UNIVERSITÄT TRIER

Jerg Gutmann
Pascal Langer
Matthias Neuenkirch

International Sanctions and Emigration

Research Papers in Economics
No. 11/23

International Sanctions and Emigration*

Jerg Gutmann† Pascal Langer‡ Matthias Neuenkirch§

First Draft: November 28, 2023
This Version: June 5, 2024

*The authors gratefully acknowledge comments and suggestions by Abishek Choutagunta, Anne van Aaken, Bernd Hayo, Daniel Meierrieks, Hashem Nabas, Hélène Thiollet, Houda Boubaker, Jérôme Valette, Léa Marchal, Marek Endrich, Mathilde Ceron, Panu Poutvaara, Pedro Christofaro Lopes, Rim Arem, Roee Sarel, Shreya Shankar, Stefan Voigt, Tanja Porčnik, Tim Krieger, Yulia Khalikova, and from participants of the DIMIG Workshop on Migration Economics & the Political Economy of Immigration at Paris 1, the MACIE Seminar at Philipps University of Marburg, the 7th International Conference on the Political Economy of Democracy and Dictatorship in Münster, and the 2024 European Public Choice Society Meeting in Vienna.

†University of Hamburg and CESifo, e-mail: jerg.gutmann@uni-hamburg.de.

‡Trier University, e-mail: pascal.langer@uni-trier.de.

§Corresponding author: Trier University and CESifo, Department of Economics, 54286 Trier, Germany, e-mail: neuenkirch@uni-trier.de.

International Sanctions and Emigration

Abstract

In this first statistical analysis of how international 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 industrialized destination countries between 1961 and 2018. We find 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 throughout 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 limited freedom of political expression, where emigration substitutes for the costly voicing of dissent. Finally, there appear to be no systematic gender differences in the migration effect of sanctions.

JEL Codes: F22, F51, J16, K38, O15.

Keywords: Exit; Gender Differences; International Sanctions; Migration; Voice.

1 Introduction

Research on the effects of international sanctions, thus far, has focused on either macro-level outcomes for society[1] or on political leaders' policy choices and their survival in office.[2] At the same time, researchers have paid much less attention to 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) and with those of non-violent protest movements (Liou et al. 2023). 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. 2024; Grauvogel and Soest 2014; Seitz and Zazzaro 2020).[3]

Here, we conduct the first statistical analysis of how sanctions cause international migrant flows originating from target countries.[4] Whereas previous studies have focused 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).[5]

1. Sanctions, for example, have adverse effects on the economy (Gutmann, Neuenkirch, et al. 2023; Hatipoglu and Peksen 2018; Neuenkirch and Neumeier 2015; Peksen and Son 2015; Shchepeleva et al. 2024), increase economic inequality and poverty (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. 2024) 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), while sanctions reduce those leaders' likelihood of staying in office (Marinov 2005).

3. Frye (2019), in contrast, does not find a rally-around-the-flag effect 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.

5. Connell et al. (2021) study the effect of a potential sender of sanctions being host to a large diaspora from a potential target country. They demonstrate that diaspora presence is associated with political opposition to imposing sanctions and argue that this is due to policymakers' fear of migration pressure from the target state.

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 (GSDB, see 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) joint EU-US (Western multilateral), (iii) EU unilateral, (iv) US unilateral, and (v) "non-Western" (i.e., imposed by China or Russia) sanctions.[6]

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 22–24% under Western multilateral sanctions. Our event study results for Western multilateral sanctions show a gradual increase in emigration throughout a sanction episode with a peak effect of 80–86% for long-lasting sanctions (relative to the final year before sanction imposition). The impact of UN sanctions on international migration is smaller than that of Western multilateral sanctions (peak effect: 30–31%) and less persistent. Our findings can be interpreted as causal because the measured increase in emigration marks a significant deviation from the pre-trend. In addition, migrant 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 limited freedom of political expression. This is consistent with emigration serving as a substitute for voicing dissent, especially where

6. 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).

the latter is costly. Finally, our results indicate no 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 panel difference-in-differences and event study estimations and discusses their congruence with our theoretical predictions. Section 5 concludes 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 using voice is a contribution 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.[7] Accordingly, emigration is one way for citizens to respond to the negative consequences of sanctions and possibly their government's inability or unwillingness to mitigate them. However, this does not mean that citizens migrate to punish the government. It is rather the individually rational choice of exit, which – if

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

taken independently by many individuals – turns out to be collectively costly to the government and potentially threatens its survival in office.

Other researchers who described the link between sanctions and migration have predicted the opposite of what follows from Hirschman's (1970) theory. Afesorgbor (2019) argues that sanctions can reduce the flow of migrants, specifically between senders and targets by adversely affecting these countries' relationships.[8] Weiner (1992) makes the related argument that sanctions are a key instrument to curtail unwanted migrant 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, 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 be intended to reduce 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.*

8. However, Cucu and Panon (2023) provide empirical evidence that asylum recognition rates are higher between countries with worse diplomatic relations, which would incentivize migration.

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. It can, therefore, not be assumed that the development of GDP per capita under sanctions is a precise metric of the hardship caused for the target population. The economic consequences of sanctions will, for example, be concentrated in particular sectors and occupations, where jobs might be lost and real wages might fall. And also only parts of society are vulnerable to a lack of access to critical imported consumption goods, such as medicine. Therefore, the hardship caused by sanctions, which may motivate emigration, cannot be broken down into a small set of well-defined and empirically operationalizable transmission channels.

Previous literature on the health and employment effects of sanctions has demonstrated disproportionate adverse effects of sanctions on women (Demir and Tabrizy 2022; Gutmann et al. 2021).[9] In addition, previous literature on gender differences in migration has demonstrated that differing migration decisions of men and women – both on the individual level and when aggregated to the country level – can be traced back to systematic gender-specific incentives (Gutmann, Marchal, et al. 2023; Neumayer and Plümper 2021; Ruyssen and Salomone 2018). This motivates our second hypothesis.

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

Not only are different societies and members of society differently affected by sanctions, but their responses to the hardship caused may also differ systematically. 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. Exit would then be chosen primarily where the government's policy raises the cost of voice. Hence, our third hypothesis is the following:

9. The evidence on how sanctions affect women's rights, however, is ambiguous (Drury and Peksen 2014; Gutmann et al. 2020).

Hypothesis 3. *The positive migration effect of sanctions is larger in countries with less freedom of political expression.*

Of course, Hypotheses 2 and 3 are only plausible if Hypothesis 1a is supported by the data. They 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 a state by dissatisfied citizens is not uncommon. But since it can be costly for society if too many citizens leave too quickly, it must be considered that not all polities allow for an unrestricted exit. The Berlin Wall, for example, was built in 1961 exactly for that reason – to stop the exodus from the German Democratic Republic to the West. To test our hypotheses, particularly Hypothesis 3, existing legal restrictions on citizens' freedom of international movement must be accounted for, as they may significantly impede exit in response to sanctions. Finally, we also evaluate empirically whether countries targeted with sanctions increase restrictions on foreign migration (see Section 5).

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

Following Gutmann, Neuenkirch, et al. (2023), 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:

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

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.[10] Our level of observation is the directed dyad- (or directed country pair-) year level. The vector of dummy variables $D^{s}_{sanc,i,t}$ distinguishes between (i) UN, (ii) joint EU-US (Western multilateral)[11], (iii) EU unilateral, (iv) US unilateral, 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.[12]

We implement two sets of fixed effects to account for various unobserved factors influencing international migrant flows. First, dyad (or country pair) fixed effects $\alpha_{i,j}$ absorb various standard control variables, such as distance, 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. Thus, 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". 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 if one is interested in measuring the total emigration effect of sanctions, as all sanctions in-

10. It is common in the migration literature to measure the dependent variable as a 'migration rate'. Log-transformation and dyad fixed effects imply that we also study the effect of sanctions on the relative deviation of migration flows from their dyad-specific conditional means.

11. 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.

