

# Education and Consanguineous Marriage

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At least one of every five marriages is consanguineous (between couples who are second cousins or closer) in the Middle East and North Africa, and the rate is higher than 50% in some parts of the world. We find that a Turkish education reform that increased mandatory schooling by 3 years made women less likely to find consanguineous marriage an acceptable practice. The reform reduced women's propensity to marry a first cousin or a blood relative, and it altered women's preferences in favor of personal autonomy, indicating that educational attainment alters behaviors and attitudes that may be rooted in culture.

## I. Introduction

It has been estimated that more than 10% of the world's population is related as second cousins or closer (Bittles and Black 2010) and that more than 1 billion people in North Africa, the Middle East, and parts of Asia live in areas where at least 20% of marriages are consanguineous, which is defined as a union between couples who are second cousins or closer (Romeo and Bittles 2014).<sup>1</sup> The rate of consanguineous marriage is as high as 50% in some regions of the world (Tadmouri et al. 2009; Hamamy et al. 2011). As we describe in section II, the underlying cultural and religious factors are likely dominant forces that determine the prevalence

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<sup>1</sup> The word "consanguinity" is derived from the Latin words of *con* (common, or the same) and *sanguis* (blood).

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of consanguineous marriage in a country, although the change in this prevalence over time and across regions within a country suggests that other factors, including economic ones, are important as well.

Using data from Turkey, where 20% of marriages are consanguineous, we investigate the impact of education on individuals' propensity to marry their first cousins or to marry blood relatives, as well as education's influence on people's tendency to approve of such marriages. We leverage a Turkish education reform that went into effect in 1997. For political reasons, the reform was implemented very quickly and rather unexpectedly, and it increased the mandatory years of education from 5 to 8 years. The details of the reform are described in section III.

Our results show that the reform made women less likely to find consanguineous marriage an acceptable practice and that the reform reduced women's propensity to marry a first cousin or a blood relative. We argue that these behavioral changes are related to changes in women's preferences and to a rise in the extent of women's self-determination. We demonstrate that women who were exposed to the education reform are less likely to be in an arranged marriage and are less likely to have met their husbands through networks of their family, relatives, or neighbors. Importantly, we provide evidence that the reform reduced women's propensity for being in a marriage into which they were forced by their families. Put differently, the exposure to the reform reduced the tendency of women to get married without their consent.

We show that women who were treated by the education reform have not altered their preferences regarding their spouse's religiosity or desired compatibility with their husband's religious sect. This suggests that a change in religiosity, which could be generated by increased educational attainment, is not the driver of the results. Similarly, the reform has not altered women's propensity to marry outside of their geographical region, indicating that a potential change in the marriage pool due to moving to a new location after getting educated and meeting the spouse in that new location is not the mechanism behind the findings. Although it is difficult to pinpoint the exact mechanism, our results suggest that an increased level of educational attainment had an impact on women's preferences. Additional evidence on this point is provided by the fact that the reform made women significantly less likely to agree with the statement that "only a son can ensure the continuation of the family blood line." A number of sensitivity tests demonstrate the robustness of these results.

Although the reform increased men's years of schooling as well, it had no significant impact on men's preferences in these dimensions. We show that men's propensity for consanguineous marriage declined because women tend to marry men who are 4 years older, on average, and as a result, some men who missed the reform are nevertheless affected by it indirectly, through the change in women's behavior.

We make contributions to several areas of investigation. First, we add to a growing body of research on nonpecuniary benefits of educational

attainment. While there is a large literature concentrating on private returns to education (Card 2001; Oreopoulos 2007), nonpecuniary benefits of education and the associated social returns have not been investigated as extensively.<sup>2</sup>

Within this literature, we contribute to a strand of research that focuses on the evolution of cultural traits and specifically to research that focuses on the influence of education on the formation of beliefs and preferences (Glaeser and Sacerdote 2008; Becker, Nagler, and Woessmann 2017; Mocan and Pogorelova 2017). Education can also alter preferences and attitudes that directly affect women's well-being. There is evidence that educational attainment makes women more intolerant of practices that are harmful to their well-being and that education makes individuals more likely to challenge authority (Friedman et al. 2016; Cannonier and Mocan 2018). We add to this literature by investigating the extent to which education affects preferences for and the practices of consanguineous marriage. In many societies around the world, the practice of consanguineous marriage is part of the fabric of culture. Nevertheless, our results reveal that the proclivity to approve this practice and the propensity to be actually in a consanguineous marriage are malleable and that these tendencies are influenced by women's educational attainment.

Second, taken together, our results may be considered as evidence of the empowerment effect of female education. Education can empower women through a number of direct channels, such as the impact on labor market opportunities and wages, and access to leadership positions in society (Duflo 2012). Education can also empower women by altering their preferences and attitudes regarding issues that affect their well-being, which can ultimately influence their decision-making.<sup>3</sup> Under the assumption that consanguineous marriage is not the optimal marital decision for most women, education empowers them as it becomes a conduit through which women switch away from such marriages.

Edlund (2018) argues that cousin marriage is not a voluntary choice for women. This implies that in societies with high incidence of blood-relative marriage, women's marriage preferences are irrelevant and that most women who marry a blood relative do so under the complete influence of their families. If this is the case, an increase in women's education cannot affect marriage outcomes through an alteration of women's preferences, because these preferences are ignored by the family. Put differently, if the family is the decision-maker on behalf of the daughter and if the increase in women's (daughters') education generates a decrease in the proclivity for cousin marriage, this may not be attributable to a

<sup>2</sup> Examples of this line of inquiry include the impact of education on civic participation (Dee 2004; Milligan, Moretti, and Oreopoulos 2004), on criminal proclivity (Machin, Marie, and Vujić 2011; Hjalmarsson, Holmlund, and Lindquist 2015), and on the production of health (Lleras-Muney 2005; Chou et al. 2010).

<sup>3</sup> An example is the impact of education on fertility preference and fertility behavior of women (Osili and Long 2008; Keats 2018).

change in preferences. Rather, it may be the result of a number of factors, such as daughters' improved labor market opportunities, family's geographic mobility triggered by enhanced education, or something else that influences family's behavior. We could not find empirical support for channels such as these that could lend themselves to investigation.

The argument that cousin marriages are imposed on women by their families is unlikely fully applicable in the context of Turkey because of the specific legal and cultural environment of the country. To the extent that Turkish women (at least some Turkish women) are allowed to freely choose their husbands from the pool of all available candidates, the finding that education makes women less likely to pick a blood relative for a husband can be considered as evidence for (albeit, not conclusively) women's changing preferences.

In addition, we find that the education reform reduced women's propensity to get married against their free will and that the reform changed women's beliefs that only a son can ensure the continuation of the family blood line. These findings provide further evidence for a significant degree of empowerment of women, generated by the education reform.

Third, our results are important for research on the determinants of infant health. A large literature in economics has demonstrated that infant health is influenced by in utero exposure to environmental hazards.<sup>4</sup> Similar to the deleterious effects of these environmental hazards, medical literature has documented substantial health risks to the offspring of consanguineous marriages, including premature birth, low birth weight, infant mortality, and serious congenital diseases. These effects, however, are larger in consanguineous marriage, in comparison to those generated by environmental hazards. We provide the details in the online appendix.

While the negative long-run impact of some undesirable birth outcomes, such as low birth weight, can be counteracted by parental investments in childhood (Heckman 2006; Cunha and Heckman 2008), permanent health problems such as congenital heart failure and cerebral palsy, which are prevalent in children of consanguineous parents, are very difficult and costly to counterbalance with *ex post* interventions. This means that the reduction in consanguineous marriage generated by an increase in education can be a vehicle through which improvements in child health can be achieved. This is especially true in developing countries, where high rates of consanguinity, low education, and public health problems related to high infant and child mortality and morbidity coexist

<sup>4</sup> For example, fetal exposure to air pollution is shown to cause infant mortality (Chay and Greenstone 2003; Currie and Neidell 2005; Greenstone and Hanna 2014). Almond, Edlund, and Palme (2009) find that in utero exposure to radiation lowers school performance of children and reduces future earnings, likely by hindering cognitive development. Along the same lines, Sanders (2012) shows that exposure to ambient pollution in utero has a detrimental effect on test scores in high school. Low birth weight, which is a significant predictor of future health and educational attainment (Currie 2009; Bharadwaj, Lundborg, and Rooth 2018), is affected by pregnant women's exposure to motor vehicle emissions (Currie and Walker 2011) or toxic air emissions from industrial plants (Currie et al. 2015).

but public policy attempts to discourage such marriages are not pursued because consanguineous marriage is considered to be a product of tradition and religion.<sup>5</sup>

The results are also potentially important for economic development. This is because human capital is an ingredient of development (Lucas 1988; Ehrlich and Pei 2020; see Hanushek and Woessmann 2020 for a review of this literature). Improvement in child health due to reduced incidence of consanguineous marriage, caused by increased educational attainment, is expected to have a positive impact on human capital and economic development.<sup>6</sup> In addition, family formation and sorting of individuals into families can have an impact on economic development through a number of mechanisms (Fernández and Rogerson 2001; Fernández 2003). Greif (2006) and Greif and Tabellini (2017) argue that the formation of the state as an institution and the evolution of its organizational structure depend, among other things, on loyalty to kinship networks and the extent of kin-based clan organizations. These authors postulate that marriage laws and practices that restricted consanguineous marriage, instituted by the medieval Church in Europe, undermined kinship groups. This move eventually led to the emergence of nuclear families and gave rise to corporation-based institutions and economic growth in Europe.<sup>7</sup>

The rest of the paper is organized as follows. Section II provides the historical background of consanguineous marriage and the health risks associated with such marriages. Section III describes the Turkish education reform details, and section IV presents the conceptual framework and empirical implementation. Section V describes the data, and section VI

<sup>5</sup> It is well established that more educated individuals are better producers of health because education yields to improvement in allocative or productive efficiency (Grossman 1972, 2006; Rosenzweig and Schultz 1982) and that an increase in maternal education improves child health (Chou et al. 2010; Grépin and Bharadwaj 2015; Makate and Makate 2016). Our results, however, underscore education as a policy lever to improve child health through a different preventative mechanism.

<sup>6</sup> At the macro level, Diebolt and Perrin (2013) add to the unified growth theory of the role played by women and gender equality, arguing that women's empowerment is a key factor of economic development. To the extent that education empowers women, education has this indirect effect on development as well.

<sup>7</sup> De La Croix, Doepke, and Mokyr (2018) argue that a shift from closed kinship systems toward a system where knowledge is transmitted across individuals helped Europe to progress economically, in comparison to regions dominated by extended families or clans. Ghosh, Hwang, and Squires (2020) demonstrate the long-term economic consequences of legally banning cousin marriages in the United States. Along the same lines, Schulz et al. (2019) argue that the movement away from kinship-based institutions led to independent and isolated nuclear or stem families, which generated personal traits such as individualism, nonconformity, and trust. Schulz (2020) shows that the prohibition of blood-relative marriages has led to the formation of self-governed cities with political structures that were precursors for parliaments. Kinship networks and nepotism, fostered by consanguineous marriages, can lead to a culture of corruption (Akbari, Bahrami-Rad, and Kimbrough 2019). Thus, education-triggered modifications in the way people sort themselves into families can have long-term cultural and macroeconomic implications.

presents the results and the robustness analyses. Section VII provides a summary and discussion.

## II. Consanguineous Marriage

A consanguineous marriage is defined as a union between two people who are second cousins or closer.<sup>8</sup> There is substantial variation between countries in the prevalence of such marriages, with rates ranging from 0.2% in the United States to 3.9% in Japan; between 10% and 40% in many counties in the Middle East, North Africa, and West Asia; and reaching 45%–50% in parts of China, Afghanistan, India, Saudi Arabia, and Syria (Hamamy et al. 2011; Global Consanguinity website [<https://consang.net>]).

Although consanguinity is more widespread among countries with majority-Muslim populations, it is not confined to Muslim nations, nor is it an exclusive product of Islam. For instance, as detailed in Bittles (1998) and Bittles and Black (2010), the rules of marriage in Islam are similar to those in the Judaic instructions stated in Leviticus 18:7–18, even though there are differences. As an example, uncle-niece marriages are forbidden in Islam, whereas they are allowed in Judaism. Buddhism permits first-cousin marriages, while the rules are more complex in Hinduism, where the interplay between religion and local customs has generated substantial heterogeneity in regulations (Bittles and Black 2010).<sup>9</sup> Close-kin marriages are permissible for Roman Catholics, with the proviso that couples receive diocesan dispensation. The Protestant Reformation accepted the guidelines in Leviticus 18:7–18, which make first-cousin marriages permissible for Protestants (Goody 1983).<sup>10</sup>

Until the early twentieth century, the practice of first-cousin marriage was accepted in Europe, and it was frequently exercised, especially among the elites.<sup>11</sup> Sabean, Teuscher, and Mathieu (2007) point out that despite differences in religious denominations between European countries, the prevalence of first-cousin marriage rose markedly everywhere in Europe

<sup>8</sup> This corresponds to an inbreeding coefficient ( $F$ ) equal to 0.0156 or higher. This, in turn, signifies that parental couples share 1/32 of their genes inherited from common ancestors and that their offspring has a probability of 1/64 (or 0.0156) of having identical gene copies of all loci, inherited from both parents. In cases of first-cousin marriage,  $F$  goes up 0.0625, and in case of uncle-niece marriages practiced in South India,  $F$  becomes 0.125 (Hamamy 2012).

<sup>9</sup> As detailed in Bittles (1998), the Aryab Hindus of northern India analyze seven generations of the groom's family and five generations of the bride's family before a consanguineous marriage is permitted. On the other hand, marriages between first cousins are favored and uncle-niece marriages are prevalent among the Dravidian Hindus in southern India.

<sup>10</sup> Orthodox churches, on the other hand, do not allow for consanguineous marriage.

<sup>11</sup> For example, Queen Victoria of England married Prince Albert, her mother's brother's son. Kuper (2002, 166) states that "[Her marriage to her cousin] was and remains a Hanoverian tradition. George I had married his father's brother's daughter, and George IV his father's sister's daughter. George V and Elizabeth II carried on this tradition, both marrying second cousins."

in the eighteenth century and has declined rapidly since the 1920s. The rise and decline of consanguinity in Europe over the past 3 centuries and its current uneven distribution between regions, as well as the variation in the prevalence of consanguinity between locations within countries, underline the importance of regional traditions and socioeconomic variables in determining the propensity for consanguineous marriage.<sup>12</sup> Similarly, that consanguineous marriage has persisted over centuries and that it is still widespread in many parts of the world (despite its detrimental effects on the offspring and the related costs for both the offspring and the family) suggest that consanguinity is associated with benefits to families.<sup>13</sup> Thus, the determinants of the formation of consanguineous marriages and endogamy (marriage within the local community) have recently been investigated as a rational decision made by individuals, given the economic and institutional constraints (Do, Iyer, and Joshi 2013; Mobarak, Kuhn, and Peters 2013; Dow, Reed, and Woodcock 2016; Bahrami-Rad 2021).<sup>14</sup>

### III. The 1997 Education Reform

In August 1997, a newly formed government in Turkey passed a law to increase mandatory education from 5 to 8 years (Law No. 4306). Before the enactment of this law, students had to complete 5 years of elementary school education, but attendance at middle school (grades 6–8) was voluntary. The reform combined elementary and middle school education and required all students, who were covered by the mandate of the law, to obtain a middle school diploma. Students who had completed the fourth grade or lower in the summer of 1997 had to comply with the new law and had to continue their education until they had completed 8 years of schooling, while those who had already completed the fifth grade in summer 1997 were exempt.

Although most children who were born in 1986 would have been enrolled in the first grade in 1992 and would have completed the fifth grade

<sup>12</sup> See the Global Consanguinity website (<https://consang.net>) for detailed information on the variation in consanguinity rates within counties.

<sup>13</sup> For example, Johow, Willführ, and Volland (2019) show that although marriages between cousins were very rare among the landless population in the eighteenth and nineteenth centuries in Krummhörn, Germany, the rate of cousin marriage was 5%–10% among large-scale farmers and that consanguineous marriages were a wealth retention/consolidation strategy, as consanguinity is found to be associated with increased intergenerational transmission of land holdings. The persistence of consanguineous marriage in developing nations may signal the existence of economic benefits generated by such marriages in the form of wealth accumulation and consolidation and network creation. If the returns to nepotism and in-group favoritism are nontrivial in the society, this can help preserve in-marriage as a social norm (Akbari, Bahrami-Rad, and Kimbrough 2019).

