Refugee Return

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Abstract

Despite record levels of forced displacement, we know little about whether, when, and where refugees return. A key limitation is measurement, as existing studies typically observe either stated return intentions or realized return, but not both for the same individuals. We address this gap with a ten-wave longitudinal survey of Ukrainian refugees, following respondents from the early months of displacement through 2025 in Europe. We link respondents' prewar home municipalities to geocoded conflict and territorial-control data, elicit beliefs about the war, and observe precise return locations within Ukraine. We document three key findings: first, return intentions strongly predict subsequent behavior, with 42 percent of respondents who planned to return soon in 2022 having returned by 2025, compared with only 1 percent of those planning to settle abroad. Second, exploiting within-person variation in local conflict exposure across survey waves, we find that higher conflict intensity reduces return to the home municipality by about 20 percent of the mean. Third, conflict reduces realized return to the home municipality, but it does not make refugees more likely to plan permanent settlement abroad. Instead, the overall decline in return plans is partly explained by increasingly pessimistic expectations about the war.

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1 Introduction

The global refugee population surged from 27 million in 2021 to 35 million by the end of 2022, the largest annual increase ever recorded.[1] A central policy and research question in such crises is whether, when, and where displaced people return. These margins shape origin-country reconstruction and host-country integration and fiscal planning (Choudhury, 2016; Marbach et al., 2018; Görlach and Motz, 2021; Bahar et al., 2024; Mayda et al., 2022; Di Maio and Leone Sciabolazza, 2023; Fasani et al., 2024).[2] Yet systematic evidence on refugee return dynamics, including how refugees form return plans, when they act on them, and where they ultimately return, remains scarce.

The literature faces three main challenges. First, measurement: previous studies observe either refugees' stated return intentions or realized return, but not both for the same individuals, so the predictive content of intentions cannot be assessed and intention-based and realized-return findings cannot be reconciled. Second, identification: conflict evolves rapidly and unevenly across space, while migration and return decisions are endogenous and likely correlated with unobserved individual characteristics, so cross-sectional comparisons confound the effect of conflict with selection into who flees.[3] Third, expectations: data rarely capture individuals' beliefs about the trajectory of the conflict or conditions upon return, so studies cannot separate the role of these expectations from that of observable constraints such as economic conditions.

We address these issues in the context of the largest displacement in Europe since the Second World War, following Russia's full-scale invasion of Ukraine in February 2022. We launched a 10-wave longitudinal survey of Ukrainian refugees between June 2022 and May 2025. Our main analysis follows 2,484 individuals who reported their home municipality and answered at least one follow-up wave, with 3.5 consecutive interviews per respondent on average. The panel structure allows us to link stated return intentions to realized mobility for the same individuals, including where they return within Ukraine, and to relate changes in return behavior to changing conditions at origin. The setting is well-suited to studying voluntary return because

[1] Additionally, there were 5.4 million asylum seekers and 62.5 million internally displaced persons in 2022. For a comprehensive global overview, see UNHCR (2023).

[2] Reconstruction needs in Ukraine alone are estimated at over $500 billion, and their returns hinge on whether the displaced come back to the places being rebuilt (World Bank et al., 2025).

[3] For example, in high-conflict areas, people can be forced to flee even when they have strong local attachment and high return intentions. In lower-conflict areas, those who leave may be disproportionately individuals with weaker attachment and lower baseline return intentions. Longitudinal data address this selection problem by following the same individuals over time. Moves are observed directly, and outcomes are compared within a person before and after changes in conflict exposure, reducing bias from changes in the composition of who remains.

the Temporary Protection Directive of the European Union (EU) grants Ukrainian refugees immediate residence and work rights in any EU member state, reducing potential confounding arising from uncertainty about legal status in the host country.

We begin by documenting three descriptive facts that highlight the value of observing both stated plans and realized mobility in a longitudinal survey. First, return during an ongoing war is sizable. Using population weights, we estimate that 11% of respondents had returned to Ukraine by the end of our panel, and that 86% of returnees went back to their home municipality. These returns also appear durable. Among respondents observed in a later wave after first returning, about 81% were still in Ukraine at their final interview. Second, early return intentions have substantial predictive content. Respondents who initially said they planned to return soon were far more likely to have returned (42%) by the end of the panel than those who initially planned to settle outside Ukraine (only 1%). This intention-realization gradient validates return plans as informative measures of future mobility.[4] Third, return intentions evolve meaningfully over time. The share of respondents planning to return soon or once it was safe remained broadly stable during the first year, but then declined steadily, by 3.5 percentage points per 100 days.[5] This pattern shows that return plans are not static survey responses. They are updated as the war continues and as refugees receive new information about conditions in Ukraine and opportunities abroad.

We then estimate how local conflict exposure affects realized return. The analysis exploits within-person changes in conflict exposure between survey waves and separates two dimensions of conflict, intensity and territorial control. To avoid endogeneity from the exact day on which respondents choose to answer the survey, we assign conflict exposure using the starting date of each wave rather than individual response dates. The identifying assumption is that, conditional on baseline return intentions, individual characteristics, survey location, prior conflict exposure, and survey-wave timing, changes in local conflict at origin are unrelated to other time-varying determinants of return. Our main result is that higher conflict intensity in the home municipality reduces the probability that refugees return to that municipality. A one standard deviation increase in conflict intensity lowers medium-run return to the home municipality by 1.5 percent-

[4] At the same time, the panel shows why intentions should be interpreted as probabilities rather than deterministic forecasts. Many respondents who initially planned to return once it was safe had not yet returned by 2025, consistent with the continued role of security, housing, family, and labor-market constraints in delaying intended returns.

[5] About one third of this decline reflects respondents who had already returned, about half reflects respondents who had shifted toward planning to settle outside Ukraine, and the remainder reflects a rise in the share reporting that they did not know.

age points, equal to 20% of the mean. The effect appears in both the short- and medium-run specifications and is concentrated among refugees from reachable, largely Ukrainian-controlled municipalities in the north, west, and center of the country. This pattern is difficult to reconcile with a purely mechanical interpretation based on occupation or ongoing fighting. Even where return is feasible, refugees are less likely to return to municipalities that experienced greater local violence.

For aggregate return to Ukraine, we estimate a statistically insignificant effect. We interpret this as a bounded null, not as evidence of a precisely estimated zero effect. The reason is that aggregate return is a coarse outcome as it combines returns to the home municipality with returns to other parts of Ukraine. These two margins may move in different directions, making the aggregate-return coefficient less precise and less directly linked to conflict conditions at origin. Our data are better suited to study return to the home municipality, where realized power exceeds 0.95 and exact return locations allow us to match each return decision to conflict in the refugee's place of origin. This is the key empirical advantage of our survey. By observing where refugees return within Ukraine, we can detect a local conflict response that would be missed in data measuring only whether they returned to the country.

We further find that the sharp decline in return intentions over the course of the war closely moves with refugees' expectations about how and when the war will end. The share expecting Ukraine to win and fully liberate all occupied territories by the end of 2024 fell from 71% in late 2022 to 35% in late 2023. Individuals who revise these expectations downward become substantially less likely to plan to return and more likely to plan to settle abroad. In a descriptive decomposition, shifting war expectations account for about 21% of the within-sample decline in return intentions.

To assess the main threats to interpretation, we report a set of robustness checks. We begin by validating the link between stated intentions and realized return. Respondents with stronger initial return plans are more likely to return when compared over common follow-up windows, such as 12, 18, and 24 months after baseline, which reduces the concern that the intention-return gradient simply reflects differences in exposure time in the panel. This relationship also remains strong after controlling for demographics, family location, host country, remote work in Ukraine, timing of departure, and prior conflict exposure. Thus, stated intentions contain predictive information beyond observable factors that are themselves associated with return.

We then examine whether selection and attrition affect the results. We report predictors of

follow-up response and inclusion in each analysis sample, and show directly that realized return does not generate large mechanical panel exit: being in Ukraine in a given wave lowers the probability of responding to the next wave by only 3 to 4 percentage points, too small to drive the intention gradient. We re-estimate the main realized-return, return-intention, and expectations specifications using inverse-probability and population weights, and bound descriptive return rates with Manski-type bounds under alternative assumptions about attriters. These checks show that attrition affects the level of observed return rates, but not the central conflict result (that is, local conflict reduces return to the home municipality). The home-municipality effect is stable as we sequentially add survey-timing fixed effects, individual controls, and prior conflict exposure, and it remains, indeed strengthens, when we include home-municipality fixed effects. We further gauge sensitivity to unobserved selection using the diagnostic of Oster (2019), to spatial dependence using Colella et al. (2023) standard errors, and to statistical power using minimum-detectable-effect calculations. The home-return result is also robust to alternative conflict measures, broader exposure definitions, alternative functional forms, and excluding respondents from long-occupied territories and the easternmost regions. Finally, because liberation may induce the most return-prone respondents to exit the intentions sample, we re-estimate the intention specifications including returnees, which recovers a medium-run liberation effect consistent with the realized-return results.

Related literature and contributions. As displacement reaches record levels, the central questions facing origin governments, host states, and international agencies are no longer only how many people flee, but whether, when, and where they return. Reconstruction sequencing, host-country fiscal and integration planning, and the design of voluntary-return programs all hinge on these answers (Alrababah and Casalis, 2024). Yet the evidence base rests largely on stated intentions whose behavioral content is unverified, and on aggregate return counts that reveal nothing about which places people return to or why. We close both gaps with a real-time, geographically precise panel of the largest refugee population in Europe since the Second World War, and make three contributions.

First, we provide the first within-person validation of refugee return intentions against geographically precise realized return during active conflict. The interpretation of return intentions is a first-order measurement problem: policy is built on them, but studies of refugee return typically observe either stated intentions or realized return, not both for the same individuals, leaving their behavioral content untested (Alrababah and Casalis, 2024). This matters more for

refugees than for economic migrants. While economic migrants return at high rates as the culmination of a planned migration cycle, with 40 to 50 percent leaving within fifteen years (Adda et al., 2022), refugee return is comparatively rare and contingent on the resolution of conflict at origin, so an intention to return need not translate into a move within any horizon the data observe. The behavioral content of refugee return intentions therefore cannot be assumed from the economic-migration literature and must be measured directly. Yet no prior work tracks the same individuals across repeated waves, observes their precise return location, and links it to time-varying conditions at origin. Doing so reveals a clear pattern and shows that stated plans rank realized return well.[6]

Second, we identify the causal effect of local conflict on refugee return using within-person variation, and distinguish a behavioral response to risk from the mechanical infeasibility of return. Most of the literature relates security and economic conditions to return through cross-sectional or intention-based designs (e.g., Camarena and Hägerdal, 2020; Zakirova and Buzurukov, 2021; Beaman et al., 2022), and a recent review flags two gaps that are especially salient for understanding return dynamics (Alrababah and Casalis, 2024). Rigorous evidence on return *during* active conflict remains scarce, and return from high-income hosts is understudied despite different constraints and selection. Our design speaks to both. By observing the exact return location, linked to municipality-level conflict intensity and district-level territorial control, we can ask not just whether refugees return but precisely where, which lets us isolate how conflict shapes return to the home municipality and separate an active, risk-driven response from the mechanical impossibility of returning to occupied ground.

Third, we open the black box of *why* return intentions shift, by directly eliciting refugees' expectations about the war. Prior work shows that intentions respond to security conditions, economic opportunity, and past conflict exposure (Arias et al., 2014; Ghosn et al., 2021; Al Husein and Wagner, 2023; Alrababah et al., 2023).[7] The migration literature has long recognized that intended durations are revised by large, persistent shocks, but typically treats those shocks as unobserved. By repeatedly eliciting refugees' beliefs about the war's outcome and tracking how the same individuals revise them, we can link these belief revisions directly to changes in return plans, opening up a channel that standard data leave latent.

[6] This is why intentions are best interpreted as probabilities rather than one-for-one predictions of behavior.

[7] Aksoy et al. (2024) study Syrian refugees and find heightened hometown violence improves school outcomes in Turkey, consistent with lower expected return. Zaiour (2023) and Bassetto and Freitas Monteiro (2026) show origin-country violence can reduce return intentions and raise plans to settle in the host country, also among non-refugee migrants.

The remainder of the paper proceeds as follows. Section II describes the survey and conflict data and discusses sample selection, attrition, and the main threats to validity. Section III presents the empirical strategy. Section IV reports the results on realized return, return intentions, and war expectations, together with a battery of robustness checks addressing these threats. Section V concludes.

2 Data

2.1 Survey of Ukrainian Refugees in Europe

We collaborated with the survey company Verian (formerly Kantar Public) to field a 10-wave online longitudinal survey of Ukrainian refugees across Europe called *Voice of Ukraine*. Respondents aged 18 and over were recruited via social media ads to participate in the baseline survey. The ads targeted Ukrainian refugees residing in the EU, the United Kingdom, Switzerland, Norway, Iceland, and Moldova. For subsequent waves, contact was made via email. The baseline was conducted between 14 June and 22 December 2022.

On average, respondents completed the first survey 194 days after leaving Ukraine. Starting from 11,783 baseline respondents with Ukrainian citizenship, 6,299 agreed to be recontacted, and 3,029 completed at least one follow-up survey. After restricting the analysis to respondents whose prewar home municipality could be matched to the conflict data, the main analysis samples, described below, contain 2,484 respondents in the long-difference sample and 8,698 respondent-wave intervals in the short-difference sample.[8]

Figure A.1a shows the distribution of Ukrainian refugees across European countries, and Figure A.1b shows the sampling rate across European destinations. In all major host countries, we achieved a sampling rate of at least 1 in 1000 refugees. Those who agreed to be recontacted were asked to complete nine follow-up surveys via email between September 2022 and May 2025. The follow-up emails explicitly asked respondents who returned to Ukraine to complete the survey. Participants received a 3 Euro voucher to encourage participation in each survey wave.

Tables A.1 and A.2 provide detailed information on the timing and number of observations for each wave, while Figure A.2 visually presents the distribution of interviews over time. The first survey wave collects information on respondents' demographic characteristics, their current

[8] In all analyses, we exclude 101 baseline survey respondents who do not hold Ukrainian citizenship.

living situation, and their intentions to return to Ukraine.

The online nature of the survey is well suited to studying a highly mobile refugee population: it allows us to recontact respondents across host countries and within Ukraine itself, whereas returnees and onward migrants mechanically exit registry-based or single-country sampling frames. The trade-off is that the baseline sample is not a probability sample of all Ukrainian refugees in Europe. Online recruitment may overrepresent refugees who are digitally connected, more responsive to survey invitations, and better informed about conditions in Ukraine. Two considerations mitigate this concern. First, as reported below (see Section A.2), our sample broadly matches the distribution of Temporary Protection beneficiaries in Eurostat data by sex, age, and host country, with women and middle-aged respondents somewhat overrepresented, and our population weights adjust for these differences on observables. Second, and more importantly, selection on unobservables such as attachment to Ukraine, risk tolerance, or information access would bias the *levels* of return intentions and return rates, but our main contribution does not rest on levels: the core analyses exploit within-individual variation, and time-invariant selection into the sample is addressed by our empirical strategy.

Where selection does matter, its likely direction is informative. If digitally connected and Ukraine-attached respondents are overrepresented, our population-weighted return rates are best read as an upper bound on return propensities in the full refugee population. Panel attrition raises a parallel concern, and we are transparent about its structure: respondents with stronger baseline return intentions are less likely to be retained, so unweighted return rates understate realized return, a pattern we correct with inverse-probability-of-attrition weights and bound directly in descriptive return rates under alternative assumptions about attriters. Crucially for the panel estimates, neither conflict intensity nor territorial control predicts follow-up response, limiting the scope for conflict-induced attrition to drive the main coefficients. We therefore treat the population-weighted return rates as the best available estimates of aggregate patterns, while the central evidence of the paper, the longitudinal link between conflict exposure, expectations, intentions, and mobility, rests on within-person variation that is robust to selection into the panel.

We measure return intentions using the following question:

> *What are your plans regarding returning to Ukraine?*

Response options are:

- *I intend to go back very soon.*
- *I intend to go back at some point later when I feel it is safe to return.*
- *I do not intend to go back and plan to settle outside Ukraine.*
- *I do not know yet.*
- *Prefer not to answer.*

We also asked respondents to report where in Ukraine they lived prior to leaving. We collected the region (*oblast*) using a drop-down menu and the municipality (*hromada*) using a write-in field.[9]

To link respondents to conflict exposure, we parsed the write-in municipality field and matched 82% of baseline respondents to a unique home municipality. Figure A.3 maps respondents' pre-departure locations. In the follow-up waves, respondents were repeatedly asked about their current location, return intentions, and expectations about the outcome of the war. For more information on our survey and wording of key questions, see Appendix A.1.1.

2.2 Conflict Data

We examine two local conflict dimensions: intensity and territorial control. To measure conflict intensity, we conduct a principal component analysis of deaths and conflict events reported by ACLED and UCDP in each municipality (see Section A.1.2). Figure A.4 maps this measure between respondents' first and last interviews, showing that conflict was concentrated along the front line, the Russian border, and major cities such as Kyiv and Dnipro, with notable variation even in Ukrainian-controlled areas far from the front line. Figure A.5 shows that although Ukraine-wide conflict intensity does not show large changes over time, there is considerable variation within regions over time.

To capture territorial control, we construct a daily dataset from the Institute for the Study of War (ISW) maps, which track the front line using publicly available information. Districts (*raions*) are classified as under Ukrainian control or (partially) occupied on a daily basis. Figure A.6 shows territorial control at the survey start: most of Ukraine was never occupied, several areas were liberated before the first wave, and Russia retained control over large territories, some held since before 2022. Figure A.7 illustrates changes during the full study period, including

[9] As of 2020, Ukraine has 27 regions, 137 districts, and 1,469 municipalities.

Ukraine's 2022 liberation of areas in Kharkiv and Kherson, and Russia's partial re-occupations in the regions of Kharkiv and Sumy in 2024.

2.3 Estimation Samples

To study changes in return and return intentions over time, we construct two complementary analysis samples. The *short-difference* sample uses consecutive survey responses. If a respondent answers N survey waves, this creates up to $N-1$ first-difference observations, each comparing outcomes and conflict exposure between consecutive responses by the same respondent. This sample uses the full panel structure, maximizes statistical power, and captures shorter-run responses to changes in conflict intensity and territorial control. The *long-difference* sample compares each respondent's baseline response with their last observed follow-up response. This sample captures medium-run changes over the panel, making it less sensitive to short-term fluctuations between consecutive observations.

Both samples are restricted to respondents whose home municipality can be linked to the conflict data. The long-difference sample contains 2,484 respondents and therefore 2,484 observations. The short-difference sample contains all valid consecutive response pairs for these respondents and includes 8,698 observations. The average time between the two responses is 229 days in the short-difference sample (SD: 184) and 628 days in the long-difference sample (SD: 336). We exclude interviews conducted within 30 days of the respondent's previous interview (less than 3% of observations). To avoid confusion with survey waves, we refer to the two observations in each pair as the first and second responses. Because the analysis focuses on return and return intentions among refugees still abroad at the start of each comparison period, we exclude short-difference observations in which the respondent had already returned to Ukraine before the first response. This makes the short-difference sample comparable to the long-difference sample, where all respondents were abroad at baseline.

2.4 Descriptive Statistics

Table A.3 describes respondents by the territorial status of their home district: continuously under Ukrainian control since 24 February 2022, liberated before the first interview, liberated between interviews, reoccupied between interviews, or continuously occupied between interviews. This variation is central to the empirical design, since it allows us to distinguish changes in local conflict intensity from changes in territorial control.

Table A.4 presents baseline characteristics for the full survey sample and for the short- and long-difference analysis samples. Respondents are primarily working-age women, about one quarter have a partner in Ukraine, and roughly two thirds have a tertiary degree. The baseline and analysis samples are closely aligned on the main observable demographic characteristics, supporting the comparability of the panel samples used in the return regressions. Table A.5 benchmarks the survey against Eurostat Temporary Protection data by sex, age group, and host country. We use these administrative data to construct population weights in the robustness analysis, ensuring that the main results are not driven by differences in sample composition.

