

Drivers of change: How intra-household preferences shape employment responses to gender reform

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Abstract

Billions of women still face legal barriers to economic inclusion, yet it is unclear whether lifting these barriers is sufficient to enhance their economic participation. We conduct a field experiment to quantify the impact of a major legal reform - the lifting of the Saudi women's driving ban - on women's employment by randomizing rationed spaces in driver's training. Two years later, women in the treatment group are 61% more likely to drive, 19% more likely to leave the house unchaperoned, and 35% more likely to be employed. However, they are also 19% more likely to require permission to make purchases. These patterns vary systematically with marital status: although physical mobility increases for all women, treatment effects on employment are only observed among never-married and widowed women, who negotiate employment with their fathers. Married and divorced women with children, over whom husbands and ex-husbands have leverage, actually *exit* the labor force and experience decreased spending autonomy. We posit that these patterns reflect differences in male family members' support for women's employment. They provide evidence that men's resistance to wives' employment poses a binding constraint to female labor force participation when legal restrictions are relaxed, but also that men are more open to granting their daughters economic rights, as has been posited in the literature. The results underscore the importance of intra-household responses to gender reforms, which have the potential to counteract legal gains in women's freedoms, and help explain why potential economic gains from lifting discriminatory laws often go unrealized.

Keywords: Female labor force participation; intrahousehold preferences; randomized control trial; mobility; female driving ban; Saudi Arabia.

JEL codes: J22, J16, J12

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1 Introduction

Worldwide, an estimated 2.4 billion women live in countries where they do not legally have the same economic rights as men, including laws that prevent women from working in certain sectors or taking certain jobs, and legal restrictions on women’s movement or work hours (World Bank, 2022). We also observe disproportionately low rates of female labor force participation in these settings (Gonzales *et al.*, 2015). But can reducing legal barriers to female economic participation lead to significant increases in women’s employment?

In addition to legal barriers, restrictive settings are likely to have strong norms against women’s employment that also curtail female work opportunities and incentives. Even if women want to work when legal barriers are lifted, intrahousehold preferences may constrain their employment responses to reform. For instance, household members may be opposed to women working because they are reluctant to cede control of earned income or share responsibility for domestic work. As a result, gender progressive legal reforms may have little impact on culturally and economically entrenched behaviors such as female employment. Indeed, women’s economic participation remains low in many settings where legal restrictions have largely been abolished, such as India and Pakistan.

Evaluating the impact of legislative reform is greatly complicated by the fact that legal regulations affect all individuals in a given society simultaneously. Cross-country panel data suggest that, as countries adopt more progressive gender laws, female labor force participation increases (Hallward-Driemeier *et al.*, 2013; Hyland *et al.*, 2020; Gonzales *et al.*, 2015). However, progressive legislation may respond to social change, making the direction of causality difficult to establish. Moreover, law changes tend to be enacted alongside similarly motivated policies, making it complicated to identify the impact of any one particular reform. As a result of these challenges, to date it is still uncertain to what extent legal barriers pose a binding constraint on female employment.

To shed light on this question, we study female employment responses to a sudden and dramatic change in women’s legal rights that occurred in the Kingdom of Saudi Arabia in 2018, when women were granted the legal right to drive after 61 years of prohibition. The reform, which was pitched as an economic stimulus designed to encourage female employment, was received with excitement but also skepticism about its potential impact given other first-order constraints to Saudi women’s labor force participation. For instance, until recently, male guardianship rules restricted both female mobility and agency over work, and women faced legal restrictions on occupational choice (Library of Congress, 2022; Alnahda Society, 2019; Government of KSA, 2017). Furthermore, given women’s historically low LFP rates, the Saudi labor market is characterized by a mismatch between labor market demand and the skills of female job-seekers (Reynolds, 2018; Platt, 2018).

In order to isolate the impact of lifting the driving ban on female labor supply, we conducted a randomized experiment in the immediate wake of the repeal that eased women’s constraints on obtaining a driver’s license. While all Saudi women were simultaneously granted the *de jure* right to drive, our experiment makes use of the fact that *de facto* access to this right was rationed by driver’s training programs. In particular, initially only

one school was granted authority to offer the required training and license testing to women, and the fee for the course was 3,000 SAR (\$800 USD), 50% of the average monthly salary of Saudi women and six times higher than the course fee for men. On account of these restrictions, two years after the ban was lifted, only 2% of women in the country had obtained a driver’s license (Saudi Arabia General Authority for Statistics, 2020). Our intervention gave a randomly chosen group of women immediate and free access to the official driving school, thus granting them *de facto* rights to the *de jure* reform.

As a result of the intervention, 54% of respondents in the treatment group received a driver’s license compared with only 10% of respondents in the control group over the proceeding two years. Moreover, access to a driver’s license led to a dramatic and statistically significant increase in beneficiaries’ independent mobility. After two years, treated respondents were 61% more likely to have driven in the previous month and made 19% more of their trips without a male chaperone. We also find large and statistically significant effects on employment: treated respondents are 35% more likely to be employed at endline. Alongside these changes in female behavior, we find evidence of a significant negative response among men to women’s newfound right in terms of their willingness to grant female family members economic autonomy. In particular, despite the increase in employment, treated women report significantly *less* independent decision-making power over spending.

To make sense of these disparate findings, we establish that the positive impacts on employment and negative impacts on female economic agency are experienced by two different subsamples of women in our study, who respond in predictably opposite ways to the opportunity to drive. In particular, we make use of variation across women in our sample in terms of their relationship with men in the household with whom they must negotiate employment, which is predictive of their degree of altruism. Women in this setting must negotiate employment with male guardians and co-parents, and, because Saudi law virtually guarantees that men have substantially more intra-household bargaining power than their wives or female dependents, male heads’ preferences weigh heavily in household allocations.¹ Additionally, men’s degree of altruism is likely to be an important determinant of male preferences over female employment on account of the fact that Saudi women generally benefit disproportionately from their own earned income because of the religious institution of *nafaqah*, which stipulates that women should not be asked to use earned income for household necessities.² As a result of this practice, purely self-interested men are less likely to be supportive of female employment than they would be if household members’ labor income were fully pooled, and altruistic household heads - who more fully internalize the utility benefits to women from working - are more likely to support female employment.

While altruism between household members is unobservable, variation in family structure generates two distinct categories of relationships between women in our study and the men with whom they negotiate em-

¹Given divorce laws, married women cannot easily threaten to leave a marriage, while men can divorce instantly without cause, and terms of divorce are substantially worse for women than men. For instance, women have to pay a significant amount of money to obtain a divorce, and fathers remain legal guardians of children., which gives them substantial leverage over physical custody. At the inception of the study, women also required permission from guardians to live alone or open a bank account, and many employers required guardian permission to take a job even though it was not required by law.

²*Nafaqah* specifies that support for household necessities is the sole responsibility of the husband, and is widely interpreted as implying that a woman is not required to pool her earned income. A related argument for why women are likely to value being employed more than men value female employment, akin to that made by Doepke and Tertilt (2009), is that women are likely to gain bargaining power from labor market experience and greater spending autonomy, which would *lower* men’s relative bargaining power, making self-interested men even less likely to support female employment.

ployment: specifically, married women and divorced women with children negotiate employment with husbands and ex-husband co-parents, who are relatives by marriage, whereas widowed and single women (and childless divorcees) negotiate employment with fathers, brothers and sons, who are relatives by blood.³ Hence, we test the prediction that blood relatives are more likely to support a woman's employment – and consequent financial empowerment – than relatives by marriage, due to differences in altruism towards the woman in question, as in Doepke and Tertilt (2009) and Case *et al.* (2000).