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

dicators would be absorbed.[13] Instead, we account for potentially confounding time-varying origin country characteristics – in line with the migration economics literature and the literature on the effects of international sanctions (e.g., Beine et al. 2019; Giménez-Gómez et al. 2019; Gutmann, Neuenkirch, et al. 2023) – 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 Section 3.2. $\epsilon_{i,j,t}$ is an 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, Neuenkirch, et al. 2023; Schmidheiny and Siegloch 2023). We examine the pre- and post-trend in migration in the 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 throughout the sanction episode, rather than solely estimating an average treatment effect. The event study specification can be formalized as follows:

$$y_{i,j,t} = \beta_{no} D_{no,i,t} + \sum_{l=2}^{3} \beta_{pre,-l} D_{pre,i,t-l} + \sum_{l=1}^{11+} \beta_{sanc,l} D_{sanc,i,tl} + \sum_{l=1}^{3} \beta_{post,+l} D_{post,i,t+l} \quad (2)$$
$$+ \sum_{s=1}^{4} \beta_{sanc}^{s} D_{sanc,i,t}^{s} + \gamma_1 X_{i,t}^{pol} + \gamma_2 X_{i,t-1}^{econ} + \alpha_{i,j} + \tau_{j,t} + \epsilon_{i,j,t}$$

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 $D_{sanc,i,tl}$, are binary variables that take the value 1 if either a UN or a Western multilateral (joint EU-US) sanction episode targeting country i was active during the l^{th}

13. Origin-year fixed effects can be included if one is only interested in sanction-induced migrant flows to specific countries, such as flows to sanction senders. However, such estimates must be interpreted carefully, as migration flows to countries other than the senders and caused by the same sanctions would be part of the counterfactual migration trend. We estimate such a model as an extension in Section 4.2.

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 ($t11+$).[14] $D_{pre,i,j,t-l}$ 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. $D^{s}_{sanc,i,t}$ is a vector of controls for other types of sanctions. In event studies on the effect of UN sanctions, the vector contains joint EU-US, EU only, US only, and non-Western sanctions. In event studies on the effect of Western multilateral sanctions, the vector contains UN, EU only, US only, 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). Our dependent variables are based on the absolute number of migrants from country i to country j in year t. For our baseline specifica-

14. The effects of longer-lasting sanctions over time are disentangled further in Section 4.2.

tions, we also distinguish male from female migrants to evaluate gender differences in the effects of sanctions.[15] Our baseline specification studies migration at the combined extensive and intensive margin and relies on a $log(y_{i,j,t} + 1)$-transformation.[16] 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.[17] Migration data is taken from the DEMIG (2015) and OECD (2020) databases.[18]

Sanctions data is obtained from the *Global Sanctions Data Base* (GSDB, see Felbermayr et al. 2020; Kirikakha et al. 2021; Syropoulos et al. 2024). Our empirical analysis focuses on the most active senders: the UN, the US, the EU, Russia, and China. In total, our dataset covers 3,932 dyad-years with UN sanctions in place, 4,480 with EU-US joint (or Western multilateral) sanctions, 2,034 with EU unilateral sanctions, 8,972 with US unilateral 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.[19] Finally, the US is the most active sender of international sanc-

15. There is no information on other socio-demographic characteristics of interest, such as education or marital status.

16. We also test for the robustness of our results at the combined extensive and intensive margin by using an inverse hyperbolic sine transformation: $log(y_{i,j,t} + \sqrt{y_{i,j,t}^2 + 1})$ (see Aihounton and Henningsen 2020; Bartlett 1947; Chen and Roth 2024, for alternatives to log-transformation in regression analysis).

17. 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.

18. Many migrants, of course, do not move to these 32 destination countries. However, nonindustrialized countries lacked the statistical capacity to record migrant inflows by origin country over many decades. Domestic migration is also not recorded and the data does not allow us to distinguish types of migration, such as legal vs illegal or skilled vs unskilled migration. Finally, this dataset does not cover immigration into sanctioned countries. Using the World Bank's net migration dataset, we explore this issue as an extension. The results for net migration (available on request) are consistent with our findings for emigration from sanctioned countries.

19. The GSDB does not measure the severity of sanctions, except for trade sanctions. We also explore the difference between types of sanctions. However, different types (arms, financial, military, trade, travel, and others) often coincide (i.e., sanction episodes are typically of more than one type), which can cause collinearity problems given that there is only a limited number of sanctions in each category. For instance, most sanctioned observations (UN: 2,981; multilateral: 3,825) entail financial sanctions. To reach conceptually clear and empirically robust results, we focus in an extension on one main subcategory of sanctions: those including trade or financial sanctions, which should be most prone to cause economic hardship. Most sanctions include one of the two types (UN: 2,981; multilateral: 3,992) and only a small fraction do not (UN: 951; multilateral: 488). Coefficient estimates for trade/financial sanctions

tions. On average, UN sanctions (/joint EU-US sanctions) are newly introduced after US unilateral sanctions were already in place for 2.9 (1.3) years. However, only 50 (25.4) percent of newly imposed UN sanctions (/joint EU-US sanctions) were preceded by US sanctions.[20]

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). Considering these control variables should reduce omitted variable bias. But since sanctions also affect income, human rights, and democracy, we may at the same time introduce bias by controlling for mediating variables. We evaluate the robustness of our results to varying the model specification with regard to control variables in Section 4.2. Moreover, we include the binary variable *Freedom of Movement*, based on version 14 of the V-Dem dataset (Coppedge et al. 2024).[21] To test our third hypothesis, we add a variable measuring freedom of political expression (*Political Expression*) and interact it with the indicators for UN sanctions and Western multilateral sanctions.[22] 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 definitions and data sources of all variables. 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

are consistent with the results below for sanctions in general. Estimates for non-trade/non-financial sanctions have to be interpreted cautiously and are available on request.

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

21. The variable takes the value 0 if there is no respect for freedom of foreign movement and 1 if freedom of foreign travel and emigration is at least weakly respected. Lagging all political control variables, all control variables, or none of the control variables by one year leaves our results virtually unchanged (estimates are available on request).

22. The freedom of political expression variable is based on the standardized first principal component of the following variables from the V-Dem dataset: freedom of discussion for men, freedom of discussion for women, freedom of academic & cultural expression, and freedom of peaceful assembly.

average, have a lower GDP per capita, are less democratic, show more infringements of human rights, provide less freedom of political expression, and experience minor conflicts and wars at a much higher frequency. These descriptive statistics underscore the importance of separating the treatment effect from the selection effect, as the political, social, and economic environment is considerably worse in sanctioned countries.

4 Empirical Results

4.1 Baseline Results

Average Treatment Effects. Table 1 shows the results of the DiD estimations for different dependent variables at the combined 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 16.8–17.7%, whereas the effect of joint EU-US sanctions amounts to a 22.4–23.8% increase in migration. Concerning gender differences, we find a slightly, but not significantly, larger effect of Western multilateral sanctions on female migration (+23.8%) as compared to male migration (+22.4%).[23] In contrast, non-Western sanctions only significantly impact male migration (+7.6%), but not total or female migration. Hence, we find no empirical support for Hypothesis 2. Finally, unilateral EU or US sanctions do not significantly affect migration flows.

23. 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 sanction episodes that coincide with large-scale conflicts.

