War-driven permanent emigration, sex ratios, and female labor force participation*

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August 8, 2023

Abstract

We investigate the drivers of female labor force participation in the presence of unbalanced sex ratios due to a scarcity of males. To do so, we exploit exogenous variation in sex ratios across cohorts and regions, using instruments based on massive emigration in the 1960s that was fueled by the Portuguese Colonial War. As the sex ratio declined, female labor force participation increased, while the marriage rate was unaffected. Female representation among top occupations increased, and the gender pay gap declined, consistent with the predominance of a demand shock favoring female labor.

KEYWORDS: Labor force participation, pay gap, sex ratio.

JEL CODES: J21, J23, J22, N34.

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*We are grateful to Steve Bond, Pierre Brochu, David Card, Jason Garred, Melanie Khamis, Adam Lavecchia, Chiara Monfardini, Heather Sarsons, Aloysius Siow, and anonymous referees for their comments. We thank the National Statistical Offices of France and Portugal, who supplied Census micro data to IPUMS, and the Portuguese Ministry of Employment, for data access. Cardoso acknowledges financial support from the Portuguese Science Foundation, under contracts 2020.03281.CEECIND/CP1615/CT0002 and 2022-01269-PTDC. Morin acknowledges financial support from the Social Sciences and Humanities Research Council of Canada (435-2020-0081).

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1 Introduction

The second half of the twentieth century saw a steady rise in female labor force participation (FLFP) throughout the world. A consensus has emerged in the past decades over a set of factors that have contributed to this advancement: Rising investment in female human capital (Goldin, 1995, 2006), household technological progress (Greenwood et al., 2005), improved access to family planning and child care infrastructures (Goldin and Katz, 2002; Bailey, 2006; Attanasio et al., 2008), and changes to social norms (Fernández et al., 2004; Fernández, 2013) have all been recognized as important FLFP driving factors.[1] As such, one might have predicted that Portugal would have remained at the bottom of OECD countries' FLFP ranking over the 1960s and 1970s given that, over these 20 years, Portugal's citizens remained relatively poor (generally without access to household labor-saving technologies), ill-educated, and with social norms and institutions not particularly prone to woman empowerment.

In 1960, Portugal's FLFP rate stood at approximately 17%, lagging well behind most OECD economies (World Bank, 2018). Nevertheless, Figure 1 shows that Portugal experienced a substantial increase in FLFP over the 1960s and 1970s, such that by 1980, it had reached 46%, a level second only to the Nordic countries, the US, and Canada. When compared to its Southern European neighbors (Greece, Italy, and Spain), Portugal's dramatic FLFP rise makes it a clear outlier (see Figures 1 and 2).

This paper proposes and tests an explanation for Portugal's rapid rise in FLFP: the severe scarcity of male labor incentivized employers to hire women, despite the prevailing social norms.

The rise in Portugal's FLFP coincided with an important reduction in the supply of working-age men driven by two main factors: war and mass emigration. Portugal's Colonial War in its African colonies induced a major drain on the Portuguese civilian

[1] See, e.g., Olivetti and Petrongolo (2016) for an exhaustive review of the literature.

labor force between 1961 and 1974. One percent of the country's population was called up to fight, which is, per capita, fivefold greater than the US Vietnam War draft (Pinto, 2011). A distinctive characteristic of Portugal's Colonial War was that it generated a substantive and permanent withdrawal of males from the civilian labor force. Between 1960 and 1973, over 1.5 million (i.e., more than one-sixth of the country's population) emigrated, mostly to France (Barreto, 2011). The attraction of France for Portuguese emigrants over that period was mainly twofold: France could be reached easily to dodge the military draft without being caught, and its economy was prospering. Most emigrants were men, and many of them were already married, initially leaving their wives behind.

The war fatality numbers presented in Figure 3 and male emigration rates measured immediately after the war—as shown by the 1975 Census line of Figure 4—exhibit a common pattern: both have an inverted-U shape for the cohorts born from the mid-1930s to the mid-1950s, with a peak for men born in the mid-1940s. Such overlap suggests that the Colonial War triggered a sizable economy-wide sex-ratio shock.

We first investigate the extent to which this sex-ratio shock had a causal impact on Portuguese females' labor force participation. Then, we investigate potential mechanisms through which the impact of this large and long-lasting shock operated by focusing on females' marriage, occupational, and wage outcomes.

We measure our outcomes of interest and the region-birth-cohort sex ratios in the early 1980s, i.e., after Portugal's substantial rise in FLFP. However, the analysis' timeline begins significantly earlier. We exploit credible exogenous sex-ratio variation (due to the massive emigration in the 1960s and early 1970s fuelled by the 1961-1974 Colonial War) and historical regional emigration patterns out of Portugal in the 1950s to deal with potential endogeneity issues. The cohorts of interest are those born from 1937 to 1954, i.e., the first and last to suffer war casualties in Africa, respectively. We infer the magnitude of the nationwide emigration shock to these cohorts by combining Portuguese historical birth records with the 1975 French Census micro-data, which identify Portuguese immigrants by birth cohort and gender. As France was by far the most popular destination for Portuguese emigrants, we can quantify the share of each cohort and gender who emigrated from Portugal. We then rely on the historical regional emigration patterns to construct a set of instruments that vary at the region-birth-cohort level. Together with region and cohort fixed effects, our instrumental variables allow us to evaluate the medium-run impact of the sex ratio on females' LFP, marriage rate, occupational allocation, and the gender wage gap.

Our paper contributes to different strands of the FLFP literature. First, it presents evidence that imbalanced sex ratios due to male scarcity can produce long-lasting improvements to the FLFP, even in the absence of important FLFP drivers like the ones cited above (e.g., female-friendly social norms). Second, we contribute to the literature studying the impact of imbalanced sex ratios on FLFP and marriage outcomes. This literature has focused on the impact of the military draft and casualties during the World Wars for the US and France (Acemoglu et al., 2004; Goldin and Olivetti, 2013; Rose, 2018; Boehnke and Gay, 2022; Brodeur and Kattan, 2022; Gay, 2023), migration flows (Angrist, 2002; Lafortune, 2013; Raphael, 2013), and historical geographic settlements or slave-trade patterns (Grosjean and Khattar, 2019; Teso, 2019). The vast majority of these studies emphasize the role of the marriage market in explaining the impact of imbalanced sex ratios on the FLFP. They argue that a relative scarcity of males reduces female marriage rates, which in turn motivates women to increase their labor supply. Our setting differs markedly. First, our emigration shock was permanent, unlike military-draft shocks. The permanent nature of our shock could then lead to different conclusions regarding its effect on FLFP compared to studies using temporary shocks (Acemoglu et al., 2004). More importantly we argue (and show) that, in our case, the male scarcity did not affect the marriage market since the majority of migrating men were already married when they left Portugal. Having this potentially important channel shut down allows us to empirically identify another channel through which an imbalanced sex ratio can affect the FLFP: employers' increased female labor demand.

We find that lower sex ratios led to higher FLFP, higher female representation among top occupations, and a smaller wage gap. In contrast to the previous literature, the sex ratio did not affect females' marriage rate. Neither did it affect females' unemployment rate. Our findings are robust to alternative specifications attempting to further control for region-cohort-specific economic shocks. Furthermore, remittances sent to nonmigrant spouses, although possibly correlated with our instruments, are not likely to affect the interpretation of our results. If anything, remittances are likely to decrease nonmigrant spouses' labor supply (see Section 5.5 for more details). As such, the magnitude of our sex-ratio parameter estimates may, in fact, be downward biased in the case of the FLFP. Although we do not rule out a role for female labor supply in explaining part of the negative relationship between sex ratios and FLFP, our results suggest that female labor demand was the dominant force in bringing women into the labor market, as hypothesized by Acemoglu et al. (2004) in their analysis of the US WWII military draft. They argue that a longer-lasting negative shock to the civilian male labor force could have led to a labor demand reaction favoring females, thereby putting upward pressure on female wages. In our case, if women's labor supply had been the dominant force, we would have expected a decline in their relative wages, an increase in their unemployment rate, and, presumably, no upgrading in female occupational allocation. Our findings would not have been possible had employers not increased the number of females they were willing to hire.

We enrich our empirical analysis by focusing on younger emigrants who are more likely to affect the marriage market. We concentrate on draft 'delinquents,' presumably, men who had emigrated still as teenagers to avoid the military draft. Hence, we can test for differential impacts of the timing of a decline in sex ratios on female labor and marriage market outcomes. We find that the age of emigrants indeed matters. Low sex ratios driven by higher rates of teenage male emigration reduce female marriage rates and increase FLFP, as is found in the previous literature.

The next section describes the institutional setting and social norms that prevailed in Portugal over the mid-twentieth century. The main datasets and the timing of the analysis are presented in Section 3. Section 4 describes the conceptual framework and the empirical strategy. Section 5 presents our results and robustness checks. We provide further evidence based on draft delinquency in Section 6. Section 7 concludes.

2 Historical and institutional settings

Throughout the mid-twentieth century, many countries experienced changing gender norms.[2] Portugal, and its neighbor Spain, were not among them. Both countries entered the century under the governance of republican systems that were subsequently overthrown by long-running dictatorships. Portugal was ruled by the nationalist dictatorship of António de Oliveira Salazar for the entirety of the mid-twentieth century (from 1926 to 1974), whereas Francisco Franco ruled Spain between 1936 and 1975.

Portugal's 1933 Constitution clearly summarizes the gender views of the times. Article 5 stipulated that all citizens are equal before the law, with the exception of women due to "differences that result from their nature and the good of the family."[3] During Salazar's reign, laws on education, voting rights, marriage, divorce, and contraception were changed such that women's civic participation became increasingly restricted to the household sphere. On the family side, divorce was outlawed in 1940 after being legal for the previous 30 years. Contraception was banned in 1942, and abortion remained illegal. Salazar closed down public nursery homes in 1937, apparently because

[2] See, e.g., the "evolutionary" and "revolutionary" phases of Goldin (2006, 2014).

[3] The 1971 amendment to the Constitution removed "and the good of the family" from this sentence, but the spirit of Article 5 remained the same.

he believed women's primary role in society was to take care of their children, and nursery homes were, therefore, unnecessary (Roseneil et al., 2020). Married women were prohibited from leaving the country or obtaining a passport without their husbands' written consent. Voting rights also differed across gender. For example, to vote in national elections, men were (until 1968) only required to know how to read and write, whereas women were required to have a secondary education diploma.

The evolution of Portugal's compulsory schooling laws went in a direction opposite to that observed in most developed countries during the first half of the twentieth century. Compulsory schooling was set to only three years of education in 1930 after already being reduced to four in 1927. Furthermore, its curriculum emphasized building nationalism, Catholic moral values, and traditional gender roles. The government's views on gender roles led to an uncommon gender-specific compulsory schooling law in 1956—four years for boys and three for girls.

Marriage, divorce, and contraception laws evolved surprisingly similarly in Spain under Franco's regime. Divorce was not permitted until 1981 (except for 1931-1939), while abortion and contraception became illegal in 1941, a few months before Portugal outlawed them. Similar to Portugal, Spain's 'permiso marital' (marital-permission laws) made it illegal for a married woman to own a property, open a bank account, get a passport, travel abroad, or be employed without her husband's consent. Women earned the right to vote in national elections in 1931, but unlike Portugal, Spain's constitutional right to vote was unambiguously recognized for both genders.