<sup>14</sup> More generally, economists have investigated, theoretically and empirically, the determinants of marriage practices (Edlund 1999; Gould, Moav, and Simhon 2008; Fenske 2015), dowry payments and bride price (Botticini and Siow 2003; Ambrus, Field, and Torero 2010; Ashraf et al. 2020), and the decisions surrounding inheritance (Chu 1991; Bernheim and Severinov 2003; La Ferrara 2007).



in summer 1997, barring grade repetition, some children who were born in the same year would have completed only the fourth grade in 1997 and therefore would have been affected by the reform. This is because of the imperfect compliance with the law that regulates school starting age in Turkey (Dinçer, Kaushal, and Grossman 2014; Cesur and Mocan 2018; Kırdar, Dayioğlu, and Koç 2018).<sup>15</sup> In addition, some families whose children were exempt from the law (those born in 1986 who had completed the fifth grade in 1997) may have decided to send their children to school for an additional 3 years to obtain the middle school diploma, to prevent them from being in a disadvantaged position in comparison to the immediately younger cohorts.<sup>16</sup> Thus, the extent to which the reform affected the cohort of 1986 is unclear.

The speed with which the law was passed was because of domestic and international politics. In 1997, Turkey was engaged in negotiations for membership in the European Union (EU), and increasing the mandatory years of education was considered the right move to increase the chances of EU membership (Dulger 2004). The law was also an attempt to limit religious education in the country.<sup>17</sup> Specifically, before the reform students who had completed 5 years of mandatory education had three options: (1) discontinue their education, (2) go on to secondary education at a traditional middle school for 3 years, and (3) go on to secondary education at a vocational school, a category that included religious schools designed to train clerics to be employed at religious organizations, including mosques. By merging the 5-year elementary education and the 3-year secondary education and making it mandatory to go to school for 8 years, the reform eliminated the stand-alone vocational middle schools. Vocational high schools, including religious schools, could admit students only after the eighth grade. It should be noted, however, that enrollment in religious vocational middle schools was 315,000 of the 2.6 million students of the same grade level (12%). Most students in these religious vocational middle schools were boys, because these schools were primarily designed as feeders into religious high schools, which were mainly created to

<sup>15</sup> The law that regulates the school starting age in Turkey states that a child may start the first grade in the fall if she is 72 months old at the end of that calendar year. This implies that children born in 1986, especially those born toward the beginning of that year, could have started school in 1992. It is well known, however, that the age cutoff is loosely enforced and that children could start primary school in the 69–80-month range (Gün and Başkan 2014). Thus, those who were born in early 1986 could start the first grade in fall 1991 rather than fall 1992. Similarly, those born in late 1986 could start school in 1992, rather than in 1991. See the appendix for the problem of using the month of birth to determine exposure to the law.

<sup>16</sup> Therefore, we follow the previous research and exclude those born in 1986 from the benchmark model (Battistin et al. 2009; Fort, Schneeweis, and Winter-Ebmer 2016; Mocan and Pogorelova 2017; Cesur and Mocan 2018; Kırdar, Dayioğlu, and Koç 2018), although, as we show, including them does not alter the results.

<sup>17</sup> More specifically, a previous government, which had a religious bent, resigned in June 1997, and the new secular government passed the education reform law on August 18, 1997. Details of this point and the political landscape in Turkey in 1997 can be found in Cesur and Mocan (2013, 80–83, 2018).



train clerics to be employed by mosques and other religious enterprises, but only men can be religious clerics in Islam. It is also important to note that these religious vocational middle schools (Imam-Hatip schools) were not based on a full religious curriculum. Rather, their curriculum consisted of standard middle school curriculum supplemented with religious material. As a result, such students were eligible to qualify to any field of study at a university, with the proviso that they did well at the university entrance exam (Aydemir and Kırdar 2017).

The reform did not involve any modifications to the curriculum; that is, neither the content nor the composition of courses was affected by the reform (Dulger 2004).<sup>18</sup> Compulsory education is free in Turkey. Thus, the reform did not involve any change in the cost of education for families. Although noncompliance is subject to fines, compliance is not strictly enforced. As a result, although the proportion with at least a middle school diploma rose above 90%, full compliance has not been achieved. Additional details regarding increased enrollment, employment of new teachers, and so on can be found in Kırdar, Dayıoğlu, and Koç (2015, 2018).<sup>19</sup>

#### IV. Conceptual Framework and Empirical Implementation

Following the discussion in the introduction, an individual's propensity for consanguineous marriage ( $M$ ) in equation (1) is affected by personal characteristics such as age and gender ( $X$ ) and malleable traits such as religiosity and political views ( $R$ ), as well as labor market activity and relevant market wages ( $W$ ). Local cultural traditions ( $C$ ) also influence the proclivity for consanguineous marriage. Alternatively,  $M$  represents various marriage characteristics, such as age at first marriage and the manner in which the individual met his/her spouse. In addition,  $M$  characterizes individuals' preferences for several attributes of a spouse and of marriage, such as

<sup>18</sup> The Ministry of Education incorporated a number of changes to increase enrollment, including hiring additional teachers, adding new classrooms to existing schools, and starting a bus system to transport students from rural localities to urban schools, as well as system of free lunches and books for low-income children (MoNE 2001; Dulger 2004). The capacity of boarding schools was also expanded to facilitate the enrollment of rural children in urban areas (Dulger 2004; World Bank 2005).

<sup>19</sup> There was another education reform in Turkey in 2012. This reform does not lend itself to an analysis as does the 1997 reform. This is because the 2012 reform generated a multitude of changes simultaneously, the joint impacts of which are unclear theoretically. For example, while the 2012 reform increased the mandatory years of education to 12 years (high school education), it also allowed students to start religious education after the fourth grade. More specifically, the 2012 reform created a new system, termed 4 + 4 + 4, where the first 4 represents primary education, the second 4 stand for middle school, and the last 4 indicates high school. Students can switch schools between these blocks (e.g., switching to a vocational school). Furthermore, the 2012 reform changed the structure and curriculum of some public schools. (see Gün and Başkan 2014 for details). Even if the 2012 reform did not have ambiguity as to its expected impact, it is not usable in our case, because our data set is based on a survey conducted in 2016. The 2012 reform affected the eighth graders who were 15 years old in 2012. Thus, the individuals who were exposed to the 2012 reform are 19 years old or younger in our data set, and they do not constitute a useful sample for our purposes.

the desirability of an educated spouse, the importance of shared religious beliefs between husband and wife, and the acceptability of the practice of marrying a blood relative. Individuals' educational attainment (*Educ*) can directly influence *M* through a number of channels, including by altering time discounting (Becker and Mulligan 1997; Perez-Arce 2017), by influencing risk aversion via the rise in cognition (Harrison, Lau, and Rutström 2007; Dohmen et al. 2010), and by altering attitudes toward matters that are related to women's well-being (Cannonier and Mocan 2018).

$$M = f_1(X, R, W, C, \text{Educ}). \quad (1)$$

Equation (2) indicates that educational attainment is a function of personal characteristics of the individual (*X*) and religiosity and political views (*R*), as well as cultural attributes (*C*). Educational attainment is also influenced by exposure to the reform (*T*), because those who were treated by the reform were required to obtain additional years of education.

$$\text{Educ} = f_1(X, R, C, T). \quad (2)$$

Because an individual's educational attainment is endogenous, estimation of equation (1) using ordinary least squares would produce a biased estimate of the impact of education on consanguineous marriage.<sup>20</sup> The education reform leveraged in this paper mandated some students to receive three additional years of schooling, while it exempted other students from this requirement (on the basis of the grade the students had completed when the law was passed). Thus, exposure to *T* in equation (2) is exogenous, and therefore the reform can, in principle, be used as an instrument for educational attainment in an effort to estimate equation (1) (Cesur and Mocan 2018; Dursun, Cesur, and Mocan 2018; Aydemir, Kırdar, and Torun 2020). This instrumental variables procedure, however, is not advisable in this particular context because of likely violation of the exclusion restriction. This is because previous research, using a variety of data sets, has shown that exposure to this reform in Turkey had a significant impact on the educational attainment of both women and men (Mocan 2014; Cesur and Mocan 2018; Kırdar, Dayıoğlu, and Koç 2018; Torun 2018; Akyol and Kırdar 2020). We confirm this finding in our data as well. That the reform influenced the educational attainment of both sexes prevents us from using this instrumental variables strategy because our outcome variables are related to various aspects of marriage. More specifically, an increase in education, triggered by the reform, may have an impact on women's propensity to marry by altering their preferences

<sup>20</sup> For example, risk aversion, which is shown to be related to educational attainment (Harrison, Lau, and Rutström 2007), can also affect the propensity for consanguineous marriage. Similarly, other difficult-to-observe personal attributes (e.g., cognitive ability) or family attributes (e.g., the family's attitudes toward female education and women's role in the society) can influence both *M* and *Educ* in eqq. (1) and (2). More specifically, unobservable factors that influence an individual's educational attainment can be correlated with her proclivity for consanguineous marriage.

or by changing the costs and benefits of marriage. But, by the same token, the same observed change in the propensity to marry can be the result of the change in the behavior or the preferences of the opposite sex, because the same education reform increased men's education as well. Thus, exposure to the reform, although exogenous, cannot be convincingly used as an instrument for female (male) educational attainment because the instrument, education reform, can plausibly affect female (male) marriage outcomes indirectly, through its impact on male (female) education.<sup>21</sup> We therefore focus on estimating the reduced-form impact of the reform, depicted by equation (3) below.

Two additional considerations are important. First, education can have an indirect effect on marriage decisions and marital preferences ( $M$ ) through its impact on personal attributes such as religiosity and political views ( $R$ ); see Becker, Nagler, and Woessmann (2017); Mocan and Pogorelova (2017); and Cesur and Mocan (2018). Second, it has been well established that education affects labor force participation and wages.<sup>22</sup> This means that vectors  $R$  and  $W$  are functions of education, which in turn implies that the reduced-form equation (3) excludes these components.

$$M = f_4(X, C, T). \quad (3)$$

We focus on equation (3) to investigate the impact of the reform on consanguineous marriage and related outcomes and preferences. The empirical counterpart of this formulation is presented by equation (4), which portrays the specific models estimated in the paper:

$$M_i = \alpha_0 + \alpha_2 T_i + \alpha_3 T_i \times (\text{YOB}_i - 1986) + \alpha_4 (1 - T_i) \times (\text{YOB}_i - 1986) + \alpha_5 \text{FE}'_i + \varepsilon_i. \quad (4)$$

The dependent variable  $M$  in equation (4) is an indicator of whether the individual is married to a first cousin. Alternatively,  $M$  stands for other related outcomes, such as whether the person is married to a blood relative, age at first marriage, the age difference between husband and wife, whether the person is in an arranged marriage, whether the person was forced into marriage against her own free will, and whether the person believes that spouse's educational attainment, spouse's political views, and compatibility of religious views are important aspects of a marriage. Using the same specification, we also investigate whether education reform had an impact on the extent to which the individual finds consanguineous marriage acceptable. Along the same lines, we analyze whether married individuals met their spouses through family or neighbors or through other avenues such as friend networks, workplace connections, the internet, and so on, and we investigate the extent to which the person agrees with

<sup>21</sup> See Kırdar, Dayıoğlu, and Koç (2018) and Akyol and Kırdar (2020), who make same argument.

<sup>22</sup> See Card (1999) and Meghir and Rivkin (2011) for excellent reviews.

the statement that only a son can ensure the continuation of the family blood line. Finally, we use the first principal component of these variables as a composite measure and as a dependent variable.

The variable  $T_i$  is binary, equal to 1 if the individual was treated by the reform, that is, was born after 1986. As described in section III, the extent to which the cohort of 1986 has been affected by the reform is unclear. Thus, following the approach of Battistin et al. (2009), Fort, Schneeweis, and Winter-Ebmer (2016), Mocan and Pogorelova (2017), and Cesur and Mocan (2018), this particular cohort is excluded from the main analysis, although adding it to the sample does not influence the results. Our main analysis sample includes individuals who were born within 8 years before or after the pivotal cohort of 1986. This choice of bandwidth for the benchmark models was based on the procedure of Imbens and Kalyanaraman (2012). We show in section VI.A that the results are not sensitive to the increase or decrease in the size of this window.

The variable YOB represents the year of birth. Thus, the model accounts for potentially differential trends in the outcomes of both the treatment and control cohorts. Following Lee and Lemieux (2010), we also estimated the models with a global quadratic trend, which did not influence the results. Local cultural characteristics are accounted for by a set of fixed effects (FE in eq. [4]). They include region-of-current-residence indicators, region-of-childhood fixed effects, an indicator of whether the individual spent his/her childhood years (up to age 15) in a village, and the interaction between the indicators of childhood region and growing up in a village.<sup>23</sup> These variables intend to capture the impact of economic factors and the extent of religiosity and traditional cultural values at the local level, including the prevalence of inbreeding in the geographic region in which the individual resides, as well as exposure to such values while growing up in the childhood region.<sup>24</sup>

Although region-of-residence and region-of-childhood dummies account for differences in customs related to marriage practices between regions, variations in personal heritage are also important. For example, minority populations in Turkey that speak Kurdish or Arabic at home have arguably different customs and traditions than those that speak Turkish. The survey does not contain a question about the primary language spoken at home. However, it contains a question about whether the survey respondent speaks a second language. The ability to speak a second language may represent different traits, depending on the region of residence. For example, in eastern Turkey, two languages being spoken may imply Turkish and Kurdish, whereas speaking two languages in the western part of the country may mean Turkish and English. To provide a more granular account for

<sup>23</sup> Age 15 is significant because it is the age at which a typical student graduates from the middle school.

<sup>24</sup> The design of the data collection permits identification of 12 regions of current residence, while the region of childhood is identified at a more granular level, which allows assignment to 26 regions.

such differences, we included in the models a dummy to indicate whether the individual speaks a second language and its interaction with region-of-residence dummies.<sup>25</sup>

It can reasonably be argued that the location of current residence and the ability to speak a second language could be endogenous, influenced by education. Thus, we also estimated the models without these variables. Finally, to analyze the sensitivity of the results, we dropped from the model all variables other than the trend terms and the treatment indicator. In all these exercises, the inference remained intact.

The model is estimated separately for men and women. The error term  $\varepsilon$  captures the impact of individual-specific idiosyncrasies that influence graduation from middle school. Standard errors are calculated in two ways. They are clustered at the childhood region-by-birth cohort level and also at the birth cohort level.<sup>26</sup> We also report the  $p$ -values adjusted for multiple-hypothesis testing, using the improved Bonferroni correction method of Simes (1986; also Benjamini and Yekutieli 2001; Newson 2010).

We employ the same formulation when we demonstrate the impact of the reform on educational attainment (Educ<sub>*i*</sub>). In this specification, the dependent variable is a dichotomous indicator that takes the value of 1 if individual  $i$  has at least a middle school diploma (8 years of schooling) and zero otherwise.

*Potential use of the month of birth.*—Some papers that analyzed the same Turkish reform used individuals' birth month to implement a sharper discontinuity design (e.g., Erten and Keskin 2020; Gulesci, Meyersson, and Trommlerová 2020). This procedure is problematic because cultural and institutional attributes of the country indicate that it is inappropriate to rely on month of birth to identify the impact of the reform. This is because in our data set, as well as in other Turkish data sets, (i) birth month is not reported in a significant proportion of cases, (ii) survey respondents' propensity to report month of birth is correlated with their socioeconomic attributes, and (iii) reported month of birth is not reliable, as there is substantial heaping in January. These issues, coupled with the fact that school starting age is not enforced, imply that it is not the best strategy to try to obtain inference based on those who are born a few months apart (in late 1986 vs. those born in early 1987). The details of each of these points are provided in the appendix.

<sup>25</sup> The ability to speak a second language may be a function of education as much as it is a function of the local culture, which would make this variable endogenous. Dropping the variable from the model, however, had no impact on the results.

<sup>26</sup> Clustering by childhood region by birth cohort is sensible to the extent that the effectiveness of the reform varied by region, especially in the early years of the reform's implementation. Because when clustering by birth cohort the number of clusters is fewer than the rule of thumb of 50, in this case we report the  $p$ -values of the bootstrapped standard errors (Cameron and Miller 2015).

## V. Data and Descriptive Statistics

We use data from the Turkish Family Structure Survey (TFSS) of 2016, a nationally representative survey conducted by the Turkish Statistical Institute and the Ministry of Family and Social Policies between June 1 and September 26. The TFSS is administered in 17,239 households. Those who are older than 15 in each household are surveyed, generating information on 35,475 individuals. The survey does not include refugees, who have moved to Turkey from Syria in large numbers since 2012.

Table 1 presents the descriptive statistics of the analysis sample. Columns 1 and 2 display information about all women, and columns 3 and 4 pertain to ever-married women. Columns 5–6 and 7–8 are related to all men and to ever-married men, respectively. The variable Middle School Degree is a dichotomous indicator that takes the value of 1 if the person has a middle school diploma, which is obtained after 8 years of education. Recall that the education reform mandated students to attain at least this particular level of education. The odd-numbered columns of table 1 display the means and standard deviation of variables for those who were treated by the reform (born after 1986), and the even-numbered columns pertain to the control group, consisting of those who escaped the mandate of the reform (those born before 1986). For both men and women the proportion with a middle school degree is substantially greater among those who were exposed to the mandate of the reform than among those who were not. This is true regardless of whether the person was ever married. Figures 1 and 2 display the proportion of women and men, respectively, who have a middle school diploma in each cohort. The number zero on the horizontal axis signifies the pivotal cohort: those born in 1986. The values on the horizontal axis identify the distance of the person's year of birth from 1986. Individuals born in 1987, 1988, and so on (where the horizontal axis takes the values of 1, 2, and so on) constitute the treatment group, and those with negative values on the horizontal axis (born before 1986) are in the control group.