In line with these predictions, heterogeneity analysis reveals striking differences across women based on their relationship to male heads. Specifically, employment responses to the reform are concentrated entirely among never-married and widowed women, and childless divorcees, whose employment rates jump to 38% relative to a control mean of 19%. Meanwhile, women who are either married or divorced with children, and thus subject to the influence of a husband or co-parent, actually exhibit the opposite response: women in these categories are 48% *less* likely to be in the labor force once they gain the opportunity to drive. Moreover, the decrease in women's financial control in response to the opportunity to drive is concentrated entirely among married and divorced women with children, suggesting endogenous responses of husbands and co-parents to women's newfound freedom over spending: at endline, only 36% of treated women with husbands or co-parents report being allowed to make purchases up to SAR 1000 (equivalent to USD 265) without family approval, relative to 53% in the control group and 44% of women without husbands / coparents (never-married, widowed or divorced without children) in the treatment group. The consistent patterns across single and widowed women - who differ in both age and presence of children - help pin down the role of male preferences over female employment as distinct from female preferences over working, which are likely to vary with a woman's age and parental responsibilities. The patterns are also robust to controlling for interactions of treatment with age and presence of young children.

Our results on female employment highlight the economic cost of legal gender discrimination in this and similar settings. In addition, the heterogeneous responses and results on controlling behavior provide novel evidence of spousal opposition to female autonomy, shedding light on why legally entrenched gender discrimination is so difficult to change in much of the world despite high potential economic returns, and the role of intrahousehold preferences in mediating female work behavior. They similarly highlight how paternal support for progressive gender norms can buttress political reform, providing novel empirical support for the theoretical model of gender reform put forward by Doepke and Tertilt (2009).

These findings contribute to three main streams of literature. First, we add rigorous experimental evidence to the literature evaluating retrospectively the impacts of changes to women's legal rights (see Doepke *et al.* (2012); Duflo *et al.* (2011) for reviews); to date this literature has produced mixed results, with some studies

³Although ex-husbands are no longer the legal guardians of divorced women, when the couple has children, ex-husbands retain substantial leverage over ex-wives by virtue of their permanent status as legal guardians and default status as administrative heads of household for their children. In the event of divorce, the father also automatically gains physical custody of boys over the age of five and girls over the age of seven, although divorced women can appeal for physical custody of children. However, the threat of losing physical custody to the children's legal guardian (father) is ever-present. For instance, a father can seek to terminate a mother's custody of their child by claiming she is 'incompetent', and the law does not define this term, leaving its interpretation up to the presiding judge.

finding evidence that strong social norms prevent reforms from binding (Roy, 2015).⁴ We build on this literature by leveraging unique experimental variation in whether a woman has any ability to exercise a new legal right. We also provide evidence on why access to this right is a binding constraint to employment only for some women and not others. In particular, we document that variation in intrahousehold preferences mediates women’s responses to their newfound legal right.

Second, we contribute to the literature exploring constraints on women’s economic participation. A broad set of papers studies whether and how state intervention can shift women’s economic participation. In developed countries, there is evidence that women’s labor supply is particularly responsive to fiscal policies such as tax credits and incentives (Blundell *et al.*, 2016; Low *et al.*, 2018; Borella *et al.*, 2023; Guner *et al.*, 2020; Eissa and Liebman, 1996; Schanzenbach and Strain, 2020; Bick and Fuchs-Schündeln, 2017; Smith *et al.*, 2003), and to other instruments of state intervention such as parental leave mandates or non-discrimination laws (Olivetti and Petrongolo, 2017; Kleven *et al.*, 2019; Rossin-Slater, 2018; Ruhm, 1998; Waldfogel, 1998; Lalive and Zweimüller, 2009; Schönberg and Ludsteck, 2014; Zabalza and Tzannatos, 1985). However, we have a more limited understanding of whether these or other means of state intervention can shift women’s workforce participation in developing countries, where more conservative social norms may serve as the binding constraint on women’s autonomy and choices (Bertrand, 2011; Jayachandran, 2015, 2021). Our results also underscore the importance of more widespread barriers to female mobility in reducing women’s employment opportunities in many settings without legal restrictions, linking to a nascent literature on gendered constraints to physical mobility (Borker, 2018; Velásquez, 2019; Siddique, 2022; Jacoby and Mansuri, 2015; Kondylis *et al.*, 2020), a subset of which tests directly whether improved transport services can increase women’s labor force participation (Christensen and Osman, 2020; Muralidharan and Prakash, 2017; Field and Vyborny, 2022; Cheema *et al.*, 2020).⁵ We add to this literature by showing that relaxing legal barriers to driving can immediately improve women’s physical mobility, which is not obvious in settings with heavy restrictions on female movement and work.

Third, our results speak to the literature on social backlash that can arise from removing legal restrictions on women’s economic and social participation. Several recent studies have demonstrated how policy and legal changes can lead to backlash when they threaten existing identities, norms or interests (Blumenstock *et al.*, 2022; Fouka, 2020; Wheaton, 2021; Abdelgadir and Fouka, 2020; Gottlieb, 2016; Mehmood *et al.*, 2022; Bursztyn *et al.*, 2020; Andrew *et al.*, 2022; Brule, 2018; Anukriti *et al.*, 2022). This stands in contrast to evidence of similar changes in the political or legal environment leading to more progressive views among men or reductions in domestic violence (Beaman *et al.*, 2009; Delaporte and Pino, 2022; Kuipers, 2020). Similarly, challenges within the home to existing gender norms can subject women to backlash in the form of marital friction, dissolution or violence (Bertrand *et al.*, 2015; Folke and Rickne, 2020; Ashraf *et al.*, 2014; Bobonis, 2011; Angelucci, 2008;

⁴In terms of identification, scholars have largely relied on natural experiments in which reforms take effect differentially across space or cohorts help to identify the impact of specific legal reforms, such as inheritance laws (Deininger *et al.*, 2013; Roy, 2015).

⁵A related literature documents women are more sensitive to commuting time in employment location choice, speculating that domestic burdens and childcare increase the cost of commute (Rosenthal and Strange, 2012; Kwon, 2022; Le Barbanchon *et al.*, 2020; Liu and Su, 2022; Kawabata and Abe, 2018; Chauvin, 2018; Farre *et al.*, 2022).

Bobonis *et al.*, 2013; Heath, 2014; Erten and Keskin, 2021; Bulte and Lensink, 2019; Luke and Munshi, 2011; Tankard *et al.*, 2019; Chowdhury and Bhuiya, 2004; Tur-Prats, 2021; Tertilt and Van Den Berg, 2012; Anderberg *et al.*, 2016; Baranov *et al.*, 2021). However, the evidence is again mixed, with some studies finding positive or null effects or heterogeneous effects across subgroups (Roy *et al.*, 2019; Angelucci, 2008; Haushofer *et al.*, 2019; Heath *et al.*, 2020; Bobonis *et al.*, 2013; Hidrobo and Fernald, 2013; Hidrobo *et al.*, 2016).

Our experiment and the variation in family structure in our sample allow us to isolate the subgroup of women who is best positioned to take advantage of the reform by increasing employment, and document how this response correlates inversely with changes in spending autonomy experienced by women in response to the reform. The fact that women with a spouse or ex-spouse experience reductions in financial autonomy and no change in employment when they get access to a driver’s license suggests that men are more open to granting daughters (or mothers or sisters) economic rights than wives, as has been posited in the literature (Doepke and Tertilt, 2009; Tertilt *et al.*, 2022).⁶ Because much of the work on backlash has focused on intimate partner violence, a margin relevant to women who are currently partnered, the relative openness to gender progressive reforms of fathers compared to husbands has received comparatively little attention in the empirical literature on gender reform and backlash.

The remainder of the paper proceeds as follows. We describe the context and sample in greater detail in Section 2. Section 3 describes the intervention and experimental design. Section 4 lays out anticipated responses for women overall and by family structure. Section 5 describes our data and empirical framework. Section 6 presents results, and Section 7 concludes.

