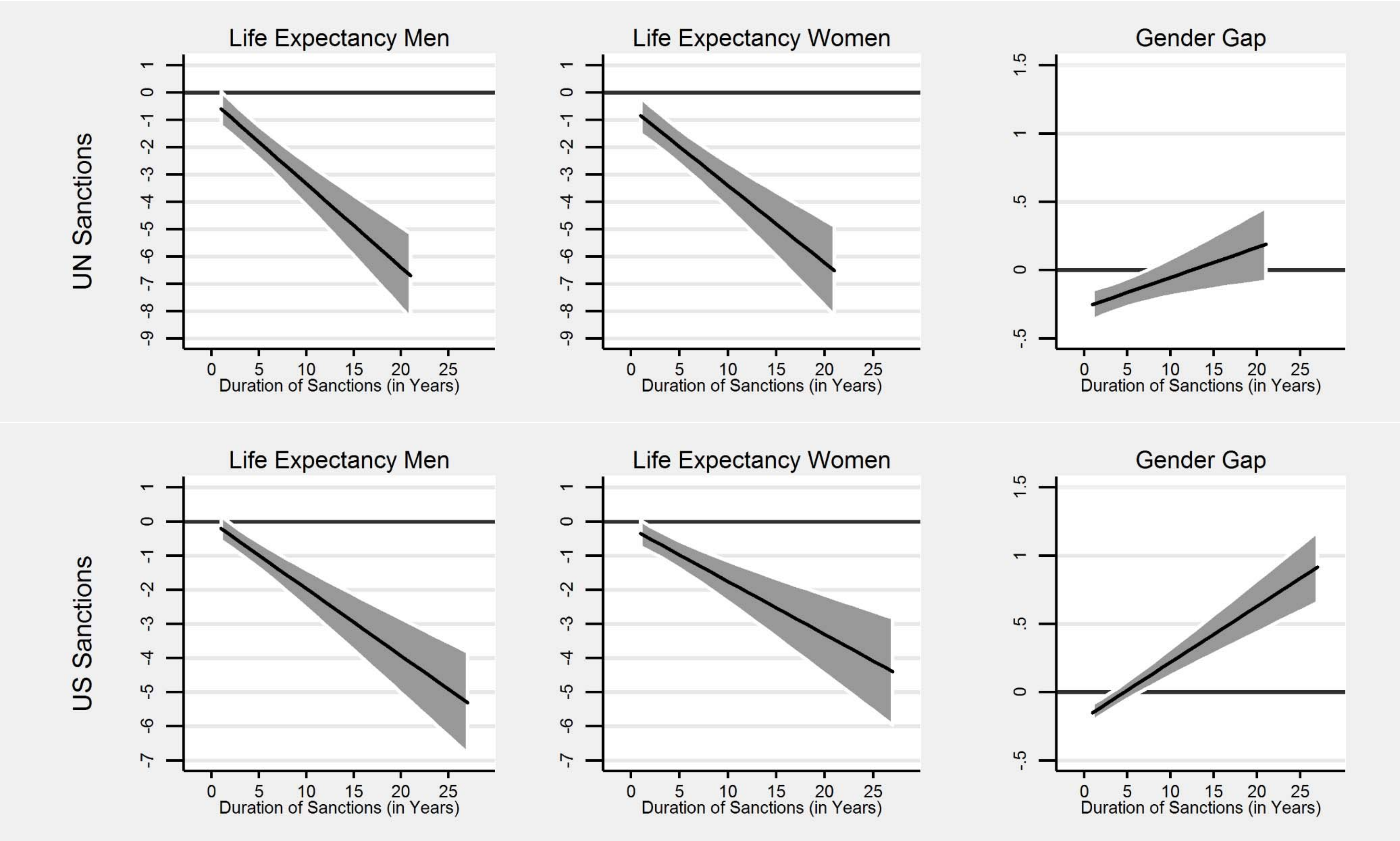
[16] Note that accounting for potential non-linearity by adding an interaction term with the number of years squared in which sanctions are in place yields qualitatively similar results.

[17] The estimates can be found in columns (1) and (2) of Table A3 in the Appendix.

Figure 2: The Impact of Sanctions on Life Expectancy Over the Duration of Sanctions



Notes: Impact of UN and US sanctions on life expectancy over time (solid lines). Shaded areas represent 90% confidence intervals. Estimates are based on the results in columns (1) and (2) of Table A3 in the Appendix. Figure A1 in the Appendix provides background information on the duration of sanctions.

Second, the detrimental effect of economic sanctions might depend on their severity. Sanctions imposed by the United Nations and the United States range from freezing private and public funds and assets to banning grants and credits to imposing embargoes on certain or all economic transactions. In line with the definitions of Wood (2008), we test if moderate and severe sanctions differ in their impact on the target countries' life expectancy compared to mild sanctions. For that purpose, we add indicator variables for the 18 and 96 observations with moderate or severe UN and US sanctions, respectively, to our baseline model.

