Development Effects of Social Eligibility Criteria for Temporary Migration

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Abstract

Temporary migration offers a pathway to higher income, but can be disruptive to communities of origin. I investigate distortions in origin communities that arise in response to social eligibility criteria that gatekeep access to those opportunities. Specifically, I study the effects of age and marital status eligibility requirements in the case of commuting Palestinian migrant labor to Israel. Over time, the minimum age threshold dropped while holding marriage fixed. Using individual-level Palestinian census data and a record of work permit regulations, I employ a triple-differences design that leverages differential cohort and regional exposure to the policy change. First, I show that men bring forward their timing of marriage in line with changes in the minimum age threshold in more highly exposed areas. Second, I find that women, nonparticipants in the work permit program, are more likely to get married earlier, have children earlier, and register declines in educational attainment in those same areas. I do not find effects on their labor market status. These findings demonstrate how policies that condition labor market access on demographics can have potentially adverse consequences for non-target populations through general equilibrium dynamics.

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1 Introduction

Temporary migration flows have been on the rise, reaching record highs in 2023, with more than 2.4 million temporary work authorizations issued in OECD countries (OECD, 2024), comprising 35% of total migration.[1] Temporary migration has been promoted as an effective poverty-alleviation mechanism in migrants' communities of origin (Bryan, Chowdhury, and Mobarak, 2014), while also enabling host countries to regulate migration flows—meeting labor shortages without adding permanent residents to their populations (Martin, Abella, and Kuptsch, 2006). Temporary migrant worker programs continue to be common in North America, western Europe, and Arab Gulf states, drawing on migrants to work in labor-intensive, low-skill sectors, including agriculture, construction, and manufacturing.[2] Access to these opportunities is mediated by skill-based occupation qualifications, as well as preferences or requirements along demographic dimensions, such as age, gender, and marital status. The latter are often meant to deter migrants from overstaying in host communities.

In this paper, I examine how some migration processes that are meant to protect the host economy can ultimately generate social distortions at the origin, threatening to counteract income gains. I focus on policies that select on demographic features, and marriage in particular. If marriage requirements induce men to get married at increasingly young ages, and this passes through to women who already marry relatively young, we could expect to see negative consequences for childbearing and human capital accumulation. In turn, this could feed into underdevelopment and erode gains from migration. This is a concern given the literature that documents a link between early female marriage and poor socioeconomic outcomes for women and their children Jensen and Thornton (2003); Field and Ambrus (2008); Chari, Heath, Maertens, and Fatima (2017).

At the origin, migration spells can be disruptive to migrant households and communities. Distortions can result from the prolonged absence of the migrant worker, implicit conditions that make some individuals more likely to secure access relative to others, or explicit eligibility criteria. While migration opportunities, temporary or otherwise, come with income gains that could support development at the origin, the social distortions they induce and how communities respond to them could undermine local development.

For receiving countries, the underlying rationale is to minimize the risk of migrants overstaying.[3] In practice, sending-country selection and employer preferences typically favor married individuals

[1] Total migrations here is defined as the sum of permanent and temporary migration. In 2023, the OECD reported 6.5 million new permanent immigrants (OECD, 2024).

[2] Some temporary migration schemes recruit workers in high-skill occupations, such as medicine and technology. However, the focus of this paper is on low-skill recruitment where temporary migration is unlikely to pave a pathway to permanent residency in the host country. For example, employment-based immigrant visas in the U.S. used to hire foreign-born nurses, notably from the Philippines, offer permanent residency (Abarcar and Theoharides, 2024), while H1-B non-immigrant visas recruit high-skill temporary workers.

[3] In some cases, the sending country imposes demographic restrictions on temporary migration, with the intent of protecting their own populations. One such example is the Family Background Report regulation in Sri Lanka, which bans women with children under the age of 5 from migrating overseas for domestic work (ILO, 2018). The ban was partially lifted in 2022, now applying to women with children under the age of 2. Similarly, Nepal has instated age floors (Adrien-Kirby, 2017; Amnesty, 2017), while Bangladesh banned women from migrating for domestic work (Bossavie, 2023).

with dependents, since their families serve as anchors incentivizing migrant return. For instance, the recruitment of Moroccan temporary migrants to work in Spain explicitly selects married women with children females.[4] In other cases, while migrant selection processes do not explicitly target on demographics, in practice such criteria guide selection. This is the case in Canada's Seasonal Agricultural Worker Program (Basok, 2000; Preibisch, 2010; Hennebry and Preibisch, 2012) and New Zealand's Recognized Seasonal Employer scheme (Gibson, McKenzie, and Rohorua, 2008), both of which favor married men with families.

I study the local effects of social eligibility requirements for migration opportunities in the context of commuting Palestinian migrant labor to Israel, where access to jobs is mediated by work permits. The setting lends itself to empirical examination due to (1) the explicit link between labor market access and migrants' demographic profile (age, marital status, and, implicitly, gender), and (2) exogenous changes in the minimum age threshold holding all else fixed. Work permits specify a minimum age and require applicants to be married. In addition, given their sectoral concentration in construction and agriculture, permits *de facto* favor men. Over time, permit eligibility criteria changed, directly altering incentives in marriage markets at the origin. Specifically, in 2007, individuals needed to be at least 30 years of age and married to qualify for a permit. By 2017, the age threshold had dropped several times, reaching 22, while still requiring marriage.

In this context, men get married on average at 25 years of age. And so, conditional on requiring marriage, by moving from a minimum eligibility age of 30 to 22, the policy approximates moving from non-marriage-restricted migration to one where the marriage requirement is a constraint with potential behavioral consequences. Each sequential drop in the age threshold exerts greater downward pressure on the timing of marriage, changing marriage incentives for younger men especially. This, in turn, influences marriage decisions for women, which potentially has broader socioeconomic repercussions.

Two additional features of the setting support identification. First, the labor force in the West Bank (i.e., the origin) exhibits a considerable reliance on jobs in Israel, both historically and contemporarily, suggesting that changes in permit eligibility can have far-reaching effects. In the 1970s through the 1990s, around 30% of West Bank employment was in Israel (Farsakh, 2005). In the contemporary period, 15-20% of the labor force in the West Bank was employed in Israel (LFS, 1996-2021). In terms of remitted income, this population contributes an even larger share. Critically, employment in Israel is restricted by sector-specific quotas. Thus, marriage incentives change for all who apply, or want to apply, for a permit, and not just those who successfully secure one.

Second, the degree to which localities rely on jobs in Israel differs, suggesting that the distortions vary across localities at the origin. Localities are finely disaggregated units with over 450 of them in the estimation sample.[5] Localities vary in the share of workers they send to Israel, some exhibiting

[4]Specifically, Komposch, Schurr, and Escriva (2024) document that "[t]o be recruited by the Moroccan authorities to work in Huelva, workers must be female, married or divorced, between 25 and 45 years old, and have children under the age of 14."

[5]After harmonizing localities across 1997, 2007, and 2017, estimates on the population of the West Bank include 471 localities, while those relying on 20% representative samples includes 458 localities.

greater reliance than others. At the same time, these shares in 2007 are highly predictive of shares in 2017, indicating that there is a high degree of persistence in relative reliance on jobs in Israel across localities. This suggests that the intensity of changes in marriage incentives will vary not only by age group, but also across localities. More reliant localities likely face more pronounced pressures and are consequently more responsive due to a combination of factors. These include the informal job search and matching, social stigma associated with these jobs, and historical dependence.

To capture marriage market distortions and their socioeconomic spillovers, I use individual-level Palestinian census data, alongside an archive of permit regulations. Census data are available for the years 1997, 2007, and 2017. They allow me to link an individual's place of work and locality of residence to a host of socioeconomic outcomes. These include marital status, age at first marriage, childbirth, educational attainment, and labor force status, which I compare before and after the set of policy changes to examine distortions in the timing of marriage for men and women. I match responses in marriage to changes in the minimum age threshold for permit eligibility, which I retrieve from an archive of permit guidance issued by the Israeli Ministry of Defense and maintained by Gisha, an Israeli not-for-profit organization that promotes Palestinian freedom of movement.[6]

I document the effect of the policy using a triple-differences design that leverages differential cohort and regional exposure to the policy changes. I document whether changes in work permit eligibility affect men's timing of marriage, before proceeding to examine spillover effects on women's marriage and related downstream outcomes. First, I establish a response in the marriage behavior of men, who are the direct beneficiaries of the policy. I motivate the preferred specification in three steps, starting with a standard difference-in-differences design. For every cohort of men that became eligible for a work permit between 2007 and 2017, some did so because of the baseline policy regime, and others because of a change in the policy. Thus, I compare the likelihood of marriage for cohorts young enough to have been affected by any of the policy changes to that of cohorts old enough to not have been affected. I then introduce regional variation as a third difference to address parallel trends violations in the standard difference-in-differences, while also documenting differential responses by regional exposure to the policy. Lastly, I decompose the effect by age to see how marriage decisions respond differentially across ages depending on the strength of the incentive for earlier marriage.

I show that declines in the minimum age for permit eligibility, while continuing to require marriage, result in earlier marriage for men. Specifically, I find that men who were young enough to become eligible for a work permit following a change in the requirements are 1 p.p. (s.e. 0.00369) more likely to be married after the policy change, relative to those old enough to not have been affected by the policy change. This is about a 1.5% increase relative to the dependent variable mean. This effect is concentrated in localities that are more exposed to the policy change, where young men exhibit a 3 p.p. (s.e. 0.00599) increase in the likelihood of marriage, or a 4.5% increase.

The response masks considerable heterogeneity by age, since the strength of the incentive to get married varies by an individual's age and the minimum age threshold in effect. I find that younger

[6]For more information, visit their page at https://gisha.org/en/.

men subject to lower minimum age thresholds display more pronounced effects, even among other treated ages. For example, 23-year-old men in 2017 (who became eligible for a permit when they were 22 in 2016) are 5.6 p.p. (s.e. 0.013) more likely to be married after the policy change. This resembles a 19% increase in the likelihood of marriage relative to a dependent variable mean of 30%.[7] While the effects on the likelihood of marriage remain sizable, they dilute for older ages both in terms of magnitude, and relative to the dependent variable mean for that age group, as expected. For example, 25-year-olds are 4.4 p.p. (s.e. 0.017) more likely to get married after the policy change in more exposed areas. This constitutes a 9% increase relative to a control mean of 47%.

Second, I document indirect effects on the marriage behavior of women, and further downstream outcomes by adapting the same identification strategy. Specifically, I directly compare differential responses in women's outcomes by age, before and after the policy changes across more or less exposed localities. Since women's marriage decisions are affected indirectly through changes in men's behavior, I cannot determine *ex ante* how marriage decisions will vary across ages. Thus, I relax the age constraint, instead examining the decisions of women between 14 and 40 years of age. Note that prior to the policy change, in 2007, women on average got married at 19. I first examine responses in their likelihood of marriage across different ages, followed by effects on the likelihood of childbearing, educational attainment, and labor market outcomes.

I find that changes in the eligibility criteria result in earlier marriage for women as well, with girls and women between 16 and 25 years of age more likely to be married in more exposed areas after the policy changes. Similar to men, younger women exhibit more pronounced increases. For instance, the coefficient for 17-year-olds is 6.1 p.p. (s.e. 0.015). This is a 36% increase in the likelihood of marriage relative to a baseline of 14% 17-year-olds married in more highly exposed areas in 2007 (prior to the policy change). In comparison, 20-year-old women are 7.3 p.p. (se.e. 0.018) more likely to be married, which is a 15% increase relative to a control mean of 48%. While it is plausible that women get married earlier without necessarily having children earlier, I find evidence to the contrary. A similar trend holds, with more pronounced effects among younger women in relative terms. Tracking the same age groups, I find that 17-year-olds exhibit a noisy 2.6 p.p. (s.e. 0.021) increase in the likelihood of having at least one child, relative to a control mean of 3%, nearly a doubling. Women who are 20 years old show a 7.2 p.p. (s.e. 0.027) increase in the likelihood of having at least one child, relative to a control mean of 26%.

Impacts on the likelihood of marriage and childbearing, especially among girls and younger women, occur at the same time that they are typically completing secondary school. To that effect, I estimate effects on secondary school completion, which is an important stepping stone for higher education. I find that women between 17 and 21 years of age are less likely to have completed secondary education in more highly exposed areas, after the policy changes. For instance, those who are 19 years old see a 12.6 p.p. (s.e. 0.027), or 19%, decline in completion rates relative to a control mean of 66%. Similarly, 21-year-olds experience a 7.5 p.p. (s.e. 0.032), or 13%, decline relative to a control mean of 59%. These effects are consistent for impacted age groups when

[7] This is the sample share of 23-year-old men married in 2007 in more exposed areas.

considering alternative educational measures, including the rate of dropping out conditional on having completed preparatory school, and enrollment in or completion of an intermediate diploma or a bachelor's degree.[8] Lastly, there are no detectable changes in women's labor force participation or employment, suggesting that women are unlikely to be substituting employment for education.

My paper is related to several strands of the literature. First, it contributes to the literature on the effects of migration on communities of origin. Policies around migration can have general equilibrium effects in communities of origin. For instance, migration leads to income gains that affect consumption patterns at the origin (Bryan, Chowdhury, and Mobarak, 2014), which has been the subject of studies on migrant remittances (Yang, 2008; Christian Dustmann, 2010). Similarly, migration can alter informal risk-sharing in communities of origin (Morten, 2019) and alter investments in human capital in response to job opportunities abroad (Antman, 2012). In this paper, I show that temporary migration can be disruptive to origin communities through social eligibility criteria. Specifically, I demonstrate how conditioning labor market access, or access to migrant work opportunities, on marriage has negative consequences for non-migrant women in communities of origin through marriage market equilibrium effects.

Second, there is a broad literature on marriage markets exploring how they work, including the determinants of marriage decisions. These span demographic and structural (Angrist, 2002; Abramitzky, Delavande, and Vasconcelos, 2011), cultural (Giuliano, 2007; Field and Ambrus, 2008; Banerjee, Duflo, Ghatak, and Lafortune, 2013; Bertrand, Kamenica, and Pan, 2015) and technological and social shifts (Goldin and Katz, 2002; Goldin, 2006; Bailey, 2006; Bailey and Collins, 2011) as drivers of marriage behavior. While some work documents how marriage markets and the economic environment interact with one another (Tertilt, 2005; Schaller, 2013; Autor, Dorn, and Hanson, 2019), the general consensus is that economic forces have limited effects on marriage market dynamics. Work by Corno, Hildebrandt, and Voena (2020) demonstrates that economic incentives, in the way of temporary aggregate income shocks, lead to substantial shifts in marriage decisions and outcomes. I contribute to this strand by showing that marriage decisions considerably respond to policy-induced economic incentives. This further establishes economic incentives, artificially engineered or otherwise, as important determinants of marriage behavior. While the policy I consider ultimately leads to a rise in earlier marriage, and especially so among girls and adolescent women, the results suggest that policies can similarly provide the incentives to combat the prevalence of child and adolescent marriage.