Table 1: Baseline Results (Log + 1)

	(1) Total Migration	(2) Male Migration	(3) Female Migration
Sanctions			
… UN	0.177***	0.169***	0.168***
	(0.052)	(0.053)	(0.050)
… Multilateral (EU-US)	0.228***	0.224***	0.238***
	(0.042)	(0.042)	(0.043)
… EU only	0.053	0.050	0.052
	(0.035)	(0.036)	(0.035)
… US only	–0.005	0.004	–0.009
	(0.025)	(0.025)	(0.024)
… Non-Western	0.034	0.076***	0.013
	(0.028)	(0.028)	(0.027)
lagged *log*(GDP pc)	0.304***	0.295***	0.318***
	(0.070)	(0.069)	(0.072)
Polity2	0.009***	0.006*	0.010***
	(0.003)	(0.003)	(0.003)
Human Rights	–0.123***	–0.116***	–0.109***
	(0.016)	(0.016)	(0.016)
Freedom of Movement	–0.031	–0.043	–0.031
	(0.055)	(0.056)	(0.053)
Interstate Conflicts			
… Minor	0.020	0.041	–0.010
	(0.063)	(0.063)	(0.066)
… Major	–0.203*	–0.209*	–0.243***
	(0.110)	(0.115)	(0.094)
Internal Conflicts w/o Intervention			
… Minor	0.024	0.027	0.037
	(0.023)	(0.024)	(0.023)
… Major	–0.010	0.028	–0.025
	(0.038)	(0.039)	(0.038)
Internal Conflicts w/ Intervention			
… Minor	0.007	0.036	0.001
	(0.045)	(0.043)	(0.043)
… Major	0.417***	0.460***	0.396***
	(0.077)	(0.078)	(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

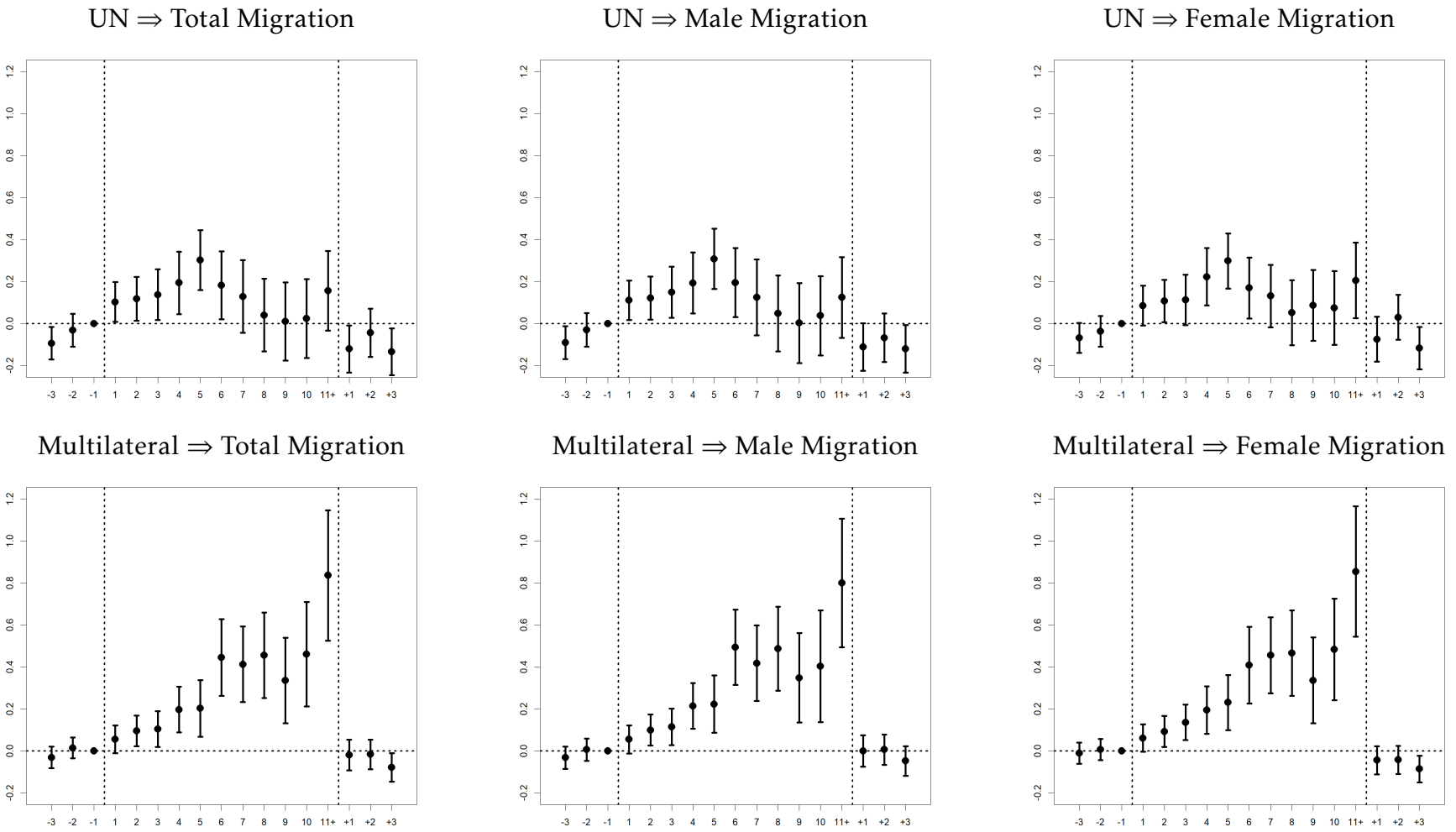
Notes: Coefficient estimates of Eq. (1) with different dependent variables. Standard errors in parentheses are clustered at the dyad level. Models include dyad fixed effects and destination-year fixed effects. ***/**/* indicates significance at the 1%/5%/10% level.

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 industrialized 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. Using the coefficient estimates for major internal conflicts with international intervention (39.6–46.0%) as a point of reference, we find that the effects of UN and Western multilateral sanctions are about half the size of large-scale civil wars. In conclusion, the migration effects of these sanctions are quantitatively relevant.

We explore the robustness of our findings in the Appendix by repeating the estimations at the combined extensive and intensive margin using an inverse hyperbolic sine transformation. The corresponding results in Table A1 are virtually identical to the baseline estimates. Next, we study migration at the intensive margin and discard all observations without migration flows. The results in Table A2 are qualitatively very similar to those in Table 1. If at all, we find larger point estimates at the intensive margin with 20.1–22.6% for UN sanctions and 24.3–26.2% for Western multilateral sanctions. In both cases, the effect on female migration is slightly (but not significantly) larger than that on male migration. The effect of non-Western sanctions on male migration is no longer significant. Hence, the intensive margin estimations further support Hypothesis 1a.

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), throughout a sanction episode (1, 2, …, 11+), and for the three years after sanctions are lifted (+1, +2, +3). Following Callaway and Sant'Anna (2021), the effect in the year before the implementation of sanctions (−1) is normalized to 0. Hence, the estimated effects of sanctions over time and the pre-/post-trend have to be interpreted relative to the final year before sanctions are implemented.

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*(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.

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 decline 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.

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 (for female migration also in the case of long-lasting sanctions, but not in year 1). The estimated effect reaches its peak of 29.8–30.9% in year 5 and declines thereafter. Western multilateral sanctions lead to a gradual increase in migrant flows throughout a sanction episode (with only the first year not being significant). The effects are particularly pronounced for very long-lasting sanctions (80.0–85.5%), but they also reach levels of 46.0–49.4% during the first ten years. While the economic effects of sanctions appear to be strongest in the first years of a sanctions episode (Gutmann, Neuenkirch, et al. 2023), 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 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.[24]

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

24. 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 year 10 should be interpreted with caution, as they are based on few observations.

countries that were subject to sanctions at some point in time is more comparable to the situation of countries in 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%) 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 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 Robustness Tests and Extensions

Robustness to Mediating Variables. Our baseline specification is conservative in that it controls for lagged GDP per capita, human rights, and democracy. Indirect effects of sanctions on migration via one of these channels are, therefore, not accounted for, while the risk of omitted variable bias from these migration determinants is reduced. We evaluate the robustness of our results towards considering the total (direct plus indirect) effects of sanctions on migration by re-estimating our baseline specification from Column (1) of Table 1 and leaving out one of the three control variables at a time.