Figure A.8 documents baseline predictors of return to Ukraine by the end of 2023 and by the most recent survey wave completed in 2024 or 2025. Return is strongly associated with family ties to Ukraine, especially having a partner who remained there, and with life-cycle characteristics. Return rates are also higher among refugees hosted in Poland and other Eastern European countries than among those hosted farther west, consistent with geographic proximity and initial destination choice being important determinants of mobility. These patterns motivate our empirical specifications, which condition on baseline return intentions, individual characteristics, geography, and survey timing.

Figure A.9 shows that return expectations evolve over time. This dynamic updating is one of the advantages of the panel design: rather than relying on a single cross-sectional measure of intentions, we observe how stated plans change as the war continues and as refugees receive new information about conditions in Ukraine and opportunities abroad.

2.5 Stated Reasons for Return

Because return is the central outcome, we also examine refugees' stated reasons for going back. In several waves, respondents were asked about return motives using versions of the same question tailored to their current situation: those intending to return, those not intending to return, and those who had already returned. Table A.6 reports the share selecting each reason, allowing up to two responses.

Across groups, the most frequently cited reasons are attachment to home and reunification with family. This reinforces the interpretation of return as a household and place-based decision, rather than a purely economic response to host-country conditions. Among respondents who had already returned, practical constraints abroad, including difficulty adjusting, financial pressure, education, and employment considerations, are also more salient. The stated-reasons evidence

therefore complements the main analysis by showing that realized return reflects both pull factors from Ukraine and constraints faced abroad.

3 Empirical strategy

To study how changes in territorial control and conflict intensity are related to refugees' return, we use the short-difference and long-difference samples introduced above.

Our main outcome is a set of return indicators measured at the second interview, Y_{it_2}, capturing actual return outcomes and return intentions (see Table 1). We regress these outcomes on conflict intensity and (levels and changes of) territorial control between the first and second interviews, (t_1, t_2), controlling for baseline covariates measured at t_0, including individual return intentions and pre-period conflict exposure. We estimate the following model:

$$Y_{it_2} = \beta_1 Conflict_{mt_1t_2} + \beta_2 Conflict_{mt_0t_1} + \beta_3^{'}\mathbf{Territory}_{dt_1t_2} + \gamma^{'}\mathbf{ReturnInt}_{it_1} + \delta^{'}\mathbf{X}_i + \psi_{t_1} + \psi_{t_2} + \theta_h + \epsilon_{it_2} \quad (1)$$

where t_0 is the start of the Russian full-scale invasion (24 February 2022) and t_1 (t_2) is the time of the first (second) response. To prevent endogeneity arising from respondents' choice of when to answer the survey, t_1 and t_2 refer to the dates on which the respective survey waves were fielded, rather than the dates on which individuals submitted their responses. $Conflict_{mt_1t_2}$ is the local conflict intensity per 30 days in one's home municipality between the two instrumented survey response dates. We also control for conflict intensity between 24 February 2022 and the first response, $Conflict_{mt_0t_1}$. Controlling for this is important as conflict before the first response and between survey waves is positively correlated ($\rho = 0.54$). Without this control, the variable $Conflict_{mt_1t_2}$ may also capture the effect of prior conflict intensity, including migrant selection, which could independently affect subsequent return.

$\mathbf{Territory}_{dt_1t_2}$ captures the change in territorial control in respondent i's home district d between the first and second interviews. Our main focus is the roughly 10% of respondents whose home districts were liberated between survey waves. We identify the effect of liberation by comparing outcomes for these respondents with those for respondents whose home districts remained occupied throughout the same period, which serves as the reference category. In con-

trast, districts that were already liberated or never occupied at the time of the first interview do not receive treatment between waves, and refugees from these districts may differ systematically from those from occupied areas. Accordingly, coefficients for these categories should be interpreted as correlations rather than causal effects.

$\mathbf{ReturnInt}_{it_1}$ measures the respondent's stated return intentions in the first interview. Because these intentions are measured at baseline, they reflect the factors shaping return plans that were already present before the first survey wave.

We control for a rich set of baseline individual characteristics, $\mathbf{X}_i$, to account for demographic and background differences that may predict return: sex, age bin (18-24; 25-34; 35-44; 45-54; 55-59; 60-64; 65 and older), partnership status, whether one had a partner left behind in Ukraine, whether the respondent left Ukraine with children below 18, having a tertiary degree, speaking English, continuing one's job in Ukraine remotely, whether one left Ukraine before 24 February 2022, and whether one completed the baseline survey in Russian. We also control for the population and population squared of the respondent's home municipality, since conflict intensity mechanically tends to be higher in more populous places. In addition, to capture whether respondents come from larger versus smaller settlements within a municipality, we control for the self-reported population of the home settlement (more than 500,000; 50,000–500,000; less than 50,000). Finally, we also control for the distance in kilometers between the centroid of the initial destination country and the home municipality.

We also include fixed effects for the initial host country during the baseline wave (θ_h), which absorb time-invariant host-country characteristics that may influence return decisions. Fixed effects for the week of the first and second survey responses (ψ_{t_1} and ψ_{t_2}) capture common time variation in return, such as geopolitical developments and changes in conflict intensity at the national level in Ukraine. In our most demanding specification of short differences, we additionally include home-municipality fixed effects, absorbing all time-invariant place-specific determinants of mobility; in that case, the conflict variables identify differential effects within the same municipalities over time. We cluster standard errors at the district level.

The identifying assumption is that, conditional on baseline return intentions, individual characteristics, geography, prior conflict exposure, initial host-country fixed effects, and survey-wave timing, changes in local conflict intensity and territorial control at origin are unrelated to other time-varying determinants of return over the same interval. Equivalently, the design requires that, after conditioning on these controls, municipalities experiencing greater between-

wave conflict would not have followed different return trajectories for reasons unrelated to the conflict itself. We assess this assumption through progressively richer controls, specifications with home-municipality fixed effects, robustness checks for alternative samples and outcomes, population and attrition reweighting, and direct tests of selective follow-up response.

We cluster standard errors at the district level, allowing arbitrary correlation in unobserved shocks among respondents from the same origin district. Overall, the estimates should be interpreted as reduced-form effects of changing wartime conditions at origin. The strength of the design is that it combines panel data on the same refugees, predetermined survey-wave exposure windows, rich controls for pre-existing return plans and prior conflict, and exact information on the municipality to which refugees return.

4 Results

4.1 Do Return Intentions Predict Actual Return?

We begin by assessing the extent to which baseline return intentions predict subsequent behavior. Table 1 reports realized return rates by respondents' initial plans, which shows a clear gradient in both samples: respondents expressing stronger intentions to return are substantially more likely to do so. Column 3 reweights observations to make the sample representative for the population of Ukrainian refugees across Europe (see Section A.2), providing our preferred estimates of overall rates of return. In particular, 42.2% of those who planned to return "very soon" in Wave 1 subsequently returned, compared with only 1.2% of those who planned to settle outside Ukraine.

Several checks support this interpretation while clarifying its limits. First, Table A.7 shows that follow-up retention differs by baseline intention: respondents who planned to return very soon are less likely to be observed again, but conditional on being retained they are much more likely to return. This pattern suggests that attrition, if anything, leads us to understate realized return among those with the strongest initial return plans. Second, Table A.8 addresses the fact that respondents are observed for different lengths of time by measuring return at common horizons. The ordering remains strongly monotone. We find that those who initially planned to return very soon have the highest return rates, followed by those planning to return when safe, those who did not know, and those planning to settle outside Ukraine. Third, Table A.9 shows that baseline intentions predict subsequent return beyond rich observable predictors, including

demographics, family-location variables, host country, remote work in Ukraine, date of leaving, and prior conflict exposure. Together, these checks show that return intentions contain predictive information about subsequent mobility.

Table 1: Return intentions and realized mobility

	(1) Short difference	(2) Long difference	(3)
		Unweighted	IPW × Pop.
Panel A: % returned by initial return intention			
Return very soon	26.7	39.7	42.2
Return when safe	4.1	11.8	11.8
Do not know	1.5	3.5	3.3
Settle outside Ukraine	0.0	1.2	1.2
Panel B: Outcomes			
Returned	4.2	10.1	11.1
Returned to home municipality	3.3	7.5	9.5
Returned elsewhere in Ukraine	1.0	2.0	1.9
Moved to third country	3.1	6.2	6.4

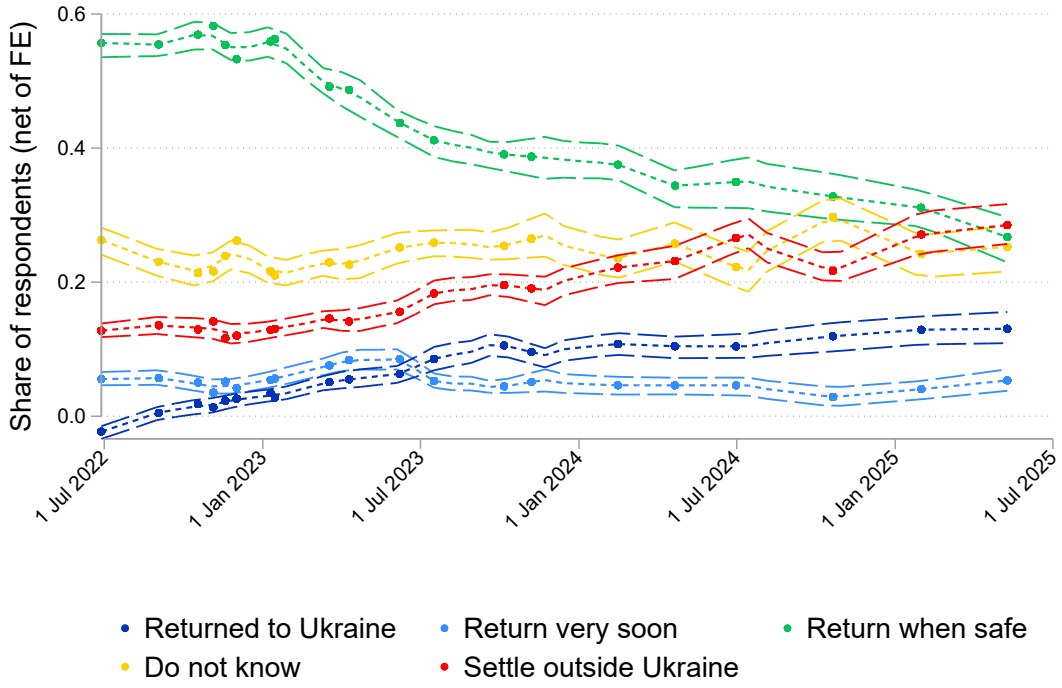
Notes: Panel A reports the share returning to Ukraine by initial return intention; Panel B reports mobility outcomes. Columns 1 and 2 are unweighted short- and long-difference estimates. Column 3 applies the product of inverse-probability weights (IPW) and population weights based on sex–age–host-country cells (Section A.2). Rows on return location use the restricted sample with reported return location. The full short-difference sample has 8,698 observations (5,769 restricted); the full long-difference sample has 2,484 observations (2,084 restricted). Tables A.7–A.9 report additional validation checks.

Next, we distinguish between two dimensions of return. The first is whether a respondent returned to Ukraine at all. The second is whether the respondent returned to their home municipality rather than to another location within Ukraine. Using weighted long differences, we estimate that 11.1 percent returned to Ukraine, of whom 86 percent returned to their home municipality.[10] At the same time, a non-trivial share of refugees relocated across host countries rather than returning to Ukraine (Figure A.10).

To interpret realized returns, it is also important to track how stated plans change over time. Figure 1 traces the evolution of intentions and realized return in the full panel after netting out individual fixed effects using a non-parametric approach. Intentions were broadly stable from summer 2022 to January 2023, coinciding with Ukraine's territorial gains. From early 2023 onwards (a period with limited territorial recovery), return intentions weakened steadily, and the share planning to settle outside Ukraine increased approximately linearly. Between January

[10] A natural concern is that returns may be temporary. Our panel evidence suggests that this is uncommon: among respondents observed in at least one wave after they first returned to Ukraine, only 19.2% were living outside Ukraine again at their last interview.

Figure 1: Within-individual return intentions and return over time



Notes: This figure shows binned scatterplots with non-parametric trends for return intentions and realized return over time, net of individual fixed effects. For each outcome, observations are assigned to 20 equally sized calendar-time bins and residualized by individual fixed effects. Smoothed 90% confidence bands are obtained from 100 bootstrap samples, resampling at the respondent level. Markers show bin means; lines show predicted means. The sample includes 13,518 responses from 3,029 respondents, including respondents for whom the home municipality is not observed.

2023 and June 2025, the share reporting plans to return soon or when safe fell by 3.5 percentage points per 100 days. Over the same period, the share who had returned to Ukraine increased by 1.2 percentage points per 100 days, and the share planning to settle outside Ukraine increased by 1.8 percentage points per 100 days, from 11% to 30%. Figures A.11 and A.12 show that intentions are generally persistent between interviews, and when they change, they typically move to adjacent categories.

In sum, initial return intentions strongly predict subsequent return, while average return intentions steadily weaken over time. These descriptive patterns, however, do not pin down the role of conflict, because return decisions and intentions may change alongside individual circumstances and broader wartime developments.

4.2 Conflict and Realized Returns

Building on the descriptive evidence above, we now quantify how conditions in respondents' home municipalities shape realized return outcomes. Table 2 reports estimates of equation 1 for

(i) return to Ukraine, (ii) return to the home municipality, and (iii) return elsewhere in Ukraine, using both the short- and long-difference samples.

Table 2: The effect of conflict on realized return

Panel A: **short difference**	Returned		
	To Ukraine	To home municipality	Elsewhere in Ukraine
	(1)	(2)	(3)
Conflict intensity (1 SD)	-0.002	-0.008***	0.002
	(0.002)	(0.002)	(0.002)
Remain occupied	ref.	ref.	ref.
Liberated between interviews	0.008	0.007	0.009
	(0.010)	(0.012)	(0.007)
Remain liberated	0.001	0.016*	-0.009
	(0.007)	(0.009)	(0.007)
Never occupied	-0.000	0.007	-0.011*
	(0.008)	(0.009)	(0.006)
Observations	8,698	5,769	5,769
R-squared	0.148	0.115	0.046
Average dep. var.	0.034	0.030	0.010
Panel B: **long difference**			
	(1)	(2)	(3)
Conflict intensity (1 SD)	-0.005	-0.015***	0.005
	(0.008)	(0.004)	(0.006)
Remain occupied	ref.	ref.	ref.
Liberated between interviews	0.046***	0.060***	-0.016
	(0.014)	(0.021)	(0.017)
Remain liberated	0.004	0.037	-0.026*
	(0.034)	(0.030)	(0.014)
Never occupied	-0.010	0.015	-0.041***
	(0.017)	(0.016)	(0.013)
Observations	2,484	2,084	2,084
R-squared	0.160	0.160	0.084
Average dep. var.	0.101	0.075	0.020

Notes: This table reports estimates of equation 1 for realized return outcomes using the short-difference (Panel A) and long-difference (Panel B) samples. Column 1 uses the full sample; Columns 2 and 3 use observations with reported return location. Conflict intensity is measured at the municipality level and territorial control at the district level. All models include first-response return intentions, settlement-type and population controls, survey-timing fixed effects, baseline covariates, week of leaving Ukraine, initial host-country fixed effects, and prior conflict. Standard errors clustered at the district level are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Conflict intensity. We find that local conflict intensity reduces return to the home municipality. A 1 S.D. increase in conflict intensity lowers the probability of returning to the home municipality by 0.8 percentage points in the short run, relative to a mean of 3.0 percent, and by

1.5 percentage points in the medium run, relative to a mean of 7.5 percent. These correspond to reductions of 27 and 20 percent of the respective means. To illustrate the magnitude, a refugee from a municipality with no local conflict (-1.44 S.D. below average) is 2.2 percentage points more likely to return home in the medium run than a refugee from an average-conflict municipality, all else equal. Tables A.11 and A.12 show that this result is robust to the gradual inclusion of controls, and Table A.11 further shows that it remains similar when municipality fixed effects are included. We summarize this stability with the selection diagnostic of Oster (2019) in Table A.13. Under the conventional bound $R^2_{\max} = 1.3\,\tilde{R}^2$, selection on unobservables would have to be about five times (short difference) and two times (long difference) as strong as selection on observables to drive the home-municipality effect to zero. Because the short-difference coefficient becomes larger after adding municipality fixed effects, any unobserved selection strong enough to overturn this result would have to operate in the opposite direction from selection on observables.

The evidence is less precise for overall return to Ukraine. We do not detect a statistically significant effect of local conflict intensity on aggregate return to Ukraine.[11] However, this null is not precise enough to be interpreted as evidence of no effect. In the long-difference specification, the coefficient is -0.005 with a standard error of 0.008, implying a 95 percent confidence interval of approximately -2.1 to $+1.1$ percentage points. Given a mean return rate of 10.1 percent, the data allow us to rule out very large declines in aggregate return, but remain consistent with smaller negative or positive effects.

Table A.14 makes this distinction explicit and helps clarify the main empirical contribution of the survey. The design is well powered for the home-municipality margin, which is the margin most directly linked to local conflict exposure. At 80 percent power and conventional size, the minimum detectable effects for return to the home municipality are 0.006 in the short-difference sample and 0.011 in the long-difference sample, both below the estimated effects. Realized power for this outcome exceeds 0.95 in both samples. By contrast, aggregate return to Ukraine and return elsewhere in Ukraine are broader margins that combine potentially offsetting movements, and the design is much less powered to detect effects on these outcomes. Realized power for the aggregate-return and elsewhere-return outcomes ranges only from 0.09 to 0.17. The absence of statistically significant effects on these broader margins should therefore be read as a bounded null rather than as a precise zero. This is precisely why observing the exact return location

[11] Table A.10 shows that conflict also has no sizeable impact on onward migration between host countries.

matters: it allows us to detect the spatial reallocation effect that would be obscured in data recording only whether refugees returned to Ukraine. Point estimates for return elsewhere in Ukraine are positive in the main specification, but they are imprecise and do not allow us to establish redirection within Ukraine on their own.

Territorial control and liberation. Table 2 also shows how changes in territorial control relate to return. In the short run, liberation of the home district does not substantially increase return, either to Ukraine overall or to the home municipality. These estimates are precise enough to be informative: for home-municipality return, the coefficient on liberation is 0.007 with a standard error of 0.012, so the 95 percent confidence interval rules out short-run increases larger than about 3 percentage points. This is consistent with return after liberation taking time, as households wait for demining, the restoration of services, and confirmation that territorial gains are durable before moving back.

In the medium run, however, respondents whose home districts were liberated are more likely to return to their home municipality than respondents whose districts remained occupied. This comparison should be interpreted as combining changes in physical reachability, safety, and local conditions, since return to districts that remain occupied is often infeasible. Relative to a mean home-return rate of 7.5 percent, liberation is associated with a 6.0 percentage-point higher probability of returning to the home municipality.

Heterogeneity. We next examine how the effects of conflict intensity vary across refugees. Table A.15 interacts conflict intensity with baseline intentions and shows that the negative association between conflict intensity and home-municipality return is concentrated among those who initially planned to return soon or when safe (the groups with the highest baseline return rates). Figure A.13 shows that the conflict-related reduction in home-municipality return is also strongest among respondents with a partner remaining in Ukraine. Table A.16 shows that the reduction in home-municipality return is largest among respondents from the North, West, and Central regions, particularly in the short run, both in absolute and relative terms. Finally, we find suggestive evidence that conflict in the home municipality redirects return migration toward other locations within Ukraine, particularly for respondents from the South and East.

4.3 Conflict and Return Intentions

The previous section showed how conflict in the home municipality affects *realized* return, distinguishing between return to Ukraine, return to the home municipality, and return elsewhere

in Ukraine. We now turn to return intentions, which help interpret these results. Intentions can adjust before households are able to move, and they allow us to distinguish changes in the willingness to return from changes in the feasibility, timing, or destination of return.