2 Setting

Our study takes place in the city of Riyadh, in the Kingdom of Saudi Arabia. With respect to gender parity, Saudi Arabia has consistently ranked in the bottom decile of countries worldwide. In 2017, Saudi stood at 138th overall out of 144 countries analyzed in the World Economic Forum’s Global Gender Gap Report, and 142nd in the sub-category of female economic participation and opportunity (World Economic Forum, 2017).⁷

Legally, male family members have substantial leverage over women in this setting. Every Saudi woman has an official male guardian: her father until marriage, then husband; divorced and widowed women come under the legal guardianship of their fathers, sons or brothers. Historically, guardians have had extensive legal say over women’s lives, including whether they may travel and where and with whom they choose to live, as well as official status as “head of household” for all administrative interactions with the government such as birth registration and children’s school enrollment (KSA Bureau of Experts, 1986, 2000; US Department of State, 2022; AlRiyadh, 2019).⁸ Although women could legally obtain employment without the approval of a

⁶A related literature posits that having daughters leads to more gender progressive attitudes and behavior (Washington, 2008; Oswald and Powdthavee, 2010).

⁷The index, which has been analyzed since 2006, is constructed from four sub-indices measuring gender gaps in a) economic participation and opportunity, b) educational attainment, c) health and survival, and d) political empowerment.

⁸Note that some of these restrictions were lifted for the first time midway through our study, detailed throughout this section. However, our assessment is that these changes are too recent to have impacted our predictions, and the legal environment at the

male guardian at the onset of this study, many employers continued to seek the written approval of the male guardian before offering a woman a job.⁹

The legal power of male family members means that Saudi women have had very little bargaining power in marriage, particularly as conceptualized by the notion of “threat point”. Women in Saudi have little opportunity to leave a marriage (or threaten to leave) because they cannot unilaterally initiate no-fault divorce, and also face substantial financial costs and uncertainty in doing so. While a man can unilaterally divorce his wife – until 2020 it was even possible for him to do so without his wife’s knowledge (US Department of State, 2022) – a woman who wishes to initiate a divorce without the husband’s consent must go through the *khul’* process, which requires a financial payment to the husband, court approval, and potentially lengthy delays, or else seek an annulment by proving to the court that she suffered harm (Sharif, 2022; Saudi Gazette, 2012). As a result, less than 1% of divorces in Saudi Arabia are wife-initiated *khul’* divorces (Daqaq, 2022). Prior to a 2021 reform, women also needed guardian permission to live independently, making it difficult even to threaten separation (Gulf News, 2021).

Moreover, even divorced women have limited bargaining power with their ex-husbands because of men’s legal status as child guardians. While women’s right to physical custody over young children was codified in 2022 after our study was complete, only fathers have the right to *legal guardianship* over children. Thus, a divorced father has the right to determine where the child will live (including moving outside the country without the mother’s consent) and make financial decisions related to children. The father can also take back physical custody if the mother remarries or if a court determines that the mother is incapable of safeguarding the child or of raising the child in accordance with the appropriate religious standards, which could potentially be influenced by the mother’s employment (Saudi Gazette, 2022; Morley, 2023; Library of Congress, 2022). Moreover, a woman could only formally register as the “head of household”, with the ability to carry out official procedures for their children such as enrolling them in school, beginning in 2019 (US Department of State, 2022). Thus, the preferences of the legal guardians of a woman and her children can be assumed to have been critical in determining her employment behavior, and a woman’s marital and parenting status determine which man assumes that role. Table A1 lays out details of women’s guardianship and head of household rights by marital status.

While women are subject to substantial influence from male family members in deciding whether to work, the religious doctrine of *nafaqah* also lays out the culturally important norm that financial support for the family’s needs is the responsibility of the husband, and not the wife (Esposito, 2003; Siddique and Gul, 2019; Library of Congress, 2022). This is widely interpreted as implying that, if a woman does work, her income is her own and she does not have to provide for her family’s financial needs (Arab News, 2005; Khan, 2021; Amini, nd; Schatzmiller, 2019; Khan, 2005).¹⁰ As a result of this and the official requirement to pay workers via direct deposit (International Labour Organisation, 2020), in Saudi Arabia, working women typically have their

onset of the study is most relevant for circumscribing behavioral responses to the reform.

⁹While a 2019 reform made this practice illegal, no penalty was established (Debees, nd).

¹⁰Note that, while in principle this doctrine gives women more autonomy conditional on working, it also supports traditional gender norms in that it encourages households to respect the traditional division of labor.

own bank accounts in which they receive salaries and social insurance, with 63% of *all* women having a bank account in 2021 (World Bank, 2021a). However, the principle of the woman’s independent decision-making over her earned income is not enforceable in practice, and until 2017, women needed the permission of a male guardian to open (and hence maintain) a bank account (Government of KSA, 2017). Indeed, in our sample, 50% of working women indicate they cannot always make independent purchasing decisions.

Starting in 2016, the government announced a series of reforms (the “Vision 2030” plan) with an explicit goal of increasing female labor force participation (FLFP) from 22% to 30% over a fourteen-year period (Government of Saudi Arabia, 2016; Alshuwaikhat and Mohammed, 2017). Together, these reforms have positioned Saudi as one of the world’s most transformed economies for women (World Bank, 2020, 2021b). The initial announcement of the broader Vision 2030 plan was followed by individual announcements about specific reforms over the following years, including a number of policies aimed at reducing legal barriers to women’s social and economic inclusion. Specific measures included a gender-neutral quota system for Saudi nationals in private sector employment, resulting in a substantial increase in female employment (Peck, 2017); ratifying several civic and labor laws to protect women’s rights in employment and criminalize gender pay discrimination; issuing a workplace anti-harassment law; and granting women the ability to hold a passport, travel and take up a job without a male guardian’s permission (Permanent Mission of KSA to UN, 2022).

One particularly high-profile reform that was included in the Vision 2030 plan was lifting the Kingdom’s 61-year ban on women obtaining driver’s licenses, which was announced in September 2017, with an effective date of June 2018.¹¹ This reform was motivated with the goal of enabling women to more easily commute to work. Even accounting for the cost of purchasing a vehicle, driving is a less costly option for many commutes in Saudi Arabia than alternatives such as ride-hailing (e.g. Uber) given that public transport options are extremely limited (Field *et al.*, 2018).

The decision to lift the driving ban was widely heralded as a major step towards female economic inclusion. However, there was also skepticism about whether granting women access to driver’s licenses would increase female employment. There are several reasons that granting women access to a driver’s license may fail to influence their employment behavior. First, the opportunity may not immediately change women’s propensity to drive or work, as comfort with both may depend on social norms that only change slowly. For instance, there may initially be a high degree of stigma associated with a woman driving alone, such that women are reluctant to drive regularly to work. Relatedly, safety and harassment concerns on the road may discourage women from driving even when they are legally allowed to, especially at early stages of policy reform (Ali *et al.*, 2021).

The same may be true of working. When a sample of Saudi female employees in the private sector were asked in 2018 about the most significant barriers to women working, the majority cited social stigma (Alnahda Society, 2019). Demand factors, including workplace discrimination and limited job opportunities for women, may also serve as binding constraints on Saudi women working. Miller *et al.* (2019) report that 73% of firms in the private sector in Saudi are all-male, and Eger *et al.* (2022) describe negative subjective beliefs about

¹¹The initial announcement of the Vision 2030 plan in 2016 was widely expected to result in the lifting of the ban on women’s driving, but the timing was uncertain.

women’s abilities among managers in Saudi firms.¹²

Perhaps most importantly, family disapproval may pose a binding constraint on driving or working. Many women in our study sample reported that male family members and men in their social networks had negative attitudes about women working, with more than half reporting that most men they know would disagree with the statements “It’s OK for a woman to have priorities outside the home”, and “It’s OK for mothers to work” (Figure A4).

