



Do sanctions lead to a decline in civil liberties?

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Abstract

In this paper, we examine the effect of US-imposed sanctions on the civil liberties of the targeted countries for the 1972–2014 period. To deal with the problem of selection and to control for the pre-sanction dynamics, we use a potential outcomes framework, which does not rely on the selection of matching variables and has the further advantage of uncovering the effect of the treatment on the outcome variable over time. What we find is that sanctions result in a decline in civil liberties, measured either by the Freedom House civil liberties index or by the Cingranelli and Richards empowerment rights index. The results are robust across various specifications.

Keywords Sanctions · Democracy · Civil liberties

JEL Classification F51 · F63 · D74

1 Introduction

Economic sanctions have been used as a foreign policy tool since as early as 432 BC.¹ However, their effectiveness is controversial.² The aim of this paper is to ask whether sanctions imposed on nondemocratic regimes reduce repression.

More specifically, we examine the effect of US-imposed sanctions from 1972 onward on civil liberties. Our interest lies in the effect of sanctions on civil liberties for two reasons.

¹ Just before the outbreak of the Peloponnesian War, the Athenian empire levied sanctions on the city of Megara, excluding Megarians from the empire's market and harbors. For many scholars, the economic consequences of those sanctions were a decisive factor in the outbreak of the Peloponnesian War (Brunt 1951).

² The recent debate over the appropriateness of imposing sanctions in order to destabilize the Maduro regime in Venezuela is suggestive: proponents of the use of sanctions claim that immiserizing the general population will destabilize the regime and eventually improve the conditions in the country. On the other hand, opponents consider the measures to be ineffective since they give the regime the opportunity to use propaganda and repression to strengthen its position further.

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First, in some cases, sanctions are used as a policy tool to improve civil liberties in non-democratic regimes. Therefore, their success should be judged on their effectiveness in reducing repression. Of course, we acknowledge the fact that sanctions often are used for reasons other than improving civil liberties. For example, none of the sanctions recently imposed by the United States on China, Iran and North Korea had improvements in human rights as a major policy goal. Even in those cases, however, improvements in civil liberties may come as additional positive side effects.³ Second, civil liberties, and political institutions in general, are found to affect various economic variables, such as growth (Knutson 2015; Acemoglu et al. 2018), foreign investment (Adam and Filippaios 2007), aid effectiveness (Denizer et al. 2013) and, thus, have a direct effect on the development path of a country.

As our analysis focuses mainly on US-imposed sanctions, we are effectively examining the sanctions imposed by the major actor in the international arena.⁴ Moreover, following the relevant literature, one of the reasons US sanctions are imposed is to protect citizens from repression (see Hufbauer et al. 2009; Gutmann et al. 2016).⁵ Hence, US foreign policies are linked closely to the problem under examination.

Over the recent years, a vast literature has emerged examining the effect of sanctions on the targeted economy (see, for example, Cortright and Lopez 2000; Daponte and Garfield 2000; Hufbauer et al. 2009; Neuenkirch and Neumeier 2016). Several contributions have investigated the effects of sanctions on civil liberties and democracy. For example, Wood (2008), Peksen and Drury (2009, 2010) and Peksen (2009) find that economic sanctions result in a statistically and quantitatively significant increase in the level of state repression as well as in a decline in the quality of democracy of the target country.⁶ More recently, Gutmann et al. (2016), using an instrumental variables (IV) strategy, verify the above findings.

The present paper complements extant studies by following a different identification strategy, namely, a potential outcomes framework, as in Acemoglu et al. (2018). The major advantage of this analysis, compared to the simple fixed effects estimator, is that it deals flexibly with endogeneity, which may exist for various reasons. For example, as argued in Nooruddin (2002), we do not have information on instances of sanctions that might have been considered, but not implemented for strategic reasons. Similarly, factors correlated with greater repression may lead to sanction imposition and affect future changes in civil liberties (Lektzian and Souva 2003; Cox and Drury 2006).⁷

We build a panel dataset of 166 countries over the 1972–2014 period. Our main variable of interest, i.e., sanction impositions, is taken from the Threat and Imposition of Sanctions

³ With that in mind, our baseline analysis includes a general category of sanction episodes and not only those with the goal of reducing repression and destabilizing the targeted regime.

⁴ To examine the robustness of our results, we also examine UN-imposed sanctions as well as sanctions imposed by all states.

⁵ Choi and James (2016) provide evidence that US military interventions also are primarily prompted by the same reason.

⁶ Specifically, Peksen and Drury (2009, 2010) find that sanctions have a negative effect on the level of democracy, as measured by the Freedom House index of civil and political liberties, which in the long run is equivalent to approximately a one-point decline in the index on its 1–7 cardinal scale. Peksen (2009) and Wood (2008) find similar effects using an index of respect for human rights and an index of political terror, as dependent variables, respectively.

⁷ Nooruddin (2002) deals with the problem of selection by estimating a selection model in the spirit of Heckman (1979). Similarly, Jing et al. (2003) estimate a two-stage model wherein sanctions are imposed according to an endogenous political process and the model therefore controls effectively for both the selection and the confounding factors problem. Here we follow another approach, mimicking randomization.

(TIES) database (Morgan et al. 2014). To measure the effect of sanctions on respect for civil liberties, we use two outcome variables, the index of civil liberties constructed by the Freedom House and the empowerment rights index constructed by Cingranelli and Richards (2010). As our analysis shows, irrespective of the outcome variable employed, sanctions typically lead to a decline in civil liberties and to an increase in the level of repression.

Although our main analysis is based on a quasi-experimental method, our first step is to replicate existing findings using a simple panel data fixed-effects model. Then, we employ semiparametric methods that explicitly deal with the dynamics of the dependent variable. Specifically, following Acemoglu et al. (2018), we model the counterfactual process for the outcome variables using (1) the regression adjustment, (2) the inverse probability weighting and (3) the doubly robust estimator, which combines (1) and (2). The advantage of all three methods is that they control for time and country fixed effects and, thus, consider fixed country and institutional characteristics, the historical relationship with the sanctioning country (i.e., the United States) and the time-specific political climate. Moreover, as the methods are semiparametric, they do not rely on the selection of instruments or matching variables, but instead use lagged values of the variables. Interestingly, and this is a further advantage of the methodology, we are able to examine the evolution of civil liberties after the year sanctions are imposed by estimating the time path of the outcome variable.

Our results can be summarized as follows: (1) sanctions increase repression, (2) that effect is present even when we control for selection and pre-sanction dynamics and (3) the effect is more pronounced when we consider sanctions imposed only to improve human rights and destabilize the regime. The estimated negative effect is by no means negligible, as it is close to half a standard deviation of the civil liberties measure in our sample. All effects are robust across alternative specifications.

The rest of the paper is structured as follows: in the following section, we present the theoretical considerations that motivate our empirical analysis. Section 3 presents our data and the empirical methodology employed. Section 4 reports the results. Finally, Sect. 5 offers some concluding remarks.

2 Theoretical considerations

The effect of sanctions on political regimes has been the focus of a number of theoretical contributions. In the present section, we examine the impact of sanctions only on the level of repression in the targeted country.

To our knowledge, the only paper that explicitly models sanctions against a dictator is by Kaempfer et al. (2004), who build on the model of Wintrobe (1998). According to this general model, the dictator maximizes personal consumption and power using two inputs, loyalty and repression. Loyalty is “purchased” by the dictator by awarding rents to his subjects. Repression, on the other hand, eliminates opposition with the use of force. Both the use of loyalty and repression come at the cost of diverting resources from other productive uses, but each of them has a different price. Kaempfer et al. (2004) show that sanctions alter the prices of loyalty and repression. In particular, they assume that even though the effect on the price of loyalty is ambiguous, the price of repression increases owing to economic sanctions, as sanctions have a direct effect on the price of the imported inputs used in repression (e.g., military and police equipment).