The results can be found in columns (3) and (4) of Table A3 in the Appendix. In case of UN sanctions, we do not find any significant differences between mild sanctions on the one hand and moderate or severe sanctions on the other hand. Estimates for the US, even indicate that moderate or severe sanctions affect the life expectancy of men to a lesser extent than mild sanctions (but the difference is only significant at the 10% level). In general, moderate and severe sanctions do not appear to be more life threatening than mild sanctions. Indeed, mild sanctions may not be as mild as their name suggests. They refer, for example, to bans on the import of military goods. However, this does frequently include dual-use items that are not only required to construct and maintain weapon systems, but also for the operation and maintenance of medical equipment.

An alternative distinction of interest here is that between targeted and non-targeted sanctions. It has been argued in the literature that targeted or "smart" sanctions allow for exerting pressure without the collateral damages associated with the use of regular sanctions. However, many have also questioned whether these types of sanctions can be the panacea they are supposed to be (see, e.g. Hufbauer and Oegg 2000). We use data by the Targeted Sanctions Consortium Database to distinguish the 46 country-years with targeted UN sanctions from the 16 country-years with non-targeted UN sanctions. The results in columns (5) and (6) of Table A3 speak for themselves. While the estimated effect of targeted sanctions is smaller than that of other sanctions, the difference is clearly not sizable or statistically significant. Taken together, these results suggest that typical categorizations of the severity and targetedness of sanctions are not helpful in predicting the harm done to the target country's population in terms of lost life expectancy.

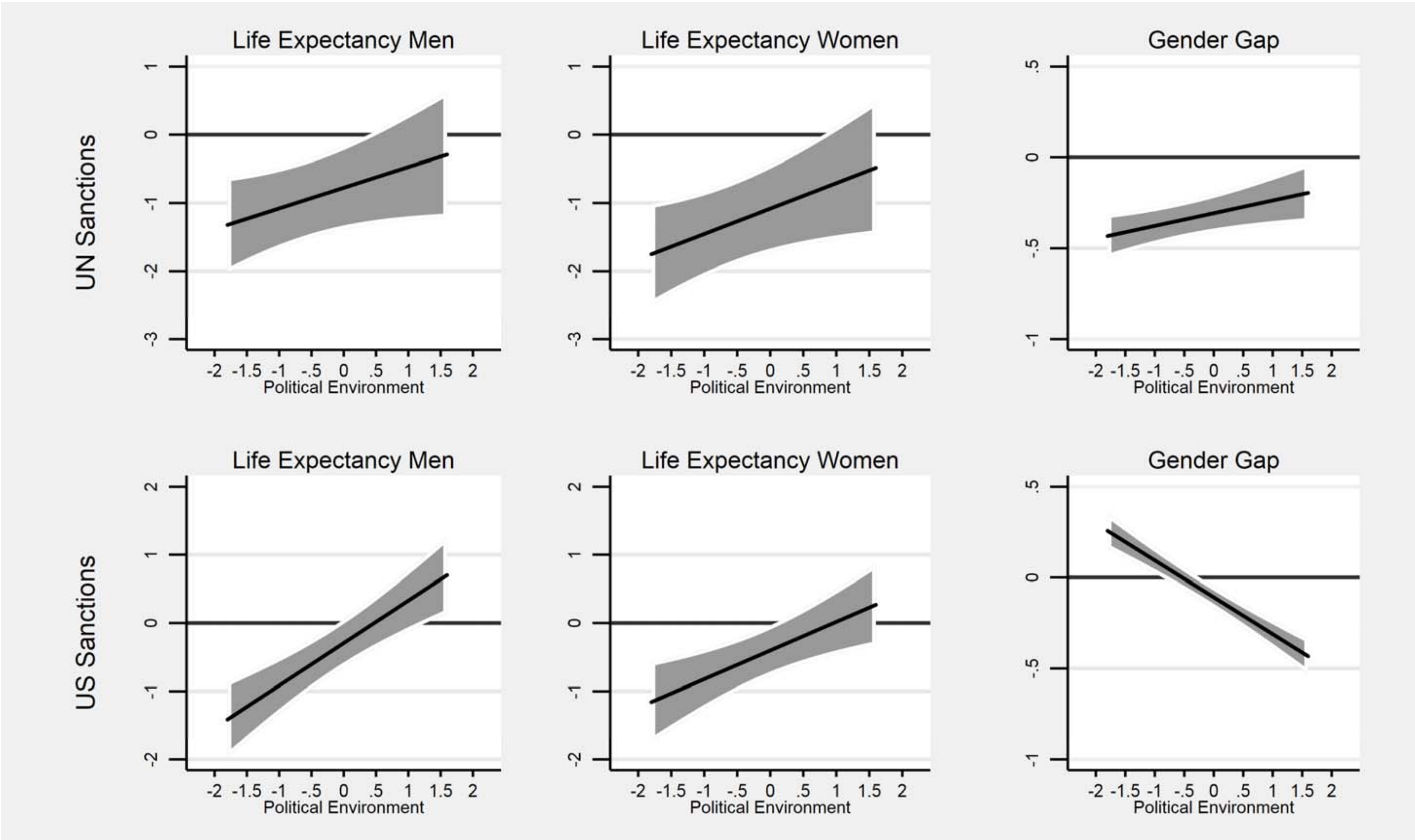
Third, we analyze if the adverse consequences of economic sanctions depend on the political environment in the target country. It can be argued that in a political environment characterized by weak democratic institutions, human rights violations as well as civil conflict, the incumbent government cares less about mitigating the adverse health consequences of economic sanctions (see also Section 2). It has been demonstrated more generally that functional democracies are more responsive in crisis management (Keefer et al. 2011). Consequently, we interact the binary sanction indicators with a variable depicting the quality of the political environment in the target country.[18] Figure 3 graphically illustrates the sanction effects for in-sample values of this indicator variable.[19]