Table 3 presents the results. Column 1 examines plans to return soon or when it is safe, while Column 2 examines plans to settle outside Ukraine. In the short-difference specification, higher conflict intensity is associated with a higher probability of planning to return soon or when safe. The long-difference estimate is similar in magnitude but less precise, and the association is attenuated in the population-weighted specifications. Across specifications, however, conflict intensity does not increase plans to settle permanently outside Ukraine. This pattern suggests that local conflict does not primarily operate by reducing the underlying willingness to return to Ukraine. It is also consistent with evidence that exposure to conflict can strengthen attachment to the home region or country (see, e.g., Lupu and Peisakhin, 2017); related evidence for Ukrainian refugees similarly links stronger attachment to higher intentions to return (Vakhitov et al., 2025).

Turning to territorial control, liberation has little effect on reported return intentions among respondents who remain abroad. One possible reason is mechanical selection: if liberation induces those most willing to return to go back, they no longer appear in the intentions sample in the next wave. To assess this channel, Table A.17 re-estimates Table 3 including returnees and assigning them the intention to return soon.[12] With this adjustment, liberation appears to shift return intentions slightly in the medium run, in line with the realized-return effects documented above.

Taken together, the realized-return and intention results point to a consistent interpretation. Higher conflict intensity reduces return to the home municipality, but it does not increase plans to settle permanently abroad. This suggests that local conflict may delay home return or make it infeasible even among refugees who continue to express an intention to return, rather than necessarily reducing the underlying willingness to return to Ukraine.

4.4 War Expectations and Return Intentions

In our panel regressions, local conflict intensity in the home municipality does not reduce return intentions among respondents who remain abroad (Table 3). Yet average return intentions

[12] We classify respondents who have already returned by the second interview as intending to return (Column 1) and not intending to settle outside Ukraine (Column 2).

Table 3: The effect of conflict on return intentions

	Return intentions	
	Return soon or when safe	Settle outside Ukraine
Panel A: **short difference**		
	(1)	(2)
Conflict intensity (1 SD)	0.015**	-0.000
	(0.007)	(0.005)
Remain occupied	ref.	ref.
Liberated between interviews	0.009	0.007
	(0.025)	(0.013)
Remain liberated	0.019	0.007
	(0.018)	(0.014)
Never occupied	-0.016	0.001
	(0.020)	(0.013)
Observations	8,398	8,398
R-squared	0.425	0.490
Average dep. var.	0.526	0.205
Panel B: **long difference**		
	(1)	(2)
Conflict intensity (1 SD)	0.019*	-0.005
	(0.011)	(0.010)
Remain occupied	ref.	ref.
Liberated between interviews	0.015	-0.024
	(0.027)	(0.016)
Remain liberated	-0.006	0.026
	(0.048)	(0.039)
Never occupied	-0.046	0.006
	(0.037)	(0.025)
Observations	2,393	2,393
R-squared	0.320	0.322
Average dep. var.	0.505	0.204

Notes: This table reports estimates of equation 1 for return intentions using the short-difference (Panel A) and long-difference (Panel B) samples. The outcomes are indicators for planning to return soon or when safe (Column 1) and planning to settle outside Ukraine (Column 2). All models include the same regressors as Table 2. The sample is restricted to observations in which the respondent had not returned to Ukraine by the first response. Standard errors clustered at the district level are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

decline markedly over time in the raw panel (Figure 1). The combination of these findings suggests that the downward trend in intentions is unlikely to be driven solely by changes in *local* conflict conditions at origin.

To shed light on what may be moving intentions over time, we examine refugees' expectations

about the war. In several survey waves, we asked respondents about the outcome of the war by the end of 2024. The expectation data show a clear shift toward pessimism. Between September 2022 and January 2023, 71% of respondents expected Ukraine to win and liberate all occupied territories by the end of 2024; by October–November 2023, this share had fallen to 35%. Figure A.14 visualizes how respondents moved across expectation categories between the early waves (September 2022–January 2023) and later waves (October–November 2023), and Figure A.15 shows that optimism declines in a close-to-linear fashion since January 2023. Over the same period, the share who plan to return (or have already returned) also falls, although more gradually.

We then ask whether changes in expectations are associated with changes in return intentions at the individual level. Using the subset of waves that include the expectations module, we estimate long-difference models relating returns and return intentions to changes in expectations, controlling for respondents' initial return intentions. For ease of interpretation, we summarize expectations with a binary indicator for whether the respondent expects Ukraine to win by the end of 2024 (versus any other response). Since expectations and intentions may be jointly shaped by attachment to Ukraine, family circumstances, host-country integration, and unobserved optimism, we interpret this exercise as descriptive rather than causal.

Table 4 presents the results. Over time, realized return increases between waves, while return intentions decline. We do not detect a statistically significant association between negative expectation updating and realized return over this horizon. However, negative updating is strongly associated with changes in stated plans: respondents who no longer expect Ukraine to win are 12.3 percentage points less likely to plan to return soon or when safe, and 4.7 percentage points more likely to plan to settle outside Ukraine. Positive updates are associated with higher plans to return, although these estimates are less precise because relatively few respondents become more optimistic.

The bottom rows of Table 4 summarize how much of the change in intentions is accounted for by respondents becoming more pessimistic about the war. Specifically, negative updating is measured by an indicator equal to one for respondents who initially expected Ukraine to win by the end of 2024 but no longer did so in the later wave. This shift accounts for 21% of the decline in plans to return and 27% of the increase in plans to settle outside Ukraine. These figures show that worsening expectations about the war's trajectory are closely associated with the aggregate decline in return intentions documented in Figure 1. Table A.18 shows that the

Table 4: The relation between changes in war expectations and return intentions

	Returned to Ukraine (1)	Return soon or when safe (2)	Settle outside Ukraine (3)
Time between interviews (100 days)	0.024***	-0.048***	0.010
	(0.006)	(0.010)	(0.007)
Does not think anymore Ukraine would win	0.010	-0.123***	0.047***
	(0.017)	(0.027)	(0.018)
Newly thinks Ukraine would win	0.019	0.076	-0.055
	(0.033)	(0.061)	(0.047)
Observations	1,624	1,624	1,624
R-squared	0.086	0.311	0.411
Average dep. var.	0.071	0.480	0.182
Time effect [%]	80	71	55
Expectations effect [%]	5	21	27
Initial return intentions and expectations	✓	✓	✓

Notes: This table relates second-response outcomes to time between interviews and changes in expectations about the war. All specifications control for first-response return intentions and war expectations. The sample consists of long differences between the first and last interview across waves 2–6, excluding respondents located in Ukraine at the first response. The mean time between survey waves is 242 days; 29.7% of respondents stop expecting Ukraine to win and liberate all occupied territories by the end of 2024. The time and expectations effects are descriptive decompositions based on coefficient estimates and mean changes in the relevant variables; they should not be interpreted causally. Heteroskedasticity-robust standard errors are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

negative association between pessimistic expectation updating and plans to return is similar when we apply inverse-probability and population weights. The corresponding increase in plans to settle outside Ukraine is directionally similar but less precise.

4.5 Robustness Checks

We conduct a range of robustness checks designed to assess the sensitivity of our results to the main identification and measurement concerns: (i) attrition and sample representativeness; (ii) alternative measures of conflict intensity and exposure; (iii) functional-form assumptions; (iv) spatial correlation in unobserved shocks; and (v) the composition of the sample with respect to respondents from occupied and liberated areas.

Attrition and sample representativeness. Attrition can matter for two distinct reasons. First, it can affect descriptive statements about the level of return if respondents who leave the panel have different return propensities from those who remain observed. Second, and more important for identification, it can bias the estimated conflict effects if exposure to conflict at origin systematically changes the probability that a respondent is observed in later waves. We therefore separate representativeness for estimating aggregate return rates from selective

attrition that would threaten the conflict-gradient estimates.

We begin by directly modelling follow-up response. Table A.19 regresses several measures of continued survey participation on baseline characteristics, first-response return intentions, conflict intensity, and territorial control. The results show that attrition is not random in levels. Respondents with weaker return intentions respond to more waves and are more likely to enter the short- and long-difference samples, implying that unweighted panel estimates understate descriptive return rates. This pattern is useful diagnostically because it identifies the main direction of descriptive selection. Crucially, however, the same table shows no evidence that conflict intensity or territorial control predicts subsequent response. Table A.20 reaches the same conclusion when we study inclusion in the main realized-return, expectations, and intentions samples. Thus, the observable determinants of panel inclusion are related to baseline mobility intentions, which we measure and control for, but not to the conflict variation that identifies the main estimates.

We then examine a more specific concern: whether realized return itself causes respondents to leave the panel. Table A.21 stacks respondent-by-wave observations and estimates whether answering wave $w+1$, conditional on answering wave w, is predicted by being in Ukraine during wave w. The mean probability of answering the next wave is about 0.64. Across specifications, being in Ukraine is associated with a 3.4–3.9 percentage-point lower probability of answering the next wave, and this association is stable when we condition on baseline and contemporaneous return intentions. This is economically modest relative to the response mean and, importantly, does not explain away the intention gradient. The same table shows that return intentions remain predictive of survey participation, which is exactly the source of selection we explicitly model and reweight. This exercise therefore rules out the strongest version of the mechanical attrition concern: respondents who return to Ukraine are not disappearing from the panel at rates large enough to drive the main results.

Next, we re-estimate the main specifications using two complementary reweighting strategies. The first uses inverse-probability weights constructed from a logit model of follow-up response, using the predictors reported in Table A.19. These weights reweight the first-difference samples back toward the baseline survey population. Consistent with attrition being concentrated among respondents with stronger return intentions, the IPW adjustment raises the estimated return rate from 3.6% to 4.3% in the short-difference sample and from 10.0% to 10.8% in the long-difference sample. The second strategy uses population weights based on EU Temporary

Protection data, reweighting observations within sex–age–host-country cells to align the sample with the broader refugee population in Europe (Section A.2). Table A.22 shows that the realized-return estimates are quantitatively similar under both IPW and population weights. The same conclusion holds for the expectations outcomes in Table A.18 and for the return-intentions outcomes in Tables A.23 and A.24. The stability of the coefficients across these two weighting schemes is important because they target different concerns: IPW addresses panel attrition, while population weights address baseline representativeness.

Finally, we quantify how much attrition could move the descriptive return rates. Table A.25 reports bounds for all baseline respondents with a valid return-intention category. The no-assumption Manski bounds are necessarily wide. If no attriters returned, the aggregate return rate is 0.029; if all attriters returned, it is 0.770. The intermediate rows impose progressively stronger assumptions about attriter behavior. If attriters returned at the same rate as retained respondents, the implied aggregate return rate is 0.111; if they returned at twice the observed rate, it rises to 0.194. A more conservative benchmark that assigns attriters the return rate of respondents who said they planned to return very soon yields an aggregate return rate of 0.319. Thus, attrition affects the descriptive level of return, but the table makes transparent how strong the assumptions would need to be to generate substantially higher return rates.

The same bounding exercise also strengthens the interpretation of the intention data. Across all intermediate assumptions, respondents who initially planned to return very soon remain much more likely to have returned than those who planned to return when safe, did not know, or planned to settle outside Ukraine. Under the observed-rate assumption, the implied return rate is 0.391 among those planning to return very soon, compared with 0.133 among those planning to return when safe, 0.042 among those who did not know, and 0.017 among those planning to settle outside Ukraine. Even when attriters are assumed to return at twice the observed rate, the same ordering remains. This is an important validation result. Attrition affects the estimated level of return, but it does not overturn the central finding that stated return plans contain substantial predictive content.

Taken together, these exercises strengthen the interpretation of the main findings. Attrition matters for descriptive return rates, and our weighting and bounding exercises make the direction and possible magnitude of this selection transparent. However, conflict intensity and territorial control do not predict follow-up response, realized return to Ukraine does not generate large mechanical panel exit, and the main regression estimates are stable across attrition and

population reweighting. The remaining threat would require time-varying unobservables that are correlated with local conflict exposure, return behavior, and subsequent survey response, over and above baseline intentions, observed characteristics, geography, prior conflict, and survey timing. The evidence in this subsection makes that scenario unlikely and shows that the core home-municipality results are not an artefact of selective panel retention.

Alternative conflict measures and exposure definitions. Our baseline results may be sensitive to how conflict is measured (events versus fatalities; ACLED versus UCDP) and to alternative definitions of exposure (home municipality versus inclusion of surrounding areas). To make the index more transparent, Table A.26 shows that the first principal component loads almost equally on the four components, and Table A.27 benchmarks the index against familiar Ukrainian municipalities. To address measurement concerns, we re-estimate our specifications using four separate conflict indicators, one at a time: the $\log(x+1)$ number of conflict events in ACLED, the $\log(x+1)$ number of conflict events in UCDP, the $\log(x+1)$ number of fatalities in ACLED, and the $\log(x+1)$ number of fatalities in UCDP (all measured per 30 days between interviews). Figure A.16 shows that the resulting estimates are very similar to the baseline results. To address exposure-definition concerns, we expand exposure beyond the home municipality to incorporate conflict in neighbouring municipalities; Figure A.17 shows that the main findings are robust to this broader definition.

Functional form. A further concern is that the estimated effects may depend on the transformation of conflict intensity. Our preferred specification uses $\log(x+1)$, which is well suited to the highly skewed distribution of conflict exposure and allows for diminishing marginal effects. Figure A.18 shows that the main pattern is not driven by the log transformation: when we instead use linear conflict measures across different radii, the estimates remain directionally similar, with the clearest and most consistent effects for return to the home municipality. As expected given the influence of high-conflict outliers in the linear specification, these estimates are smaller and less precise. Overall, this exercise supports the interpretation that local conflict reduces return to home municipalities, while reinforcing the use of the log specification as our preferred and more stable measure of conflict exposure.

Inference with spatial correlation. Because conflict shocks are spatially correlated, conventional standard errors may overstate precision. We therefore allow for spatial correlation in the error structure using Conley-type standard errors. Figure A.19 shows that inference is similar to the baseline.

Sample composition: occupied versus liberated areas. Finally, respondents from areas that remained occupied may differ systematically from those from liberated areas, potentially affecting return behavior and its correlation with conflict measures. Table A.28 addresses this concern by excluding respondents from territories occupied prior to 24 February 2022 and from the two eastern-most oblasts (Donetsk and Luhansk). The results remain consistent with our baseline findings.

5 Conclusion

We provide new evidence on refugee return using a rich ten-wave panel of Ukrainian refugees. The data follow the same individuals from the early months of displacement through 2025, link their prewar home municipalities to geocoded measures of conflict intensity and territorial control, distinguish return to the home municipality from return elsewhere in Ukraine, and elicit expectations about the trajectory of the war. This combination lets us connect stated plans, realized mobility, local conflict conditions, and beliefs within the same individuals over time. Early return intentions are highly predictive of subsequent behavior: refugees who initially planned to return soon were far more likely to return than those who planned to settle abroad, though intentions rank realized return more accurately than they predict its level. Local conflict reduces return to the home municipality, and because this effect is concentrated in reachable, government-controlled areas, it reflects an active response to risk rather than the mechanical infeasibility of return. Return to the home municipality rises after liberation. Finally, return intentions weakened over the course of the war, closely tracking increasingly pessimistic expectations about Ukraine's prospects.

These findings carry implications for return policy and reconstruction planning. Because return is sizable but highly local, and because conflict suppresses return to specific origin areas rather than to the country as a whole, reconstruction support is likely to be most effective when spatially targeted toward the origin areas where housing, infrastructure, services, and property-related assistance can make return feasible, rather than calibrated to a single national return margin. The close link between war expectations and return intentions further suggests that refugees' beliefs about security and the future trajectory of the conflict are as central to planned return as conditions on the ground.

More broadly, our results speak to a question that recurs across refugee crises. Return

intentions contain meaningful and recoverable information about future mobility, but they systematically overstate how much return will occur, and realized return depends critically on local conditions at origin and on beliefs about how the displacement-causing conflict will resolve. Observing both stated plans and realized mobility for the same individuals, and the precise locations to which they return, is what makes these patterns visible, and similar designs could clarify return dynamics well beyond the Ukrainian case.

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A Appendix

A.1 Detailed Description of Data

A.1.1 Verian Survey "Voice of Ukraine"

The survey includes a wide range of background variables relating to demographics, employment status, and home municipality. In every wave, we elicit where people are located:

Current location (all waves) *In which country are you currently located?* [drop-down menu]

Respondents answer this question from a list of countries. This list also includes Ukraine, which enables us to identify those who have returned. In several follow-up waves, we ask several additional questions:

Location of return (waves 3, 6–10) *After your return to Ukraine, where do you live now?* With the following answer options:

- In the same village/town/city where I lived before I fled
- In another village/town/city in the same district
- In another district in the same oblast
- In another oblast. If yes, which oblast? (drop-down menu of oblasts)
- Do not know
- Prefer not to answer

We interpret the first response option as a return to the home municipality. In principle, some respondents selecting the second option may also have returned to the same municipality. However, this category may also include individuals who returned to a different municipality within the same district. To remain conservative, we therefore do not classify respondents choosing the second option as having returned to their home municipality. This distinction is largely inconsequential, as only 2% of all returnees across survey waves select the second option, whereas 75% select the first.

Importantly, to elicit return intentions, we ask individuals located outside Ukraine the following question on return intentions in every wave:

Return intentions (all waves) *What are your plans regarding returning back to Ukraine?* With the following answer options:

- I intend to go back very soon
- I intend to go back at some point later when I feel it is safe to return
- I do not intend to go back and plan to settle outside Ukraine
- Do not know yet

- Prefer not to answer

In addition to the aforementioned question and various demographic variables, we use several other questions directly in the main text:

Expectations about the outcome of the war (waves 2–6) *What do you find the most likely outcome of the war by the end of 2024?* With the following answer options:

- Ukraine wins and Russia withdraws from all territory it currently occupies
- Ukraine cedes some territory to Russia as part of peace agreement
- There is ceasefire
- Russia wins and annexes big parts of Ukraine
- The war continues
- Do not know
- Prefer not to answer

Data cleaning and processing To determine an individual's place of residence before they evacuated during the war, the baseline wave of the survey asks: (i) which region they lived in before February 24, 2022, and (ii) the specific locality through a write-in field. 18% of respondents did not answer the latter question. To match individuals with the municipality (Ukrainian: hromada) of their residence before the war, we utilize geospatial data on Ukraine's administrative divisions as of 2020 from the United Nations Office for the Coordination of Humanitarian Affairs (UN OCHA) (United Nations, 2023). The average municipality has 30,800 inhabitants, and the median has 13,200. Larger cities comprise a single municipality. The spatial files encompass all 1,469 municipalities, nested in 137 districts (raions) across 27 regions (oblasts) and six macro-regions. These localities generally align with administrative divisions, but for 550 individuals, the localities had to be manually matched to municipalities within the specified regions. Localities were classified into municipalities using the Ukrainian government website `https://gromada.info/`. However, since not all region-municipality pairs are unique, we were unable to assign a unique municipality to 12 respondents, and thus classified their municipality as missing.

A.1.2 ACLED and UCDP: conflict intensity

To obtain measures of local conflict intensity, we use the Armed Conflict Location Event Data Project (ACLED) (Raleigh et al., 2010) and the Uppsala Conflict Data Program's Georeferenced Event Dataset (UCDP-GED) 25.1 (Sundberg and Melander, 2013) databases.

ACLED and UCDP collect news reports of conflict data that are human-coded using standardized methods and, if possible, geocode the event. ACLED records relevant events, including violence, which happened at a specific place and time, and are documented in a publicly available source. ACLED includes the primary actor, the type of conflict, and the number of reported

fatalities, among others. UCDP-GED is also an event-level dataset, but with the strict inclusion criterion that at least one death should have been recorded.[13]

In many cases, death tolls are estimates and may vary between UCDP and ACLED for the same event. Death tolls may not be known or may be measured with error. Furthermore, the events may also be included in less severe instances (especially for ACLED), but on average, may provide a reasonable summary measure of conflict. Although this introduces some measurement error, Ukrainian refugees may be no better informed than what ACLED and UCDP can infer from news reports. Because of these concerns, we use both the number of events as well as the number of deaths from both ACLED and UCDP in the following analysis. Between February 24, 2022, and May 28, 2025, ACLED recorded 85,298 events and 61,446 fatalities, while UCDP recorded 11,099 events and 157,015 fatalities. In the following analysis, we only use events that are exactly geocoded or geocoded at the municipality level. This drops less than 1% of the events in ACLED and 15% of the events in UCDP.