Hence, the above model shows that the net effect of sanctions on the level of repression depends on the response of domestic opposition groups. If the domestic opposition stands to gain from sanctions, e.g., because they attract more support from the public, the price of loyalty will increase. With higher prices for both repression and loyalty, the price of holding power also increases. If sanctions reduce dictator rents, the net effect will be a reduction in repression and/or overthrow of the dictator if he can no longer achieve the minimum level of support required to stay in power. On the other hand, if sanctions impoverish the general population, to the extent that they reduce the power of the domestic opposition, the price of repression is reduced for the dictator. That possibility causes a substitution of repression for loyalty, resulting in a worsening of civil liberties for the country's population.⁸

Acemoglu and Robinson (2005) offer additional arguments that support the view that sanctions induce repression. As economic sanctions reduce or completely halt trade flows, the targeted country becomes economically isolated. For a low-income country, isolation implies that the returns to labor, i.e., the relatively abundant factor, decline, whereas the returns to capital, i.e., the relatively scarce factor, increase. This, in turn, implies an increase in the level of income inequality and subsequently stronger demands for redistributive policies. Therefore, democracy becomes less appealing to the rich elite. Following the rationale of Acemoglu and Robinson (2005), as the rich elite have more to fear from democracy, they are more willing to use repression either to keep the nature of an existing autocratic regime or to limit the power of the poor within a democratic one.

Building on their own work on political accountability in a dictatorship, Bueno de Mesquita and Silverson (1995) show that participation in an international war creates a “rally around the flag” effect, i.e., a patriotic response in support of the regime and against the “foreign intervention” (see also Galtung 1967; Nooruddin 2002). Sanctions, in essence, are a form of war and, thus, a similar effect is predicted in that setting as well. A (rational) dictator stays in power trading off forms of support and repression. One way of earning support is by distributing rents to his supporters. Repression, on the other hand, limits the power of the opposition, but at the cost of creating distortions in the economy. An exogenous increase in support predicted by the rally around the flag effect makes repression a less costly alternative, i.e., it reduces the marginal cost of repression, eventually resulting in more of it in equilibrium.

Following the above theoretical predictions, we may express our testable hypothesis as follows:

Hypothesis Economic sanctions increase the level of repression and have a negative effect on respect for civil liberties in the targeted nation.

Arguably, this hypothesis contradicts the conventional wisdom, which suggests that a dictator facing sanctions is not able to acquire the economic and military resources used as inputs in the production of repression (Peksen 2009). If that is the case, then sanctions will be able to deliver improvements in civil liberties and human rights in sharp contrast to the predictions of the political economy models presented above. In the following section, we test the empirical validity of our hypothesis.

⁸ Kaempfer et al. (2004) consider a policy of using the sanctions to target directly the means of repression as more effective, since it increases the price of repression and thus destabilizes the regime.

3 Data and empirical methodology

Our sample consists of 166 countries over the 1972–2014 period.⁹ To measure the degree of respect for civil liberties in each country, we employ two different independent variables. The first one is the civil liberties Gastil index, constructed by Freedom House. The Gastil index is based on ideals about civil liberties; countries that come closer to those ideals receive lower ratings.¹⁰ We normalize the value of the index onto a [0,1] range so that 0 (*resp.* 1) corresponds to a low (*resp.* high) level of respect for civil liberties. As an alternative measure, we use the empowerment rights index, taken from the Cingranelli and Richards human rights index. The empowerment rights index is a synthetic indicator of the rights to foreign and domestic movement, freedom of speech, freedom of assembly and association, electoral self-determination and freedom of religion (see Cingranelli and Richards 2010). We also convert the index onto a [0,1] scale so higher values indicate less repression.

The *Sanctions* dummy variable is taken from Morgan et al. (2014), TIES database, which codes all sanction episodes after 1945. That dataset provides extensive information about all sanction episodes. Specifically, it includes details about the first and last year of each occurrence, whether actually implemented or just threatened, the countries involved, the issue over which sanctions were imposed, the cost of the sender and of the target country, and so on. In our dataset, we include imposed sanctions, where at least one of the senders is the United States.¹¹

We focus mainly on US-imposed sanctions for a number of reasons. First, the United States participated as a sender in most of the documented sanctions from 1946 onward.¹² Second, particularly after the end of the Cold War, the United States became the major hegemonic power in the international political arena. Third, according to Hufbauer et al. (2009) and Gutmann et al. (2016), US sanctions primarily are imposed to coerce states to stop threatening other states, to foster and protect democracy and to protect citizens from political repression (also see Choi and James 2016). Therefore, US-imposed sanctions appear to be the most relevant case of sanctions for hypothesis testing. Last but not least, by studying only US sanctions, we are able effectively to control for the institutional environment of the sender state (Lektzian and Souva 2001).

The TIES database contains a number of cases in which both the sender and the targeted country are high-income democratic countries. Such observations relate to disputes between two states over purely economic issues and do not, *á priori*, appear to be related to (or expected to influence) the targeted regime's type. For example, in 1993, the United

⁹ The countries in our sample are listed in the “Appendix”.

¹⁰ The political liberties ideals involve, as Gastil (1982, p. 7) points out, the “rights to participate meaningfully in the political process. In a democracy this means the right of all adults to vote and compete for public office, and for elected representatives to have a decisive vote on public policies”. Civil liberties ideals involve a series of various economic, political and civil liberties enjoyed by the citizens of the country, such as freedom of expression and belief, association and organization rights, rule of law and personal autonomy and economic rights. Similarly, in the words of Gastil (*ibid.*), “Civil liberties are rights to free expression, to organize or demonstrate, as well as rights to a degree of autonomy such as is provided by freedom of religion, education, travel, and other personal rights”.

¹¹ As robustness analysis, we also examine the effect of sanctions imposed by the United Nations and sanctions imposed by all states.

¹² The United States participated in 46.5% of all sanctions imposed and issued 56.4% of all sanction threats from 1946 up to 2004.

States threatened to impose sanctions on Japan because of discrimination in the procurement of construction, architectural and engineering services. Those sanctions, of course, were not related to the type of regime in Japan. In fact, the entire episode was terminated before sanctions actually were implemented, as Japan announced an action plan to reform its public sector construction market (see Services 2001 for more details). Several US sanction episodes involve similar disputes. Therefore, we exclude all observations that involve disputes only over environmental policies, trade practices, implementation of economic reforms and other minor issues. In contrast, we include all sanctions that were imposed with the aim of altering political and military behavior; destabilizing the regime; releasing property, materials or citizens; solving territorial disputes; denying access to strategic materials; retaliating for alliance or alignment choices; improving human rights; ending weapons and material proliferation; terminating support for non-state actors; and deterring or punishing drug trafficking processes. If an episode is reported to be implemented for one of the latter reasons, we include it in the definition of *Sanctions* irrespective of whether a secondary issue from the excluded former group is mentioned.

To establish the robustness of our results, we also examine cases of sanctions imposed only to destabilize the regime or to improve human rights. Because sanctions for those purposes are imposed on nondemocratic regimes, we expect all relevant effects to be more pronounced in these cases. Additionally, we perform a placebo type of analysis by examining sanctions imposed only for reasons that were excluded in our initial measure. As argued above, such sanction episodes are not related to the type of the regime, but instead reflect disputes over purely economic issues, most of the time among countries with strong respect for civil liberties. It is, then, natural to expect a nonsignificant effect of sanctions on civil liberties. Our dataset includes all imposed sanctions until 2005 that were removed or remained effective until 2014.

To estimate the effect of sanctions on civil liberties, we follow two strategies. First, we estimate a simple linear fixed effects panel regression of the form:

$$CL_{i,t} = \beta * sanctions_{i,t} + \gamma * X_{i,t} + \mu_t + \lambda_i + u_{i,t}, \quad (1)$$

where $CL_{i,t}$ is the civil liberties index, i.e., either the Freedom House index or the empowerment rights index, and $X_{i,t}$ is a vector of control variables. Parameters μ_t , λ_i represent the time and country fixed effects, respectively. In accordance with Acemoglu et al. (2009), we estimate the above model using the fixed effects within estimator to account for time-invariant characteristics. The country fixed effects may account for the effect of historical factors that affect democracy and may be the driving forces behind critical events that determine whether a country ends up being a democracy or not (Acemoglu et al. 2009). Time fixed effects can control for changes in the international environment, e.g., the third wave of democratization (Huntington 1993), the 1989 collapse of the Soviet Union and the rise of the United States as a single hegemonic power in the world system.