On the educational side, Franco's regime abandoned coeducation and implemented a new curriculum to instill patriotism, the Catholic church's values, and traditional gender roles (Flecha García, 2011). Minimum compulsory schooling laws were more ambitious in Spain than in Portugal. Already in 1909, compulsory education was raised from age 9 to age 12, and it was raised again to age 14 in 1964. Also, a modernization of the educational system took place starting in the 1950s. In particular, secondary education was restructured into three cycles, which stimulated students from lower social backgrounds to complete compulsory education.[4]

Not only did the laws and social norms in Portugal and Spain evolve similarly over the mid-twentieth century, but the timing of the end of their respective dictatorships also coincided. Portugal's Estado Novo fell in 1974 following the Carnation Revolution, and Franco died in 1975. These two events marked the beginning of the transition to democracy in the Iberian Peninsula.

The similarities in prevailing social norms and laws regarding gender roles in Portugal and Spain and the relatively low investment in Portuguese women's human capital suggest that, over the mid-twentieth century, the evolution of Portugal's FLFP would have been, at best, similar to Spain's.

Figure 1 ranks the FLFP rates of OECD countries across the 1960-1990 period. Not surprisingly, countries known to have progressive norms towards women and early childcare-policy adopters (e.g., Scandinavian countries) ranked high throughout the period. Portugal and Spain shared the bottom of this ranking in 1960 with FLFP rates below 20%. Given the discussion above, finding these two countries at the bottom of the ranking should come as no surprise. Having said this, the striking fact of Figure 1 is Portugal's remarkable progress up the distribution of FLFP rates, especially between 1970 and 1980.[5] Conversely, Spain remained in the lower tail of the distribution throughout the 1960-1990 period.

To further highlight Portugal's particular FLFP progress over the second half of the twentieth century, we focus on a Northern versus Southern Europe comparison. Doing so allows us to contrast Portugal's progress to those of FLFP leaders and laggers—Northern (Southern) European countries have been seen as forerunners (laggers) in

[4]Online Appendix A provides further details on the institutional setting, including a list of significant policies with a gender focus.

[5]Norway experienced a similar progress over that period. However, unlike Portugal, Norway expanded its subsidized childcare program significantly and introduced paternity leave over that period, which could explain part of its progress.

FLFP for many decades (Olivetti and Petrongolo, 2016). Figure 2 shows that Finland, Denmark, and Sweden already presented high FLFP rates in 1960, increasing to more than 60% in 1990. On the other hand, Italy and Spain are representative of the Southern European model, with persistently low FLFP rates—in 1990 they had not yet reached rates observed in Nordic countries three decades earlier. Norway and Portugal presented peculiar trends. In 1960, Norway's FLFP was similar to that of Southern European countries, but by 1990 it was at the forefront of Northern Europe. At the same time, Portugal started to diverge from the Southern European pattern, and by 1980 it was a clear outlier with a FLFP rate above Finland's and closer to Denmark and Norway's than to those of its Southern European neighbors. Given the closeness of Spain and Portugal on multiple dimensions beyond just geography (e.g., similar institutions and religion), their diverging FLFP trends have often puzzled social scientists.

While the political scene and social norms towards women were very similar in Portugal and Spain over the mid-twentieth century, we argue that one particular event in Portugal triggered its strong increase in FLFP. The late 1950s and early 1960s were marked by a widespread movement among African colonies for independence from their European colonizers (e.g., Belgium, France, and the UK). That trend gave ideological and political support to independence aspirations in Angola, Mozambique, Guinea-Bissau, Cabo Verde, and São Tomé and Príncipe, the Portuguese colonies in Africa at the time. Instead of granting independence to its colonies, Portugal decided to engage in a war against pro-independence movements in Angola, Guinea-Bissau, and Mozambique. 1961 marked the start of guerrilla warfare and a massive deployment of Portuguese troops in Africa that lasted until 1974. The rate of war fatalities by birth cohorts, depicted in Figure 3, shows the evolution of the war's intensity. It has an inverted-U-shaped pattern, with a peak for the mid-1940s birth cohorts and considerable variation within cohorts across regions.

Importantly, an impressive migration outflow was triggered by the life-threatening conditions faced by men in the colonial war, political repression, and the attraction of prosperity in Continental Europe. Overall, an estimated 1.5 million Portuguese (in majority men) emigrated over the 1960s and early 1970s, out of an initial population of 9 million. The emigration was large enough to generate significant variation in sex ratios across regions and cohorts and detectable years after the massive emigration period ended. The flow of emigrants out of Portugal increased from about 20,000 per year in 1950 to more than 150,000 in 1969 (Antunes, 1970). As the flow increased, its destination changed. In 1950, nearly two-thirds of Portuguese emigrants headed to Brazil, and only 1% to France. However, by 1969, 72% of Portuguese emigrants were going to France and less than 2% to Brazil (Antunes, 1970).[6] Two factors can explain this destination change. First, as the war in Africa intensified, legal emigration became more restricted, and thus illegal emigration increased significantly. Since Portuguese illegal migrants could reach France more easily without being caught (by land travel), France became a more attractive destination than Brazil, which required boat travel. Second, France experienced a prolonged period of solid growth and prosperity between 1945 and 1975 (known as the "Glorious Thirty"), during which it welcomed immigrants.

Since France attracted most Portuguese emigrants, its censuses are informative about the magnitude, timing, and gender composition of the Portuguese emigration outflow of interest. Figure 4 shows that around 8% to 12% of each male cohort born in Portugal in the 1940s is observed living in France by the mid-1970s. Importantly, there is significant cross-cohort variation in emigration rates. Not surprisingly, the prime incidence of male out-migration from Portugal coincided with the peak of cohort exposure to death risk in the war in Africa, suggesting that the war acted as a push factor throughout Portugal. We can shed light on the timing of male emigration by comparing the evolution of male emigration rates across census years in Figure 4. The

[6]The next three most popular destinations were Germany (9%), the US (9%), and Canada (4%).

vertical distance between the lines reveals that the main drain of men born in the 1940s occurred between 1968 and 1975, during the colonial war. For men already in their 20s at the beginning of the war (born in 1941 or earlier), the mass emigration started between 1962 and 1968, providing further evidence that the war played an important role in explaining the observed emigration patterns. Unlike Portugal, Spain's emigration to France stayed relatively small and stable between 1962 and 1975 (Online Appendix Figure B.1). Spain's emigration pattern suggests that the attraction of France's prosperity was gradual, which contrasts with Portugal's emigration pattern.

The gender composition of the migration flow was initially unbalanced. In the late 1960s, France had 1.5 to 2.5 Portuguese men per woman. In 1968, an impressive 48% of married Portuguese males living in France did not have their spouse in the household, showing that a large share of Portuguese male emigrants were married and initially left their wives behind (see also Online Appendix Figure B.2). Later, women made up an increasing proportion of emigrants, especially among cohorts born after 1947 (Online Appendix Figures B.2 and B.3).

Finally, Figure 4 shows that, after 1975, the stock of male immigrants observed in France hardly changed. The low return of Portuguese emigrants ensured that the demographic shock was permanent.[7] Two to three decades later, the "missing men" were still living in France. The magnitude of such a male outflow from Portugal's labor market was profound. Irrespective of female outflows, a major workforce scarcity was bound to occur in an economy growing at 4 to 12% per year and where the labor force was composed almost exclusively of males (in the early 1960s).

[7] Unlike the US, the Portuguese labor market shock did not dissipate with the end of the war.

3 Timeline of the analysis and main datasets

Panel A of Table 1 reports the timeline of the analysis. The war and emigration shock occurred between 1961 and 1974, and we observe our outcomes of interest in 1981 (except for wages, which we observe in 1985 due to availability). Hence, we undertake a medium- to long-term analysis of the impact of shocks that occurred in the 1960s and early 1970s. The assumption is that women's labor market integration decisions took place over the 1960s and early 1970s, with lasting effects captured in 1981.

Panel B provides information on some of the 18 birth cohorts we analyze. We focus on cohorts born between 1937 and 1954 since they correspond to the first (1937) and the last (1954) cohorts to suffer war fatalities among soldiers, corporals, or non-commissioned sergeants.[8] Not surprisingly, these cohorts also experienced high emigration rates, especially in the late 1960s and early 1970s, as discussed in Section 2. Women from these cohorts were between 6 and 23 years of age at the start of the war but in their prime working years when observed in 1981 (aged between 26 and 43).

Panel C lists the primary data sources used in this paper. We analyze female labor- and marriage-market outcomes of these 18 cohorts using the 1981 Portuguese Census microdata (accessed through IPUMS-International (Minnesota Population Center, 2015)). This census is the earliest one with available microdata. We compute the sex ratios, FLFP rates, occupational outcomes, marriage rates, and a set of control variables by birth cohort and (NUTS 3) region. The smallest territorial units provided in the IPUMS Portuguese Census are NUTS 3, which are comparable to 'districts' (Portuguese administrative divisions).[9] Mainland Portugal had 18 districts and 20

[8]Online Appendix B describes the TerraWeb (2016) dataset on Portuguese Colonial War fatalities.

[9]The Nomenclature of Territorial Units for Statistics (NUTS) code has three nested levels of aggregation (NUTS 1, NUTS 2, and NUTS 3), with NUTS 3 being the most disaggregated division level. While NUTS favors administrative divisions, the main factor for defining NUTS regions is their average population size. NUTS 3 are statistical divisions for which the average population size of each unit should be between 150,000 and 800,000. Continental Portuguese districts are first-level administrative subdivisions, similar to states in the US. Each district spans multiple municipalities. For example, the Lisbon district comprises 16 municipalities, including the city of Lisbon.

NUTS-3 regions in 1981.[10] Given that we have 18 birth cohorts and 20 regions, our sample consists of 360 observations.

We use the longitudinal linked employer-employee data from Portugal's *Quadros de Pessoal* (QP) to analyze the wage structure. QP covers the population of firms with wage earners in the economy's private sector and all their workers. It reports each worker's gender, birth date, highest education level achieved, earnings, and work hours (among other variables). Since QP data are available starting in 1985, we match the 1981 Census data to the 1985 QP data at the cohort-region cell level.

Looking ahead, we will instrument both the sex ratio and market size using past emigration information. Recall that France absorbed approximately three-quarters of the Portuguese emigration over the 1960s and early 1970s. As such, we use the number of immigrants of Portuguese origin observed in the 1975 French Census, by birth cohort and gender, to construct our emigration instruments. The French Census reports Portuguese immigrants' birth cohort and gender but not their region of origin. Hence, we follow a standard procedure in the literature (described in Section 4) to allocate the flow of migrants out of Portugal back to their region of origin according to regional historical emigration patterns before the shock. In our case, we use the geographical distribution of legal emigration out of Portugal during the 1950s (found in Baganha (1994)) to capture historical emigration patterns.