The results in Table OA5 show that excluding the indicator for democracy (Column 4) leaves the results virtually unchanged. Excluding income per capita (Column 2) slightly reduces the point estimates for UN and multilateral sanctions. This is in line with the positive estimate for lagged GDP per capita in Column (1) and the detrimental effect of sanctions on income (Neuenkirch and Neumeier 2015; Gutmann, Neuenkirch, et al. 2023). Most striking is the change in coefficient estimates (also for conflicts) when excluding the indicator for human rights (Column 3). This suggests that the effects of sanctions on migration are possibly underestimated if we control for human rights. Nevertheless, this extension also underscores the robustness of our

results to different model specifications.

Robustness to Sanctions Objectives. The GSDB records nine types of objectives or reasons for imposing sanctions (democracy, human rights, destabilize regime, policy change, prevent war, end war, territorial conflict, terrorism, and others) based on information from official documents. Table OA6 in the Online Appendix lists the frequency of observations for each sanction objective and the different senders. It reveals some interesting patterns. Not surprisingly, ending wars is the most common objective of UN sanctions, followed by preventing war and human rights violations. Multilateral sanctions are particularly often justified with the target country's human rights situation and with supporting democracy. US and non-Western sanctions are more likely than other sanctions to aim at policy changes.

To test whether the measured sanction-induced migration is driven by sanctions imposed with a particular objective, we repeat the baseline estimations while excluding sanction cases with one objective at a time.[25] If excluding sanctions with a particular objective would fundamentally alter our estimates, this could hint at an omitted variable bias not accounted for by our fixed effects and control variables. Table OA7 in the Online Appendix shows the results of this jackknife-style robustness test.

A couple of findings seem worth highlighting. First, the effect of UN sanctions and Western multilateral sanctions remains significant throughout all specifications (and highly significant in most of them). The only exception is found when excluding sanctions that aim at ending war, as the significance of the estimates for UN sanctions is less pronounced in this case. Second, we do not find consistently significant results for other sanction senders.[26]

25. Note that sanction episodes can have multiple objectives.
26. Since this robustness test is based on nine regressions with partially overlapping samples, we refrain from interpreting estimates that are not consistently significant.

Effects of Sanctions Conditional on Income Groups. As another extension, we analyze the effects of sanctions across different country-income groups.[27] We rely on the World Bank classification and merge low and lower-middle-income countries into one group and upper-middle and high-income countries into another. Table OA8 in the Appendix shows the results of estimating Eq. (1) for each of the two resulting income groups.

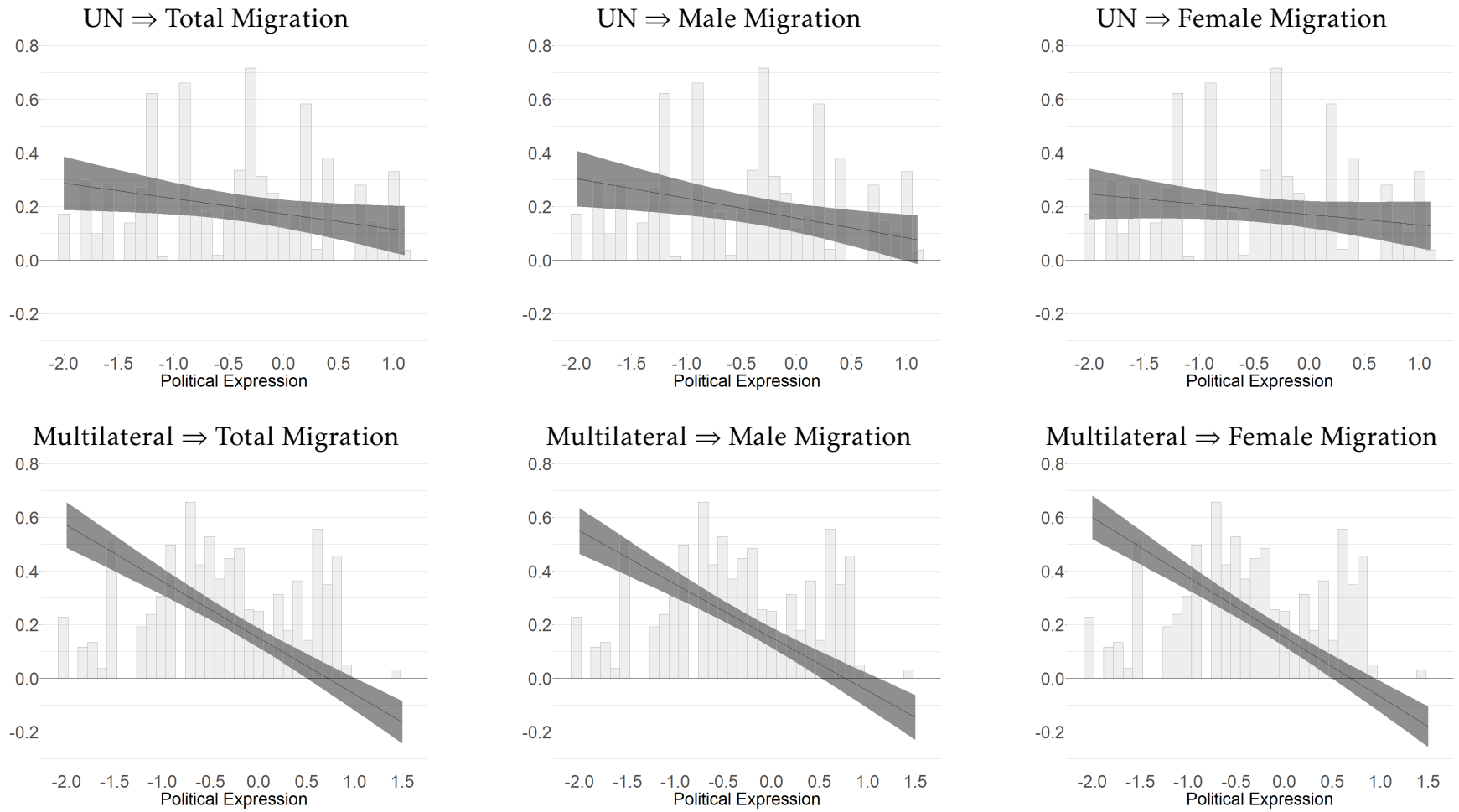
The effects of UN and Western multilateral sanctions are more or less the same across income groups. If at all, the point estimates are (slightly) larger for high-income countries, but they are also less precisely estimated. The difference in the coefficients for multilateral sanctions compared to the 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.[28] Finally, we detect some differences in the effects of unilateral US sanctions and non-Western sanctions on migrant flows from low-income countries (negative) and high-income countries (positive). There is no apparent theoretical justification for these subsample effects, which cancel each other out in the full sample. While it is plausible that sanctions suppress emigration only in low-income countries by depriving regular citizens of the resources needed for migration – especially to the Global North – it is unclear why that would not equally apply to multilateral and UN sanctions. Yet, these more powerful sanctions have virtually the same effect in low- and high-income countries.

Effects of Sanctions Conditional on Freedom of Political Expression. To test Hypothesis 3, we extend Eq. (1) with a variable measuring freedom of political expression and two interactions of this variable with the indicators for UN sanctions and Western multilateral sanctions.

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

28. When we restrict the sample for the baseline estimations in Table 1 to start in 1987, the effect of Western multilateral sanctions shrinks as well.

Figure 2: Effects of UN and Western Multilateral Sanctions Conditional on Freedom of Political Expression (Log + 1)



Notes: Effects of sanctions conditional on freedom of political expression 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*(GDP pc), Polity2, Human Rights, Freedom of Movement, Political Expression, 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 political expression under UN and Western multilateral sanctions. See also Table OA9 in the Online Appendix.

Table OA9 in the Online Appendix shows the results. Figure 2 provides a straightforward visualization of the marginal effects of sanctions on international migration, conditional on the level of freedom of political expression in the target country. For both interactions, the estimated effects are visualized over the whole range of observed freedom of political expression 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 freedom of political expression, which is underlined by the non-significance of the negative interaction terms (see Table OA9 in the Online Appendix). In contrast, the effect of Western multilateral sanctions is strongly moderated and shrinks by 20–22% with each additional standard deviation in the indicator for freedom of political expression.[29] Hence, we find, in line with Hypothesis 3, that increased migration (i.e., exit) in response to sanctions only occurs if freedom of political expression is limited (i.e., voice).