The vector $X_{i,t}$ contains a series of control variables, allowing us correctly to specify the fixed effects model. Higher GDP per capita, i.e., greater economic wealth, is expected to lead to a stronger and better educated middle class, which demands more access to the decision-making process and, thus, to more civil liberties (Lipset 1959; Kaempfer and Lowenberg 1988; Barro 1999). As an additional control variable, we enter the share of international trade in GDP, since enhancing economic ties with the rest of the world is expected to lead to a pro-democracy movement in less democratic countries (Levitsky and Way 2010). Similarly, our vector of controls also includes the log of the total population, as, in the absence of market institutions, population increases may lead to resource scarcities and a tendency for governments to implement more repressive policies (Henderson

1998). According to Poe et al. (1999), governments facing anti-government armed forces adopt restrictions on basic rights to maintain control over society. Therefore, we also include two dummy variables for the occurrence of an external or an internal civil war. To account for the effect of political stability, we control for the total number of years since the last political transition. Finally, we introduce an index of lagged democracy that does not capture civil liberties to account for factors related to the type of the political regime (taken from Cheibub et al. 2010, as extended by Bjørnskov and Rode 2017).¹³

Although the fixed effects model described above can be very helpful in uncovering a relationship between civil liberties and sanctions, it fails to consider the fact that the sample includes only those cases in which sanctions were imposed. Nevertheless, in many instances, sanctions may have been contemplated but not used for strategic reasons (see also Nooruddin 2002). Hence, the OLS fixed effects estimates could be biased. To make matters even worse, the pre-sanction dynamics may, in fact, be responsible for any observed negative effect on civil liberties, as sanctions for human rights violations are imposed on countries where civil liberties already are deteriorating.

To deal with both issues, we use the semiparametric method of Acemoglu et al. (2018) to model the counterfactual scenario, i.e., cases when sanctions are not imposed to change the path of civil liberties. Following that analysis, any deviation from the counterfactual is attributed to the treatment, i.e., to *Sanctions*. The method is ideal in our setting for several reasons. First, it does not rely on the choice of characteristics for matching, as matching is performed using (3-year) lagged values of the dependent variable and time effects. Second, the method examines the effect of sanctions on the evolution of civil liberties from that period onward. Therefore, it treats sanction episodes with different durations equally, based only on the effect at $t=0$, i.e., the imposition year. Third, it estimates the effect over time, allowing us to uncover the changes on the outcome variable, i.e., the measure of civil liberties, after a “random” treatment, i.e., sanction imposition. Therefore, we can estimate and plot the average treatment effect on the treated (ATET) of sanctions on civil liberties for $t=-4, -3, \dots, 0, 1, 2, \dots, 12$. Furthermore, we are able to use three different estimators to model the counterfactual scenario.

The first estimator follows Kline (2011). It begins by splitting the observations/country-year pairs into two groups: one with sanctions imposed and one with no sanctions imposed. Assuming that the path of civil liberties in both groups can be modeled by their lagged values and time effects, the effect of sanctions can be computed by the difference between the OLS-predicted dependent variable for the two groups. Exploiting the panel structure of our dataset, we model the change in civil liberties at each year $t=-4, -3, \dots, 0, 1, \dots, 12$, from the civil liberties at $t=0$ by performing this regression adjustment.

The second approach builds on the semiparametric inverse probability weighting method of Angrist et al. (2018). According to that method, once we control for time and country effects, lagged sanctions and the dynamics of civil liberties, changes in *CL* are random.¹⁴ Then, any differences in civil liberties between observations/country-year pairs that have received sanctions or not can be attributed to the effects of sanctions. Using a probit model, we first estimate the propensity to receive sanctions at t , conditional on not having sanctions at $t-1$, using lags of *CL* and time (year) effects as control variables.¹⁵ Then,

¹³ Variable definitions, sources and descriptive statistics are given in the “Appendix” (Table 8).

¹⁴ See also Angrist and Kuersteiner (2011) for the technical details.

¹⁵ As country fixed effects are cancelled out when we take changes in the *CL* variable, no reason exists to include them at the first stage.

the effect of *Sanctions* on the change in civil liberties is the weighted average of changes in *CL*, with weights given by the inverse of the propensity score if the country receives sanctions at time t , and minus the inverse of one minus the propensity score if the country does not receive sanctions. In this manner, the effect of treatment is a weighted average of changes across observations. However, country-year pairs that, according to their pre-sanction dynamics, are expected to receive sanctions obtain a lower weighting, whereas those not experiencing adverse civil liberties dynamics prior to $t=0$ receive a higher weighting. In a manner similar to the regression adjustment, the inverse probability weighting method is performed on the change in civil liberties for each year from its value at $t=0$.¹⁶

Each method described above relies on the assumptions of the underlying statistical model. The regression adjustment assumes that the correct model for the dependent variable is a simple OLS model. In contrast, the inverse probability weighting method assumes that the propensity to receive sanctions is modeled properly by a probit model. The difference between the two methods is evident. The regression adjustment models outcomes, i.e., changes in civil liberties, whereas the inverse probability weighting models the probability of receiving sanctions. A final approach combines the regression adjustment and inverse probability weighting to obtain doubly robust estimates. In simple terms, the doubly robust estimator implements a regression adjustment on the weighted changes in civil liberties for each time t from $t=0$.¹⁷ The major advantage of the last method is that it requires only one model, OLS or probit, to be correctly specified to derive correct estimates for the effect of treatment (see Wooldridge 2010). Given that property and the fact that the baseline estimation yields similar results for all three methods, we use the doubly robust estimates in all our robustness analyses.¹⁸

The following section presents all our empirical results.

¹⁶ We can test whether significant pre-treatment dynamics exist by estimating the effect of treatment for years prior to the treatment. The figures and tables in the following section show that the estimator matches the pre-treatment dynamics, as in most cases the effect for $t=-4, \dots, -1$ turns out insignificant, providing evidence that the estimator removes any (pre-treatment) dynamics in the dependent variable that may be correlated with the treatment.

¹⁷ The weights used are calculated according to the inverse probability weighting scheme.

¹⁸ The three models discussed above rely on three basic assumptions to estimate the ATET: (i) conditional Independence, i.e., after conditioning on the covariates, the outcomes (civil liberties) are conditionally independent of the control-level potential outcome, (ii) overlap, i.e., each treated observation has a positive probability of being allocated to each treatment level and (iii) i.i.d., which in our setting rules out interactions among countries in each period. If we were to estimate the ATEs (Average Treatment Effects), those assumptions would have taken a more restrictive form, requiring both conditional independence and overlap to hold for both treatment statuses (Wooldridge 2010). For more details on the assumptions, see Imbens and Wooldridge (2009) and Angrist and Pischke (2009). To inspect visually whether the overlap assumption holds in Fig. 3 in the “Appendix”, we present the smoothed densities, using a standard Epanechnikov kernel, of the estimated propensities of receiving sanctions between the two groups. As the reader can verify, considerable overlap is found among treated and control propensities. What is more important, the control observations cover the support for all treated observations, when either the Freedom House or the empowerment rights index is used. That evidence provides support for the required overlap assumption and gives suggestive evidence in favor of our empirical strategy.

4 Results

4.1 Fixed effects results

We first estimate the simple panel fixed effects regression with country and time fixed effects. In Table 1, the dependent variable is the Freedom House index of civil liberties, and in Table 2, the empowerment rights index.

