

International Sanctions and Emigration

Jerg Gutmann, Pascal Langer, Matthias Neuenkirch

Impressum:

CESifo Working Papers

ISSN 2364-1428 (electronic version)

Publisher and distributor: Munich Society for the Promotion of Economic Research - CESifo GmbH

The international platform of Ludwigs-Maximilians University's Center for Economic Studies and the ifo Institute

Poschingerstr. 5, 81679 Munich, Germany

Telephone +49 (0)89 2180-2740, Telefax +49 (0)89 2180-17845, email office@cesifo.de

Editor: Clemens Fuest

<https://www.cesifo.org/en/wp>

An electronic version of the paper may be downloaded

- from the SSRN website: www.SSRN.com
- from the RePEc website: www.RePEc.org
- from the CESifo website: <https://www.cesifo.org/en/wp>

International Sanctions and Emigration

Abstract

In this first empirical analysis of how sanctions affect international migration, we apply two estimation strategies, a panel difference-in-differences model and an event study approach. Our dataset covers 79,791 dyad-year observations, reflecting migration flows from 157 origin countries to 32 (largely OECD) destination countries between 1961 and 2018. The data supports that UN and joint EU-US sanctions increase emigration from target countries by around 20 percent. Our event study results for joint EU-US sanctions imply a gradual increase in emigration over the course of a sanction episode. The impact of UN sanctions on international migration is smaller and less persistent. Moreover, the effects are driven by target countries with fewer political rights and civil liberties, where emigration substitutes for the costly voicing of dissent. Finally, our results do not support systematic gender differences in the effect of sanctions on migration.

JEL-Codes: F220, F510, J160, O150.

Keywords: exit, gender differences, international sanctions, migration, voice.

Jerg Gutmann
University of Hamburg / Germany
jerg.gutmann@uni-hamburg.de

Pascal Langer
Trier University / Germany
pascal.langer@uni-trier.de

*Matthias Neuenkirch**
Trier University / Germany
neuenkirch@uni-trier.de

*corresponding author

First Draft: November 28, 2023

This Version: January 5, 2024

The authors gratefully acknowledge comments and suggestions by Abishek Choutagunta, Anne van Aaken, Hashem Nabas, Helene Thiollet, Houda Boubaker, Lea Marchal, Marek Endrich, Pedro Christofaro Lopes, Rim Adem, Roee Sarel, Shreya Shankar, Stefan Voigt, Tanja Porčnik, Yulia Khalikova, and from participants of the DIMIG Workshop on Migration Economics & the Political Economy of Immigration and the MACIE Seminar at Philipps University of Marburg.

1 Introduction

Research on the effects of international sanctions, thus far, has focused on either macro-level outcomes for society¹ or on political leaders' policy choices and their survival in office.² At the same time, researchers have paid much less attention to the question how sanctions affect citizens' decision-making. Some research argues that sanction threats and imposed sanctions incentivize anti-government protest (Grauvogel et al. 2017; Liou et al. 2021), which in turn might increase the likelihood that the government complies with the sender's political demands (Attia et al. 2020). Other studies, however, emphasize that sanctions would cause a rally-around-the-flag effect that strengthens the regime's popularity and, thus, its grip on the target country (Eichenberger and Stadelmann 2022; Gold et al. 2023; Grauvogel and Soest 2014; Seitz and Zazzaro 2020).³

Here, we provide the first empirical analysis of how sanctions cause international migration flows originating from target countries.⁴ Whereas previous studies have focused their attention on whether citizens react to sanctions by voicing criticism or support of the government, we ask whether some citizens respond by exiting the polity altogether. There is only limited qualitative evidence for an emigration-inducing effect of sanctions. Bossuyt (2000), for example, reports that emigration from Iraq skyrocketed under sanctions (see Connell et al. 2021, for a similar discussion of the case of Haiti).

1. Sanctions, for example, have adverse effects on the economy (Gutmann et al. 2023b; Neuenkirch and Neumeier 2015), increase economic inequality (Afesorgbor and Mahadevan 2016; Moteng et al. 2023; Neuenkirch and Neumeier 2016), harm the health of target populations – especially that of their most vulnerable members (Gutmann et al. 2021), and reduce international trade (Crozet and Hinz 2020; Felbermayr et al. 2020; Gutmann et al. 2023a) and capital flows (Besedeš et al. 2017; Biglaiser and Lektzian 2011; Mirkina 2018).

2. When facing sanctions, political leaders are more likely to violate basic, political, and civil rights (Adam and Tsarsitalidou 2019; Gutmann et al. 2020; Peksen and Drury 2009; Wood 2008) as well as property rights (Lee et al. 2023) because sanctions reduce leaders' likelihood of staying in office (Marinov 2005).

3. Frye (2019), in contrast, does not find a rally-around-the-flag in a survey experiment conducted in the context of sanctions imposed after Russia's illegal annexation of Crimea.

4. As it is common in the social science literature, we use the terms origin and destination country to describe migration flows and target and sender country to describe the imposition of sanctions.

Özdamar and Shahin (2021) identify the possible migration effect of sanctions as a central open research question in the literature on the effects of sanctions and, so far, no empirical study has addressed this question. Our study also contributes to a literature that has identified conflict events as a major driver of emigration (Davenport et al. 2003; Dreher et al. 2011; Moore and Shellman 2004). This literature has ignored international sanctions as a potentially important political shock responsible for emigration decisions.

In our empirical analysis, we apply two estimation strategies, a panel difference-in-differences (DiD) model and an event study approach. Our dataset combines data from the Global Sanctions Data Base (Felbermayr et al. 2020) with migration data from the OECD (2020) and the Determinants of International Migration (DEMIG 2015) dataset. It covers 79,791 dyad-year observations, reflecting migration flows from 157 origin countries to 32 destination countries between 1961 and 2018. We distinguish the effects of (i) UN, (ii) EU unilateral, (iii) US unilateral, (iv) joint EU-US (Western multilateral), and (v) “non-Western” (i.e., imposed by China or Russia) sanctions.⁵

Our findings suggest that UN and Western multilateral sanctions have a significant positive effect on migration. Migration flows from the target country increase by 17–18% under UN sanctions and by 23–24% under Western multilateral sanctions. Our event study results for Western multilateral sanctions show a gradual increase in emigration over the course of a sanction episode with a peak effect of 80–86% for long-lasting sanctions (relative to the final year before sanctions imposition). The impact of UN sanctions on international migration is smaller than that of Western multilateral sanctions (peak effect of 30–31%) and less persistent. Our findings can be interpreted as causal, as the measured increase in emigration marks a significant deviation from the pre-trend. In addition, migration flows return to their pre-sanction level once sanctions are lifted. The results (in particular those for Western multilateral sanctions) are driven by target countries with fewer political rights and civil liberties. This is in line with emigration serving as a substitute for voicing dissent, especially where the latter

5. Chinese and Russian sanctions are merged into a single dummy variable due to the low number of dyad-years with Chinese sanctions in place (653, less than 1% of all observations in the dataset).

is costly. Finally, our results are not indicative of gender differences in the effects of sanctions on migration.

Section 2 outlines our theoretical arguments and derives testable hypotheses. Section 3 explains the estimation approach and the data used to test our hypotheses. Section 4 presents the results of our event study and panel difference-in-differences estimations and discusses their congruence with our theoretical predictions. Section 5 draws conclusions and outlines the need for further research.

2 Theory

In his seminal book on “Exit, Voice, and Loyalty”, Hirschman (1970) contrasts two central accountability mechanisms the members of an economic, political, or social organization can rely on vis-à-vis the organization’s leadership. He argues that economists, with their trust in the virtues of competition, have disregarded the possible contribution of voice in ensuring the accountability of leaders, just as political scientists, with their focus on protest and voting, have neglected the role of exit. Another reason for economists’ and political scientists’ different analytical lenses is that exit is generally a private and often even a covert decision, whereas voice means contributing to a public good. Not surprisingly, political scientists are less interested in decisions that are typically not politically motivated and economists doubt citizens’ ability to overcome the collective action problem of voice in most circumstances. Here, we argue that political scientists have also largely disregarded the possible role of exit in the discussion of how citizens deal with the economic and political pressure created by international sanctions.⁶ Accordingly, emigration is one way in which citizens can respond to the negative consequences of sanctions and possibly their government’s inability or unwillingness to mitigate these consequences.

Other researchers who have described the link between sanctions and migration, however, have predicted the opposite of what follows from Hirschman’s (1970) the-

6. Studies on how capital flows respond to sanctions (Besedeš et al. 2017; Biglaiser and Lektzian 2011; Mirkina 2018) are clearly an exception.

ory. Afesorgbor (2019) argues that sanctions can reduce the flow of migrants, specifically between senders and targets by adversely affecting these countries' relationships. Weiner (1992) makes the related argument that sanctions are a key instrument to curtail unwanted migration flows towards the sanction-sending country (see also Portela and Charron 2023). Accordingly, emigration can be limited by imposing sanctions on states from which major migrant flows originate and, thereby, putting pressure on their governments to reduce these flows. Weiner (1992) even refers to the unusual case where Palestinians as a third party (i.e., neither the origin nor the destination country of migrants) threatened sanctions against carriers who brought Soviet Jews to Israel. A more recent example is US President Trump's threat to impose tariffs on all Mexican goods if Mexico did not curb the flow of migrants to the US southern border. Before sanctions were imposed, Mexico agreed to take adequate measures (Hufbauer and Jung 2020). Sanctions do not need to have the goal of reducing emigration from the target country. If they successfully target some of the causes of migration, such as conflict and human rights violations, sanctions can curb migration flows as a side effect. The imposition of sanctions might lead to reduced emigration even before human rights violations and conflicts end, as long as citizens believe that sanctions will improve living conditions in the target country in the foreseeable future. Finally, sanctions may lead to less emigration by depriving citizens of the financial resources needed for international migration.