As evident from figures 1 and 2 and the first row of table 1, the reform increased the propensity to obtain a middle school diploma for both men and women, and the impact was stronger for women. That this Turkish reform had a significant impact on middle school completion for both men and women has been demonstrated by previous work using a variety of data sets (Mocan 2014; Kırdar, Dayıoğlu, and Koç 2015; Dursun and Cesur 2016; Aydemir and Kırdar 2017; Cesur and Mocan 2018; Torun 2018; Dursun, Cesur, and Kelly 2022).<sup>27</sup>

Figure 3 presents the proportion of ever-married women who have a middle school diploma, and figure 4 displays the same information for

<sup>27</sup> We also calculated the proportion of individuals with at least a high school diploma. These proportions were the same between the treatment and control groups in the ever-married-men sample (0.52 vs. 0.53), but the proportion with a high school diploma was higher for the treated groups in all other samples (0.55 vs. 0.37 for all women, 0.40 vs. 0.35 for ever-married women, 0.63 vs. 0.54 for all men), indicating that the reform had a positive spillover effect on high school education.

TABLE 1  
DESCRIPTIVE STATISTICS BY EXPOSURE TO THE EDUCATION REFORM

Variables	All Women		Ever-Married Women		All Men		Ever-Married Men	
	Treatment (1)	Control (2)	Treatment (3)	Control (4)	Treatment (5)	Control (6)	Treatment (7)	Control (8)
Middle School Degree	.806 (.396)	.474 (.499)	.741 (.438)	.454 (.498)	.936 (.245)	.695 (.461)	.872 (.334)	.679 (.467)
Second Language	.409 (.492)	.312 (.463)	.363 (.481)	.300 (.458)	.410 (.492)	.366 (.482)	.383 (.487)	.371 (.483)
Married to a First Cousin			.08 (.272)	.091 (.287)			.092 (.289)	.076 (.266)
Married to a Blood Relative			.202 (.401)	.203 (.402)			.196 (.397)	.184 (.387)
Marriage characteristics:								
Age at First Marriage			20.535 (3.007)	21.42 (4.27)			23.351 (2.490)	25.368 (3.644)
Age Difference between Husband and Wife			4.653 (3.854)	4.004 (4.035)			2.071 (3.012)	3.048 (3.644)
Arranged Marriage			.46 (.499)	.555 (.497)			.365 (.482)	.457 (.498)



Forced into Marriage			.046	.087		.033	.039
			(.208)	(.282)		(.178)	(.195)
Met through Family/Relatives/Neighbors			.607	.673		.571	.626
			(.489)	(.469)		(.495)	(.484)
Marriage preferences:							
It Is OK to Marry a Blood Relative	.118	.141	.151	.145	.144	.21	.179
	(.323)	(.348)	(.358)	(.352)	(.351)	(.407)	(.384)
Spouse's Education Is Not Important	.145	.139	.179	.142	.205	.232	.235
	(.352)	(.346)	(.383)	(.349)	(.404)	(.422)	(.424)
Spouse's Religious Sect Is Important	.809	.816	.839	.825	.698	.746	.717
	(.393)	(.387)	(.368)	(.380)	(.459)	(.436)	(.450)
Similar Political Views Are Important	.57	.591	.56	.59	.446	.468	.437
	(.495)	(.492)	(.497)	(.492)	(.497)	(.499)	(.496)
Only a Son Can Ensure the Continuation of the Family							
Blood line	.206	.243	.238	.245	.394	.371	.343
	(.405)	(.429)	(.426)	(.430)	(.489)	(.484)	(.475)
Observations	2,602	3,265	1,657	3,038	1,990	673	2,159

Note.—The data are from the 2016 TFSS. The treatment group consists of those who were born between 1987 and 1994. The control group consists of those who were born between 1978 and 1985. The 1986 cohort is excluded, as exposure to the reform is unclear for this cohort.

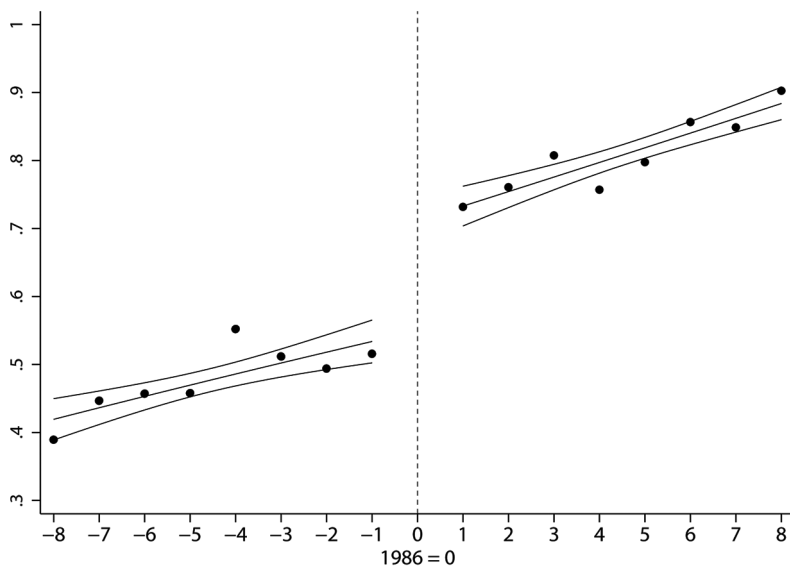


Figure 1.—Proportion of all women with at least a middle school degree by birth cohort. Sample includes all women born within the bandwidth of 8 years around the pivotal 1986 cohort. The 1986 cohort is excluded from the sample, as exposure to the reform is unclear for this cohort.

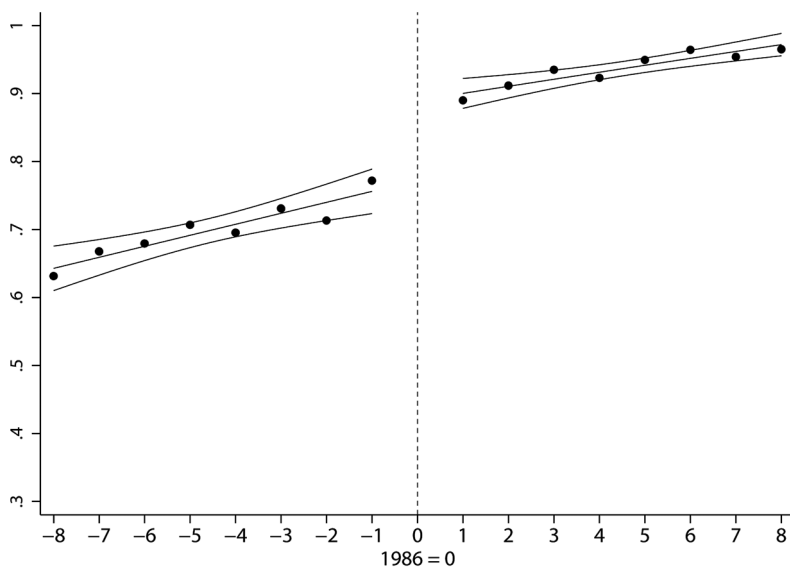


Figure 2.—Proportion of all men with at least a middle school degree by birth cohort. Sample includes all men born within the bandwidth of 8 years around the pivotal 1986 cohort. The 1986 cohort is excluded from the sample, as exposure to the reform is unclear for this cohort.

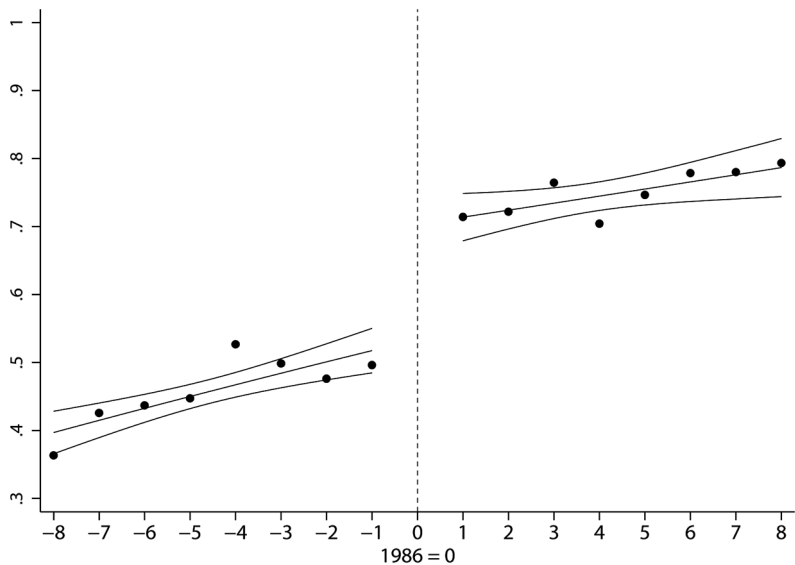


Figure 3.—Proportion of ever-married women with at least a middle school degree by birth cohort. Sample includes ever-married women born within the bandwidth of 8 years around the pivotal 1986 cohort. The 1986 cohort is excluded from the sample, as exposure to the reform is unclear for this cohort.

men. A comparison between figures 1–2 and 3–4 shows that while there was a steady rise in the proportion of all women and all men with middle school education after the reform (figs. 1, 2), this proportion leveled off for younger cohorts in the ever-married sample (figs. 3, 4). This implies that noncompliance with the reform was more prevalent among the ever-married individuals of younger cohorts.

Table 1 shows that about 9% of the sample is married to a first cousin and that about 20% are married to a blood relative (which consists of cousins and other blood relatives from either the mother’s or the father’s side). These rates are consistent with those reported by earlier surveys (Tunçbilek and Koç 1994; Kaplan et al. 2016). Although there is no significant overall difference in this rate between the treatment and control groups, figures 5–8, coupled with figures 3 and 4, depict a more complete and accurate picture. Specifically, figures 5–8 reveal an increase in the propensity for first-cousin and blood-relative marriages among the younger cohorts.<sup>28</sup> Figures 5–8 are, of course, based on the ever-married sample, and as mentioned above, noncompliance with the reform is more prevalent among ever-married individuals. This means that some of those who were in the treatment group of the ever-married sample were, in fact,

<sup>28</sup> Figures 5–8, as well as all other figures related to outcomes, present outcomes net of the control variables (i.e., the set of fixed effects)

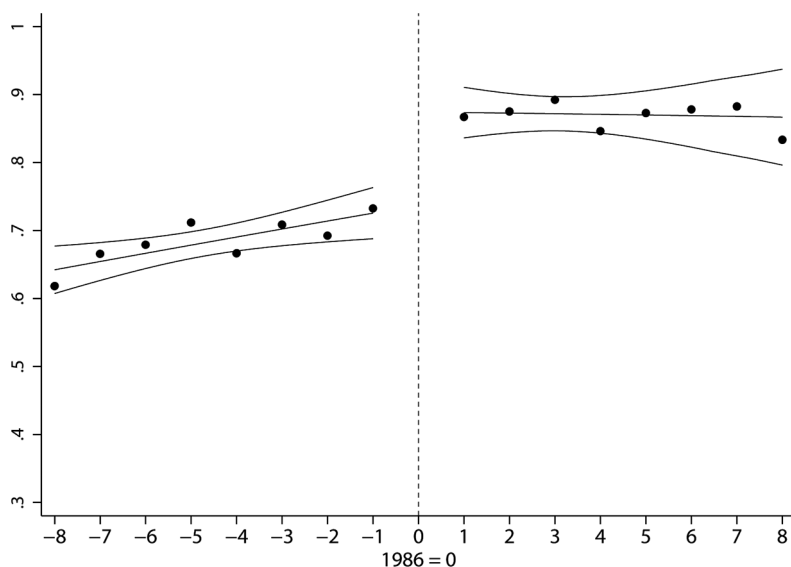


Figure 4.—Proportion of ever-married men with at least a middle school degree by birth cohort. Sample includes ever-married men born within the bandwidth of 8 years around the pivotal 1986 cohort. The 1986 cohort is excluded from the sample, as exposure to the reform is unclear for this cohort.

not treated by the reform and that these individuals are the ones who have higher rates of first-cousin or blood-relative marriage. To be exact, one-quarter of individuals in the treated group of ever-married women do not have a middle school diploma. These women married 2 years younger, in comparison to women who were also in the treatment group but received a middle school diploma (19 years of age vs. 21). Noncompliers are almost 2.5 times more likely to have married a first cousin (14.2% vs. 5.9%) or a blood relative (35.4% vs. 14.8%).<sup>29</sup> The strong correlation between noncompliance and high propensity to marry a first cousin or blood relative suggests that this pattern (refusal to go to middle school, or dropping out of school, and marrying a blood relative) is likely driven by the individual's or her family's attachment to customs and traditional values. Because we classify these individuals as having been treated by the reform, we would tend to underestimate the reform's impact on consanguinity.<sup>30</sup>

<sup>29</sup> The noncompliance rate among the treated ever-married men (12.7%) is half the rate of ever-married women. The difference between compliers and noncompliers in consanguinity among ever-married men is similar to that found in ever-married women. Ever-married men who have not received a middle school diploma are more likely to have married a first cousin (20.9% vs. 7.5%) or to have married a blood relative (39.5% vs. 16.7%).

<sup>30</sup> It is also important to note that the sample size is smaller in younger groups, which is particularly the case in the ever-married sample. This is the result of the fact that individuals need to be both younger and be married to be included in this group. For example, in fig. 8 the observation pertaining to men born in 1994 (when the horizontal axis takes the value of

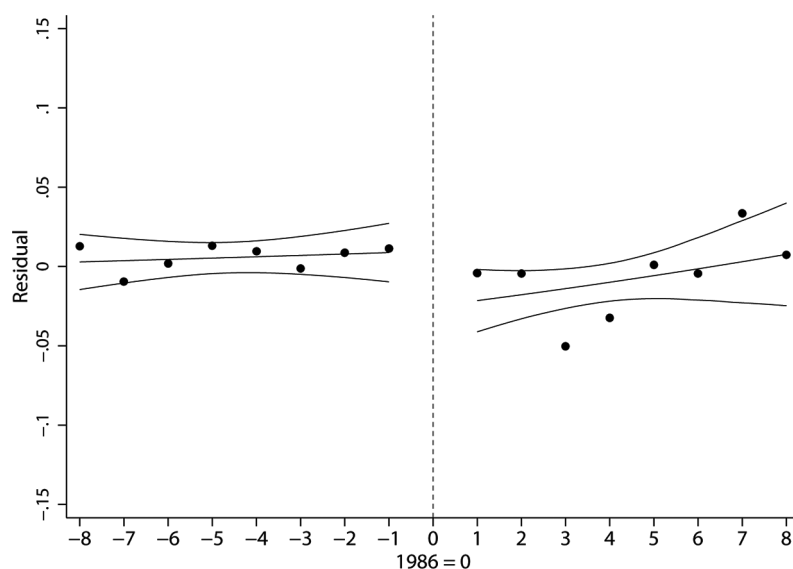


Figure 5.—Proportion of women married to a first cousin by birth cohort. Sample includes ever-married women born within the bandwidth of 8 years around the pivotal 1986 cohort. The 1986 cohort is excluded from the sample, as exposure to the reform is unclear for this cohort. Residuals are obtained after controlling for region of current residence, region of childhood fixed effects, an indicator of whether the person grew up in a village, the interaction between the latter two variables, an indicator of whether the person speaks a second language, and the interaction of second language with region of residence fixed effects.

Table 1 shows that there are differences in the second-language rates between the treatment and control groups, especially in the sample of all women. However, any difference between the control and treatment groups is explained by cohort effects. Specifically, regressions where the second-language dummy is regressed on cohort trends and the treatment dummy reveal that the coefficient of the treatment dummy is always small (about 0.014) and never different from zero. This is true even in simple models that omit all fixed effects. Figure A.1 (figs. A.1–A.5 are available online) presents this information graphically and reveals no jump in the second-language rates attributable to exposure to the reform.<sup>31</sup>

The middle section of table 1 presents the descriptive statistics of marriage characteristics. If the individual was divorced before the current marriage, the first two questions in this section pertain to the first marriage, although 93% of the sample are in their first marriage. The variable

8), is based on only 18 men in this group. Similarly, in fig. 7, there are only 121 ever-married women who were 22 years old during the survey year of 2016 (who were born in 1994). We nevertheless include in the regressions all individuals who are within the 8-year bandwidth and give them equal weight.

<sup>31</sup> As explained in sec. VI.A, dropping the variable Second Language has no impact on the inference.