Target-Sender Migration Flows. As a final extension, we estimate Eq. (1) with added origin-year fixed effects and study migration flows specifically from target to sender countries. Our sanctions indicators are now recoded to take the value 1 only for dyad-years from a sanction target to a sanction sender.[30] In this research design, effects of UN sanctions cannot be estimated, as all countries are bound by UN sanctions and the corresponding sanctions indicator would be absorbed by the origin-year fixed effects. Chinese and Russian (i.e., non-Western) sanctions also cannot be studied here because our dataset does not cover bilateral migration flows to these countries.

We are now estimating whether sanctions lead to increased migration from the target to the sender country, which is not our research question but addresses an interesting complementary question. For the target country, the destination of their em-

29. A significant negative effect of Western multilateral sanctions on migration is estimated for freedom of political expression larger than 0.94–1.11, that is, for less than 0.5% of the observations under Western multilateral sanctions. Thus, it seems more plausible to assume a null effect on migration for sanction targets with high freedom of political expression.

30. Note that conventional sanctions indicators, as they are used throughout the sanctions literature and as they are used in the rest of the paper, would be absorbed by the origin-year fixed effects. In other words, once we include origin-year fixed effects, we can only compare migration flows from the target country to different destination countries with each other.

igrating citizens will generally not matter much. But it may be of great relevance to the sanction sender, if sanctioning a country may cause a wave of migrants from that country to arrive at its border. In other words, we are testing the argument referred to in footnote 5, but based on realized migration flows and not focused on the political decision to impose sanctions given expectations about migration flows. Finally, it is important to stress that our estimates do not indicate whether migrant flows from sanction target to sanction sender change, but whether they change relative to migrant flows to other (non-sender) destination countries. This is because flows from the target to other countries than the sender are now due to the use of "dyadic sanctions" part of the counterfactual.

The results in Table OA10 indicate that multilateral Western and unilateral EU sanctions lead to more migration to the respective sender countries than to other industrialized countries. This implies that particularly EU countries have to bear the costs of their sanctions in terms of increased immigration pressure, whereas the United States do not pay the same price.

5 Conclusion

We conduct the first statistical analysis of how sanctions affect international migrant 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. Emigration flows increase, on average, by 17–18% under UN sanctions and by 22–24% under Western multilateral sanctions. Our event study results of Western multilateral sanctions imply a gradual increase in emigration throughout 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 persis-

tent. 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 demonstrating that women are disproportionately affected by sanctions (Demir and Tabrizy 2022; Gutmann et al. 2021) and migration is partially driven by gender-specific incentives (Gutmann, Marchal, et al. 2023; Neumayer and Plümper 2021; Ruyssen and Salomone 2018).

The positive migration effect, particularly that for Western multilateral sanctions, is driven by countries with limited freedom of political expression. 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 claiming 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. 2024; 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.

Descriptive statistics support that countries under sanctions tend to reduce their citizens' freedom of foreign movement. Figure OA3 in the Online Appendix shows that during the second to fifth year of a sanctions episode roughly 7–8% of the countries impose restrictions on the freedom to emigrate. These governments must feel harmed or at least threatened by emigration. Cuba's requirement for a special permit before medical doctors can travel abroad, introduced in 2015, is an example of such a restriction. Cuba's government complained at the time that its health services were seriously affected by the emigration of doctors and it blamed the US for encouraging emigration from Cuba for political reasons.[31] Its medical sector earns Cuba much

31. https://www.bbc.com/news/world-latin-america-34979512.

needed foreign currency after it has endured decades of US economic sanctions. Nevertheless, omitting the migration regulation indicator from our model specification does not change our estimated emigration effect of sanctions, which suggests that while sanctioned governments might want to limit emigration, we do not find evidence that they are successful in this effort (see Table OA11 in the Online Appendix).

For sanction senders, our results imply a potential unintended consequence of using sanctions to settle international disputes. Sanctions are not only potentially harmful to the sender country's economy, but they can also trigger international migration waves that may cause additional economic and political costs for the sender (see also Connell et al. 2021).

An important limitation of cross-country research on migration is the availability of bilateral migration data for only a limited number of destination countries. Therefore, our results are reflective of migration to industrialized 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, necessary 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.

Aihounton, G. B. D., and A. Henningsen. 2020. "Units of measurement and the inverse hyperbolic sine transformation." *The Econometrics Journal* 24 (2): 334–351.

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.

Bartlett, M. S. 1947. "The use of transformations." *Biometrics* 3 (1): 39–52.

Beine, M., P. Bourgeon, and J.-C. Bricongne. 2019. "Aggregate fluctuations and international migration." *The Scandinavian Journal of Economics* 121 (1): 117–152.

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.

Chen, J., and J. Roth. 2024. "Logs with zeros? Some problems and solutions." *The Quarterly Journal of Economics* 139 (2): 891–936.

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. I. Lindberg, J. Teorell, D. Altman, F. Angiolillo, et al. 2024. "Varieties of Democracy (V-Dem) Project," www.v-dem.net/about/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.

Cucu, F., and L. Panon. 2023. "Asylum policies and international tensions." *European Economic Review* 151:104322.

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.

Giménez-Gómez, J.-M., Y. M. Walle, and Y. Z. Zergawu. 2019. "Trends in African migration to Europe: Drivers beyond economic motivations." *Journal of Conflict Resolution* 63 (8): 1797–1831.

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. 2024. "To Russia with love? The impact of sanctions on regime support." *CESifo Working Paper* 11033.

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., L. Marchal, and B. Simsek. 2023. "Women's rights and the gender migration gap." *CESifo Working Paper* 10222.

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.

———. 2023. "The economic effects of international sanctions: An event study." *Journal of Comparative Economics* 51 (4): 1214–1231.

———. 2024. "Do China and Russia undermine western sanctions? Evidence from DiD and event study estimation." *Review of International Economics* 32 (1): 132–160.

Hatipoglu, E., and D. Peksen. 2018. "Economic sanctions and banking crises in target economies." *Defence and Peace Economics* 29 (2): 171–189.

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.

———. 2023. "Pressures from home and abroad: Economic sanctions and target government response to domestic campaigns." *Journal of Conflict Resolution* 67:297–325.

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, 1800-2018." 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.

Neumayer, E., and T. Plümper. 2021. "Women's economic rights in developing countries and the gender gap in migration to Germany." *IZA Journal of Development and Migration* 12 (1): 13.

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.

Peksen, D., and B. Son. 2015. "Economic coercion and currency crises in target countries." *Journal of Peace Research* 52 (4): 448–462.

Portela, C., and A. Charron. 2023. "The evolution of databases in the age of targeted sanctions." *International Studies Review* 25 (1): viac061.

Ruyssen, I., and S. Salomone. 2018. "Female migration: A way out of discrimination?" *Journal of Development Economics* 130:224–241.

Schmidheiny, K., and S. Siegloch. 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.

Shchepeleva, M., M. Stolbov, and L. Weill. 2024. "Do sanctions trigger financial crises?" *Finance Research Letters* 64:105467.

Syropoulos, C., G. J. Felbermayr, A. Kirilakha, E. Yalcin, and Y. V. Yotov. 2024. "The global sanctions data base–release 3: Covid-19, Russia, and multilateral sanctions." *Review of International Economics* 32 (1): 12–48.

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 (Inverse Hyperbolic Sine Transformation)

	(1) Total Migration	(2) Male Migration	(3) Female Migration
Sanctions			
… UN	0.191***	0.185***	0.175***
	(0.053)	(0.055)	(0.052)
… Multilateral (EU-US)	0.228***	0.227***	0.240***
	(0.043)	(0.044)	(0.044)
… EU only	0.056	0.052	0.054
	(0.037)	(0.038)	(0.037)
… US only	–0.006	0.005	–0.009
	(0.026)	(0.026)	(0.025)
… Non-Western	0.023	0.070**	0.001
	(0.029)	(0.029)	(0.029)
Observations	79,791	79,791	79,791
R^2	0.937	0.926	0.934
Within-R^2	0.018	0.016	0.018

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*(GDP pc), Polity2, Human Rights, Freedom of Movement, and six conflict indicators; coefficient estimates are available on request), dyad fixed effects, and destination-year fixed effects. ***/**/* indicates significance at the 1%/5%/10% level.