Summing up our theoretical considerations, we arrive at the following set of opposing hypotheses:

Hypothesis 1a. *Sanctions increase emigration from the target country.*

Hypothesis 1b. *Sanctions curtail emigration from the target country.*

If the adverse conditions created by sanctions lead to emigration, it can be expected that not all members of society and not all societies are equally affected. Previous literature on the health and employment effects of sanctions has demonstrated a dispro-

portionate adverse effect of sanctions on women (Demir and Tabrizy 2022; Gutmann et al. 2021).⁷ This motivates our second hypothesis.

Hypothesis 2. *The positive migration effect of sanctions on women is larger than that on men.*

Moreover, if the argument by Hirschman (1970) and others that voice and exit are substitutes is taken seriously, the attractiveness of exit should depend on the costliness of using voice in general. Exit would then be chosen primarily where the government makes voice costly. Hence, our third hypothesis is the following:

Hypothesis 3. *The positive migration effect of sanctions is larger in countries with fewer political rights and civil liberties.*

Note that Hypotheses 2 and 3 are only plausible if Hypothesis 1a is supported by the data and are, thus, formulated conditional on sanctions having a positive effect on emigration from the target country.

Hirschman (1978) surveys the historical discussion on and case evidence for the effect of exit by either capital or citizens on the remaining society. Exit from states by dissatisfied citizens is not uncommon, but since it can be costly for society if too many citizens leave too quickly, it must be taken into consideration that not all polities allow for an unrestricted exit. To test our hypotheses, and in particular Hypothesis 3, existing legal restrictions on the freedom of international movement of citizens need to be accounted for, as they may significantly impede exit in response to sanctions.

3 Estimation Strategy and Data

3.1 Estimation Strategy

As migration depends on a variety of economic, political, social, and individual push and pull factors, it is not trivial to estimate the effect of economic sanctions on em-

7. The evidence on the effect of sanctions on women's rights, however, is ambiguous (Drury and Peksen 2014; Gutmann et al. 2020).

igration (e.g., Gibson and McKenzie 2011). Moreover, sanctions are typically imposed on politically unstable, and thus economically vulnerable, countries (Gutmann et al. 2021; Jing et al. 2003). Thus, causal effects have to be carefully identified.

Following Gutmann et al. (2023b), we apply two estimation strategies, a standard *panel DiD* model as well an *event study* approach. The panel DiD model can be expressed as follows:

$$y_{i,j,t} = \sum_{s=1}^5 \beta_{sanc}^s D_{sanc,i,t}^s + \gamma_1 X_{i,t}^{pol} + \gamma_2 X_{i,t-1}^{econ} + \alpha_{i,j} + \tau_{j,t} + \epsilon_{i,j,t} \quad (1)$$

The dependent variable $y_{i,j,t}$ is the log-transformed absolute number of people who migrate from country i to country j in year t .⁸ Our level of observation is the directed dyad- (or directed country pair-) year level. The vector of dummy variables $D_{sanc,i,t}^s$ distinguishes between (i) UN, (ii) EU unilateral, (iii) US unilateral, (iv) joint EU-US (Western multilateral),⁹ and (v) non-Western sanctions (i.e., imposed by China or Russia). These are our key variables of interest and take the value 1 if sanctions are imposed against a country i in year t , and 0 otherwise. Sanctions enacted by the UNSC are not counted as EU, US, or non-Western sanctions.¹⁰

We implement two sets of fixed effects to account for various unobserved factors influencing international migration flows. First, dyad (or country pair) fixed effects $\alpha_{i,j}$ absorb several standard control variables, such as distance, the presence of a common border, shared languages, and time-invariant cultural and genetic proximity. In addition, dyad fixed effects nest the less granular origin and destination fixed effects, that is, they account for all time-invariant origin and destination country characteristics. Second, destination-year fixed effects $\tau_{j,t}$ capture the annual political, macroeconomic, and social conditions within destination countries, often referred to as “pull factors.”

8. It is common in the migration literature to employ a ‘migration rate’ as the dependent variable, but log-transformation already makes sure that we explain relative changes in migration flows. Moreover, the inclusion of dyad fixed effects implies that we study the effect of sanctions on the deviation of migration flows from their dyad-specific conditional means.

9. We use the term “multilateral” here to refer to sanctions imposed by both the EU and the US, but this does not imply that these sanctions were coordinated between the EU and the US.

10. The three types of Western sanctions are by construction disjunctive. However, Western and non-Western sanctions can coincide.

This means that, for example, migration policy regimes in destination countries are fully accounted for, even if they change over time. These fixed effects also nest the less granular year fixed effects, which capture all global time trends in migration and the global political environment. The inclusion of origin-year fixed effects, however, is not feasible in our empirical design, as these would absorb all sanctions indicators. Instead, potentially confounding time-varying origin country characteristics are accounted for by including a set of economic and political control variables. $X_{i,t}^{pol}$ represents political and $X_{i,t-1}^{econ}$ represents one-year lagged economic control variables for the origin country, described in detail in Subsection 3.2. $\epsilon_{i,j,t}$ is the idiosyncratic error term.

Our second estimation strategy, the event study approach, is used to compare migration during the treatment period with the trends in migration before and after the imposition of sanctions (Dai et al. 2021; Gutmann et al. 2023b; Schmidheiny and Sieglöckh 2023). We examine the pre- and post-trend in migration for a period of three years before and after each sanction episode. By comparing these observations to non-sanctioned dyad-years, we can assess whether migration flows systematically increase before sanctions are imposed. This enables us to separate the impact of sanctions from the factors that led to their imposition. An additional benefit of the event study design is that it allows us to analyze how the treatment effect evolves over the course of the sanction episode, rather than solely estimating an average treatment effect. The event study specification can be formalized as follows:

$$y_{i,j,t} = \beta_{no}D_{no,i,t} + \sum_{l=2}^3 \beta_{pre,-l}D_{pre,i,t-l} + \sum_{l=1}^{11+} \beta_{sanc,l}D_{sanc,i,t,l} + \sum_{l=1}^3 \beta_{post,+l}D_{post,i,t+l} \quad (2)$$

$$+ \sum_{s=1}^4 \beta_{sanc}^s D_{sanc,i,t}^s + \gamma_1 X_{i,t}^{pol} + \gamma_2 X_{i,t-1}^{econ} + \alpha_{i,j} + \tau_{j,t} + \epsilon_{i,j,t}$$

The control variables ($X_{i,t}^{pol}$ and $X_{i,t-1}^{econ}$), fixed effects ($\alpha_{i,j}$ and $\tau_{j,t}$), and the idiosyncratic error term ($\epsilon_{i,j,t}$) are defined as in Eq. (1). The event study indicators, denoted as $D_{sanc,i,t,l}$, are binary variables that take the value 1 if either a Western multilateral

(joint EU-US) or a UN sanction episode targeting country i was active during the l^{th} consecutive year. To capture the effects of sanctions over time, we employ individual dummy variables for each of the first ten years in a sanction episode ($t1$ to $t10$). Because longer-lasting sanctions are rare, we combine all sanction-years after the tenth year of an episode in one dummy variable ($t11+$). A part of our robustness tests, the effects of these longer-lasting sanctions are further disentangled. $D_{pre,i,j,t-l}$ and $D_{post,i,j,t+l}$ are five dummy variables that identify the years three and two preceding and each of the three years following a sanction episode. These variables enable us to evaluate the trends in emigration from a sanctioned country before sanctions are imposed and after they are lifted. $D_{sanc,i,t}^s$ is a vector of controls for other types of sanctions. In event studies on the effect of Western multilateral sanctions, it contains UN, EU only, US only, and non-Western sanctions dummies. In event studies on the effect of UN sanctions, the vector contains EU only, US only, joint EU-US, and non-Western sanctions. Finally, $D_{no,i,t}$ is a dummy variable identifying (i.e., coded 1 for) all observations where neither the sanction variables of interest (i.e., dummy variables for either UN sanctions or Western multilateral sanctions) nor the corresponding pre- and post-trends are coded 1. Hence, the estimated effects of sanctions (over time) and the pre-/post-trend coefficients are to be interpreted as deviations from the final year before the imposition of sanctions ($t - 1$), that is, the year for which we have omitted the pre-trend dummy from Eq. (2) (cf., Callaway and Sant'Anna 2021).

Eqs. (1) and (2) are estimated using ordinary least squares and the standard errors are clustered at the dyad level.

3.2 Data

Our final dataset contains 79,791 observations for which we have complete data on migration, sanctions, and all control variables. It covers migration flows from 157 origin countries to 32 destination countries (31 of which are OECD members), corresponding to a total of 4,596 dyads, between 1961 and 2018 (see Table OA1 in the Online

Appendix for a list of countries).¹¹ Our dependent variables are based on the absolute number of migrants from country i to country j in year t .¹² For each model and specification, we also distinguish male from female migrants to examine whether there are gender differences in the effects of sanctions.¹³ Our baseline specification studies migration at the extensive and intensive margin and relies on a $\log(y_{i,j,t} + 1)$ transformation. In addition, we assess the robustness of our results by estimating a model of only the intensive margin using a $\log(y_{i,j,t})$ transformation.¹⁴ Migration data is taken from the DEMIG (2015) and OECD (2020) databases.

Sanctions data is obtained from the *Global Sanctions Data Base* (Felbermayr et al. 2020; Kirikakha et al. 2021; Syropoulos et al. 2023). In our empirical analysis, we focus on the most active senders, that is, the UN, the US, the EU, Russia, and China. In total, our dataset covers 3,932 dyad-years with UN sanctions in place, 2,034 with EU unilateral sanctions, 8,972 with US unilateral sanctions, 4,480 with EU-US joint (or Western multilateral) sanctions, and 5,561 with non-Western (i.e., Chinese or Russian) sanctions. UN and Western multilateral sanctions naturally occur less frequently, since more parties have to agree on their imposition and for UN sanctions unanimous consent of five veto powers is required. Moreover, Wood (2008) and Hufbauer et al. (2009) document that UN sanctions are, on average, less comprehensive than US unilateral or multilateral sanctions.¹⁵ Finally, the US are typically most active in imposing sanctions. On average, UN sanctions are introduced after US unilateral sanctions were already in place for 2.9 years; but in 50 percent of these cases, no US sanctions were

11. Note that migration within a country is not recorded in the dataset, but it would also not allow evading the impact of sanctions. Some migrants, of course, do not move to these 32 destination countries. However, non-OECD countries have been lacking the statistical capacity to record migrant inflows by origin country over many decades.