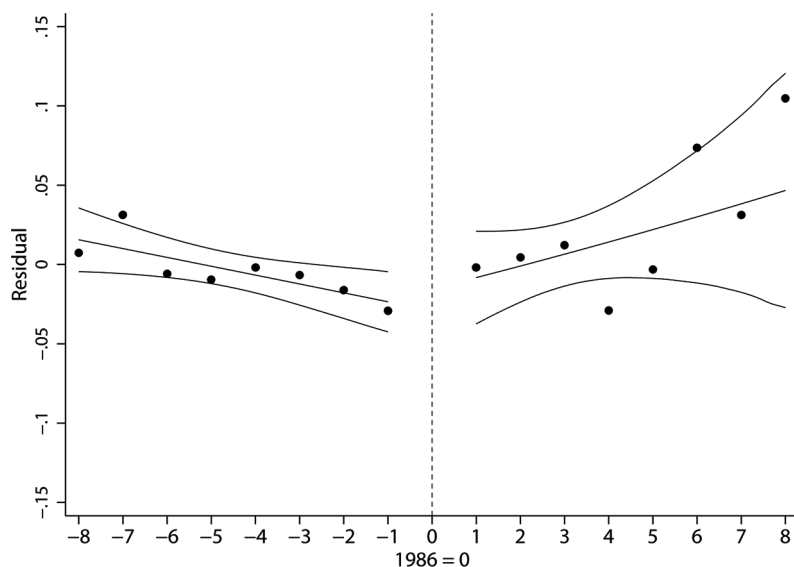


Figure 6.—Proportion of men married to a first cousin by birth cohort. Sample includes ever-married men born within the bandwidth of 8 years around the pivotal 1986 cohort. The 1986 cohort is excluded from the sample, as exposure to the reform is unclear for this cohort. Residuals are obtained after controlling for region of current residence, region of childhood fixed effects, an indicator of whether the person grew up in a village, the interaction between the latter two variables, an indicator of whether the person speaks a second language, and the interaction of second language with region of residence fixed effects.

Arranged Marriage is an indicator that takes the value of 1 if the person was married through the initiative of a matchmaker. This variable takes the value of zero if the person married the spouse (with or without the consent of the family) without an intermediary or eloped. The variable Forced into Marriage is a dichotomous indicator that equals 1 if the person was forced by the family to marry his/her spouse, despite the fact that he/she did not want to marry that particular person. Less than 5% of women in the treatment group are in marriages to which they did not consent. The rate is twice as high among women in the control group. The variable Met through Family/Relatives/Neighbors is another indicator that takes the value of 1 if the person met his/her spouse through family, relatives, or neighbors, and it is zero if the future spouse was met through the network of school, work, or friends or through the internet or a dating agency. The propensity to have found a spouse through family, relative, or neighbor networks is lower among those who were exposed to the reform. Figures 9–13 pertain to these five variables for women. Figure A.2 displays the same information for men.

The bottom section of table 1 displays preferences regarding attributes in a spouse. The variable It Is OK to Marry a Blood Relative takes the value of 1 if the individual declared that it is acceptable to marry a close blood

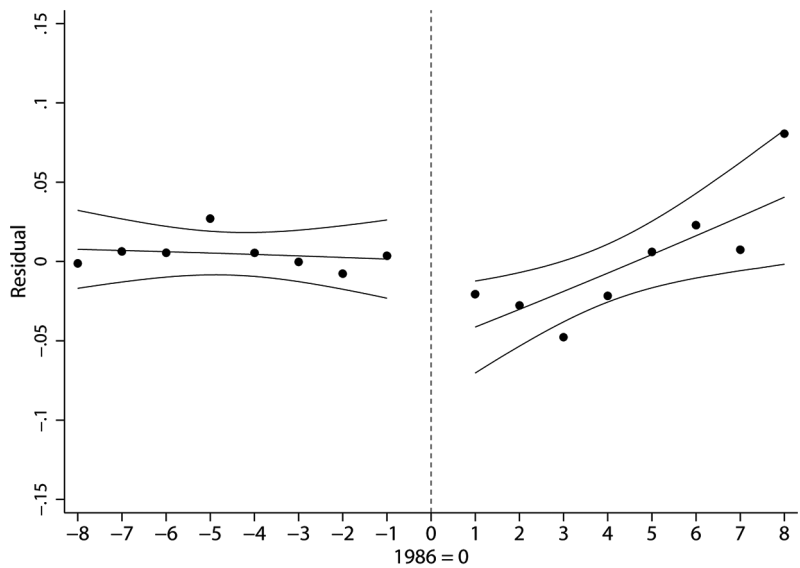


Figure 7.—Proportion of women married to a blood relative by birth cohort. Sample includes ever-married women born within the bandwidth of 8 years around the pivotal 1986 cohort. The 1986 cohort is excluded from the sample, as exposure to the reform is unclear for this cohort. Residuals are obtained after controlling for region of current residence, region of childhood fixed effects, an indicator of whether the person grew up in a village, the interaction between the latter two variables, an indicator of whether the person speaks a second language, and the interaction of second language with region of residence fixed effects.

relative, such as a first cousin. The variable Spouse’s Education Is Not Important equals 1 if the person believes that is unimportant or very unimportant for the spouse to be well educated and zero if the person believes that this spousal attribute is somewhat important, important, or very important. Similarly, Spouse’s Religious Sect Is Important and Spouse’s Political Views Are Important are variables that reveal the extent to which these attributes are somewhat important, important, or very important in a spouse. The proportions of ever-married women who agreed with these statements are displayed in figures 14–17 by birth cohort, and figure A.3 presents the same information for all women. The variable Only a Son Can Ensure the Continuation of the Family Blood Line takes the value of 1 if the survey respondent agreed or strongly agreed with the statement. Figure A.4 presents the proportions of ever-married women and men who agreed with this statement.

VI. Results

Table 2 presents the estimation results of equation (4) by gender and demonstrates that the reform has generated an increase in the propensity for having at least a middle school education by almost 14 percentage points



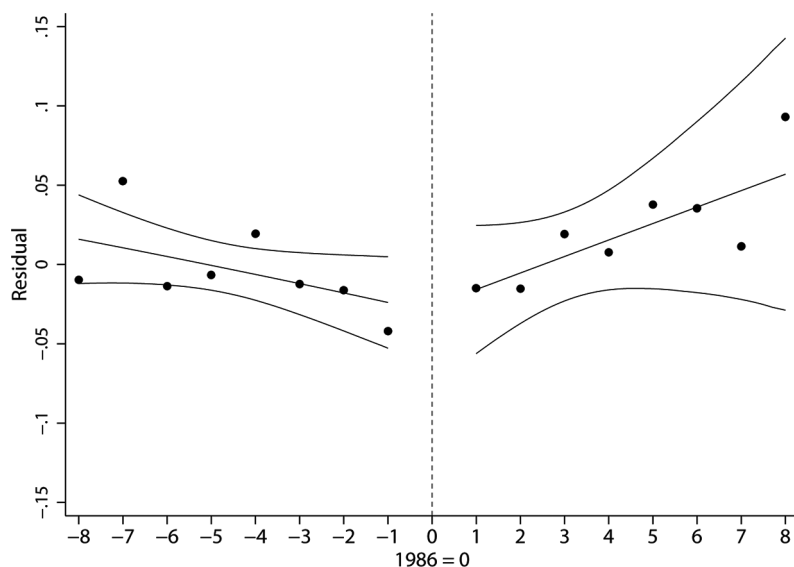


Figure 8.—Proportion of men married to a blood relative by birth cohort. Sample includes ever-married men born within the bandwidth of 8 years around the pivotal 1986 cohort. The 1986 cohort is excluded from the sample, as exposure to the reform is unclear for this cohort. Residuals are obtained after controlling for region of current residence, region of childhood fixed effects, an indicator of whether the person grew up in a village, the interaction between the latter two variables, an indicator of whether the person speaks a second language, and the interaction of second language with region of residence fixed effects.

for all women and about 13 percentage points for all men in our sample (col. 1).<sup>32</sup> Table 2 also shows that the impact of the reform on educational attainment is similar between all women and ever-married women and that the same is true for men. Also evident from table 2 is that the reform had a positive impact on high school completion as well but that it had no discernible effect on college education.

Table 3 presents the reduced form (intent-to-treat) estimates obtained from equation (4). Columns 1 and 2 display the estimated effect of the reform on the probability of marrying a first cousin and marrying a blood relative, respectively, for ever-married individuals. Panel A pertains to females, and panel B displays the results for males. Recall that marriage to a first cousin is a subset of marriages to blood relatives, as the latter includes marriages to individuals who are related by blood but are not first

<sup>32</sup> These magnitudes are very similar to those reported by Dursun, Cesur, and Mocan (2018), who used the Turkish Statistical Institute's Health Survey and the Tobacco Survey of the same institute. They are slightly smaller than those reported by Mocan (2014), who used a large sample from the Turkish Household Labor Force Survey (THLS), and those reported by Kırdar, Dayıoğlu, and Koç (2018). Torun (2018), who also used the THLS, reported an increase in the propensity to complete at least middle school of 17 percentage points for females and 11 percentage points for males.

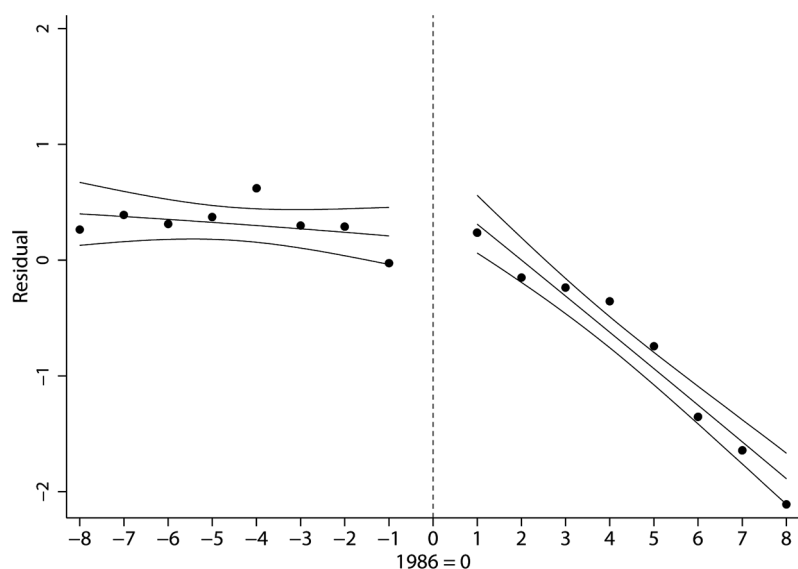


Figure 9.—Age at first marriage for women by birth cohort. Sample includes ever-married women born within the bandwidth of 8 years around the pivotal 1986 cohort. The 1986 cohort is excluded from the sample, as exposure to the reform is unclear for this cohort. Residuals are obtained after controlling for region of current residence, region of childhood fixed effects, an indicator of whether the person grew up in a village, the interaction between the latter two variables, an indicator of whether the person speaks a second language, and the interaction of second language with region of residence fixed effects.

cousins. Entries in parentheses report the standard errors clustered at the childhood region-by-birth cohort level. Childhood region is relevant because it is where the individual lived until age 15 and was exposed to the education reform. Using the region of residence instead did not alter the standard errors appreciably. The  $p$ -values adjusted for multiple-hypothesis testing are reported in curly brackets. Entries in square brackets are the wild-bootstrapped  $p$ -values of the estimated coefficients, obtained from clustering the standard errors at the birth cohort level. Columns 1 and 2 show that the reform decreased women's propensity to marry a first cousin by 3.6 percentage points (about 42% from the baseline) and that it decreased women's propensity to marry a blood relative by 5.6 percentage points (28% from the baseline).<sup>33</sup> On the other hand,

<sup>33</sup> Analyzing the impact of the same Turkish education reform, Asker (2020) concludes that the reform had no impact on women's propensity for first-cousin marriage. He reports a negative, but nonrobust, impact on the propensity for consanguinity of women raised in urban areas. The author stacks surveys that are conducted in different years (e.g., the 2008 Demographic Health Survey [DHS], the Domestic Violence against Women Surveys of 2008, and the 2016 TFSS), which generates an analysis sample of married women in the age range 17–35. Specifically, the treated women (those who were exposed to the reform) were 17–21 years old when they were surveyed in 2008. This creates a potentially severe sample selection issue, because young married women (17–21 years old) who were “treated” by

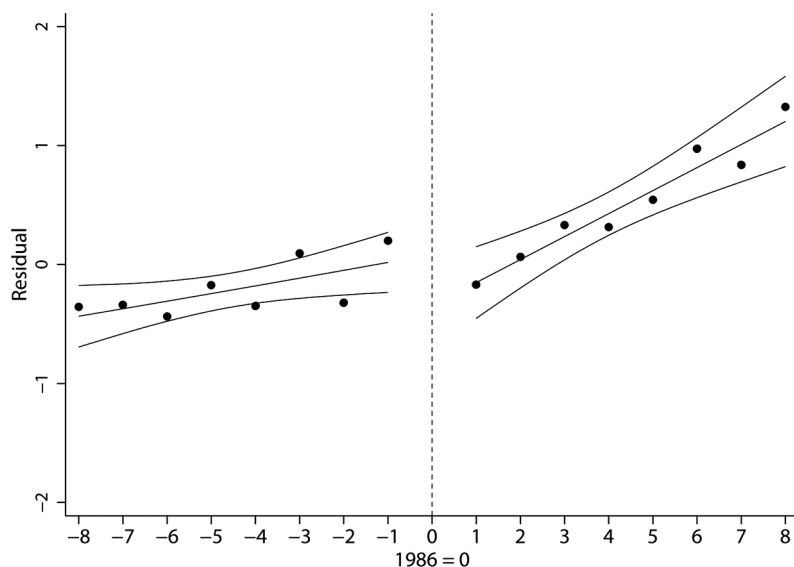


Figure 10.—Age difference for women between husbands and wives by birth cohort. Sample includes ever-married women born within the bandwidth of 8 years around the pivotal 1986 cohort. The 1986 cohort is excluded from the sample, as exposure to the reform is unclear for this cohort. Residuals are obtained after controlling for region of current residence, region of childhood fixed effects, an indicator of whether the person grew up in a village, the interaction between the latter two variables, an indicator of whether the person speaks a second language, and the interaction of second language with region of residence fixed effects.

the reform had no impact on men's proclivity for consanguineous marriage, indicated by small and statistically insignificant point estimates in panel B. For example, columns 1 and 2 of panel B reveal that the coefficient of the reform is 0.013 in the model that explains men's propensity to marry a first cousin, that it is 0.003 when the dependent variable is being married to a blood relative, and that both are highly insignificant.

That the reform had an impact on women's propensity for consanguineous marriage but had no impact on men is surprising. An explanation for this finding can be provided by the fact that, as displayed in table 1, husbands are on average 4 years older than the wives, which indicates that the first four cohorts of women who were affected by the reform are married to four cohorts of men who missed the reform by a few years. More specifically, in figure 5 the first cohorts of women fully affected by the reform were born in the years 1987–90. They correspond to time periods 1–4 on the horizontal axis. These women are typically married to men who

the reform are likely different from 17–21-year-old single women who were also treated by the reform but are not in the analysis sample because they are not married. To the extent that marrying at a young age is positively correlated with the strength of traditional cultural values, this selection would bias the impact of the reform.

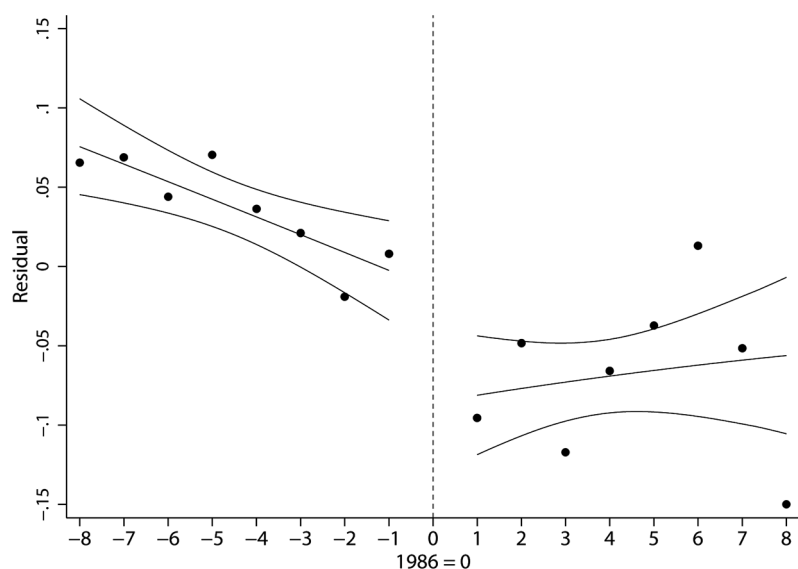


Figure 11.—Proportion of women by birth cohort who are in an arranged marriage. Sample includes ever-married women born within the bandwidth of 8 years around the pivotal 1986 cohort. The 1986 cohort is excluded from the sample, as exposure to the reform is unclear for this cohort. Residuals are obtained after controlling for region of current residence, region of childhood fixed effects, an indicator of whether the person grew up in a village, the interaction between the latter two variables, an indicator of whether the person speaks a second language, and the interaction of second language with region of residence fixed effects.

belong to cohorts  $-4$ ,  $-3$ ,  $-2$ , and  $-1$  in figure 6, and as evident from the figure, the proportion of men married to first cousins is lower in these particular cohorts. This means that the insignificant effect of the reform on men is likely because about half of the men in the control group (those born between 1982 and 1985) are indirectly affected by the reform by virtue of the fact that the reform reduced the propensity for consanguineous marriage for women these men tend to marry. This, in turn, implies that the results in columns 1 and 2 of panel B in table 3 reflect a downward bias of the reform's impact on men's propensity for consanguineous marriage.