We construct a measure of conflict intensity separately for each sample so that a one–standard-deviation change is defined within the same sample. For each first difference in the data (short or long), we calculate the number of events and deaths in both ACLED and UCDP per 30 days between the fielding dates of the respective survey waves. We use survey wave fielding dates rather than individual response dates to avoid endogeneity arising from within-wave response timing.

We then apply the $log(x+1)$ transformation to all four measures. Because these variables capture similar aspects of local conflict intensity and are strongly correlated,[14] we combine them using principal component analysis and use the standardized first principal component, $Conflict_{mt_1t_2}$.[15]

A.1.3 ISW: Territorial control

To capture whether an individual's home district is under Ukrainian control, contested by fighting, or occupied by Russian forces, we construct a daily dataset of the position of the front line. Districts with any occupied area are coded as occupied, so contested districts are included in the occupied category. To construct the dataset, we draw on the (almost) daily updated maps of the war in Ukraine provided by the Institute for the Study of War (ISW) between June 2022 and May 2025 (Institute for the Study of War, The Critical Threats Project, 2023). Since the start of the war, ISW has been providing reports with maps visualizing the state of the war based on publicly available information sourced from news outlets, social media, and satellite imagery. Importantly, these maps include a line approximately indicating the front line of the conflict. The constructed dataset is on the district level (average size of 4,406 km2) rather than the municipality level (average size of 342 km2). This makes it possible to realistically capture

[13] Raleigh and Kishi (2019) compare different conflict datasets and show that in the case of the 2018 conflict in Donbas, Ukraine, ACLED and UCDP give more plausible results than automated conflict datasets. Therefore, we do not use these datasets. They also find that ACLED captures more events that only appeared in non-English speaking media than UCDP, which is an advantage of ACLED in the current context.

[14] Pairwise correlations range from 0.69 to 0.77.

[15] On the long difference sample, the loadings of the first principal component are 0.47 for ACLED events, 0.50 for ACLED deaths, 0.55 for UCDP events, and 0.47 for UCDP deaths.

meaningful changes in the position of the front line with respect to the locality of origin. As municipalities are relatively small, a municipality may be liberated but an adjacent municipality could still be on the front line. By using the district as the level of analysis, we are better able to capture whether localities' status changes from the zone of conflict to being firmly under Ukrainian control. For instance, upon the withdrawal of Russian forces and advancements achieved by the Ukrainian military, several districts in the Kharkiv region were liberated.

We proceed by classifying districts in one of the following two categories:

1. The district is marked as "Under Ukrainian control" if the full district is under control of the Ukrainian government.

2. The district is marked as "Occupied" if any area inside the district is occupied by Russian forces.

Based on these two statuses, we can identify changes in a district's control between two dates. We also classify districts by their status prior to the first date, distinguishing, for example, between districts that were never occupied and those that had been liberated and remained liberated. Table A.3 reports the prevalence of these levels and transitions for both the full sample and the restricted first-differences sample.

A.2 Reweighting

We calculate survey weights to make our results more representative of (i) our baseline sample and (ii) the whole population of Ukrainian refugees. For (i), we estimate a logit model of follow-up response using the same baseline covariates as in the main specifications and obtain predictions of the probability that an individual answers in a given short- or long-difference pair. To calculate the inverse probability weight of every observation, we invert the predicted probability from the logit model.

The weights for (ii) are obtained by taking the weights obtained for (i) and multiplying those by the inverse sampling rate. The inverse sampling rate is given by the number of registered Ukrainian refugees per sex–age bin–destination country cell in December 2022, divided by the number of respondents per cell in the baseline wave. For several of these bins, we have zero respondents in our survey and no data to weight. These are males aged 18-34 in Iceland, Luxembourg, and Malta, 35-64 in Denmark and Iceland, and 65+ in Denmark, Estonia, Luxembourg, Norway, and Cyprus. For Hungary, Moldova, and the United Kingdom, we do not have detailed information on the number of refugees by cell, so we weight these observations only for the whole country.

A.3 Appendix Tables and Figures

Table A.1: Survey waves, number of respondents and timing

Wave	N	First month	Last month	Place of return
1	11,783	June 2022	January 2023	-
2	1,005	September 2022	November 2022	✗
3	1,610	January 2023	January 2023	✓
4	1,411	April 2023	April 2023	✗
5	1,218	July 2023	July 2023	✗
6	1,175	October 2023	November 2023	✓
7	1,112	February 2024	February 2024	✓
8	1,027	June 2024	July 2024	✓
9	961	October 2024	November 2024	✓
10	970	April 2025	May 2025	✓
Total	22,272	June 2022	May 2025	✓

Notes: Number of respondents by wave and first and last interview month per wave. 6,299 wave 1 respondents agreed to be recontacted for follow-up surveys. As wave 2 has been fielded before the end of the baseline survey wave, not all respondents had the opportunity to answer wave 2, which explains the low number of responses. The last column indicates whether or not the place of return in Ukraine is elicited.

Table A.2: Number of answered survey waves per respondent

	Number of respondents
Number of waves	
1	8,754
2	992
3	528
4	293
5	255
6	226
7	196
8	196
9	223
10	120
Total	11,783

Notes: Number of unique respondents by the number of waves they have answered.

Table A.3: Changes in territorial control in the short- and long-difference samples

	short difference		long difference	
	full	waves with return location	full	waves with return location
Remain occupied	0.21	0.21	0.25	0.27
Liberated between interviews	0.05	0.06	0.11	0.10
Remain liberated	0.43	0.41	0.34	0.34
Occupied again between interviews	0.02	0.03	0.00	0.01
Never occupied	0.29	0.29	0.29	0.29
Observations	8698	5769	2484	2084

Notes: Share of observations by (change in) territorial control for the short difference and long difference sample, full and with return location.

Table A.4: Demographic characteristics of the main samples

	Baseline	short difference	long difference
Controls:			
Male	0.11	0.13	0.11
Age 18-24	0.05	0.04	0.04
Age 25-34	0.20	0.21	0.21
Age 35-44	0.27	0.33	0.31
Age 45-54	0.22	0.23	0.23
Age 55-59	0.08	0.06	0.06
Age 60-64	0.10	0.09	0.09
Age 65+	0.08	0.05	0.06
Married or partner	0.56	0.57	0.57
Left behind: partner	0.24	0.25	0.27
Left behind: children	0.17	0.16	0.16
With children under 18	0.56	0.58	0.60
Tertiary educated	0.66	0.74	0.73
Speaks English	0.40	0.49	0.47
Answered in Russian	0.10	0.09	0.11
Large city (>500k)	0.59	0.58	0.61
Small city (50-500k)	0.26	0.29	0.30
Small town, village or rural (<50k)	0.15	0.13	0.09
Municipal population [1000s]	1062.37	1108.13	1150.33
Distance between municipality and host country [km]	1516.34	1566.94	1536.60
Continued job in Ukraine remotely	0.15	0.18	0.18
Left before the 24th of February	0.06	0.07	0.06
Days between survey responses		163.35	566.02
Return intentions:			
Baseline: return soon	0.07	0.04	0.05
Baseline: return when safe	0.57	0.55	0.58
Baseline: do not know	0.24	0.27	0.25
Baseline: settle outside Ukraine	0.08	0.12	0.10
Baseline: prefer no answer	0.03	0.02	0.02
First response: return soon		0.05	0.05
First response: return when safe		0.51	0.58
First response: do not know		0.25	0.25
First response: settle outside Ukraine		0.17	0.10
First response: prefer no answer		0.01	0.02
Second response: Returned to Ukraine		0.03	0.10
Second response: return soon		0.05	0.05
Second response: return when safe		0.46	0.41
Second response: do not know		0.25	0.25
Second response: settle outside Ukraine		0.20	0.18
Second response: prefer no answer		0.01	0.01
Observations	11783	8698	2484

Notes: Descriptive statistics of all individual-level controls and return intentions in the baseline, short difference and long difference estimation sample for whom information on the home municipality and controls are non-missing.

Table A.5: Demographic characteristics of the main samples compared to registry data

	Dataset			
	Baseline	short difference (full)	long difference (full)	TPS (Eurostat)
Female	0.88	0.87	0.89	0.78
18 - 34	0.26	0.24	0.25	0.38
35 - 64	0.66	0.71	0.69	0.53
65 and older	0.07	0.05	0.06	0.08
Czechia	0.05	0.04	0.04	0.10
Germany	0.23	0.28	0.27	0.18
Italy	0.05	0.04	0.04	0.03
Poland	0.28	0.23	0.25	0.36
Spain	0.04	0.04	0.04	0.04
Other	0.33	0.34	0.33	0.29
N	11,783	8,698	2,484	4,377,305

Notes: Gender, age, and destination country distribution of all baseline survey respondents, those in the full short and long difference sample and beneficiaries of Temporary Protection Status (TPS) by December 2022. Data on TPS registrations originates from Eurostat table *migr_ asytpfq*. For detailed descriptive statistics of the baseline, short difference and long difference samples, see Table A.4.

Table A.6: Stated reasons for returning to Ukraine

Reason	(1) Intended reasons to return Refugees abroad intending to return	(2) Hypothetical reasons to return Refugees abroad not intending to return	(3) Realized reasons for return Returnees
Homesickness	49.3	27.5	33.6
Reuniting with spouse or relatives	29.6	17.1	36.5
Situation in Ukraine has stabilized	26.7	34.9	7.3
Professional / work-related reasons	11.0	11.7	13.5
Lack of funds for living abroad	9.8	14.0	16.4
Difficulty adjusting to life abroad	9.1	6.1	11.3
Looking after house or property	9.0	8.6	4.7
Continuing education (self or children)	7.1	2.0	12.0
Feeling not welcome abroad	6.3	5.4	5.1
Inability to find a job abroad	4.4	10.7	10.2
Insufficient support from local authorities	3.4	2.5	4.7
Joining the armed forces	0.3	1.7	0.7
Other reasons	4.2	12.5	7.3
Prefer not to answer	0.5	5.5	0.7
Observations	1,896	1,678	274

Notes: The table reports the share of respondents selecting each reason, pooled across survey waves and unweighted. All three questions asked respondents to choose up to two reasons, so columns sum to more than 100%. Column (1) reports answers to the question "Which of the following are main reasons for you to go back to Ukraine?", asked of refugees abroad who intend to return (waves 4–6). Column (2) reports answers to "If you consider arguments in favor of going back to Ukraine, which of the following would be main reasons for you to go back to Ukraine?", asked of refugees abroad who do not intend to return (waves 4–6). Column (3) reports answers to "What were the main reasons for your return to Ukraine?", asked of respondents who had already returned (waves 2–9). The same set of response options was offered in all three questions. *Observations* reports the number of respondents asked each question.

Table A.7: Follow-up retention and realized return by baseline intention

Baseline intention	Retention rate	Return rate among retained
Return very soon	0.196	0.391
Return when safe	0.254	0.133
Settle outside Ukraine	0.310	0.017
Do not know	0.274	0.042

Notes: The sample includes all baseline respondents with a valid return-intention category (N = 11,783). Retention is defined as being observed in at least one follow-up wave. The return rate is the share ever observed back in Ukraine among retained respondents.

Table A.8: Cumulative return by months since baseline and baseline intention

Baseline intention	6m	12m	18m	24m	30m
Return very soon	0.308	0.418	0.473	0.500	0.531
N	*117*	*91*	*74*	*58*	*32*
Return when safe	0.099	0.124	0.152	0.157	0.178
N	*1,357*	*1,118*	*894*	*737*	*416*
Do not know	0.026	0.034	0.038	0.055	0.055
N	*613*	*505*	*426*	*344*	*201*
Settle outside Ukraine	0.004	0.015	0.018	0.035	0.036
N	*248*	*201*	*167*	*144*	*83*
Total	0.080	0.103	0.122	0.132	0.143
N	*2,335*	*1,915*	*1,561*	*1,283*	*732*

Notes: The sample includes retained respondents observed in at least one follow-up wave. Cells report the share who had returned to Ukraine by the relevant horizon. Return timing combines two sources: whether the respondent is observed back in Ukraine in a follow-up wave, and the respondent's self-reported month of return, collected from those who indicated in a later wave that they had returned. A respondent is counted as having returned by a given horizon if the earlier of these two dates falls within that many months of the baseline interview. N is the number of respondents whose panel reaches that horizon. Estimates are fixed-horizon within-panel return rates, not population return rates.

Table A.9: Predicting return within 24 months, nested logit models

	(1) Pre-determined	(2) + At leaving	(3) + Post-leaving	(4) + Intention	(5) (2) + Intention
Return very soon				5.015***	5.028***
				(0.960)	(0.942)
Return when safe				ref.	ref.
Do not know				0.315***	0.296***
				(0.069)	(0.064)
Settle outside Ukraine				0.095***	0.092***
				(0.056)	(0.054)
Pseudo-R^2	0.007	0.044	0.076	0.160	0.135
AUC	0.568	0.666	0.707	0.785	0.760
Observations	2,800	2,800	2,800	2,800	2,800

Notes: The outcome is observed return within 24 months of baseline. Entries are odds ratios from a logit model, with delta-method standard errors in parentheses and significance stars based on the underlying logit coefficient; "return when safe" is the reference intention category (odds ratio of one). All columns use the same complete-case sample, and controls are added cumulatively. Column 1 includes pre-determined characteristics: sex, age category, tertiary education, answering in Russian, and macro-region of origin. Column 2 adds characteristics measured around leaving Ukraine: having a partner, a partner left behind, children left behind, children under 18, having left before 24 February 2022, and the month of leaving. Column 3 adds post-leaving choices: destination country and whether the respondent continued working for a Ukrainian employer. Column 4 adds baseline return intentions. Column 5 adds baseline return intentions to the Column 2 specification, omitting the post-leaving controls. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.10: The effect of conflict on moving to a third country

Outcome:	Moved to other country outside Ukraine	
	short difference	long difference
	(1)	(2)
Conflict intensity (1 SD)	0.003	-0.001
	(0.002)	(0.006)
Remain occupied	ref.	ref.
Liberated between interviews	-0.001	-0.014
	(0.010)	(0.010)
Remain liberated	-0.000	-0.011
	(0.007)	(0.025)
Never occupied	0.004	0.016
	(0.007)	(0.016)
Observations	8,698	2,484
R-squared	0.059	0.108
Average dep. var.	0.022	0.062
Full baseline controls	✓	✓

Notes: This table shows regression results from equation 1 for whether a respondent moved to another host country, using the full short-difference (column 1) and long-difference (column 2) samples. For details on the specification, controls, and data, see the notes to Table 2. Standard errors clustered at the district level are reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.11: The effect of conflict on mobility (short difference)

Panel A: **Returned (full sample)**					
	(1)	(2)	(3)	(4)	(5)
Conflict intensity (1 SD)	-0.005*	-0.002	-0.003	-0.002	-0.004
	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)
Remain occupied	ref.	ref.	ref.	ref.	
Liberated between interviews	0.017*	0.012	0.008	0.008	
	(0.009)	(0.010)	(0.010)	(0.010)	
Remain liberated	0.000	0.007	0.001	0.001	
	(0.007)	(0.005)	(0.007)	(0.007)	
Never occupied	0.004	0.008	0.002	-0.000	
	(0.006)	(0.005)	(0.008)	(0.008)	
Observations	8,698	8,698	8,698	8,698	8,631
R-squared	0.117	0.133	0.148	0.148	0.178
Average dep. var.	0.034	0.034	0.034	0.034	0.034
Panel B: **Returned to home municipality in Ukraine**					
	(1)	(2)	(3)	(4)	(5)
Conflict intensity (1 SD)	-0.010***	-0.006***	-0.008***	-0.008***	-0.014***
	(0.004)	(0.002)	(0.002)	(0.002)	(0.004)
Remain occupied	ref.	ref.	ref.	ref.	
Liberated between interviews	0.015	0.012	0.008	0.007	
	(0.011)	(0.012)	(0.012)	(0.012)	
Remain liberated	0.014*	0.019**	0.016*	0.016*	
	(0.008)	(0.008)	(0.009)	(0.009)	
Never occupied	0.012	0.017**	0.010	0.007	
	(0.007)	(0.006)	(0.009)	(0.009)	
Observations	5,769	5,769	5,769	5,769	5,769
R-squared	0.072	0.091	0.115	0.115	0.147
Average dep. var.	0.030	0.030	0.030	0.030	0.030
Panel C: **Returned elsewhere in Ukraine**					
	(1)	(2)	(3)	(4)	(5)
Conflict intensity (1 SD)	-0.001	0.002	0.002	0.002	0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
Remain occupied	ref.	ref.	ref.	ref.	
Liberated between interviews	0.006	0.009	0.010	0.009	
	(0.006)	(0.007)	(0.007)	(0.007)	
Remain liberated	-0.010*	-0.008*	-0.008	-0.009	
	(0.005)	(0.005)	(0.006)	(0.007)	
Never occupied	-0.011***	-0.008**	-0.008	-0.011*	
	(0.003)	(0.004)	(0.005)	(0.006)	
Observations	5,769	5,769	5,769	5,769	5,769
R-squared	0.019	0.031	0.046	0.046	0.150
Average dep. var.	0.010	0.010	0.010	0.010	0.010
Initial return intentions and geographic controls	✓	✓	✓	✓	✓
Survey timing FE		✓	✓	✓	✓
Individual controls			✓	✓	✓
Prior conflict				✓	✓
Municipality FE					✓

Notes: This table reports estimates of equation 1 for realized return outcomes in the short-difference sample. Panel A uses the full sample; Panels B and C use observations with reported return location. Columns add controls sequentially: baseline intentions and geographic controls, survey-timing fixed effects, individual controls, prior conflict, and municipality fixed effects. Conflict intensity is measured at the municipality level and territorial control at the district level. Standard errors clustered at the district level are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.12: The effect of conflict on mobility (long difference)

Panel A: **Returned (full sample)**				
	(1)	(2)	(3)	(4)
Conflict intensity (1 SD)	-0.028***	-0.010	-0.012*	-0.010
	(0.009)	(0.006)	(0.006)	(0.007)
Remain occupied	ref.	ref.	ref.	ref.
Liberated between interviews	0.024*	0.049***	0.043***	0.044***
	(0.014)	(0.011)	(0.012)	(0.012)
Remain liberated	-0.014	0.022	0.010	0.012
	(0.032)	(0.031)	(0.032)	(0.032)
Never occupied	-0.022	0.012	-0.007	-0.026
	(0.016)	(0.011)	(0.014)	(0.018)
Observations	2,084	2,084	2,084	2,084
R-squared	0.081	0.109	0.162	0.163
Average dep. var.	0.095	0.095	0.095	0.095
Panel B: **Returned to home municipality in Ukraine**				
	(1)	(2)	(3)	(4)
Conflict intensity (1 SD)	-0.027***	-0.013***	-0.015***	-0.015***
	(0.008)	(0.004)	(0.004)	(0.004)
Remain occupied	ref.	ref.	ref.	ref.
Liberated between interviews	0.049**	0.069***	0.060***	0.060***
	(0.024)	(0.023)	(0.020)	(0.021)
Remain liberated	0.023	0.053*	0.037	0.037
	(0.029)	(0.028)	(0.030)	(0.030)
Never occupied	0.019	0.046***	0.023	0.015
	(0.015)	(0.012)	(0.014)	(0.016)
Observations	2,084	2,084	2,084	2,084
R-squared	0.084	0.108	0.160	0.160
Average dep. var.	0.075	0.075	0.075	0.075
Panel C: **Returned elsewhere in Ukraine**				
	(1)	(2)	(3)	(4)
Conflict intensity (1 SD)	-0.001	0.003	0.004	0.005
	(0.005)	(0.006)	(0.006)	(0.006)
Remain occupied	ref.	ref.	ref.	ref.
Liberated between interviews	-0.025*	-0.019	-0.016	-0.016
	(0.013)	(0.016)	(0.017)	(0.017)
Remain liberated	-0.036***	-0.031**	-0.027**	-0.026*
	(0.012)	(0.013)	(0.013)	(0.014)
Never occupied	-0.041***	-0.033***	-0.030***	-0.041***
	(0.007)	(0.008)	(0.008)	(0.013)
Observations	2,084	2,084	2,084	2,084
R-squared	0.025	0.045	0.083	0.084
Average dep. var.	0.020	0.020	0.020	0.020
Initial return intentions and geographic controls	✓	✓	✓	✓
Survey timing FE		✓	✓	✓
Individual controls			✓	✓
Prior conflict				✓

Notes: This table reports estimates of equation 1 for realized return outcomes in the long-difference sample. Panel A uses the full sample; Panels B and C use observations with reported return location. Columns add controls sequentially: baseline intentions and geographic controls, survey-timing fixed effects, individual controls, and prior conflict. Conflict intensity is measured at the municipality level and territorial control at the district level. Standard errors clustered at the district level are in parentheses.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.13: Coefficient stability and selection on unobservables

	Short difference (1)	Long difference (2)
Uncontrolled coefficient	−0.010	−0.027
R^2	0.072	0.084
Controlled coefficient	−0.008	−0.015
R^2	0.115	0.160
Oster δ ($R^2_{\max} = 1.3\,\tilde{R}^2$)	4.99	1.98
Bias-adjusted coefficient ($\delta = 1$)	−0.006	−0.007

Notes: Coefficients are from the main tables on the effect of conflict intensity, measured in standard-deviation units, on return to the home municipality. The uncontrolled specification includes first-response return intentions and geographic controls (Tables A.11 and A.12, column 1); the controlled specification adds survey-timing fixed effects, individual controls, and prior conflict (column 4). δ is the selection on unobservables, relative to observables, required to drive the coefficient to zero (Oster, 2019), under $R^2_{\max} = 1.3, \tilde{R}^2$. Values are obtained from `psacalc`. Adding municipality fixed effects (Table A.11, column 5) moves the short-difference coefficient to −0.014, so δ is negative in that specification, indicating robustness to any same-signed selection.