3 Experimental design

Our empirical study was designed to assess the net impact of access to a driver’s license on women’s labor force participation. A key complication in investigating the impact of any nationwide policy reform, including the lifting of the female driving ban, is constructing reasonable comparison groups of women who are and are not affected by the reform. To do so, we experimentally vary whether women in our study have access to a driver’s license once all women have been granted the legal right to drive. Our study began in the wake of the driving reform, with treatment beginning 12 months after the reform was enacted (Figure A1). At that time, there were only seven driving schools for women sanctioned by the government, and just one in Riyadh, which had a waitlist of several months. Obtaining a license required extensive in-person training sessions, including 30 hours of classroom and in-vehicle instruction; moreover, the training fee was set at 3,000 SAR (\$800 USD), almost half the average monthly salary for women and six times the fee for men (Saudi Gazette, 2019; AP News, 2018). This particular feature of the regulatory environment enables us to identify the causal impact of gaining access to a driver’s license by randomizing rationed spots in the training course and subsidizing the expense of learning to drive.

Specifically, with the cooperation of the Saudi government, we designed an experimental intervention that granted free and immediate access to driver’s education at the Saudi Driving School in Riyadh for a randomly chosen set of women who reported interest in driving. In particular, we ran a field experiment in which both immediate entrance to the driving school and subsidies covering all costs associated with completing the driver’s training course and acquiring a license were randomly offered to 375 of the 606 women enrolled in our study. The subsidy covered the full cost of the official driver’s training course, all license exam and transactions fees, and prearranged travel to and from training.

While this particular treatment does not perfectly mimic the policy reform of granting individuals the right to drive, the rationed enrollment in driver’s training centers and high cost barriers to female attendance arguably constituted a “partial” implementation of the reform on the part of the Saudi government at the time of our study. In most parts of the developed world, driver’s training is not rationed and is heavily subsidized by the government. Moreover, even in Saudi, the cost for men is substantially lower. This suggests that the initial barriers to entry were possibly attempts by the government to actively slow the pace of reforms, and are likely

¹²Similarly, Miller *et al.* (2022) show that the cost of integrating women into all-male firms can be binding for many, particularly where social norms impose gender segregation.

to fall over time. Hence, one interpretation is that our intervention shifts a randomly chosen set of women into a “full reform” environment.

The intervention was implemented by Alnahda Society, a Riyadh-based non-profit organization dedicated to improving the well-being of Saudi women through programming and policy research.¹³ Women in our study were recruited from among the beneficiaries of social programs offered through Alnahda and two sister organizations. All three NGOs target needy families, in particular widowed and divorced mothers, for programs such as financial support, skills training, counseling, and legal aid. As such, women who enrolled in our study are disproportionately poor, living in households with an average monthly income of 2,500 SAR (667 USD), less than a quarter of the national median income (King Khalid Foundation, 2013).

Table A2 compares our experimental sample with Saudi population statistics. Other than income, women in the study sample have a similar age profile to the national population, but they are less likely to be married and much more likely to be divorced or widowed. This balanced distribution of women across marital status categories is a feature of our sample that is particularly useful for understanding how women’s position in the household mediates female employment responses to the revocation of the driving ban. Women in our experimental sample are also more likely to be in the labor force than the national average, reflecting our partner organizations’ emphasis on assisting women in situations of economic hardship. Another survey conducted by Alnahda of its beneficiary population shows that employed beneficiaries primarily work in the service industry (retail, cleaning, cooking), and a smaller share work in schools or for NGOs. This income level in the Saudi context implies that most households do have a car, but share access to it among multiple adults (Table A3).¹⁴ Interim survey data from a subsample of the control group confirm that cars are the main mode of transportation (Table A4): only a small minority walk or take the bus, and most women are driven by a family member or paid driver, or rely on taxis and rideshares. Thus access to a driver’s license has the potential to be a binding constraint for this population.

Our experiment thus obtains an estimate of the impact of access to a driver’s license on low-income women in Riyadh. While treatment effects are arguably anticipated to be larger for this population than they would be among wealthier women, this nonetheless represents a non-trivial proportion of the Saudi population and one for whom access to employment is particularly valuable. Hence, the degree to which our estimates of employment effects deviate from the population-level impact of the reform depends on two factors outside the scope of our analysis - the extent to which the reform alone impacts work behavior, absent changes in licensing, and the degree to which the employment response differs across income groups.

It is also relevant to note that, as is generally the case in program evaluations, women who enrolled in our RCT sample are those interested in obtaining a license, at least when it is offered with assistance. Hence, the correspondence between our estimates and the population-level impact will also depend on the fraction of women who are interested in driving. However, population-level data indicate that interest in driving was

¹³Financial support for the study was received from Uber’s Masaruky Initiative.

¹⁴The typical monthly lease payment on a car is 889 SAR (Field *et al.*, 2018), a large fraction of the average 2,500 SAR monthly household income of Alnahda beneficiaries.

extremely high at this point in time: according to an Arab News/YouGov poll conducted in 2017, 80% of women reported a desire to drive and apply for a license pending the change in law (Arab News, 2018). Similarly, 85% of Alnahda beneficiaries indicated interest in driving in the baseline eligibility screening.

Recruitment took place in six cohorts over 15 months, from May 2018 until August 2019. Alnahda conducted initial enrollment with each of the 615 adult woman from its full roster of beneficiary households at a series of regular beneficiary meetings. All beneficiaries were administered a short survey collecting key baseline characteristics, including age, car ownership, and interest in registering for driver’s training. Ninety-five women (15%) were dropped because they indicated that they would not want to register for driver’s training even if the cost were reduced, and another 54 (9%) were dropped because they indicated they had already succeeded in enrolling in driver’s training, leaving a total of 466 Alnahda beneficiaries.¹⁵ To increase the experimental sample, recruitment was expanded beyond Alnahda’s beneficiary pool to two partner organizations, who nominated a further 231 women interested in driver’s training from their beneficiary rolls. Among this pool of 697 women, 91 were dropped from the study sample due to protocol problems (such as individuals who could not be reached again after baseline and so were not assigned to a treatment arm, or who completed the baseline after another household member had already been assigned to a treatment arm). The final study sample consisted of 606 participants, who were randomly assigned to either treatment or control.

Of these, 375 participants (62%) were allocated to the treatment group, while 231 (38%) were allocated to the control group.¹⁶ Randomization was stratified by recruitment cohort, age group, car ownership, and above-median anticipated likelihood of driving. Because the study allowed enrollment of more than one family member, randomization was conducted at the household level to minimize spillovers.¹⁷

Participants assigned to the treatment group were offered driver’s training in one of two cycles, July-September 2019 and October-December 2019. Individuals assigned to the treatment group were invited to attend an event organized by Alnahda in which they were enrolled in the driver’s training course. In total, three sign-up events were organized between May and August 2019.¹⁸

4 Anticipated responses

Affording women the right to drive was heralded as a major step towards female economic advancement, with the potential to encourage workforce participation through several channels. First, the announcement of the lifting

¹⁵Note that a 9% rate of enrollment for the heavily rationed spots in driver’s training might be higher than expected given how few spots were available at the time the study was initiated. This could reflect a small proportion of women who have a strong motivation and family or other support to obtain a license; note that at the two year follow-up, only 10% of the RCT control group had received a license. The baseline enrollment rate could also reflect several different sources of reporting error: prospective enrollees may be mistaking wait-list sign-up for enrollment in a course, or they may be making up an excuse for non-participation. We dropped all women who reported being currently enrolled in driver’s training.

¹⁶Women were assigned to treatment in this ratio in order to meet the obligation to the funder to enroll a target number of women into the training.

¹⁷Alnahda enrolled all interested women within all its beneficiary households, such that 23% of households in our sample include more than one respondent; including or excluding an indicator to control for this feature, does not change our results.

¹⁸In addition to our main treatment, we also cross-randomized a light-touch information treatment informing respondents of the availability of a government subsidy for rideshare costs (Uber). Because of limited evidence of effects on expected commute cost at the time of our short-term interim follow-up survey, we registered our second stage pre-analysis plan based on testing for effects of the main treatment, rather than the cross-randomized treatment. Appendix B provides more information about this component and the original specifications described in the first stage pre-analysis plan.

of the ban could change expectations around female employment and mobility, and potentially increase firms' willingness to hire women. For instance, after the reform firms may be less likely to screen out female candidates expecting that they will have difficulty arriving at work consistently. The government's endorsement of women's independent mobility could also shift social norms around female labor force participation, potentially lowering the immediate or anticipated stigma working women face, or reducing workplace discrimination and sexual harassment. If norms change as a result of the legal amendment, or if the policy prompts discussion that leads women to update perceptions about norms, we might observe labor market responses to the policy even in the absence of changes in the ability to get a license. Moreover, even if norms change slowly, it is possible that female labor force participation responds to the mere prospect of change.