A second strand within the marriage market literature explores labor market-demographic linkages. This literature primarily focuses on how policies or technology shocks affect economic and demographic outcomes (Goldin, 2006; Bailey, 2006), or how changes in economic opportunities affect marriage market outcomes (Jensen, 2012; Schaller, 2016; Kearney and Wilson, 2018; Autor, Dorn, and Hanson, 2019). However, this literature has largely studied within-person effects. My study contributes to this literature by documenting comprehensive cross-gender spillover effects through

[8] Preparatory school is defined as having completed 9th grade, and is the level that directly precedes completing secondary school, which is achieved upon completing 12th grade.

marriage market equilibrium mechanisms. I show how a male-targeted labor policy systematically affects female socioeconomic outcomes through policy-induced changes in marriage market incentives. Unlike existing work that studies demographic responses to direct economic opportunities, I provide evidence of how policies can create cascading demographic effects across gender lines through marriage market distortions, demonstrating that the scope of policy impact extends far beyond direct beneficiaries when policies explicitly condition economic access on demographic characteristics.

Lastly, this work contributes to the policy evaluation literature, which has increasingly recognized that interventions often generate effects beyond their intended scope. Those effects can be positive, compounding gains Duflo (2001); Muralidharan and Prakash (2017), or negative, whereby they compromise or undo benefits (McKenzie and Rapoport, 2011; Crépon, Duflo, Gurgand, Rathelot, and Zamora, 2013). Most policy-evaluation research focuses on spillovers within the same policy domain (e.g., labor programs affecting other labor outcomes), or examines unintended consequences for the same target population.[9] I contribute to this literature by documenting cross-demographic spillover effects from a targeted policy intervention. I show how work permit eligibility criteria that explicitly condition economic opportunities on marital status have adverse consequences for women's marriage timing, timing of fertility, and educational attainment through marriage market equilibrium effects. Critically, the policy *de facto* targets men, but has far-reaching repercussions for women's development outcomes. This analysis demonstrates that policies conditioning benefits on demographic characteristics can generate systematic social distortions that extend beyond the intended beneficiary population. In turn, this supports a reconceptualization of who the target population is, by considering impact across interconnected social and economic domains in anticipation of reasonably foreseeable spillover effects.

2 Setting

Palestinian commuting laborers to Israel require work permits, which have age and marital status criteria, to access better-paying jobs in Israel. Eligibility criteria changed such that the age threshold dropped several times while marriage continued to be binding, distorting incentives in the Palestinian marriage market.

Changes in work permit eligibility have far-reaching effects because they explicitly tie male labor access to marital status, in a context where Palestinian employment in Israel is sizable. These effects vary spatially, further supporting identification in this setting.

2.1 Target population, policy change, and the marriage market

Since the early 1990s, commuting Palestinian laborers have required individual work permits to access better-paying jobs in Israel. These permits have age and marital status criteria, explicitly

[9] Work by McKenzie and Rapoport (2011) is one such example where the authors consider spillover effects beyond the target population. Specifically, while increased returns to migration lowers the chances of boys completing junior high school, they also document declines in education attainment for girls due to increased housework.

conditioning labor market access on demographic factors. Over the years, the criteria changed, altering the incentives for marriage around certain ages. In the study period spanning 2007 though 2017, the criteria underwent three such changes. In 2007, permits required that one is at least 30 years of age and married. In 2012, the age requirement alone dropped to a minimum of 26 years old. Similarly, in 2013 the age threshold alone dropped to 24 years of age, and finally in 2015 it dropped to 22 years of age. As marriage continued to be binding, each sequential drop in the age threshold exerted greater downward pressure on the age of marriage.

The changes in permit eligibility shock one side of the marriage market, directly altering the incentives for marriage among men. While the policy allows both men and women to work in Israel so long as they clear permit criteria and manage to secure a work permit through an employer, in practice men primarily work these jobs. This is a function of local economic conditions and the sectoral composition of work opportunities in Israel. The Palestinian labor market is characterized by a low female labor force participation rate, and high female unemployment. Over the course of the study period, female labor force participation was around 18% and female unemployment at 24% – labor market conditions are more severe among younger women who became eligible for work in Israel following the policy changes. This suggests that women are less likely to commute to work in Israel, even when given the opportunity and barring the commute itself being a prohibitive force.

Additionally, the sectoral composition of the work-permit quota supports male over female employment. Palestinian employment in Israel is governed by a sector-specific quota. While the quota has been rising over time, the sectoral composition remained stable with employment concentrated in labor-intensive sectors. Construction has consistently comprised roughly two-thirds of Palestinian employment in Israel, followed by agriculture at around 15%.

The work permit regime in Israel/Palestine then effectively conditions labor market access for men on marriage, with each policy change placing further downward pressure on men's age at marriage. The staggered decline in the age threshold will likely induce differential responses in marriage behavior by age. First, in the West Bank, men get married on average at 25 years of age, and over 90% are married by 35. Lower age thresholds then have greater room for response because men are typically not married by then, and marriage binds for jobs access. Second, younger men are still earlier on in their work life-cycle, suggesting greater flexibility in switching jobs or pursuing different opportunities, relative to older men who are further along in their careers and more likely to be locked-in their current jobs. Thus, younger men are both less likely to meet the conditions for a permit while also valuing it more, inducing a greater behavioral response to the policy changes.

2.2 Policy reach

Palestinian commuting laborers to Israel constitute a sizable share of Palestinian employment, both historically and contemporarily, such that changes in permit eligibility have considerable reach. Historically, the share of workers employed in Israel in total employment for the West Bank hovered around 30%. Over the past two decades, on average 15% of West Bank employment was in Israel or Israeli settlements in the West Bank, with that share trending upwards over time. The reliance

of Palestinian workers on jobs in Israel suggests that changes in permit eligibility are likely to pass-through to Palestinian communities. Critically, eligibility and potential access to jobs are sufficient to bring about behavioral responses among the target population, especially since demand for permits exceeds supply. Thus, the reach of the policy extends beyond those who ultimately secure a job in Israel due to changes in eligibility criteria to encompass behavioral responses by those who are now merely eligible for a work permit.

While men are the de facto targets of the policy, conditioning labor access on marriage inadvertently draws in women. The policy's reach then extends to women's marriage decisions and any pursuant socioeconomic repercussions in response to the marriage market shock on men's side. As such, a policy that targets men but ties their labor market access to demographic filters winds up potentially having unintended general equilibrium effects stemming from the social rearrangement it induces in communities.

2.3 Labor flows across localities

The work permit regime generally applies uniformly across the West Bank, but in practice policy reach varies spatially. As described in section 3.1 below, localities exhibit persistent and differential reliance on jobs in Israel, such that changes in eligibility criteria may have more pronounced effects in places that send more labor to Israel. This is consistent with network effects, weaker social stigma, and historical reliance. First, employer-employee matches are highly informal and unregulated. Permits are issued to employers, and so employees need to find employers who are prepared to hire and obtain a work permit for them. The most common way of finding employers is by word-of-mouth, through family, friends, and community members. Thus, localities that send more labor to Israel are likely to be more responsive to changes in the permit policy because policy information is more likely to flow to those communities and job matches are more easily facilitated by those who already work in Israel. This is especially the case since even localities that exhibit greater reliance on jobs in Israel are not fully saturated for competition effects between laborers to dominate.

Second, there is social stigma in Palestinian society around working in Israel given the political dynamics in Israel/Palestine. This is moderated in communities that exhibit a higher reliance on jobs in Israel where they seek out those jobs due to economic need. In communities that are less exposed to employment in Israel, this taboo serves as an inhibiting force, and could interfere with the flow of information on accessing those jobs and one's ability to secure a permit. Lastly, and relatedly, localities in the modern period exhibit persistence in their degree of reliance on jobs in Israel over time. This phenomenon dates back to the late 1960s, where jobs in Israel absorbed even more labor from the West Bank than they did in the contemporary period. Modern day locality-level persistence combined with deep historical labor ties suggest that locality-level exposure to the Israeli labor market is differentially entrenched across West Bank localities. These locality-level dynamics imply that while the policy applies across the West Bank, variation in the degree of exposure may lead to more pronounced effects in some geographies over others. Specifically, more exposed localities, as defined by their degree of reliance on jobs in Israel, are likely to be more

responsive to changes in permit eligibility.

3 Data and descriptive statistics

I use individual-level Palestinian census data, which allow me to link place of work and locality of residence to a host of socioeconomic outcomes. To track policy changes, I rely on an archive of permit guidance specifying changes in sector quotas and permit requirements. Lastly, I collect survey data to shed further light on both the institution and processes governing the work permit regime, as well as underlying mechanisms.

3.1 Palestinian census data

I use 20% representative Palestinian census samples for the years 1997, 2007, and 2017. The data are at the individual level. Key variables include age, gender, labor force status, educational attainment, marital status, age at first marriage, number of children, place of work, and locality of residence. The locality identifier allows me to define an individual's exposure to the policy changes at the locality level, and to then examine spatially heterogenous responses to the policy changes as a function of the degree of exposure. These responses include an individual's timing of marriage, educational attainment, and childbearing/household formation decisions.

I harmonize localities across census years by aggregating or merging units to the largest common locality. There are 11 governorates in the West Bank that contained 666, 524, and 580 localities in the 1997, 2007, and 2017 censuses, respectively. Locality boundaries changed over time, with some being absorbed by other localities, others splitting from larger localities, and others yet newly emerging or disappearing altogether. The harmonization over 2007 and 2017 yields 501 harmonized West Bank localities, and that spanning 1997 through 2017 yields 491 West Bank localities.[10] Figure 10 maps variation in the 2007 share of workers in Israel across the harmonized localities of the West Bank.

To allow for spatial heterogeneity, I define a measure of locality-level exposure that captures the extent to which a given locality is likely to respond to changes in permit eligibility. As I argued in section 2.3, one such measure is the share working in Israel in a given locality. In table X I compare localities that send more labor to Israel (more exposed) to those that send less (less exposed) in 2007, split at the median share working in Israel.

3.2 Permit quotas and eligibility criteria

To track changes in permit sector quotas and eligibility criteria, I refer to an archive of policy guidance maintained by Gisha, an Israeli not-for-profit organization that focuses on the freedom of movement of Palestinians. Permit guidance is set by the Coordinator of Government Activities in the

[10] Unless noted otherwise, I rely on the harmonization scheme over the 2007 and 2017 census years to maintain larger geographic coverage. I bring in 1997 data, and by extension rely on the 1997-2017 harmonization scheme, in conducting robustness checks and addressing identification assumptions including parallel trends.

Territories (COGAT), a unit in the Israeli Ministry of Defense that is responsible for administering civilian policy in the West Bank and Gaza.

While the individual permit system was introduced in the 1990s, to the best of my knowledge public documentation of permit protocols dates back to 2005. Starting in 2012, Gisha waged a couple of legal battles in Israeli court to get the Israeli government and military to publish all written procedures they work according to, these include permit procedures. In 2014, they received the procedures from previous years and went to court again to make the military publish to the public every new and valid procedure. However, under the Israeli Freedom of Information law, they could only request procedures seven years prior to the request date. Thus, publicly released procedures only go back to 2005.

I compiled the data in those reports to track trends in sector quotas and permit eligibility criteria in the West Bank over time. Appendix figure A.1 shows the evolution of the total quota, as well as the quota for construction. Over this period, construction maintained a stable share of total quotas with an average of 64%. While each sector has its own quota and eligibility criteria, I focus on construction not only because it comprises a majority of the quota, but also since it shares the same criteria with some of the remaining sectors. Appendix figures A.2 and A.3 show examples of work permit allowances from COGAT-issued procedures from May 2011 and September 2017, where this variation across sectors is apparent.

4 Empirical strategy

The analysis proceeds in two steps. First, I document whether changes in work permit eligibility affect the timing of marriage for men, who are directly impacted by the policy. Next, I examine the indirect effects of the policy changes on the community of origin, through marriage market equilibrium effects. Specifically, I estimate effects on women's timing of marriage insofar as it is affected by changes in men's marriage incentives and decisions, and related downstream effects on their fertility, educational attainment, and labor market status.

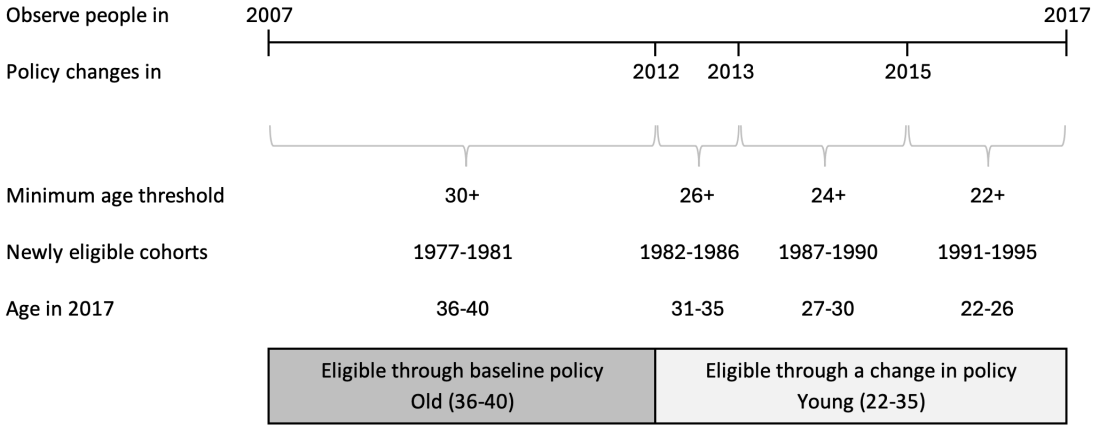
4.1 Direct effects of eligibility changes on men

To assess men's policy response, I develop the preferred empirical specification in three stages. I first rely on a standard difference-in-differences design to compare differences in the likelihood of marriage between men young enough to have been exposed to the policy changes and those old enough to not have been, before and after the policy changes. The design, however, poses serious threats to the parallel trends assumption. As a result, I introduce a third difference that leverages regional variation in exposure to the policy changes. The triple-differences design captures the differential response in the likelihood of marriage among men young enough to have been exposed to the policy changes in more highly exposed areas. Lastly, I allow the response to vary by age since even among men exposed to the policy changes, the extent to which the policy changes exert downward pressure on the age of marriage for work eligibility differs.

4.1.1 Difference-in-differences by cohort exposure

Changes in permit eligibility apply across the West Bank, but in the period of study they affect cohorts of men differentially. Specifically, some cohorts are old enough to have become eligible under the baseline policy regime, while others only become eligible, or become eligible earlier, because of a policy change. This yields two coarse sets of cohorts in the post period, those who are exposed to the policy changes and those who are not. To capture aggregate effects, I compare the marriage decisions of exposed and non-exposed cohorts in the post-period to the decisions of their counterparts, in terms of age, prior to the policy changes. I describe this in figure 1, and elaborate on it below.