12. This standard dataset of bilateral migration flows does not allow us to distinguish different types of migration, such as legal or illegal migration.

13. Note that information on other socio-demographic characteristics of interest, such as education or marital status, is unfortunately not available.

14. Depending on the dependent variable (total migration, male migration, or female migration), this robustness test is based on up to 11,000 observations less than the baseline specification.

15. Note that the *Global Sanctions Data Base* does not measure the level of severity of sanctions, with trade sanctions being the only exception.

previously in place. Joint EU-US sanctions are preceded by 1.3 years of US sanctions, but only in 25.4 percent of the cases were US sanctions previously in place.¹⁶

To account for the economic causes of migration in the origin countries, we control for real GDP per capita (in logs and lagged by one year due to potential reverse causality). Political and social causes of migration are incorporated through the *Polity2* democracy index (Marshall and Gurr 2020) and the *Human Rights Protection Score* by Fariss (2019). Moreover, we include the binary variable *Freedom of Movement*, based on version 12 of the V-Dem dataset (Coppedge et al. 2022).¹⁷ To test our third hypothesis, we add a variable measuring *Civil Liberties* and interact it with the indicators for UN sanctions and Western multilateral sanctions.¹⁸ Finally, we control for the occurrence of conflicts and wars based on data by Gleditsch et al. (2002) and Davies et al. (2022) and distinguish between two levels of intensity (minor and war) and three different scopes of conflict (interstate, intrastate without intervention, and intrastate with intervention).

Table OA2 in the Online Appendix provides a detailed description of all variables alongside their definitions and data sources. Tables OA3 and OA4 show descriptive statistics. In general, migration flows are larger if an origin country is subject to sanctions. However, sanctioned countries, on average, have a lower GDP per capita, are less democratic, show more infringements of human rights, provide fewer civil liberties, and experience minor conflicts and wars at a much higher frequency. Taken together, this underscores the importance of separating the treatment effect from the selection effect, as the political, social, and economic environment is clearly worse in sanctioned countries.

16. For UN sanctions, the corresponding figures for the other senders are as follows. EU: 0.1 years and 7.4 percent, Russia: 0.1 years and 1.1 percent, China: 0 years and 0 percent. Joint EU-US sanctions are, on average, preceded by 1.6 years of EU unilateral sanctions and in 25.9 percent of the cases.

17. The variable takes the value 0 in the case of no respect for freedom of movement and 1 if freedom of foreign travel and emigration is at least weakly respected.

18. The Civil Liberties variable is based on the standardized first principal component of the variables Freedom of Discussion for Men, Freedom of Discussion for Women, Freedom of Academic & Cultural Expression, and Freedom of Peaceful Assembly from the V-Dem dataset.

4 Empirical Results

4.1 Baseline Results

Average Treatment Effects. Table 1 shows the results of the DiD estimations for different left-hand side variables at the extensive and intensive margin, that is, after a $\log + 1$ transformation. UN and Western multilateral sanctions have a significant positive effect on migration, confirming Hypothesis 1a. On average, UN sanctions increase migration flows from the target country by roughly 17.4–18.4%, whereas the effect of joint EU-US sanctions amounts to a 22.9–24.3% increase in migration. Concerning gender differences, we find a slightly, but not significantly, larger effect of Western multilateral sanctions on female migration (+24.3%) as compared to male migration (+22.9%).¹⁹ In contrast, non-Western sanctions only have a significant impact on male migration (+7.6%), but not on total and female migration. Hence, we find no robust empirical support for Hypothesis 2. Finally, unilateral EU or US sanctions do not significantly affect migration flows.

Turning to the control variables, we find positive coefficients for real GDP per capita and democracy, indicating more migration from economically well-developed democracies to OECD countries. Moreover, human rights infringements (indicated by a negative coefficient) and major internal conflicts with international intervention lead to more emigration, whereas major interstate conflicts appear to curtail migration flows. When using the coefficient estimates for major internal conflicts with international intervention (39.4–45.9%) as a point of reference, we find that the effects of UN and Western multilateral sanctions have about half the effect size of large-scale civil wars. In conclusion, the migration effects of these sanctions are quantitatively relevant.

19. The baseline estimates for UN sanctions and joint EU-US sanctions remain virtually unchanged if we interact the sanction variables with the indicators for major conflicts (estimates are available on request). Hence, the effects of sanctions on migration are not driven by episodes that coincide with large-scale conflicts.

Table 1: Baseline Results (Log + 1)

	(1) Total Migration	(2) Male Migration	(3) Female Migration
Sanctions			
... UN	0.184*** (0.052)	0.175*** (0.053)	0.174*** (0.050)
... EU only	0.051 (0.035)	0.049 (0.036)	0.050 (0.035)
... US only	-0.001 (0.025)	0.007 (0.025)	-0.005 (0.024)
... Multilateral (EU-US)	0.233*** (0.041)	0.229*** (0.041)	0.243*** (0.042)
... Non-Western	0.034 (0.028)	0.076*** (0.028)	0.014 (0.027)
lagged $\log(\text{GDP pc})$	0.297*** (0.070)	0.287*** (0.068)	0.311*** (0.072)
Polity2	0.008** (0.003)	0.005 (0.003)	0.009*** (0.003)
Human Rights	-0.124*** (0.016)	-0.118*** (0.016)	-0.111*** (0.016)
Freedom of Movement	0.048 (0.054)	0.032 (0.054)	0.043 (0.052)
Interstate Conflicts			
... Minor	0.018 (0.063)	0.040 (0.063)	-0.012 (0.066)
... Major	-0.197* (0.110)	-0.203* (0.115)	-0.237** (0.094)
Internal Conflicts w/o Intervention			
... Minor	0.023 (0.023)	0.026 (0.024)	0.037 (0.023)
... Major	-0.010 (0.037)	0.028 (0.039)	-0.025 (0.038)
Internal Conflicts w/ Intervention			
... Minor	0.006 (0.045)	0.035 (0.043)	0.000 (0.043)
... Major	0.415*** (0.078)	0.458*** (0.078)	0.394*** (0.076)
Observations	79,791	79,791	79,791
R ²	0.940	0.930	0.938
Within-R ²	0.020	0.018	0.020

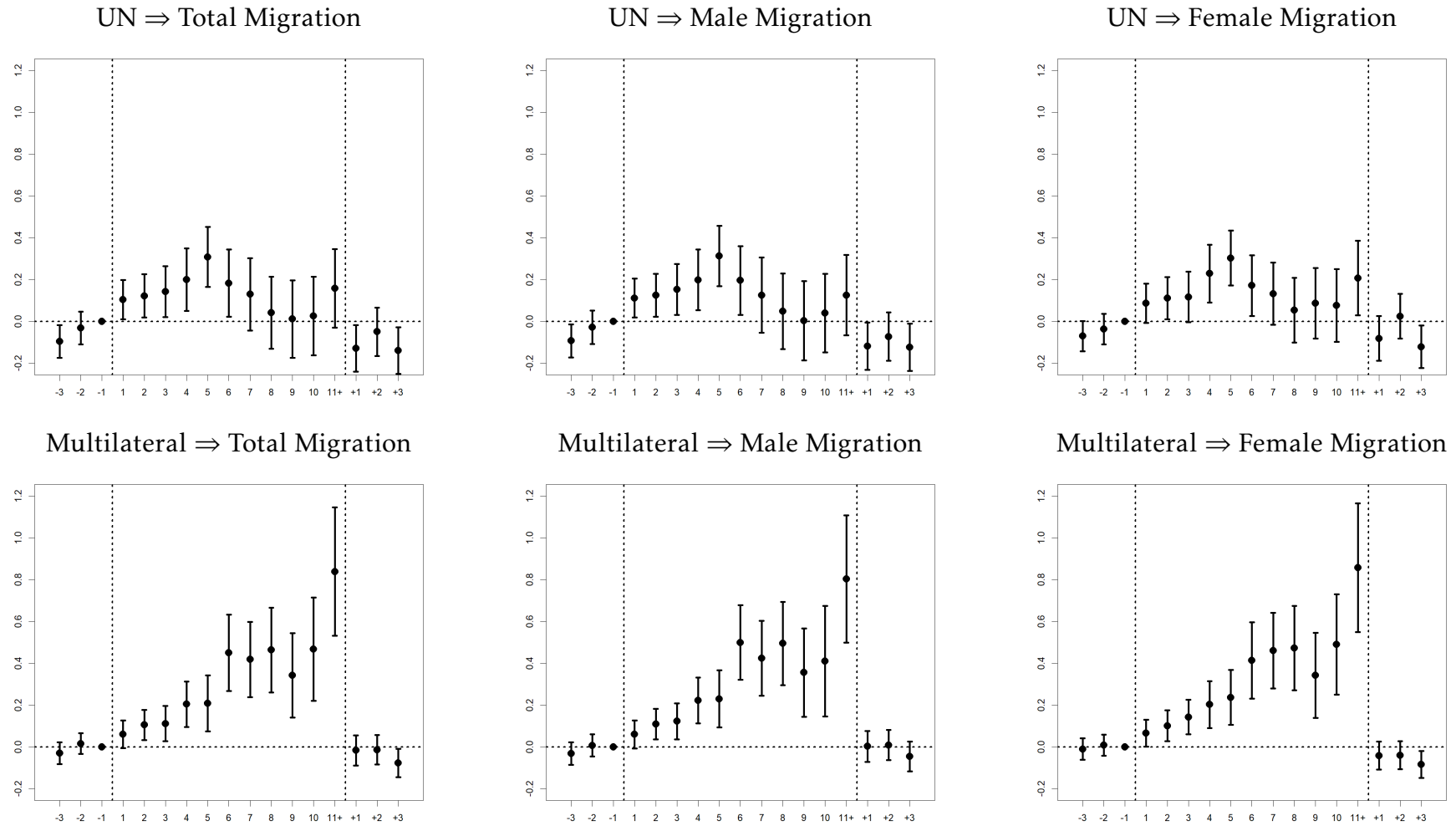
Notes: Coefficient estimates of Eq. (1) with different dependent variables. Standard errors in parentheses are clustered at the dyad level. Models include dyad fixed effects and destination-year fixed effects. ***/**/* indicates significance at the 1%/5%/10% level.