Importantly, our experimental design holds constant this entire category of channels, as all women in the study experienced the same changes in actual and expected labor market conditions and social environment as a result of the legal reform. Instead, our experiment varies whether respondents have the immediate opportunity to exercise their newfound legal right to drive, holding the social environment constant. As such, the treatment effect we estimate encompasses three principal mechanisms through which the opportunity to drive might impact female employment.

Most directly, access to a driver's license reduces women's expected financial and time costs of commuting to work and/or searching for jobs. The reduction in commuting costs could affect women even if they do not have regular access to a car if they anticipate negotiating access to a shared car or purchasing one in the future. Driving avoids the time costs incurred by male family members acting as chauffeurs. Moreover, the possibility of driving oneself to work, even sporadically, can increase willingness to accept a job by reducing concerns over absenteeism or tardiness due to intermittent commuting barriers. For instance, relying on male family members may entail a fair degree of commuting uncertainty if they are not always willing and available to chauffeur. The reduction in the cost of commuting can be thought of as changing the effective female wage. Finally, lower transport costs can potentially ease financial barriers to job search, either directly (e.g. helping applicants get to interviews) or indirectly by expanding social networks. This can be thought of as reducing the fixed cost of working or increasing a woman's expected wage if she can now search over a larger set of jobs.

Second, access to a driver's license better enables a woman to travel unaccompanied more generally, which potentially increases the value of leisure time but also the value of consumption. Greater spending opportunities increase the household value of consumption, and therefore the household's incentives to choose work over leisure, while greater opportunities for leisure decrease the incentives to work in response to the opportunity to drive, such that the net effect of these channels is ambiguous.

Third, the opportunity to drive and earn own income gives a woman greater autonomy over spending, in part because of the Islamic legal principal of *nafaqah*. As discussed in Section 2, *nafaqah* stipulates that men must pool income, but women should not be asked to do so.¹⁹ As shown in Table A4, the most common type

¹⁹A similar argument could be made based on the fact that, if a woman drives herself to shop, it is more difficult for male household members to monitor her spending. However, given that male guardians retain control over women's bank accounts, we ignore the possibility that women driving exacerbates information asymmetries with respect to spending (or working).

of errand for women in the control group is “personal shopping”, which 21% of women report doing on the reference day - exactly the same proportion as those who report commuting. Many other categories of errands involve personal consumption such as meeting friends for meals. If household members do not pool income, the higher autonomy of spending that a woman gains from earned income increases her personal incentives to work, and decreases the value of her work for other household members.

In some settings it is also possible that the legal right to drive increases a woman’s bargaining power within the household by improving her outside options in the event of divorce or separation (Aizer, 2010). If her bargaining power increases, a woman may find it easier to negotiate favorable employment outcomes (which could be positive or negative, depending on preferences) with family members once the reform has taken place, even if she does not ultimately drive herself to work. The bargaining environment can also potentially apply to divorced spouses bargaining over expenditures on children as in Del Boca and Flinn (1994); Welling and Bearance (2002).

However, Saudi Arabia is a unique setting in that women are arguably unable to credibly threaten to leave a marriage because of legal barriers to female autonomy. As discussed in Section 2, married women do not have the legal right to initiate no-fault divorce, and face substantial financial costs and procedural obstacles to initiating divorce at all. This means that the right to effectively walk out on a marriage almost exclusively pertains to the husband. Perhaps most importantly, ex-husbands retain an uncommon degree of leverage over their ex-wives through their default status as child guardians and heads of household, making divorce a particularly bad outcome for women. Were a divorced woman to take advantage of new opportunities to commute independently and work, her ex-husband could in theory ask a court to take away her physical custody of their children. The lack of a credible outside option, in which the woman can exit the partnership and simply choose to work and live independently, suggests that changes in bargaining power are unlikely to be an important channel through which the right to drive impacts women’s employment behavior.

Because women’s bargaining power in the household is so severely restricted, for simplicity we assume that the household decision over female work can be reasonably approximated by a unitary framework with a male acting as the sole decision-maker over household employment allocations. While both the lower commuting costs and greater spending opportunities should increase the decision-maker’s incentives for a woman to work, the latter channel could simultaneously discourage self-interested men from allowing women to take jobs because they are less able to benefit directly from women’s income, making the net effect ambiguous. This underscores the importance of looking for heterogeneity in treatment effects by men’s preferences over women’s work, and in particular, how much men in the household internalize the benefit to women of working. In this context, employment responses will only be observed when the male decision-maker is sufficiently supportive of women working outside the home. Moreover, when men are not supportive, we can expect to observe endogenous responses to women’s newfound freedom to drive, such as tightening of financial control, that disincentivize female employment.

To test this, we distinguish between employment responses among women who are subject to the influence

of a husband or co-parent (married women and divorced women with children) versus those whose behavior is governed exclusively by their blood relatives (never-married women, widowed women, and childless divorcees).

²⁰ This difference is relevant because, as in Doepke and Tertilt (2009); Tertilt *et al.* (2022), it is reasonable to assume that individuals are less likely to feel altruism towards spouses than offspring.²¹ As outlined above, the altruism of the male decisionmaker towards the woman matters in this setting because of the Islamic legal principle and corresponding social norm that women retain control over earnings. Because of this, self-interested husbands are less likely to choose female employment even when the woman’s effective wage rises. Fathers, on the other hand, are more likely to internalize the benefit to their daughters of female consumption, so are more likely to endorse women’s employment when the effective wage rises.

An auxiliary prediction that we test with our survey data is that treatment is expected to lead male family members to substitute towards alternative forms of expenditure monitoring to compensate for women’s increased autonomy over earned income, and this tendency should be stronger for husbands and co-parents than it is for fathers and sons. Indeed, if some men respond by tightening financial monitoring and control, we could even observe women who would otherwise have sought employment exiting the labor force, as their incentive to work is reduced. In our data, we capture increased attempts to monitor spending by asking women to report the degree to which they are required to seek permission before making purchases.

It is important to note that access to a driver’s license could also affect FLFP through alternative channels that we have not emphasized thus far. First, the experienced independence of driving could lead to changes in a woman’s own preferences and attitudes. As described earlier, the ability to get a license complemented several other reforms that took place around this time which together expanded in radical ways women’s opportunity for advancement. While these reforms were experienced by all women in this setting, in this context, the experience of driving alone has the potential to make these new opportunities particularly salient. Second, women’s social networks might expand as a result of lower mobility costs, thereby either influencing her own norms and beliefs, or lowering her cost of finding employment. We test for evidence of both channels in Section 6.4 by assessing whether treatment altered women’s first-order beliefs about gender norms, and also whether treatment increased women’s access to social networks.

5 Followup data and analysis

The main follow-up survey was rolled out between July 2021 and February 2022, 1.5 - 2.5 years after the intervention (see timeline, Figure A1). The survey was conducted by telephone, and participants were offered prize drawings and gift vouchers as an incentive to participate.²² The survey included questions on mobility, employment and job search, decision-making power, first- and second-order gender attitudes, and social and civic

²⁰Widows may also have sons or brothers as guardians, but it is plausible that the difference in altruism applies to blood relatives versus non-blood relatives more generally.

²¹In fact, a very analogous point is made by Doepke and Tertilt (2009), who argue that men more likely to endorse progressive gender roles that give women more financial independence when it comes to their daughters compared to their wives.