Table A2: Baseline Results (Log)

	(1) Total Migration	(2) Male Migration	(3) Female Migration
Sanctions			
… UN	0.201***	0.203***	0.226***
	(0.057)	(0.060)	(0.058)
… Multilateral (EU-US)	0.243***	0.247***	0.262***
	(0.046)	(0.048)	(0.050)
… EU only	0.049	0.045	0.059
	(0.039)	(0.040)	(0.040)
… US only	–0.004	0.006	–0.012
	(0.026)	(0.028)	(0.026)
… Non-Western	0.008	0.040	–0.022
	(0.027)	(0.029)	(0.028)
Observations	72,267	69,106	68,527
R^2	0.931	0.918	0.926
Within-R^2	0.022	0.020	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*(GDP pc), Polity2, Human Rights, Freedom of Movement, and six conflict indicators; coefficient estimates are available on request), dyad fixed effects, and destination-year fixed effects. ***/**/* indicates significance at the 1%/5%/10% level.

Online Appendix

Data Description

Table OA1: List of Countries

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**
Total Migration / Male Migration / Female Migration	Natural logarithm [$log(y+1)$ & $log(y)$] and inverse hyperbolic sine [$log(y+\sqrt{y^2+1})$] of total / male / female bilateral migration. *Source:* OECD International Migration Database (OECD 2020), DEMIG C2C dataset (DEMIG 2015).
Sanctions	Binary indicators for country-years with sanctions in place. *Source:* GSDB (Felbermayr et al. 2020; Kirikakha et al. 2021; Syropoulos et al. 2024).
lagged log(GDP pc)	Natural logarithm of real GDP per capita in 2015 USD, lagged by one year. *Source:* World Development Indicators (World Bank 2023).
Polity2	Democracy indicator that ranges from strongly democratic (+10) to strongly autocratic (–10). *Source:* Polity5 dataset (Marshall and Gurr 2020).
Human Rights	Latent human rights variable with higher values indicating a better protection of human rights. *Source:* 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. *Source:* V-Dem, version 14 (Coppedge et al. 2024).
Political Expression	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. *Source:* V-Dem, version 14 (Coppedge et al. 2024).
Minor Conflict / Major Conflict	Armed conflicts resulting in 25 to 999 / at least 1,000 battle-related deaths in a given year. *Source:* UCDP/PRIO Armed Conflict Dataset (Gleditsch et al. 2002; Davies et al. 2022).
Interstate Conflict	Conflicts between two states. *Source:* 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 rebel groups with / without military intervention by foreign governments. *Source:* UCDP/PRIO Armed Conflict Dataset (Gleditsch et al. 2002; Davies et al. 2022).

Table OA3: Descriptive Statistics

	All Observations		**No Sanctions**		**Sanctions**	
	Mean	N	Mean	N	Mean	N
Total Migration						
$\ldots log(\ldots + 1)$	4.06	79,791	3.87	55,701	4.49	24,090
$\ldots log(\ldots)$	4.39	72,267	4.24	49,719	4.72	22,548
$\ldots ihs(\ldots)$	4.65	79,791	4.45	55,701	5.11	24,090
Male Migration						
$\ldots log(\ldots + 1)$	3.49	79,791	3.31	55,701	3.90	24,090
$\ldots log(\ldots)$	3.92	69,106	3.78	47,249	4.21	21,857
$\ldots ihs(\ldots)$	4.05	79,791	3.85	55,701	4.49	24,090
Female Migration						
$\ldots log(\ldots + 1)$	3.38	79,791	3.22	55,701	3.76	24,090
$\ldots log(\ldots)$	3.81	68,527	3.68	47,015	4.10	21,512
$\ldots ihs(\ldots)$	3.93	79,791	3.75	55,701	4.33	24,090
lagged log(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
Political Expression	0.39	79,791	0.47	55,701	0.18	24,090
	$X = 1$	N	$X = 1$	N	$X = 1$	N
Freedom of Movement	74,719	79,791	53,564	55,701	21,155	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

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-US joint	4,480	… High Income	10,265
… EU only	2,034		
… US only	8,972		
… 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.

Additional Results

Table OA5: Results without Mediating Variables (Log + 1)

	(1) Baseline Results	(2) Excl. GDP	(3) Excl. Human Rights	(4) Excl. Democracy
Sanctions				
… UN	0.177***	0.135***	0.222***	0.176***
	(0.052)	(0.050)	(0.053)	(0.052)
… Multilateral (EU-US)	0.228***	0.216***	0.259***	0.214***
	(0.042)	(0.043)	(0.042)	(0.042)
… EU only	0.053	0.053	0.076**	0.064*
	(0.035)	(0.035)	(0.035)	(0.035)
… US only	–0.005	–0.017	0.012	–0.010
	(0.025)	(0.026)	(0.025)	(0.025)
… Non-Western	0.034	0.008	0.018	0.024
	(0.028)	(0.030)	(0.028)	(0.027)
lagged *log*(GDP pc)	0.304***		0.274***	0.293***
	(0.070)		(0.070)	(0.070)
Polity2	0.009***	0.007**	0.005	
	(0.003)	(0.003)	(0.003)	
Human Rights	–0.123***	–0.111***		–0.114***
	(0.016)	(0.016)		(0.016)
Freedom of Movement	–0.031	0.007	–0.053	–0.004
	(0.055)	(0.060)	(0.055)	(0.056)
Interstate Conflicts				
… Minor	0.020	–0.014	0.043	0.016
	(0.063)	(0.071)	(0.064)	(0.063)
… Major	–0.203*	–0.204*	–0.181	–0.196*
	(0.110)	(0.110)	(0.110)	(0.110)
Internal Conflicts w/o Intervention				
… Minor	0.024	0.015	0.085***	0.028
	(0.023)	(0.024)	(0.023)	(0.023)
… Major	–0.010	–0.023	0.097***	–0.006
	(0.038)	(0.037)	(0.036)	(0.038)
Internal Conflicts w/ Intervention				
… Minor	0.007	0.008	0.109**	0.015
	(0.045)	(0.045)	(0.044)	(0.045)
… Major	0.417***	0.395***	0.519***	0.429***
	(0.077)	(0.076)	(0.076)	(0.077)
Observations	79,791	79,791	79,791	79,791
R^2	0.940	0.940	0.940	0.942
Within-R^2	0.020	0.014	0.014	0.019

Notes: Coefficient estimates of Eq. (1) for total migration and different sets of control 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. Column (1) replicates the baseline results from Table 1.

Table OA6: Frequency of Sanction Objectives

Objective	**UN**	**Multilateral (EU-US)**	**EU only**	**US only**	**Non-Western**
Democracy	218	1,635	456	1,568	326
Human Rights	1,117	2,363	486	1,862	370
Destabilizing Regime	0	14	0	853	30
Policy Change	302	332	376	1,169	1,007
Prevent War	1,009	206	0	306	0
End War	1,980	330	133	331	0
Territorial Conflict	129	0	0	461	154
Terrorism	501	845	196	918	0
Other	62	270	593	2,775	765

Notes: Frequency of observation of the different sanction objectives by sender in the full sample with all control variables (see Table OA3). Total number of observations in the dataset: 79,791. Sanction episodes can have multiple objectives.