In both tables, we experiment with different definitions of the *Sanctions* variable, depending on the issues for which sanctions are imposed. In the first column, we use the more general definition of sanctions. In column (2), we use sanctions imposed only for human rights violations or for destabilizing the regime. In the third column, we exclude from the cases of column (1) sanctions that are imposed for human rights violations or destabilizing the regime. In column (4), we perform a placebo-type analysis by considering sanctions imposed for the improvement of environmental policies, trade issues or the implementation of economic reforms, i.e., issues not directly related to civil liberties. Finally, in columns (5) to (7), we examine the robustness of our results by excluding countries with the lowest scores on the respective civil liberties index (column 5), Latin American countries (column 6, which have received a disproportionately large share of US sanctions), and ex-communist countries (column 7).

The overall picture that emerges in both Tables 1 and 2 is of a decline in the index of civil liberties by up to 0.08 and 0.14 for each of the two measures, respectively, only in columns (1), (2) and (5) to (7). The effects are quantitatively significant; according to our data, they are approximately equal to a half standard deviation reduction in the level of civil liberties and the empowerment rights index, respectively. Moreover, we note that the corresponding figures in columns (1), (2) and (5) to (7) fall into overlapping confidence intervals; therefore, we cannot reject the hypothesis that the estimated coefficients are the same across all four models. It should also be noted that the effects are significant at the 1% level. In contrast, as expected, in columns (3) and (4), *Sanctions* lose their statistical significance. Hence, it appears that the adverse effect on civil liberties is present only for sanctions imposed for issues related to civil liberties.

Regarding the rest of the control variables, only the lagged democracy dummy, the duration of the regime and the incidence of civil war have a statistically significant sign in both tables. In all cases, the signs are consistent with the ones discussed in the previous section. On the other hand, GDP per capita is positive and statistically significant at the 5% level only in Table 1, whereas interstate war is positive, in contrast to our priors, and significant in Table 2. The rest of the control variables are insignificant.

4.2 Semiparametric estimates

As we argued in Sect. 3, a potential major shortcoming of the fixed effects model is the fact that countries selected to receive sanctions may differ in their potential civil liberties indexes than those not receiving sanctions. The adopted potential outcomes

Table 1 Fixed effects model, dependent variable: Freedom House civil liberties

	(1) Sanctions	(2) Sanctions HR and destabilization	(3) Excl. HR and destabilization	(4) Sanctions not related to the regime	(5) CL > 0	(6) No Latin America	(7) No former communist
Sanctions	−0.081*** (−3.429)				−0.078*** (−2.870)	−0.087*** (−3.048)	−0.075*** (−2.707)
Democracy lagged	0.189*** (7.383)	0.192*** (7.584)	0.197*** (7.772)	0.199*** (7.895)	0.167*** (6.379)	0.174*** (6.379)	0.168*** (7.093)
GDP per capita	0.061*** (2.300)	0.062*** (2.315)	0.060*** (2.267)	0.061*** (2.295)	0.058*** (2.099)	0.050* (1.911)	0.056*** (1.999)
Trade openness	0.000 (1.195)	0.000 (1.364)	0.000 (1.304)	0.000 (1.317)	0.000 (0.632)	0.000 (1.214)	0.000 (0.696)
Regime duration	−0.003*** (−3.449)	−0.003*** (−3.227)	−0.003*** (−3.261)	−0.003*** (−3.145)	−0.002*** (−2.195)	−0.003*** (−3.353)	−0.002*** (−2.942)
Interstate war	0.013 (0.721)	0.010 (0.545)	0.015 (0.869)	0.006 (−0.338)	0.007 (−0.428)	0.009 (0.520)	0.004 (0.204)
Civil war	−0.075*** (−4.510)	−0.078*** (−4.869)	−0.078*** (−4.737)	−0.080*** (−4.954)	−0.059*** (−4.236)	−0.069*** (−3.619)	−0.071*** (−4.843)
Population	−0.065 (−1.230)	−0.076 (−1.377)	−0.069 (−1.276)	−0.077 (−1.395)	−0.032 (−0.672)	−0.048 (−0.945)	−0.051 (−1.057)
Sanctions (human rights and destabilization)		−0.097*** (−3.629)					
Sanctions (excluding human rights and destabilization of the regime)			−0.053* (−1.794)				
Sanctions (not related to the regime)				0.006 (0.420)			
Obs	5370	5370	5370	5370	5048	4612	5141
R-sq	0.364	0.359	0.355	0.351	0.312	0.353	0.312
F-test	11.282	10.284	10.425	9.840	10.367	10.268	10.887
F-time	4.974	4.971		4.391	4.835	4.544	5.202

Robust t-statistics in parenthesis. F-test denotes the significance of the model and F-time denotes F test for the significance of the time fixed effects

* $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$

Table 2 Fixed effects model, dependent variable: empowerment rights index

	(1) Sanctions	(2) Sanctions HR and destabilization	(3) Excl. HR and destabilization	(4) Sanctions not related to the regime	(5) CL > 0	(6) No Latin America	(7) No former communist
Sanctions	-0.141*** (-5.054)				-0.118*** (-4.298)	-0.137*** (-4.341)	-0.136*** (-4.136)
Democracy lagged	0.135** (5.467)	-0.142*** (-5.319)	0.147*** (5.718)	0.150*** (5.579)	0.125*** (5.067)	0.113*** (3.886)	0.116*** (4.690)
GDP per capita	0.038 (0.931)	0.040 (0.963)	0.039 (0.912)	0.042 (0.979)	-0.019 (-0.459)	0.044 (1.083)	0.034 (0.793)
Trade openness	0.000 (-0.377)	-0.000 (-0.281)	-0.000 (-0.305)	0.000 (-0.307)	0.000 (0.804)	-0.000 (-0.695)	-0.000 (-0.460)
Regime duration	-0.003*** (-3.210)	-0.003*** (-3.082)	-0.003*** (-2.956)	-0.003*** (-2.984)	-0.003*** (-2.716)	-0.003*** (-3.211)	-0.002*** (-2.493)
Interstate war	0.058** (2.239)	0.056** (2.133)	0.060** (2.372)	0.048* (1.911)	0.035 (1.525)	0.057** (2.103)	0.057** (2.212)
Civil war	-0.046** (-1.987)	-0.060** (-2.441)	-0.050** (-2.089)	-0.058** (-2.324)	-0.044* (-1.913)	-0.032 (-1.327)	-0.040* (-1.734)
Population	-0.004 (-0.065)	-0.037 (-0.485)	-0.012 (0.162)	-0.033 (-0.430)	-0.083 (-1.313)	0.037 (0.514)	0.055 (0.838)
Sanctions (human rights and destabilization)		-0.154*** (-4.183)					
Sanctions (excluding human rights and destabilization of the regime)			-0.092*** (-2.622)				
Sanctions (not related to the regime)				0.020 (1.258)			
Obs	4216	4216	4216	4216	3947	3647	4024
R-sq	0.175	0.162	0.158	0.149	0.222	0.162	0.126
F-test	10.508	10.427	10.221	9.967	11.011	7.981	9.871
F-time	7.361	7.226	7.320	7.195	8.063	6.239	7.834

Robust t-statistics in parenthesis. F-test denotes the significance of the model and F-time denotes F test for the significance of the time fixed effects

* $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$

Table 3 Potential outcomes model

	(1) – 4 to 0 years	(2) 1 to 3 years	(3) 4 to 6 years	(4) 7 to 9 years
<i>Regression adjustment</i>				
ATET on	– 0.008	– 0.079 [#]	– 0.052*	– 0.024
Civil liberties	(– 1.91)	(– 4.39)	(– 2.26)	(– 0.92)
ATET on empowerment	– 0.015	– 0.075 [#]	– 0.098 [#]	– 0.063
Rights index	(1.47)	(3.75)	(3.62)	(1.85)
<i>Inverse probability weighting</i>				
ATET on	– 0.018	– 0.045 [#]	– 0.076 [#]	– 0.028
Civil liberties	(– 1.97)	(– 3.75)	(– 2.81)	(– 0.93)
ATET on empowerment	0.023	– 0.049	– 0.107 [#]	– 0.099*
Rights index	(1.36)	(2.06)	(4.28)	(2.82)
<i>Doubly robust</i>				
ATET on	– 0.013	– 0.081*	– 0.054*	– 0.024
Civil liberties	(– 1.87)	(– 3.86)	(– 2.16)	(– 0.92)
ATET on empowerment	– 0.019	– 0.079 [#]	– 0.111 [#]	– 0.076*
Rights index	(1.93)	(3.43)	(4.26)	(2.17)