²²We carried out a short-term interim follow-up survey in early 2020, a few months after the completion of the intervention; Appendix B presents outcomes from the interim survey.

engagement. The final response rate to the main follow-up survey was 80% and is balanced across treatment arms (Table A5). Table A3 shows mean values of key baseline characteristics among survey respondents, confirming that characteristics are overall well balanced across the treatment and control groups.

The survey elicited information both on the respondents' driving behavior and on mobility as a whole. Enumerators asked each respondent to report whether she had started the driver's training course, whether she had completed it, and whether she had received a license. Regardless of whether the respondent held a license, she was asked about her frequency of driving in the last month. While 19% of respondents in the control group had started the training by the time of endline, only 10% had received a license. Despite this, 34% of the control group reported driving, revealing that a non-trivial number of respondents drive despite not having a license.²³

To distinguish effects on driving from effects on independent mobility, respondents were asked about their chaperoned and unchaperoned trips outside the house in the last seven days (i.e. travel with or without a male family member *mahrem*). Overall, women are fairly immobile in this setting. The bottom quartile of respondents in the control group report leaving the house only two or fewer times in the last week, and about 5% had not left the house at all. However, women in the upper quartile are mobile enough for regular commuting: 27% of respondents reported leaving the house six or more times in the last week. Of course, independent mobility is quite a bit lower. About half of women in the control group reported that they had not left the house without a male chaperone even a single time in the last week, and only about one in five reported leaving the house without a chaperone on a daily basis (more than 6 times over the last 7 days).

Respondents were also asked if they were employed and whether they were searching for a job, and those who reported any search were asked to report the number of job applications they made in the last month. Twenty-one percent of respondents in the control group reported being employed, 57% unemployed and searching, and 22% were out of the labor force (neither working nor searching). Women in the control group experienced an upward trend in employment from baseline to follow-up, consistent with generalized employment responses to the broad set of gender reforms discussed in Section 2: 21% of women in the control group are employed at follow-up compared to only 16% at baseline (Table A3).

To capture the intra-household constraints women might face, we collected two questions on permissions, which were asked as a Likert scale in order to capture more granular variation. Among women in the control group, 51% "completely disagree" with the statement "I can leave the house to meet a friend without asking anyone in my family first", while only 24% "completely agree". For the statement "I can make a purchase of 1000 SAR (approximately USD 265) without asking anyone in my family first", 31% of women in the control group "completely disagree", while 48% "completely agree". Note that the latter measure does not simply proxy for women's individual income: 32% of *working* women in the control group somewhat or completely *disagree* that they can make purchases without permission, despite the fact that this purchase amount is under a third of the median working woman's monthly income in this sample. To convert these into outcome variables, we construct binary indicators for above-median responses on the Likert scale for each statement. For the first

²³Anecdotally, driving without a license is more common for short trips within the respondent's neighborhood where they are less likely to encounter traffic police than for longer trips on main roads, where traffic police patrol and check licenses regularly.

statement this above-median split corresponds to a “neither agree nor disagree”, “agree” or “completely agree” response, while for the second statement it corresponds to a “completely agree” response.

In order to characterize participants’ first-order gender attitudes and second-order beliefs about others’ attitudes, women were asked to rate their level of agreement on multiple statements invoking the perceived role of women in Saudi society, including: “On the whole, men make better business executives than women do”, “A woman’s priority should be in the home and with her family”, “When a mother works for pay, the children suffer”, “It’s OK for me to put my own needs above those of my family”, and “The government should allow a national women’s soccer team”. They were also asked “What is the ideal age for a woman to have her first child?”. The first three of these questions were used to capture participants’ second-order beliefs among male family members as well as men and women in their social network. Specifically, participants were asked how many people in the above-mentioned groups would “somewhat” or “completely” agree with each of the three statements. Figure A4 shows descriptive statistics from the control group on these outcomes. There is substantial variation in attitudes across the sample. In general, respondents’ second-order beliefs about males are more conservative than their beliefs about females, and reports of male family members’ beliefs are more polarized than reports about male social contacts, which makes sense if social contacts represent a wider distribution of political views.

Finally, we collected measures of social and civic engagement, including the number of people the respondent had met in the past seven days, the number she had spoken to on the phone, whether she plans to vote in the upcoming election, and whether she is interested in signing up for Alnahda volunteering and leadership programs.

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We registered all outcomes presented in the paper in the pre-analysis plan (PAP) on the AEA RCT Registry (available ungated at AEARCTR-0005551) in two stages: a first stage before the short-term interim follow-up survey, and a second stage before the main follow-up survey. The paper presents outcomes from the main follow-up survey, following the second stage of the PAP. Appendix B describes the PAP in more detail, including variations between the PAP and our main analysis.

Throughout our analysis, we estimate a simple intent-to-treat (ITT) specification:

$$Y_{ij} = \beta_0 + \beta_1 TREAT_i + \gamma X_i + \mu_j + \epsilon_{ij} \quad (1)$$

where Y_{ij} is an outcome of interest for respondent i recruited in cohort j ; X_i is a vector of baseline controls prespecified in the PAP, including all variables in Table A3: age (in bins), indicators for each education level, indicators for employment status, marital status (indicators for married, never-married, and widowed), household size (number of members), and number of cars owned (indicators for one car and for more than one car); and μ_j are fixed effects for the randomization cohort. Standard errors are clustered at the household level, the unit of randomization.

²⁴The enumerator provided further information about these programs over text as a follow-up to the survey, and tracked the number of visits to each individual’s trackable link by the respondent or others with whom she shared the link.

6 Results

6.1 Individual responses to treatment

Granting women free and immediate access to driver’s training had a large first-stage effect on female driving. Table 1, Columns 1-3 show our ITT estimates of the impact of treatment on women’s *de facto* access to driving. The intervention leads to a dramatic and persistent shift in the probability of enrolling in driver’s training and obtaining a driver’s license. At follow-up, two years after treatment, only 19% of respondents in the control group report having started driver’s training, while 81% of those in the treatment group had done so (Column 1). Likewise, assignment to treatment increases the probability of successfully completing the training and obtaining a license (Column 2): 54% of respondents in the treatment group had received a driver’s license by follow-up, while only 10% of respondents in the control group had done so. Note that the probability of successful completion conditional on starting the training is also higher, at 67% in the treatment group versus 53% in the control group, which likely reflects the fact that women in the control group who started training by the time of follow-up were likely to have started the course much later than those in the treatment group.²⁵ The incomplete take-up in the treatment group is striking given that the RCT sample only included women who had indicated interest in driving in the baseline survey, and provides immediate evidence of relevant constraints to women driving beyond individual openness to getting a license.²⁶

Column 3 in Table 1 explores treatment impacts on women’s use of their newfound right to drive. About one-third of respondents in the control group had driven in the last month (column 3). The experimental results reveal that access to a license had a large impact on women’s experience on the road: women in the treatment group are 61% (20 p.p.) more likely to have driven in the previous month.

Columns 4-6 of Table 1 examine impacts on physical mobility beyond driving. Women in the control group report leaving the house on average five times in the last seven days, and report leaving without a male chaperone on about 43% of these occasions. Effects on women’s frequency of travel outside the home are positive but imprecisely estimated; the point estimate suggests a 14% increase in trips, the equivalent of one trip per 10-11 days (column 4). Treatment increases the share of trips that treated women make without a male chaperone by 19% (8.4 pp) (column 5). As shown in Figure 1, and reflected in Column 6 of Table 1, treatment essentially shifts 18% of women from making *all* trips with a male chaperone to traveling unaccompanied several times a week or more. About half of women in the control group have not left home unchaperoned a single time in the last week; this drops to 41% in the treatment group. These patterns are reflected in similar shifts in the frequency of driving and frequency of travel overall (Figure A3, Panels A and B).

Table 2 shows treatment impacts on women’s economic inclusion, including labor force outcomes and financial control. Overall, treatment induces a large and significant increase in women’s rate of employment at follow-up.

²⁵At the interim followup, a few months after treatment, 5% of control group surveyed had started the training and 74% of the treatment group had done so.

