The Effect of Gender Norms on Intergenerational Mobility in Türkiye*

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Abstract

This paper investigates whether and to what extent local gender norms causally shape intergenerational educational mobility of women in Türkiye. I combine three waves of Turkish Demographic and Health Surveys (TDHS) between 2008-2018 with retrospective migration histories and construct a region $\times$ year indicator of gender norms for the period 1993-2018. The analysis focuses on daughters aged 21-49 whose mothers attained at most primary school and who migrated internally between ages 12-29. Intergenerational mobility is measured using mother-daughter educational attainment. The empirical strategy compares mobility outcomes of daughters who move across regions at different ages, and exploits variation in exposure to local gender norms generated by differences in age at move. The results show that moving to a more gender-egalitarian region at key schooling margins increases bottom-to-up mobility linearly in exposure time, whereas a placebo outcome – bottom persistence – exhibits no meaningful effect. Heterogeneity analyses indicate that these gains are driven by moves into more egalitarian destinations, while moves into more patriarchal regions do not generate symmetric losses, consistent with the idea that earlier exposure to egalitarian norms is difficult to undo. Additional evidence shows that daughters who move to destinations with smaller gender-norm gaps between origin and destination – i.e., lower cultural distance – experience larger increases in upward mobility, in line with an adaptation mechanism.

Keywords: gender norms, intergenerational mobility, education

JEL codes: I24, J62, R00

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1 Introduction

Intergenerational educational mobility of women is central to understanding how inequality persists or declines across generations (Corak, 2013; Chetty et al., 2014; Branden, 2019; Iversen et al., 2019; Neidhöfer, 2019), yet it is shaped not only by household resources and schooling infrastructure, but also by the social norms that govern women's roles in the family and the labor market (Fernández and Fogli, 2009; Alesina et al., 2013; Alesina and Giuliano, 2015; Jayachandran, 2021). Conservative gender norms can constrain girls' schooling decisions both directly and indirectly.[1] From this point of view, this study aims to provide causal evidence on whether, and to what extent, local gender norms shape intergenerational educational mobility of women in Türkiye.

Türkiye provides a particularly interesting setting to study the links between gender norms and intergenerational mobility. Existing evidence documents low intergenerational mobility and strong persistence in educational attainment, especially for women (Aydemir and Yazici, 2019; Bakış and Filiztekin, 2025; Tansel, 2015). Gender inequality is visible across many domains, from education (Tansel, 2002) and labor market participation (Tunalı et al., 2021) to political representation (Çakır et al., 2024). At the same time, gender norms are both conservative and slow-moving (see Dinçer et al. (2014); Erbay and Usta (2024)). Causal evidence suggests that the gender gap in educational attainment at lower levels is largely driven by patriarchal parental attitudes (Caner et al., 2016). These constraints interact with sizeable cross-regional gaps in human capital and welfare outcomes ((Aşık et al., 2023; Pamuk, 2014)).[2] While these patterns are widely discussed in academic and policy circles, there is relatively little evidence on the causal role of gender norms themselves.

Using migration patterns across regions and variation in age at move between ages 12 and 29, I assess the role of gender norms in shaping intergenerational educational mobility by asking

[1] Direct constraints arise from limited parental aspirations (e.g. Zafar (2013); Huber and Paule-Paludkiewicz (2024)), while indirect restrictions operate through lower perceived returns to education and reduced access to acceptable jobs (e.g. Bertrand (2011, 2020); Cortés and Pan (2023).

[2] For instance, according to statistics from TURKSTAT (2024), the share of 25–34 year-olds with at least upper secondary education is close to 50% in some eastern provinces, but approaches 90% in Ankara, the capital.

whether daughters who grow up in regions with more egalitarian norms exhibit higher upward mobility or lower persistence at the bottom, relative to their low-educated mothers. The empirical analysis draws on multiple waves of the Turkish Demographic and Health Surveys (TDHS) from 1993 to 2018, which allow me both to measure mother–daughter educational mobility and to construct an attitudinal proxy for local gender norms at region $\times$ year level. The main analysis sample consists of women who (i) were aged 21–49 at the time of the survey, (ii) resided in Türkiye during their childhood, and (iii) changed their region only once in 1993 or after at ages 12-29.

A distinctive feature of the TDHS is that it collects retrospective migration histories for women from age 12 onward, which allows me to observe each respondent's origin and destination regions and the year of migration. A simple comparison of women who move to different destinations would confound the impact of gender norms with selection into migration, since migration decisions are endogenous. To address this, I follow an exposure-based approach in the spirit of Chetty and Hendren (2018). Rather than comparing movers to different destinations in levels, I exploit variation in the timing of moves across daughters who experience similar origin-destination gender-norm gaps. Put differently, I compare the outcomes of daughters who move to more egalitarian or more patriarchal regions at different ages. In this setting, the identification assumption is that although individuals/families self-select into migration, the unobserved selection into *when* a daughter moves between a given origin and destination does not systematically vary with her potential gains from more egalitarian norms. In other words, selection may affect outcome levels but it should not generate differential effects across age at move.

In this empirical setting, the identifying variation comes from *when* daughters move, not *where* they move. For a given origin by 5-year birth cohort, destination region, and migration year, daughters face essentially the same difference in gender norms between origin and destination. The key source of variation is that some daughters move earlier and some daughters later.

Thus, they spend a different number of years under the destination's gender norms.[3] Under the age-invariant selection assumption, differences in outcomes by age at move can be interpreted as the causal effect of spending one more year in more egalitarian or more patriarchal environment.

By investigating the impact of gender norms on intergenerational educational mobility of women, this paper contributes to the literature in three ways. First, it constructs mother-daughter mobility measures for a middle-income country setting, where evidence on intergenerational mobility for women remains relatively scarce.[4] Second, for Türkiye specifically, it is, to my knowledge, the first study to map mobility at a fine regional level, documenting substantial spatial heterogeneity in daughters' chances of escaping low educational status.[5] Third, by exploiting variation in exposure to local gender norms over the life course of daughters, the paper provides causal evidence that more egalitarian gender norms during childhood and adolescence substantially raise intergenerational educational mobility and it contributes to the broader "culture" literature (see Bertrand (2011); Alesina and Giuliano (2015); Fernández and Fogli (2009); Luo et al. (2025); Guiso et al. (2006); Sanna (2024)) by bringing intergenerational mobility outcomes from a developing-country setting into a analysis.

The estimated findings point to a clear and intuitive pattern. Gender norms matter most when girls exposed to them around the transition between schooling levels. When daughters from low-educated families move in regions with more egalitarian norms and do so early enough (between ages 12 and 14), they become more likely to climb out of the bottom of the educational distribution. Moves that occur later in schooling career, when most decisions about continuation have already taken, do not show any meaningful impact on bottom-to-up mobility. Consistent with the timing, there is essentially no effect of gender norms on bottom persistence of women.

[3] In section 4, I formalize this as a remainder-age exposure model that relates mobility outcomes to the number of years a girl has left under the destination environment at key schooling ages.

[4] The availability of suitable datasets to observe parent-child characteristics is limited in less-developed and developing countries, and most of the studies focus on father-son pairs (e.g. Azam and Bhatt (2015); Hossain and Beretta (2025); Kundu and Sen (2023)) or on the most educated parent-child combinations (e.g., Alesina et al. (2021); Oztunali and Torul (2022).

[5] Previous studies provide insights on regional patterns (e.g. Aydemir and Yazici, 2019; Bakış and Filiztekin, 2025); but do not directly map mobility.

Once primary schooling is completed, additional years in a more or less patriarchal places do not change the likelihood of staying at the bottom. This combination, namely strong effects on bottom-to-up mobility at early ages and null effects on bottom persistence, suggest that local gender norms operate precisely at the margins where continuation decisions are made. Heterogeneity analysis further suggest that moves to destinations with smaller gender-norm gaps facilitates adaptation (Jancec, 2014; Liu et al., 2024) and increase the probability of upward mobility.

The paper proceeds as follows. Section 2 introduces the Turkish context, describing the education system, patterns in intergenerational educational mobility, and persistent gender norms. Section 3 describes both analysis sample and gender-norm measure. Section 4 outlines identification strategy with possible threats to it. Section 5 presents the main estimates and assesses the validity of empirical strategy. Section 6 tests the robustness of findings across specifications. Section 7 explores underlying mechanisms driving results, and Section 8 concludes with implications for the policy.

2 The Turkish Background

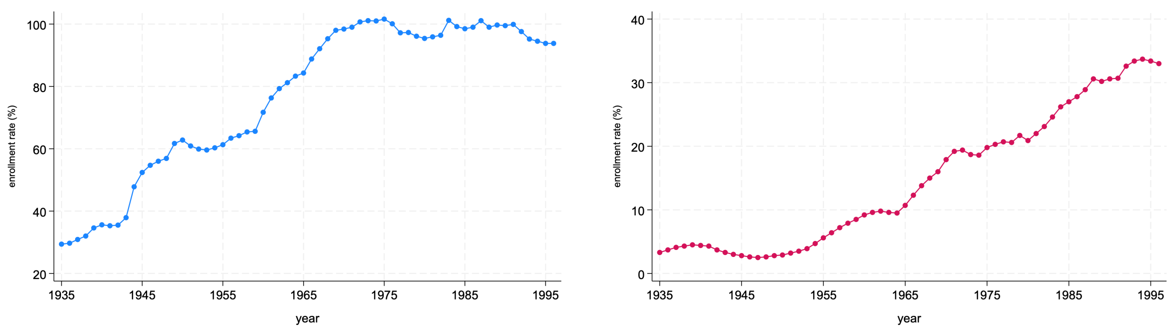
2.1 Education System

Formal education in Türkiye is highly centralized and organized in four main tiers: pre-primary, primary, lower secondary, and upper secondary, followed by higher education. The Ministry of National Education (MoNE) regulates and finances schooling from pre-primary through upper secondary, while the Council of Higher Education oversees universities. Public schools are tuition-free, and since the early 2010s a total of 12 years of schooling has been compulsory. Primary and secondary schooling operate under a common national framework with a standardized curriculum, common exams, and central rules on grade progression and enrollment ages.

Over the last three decades, educational attainment in Türkiye has increased substantially, mainly due to major compulsory schooling reforms. The first and most relevant for this study is the 1997 education reform. Before 1997, compulsory schooling covered only the 5 years of primary school (grades 1–5, ages 6–11). Continuation into lower secondary was not compulsory and required an active decision by families, so completion of grade 5 marked a major margin at which many children left school, especially in poorer or rural areas. The gross enrollment rates at primary and lower secondary school in Figure 1 illustrates this pattern. While primary school enrollment reached nearly 100% by 1970s, lower secondary enrollment remained at about 33% level in 1996, just before the reform. This underscores the limited reach of post-primary education at the time.

Figure 1: Gross Enrollment Rates at Primary and Lower Secondary School

(a) Primary school **(b)** Lower secondary school



Notes: The figures show the gross enrollment rates at primary school in panel (a) and at lower secondary school at panel (b) from 1935 to 1996. The gross enrollment rate at primary (lower secondary) is calculated by dividing the number of primary (lower secondary) school students by the population aged 5-9 (10-14).

The 1997 reform unified primary and lower secondary into a single 8-year basic education cycle and extended compulsory schooling from 5 to 8 years (grades 1–8, ages 6-14). Individuals born in 1987 and later were fully exposed to the new policy.[6] The reform was accompanied by substantial investments in school infrastructure and teacher hiring, particularly outside the major urban centers, and it sharply increased lower-secondary completion rates (Dulger, 2001). Several empirical studies show that the reform significantly raised educational attainment and reduced gender disparities (Kırdar et al., 2016; Erten and Keskin, 2019; Patrinos et al., 2021), whereas its impacts on labor market outcomes were modest (Aydemir and Kırdar, 2017; Torun,

[6]Since there is a possibility of enrolling in school one year later, the 1986 cohort is generally regarded as a fuzzy cohort in studies.

2018; Mocan, 2014).

For this paper, the 1997 reform matters for two reasons. First, by making lower secondary schooling compulsory, it mechanically reduces bottom persistence – the probability of obtaining at most primary school conditional on having a low-educated mother – even in the absence of any change in the local gender norms. Any effect of gender norms on bottom persistence therefore needs to be disentangled from this policy induced shift. Second, Kırdar et al. (2016) provide evidence that the reform also generated spillovers on post-compulsory schooling, particularly upper secondary completion. This implies the estimated effects on bottom-to-up mobility – the probability of obtaining at least upper secondary education conditional on having a low-educated mother – could partly reflect the impact of the policy rather than local gender norms.[7] At the same time, schooling reforms may alter the salience or expression of patriarchal norms within the education system. However, Dinçer et al. (2014), Erbay and Usta (2024), and Erten and Keskin (2022) show that the 1997 reform had no effect on attitudes toward gender roles, domestic violence, and gender stereotypes. So, it suggests that changing in formal schooling levels does not translate into rapid shifts in underlying gender norms.

A second major reform in 2012 extended compulsory schooling from 8 to 12 years (4+4+4) and restructured the system into three 4-year stages: 4 years of primary, 4 years of lower secondary, and 4 years of upper secondary (grades 1–12, ages 6–18). This paper focuses on cohorts born between 1964 and 1997, who were not exposed to the 2012 reform and are therefore unaffected by this later change.

An important feature of the Turkish system is the strong institutional pressure towards continuous, age-synchronized progression. The legal framework is built around a narrow starting age for grade 1,[8] and assumes that students move up one grade per year with their cohort. Grade repetition is formally possible but tightly regulated and relatively uncommon, and late entry or temporary withdrawal are treated as deviations that schools and local authorities are

[7] The main analysis captures any origin differences in enrollment rates by including origin×cohort fixed effects. Additionally, Section 6 formally examines the sensitivity of results to the 1997 reform.

[8] Legally, children who have completed 72 months begin school. However, depending on the child's development, it may be possible to postpone enrollment for up to one year.

expected to minimize. As a result, the age–grade relationship is tight for most cohorts: completion of primary, lower secondary, and upper secondary maps closely to ages around 11, 14, and 17–18, respectively. Intentional "gap years" between stages of schooling are rare. Students who interrupt schooling typically exit into non-enrollment or distance/open education rather than re-entering the standard track. This institutional structure supports the interpretation of "age at move" and "years spent until a given schooling horizon" as proxies for the amount of schooling that can still be influenced by local conditions.

2.2 Intergenerational Educational Mobility

Empirical work on intergenerational educational mobility in Türkiye is still relatively scarce, mainly due to the lack of suitable data linking parents and children across cohorts. Most existing studies rely on surveys with retrospective questions on parental education, census microdata, or specialized modules that include limited mobility-relevant information. This small literature consistently points to low educational mobility and particularly strong persistence in Türkiye.

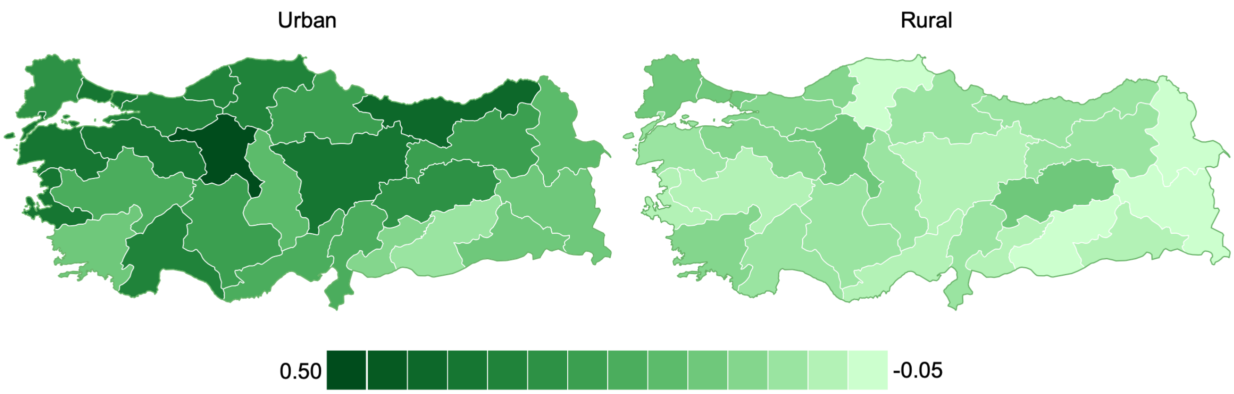
Although these studies rely on different sources, cohort ranges, and mobility measures, they report similar estimates. For father-child (mother-child) pairs, Tansel (2015) provides intergenerational regression coefficients in the range 0.60-0.63 (0.58-0.88) across cohorts, Aydemir and Yazici (2019) obtain estimates of 0.73 (0.77), and Oztunali and Torul (2022) find coefficients between 0.55 and 0.64 across cohorts.[9] The corresponding intergenerational correlation coefficients are 0.43–0.50 (0.35–0.43) in Tansel (2015), 0.42–0.50 in Oztunali and Torul (2022), and 0.56 (0.53) in Aydemir and Yazici (2019). These numbers point to very high persistence in educational attainment in Türkiye, at levels comparable to those reported for several Sub-Saharan African countries (e.g. Alesina et al., 2021).