Coefficients show the average treatment effects on the treated (ATET). We present the ATET, using the regression adjustment, inverse probability weighting and the doubly robust estimation procedure, which combines the two previous methods, to compute the counterfactual. The number of observations is the same as in column (1) in Tables 1 and 2. T-statistics obtained using 100 bootstraps are presented in the parentheses. All results are for 58 treatments in the Freedom House index and 50 treatments for the empowerment rights index

* $p < 0.05$; [#] $p < 0.01$

framework deals with that problem. In Table 3, we present the baseline results for the treatment variable *Sanctions*, which corresponds to the dependent variable used in columns (1) and (5)–(7) in the previous tables. In each column, we present the ATET for $t = -4$ up to $t = 0$ and, for the next 10 years, in 3-year intervals. The different panels in the table provide the estimates of the regression adjustment, the inverse probability weighting, and the doubly robust methods for both the civil liberties and the empowerment rights indexes.

Table 3 reveals that *Sanctions* cause a statistically significant decline in civil liberties. The effect seems to be quantitatively the same as the one estimated in the fixed effects model. To obtain a clearer idea about the effect of sanctions over time, we plot the impulse response functions in Figs. 1 and 2 for each of the two dependent variables, using the results of the doubly robust scheme.¹⁹

The solid line in both figures plots the ATET over time, whereas the dotted lines plot the 95% confidence intervals. The figures shed light on the results of Table 3: the identified negative effects are explained by the large negative initial response (1–4 years) after

¹⁹ We conceive the doubly robust method as being more general, since it combines the two other methods and we rely solely on it for the robustness analysis. The associated impulse response functions for the regression adjustment and the inverse probability weighting methods are shown in the “Appendix”. The remaining results are available from the authors and are qualitatively similar to the ones presented here.

the imposition of sanctions. That negative effect prevails over the subsequent years, as any changes thereafter are statistically insignificant. What Table 3 and the two figures indicate is a roughly 0.1 decline in the index of civil liberties within the first 5 years after the imposition of sanctions.²⁰

In the next tables, we examine the robustness of the above results. In Tables 4 and 5, we employ the same robustness tests as in Sect. 4.1. First, we experiment with the definition of sanctions, and we then examine the effect of excluding countries with very low civil liberties, i.e., Latin American countries and former communist countries. Table 4 presents the results when the outcome variable is the civil liberties index, and Table 5 presents the results when the outcome variable is the empowerment rights index.

The results can be summarized as follows: the negative effect of sanctions on civil liberties is present when we consider sanctions imposed for issues related to human rights violations and the destabilization of the targeted regime. When those two issues are not included, the negative effect identified is smaller. Moreover, when we consider sanctions for improving environmental policy, trade issues and economic reforms, no statistically significant effect is evident (third line in Tables 4 and 5). Finally, our main result holds even when we exclude very undemocratic countries as well as countries that have been targets of a large share of US sanctions (i.e., Latin American and former communist countries).

In Table 6, we perform a series of additional robustness tests. First, we examine the effect of US sanctions when an international institution (the United Nations) also participated as a sanction sender. We then examine the effect of multilateral and unilateral sanctions on the outcome variables. In all three cases, the main results of the previous tables remain unchanged for both measures of *CL*. However, when comparing the effects across the three cases, it appears that the largest (negative) effect is present in the case of multilateral sanctions, when an international institution also participated as a sender. Therefore, we may conclude that multilateral sanctions, especially when international institutions also participate, appear to have a more adverse effect on civil liberties than unilateral sanctions.²¹ The difference can be attributed to the fact that more repressive regimes are more likely to be targets of multilateral rather than unilateral sanctions. Those regimes are more prone to use repression as a means of securing their positions.

As a further test, we examine whether a differential effect exists between high-cost and low-cost (for the targeted country) sanctions. Following the definitions of the TIES database, sanctions were coded as low cost when no evidence exists that the health of the targeted economy was impacted, corresponding to a GNP loss of less than 1% and an increase in inflation and/or unemployment rate of less than 5%. The results confirm our general findings: the negative effect on civil liberties exists both in high- and low-cost sanctions. However, the estimated effect for high-cost sanctions is twice as large as that found for low-cost sanctions. That result is consistent with our theoretical considerations: the rally around the flag effect must be more pronounced when significant damage is imposed on the targeted economy. Therefore, a dictator can use repression more effectively as a means

²⁰ We should note that in all cases the pre-sanction dynamics are found to be either statistically or quantitatively insignificant, i.e., the coefficients for the years $t = -4, -3, -2, -1$ are statistically insignificant or have very small values. These findings imply that countries are made comparable in terms of pre-treatment dynamics. This result remains valid in our robustness tests as well.

²¹ The finding also verifies the analysis of Kaempfer and Lowenberg (1999), who show that unilateral sanctions are more effective in reducing repression than multilateral sanctions.