Figure 1: Sample selection based on policy changes between 2007-2017



To define young and old cohorts, I first establish the set of directly affected cohorts and ages in the study period. I consider the population of males from the West Bank belonging to cohorts that became eligible for a work permit between 2007 and 2017, under the reigning permit regime in any given year. This corresponds to cohorts born between 1977 and 1995, inclusive. Those born in 1977 turned 30 in 2007, and so they became eligible for a permit under the baseline permit criteria. In each following year, the younger cohort becomes eligible. In 2012, however, while the 1982 cohort that turns 30 that year becomes eligible, so do individuals ages 26-29 (born 1983-1986) due to the policy change that reduced the minimum age threshold to 26. Following similar logic through 2017, those born in 1995 turn 22 in 2017, making them the youngest cohort to become eligible under the third and last permit criteria change in that period, requiring that individuals be at least 22 years old and married. In 2017, when I observe individuals in the census, these correspond to men aged 22-40. Thus, in 2007 and 2017, I restrict the sample to males from the West Bank aged 22-40.

Next, I split the sample into young and old men depending on when they became eligible for a permit. I consider a man young if he became eligible because of any one of the three policy changes. The first cohort to be affected by the policy change is that of 1982: individuals who turned 30 in 2012, the year of the first policy change. Those who turn 30 in 2012 would have been eligible

under the baseline policy regime. However, this cohort is partially exposed to the policy insofar as those who turn 30 in December, for example, can now qualify for a permit sooner. It is conceivable, therefore, that the policy change can bring forward a marriage decision even for those who would have turned 30 and qualified under the baseline regime. As such, cohorts that are exposed to the policy are those born between 1982 and 1995, while those who were old enough to not have been exposed are those born between 1977 and 1981. These correspond to an exposed population aged 22-35, and a non-exposed population aged 36-40 in 2017.

Therefore, the difference-in-differences design compares changes in the likelihood of being married for young (22-35) and old (36-40) men before (2007) and after (2017) the set of permit eligibility changes. The baseline specification is:

$$\text{Ever married}_{iatl} = \alpha + \beta(\text{Post}_t \times \text{Young}_i) + \gamma_t + \delta_a + \nu_l + \varepsilon_{iatl}, \tag{1}$$

where $\text{Ever married}_{iatl}$ is a dummy indicating that individual i in locality l, aged a in census year t is married, Young_i is a dummy indicating that individual i is between ages 22-35 (0 for those ages 36-40), and Post_t is a dummy indicating census year t is 2017 (0 for 2007). β is the coefficient of interest, capturing the effect of the policy changes on the likelihood of younger men getting married. I include census-year fixed effects γ_t to account for time-varying effects across localities. δ_a is a vector of age fixed effects, controlling for different likelihoods of being married at different ages. Lastly, I include locality fixed effects, ν_l, to control for time-invariant unobservable characteristics within localities. Robust standard errors are clustered at the locality level.

The identifying assumption is that the marriage behavior of younger individuals tracks that of old individuals over time, in the absence of the policy change. However, the evolution of the likelihood of marriage with respect to age suggests that the parallel trends assumption is violated in this context. As figure A.4 shows, the share of ever married men is consistently high and stable by the early 30s. The convergence in the share of men ever married with age, which is also stable across time, suggests that shocks to the marriage market might not show up for older individuals, despite distributional shifts among younger individuals. As a result, due to the eventual convergence in the rate of marriage for older individual, old and young are poor comparison groups in a difference-in-differences setting. In the following section, I propose a triple-differences design that might address this concern.

4.1.2 Triple differences

To alleviate concerns stemming from the difference-in-difference design, I incorporate a third difference, leveraging variation in regional exposure to the policy changes. In addition, the difference-in-differences design might capture the effects of other policies or interventions that differentially affected the likelihood of marriage for men. By introducing a third difference that captures the degree of local exposure to the policy change, I can better attribute marriage market effects to changes in permit eligibility. This is subject to testable assumptions both on how the degree of

regional exposure affects the likelihood of earlier marriage, and on the extent to which marriage is patrilocal. I account for differential regional exposure to the policy changes using locality-level heterogeneity in the degree of reliance on jobs in Israel at baseline. Specifically, this is the share of workers in Israel per locality in 2007, where I define localities with an above-median share as high-exposure localities. As discussed in section 2.3, localities that send more labor to Israel are likely more exposed to policy changes due to network effects, weaker social stigma, and historical dependence. If the policy is in fact broadly relevant, more exposed localities are more likely to exhibit detectable changes in marriage decisions in line with changes in the minimum age threshold. The triple-difference specification is:

$$\text{Ever married}_{iatl} = \beta_0 + \beta_1(\text{High exp}_l \times \text{Post}_t \times \text{Young}_i) + \beta_2(\text{High exp}_l \times \text{Young}_i) + \beta_3(\text{Post}_t \times \text{Young}_i) + \beta_4(\text{High exp}_l \times \text{Post}_t) + \gamma_t + \delta_a + \nu_l + \varepsilon_{iatl}, \quad (2)$$

where High exp_l is a dummy indicating locality l has an above-median share of workers in Israel in 2007. All other variables are as previously defined, and robust standard errors are clustered at the locality level. The coefficient of interest is β_1, for $a = 22 - 40$, capturing the differential response in the likelihood of marriage for young men after the policy changes in more highly exposed areas.

The triple-differences parallel trends assumption is more flexible than its difference-in-difference counterpart. In this case, the identifying assumption is that in the absence of changes in the minimum age threshold, the difference in the evolution of the likelihood of marriage between younger and older men is the same in more and less exposed localities. Importantly, it accommodates locality- and age-specific trends. For instance, I showed that high and low exposure localities differ systematically in the likelihood and age of marriage prior to the policy change, as well as along other margins of interest. These differences, however, are differenced out in a triple differences design. I test and discuss this assumption in greater detail in section 6.1.

4.1.3 Triple-difference with age heterogeneity

Lastly, I allow for differential responses to the policy changes by age due to two primary considerations. First, as described in section 3.2, earlier permit allowances applied to multiple ages, making it more difficult to cleanly define cohort exposure. For example, in the September 22, 2008, guide, there were 19,500 permits for workers in construction, non-seasonal agriculture, and industry and services. These had the following age requirements: 12,800 for those at least 35 years old, 5,700 for those at least 30 years old, and 1,000 for those at least 28 years old. One approach is to take the minimum age threshold to define exposed cohorts, and another is to use the age threshold with the largest permit allowance, each of which will bias aggregate results differently. Thus, by allowing for differential responses by age, I can estimate effects while consistently and accurately accounting for how each cohort was affected by the policy changes.[11]

[11] For exposition, I use a minimum age threshold of 30 for the baseline policy in 2007. This can be seen in figure 1. This strikes a balance between exposed ages and the distribution of permit allowances across them. For instance,

Second, each sequential drop in the age threshold exerts greater downward pressure on the age of marriage for men to qualify for a permit. This is the case both across policy waves and within each wave. This is in part driven by the fact that on average men in the sample get married at the age of 25, suggesting that drops in the age threshold below 25 disrupt marriage incentives to a greater degree. Additionally, within each policy wave, younger individuals again face more disruptive incentives. For example, when the minimum age threshold dropped from 26 to 24, the incentives a 25 year old faces to get married is weaker than that a 24 year-old does because the likelihood of marriage is increasing in age. Lastly, older men who become eligible for work permits are more likely to already be settled in a job relative to younger men who more recently entered the labor force (and face starker employment prospects), making younger men potentially more responsive to changes in eligibility.

To capture differential responses by age, I decompose the aggregate effect in equation 2 as follows:

$$\begin{aligned}\text{Ever married}_{iatl} = \beta_0 + \sum_{a=22}^{40} \beta_{1a}(\text{High exp}_l \times \text{Post}_t \times 1[\text{Age}_i = a]) \; + \; \sum_{a=22}^{40} \beta_{2a}(\text{High exp}_l \times 1[\text{Age}_i = a]) \\ + \; \sum_{a=22}^{40} \beta_{3a}(\text{Post}_t \times 1[\text{Age}_i = a]) \; + \; \beta_4(\text{High exp}_l \times \text{Post}_t) + \gamma_t + \delta_a + \nu_l + \varepsilon_{iatl}, \quad (3)\end{aligned}$$

where $1[\text{Age}_i = a]$ is a dummy indicating that individual i is age a, for $a = 22 - 40$. All other variables are as previously defined, and robust standard errors are clustered at the locality level. β_a coefficients capture differential responses in the likelihood of marriage by age, as a result of changes in permit eligibility requirements. The omitted category is $a = 36$, and so β_{1a} are interpreted relative to those 36 years old.

4.2 Indirect effects of eligibility changes on women

After exploring the direct effects of the policy changes on men's timing of marriage, I examine women's response to this marriage market shock. This includes shifts in their timing of marriage, as well as other socioeconomic outcomes. Namely, these are childbearing, educational attainment, and labor force status. To do so, I apply the triple-differences specification in equation 3 to the sample of women between 14 and 40 years of age. Since women are affected only to the extent that men are, it is unclear which ages will be most impacted by and responsive to this shock. Thus, I estimate responses starting at 14 years of age, the earliest age at which respondents are asked about

while roughly 66% of the quota is targeted to those 35 years and older, a still sizable share of 29% is targeted to those at least 30 years old. In comparison, only 5% is dedicated to those 28 years and older. And so while 28 year olds are minimally exposed, those 30 and above are competing for a more considerable number of permits. Including 30 year olds additionally adds control groups who are younger and potentially more comparable to younger groups that are exposed to the policy change.

their marital status in the census.[12] The specification is as follows:

$$\begin{aligned} Y_{iatl} = \beta_0 + \sum_{a=14}^{40} \beta_{1a}(\text{High exp}_l \times \text{Post}_t \times 1[\text{Age}_i = a]) \; + \; \sum_{a=14}^{40} \beta_{2a}(\text{High exp}_l \times 1[\text{Age}_i = a]) \\ + \; \sum_{a=14}^{40} \beta_{3a}(\text{Post}_t \times 1[\text{Age}_i = a]) \; + \; \beta_4(\text{High exp}_l \times \text{Post}_t) + \gamma_t + \delta_a + \nu_l + \varepsilon_{iatl}, \end{aligned} \tag{4}$$

where Y_{itl} is the outcome of interest for female i living in locality l age a in censes year t. Outcomes are: Ever married$_{iatl}$, Any child$_{iatl}$, Completed secondary$_{iatl}$, Dropped out$_{iatl}$, Higher education$_{iatl}$, LFP$_{iatl}$, and Employed$_{iatl}$. Ever married$_{iatl}$ is a dummy indicating female i in locality l aged a in census year t is ever married. Any child$_{iatl}$ is an indicator for female i in locality t age a in census year t having at least one child. Completed secondary$_{iatl}$ is a dummy for female i in locality l age a in census year t having completed 12th grade with *Tawjihi* (national certificate examination). Dropped out$_{iatl}$ is a dummy for individual i in locality l age a in census year t having enrolled and left school, conditional on completing preparatory school.[13] Higher education$_{itl}$ is a dummy for individual i in locality l in census year t having completed or currently enrolled in an intermediate diploma or bachelor's degree. LFP$_{iatl}$ is a dummy for individual i in locality l in census year t being in the labor force. Lastly, Employed$_{iatl}$ is a dummy for individual i in locality l in census year t being employed.

All other variables are as previously defined, and robust standard errors are clustered at the locality level. The coefficients of interest are β_{1a}, for $a = 14 - 40$, capturing the differential response in the outcome of interest for women after the policy changes in more highly exposed areas, by age.

5 Results

In this section, I first show that the policy changes affect men's timing of marriage in line with changes in the age thresholds. I attribute the effects to younger men, and especially those in more highly exposed areas. Next, I show results for women's timing of marriage in response to this shock in the marriage market, along with other spillover effects on their fertility, education, and employment.

5.1 Direct effects on men's timing of marriage

I begin by examining the results of the aggregate difference-in-differences and triple-differences strategies to see if men's timing of marriage responds to changes in the minimum age threshold. The results are in table 1. Column 1 shows the results from the difference-in-differences specification

[12] In the 1997 and 2007 censuses, this question was asked of respondents 12 years old and above. However, in the 2017 census, they only asked respondents who were at least 14 years old. As such, I rely on the 2017 coding, and consider marital status starting at 14 years of age in my analysis.

[13] In census, they record educational attainment, with the following options in order of greater attainment: illiterate, can read and write, elementary (completed 6th grade), preparatory (completed 6th grade), secondary (completed 12th grade with *Tawjihi*), intermediate diploma, bachelor, higher diploma, master, PhD. Conditioning on having completed preparatory school will be discussed in the results section.

in 1, while column 2 shows estimates from the triple-differences specification in equation 2. While suggestive, the column 1 shows that young men are 1 percentage point more likely to get married after the policy change. It is a suggestive though relatively small effect of 1.5%. Incorporating locality variation and exposure, in column 2, reveals a 3 percentage point increase in the likelihood of young men getting married after the policy changes in more highly exposed areas. These results show that the increases are concentrated in more highly exposed localities. Overall, the aggregate results indicate that exposure to the policy changes brings about a response in young men's timing of marriage (i.e. men exposed to the policy), consistent with the incentives induced by the decline in the minimum age threshold to quality for a work permit while still requiring marriage.

Table 1: Changes in men's likelihood of marriage after minimum-age eligibility changes

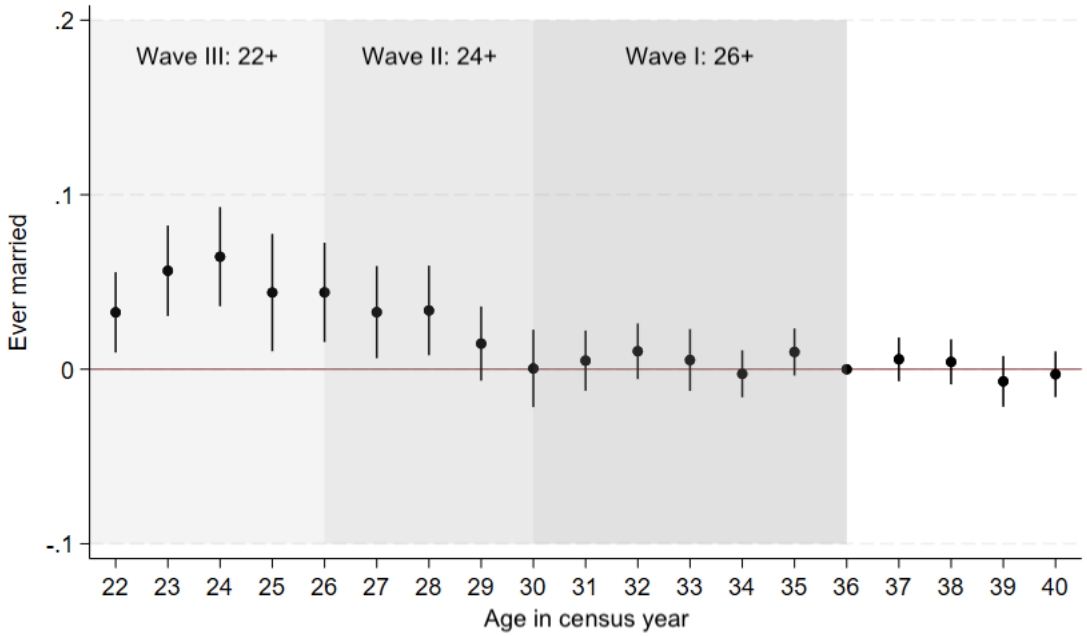
	(1)	(2)
High Exposure x Post x Young		0.0305
		(0.00599)
		[0.000]
Post x Young	0.0102	-0.0039
	(0.00369)	(0.00347)
	[0.006]	[0.260]
High exposure x Post		-0.0014
		(0.00299)
		[0.639]
High exposure x Young		0.0428
		(0.01495)
		[0.004]
Observations	623,470	623,470
Localities	471	471
Adjusted R^2	0.378	0.379
Dependent variable mean	0.678	0.678

Notes: Robust standard errors, clustered at the locality level, are in parentheses, and p-values are reported in brackets. Regressions includes age, year, and locality fixed effects. These results are based on census population data of the West Bank for 2007 and 2017, instead of the 20% representative samples used in later analysis. Recall that the analysis is restricted to men ages 22-40, with those between 22-25 coded as young. The mean of the dependent variable captures the likelihood of marriage across the estimation sample. Column (1) estimates equation 1, while column (2) estimates equation 2.