We explore the robustness of our results in Table A1 in the Appendix where we repeat these estimations at the intensive margin. In general, the results are qualitatively very similar to those in Table 1. If at all, we find larger point estimates at the intensive margin with 20.7–23.2% for UN sanctions and 24.9–26.8% for Western multilateral sanctions. In both cases, the effect on female migration is slightly (but not significantly) larger than that on male migration. Moreover, the effect of non-Western sanctions on male migration remains no longer significant. Hence, the intensive margin estimations further support Hypothesis 1a and provide some mild evidence in favor of Hypothesis 2.

Effects of Sanctions over Time. Figure 1 shows the point estimates and the corresponding 95% confidence bands for UN sanctions (upper panel) and Western multilateral sanctions (lower panel) during the three years before a sanction episode ($-3, -2, -1$), over the course of a sanction episode ($1, 2, \dots, 11+$), and for the three years after sanctions are lifted ($+1, +2, +3$). Following Callaway and Sant’Anna (2021), the effect of the year before the implementation of sanctions (-1) is normalized to a value of 0. Hence, the estimated effects of sanctions over time and the pre-/post-trend have to be interpreted relative to the final year before the implementation of sanctions.

There are no significant pre-trends observable before the imposition of Western multilateral sanctions. In addition, migration flows return to their pre-treatment (i.e., -1) levels once sanctions are lifted with even a slight reduction in migration after three years. In the case of UN sanctions, we observe slow upward pre-trends and some evidence for a reduction in migration relative to pre-treatment levels once sanctions are lifted. However, the effects estimated in the treatment period mark a clear deviation from any pre-trend, which supports a causal interpretation of the results.

Figure 1: Effects of UN and Western Multilateral Sanctions Over Time (Log + 1)



Notes: Effects of sanctions over time (1, 2, ..., 11+) alongside pre-trend (-3, -2, -1) and post-trend (+1, +2, +3) according to an estimation of Eq. (2) for different dependent variables. Standard errors are clustered at the dyad level. Models include control variables (other sanction indicators, lagged $\log(\text{GDP pc})$, Polity2, Human Rights, Freedom of Movement, and six conflict indicators), dyad fixed effects, and destination-year fixed effects. 95% confidence bands are indicated by whiskers.

Mirroring the results in Table 1, the impact of UN sanctions on international migration is smaller than that of Western multilateral sanctions. Nevertheless, the effect is statistically significant during the first six years of a sanction episode (and for female migration also in the case of long-lasting sanctions). The estimated effect reaches its peak of 30.3–31.3% in year 5 and declines thereafter. Western multilateral sanctions lead to a gradual increase in migration flows during the entire course of a sanction episode (with only the first year for total and male migration not being significant). The effects are particularly pronounced for very long-lasting sanctions (80.4–85.8%), but they also reach levels of 46.8–50.0% during the first ten years. While the economic effects of sanctions appear to be strongest in the first years of a sanctions episode (Gutmann et al. 2023b), it is plausible that the effect on migration builds up over the years. Emigration is a costly strategic decision and is often based more on expectations regarding future living conditions than only on the status quo. Doxey (1996), for example, describes the increase in white Rhodesian emigration after their initial false expectation that sanctions against their country would not be long-lasting. Finally, we do not detect systematic gender differences in both panels of Figure 1.²⁰

We explore the robustness of our results and repeat the event study estimations while excluding origin countries that were never subject to UN sanctions or Western multilateral sanctions. Arguably, the social, political, and economic situation in countries that were subject to sanctions at some point in time is more comparable to the situation of countries that enter the treatment group. This yields a more conservative counterfactual, but comes at the cost of reduced estimation efficiency. Figure OA2 in the Online Appendix shows the results. Using the restricted control group, we observe slightly larger peak effects of UN sanctions in year 5 (32.6–34.4%) as compared to the baseline results in Figure 1. For Western multilateral sanctions, the peak effects (39.0%–43.8%) and the effects of long-lasting sanctions (67.1%–69.5%) are smaller in

20. Figure OA1 in the Online Appendix shows the corresponding event study plots for up to 20 years of a UN or Western multilateral sanction episode. The effects of Western multilateral sanctions increase up until their peak in year 18/19 of a sanction episode. The impact of UN sanctions on migration peaks a second time in year 16 and is particularly pronounced for very long-lasting sanctions. However, the estimates beyond the tenth year should be interpreted with caution, as they are based on only a small number of observations.

the robustness test. In general, our findings that Western multilateral sanctions trigger more emigration than UN sanctions and that there are no systematic gender differences in emigration due to sanctions are both supported by this robustness test.

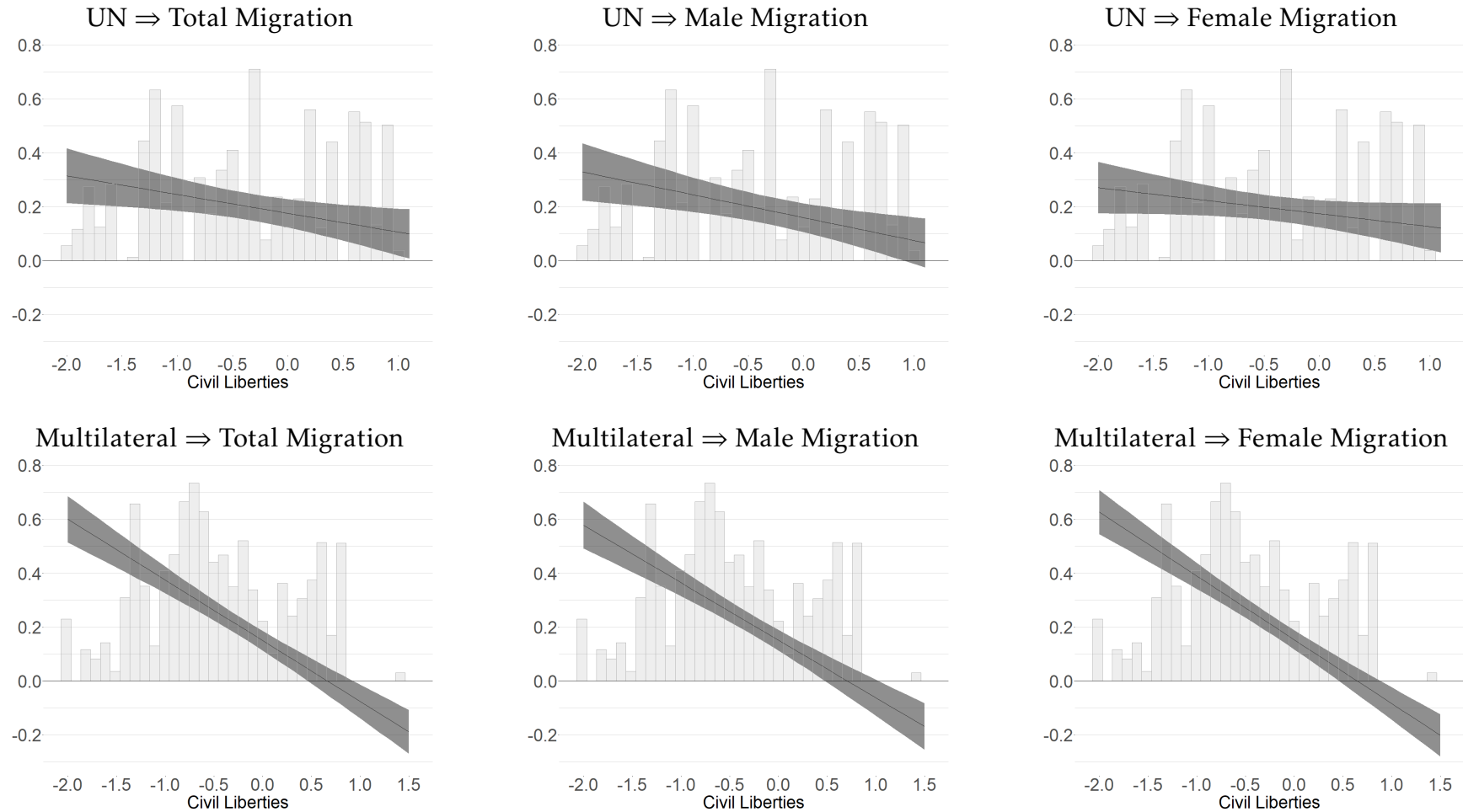
4.2 Extensions

Effects of Sanctions at Different Levels of Civil Liberties. To test Hypothesis 3, we extend Eq. (1) with a variable measuring civil liberties and two interactions of this variable with the indicators for UN sanctions and Western multilateral sanctions. Table OA5 in the Appendix shows the results. Figure 2 provides a straightforward visualization of the effects of sanctions on international migration, conditional on the level of civil liberties in the target country. For both interactions, the estimated effects are visualized over the whole range of observed civil liberties values of target countries, although the estimated effect is supported by very little data at the top end of the distribution.