To make this point more clear, we shifted the data points for men in figure 6 forward by 4 years and superimposed them on figure 5 (women). That is, we matched the 1982 cohort of men with the 1986 cohort of women, the 1983 cohort of men with the 1987 cohort of women, and so on. The idea behind this exercise is to match the cohorts of men and women who are likely to marry each other, given that husbands are 4 years older than their wives, on average. The result, displayed in figure 18, shows that the proportions of first-cousin marriages are not significantly different between men and women within most of these cohorts that are matched by

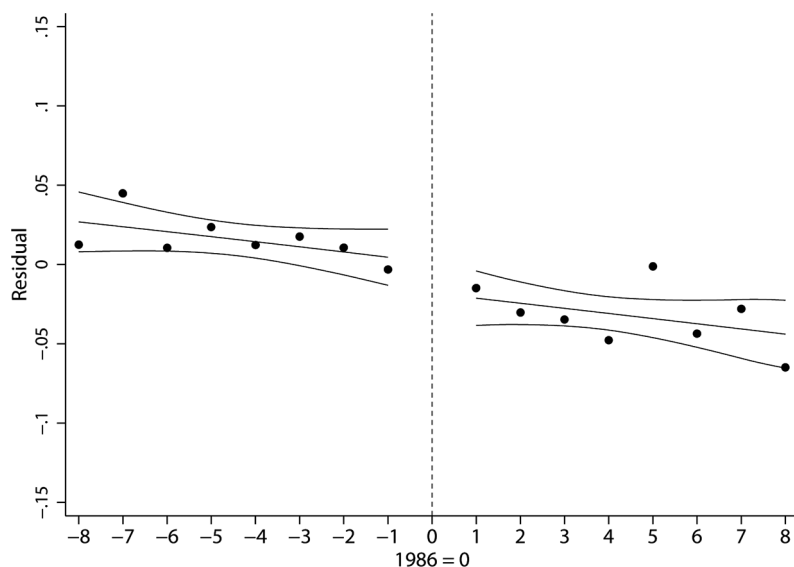


Figure 12.—Proportion of women by birth cohort who are forced into marriage. Sample includes ever-married women born within the bandwidth of 8 years around the pivotal 1986 cohort. The 1986 cohort is excluded from the sample, as exposure to the reform is unclear for this cohort. Residuals are obtained after controlling for region of current residence, region of childhood fixed effects, an indicator of whether the person grew up in a village, the interaction between the latter two variables, an indicator of whether the person speaks a second language, and the interaction of second language with region of residence fixed effects.

the husband-wife age difference. It is also noticeable that there is a drop in figure 18 when the horizontal axis is positive. This region includes women who were treated by the reform and the corresponding cohorts of men who were in these women's marriage pool.<sup>34</sup> This procedure is meaningful to the extent that the reform influenced women's attitudes and behavior regarding consanguineous marriage and that the consanguineous marriage of men is influenced as a result. This conjecture is supported by the data because, as we demonstrate below, the reform influenced women in a number of domains related to marriage preferences but had no impact on men's attitudes or behavior in any outcome analyzed.

Figure 19 displays the same information as figure 18, but it pertains to blood-relative marriages. That is, to obtain figure 19, we moved the cohorts

<sup>34</sup> When we ran the regression for the propensity for marrying a first cousin in this pooled sample shown in fig. 18, we found that the coefficient of the reform was  $-0.020$  ( $p = .084$ ,  $N = 9,068$ ) and that the impact of the reform was not different between men and women (the interaction term of the reform indicator and gender indicator was very small and highly insignificant). This regression included the cohort of 1986, as displayed in fig. 18. Dropping this cohort produced an impact of the reform on first-cousin marriage of  $-0.024$  ( $p = .069$ ,  $N = 8,562$ ).

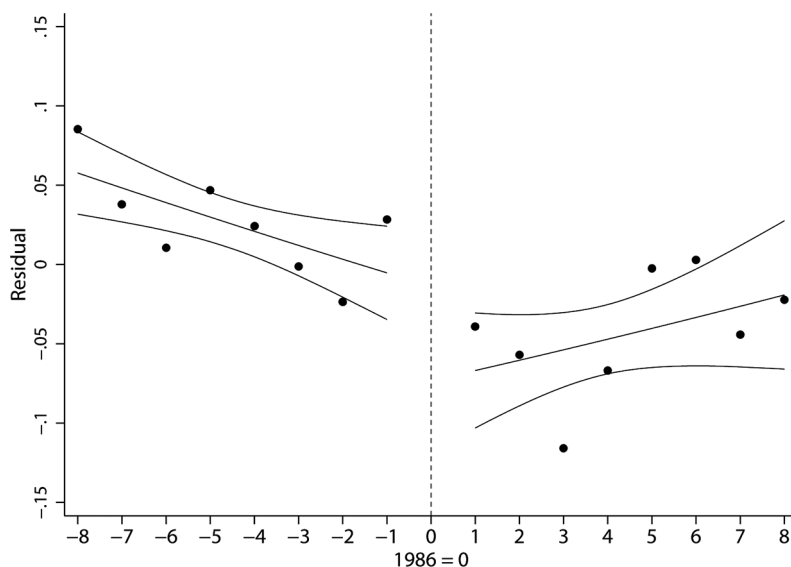


Figure 13.—The proportion of women by birth cohort who met their spouses through family/relatives/neighbors. Sample includes ever-married women born within the bandwidth of 8 years around the pivotal 1986 cohort. The 1986 cohort is excluded from the sample, as exposure to the reform is unclear for this cohort. Residuals are obtained after controlling for region of current residence, region of childhood fixed effects, an indicator of whether the person grew up in a village, the interaction between the latter two variables, an indicator of whether the person speaks a second language, and the interaction of second language with region of residence fixed effects.

of men in figure 8 up by 4 years and superimposed them on figure 7. Once again, the assumption here is that the cohorts of men born between 1982 and 1985 are de facto affected by the reform because these men's marriage pool (younger women who were exposed to the reform) has altered its behavior toward consanguineous marriage. Figure 19 is similar to figure 18, and running a pooled regression using the cohort-matched sample of figure 19 indicated that the reform reduced the propensity to marry a blood relative.<sup>35</sup>

Column 3 of table 3 reveals that the reform increased the age at first marriage for women by about half a year. This could be the result of preferences changing in favor of delaying marriage. It could also be a mechanical result of "incapacitation" because of staying in school longer. To the extent that those who are still in school are less likely to get married,

<sup>35</sup> When we ran the model in this pooled sample shown in fig. 19, we found that the coefficient of the reform was  $-0.026$  ( $p = .097$ ,  $N = 9,068$ ) and that the impact of the reform was not different between men and women (the interaction term of the reform indicator and gender indicator was very small and highly insignificant). This regression included the cohort of 1986, as displayed in fig. 19. Dropping this cohort produced an impact of the reform on first-cousin marriage of  $-0.025$  ( $p = .170$ ,  $N = 8,562$ ).

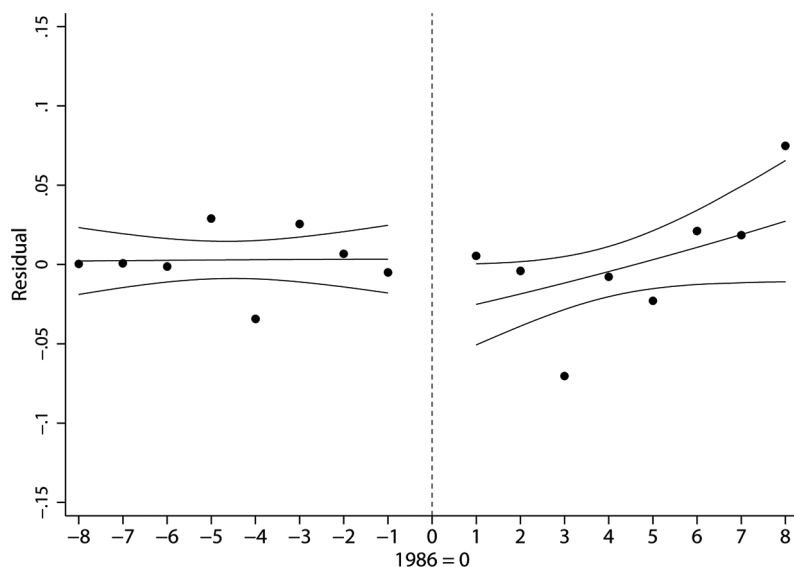


Figure 14.—Proportion of ever-married women by birth cohort who declare that it is OK to marry a blood relative. Sample includes ever-married women born within the bandwidth of 8 years around the pivotal 1986 cohort. The 1986 cohort is excluded from the sample, as exposure to the reform is unclear for this cohort. Residuals are obtained after controlling for region of current residence, region of childhood fixed effects, an indicator of whether the person grew up in a village, the interaction between the latter two variables, an indicator of whether the person speaks a second language, and the interaction of second language with region of residence fixed effects.

exposure to the reform may have delayed age at marriage mechanically.<sup>36</sup> If staying in school because of mandatory education is in fact a constraint for adolescent girls who would have married otherwise, there would be an increase in the rate of the female marriage at the ages of 14–15, right after the completion of 8 years of mandatory education. Figure A.5A displays the distribution of age at first marriage in the treatment group and reveals that there is no heaping at ages 14 or 15. Figure A.5B presents the same distribution for the control group. A comparison of these figures reveals that the proportion of marriages at age 15 or younger is lower, rather than higher, among females in the treatment group, in comparison to those in the control group (2.59% vs. 4.64%). The same is true when we consider the group for which age at marriage is 16 or lower. Less than 7% of the females in the treatment group are married at age 16 or younger, whereas the rate is 10.1% for the control group, which was not exposed to the education mandate.<sup>37</sup> This information does not support the conjecture that

<sup>36</sup> Conversely, Field and Ambrus (2008) show that early marriage in rural Bangladesh is an impediment to female education.

<sup>37</sup> As table 2 demonstrates, the reform not only increased the propensity to receive a middle school diploma but also had a positive impact on high school graduation, although high



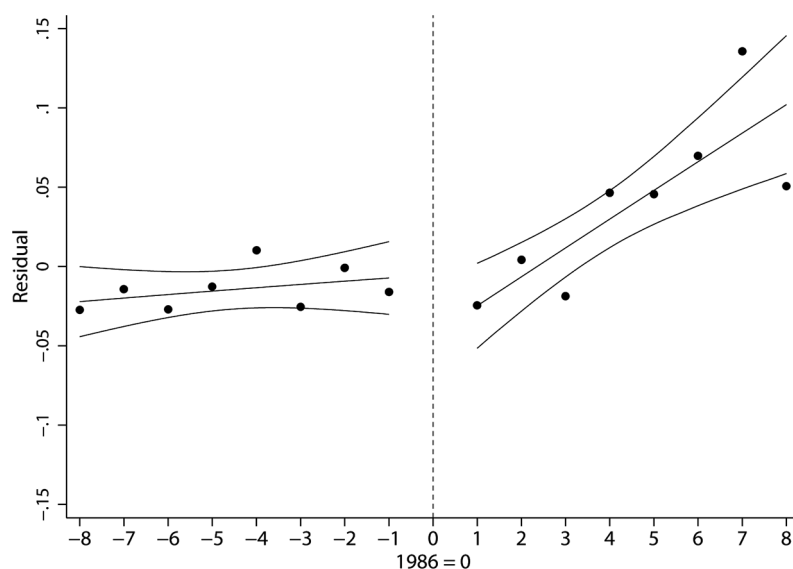


Figure 15.—Proportion of ever-married women by birth cohort who declare that spouse's education is not important. Sample includes ever-married women born within the bandwidth of 8 years around the pivotal 1986 cohort. The 1986 cohort is excluded from the sample, as exposure to the reform is unclear for this cohort. Residuals are obtained after controlling for region of current residence, region of childhood fixed effects, an indicator of whether the person grew up in a village, the interaction between the latter two variables, an indicator of whether the person speaks a second language, and the interaction of second language with region of residence fixed effects.

the increase in female age at first marriage, displayed in table 3, is the result of an “incapacitation” effect due to schooling.

Recall that the reform increased educational attainment of men also, but panel B of table 3 shows that the reform had no statistically significant impact on men's age at first marriage. Column 4 in panel A of table 3 shows that the reform reduced the age gap between husbands and wives by 0.44 years for ever-married women (although the coefficient is significant only at the 12% level when standard errors are clustered by birth cohort). Given that the reform had no impact on men's behavior (see panel B, where none of the estimated coefficients is different from zero), this finding suggests that the narrowing of the husband-wife age difference is driven by women delaying their age at first marriage.

As shown in table 1, about 52% of ever-married women and 43% of ever-married men got married through an arranged marriage, where an

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school education was not mandatory. Delaying marriage to just after high school graduation would mean marriage at the age of 17 or 18. Figure A.5 shows that there is no bunching at age 17 or 18 either. The percentage of females who are married at the age of 18 or younger is 29.5 in the treatment group, and it is 28.33 in the control group.

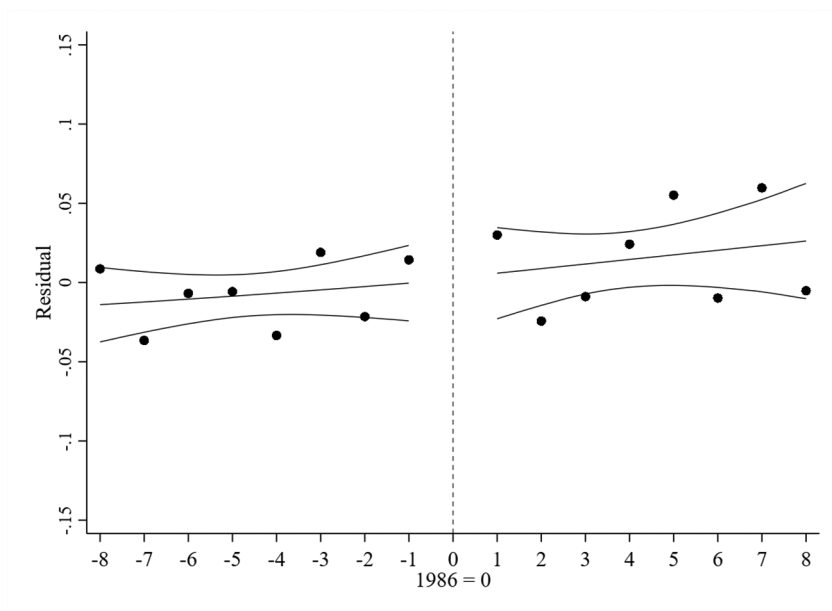


Figure 16.—Proportion of ever-married women who state that spouse's religious sect is important. Sample includes ever-married women born within the bandwidth of 8 years around the pivotal 1986 cohort. The 1986 cohort is excluded from the sample, as exposure to the reform is unclear for this cohort. Residuals are obtained after controlling for region of current residence, region of childhood fixed effects, an indicator of whether the person grew up in a village, the interaction between the latter two variables, an indicator of whether the person speaks a second language, and the interaction of second language with region of residence fixed effects.

informal matchmaker (a relative, a family friend, or a friend) introduced the future bride and groom to each other, typically with the consent of the families. Column 5 of table 3 shows that women who were exposed to the reform are about 7 percentage points (about 13%) less likely to have an arranged marriage orchestrated by a matchmaker.<sup>38</sup> On the other hand, the reform has no impact on men's propensity to get married through a matchmaker.

Column 6 of table 3 shows that the reform reduced women's likelihood of being married to someone whom they did not want to marry by 2 percentage points (29%).<sup>39</sup> In other words, the reform reduced women's propensity for having been forced by their families to marry a particular person. This is a strong impact, given that the prevalence of this outcome among married women is only 7%. Finally, the last column

<sup>38</sup> The  $p$ -value of the estimated effect rises to .107 when the standard errors are clustered at the birth cohort level, but as we show in table A.8 (tables A.1–A.16 are available online), the impact is statistically significant, with  $p$ -values less than .05 with the same clustering in models using other bandwidth sizes.

<sup>39</sup> While the estimated coefficient is not statistically significant when the standard errors are clustered at the childhood region by birth year, they are significant, with a  $p$ -value of .028, when the standard errors are clustered by birth year. Later in the paper, we show the robustness of this result.