Finally, we use historical data on the number of births by cohort, gender, and district, available on the National Statistical Office's website (Portugal, INE, 2017b), to construct our emigration rates (i.e., emigrants/births). The information on the number of births and the shares of emigration in the 1950s are available at the district level. Since municipalities do not cover multiple districts or NUTS, we use

[10]Online Appendix Figure B.4 shows a map of mainland Portugal's district-NUTS-3 correspondence. We exclude the Azores and Madeira from our analysis. These two autonomous regions are archipelagos located in the Atlantic Ocean and they had emigration patterns that differed significantly from mainland Portugal at the time. Furthermore, the *Quadros de Pessoal*, which we use for our wage analysis, does not provide full firm coverage in the archipelagos.

municipality-population information from Pordata (2017), and municipality-district and municipality-NUTS 3 correspondences to convert births (and emigration shares) per district into births per NUTS 3. Online Appendix B provides more details on how we handle the Census data and convert district into NUTS 3 information.

Table 2 provides descriptive statistics on our 360 cohort-region observations. The size of each birth cohort is around 100,000 individuals for each gender. The sex ratio at birth was 1.07 and relatively homogeneous across cohorts, in line with the worldwide average (James, 1987; Parazzini et al., 1998). However, the average sex ratio dropped to 0.94 for 26- to 43-year-olds, and its range across cohorts widened substantially. On the one hand, we would expect the sex ratio to decline beyond birth, given that the mortality rate is typically higher for males than females. On the other hand, in the absence of exogenous shocks, we would expect a smaller decline in the average sex ratio than observed. For instance, in the 1980 US Census, the sex ratio was 0.97 for individuals aged 26 to 43 (Minnesota Population Center, 2015). An overwhelming 89% of the females were married, widowed, or divorced. The FLFP averaged 0.54, a relatively high value by international standards for the cohorts under study. The gender wage gap was 25 log points. In 1981, females comprised 39% of the employment in the top occupations (managers, senior officials, and legislators). Employed women averaged 4.6 years of education in the private sector (5.3 in the private and public sectors combined), compared to 4.2 in the overall population. Turning to the 1975 French Census information, 6% of the population born in Portugal between 1937 and 1954 had moved to France. That rate was one percentage point higher for males than females, given that, by then, the previous gender imbalance had partly closed, as female emigration followed the male pattern, but with a delay (see Online Appendix Figure B.3).

4 Conceptual framework and empirical strategy

To motivate our empirical strategy and help interpret our results, we use the conceptual frameworks of Angrist (2002) and Acemoglu et al. (2004). We first rely on Angrist (2002)'s design to investigate the effect of the sex ratio on the female marriage rate, labor force participation, and occupational upgrading. Next, we use Acemoglu et al. (2004)'s framework to examine in more detail the relative roles of female labor supply and demand in explaining the effect of the sex ratio on FLFP.

4.1 Sex ratio, FLFP, marriage, and occupational upgrading

Angrist (2002) exploits immigration flows in the US as drivers of sex-ratio variation. He considers a production function where the market size and its gender composition interact to produce marriages and labor market outcomes, y_{cr}, in the following way:

$$y_{cr} = ln[\theta_r R_{cr}^{\beta} mkt_{cr}^{\delta}], \tag{1}$$

where the sex ratio R_{cr} is the number of males to females for birth cohort c in region r, and mkt_{cr} is the market size (e.g., the total population of cohort c in region r). In our case, the first outcomes we consider are the 1981 female marriage and FLFP rates.[11] θ_r can be seen as an efficiency parameter that varies across regions ('matching' efficiency, in the case of the marriage market). Equation (1) suggests the following functional form as the basis for a regression:

$$y_{cr} = \beta \ln R_{cr} + \delta \ln mkt_{cr} + \phi_r, \tag{2}$$

[11] The marriage (FLFP) rate is defined as the proportion of women in region r and cohort c that are ever-married (labor-market active).

where $\phi_r = ln\theta_r$. The model predicts that, as the sex ratio increases, females' marriage rate and unearned income will increase, leading to a decrease in labor supply (Becker, 1981; Grossbard-Shechtman, 1984, 1993).

For ease of comparison, we use the same functional form for our occupation outcome, as is done in Angrist (2002) and Grosjean and Khattar (2019). Since we find a negative effect of the sex ratio on FLFP, looking at the link between the sex ratio and occupational upgrading will be our first step in identifying the potential mechanisms through which this effect operates. Our occupational outcome of interest is the female representation among 'top occupations' (i.e., managers, senior officials, and legislators). These occupations are traditionally among the most male-dominated ones.

Our main regression equation for the marriage rate, FLFP, and female representation among top occupations builds on equation (2) and adds two sets of controls to relax the model. More specifically, we estimate

$$y_{cr} = \alpha + \beta \ln R_{cr} + \delta \ln mkt_{cr} + \lambda edu_{cr} + \phi_r + \pi_c + \varepsilon_{cr}, \tag{3}$$

where we control for the average number of education years, $educ_{cr}$, as well as region and cohort fixed effects (ϕ_r and π_c, respectively). All regressions are performed at the cohort-region cell level, using the number of individuals in the cells as analytical weights.[12] Standard errors are clustered at the region level.

The main challenge in estimating equation (3) is that our outcomes (such as FLFP) and the sex ratio may be driven by regional shocks that are cohort-specific. Similarly, the market size may be endogenous, as argued in Angrist (2002). Online Appendix C presents a simple accounting exercise that illustrates this endogeneity problem. Essen-

[12]We use the total number of females in the case of the female marriage and participation rates, and total employment in the case of the occupational outcome.

tially, the accounting exercise shows that cohort c's sex ratio in year t, R^c_{rt}, is

$$R^c_{rt} \approx R^c_{rc} + \left[j^{mc}_{rt} - l^{mc}_{rt}\right] - \left[j^{fc}_{rt} - l^{fc}_{rt}\right], \tag{4}$$

where R^c_{rc} is the sex ratio at birth for cohort c. The terms j^{gc}_{rt} and l^{gc}_{rt} represent the inflow and outflow rates (i.e., flows relative to births) between years c and t, for cohort c, region r, and gender g. Since sex ratios at birth are relatively homogeneous, heterogeneity in sex ratios at any moment is driven by the gender imbalance in population movements. Clearly, our outcomes and the gender composition of net population movements could react to cohort-specific regional shocks.[13] Therefore, we need to find an instrument for the gender composition of population movements. Likewise, an instrument is required for the market's overall size, as it also depends on population movements.

4.1.1 Instruments for the sex ratio and market size

In order to instrument the sex ratio and the market size, we exploit the fact that Portugal experienced substantial emigration over the 1960s and early 1970s, triggered, in large part, by the life-threatening conditions faced by men in the war.

The emigration gender composition is meant to instrument the sex ratio:

$$emig\_gender_{cr} = \frac{E^{mc}_{r,81}}{B^{mc}_r} - \frac{E^{fc}_{r,81}}{B^{fc}_r}, \tag{5}$$

where $E^{gc}_{r,81}$ and B^{gc}_r stand for the 'predicted' number of emigrants in 1981, and the number of births (for cohort c, gender g, and region r), respectively. This instrument reports the expected gender gap in emigration rates and should be negatively correlated

[13] Even if women were not mobile following an economic shock (or less so than men), and we ignore the reverse causality issue, the sign of the bias of the OLS estimator is not obvious. It will depend on whether females are more likely to enter the labor force due to necessity or opportunity.

with $\ln R_{cr}$. Similarly, the overall emigration rate will instrument $\ln mkt_{cr}$:

$$Erate_{cr} = \frac{E^{mc}_{r,81} + E^{fc}_{r,81}}{B^{mc}_{r} + B^{fc}_{r}}. \tag{6}$$

We combine the number of Portuguese immigrants observed in the 1975 French Census by cohort and gender and historical emigration patterns from each Portuguese region during the 1950s to predict the number of emigrants in 1981, $E^{gc}_{r,81}$, using a 'leave-own-out' procedure:[14]

$$E^{gc}_{r,81} = \left[I^{gc}_{75} \left(1 - \frac{E_{r,50s}}{\sum\limits_{r} E_{r,50s}} \right) \right] \times \frac{E_{r,50s}}{\sum\limits_{r} E_{r,50s}}, \tag{7}$$

where I^{gc}_{75} is the number of Portuguese from gender g and cohort c observed in the French Census, and $E_{r,50s}$ is the number of Portuguese emigrants from region r in the 1950s. The term in squared brackets is our aggregate cohort-gender specific emigration measure, I^{gc}_{75}, adjusted by subtracting each region's own emigration from the national total. The last fraction in equation (7) allocates the stock of immigrants observed in France back to their region of origin based on region r's share of the overall Portuguese emigration in the 1950s.

Our motivation for using equation (7) to predict the number of emigrants parallels that of Card (2009) when using earlier immigrant settlement patterns to identify the impact of immigration on the US labor market. In essence, we rely on new emigrants' tendency to be predominantly drawn from regions where emigration was prevalent earlier on. As such, we avoid the mechanical relationship between the instrument and the endogenous variable that would likely hold if we were to consider the actual movers

[14] We follow the emigration literature and construct a 'leave-own-out' aggregate-emigration measure for each region (i.e., each region's aggregate-emigration measure excludes their own emigration) to mitigate the potential effect of prior region-cohort-specific shocks on our nationwide emigration measure (see, e.g., Wozniak and Murray (2012) and Hunt (2017)). However, computing $E^{gc}_{r,81}$ including one's own region (i.e., using $E^{gc}_{r,81} = I^{gc}_{75} \times E_{r,50s} / \sum\limits_{r} E_{r,50s}$) yields very similar results.

over the late 1960s and 1970s, which would undermine the exogeneity assumption.

Recent studies have developed frameworks for the identification of shift-share instruments (SSIV), a type of instrument similar to ours. In fact, if the sex ratio at birth were to be constant across cohorts and regions, $emig\_gender_r^c$ could be written as a standard SSIV. Essentially, SSIVs are constructed by interacting an aggregate-level shock variable (e.g., national-level immigration flows) and an exposure-shares variable (e.g., historical immigrant settlement patterns in Card (2009) or the 1950 regional differences in emigration in our case).

Goldsmith-Pinkham et al. (2020) argue that the exogeneity of the exposure shares is the crucial condition necessary for the validity of SSIVs. We do not claim that our exposure shares are as good as random. Economic conditions may have contributed to driving Portuguese people abroad. Historically, less-educated, poorer, and rural regions sent a higher fraction of their population abroad. Figure 5 shows maps of the spatial distribution of emigration rates in the 1950s along with 1950 regional characteristics. The top-left map shows that emigration in the 1950s was more prevalent in Northern Portugal (regions closer to France). Comparing the spatial distribution of emigration rates to that of 1950 regional population sizes, shares of the active population working in agriculture, or literacy rates does not suggest clear patterns between regions' 1950 characteristics and emigration. However, the bottom-middle map suggests that individuals from lower activity-rate regions were more likely to emigrate, hinting that the lack of economic opportunity may play a role in explaining regional differences in emigration.[15] However, our region fixed effects should control for historical (time-invariant) regional differences both in emigration rates and economic conditions. Using regional emigration patterns that were prevalent more than 20 years before our observed out-

[15]Online Appendix Table E.1 shows that a one-percentage-point increase in activity rate is associated with a 0.5 p.p. decrease in the emigration rate. Population size is also negatively correlated (at a 10% significance level) with the emigration rate, but the magnitude of the parameter estimate is small. A 10,000 population increase is associated with a 0.04 p.p. decrease in the emigration rate.

comes should also minimize the likelihood of our instruments being correlated with 1981 cohort-region-specific economic shocks.