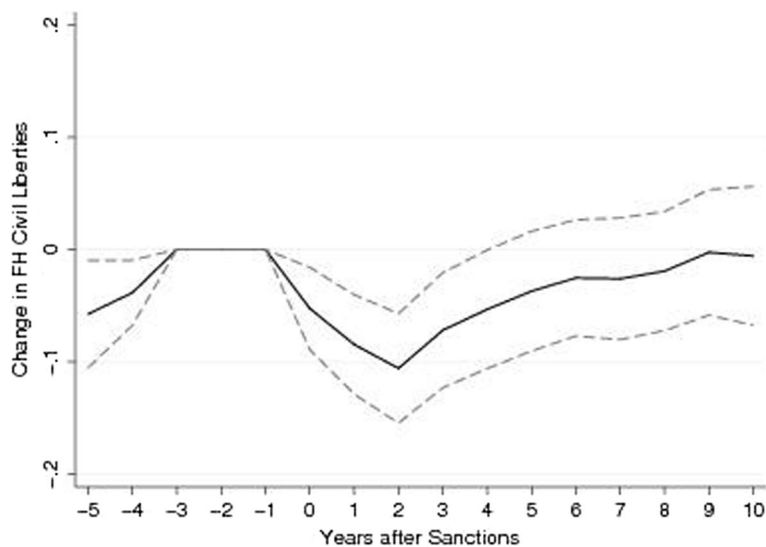


Fig. 1 ATET of sanctions at $t=0$ on Freedom House civil liberties

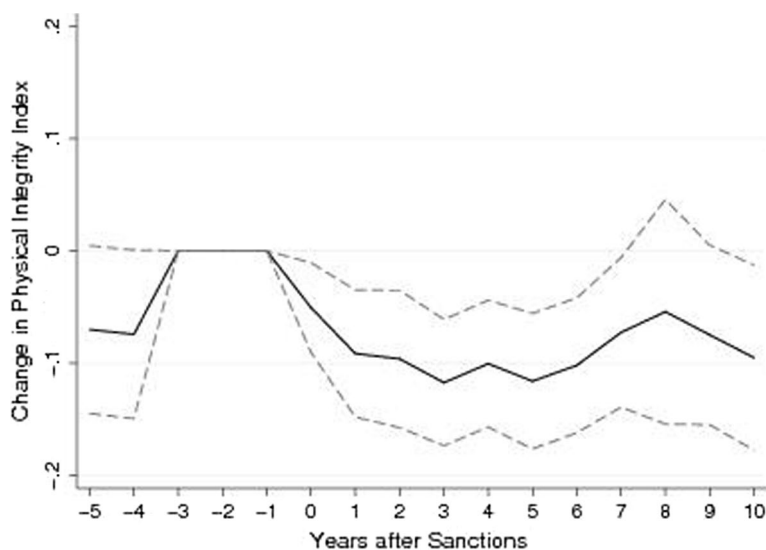


Fig. 2 ATET of sanctions at $t=0$ on empowerment rights index

Table 4 Doubly robust estimates, Freedom House civil liberties, robustness

	(1)	(2)	(3)	(4)
	– 4 to 0 years	1 to 3 years	4 to 6 years	7 to 9 years
<i>ATET on Freedom House civil liberties</i>				
Sanctions human rights and destabilization [34]	– 0.013 (– 1.61)	– 0.016 [#] (– 3.74)	– 0.080* (– 2.06)	– 0.010 (– 0.24)
Excluding human rights and destabilization [39]	– 0.008 (– 1.50)	– 0.045* (– 0.237)	– 0.021 (– 0.68)	– 0.041 (– 1.21)
Sanctions not related to the regime [68]	– 0.001 (– 0.25)	– 0.004 (– 0.36)	0.005 (– 0.041)	– 0.001 (– 0.07)
Civil liberties > 0 [47]	– 0.012 [#] (– 2.88)	– 0.076 [#] (– 3.30)	– 0.045 (– 1.55)	– 0.001 (– 0.03)
No Latin America [58]	– 0.008 (– 1.71)	– 0.074 [#] (– 3.08)	– 0.049* (– 2.13)	– 0.040 (– 1.73)
No former communist countries [49]	– 0.010 (– 1.94)	– 0.082 [#] (– 3.57)	– 0.047 (– 1.81)	– 0.010 (– 0.36)

Coefficients show the average treatment effect on the treated (ATET), using the doubly robust estimation procedure, which combines the regression adjustment and the inverse probability weighting method to compute the counterfactual. The number of observations is the same as in columns (2) to (7) in Tables 1 and 2. T-statistics obtained using 100 bootstraps are presented in the parentheses. Number of treatments are provided in square brackets

* $p < 0.05$; [#] $p < 0.01$

Table 5 Doubly robust estimates, empowerment rights Index, robustness

	(1)	(2)	(3)	(4)
	– 4 to 0 years	1 to 3 years	4 to 6 years	7 to 9 years
<i>ATET on Freedom House civil liberties</i>				
Sanctions human rights and destabilization [20]	– 0.025 (– 1.90)	– 0.129 [#] (– 2.74)	– 0.116* (– 2.46)	– 0.028 (– 0.46)
Excluding human rights and destabilization [30]	– 0.014 (– 0.82)	– 0.041 (– 1.09)	– 0.107* (– 1.75)	– 0.118* (– 2.15)
Sanctions not related to the regime [64]	– 0.003 (– 1.05)	0.022 (0.88)	0.018 (0.51)	0.004 (0.07)
Civil liberties > 0 [28]	– 0.019 (– 4.46)	– 0.092 [#] (– 3.68)	– 0.092* (– 2.85)	– 0.044 (– 1.15)
No Latin America [33]	– 0.010 (– 1.03)	– 0.077 [#] (– 3.34)	– 0.112 [#] (– 3.87)	– 0.082* (– 2.16)
No former communist countries [33]	– 0.030 (– 1.51)	– 0.077 [#] (– 3.23)	– 0.109 [#] (– 3.89)	– 0.064 (– 1.57)

See notes in Table 4

Table 6 Doubly robust estimates, robustness II

	(1) – 4 to 0 years	(2) 1 to 3 years	(3) 4 to 6 years	(4) 7 to 9 years
<i>ATET on Freedom House civil liberties</i>				
Institutions participating [67]	– 0.005 (– 0.53)	– 0.177 [#] (– 3.77)	– 0.122 [#] (– 3.12)	– 0.091* (– 2.52)
Multilateral sanctions [15]	– 0.007 (– 0.77)	– 0.130 [#] (– 3.02)	– 0.100 [#] (– 3.22)	– 0.089 [#] (– 2.87)
Unilateral sanctions [43]	– 0.011* (– 2.00)	– 0.062 [#] (– 2.69)	– 0.036 (– 1.01)	0.003 (0.07)
High cost [15]	– 0.010 (1.02)	– 0.166 [#] (– 3.13)	– 0.170 [#] (– 3.86)	– 0.134 [#] (– 2.73)
Low cost [43]	– 0.015 [#] (– 3.71)	– 0.058 [#] (– 2.63)	– 0.021 (0.65)	0.008 (0.24)
Removal [59]	0.009 (0.83)	0.028 (1.47)	– 0.001 (– 0.04)	– 0.028 (– 1.05)
<i>ATET on empowerment rights index</i>				
Institutions participating [8]	– 0.038 (– 1.89)	– 0.105 [#] (– 3.88)	– 0.068 (– 1.23)	– 0.010 (– 0.16)
Multilateral sanctions [12]	– 0.032 (– 1.61)	– 0.087 [#] (– 2.80)	– 0.100* (– 2.17)	– 0.058 (0.90)
Unilateral sanctions [25]	– 0.010 (0.89)	– 0.072 [#] (– 3.02)	– 0.119 [#] (– 3.42)	– 0.093* (– 2.16)
High cost [10]	– 0.031 (– 1.01)	– 0.169 [#] (– 5.82)	– 0.239 [#] (– 3.03)	– 0.126 (– 1.43)
Low cost [27]	– 0.015 (– 1.36)	– 0.051* (– 2.04)	– 0.072 [#] (– 3.02)	– 0.061 (– 1.65)
Removal [46]	– 0.001 (0.09)	0.023 (0.76)	0.050 (1.37)	0.046 (1.06)

See notes in Table 4

of securing his position. In a similar vein, costly sanctions that impoverish the population reduce the price of repression, resulting in the worsening of civil liberties.

Using the above methodology, we also are able to examine the effects of sanction removal on civil liberties. The final line in each panel of Table 6 examines that effect. According to the results, sanction removal has no effect on the level of civil liberties, a finding consistent with the rally around the flag effect of sanctions, as the theory does not predict a symmetric effect when sanctions are removed.

A final set of robustness tests is reported in Table 7. First, we examine whether our results hold when we enter all sanction impositions irrespective of the sender state. Although we acknowledge that the strategy may not be appropriate, as we do not control for the institutional environment of the sender, it may provide some indication of whether our previous

Table 7 Doubly robust estimates, robustness III

	(1) – 4 to 0 Years	(2) 1 to 3 years	(3) 4 to 6 years	(4) 7 to 9 years
<i>ATET on Freedom House civil liberties</i>				
All sanctions [67]	– 0.009 (– 1.81)	– 0.072 [#] (– 3.79)	– 0.068 [#] (– 2.95)	– 0.035 (– 1.29)
UN sanctions [11]	– 0.012 (– 1.09)	– 0.106 [#] (– 2.87)	– 0.101* (– 2.10)	– 0.121* (– 2.37)
One year [20]	– 0.019 (– 1.26)	– 0.088 [#] (– 2.83)	– 0.041 (– 0.95)	– 0.056 (– 1.33)
Multi-year [38]	– 0.019* (– 2.31)	– 0.079 [#] (– 3.16)	– 0.064* (– 2.13)	– 0.014 (– 0.46)
No Europe [58]	– 0.020* (– 2.48)	– 0.088 [#] (– 3.82)	– 0.060* (– 2.31)	– 0.022 (– 0.79)
<i>ATET on empowerment rights index</i>				
All sanctions [55]	– 0.026* (– 2.61)	– 0.075 [#] (– 3.94)	– 0.092 [#] (– 3.06)	– 0.033 (– 0.92)
UN sanctions [11]	– 0.035* (– 2.05)	– 0.083 [#] (– 2.96)	– 0.148* (– 2.14)	– 0.146 (– 1.69)
One year [21]	– 0.036 (– 1.24)	– 0.074* (– 2.04)	– 0.101* (– 2.11)	– 0.061 (– 1.08)
Multi-year [29]	– 0.026 (– 1.73)	– 0.083 [#] (– 2.96)	– 0.121 [#] (– 3.04)	– 0.094 (– 1.77)
No Europe [50]	– 0.030 (– 1.76)	– 0.087 [#] (– 4.14)	– 0.117 [#] (– 4.19)	– 0.082* (– 2.15)

See notes in Table 4

results hold only for US-imposed sanctions. The estimated treatment effects are qualitatively similar to those obtained when only US-imposed sanctions are considered.²² The same results also are obtained when we examine only UN sanctions. Once again, we should note that for all sanctions included in the last category, the United States also is one of the senders.