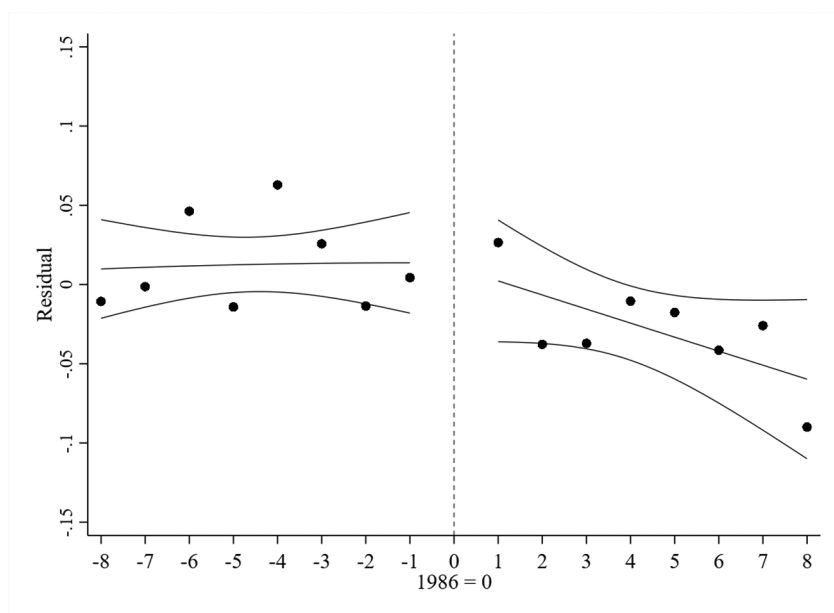


Figure 17.—Proportion of ever-married women who state that it is important to have political views similar to the spouse's. Sample includes ever-married women born within the bandwidth of 8 years around the pivotal 1986 cohort. The 1986 cohort is excluded from the sample, as exposure to the reform is unclear for this cohort. Residuals are obtained after controlling for region of current residence, region of childhood fixed effects, an indicator of whether the person grew up in a village, the interaction between the latter two variables, an indicator of whether the person speaks a second language, and the interaction of second language with region of residence fixed effects.

of table 3 reveals that the reform altered women's propensity to meet their future husbands via their families, neighbors, or relatives. The left-out category of this dependent variable includes meeting the spouse through friends, school, or the workplace or on the internet. Thus, this result, consistent with those of columns 5 and 6, indicates that the reform increased women's propensity to make their marriage decisions independently.

Table 4 presents the results of the analyses where we investigate the impact of the reform on women's marriage preferences. Panel A displays the results for ever-married women, and panel B reports the results for all women. The results for men, displayed in table A.1, reveal that the reform had no impact on the outcomes displayed in table 4 for men.<sup>40</sup>

<sup>40</sup> It has been shown that an increase in education, when the baseline education is low, exerts little to no impact on men's behavior and preferences in developing countries, although it affects women. For example, Cesur and Mocan (2018) find that the same Turkish education reform had an impact on women's religiosity and the propensity to vote for religious parties but no impact on men on these dimensions. Similarly, Cannonier and Mocan (2018) find that an education reform that targeted primary school-age children in Sierra Leone altered women's attitudes on matters that affect women's health, on the number of desired children, and on attitudes regarding violence against women. The same reform, however, had no impact on men along these dimensions.

TABLE 2  
EFFECT OF EXPOSURE TO THE EDUCATION REFORM ON EDUCATIONAL OUTCOMES

Variables	Middle School (1)	High School (2)	College (3)	Middle School (4)	High School (5)	College (6)
	All Women			Ever-Married Women		
Reform	.139*** (.027)	.043* (.023)	.031 (.026)	.138*** (.030)	.071*** (.025)	.024 (.026)
Observations	[.0140] 5,867	[.0826] 5,867	[.409] 5,867	[.0170] 4,695	[.0434] 4,695	[.164] 4,695
	All Men			Ever-Married Men		
Reform	.128*** (.024)	.086*** (.031)	.037 (.031)	.143*** (.033)	.138*** (.039)	.029 (.036)
Observations	[.0010] 4,499	[.0066] 4,499	[.132] 4,499	[.0014] 2,832	[.0140] 2,832	[.0706] 2,832

Note.—The Reform variable is equal to 1 if the respondent was born between 1987 and 1994 and equal to zero if the respondent was born between 1978 and 1985. The 1986 cohort is excluded from the sample, as exposure to the reform is unclear for this cohort. The  $p$ -values related to bootstrapped standard errors, clustered by birth cohort level, are given in square brackets. All regressions control for trend in the outcome variable separately for treatment and control groups, region-of-current-residence and region-of-childhood fixed effects, an indicator of whether the person grew up in a village, the interaction between the latter two variables, an indicator of whether the person speaks a second language, and the interaction of second language with region of residence fixed effects. See the text for details. The entries in parentheses are standard errors of the estimated coefficients, clustered by childhood region by birth cohort.

\*  $p < .1$ .

\*\*\*  $p < .01$ .

TABLE 3  
EFFECT OF EXPOSURE TO THE EDUCATION REFORM ON MARRIAGE CHARACTERISTICS

Variables	Married to a First Cousin (1)	Married to a Blood Relative (2)	Age at First Marriage (3)	Age Difference (4)	Arranged Marriage (5)	Forced into Marriage (6)	Met through Family/Relatives/Neighbors (7)
A. Ever-Married Women							
Reform	-.036** (.017)	-.056** (.024)	.460* (.235)	-.439* (.242)	-.072** (.031)	-.020 (.016)	-.060** (.029)
Observations	[.047] 4,695	[.047] 4,695	[.086] 4,695	[.088] 4,695	[.086] 4,341	[.210] 4,341	[.086] 4,695
B. Ever-Married Men							
Reform	.013 (.022)	.003 (.034)	-.343 (.214)	.073 (.264)	.029 (.040)	.003 (.014)	-.012 (.045)
Observations	[.927] 2,823	[.927] 2,823	[.547] 2,823	[.848] 2,823	[.848] 2,645	[.848] 2,645	[.848] 2,823

Note.—The Reform variable is equal to 1 if the respondent was born between 1987 and 1994 and equal to zero if the respondent was born between 1978 and 1985. The 1986 cohort is excluded from the sample, as exposure to the reform is unclear for this cohort. The *p*-values adjusted for multiple-hypothesis testing are displayed in curly brackets; *p*-values related to bootstrapped standard errors clustered by birth cohort are given in square brackets. All regressions control for trend in the outcome variable separately for treatment and control groups, region-of-current-residence and region-of-childhood fixed effects, an indicator of whether the person grew up in a village, the interaction between the latter two variables, an indicator of whether the person speaks a second language, and the interaction of second language with region of residence fixed effects. See the text for details. The entries in parentheses are standard errors of the estimated coefficients, clustered by childhood region by birth cohort.

\*  $p < .1$ .

\*\*  $p < .05$ .

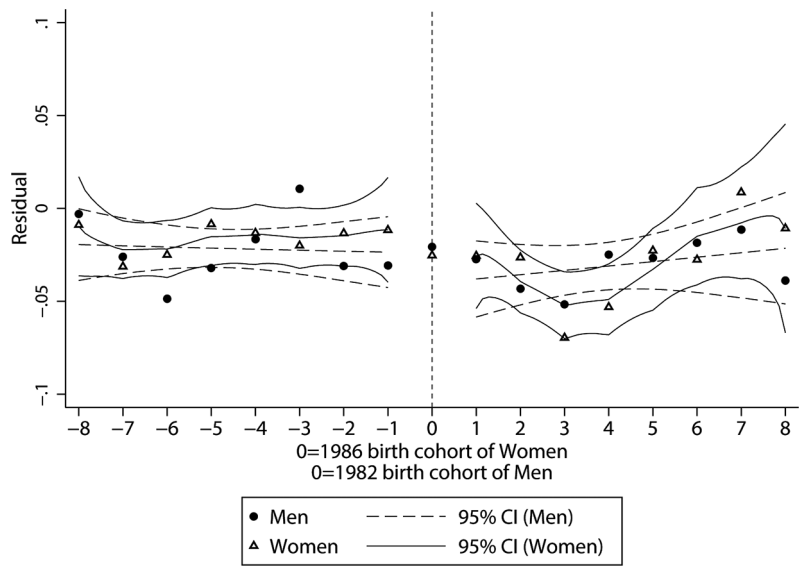


Figure 18.—Proportion of women and 4-year-old men married to a first cousin by birth cohort. Sample includes ever-married women (men) born within the bandwidth of 8 years around the pivotal 1986 (1982) cohort. The figures present residuals that are obtained after controlling for region of current residence, region of childhood fixed effects, an indicator of whether the person grew up in a village, the interaction between the latter two variables, an indicator of whether the person speaks a second language, and the interaction of second language with region of residence fixed effects separately for women and men. CI = confidence interval.

In panel A of table 4, column 1 presents the estimates of the education reform on the extent to which ever-married women find a union between two blood relatives acceptable. The reform reduced the acceptability of such marriages by 3.8 percentage points for ever-married women, although the *p*-value obtained from bootstrapped standard errors clustered by birth cohort is larger than conventional levels. This translates into a 25.3% decline in women’s approval of marriages to blood relatives. Column 2 of table 4 reveals that the reform changed ever-married women’s preferences in favor of more educated husbands. On the other hand, the point estimates in columns 3 and 4 are small and not different from zero. This indicates that the reform did not alter the extent to which women believe that it is important for a married couple to belong to the same religious sect (col. 3) or the extent to which they think that a wife and her husband should share the same political views (col. 4).<sup>41</sup>

<sup>41</sup> As mentioned above, using the exposure to the reform as an instrument for education may violate the exclusion restriction. Therefore, in this context, the results from instrumental variables regressions may generate a biased estimate of the impact of education. Nevertheless, for full transparency we also report the instrumental variables regressions in table A.2.

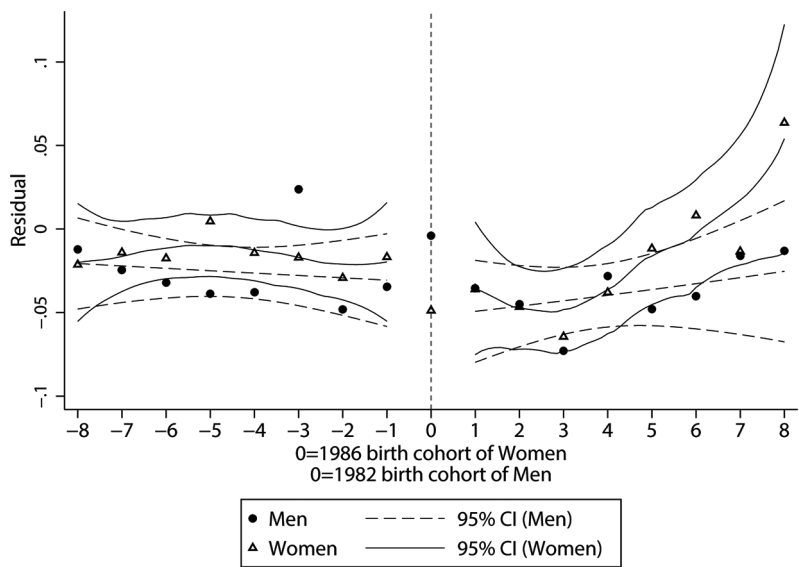


Figure 19.—Proportion of women and 4-year-old men married to a blood relative by birth cohort. Sample includes ever-married women (men) born within the bandwidth of 8 years around the pivotal 1986 (1982) cohort. The figures present residuals that are obtained after controlling for region of current residence, region of childhood fixed effects, an indicator of whether the person grew up in a village, the interaction between the latter two variables, an indicator of whether the person speaks a second language, and the interaction of second language with region of residence fixed effects separately for women and men. CI = confidence interval.

In summary, the results reveal that the reform reduced acceptability of consanguineous marriage to women and lowered women’s likelihood of marrying a first cousin or a blood relative. The reform increased women’s age at first marriage, reduced the age difference between husband and wife, and made women less likely to declare that education is an unimportant attribute of a husband. The reform also caused women to make their marriage decisions more independently from the influence of their families. Specifically, the reform reduced women’s propensity to get involved in an arranged marriage, reduced their propensity for marrying somebody against their will (having been forced into marriage by their family), and made women more likely to meet their spouse through school, friends, workplace, the internet, or other avenues, as opposed to meeting their spouse via family, relatives, or neighbors. These results are not sensitive to control variables, and the impact of the reform is confirmed when we use aggregate measures as dependent variables, rather than as specific indicators.

A. Sensitivity Analyses and Placebo Tests

We implemented a number of robustness analyses. For example, we investigated the sensitivity of the results to alternative formulations of trends, to



TABLE 4  
EFFECT OF EXPOSURE TO THE EDUCATION REFORM ON MARRIAGE PREFERENCES

Variables	It Is OK to Marry a Blood Relative (1)	Spouse's Educa- tion Is Not Important (2)	Spouse's Religious Sect Is Important (3)	Similar Political Views Are Important (4)
A. Ever-Married Women				
Reform	-.038* (.022) {.087} [.210]	-.039* (.021) {.192} [.111]	.002 (.023) {.924} [.949]	-.003 (.030) {.924} [.921]
Observations	4,695	4,695	4,695	4,695
B. All Women				
Reform	-.032* (.019) {.097} [.209]	-.011 (.019) {.553} [.465]	-.013 (.021) {.553} [.669]	-.016 (.027) {.553} [.481]
Observations	5,867	5,867	5,867	5,867

Note.—The Reform variable is equal to 1 if the respondent was born between 1987 and 1994 and equal to zero if the respondent was born between 1978 and 1985. The 1986 cohort is excluded from the sample, as exposure to the reform is unclear for this cohort. The *p*-values, adjusted for multiple-hypothesis testing, are displayed in curly brackets; *p*-values related to bootstrapped standard errors clustered by birth cohort are given in square brackets. All regressions control for trend in the outcome variable separately for treatment and control groups, region-of-current-residence and region-of-childhood fixed effects, an indicator of whether the person grew up in a village, the interaction between the latter two variables, an indicator of whether the person speaks a second language, and the interaction of second language with region of residence fixed effects. See the text for details. The entries in parentheses are standard errors of the estimated coefficients, clustered by childhood region by birth cohort.

\* *p* < .1.

the use of triangular weights, the inclusion and exclusion of control variables, the aggregation of dependent variables into groups, and the treatment of the 1986 cohort. These analyses are presented in tables A.3–A.7.

The empirical specifications effectively compare individuals who differ in age by 8 years. There was no sudden change in the country, other than the education reform, that could have affected a cohort of individuals differently from another cohort. However, by design, those who were treated by the reform are younger than those who were not treated. It can be argued that the results may be driven by this age effect, because younger individuals may be more rebellious against social norms, and that people may conform to cultural norms and traditions as they get older. That the models control for time trends that vary between pre- and posttreatment periods and vary between women and men (because we estimate the models separately by sex) and that we find a significant effect for women, but not for men, argue against this conjecture and indicate that the results are unlikely to be driven by an across-the-board age effect. Nevertheless, as a second exercise, we estimated the models

of table 3 using different bandwidths, which are displayed in table A.8. The table presents the results obtained from models that are based on bandwidths of 6, 7, 9, and 10 years. It also displays the results of the same exercise for the two outcomes of table 4 that are significantly affected by the reform (It Is OK to Marry a Blood Relative and Spouse's Education Is Important).

The sample size gets smaller as the bandwidth becomes narrower, which is expected to negatively affect the precision of the estimates. On the other hand, the control and treatment groups become arguably more dissimilar as the bandwidth gets wider. Nevertheless, the estimated coefficients are rather stable across different bandwidths, indicating that while narrowing or widening the age intervals of the treatment and comparison groups changes the sample composition, it has no meaningful impact on the results.

We also implemented placebo tests by imposing incorrect reform years on the data. The first cohort that was fully affected by the reform is the cohort of 1987. We ran regressions on the same sample but assumed that the first affected cohort was 1988, 1990, or 1991. Similarly, we moved the reform year back in time, taking the cutoff year as 1985, 1984, or 1983. In the six regressions we ran (based on these six false reform years) where the dependent variable was middle school completion, the point estimate of the reform was negative in three regressions (one being statistically significant) and positive in the other three (one being significant). Thus, the use of placebo reform years eliminates the true effect of the reform on educational attainment.

We reestimated regressions by employing the same sample of ever-married women and by using as dependent variables the seven marriage attributes that are significantly affected by the reform (panel A of table 3) and the two marital preferences where reform had a statistically significant impact in table 4 (It Is OK to Marry a Blood Relative and Spouse's Education Is Important). We estimated these models six times, using six placebo reform years. Of the 54 coefficients generated from this exercise, seven flipped signs (in comparison to the results reported in panel A of table 3 and cols. 1 and 2 of table 4), and only one was statistically significant.

In an alternative exercise, we moved the reform years back and forth as described above, but each time we kept the 8-year window on both sides of the false reform dates. This procedure changed the sample composition in comparison to the sample used in tables 3 and 4. Of the 54 coefficients estimated, only 14 were significant, and 13 coefficients had signs opposite those reported in tables 3 and 4.