The impact of both UN and US sanctions on life expectancy clearly depends on the political environment in the target countries. A one standard deviation increase in the quality of the political environment is associated with a decrease in the adverse consequences of UN sanctions by 0.3–0.4 years. The effect of US sanctions depends even more strongly on the political environment. Here, life expectancy is only significantly reduced in countries with a below-average quality. Finally, when conditioning on the political environment, women are more strongly affected by UN sanctions than men throughout the whole range of realizations of the corresponding indicator variable, whereas the effect of US sanctions on the gender gap turns negative only in case the political environment is of above-average quality.

[18] This variable is generated by applying a principal component analysis to the Polity 2 indicator (loading: 0.18), the Political Terror Scale (loading: –0.70), and an indicator variable for the occurrence of conflicts (loading: –0.69), and explains 51% of the total variation in these variables. Given these loadings, an increase in the variable implies a better political environment. To facilitate interpretation, we normalize the principal component to a mean of 0 and a standard deviation of 1. Note that adding the individual variables as interaction terms to the baseline model does not yield significant coefficients.

[19] The estimates can be found in columns (5) and (6) of Table A3 in the Appendix.

Figure 3: The Impact of Sanctions on Life Expectancy Depending on Political Environment



Notes: Impact of UN and US sanctions on life expectancy for different political environments (solid lines). Shaded areas represent 90% confidence intervals. Estimates are based on the results in columns (5) and (6) of Table A3 in the Appendix. See also footnote 26.

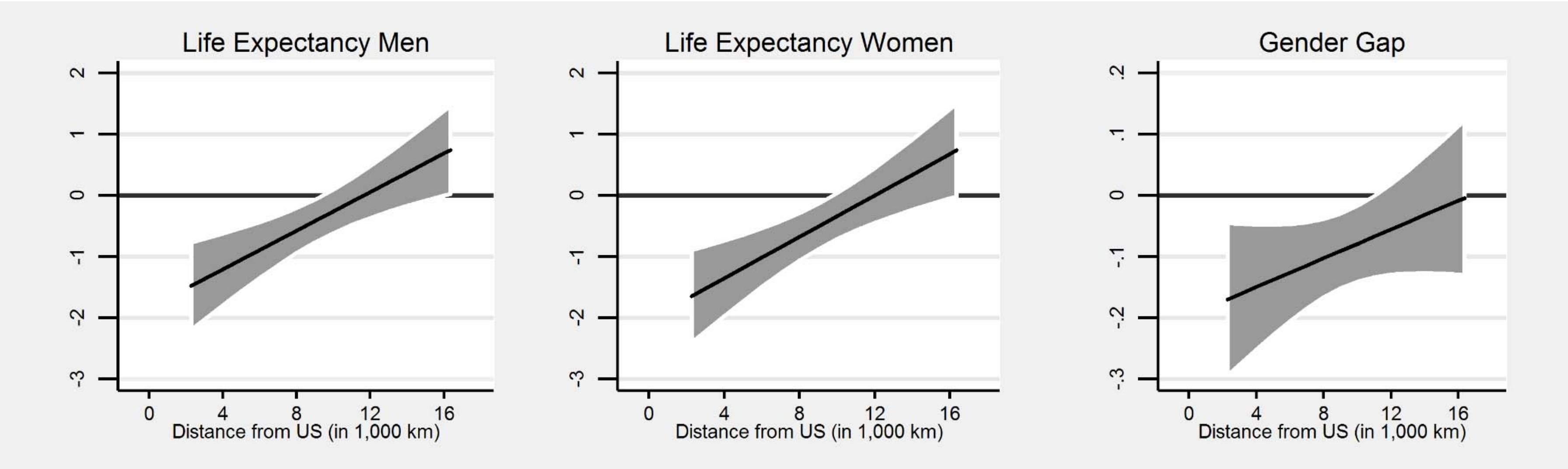
5.3. Robustness Checks

Thus far, our results indicate that economic sanctions do have a sizeable and significant effect on the life expectancy of men and women. However, whether our estimates actually have a causal interpretation depends on the validity of the matching procedure applied in the previous analysis. For our identification strategy to be valid, the treatment—that is, the imposition of economic sanctions by the United Nations or the United States—needs to be unconfounded, implying that adjusting our sample for differences in observable pre-treatment characteristics will remove the bias from the comparison of the treatment and control groups (see Section 3.1.). The assumption of unconfoundedness would be violated if (i) pre-treatment characteristics that affect both the likelihood of being targeted by sanctions and life expectancy are missing in our matching procedure and, at the same time, (ii) the pre-treatment characteristics actually included in our matching procedure are not suitable proxies for the omitted covariates.