²⁶Anecdotally, difficulties with taking time away from family commitments to attend the fixed training schedule, getting permission from male family members, and lack of confidence in their own ability to drive were among the reasons treated respondents did not participate in training.

Specifically, women assigned to driver’s training are 35% (7.4 p.p.) more likely to be employed at follow-up relative to the control group.²⁷ Most of this transition appears to be a shift out of unemployment (column 2) rather than out of non-participation (column 3). While women in the control group experience an upward trend in employment from baseline to follow-up (an increase from 16% to 21%), consistent with generalized employment responses to the broad set of gender reforms meant to encourage female employment discussed in Section 2, access to a driver’s license dramatically increases employment over and above the control group trend.

Like any change in the cost of mobility, access to a driver’s license simultaneously reduces the cost of in-person job search (such as travel to enquire about vacancies, drop off CVs, and attend interviews) as well as the cost of regular commuting to and from work. Figure A3, Panel A, shows that treatment shifts women from never driving to driving a few times a week or daily, consistent with a commuting cost effect. In addition, if search costs were the key constraint, we might expect to see women moving from out of the labor force into unemployment, which does not occur (Table 2, columns 2 and 3); or to increase on-the-job search, which also does not shift (column 4). This suggests that the cost of regular commuting, rather than the cost of in person job search, may be a more central constraint for women in our setting.

Interestingly, the point estimate on labor force participation (Table 2, column 3) reveals that a slightly larger fraction of treated women are out of the labor force at endline compared to control group women, suggesting potentially heterogeneous treatment effects through which some women shift from unemployment to non-participation as a result of treatment.

6.2 Intra-household responses to treatment

We next examine how access to a driver’s license changes household restrictions placed on women’s mobility and economic independence. As outlined in Section 4, if women lack a credible outside option, household members may respond to women’s increased opportunity to drive by simply imposing new restrictions - such as monitoring bank account use - that reduce women’s incentive or opportunity to work.

To explore these effects, Table 2, Columns 5-6 test whether the treatment affects independent decision-making power over mobility and expenditure. It is first worth noting that women in this setting face a low degree of control over movement and spending: only one third of control group respondents “somewhat” or “completely” agree that they are allowed to leave the house to meet a friend without asking for permission, and less than half completely agree that they can make a purchase without permission from family members. Our estimates indicate that access to a driver’s license has no detectable effect on women’s ability to leave the house without permission, but results in a large and significant *decrease* in their freedom over spending: the proportion of women who say that they can make a purchase of 1000 SAR (approximately USD 265) without permission from a family member drops by 19% (9 p.p.; column 6). This result is consistent with the idea that women’s

²⁷Ex ante, we anticipated that the intervention would shift women from being inactive in the labor force into search; thus our pre-analysis plan specified job search outcomes. In fact, the main margin of effects we find is from unemployed searching into employment.

newfound ability to travel independently and spend without direct observation by male family members led male family members to substitute towards alternative forms of expenditure monitoring and control.

Endogenous responses by male household members may also manifest as a change in men’s attitudes and beliefs about gender roles. Although we do not collect survey data from men, we investigate this possibility by examining treatment effects on women’s beliefs about the attitudes of men and women in their social network (Table A6). We construct an index from the gender attitude questions and report impacts on this index along with the individual items. Individual survey items are coded such that positive values reflect more “progressive” attitudes favoring an expansion of women’s rights and roles. We observe no impact on second order beliefs about other women’s attitudes (Panel A), but we see suggestive evidence of negative effects on respondents’ beliefs about men’s gender attitudes (Panels B-C). It is relevant to note that these results may reflect a reactionary change in men’s attitudes, who may either update their beliefs as they experience female family members and contacts driving and working or seek to rationalize their resistance to women joining the labor force. However, they could also be explained by women updating their beliefs about men’s attitudes once they experience negative reactions from men to women driving and working. Either case is consistent with a negative reaction to women’s opportunity to drive from men, which could be triggered by negotiation over access to a shared vehicle, driving or employment.

These findings are particularly surprising in light of the results on women’s employment. Although access to driver’s training increases employment by 35%, rather than increasing the proportion of women who have financial independence, it leads to male family members curtailing women’s financial autonomy and women experiencing more conservative attitudes in their interactions with men.

6.3 Heterogeneous responses by family structure

An open question is whether the imposition of greater restrictions on women given access to a driver’s license reflects a response to their labor force participation or, conversely, a means of deterring them from entering the labor market. We can distinguish these two possibilities by examining whether the changes in employment and imposition of restrictions are experienced by the same subgroup of responders. With this in mind, we next examine heterogeneous responses by family structure. As described in Section 2, the importance of male family members in determining women’s behavior motivates an investigation of heterogeneous responses by women’s relationship to men who influence employment allocations. In particular, as motivated in Section 4, we expect female employment to respond to lower commuting costs only if male family members are sufficiently open to female employment, which is likely to differ systematically depending on whether they are related to the woman by blood or by marriage. For the same reason, differences in backlash should also differ by degree of altruism, but in the opposite direction.

With this hypothesis in mind, Table 3 presents heterogeneous treatment effects according to whether women are subject to the influence of a husband or co-parent (married women and divorced women with children) versus a male blood relative (a father, son or brother). It is first worth noting that driving responses to treatment are

observed across all these groups (Columns 1-3 of Table 3). However, women subject to the influence of a blood relative rather than a husband or ex-husband co-parent exhibit the largest changes in independent mobility, as measured by the share of trips that are unchaperoned (Column 5).²⁸

Meanwhile, the employment response is entirely driven by women subject to the influence of a blood relative rather than a husband or co-parent (Panel B, Column 1). The employment rate of women in the former category almost doubles, from 19% to 38%, while there is no increase in employment for women in the latter category.²⁹ In fact, women with a husband or co-parent actually respond to treatment by *exiting* the labor force - treated women in these categories are actually 48% (10 pp) more likely to be out of the labor force at endline (Table 4, Column 3).

As shown in Table 4, Column 6, household restrictions on female financial autonomy are concentrated among women with husbands or co-parents, who exhibit a decrease in labor force participation in response to the reform. Although this subgroup of women experiences no increase in earned income in response to the reform, they face substantial reductions in financial autonomy.³⁰ Hence, the change in financial autonomy appears to be a factor *mediating* as opposed to *reacting to* female employment. This is consistent with the prediction that blood relatives are more likely to internalize the value women gain from earning and spending their own money. In contrast, a husband or co-parent may respond to a woman's potential to earn and spend independently by monitoring and controlling her spending more tightly, which in turn can effectively reduce these women's incentive to work.

Table A7 presents the same outcomes further disaggregating impacts by individual subcategory. Panel A, Column 1 verifies that the employment effects within category are not driven by any one subgroup. These estimates show that both single and widowed women experience significant increases in employment, and that both married and divorced women experience statistically equivalent reductions in labor force participation, although the result is only significant when the two subgroups of women are pooled.

The results also indicate that both married women and divorced women with children experience reductions in financial control, while single women experience net *gains* in control over spending, although the result is not statistically significant. Meanwhile, widowed and divorced women without children actually exhibit reduced financial control; however it is important to note that their reported level of spending autonomy far exceeds that of any other group in the control group. Hence, we still observe the pattern at endline that both subcategories of treated women with husbands or ex-husbands have lower spending autonomy than both subcategories of treated women who negotiate spending and employment with blood relatives.

There are other demographic differences across women in different family structures that might explain the

²⁸This pattern is driven by never-married women (Table A7, Panel A, Column 5). While ever-married women report driving as much as never-married women, they appear to be largely chaperoned by males when they do so.

²⁹The employment rate of never-married women jumps from 25% to 38%, and the employment rate of widows jumps from 8% to 33% (Table A7, Panel B, Column 1). Married and divorced women's rate of work differs little across treatment and control, and are 26% and 22%, respectively, at endline. Strikingly, the opportunity to drive brings divorced women from the most to the least employed category of women, despite the fact that they have roughly the same number of young children as married and widowed women and similar levels of tertiary education as widowed women (Table A8).