These figures are already low by international standards when averaged over both genders, but the picture becomes even more concerning once we focus on women. Aydemir and Yazici (2019), for example, report intergenerational regression coefficients of 0.80 and 0.93

[9] Oztunali and Torul (2022) use the parent with the highest educational attainment in the household. Given the sizeable gender gap in educational attainment in Türkiye, this typically corresponds to fathers (see Figure 4).

for father–daughter and mother–daughter pairs, respectively. This indicates an extremely tight link between parental and daughters' education and substantially lower mobility prospects for women than for men. More recently, Bakış and Filiztekin (2025) estimate bottom-to-up mobility at the national level. They find that probability of attaining at least upper secondary education, conditional on having a low-educated parent, is 27.5% for men, but only 14.8% for women. In my sample, bottom-to-up mobility of women, defined with respect to maternal education, is 20.8% among movers and 26.7% among stayers who never changed their childhood region.[10]

Figure 2: Bottom-to-up Mobility across Regions for Stayers



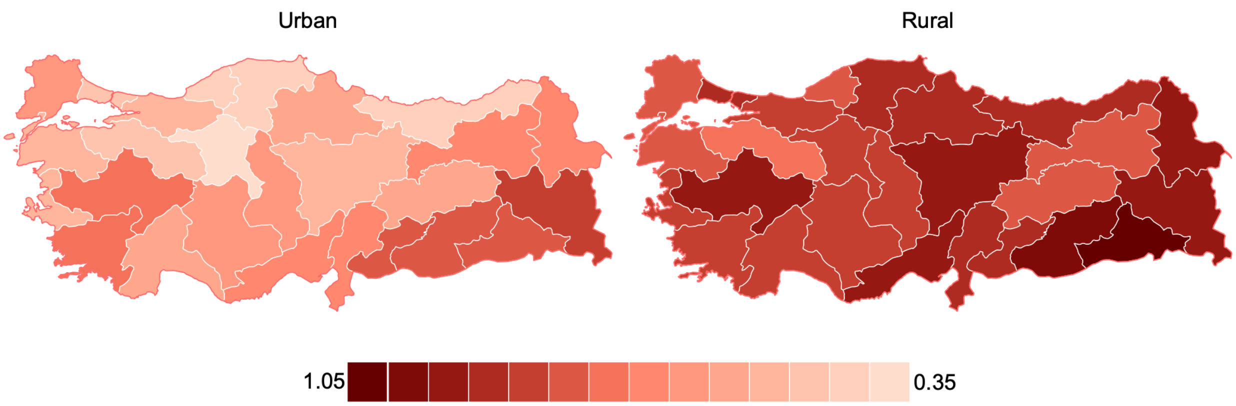
Notes: The figure plots the distribution of bottom-to-up mobility across regions after netting out survey-year effects. The sample consists of women born between 1964 and 1997 who never changed their childhood region. Higher (lower) values indicate regions with higher (lower) upward mobility opportunities for women from low-educated families.

Figures 2 and 3 display the regional distribution of bottom-to-up mobility and bottom persistence among stayers, respectively. These maps provide the main motivation for this study, and reveal two main patterns. First, differences between urban and rural areas within the same NUTS-2 region are dramatic. Women growing up in rural areas always face lower mobility and higher persistence. The unweighted mean of bottom-to-up mobility across regions is 25.7% for urban areas, but only 4.5% for rural areas. The corresponding figures for bottom persistence are

[10]Differences between the estimates of Bakış and Filiztekin (2025) and my figures reflect both the cohort range (1950-1985 vs. 1964-1997) and the definition of parental education (parent with the highest educational attainment vs. maternal education). When I focus on cohorts born between 1955-1995 and use the parent with the highest educational attainment as a measure of parental education, bottom-to-up mobility in my data is 17.2%.

60.7% and 86.1%, respectively. Second, beyond the urban-rural divide, there is substantial heterogeneity across NUTS-2 regions. Conditional on living in an urban area, a woman in Ankara has 46.6% probability of attaining at least upper secondary education, compared to only 5.8% for a woman in the Şanlıurfa region. Similarly, the probability of remaining at the bottom of the educational distribution is roughly twice as high for a women living in Van region (80.9%) as for one living in Ankara (39.9%).

Figure 3: Bottom Persistence across Regions for Stayers



Notes: The figure plots the distribution of bottom persistence across regions after netting out survey-year effects. The sample consists of women born between 1964 and 1997 who never changed their childhood region. Higher (lower) values indicate regions with higher (lower) persistence for women from low-educated families.

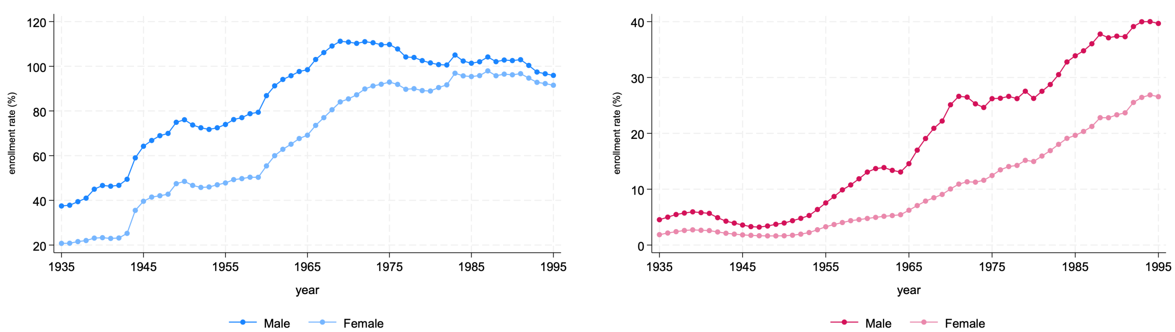
These pronounced spatial and urban–rural disparities highlight that low educational mobility in Türkiye is not a uniform phenomenon. Instead, women's chances of moving out of the bottom of the educational distribution depend strongly on where they grow up. This raises the question of which local factors drive these differences – whether they reflect variation in economic conditions, school supply, policy implementation, or more deeply rooted social norms. The sharp disadvantage of women in rural and eastern regions, combined with persistently conservative gender attitudes documented in other work (see Caner et al. (2016)), motivates a closer examination of the role of local gender norms as a potential mechanism behind the observed heterogeneity in intergenerational educational mobility.

2.3 Prevalence of Gender Norms

Despite the efforts to expand schooling in Türkiye, gender disparities in educational attainment have persisted across cohorts until very recently. Historically, girls were less likely than boys to attend school even at the compulsory schooling level, as shown in panel (a) in Figure 4. Beyond the compulsory level, gender differences became particularly stark: dropout rates rise sharply for girls. Panel (b) from Figure 4 shows that lower secondary attainment increases more in favor of boys, even though both genders experienced improvement over time. As a result, the gender gap in completion at lower secondary schooling and above widens over time from 1935 to 1996.

Figure 4: Gross Enrollment Rates by Gender at Primary and Lower Secondary School

(a) Primary school **(b)** Lower secondary school



Notes: The figures show the gross enrollment rates by gender at primary school in panel (a) and at lower secondary school in panel (b) from 1935 to 1996. The gross enrollment rate at primary (lower secondary) is calculated by dividing the number of primary (lower secondary) school students by the population aged 5-9 (10-14).

These gaps reflect not only economic constraints but also deeply entrenched gender norms that prioritize boys' schooling over girls'. A large body of work documents that parents in Türkiye have historically seen education as more important for sons, while daughters' schooling is viewed as secondary to expectations of early marriage, domestic responsibilities, and limited labor force participation (Alat and Alat, 2011; Caner et al., 2016; Smits and Hoşgör, 2006; Çınar and Köse, 2018). Household-level studies show that poverty, low parental education, and gender-role attitudes all reduce the probability that girls remain in school, especially at the transition from primary to lower secondary (Tansel, 2002; Bakış et al., 2009). In rural and more conservative regions, girls are often withdrawn from school around puberty because continued attendance is perceived as incompatible with modesty norms, safety concerns, or expectations that daughters should help with housework and care responsibilities (Alat and Alat, 2011; Smits and Hoşgör, 2006; Gulesci and Meyersson, 2013; Erten and Keskin, 2019). Parallel to this, Otaran et al., 2004 emphasize traditional attitudes favoring early marriage and unpaid domestic work as major barriers to girls' completion of the lower secondary education.

Empirical evidence confirms that cultural attitudes toward gender roles are a fundamental determinant of girls' lower attainment in Türkiye. Using TDHS, Caner et al., 2016 show that mothers' traditional views strongly predicted daughters' likelihood of leaving school after grade 5, even controlling for household resources. As mentioned in Section 2.1, the 1997 reform, by extending compulsory schooling to 8 years, reduced lower-secondary dropout rates across the country, but the effect was particularly pronounced among girls, narrowing the gender gap in that level (Kırdar et al., 2016). However, Caner et al., 2016 also prove that the reform's impact was highly heterogeneous: in households where parents held egalitarian views, girls' attainment rose sharply, while in households with traditional views, gains were much smaller. The World Bank, 2018 also highlights that although girls' enrollment rates rose substantially after 1997; regional and socioeconomic disparities remained. This indicates that legal enforcement and supply-side expansion were not sufficient to fully offset normative resistance.

The interaction between policy and norms is also visible in subsequent initiatives. Campaigns such as "Haydi Kızlar Okula!" (Girls, Let's Go to School), launched in mid-June 2003 by MoNE and UNICEF, and "Baba Beni Okula Gönder" (Daddy, Send me to School), initiated by a major media foundation in 2005, explicitly targeted traditional families and conservative regions with financial support and community outreach to convince parents to keep daughters in school at the compulsory level (UNICEF, 2003; Aydın Doğan Foundation, 2005). These campaigns imply that the binding constraint for many girls is not only access to schools but also parental beliefs about appropriate roles for daughters.

The TDHS-based attitudinal measures illustrate the scale and spatial dispersion of these gender norms. A non-trivial share of ever-married women agreed with statement “It is better for the male child to be educated than the female child”, with national averages around 30% in 1998, just after the education reform. These attitudes also display substantial regional variation. Particularly, the share of women who prioritize sons’ schooling over girls’ ranges from about 6% to more than 65%. Similarly, the proportion agreeing ”The important decisions in the family should be made by the male family members” or ”Men are usually wiser than women” exceeds 70% in some regions while in others it is as low as around 10%. Finally, a large share of women – between roughly 19% and 96% across regions – agree that woman should not argue with her husband even if she disagrees with him.[11] Overall, these statistics portray a social environment in which many women have limited bargaining power to voice preferences, make decisions independently, or pursue aspirations that conflict with husbands’ and elders’ expectations. In such setting, gender norms can easily dominate girls’ and young women’s preferences regarding schooling and work, pushing them out of school and confining them into the domestic sphere.

From an economic perspective, these norms operate as binding constraint on both the input (education) and the return (labor market participation and earnings) sides. Even when compulsory schooling reforms raise years of education, conservative norms can dampen the translation of additional schooling into formal employment or higher-quality jobs. For instance, in 2024, male labor force participation is 71.4%, while it is only 36.3% for females, remaining low by international standards (The World Bank, 2024). Aydemir and Kırdar, 2017 also find that the 1997 reform had limited effects on female labor market participation, which is consistent with the idea that norms restrict the set of acceptable jobs for women and limit their effective labor supply. A recent work on female labor supply in Türkiye similarly highlights the importance of traditional gender roles, expectations about unpaid work such as household chores and childcare as the key drivers of low participation, even among relatively educated women (Tatoğlu, 2022).

[11] All figures are based on 1998 TDHS microdata and computed at the regional level. Exact values and their use in constructing the gender-norm indicator are discussed in Section 3.2.

In this context, educational attainment is the first rung of the intergenerational mobility ladder, and many women never reach it at the same rate as men. Compulsory schooling policies and targeted campaigns can relax institutional and financial constraints, but they do so within a normative environment that remains patriarchal in many regions. For the purposes of this paper, these patterns underscore why it is essential to model local gender norms as a part of environment in which girls grow up, and to examine how cross-regional variation in those norms shapes both the probability climbing the rungs of the ladder or staying at the bottom of the educational distribution.

3 Data

This study relies on data from the TDHS, a nationally representative dataset that offers rich information on demographic and social characteristics of women aged 15-49. Conducted approximately every five years since 1968, the TDHS provides detailed records on individuals' year and province of birth, childhood place of residence, migration history after age 12, educational attainment, employment, and marriage outcomes. In addition to these demographic characteristics, it includes an extensive section on women empowerment, which captures women's attitudes toward gender equality, decision-making autonomy, and the acceptability of domestic violence. These features make the dataset well suitable for analyzing both intergenerational educational mobility and gender norms within Türkiye.

Based on this information, Section 3.1 describes the sample construction for the analysis, and Section 3.2 explains how gender norms are measured.

3.1 Sample Construction

The main analysis draws on the 2008, 2013, and 2018 waves of the TDHS, which provide consistent information on women's educational attainment, their parents' education levels, and comprehensive migration histories. For each woman, TDHS records education as a continuous measure (years of schooling) and as categories (educational levels). Because parental education is reported only in categories, I reclassify daughters' education on the same categorical scale.[12] This approach minimizes potential measurement error arising from differences in reporting scales and ensures that both generations are evaluated on a consistent educational classification.

Intergenerational educational mobility is measured relative to the mother's education. This focus captures within-gender transmission mechanisms and aligns with evidence that maternal schooling is a strong predictor of daughters' educational investments and aspirations (Carneiro et al., 2013; Karaoğlan et al., 2024). The analysis focuses on women whose mothers had low educational attainment, defined as at most primary school education. Two outcomes summarize mobility:

1. **Bottom-to-up mobility** is the conditional probability that a daughter from the bottom of the maternal distribution attains at least upper secondary education.