Table A.14: Minimum detectable effects and realized power

	Returned		
Panel A: short difference	To Ukraine	To home municipality	Elsewhere in Ukraine
	(1)	(2)	(3)
Conflict intensity (1 SD)	−0.002	−0.008	0.002
	(0.002)	(0.002)	(0.002)
MDE (80% power)	0.006	0.006	0.006
MDE (90% power)	0.006	0.006	0.006
Power at estimate	0.17	0.98	0.17
Observations	8,698	5,769	5,769
Average dep. var.	0.034	0.030	0.010
Panel B: long difference	(1)	(2)	(3)
Conflict intensity (1 SD)	−0.005	−0.015	0.005
	(0.008)	(0.004)	(0.006)
MDE (80% power)	0.022	0.011	0.017
MDE (90% power)	0.026	0.013	0.019
Power at estimate	0.09	0.96	0.13
Observations	2,484	2,084	2,084
Average dep. var.	0.101	0.075	0.020

Notes: For each coefficient on conflict intensity in Table 2, this table reports the minimum detectable effect (MDE) and realized power. The MDE equals $(z_{0.975} + z_q) \times \text{SE}$, the smallest true effect detectable at power q in a two-sided test of size 0.05. Power at estimate equals $\Phi(|\hat{\beta}|/\text{SE} - z_{0.975})$. Standard errors are clustered at the district level, as in Table 2.

Table A.15: Heterogeneity by initial return intentions

	Returned to Ukraine	Returned to home municipality	Returned elsewhere
Panel A: **Short differences**			
	(1)	(2)	(3)
Conflict intensity (1 SD) × Return soon	-0.012	-0.045***	0.007
	(0.023)	(0.014)	(0.014)
Conflict intensity (1 SD) × Return when safe	-0.004	-0.011***	0.002
	(0.003)	(0.003)	(0.003)
Conflict intensity (1 SD) × Don't know	-0.001	-0.001	0.003
	(0.002)	(0.002)	(0.002)
Conflict intensity (1 SD) × Settle outside Ukraine	0.002	-0.002	0.003
	(0.002)	(0.003)	(0.002)
Observations	8,698	5,769	5,769
R-squared	0.148	0.118	0.046
Average dep. var.	0.034	0.030	0.010
Panel B: **Long differences**			
	(1)	(2)	(3)
Conflict intensity (1 SD) × Return soon	-0.050	-0.067**	-0.014
	(0.033)	(0.028)	(0.017)
Conflict intensity (1 SD) × Return when safe	-0.009	-0.024***	0.008
	(0.010)	(0.007)	(0.007)
Conflict intensity (1 SD) × Don't know	0.004	0.006	0.003
	(0.008)	(0.005)	(0.006)
Conflict intensity (1 SD) × Settle outside Ukraine	0.019*	0.018**	0.000
	(0.010)	(0.008)	(0.005)
Observations	2,484	2,084	2,084
R-squared	0.162	0.165	0.085
Average dep. var.	0.101	0.075	0.020
Full baseline controls	✓	✓	✓

Notes: This table reports estimates from equation 1 for the short- and long-difference samples, interacting conflict intensity with baseline return intentions. All columns follow the specification of Table 2 but add interactions between conflict intensity and the levels of first-interview return intentions. For details on the specification, controls, and data, see the notes to Table 2. Standard errors clustered at the district level are reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.16: The effect of conflict on return by region

Outcome	Returned to home municipality		Returned elsewhere	
	South and East Ukraine	North, West and Central Ukraine	South and East Ukraine	North, West and Central Ukraine
Panel A: **short difference**				
	(1)	(2)	(3)	(4)
Conflict intensity (1 SD)	-0.007**	-0.025***	0.002	0.001
	(0.003)	(0.009)	(0.004)	(0.003)
Observations	3,061	2,701	3,061	2,701
R-squared	0.127	0.155	0.070	0.064
Average dep. var.	0.022	0.039	0.013	0.006
Panel B: **long difference**				
	(1)	(2)	(3)	(4)
Conflict intensity (1 SD)	-0.019***	-0.042	0.009	0.004
	(0.007)	(0.032)	(0.008)	(0.011)
Observations	1,112	961	1,112	961
R-squared	0.174	0.253	0.123	0.191
Average dep. var.	0.057	0.097	0.030	0.009

Notes: This table reports estimates from equation 1 for the same specification as Table 2, but splitting the sample by region of origin (South and East Ukraine versus North, West, and Central Ukraine). For details on the specification, controls, and data, see the notes to Table 2. Standard errors clustered at the district level are reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.17: The effect of conflict on return intentions including returnees

	Returned, return soon or when safe	Settle outside Ukraine
Panel A: **Short differences**		
	(1)	(2)
Conflict intensity (1 SD)	0.014**	-0.001
	(0.007)	(0.005)
Remain occupied	ref.	ref.
Liberated between interviews	0.009	0.006
	(0.025)	(0.013)
Remain liberated	0.017	0.008
	(0.018)	(0.014)
Never occupied	-0.019	0.002
	(0.020)	(0.013)
Observations	8,698	8,698
R-squared	0.430	0.491
Average dep. var.	0.542	0.198
Panel B: **Long differences**		
	(1)	(2)
Conflict intensity (1 SD)	0.017*	-0.005
	(0.009)	(0.009)
Remain occupied	ref.	ref.
Liberated between interviews	0.037	-0.029*
	(0.028)	(0.017)
Remain liberated	0.007	0.019
	(0.048)	(0.039)
Never occupied	-0.041	-0.001
	(0.037)	(0.025)
Observations	2,660	2,660
R-squared	0.320	0.316
Average dep. var.	0.555	0.184

Notes: This table reports estimates from equation 1 using the same specification as Columns 1 and 2 of Table 3, but including respondents who have returned by the second interview. Returnees are coded as 1 in Column 1 and 0 in Column 2. For details on the specification, controls, and data, see the notes to Table 3. Standard errors clustered at the district level are reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.18: War expectations and return intentions: IPW and population weights

	Returned to Ukraine		Return soon or when safe		Settle outside Ukraine	
	IPW	IPW × Pop.	IPW	IPW × Pop.	IPW	IPW × Pop.
Time between interviews (100 days)	0.023***	0.014**	-0.043***	-0.034**	0.010	0.013
	(0.007)	(0.007)	(0.011)	(0.014)	(0.007)	(0.010)
Does not think anymore Ukraine would win	0.008	-0.001	-0.099***	-0.092**	0.037**	0.025
	(0.019)	(0.018)	(0.029)	(0.040)	(0.018)	(0.025)
Newly thinks Ukraine would win	-0.002	-0.018	0.066	0.048	-0.054	-0.003
	(0.029)	(0.029)	(0.066)	(0.076)	(0.056)	(0.095)
Observations	1,452	1,444	1,452	1,444	1,452	1,444
R-squared	0.080	0.111	0.314	0.322	0.418	0.421
Average dep. var.	0.072	0.062	0.517	0.526	0.165	0.188

Notes: This table repeats Table 4 using inverse-probability weights (IPW) and the product of IPW and population weights. The outcomes are indicators for having returned to Ukraine, planning to return soon or when safe, and planning to settle outside Ukraine. Standard errors are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.19: Predictors of follow-up response

	(1) Number of waves	(2) short difference $\beta \times 100$	(3) long difference $\beta \times 100$
Return very soon	-0.240***	-0.065	-0.423***
	(0.091)	(0.059)	(0.145)
Return when safe	ref.	ref.	ref.
Settle outside Ukraine	0.112*	0.076	0.204**
	(0.065)	(0.067)	(0.098)
Do not know	0.314***	0.335***	0.455***
	(0.065)	(0.075)	(0.171)
Prefer not to answer	-0.051	0.021	-0.240
	(0.118)	(0.078)	(0.263)
Male	0.251***	0.189**	0.265*
	(0.087)	(0.083)	(0.152)
Tertiary educated	0.268***	0.238***	0.324***
	(0.046)	(0.043)	(0.081)
Speaks English	0.207***	0.143***	0.447***
	(0.044)	(0.041)	(0.109)
Conflict intensity (1 SD)	-0.012	-0.044	-0.042
	(0.039)	(0.034)	(0.079)
Remain occupied	ref.	ref.	ref.
Liberated between interviews	0.047	0.036	-0.435
	(0.075)	(0.079)	(0.487)
Remain liberated	0.058	-0.074	-0.040
	(0.092)	(0.067)	(0.162)
Never occupied	-0.152	-0.110	-0.191
	(0.104)	(0.090)	(0.186)
Observations	9616	424629	77769
Average dependent variable	0.960	0.022	0.034
Estimator:	Poisson	OLS	OLS

Notes: This table shows Poisson estimates on the number of waves answered by individual (column 1) and OLS estimates on whether an individual answers in a given full short- or long-difference (columns 2 and 3), on the sample of all possible short- and long differences, respectively. For the short difference sample, with $N = 10$ waves this means $N(N-1)/2 = 45$ observations per individual. For the long difference sample, this means $N - 1$ observations per individual. All models use the same controls as the models shown in Table 2. Only individuals without missing covariate values are included in the analysis. Measures of conflict and controls for timing are imputed for non-respondents by drawing a date from the empirical distribution of follow-up response dates, and calculating measures of territorial control and conflict in the same way as for respondents. Standard errors clustered at the district level are shown in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.20: Predictors of inclusion in the main analysis samples

	Table 2 (SD)	Table 2 (LD)	Table 3 (SD)	Table 3 (LD)	Table 4
Return very soon	0.744***	0.744***	0.724***	0.724***	0.579***
	(0.069)	(0.069)	(0.068)	(0.068)	(0.077)
Return when safe	ref.	ref.	ref.	ref.	ref.
Settle outside Ukraine	1.265***	1.265***	1.279***	1.279***	1.525***
	(0.100)	(0.100)	(0.101)	(0.101)	(0.141)
Do not know	1.121**	1.121**	1.132**	1.132**	1.118*
	(0.058)	(0.058)	(0.059)	(0.059)	(0.073)
Male	1.177**	1.177**	1.170**	1.170**	1.264**
	(0.088)	(0.088)	(0.087)	(0.087)	(0.116)
Tertiary educated	1.268***	1.268***	1.249***	1.249***	1.350***
	(0.064)	(0.064)	(0.063)	(0.063)	(0.089)
Speaks English	1.223***	1.223***	1.239***	1.239***	1.332***
	(0.061)	(0.061)	(0.062)	(0.062)	(0.083)
Observations	11,001	11,001	11,001	11,001	11,001

Notes: Entries are odds ratios from logit models predicting inclusion in the corresponding analysis sample, with delta-method standard errors in parentheses. "Return when safe" is the omitted baseline-intention category. All columns include the full set of baseline controls used in the main specifications; selected coefficients are reported. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.21 Answering wave $w + 1$ conditional on answering wave w

	(1)	(2)	(3)
In Ukraine during wave w	-0.0391*	-0.0338	-0.0388
	(0.0227)	(0.0236)	(0.0247)
Return very soon		0.0415	0.0487
		(0.0312)	(0.0316)
Settle outside Ukraine		0.0812***	0.0405
		(0.0214)	(0.0258)
Do not know		0.0244	0.0214
		(0.0165)	(0.0179)
Return very soon (wave w)			-0.0449
			(0.0273)
Settle outside Ukraine (wave w)			0.0608***
			(0.0205)
Do not know (wave w)			-0.0274*
			(0.0165)
Observations	8,323	8,323	8,243
Mean dep. var.	0.639	0.639	0.640
R-squared	0.005	0.008	0.012

Notes: The dependent variable equals one if the respondent answers wave $w+1$, conditional on answering wave w. The sample stacks respondent-by-wave observations for waves $w = 3, \ldots, N-1$. All columns include wave fixed effects. "Return when safe" is the omitted category for baseline and contemporaneous intentions. Standard errors clustered at the respondent level are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.22: Robustness test: inverse probability and population weights

Panel A: **short difference**	Returned to Ukraine		Return to home municipality		Return elsewhere in Ukraine	
	(1)	(2)	(3)	(4)	(5)	(6)
Conflict intensity (1 SD)	-0.002	-0.002	-0.009***	-0.009***	0.005	0.006
	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
Remain occupied	ref.	ref.	ref.	ref.	ref.	ref.
Liberated between interviews	0.015	0.016	0.018	0.017	0.018*	0.014
	(0.011)	(0.015)	(0.014)	(0.017)	(0.010)	(0.009)
Remain liberated	0.004	-0.004	0.028**	0.023	-0.005	0.002
	(0.009)	(0.013)	(0.011)	(0.016)	(0.009)	(0.008)
Never occupied	0.004	0.006	0.019	0.015	-0.005	0.008
	(0.011)	(0.011)	(0.012)	(0.015)	(0.010)	(0.010)
Observations	8,698	8,691	5,769	5,764	5,769	5,764
R-squared	0.154	0.210	0.139	0.246	0.076	0.108
Average dep. var.	0.041	0.041	0.036	0.039	0.013	0.012
Panel B: **long difference**						
	(1)	(2)	(3)	(4)	(5)	(6)
Conflict intensity (1 SD)	-0.006	-0.002	-0.016***	-0.019***	0.004	0.009
	(0.008)	(0.008)	(0.005)	(0.007)	(0.006)	(0.005)
Remain occupied	ref.	ref.	ref.	ref.	ref.	ref.
Liberated between interviews	0.051***	0.035*	0.059***	0.038	-0.016	-0.010
	(0.014)	(0.019)	(0.018)	(0.024)	(0.020)	(0.014)
Remain liberated	-0.004	0.033	0.043	0.064	-0.041***	-0.020*
	(0.035)	(0.048)	(0.035)	(0.044)	(0.015)	(0.011)
Never occupied	-0.013	0.023	0.013	0.027	-0.042***	-0.014
	(0.017)	(0.023)	(0.018)	(0.024)	(0.013)	(0.012)
Observations	2,484	2,484	2,084	2,084	2,084	2,084
R-squared	0.176	0.225	0.185	0.281	0.095	0.097
Average dep. var.	0.109	0.111	0.086	0.095	0.022	0.019
Full baseline controls	✓	✓	✓	✓	✓	✓
Weights	IPW	IPW × Pop.	IPW	IPW × Pop.	IPW	IPW × Pop.

Notes: This table reports weighted estimates of equation 1 for return to Ukraine, return to the home municipality, and return elsewhere in Ukraine, using the short-difference sample (Panel A) and the long-difference sample (Panel B). All columns follow the specification in Table 2 but apply different weights. Odd columns use inverse probability weights (IPW) based on a logit model of follow-up response, and even columns use IPW × population weights that match the sample to the distribution of refugees across sex–age–host-country cells using Eurostat data. For details, see Section A.2. Standard errors clustered at the district level are reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.23: Return intentions, short differences (IPW and population weights)

	Return soon or when safe		Settle outside Ukraine	
	IPW	IPW × Pop.	IPW	IPW × Pop.
Conflict intensity (1 SD)	0.016*	0.009	0.002	0.001
	(0.009)	(0.009)	(0.005)	(0.006)
Remain occupied	ref.	ref.	ref.	ref.
Liberated between interviews	-0.029	-0.016	0.013	0.019
	(0.026)	(0.023)	(0.015)	(0.021)
Remain liberated	-0.001	0.003	0.022	0.013
	(0.020)	(0.024)	(0.014)	(0.017)
Never occupied	-0.038	-0.048**	0.019	0.024
	(0.024)	(0.021)	(0.013)	(0.020)
Observations	8,398	8,391	8,398	8,391
R-squared	0.407	0.427	0.467	0.485
Average dep. var.	0.561	0.554	0.175	0.196

Notes: This table repeats the short-difference specifications in Table 3 using inverse-probability weights (IPW) and IPW × population weights. All columns include the same controls as Table 3. Standard errors clustered at the district level are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.24: Return intentions, long differences (IPW and population weights)

	Return soon or when safe		Settle outside Ukraine	
	IPW	IPW × Pop.	IPW	IPW × Pop.
Conflict intensity (1 SD)	0.020	0.002	-0.004	0.004
	(0.012)	(0.014)	(0.010)	(0.014)
Remain occupied	ref.	ref.	ref.	ref.
Liberated between interviews	0.016	0.031	-0.033**	0.010
	(0.025)	(0.021)	(0.015)	(0.028)
Remain liberated	-0.021	-0.046	0.025	0.039
	(0.050)	(0.045)	(0.036)	(0.046)
Never occupied	-0.059	-0.060*	0.015	0.056*
	(0.042)	(0.035)	(0.024)	(0.032)
Observations	2,393	2,393	2,393	2,393
R-squared	0.319	0.356	0.295	0.334
Average dep. var.	0.531	0.522	0.180	0.206

Notes: This table repeats the long-difference specifications in Table 3 using inverse-probability weights (IPW) and IPW × population weights. All columns include the same controls as Table 3. Standard errors clustered at the district level are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.25: Attrition bounds on the return rate

Assumption for attriters	Total	Return very soon	Return when safe	Do not know	Settle outside Ukraine
Attriters never return (lower bound)	0.029	0.077	0.034	0.012	0.005
Attriters return at the observed rate	0.111	0.391	0.133	0.042	0.017
Attriters return at 1.5× observed	0.153	0.548	0.183	0.057	0.023
Attriters return at 2× observed	0.194	0.704	0.232	0.072	0.029
Attriters return at the very-soon rate	0.319	0.391	0.325	0.295	0.275
Attriters all return (upper bound)	0.770	0.881	0.780	0.738	0.695

Notes: The sample includes all baseline respondents with a valid return-intention category (N = 11,783). Each cell combines observed return among retained respondents with the counterfactual return rate for attriters stated in the row. The first and last rows give the Manski no-assumption bounds, holding observed returns fixed and setting attriter return to zero and one; intermediate rows impose progressively stronger assumptions. The very-soon-rate row is a conservative benchmark, not a preferred estimate.

Table A.26: Loadings of the conflict PCA index

	PC1	PC2	PC3	PC4
UCDP events (log)	0.506	-0.577	-0.097	0.633
UCDP deaths (log)	0.520	-0.330	-0.235	-0.752
ACLED events (log)	0.482	0.702	-0.492	0.179
ACLED deaths (log)	0.491	0.255	0.833	-0.032

Notes: The table reports loadings from the principal component analysis of the four between-interview log conflict components. PC1 loads almost equally on all four components and is the standardized conflict-intensity measure used in the main specifications.