However, this could mask heterogeneity across different ages, which are exposed differentially to the policy changes. Differential exposure stems from two sources. First, each policy change affected a different set of cohorts. For example, when in 2013 the age threshold dropped from 26 to 24, 22 year olds remained ineligible, and would be so for another two years. Second, with sequential drops in the age threshold, the incentive for earlier marriage is greater because the younger one is the less likely they are to be married. In this context, the average age of marriage is 25, and so a 26 minimum age threshold is more aligned with trends in society than a 24 minimum age threshold, which might induce earlier marriage for the sake of eligibility. Equation 3 estimates these differential

responses after the set of policy changes by age in more highly exposed. Figure 2 shows the β_{1a} coefficients for ages $a = 22 - 40$, with age 36 being the omitted category. We see that age groups that became eligible for a work permit due to the third and final policy change (22-26 year olds as observed in 2017) exhibit greater increases in the likelihood of marriage. This is followed by the younger ages that became eligible because of the second policy change (26-27 year olds as observed in 2017), before the effects dissipate.

Figure 2: Men's likelihood of marriage after policy changes in high exposure localities



Notes: These estimates are based on the census *population* of men 22-40 in the West Bank for the years 2007 and 2017. The figure plots the coefficient on the interaction between High $\exp_l$ x Post_t x 1[Age_i = a], capturing the differential effect of the policy change in more highly exposed areas for ages $a = 22 - 40$ (where $a = 36$ is omitted) on a man's likelihood of marriage. Recall that, as observed in 2017, men between 22-35 become eligible due to a change in policy, while those or ages 36-40 became eligible under the baseline policy.

Note that the effects are more sizable for younger individuals in more highly exposed areas not only in terms of the coefficient size itself, but also relative to the dependent variable mean, which is lower for younger ages. For example, only 23.5% of 23 year-old men are married in 2007, while 42% of 25 year-olds are married in 2007. Thus, the 5.6 percentage point increase in the likelihood of marriage among 23 year-olds resembles a 24% increase relative to the dependent variable mean for that age, while the 4.4 percentage point increase for 25 year-olds resembles a 10% increase relative to the dependent variable mean for 25 year olds. Even among 28 year-old men in more highly exposed localities, there is a 3.4 percentage point increase in the likelihood of marriage after the policy changes. This resemembles roughly a 5% increase relative to a control mean of 71%. And so, while the effects are sizable among young men, even among that subgroup, younger men show

more pronounced increases in the likelihood of marriage relative to their age group's likelihood of marriage.[14]

Results on men's likelihood of marriage pursuant of the policy changes, taking into account heterogeneous age and regional effects, indicate that men get married earlier to become eligible for a permit, especially in more highly exposed localities where permit eligibility is more likely to translate to securing a job in Israel.

5.2 Spillover effects on women's socioeconomic outcomes

In this section, I explore the effect of the marriage market shock on women's socioeconomic outcomes in more highly exposed localities, in the aftermath of eligibility changes. I first examine how women's timing of marriage responds to this shock, and the extent to which this is reflected in childbearing outcomes. Next, I explore spillover effects in the spheres of education and the labor market.

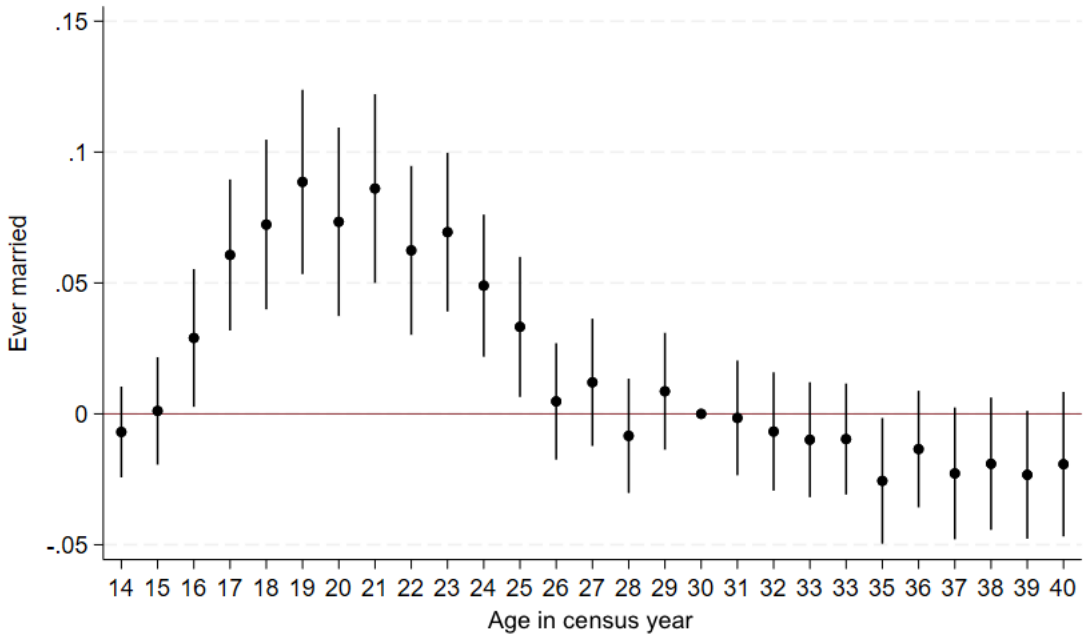
5.2.1 Timing of marriage

Having established that the work permit regime is a prominent institution that shapes the incentives and behaviors of men in order to qualify for a permit, I focus on the spillovers it has on women. Changes in the minimum age threshold for permit eligibility as marriage continues to be binding alters the marriage market through men's behavior. This constitutes a shock to women, where it is not a priori clear which ages will respond nor how. There is greater demand for marriage among younger men, and so I first explore what this does to the likelihood of marriage among women in more highly exposed areas between the ages of 14 and 40. Figure 3 shows the estimation results for equation 4 for the outcome Ever married$_{iatl}$, plotting the β_{1a} coefficients for $a = 14 - 40$ (where $a = 30$ is the omitted category). We see that girls and women between 16 and 25 years of age in more highly exposed areas are more likely to be married after the policy changes. While the coefficients increase, peak at 19, and then decline, relative to the dependent variable mean, younger women exhibit the most pronounced increased. This is shown in figure 4, where I plot the coefficient relative to the control mean for each age group.[15] For instance, for 16 and 17 year olds, the effect resembles a 35-45% increase relative to the dependent variable mean, which is a much starker increase relative to the effects we see among younger men.

[14]Note that the control means cited refer to the share of men married of a given age in 2007, using data on the *population* of the West Bank. I do not distinguish between high and low exposure areas. If I compare these coefficients to the the rate of marriage per age group in 2007 in high exposure areas, the relative effects sizes will decline since marriage rates are generally higher in those areas, but the dissipation of effects with age continues to hold. For example, using the 2007 representative census sample, the share of men married in high exposure areas is 30% for 23 year-olds, 47% for 25 year-olds, and 75% for 28 year-olds. Using this as the control mean gives relative effect sizes of 19%, 9%, and 5%, respectively.

[15]The control mean is the sample estimate of the share of girls or women married at each age, for ages 14-40, in more highly exposed areas prior to the policy changes (2007). Using the unconditional share married at each age in the pre period leads to a modest increase in the relative effect size, while maintaining the pattern of more pronounced effects at younger ages, as shown in figure A.5.

Figure 3: Women's likelihood of marriage after policy changes in high exposure localities



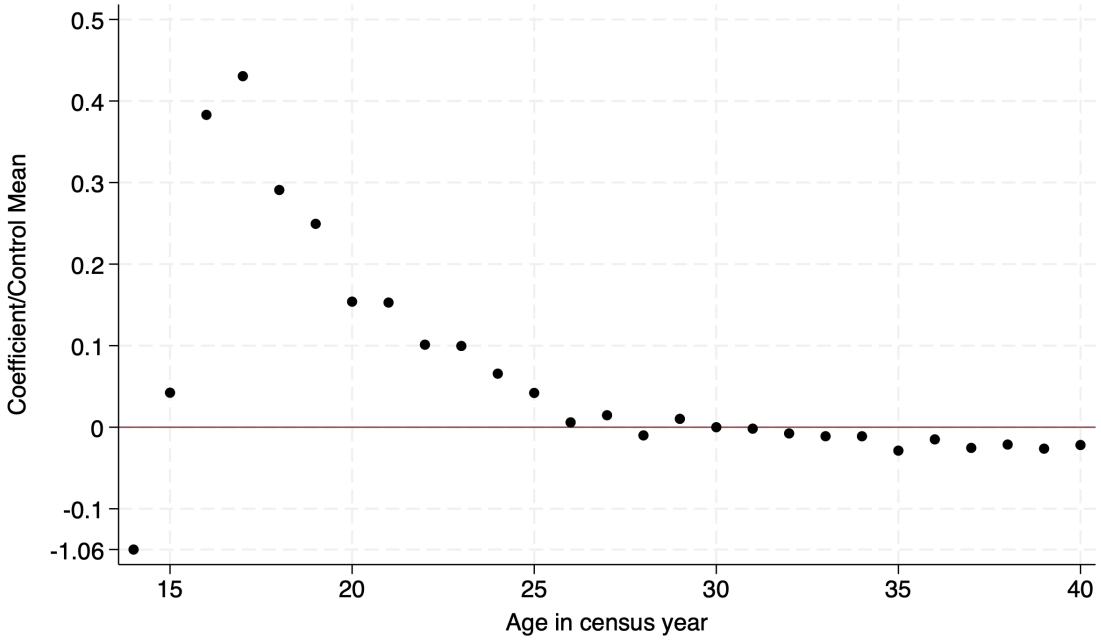
Notes: These estimates are based on the census *population* of women 14-40 in the West Bank for the years 2007 and 2017. The figure plots the coefficient on the interaction between High exp$_l$ x Post$_t$ x 1[Age$_i$ = a], capturing the differential effect of the policy change in more highly exposed areas for ages $a = 14 - 40$ (where $a = 30$ is omitted) on a woman's likelihood of marriage.

5.2.2 Fertility, education, and work

The shock to women's marriage market through declines in the minimum age threshold for married men to be eligible for a work permit led to an increase in girls' and young women's likelihood of marriage in more exposed localities. A broad literature documents the effects of earlier marriage, and in some cases child marriage specifically (defined as marriage before the age of 18), on women's and household's outcomes (Field and Ambrus, 2008; Duflo, Dupas, and Kremer, 2015; Ashraf, Bau, Nunn, and Voena, 2020; Corno, Hildebrandt, and Voena, 2020). It has established a link between timing of marriage on the one hand, and fertility, educational, and labor market outcomes on the other. This motivates the outcomes I consider in this section.

First, I consider the effect of earlier marriage on fertility. Since the women impacted are too young to have completed their fertility, I cannot examine the effect of fertility per se. Instead, I inspect whether earlier marriage results in earlier childbearing and household formation. It is plausible in this setting that while men and women bring forward their timing of marriage to access job opportunities, the timing of childbearing is unaffected. To test this, I estimate equation 4 for Any child$_{itl}$. Figure 5 shows the β_{1a} coefficients for $a = 14 - 40$, again where $a = 30$ is the omitted category. The likelihood of women having at least one child increases among girls and younger women, with effects (though noisy) as young as 17, a year after I first documented increases in the

Figure 4: Relative effect on women's likelihood of marriage in more exposed localities post policy



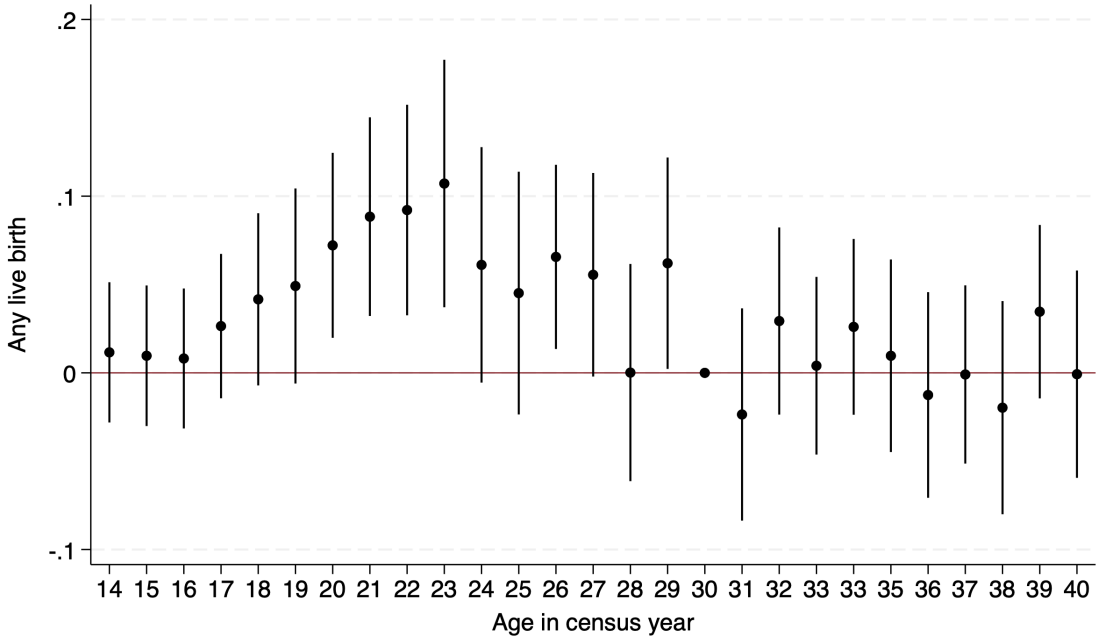
Notes: Y-axis break between -0.1 and -1.06. The relative effect size per age is computed by dividing the coefficient by the share of girls or women married in more highly exposed areas, prior to the policy change (i.e. in the baseline year of 2007). While the coefficient is estimated on the population of women 14-40 in 2007 and 2017 in the West Bank, the means are computed using 20% representative census samples for the same group.

likelihood of marriage. Similar to patterns in the likelihood of marriage, effects are most pronounced among girls and younger women. To contextualize some magnitudes, 17 year-old girls exhibit a 2.6 percentage point increase in the likelihood of having at least one child, relative to a control mean of 3%.[16] Women who are 20 years old show a 7.2 percentage point increase in the likelihood of having at least one child, relative to a control mean of 26%.