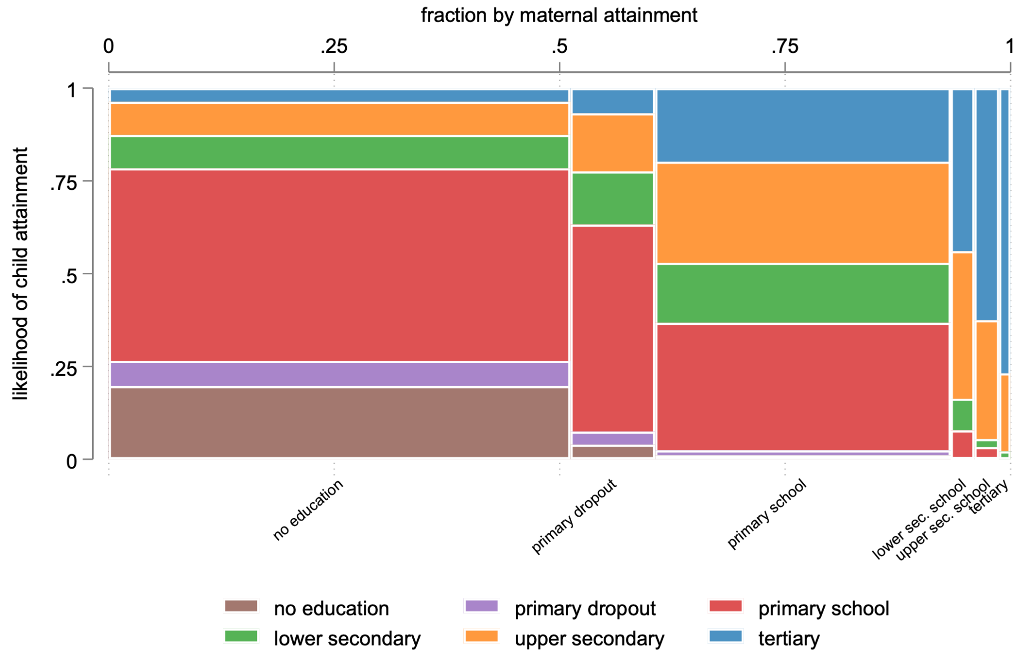
2. **Bottom persistence** is the conditional probability that a daughter from the bottom of the maternal distribution remains at the bottom.

Two mobility measures capture the most relevant dimension of educational mobility in the Turkish context, where earlier female cohorts are concentrated at the lower end of the educational distribution. To illustrate the intergenerational structure of education, Figure 5 shows the mother–daughter transition matrix, highlighting this left-skew: more than half of mothers have no education or diploma while approximately 32.8% completed at most primary school. This motivates focusing on daughters from low-educated families.

A distinctive advantage of the TDHS is the inclusion of women's migration histories, allowing trajectories to be observed with considerable precision. For each migration after age 12, the data report the number of moves, the origin and destination provinces, the residence type (urban

[12]Both parental and own education levels are in six categories, which are: (i) no education, (ii) primary school dropout, (iii) primary school completed, (iv) lower secondary school completed, (v) upper secondary school completed, and (vi) tertiary completed.

Figure 5: Transition Matrix



Notes: The sample includes mothers and their daughters who (i) aged 21–49 at the time of the survey, and (ii) resided in Türkiye during their childhood. Fractions are calculated using sample weights.

or rural) in both locations, the month and year of migration, and the stated reason for migration. This information makes it possible to identify women who (or whose families) moved across regions during childhood and adolescence when schooling-continuation decisions are made. Additionally, having information on the origin and destination as well as the migration year allows me to align each individual's trajectory with time-varying regional gender-norm indicator in the empirical strategy.

The main estimation sample includes women who (i) aged 21–49 at the time of the survey, (ii) resided in Türkiye during their childhood, (iii) changed their region only once in 1993 or after between ages 12 and 29. The regional classification follows the 26 NUTS-2 regions of Türkiye, each divided into urban and rural areas, yielding 52 regions in total. Restricting moves to 1993 and later aligns the timing of each woman's move with the availability of region$\times$year gender-norm indicator, described in Section 3.2. To ensure consistent exposure measurement and avoid complications from multiple moves, I further restrict to single-move cases. These choices balance internal validity (clear timing and exposure) with sample size.

Table 1: Summary Statistics

	Mean	Std. dev.	Min.	Max.	# of obs.
Panel A: Educational outcomes					
Bottom-to-top mobility	0.21	0.41	0	1	3,315
Bottom persistence	0.64	0.48	0	1	3,315
Years of schooling	6.40	3.82	0	21	3,315
Panel B: Individual & family characteristics					
Age	31.20	6.40	21	49	3,315
Father with high education	0.05	0.22	0	1	3,170
Father with no education	0.11	0.42	0	1	3,170
Turkish mother tongue	0.76	0.43	0	1	3,315
Rural childhood	0.56	0.50	0	1	3,312
Panel C: Migration-related information					
Age at migration	20.97	3.82	12	29	3,315
Changed NUTS-2 at migration	0.68	0.47	0	1	3,315
Changed residency type at migration	0.61	0.49	0	1	3,315

Notes: The table presents summary statistics for the estimation sample. It includes women aged 21 or older at the time of the survey, resided in Türkiye during their childhood and migrated only once between the ages of 12 and 29. All women in the sample have low-educated mothers. All reported mean values are calculated using sample weights.

Table 1 reports summary statistics for the estimation sample. Roughly 64% of daughters stays at the bottom of the educational distribution as their mothers, while only 21% achieve upper secondary education. These metrics underscore the restricted upward mobility among women from low-educated background. Respondents, on average, have 6.4 years of schooling and are 31 years old at the time of the survey. Conditional on having low-educated mother, only 5% report a highly educated father and 11% have father without a formal education, which indicates that most of the women in the sample have low parental human capital. More than half lived in rural areas during their childhood, signaling relatively limited local educational resources. On average, women moved at age 21, with 68% relocating to a different NUTS-2 region and 61% experiencing a change in residence type (urban/rural).

3.2 Measuring Gender Norm

This section outlines the construction of the gender-norm indicator using six waves of the TDHS: 1993, 1998, 2003, 2008, 2013, and 2018. These surveys consistently include modules on gender roles, providing reliable data to capture cultural beliefs across regions and time. We construct regional (NUTS2 $\times$ urban/rural) measures to proxy the broader social environment in which women lived, while the empirical analysis is conducted at the individual level.

The gender-norm indicator captures the extent to which respondents endorse patriarchal values and traditional gender hierarchies. It is, basically, based on three agree–disagree statements: (i) Men are wiser than women, (ii) Important decisions are made by men, and (iii) It is better for a male child to be educated than for a female child. Each item is coded as 1 if the respondent agrees and 0 otherwise. Together, these questions capture beliefs about male authority, decision-making dominance, and educational preference for sons, providing a concise measure of the social acceptability of gender inequality.

To align attitudes with migration locations and timing in the empirical analysis, I build region$\times$year measures as follows:

1. ***Aggregation.*** Individual-level responses are aggregated at the NUTS-2 $\times$ residence type (urban/rural) level using sample weights for each survey year, based on each respondent's information at the time of the survey.[13] This produces a set of three gender-attitude item means, namely $\bar{x}_{1,r,s}$, $\bar{x}_{2,r,s}$, and $\bar{x}_{3,r,s}$, for each region r across survey years s.

2. ***Interpolation.*** I linearly interpolate each item's regional mean between survey waves to generate an annual series from 1993 to 2018. This creates a time-varying measure that can be merged to each woman's migration year without imposing structure beyond smooth paths between observed survey points.[14]

3. ***Averaging.*** For each region r year t, I form composite gender-norm indicator as the simple

[13]The construction sample is restricted to women aged 30 and older to capture stable gender attitudes. "Don't know/refuse" category is treated as missing.

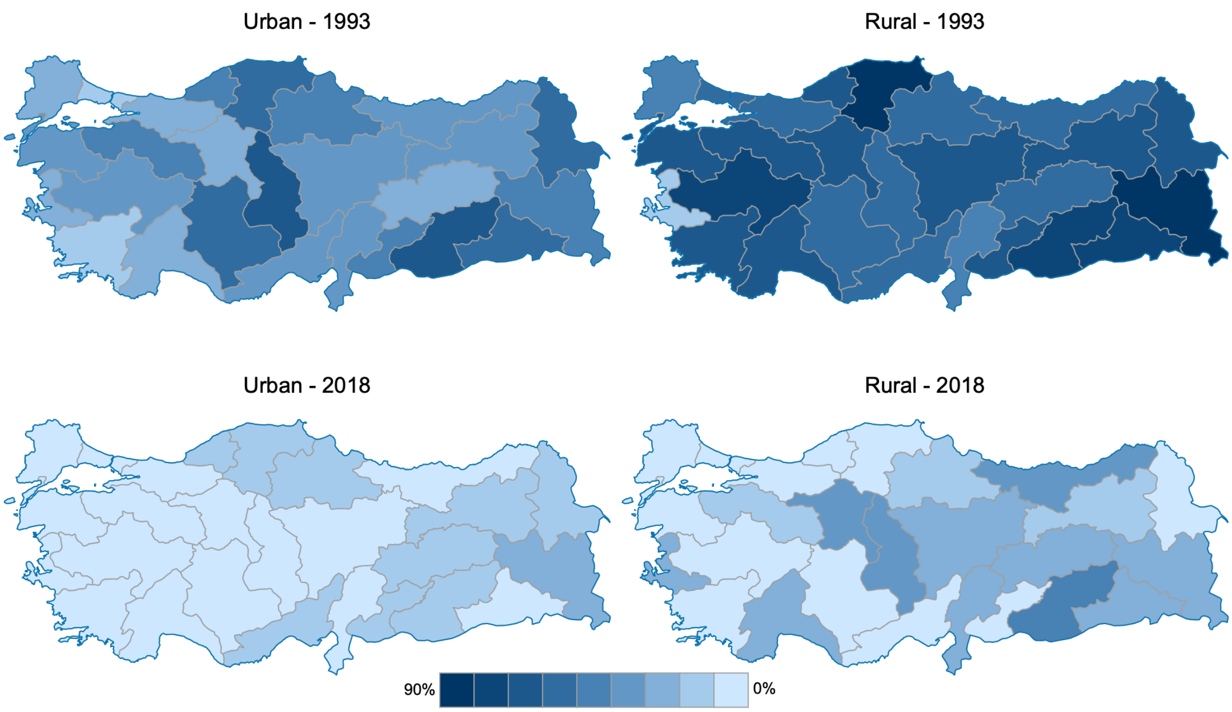
[14]Section 6 verifies that results are similar if norms are lagged or if no interpolation is used.

average of three item means:

$$GN_{r,t} = \frac{1}{3}(\bar{x}_{1,r,t} + \bar{x}_{2,r,t} + \bar{x}_{3,r,t})$$

which is interpretable as the share of endorsing patriarchal statements, ranging between 0 and 1. The choice of a simple average follows common practice in TDHS-based work and keeps the scale transparent.[15]

Figure 6: Distribution of Gender Norms across Regions, 1993 and 2018



Notes: The figure plots the distribution of the gender-norm indicator across regions in selected years. The gender-norm indicator refers to the endorsement of patriarchal attitudes at NUTS-2 × urban/rural regions. Higher values denote more patriarchal values; lower values indicate more gender-egalitarian attitudes. Maps show the first (1993) and last (2018) measurement years.

To visualize the spatial and temporal variation in the indicator, Figure 6 plots the distribution of gender norms across regions (urban and rural areas at NUTS-2 level) in 1993 and 2018, the first and the last years of measurement. The corresponding maps for the remaining

[15]Robustness checks confirm that results are stable when using z-scored items or an index with PCA.

survey years are provided in Figure A1. Each cell shows the fraction of women in a given region and year who endorse patriarchal statements. Lighter shading (smaller fractions) indicates more gender-egalitarian places, whereas darker shading (larger fractions) reflects stronger patriarchal attitudes. The maps highlight three salient patterns. First, there is a pronounced urban-rural divide within the same NUTS-2 regions, particularly in 1993: urban areas tend to be less patriarchal, while rural areas exhibit higher and more homogeneous patriarchal values. Second, beyond the urban-rural split, there is substantial cross-regional variation at NUTS-2 level. Third, between 1993 and 2018, almost every region shows improvement over time, with lower fractions in both urban and rural areas. This evolving pattern motivates exploiting both within-region (over time) and between-region variation in the empirical analysis.

To estimate the causal effect of gender norms on intergenerational educational mobility, I merge the gender-norm indicator with the individual-level dataset based on individual's region of exposure and the migration year. Specifically, for an individual i who moves from origin o to destination d in year t, I attach the origin and destination values of the indicator in that year, i.e., $GN_{o,t}$ and $GN_{d,t}$. For example, a woman who moved from Region A to Regin B in 2002 is matched to $GN_{A,2002}$ and $GN_{B,2002}$. The empirical analysis then uses the difference between destination and origin values as the key explanatory variable, capturing the change in normative environment that a woman experienced upon migration.

4 Empirical Strategy

This paper aims to measure the extent to which cross-regional differences in daughters' intergenerational mobility are causally driven by local gender norms. For this, I ask: *by how much would a daughter's mobility outcomes improve, on average, if she grew up in a place where the gender-norm indicator is 1 percentage point lower*? To answer this question, the empirical setting follows the exposure framework of Chetty and Hendren, 2018 by focusing on women who changed their region between ages of 12-29. Specifically, I test whether movers' mobility outcomes vary systematically with the destination-origin gap in gender norms at the time of the move. While Chetty and Hendren, 2018 study neighborhood effects, I adopt the framework to a semi-parametric exposure profile in which the effect of a 1 pp change in gender norms varies with the time spent in the destination. The key idea is that movers who share the same origin-destination route and move in the same year face the same destination environment, but arrive with differing years of exposure depending on their age at move.

The mobility outcomes in the analysis are binary indicators. Particularly,

- Bottom-to-up mobility = $\mathbf{1}\{\text{daughter}_i \geq \text{upper secondary} \mid \text{mother}_i \leq \text{primary}\}$,
- Bottom persistence = $\mathbf{1}\{\text{daughter}_i \leq \text{primary} \mid \text{mother}_i \leq \text{primary}\}$

Each variable equals 1 if the daughter attains the specified education threshold given her mother's schooling (at most primary), and 0 otherwise.

The key explanatory variable is based on a region $\times$ year gender-norm indicator. For a woman who moves from origin o to destination d in year t, I define the destination-origin gap in norms at the move:

$$\Delta GN_{odt} = GN_{d,t} - GN_{o,t}$$

Since higher GN corresponds to more patriarchal values, a negative ΔGN_{odt} indicates moving to a more egalitarian destination while a positive ΔGN_{odt} means that a woman moved to a more patriarchal environment.

The main specification implements an exposure-years estimand via a **remainder-age** model. Basically, the "age-at-move" concept in Chetty and Hendren, 2018 is mapped into exposure years. [16] Because regional norms can affect schooling only up to certain ages, I set an educational horizon H as the last age at which local gender norms can still plausibly affect the schooling margin, and define remaining exposure:

$$Exposure_i = max(0, H - a_i)$$

[16] Exposure year is mechanically collinear with age at move because the sample includes women who moved only once.

with a_i is the age at move. Intuitively, holding origin, destination, and migration year fixed, moving at 13 yields $H-13$ exposure years, while moving at 17 yields $H-17$ years. In the baseline model, I set $H=21$ and examine $H \in \{16,18,24\}$ as robustness. The semi-parametric baseline equation is:

$$Y_i \;=\; \alpha_{oc}+\delta_d+\theta_t \;+\; \sum_{h=0}^{9} \beta_h \left[\Delta GN_{odt} \cdot \mathbf{1}\{Exposure_i = h\}\right] \;+\; X_i'\gamma+\varepsilon_i \tag{1}$$

where Y_i is either bottom-to-up mobility or bottom persistence (both are indicator outcomes). The model includes origin fixed effects by 5-year birth cohorts (α_{oc}) to absorb origin-specific cohort patterns and destination fixed effects (δ_d) to net out destination place effects. Migration-year fixed effects (θ_t) capture calendar shocks at the time of moving, such as macro cycles and policy timing. The individual-level controls (X_i) include six indicators for father's education, mother-tongue dummies, and survey-wave dummies to capture pre-move characteristics correlated with both migration timing and schooling. Standard errors are clustered at origin region level.

Interacting the gender-norm gap, ΔGN_{odt}, with exposure indicators recovers a profile of coefficients $\{\beta_h\}$ that capture how the effect of moving to a more or less patriarchal place changes with the years of exposure. In other words, each β_h measures the percentage point change in the mobility outcome associated with a 1-pp increase in ΔGN for movers who have exactly h exposure years. In this setting, identification comes from within-cell comparisons among movers who share the same origin by cohort, destination, and move year but moved at different ages. Because they encounter the same destination environment at the time of moving, the only systematic difference is how many school years they will still complete there. This age-at-move variation generates different remaining exposure to destination norms and identifies how mobility outcomes change with exposure time.