*Sample composition.*—Because the bulk of the analysis is focused on ever-married women, younger women who marry later than average are less likely to be included in the analysis sample. For example, 22-year-old married women are in the regression sample of tables 3 and 4, but 22-year-old single women are not. The probability of getting married (and thus being included in our analysis sample) goes up with age. This is problematic to the extent that young women who are not yet married and therefore

not included in the analysis sample may be systematically different from other young women who are in the sample because they are married. To address this issue, we reestimated the models by shortening the bandwidth for the treatment group while keeping the bandwidth of the control group at 8 years.

In the data, we find that 38% of women who are 22 years old are married, divorced, or widowed. The rate is 43.5% for women who are 23 years old and 50% at age 24. Thus, we reestimated the models with bandwidths of 7 and 6 years for those who were exposed to the reform. The bandwidth for the control group was kept intact. The results are reported in table A.9. The first column is the benchmark specification as displayed in tables 3 and 4 (with bandwidth of 8 years for both control and treatment groups). The second and third columns display, respectively, the results obtained from specifications where the 22-year-old and 22–23-year-old women in the treated group are excluded. The table shows that the results are insensitive to dropping these younger women from the sample.

The proportions of women who are married at age 18 and older are about the same in the sample of ever-married treatment and control groups (0.83 vs. 0.82). The proportion of ever-married women whose age at first marriage is 20 or older is 0.59 in the treatment group, and it is 0.62 in the control group. On the other hand, these proportions are 0.37 and 0.43 in the treatment and control groups, respectively, for women married at age 22 or older. This is because women in the control group are older. Thus, their proportion who are married at older ages is higher.

The oldest age at which a woman in the treatment group can be married is 28 (because the oldest woman in the treatment group is 28 years old). We dropped women from the control group if their age of first marriage was older than 28, creating a sample in which all women were married when they were 28 years of age or younger, regardless of their treatment status. Running the models in this sample provided very similar coefficients and standard errors. Restricting the sample further to those who were married between 18 and 28 did not change the results either (table A.10). Although these analyses should be taken with caution because the samples are created using an endogenous variable (age at marriage), that the results are insensitive to the sample composition is reassuring.

As a final exercise to investigate the robustness of the results to the composition of ever-married sample, we focus on the question of whether it is OK to marry a blood relative, which was asked of all women, regardless of their marital status. We analyzed the answers of unmarried women to this question, which are reported for samples using three different bandwidths. The results, reported in table A.11, show that the reform reduced unmarried women's propensity to indicate that it is OK to marry a blood relative, as it did for married women.

*B. Heterogeneous Impact of the Reform*

Did the reform affect women's propensity for consanguineous marriage differently in different regions of the country? The western region of Turkey is economically more developed and more urban, including big cities such as Istanbul and Izmir. The eastern part of the country is more conservative and has a higher proportion of residents with Kurdish and Arabic heritage. As column 1 of table A.12 reveals, the rate of middle school completion is lower in the eastern part of the country, but the reform had a significant impact on the propensity to complete middle school. The rates of first-cousin and blood-relative marriage are 12.4% and 28.0%, respectively, in the east, while the corresponding rates are 6.5% and 15.6% in the west. As displayed in column 2 of the top panel of table A.12, when we estimate the models in the sample of eastern residents, we find that the coefficient of the reform in cousin-marriage regression is  $-0.054$  ( $SE = 0.031$ ), and it is  $-0.123$  ( $SE = 0.040$ ) in the blood-marriage regression.<sup>42</sup> The corresponding coefficients in the western sample are smaller ( $-0.029$ ,  $SE = 0.020$ , for first-cousin marriage and  $-0.022$ ,  $SE = 0.027$ , for marriage to a blood relative). The impact of the reform on the propensity of marrying a blood relative is 44% in the east, while it is 14% in the west.

The middle and bottom panels of table A.12 show that the same inference is obtained when we divide the sample into east versus west by the childhood region or by urban versus rural residence in childhood. The latter classification is based on the information in the data that allows us to determine whether the individual lived in a village, in a town, or in a city until age 15.

The summary of table A.12 is that the prevalence of first-cousin marriage is twice as high in the eastern region of the country but that the impact of the reform is also bigger in magnitude and statistically significant in the east. The same is true in the sample of individuals who grew up in villages, in comparison to those who grew up in towns or cities. The rate of blood-relative marriage is also twice as high in the east and in the rural sample, and the reform has a significant impact on these marriages. The impact on blood-relative marriage is weaker in the west. Thus, although the results are similar between regions, they indicate a more robust reaction in the eastern region of the country, which is economically less developed and more conservative, in comparison to the western region. To the extent that the attributes of eastern Turkey are more aligned with those of developing countries, these results suggest that increased education would have similarly meaningful impacts in developing nations.

<sup>42</sup> The western part consists of the region of Marmara (which includes Istanbul), the Aegean region (which includes the city of Izmir), the region of the western Black Sea, the region of the Mediterranean, and the capital city of Ankara.

We also investigated whether the impact of the reform depends on the extent of consanguinity when the reform was implemented. To that end, we obtained the rate of blood-relative marriage in 1993, which is available only at a coarse regional level.<sup>43</sup> Adding these baseline rates and their interactions with reform exposure showed that if a region's rate of consanguineous marriage was 1% higher in 1993, women's propensity for marrying a cousin or a blood relative is 1 percentage point higher today in that region. These results are reported in table A.13, where models with and without control variables are estimated with different bandwidths. The results also reveal that the impact of the reform is stronger on women who reside in regions that had higher baseline consanguinity rates. In the western region of Turkey, with a consanguinity rate of 13.6% in 1993, the reform reduced women's propensity for marrying a blood relative by 1.5 percentage points, whereas the reform led to a 10 percentage point decline in the east, where the blood-marriage rate was 34.3% before the implementation of the reform.<sup>44</sup>

### C. *Potential Channels*

In this section, we investigate some potential mechanisms behind the results. This analysis allows us to rule out some channels, demonstrates the doubtful nature of some others, and points to preference alteration as a likely mechanism, although it is not possible to determine with certainty the specific channel that is the primary reason for the results identified in the paper.

#### 1. Cognitive Dissonance

Panel A of table 4 shows that exposure to the education reform lowered married women's proclivity to approve of consanguineous marriage. It could be the case that cognitive dissonance might alter women's responses to questions gauging their attitudes toward such marriages. More specifically, women might indicate their approval of marriages to blood relatives because they themselves are (or were in the past) married to a blood relative. To investigate this point, we estimated the model using all women, including those who were never married. The results, reported in column 1 of panel B, show that neither the coefficient nor the standard error changes appreciably, suggesting that the impact is not confounded by marital status. Additional evidence on this point is presented in table A.11, where the

<sup>43</sup> This information is reported by Koç and Eryurt (2017) for five regions of the country: east, west, north, south, and central. These five regions are based on DHS classifications used by the authors.

<sup>44</sup> Note that in table A.12 "east" and "west" refer to the eastern and western halves (approximately) of the county (see n. 41). In the analysis reported in table A.13, the consanguinity rates of 1993 are reported for five regions of the country, and here "west" ("east") refers to the regions that constitute a subset of the western (eastern) half of the country.

same results are obtained from the (much smaller) sample of unmarried women.

## 2. The Impact of Husbands

It can be argued that the results could be attributed to the influence of husbands. Under this conjecture, the observed impact of the education reform on women's attitudes and behavior toward consanguinity emerges not because women in the treatment group were affected by the reform directly. Rather, the reform's impact on women could appear because they were indirectly affected by the reform through its effect on their husbands. There are a few pieces of evidence against this hypothesis. First, although the reform had a significant positive impact on men's education (table 2), it had no impact on men's marriage characteristics (panel B of table 3) or men's marriage preferences (table A.1). Thus, it is doubtful that men would influence their wives, as men themselves are not influenced by the reform in the dimensions analyzed in this paper.

Related to a potential mechanism related to husbands, table 4 shows that the reform enhanced women's belief in the importance of their husband's education. To investigate whether women who were "treated" by the reform are more likely to marry a man who is more educated than them, we ran regressions where the dependent variable is an indicator that takes the value of 1 if the husband has more education than the wife and zero otherwise. The results show that women who were exposed to the reform are no more likely to marry a husband who is more educated, compared to women in the control group.<sup>45</sup> This means that even though women who were exposed to the reform are more likely to believe that spouse's education is an important aspect of marriage, they are no more likely to marry a man who is more educated than them than women who were not exposed to the reform.

Finally, using the information of husband-wife education matching, we divided the sample into two groups: (i) women who are less educated than their husbands ( $N = 1,399$ ) and (ii) women who have at least the same level of education as their husbands ( $N = 1,795$ ). When we ran the models to investigate women's propensity to marry a first cousin or a blood relative, we found that the impact of the reform was significant in both groups, with similar magnitudes (table A.14). This result indicates that the impact of the reform on women is independent of the education level of husbands. Taken together, these results suggest that the influence of the reform on women is unlikely driven by their husbands' influence.

<sup>45</sup> The coefficient was  $-0.003$  ( $SE = 0.038$ ). The sample size in this regression is smaller ( $N = 3,194$ ) because husbands' education information is obtained directly from them, and if a husband is not present during the survey interview, his education information is missing.

### 3. Economic Factors

Previous research has shown that the reform had no significant effect on women's labor force participation behavior but that it had a substantial effect on women's wages (Mocan 2014; Torun 2018). The rise in women's wages may have increased women's bargaining power (with their parents) regarding their free choice of a spouse. It should be noted, however, that in Turkey consanguineous marriage has its roots in cultural traditions more than in economic considerations. Evidence supporting this conjecture is obtained from the same household survey used in this paper. The survey includes a question about the reasons why the respondents find consanguineous marriage acceptable. Specifically, those who agreed with the statement that "it is OK to marry a blood relative" were asked a follow-up question as to their reasons for finding such unions acceptable. The options are (i) not to divide up wealth, (ii) to preserve family roots, (iii) because husband and wife get along better if they are blood relatives, (iv) because the elders of the family get more respect in blood-relative marriages, (v) to preserve custom and traditions, and (vi) other reasons.

Table 5 presents the distribution of the responses. At least 46% of the respondents in each category indicate that the main reason for the acceptability of consanguineous marriage is the preservation of family roots. Almost 30% of women and about 20% of men state that they approve of the practice of consanguineous marriage because husbands and wives get along better in such marriages. Only less than 0.5% of women and about 1% of men state preserving wealth as the main justification for consanguineous marriage, and the rate is less than 2% among those who are married to a cousin (cols. 5, 6). Thus, table 5 depicts that in Turkey the protection of family wealth is not a reason for endorsement of consanguineous marriage. The main reason for its acceptance is concern about the preservation of customs and traditions.

### 4. Improved Health Knowledge

Education expands individuals' health knowledge, which can lead to a change in their health behavior.<sup>46</sup> Health complications of consanguineous marriage cannot be learned in school in Turkey, because neither the middle school curriculum nor the high school curriculum includes health education.<sup>47</sup> On the other hand, the more educated will have better cognitive skills, and educational attainment may allow more efficient access to health information. The data set does not contain any information on health knowledge. Thus, we cannot test whether those who were exposed to the reform have better knowledge about the health effects of

<sup>46</sup> See Grossman (1972, 2008) for the theoretical framework regarding how education can affect health and the references cited in Altindag and Mocan (2014) for empirical applications.

<sup>47</sup> Although the Ministry of Education introduces it sporadically as an elective class.



TABLE 5  
WHY CONSANGUINEOUS MARRIAGE IS ACCEPTABLE

Reason Given by Respondent	All Women (1)	Ever-Married Women (2)	All Men (3)	Ever-Married Men (4)	Women in Cousin Marriage (5)	Men in Cousin Marriage (6)
Not to divide up the wealth	.39	.44	1.13	1.33	.91	1.33
To preserve the roots of the family	47.72	47.97	48.16	49.24	45.66	47.02
Because husband and wife get along better if they are blood relatives	29.73	29.28	20.26	19.89	30.59	17.22
Because the elders of the family get more respect in blood-relative marriages	9.26	9.28	10.34	9.85	10.50	13.25
To preserve customs and traditions	10.30	10.44	15.16	14.58	10.50	17.22
Other reasons	2.61	2.61	4.96	5.11	1.83	3.97

Note.—The samples in each column correspond to the regression samples reported in the tables. The percentages in each column add up to 100.

an action or behavior (consanguineous marriage, smoking, vaccination, etc.). However, that exposure to three additional years of education alters women's preferences but has no impact on men suggests that either (i) increased cognitive skills, due to additional schooling, are not an important driver of the results or (ii) the increase in schooling at this low margin is sufficient to register a change in women's preferences and behaviors, although it is not powerful enough to generate an impact for men.

## 5. Decline in Religiosity

It has been shown that an increase in educational attainment has a negative impact on religiosity (Hungerman 2014; Becker, Nagler, and Woessmann 2017; Mocan and Pogorelova 2017). Using the same Turkish education reform analyzed in this paper, Cesur and Mocan (2018) show that increased education, due to the reform, decreased women's propensity to identify themselves as religious, reduced their propensity to wear a head cover (head scarf, turban, or burka), and increased their tendency to identify themselves as modern, as opposed to traditional or religiously conservative. The reform, however, had no impact on men's religiosity. The results of Cesur and Mocan (2018) therefore suggest that the impact of the reform on the decline in women's religiosity could be an avenue through which women's propensity for consanguineous marriage is weakened. However, we provide evidence indicating that the potential change in religiosity is not the driver of the results. First, recall that the reform had no impact on women's beliefs regarding the importance of their husband's religious sect (see table 4). In addition, we extracted a new variable from the data set, which gauges the extent to which the respondents agree with the statement that "it is important to have a religious spouse." Using this indicator as an outcome, we find that the reform had no impact on people's



beliefs that religiosity is an important attribute of a spouse. As table A.15 reveals, the estimated coefficients are small and never statistically different from zero for either men or women, regardless of the bandwidth. These results indicate that although the reform likely had a negative impact on religiosity, it did not alter women's preferences regarding the appeal of a religious husband. Thus, it is unlikely that a change in religiosity is driving the reduced propensity for consanguinity.

## 6. Geographic Mobility and Exposure to "Others"

It is possible that increased education and the associated wage effect on women identified by previous work (Mocan 2014; Torun 2018) may have motivated women to leave their hometowns and to move to different locations to pursue better labor market opportunities. If such behavior is prevalent, geographically mobile women would be in a different marriage market, and their propensity to marry a cousin or a blood relative would be lower not because of a change in preferences but because of the change in the marriage pool. To investigate the validity of this hypothesis, we created an indicator that equals 1 if both the wife and husband grew up in the same province (81 provinces). If the mobility hypothesis is true, the reform should lower the probability of having been married to somebody who is also from the same province. Alternatively, we created another indicator variable, which takes the value of 1 if the husband and wife grew up in the same province and also grew up in a similar settlement of that province (a village vs. a town/city). For example, if a woman grew up in the city of Ankara but her husband grew up in a village around Ankara, this variable takes the value of zero, because even though both of them are from the province of Ankara, one of them grew up in an urban area of the province whereas the other grew up in a village of that province.

Table A.16 displays the results and shows that exposure to the reform had no impact on the probability of a couple having grown up in the same province (panel A) or in the same province and same type of settlement (panel B). Thus, the impact of the reform on consanguinity is unlikely to be driven by geographic mobility.

Another explanation may involve being "outside of the home" during the adolescent years. Exposure to the mandate of the reform compelled individuals to attend school when they were between the ages of 12 and 15. This might help women create new friendship networks that could not have been possible otherwise.<sup>48</sup> Attending school and being able to spend time outside of the home during the adolescent years could have

<sup>48</sup> As shown elsewhere (Mocan 2014; Kırdar, Dayıoğlu, and Koç 2018) and in our table 2, the reform also had a spillover effect, as it increased high school education as well. This means that some females who were treated by the reform were exposed to these influences for additional years beyond the 3 years mandated by the reform.

allowed females to get exposure to ideas and experiences other than those provided by their family and relatives.<sup>49</sup>

*D. Do the Results Reflect Women's Empowerment or the Decisions of Their Families on Behalf of Women?*

It can be argued that even though women who were exposed to the education reform reduce their propensity to marry a cousin or a blood relative, this outcome does not reflect women's own decisions. Instead, their parents may have decided that increased educational attainment of their daughter made it now optimal for her to marry somebody else, rather than her cousin. In this scenario, consanguinity declines as a result of the reaction of women's families and not because of women's improved autonomy regarding their own marital decisions. Although we cannot dismiss the influence of the family, it is unlikely that this is the primary driver of the results. We posit that the reform improved women's independence regarding their marital decisions and that it changed women's preferences regarding marital decisions. The net result is the empowerment of women.

This is reflected by a number of observations. First, as discussed above, the reform made women less likely to go along with the wishes of the family and marry somebody they do not want to marry. That is, the reform made women more resilient in terms of resisting the pressure of the family regarding the marriage decision. Second, the reform made women less likely to marry somebody they met through the network of family, relatives, or neighbors. Instead, exposure to the reform made women more likely to meet their husbands through their network of friends, workplace colleagues, school, the internet, or a dating agency. As table A.16 shows, this is not an artifact of the change in the marriage pool due to geographic mobility. Third, the reform made women less likely to be in a marriage arranged by a matchmaker. Fourth, the reform altered women's preferences in favor of an educated husband.