Table A.27: Conflict intensity in large urban hromadas

City (hromada)	Oblast	Population (000s)	Conflict index
Donetska	Donetska	979	2.516
Makiivska	Donetska	395	0.955
Zaporizka	Zaporizka	771	0.945
Kharkivska	Kharkivska	1,452	0.599
Dniprovska	Dnipropetrovska	1,000	0.377
Kyiv	Kyiv	2,845	0.149
Mykolaivska	Mykolaivska	496	0.065
Mariupolska	Donetska	475	-0.009
Odeska	Odeska	1,015	-0.084
Kryvorizka	Dnipropetrovska	659	-0.174
Luhanska	Luhanska	473	-0.175
Lvivska	Lvivska	786	-0.784

Notes: The table reports the standardized PCA conflict-intensity measure for the 12 hromadas with the highest within-1 km population in the long-difference estimation sample, sorted by mean conflict intensity.

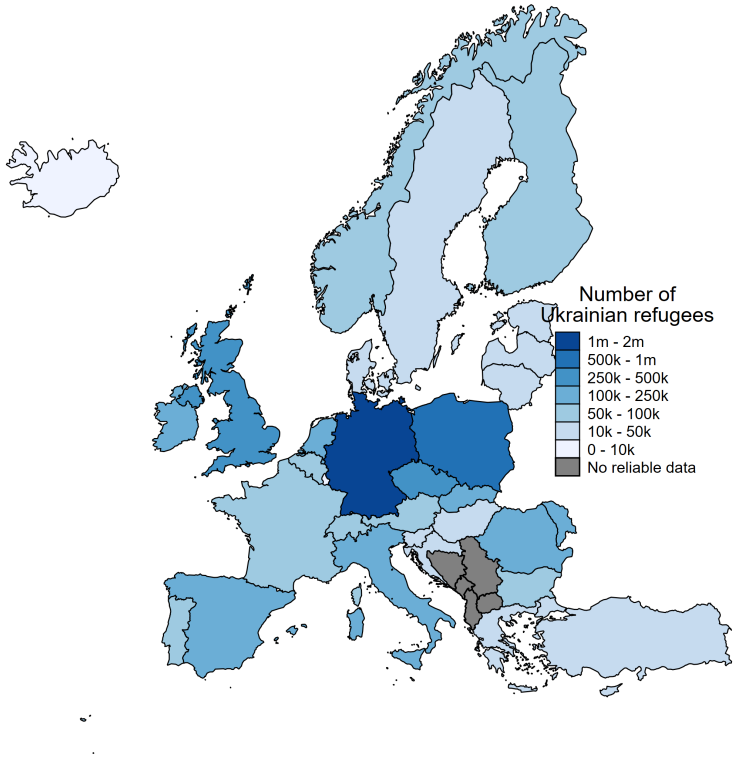
Table A.28: Robustness test: omitting selected occupied areas

Sample:	Returned to home municipality		
		Without respondents from:	
	Long difference	Previously occupied territories	Donetsk and Luhansk
	(1)	(2)	(3)
Conflict intensity (1 SD)	-0.015***	-0.015***	-0.013*
	(0.004)	(0.005)	(0.007)
Remain occupied	ref.	ref.	ref.
Liberated between interviews	0.060***	0.058***	0.059***
	(0.021)	(0.019)	(0.019)
Remain liberated	0.037	0.037	0.037
	(0.030)	(0.030)	(0.030)
Never occupied	0.015	0.012	0.005
	(0.016)	(0.017)	(0.020)
Observations	2,084	2,014	1,860
R-squared	0.160	0.164	0.175
Average dep. var.	0.075	0.077	0.083

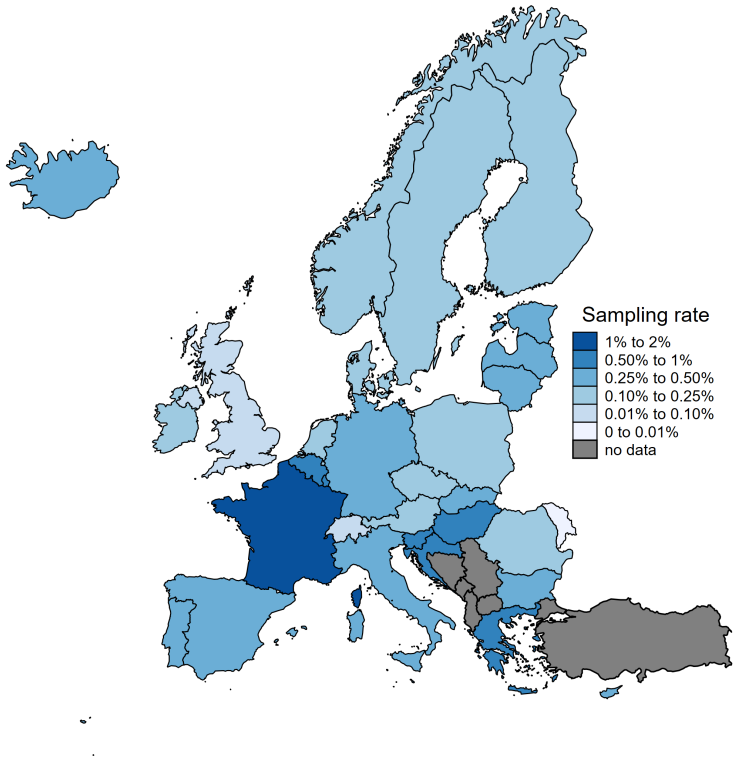
Notes: This table reports estimates from equation 1 for the long-difference sample. Column 2 excludes respondents from territories that were already occupied prior to 24 February 2022, and Column 3 excludes respondents from Donetsk and Luhansk oblasts. For details on the specification, controls, and data, see the notes to Table 2. Standard errors clustered at the district level are reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Figure A.1: Number and sampling rate of Ukrainian refugees

(a) Number of Ukrainian refugees across Europe

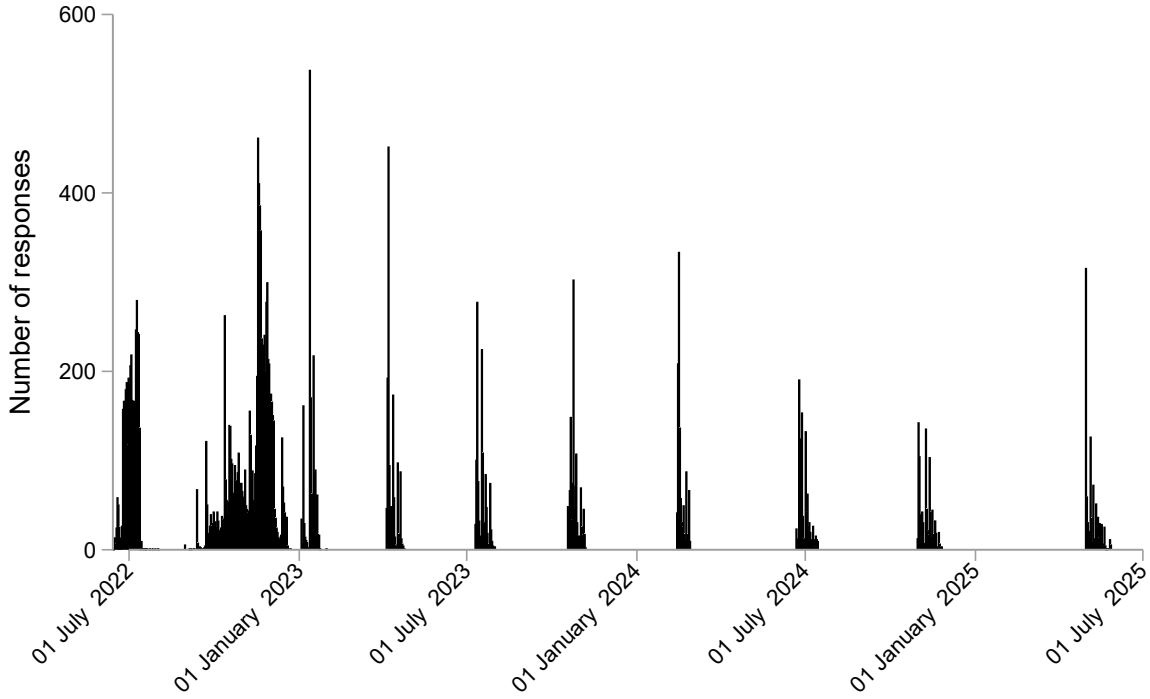


(b) Sampling rate of Ukrainian refugees across Europe



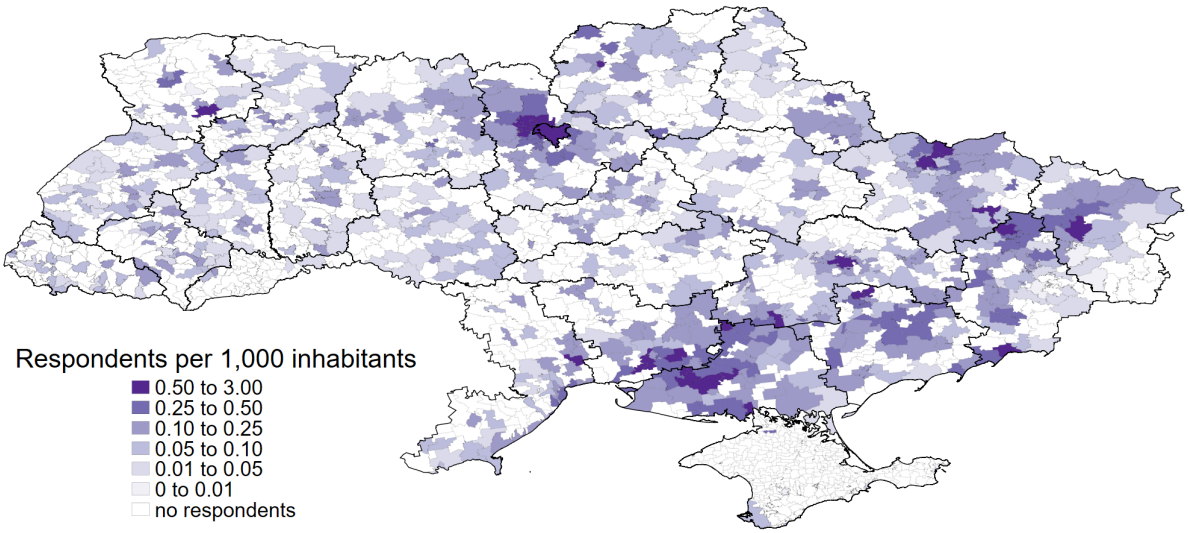
Notes: Panel (a) shows the number of Ukrainian refugees who were beneficiaries of Temporary Protection Status by December 2022, by host country, using Eurostat table *migr_asytpsm*. Panel (b) shows the sampling rate by host country, calculated as the number of baseline respondents in each initial destination country divided by the number of Temporary Protection beneficiaries in December 2022.

Figure A.2: Distribution of interview dates



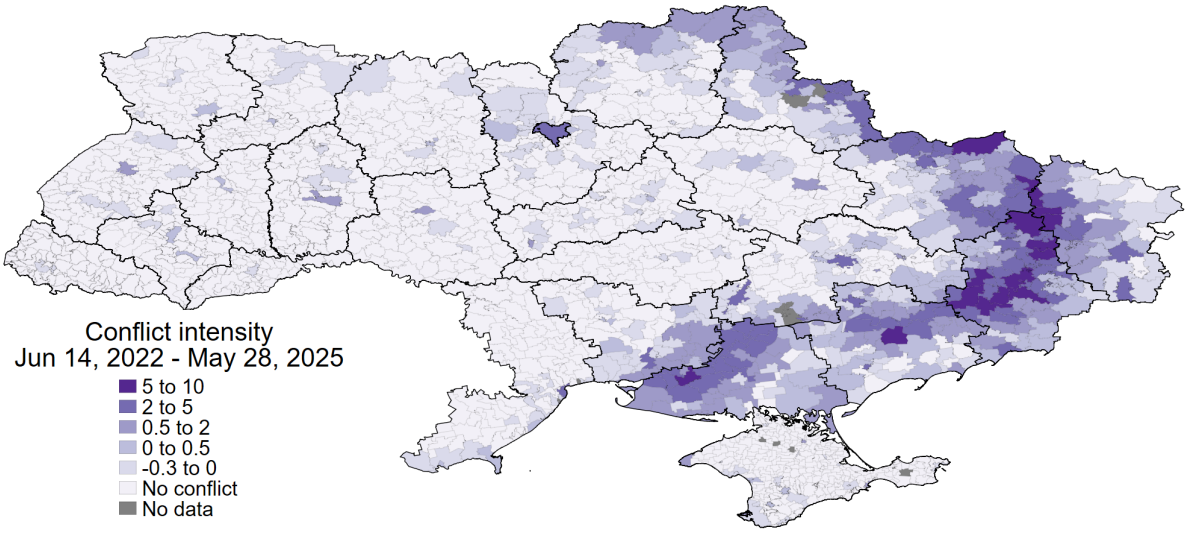
Notes: This figure shows the distribution of interview dates across all ten survey waves. Each bin represents one day. N = 22,272.

Figure A.3: Home municipalities of respondents



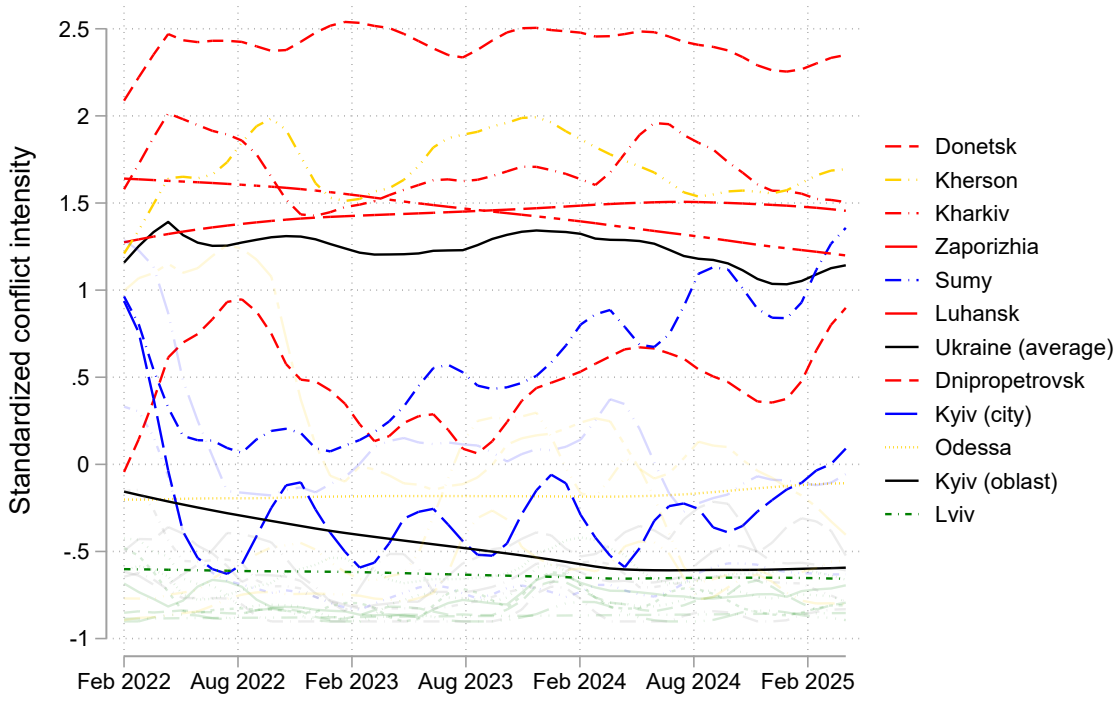
Notes: This figure shows the distribution of respondents by home municipality in Ukraine. Respondents for whom no home municipality could be uniquely determined are excluded. N = 9,677.

Figure A.4: Conflict intensity in home municipalities between first and last interviews



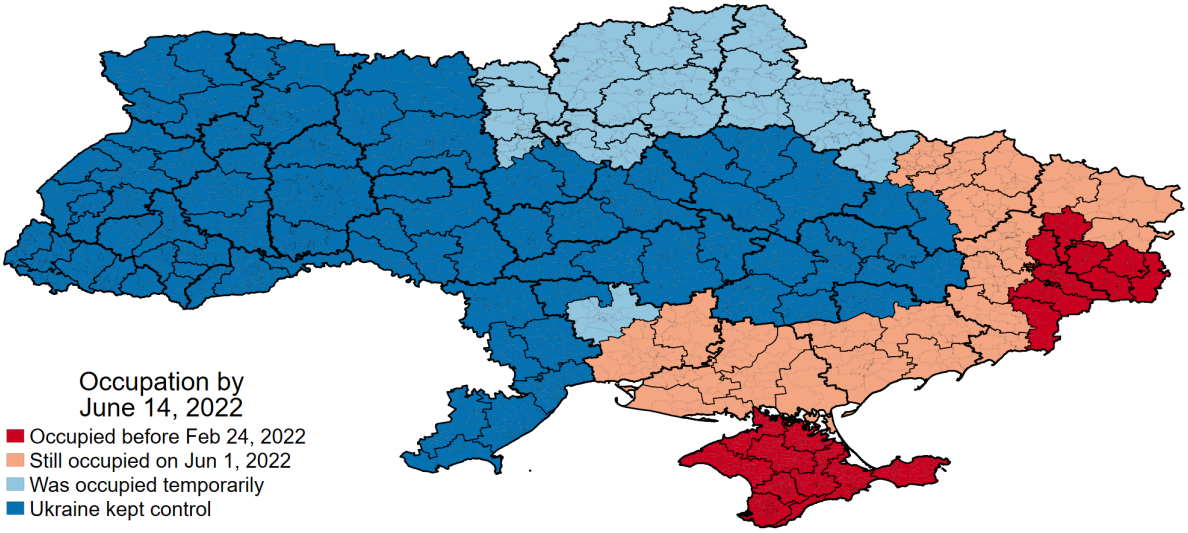
Notes: This figure shows the first principal component of conflict intensity across home municipalities in Ukraine.

Figure A.5: Conflict intensity over time across oblasts of Ukraine



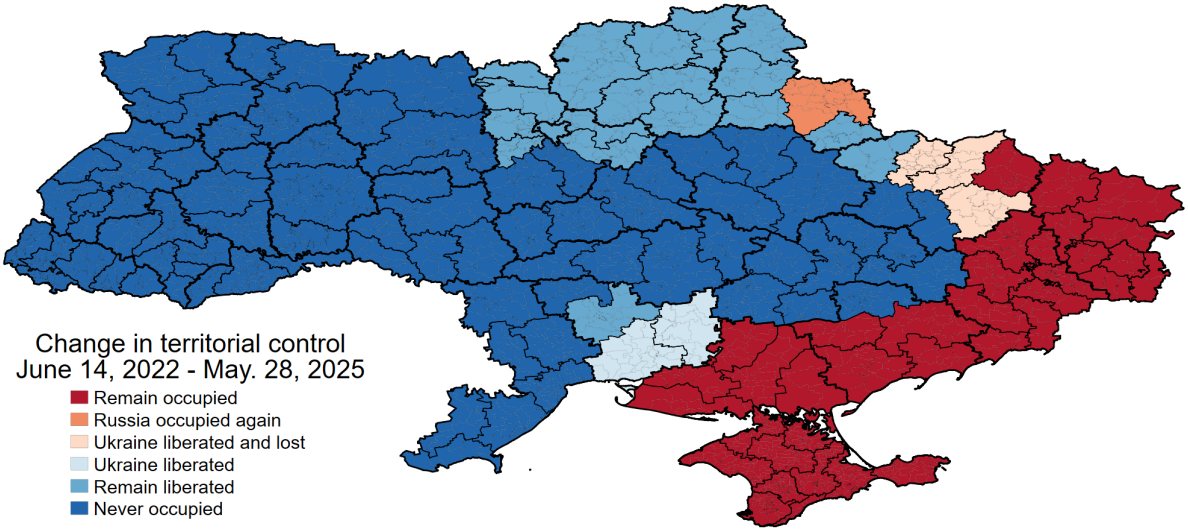
Notes: This figure shows smoothed polynomial trends in conflict intensity across oblasts of Ukraine between February 2022 and April 2025, using an Epanechnikov kernel with a one-month bandwidth. Conflict intensity is constructed analogously to the measure used in the regression analysis: we aggregate the four UCDP- and ACLED-based measures of conflict intensity at the oblast-month level, apply the $\log(x+1)$ transformation, calculate the first principal component, and standardize it. We highlight the oblasts with the highest conflict intensity, as well as Kyiv city, Kyiv oblast, Lviv, and Odessa. Oblasts are grouped by macro-region; Kyiv city is included in the North.