There are two points to clarify why this estimand is appropriate. First, the educational horizon H formalizes the idea that local gender norms matter only up to the ages at which the key schooling decisions are made. Beyond H, extra post-move years should not change the outcome, so setting exposure to zero prevents late-teen moves from mechanically diluting the per-year effect. This captures the exposure cutoff in the framework of Chetty and Hendren, 2018 and is consistent with experimental and quasi-experimental evidence showing large benefits for moves when young and weak short-run effects for late-teen moves. Second, moves that occur after H still contribute to identifying the fixed effects but carry $Exposure_i = 0$, as they should if the outcome margin is already decided.

The identification strategy relies on comparisons among movers who share the same origin $\times$ cohort, destination, and move year but differ in how many years remain until the educational horizon. In this setting, the key assumption is *age-invariant selection* – selection into destinations may differ in levels, but does not vary with the child's age at move. In other words, after conditioning on fixed effects and pre-move controls, the exact timing of the move within the window up to H is as-good-as random with respect to child's potential outcomes. Under this assumption, comparing movers on the same route in the same year but with different remaining exposure years isolates the causal effects of gender norms.

4.1 Threats to Identification Strategy

Identification assumption relies not only selection on levels, but also age-invariant selection, which is a strong assumption to satisfy. This section discusses concerns for selection and omitted variable bias, and explains briefly how to address them.

The main threat to the age-invariant selection assumption is *strategic timing* of the move. For instance, families with stronger resources or optimistic expectations might choose to move earlier for daughters who are expected to gain more from egalitarian places and later for those expected to gain less. In this case, selection would vary with age at move and the estimated effect of exposure would be upward biased. I check this issue in two ways. First, I run balance tests, regressing pre-move (observable) covariates on the remainder-age exposure bins within the full set of fixed effects. This examines whether observable characteristics trend with remaining years of exposure. Second, I replace the years-of-exposure with age-at-move indicators interacted with the destination-origin gender-norm gap, similar to Chetty and Hendren, 2018 specification. This allows the effect vary non-parametrically with age at move and provides a direct test for systematic age-dependent gains.

A second concern is anticipatory adjustments before the move even in the absence of strategic timing. For example, a family that plans to move to a large city could adjust schooling choices or investments before migration, mechanically creating effects in the pre-period. To diagnose this, I employ an event-time design, which estimates dynamics around the move by including leads and lags of exposure. Flat and insignificant pre-move coefficients provide evidence against anticipatory adjustments or age-varying selection.

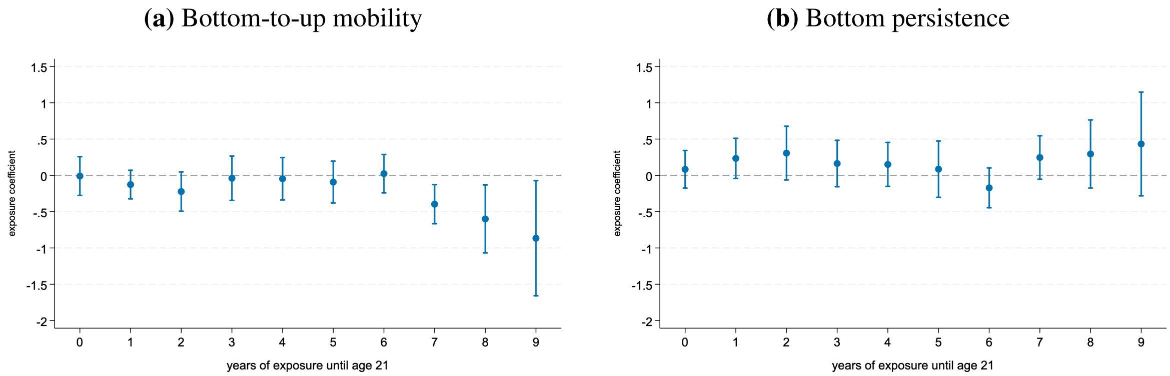
Third, endogenous shocks that trigger the migration may also directly affect outcomes. For instance, a parental job promotions could both cause the move and raise the daughter's mobility through higher resources, potentially in proportion to time spent in the destination. To mitigate this bias, I include indicators for stated reason for migration, capturing 26 specific motives that I group into seven broad categories. These controls condition on the type of shock that initiated the move and absorb systematic differences across migration motives. As an additional robustness check, I re-estimate results by leaving one (broad) category out. Basically, I sequentially drop all moves associated with one broad reason and verify that the estimated exposure effects remain stable. The absence of any category whose exclusion significantly changes the coefficients suggests that no single migration motive is driving the baseline pattern.

Finally, I use a placebo outcome that should be ”sealed” before post-move exposure can matter: bottom persistence. Because primary school ends around age 11, additional exposure after this threshold should not affect this outcome, whereas any residual age-varying selection or omitted shocks correlated with age at move might still show up. A near-zero slope for exposure on this placebo supports the empirical design's validity.

5 Estimation Results

This section presents estimates from empirical model on the mobility outcomes of women. Figure 7 plots the effect of gender norms on intergenerational mobility by years of exposure until age 21, with panel (a) for bottom-to-up mobility and panel (b) for bottom persistence. I discuss each panel separately because they speak to different aspects of identification and interpretation.

Figure 7: Semiparametric Exposure Effects of Gender Norms



Notes: The figure plot semi-parametric exposure effects of local gender norms for bottom-to-up mobility in panel (a), and for bottom persistence in panel (b). Points show exposure-specific coefficients; vertical bars denote 95% confidence intervals.

Panel (a) highlights two points. First, remaining at least seven years in the destination is consequential for completing upper secondary. Intuitively, moves at ages 12-14 (7-9 years of exposure) still occur before typical entry into upper secondary in Türkiye (roughly age 15), so the local environment can still shape track choice, and thus mobility. The coefficients at 7-9 years decline approximately monotonically, with similar per-year effects. Second, near-zero coefficients for exposure years 0-6 (moves at ages 15-21) are internal validity check. If upper secondary has already begun or been completed, moving later should not affect bottom-to-up mobility, and the estimates align with that timing logic.

Panel (a) in Figure A2 plots the estimated coefficients for bottom-to-up mobility, together with fitted lines estimated separately for $Exposure_i \leq 6$ and $Exposure_i > 6$. Quantitatively, a 10 pp decrease (increase) in the gender-norm gap – moving into a more egalitarian (patriarchal) destination – raises (lowers) bottom-to-up mobility by 0.087 pp at 9 years of exposure, 0.060 pp at 8 years, and 0.040 pp at 7 years. Regressing β_h on years of exposure for $Exposure_i > 6$ provides an average annual exposure effect of gender norms of about 0.235. This implies that for each additional year in this age bracket, a girl with low-educated mother increases her probability of completing at least upper secondary by 2.35 percentage points when she moves to destination where gender-norm indicator is 10 pp lower than the origin region. Concretely, if a girl moves from Şanlıurfa (urban) region to Ankara (urban) within this age bracket, her probability of being upwardly mobile, on average, increases by 4.33 pp (0.235×18.41) in each year. If she moves at age 12, her bottom-top mobility increases by roughly 13 pp (4.33×3). This explains 31.8% ($12.99 \div 40.8$) of the difference in bottom-to-up mobility between Şanlıurfa and Ankara.

Data limitations prevent observing higher exposure years. However, under a linear extrapolation to earlier ages, a girl who moves at birth to a destination with a 10 pp lower gender-norm indicator would experience 32.9 (14×2.42) pp increase in bottom-to-up mobility by age 14.[17] For shorter exposure durations, the slope is close to zero. The mean of β_h for $Exposure_i \leq 6$ is -0.073, which is smaller than the selection effects reported by Alesina et al., 2021 and Chetty and Hendren, 2018. Substantively, it means that girls who move to regions where gender-norm indicator is 10 pp lower have only about a 0.007 pp higher likelihood to complete upper secondary education purely due to spatial sorting.

Panel (b) in Figure 7 shows the insignificant exposure effects for the bottom persistence. Institutionally, primary school completion occurs around age 11. Under this timing, moves that occur after primary completion (after age 11) should not change bottom persistence. Consistent with that prediction, all exposure-year estimates are statistically indistinguishable from zero. The fact that most point estimates are slightly positive does not warrant a substantive interpretation: given imprecision, they are compatible with sampling noise or mild "selection on levels" rather than an effect of exposure per se. Panel (b) in Figure A2 also shows this mild selection

[17] This linear extrapolation to earlier ages should be treated as illustrative rather than a formal estimate.

effect. Particularly, girls who move to destination where gender-norm indicator is 10 pp higher have 0.018 pp higher probability to stay at the bottom of the educational distribution. Given the null impact of gender norm and very little selection effect on bottom-persistence, it is possible to read this outcome as a placebo check that supports the identification strategy. Specifically, once the relevant schooling stage is already completed, additional years of exposure in the destination do not change the bottom-persistence.

Taken together, these patterns matter for Türkiye because they isolate a channel – local gender norms – through which otherwise similar girls diverge at key educational thresholds. The results show that years of exposure until age 21 have no bite for upper secondary completion once schooling is already underway. Yet, they are sizable when exposure begins before age 15. Hence, moving into a more egalitarian region before the transition from lower to upper secondary plausibly shifts track choice for girls whose continuation margins are more norm-sensitive. The placebo outcome – bottom persistence – shows no statistically detectable exposure effects at any horizon. As expected, moves after primary completion should not shift the probability of staying at the bottom, and the uniformly insignificant exposure-year estimates are exactly what this timing predicts. This placebo pattern strengthens the main claim: measured differences in intergenerational mobility across regions partly reflect causal effects of local gender norms rather than pure selection.

5.1 Validity of Empirical Design

In this section, I check the validity of the key identifying assumption – the potential outcomes of girls who move to better versus worse regions do not vary with the amount of time spent in the destination – using a series of tests that focus on different forms of selection and omitted variable bias.

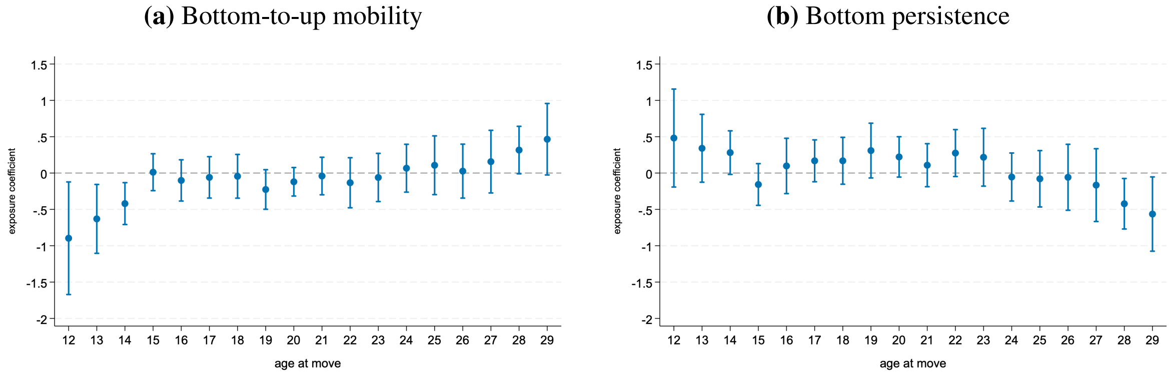
5.1.1 Testing Age-invariant Selection

First, I assess age-invariant selection using balanced tests that regress pre-move (observable) covariates on exposure bins within the baseline fixed-effects. Since moves between ages 12-21 are typically family-driven, it is informative to ask whether families who move earlier vs. later differ in pre-move characteristics. For this, exposure years are grouped into 4 bins, namely 0, 1-3, 4-6, and 7-9 years of exposure until age 21. For each bin, I regress of paternal education, maternal education, father's literacy, and Turkish mother tongue on the exposure-bin indicators, controlling for origin$\times$cohort, destination, migration year fixed effects and covariates. In this setting, the objective is to detect any correlation between remaining exposure years and pre-move characteristics. As reported in Table A1, coefficients are statistically insignificant for all four outcomes across the 0, 1–3, 4–6, and 7–9 years-bins. These findings are consistent with the identification strategy. Daughters who move across regions at different ages do not come from systematically different families on these pre-move dimensions, which reduces concerns about age-varying selection.

Second, I replace exposure-year indicators with age-at-move indicators in equation 1. Figure 8 plots the estimated coefficients by each age-at-move for bottom-to-up mobility in panel (a) and for bottom persistence in panel (b). The results align closely with the main findings. Moves at age 12 show the largest effect on bottom-to-up mobility. Effects get smaller at 13-14 and are statistically insignificant from 15 onward. This smooth, monotone decline matches the institutional timing in Türkiye: late moves occur after the key window when local norms can matter. For bottom persistence, the coefficients are statistically insignificant for moves at 12-27, as expected once primary education is already completed.[18] In general, these findings mirror the quasi-experimental design and support the identification: effects appear before institutional thresholds and vanish after, consistent with age-invariant selection.

[18]Only moves at age 28 and 29 show significant effects at conventional level, which likely reflects the selection at very late moves rather than exposure. Moreover, this selection operates in the opposite direction of the expected treatment effect.

Figure 8: Semiparametric Exposure Effects of Gender Norms by Age at Move



Notes: The figures plot semi-parametric exposure effects of local gender norms by age at move for bottom-to-up mobility in panel (a), and for bottom persistence in panel (b). Points show exposure-specific coefficients; vertical bars denote 95% confidence intervals.

5.1.2 Event-time Design

This part introduces an event-time specification that complements empirical model. The basic idea is to ask a counterfactual question: what would happen if we "slide" the ΔGN shock to earlier versus later points in a girl's schooling career? Operationally, for each migrant, I construct a pseudo-panel of yearly observations spanning ages 8 to 24. For example, suppose a girl actually moved at age 14 in 2002 from origin o to destination d. In the event-time design, I follow this same girl from 1996 (age 8) to 2012 (age 24), and for each calendar year in this window, I assign the corresponding ΔGN as if the move had occurred in that year. Because the gender-norm indicator varies over time at the region level, the implied gap also changes over time, so each "hypothetical" move year corresponds to a potentially different gender-norm difference between origin and destination.

Formally, the setting creates multiple observations per individual indexed by event time k, where $k = 0$ denotes the girl's actual migration, $k < 0$ corresponds to hypothetical moves that occur $|k|$ years before the true move, and $k > 0$ corresponds to hypothetical moves that occur $|k|$ years after the true move. The specification includes up to five pre-period years ($k = -5, \ldots, -1$) and seven post-period years ($k = 0, \ldots, 6$) around the move. For each k, I compute counterfactual gender-norm gap $\Delta GN_{odt(k)}$ that the girl would face if she moved in calendar year $t + k$. I then estimate a model analogous to the baseline specification:

$$Y_{i,k} = \alpha_{oc} + \delta_d + \theta_t + \sum_{\tau \in K, \tau \neq \tau_{-1}} \beta_\tau \left[\Delta GN_{odt(k)} \cdot \mathbf{1}\{k = \tau\} \right] + X_i' \gamma + \varepsilon_{i,k} \tag{2}$$

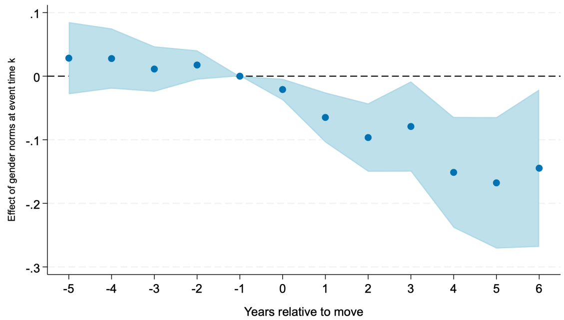
where the outcome, bottom-to-up mobility or bottom persistence, is repeated: $Y_{i,k} = Y_i$ for all $k \in K$. Here, I replace the exposure-year indicators in equation 1 with event-time indicators, $1\{k = \tau\}$, and interact them with $\Delta GN_{odt(k)}$. Intuitively, the coefficients on these interactions, β_τ, capture how the effect of a given norm gap varies with the timing of exposure in the lifecycle. For example, what is the effect of being exposed to a given ΔGN five years before the actual move, at the actual move year, or five years after?