Furthermore, our instruments can be valid even if concerns remain regarding the exogeneity of the 1950s emigration shares. Indeed, Borusyak and Hull (2020) and Borusyak et al. (2022) propose a different framework, where they show that while the exogeneity of the exposure shares is a sufficient condition for the validity of SSIVs, it is not necessary. SSIVs can still be valid even if exposure shares are endogenous. The crucial assumption in their framework is the exogeneity of the aggregate-level shocks (in our case, the national-level emigration shocks across cohorts). Based on Borusyak et al. (2022)'s framework, the inverted-U shape relationship between emigration rates and birth cohorts illustrated in Figure 4 should give some support to the validity of our instruments. First, it makes it unlikely that our instruments are correlated with national-level trends in our outcomes of interest. Second, if, over our sample period, the war in Africa was the main reason for emigration at the national level (as supported by a comparison of Figures 3 and 4), our instruments should be less likely to be correlated with other cohort-region-specific economic shocks that could affect our outcomes of interest in 1981 (especially once we control for cohort and region fixed effects). We will provide direct evidence supporting the validity of our instruments and address the main potential threats (i.e., cohort-region-specific economic shocks) to our identification strategy in Section 5.5.

Finally, Figure 5 also presents some visual evidence regarding the relevance of our instruments. The emigration-rate map (top-left) and the bottom-right map showing the average drop in sex ratio between birth and 1981 (i.e., $R^c_{rc} - R^c_{rt}$) suggest a negative correlation between the 1950s emigration and the 1981 sex ratio. Online Appendix Table E.1 confirms this correlation: a one-p.p. increase in the emigration rate is associated with a 0.011 decrease in the sex ratio.

4.2 Sex ratio and wage gap

Our conceptual framework for analyzing the effect of sex ratio on the gender wage gap comes from Acemoglu et al. (2004). They exploit variation in WWII military conscription across US regions to study the impact of an increase in female labor supply on wages. They assume a competitive labor market with imperfectly substitutable factors: capital, K_{cr}, female labor, F_{cr}, and male labor, M_{cr}. These factors interact to produce a single good, Y_{cr}, through a nested CES production function:

$$Y_{cr} = A_{cr} K_{cr}^{\alpha} [(1-\lambda)(P_{cr}^{m} M_{cr})^{\rho} + \lambda (P_{cr}^{f} F_{cr})^{\rho}]^{\frac{1-\alpha}{\rho}}, \tag{8}$$

where P_{cr}^{m} and P_{cr}^{f} are gender-specific productivity indices, λ is a share parameter, and A_{cr} is a gender-neutral productivity factor. The elasticity of substitution between female and male labor is $\sigma_{MF} = 1/(1-\rho)$. This setting motivates a wage regression equation of the form:

$$\ln w_{cr}^{g} = \alpha + \chi \ln(F_{cr}/M_{cr}) + \pi f_g + \eta f_g \times \ln(F_{cr}/M_{cr}) + \epsilon_{crg}, \tag{9}$$

where f_g is a female dummy variable, and g stands for gender (and we omit control variables for simplicity). The coefficient on the interaction term, η, captures $-1/\sigma_{MF}$ while $\chi + \eta$ is the inverse elasticity of demand for female labor.

The model predicts that, under fixed capital and male labor, an increase in female labor will unambiguously decrease female wages. In contrast, its effect on male wages is ambiguous. Male wages will fall (rise) if male and female labor are substitutes (q-complements). Acemoglu et al. (2004) find that an increase in female labor supply reduces female earnings, as their estimates for χ and η are negative and statistically significant. They also find imperfect substitution between male and female labor, as increased female labor supply reduced males' wages to a lower extent than females'.

This conceptual framework and findings will help us interpret our FLFP results. If the primary factor driving the observed FLFP increase were a shift in female labor supply, we would expect similar results (at least for the sign of the parameter estimates).

As in the case of the marriage rate, FLFP, and occupational upgrading, our data allow us to relax Acemoglu et al. (2004)'s setting and estimate

$$\ln w_{cr}^{g} = \alpha + \sigma \ln R_{cr} + \pi f_g + \beta f_g \times \ln R_{cr} + \nu \ln mkt_{cr} + \lambda edu_{cr} + \gamma_r + \zeta_c + \epsilon_{crg}, \quad (10)$$

where γ_r and ζ_c are region and cohort fixed effects, respectively.[16] Our hourly wage measure is the ratio between monthly earnings (base wage plus all regularly paid components, such as food and transportation allowances) and the number of regular work hours. β in equation (10) captures the effect of the sex ratio on the gender pay gap. A positive β would mean that, as the sex ratio decreases (say, if the relative number of females increases), the gender gap increases. All regressions are performed at the cohort-region cell level using cell-level employment as weights, and standard errors are clustered at the region level. We will treat the (log of) sex ratio and the market size as endogenous variables, and they will be instrumented using $emig\_gender_{cr}$ and $Erate_{cr}$. In addition, $f_g \times emig\_gender_{cr}$ will instrument $f_g \times \ln R_{cr}$.

Finally, when dealing with employment outcomes (occupational allocation and the gender pay gap), we account for the possibility that women's selection into the labor market may have changed, leading to compositional effects. In particular, a change in the sex ratio may lead women with higher (or lower) qualifications to join the labor market, making women's educational attainment endogenous. Hence, we instrument employed individuals' education using the education of the respective gender and cohort in the overall population.

[16] Note that the sex ratio (number of males per female) is the inverse of females per male, used in Acemoglu et al. (2004). Therefore, our parameters of interest (σ and β) are expected to have the opposite sign to the ones in their article.

5 Results

5.1 Female labor force participation

We begin by estimating the effect of the sex ratio on female labor force participation. OLS results, reported in column (1) in the top panel of Table 3, point to a negligible partial correlation between the sex ratio and labor market participation. This finding is likely to be misleading due to the endogeneity of the sex ratio and the market size. In columns (2) to (5), we account for the endogeneity of these regressors using our instruments. We start by focusing on females' own cohort only. However, it is reasonable to assume that the labor and marriage markets are not cohort-specific, as individuals born in adjacent years could influence each other (e.g., if they compete for the same husband, or job). We, therefore, also consider cohort windows (from own birth cohort plus or minus one year to own cohort plus or minus three years).

The IV evidence points to more substantial impacts than its OLS counterpart. The estimated coefficient of the (log) sex ratio is -0.39 and statistically significant at 1%. Hence, if the sex ratio were to decrease by 10%, say from 1 to 0.9, the FLFP would increase by 3.9 p.p. A potential explanation for why the IV estimates are more negative than the OLS ones is that males could react more to 'booming' regions, leading to a positive correlation between the sex ratio and overall labor force/employment rate (of females and males). Our results are robust across cohort windows.[17] The Sanderson-Windmeijer F-statistics in Table 3 and first-stage results (Online Appendix Table D.1) point to relevant instruments, without being "too strong", thus avoiding some of the concerns regarding the validity of the exclusion restriction (Jaeger et al., 2018). Nevertheless, reduced-form results (Panel A of Online Appendix Table D.4)

[17] The increase in the magnitude of the sex ratio coefficient estimates, as we consider wider cohort windows, is also observed in the OLS results. However, the OLS coefficient estimates for wider cohort windows remain much smaller in magnitude than our IV estimates (see Online Appendix Table D.7). The IV and OLS results both suggest that considering surrounding birth cohorts matters.

suggest that our instruments are strong enough to affect the FLFP significantly.

Our FLFP results align with the previous literature, suggesting that a relative scarcity of men leads women to enter the labor market. Both labor demand and supply channels predict that a decrease in the sex ratio will increase females' LFP.

5.2 Marriage market

We now investigate the potential role played by the marriage market. Panel B of Table 3 points to a small and statistically insignificant impact of the sex ratio on the marriage market in Portugal.[18] None of the reduced-form sex-ratio parameters are statistically significant (Online Appendix Table D.4, Panel B). Such a finding further supports the hypothesis that the effect of the sex ratio on the marriage rate is null (Angrist and Krueger, 2001; Chernozhukov and Hansen, 2008). Our results should not be surprising. As mentioned in Section 2, an overwhelming majority of emigrating men were already married and initially left their wives behind. So, our instruments affected the sex ratio faced, in large part, by married women, and, therefore, the (married) male scarcity did not affect the marriage rate significantly. In Section 6, we will show that the sex ratio affected the marriage rate of women who were younger during the peak of the war.

Divorce increased significantly in Portugal following its legalization in 1975. Importantly for the interpretation of our results, if male emigration increased the risk of divorce, women could increase their labor supply to insure against the potential financial consequences of facing a divorce (Goldin, 2006). We investigate this possibility by estimating the effect of the sex ratio on the female divorce rate. Online Appendix Table F.2 suggests that a decrease in the sex ratio does not lead to an increase in divorce.

[18] We present the results centered on one's own cohort for consistency reasons, but one could argue that there is a tendency for women to marry older men (two years older, at the median). Online Appendix Table F.1 presents results when we center males' cohort window at -2 years (e.g., for women born in 1940, the "own cohort +-1 year" cohort window is composed of males born between 1937 and 1939). These results also suggest that the effect of the sex ratio on the marriage rate is null.

The sex-ratio coefficient estimates are positive, but small and statistically insignificant for all of our specifications, suggesting that the post-1975 increase in divorce is not a major cause for concern for the interpretation of our results.

Our findings on the link between the sex ratio and the marriage market contrast with those from the previous literature. Indeed, the majority of previous studies on the subject find that the marriage market is an important channel through which the sex ratio affects the female labor supply (e.g., see Angrist (2002), Teso (2019), and Boehnke and Gay (2022)). Abstracting from the marriage-market channel allows us to focus on the role of another channel through which an imbalanced sex ratio could affect the FLFP: employers' increased female labor demand.

5.3 Occupational upgrading

In order to investigate the potential channels behind the FLFP results, we begin by looking at the link between the sex ratio and occupational upgrading. Table 4 reports descriptive evidence on the evolution of occupational allocation across gender, using the 1960 and 1981 Censuses.[19] Panel A shows the distribution of females across occupations (sh^f), whereas Panel B shows the corresponding distribution for males (sh^m). Panel C presents the relative incidence (sh^m/sh^f) for each occupation. We classify an occupation as male-dominated ("blue") if its 1960 relative incidence is greater than two and female-dominated ("pink") if it is less than 0.5. Occupations with a relative incidence between 0.5 and 2.0 are classified as mixed gender. The 1960 blue occupations were managers, senior officials, and legislators; skilled agriculture and fishery workers; plant and machinery occupations; elementary occupations; and Armed Forces. The pink occupations were professionals; and service workers and shop and market sales.

[19]Our occupation-upgrading analysis (presented in Tables 4 and 5) combines private- and public-sector occupations. Online Appendix B details how we handle the conversion of the occupational classification in the original 1960 Census of Portugal into the IPUMS classification.

The trend from 1960 to 1981 is striking. All occupations that were predominantly male in 1960—except for elementary occupations—saw remarkable increases in women's representation. The share of women employed in top occupations (i.e., managers, senior officials, and legislators) underwent an eleven-fold increase, from 0.4% of employed women in 1960 to 4.3% by 1981. The increase was less than three-fold for men, from 1.7% to 4.4%. Therefore, the gender-relative incidence in top occupations reached parity (1.04) in 1981.