One way to address this concern is to utilize exogenous variation in treatment intensity. For US economic sanctions, a variable that can be considered exogenous is the target country's geographical distance from Washington, DC. For instance, Neuenkirch and Neumeier (2015) find that the detrimental effect of US sanctions on the target's GDP growth decreases significantly with the target's distance from the United States. Consequently, we interact our binary US sanction indicator with a variable that measures the target's distance from Washington, DC. To facilitate interpretation, we again refrain from showing the coefficient estimates and instead graphically illustrate the sanction effects.[20] The left panel of Figure 5 illustrates the development of the US sanction effect on men with increasing distance from Washington, DC, the middle panel shows the effect on women, and the right panel demonstrates the gender gap along with 90% confidence bands.

[20] The estimates can be found in Table A4 in the Appendix.

Figure 5: The Impact of Sanctions on Life Expectancy: Distance from the United States



Notes: Impact of US sanctions on life expectancy for different distances from the United States (solid lines). Shaded areas represent 90% confidence intervals. Estimates are based on the results of Table A4 in the Appendix. Figure A2 in the Appendix provides background information on the distance from the United States for observations subject to US sanctions.

The left (middle) panel of Figure 3 reveals that US sanctions have a significantly negative impact on the life expectancy of men (women) in target countries that are less than 9,437 kilometers (9,808 km) away from the United States. The marginal country in our sample for men is Niger, with 8,166 km distance, and for women it is Cameroon, with 9,657 km distance. Thus, US sanctions exert a negative effect on life expectancy even in geographically remote target countries. Confirming H2, US sanctions have a larger negative effect on the life expectancy of women compared to that of men for distances up to 11,039 km. Consequently, women are more affected by US sanctions than men at any distance from the United States for which we estimate a significant effect of sanctions on life expectancy. This concerns roughly 60% of the country-years subject to US sanctions in our sample. Interestingly, the distance of 11,000 km is very close to that estimated by Neuenkirch and Neumeier (2015) for the distance up to which US sanctions significantly reduce a country's growth rate of income per capita.

Another way to check the validity of interpreting our estimates as causal effects is to use a placebo treatment, that is, a "treatment" in a comparable situation for which we would not expect to measure a statistically significant effect. Here, we rely on the threat of sanctions as such a treatment. Morgan et al.'s (2014) dataset allows us to create indicator variables that take the value 1 in the year of a sanction threat by the United Nations or the United States, but only when there was no actual UN or US sanction against the target in the same year.[21] This definition yields a total of 90 country-years with sanction threats but without actual sanctions by the United Nations or the United States. Then, we repeat the entropy balancing algorithm with the treatment group now consisting of countries that were subject to sanctions plus those threatened with sanctions. As before, we estimate separate effects on the life expectancy of men and women and employ seemingly unrelated regression estimation to account for correlated error terms. This time, though, we estimate the effects of three different types of treatment: (i) UN sanctions, (ii) US sanctions, and (iii) sanction threats. Next, we test for differences between actual sanction episodes and sanction threats. Arguably, the initial social, political, and economic situation in countries threatened with the imposition of sanctions by the United Nations or the United States should be roughly comparable to the situation in countries actually

[21] Note that employing separate indicators for UN sanction threats and US sanction threats is not feasible due to the very low frequency of UN sanction threats.

exposed to sanctions. Therefore, significant differences between sanctions and sanction threats would indicate that sanctions do indeed have a causal effect on life expectancy.

In a next step, we recode the sanction threat indicators to take into account some of the years after a threat has been made. More precisely, our sanction threat indicator now takes the value 1 in the year a country was threatened with UN or US sanctions plus the (two) following year(s), but only for as long as the threat has not evolved into the actual imposition of sanctions within this time window. The idea here is that a sanction threat is valid not only in the year it is made; and the conditions for imposing sanctions should also be comparable during the immediate aftermath of the threat. This extension yields a total of 176 (260) sanction-threat-years by the United Nations or the United States. As before, we repeat the entropy balancing algorithm with the treatment group comprising countries that were either sanctioned or subject to a sanction threat in this two-year (three-year) window and estimate seemingly unrelated models to test for differences between these groups. Table 6 sets out the results of this placebo or falsification test.

The treatment effects of UN and US sanctions that were implemented are negative and significantly different from zero for both sexes and throughout all placebo tests. The estimated negative effect of UN sanctions is slightly smaller compared to the baseline results in Table 5, whereas the statistical significance for US sanctions is slightly more pronounced.[22] More importantly, we find no significant decrease in life expectancy when the United Nations or the United States threatened the target country with the imposition of sanctions but do not make good on their threat. In addition, the tests for differences between sanctions and sanction threats are significant for both sexes throughout all placebo tests. Hence, we are confident that the decrease in life expectancy that we measured is caused by the imposition of UN and US sanctions and is not only due to a particularly poor social, political, and economic situation in the target countries.

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[22] One reason for these differences might be the different composition of the treatment group and the resulting weighted control group in the placebo tests.