This event-time setup mainly provides two advantages. First, it delivers a dynamic profile of the effect of gender norms by (hypothetical) exposure. If the effects are genuinely "proportional to exposure time" and accumulate smoothly with years spent in a more egalitarian region, we should see that the post-period coefficients increase in magnitude as k moves from 0 to positive values, while the pre-period coefficients remain close to zero. Second, the pre-period coefficients, $k < 0$, serve as a diagnostic for age-varying selection and anticipatory adjustments. If families systematically time moves based on the child's unobserved gains from more egalitarian norms, we would expect to see non-flat patterns in the leads, because the "hypothetical" exposure several years before the actual move would already be correlated with the child's potential outcomes.

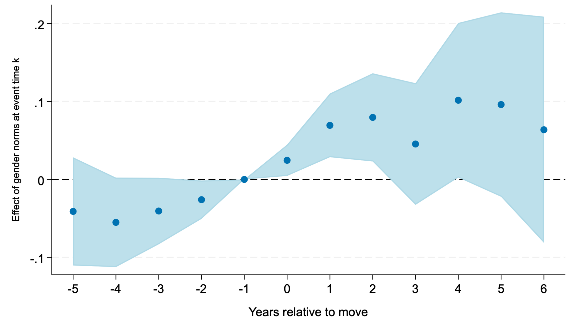
Figure 9 plots the estimated event-time coefficients relative to the reference period $\tau = -1$ for the bottom-to-up mobility in panel (a) and for bottom persistence in panel (b). The patterns are broadly consistent with the baseline remainder-age model. Pre-move coefficients are small and statistically insignificant across the full 5-year window for both outcomes. This indicates that there is no strong evidence of anticipatory adjustments or systematic age-varying selection. For bottom-to-up mobility in panel (a), post-move coefficients gradually increase in magnitude, which suggests that the effect of moving to a more gender-egalitarian region grows with the number of years spent in the destination and becomes almost constant after 4 years.

Figure 9: Exposure Effects with Event-time Design

(a) Bottom-to-up mobility



(b) Bottom persistence



Notes: The figures plot exposure effects of local gender norms on bottom-to-up mobility in panel (a), and on bottom persistence in panel (b) with event-time design. Sample weights are used in the estimation and standard errors are clustered at individual level. Points show exposure-specific coefficients; colored area denotes 95% confidence intervals.

Unlike the main findings in Figure 7, the event-time estimates yield statistically significant effects up to two years after the move on bottom persistence, shown in panel (b). In principle, this could reflect a genuine effect of more egalitarian gender norms on the probability of avoiding the bottom of the distribution, which I could not observe in Figure 7. However, another plausible interpretation is that the event-time design partly captures the impact of compulsory schooling reform that extended mandatory education from primary to lower secondary. For cohorts exposed to this reform, bottom persistence should mechanically decline zero since these cohorts are required to complete at least lower secondary schooling.[19]

Hence, this event-time analysis strengthens the causal interpretation of the main estimates.[20] It shows that the gains from more egalitarian gender norms are concentrated in the post-migration period and increase with the length of exposure in adolescence and early adulthood, while the absence of significant pre-trends is consistent with the identification assumption.

[19] I conduct robustness checks to assess the sensitivity of the results to the compulsory schooling reform in Section 6.

[20] This analysis also comes with several limitations. So, I treat it primarily as a diagnostic rather than as the main source of quantitative magnitudes.

5.1.3 Displacement Shocks

Another concern for the identification is that shocks triggering migration may themselves directly affect daughters' educational outcomes, in ways that are correlated with both the timing of the move and the gender-norm environment in the destination. For instance, a parental job promotion might lead to a relocation from a more patriarchal to a more gender-egalitarian region. Such a promotion plausibly raises household resources and aspirations, potentially improving the daughter's chances of completing upper secondary education in proportion to the time she spends in the post-shock, higher-income environment. If families experiencing these positive shocks also tend to move when daughters are younger, the remainder-age specification could attribute to exposure to more egalitarian gender norms what is in fact driven by endogenous, time-varying improvements in family circumstances.

To mitigate this form of bias, I exploit detailed information on the stated reason for migration, which is recorded in 26 exclusive categories (e.g. job change, marriage, family reunification, education, health, security). In the baseline specification, I include a full set of indicators for all 26 reasons. This absorbs systematic level differences in daughters' outcomes across migration motives and restricts identification to variation in remaining exposure time among girls who share the same origin by cohort, destination, migration year, parental background, mother tongue, and reported migration reason, but differ in how long they are exposed to the destination's gender norms before reaching the educational horizon.

However, using only a set of reason dummies could still leave room for bias if particular types of motives such as upward career moves, or forced displacement combine strong direct effects on daughters' schooling trajectories. To probe this, I group 26 reasons into seven broad categories (education-related, marriage, economic/job-related, other personal motives, partner-related, family-related, and other motives). Then, I implement a robustness check by leaving one group out: in each iteration I omit all moves driven by one broad category from the estimation sample and re-estimate the exposure-bin model.[21]

[21] I do not drop the individuals who migrated for marriage, as they constitute 64.2% of the sample.

Table A2 presents the estimated exposure-bin coefficients on bottom-to-up mobility, similar to the specification employed in section 5.1.1, for full sample and each leave-one-category-out samples after controlling for migration reason.[22] The coefficients for each exposure bin in each column are mostly stable and change little. One possible concern is that significant effect of gender norms on bottom-to-up mobility by 1-3 years exposure. But this will be sign of higher selection effect than the reported one in Section 5. Table **??** shows the .... Additionally, no specific class of moves appears to drive the results, and there is no evidence that the estimated effects are concentrated in motive types that are particularly susceptible to resource shocks or other endogenous changes.

5.1.4 Placebo Outcome

As an additional validity check for the identification strategy, I use a "bottom-persistence" outcome that is, by construction, largely predetermined at the time of migration. In the Turkish context, primary school is typically completed by age 11, and progression to lower-secondary education follows immediately. Late (re)entry into lower-secondary after a gap year is extremely rare due to strict grade-age rules and the absence of a gap-year tradition. Hence, for the vast majority of girls, the decision to remain at the primary level versus continue into lower-secondary is effectively fixed by age 11.

This institutional feature implies that migration after age 11 cannot plausibly affect the bottom persistence through schooling choices. A girl who moves at age 13 or 14 has already either enrolled in lower-secondary or exited at the primary threshold. Changing the regional gender-norm environment at that point may still matter for later outcomes (e.g. upper-secondary completion or tertiary enrollment), but it cannot change the fact that she has already "escaped" or "remained in" the bottom. In other words, conditional on having a mother with at most primary education, bottom persistence should be invariant to the timing and destination of moves that occur after primary school completion. This makes bottom persistence a natural placebo: the

[22] I do not report results for bottom persistence since main findings do not show any statistical significance for this outcome.

empirical design should not generate systematic effects of more gender-egalitarian destinations on the probability of staying at the bottom.

Discussed previously, panel (b) in Figure 7 plots the estimated coefficients from the remainder-age specification using bottom persistence as a dependent variable. Consistent with the placebo logic, the coefficients are small in magnitude and statistically indistinguishable from zero across the relevant range of exposure ages. Exposure to more gender-egalitarian regions does not measurably reduce the probability of remaining at primary school, in contrast to the clear gains observed for bottom-to-up mobility. Additionally, panel (b) in Figure A2 shows coefficients with fitted line..

Overall, the bottom-persistence estimates support the identifying assumption that the empirical design captures the causal effect of gender norms on margins of schooling that are still "at risk" at the time of the move, rather than picking up generic differences in family types or unobserved shocks that would mechanically shift all education outcomes. If time-varying selection into destinations were strongly correlated with daughters' unobserved propensity to stay at the bottom, one would expect to see meaningful effects also on bottom persistence. The absence of such effects strengthens the interpretation of the main results as reflecting causal exposure effects of gender norms rather than age-varying selection bias.

6 Robustness Checks

In this section, I provide contextual robustness checks for the estimates. I examine the role of the 1997 compulsory schooling reform, alternative educational horizons, migration motives, flexible cohort-specific effects, age constraints, and different constructions of the gender-norm indicator.

The 1997 Education Reform extended compulsory schooling from five to eight years nationwide and was implemented starting in the 1997–98 school year. The law targeted cohorts who were in grade 4 or below at the time, roughly corresponding to those born in 1986–1987 and later. A large literature shows that the reform substantially increased lower-secondary completion and years of schooling, especially for rural women, and narrowed gender and urban–rural gaps in attainment (Kırdar et al., 2016). At the same time, the reform involved sizable investments in school infrastructure and middle-school capacity that were rolled out unevenly across regions. This raises a natural concern for the empirical design: if more gender-egalitarian regions also experienced larger or more effective implementation of the reform, the estimated effect of moving to a more egalitarian region could partly reflect policy-driven improvements in schooling rather than the causal impact of local gender norms. In the main specification, origin$\times$cohort fixed effects absorb all origin-specific level shifts in schooling associated with the reform for a given birth cohort, but they do not, by construction, rule out interactions between the reform and the destination–origin gender-norm gap. To solve this issue, I enhance the remainder-age model in two ways. First, I introduce an indicator for reform-exposed cohorts and interacted it with the gender-norm gap, which allows the slope of gender norms to differ flexibly between pre- and post-reform cohorts. Second, I further strengthen the model with destination$\times$cohort fixed effects, so that any destination-specific cohort shocks such as region-specific variation in school construction, enforcement, or parallel local policies are absorbed non-parametrically. The estimates reported in Figure A3 and Figure A4 show that these adjustments leave the main results unchanged. The coefficient on the baseline gender-norm exposure remains stable in magnitude and statistical significance when the interaction of treated-cohort indicator with the gender-norm gap, and destination$\times$cohort fixed effects are added, confirming that baseline estimates are robust.

As a second robustness check, I vary the educational horizon used to define the exposure window. The baseline specification sets the horizon at $H = 21$. I re-estimate the model using $H = 16$, $H = 18$ and $H = 24$, which correspond to earlier and later cutoffs for the completion of formal schooling and thus change the number of remaining exposure years in the destination. Figure A5 plots the estimated coefficients for all horizons. Across all three horizons, the estimated coefficients on gender-norm exposure are very similar in magnitude and precision, and the pattern across exposure years is preserved. This indicates that the main findings are not driven by a particular choice of educational horizon and are robust to alternative assumptions about the timing of educational attainment.

A natural concern is that moves motivated by education may embody a distinct source of endogeneity. Households who relocate explicitly for schooling are plausibly selecting destinations based on expected educational returns rather than local gender norms. These moves also tend to occur precisely at ages when schooling choices are most malleable, so their timing is mechanically correlated with the outcome margins of interest. Excluding this subgroup therefore isolates variation in ΔGN that is less likely to reflect forward-looking educational investments and more consistent with the identifying assumption of age-invariant selection. Even though the exposure-bin coefficients are presented in Section 5.1.3, I re-estimate the remainder-age specification after removing all individuals whose stated reason for migration is education. As shown in Figure A6, the exposure coefficients that are nearly unchanged relative to the baseline. The pattern remains monotonic, effect sizes remain comparable in magnitude, which indicates that the main findings are not driven by education-related moves.

Another robustness check augments the baseline specification with cohort $\times$ ΔGN interactions. The concern is that age-at-move, correspondingly exposure-years, distribution differs systematically across cohorts due to the starting year of gender-norm indicator. For women born between 1964-1970, I only observe their locations starting at age 23, so these cohorts contribute moves that occur at relatively late ages. On the other hand, for women born in 1981 and later, locations are observed from age 12 onwards. This suggests that exposure variation is generated at different parts of the life cycle for different cohorts, and older cohorts do not contribute to the support of higher exposure years. This unbalanced age-at-move distribution across cohorts could bias the estimates. To address this issue, I allow for flexible cohort specific effects of ΔGN, so that the effects of gender norms vary across cohorts. Specifically, identification of the exposure effect comes from within-cohort variation in remainder-age exposure. Figure A7 displays the estimated results by including cohort interactions, which are very similar in magnitude and precision to the baseline estimates.

In order to maximize the sample size, the main sample is restricted to women aged 21 and older at the time of the survey. A potential concern is that age 21 may still be too early for some respondents to have fully completed their educational attainment, even though defining bottom-to-up mobility at age 18 is a reasonable benchmark in the Turkish context. To address this, I re-estimate the exposure effects on a subsample restricted to women who were at least 24 years old at the time of the interview. Figure A8 shows that the estimated effects are very similar, and the results are robust to using a more conservative age cutoff for completed schooling.

I also assess whether the findings depend on how gender norms are measured. Instead of the baseline composite, I first transform each of the three underlying items into standardized scores (mean zero, unit variance) over the full region–year panel and then average these standardized items so that each question enters on a comparable scale. Next, I build an alternative index with principal component analysis that summarizes the main common variation across the three items. Figures A9 and A10 present exposure coefficients using standardize values and index of gender norm, respectively. They show that the estimated effects of gender-norm exposure are highly similar across these alternative measures, indicating that the results are not sensitive to the specific scaling or weighting of the gender-norm indicator.

Next, I examine whether the results are robust to the way I assign gender norms to non-survey years. In the baseline specification, the region$\times$year gender-norm indicator is linearly interpolated between survey waves. As a first robustness check, I instead hold the most recent survey value constant until the next survey, effectively constructing a step-function in time (e.g., the 1993 value is assigned to 1994–1997). As a second check, I re-estimate the model using lagged gender-norm indicator, so that exposure is measured with respect to the normative environment observed in the previous survey year rather than contemporaneously. The estimates reported in Figure A11 and Figure A12 show that the coefficients on gender-norm exposure remain very similar in magnitude and precision under both alternatives, indicating that the main findings are robust to alternative time-assignment rules for the gender-norm indicator.

Table 2: Exposure Effect and Selection Effect

			Migration motives				Education horizons			
	Baseline	Age-at-move	Motives	Except educ. motives	Education Policy	Destination $\times$Cohort	H=16	H=18	H=24	Cohort $\times \Delta GN$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Exposure effect	-0.235	0.238	-0.198	-0.368	-0.180	-0.263	-0.231	-0.230	-0.237	-0.160
	(0.018)	(0.011)	(0.035)	(0.009)	(0.030)	(0.052)	(0.014)	(0.014)	(0.011)	(0.016)
Selection effect	-0.073	0.024	-0.110	-0.083	-0.080	-0.055	-0.072	-0.044	-0.065	-0.295
# of obs.	3,150	3,150	3,142	3,032	3,150	3,150	3,150	3,150	3,150	3,150

Notes: The table reports estimates of annual exposure effects of gender norms on bottom-to-up mobility as well as selection effects. The exposure effect can be interpreted as the impact of spending an additional year of childhood between ages of 12 and 15 in the destination region where the gender norm indicator is 100 pp higher than the origin region. Robust standard errors are shown in parenthesis. Each column reports exposure effect from unweighted OLS regressions of the β_h coefficients on exposure years if $Exposure_i > 6$ and selection effect defined as the mean value of the β_h estimates for $Exposure_i \leq 6$. Column (1) reports the estimates from the main analysis. Column (2) uses age-at-move indicators instead of years of exposure until age 21. Column (3) adds the stated reason of migration in 26 categories to the specification in column (1). Column (4) restricts sample into those whose reason of migration is not education-related. Column (5) and (6) adds interaction of treated$\times$norm-gap and destination$\times$cohort fixed effects respectively. Column (7), (8), and (9) redefine education horizon as 16, 18, and 24, respectively. Column (10) adds interaction of cohort$\times$norm-gap to the specification in column (1). Column (11) restricts the sample into women aged at least 24 at the time of survey. Column (12) standardized values of attitudinal questions to construct gender-norm indicator, while column (13) creates the indicator employing principal component analysis. Column (14) do not interpolate regional means of attitudinal questions between survey years and holds the most recent survey value constant until the next survey. Column (15) uses the one year lagged values of gender-norm indicator.