We are interested in testing whether the sex ratio affected female representation among top occupations. In particular, if employers' female labor demand was a dominant force behind the observed increase in FLFP, we would expect female representation among top occupations to increase as the sex ratio decreases (once we control for market size). The estimation strategy is the same as when we look at FLFP and the marriage rate, except for two notable changes. First, since we are looking at occupational allocation, we control for the market size using the log of employment instead of the population size (but still treat it as endogenous). Second, given that the outcome is the proportion of females among top-occupation workers, we control for the gender gap in the education of employed individuals instead of female education itself. We argue that the gender difference in educational attainment is more relevant in predicting female representation in top occupations. The larger the education gap in favor of men, the smaller the female representation should be. Following our reasoning presented in Section 4.1.1, we treat the education gender gap of employed individuals as endogenous and instrument it with the overall population gender gap in education.

Table 5 reports that a 10% decline in the sex ratio, such as from 1 to 0.9, increases the female share among managers, senior officials, and legislators by 4–6 p.p. Although slightly less precisely estimated than in the case of the FLFP, our sex-ratio parameters are all statistically significant at the 5 or 10% significance levels except for the "+-3 years" cohort window specification, where the p-value is just above 0.1 (0.109). We can see that the effect is sizeable once compared to the outcome's average (39%). As expected, a larger gender gap in education leads to a smaller share of females within top occupations. Comparing the sex-ratio and education-gap parameter estimates, we can see that a 10% increase in the sex ratio has essentially the same effect as increasing the educational gap by 1–1.4 years.[20] Despite finding that the sex ratio had a sizable effect on the female representation among top occupations, Table 5 and Online Appendix Table F.3 also suggest that females and males were not seen as perfect substitutes, which is in line with the recent literature on imperfect worker substitutability (see, e.g., Hensvik and Rosenqvist, 2019; Azmat et al., 2022; Jäger and Heining, 2022). In particular, Online Appendix Table F.3 shows that employers were disproportionately increasing the population share of men in top occupations relative to their woman counterparts when men were becoming relatively more abundant.[21]

5.4 Gender pay gap

We now investigate whether the observed sex-ratio variations lead to changes in the gender pay gap. To do so, we rely on Acemoglu et al. (2004)'s framework and wage information from QP.[22] Table 6 shows that while the sex ratio does not seem to affect males' wages significantly, the negative "ln Sex Ratio $\times$ Female" parameter estimates suggest that an increase in the sex ratio actually increases the gender pay gap. A 10% decline in the sex ratio reduces the gender wage gap by 3-4 p.p. While the p-values for

[20] The first-stage Sanderson-Windmeijer F-stats for the sex ratio and market size are very similar to those reported for the FLFP and the marriage rate. Furthermore, the p-values for the emigration gender gap reduced-form parameter fluctuate between 0.06 and 0.15, which brings additional evidence of a significant causal link between the sex ratio and female representation among top occupations.

[21] The scarcity of male labor also led some employers to innovate or modernize their production to facilitate the integration of female labor. Cardoso and Morin (2023) document that, in reaction to the male labor scarcity, Portuguese salt farming companies adopted a new technology to allow women to perform tasks that previously involved very physical activities (e.g., carrying a 50 kg basket on top of one's head)—tasks for which, at the time, women could have been seen as poor substitutes for men.

[22] Since QP does not cover public-sector firms, the sample of workers analyzed in this section should be seen as a subsample of the individuals analyzed in the Census. For this exercise, we use both female and male cohort-region cells resulting in a sample size of 720 observations.

this parameter are above 0.1 when we focus on own cohort and own cohort +-1 year (with p-values of 0.19 and 0.15, respectively), they decrease monotonically as we widen the cohort window and reach 0.06 when looking at own cohort +-3 years. Finding that the wage gap shrinks as the sex ratio decreases further supports the hypothesis that labor demand was vital in driving Portugal's rise in female employment during the 1960s and 1970s. Indeed, if the primary explanation for the rise in female employment were an increase in female labor supply, we would have expected a widening (as opposed to a decline) of the gender wage gap (Acemoglu et al., 2004; Boehnke and Gay, 2022). The IV coefficient estimates for "ln Sex Ratio" also inform us about the male emigration selection. The estimates are small (especially for own cohort +-1 to +-3 years) and statistically insignificant, suggesting that emigrating males did not have, on average, particularly bad or good jobs. In turn, this finding, combined with the negative "ln Sex Ratio $\times$ Female" parameter estimates, suggests that females found better jobs as the sex ratio decreased. The results on the gender pay gap, therefore, corroborate the evidence on occupational upgrading.[23]

One thing to remember when interpreting the IV results is that our estimates capture a weighted average effect of the sex ratio on our outcomes by weighting cohort-region cells proportionally to the first-stage impacts on the sex ratio (Angrist and Pischke, 2009). In our case, the delayed female emigration and the return of Portuguese expatriates from Africa after the 1974 revolution (the "retornados") could affect the interpretation of our results. These events may have affected the link between our instruments and the 1981 sex ratios—the 1981 sex ratios include retornados. In particular, cohort-region cells that experienced more female emigration between 1976 and 1981 or with sex ratios among retornados that favor males will be weighted less in computing the IV estimates. In the end, our IV sex-ratio coefficient estimates will put more

[23]As in the case of occupational upgrading, the wage-regression results suggest that females and males were seen as imperfect substitutes. Had they been perfect substitutes, the sex ratio would not have affected the gender wage gap (Acemoglu et al., 2004).

weight on cohort-region cells for which the male-biased emigration was long-lasting.[24]

5.5 Robustness checks and threats to identification

In this section, we provide further evidence in support of our IV approach. We do so in two ways. First, we use Stock-Wright S statistics to jointly test that our sex ratio and market size parameters are equal to zero and that our instruments are valid. This class of test is particularly relevant in our case given that some of our regression results (e.g., for the marriage rate and, as we will discuss below, for the unemployment rate) suggest that these parameters are not different from zero.[25] Second, we further tackle endogeneity concerns by investigating the robustness of our findings to alternative strategies that control for shocks that could be correlated with our instruments.

Panel B of Online Appendix Table D.4 shows that the Stock-Wright S statistics (and Anderson-Rubin tests) do not reject the null hypothesis that the instruments are valid and the endogenous-variable parameters are 0 at standard significance levels for any of our specifications. Such evidence supports the validity of our emigration instruments when investigating the effect of the sex ratio on the marriage rate and that this effect is indeed null.

Next, we estimate equation (3) for the following complementary outcomes: female employment and unemployment rates (Online Appendix Table F.5). The sex-ratio coefficient estimates for the female employment regressions are strikingly close to those obtained when looking at FLFP, suggesting that the vast majority of females who joined the labor force ended up being employed. Hence, labor demand absorbed any increase in female labor supply.

[24]While retornados settled disproportionately in the Lisbon and Setubal regions, Online Appendix Table F.4 shows that their sex ratios are not correlated with our instruments, once we control for cohort and region fixed effects.

[25]Online Appendix D describes the intuition behind tests based on Stock-Wright S statistics, and provides more detailed results regarding the validity of our instruments.

The results for the unemployment rate (bottom panel of Online Appendix Table F.5) are informative for two reasons. First, the sex-ratio parameters in the unemployment model are never statistically significant at conventional significance levels, corroborating the idea that the increase in FLFP in response to the decline in sex ratios was absorbed by employment. If anything, the estimates are positive, suggesting (if they were statistically significant) that females were less likely to be unemployed when more males were absent. We argue that such a finding is evidence against a 'pure' labor supply story. Second, the Stock-Wright S statistics from the unemployment IV regressions do not reject the null hypothesis that the endogenous-variable parameters are zero *and* that the exclusion restriction is valid at conventional rejection levels. These results further support the validity of our instruments. While one could argue that our instruments are valid for unemployment but not for the FLFP, one would need a peculiar environment where our instruments are correlated with the error term of the FLFP equation but uncorrelated with the error term in the unemployment equation.

As mentioned in Section 4.1.1, Borusyak et al. (2022)'s framework suggests that the validity of our instruments depends on the exogeneity of our aggregate-level shocks, and Figure 4 supports this hypothesis. However, given that we cannot directly test the validity of our instruments for outcomes such as the FLFP, we investigate the robustness of our findings by controlling for different types of shocks that could be correlated with our instruments. Since we control for region and cohort fixed effects, the main threats to identification are unobserved cohort-region-specific shocks or trends that could increase the propensity of males to emigrate (or result from emigration itself) and have an independent effect on the FLFP. One such shock is remittances, which were important in Portugal between 1969 and 1981, i.e., equivalent to 9% of the country's GDP and 58% of its exports, with a peak in 1978-1979 at 12% and 70%, respectively (Chaney, 1986, p.209). Such a volume of remittances meant a large positive income shock to Portuguese households. The available empirical evidence on the direct effects of remittances on nonmigrants' labor supply points predominantly to a *reduction* of FLFP (Amuedo-Dorantes and Pozo, 2006; Hanson, 2007; Airola, 2008; Yang, 2008; Antman, 2013). Beyond their impact at the household level, remittances could ultimately positively affect regional development (Clemens and McKenzie, 2018). However, our region fixed effects will capture any remittance 'development effects' as long as they benefited different birth cohorts equally. Still, in the short run, male emigration could motivate women to join the labor market to compensate for their migrant husbands' lost income. Using policy rules regarding the migration of Tongans to New Zealand and of Filipinos to South Korea as sources of exogenous variation, Gibson et al. (2011) and Clemens and Tiongson (2017), respectively, find no evidence that emigration affects the labor force participation of nonmigrant spouses. Based on these findings, we expect the direct effects of migration and associated remittances, if anything, to bias the magnitude of our sex-ratio parameter estimator downward.

In any case, we address the issue of cohort-region-specific shocks more generally using multiple strategies. First, we consider gender gaps instead of female levels as outcome variables in models where we do not already consider gender gaps (i.e., for FLFP and marriage).[26] Doing so should take care of cohort-region-specific shocks that affect females and males equally. Online Appendix Table F.6 shows that the effect of the sex ratio on the gender gap in LFP is essentially the negative of its effect on FLFP. The same is true for the gap in marriage rates, and the parameters remain statistically insignificant. Such findings suggest that our results in Table 3 are not driven by unobserved cohort-region shocks affecting female and male labor force participation (or marriage rates) equally.