Impacts on the likelihood of marriage and childbearing, especially among girls and younger women, coincide with their secondary schooling. Secondary schooling in this context is achieved once 12th grade is completed with a *Tawjihi* certificate—a national certificate examination that directly affects what fields of study and universities one qualifies for. This is typically completed around 17 and 18 years of age. Thus, I estimate equation 4 for the outcome variable Completed secondary$_{iatl}$ for females aged 17-30. I plot the β_1 coefficients in figure 6. We see that girls and women between 17 and 21 years of age are less likely to have completed secondary education. Note that the effects for those 17 and 18 years of are partially mechanical since many are still enrolled at that age. For instance, in 2007, only 8% of 17 year olds completed secondary school. While this drastically improves by 18, increased to 61%, it is still lower than other years because not enough

[16]The control mean is defined as the share of women with any child in high exposure areas in 2007 (prior to the policy change), by age.

Figure 5: Change in women's likelihood of having at least one child in high exposure localities



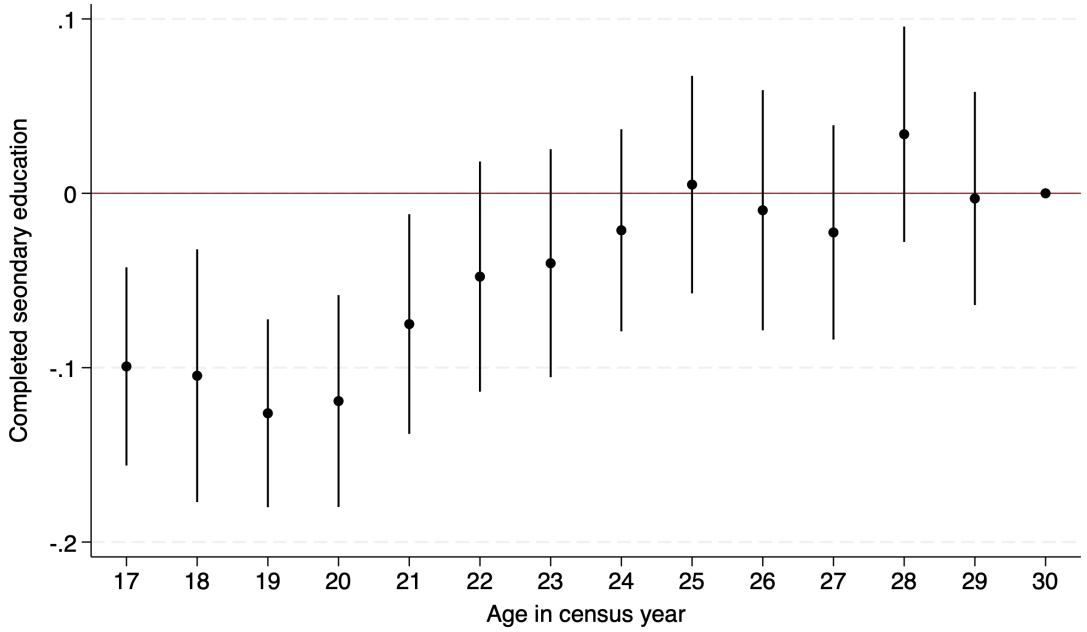
Notes: These estimates are based on the census sample of women 14-40 in the West Bank for the years 2007 and 2017. The figure plots the coefficient on the interaction between High $\exp_l$ x Post_t x 1[Age_i = a], capturing the differential effect of the policy change in more highly exposed areas for ages $a = 14 - 40$ (where $a = 30$ is omitted) on a woman's likelihood of having at least one child.

time has lapsed to observe current students' outcomes. Those 19-21 years of age, however, register clear declines in secondary school completion. Those 19 years old see a 12.6 percentage point, or 19%, decline in completion rates relative to a control mean of 66%.[17] Those those who are 21 years old experience a 7.5 percentage point, or 13%, decline relative to a control mean of 59%. Similar to previous outcomes, girls' and younger women are adversely impacted, with the younger showing greater relative effects.

To further corroborate this finding, I consider the alternative measure of dropping out having completed preparatory schooling. Preparatory school concludes in 9th grade, and is the level of education directly preceding secondary school. As such, to detect dropout decisions in the intermediate years, I condition on having completed preparatory education. I estimate equation 4 for the outcome variable Dropped out_{itl}, and plot the β_{1a} coefficients in figure 7. I find that the same ages that exhibit declines in secondary school completion rates show higher rates of dropping out. This also largely holds when examining unconditional drop-out rates, as shown in figure A.6. This suggests that declines in secondary school completion are consistent with evidence on the decision to actively drop out of school around that time. To track the same example of 19 and 21 year old women, I find that 19 year olds are 8.5 percentage points more likely to drop out of school

[17]The control mean is again defined using the 2007 share of females who have completed secondary school in more exposed localities.

Figure 6: Change in women's likelihood of completing secondary school in high exposure localities



Notes: These estimates are based on the census sample of women 14-40 in the West Bank for the years 2007 and 2017. The figure plots the coefficient on the interaction between High $\exp_l$ x Post_t x 1[Age_i = a], capturing the differential effect of the policy change in more highly exposed areas for ages $a = 17 - 30$ (where $a = 30$ is omitted) on a woman's likelihood of completing secondary school.

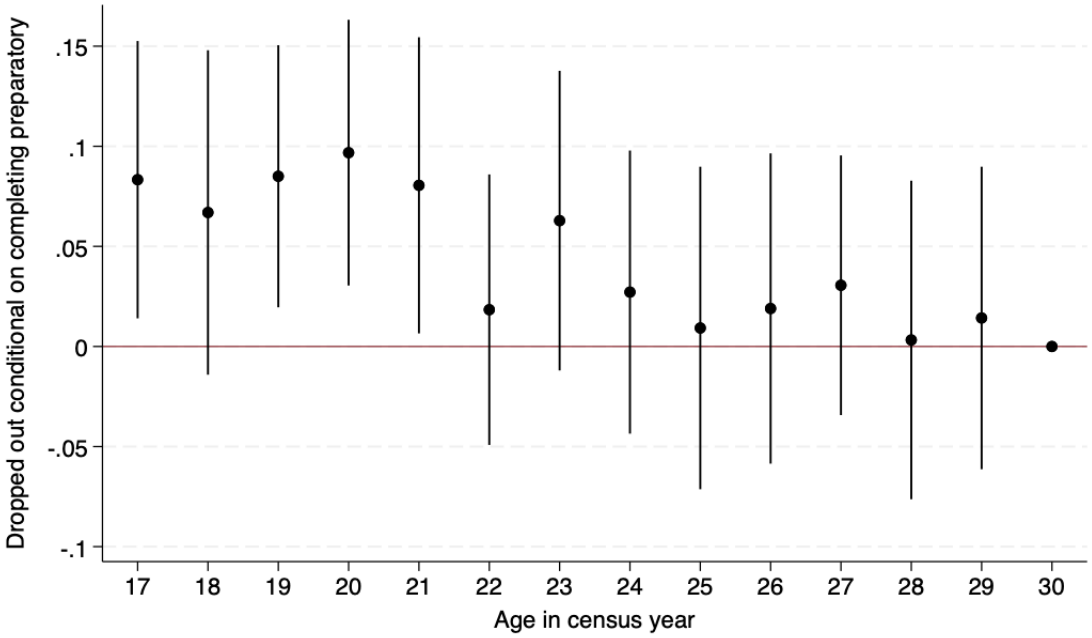
conditional on having completed preparatory school, which resembles a 34% increase relative to a control mean of 25%.[18] As for 21 year olds, then they similarly are 8 percentage points more likely to drop out of school, a 28% increase in the likelihood relative to a control mean of 29%.

Lastly, in terms of education outcomes, I examine whether lower secondary school completion and higher dropout rates are a temporary interruption or a more persistent choice. To do so, I examine enrollment in and completion of higher education for the same sample of women aged 17 through 30. Higher education here refers to the degree right after high school completion, whether an intermediate diploma or a bachelor's degree. Similar to other outcomes, I estimate equation 4 for Higher education_{iatl}, and plot the β_{1a} coefficients in figure 8. I find that for ages that have typically completed secondary school, those over 18, women are less like to be enrolled in or have completed an intermediate diploma or a bachelor's degree. Following the same example of women 19 and 21 years of age, I find that 19 year-olds are 8.3 percentage points less likely to be enrolled in or have completed higher education. This is a 16% decline relative to a control mean of 52%.[19] 21 year-old women are 7.5 percentage points less likely to be enrolled in or have completed higher education, an effect size of -16% relative to a control mean of 47%.

[18]The control mean is again defined using the 2007 share of females who have dropped out of school conditional on having completed preparatory school in more exposed localities.

[19]The control mean is defined using the 2007 share of females who were enrolled in or have completed higher education (a 2-year intermediate diploma or a bachelor's degree in high exposure localities, by age.

Figure 7: Change in women's likelihood of dropping out of school

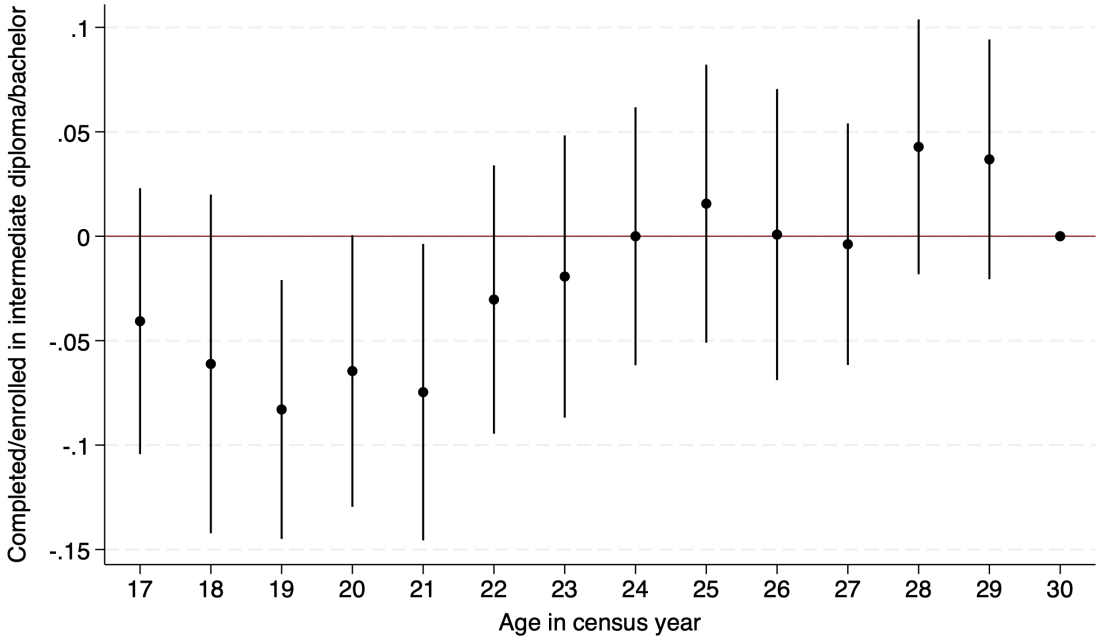


Notes: These estimates are based on the census sample of women 17-30 in the West Bank for the years 2007 and 2017. The figure plots the coefficient on the interaction between High $\exp_l$ x Post_t x 1[Age_i = a], capturing the differential effect of the policy change in more highly exposed areas for ages $a = 17 - 30$ (where $a = 30$ is omitted) on a woman's likelihood of dropping out of school conditional on having completed preparatory school.

Women in more highly exposed localities then exhibit increases in the likelihood of earlier childbearing, along with declines in their educational attainment. Education attainment is impacted with higher dropout rates prior to completing secondary education. These decisions persist and pass-through to enrollment and completion of higher education. One possibility is that these women are substituting employment for education. Thus, I check whether women in more highly exposed areas in the post period, especially ages that experience higher likelihoods of marriage and childbearing, and lower educational attainment, also show higher rates of labor force participation or employment. To do so, I estimate equation 4 on the outcomes LFP_{iatl} and Employed_{ialt}. I do not find any such evidence, as seen in figures A.7 and A.8, where I plot the β_{1a} coefficients for $a = 17 - 30$, with the omitted category $a = 30$.

This is a context in which both female labor force participation and employment rates are low, and even more so among the youth. This suggests that if there were an uptick in female employment or labor force participation, it would be detectable. Thus, it appears that while women get married and start households earlier, as well as drop-out of school, they do not turn towards employment, likely fully dedicating their time to their households. While these results cannot speak to the causal pathway between the different socioeconomic effects documented in this section, they outline how different aspects of women's livelihood might be affected by changes in work permit eligibility,

Figure 8: Change in women's likelihood of enrolling in or completing higher education



Notes: These estimates are based on the census sample of women 17-30 in the West Bank for the years 2007 and 2017. The figure plots the coefficient on the interaction between High $\exp_l$ x Post_t x 1[Age_i = a], capturing the differential effect of the policy change in more highly exposed areas for ages $a = 17 - 30$ (where $a = 30$ is omitted) on a woman's likelihood of being enrolled in or having completed higher education (intermediate 2-year diploma or a bachelor's degree).

indirectly and to the extent that it affects men in their communities.

6 Robustness checks

6.1 Parallel trends

The difference-in-differences strategy developed in section 4.1.1 relies on the assumption that the evolution of the likelihood of marriage among younger individuals is the same as that for older individuals over time, in the absence of the policy change. However, this need not be the case, especially in a context as this where the share of individuals married is high and stable by 30 over time, as seen in figure A.4. This convergence suggests that changes in marriage behavior among younger individuals won't necessarily show up in the marriage behavior of those older than 30, but might manifest in distributional shifts among younger individuals. This is evident in the pre-trends in appendix figure A.9, which reveals differential pre-period changes between young and old individuals between 1997 and 2007. This figure estimates equation 1 by age, for ages 22 through 40, omitting age 36. As such, while the results from the difference-in-differences specification are suggestive of changes in men's marriage behavior pursuant of changes in work permit eligibility, they cannot be interpreted as causal.

By adding a third difference, exploiting differential exposure to the policy change across localities, the parallel trends assumption I invoke is instead that in the absence of changes in permit eligibility criteria, the difference in the evolution of the likelihood of marriage between younger and older men is the same in more and less exposed localities. I conduct a placebo test, based on equation 3, only using 1997 as the pre-period and 2007 as the post-period, with 36 being the omitted category. Appendix figure A.10 displays the β_{1a} coefficients for $a = 22 - 40$, providing evidence in support of the triple-differences parallel trends assumption.