Table 2: Continued

	Age $\geq$ 24	Standardized norms	Norm index	Without interpolation	Lagged norms
		Definition of gender norm indicator			
	(11)	(12)	(13)	(14)	(15)
Exposure effect	-0.320	-0.041	-0.024	-0.323	-0.210
	(0.031)	(0.002)	(0.001)	(0.076)	(0.035)
Selection effect	-0.052	-0.013	-0.008	-0.067	-0.047
# of obs.	2,737	3,150	3,150	3,150	2,990

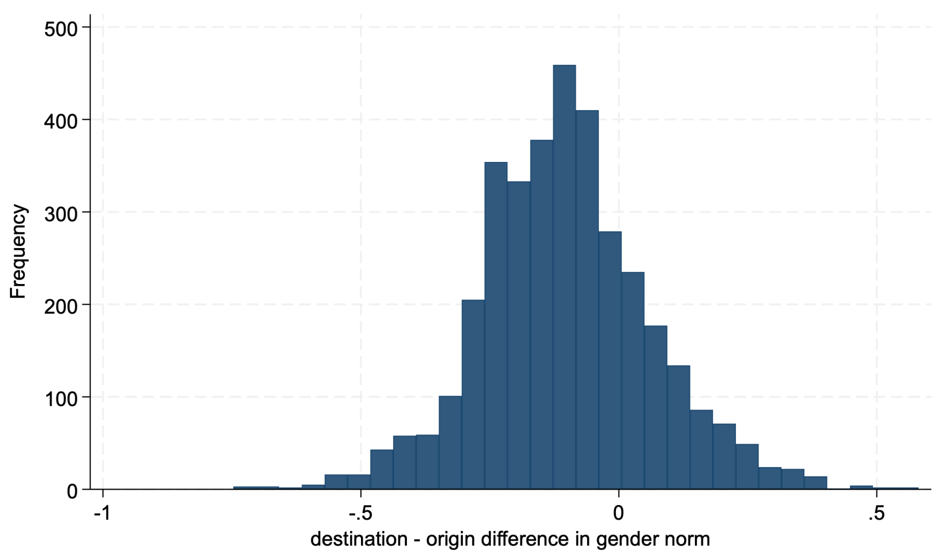
Notes: The table reports estimates of annual exposure effects of gender norms on bottom-to-up mobility as well as selection effects. The exposure effect can be interpreted as the impact of spending an additional year of childhood between ages of 12 and 15 in the destination region where the gender norm indicator is 100 pp higher than the origin region. Robust standard errors are shown in parenthesis. Each column reports exposure effect from unweighted OLS regressions of the β_h coefficients on exposure years if $Exposure_i > 6$ and selection effect defined as the mean value of the β_h estimates for $Exposure_i \leq 6$. Column (1) reports the estimates from the main analysis. Column (2) uses age-at-move indicators instead of years of exposure until age 21. Column (3) adds the stated reason of migration in 26 categories to the specification in column (1). Column (4) restricts sample into those whose reason of migration is not education-related. Column (5) and (6) adds interaction of treated×norm-gap and destination×cohort fixed effects respectively. Column (7), (8), and (9) redefine education horizon as 16, 18, and 24, respectively. Column (10) adds interaction of cohort×norm-gap to the specification in column (1). Column (11) restricts the sample into women aged at least 24 at the time of survey. Column (12) standardized values of attitudinal questions to construct gender-norm indicator, while column (13) creates the indicator employing principal component analysis. Column (14) do not interpolate regional means of attitudinal questions between survey years and holds the most recent survey value constant until the next survey. Column (15) uses the one year lagged values of gender-norm indicator.

Finally, Table 2 reports the estimated exposure and selection effects for the baseline specification and all robustness checks. Since columns (12) and (13) rely on alternative constructions of gender-norm indicator, the scale of the coefficients in these columns is not directly comparable. Focusing on the remaining columns, the implied effect of spending an additional year of childhood between ages of 12 and 15 in the destination region where gender-norm indicator is 10 pp lower ranges from 1.60 to 3.68 pp. The baseline estimate, specified in Column (1), lies within this range. Even taking the lower bound, the estimated effect accounts for more than 20% of the regional difference in bottom-to-up mobility between the lowest mobility region (Şanlıurfa) and the highest mobility region (Ankara).

7 Mechanism

Although the main analysis already isolates the role of gender norms as a key mechanism, I further explore the channels underlying this effect by examining heterogeneity in gender-norm exposure across subsamples.

Figure 10: Differences in the Gender-norm Indicator between Destination and Origin



Notes: The figure plots the distribution of ΔGN_{odt}, destination minus origin differences in the gender-norm indicator. μ = -0.104, p_{50} = -0.109, σ = 0.166

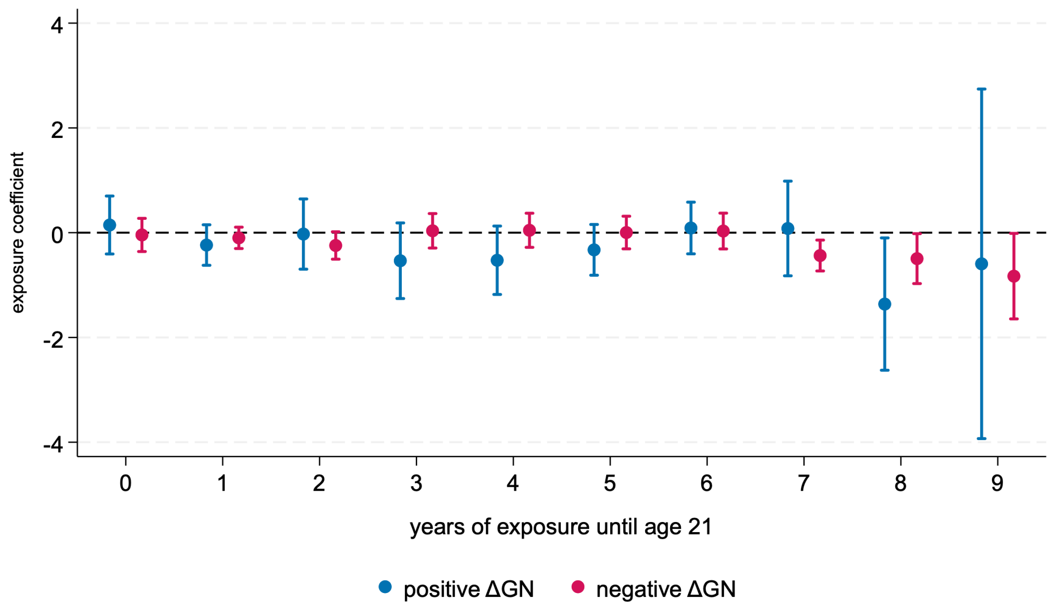
Before moving to estimated results, Figure 10 plots the histogram of ΔGN_{odt}. Both mean and median is negative, -0.104 and -0.109, respectively. This indicates that women (and their families) generally move to regions with less patriarchal norms, even though flows run in both directions. About 76% of moves are to destinations with lower values of gender-norm indicator, that is more egalitarian places.

Figure 11 examines whether the effect of gender norms on bottom-to-up mobility differs by the sign of the destination-origin gap, ΔGN. Given the limited sample size, instead of dividing the sample into two, the model includes an indicator for the sign of ΔGN, which is interacted with exposure year $\times$ ΔGN term. As discussed above, a positive ΔGN indicates moves to more patriarchal regions relative to the origin, while a negative ΔGN refers to moves to more egalitarian destinations. The estimates show that the effect of gender norms is entirely driven by moves with ΔGN is negative. That means, women who move to more egalitarian places observe experience higher bottom-to-up mobility. However, women who move from a relatively egalitarian to a more patriarchal region do not exhibit lower upward mobility. The coefficients are statistically insignificant in all but one case (moves at age 13). After living at least 12 years in a more egalitarian environment, subsequent exposure to more patriarchal setting does not appear to limit educational attainment. Although I cannot observe moves at earlier ages due to data limitations, this pattern is consistent with the idea that early-childhood exposure to local gender norms is particularly consequential for later-life educational outcomes.

Next, I explore whether the exposure effect varies along the distribution of the destination-origin gap. Analogous to the previous specification, I interact an indicator for being above the median of ΔGN with the exposure year $\times$ ΔGN term. An ”above-median” ΔGN denotes moves where $\Delta GN \geq -0.109$, while a ”below-median” ΔGN captures moves where $\Delta GN < -0.109$. In practice, the ”above-median” group corresponds to moves into more patriarchal regions ($\Delta GN > 0$) or to destinations with relatively small gaps with the origin ($0 > \Delta GN > -0.109$), whereas the below-median group refers to moves with larger improvement in gender norms.

The estimates in Figure 12 indicate that moves to substantially more egalitarian regions with large gaps have almost no impact on bottom-to-up mobility. Figure 13, which interacts

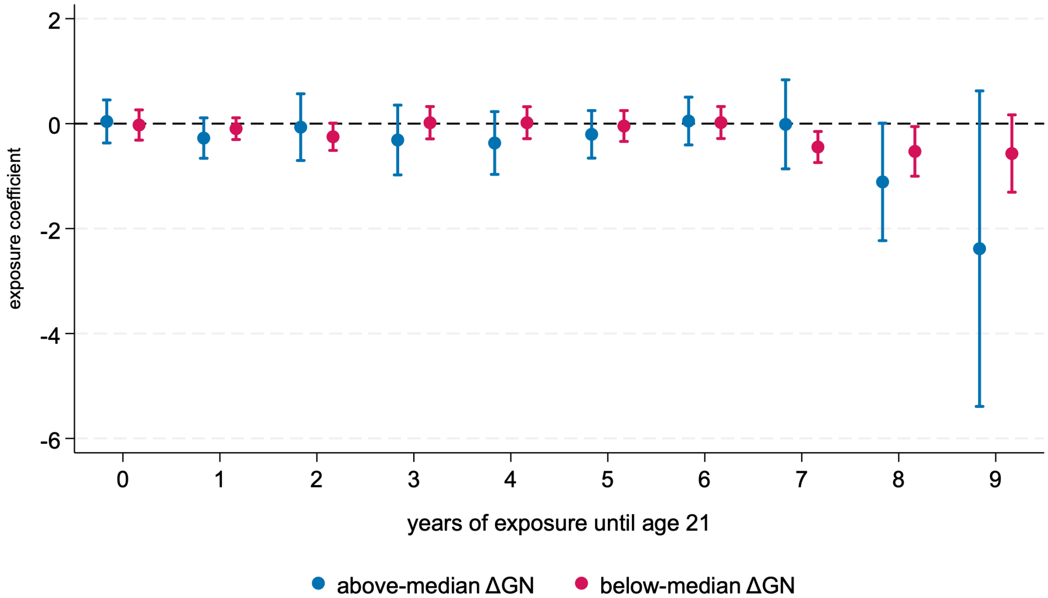
Figure 11: Semiparametric Exposure Effects of Gender Norms, by sign of ΔGN



Notes: The figures plot semi-parametric exposure effects of local gender norms on bottom-to-up mobility by subsamples defined according to the sign of ΔGN. Points show exposure-specific coefficients; vertical bars denote 95% confidence intervals.

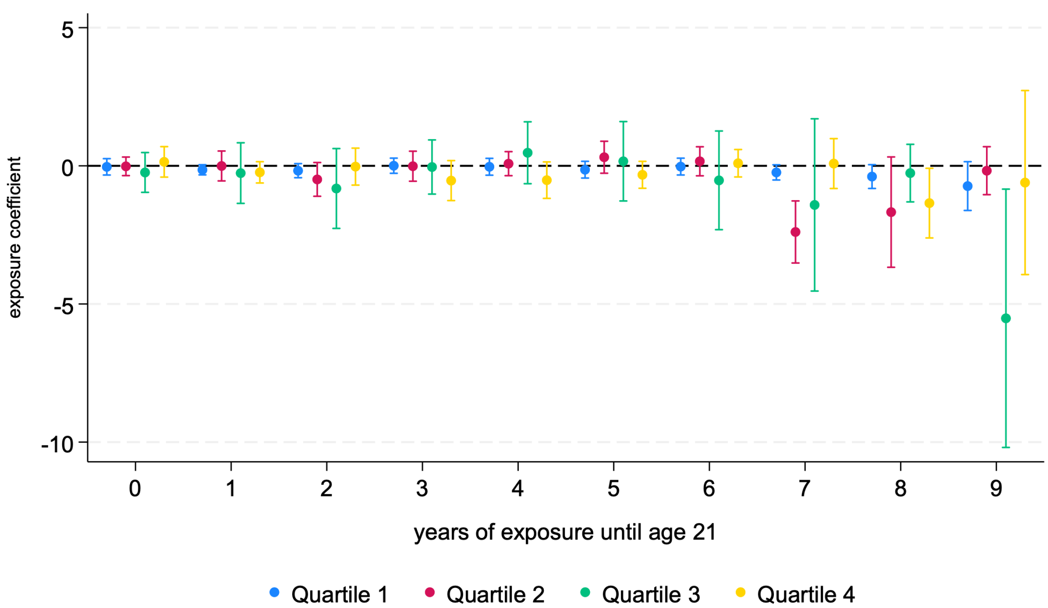
exposure year $\times \Delta GN$ term with quartile indicators, also supports this pattern. Moves in the first quartile correspond to large improvements in norms (i.e., moves to more egalitarian regions with $\Delta GN < -0.22$), whereas the fourth quartile comprises moves in the right tail (i.e., $\Delta GN > -0.011$, implying small or even adverse changes in norms). The results show that the main effects are not driven by the most extreme improvements (first quartile); instead, they are larger for moves in the second and third quartiles. This suggests that when the change in gender norms is too large—moving from a very patriarchal to a very egalitarian region—educational decisions do not adjust quickly, consistent with slow adaptation to a large shock. By contrast, moves to more egalitarian regions with more moderate gaps appear to facilitate faster adaptation, allowing local gender norms to more effectively translate into higher upward mobility.

Figure 12: Semiparametric Exposure Effects of Gender Norms, by median of ΔGN



Notes: The figures plot semi-parametric exposure effects of local gender norms on bottom-to-up mobility by subsamples defined according to the median of ΔGN. Points show exposure-specific coefficients; vertical bars denote 95% confidence intervals.

Figure 13: Semiparametric Exposure Effects of Gender Norms, by quartile of ΔGN



Notes: The figures plot semi-parametric exposure effects of local gender norms on bottom-to-up mobility by subsamples defined according to the quartile of ΔGN. Points show exposure-specific coefficients; vertical bars denote 95% confidence intervals.

8 Conclusion

This paper has shown that local gender norms are an important and genuinely causal component of intergenerational educational mobility for women in Türkiye, over and above family background and region fixed factors. Using internal migration between ages 12–29 and an exposure design, the analysis exploits variation in the timing of moves across regions with different gender-norm environments, conditional on origin, destination, cohort and move-year fixed effects. Within this framework, daughters of low-educated mothers who move earlier into more gender-egalitarian regions exhibit systematically higher probabilities of "bottom-to-up" mobility—that is, escaping the compulsory-schooling category—than otherwise similar daughters who move later, while a placebo outcome (bottom persistence at the compulsory threshold) shows no effect. This pattern is fully consistent with childhood exposure effects documented in the neighborhood-mobility literature (Chetty and Hendren, 2018; Alesina et al., 2021), but here the "treatment" is a cultural environment rather than income or poverty.