Alternatively, we can control for cohort-region shocks more directly by including region-specific cohort trends or pre-war regional characteristics that we interact with

[26]Since we are interested in the LFP gap, we use the gender gap in educational attainment (as opposed to the level) as a control.

cohort fixed effects. Including region-specific cohort trends allows each region to experience different growth paths in our outcomes of interest. Doing so allows to control for cohort-region shocks in a very flexible way across regions but imposes that the evolution of the shocks across cohorts is linear. Including interactions of regional characteristics and cohort fixed effects allows for more flexibility regarding the outcome evolution across cohorts but imposes more restrictions on how regional characteristics affect the outcomes. We use three regional characteristics that could impact our outcomes of interest and that are available for 1960 (before our period of analysis): the share of the working population in agriculture, the activity rate, and the unemployment rate. We interact each of these characteristics with our cohort fixed effects to add 54 variables (3 characteristics per cohort) to our regression model. Online Appendix Tables F.7 and F.8 compare the own-cohort sex-ratio estimates from our primary estimations for FLFP, marriage rate, representation among top occupations, and wages (in odd-numbered columns) to those obtained when adding the region-specific cohort trends or interactions of regional characteristics and cohort fixed effects (in even-numbered columns), respectively.[27] Despite adding considerable noise to our estimation when incorporating either set of additional controls, our sex-ratio coefficient estimates stay (surprisingly) close to our main estimates. The sex-ratio parameters remain statistically significant at 10% for FLFP and close to zero (and statistically insignificant) in the case of the female marriage rate. The sex-ratio-female interaction parameters become statistically significant at 5% or 10% (depending on the additional controls) in the wage regression. While the sex-ratio parameter for top occupations is no longer sta-

[27]The main drawback of adding region-specific trends, or adding interactions of regional characteristics and cohort fixed effects, to our main specifications is that estimating such a model is data-demanding since we already control for region and cohort fixed effects. The region-specific trends and regional characteristics interacted with cohort fixed effects are likely to capture much of the remaining cross-cohort emigration variation, affecting our estimates' precision (and our first-stage F statistics). This is especially the case when we consider our regressors over cohort windows, as they consist more or less of rolling averages that smooth their cross-cohort variation. The first-stage F-statistics drop below 5 for most of our instrumented variables, and many estimates become uninformative. We, therefore, present results focusing on the own-cohort analysis.

tistically significant when including cohort-specific trends, the estimate remains close to our main one. Furthermore, it increases in magnitude and becomes statistically significant at 1% when we include interactions of regional characteristics and cohort fixed effects. So, overall, even though these estimation strategies use only emigration (and sex ratio) variation purged of region and cohort fixed effects, as well as their additional flexible controls, to identify the effect of the sex ratio, the results presented in Online Appendix Tables F.7 and F.8 are in line with our original results and provide further evidence that cohort-region shocks do not drive our main findings.[28]

6 Further evidence

We further investigate the fact that many emigrating men were already married as an explanation for why we do not observe a significant impact of sex ratios on female marriage rates. We introduce another set of instruments that rely on the emigration of young males, who were less likely to be married upon leaving the country. As the war in Africa prolonged, the need for human resources increased steadily, and virtually every non-disabled man was drafted (Cann, 1996). The fear of being sent to war led many men to dodge the military draft and emigrate illegally.

Column (1) of Table 7 reports the number of individuals registered in the military census due for medical assessment the year they turned 20. Comparing the number of individuals in the military records to the national demographic statistics reveals that approximately 85% of each cohort is captured in the military registry. The missing

[28]As additional checks, we investigate whether our results are sensitive to alternative standard-error computation methods. Online Appendix E shows how to modify our instruments and estimate our regressions at the cohort (shock) level (Borusyak et al., 2022) to alleviate Adão et al. (2019)'s concern regarding standard errors produced by standard shift-share regressions. We also compute standard errors following Roodman et al. (2019) to account for the small number of clusters and standard errors, allowing for two-way clustering (at the cohort and region levels). The results are robust to these alternative standard-error computation methods and are presented in Online Appendix Tables F.9 to F.16.

15% comes from infant and child mortality, which was high at the time, together with legal emigration before age 20 (presumably family emigration). Columns (2) and (3) highlight the increase in draft delinquency, as the share of young men due for medical assessment who did not show up increased steadily from 12% in 1961 to 20% in 1972.[29] Draft delinquency varied by region. The link between draft delinquency and regional historical emigration is noted by the Armed Forces General Staff (the body responsible for the armed forces' planning) in Portugal, EME (1988), and illustrated for the 1948 cohort in Figure 6.[30] Draft delinquency was thus driven by the war threat, and regional historical emigration.

We use the information on draft delinquency to construct instruments for the sex ratio and the market size. Since regional information on draft delinquency is available for the 1948 cohort only, our instruments will vary exclusively at the regional level. The 1948 cohort was 13 years of age at the start of the war, and due for medical assessment in 1968, just after the war's peak (Figure 3). A region's delinquency rate is $\frac{D_r}{C_r}$, where D and C stand for the number of draft dodgers and individuals in the military census, respectively. We interpret the draft delinquency rate as an indicator of out-migration, specifically for teenage men.[31] We use this variable to instrument the sex ratio. Next, we construct a regional *registration rate*, $\frac{C_r}{B_r}$, where C keeps its meaning, and B stands for the cohort birth size. Since child mortality and legal emigration are expected to be gender-balanced, we rely on registration rates to instrument market size.

Since we rely on a single birth cohort and cross-regional variation, we cannot include

[29]Table 7 also shows a gradual lowering of standards to be considered apt for service. In 1961, 23% of those assessed were judged either unfit for military service or had a justification for deferring the medical assessment. By 1968, that share had dropped to 5% (Columns 11-12). As a result, the share of those who were actually called for service increased from 75% in 1961 to 93% in 1968 (Column 10), which may have further increased draft delinquency.

[30]This information is reported in Portugal, EME (1968). For the other cohorts, draft delinquency information is only reported at the national level.

[31]Figure 4 supports our interpretation. We can observe close to 5% of the 1948 Portuguese male cohort in the 1968 French Census, suggesting that many of the males born in 1948 emigrated before age 20. Online Appendix Figure B.2 also suggests they were almost all single.

cohort and region fixed effects in the regressions. In order to control for regional economic conditions, we use the following region attributes in 1960: the unemployment rate, the activity rate, and the share of the working population in agriculture.

Table 8 presents the effect of the sex ratio on the FLFP and the female marriage rate. While the results should be interpreted with care given the small sample size, Panel B suggests that an increase in the sex ratio increases females' marriage rate. Taking these results at face value would suggest that facing a lower sex ratio at a younger age might decrease females' marriage likelihood. Interestingly, the magnitude of the sex-ratio coefficient estimate for the FLFP is larger than the ones observed in Table 3, which is in line with the female labor demand and labor supply reinforcing each other in this case.

7 Conclusion

A large and persistent economy-wide shock to sex ratios accelerated the female integration into the Portuguese labor market during the 1960s and 1970s. We provide robust evidence that the rise in FLFP was accompanied by female occupational upgrading and a closing of the gender pay gap. All three outcomes responded to changes in the sex ratios. Combined, they suggest an increased willingness of employers to hire female workers in a tight labor market. If the increase in women's desired labor supply had been the dominant force behind their rising employment, we would have witnessed a decline in their relative wages, an increase in their unemployment rate, and no obvious occupational upgrading.

The Portuguese historical experience differs from those in the previous literature, and it can shed light on other sources of the rise in FLFP observed over the past century. First, prominent explanations for the rise in FLFP are unlikely to hold in Portugal. Laws, institutions, and social norms restricted women's participation to the household sphere. Likewise, female investment in schooling cannot explain the 1960s and 1970s rise in FLFP given the Portuguese institutional setting, as they were minimal over that period. Second, most males who emigrated were already married, initially leaving their wives and children behind. This fact mitigated the impact of the sex-ratio shock on the female labor supply via the marriage market. Third, the male-scarcity shock was remarkably long-lasting in Portugal, especially for older cohorts, with the stock of individuals who emigrated during the 1960s and early 1970s still observed living in France decades later.

Our contribution goes beyond explaining the puzzle of high FLFP in Portugal, in contrast to the traditional Southern European model. A more general insight from our findings is that the channels through which sex ratio imbalances impact female labor market integration are influenced by historical context. Focusing on imbalances driven by a major military deployment and massive permanent emigration of men in a tight labor market unveils alternative channels that were not highlighted before.

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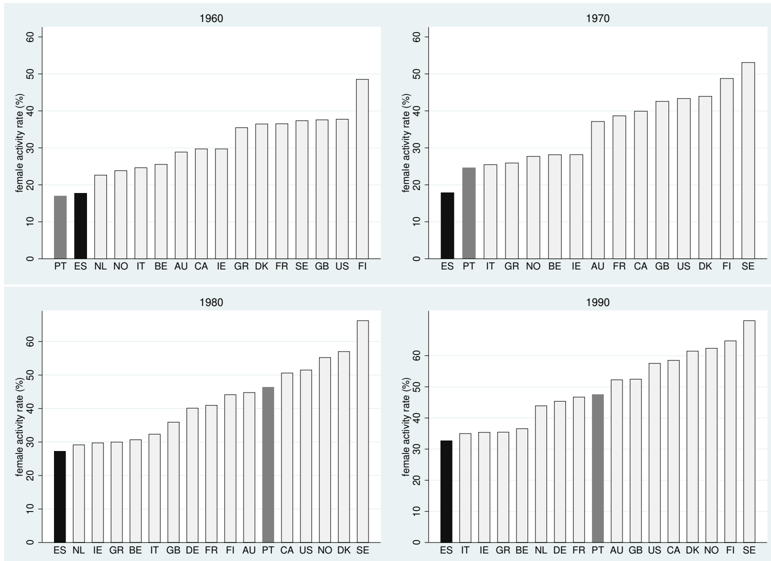
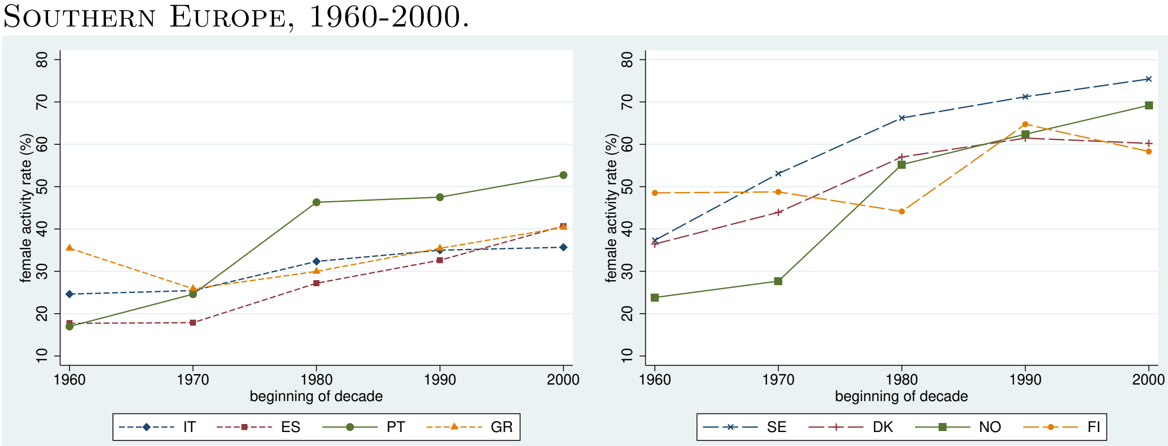
Figure 1: Female labor force participation rate, cross-country comparison, 1960-1990.