Conceptually, however, a concern is that the triple-differences design fails to address concerns stemming from the differential trends in the likelihood of marriage for younger versus older individuals. To illustrate, in the current triple-differences specification in equation 3, the β_1 coefficient on the triple interaction term for a 23-year-old says that the difference in marriage rates between high and low exposure localities increased by 5.6 percentage points from 2007 to 2017, relative to how this same difference changed for 36-year-olds. If most people are married by 36, then 36-year-olds likely have little room for policy-induced changes or other trends in marriage rates - there is a ceiling effect. The triple difference relies on the assumption that non-policy-related trends affect all age groups similarly. Because 36-year-olds have hit a ceiling, they can not fully capture non-policy-related time-variant trends in the likelihood of marriage. This suggests that the coefficient for a 23-year-old, and generally younger individuals, picks up not only the effect of the policy, but also any non-policy-related trends that the 36-year-old control group could not difference out. Depending on that trend, the current results can be biased upwards or downwards.

6.2 Alternative specification for marriage market effects

To address threats to identification when using ever married as an outcome, I propose an alternative measure of marriage timing for a narrower set of ages. Specifically, I focus on the last policy change, which led to a reduction in the minimum age threshold from 24 to 22 in 2015, and estimate the effect of men's likelihood of marriage by 22. Referring back to figure 1, this corresponds to men ages 22-30. First, I run a standard difference-in-differences with age heterogeneity to capture changes in the likelihood of marriage by 22 before and after the policy. I run the following specification:

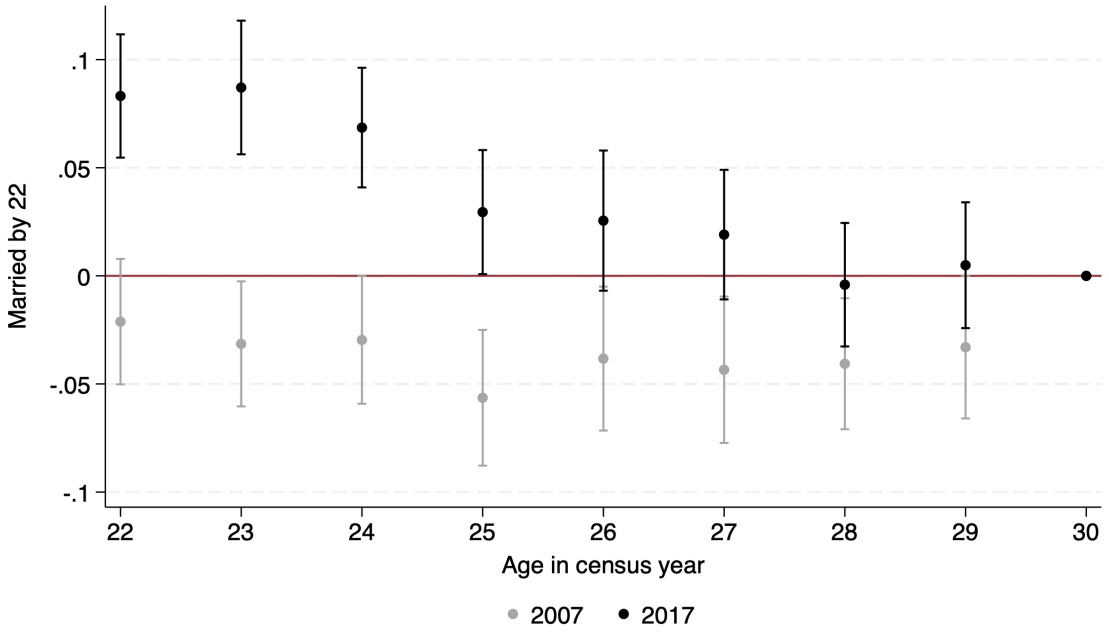
$$\text{Married by } 22_{iatl} = \beta_0 + \sum_{a=22}^{30} \beta_{1a}(\text{Year2007}_t \times 1[\text{Age}_i = a]) + \sum_{a=22}^{30} \beta_{2a}(\text{Year2017}_t \times 1[\text{Age}_i = a]) + \gamma_t + \delta_a + \nu_l + \varepsilon_{iatl}, \tag{5}$$

where the coefficients of interest are β_{1a} and β_{2a}, and the omitted category is $a = 30$. For a given age, the β_1 coefficient captures the change in the likelihood of marriage between 1997 and 2007, relative to the same difference for 30-year-olds. Note that this is prior to the policy change, and these coefficients serve as a placebo test for differential trends in the likelihood of marriage by 22 prior to the policy change. The β_{2a} coefficients capture the effects of the policy. For a given age between 22 and 29, the β_2 coefficient captures the difference in likelihood of marriage between 1997

and 2017. In the absence of pre-trends, this captures the effect of the policy on the likelihood of marriage by 22. While I cannot get at pre-trends due to data limitations restricting me to two pre-periods, the placebo test conveyed in β_{1a} coefficients provide some suggestive evidence.

I plot β_{1a} and β_{2a} coefficients in figure 9. There do not appear to be unexpected or large differential trends prior to the policy change. There is evidence of some declines in the likelihood of marriage by 22 in 2007 relative to 1997, which is consistent with global trends in delays in marriage. The differences in 2017 relative to 1997, however, show a reversion relative to that trend. This is especially the case for those ages 22 through 24, who became eligible when they turned 22 under the third policy change. Note that there are still some increases for the 25-27 age groups in 2017. As such, I perform a joint F-test of the null hypothesis that the effects are equal for younger (22-24) and older (25-27) men. I find evidence of age-dependent treatment effects, with the test rejecting the null hypothesis ($F(5, 456) = 10.78$, $p < 0.001$) that treatment effects are equal across young and older groups. Figure A.11 performs the same exercise only testing for the probability that someone is married *at* 22. This introduces more noise, but provides evidence consistent with these findings and decline the minimum age threshold to 22 while requiring marriage.

Figure 9: Men's likelihood of marriage by 22 before and after policy changes relative to 1997



Notes: These estimates are based on the census sample of men 22-30 in the West Bank for the years 1997, 2007, and 2017. The figure plots the coefficient on the interactions between Year2007$_t$ x 1[Age$_i$ = a] and Year2017$_t$ x 1[Age$_i$ = a] for ages $a = 22 - 30$, with $a = 30$ being the omitted category. The former captures differential changes in the likelihood of marriage by 22 by age between 1997 and 2007, relative to the same difference for 30-year-olds. The latter is defined similarly, only for effects between 1997 and 2017, capturing the effects of the policy in the absence of pre-trends.

Next, to test the extent to which I can attribute the effects to the policy, I add regional variation

as a third difference, as follows:

$$\begin{aligned}
&\text{Married by } 22_{iatl} = \beta_0 + \\
&\sum_{a=22}^{30} \beta_{1a}(\text{High exp}_l \times 1[\text{Yr}_t = 2007] \times 1[\text{Age}_i = a]) \; + \; \sum_{a=22}^{30} \beta_{2a}(\text{High exp}_l \times 1[\text{Yr}_t = 2017] \times 1[\text{Age}_i = a]) + \\
&\sum_{a=22}^{30} \beta_{3a}(1[\text{Yr}_t = 2007] \times 1[\text{Age}_i = a]) + \sum_{a=22}^{30} \beta_{4a}(\text{High exp}_l \times 1[\text{Age}_i = a]) \; + \beta_5(\text{High exp}_l \times 1[\text{Yr}_t = 2007]) + \\
&\sum_{a=22}^{30} \beta_{6a}(1[\text{Yr}_t = 2017] \times 1[\text{Age}_i = a]) + \sum_{a=22}^{30} \beta_{7a}(\text{High exp}_l \times 1[\text{Age}_i = a]) \; + \beta_8(\text{High exp}_l \times 1[\text{Yr}_t = 2017]) + \\
&\gamma_t + \delta_a + \nu_l + \varepsilon_{iatl}, \qquad (6)
\end{aligned}$$

which adapts equation 3 by using 1997 as a pre-period, embedding a placebo test. The coefficients of interest are β_{1a} and β_{2a} for $a = 22 - 30$, with 30 being the omitted category. For a given age, β_1 captures the difference in the likelihood of marriage by 22 between high and low exposure localities from 1997 to 2007, relative to how the same difference changed for 30-year-olds. This serves as a placebo test checking for differential trends in marriage rates by 22 across different groups prior to the policy change. The β_2 coefficient for a given age between 22 and 29, similarly captures the difference in the likelihood of marriage by 22 between high and low exposure localities from 1997 to 2017, relative to how the same difference changed for 30-year-olds. I include age, year, and locality fixed effects, and cluster standard errors at the locality level.

Here, I define High exp_l as 1 for localities with an above-median share of workers in Israel in 1997, changing the baseline exposure from 2007 to 1997. While both measures are highly correlated, with a correlation coefficient of 0.69, they do not perfectly overlap. Relying on 1997 locality shares of labor in Israel gives a more exogenous, pre-determined measure of exposure to the policy, minimizing any endogenous sorting in anticipation of the policy changes between 2007 and 2017. For robustness, I report results using 2007 exposures in the appendix.

Figure A.12 plots the β_{1a} and β_{2a} for $a = 22 - 29$ relative to 30-year-olds. While the point estimates are noisier, they paint a similar picture, with increases in the likelihood of marriage by 22 in 2017 for those 22-24 year-olds. This similarly holds when considering the likelihood of being married *at* 22, as seen in figure A.13.

7 Conclusion

This paper shows that conditioning labor access on demographic characteristics can have unintended consequences on those not initially targeted by the policy. While the work permit policy *de facto* targeted men, by tying eligibility to marital status, the policy introduced marriage market linkages that ultimately affect women. The policy changes dropping the age threshold while continuing to require marriage placed downward pressure on the timing of marriage for men. Marriage market equilibrium effects were such that women were also incentivized to marry earlier, leading to a rise in marriage among younger women and adolescent girls. Marriage decisions then spill over into

fertility, education, and labor market domains for women, in a cascade of unintended effects through a shuffling of the marriage market.

These results demonstrate one such example of the distortionary effects temporary migration might have at the origin, through the channel of eligibility requirements. While temporary migration and guest worker programs are economically enticing, helping migrants secure higher income relative to local options, by failing to account for the consequences of these migration spells on the communities of origin, we underestimate the cost of such policies. In this context, women's development outcomes showed signs of regression, which should be taken into account when assessing this policy. This is true in any context in which temporary migration impacts the communities of origin, positively or otherwise, and especially critical in fraught areas, where negative unintended consequences might further exacerbate the situation. While the context speaks to temporary migration settings in which married individuals are favored, these findings fundamentally speak to settings in which labor market access is conditional on marriage, in a way that interferes with the marriage decisions of younger individuals.

Currently, the findings do not address the welfare effects of the policy that balance income grains from these job opportunities, and both direct and indirect marriage market equilibrium effects documented in the paper. Moving forward, I will collect survey data, and supplement it with existing administrative data, to think more broadly about the welfare implications of this policy in this context, and understand the conditions under which the policy is net positive, negative, and neutral.[20] This study captures short-term effects of conditional labor market access on marriage, with relevance to temporary migration settings, especially as flows continue to rise and interact with migration policy discourse OECD (2024). There is still scope, however, for studying the longer-run effects of these short-term shocks and decisions on development outcomes for families and communities that rely on these opportunities (i.e. low-skill job abroad), whether in this context or adjacent ones of temporary migration.

[20]Data collection efforts are currently pending grant funding decisions.

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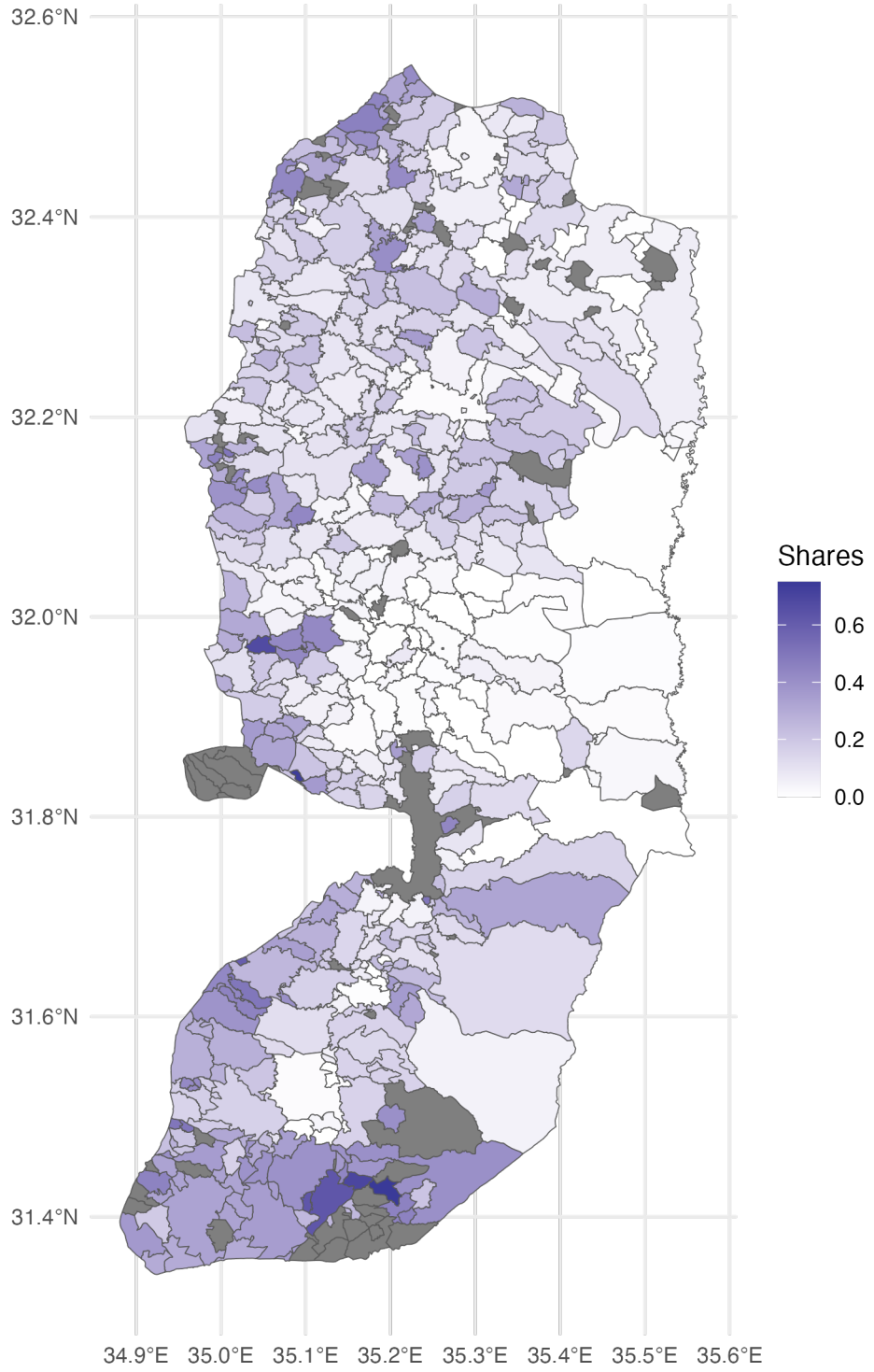
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Figures