The effect of UN sanctions on migration depends only weakly on the level of civil liberties, which is underlined by the non-significance of the negative interaction terms in the case of total migration and female migration (see Table OA5 in the Online Appendix). In contrast, the effect of Western multilateral sanctions is strongly moderated and shrinks by 21–24% with each additional standard deviation in the indicator for civil liberties.²¹ Hence, we find in line with Hypothesis 3 that increased migration (i.e., exit) in response to sanctions only occurs if there are relevant constraints on civil liberties (i.e., voice).

21. A significant negative effect of Western multilateral sanctions on migration is estimated for levels of civil liberties larger than 0.84–0.97 or, put differently, for less than 2% of the observations under Western multilateral sanctions. It, thus, seems more plausible to assume a null effect on migration for sanction targets with relatively high levels of civil liberties.

Figure 2: Effects of UN and Western Multilateral Sanctions at Different Levels of Civil Liberties (Log + 1)



Notes: Effects of sanctions at different levels of civil liberties according to an estimation of Eq. (1) for different dependent variables. Standard errors are clustered at the dyad level. Models include control variables (other sanction indicators, lagged $\log(\text{GDP pc})$, Polity2, Human Rights, Freedom of Movement, Civil Liberties, and six conflict indicators), dyad fixed effects, and destination-year fixed effects. 95% confidence bands are indicated by dark-gray shaded areas. Light-gray vertical bars illustrate the full distribution of civil liberties under UN and Western multilateral sanctions. See also Table OA5 in the Online Appendix.

Effects of Sanctions Across Different Income Groups. As a final exercise, we analyze the effects of sanctions across different income groups. For that purpose, we rely on the World Bank classification and merge low and lower-middle income countries into one group as well as upper-middle and high income countries into another one. Table A2 in the Appendix shows the results of an estimation of Eq. (1) for the two resulting income groups.

The effects of UN and Western multilateral sanctions are more or less the same across income groups. If at all, the point estimates are (slightly) larger for high-income countries, but they are also less precisely estimated. The difference in the coefficients for multilateral sanctions as compared to the baseline estimates in Table 1 might be due to the loss of roughly 8,000 observations, as the World Bank classification is only available since 1987.²² Finally, we detect some differences in the effects of unilateral US sanctions and non-Western sanctions on migration flows from low-income countries (negative) and from high-income countries (positive). There is no apparent theoretical justification for these subsample effects, which cancel each other out in the full sample.²³

5 Conclusions

Our paper provides the first empirical analysis of how sanctions affect international migration flows originating from target countries. We apply two estimation strategies, a panel difference-in-differences model and an event study approach. Our dataset includes 79,791 dyad-year observations, reflecting migration flows from 157 origin countries to 32 destination countries between 1961 and 2018.

Our key findings suggest that UN and Western multilateral sanctions have a significant positive effect on migration, as emigration flows increase, on average, by 17–18% under UN sanctions and by 23–24% under Western multilateral sanctions. Our event

22. Indeed, when restricting the sample for the baseline estimations in Table 1 to start in 1987, the effect of Western multilateral sanctions shrinks to 16.6–16.8%.

23. We also considered splitting the dataset into the Cold War period and the period thereafter. However, due to limited data availability before 1990, this would leave us with very unbalanced subsamples of 10,993 (Cold War) and 68,798 (post-Cold War) observations.

study results of Western multilateral sanctions imply a gradual increase in emigration over the course of a sanction episode with a peak effect of 80–86% for long-lasting sanctions (relative to the final year before sanctions are imposed). The impact of UN sanctions on international migration is smaller, with a peak effect of 30–31%, and less persistent. Our findings can be interpreted as causal, since the increase in emigration marks a significant deviation from the pre-trend. In addition, migration flows return to their pre-sanction level once sanctions are lifted. Our results are not indicative of gender differences in the effects of sanctions on migration. This finding contrasts with previous research that finds women to be disproportionately affected by sanctions (Gutmann et al. 2021).

The results (in particular those for Western multilateral sanctions) are driven by countries with fewer political rights and civil liberties. This is in line with emigration serving as a substitute for voicing dissent, especially where the latter is costly (see also Hirschman 1970). The fact that sanctions lead dissatisfied citizens to emigrate may help to reconcile arguments in the literature that sanctions can cause both protest (Grauvogel et al. 2017; Liou et al. 2021) and rally-around-the-flag effects (Eichenberger and Stadelmann 2022; Gold et al. 2023; Grauvogel and Soest 2014; Seitz and Zazzaro 2020). Especially in illiberal target countries, where protest and free speech are suppressed, those opposed to the regime may emigrate during sanctions, allowing for a consolidation of regime support among the remaining population.

For sanction senders, our results imply a potential unintended consequence of settling international conflicts using sanctions. Sanctions are not only detrimental to the sender country's economy, they can also trigger waves of international migration that may cause additional economic and political costs for the sender.

An important limitation of cross-country research on migration is the availability of bilateral migration data for only a limited number of destination countries. Our results are, therefore, reflective of migration to OECD countries (the only non-OECD member being South Africa), but they might not automatically generalize to South-South migration. Case studies of countries targeted by economic sanctions that would

also consider emigration to non-OECD countries are, thus, urgently needed to evaluate the external validity of our results.

References

- Adam, A., and S. Tsarsitalidou. 2019. "Do sanctions lead to a decline in civil liberties?" *Public Choice* 180:191–215.
- Afesorgbor, S. K. 2019. "The impact of economic sanctions on international trade: How do threatened sanctions compare with imposed sanctions?" *European Journal of Political Economy* 56:11–26.
- Afesorgbor, S. K., and R. Mahadevan. 2016. "The impact of economic sanctions on income inequality of target states." *World Development* 83:1–11.
- Attia, H., J. Grauvogel, and C. von Soest. 2020. "The termination of international sanctions: Explaining target compliance and sender capitulation." *European Economic Review* 129:103565.
- Besedeš, T., S. Goldbach, and V. Nitsch. 2017. "You're banned! The effect of sanctions on German cross-border financial flows." *Economic Policy* 32 (90): 263–318.
- Biglaiser, G., and D. J. Lektzian. 2011. "The effect of sanctions on U.S. foreign direct investment." *International Organization* 65 (3): 531–551.
- Bossuyt, M. 2000. *The adverse consequences of economic sanctions on the enjoyment of human rights*. Working paper prepared for the ESC Commission on Human Rights. United Nations Economic and Social Council.
- Callaway, B., and P. H. C. Sant'Anna. 2021. "Difference-in-differences with multiple time periods." *Journal of Econometrics* 225 (2): 200–230.
- Connell, B. J., S. L. Moya, and A. J. Shin. 2021. "Migration and economic coercion." *Foreign Policy Analysis* 17 (3): orab019.
- Coppedge, M., J. Gerring, C. H. Knutsen, S. Lindberg, J. Teorell, N. Alizada, D. Altman, et al. 2022. "V-Dem [Country-Year/Country-Date] Dataset v12." *Varieties of Democracy (V-Dem) Project*.

- Crozet, M., and J. Hinz. 2020. "Friendly fire: The trade impact of the Russia sanctions and counter-sanctions." *Economic Policy* 35 (101): 97–146.
- Dai, M., G. J. Felbermayr, A. Kirilakha, C. Syropoulos, E. Yalcin, and Y. V. Yotov. 2021. "Timing the impact of sanctions on trade." In van Bergeijk 2021, 411–437.
- Davenport, C., W. Moore, and S. Poe. 2003. "Sometimes you just have to leave: Domestic threats and forced migration, 1964-1989." *International Interactions* 29 (1): 27–55.
- Davies, S., T. Pettersson, and M. Öberg. 2022. "Organized violence 1989-2021 and drone warfare." *Journal of Peace Research* 59 (4): 809–825.
- DEMIG. 2015. "DEMIG C2C, version 1.2." www.migrationinstitute.org.
- Demir, F., and S. S. Tabrizy. 2022. "Gendered effects of sanctions on manufacturing employment: Evidence from Iran." *Review of Development Economics* 26 (4): 2040–2069.
- Doxey, M. P. 1996. *International sanctions in contemporary perspective*. Second Edition. New York: St Martin's Press.
- Dreher, A., T. Krieger, and D. Meierrieks. 2011. "Hit and (they will) run: The impact of terrorism on migration." *Economics Letters* 113 (1): 42–46.
- Drury, A. C., and D. Peksen. 2014. "Women and economic statecraft: The negative impact international economic sanctions visit on women." *European Journal of International Relations* 20 (2): 463–490.
- Eichenberger, R., and D. Stadelmann. 2022. "Sanctions are costly for citizens but beneficial for autocrats: A political-economic perspective." *The Economists' Voice* 19 (2): 109–123.
- Fariss, C. J. 2019. "Yes, human rights practices are improving over time." *American Political Science Review* 113 (3): 868–881.

- Felbermayr, G., A. Kirilakha, C. Syropoulos, E. Yalcin, and Y. V. Yotov. 2020. "The Global Sanctions Data Base." *European Economic Review* 129:103561.
- Frye, T. 2019. "Economic sanctions and public opinion: Survey experiments from Russia." *Comparative Political Studies* 52 (7): 967–994.
- Gibson, J., and D. McKenzie. 2011. "The microeconomic determinants of emigration and return migration of the best and brightest: Evidence from the Pacific." *Journal of Development Economics* 95 (1): 18–29.
- Gleditsch, N. P., P. Wallensteen, M. Eriksson, M. Sollenberg, and H. Strand. 2002. "Armed conflict 1946-2001: A new dataset." *Journal of Peace Research* 39 (5): 615–637.
- Gold, R., J. Hinz, and M. Valsecchi. 2023. "To Russia with love? The impact of sanctions on regime support." *Kiel Working Paper* 2212.
- Grauvogel, J., A. A. Licht, and C. von Soest. 2017. "Sanctions and signals: How international sanction threats trigger domestic protest in targeted regimes." *International Studies Quarterly* 61 (1): 86–97.
- Grauvogel, J., and C. von Soest. 2014. "Claims to legitimacy count: Why sanctions fail to instigate democratisation in authoritarian regimes." *European Journal of Political Research* 53 (4): 635–653.
- Gutmann, J., M. Neuenkirch, and F. Neumeier. 2020. "Precision-guided or blunt? The effects of US economic sanctions on human rights." *Public Choice* 185:161–182.
- . 2021. "Sanctioned to death? The impact of economic sanctions on life expectancy and its gender gap." *Journal of Development Studies* 57 (1): 139–162.
- . 2023a. "Do China and Russia undermine western sanctions? Evidence from DiD and event study estimation." Forthcoming, *Review of International Economics*.
- . 2023b. "The economic effects of international sanctions: An event study." Forthcoming, *Journal of Comparative Economics*.