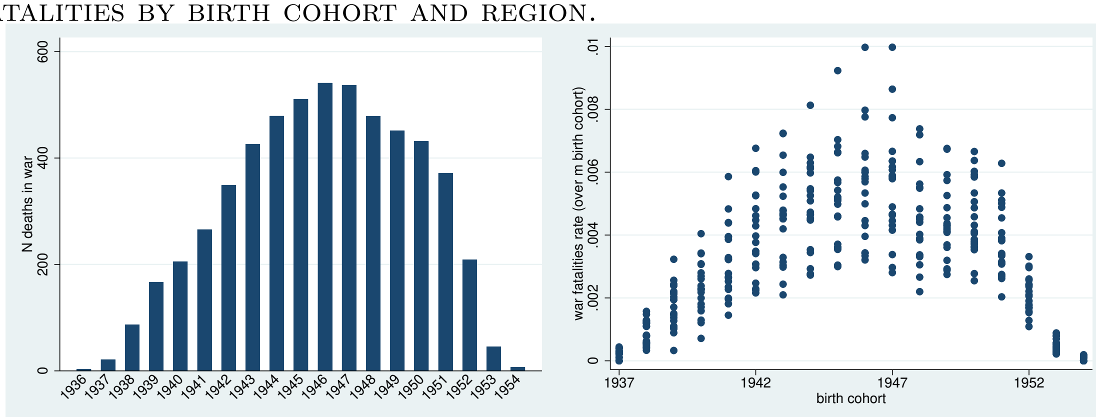
Source: World Bank (2018). Notes: Refers to population aged 15 and older. For each year t, we consider the earliest available data, at t or $t+1$. The exceptions are France (1962 and 1975), Sweden (1965, 1975, and 1982), and Germany (1983). The data for (Federal Republic of) Germany start in 1983. No data are reported for the Netherlands between 1960 and 1977.

Figure 2: Trends in female labor force participation, Northern vs. Southern Europe, 1960-2000.



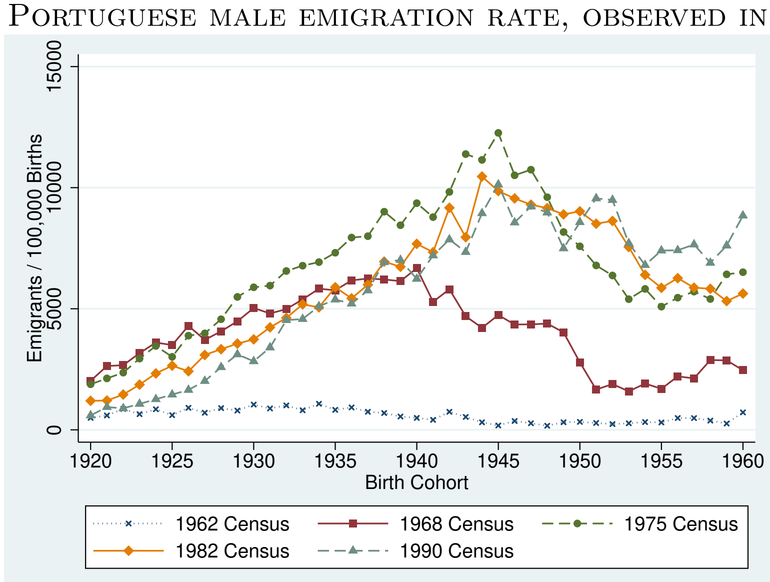
Source: World Bank (2018). Notes: Refers to population aged 15 and older. For each year t, we consider the earliest available data, at t or $t+1$. The exception is Sweden (1965, 1975, and 1982).

Figure 3: Number of war fatalities by birth cohort and rate of war fatalities by birth cohort and region.



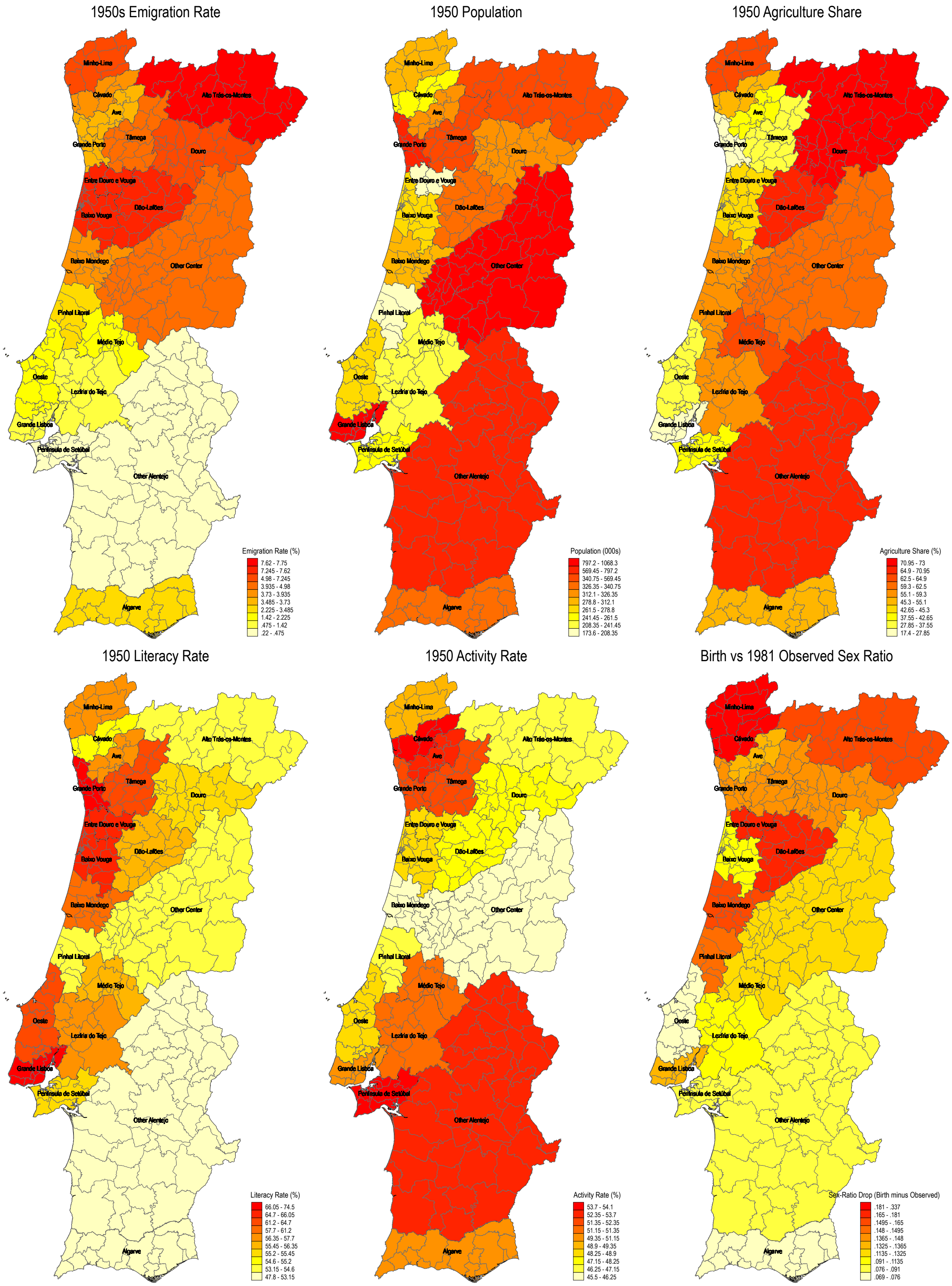
Sources: War casualties - TerraWeb (2016); births - Portugal, INE (2017b); for the conversion of districts into regions, see Online Appendix A. Considers deaths of soldiers, corporals, and non-commissioned sergeants born in mainland Portugal that occurred in 1961-1974 and were allocated to a birth cohort.

Figure 4: Portuguese male emigration rate, observed in France.



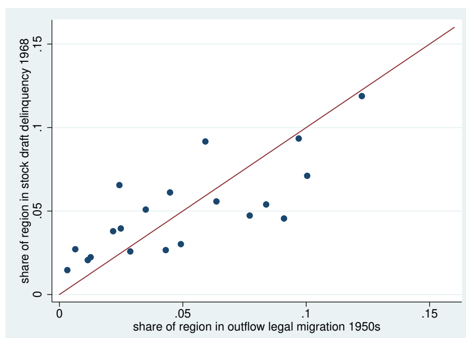
Source: Minnesota Population Center (2015); Portugal, INE (2017b). Note: The emigration rate for each cohort is defined as the number of Portuguese male immigrants observed in the French Census divided by the respective number of births in Portugal.

Figure 5: 1950s emigration rates and region characteristics.



Sources: INE (1952), Baganha (1994), and IPUMS-International (Minnesota Population Center, 2015).

Figure 6: Regional emigration and draft delinquency.



Sources: Own computations based on Portugal, EME (1968) and Baganha (1994).

Table 1: TIMELINE OF THE ANALYSIS, DATA SOURCES, AND AGE OF BIRTH COHORTS.

	Year				
	1950s	1961	1974	1975	1981
			A. National events		
	Before the war	War in Africa begins	War ends		Outcomes
			*B. Age of birth cohorts**		
Cohort					
1937	12–21	23 (first cohort of fatalities)	36	37	43
1940	9–18	20 (start military service)	33	34	40
1941	8-17	19 (military registration)	32	33	39
1948	1–10	12	25	26	32
1951	-2–7†	9	22	23	29
1954	-5–4†	6	19 (last cohort of fatalities)	20	26
			C. Data sources		
PT emigration data	Baganha (1994) & INE				
FR immigration data				FR Census	
PT outcome data					PT Census & QP**

Notes: The 1937 and 1954 cohorts were, respectively, the first and last cohorts to suffer a war fatality among soldiers, corporals or non-commissioned sergeants (according to our own computations based on TerraWeb (2016)). Law 2135/1968, enforced starting in 1969, anticipated the registration and medical assessment to age 18. *We infer the birth cohort as 1981-age-1 since the Portuguese Census reports age as of March 16, 1981, and, therefore, the majority of individuals had not yet reached their birthday. See the text and Online Appendix B for further details. † "-2" and "-5" indicate that the 1951 and 1954 cohorts were not born yet in the early 1950s. ** We use the 1985 QP data (its first available year).

Table 2: Descriptive statistics.

Variable	Mean	Std. Dev.
Age	34.20	5.22
Sex ratio at birth (R^c_{rc})	1.07	0.02
Sex ratio in 1981 Census ($R^c_{r,81}$)	0.94	0.13
Female ever married rate	0.89	0.05
Average number of own children in household	1.79	0.52
Female labor force participation (FLFP) rate	0.54	0.14
Male labor force participation rate	0.97	0.02
Female (ln) real hourly wage	0.98	0.20
Male (ln) real hourly wage	1.23	0.22
Real (ln) hourly wage gap ($\ln w^m - \ln w^f$)	0.25	0.09
Female share among top occupations*	0.39	0.20
Female average education years	4.23	1.43
Average education years of employed females (QP)	4.64	1.08
Average education years of employed females (Census)	5.30	1.52
Male average education years	5.17	1.08
Average education years of employed males (QP)	5.04	0.89
Average education years of employed males (Census)	5.22	1.06
Emigration rate (FR 1975 Census)†	0.06	0.05
Gender gap in emigration rate (FR 1975 Census)†	0.01	0.01
N	360	

Sources: Own computations based on Minnesota Population Center (2015), Portugal, INE (2017b), and Portugal, MTSS (2005). Note: Refers to cohorts born 1937-1954, observed in 1981. Computed on cohort-region cells, using as weights the female population in the cell. *We use 356 observations to compute the average female share among top occupations since four cohort-region cells have no observations (female or male) in top occupations. †The aggregate emigration measures used to estimate the emigration rates are computed following a 'leave-own-out' procedure (see Section 4 for details). The gender gap in emigration is the rate of male emigration minus the rate of female emigration.

Table 3: Sex ratio, female labor force participation, and marriage rate.