Table OA7: Results excluding Sanction Objectives (Log + 1)

	Excl. "Democracy"	**Excl. "Human Rights"**	**Excl. "Destab. Regime"**	**Excl. "Policy Change"**	**Excl. "Prevent War"**
… UN	0.159***	0.160***	0.165***	0.222***	0.207***
	(0.057)	(0.061)	(0.0518)	(0.052)	(0.057)
… Multilateral (EU-US)	0.441***	0.187***	0.193***	0.262***	0.180***
	(0.063)	(0.046)	(0.036)	(0.046)	(0.040)
… EU only	0.153***	0.036	0.044	0.058	0.061*
	(0.039)	(0.042)	(0.035)	(0.036)	(0.035)
… US only	–0.053*	–0.026	0.007	–0.018	0.012
	(0.029)	(0.027)	(0.024)	(0.026)	(0.026)
… Non-Western	0.049*	0.053**	0.049*	0.065**	0.046*
	(0.028)	(0.027)	(0.026)	(0.031)	(0.027)
Observations	75,716	73,721	78,924	76,938	78,270
R^2	0.941	0.943	0.941	0.941	0.941
Within-R^2	0.023	0.015	0.018	0.021	0.018

	Excl. "End War"	**Excl. "Territ. Conflict"**	**Excl. "Terrorism"**	**Excl. "Other Object."**
… UN	0.134**	0.188***	0.296***	0.190***
	(0.063)	(0.054)	(0.067)	(0.053)
… Multilateral (EU-US)	0.185***	0.227***	0.147***	0.253***
	(0.040)	(0.042)	(0.041)	(0.046)
… EU only	0.086**	0.054	0.044	0.050
	(0.034)	(0.035)	(0.037)	(0.047)
… US only	0.011	0.005	–0.012	0.010
	(0.026)	(0.025)	(0.026)	(0.031)
… Non-Western	0.046*	0.037	0.050*	0.029
	(0.027)	(0.028)	(0.028)	(0.030)
Observations	77,017	79,170	77,331	75,536
R^2	0.943	0.941	0.941	0.939
Within-R^2	0.018	0.020	0.019	0.021

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

Table OA8: Results for Low- and High-Income Countries (Log + 1)

	Low-Income Countries	**High-Income Countries**
Sanctions		
… UN	0.182***	0.198*
	(0.043)	(0.114)
… Multilateral (EU-US)	0.112***	0.151***
	(0.033)	(0.048)
… EU only	0.037	0.067
	(0.039)	(0.053)
… US only	–0.041*	0.112**
	(0.024)	(0.046)
… Non-Western	–0.132**	0.104***
	(0.055)	(0.028)
Observations	37,526	34,290
R^2	0.949	0.956
Within-R^2	0.022	0.008

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

Table OA9: Results including Political Expression (Log + 1)

	(1) Total Migration	(2) Male Migration	(3) Female Migration
Sanctions			
… UN	0.173***	0.157***	0.170***
	(0.053)	(0.054)	(0.052)
… Multilateral (EU-US)	0.151***	0.152***	0.154***
	(0.040)	(0.039)	(0.041)
… EU only	0.055	0.051	0.055
	(0.035)	(0.036)	(0.035)
… US only	0.002	0.011	–0.003
	(0.025)	(0.025)	(0.023)
… Non-Western	0.039	0.081***	0.017
	(0.028)	(0.028)	(0.027)
Political Expression	0.003	0.005	–0.013
	(0.034)	(0.034)	(0.033)
UN × Political Expression	–0.057	–0.074	–0.038
	(0.048)	(0.049)	(0.047)
Multilateral × Political Expression	–0.210***	–0.199***	–0.223***
	(0.057)	(0.059)	(0.056)
Observations	79,791	79,791	79,791
R^2	0.940	0.930	0.939
Within-R^2	0.021	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*(GDP pc), Polity2, Human Rights, Freedom of Movement, and six conflict indicators; coefficient estimates are available on request), dyad fixed effects, and destination-year fixed effects. ***/**/* indicates significance at the 1%/5%/10% level.

Table OA10: Results using Dyadic Sanctions Measures (Log +1)

	Total Migration
Sanctions	
… Multilateral (EU-US) – Dyadic Measure	0.138**
	(0.054)
… EU only – Dyadic Measure	0.133***
	(0.042)
… US only – Dyadic Measure	–0.110
	(0.071)
Observations	79,791
R^2	0.944
Within-R^2	0.003

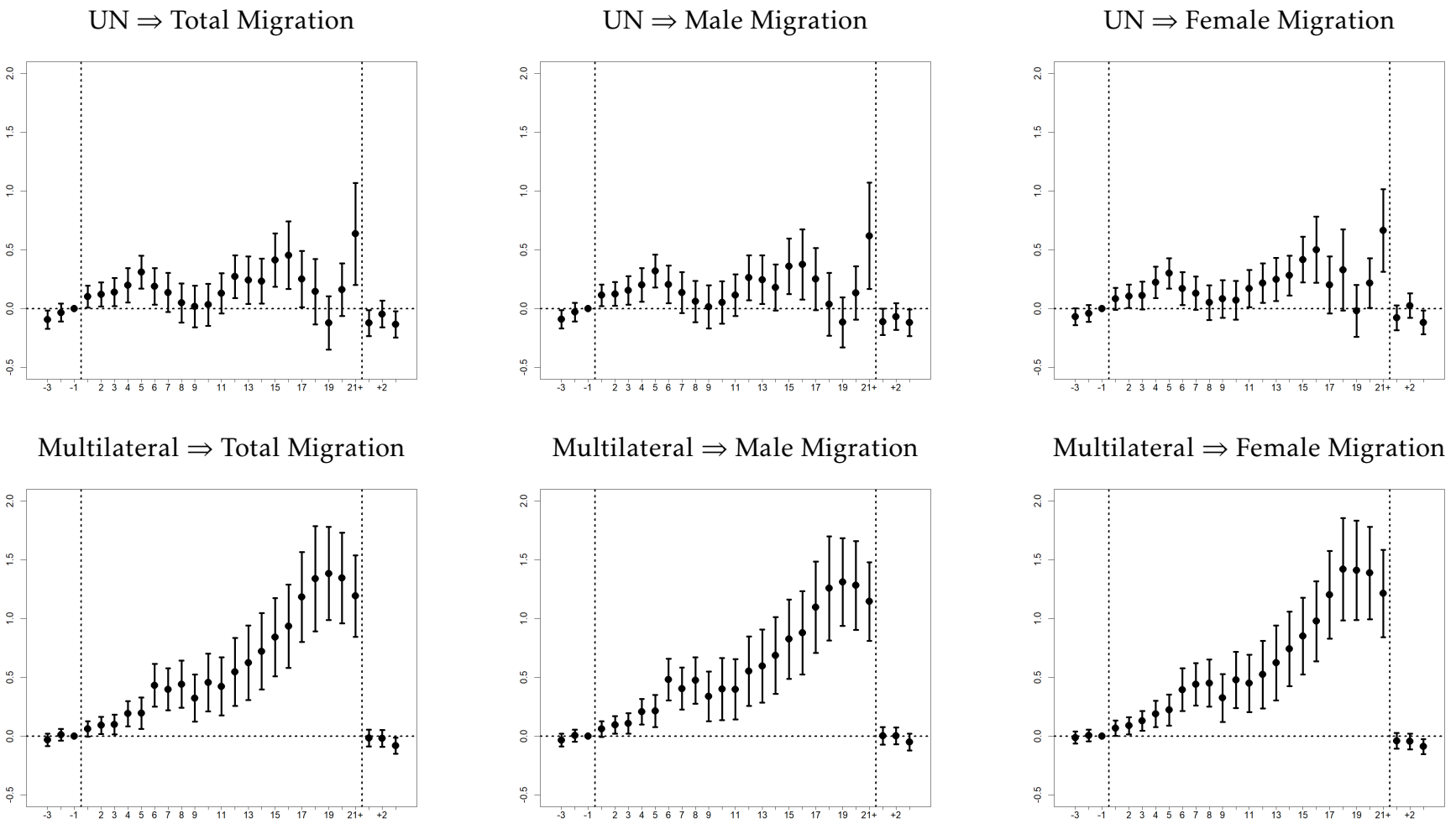
Notes: Coefficient estimates of a modified version of Eq. (1). Standard errors in parentheses are clustered at the dyad level. Models include dyad fixed effects, destination-year fixed effects, and origin-year fixed effects. ***/**/* indicates significance at the 1%/5%/10% level.