Figure A.6: Territorial control before the first survey wave



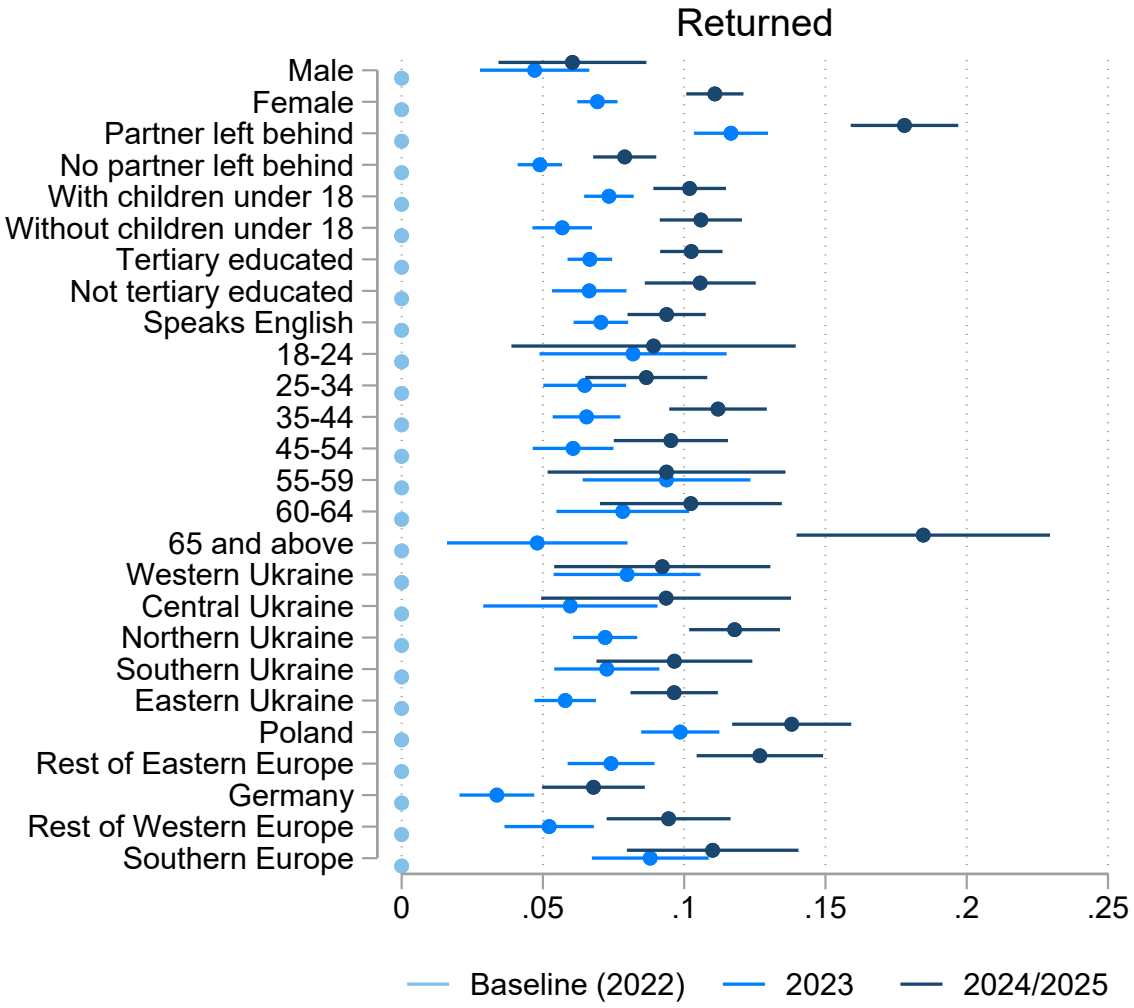
Notes: This figure shows territorial control before the first interview, on June 14, 2022. Occupied districts are either partially or fully occupied.

Figure A.7: Changes in territorial control between first and last interviews



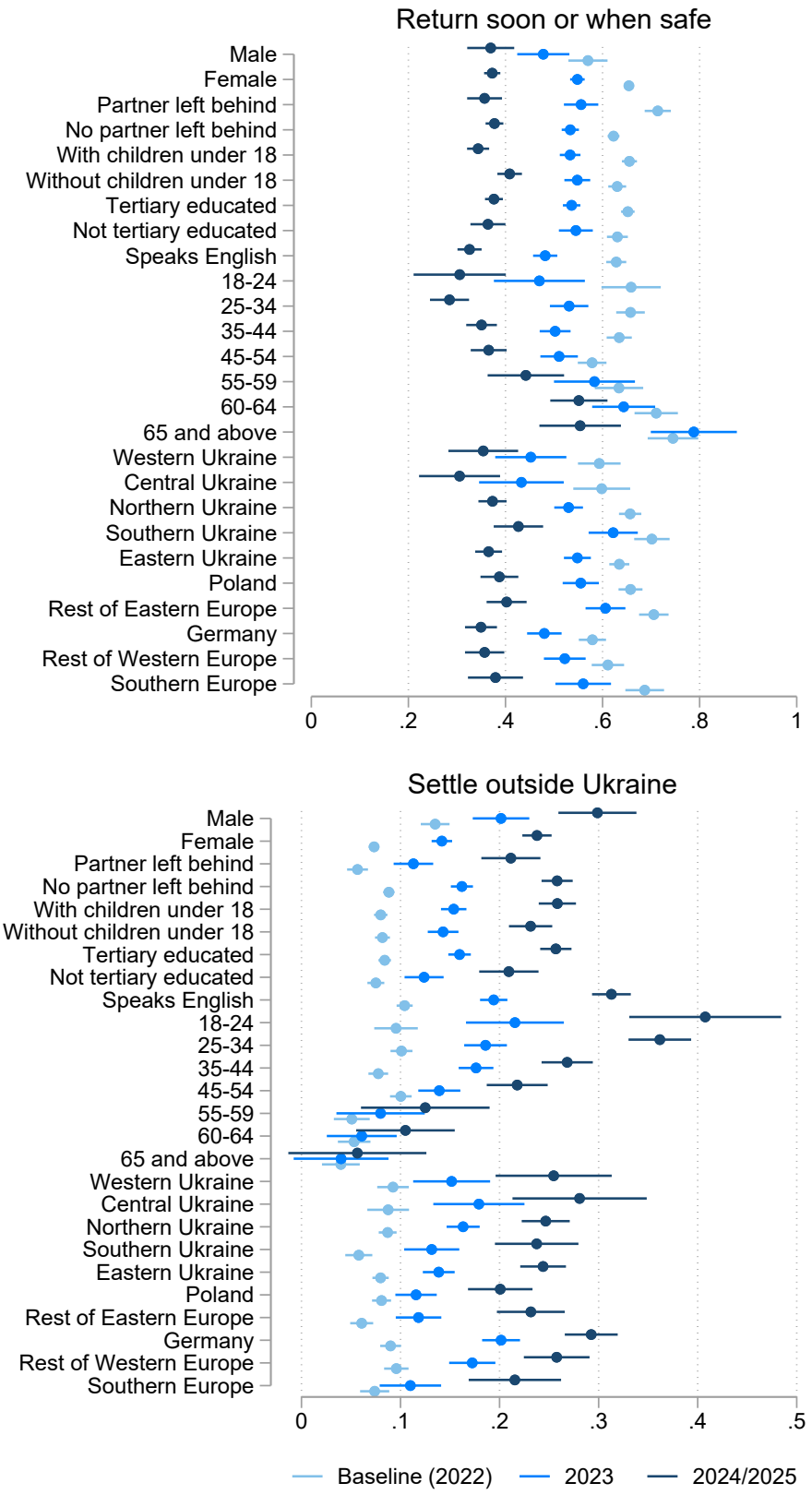
Notes: This figure shows changes in territorial control between the first baseline survey date, June 14, 2022, and the last Wave 10 interview date, May 28, 2025, across home districts in Ukraine. Occupied districts are either partially or fully occupied.

Figure A.8: Predictors of return to Ukraine



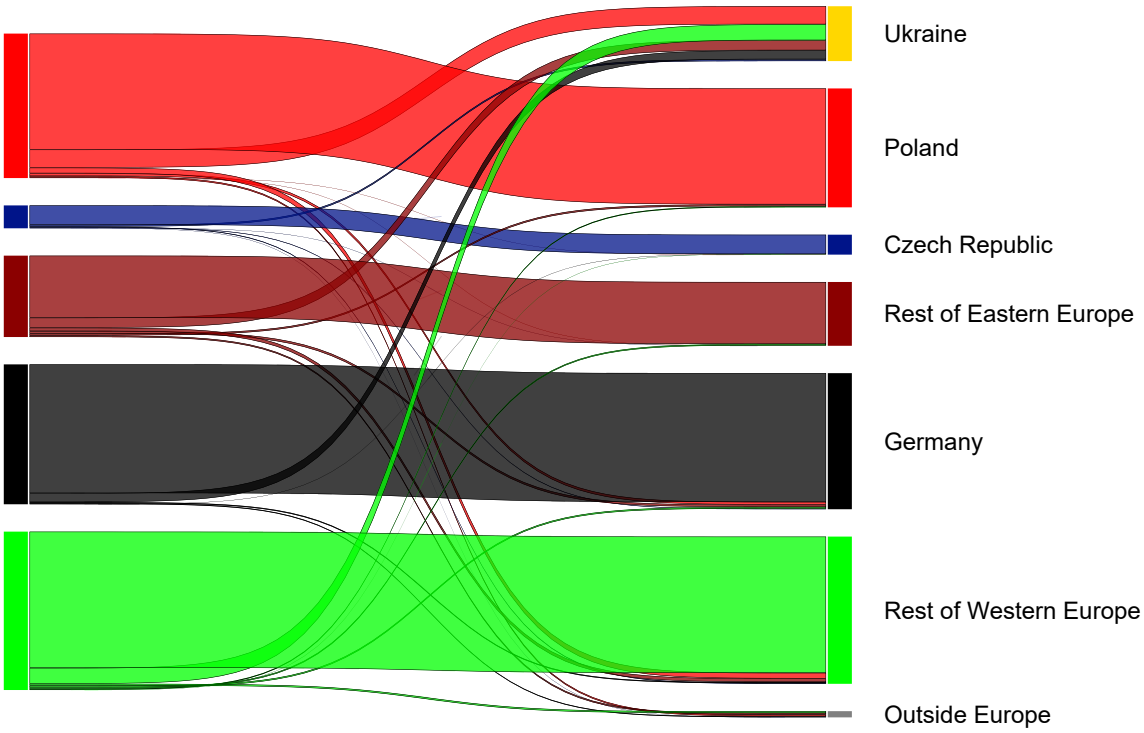
Notes: This figure shows coefficient plots from multivariate OLS regressions with 95% confidence intervals, based on standard errors clustered at the district level. The outcome is an indicator for whether the respondent returned to Ukraine. Estimates are shown separately for the baseline wave in 2022 (N = 9,527), waves 3–6 in 2023 (N = 3,788), and waves 7–10 in 2024 and 2025 (N = 4,721). The specifications include the same controls as Table 2, except for the distance between the initial destination country and the home municipality.

Figure A.9: Predictors of return intentions



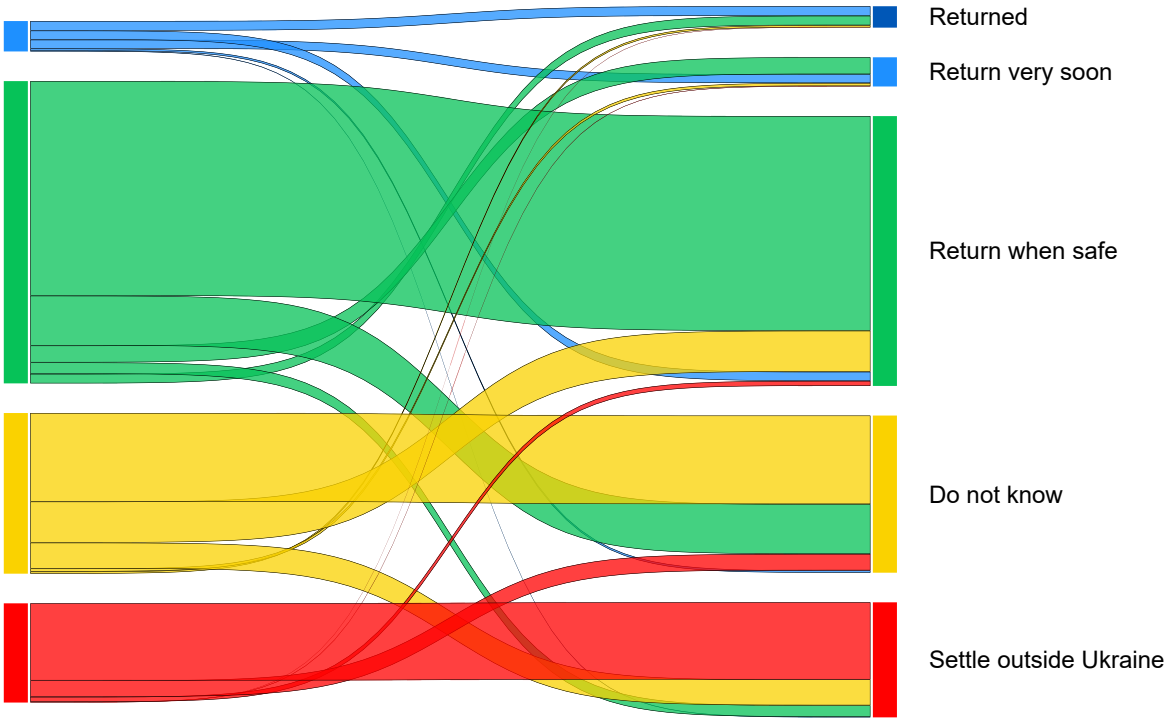
Notes: This figure shows coefficient plots from multivariate OLS regressions with 95% confidence intervals. The outcomes are indicators for planning to return soon or when safe and planning to settle outside Ukraine. The omitted group consists of respondents who had returned, answered "do not know," or preferred not to answer. Estimates are shown separately for the baseline wave in 2022 (N = 9,527), waves 3–6 in 2023 (N = 3,788), and waves 7–10 in 2024 and 2025 (N = 4,721). The specifications include the same controls as Table 2, except for the distance between the initial destination country and the home municipality.

Figure A.10: Changes in residence over time: long differences



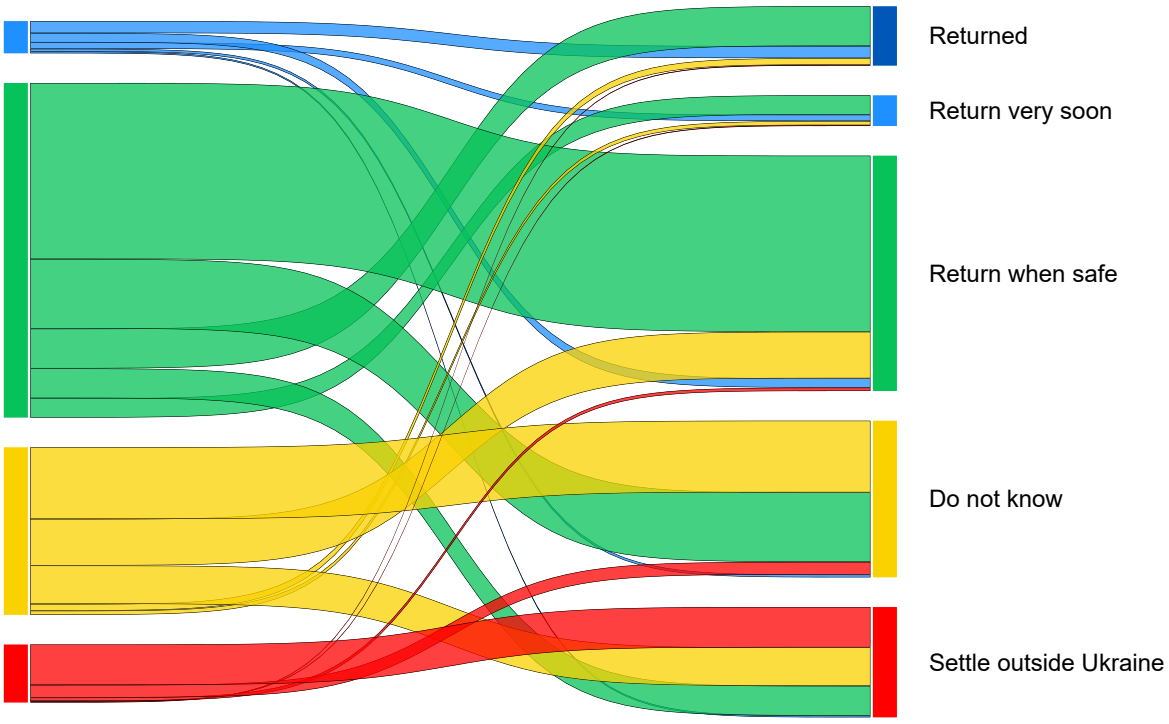
Notes: This figure shows changes in current location in the long-difference sample, including respondents whose home municipality is unknown. N = 2,991.

Figure A.11: Changes in return status and intentions over time, short differences



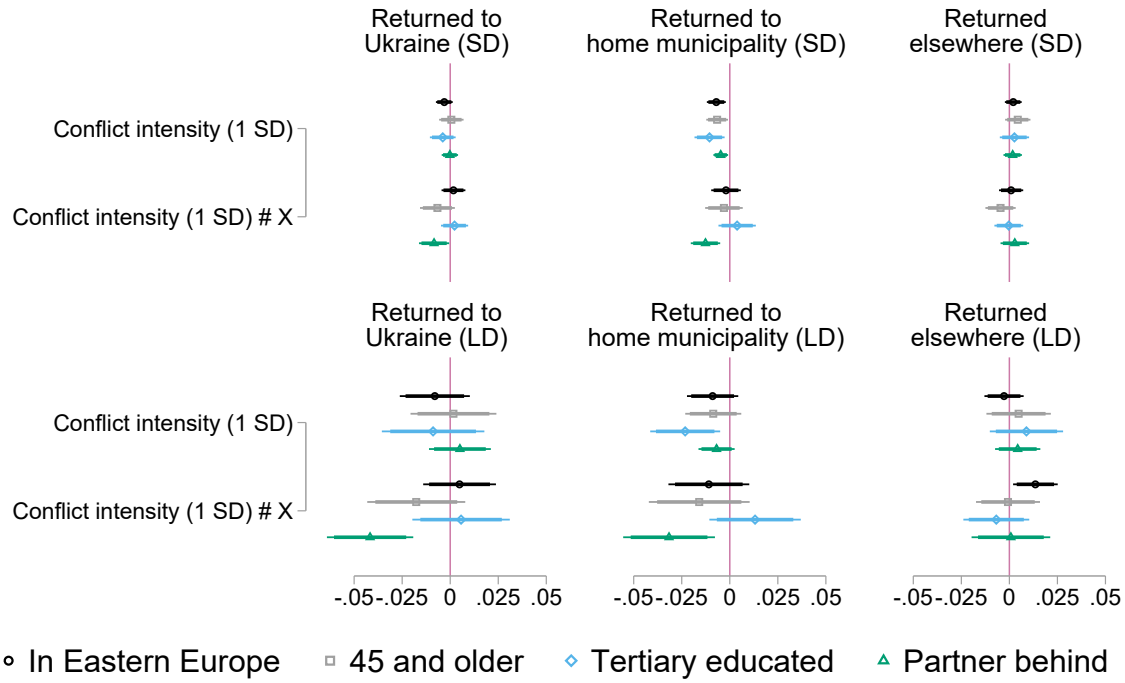
Notes: This figure shows changes in return status and return intentions in the full short-difference sample, including respondents whose home municipality is unknown. N = 9,741.