In Table 7, we ask whether US sanctions have the same effect if imposed for 1 year or for many years. Our results reveal that sanctions applied for a short time have adverse effects only for the first 3 years following imposition. On the other hand, multiyear sanctions have effects that last for up to 6 years after initial imposition.

A final robustness test in Table 7 is undertaken by excluding European countries from the sample. As most of those nations are developed countries where human rights are strongly protected and, thus, should not be expected to be targeted by US sanctions for safeguarding human rights and destabilizing their political regime, we want to ensure that our results are not driven by their inclusion in our initial sample. Our main results remain valid. In fact, the exclusion of the European countries from the sample increases slightly the point estimates and the precision of all estimated effects.

²² We should note that in 58 out of 67 sanction episodes in our sample, the United States also participated as a primary or secondary sender.

5 Conclusions

In this paper, we have evaluated the effects of sanctions on civil liberties. Overall, our findings suggest that US-imposed sanctions result in compromise civil liberties in targeted countries. That result is robust across various specifications, both in the standard fixed effects panel data model and in a potential outcomes framework.

From a policy perspective, our findings have important implications. First, they indicate that sanctions imposed on nondemocratic regimes do not improve citizens' well-being. On average, they appear to worsen things. Our analysis indicates further that the estimated negative effects appear to be nonnegligible and robust across specifications.

From a theoretical perspective, this result is not surprising: sanctions tend to strengthen a dictator's position by lowering the prices of repression and loyalty, thereby creating a "rally around the flag" effect. Then, in accordance with our findings, as long as sanctions, at least to a certain extent, aim at reducing repression in the target state, other forms of persuasion must be applied.

The question then arises as to why countries, and especially the United States, impose sanctions ostensibly to improve civil liberties. After all, as our results are widely confirmed by the previous literature and have solid theoretical underpinnings, it is difficult to argue that a rational government will make the same mistake (more than) twice. One potential explanation might be the fact that sanctions are imposed for reasons other than those stated officially. Accordingly, it might be the case that regime destabilization and civil liberties' protections are excuses used by the United States to promote its general interests. Alternatively, following a vast array of literature (see, for example, Weeks 2014) showing that repressive regimes tend to provoke international conflict, it could be argued that sanctions are the rational response of sender countries instigated by the targeted country's regime. In the latter case, and given our results, sanctions are provoked by the "dictator" to increase domestic support and, ultimately, strengthen his position. Although such explanations are pure speculation, they may provide some interesting questions for future research.

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Appendix

See Figs. 3, 4, 5, 6 and 7 and Table 8.

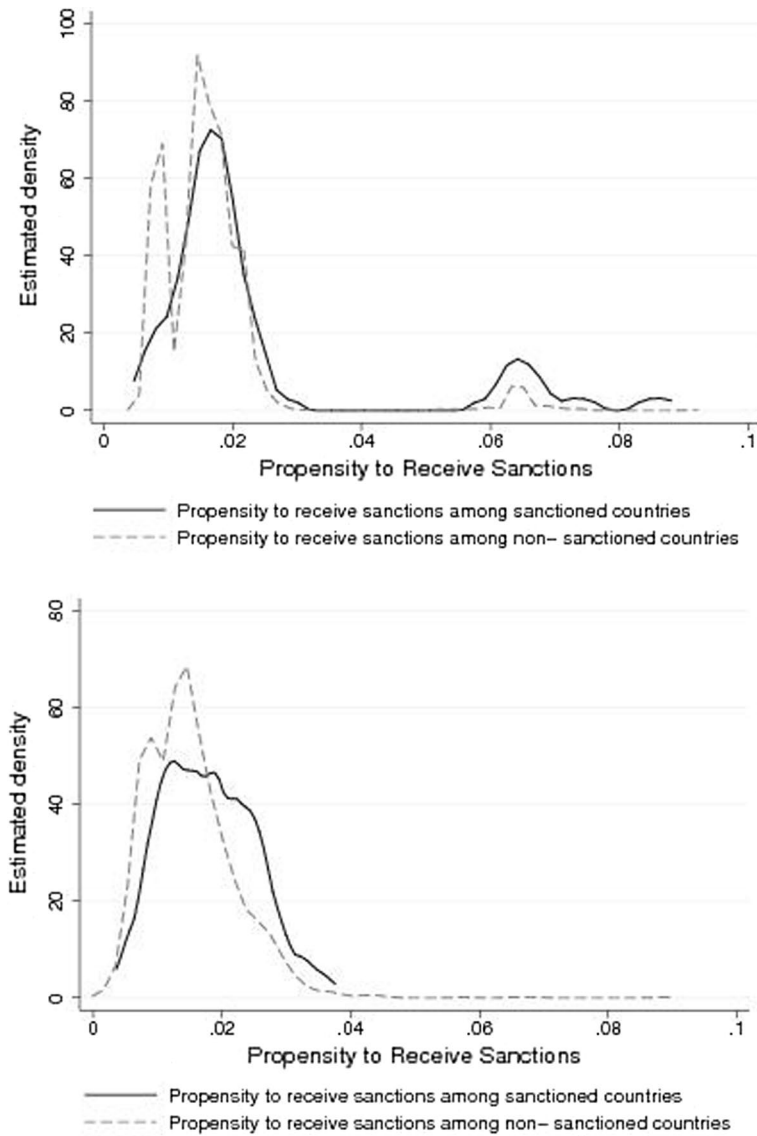


Fig. 3 Smoothed density for the propensity to receive sanctions. *Notes* The figures plot the smoothed density of the estimated propensities to receive sanctions. The top figure uses the Freedom House civil liberties index and the bottom figure the empowerment rights index. The black line plots the density for countries that received sanctions, while the gray line plots the density for the control countries, which have not received sanctions. The densities are smoothed using a standard Epanechnikov kernel

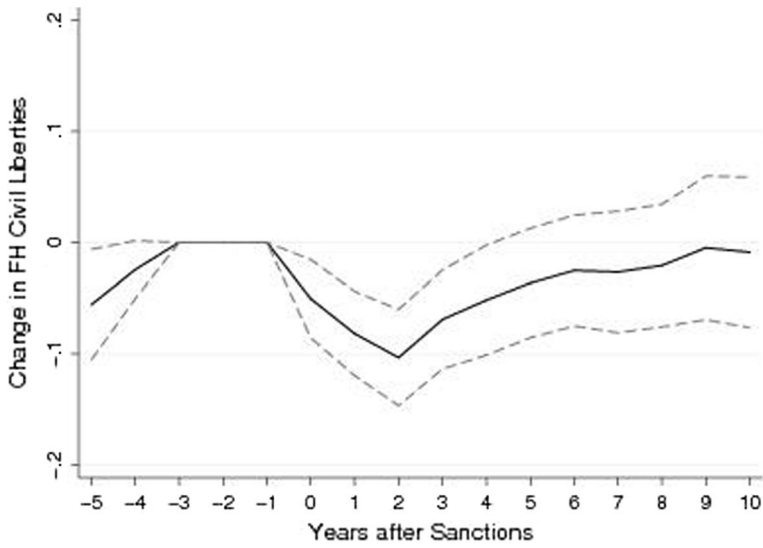


Fig. 4 ATET of sanctions on Freedom House civil liberties index (regression adjustment)