Quantitatively, the estimated exposure effects are economically meaningful. A 10 pp decrease in the destination–origin gender-norm gap, held throughout the remaining schooling window, raises bottom-to-up mobility roughly 2.35 pp each year. In the benchmark remainder-age specification, this implies that a girl who moves from a relatively patriarchal region such as Şanlıurfa to a more egalitarian region such as Ankara while still in lower secondary school gains several percentage points (approximately 13 pp at most) in the probability of attaining at least upper secondary education. These magnitudes are comparable to those found for other "place-based" determinants of mobility – such as school supply or neighborhood quality – in related work for Türkiye and other middle-income settings (Aydemir and Yazici, 2019; Bakış and Filiztekin, 2025).

The heterogeneity analyses sharpen this picture and help interpret the underlying mechanisms. First, the gains in upward mobility are driven by moves into more gender-egalitarian destinations; moves into more patriarchal regions do not generate symmetric losses. This asymmetry is consistent with the idea that early exposure to egalitarian norms – regarding girls' schooling, work and autonomy – creates aspirations and expectations that are difficult to fully reverse later in adolescence or early adulthood. Second, exposure effects are strongest when the gender-norm gap between origin and destination is moderate rather than extreme. Daughters who move into regions with very large norm gaps do not experience proportionally larger gains, which is in line with an adaptation mechanism: large cultural distance may hinder assimilation into the destination's normative environment and limit the extent to which local norms translate into concrete schooling decisions. Together, these patterns point to gender norms operating as a gradual, path-dependent constraint that interacts with the timing and "cultural distance" of migration.

A series of diagnostic exercises supports the causal interpretation. Event-time and age-at-move specifications show flat pre-trends and effects that emerge around key schooling margins, rather than mechanical trends around the move. Placebo regressions based on bottom persistence yield precisely estimated zero effects, which reduces concerns that unobserved shocks correlated with migration might mechanically drive all education outcomes. The estimates are robust to alternative definitions of the schooling horizon, to alternative constructions of the gender-norm index, to restricting the sample away from likely "education migrants," and to including interactions between gender-norm gap and daughter's cohort that flexibly absorb cohort-specific composition in observed locations. While, as in any movers design, the identifying assumption remains that selection into the timing of moves is not correlated with idiosyncratic gains from more egalitarian norms, the empirical checks substantially narrow the class of plausible threats.

The findings have two broader implications. Substantively, they demonstrate that gender norms are not merely a correlate of regional mobility regimes but a causal determinant of whether daughters of low-educated mothers escape the bottom of the educational distribution. This complements a growing literature documenting that gender bias and patriarchal norms suppress girls' educational attainment in low- and middle-income countries, and shows that such norms also shape intergenerational mobility patterns rather than just levels of schooling (Emran et al., 2025). From a policy perspective, the results suggest that equalizing formal access to schooling – through school construction or compulsory schooling reforms – may be insufficient to close regional gaps in women's mobility when local norms remain strongly conservative. Interventions that relax normative constraints and support more egalitarian attitudes through information, role models, or community-based programs are likely to interact with place-based education policies in shaping women's long-run opportunities.

At the same time, the analysis is subject to several limitations that delineate avenues for future research. The estimates are local to internal migrants whose mothers have at most primary education, and they capture mobility effects only up to daughters' achieved education – not their subsequent labor-market, marriage, or fertility outcomes, where gender norms might matter at least as much. The gender-norm index aggregates attitudinal responses at the region $\times$ year level, inevitably smoothing intra-regional heterogeneity and potentially conflating norms with other slow-moving cultural or institutional features. Finally, the movers design abstracts from general-equilibrium responses: if large numbers of families were induced to move to more egalitarian regions, congestion, school quality, and labor-market competition could mitigate some of the gains.

Despite these caveats, the paper provides, to my knowledge, the first causal evidence that within-country variation in gender norms can systematically shape intergenerational educational mobility of women via differential exposure during adolescence and early adulthood. In a country like Türkiye, where both gender gaps in education and regional disparities in development remain pronounced, this underscores the centrality of norms – as much as resources and institutions – in understanding who manages to escape the bottom of the educational ladder and why.

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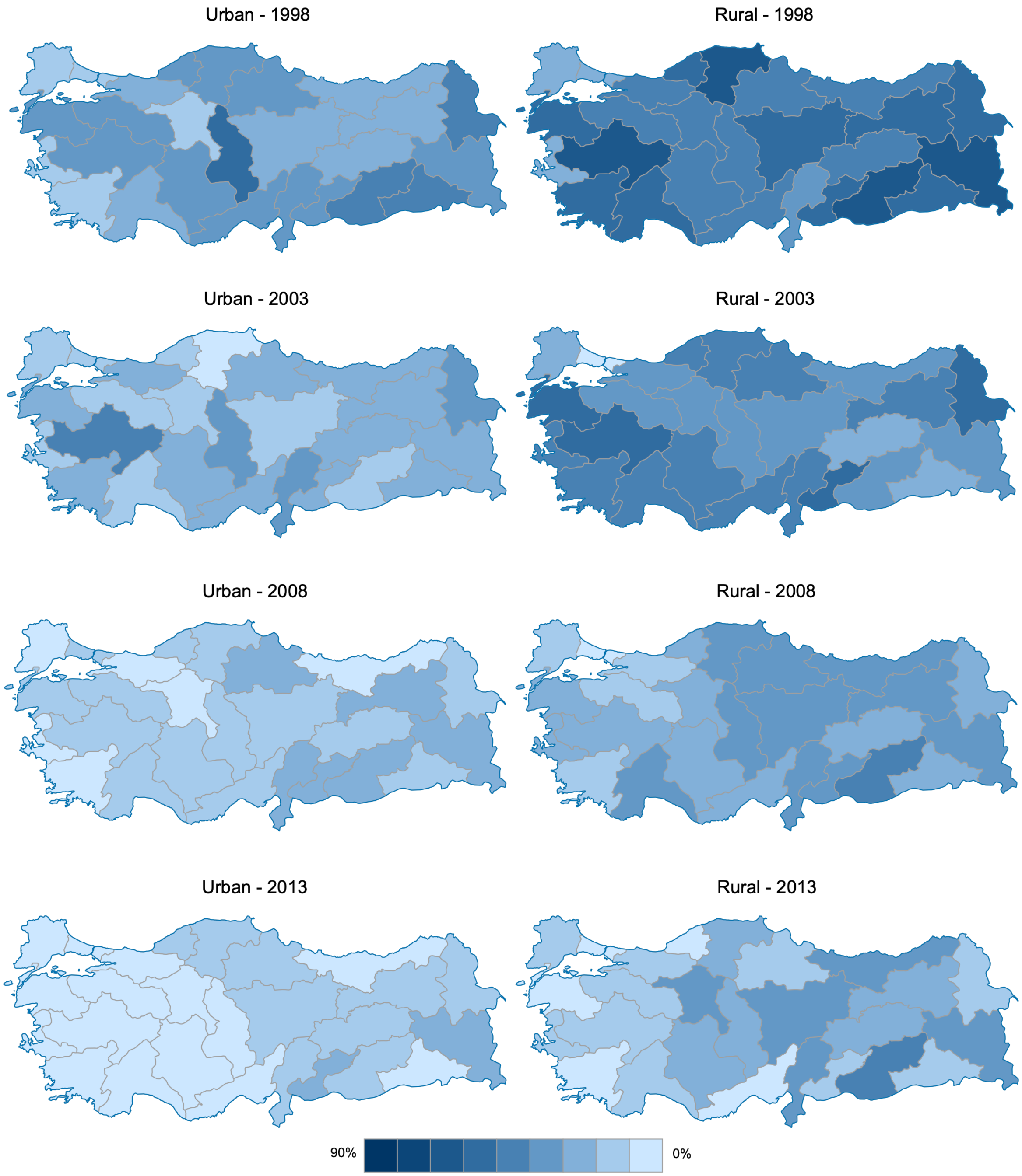
Appendix

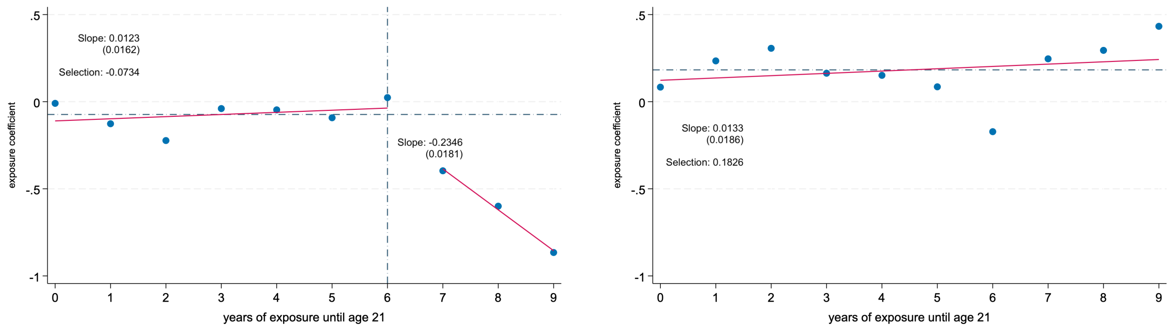
Figure A1: Distribution of Gender Norm Indicator across Regions, Remaining Survey Years



Notes: The figures plot the distribution of the gender-norm indicator across regions in selected years. The gender-norm indicator refers to the endorsement of patriarchal attitudes at NUTS-2 × urban/rural regions. Higher values denote more patriarchal values; lower values indicate more gender-egalitarian attitudes.

Figure A2: Semiparametric Exposure Effects of Gender Norms, with Fitted Lines for Exposure & Selection

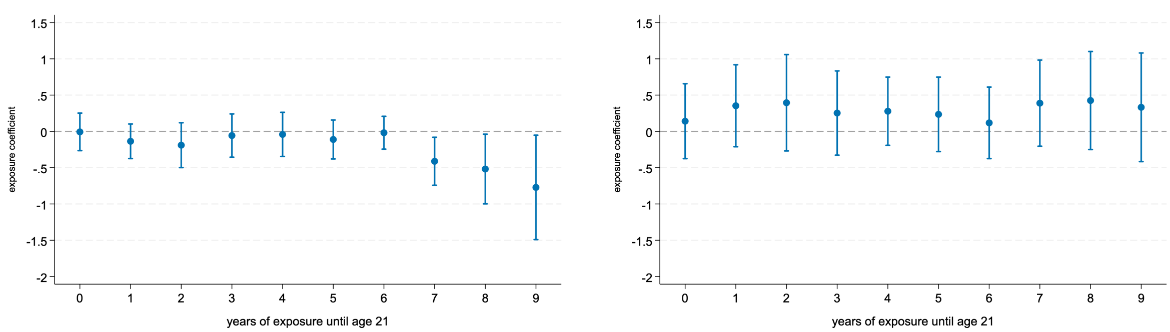
(a) Bottom-to-up mobility **(b)** Bottom persistence



Notes: The figures plot semi-parametric exposure effects of local gender norms on bottom-to-up mobility in panel (a), and on bottom persistence in panel (b) with fitted lines for exposure and selection effects. Points show exposure-specific coefficients; vertical bars denote 95% confidence intervals.

Figure A3: Semiparametric Exposure Effects of Gender Norms, controlling for the Compulsory Schooling Policy

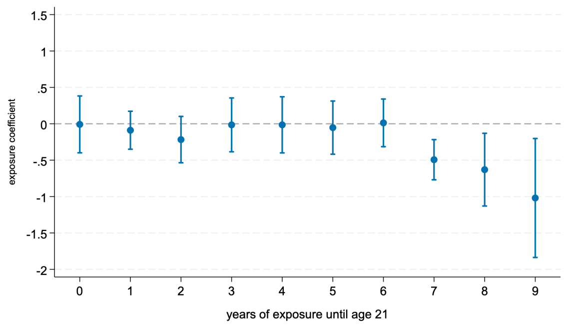
(a) Bottom-to-up mobility **(b)** Bottom persistence



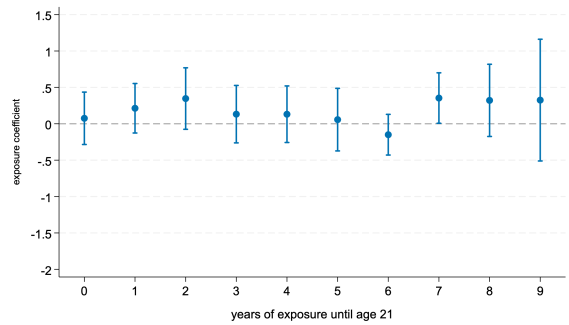
Notes: The figures plot semi-parametric exposure effects of local gender norms on bottom-to-up mobility in panel (a), and on bottom persistence in panel (b). The specification adds interaction of indicator variable for cohorts affected by the policy and the gender-norm gap into the baseline model. Points show exposure-specific coefficients; vertical bars denote 95% confidence intervals.

Figure A4: Semiparametric Exposure Effects of Gender Norms, controlling for Destination×Cohort Fixed Effects

(a) Bottom-to-up mobility



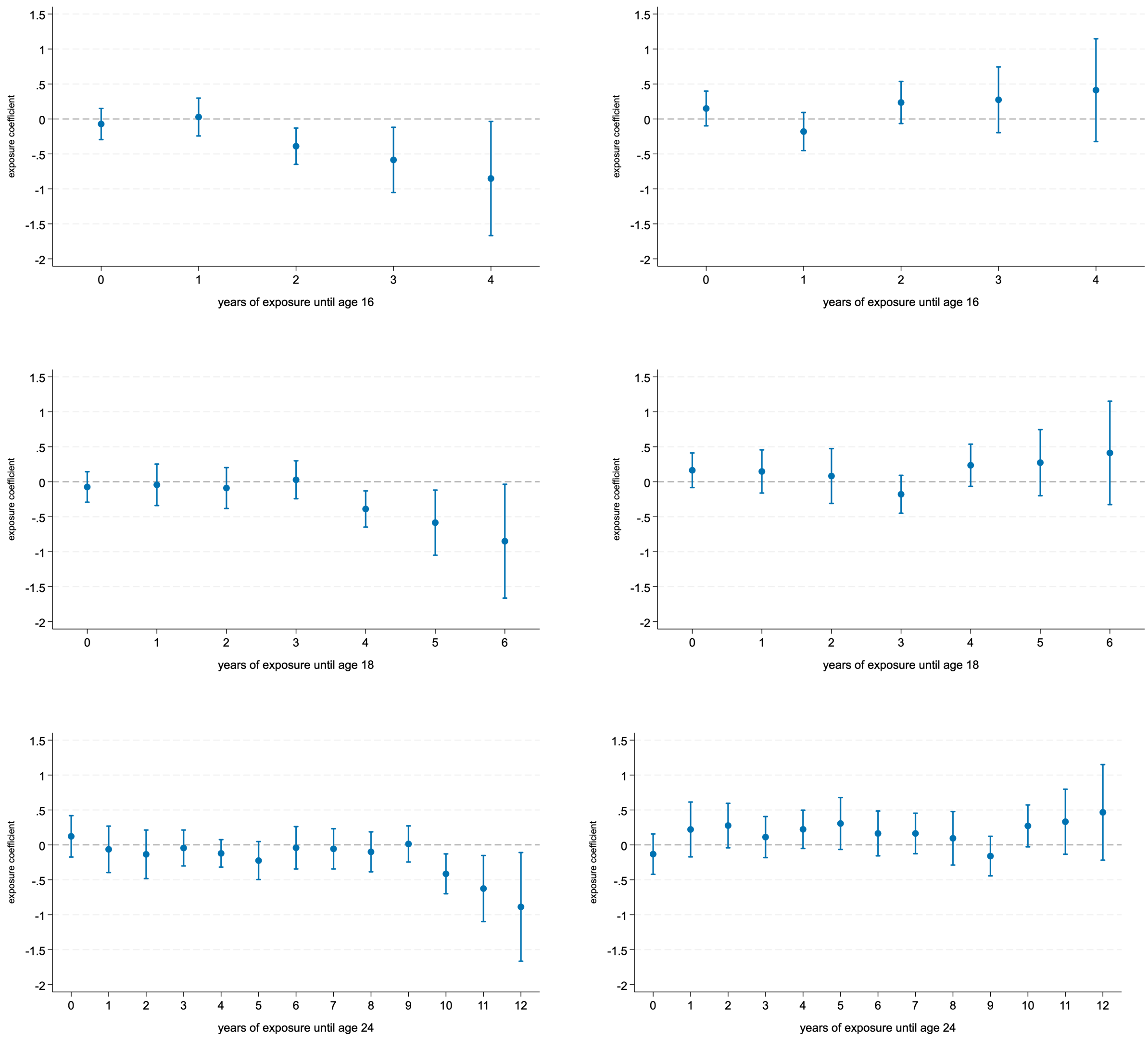
(b) Bottom persistence



Notes: The figures plot semi-parametric exposure effects of local gender norms on bottom-to-up mobility in panel (a), and on bottom persistence in panel (b). The specification adds the destination × cohort fixed effects to the baseline model. Points show exposure-specific coefficients; vertical bars denote 95% confidence intervals.