Figure 10: Share of workers in Israel per locality in 2007



Appendix A Additional Figures and Tables

Figure A.1: Total and construction work permit quotas over time

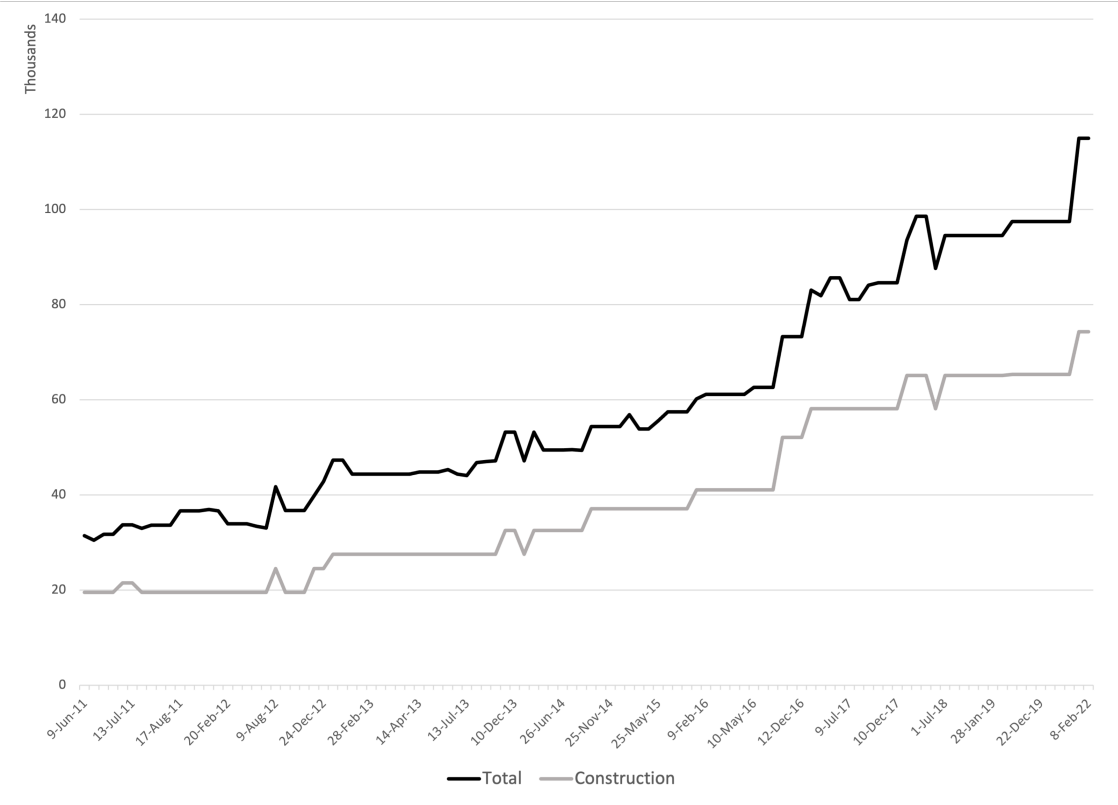


Figure A.2: Work permit quota and eligibility criteria in May 2011 guidance

סוג מכסה	ענף	מכסה מאושרת לענף	תת-ענף	מכסה מאושרת לתת-ענף	קריטריון	מכסה מאושרת לקריטריון	הערות	תוקף מכסה
קבועים	בניין	19,500	-	-	גיל 35 נ + ילד	14,500	-	מכסות קבועות
					גיל 28 נ + ילד	1,000	-	
					גיל 26 ומעלה	4,000	טרם מומש	
	תעשייה ושירותים	2,250	-	-	גיל 35 נ + ילד	2,250	מתוכם כניסת 500 פועלים מא.ת. איו"ש לישראל בנוהל "הרכבות" - עפ"י קריטריון הגיל התקף בא.ת. איו"ש, ללא לינה ובליווי המעסיק.	
	חקלאות לא עונתית	1,750	-	-	גיל 35 נ + ילד	1,750	-	
	חקלאות עונתית "נודדת"	3,000	פרדסים ומטעים	3,000	גיל 35 נ + ילד	3,000	-	
	בריאות	1,650	בריאות ישראל	150	גיל 21 ומעלה	-	-	
קבועים באזורים מיוחדים			בריאות מזרח י-ם	1500	גיל 21 ומעלה	-	-	מכסות קבועות
	מלונאות - מזרח י-ם	300	-	-	גיל 25 ומעלה	300	-	
	א.ת עטרות	2,000	-	-	גיל 21 ומעלה	1500	-	
					גיל 35 נ + ילד	250		
					גיל 30 נ+ ילד	250		
עונתיים	חקלאות עונתית	1,000	תות שדה	1,000	גיל 28 נ + ילד	1,000	-	מכסה מאושרת עד 15/06/2011
סה"כ הרשאה ל-31,450 פועלים לתעסוקה בישראל מאיו"ש								

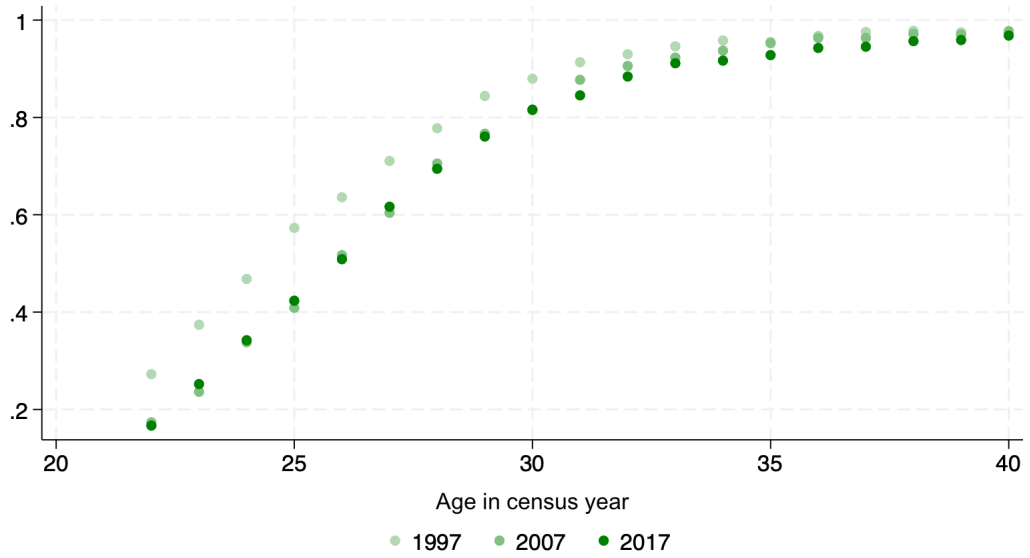
Notes: This table is from COGAT-issued guidance dated May 19, 2011, retrieved from the archive maintained by Gisha. It describes the distribution of work quotas across different sectors, along with demographic criteria per sector. The second column from the right describes the different sectors (construction, industry and services, non-seasonal agriculture, "moving" seasonal agriculture, health, Hotels in East Jerusalem, Atarot industrial zone, and seasonal agriculture), the third the allotted quota per sector, and the sixth the associated demographic criteria.

Figure A.3: Work permit quota and eligibility criteria in September 2017 guidance

Type of quota	Sector	Quota per sector	Sub-sector	Quota per subsector	Criterion	Quota per criterion	Validity of quota
Permanent	Construction	30,000	-	-	Age 22 married	30,000	Permanent quota (Of which 16,000 overnight permits under criterion of 22 married +)
	Residential construction	28,100	-	-	Age 22 married	28,100	
	Industry and services	4,450	-	-	Age 22 married	4,450	
	Non-seasonal agriculture	6,250	-	-	Age 22 married	6,250	
	Health in Israel	270	-	-	Age 21 and over	270	
Permanent in special areas	Health in East Jerusalem (medical staff in East Jerusalem hospitals)	1,980	-	-	Age 21 and over	1,980	
	Hotels in East Jerusalem	300	-	-	Age 21 and over	300	
	Industrial Park Atarot	2,500	-	-	Age 21 and over	1,700	Permanent quotas
					Age 22 married	800	
Seasonal	Seasonal agriculture	10,750	Orange groves and orchards	4,250	Age 22 married	4,250	All year round
			Olive harvest	3,000	Age 21 married (family procedure)	3,000	27/08/17-15/01/18
			Almonds	2,000	Age 22 married	2,000	2017 season
			Seasonal fruit and vegetables	1,500	Age 22 married	1,500	Varying crops.

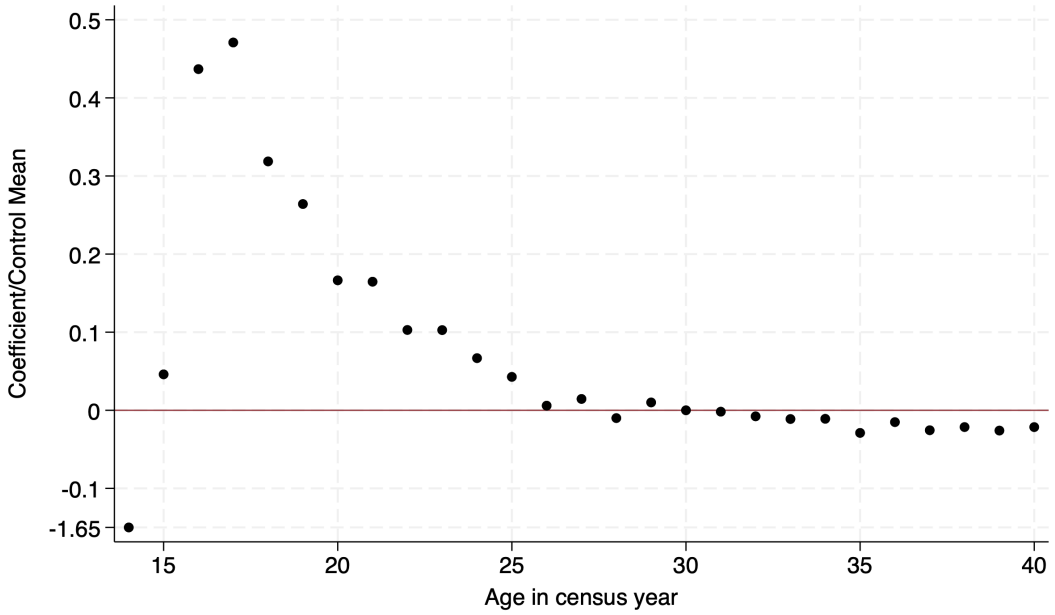
Notes: This table is from COGAT-issued guidance dated September 18, 2017. It describes the distribution of work quotas across different sectors, along with demographic criteria per sector. The table has been unofficially translated by Gisha. For further inquiries, reach out to info@gisha.org.

Figure A.4: Share of men ever married by age



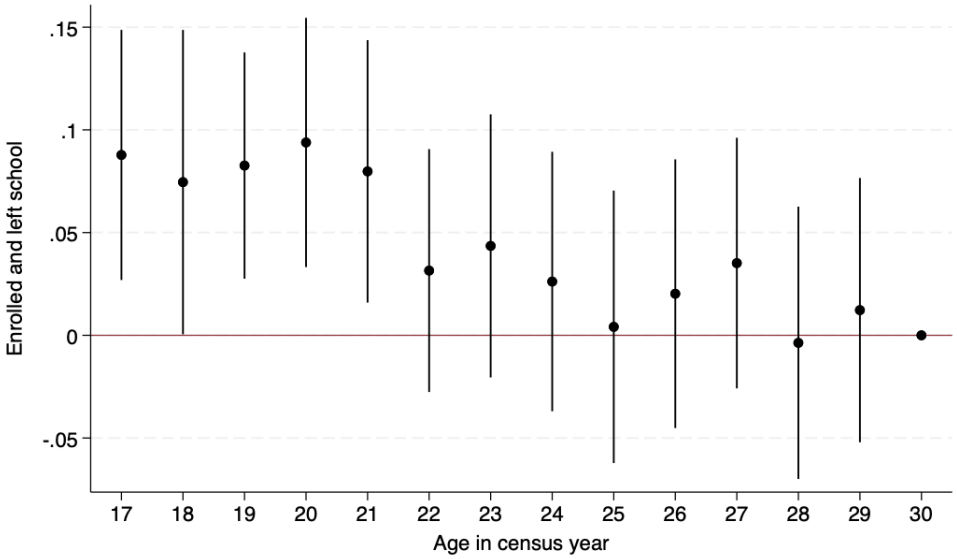
Notes: The values are estimated using the 1997, 2007, and 1997 individual-level Palestinian census samples (20% representative). For each age group within a given year, it reflects the share of men ever married in the West Bank.

Figure A.5: Relative effect on women's likelihood of marriage in more exposed localities post policy



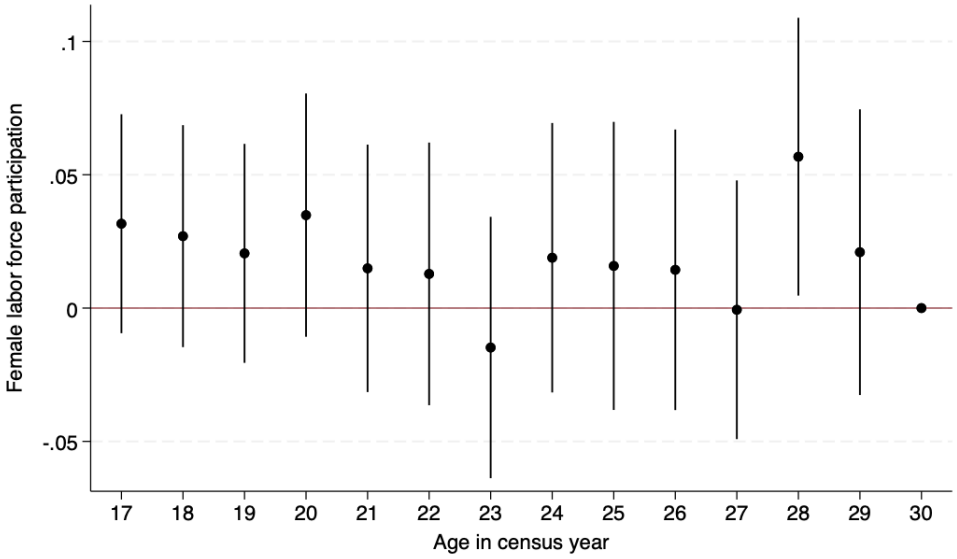
Notes: The relative effect size per age is computed by dividing the coefficient by the share of girls or women married in more highly exposed areas, prior to the policy change (i.e. in the baseline year of 2007). Both the coefficients and the unconditional pre-period mean are estimated on the population of women 14-40 in 2007 and 2017 in the West Bank.