Fifth, we analyzed the responses to a survey question gauging the extent to which women agree with the statement that "only a son can ensure the continuation of the family blood line." As displayed at the bottom of table 1, the proportion of women (ever-married women) who agree with this statement is 0.226 (0.242). Figure A.4 displays this information, net of exogenous covariates, by cohorts of ever-married women and ever-married men. Table 6 displays the impact of the reform on the propensity to agree with this statement. The results are presented for the benchmark model with the bandwidth of 8 years. The results with bandwidths of 6, 7, 9, and 10 years are very similar to those reported in the table. As shown in

<sup>49</sup> When the reform law was passed in 1997, the labor force participation rate of women aged 15–24 was 32%, while the labor force participation rate for men was 63%. This difference indicates that girls and young women had a much more limited exposure to social life outside of the home, compared to boys and young men.

TABLE 6  
EFFECT OF EXPOSURE TO THE EDUCATION REFORM ON THE PROPENSITY TO AGREE WITH THE  
STATEMENT "ONLY A SON CAN ENSURE THE CONTINUATION OF THE FAMILY BLOOD LINE"

Variables	Ever-Married Women (1)	All Women (2)	Ever-Married Men (3)	All Men (4)
Reform	-.068** (.027) [.015]	-.041* (.024) [.079]	.007 (.043) [.859]	-.043 (.030) [.109]
Observations	4,695	5,867	2,832	4,499

Note.—The Reform variable is equal to 1 if the respondent was born between 1987 and 1994 and equal to zero if the respondent was born between 1978 and 1985. The 1986 cohort is excluded from the sample, as exposure to the reform is unclear for this cohort. The *p*-values related to bootstrapped standard errors clustered by birth cohort are given in square brackets. All regressions control for trend in the outcome variable separately for treatment and control groups, region-of-current-residence and region-of-childhood fixed effects, an indicator of whether the person grew up in a village, the interaction between the latter two variables, an indicator of whether the person speaks a second language, and the interaction of second language with region of residence fixed effects. The entries in parentheses are standard errors of the estimated coefficients, clustered by childhood region by birth cohort.

\*  $p < .1$ .

\*\*  $p < .05$ .

table 6, the reform reduced married women's propensity to agree with the statement that only a son can ensure the continuation of the family blood line by about 7 percentage points. This impact, which represents a 29% decline in the sentiment, is statistically significant and robust to the variations in bandwidth. The impact is smaller in magnitude in the sample of all women (4 percentage points), but it still translates to an 18% decline in the propensity to agree with the statement.<sup>50</sup> These results, taken together, suggest that the reform may have positively influenced women's self-confidence and independence.

## VII. Summary and Discussion

Consanguineous marriage is a remarkably common phenomenon, especially in developing nations. In most countries of the Middle East and North Africa, the rate of consanguineous marriage is more than 25%, and it is as high as 50% in some parts of the world. As detailed in section II, although such inbreeding is more prevalent in Muslim societies, it is not strictly a function of religion. The variation in consanguinity between countries that adhere to the same religion and the change in the prevalence of consanguineous marriage over time indicate that the intensity of this practice is the result of the interplay between religion, cultural norms, institutions, and economic conditions.

In this paper, we use data from Turkey, where 20% of marriages are consanguineous. The nationally representative data set contains detailed

<sup>50</sup> Columns 3 and 4 reveal that, consistent with the results reported above, the reform does not influence men's beliefs regarding the continuation of the family blood line as a function of the sex of the progeny.

information on individuals' marriage attributes and marital preferences, along with their personal characteristics. We leverage an education reform that increased the mandatory schooling by 3 years for students who completed the fourth grade or who were in lower grades in summer 1997 but exempted older students (those who completed the fifth grade) from the mandate of the reform. We confirm the result of previous research that this reform generated an increase in the probability of having at least a middle school education (which requires completion of 8 years of schooling).

We find that the reform made women less likely to find consanguineous marriage an acceptable practice and that the reform lowered women's propensity to marry a first cousin or a blood relative. It has been argued that consanguineous marriage is not a voluntary choice for women in traditional Muslim societies (Edlund 2018). Women may be denied the right to decide their own marriage, and they may be forced into these arrangements by their fathers or, more generally, by their families. To the extent that women's decisions to marry a cousin or a blood relative are under the influence of dominant males of the patriarchal family, as suggested by Edlund (2018), our results indicate that education increases women's autonomy in marriage decisions.

It can be conjectured that although the reform led to a decrease in women's propensity for consanguineous marriage, this may reflect the modifications in the decisions that the families make on behalf of their daughters, rather than women's improved autonomy to make their own marital decisions. However, we provide evidence indicating that higher educational attainment, generated by the reform, led to more independence for women. First, women who were treated by the reform are less likely to be in an arranged marriage, and they are more likely to have met their husbands through networks outside of their family, relatives, or neighbors. Second, a unique feature of our data set is the ability to determine whether the individuals got married to their spouses voluntarily (by their consent) or whether they were forced into it by their family. We find that exposure to the reform reduced women's probability of having been married against their will.<sup>51</sup> These findings indicate that the reform increased women's autonomy, as exposure to the reform enhanced their ability to make marriage decisions independent of their families.

We also find that the reform made women less likely to agree with the idea that only a son can ensure the continuation of the family blood line, which implies that the reform altered women's beliefs and attitudes in favor of females. Taken together, these results indicate that exposure to the education reform increased women's inclination to make marriage decisions that are consistent with their own preferences, instead of going along

<sup>51</sup> Although this point estimate is nonsignificant at conventional levels, arguably because of limited variation of the variable in the sample, the effect is robust to variations in bandwidth and other variations in model specifications.

with the wishes of the family or the customs of the society. Put differently, the reform had an empowerment effect on women.<sup>52</sup> We show that these results do not emerge because of a change in religiosity or geographic mobility.

The results show that exposure to the reform made women delay their first marriage and that it changed women's preferences in favor of an educated husband. The reform had no significant impact on men's preferences, even though it increased men's education as well. The propensity for consanguineous marriage for men went down because women tend to marry men who are 4 years older, on average, and therefore some men who missed the reform are nevertheless affected by it indirectly, through the change in women's behavior. That the reform (i) had no impact on men's attitudes, coupled with (ii) the findings that the same impact on women is observed regardless of whether women are more or less educated than their husbands, and that (iii) the change in attitudes toward consanguinity is also observed among unmarried women who were exposed to the reform indicate that the husbands are not the likely conduit through which the reform influenced women.

The results are consistent with the predictions of the economic framework of family formation. Starting with Becker (1973, 1974), various formulations of the rational choice model of the marriage decision underscored the gains from marriage through aggregate household consumption and the division of consumption between couples, as opposed to remaining single (e.g., Manser and Brown 1980; Chiappori, Iyigun, and Weiss 2009). The impact of search frictions and the costs of searching for a mate, ranging from social security taxes to sex ratios, are also highlighted (e.g., Weiss 1997; Ehrlich and Kim 2007; Brainerd 2017; Pestel 2021). These frictions, as well as preferences, influence the sorting patterns and the propensity to marry within the group (endogamy; e.g., Kalmijn 1998; Hitsch, Hortaçsu, and Ariely 2010). The increase in education reduces search costs of a partner who is outside of the family network by increasing exposure to such individuals. In addition, that we find evidence on the change in women's preferences about consanguineous marriage, coupled with the finding of Hitsch, Hortaçsu, and Ariely (2010) that preferences are an important cause of sorting in marriage, indicates the impact of education on marital preferences.

These results are potentially important for a number of reasons. First, they contribute to our understanding of nonpecuniary effects of educational attainment. They suggest that education alters women's preferences and that it affects behavior in a setting where such behavior (marrying a first cousin or a blood relative) is part of the culture of the society.

<sup>52</sup> In a different context, these findings resemble the results of Jensen and Oster (2009), who find that exposure to cable TV in India reduced women's preference for sons and increased women's autonomy (their ability to go out without permission or to participate in household decision-making).

The results also have implications for child health. The practice of consanguineous marriage is associated with increased mortality and morbidity and serious health consequences for the offspring. As summarized in the online appendix, it has been shown that the probability of a premature birth is higher for babies of consanguineous parents. Prenatal and neonatal mortality rates are also significantly higher for the newborns of such couples, and the same is true for post-neonatal mortality, infant mortality, and under-5 mortality rates for the progeny of first-cousin marriages. Infants born to consanguineous parents are lighter at birth, and they suffer from a number of health problems as children, including being stunted and having learning disabilities. The detrimental impact of low birth weight on future outcomes (Currie 2009) can, in principle, be counterbalanced by investment into these children after birth (Heckman 2006; Cunha and Heckman 2008). There are, however, a number of other serious permanent health problems and disorders generated by consanguinity, including deafness, blindness, childhood glaucoma, congenital heart defects, and cerebral palsy.

Although the harmful health effects of consanguineous marriage are well documented in the medical literature, public policy attempts to discourage these marriages are not pursued, because such attempts are presumed to create a backlash in the target populations.<sup>53</sup> Along the same lines, as summarized by Modell and Darr (2002), several experts in the medical and social sciences argue that consanguineous marriage is engraved in the fabric of the society in many countries and that therefore it would be inappropriate to discourage it at the population level. Thus, a typical policy recommendation is to identify at-risk families and provide them with genetic counseling.<sup>54</sup> Given this hands-off approach, policies that increase female education can be a vehicle through which the prevalence of consanguineous unions and the related health risks can be diminished. For example, a back-of-the-envelope calculation reveals that in Turkey about 200,000 children are born each year to parents who are blood relatives and that about half of these children are progeny of first cousins. In the absence of the education reform, there would have been 56,000 additional children born to parents who are blood relatives, and 36,000 of these children would be to first-cousin marriages.<sup>55</sup> Without the reform, there would

<sup>53</sup> An example of such a situation is the incident in the United Kingdom in February 2008, where the environment minister Phil Woolas told the *Sunday Times* about the increased risk of genetic problems and birth defects among the children of first-cousin marriages. He underlined that such marriages are a cultural, not a religious, issue. Nevertheless, these statements prompted the Muslim Public Affairs Committee to call on the Prime Minister Gordon Brown to fire Woolas, as they were interpreted as being Islamophobic ([http://news.bbc.co.uk/2/hi/uk\\_news/7237663.stm](http://news.bbc.co.uk/2/hi/uk_news/7237663.stm)).

<sup>54</sup> These recommendations emerged after the two meetings of experts organized by the Regional Office of the Eastern Mediterranean of the World Health Organization in 1994 and 1996. Modell and Darr (2002) summarizes the discussion at these meetings and the recommendations that emerged from them.

<sup>55</sup> Consanguineous marriages may be associated with higher fertility rates (some of which may be due to replacement fertility because of the infant mortality-enhancing effect

have been at least 580 additional infant deaths per year.<sup>56</sup> Furthermore, there would be permanent birth defects and serious congenital malformations among the more than 55,000 surviving children of consanguineous parents each year.

A high rate of consanguinity, coupled with a high fertility rate in some developing countries, produces large cohorts of children born to consanguineous couples. For example, using 14 countries in the Middle East and North Africa, where data on the number of births and the rate consanguinity are available,<sup>57</sup> we estimate that there are 3 million children born in these nations each year to consanguineous marriages.<sup>58</sup> If female educational attainment affects marriage preferences and decisions in most countries in the same way as it does in Turkey, education can be a potent policy tool to improve child health.

Finally, given the importance of human capital in economic development (Hanushek and Woessmann 2020), the associated improvements in physical health and cognition can have nontrivial effects on economic development. To the extent that consanguineous marriage promotes and preserves kinship clan networks and strong bonds to extended family, it affects institutional structure, corruption, trust, and economic growth (Greif and Tabellini 2017; Akbari, Bahrami-Rad, and Kimbrough 2019; Schulz et al. 2019). Thus, the decline in consanguinity is also expected to affect long-run economic growth and cultural change.

## Appendix

### The Misuse of the Month of Birth in Turkish Education Reform Studies

In the analyses of the impact of the Turkish education reform, it is incorrect to use individuals' month of birth to classify them into treatment and control groups, for a number of important reasons. First, it is widely known that the school-starting-age law is not enforced in Turkey. Although the law stipulates that a child may start the first grade in the fall of a given year if she is 72 months old at the end of that calendar year, the age cutoff is not enforced (Dincer, Kaushal, and Grossman 2014; Cesur and Mocan 2018; Kırdar, Dayıoğlu, and Koç 2018; Torun 2018). This means that those who were born in 1986, and especially those with birthdays in later months of the year (those who would be younger for the cohort of first graders), were likely to start school in 1993 and were exposed to the reform.<sup>59</sup> Thus, the

of consanguineous marriage). The reduction in fertility due to the decline in consanguinity is not accounted for in this number.

<sup>56</sup> This calculation makes use of the information that 20% of marriages are consanguineous in the country, that the infant mortality rate is 9 deaths per 1,000 live births, and that the infant mortality rate is 2.5 times higher among the offspring of consanguineous marriages.

<sup>57</sup> The data can be downloaded from <https://population.un.org/wpp/Download/Standard/Fertility/>.

<sup>58</sup> These countries are Turkey, Algeria, Egypt, Libya, Morocco, Tunisia, Iraq, Jordan, Kuwait, Lebanon, Omar, Qatar, Sudan, and Saudi Arabia.

<sup>59</sup> Similarly, those with birthdays in early 1987 may have started school in 1992 and would not have been exposed to the reform. Furthermore, there is no social promotion in Turkey,



treatment status of those born in 1986 is unclear, and this is even more questionable for those who were born at the end of 1986.

Second, month of birth provides noisy and incomplete information in all Turkish data sets, primarily because of the attitude and behavior of parents, which lead to delays between a birth and its registration to official documents. For example, in our analysis sample, 24% of all women have missing birth months (1,384 of 5,867 observations). Our ever-married women sample contains 4,695 observations, and 1,103 of these (24%) have not reported their month of birth. The rate of missing birth month is 21% in the sample of all men and 19% in the sample of ever-married men. Furthermore, the propensity for a missing birth month is nonrandom, but it is correlated with the education level and geographic region of the respondents. For example, when we regress the indicator for missing birth month on an indicator of whether the individual lived in a village until age 15, the estimated coefficient is 0.035 (SE = 0.012), indicating that those who grew up in a village are 3.5 percentage points more likely not to know (or not to report) their month of birth. We also find that those who have a middle school diploma and those who have at least a high school diploma are more likely (respectively, 8.1 percentage points with  $p$ -value = .00 and 6 percentage points with  $p$ -value = .00) to report their month of birth than those with less than middle school education. Similarly, those who currently reside in the western part of the country are more likely to report their month of birth than those who live in the south, and those who live in the Black Sea region and in the east are less likely to report their month of birth.

Another general problem, which is related to the second issue above, is that about one in five Turkish birth certificates list January as the month of birth. In our particular case, in the sample of all women who did report their month of birth, 17.8% list January as their birth month. Similarly, in the sample of ever-married women 18.1% indicate that January is their birth month. The rates are 17.1% and 16% in the sample of all men and ever-married men, respectively. In each subsample, a  $\chi^2$  test for the equality of the reported birth month distribution to a uniform distribution is rejected, with a  $p$ -value of .00.<sup>60</sup>

Misreporting of birth month as a cultural phenomenon is well known in Turkey, and it is occasionally covered by the media.<sup>61</sup> The same phenomenon is also observed among the refugees to the United States. As reported in a piece by National Public Radio, 14% of the 80,000 refugees to the United States in 2009 have January 1 as their birthday (NPR 2019).

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and grade repetition in elementary school was 5% in the 1990s. (Dursun, Cesur, and Kelly 2022). This means that about 5% of students of the 1986 cohort were treated by the reform because they were grade repeaters and were going to school with the 1987 cohort when the law was passed.

<sup>60</sup> Our data set is not a special case, but it is the part of this norm. For example, in the 2014 THLS of Turkey, 12.6% of observations have missing month of birth. Of the 344,237 individuals who reported a birth month, 19.2% declared January as their month of birth. In the 2013 DHS of Turkey, 12% of more than 9,700 women and 12.9% of more than 7,200 married women have January birth months. Similarly, the 2008 DHS of Turkey has more than 7,400 ever-married women, 12% of whom have a January birth month. In the Domestic Violence against Women 2008 data set, there are more than 10,800 (9,800) women (ever-married women), and 13.3% (13.5%) of them reported their birth months as January.

<sup>61</sup> *Binlerce kişinin 1 Ocak'ta doğması tesadüf değil!* (<https://www.cumhuriyet.com.tr/haber/binlerce-kisinin-1-ocakta-dogmasi-tesaduf-degil-309688>). The headline reads, "It is not a Coincidence that Tens of Thousands of People Have January 1st as Their Birthdays."



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