Table 6: The Impact of Sanctions on Life Expectancy: Placebo Test Using Sanction Threats

	1-Year Threat Window		2-Year Threat Window		3-Year Threat Window	
	(1)	(2)	(3)	(4)	(5)	(6)
	Δ(LE Men)	Δ(LE Women)	Δ(LE Men)	Δ(LE Women)	Δ(LE Men)	Δ(LE Women)
UN Sanctions	−1.01***	−1.17***	−0.82***	−0.90***	−0.80***	−0.86***
	(0.32)	(0.34)	(0.31)	(0.32)	(0.30)	(0.32)
US Sanctions	−0.42**	−0.46**	−0.35**	−0.37**	−0.34**	−0.36**
	(0.17)	(0.18)	(0.16)	(0.16)	(0.15)	(0.16)
Sanction Threats	−0.08	−0.06	−0.04	−0.02	−0.00	0.01
	(0.18)	(0.19)	(0.13)	(0.14)	(0.11)	(0.12)
Difference UN Sanctions – Threats	−0.93**	−1.11***	−0.78**	−0.88**	−0.80**	−0.87**
	(0.37)	(0.39)	(0.34)	(0.35)	(0.33)	(0.34)
Difference US Sanctions – Threats	−0.34	−0.40*	−0.31*	−0.35*	−0.34**	−0.37**
	(0.23)	(0.24)	(0.18)	(0.19)	(0.17)	(0.17)
Weighted Observations	712	712	884	884	1,052	1,052
R^2	0.23	0.21	0.21	0.19	0.20	0.18
Error Term Correlation (1) and (2)	0.98		0.98		0.98	
Test for Independence (1) and (2)	2,399.4***		2,388.3***		2,376.7***	

Notes: Average treatment effect on the treated obtained by seemingly unrelated weighted least squares regression with standard errors in parentheses. Models include country- and year-fixed effects and the set of matching covariates as control variables. ***/**/* indicates significance at the 1%/5%/10% level.

Next, we test if our results are driven by particularly long-lasting sanction episodes. For that purpose, we exclude all observations where sanctions have been in place for more than ten years from our sample. This leaves us with 44 (instead of 64) country-years for UN sanctions and 177 (instead of 215) country-years for US sanctions. We then repeat the entropy balancing algorithm with the treatment group now consisting of countries that were subject to sanctions for ten years or less and estimate a seemingly unrelated regression model.[23] Overall, the (absolute) size of the treatment effects increases marginally in this robustness test. In addition, the significance of US sanction episodes is more pronounced. Hence, we are confident that our key results are not driven by outlier countries that have been subject to sanctions for a prolonged period of time.

Finally, it is worth noting that identification of causal effects with entropy balancing depends on the assumption that no important covariates are omitted, which is inherently untestable. The robustness tests in this section significantly strengthen our argument that we measure causal average treatment effects, at least to the extent to which this is possible with observational data. Our first robustness test interacts US sanctions with the sanctioned country's distance from the US. While it is plausible that countries sanctioned by the US differ from other countries beyond the covariates we account for, there is no reason to assume that the effect of these omitted variables is correlated with the distance to the US. As we find that the adverse effects of sanctions fade out with increasing distance from the US, this is difficult to explain as the result of an omitted variable bias. Our second robustness check compares the effect of sanctions to a placebo treatment that is imposed on countries in a comparable situation. As one would expect, we find no effect of sanction threats by themselves on the life expectancy in targeted countries. This is again difficult to reconcile with the idea that sanctions are simply imposed on countries where life expectancy would deteriorate for other, unobservable reasons.

5.4 Transmission Channels

To round off our analysis, we shed some light on the channels linking sanctions to changes in life expectancy. For that purpose, we study whether economic sanctions affect the mortality rate of children under the age of five years (which also captures infant mortality; data source: US Census Bureau 2013), the number of Cholera deaths (data

[23] To conserve space, we do not report these results in detail. They are available on request.

source: WHO), public expenditure on health care per capita (data source: World Bank), as well as the share of the population that has access to basic sanitation (data source: World Bank). Limited data availability puts severe constraints on researchers interested in the analysis of such transmission channels. Cholera deaths, health expenditures, and access to sanitation serve here as proxies for some of the main health related effects of sanctions. Maintaining or increasing public health expenditures reflects a direct effort by the government to counteract negative health effects caused by economic sanctions. Similarly, increasing or even only upholding access to sanitation in a country targeted by sanctions is a measure of the government's willingness and capability to maintain health related public infrastructure. Preserving health expenditures and public infrastructure are typically challenging tasks for governments facing severe fiscal pressure and rising prices of medical goods due to sanctions. The number of Cholera deaths indicates the extent to which increasing poverty and failing public (health) infrastructure lay the ground for the spreading of epidemics, as it was, for instance, attributed to the UN's sanctions on Iraq (see McCarthy 2000).

Table 7 shows the bivariate correlations between life expectancy and the proxy variables for the sanitary conditions and public health in a country. The figures suggest that all of our indicators are strongly correlated with life expectancy, thus representing potentially important transmission channels through which sanctions may adversely affect the target country's population.