Figure A5: Semiparametric Exposure Effects of Gender Norms, setting Different Educational Horizons

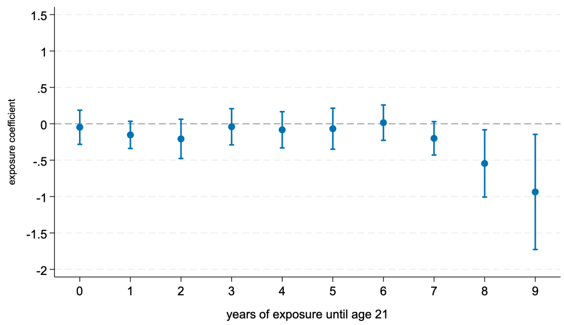
(a) Bottom-to-up mobility **(b)** Bottom persistence



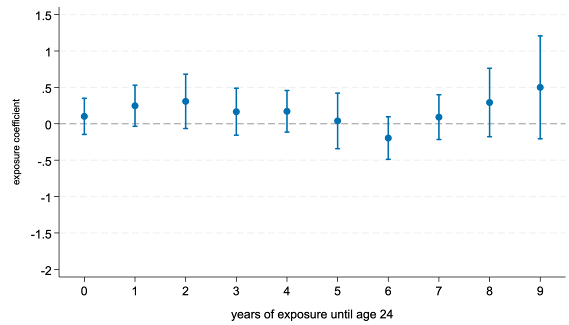
Notes: The figures plot semi-parametric exposure effects of local gender norms on bottom-to-up mobility in panel (a), and on bottom persistence in panel (b) by setting educational horizons to different years. Points show exposure-specific coefficients; vertical bars denote 95% confidence intervals.

Figure A6: Semiparametric Exposure Effects of Gender Norms, excluding Education-motivated Migrants

(a) Bottom-to-up mobility



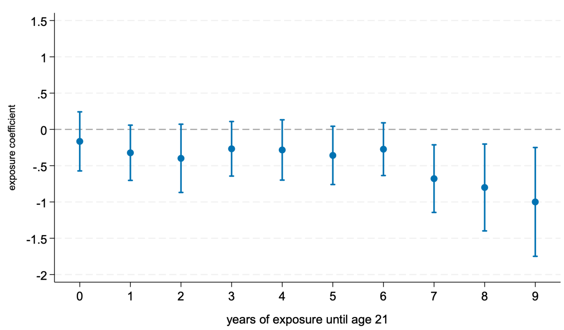
(b) Bottom persistence



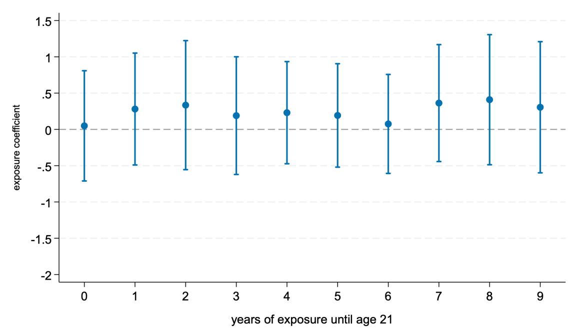
Notes: The figures plot semi-parametric exposure effects of local gender norms on bottom-to-up mobility in panel (a), and on bottom persistence in panel (b) by excluding individuals whose stated reason for migration is education. Points show exposure-specific coefficients; vertical bars denote 95% confidence intervals.

Figure A7: Semiparametric Exposure Effects of Gender Norms, controlling for Cohort $\times$ ΔGN

(a) Bottom-to-up mobility



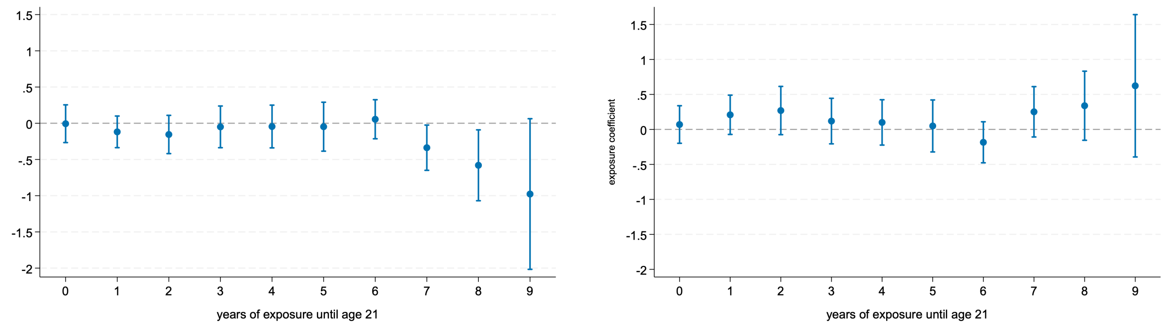
(b) Bottom persistence



Notes: The figures plot semi-parametric exposure effects of local gender norms on bottom-to-up mobility in panel (a), and on bottom persistence in panel (b). The specification adds the interaction of cohort dummies with gender-norm gap to the baseline model. Points show exposure-specific coefficients; vertical bars denote 95% confidence intervals.

Figure A8: Semiparametric Exposure Effects of Gender Norms, focusing on Age ≥ 24

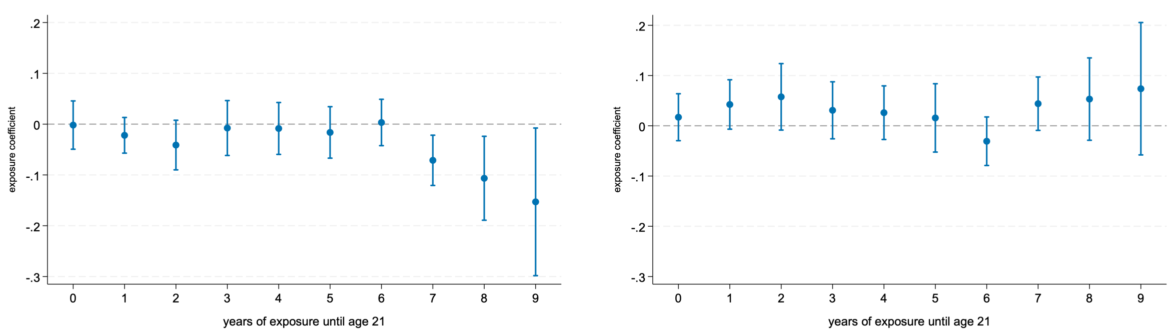
(a) Bottom-to-up mobility   **(b)** Bottom persistence



Notes: The figures plot semi-parametric exposure effects of local gender norms on bottom-to-up mobility in panel (a), and on bottom persistence in panel (b). The sample includes women who are at least 24 years old at the time of survey. Points show exposure-specific coefficients; vertical bars denote 95% confidence intervals.

Figure A9: Semiparametric Exposure Effects, using Standardized Variables for the Gender-Norm Indicator

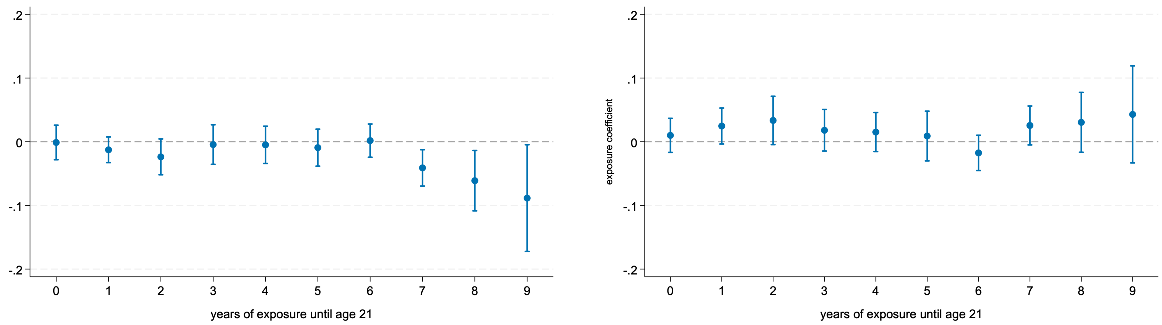
(a) Bottom-to-up mobility   **(b)** Bottom persistence



Notes: The figures plot semi-parametric exposure effects of local gender norms on bottom-to-up mobility in panel (a), and on bottom persistence in panel (b). Gender-norm indicator is calculated using standardized values of three items. Points show exposure-specific coefficients; vertical bars denote 95% confidence intervals.

Figure A10: Semiparametric Exposure Effects, using Gender-Norm Index

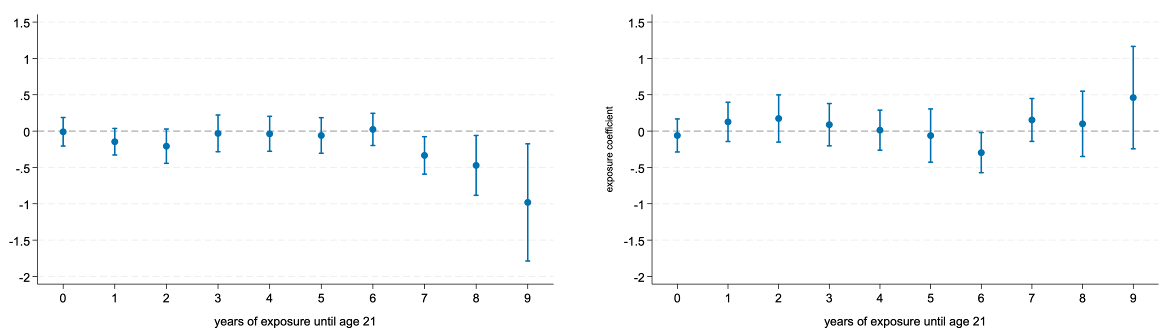
(a) Bottom-to-up mobility **(b)** Bottom persistence



Notes: The figures plot semi-parametric exposure effects of local gender norms on bottom-to-up mobility in panel (a), and on bottom persistence in panel (b). Gender-norm indicator is calculated using principal component analysis. Points show exposure-specific coefficients; vertical bars denote 95% confidence intervals.

Figure A11: Semiparametric Exposure Effects of Gender Norms, without Interpolation

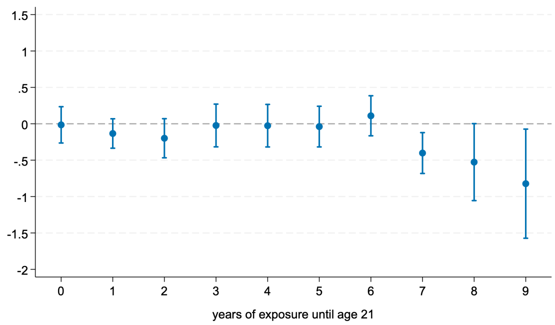
(a) Bottom-to-up mobility **(b)** Bottom persistence



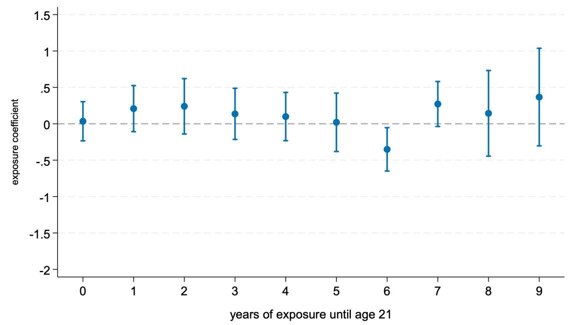
Notes: The figures plot semi-parametric exposure effects of local gender norms on bottom-to-up mobility in panel (a), and on bottom persistence in panel (b). Gender-norm values between two surveys are calculated by holding the most recent survey value constant until the next survey instead of interpolation. Points show exposure-specific coefficients; vertical bars denote 95% confidence intervals.

Figure A12: Semiparametric Exposure Effects of Gender Norms, with Lagged Values of Gender-norm Indicator

(a) Bottom-to-up mobility



(b) Bottom persistence



Notes: The figures plot semi-parametric exposure effects of local gender norms on bottom-to-up mobility in panel (a), and on bottom persistence in panel (b). One-year lagged gender-norm indicator is used in the analysis. Points show exposure-specific coefficients; vertical bars denote 95% confidence intervals.

Table A1: Balanced Test with Pre-move Characteristics

	Paternal education	Maternal education	Literate father	Turkish mother tongue
	(1)	(2)	(3)	(4)
0 year of exposure	0.021	0.035	0.016	0.058
	(0.333)	(0.279)	(0.084)	(0.083)
1-3 years of exposure	-0.341	-0.300	-0.039	0.068
	(0.269)	(0.324)	(0.074)	(0.113)
4-6 years of exposure	0.117	-0.182	0.056	-0.051
	(0.406)	(0.287)	(0.085)	(0.117)
7-9 years of exposure	-0.127	-0.260	0.102	-0.007
	(0.406)	(0.356)	(0.090)	(0.102)
# of observations	3,150	3,150	3,148	3,150

Notes: The table reports balance tests that regress pre-move covariates (paternal education, maternal education, father's literacy, Turkish mother tongue) on exposure-year bins interacted with gender-norm gap. All regressions include origin×cohort, destination,and migration-year fixed effects and survey-wave dummies. Additionally, column (1) include mother tongue dummies, column (4) include paternal education levels, and column (2) and (4) include both covariates. Standard errors are clustered at origin-region level. The estimated coefficients show the association between remaining exposure years and pre-move traits, conditional on fixed effects and covariates.

Table A2: Exposure–Bin Estimates with Reason-for-Move Controls and Leave-One-Category-Out Samples

	Motive controls	Except education related	Except economic related	Except other personal	Except partner related	Except family related	Except other
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
0 year of exposure	0.063	-0.065	-0.029	-0.096	-0.016	-0.040	-0.048
	(0.111)	(0.110)	(0.117)	(0.108)	(0.122)	(0.124)	(0.117)
1-3 years of exposure	-0.200**	-0.176*	-0.151	-0.222**	-0.143	-0.231**	-0.191**
	(0.092)	(0.094)	(0.105)	(0.091)	(0.103)	(0.092)	(0.095)
4-6 years of exposure	-0.041	-0.050	-0.011	-0.072	0.013	-0.105	-0.032
	(0.117)	(0.112)	(0.118)	(0.117)	(0.120)	(0.121)	(0.124)
7-9 years of exposure	-0.353***	-0.270**	-0.379***	-0.350***	-0.307**	-0.494***	-0.387***
	(0.115)	(0.120)	(0.131)	(0.116)	(0.122)	(0.124)	(0.142)
# of observations	3,142	3,032	2,930	3,090	2,755	2,914	2,999

Notes: Outcome: Bottom-to-up mobility. Reported coefficients are slopes of ΔGN within each exposure bin. Each column includes baseline fixed effects and covariates from the main specification as well as the stated reason of migration in 26 categories. Standard errors are clustered at the origin region level. Significance: ***p<0.01, **p<0.05, *p<0.1.