Figure A.6: Change in women's likelihood of dropping out of school



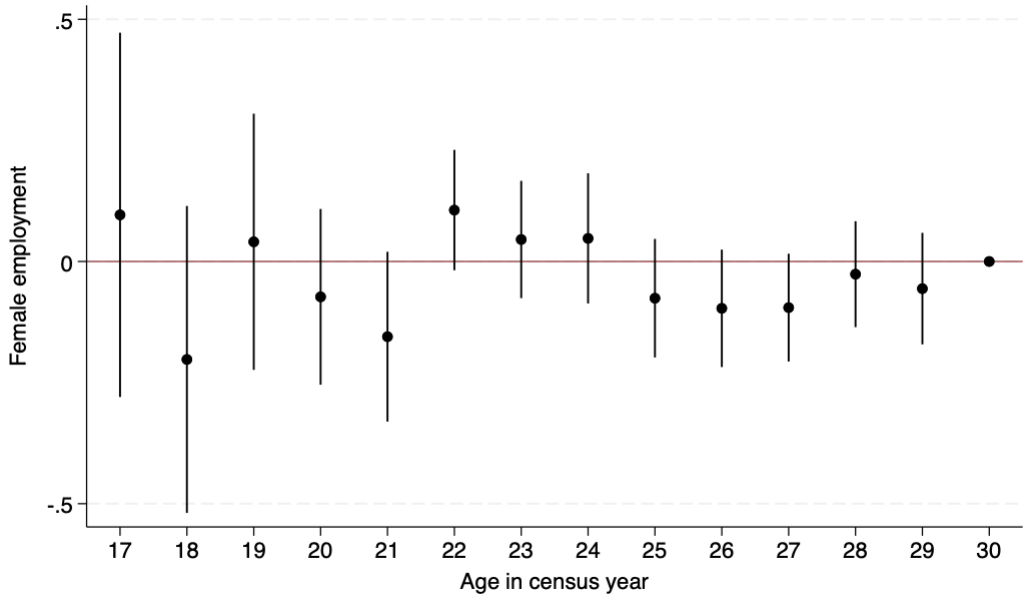
Notes: These estimates are based on the census sample of women 17-30 in the West Bank for the years 2007 and 2017. The figure plots the coefficient on the interaction between High $\exp_l$ x Post_t x 1[Age_i = a], capturing the differential effect of the policy change in more highly exposed areas for ages $a = 14 - 40$ (where $a = 30$ is omitted) on a woman's likelihood of dropping out of school.

Figure A.7: Change in women's likelihood of joining the labor force



Notes: These estimates are based on the census sample of women 17-30 in the West Bank for the years 2007 and 2017. The figure plots the coefficient on the interaction between High $\exp_l$ x Post_t x 1[Age_i = a], capturing the differential effect of the policy change in more highly exposed areas for ages $a = 17 - 30$ (where $a = 30$ is omitted) on a woman's likelihood of being in the labor force.

Figure A.8: Change in women's likelihood of being employed



Notes: These estimates are based on the census sample of women 17-30 in the West Bank for the years 2007 and 2017. The figure plots the coefficient on the interaction between High $\exp_l$ x Post_t x 1[Age_i = a], capturing the differential effect of the policy change in more highly exposed areas for ages $a = 17 - 30$ (where $a = 30$ is omitted) on a woman's likelihood of being employed.

Figure A.9: Placebo test for difference-in-differences design

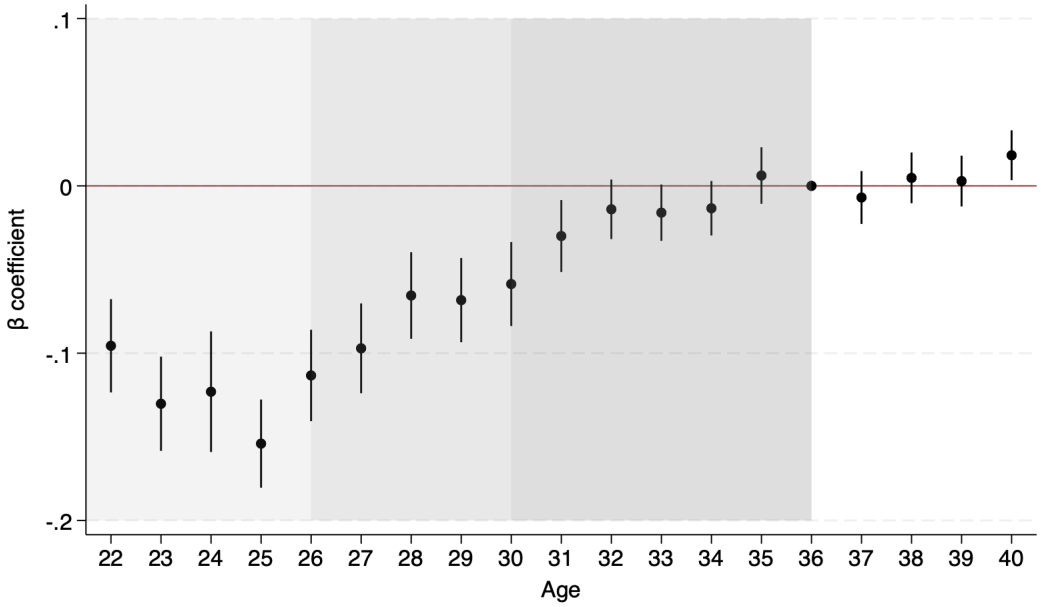


Figure A.10: Placebo test for triple-differences design

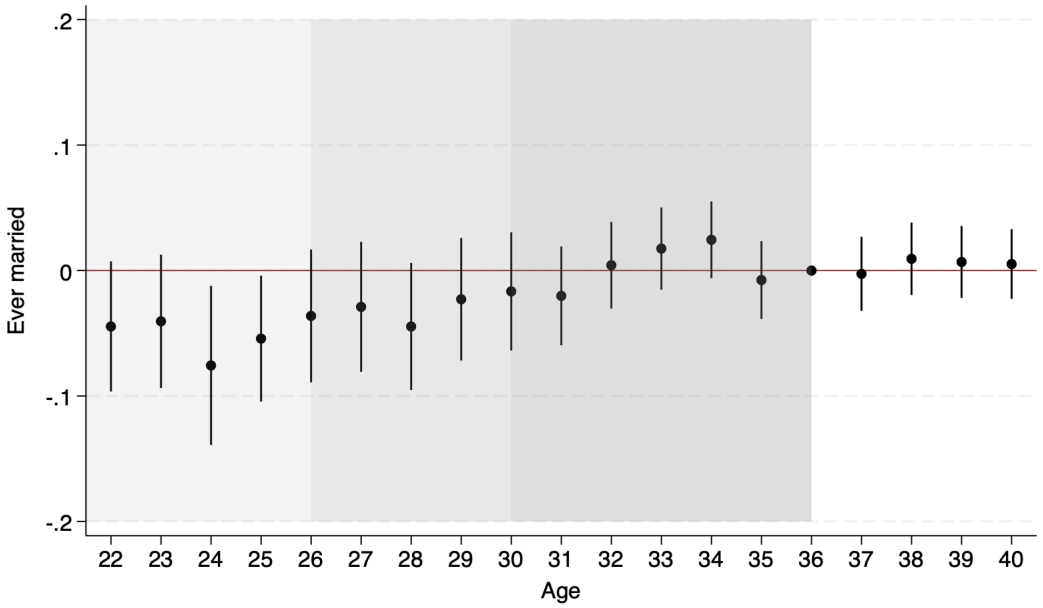
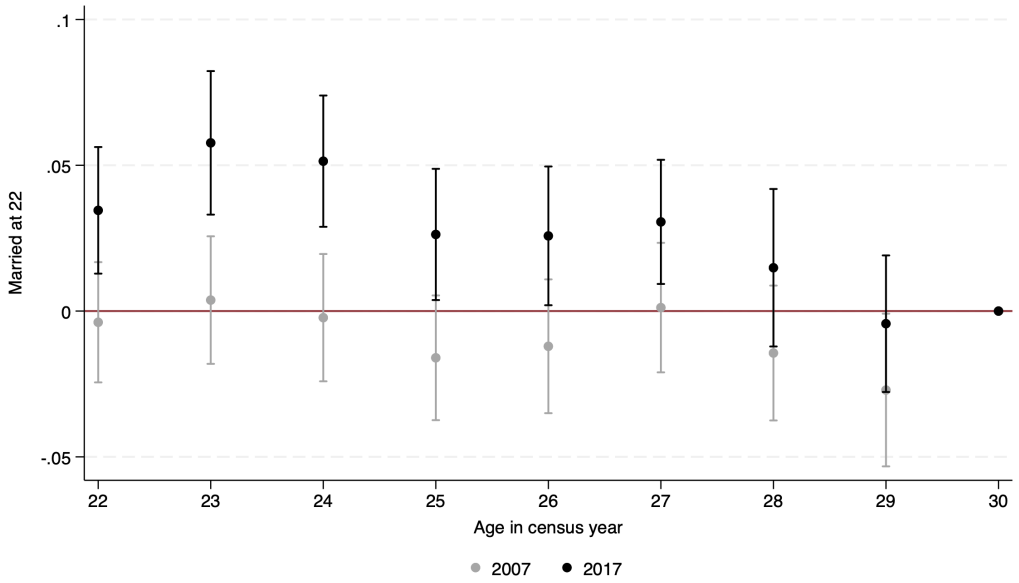
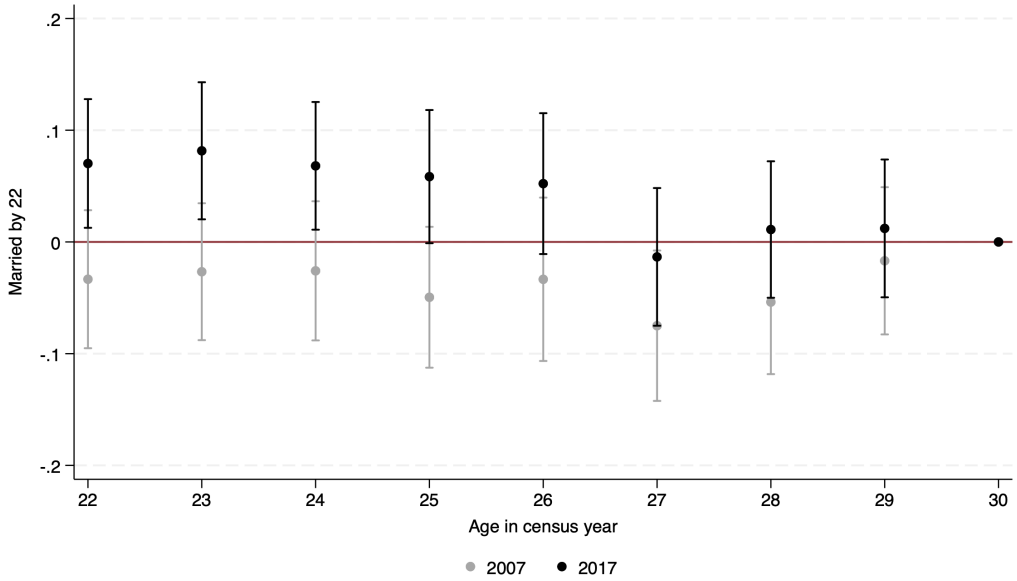


Figure A.11: Men's likelihood of marriage by 22 before and after policy changes relative to 1997



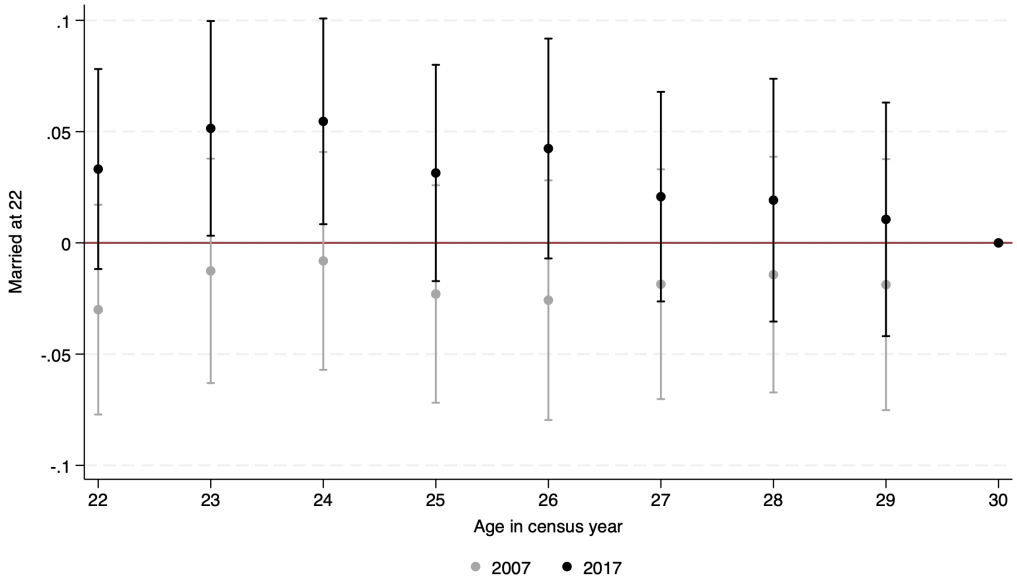
Notes: These estimates are based on the census sample of men 22-30 in the West Bank for the years 1997, 2007, and 2017. The figure plots the coefficient on the interactions between Year2007$_t$ x 1[Age$_i$ = a] and Year2017$_t$ x 1[Age$_i$ = a] for ages $a = 22 - 30$, with $a = 30$ being the omitted category. The former captures differential changes in the likelihood of marriage by 22 by age between 1997 and 2007, relative to the same difference for 30-year-olds. The latter is defined similarly, only for effects between 1997 and 2017, capturing the effects of the policy in the absence of pre-trends. The dependent variable is a binary indicating whether a man is first gets married at 22, conditional on not having been married.

Figure A.12: Men's likelihood of marriage by 22 before and after policy changes in high exposure localities relative to 1997



Notes: These estimates are based on the census sample of men 22-30 in the West Bank for the years 1997, 2007, and 2017. The figure plots the coefficient on the interactions between High $\exp_l$ x Year2007$_t$ x 1[Age$_i$ = a] and High $\exp_l$ x Year2017$_t$ x 1[Age$_i$ = a] for ages $a = 22 - 30$, with $a = 30$ being the omitted category. The former captures differential changes in the likelihood of marriage by 22 by age between 1997 and 2007 across high and low exposure localities, relative to the same difference for 30-year-olds. The latter is defined similarly, only for effects between 1997 and 2017, capturing the effects of the policy as observed in more highly exposed localities in the absence of pre-trends. The dependent variable is a binary indicating whether a man is first gets married by 22.

Figure A.13: Men's likelihood of marriage at 22 before and after policy changes in high exposure localities relative to 1997



Notes: These estimates are based on the census sample of men 22-30 in the West Bank for the years 1997, 2007, and 2017. The figure plots the coefficient on the interactions between High $\exp_l$ x Year2007$_t$ x 1[Age$_i$ = a] and High $\exp_l$ x Year2017$_t$ x 1[Age$_i$ = a] for ages $a = 22 - 30$, with $a = 30$ being the omitted category. The former captures differential changes in the likelihood of marriage by 22 by age between 1997 and 2007 across high and low exposure localities, relative to the same difference for 30-year-olds. The latter is defined similarly, only for effects between 1997 and 2017, capturing the effects of the policy as observed in more highly exposed localities in the absence of pre-trends. The dependent variable is a binary indicating whether a man is first gets married at 22, conditional on not having been married.