Table 7: Bivariate Correlations

	Life Expectancy	Child Mortality	Log Cholera Deaths	Log Health Exp. pc	Access to Sanitation
Life Expectancy	1.00				
Child Mortality	−0.83	1.00			
Log Cholera Deaths	−0.39	0.35	1.00		
Log Health Exp. pc	0.49	−0.62	−0.20	1.00	
Access to Sanitation	0.54	−0.55	−0.28	0.57	1.00

Notes: Correlation coefficients are computed based on 274 country-year observations for which data on all five variables is available.

We empirically test whether these indicators are related to the imposition of sanctions using entropy balancing. In the matching procedure, we use the same covariates as in our baseline specification plus the first lag of the respective dependent variable, which replaces the lagged realizations of the life-expectancy variable and its gender gap. Note that the number of observations decreases notably when including data on the number of Cholera deaths, health care expenditure, and access to sanitation to our sample. However, the number of country-years with UN and US sanctions in place remains sufficiently large to obtain meaningful estimates.[24] The results are shown in Table 8.

Table 8: Transmission Channels

	(1) Δ(Child Mort.)	(2) Δ(Log Chol. D.)	(3) Δ(Log Health Exp. pc)	(4) Δ(Sanitation)
UN Sanctions	6.19*	1.35*	−0.20***	0.03
	(3.48)	(0.69)	(0.04)	(0.03)
US Sanctions	2.37	0.32	−0.01	0.03
	(2.03)	(0.33)	(0.04)	(0.03)
R^2	0.12	0.49	0.89	0.99
Weight. Obs.	532	256	168	186

Notes: Average treatment effect on the treated obtained by weighted least squares regression with standard errors in parentheses. Models include country- and year-fixed effects and the set of matching covariates as control variables. ***/**/* indicates significance at the 1%/5%/10% level.

Overall, we find that UN sanctions are associated with a significant increase in child mortality and the number of Cholera deaths and a decrease in public spending on health care, indicating that sanitary and medical conditions deteriorate in a country subject to UN sanctions. The estimated effects appear to be sizeable. On average, the number of children that die within the first five years of life (the number of Cholera deaths) increases by about 6.2 per 1,000 births (135 percent) when UN sanctions are imposed, while public spending on health care per capita decreases by 20 percent.

However, we do not detect any significant association between the imposition of US sanctions and our indicators for the sanitary and conditions and public health in a country, although the estimated coefficients show the expected signs. One reason for the lack of significance could be that US sanctions affect life expectancy through different channels than UN sanctions. Another reason, also supported by our smaller coefficient estimate for the effect of US sanctions on life expectancy, could be that the treatment effect of US sanctions simply is too small and noisy. Our findings contradict Peksen's (2011) observation that child mortality is primarily raised by US sanctions.

[24] The number of country-years with UN (US) sanctions in place is 53 (85) when using the number of Cholera deaths as the dependent variable, 40 (44) when using health care expenditure, and 40 (53) when using the indicator for access to basic sanitation.

6. Conclusions

Although life expectancy, and health more generally, has been recognized as a crucial development outcome, studies on how political and social phenomena affect the average life expectancy of a country's population are still surprisingly scarce (see, e.g., Bergh and Nilsson 2010). In this paper, we analyze the association between UN and US economic sanctions on the one hand and life expectancy and its gender gap in the target countries on the other hand.

Our results indicate that, on average, life expectancy decreases by about 1.2–1.4 years during an episode of UN sanctions. The corresponding average decrease of 0.4–0.5 years under US sanctions is significantly smaller. In addition, we find evidence that women are affected more severely by the imposition of sanctions; sanctions are not "gender-blind." This finding confirms claims in the qualitative literature on the effects of sanctions. Hence, sanctions are adverse shocks on a society comparable to both violent conflict and natural disasters, which have been shown to affect women more than men. We also detect some effect heterogeneity, as the reduction in life expectancy accumulates over time. Furthermore, countries with a better political environment are less severely affected by economic sanctions. Finally, we provide some evidence that an increase in child mortality and Cholera deaths as well as a decrease in public spending on health care are transmission channels through which UN sanctions adversely affect life expectancy in the targeted countries.

We document the qualitative robustness of our results with two additional exercises. First, we utilize exogenous variation in the effectiveness of US sanctions and show that their adverse effect on life expectancy decreases with the target country's distance from the United States. Second, we rely on the threat of sanctions as a placebo treatment and show that the estimated decrease in life expectancy is indeed caused by the imposition of UN and US sanctions, not by the threat of sanctions or by a particularly bad social, political, and economic situation in target countries. These additional tests support the interpretation of our results as estimates of causal treatment effects.

The substantial decrease in life expectancy, which is, according to previous studies, accompanied by reduced income per capita and increased income inequality, is particularly disconcerting when considering that sanctions fail to achieve their goals 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 that bears the burden of UN and US economic sanctions, which is particularly worrisome given that the regimes against which sanctions are directed typically lack democratic legitimacy.

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Appendix

Table A1: Variable Definitions and Sources

Distance from US. Distance of the target country's capital from Washington, DC in 1,000 kilometers. *Source*: Gleditsch and Ward (2001).