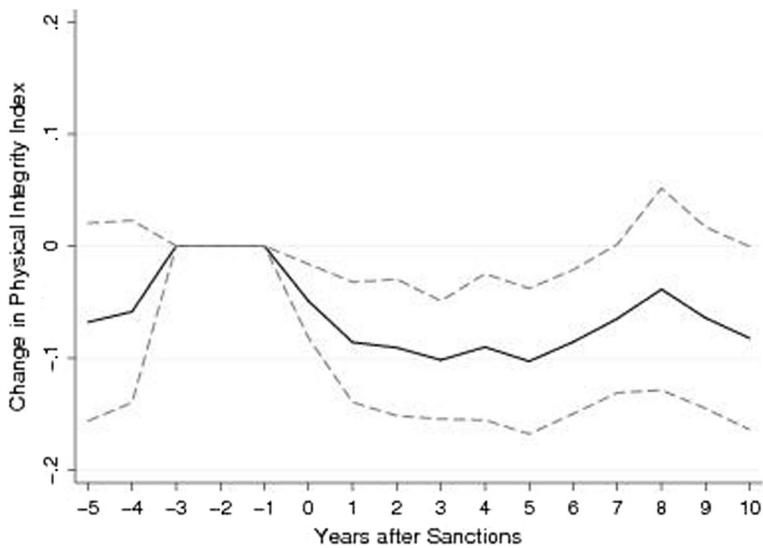


Fig. 5 ATET of sanctions on empowerment rights index (regression adjustment)

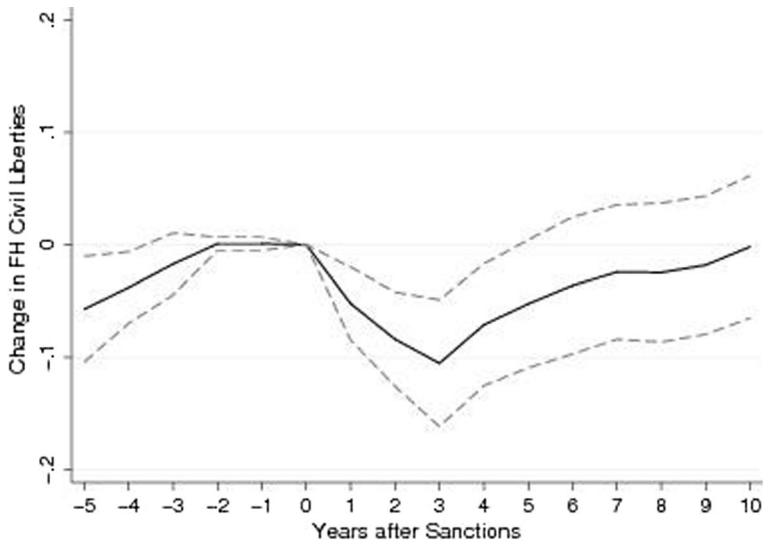


Fig. 6 ATET of sanctions on Freedom House civil liberties index (inverse probability weighting)

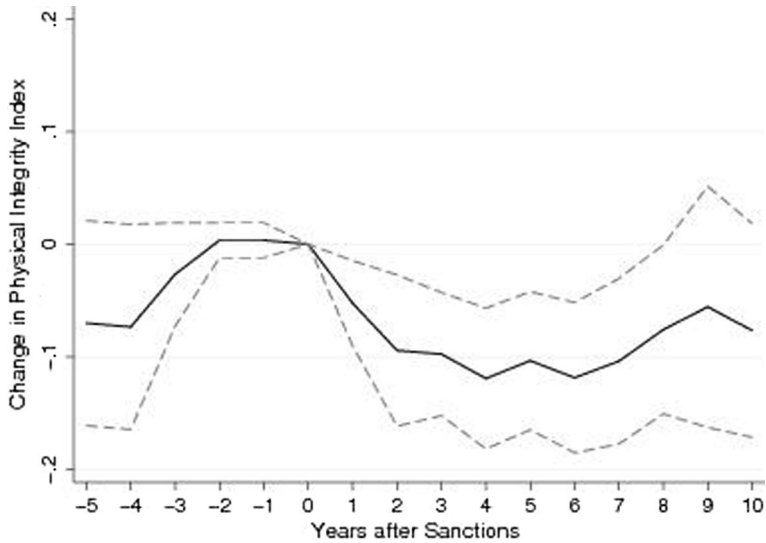


Fig. 7 ATET of sanctions on empowerment rights index (inverse probability weighting)

Table 8 List of Variables and descriptive statistics

Variable	Mean	SD	Min	Max	Source	Definition
<i>Dependent variables</i>						
Civil liberties index	0.54	0.30	0	1	Freedom House	Freedom House index of civil liberties, converted to [0,1] scale
Empowerment rights index	0.57	0.31	0	1	Cingranelli and Richards (2010)	Synthetic indicator of rights to foreign and domestic movement, freedom of speech, freedom of assembly and association, worker's rights, electoral self-determination and freedom of religion
<i>Controls</i>						
Civil war	0.19	0.39	0	1	COW project	Dummy variable that takes the value of 1 in the presence of intrastate armed conflict
Sanctions	0.008	0.089	0	1	Morgan et al. (2014)	Dummy variable, equal to 1 when sanctions are imposed
Democracy, lagged	0.45	0.49	0	1	Bjørnskov and Rode (2017)	Dichotomous indicator of democracy. Equal to 1, if free and fair elections were conducted, and if there was a peaceful turnover of legislative and executive offices following those elections
GDP per capita	8.13	1.60	4.75	11.61	World Development Indicators	Log of real (2005 constant USD) GDP per capita
Interstate war	0.009	0.093	0	1	COW project	Dummy variable that takes the value of 1 in the presence of an external war.
Trade openness	81.19	51.05	2.53	89.91	World Development Indicators	Imports plus exports, divided by GDP
Population	15.90	1.54	12.51	20.96	World Development Indicators	Log of total population.
Regime duration	23.54	27.37	0	164	Polity IV project	Number of years after the last regime change

Country list

Afghanistan, Albania, Algeria, Angola, Argentina, Armenia, Australia, Austria, Azerbaijan, Bahrain, Bangladesh, Belarus, Belgium, Benin, Bhutan, Bolivia, Bosnia, Botswana, Brazil, Bulgaria, Burkina Faso, Burundi, Cambodia, Cameroon, Canada, Cape Verde, Central African Republic, Chad, Chile, China, Colombia, Comoros, Congo Brazzaville, Congo Kinshasa, Costa Rica, Croatia, Cuba, Cyprus, Czech Republic, Czechoslovakia, Denmark, Djibouti, Dominican Republic, East Timor, Ecuador, Egypt, El Salvador, Equatorial Guinea, Eritrea, Estonia, Ethiopia, Fiji, Finland, France, Gabon, Gambia, Georgia, Germany, Ghana, Greece, Guatemala, Guinea, Guinea-Bissau, Guyana, Haiti, Honduras, Hungary, India, Indonesia, Iran, Iraq, Ireland, Israel, Italy, Ivory Coast, Jamaica, Japan, Jordan, Kazakhstan, Kenya, Korea North, Korea South, Kosovo, Kuwait, Kyrgyzstan, Laos, Latvia, Lebanon, Lesotho, Liberia, Libya, Lithuania, Luxembourg, Macedonia, Madagascar, Malawi, Malaysia, Mali, Mauritania, Mauritius, Mexico, Moldova, Mongolia, Montenegro, Morocco, Mozambique, Myanmar, Namibia, Nepal, Netherlands, New Zealand, Nicaragua, Niger, Nigeria, Norway, Oman, Pakistan, Panama, Papua New Guinea, Paraguay, Peru, Philippines, Poland, Portugal, Qatar, Romania, Russia, Rwanda, Saudi Arabia, Senegal, Serbia, Sierra Leone, Singapore, Slovenia, Solomon Islands, Somalia, South Africa, Spain, Sri Lanka, Sudan, Suriname, Swaziland, Sweden, Switzerland, Syria, Taiwan, Tajikistan, Tanzania, Thailand, Togo, Tunisia, Turkey, Turkmenistan, United Arab Emirates, Uganda, Ukraine, United Kingdom, Uruguay, Uzbekistan, Venezuela, Vietnam, Yemen, Yugoslavia, Zambia, Zimbabwe.

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