Figure A.12: Changes in return status and intentions over time, long differences



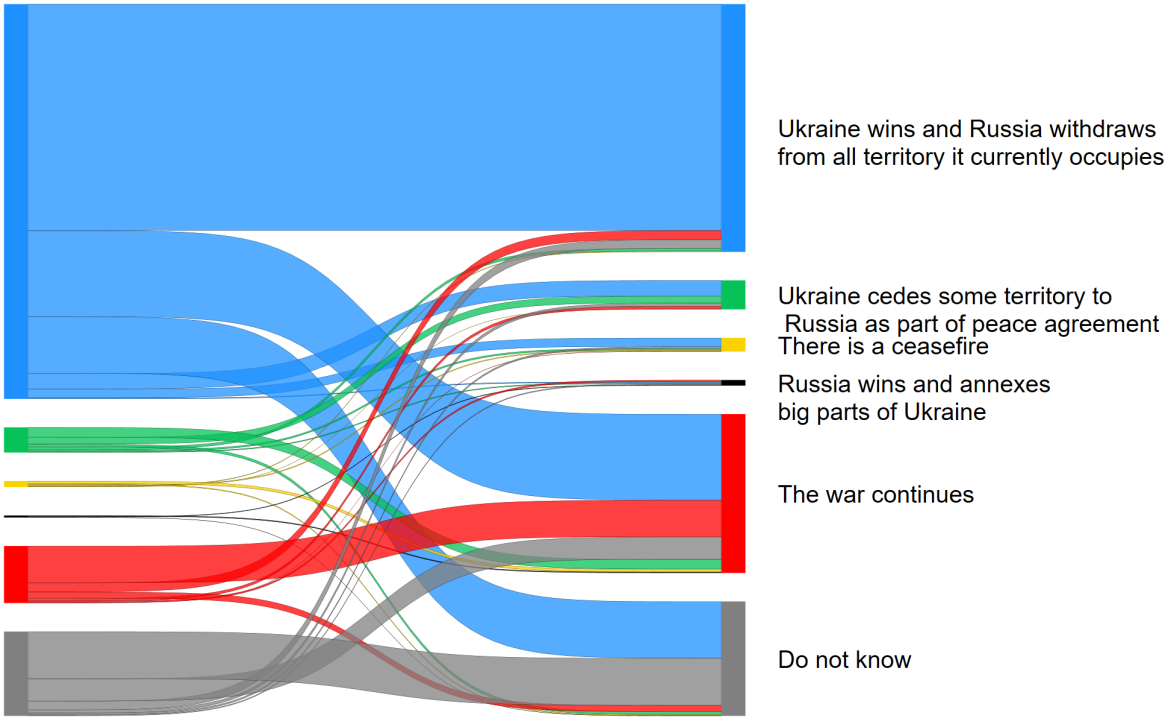
Notes: This figure shows changes in return status and return intentions in the full long-difference sample, including respondents whose home municipality is unknown. N = 2,991.

Figure A.13: Heterogeneity in the effects of conflict



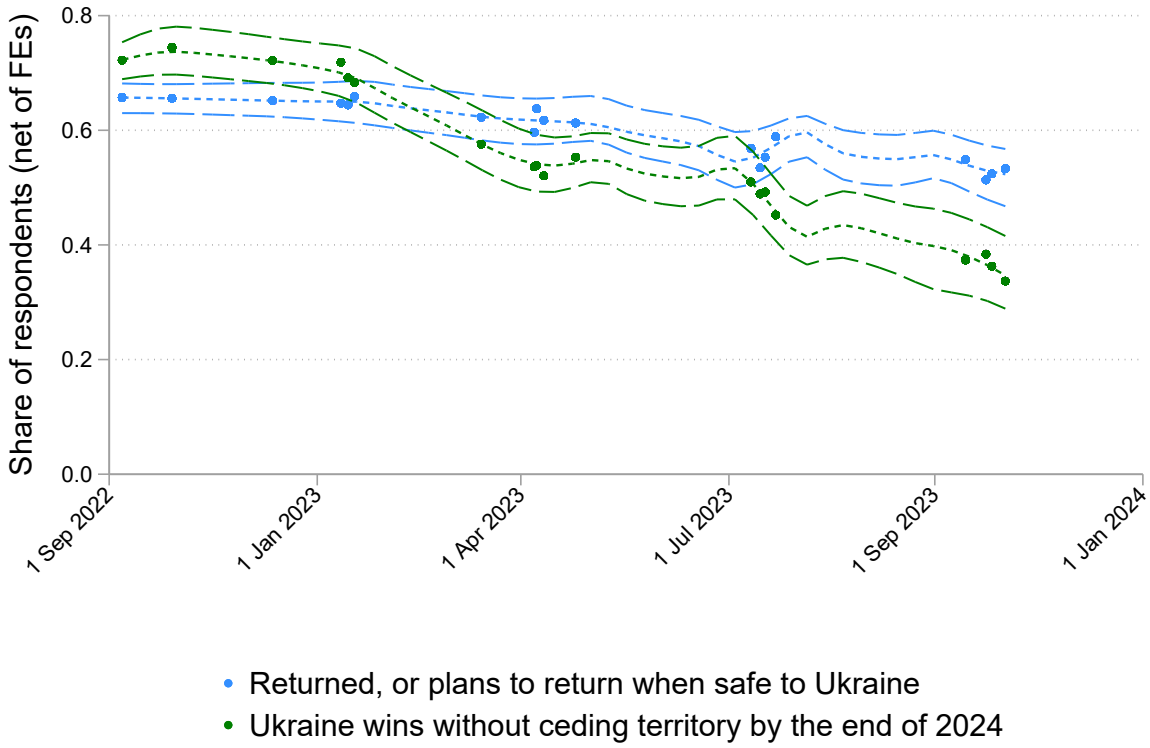
Notes: This figure shows the effect of conflict intensity in the short (top) and long (bottom) difference samples across four dimensions of heterogeneity: residing in an Eastern European country at baseline, being aged 45 or older, having a tertiary degree, and having a partner remaining in Ukraine at baseline. For each dimension, we define a binary indicator and interact it with the standardized conflict-intensity measure in the specification. For details on the specification, controls, and data, see the notes to Table 2. The figure shows 90% and 95% confidence intervals based on standard errors clustered at the district level.

Figure A.14: Changes in expectations about the outcome of the war



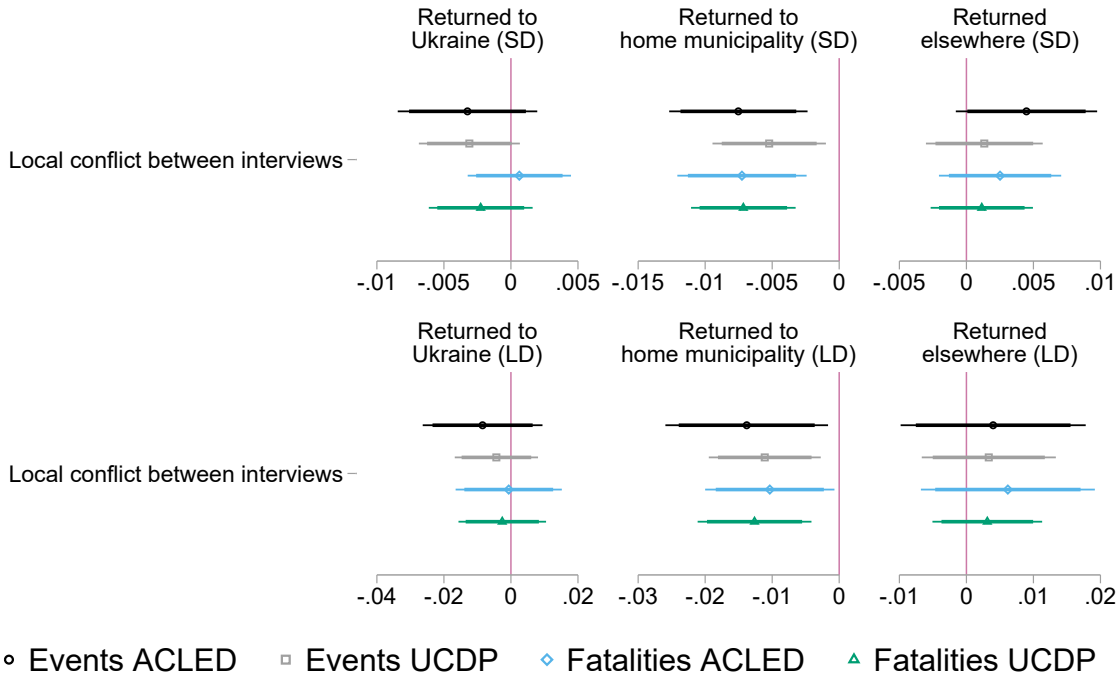
Notes: This figure shows individual-level changes in expectations about the outcome of the war by the end of 2024. The sample consists of first differences between the first interview in waves 2–3 (September 2022–January 2023) and the interview in wave 6 (October–November 2023). The average time between interviews is 243 days. N = 1,624.

Figure A.15: Expectations about Ukraine's victory and plans to return over time



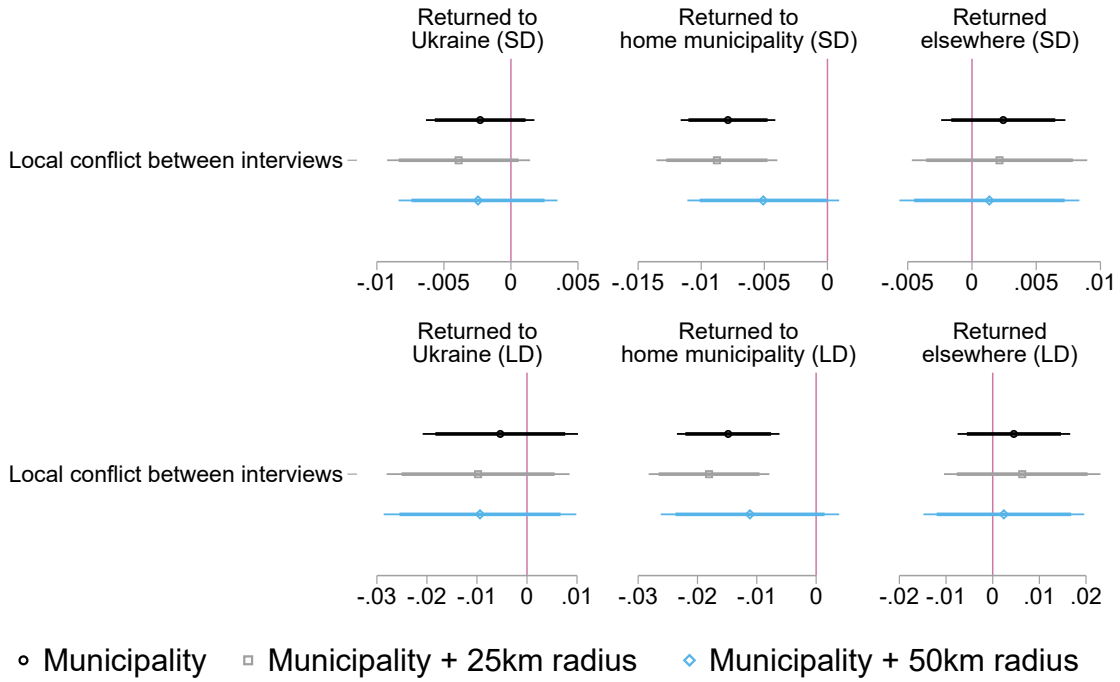
Notes: This figure shows binned scatterplots with non-parametric trends for the share of respondents expecting Ukraine to win the war by the end of 2024 and the share who had returned, planned to return soon, or planned to return when safe. Outcomes are shown over calendar time, net of individual fixed effects, with 90% confidence bands based on bootstrap resampling at the respondent level. The binned scatterplots use 20 bins. For details on the construction of the figure, see the notes to Figure 1. The sample is based on waves 2–6. N = 5,357.

Figure A.16: Robustness to alternative conflict measures



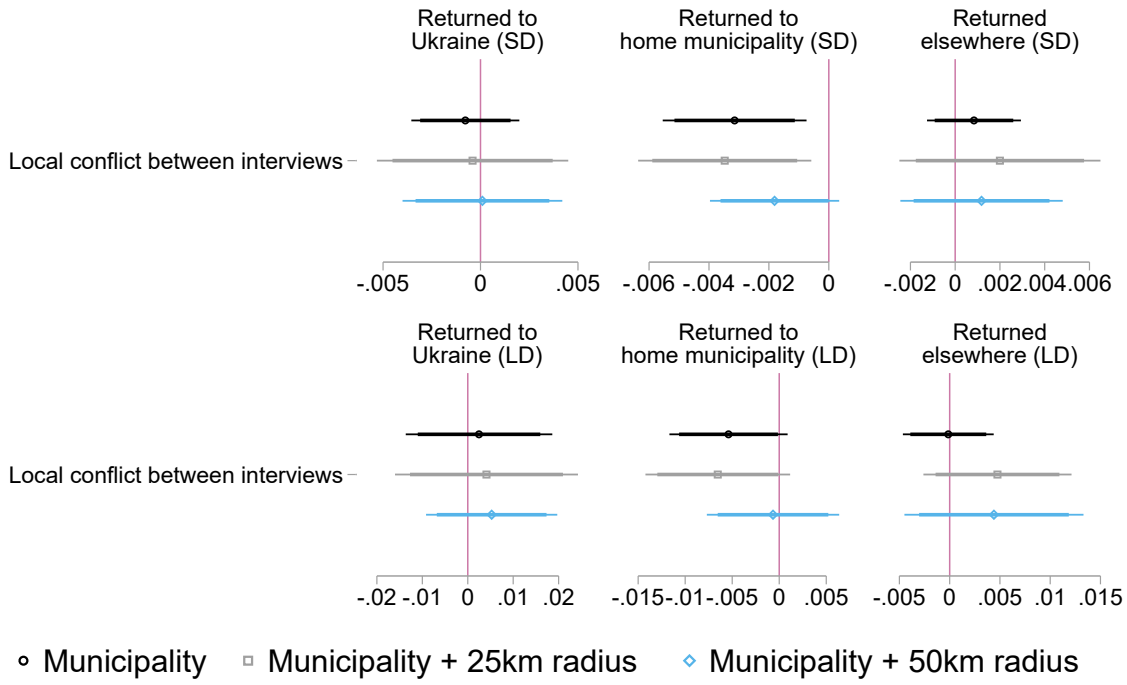
Notes: This figure shows the effect of conflict intensity in the short-difference sample (top panel) and long-difference sample (bottom panel) using alternative measures of conflict. We replace the PCA-based conflict-intensity measure with each of its four components, one at a time: log events in ACLED, log events in UCDP, log fatalities in ACLED, and log fatalities in UCDP, each measured per 30 days between interviews. For details on the specification, controls, and data, see the notes to Table 2. The figure shows 90% and 95% confidence intervals based on standard errors clustered at the district level.

Figure A.17: Robustness to broader measures of conflict exposure



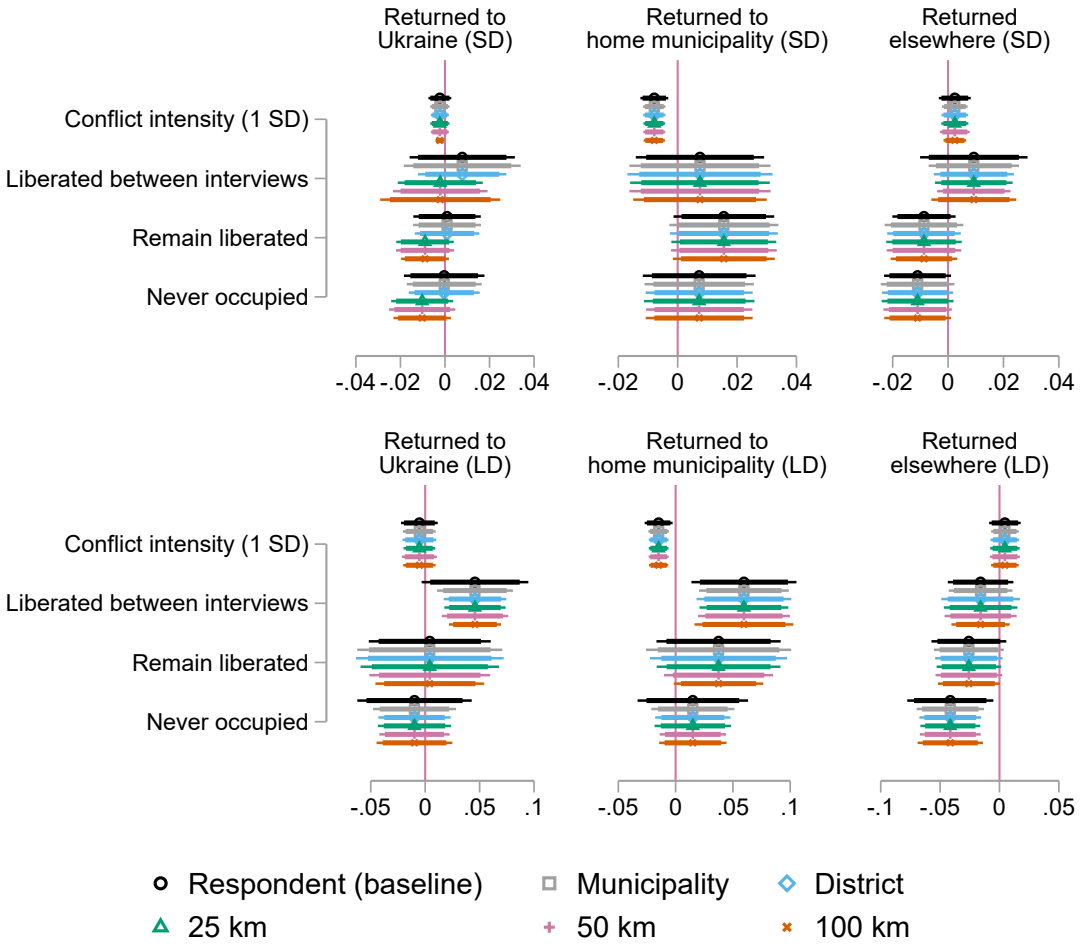
Notes: This figure shows the effect of conflict intensity in the short-difference sample (top panel) and long-difference sample (bottom panel) using broader measures of exposure around the home municipality. We report estimates based on conflict within the home municipality and conflict within 25- and 50-kilometer radii around the municipality. Each estimate is obtained from a separate regression. For details on the specification, controls, and data, see the notes to Table 2. The figure shows 90% and 95% confidence intervals based on standard errors clustered at the district level.

Figure A.18: Robustness to broader measures of conflict exposure, linear specification



Notes: This figure shows the effect of conflict intensity in the short-difference sample (top panel) and long-difference sample (bottom panel) using broader measures of exposure around the home municipality. We report estimates based on conflict within the home municipality and conflict within 25- and 50-kilometer radii around the municipality. Unlike the baseline specification, these measures do not apply the $\log(x+1)$ transformation. Each estimate is obtained from a separate regression. For details on the specification, controls, and data, see the notes to Table 2. The figure shows 90% and 95% confidence intervals based on standard errors clustered at the district level.

Figure A.19: Robustness to spatially correlated standard errors



Notes: This figure shows the effect of conflict intensity in the short-difference sample (top panel) and long-difference sample (bottom panel) under alternative assumptions about the variance-covariance matrix. The figure reports 90% and 95% confidence intervals using standard errors that allow for arbitrary correlation at the individual, municipality, and district levels, as well as within 25-, 50-, and 100-kilometer radii around the home municipality (Colella et al., 2023). For details on the controls and fixed effects, see the notes to Table 2.