Globalization. Total globalization as measured by the KOF Globalization Index. *Source*: Dreher (2006).

Human Rights Violations. 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.

Life Expectancy. Average number of years a group of people born in the same year can be expected to live if mortality at each age remains constant in the future. *Source*: US Census Bureau (2013).

Log Off. Dev. Ass./Capita. Natural log plus one-transformation of net official development assistance per capita. *Source*: World Bank.

Log Population. Natural logarithm of total population size. *Source*: United Nations.

Log Real GDP/Capita. Natural logarithm of real GDP per capita in 2005 US dollars. *Source*: United Nations.

Major Conflicts. Interstate armed conflict or internal armed conflict with or without intervention from other states resulting in at least 1,000 battle-related deaths in a given year. *Source*: Gleditsch et al. (2002).

Minor Conflicts. Interstate armed conflict or internal armed conflict with or without intervention from other states resulting in between 25 and 999 battle-related deaths in a given year. *Source*: Gleditsch et al. (2002).

Polity 2. Democracy indicator that ranges from strongly democratic (+10) to strongly autocratic (–10). *Source*: Marshall et al. (2016).

Population Growth. First difference of natural logarithm of total population size. *Source*: United Nations.

Real GDP/Capita Growth. First difference of natural logarithm of real GDP per capita in 2005 US dollars. *Source*: United Nations.

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

Sanctions. As defined in Table 1. *Source*: Wood (2008), Hufbauer et al. (2009), Neuenkirch and Neumeier (2015).

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).

Table A2: List of Countries in Sample

Afghanistan (12/2/15.58), Albania (23/0/0.10), Algeria (25/0/1.18), Argentina (29/0/1.24), Armenia (21/0/1.45), Bahrain (24/0/0.30), Bangladesh (31/0/5.10), Benin (33/0/4.89), Bolivia (20/0/1.10), Botswana (31/0/0.04), Brazil (35/7/9.85), Burma (36/25/4.21), Burundi (33/0/11.91), Cambodia (25/9/2.35), Cameroon (36/7/9.30), Central African Republic (29/3/19.69), Chile (22/0/0.25), China (12/0/1.02), Colombia (36/3/5.23), Congo, Dem. Rep. (36/18/25.98), Congo, Rep. (35/0/5.34), Costa Rica (28/0/0.03), Croatia (16/0/0.07), Cuba (12/0/0.79), Cyprus (13/0/0.00), Dominican Rep. (32/0/0.42), Ecuador (22/5/0.35), Egypt (15/0/0.66), El Salvador (19/0/0.36), Fiji (21/7/0.01), Gabon (36/0/1.49), Gambia (29/5/5.31), Ghana (36/0/2.14), Guatemala (12/0/0.15), Guyana (32/0/0.03), Haiti (36/18/4.73), Honduras (36/1/0.34), India (21/3/1.73), Indonesia (32/9/1.42), Israel (14/0/0.04), Jamaica (30/0/0.02), Jordan (18/0/0.53), Kazakhstan (21/0/3.21), Kenya (33/4/1.54), Kuwait (6/0/0.05), Kyrgyzstan (20/0/0.78), Laos (17/0/4.74), Lesotho (36/0/0.09), Liberia (36/21/8.01), Libya (7/1/0.24), Malawi (34/2/1.61), Malaysia (32/0/0.10), Mali (35/0/3.22), Mauritania (35/0/15.66), Mauritius (29/0/0.03), Mexico (32/0/1.58), Moldova (15/0/0.06), Mongolia (22/0/0.72), Morocco (30/0/3.88), Mozambique (32/0/13.14), Namibia (22/0/0.02), Nepal (36/0/2.09), Nicaragua (36/18/0.41), Niger (35/5/2.45), Pakistan (12/0/1.46), Panama (32/4/0.04), Papua New Guinea (30/0/0.23), Paraguay (36/6/0.68), Peru (31/5/0.89), Philippines (32/0/1.07), Qatar (10/0/0.03), Rwanda (31/15/0.80), Saudi Arabia (16/0/1.10), Senegal (36/0/2.98), Serbia (11/0/0.01), Sierra Leone (36/14/13.40), Singapore (6/0/0.02), Slovenia (7/0/0.00), South Africa (18/0/0.19), South Korea (10/0/0.45), Sri Lanka (14/0/0.03), Swaziland (36/0/0.14), Syria (31/27/0.51), Tajikistan (20/0/2.58), Thailand (22/2/0.10), Togo (31/0/8.43), Trinidad and Tobago (31/0/0.00), Tunisia (26/0/0.88), Turkey (32/0/0.29), Uganda (33/0/6.73), Ukraine (7/0/0.42), United Arab Emirates (10/0/0.04), Uruguay (28/0/0.23), Venezuela (22/0/4.90), Vietnam (14/0/1.10), Yemen (15/0/2.63), Zambia (32/3/1.87), Zimbabwe (30/17/1.40)

Notes: First figure in parentheses is the number of total observations for a country; second figure indicates the number of years with sanctions against that country; third figure denotes the number of observations of a country in the weighted control group.