- Hirschman, A. O. 1970. *Exit, voice, and loyalty: Responses to decline in firms, organizations, and states*. Cambridge and London: Harvard University Press.
- . 1978. "Exit, voice, and the state." *World Politics* 31 (1): 90–107.
- Hufbauer, G. C., and E. Jung. 2020. "What's new in economic sanctions?" *European Economic Review* 130:103572.
- Hufbauer, G. C., J. J. Schott, K. A. Elliott, and B. Oegg. 2009. *Economic sanctions reconsidered*. Third Edition. Washington: Peterson Institute.
- Jing, C., W. H. Kaempfer, and A. D. Lowenberg. 2003. "Instrument choice and the effectiveness of international sanctions: A simultaneous equations approach." *Journal of Peace Research* 40 (5): 519–535.
- Kirikakha, A., G. J. Felbermayr, C. Syropoulos, E. Yalcin, and Y. V. Yotov. 2021. "The Global Sanctions Data Base (GSDB): an update that includes the years of the Trump presidency." In van Bergeijk 2021, 62–106.
- Lee, H., D. Lektzian, and G. Biglaiser. 2023. "The effects of economic sanctions on foreign asset expropriation." *Journal of Conflict Resolution* 67:266–296.
- Liou, R. Y.-L., A. Murdie, and D. Peksen. 2021. "Revisiting the causal links between economic sanctions and human rights violations." *Political Research Quarterly* 74 (4): 808–821.
- Marinov, N. 2005. "Do economic sanctions destabilize country leaders?" *American Journal of Political Science* 49 (3): 564–576.
- Marshall, M. G., and T. R. Gurr. 2020. "Polity5: Political Regime Characteristics and Transitions, 1800-2028." Accessed April 23, 2020. <https://www.systemicpeace.org/inscrdata.html>.
- Mirkina, I. 2018. "FDI and sanctions: An empirical analysis of short- and long-run effects." *European Journal of Political Economy* 54:198–225.

- Moore, W. H., and S. M. Shellman. 2004. "Fear of persecution: Forced migration, 1952-1995." *Journal of Conflict Resolution* 48 (5): 723–745.
- Moteng, G., C. Raghutla, H. Njangang, and L. N. Nembot. 2023. "International sanctions and energy poverty in target developing countries." *Energy Policy* 179:113629.
- Neuenkirch, M., and F. Neumeier. 2015. "The impact of UN and US economic sanctions on GDP growth." *European Journal of Political Economy* 40:110–125.
- . 2016. "The impact of US sanctions on poverty." *Journal of Development Economics* 121:110–119.
- OECD. 2020. "International Migration Database." <https://stats.oecd.org/Index.aspx?DataSetCode=MIG>.
- Özdamar, Ö., and E. Shahin. 2021. "Consequences of economic sanctions: The state of the art and paths forward." *International Studies Review* 23 (4): 1646–1671.
- Peksen, D., and A. C. Drury. 2009. "Economic sanctions and political repression: Assessing the impact of coercive diplomacy on political freedoms." *Human Rights Review* 10 (3): 393–411.
- Portela, C., and A. Charron. 2023. "The evolution of databases in the age of targeted sanctions." *International Studies Review* 25 (1): viac061.
- Schmidheiny, K., and S. Sieglöcher. 2023. "On event studies and distributed-lags in two-way fixed effects models: Identification, equivalence, and generalization." *Journal of Applied Econometrics* 38 (5): 695–713.
- Seitz, W., and A. Zazzaro. 2020. "Sanctions and public opinion: The case of the Russia-Ukraine gas disputes." *Review of International Organizations* 15 (4): 817–843.
- Syropoulos, C., G. J. Felbermayr, A. Kirilakha, E. Yalcin, and Y. V. Yotov. 2023. "The global sanctions data base—release 3: Covid-19, Russia, and multilateral sanctions." Forthcoming, *Review of International Economics*.

van Bergeijk, P. A., ed. 2021. *Research handbook on economic sanctions*. Cheltenham and Northampton: Edward Elgar.

Weiner, M. 1992. "Security, stability, and international migration." *International Security* 17 (3): 91–126.

Wood, R. M. 2008. "'A hand upon the throat of the nation': Economic sanctions and state repression, 1976-2001." *International Studies Quarterly* 52 (3): 489–513.

World Bank. 2023. "World Development Indicators." <https://data.worldbank.org/>.

Appendix

Table A1: Baseline Results (Log)

	(1) Total Migration	(2) Male Migration	(3) Female Migration
Sanctions			
... UN	0.207*** (0.057)	0.209*** (0.060)	0.232*** (0.058)
... EU only	0.047 (0.039)	0.045 (0.040)	0.058 (0.040)
... US only	0.000 (0.026)	0.010 (0.028)	-0.008 (0.026)
... Multilateral (EU-US)	0.249*** (0.045)	0.254*** (0.047)	0.268*** (0.049)
... Non-Western	0.008 (0.027)	0.040 (0.029)	-0.022 (0.028)
Observations	72,267	69,106	68,527
R ²	0.931	0.918	0.926
Within-R ²	0.022	0.019	0.024

Notes: Coefficient estimates of Eq. (1) and different dependent variables. Standard errors in parentheses are clustered at the dyad level. Models include control variables (lagged $\log(\text{GDP pc})$, Polity2, Human Rights, Freedom of Movement, and six conflict indicators; estimates are available on request), dyad fixed effects, and destination-year fixed effects. ***/**/* indicates significance at the 1%/5%/10% level.

Table A2: Results for Low- and High-Income Countries (Log + 1)

	(1) Total Migration	(2) Male Migration	(3) Female Migration
Panel A: Low-Income Countries			
Sanctions			
... UN	0.174*** (0.044)	0.173*** (0.044)	0.141*** (0.042)
... EU only	0.053 (0.039)	0.062 (0.041)	0.038 (0.036)
... US only	-0.042* (0.024)	-0.029 (0.026)	-0.045* (0.023)
... Multilateral (EU-US)	0.136*** (0.033)	0.146*** (0.033)	0.131*** (0.032)
... Non-Western	-0.122** (0.055)	-0.127** (0.056)	-0.129** (0.064)
Observations	37,526	37,526	37,526
R ²	0.949	0.939	0.947
Within-R ²	0.019	0.020	0.017
Panel B: High-Income Countries			
Sanctions			
... UN	0.195* (0.114)	0.209* (0.119)	0.186* (0.106)
... EU only	0.059 (0.053)	0.066 (0.054)	0.039 (0.062)
... US only	0.109** (0.046)	0.114** (0.045)	0.098** (0.044)
... Multilateral (EU-US)	0.149*** (0.048)	0.159*** (0.048)	0.148** (0.063)
... Non-Western	0.104*** (0.028)	0.129*** (0.028)	0.089*** (0.027)
Observations	34,290	34,290	34,290
R ²	0.956	0.948	0.954
Within-R ²	0.008	0.008	0.006

Notes: Coefficient estimates of Eq. (1) and different dependent variables. Standard errors in parentheses are clustered at the dyad level. Models include control variables (lagged $\log(\text{GDP pc})$, Polity2, Human Rights, Freedom of Movement, and six conflict indicators; estimates are available on request), dyad fixed effects, and destination-year fixed effects. ***/**/* indicates significance at the 1%/5%/10% level.

Online Appendix

Table OA1: List of Countries in the Dataset

Origin Countries (number of observations; number of sanctioned observations)