Table OA11: Results excluding Freedom of Movement (Log + 1)

	(1) Baseline Results	(2) Excl. Freedom of Movement
Sanctions		
... UN	0.177***	0.179***
	(0.052)	(0.052)
... Multilateral (EU-US)	0.228***	0.231***
	(0.042)	(0.041)
... EU only	0.053	0.055
	(0.035)	(0.035)
... US only	–0.005	-0.004
	(0.025)	(0.025)
... Non-Western	0.034	0.034
	(0.028)	(0.028)
Observations	79,791	79,791
R^2	0.940	0.940
Within-R^2	0.020	0.020

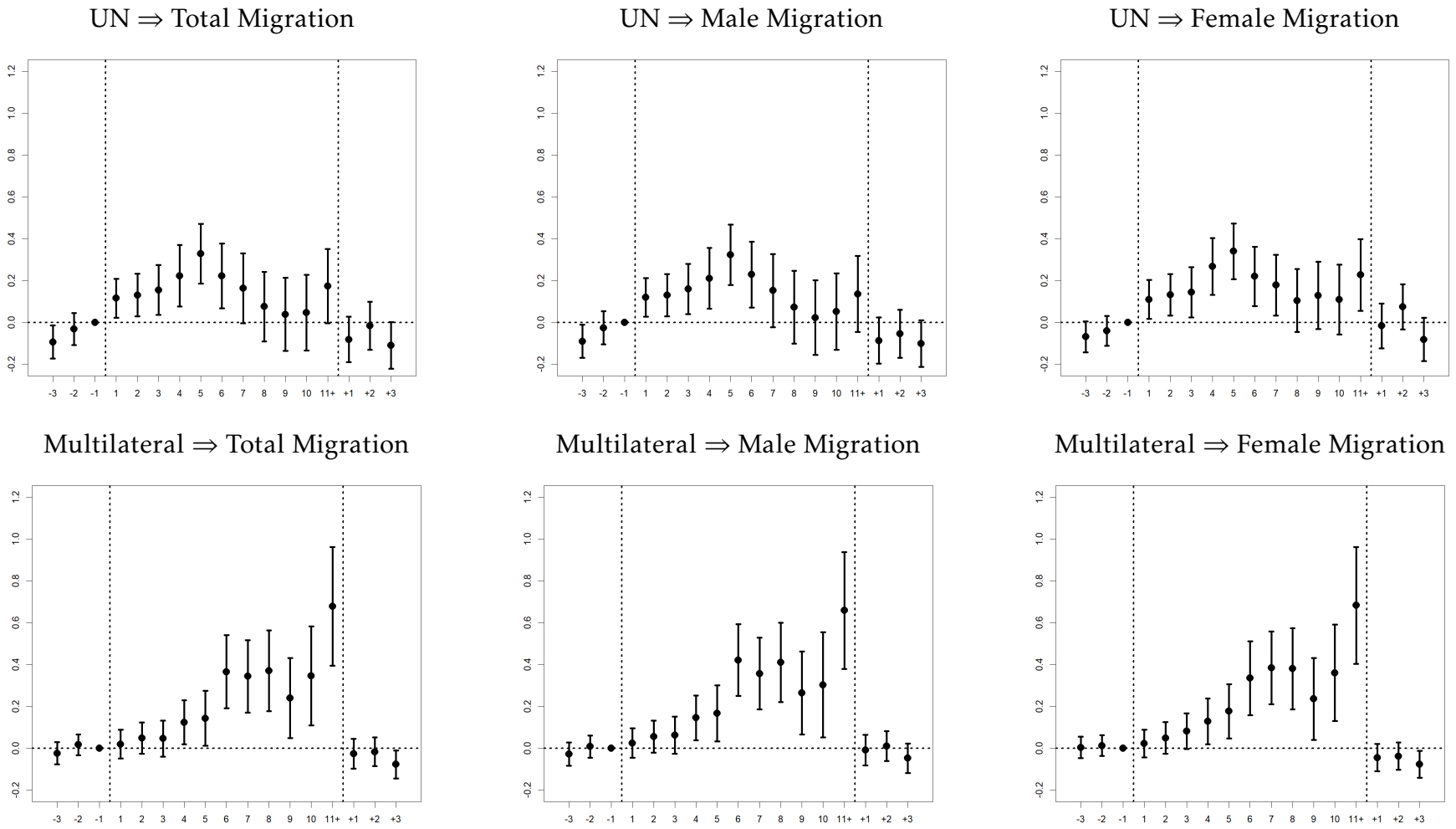
Notes: Coefficient estimates of Eq. (1) for total migration and different sets of control variables. Standard errors in parentheses are clustered at the dyad level. Models include control variables (lagged *log*(GDP pc), Polity2, Human Rights, Freedom of Movement, and six conflict indicators; coefficient estimates are available on request), dyad fixed effects, and destination-year fixed effects. ***/**/* indicates significance at the 1%/5%/10% level. Column (1) replicates the baseline results from Table 1 in the paper.

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



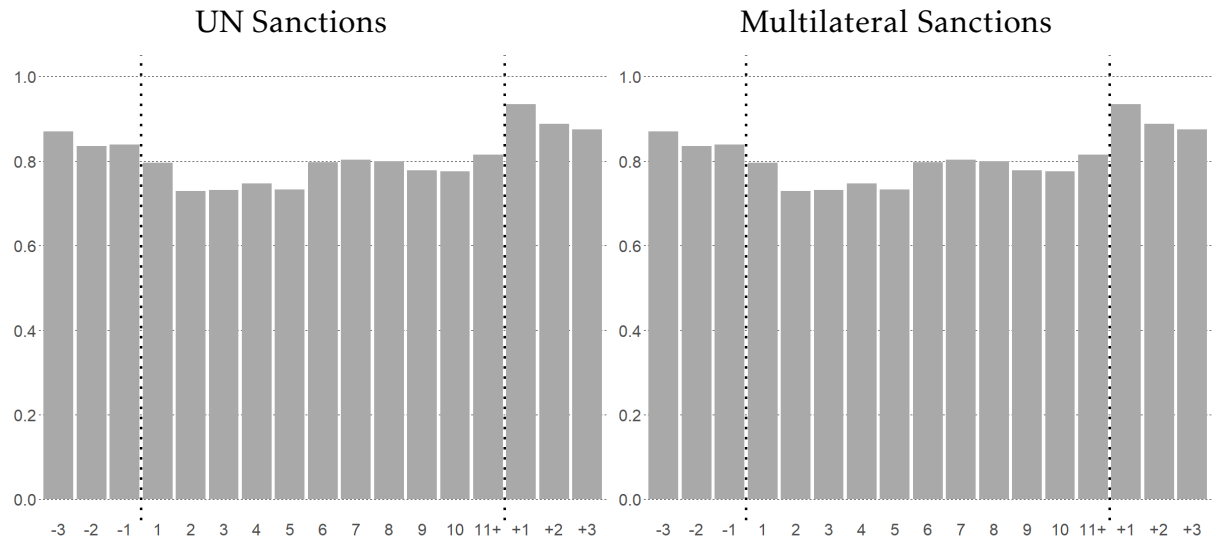
Notes: Effects of sanctions over time (1, 2, …, 21+) alongside pre-trend (−3, −2, −1) and post-trend (+1, +2, +3) according to an estimation of a modified version of Eq. (2) for different dependent variables. Standard errors are clustered at the dyad level. Models include control variables (other sanction indicators, lagged *log*(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*(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 OA3: Frequency of Freedom of Movement



Notes: Bar charts show the distribution of the freedom of foreign movement variable throughout a sanctions episode (1, 2, …, 11+) alongside pre-trend (−3, −2, −1) and post-trend (+1, +2, +3).