	OLS own cohort	IV own cohort	IV own cohort +-1 year	IV own cohort +-2 years	IV own cohort +-3 years
	(1)	(2)	(3)	(4)	(5)
			A. Female participation		
ln Sex Ratio	-0.044	-0.390***	-0.492***	-0.457***	-0.515***
	(0.026)	(0.114)	(0.146)	(0.175)	(0.190)
ln Pop	0.143***	0.275**	0.308***	0.278**	0.310***
	(0.048)	(0.126)	(0.108)	(0.113)	(0.119)
First-stage Sanderson-Windmeijer F-stats					
ln Sex Ratio		29.96	20.69	17.13	11.02
ln Pop		10.45	12.69	12.41	13.56
			B. (Ever) Marriage rate		
ln Sex Ratio	0.010	0.034	0.046	0.052	0.046
	(0.010)	(0.050)	(0.061)	(0.066)	(0.074)
ln Pop	-0.012	-0.051	-0.042	-0.046	-0.037
	(0.015)	(0.032)	(0.033)	(0.036)	(0.039)
First-stage Sanderson-Windmeijer F-stats					
ln Sex Ratio		29.96	20.69	17.13	11.02
ln Pop		10.45	12.69	12.41	13.56
N	360	360	360	360	360

Notes: Under "+- j years", all explanatory variables for cohort c are computed using cohorts c-j to c+j. We treat as endogenous: the sex ratio and the population size. Instruments: gender composition of emigration and rate of emigration (both excluding own region). The gender composition of emigration is the rate of male emigration minus the rate of female emigration, either one over the respective birth cohort. The rate of emigration is the number of emigrants over the respective birth cohort. All regressions control for the average number of years of education as well as region and cohort fixed effects. The cell-size analytic weight is the number of females in the population. Standard-errors clustered at the region level, in parenthesis. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 4: Distribution of active population by occupations, separately by gender, Portugal, Census 1960 and 1981.

IPUMS Standardized Occupation	Occup. Type	Year 1960	Year 1981
	(1)	(2)	(3)
A. Distribution of females by occupation (%)			
Legislators, senior officials, managers	blue	0.4	4.3
Professionals	pink	7.6	10.4
Clerks	mixed	6.1	13.4
Service workers and shop and market sales	pink	39.4	27.3
Skilled agricultural and fishery workers	blue	17.6	20.0
Crafts and related trades workers	mixed	28.4	22.8
Plant and machine operat. and assemblers	blue	0.2	1.1
Elementary occupations	blue	0.2	0.8
Armed forces	blue	0.0	0.0
B. Distribution of males by occupation (%)			
Legislators, senior officials, managers	blue	1.7	4.4
Professionals	pink	1.9	5.4
Clerks	mixed	4.8	8.6
Service workers and shop and market sales	pink	10.8	14.2
Skilled agricultural and fishery workers	blue	49.4	17.4
Crafts and related trades workers	mixed	27.1	38.8
Plant and machine operat. and assemblers	blue	3.1	6.3
Elementary occupations	blue	0.5	4.0
Armed forces	blue	0.7	0.8
C. Relative incidence, M / F (panel B / A)			
Legislators, senior officials, managers	blue	3.88	1.04
Professionals	pink	0.25	0.52
Clerks	mixed	0.79	0.64
Service workers and shop and market sales	pink	0.28	0.52
Skilled agricultural and fishery workers	blue	2.81	0.87
Crafts and related trades workers	mixed	0.95	1.70
Plant and machine operat. and assemblers	blue	12.90	5.75
Elementary occupations	blue	2.31	4.99
Armed forces	blue	.	176.44

Sources: Distribution of active population by occupation and gender in 1960 - Portugal, INE (2017a); conversion of occupational classification ISCO-58 used in census 1960 into IPUMS standardized occupation classification - International Labour Organization (1969) and Minnesota Population Center (2015); 1981 occupational distribution - Minnesota Population Center (2015). Note: Full data set, without any cohort or age constraints imposed.

Table 5: Sex ratios and occupational allocation.

	OLS own cohort	IV own cohort	IV own cohort +-1 year	IV own cohort +-2 years	IV own cohort +-3 years
	(1)	(2)	(3)	(4)	(5)
Female share within top occupations (managers, senior officials, legislators)					
ln Sex Ratio	-0.162**	-0.405*	-0.550**	-0.502*	-0.552
	(0.067)	(0.210)	(0.270)	(0.290)	(0.342)
ln Emp	-0.019	0.119	0.146	0.118	0.146
	(0.108)	(0.207)	(0.205)	(0.203)	(0.203)
Education Gap M-F	-0.036*	-0.042*	-0.039	-0.041*	-0.040*
	(0.018)	(0.025)	(0.026)	(0.024)	(0.024)
First-stage Sanderson-Windmeijer F-stats					
ln Sex Ratio		36.46	26.23	25.97	24.24
ln Emp		10.90	11.51	12.07	13.78
Education Gap		156.5	248.6	256.4	278.0
N	356	356	356	356	356

Notes: Under "+- j years", all explanatory variables for cohort c are computed using cohorts c-j to c+j. We treat as endogenous: the sex ratio, the employment size, and the gender education gap in employment. Instruments: gender composition of emigration and rate of emigration, both excluding own region, and education gender gap in the population. The gender composition of emigration is the rate of male emigration minus the rate of female emigration, either one over the respective birth cohort. The rate of emigration is the number of emigrants over the respective birth cohort. The education gap is the average years of education of males minus that of females. All regressions include sets of dummy variables for cohort and region. The cell-size analytic weight is the level of employment. Four region-cohort cells have no employment in the top occupation, thus explaining the slight reduction in the sample size. Standard-errors clustered at the region level, in parenthesis. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 6: Sex ratios and the gender pay gap.

	OLS own cohort	IV own cohort	IV own cohort +-1 year	IV own cohort +-2 years	IV own cohort +-3 years
	(1)	(2)	(3)	(4)	(5)
		ln hourly wages			
ln Sex Ratio	0.047**	0.167	-0.009	-0.046	-0.092
	(0.023)	(0.127)	(0.243)	(0.266)	(0.351)
ln Sex Ratio × Female	-0.043	-0.267	-0.268	-0.331*	-0.405*
	(0.040)	(0.190)	(0.185)	(0.199)	(0.219)
ln Emp	0.089***	0.155***	0.206**	0.227**	0.257**
	(0.031)	(0.059)	(0.090)	(0.091)	(0.121)
Educ	0.161***	0.257***	0.263***	0.266***	0.271***
	(0.019)	(0.037)	(0.035)	(0.032)	(0.030)
Educ × Female	-0.012**	-0.029***	-0.030***	-0.030***	-0.030***
	(0.006)	(0.009)	(0.009)	(0.009)	(0.009)
Female	-0.127***	-0.017	-0.008	-0.010	-0.010
	(0.036)	(0.052)	(0.054)	(0.053)	(0.054)
First-stage Sanderson-Windmeijer F-stats					
ln Sex Ratio		88.19	76.74	43.00	28.39
ln Sex Ratio × Female		35.07	29.14	24.95	19.74
ln Emp		43.06	52.88	39.56	19.90
Educ		34.09	42.91	37.22	32.09
Educ × Female		71.88	96.99	95.89	82.36
N	720	720	720	720	720

Notes: Under "+- j years", all explanatory variables for cohort c are computed using cohorts c-j to c+j. We treat as endogenous: the sex ratio, the employment size, and the education of employed workers in each cell. Instruments: gender composition of emigration and rate of emigration (both excluding own region), and education of the population in the cell. The gender composition of emigration is the rate of male emigration minus the rate of female emigration, either one over the respective birth cohort. The rate of emigration is the number of emigrants over the respective birth cohort. All regressions include sets of dummy variables for cohort and region. The cell-size analytic weight is the level of employment. Standard-errors clustered at the region level, in parenthesis. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 7: MILITARY RECRUITMENT IN THE METROPOLE (MAINLAND AND ISLANDS), 1961-1972.

	Registered	Medical assessment avoided						Assessments	Assessment results				
		Delinquency		Deferral		Volunteer			Called		Exempted	Deferred	Junta
	N	N	share	N	share	N	share	N	N	share	share	share	share
Year	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
1961	75,366	8,722	0.12	640	0.01	1,310	0.02	64,694	48,832	0.75	0.19	0.04	0.00
1962	79,357	10,211	0.13	753	0.01	1,445	0.02	66,948	57,073	0.85	0.09	0.05	0.01
1963	85,410	13,328	0.16	317	0.00	1,578	0.02	70,187	59,676	0.85	0.08	0.03	0.03
1964	86,977	14,357	0.17	980	0.01	2,375	0.03	69,265	61,249	0.88	0.08	0.03	0.00
1965	90,289	16,972	0.19	855	0.01	2,261	0.03	70,201	64,805	0.92	0.06	0.01	0.01
1966	87,506	16,008	0.18	650	0.01	2,475	0.03	68,373	63,342	0.93	0.05	0.01	0.02
1967	86,065	16,512	0.19	485	0.01	2,299	0.03	66,769	62,017	0.93	0.04	0.01	0.02
1968	95,634	17,838	0.19	220	0.00	2,026	0.02	75,550	70,504	0.93	0.04	0.01	0.02
1969			0.20										
1970	88,693	18,554	0.21						63,996				
1971	91,363	15,644	0.17						65,746				
1972	92,613	18,841	0.20						66,681				

Sources: Registered, draft delinquency and called - Portugal, EME (1988), p. 258; remaining information - Portugal, EME (1968), p. 8-13, appendix 2. Notes: We rely on Cann (1996) for the translation of the original terminology in Portuguese. Registered means listed in the military census the year the cohort is due for medical assessment. Draft delinquency refers to no-shows for medical assessment (see text for further details). Main reasons for medical assessment deferral: attendance of Catholic seminar; member of crew of cod fishing vessel. Volunteers were already serving, the overwhelming majority in the Navy or the Air Force. Reasons for exemption: unfit for service due to congenial or acquired malformations, or disease. Reasons for enrolment deferral: postponed incorporation due to incomplete physical development or sent to a Military Junta for further evaluation of progress. A further negligible share in 1961 and 1962 died between registration and assessment dates.

Table 8: Sex ratio, female labor force participation, and marriage rate (instrumental variables draft delinquency).

	OLS own cohort	IV own cohort
	(1)	(2)
	A. Female participation	
ln Sex Ratio	-0.239*	-0.634***
	(0.121)	(0.243)
ln Pop	-0.024	-0.038
	(0.045)	(0.077)
First-stage Sanderson-Windmeijer F-stats		
Sex Ratio		9.40
ln Pop		20.87
	B. (Ever) Marriage rate	
ln Sex Ratio	0.143**	0.286**
	(0.062)	(0.142)
ln Pop	-0.004	-0.012
	(0.038)	(0.046)
First-stage Sanderson-Windmeijer F-stats		
Sex Ratio		9.40
ln Pop		20.87
N	20	20

Sources: Minnesota Population Center (2015); Portugal, INE (2017b); Portugal, EME (1968). Notes: We treat as endogenous the sex ratio and the population size. Instruments: draft delinquency rate and share of birth cohort registered in the military census; the delinquency rate is the number of draft dodgers over the number of individuals in the military census; the registration rate is the number of individuals in the military census over the birth cohort, all referring to males born 1948. The cell-size analytic weight is the number of females in the population. Standard-errors clustered at the region level, in parenthesis. * significant at 10%; ** significant at 5%; *** significant at 1%.