Table A3: The Impact of Sanctions on Life Expectancy: Effect Heterogeneity

	(1) Δ(LE M)	(2) Δ(LE W)	(3) Δ(LE M)	(4) Δ(LE W)	(5) Δ(LE M)	(6) Δ(LE W)	(7) Δ(LE M)	(8) Δ(LE W)
UN Sanctions	−0.28	−0.56	−1.16***	−1.49***	−1.39***	−1.53***	−0.78**	−1.08***
	(0.40)	(0.42)	(0.40)	(0.42)	(0.46)	(0.48)	(0.37)	(0.39)
... Years	−0.31***	−0.28***						
	(0.05)	(0.05)						
… Moderate/Severe			−0.30	−0.10				
			(0.48)	(0.51)				
… Targeted Sanctions					0.37	0.15		
					(0.48)	(0.51)		
… Political Environment							0.30	0.37*
							(0.20)	(0.21)
US Sanctions	−0.01	−0.20	−0.55**	−0.59**	−0.39*	−0.47**	−0.29	−0.40*
	(0.23)	(0.24)	(0.23)	(0.24)	(0.21)	(0.23)	(0.21)	(0.22)
... Years	−0.20***	−0.16***						
	(0.04)	(0.04)						
… Moderate/Severe			0.52*	0.41				
			(0.30)	(0.31)				
… Political Environment							0.62***	0.42**
							(0.15)	(0.16)
R^2	0.31	0.28	0.30	0.28	0.29	0.28	0.30	0.28
Weighted Observations	532	532	532	532	532	532	532	532
Error Term Correlation	0.99		0.99		0.99		0.99	
Test for Independence	2419.8***		2419.3***		2419.1***		2420.9***	

Notes: Average treatment effect on the treated obtained by seemingly unrelated weighted least squares regression with standard errors in parentheses. Models include country- and year-fixed effects and the set of matching covariates as control variables. ***/**/* indicates significance at the 1%/5%/10% level.

Table A4: The Impact of Sanctions on Life Expectancy: Distance from the United States

	(1) Δ(LE Men)	(2) Δ(LE Women)
UN Sanctions	−1.10***	−1.37***
	(0.35)	(0.37)
US Sanctions	−1.84***	−2.04***
	(0.54)	(0.57)
… Distance to Wash. DC	0.16***	0.17***
	(0.05)	(0.06)
R^2	0.30	0.28
Weighted Observations	532	532
Error Term Correlation (1) and (2)	0.99	
Test for Independence (1) and (2)	2,418.7***	

Notes: Average treatment effect on the treated obtained by seemingly unrelated weighted least squares regression with standard errors in parentheses. Models include country- and year-fixed effects and the set of matching covariates as control variables. ***/**/* indicates significance at the 1%/5%/10% level.

Figure A1: Frequency of Sanction Duration by Type of Sanction

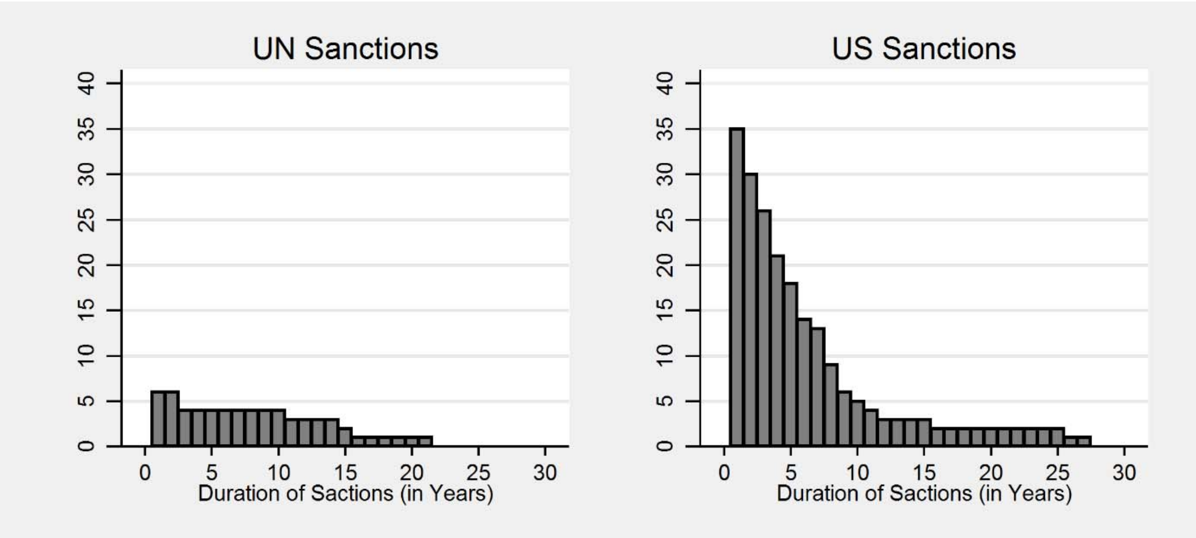

Figure A2: Frequency of Distances from the United States for Observations Subject to US Sanctions