Afghanistan (124; 124), Albania (505; 112), Algeria (623; 112), Angola (502; 156), Argentina (678; 95), Armenia (467; 0), Australia (698; 132), Austria (738; 148), Azerbaijan (457; 99), Bahrain (436; 0), Bangladesh (507; 0), Belarus (483; 414), Belgium (697; 134), Benin (471; 250), Bhutan (422; 0), Bolivia (542; 208), Botswana (464; 0), Brazil (675; 36), Bulgaria (599; 209), Burkina Faso (456; 23), Burundi (477; 154), Cambodia (433; 194), Cameroon (511; 45), Canada (447; 377), Cape Verde (439; 0), Central African Republic (433; 232), Chad (470; 46), Chile (622; 119), China (599; 569), Colombia (588; 421), Comoros (403; 0), Costa Rica (520; 377), Croatia (461; 240), Cuba (610; 610), Cyprus (506; 461), Czech Republic (429; 126), Denmark (657; 129), Dominican Republic (506; 197), Ecuador (534; 84), Egypt (617; 253), El Salvador (475; 23), Equatorial Guinea (415; 43), Eritrea (247; 143), Estonia (417; 152), Eswatini (438; 0), Ethiopia (549; 109), Fiji (430; 311), Finland (633; 129), France (817; 528), Gabon (435; 0), Gambia (451; 194), Georgia (465; 183), Germany (689; 148), Ghana (550; 25), Greece (741; 208), Guatemala (498; 364), Guinea (470; 349), Guinea-Bissau (445; 194), Guyana (434; 0), Haiti (471; 437), Honduras (490; 24), Hungary (501; 143), India (667; 371), Indonesia (570; 477), Iran (641; 584), Iraq (463; 421), Ireland (572; 420), Israel (444; 0), Italy (800; 139), Jamaica (470; 198), Japan (681; 0), Jordan (544; 28), Kazakhstan (465; 12), Kenya (552; 154), Kosovo (14; 0), Kuwait (442; 0), Kyrgyzstan (442; 0), Laos (373; 24), Latvia (432; 154), Lebanon (345; 345), Lesotho (443; 62), Liberia (371; 327), Libya (406; 406), Lithuania (448; 160), Luxembourg (520; 114), Madagascar (497; 202), Malawi (459; 48), Malaysia (525; 0), Mali (489; 93), Mauritania (424; 74), Mauritius (473; 0), Mexico (602; 0), Moldova (463; 376), Mongolia (488; 0), Montenegro (292; 97), Morocco (619; 0), Mozambique (468; 0), Myanmar (513; 467), Namibia (426; 0), Nepal (528; 17), Netherlands (754; 136), New Zealand (481; 0), Nicaragua (485; 86), Niger (464; 108), Nigeria (562; 433), North Macedonia (458; 0), Norway (625; 224), Oman (425; 0), Pakistan (585; 221), Panama (509; 217), Papua New Guinea (420; 0), Paraguay (492; 10), Peru (568; 63), Philippines (570; 386), Poland (518; 154), Portugal (657; 176), Qatar (350; 0), Romania (537; 207), Russia (555; 153), Rwanda (492; 246), Saudi Arabia (515; 25), Senegal (493; 0), Serbia (488; 377), Sierra Leone (486; 262), Singapore (482; 0), Slovakia (426; 126), Slovenia (421; 124), Solomon Islands (392; 0), Somalia (145; 145), South Africa (586; 150), South Korea (520; 10), Spain (706; 139), Sri Lanka (547; 5), Sudan (549; 471), Suriname (430; 14), Sweden (684; 149), Switzerland (602; 0), Syria (598; 416), Tajikistan (444; 0), Tanzania (458; 97), Thailand (551; 213), Timor-Leste (302; 0), Togo (474; 172), Tunisia (596; 214), Turkey (675; 89), Turkmenistan (431; 19), Uganda (480; 0), Ukraine (506; 215), United Arab Emirates (451; 0), United Kingdom (775; 149), United States (823; 139), Uruguay (553; 15), Uzbekistan (455; 208), Vietnam (548; 374), Yemen (481; 190), Zambia (500; 21), Zimbabwe (473; 379).

Destination Countries (number of observations)

Australia (3266), Austria (3182), Belgium (1998), Canada (3139), Chile (1831), Czech Republic (3682), Czechoslovakia (884), Denmark (4858), Estonia (2286), Finland (4440), France (1774), Germany (5897), Hungary (1685), Ireland (30), Israel (723), Italy (2866), Latvia (462), Luxembourg (2127), Mexico (1144), Netherlands (1923), New Zealand (5216), Norway (3241), Poland (1065), Portugal (282), Slovakia (3206), Slovenia (1974), South Africa (1750), South Korea (2890), Spain (4100), Sweden (3559), Switzerland (1367), Turkey (382), United States (2562).

Table OA2: Definitions of Variables and Data Sources

Variable	Definition & Source
$\log(\text{Total Migration}) / \log(\text{Male Migration}) / \log(\text{Female Migration})$	Natural logarithm [$\log(y + 1)$ & $\log(y)$] of total / male / female bilateral migration. <i>Source:</i> OECD International Migration Database (OECD 2020), DEMIG C2C dataset (DEMIG 2015).
Sanctions	Binary indicators for country-years with sanctions in place. <i>Source:</i> Global Sanctions Data Base (Felbermayr et al. 2020; Kirikakha et al. 2021; Syropoulos et al. 2023).
lagged $\log(\text{GDP pc})$	Natural logarithm of real GDP per capita in 2015 USD, lagged by one year. <i>Source:</i> World Bank (World Bank 2023).
Polity2	Democracy indicator that ranges from strongly democratic (+10) to strongly autocratic (-10). <i>Source:</i> Polity5 Dataset (Marshall and Gurr 2020).
Human Rights	Latent human rights variable with higher values indicating a better protection of human rights. <i>Source:</i> Human Rights Protection Scores (Fariss 2019).
Freedom of Movement	Binary variable that takes the value 0 in the case of no respect for freedom of movement and 1 if freedom of freedom of foreign travel and emigration is at least weakly respected. <i>Source:</i> V-Dem version 12 (Coppedge et al. 2022).
Civil Liberties	Standardized first principal component of Freedom of Discussion for Men, Freedom of Discussion for Women, Freedom of Academic & Cultural Expression, and Freedom of Peaceful Assembly. <i>Source:</i> V-Dem version 12 (Coppedge et al. 2022).
Minor Conflict / Major Conflict	Armed conflicts resulting in 25 to 999 / at least 1,000 battle-related deaths in a given year. <i>Source:</i> UCDP/PRIO Armed Conflict Dataset (Gleditsch et al. 2002; Davies et al. 2022).
Interstate Conflict	Conflicts between two states. <i>Source:</i> UCDP/PRIO Armed Conflict Dataset (Gleditsch et al. 2002; Davies et al. 2022)
Intrastate Conflict w/ Intervention / w/o Intervention	Conflicts between a government and one or more rebel groups with / without military intervention of foreign governments. <i>Source:</i> UCDP/PRIO Armed Conflict Dataset (Gleditsch et al. 2002; Davies et al. 2022)

Table OA3: Descriptive Statistics

	All Observations		No Sanctions		Sanctions	
	Mean	<i>N</i>	Mean	<i>N</i>	Mean	<i>N</i>
Total Migration						
... <i>log</i> (... + 1)	4.06	79,791	3.87	55,701	4.49	24,090
... <i>log</i> (...)	4.39	72,267	4.24	49,719	4.72	22,548
Male Migration						
... <i>log</i> (... + 1)	3.49	79,791	3.31	55,701	3.90	24,090
... <i>log</i> (...)	3.92	69,106	3.78	47,249	4.21	21,857
Female Migration						
... <i>log</i> (... + 1)	3.38	79,791	3.22	55,701	3.76	24,090
... <i>log</i> (...)	3.81	68,527	3.68	47,015	4.10	21,512
lagged <i>log</i> (GDP pc)	8.44	79,791	8.51	55,701	8.29	24,090
Polity2	3.67	79,791	3.95	55,701	3.04	24,090
Human Rights	0.45	79,791	0.59	55,701	0.12	24,090
Civil Liberties	0.36	79,791	0.48	55,701	0.16	24,090
	<i>X</i> = 1	<i>N</i>	<i>X</i> = 1	<i>N</i>	<i>X</i> = 1	<i>N</i>
Freedom of Movement	74,725	79,791	53,642	55,701	21,083	24,090
Minor Conflict	11,190	79,791	6,370	55,701	4,820	24,090
... Interstate	492	79,791	283	55,701	209	24,090
... Internal w/ Intervention	1,717	79,791	1,111	55,701	606	24,090
... Internal w/o Intervention	8,981	79,791	4,976	55,701	4,005	24,090
Major Conflict	2,653	79,791	1,093	55,701	1,560	24,090
... Interstate	176	79,791	89	55,701	87	24,090
... Internal w/ Intervention	807	79,791	107	55,701	700	24,090
... Internal w/o Intervention	1,670	79,791	897	55,701	773	24,090

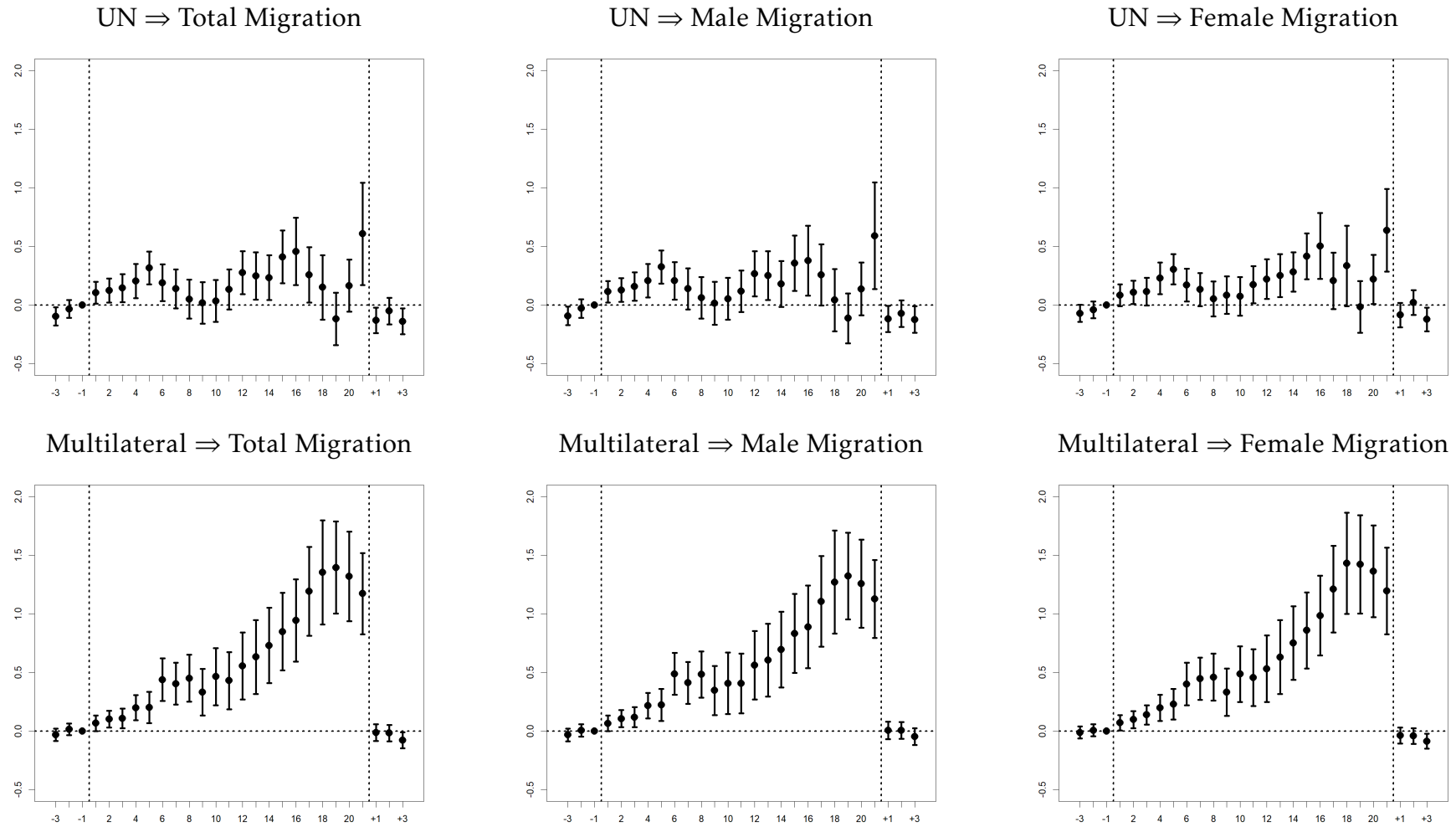
Notes: Mean values and non-zero observations ($X = 1$) for all LHS variables and control variables. Columns '*N*' show the number of observations in the full dataset, without sanctions in place, and with sanctions in place. The smaller number of observations for the 'log transformation' is due to dyad-years with zero migration flows.

Table OA4: Frequency of Sanctions in the Dataset

Panel A: Panel DiD Estimations			
Sanctions in General			
... No Sanctions	55,701		
... Sanctions	24,090		
Sanction Senders		Sanction Targets	
... UN	3,932	... Low Income	12,536
... EU only	2,034	... High Income	10,265
... US only	8,972		
... EU-US joint	4,480		
... Non-Western	5,561		
Panel B: Event Study Approach			
UN Sanctions		Western Multilateral Sanctions	
... Pre-Trend -3 Years	354	... Pre-Trend -3 Years	714
... Pre-Trend -2 Years	406	... Pre-Trend -2 Years	764
... Pre-Trend -1 Year	435	... Pre-Trend -1 Year	843
... Year 1	353	... Year 1	794
... Year 2	366	... Year 2	670
... Year 3	331	... Year 3	479
... Year 4	265	... Year 4	372
... Year 5	262	... Year 5	304
... Year 6	227	... Year 6	188
... Year 7	188	... Year 7	186
... Year 8	180	... Year 8	152
... Year 9	171	... Year 9	109
... Year 10	178	... Year 10	99
... Year 11+	1,411	... Year 11+	1,127
... Post-Trend +1 Year	425	... Post-Trend +1 Year	853
... Post-Trend +2 Years	394	... Post-Trend +2 Years	776
... Post-Trend +3 Years	337	... Post-Trend +3 Years	731

Notes: Frequency of observations of the different sanction indicators for which all control variables (see Table OA3) are available. Total number of observations in the dataset: 79,791. Sanctions enacted by the UNSC are not counted as US, EU, or non-Western sanctions. The three types of Western sanctions are by construction disjunctive. However, Western and non-Western sanctions can coincide.

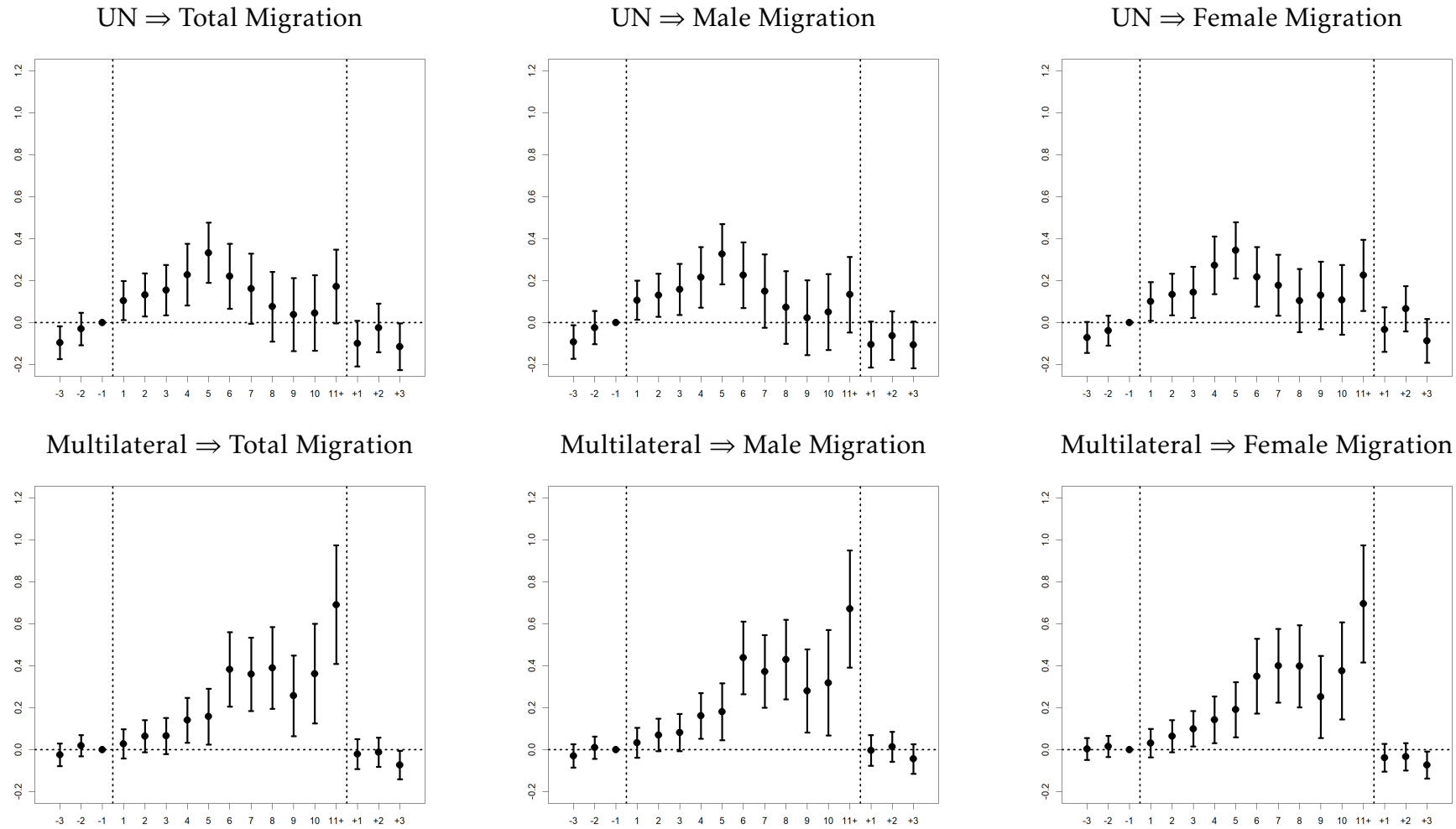
Figure OA1: Effects of UN and Western Multilateral Sanctions Over Time (Log + 1): Extension 20 Years



35

Notes: Effects of sanctions over time (1, 2, ..., 21+) alongside pre-trend (-3, -2, -1) and post-trend (+1, +2, +3) according to an estimation of a modified version of Eq. (2) for different dependent variables. Standard errors are clustered at the dyad level. Models include control variables (other sanction indicators, lagged $\log(\text{GDP pc})$, Polity2, Human Rights, Freedom of Movement, and six conflict indicators), dyad fixed effects, and destination-year fixed effects. 95% confidence bands are indicated by whiskers.

Figure OA2: Effects of UN and Western Multilateral Sanctions Over Time (Log + 1): Excluding Never-Sanctioned Origin Countries



Notes: Effects of sanctions over time (1, 2, ..., 11+) alongside pre-trend (-3, -2, -1) and post-trend (+1, +2, +3) according to an estimation of Eq. (2) for different dependent variables, excluding origin countries that were never subject to UN sanctions or Western multilateral sanctions. Standard errors are clustered at the dyad level. Models include control variables (other sanction indicators, lagged $\log(\text{GDP pc})$, Polity2, Human Rights, Freedom of Movement, and six conflict indicators), dyad fixed effects, and destination-year fixed effects. 95% confidence bands are indicated by whiskers.

Table OA5: Results incl. Civil Liberties (Log + 1)

	(1) Total Migration	(2) Male Migration	(3) Female Migration
Sanctions			
... UN	0.176*** (0.053)	0.159*** (0.053)	0.174*** (0.052)
... EU only	0.047 (0.035)	0.045 (0.036)	0.047 (0.035)
... US only	0.005 (0.024)	0.013 (0.025)	-0.000 (0.023)
... Multilateral (EU-US)	0.149*** (0.040)	0.151*** (0.039)	0.153*** (0.041)
... Non-Western	0.038 (0.027)	0.080*** (0.028)	0.016 (0.027)
Civil Liberties	-0.009 (0.033)	-0.007 (0.033)	-0.022 (0.032)
UN × Civil Liberties	-0.070 (0.049)	-0.085* (0.050)	-0.048 (0.048)
Multilateral × Civil Liberties	-0.225*** (0.059)	-0.213*** (0.060)	-0.237*** (0.057)
Observations	79,791	79,791	79,791
R ²	0.940	0.930	0.939
Within-R ²	0.022	0.020	0.023

Notes: Coefficient estimates of Eq. (1) and different dependent variables. Standard errors in parentheses are clustered at the dyad level. Models include control variables (lagged $\log(\text{GDP pc})$, Polity2, Human Rights, Freedom of Movement, and six conflict indicators; estimates are available on request), dyad fixed effects, and destination-year fixed effects. ***/**/* indicates significance at the 1%/5%/10% level.