³⁰Divorced women are 30% less likely to say that they can make a purchase without permission (Table 4, Column 6). The effect appears somewhat attenuated for married and widowed women, although the differences are imprecisely estimated; for never-married women, the estimates suggest that treatment *increased* financial autonomy (although this is also imprecisely estimated).

strong pattern of heterogeneous responses. For instance, never-married women are younger, more educated, have higher rates of baseline labor force participation, and have no children of their own (Table A8). It is first worth noting that none of these differences explains the simultaneous impacts observed among both never-married and widowed women, who look very similar to divorced women in terms of age, education, and number of children. That is, there is no other demographic characteristic that never-married and widowed women have in common that differs systematically from married and divorced women. However, to test whether any of the key differences explains our results, we estimate an alternative specification that includes treatment interactions with age, number of children in the household, and education. Appendix Table A9 shows the results. The pattern of results does not attenuate, but in fact becomes stronger: never-married and widowed women are still responsible for the employment effects.

6.4 Alternative mechanisms

As discussed in Section 4, another potential channel for treatment to affect employment is through changes in women’s own beliefs or preferences in response to the opportunity to drive. Table A10 shows treatment effects on the respondent’s own beliefs about gender roles. Treatment does not have a statistically detectable effect on women’s own gender attitudes, suggesting that this is not a meaningful channel of influence.³¹

Following our pre-analysis plan, we also estimate effects of the intervention on the extent of women’s social networks and civic engagement. Estimates are shown in Appendix Table A11. We do not detect significant effects of treatment on these outcomes. The treatment effect on the number of individuals that a respondent met in person is positive (corresponding to an increase of one additional individual in a one week period) but imprecisely estimated. This could either reflect the fact that driving is not a binding constraint on women’s social engagement, or possibly that social engagements are not sanctioned as readily as employment opportunities by household members.

7 Discussion

Our results suggest that de facto access to the de jure right to drive led to a substantial increase in employment. In light of our results, how much of the steady increase in Saudi women’s employment since 2018 can be attributed to access to driver’s licenses? We estimate an intent-to-treat effect of a 7.4 percentage point increase in employment and a 43 percentage point increase in obtaining a license. If we assume that the treatment only affects employment through obtaining a license, rescaling the treatment effect to account for incomplete takeup suggests that receiving a license would increase employment by 17 percentage points. However, as of 2020, only

³¹We cannot rule out a moderate effect size: the estimated treatment effect on an index of “progressive” gender attitudes among respondents is 0.12 SD with standard error 0.1. This could be consistent with treatment directly affecting attitudes, and thus increasing the wedge in preferences between women and their male family members, with only never-married and widowed women given the latitude to act on a new desire to work by fathers and sons. Alternatively, it is also possible that treatment affects attitudes through employment itself. However, all these results are imprecisely estimated, and treatment effects on the individual index elements vary in direction; there is no clear evidence that changes in women’s own attitudes or preferences are a key channel through which employment impacts are realized.

2% of Saudi women nationwide had received a driver's license. Our treatment effect estimates would thus imply that access to a license could have led to merely a 0.34 percentage point increase in employment, or just 4.1% of the total increase in employment from 2018 to 2020 (Figure A2).

Of course, the sample in our experiment is not representative of the national sample. Our experimental sample is in fact employed at a lower rate than the national sample, and much more likely to be unemployed and searching (Table A2). Given their desire to work, this could suggest that the treatment effects on employment are likely to be larger in our sample than at a national level; conversely, they may also lack skills and experience, limiting the employment response. However, as an upper bound, even if *all* the women nationwide who received licenses were constrained from working only by the lack of a driving license (i.e. if the treatment effect on employment for them were a 100 percentage point increase), this would still imply only a 2 percentage point increase in female employment, or 24% of the total increase. Of course, this is an extreme upper bound; some women who already worked even before 2018 received licenses, and some likely received licenses without taking up work.

These back-of-the-envelope calculations show that, despite the fact that offering driver's licenses led to a 35% increase in employment in our study, other factors besides licenses themselves are responsible for the vast majority of the increase in women's work in Saudi Arabia over this period. This includes the potential indirect effects of the driving reform, which affect even women who do not receive licenses – including both the treatment and the control group in our study – that are discussed in Section 4. It also includes the other reforms and social and economic changes taking place over this period discussed in Section 2, including new labor laws to allow women to work in previously male-reserved occupations and shifts, and criminalize gender pay discrimination; a new workplace anti-harassment law.

One clear and immediate implication of our results for the Saudi context is the importance of expanding access to driver's training centers. While a survey suggested that 80% of women would like to take up drivers' licenses, only 2% had received them as of the last available statistics in 2020. Although there are no longer long waitlists for driver's training, the limited number of training centers nationwide and the high fee charged to women still present significant barriers. Our results suggest that increasing the geographical coverage of centers and eliminating the difference in price for women and men to obtain a driver's license could have substantial impacts on women's employment.

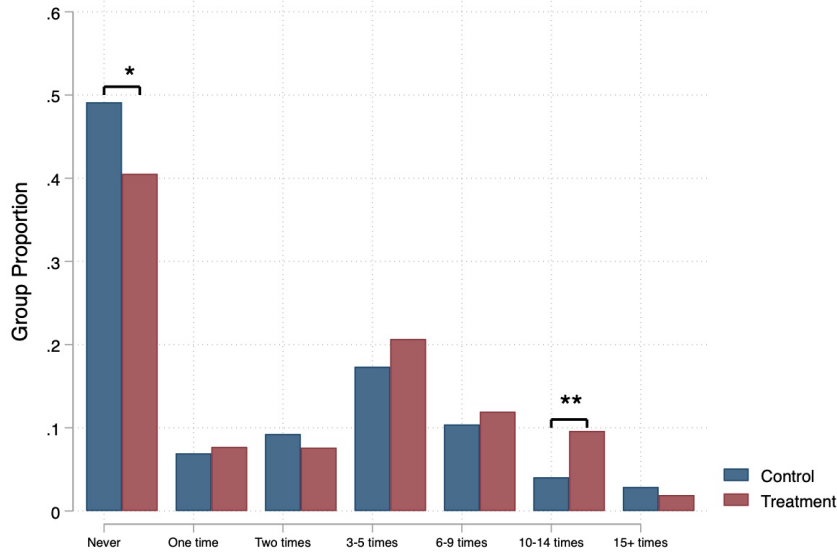
Our results also speak to the importance of transport constraints in other contexts for labor force outcomes. While we study the impact of the legal access to the right to drive, women in other settings face constraints to mobility due to safety and social norms, while other disadvantaged groups - such as low income households or immigrants in the U.S. - may have difficulty obtaining a driver's license or financing a vehicle. Our results on employment reinforce the economic and social importance of addressing such barriers.

More broadly, our results underscore the importance of considering intra-household responses to gender reforms, which have the potential to counteract gains in freedom for women in societies where women have very low bargaining power in marriage. Conversely, this suggests the possibility for gender reforms in different

domains, such as the lifting of the driving ban and recent reforms to the Saudi male guardianship rules, to have a greater effect in combination. This may help to explain in part why such a large increase in FLFP has accompanied the combination of multiple reforms in recent years in the country.

Tables and figures

Figure 1: Treatment effects on unaccompanied travel



Notes: This figure shows the results of a series of estimates of equation 1 in which the outcome variables are mutually exclusive and exhaustive indicators for the frequency of travel. Each control group bar shows the control group mean, while the treatment bar shows the sum of the control group mean and the ITT treatment effect β_1 . Regressions include individual and household controls: age (in bins), indicators for each education level, indicators for employment status, marital status (indicators for married, never-married, and widowed), household size (number of members), and number of cars owned (indicators for one car and for more than one car), and randomization cohort fixed effects. SEs clustered at the household level. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$.

Table 1: Treatment effects on driving and mobility

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---------------|---------------------------|---------------------|---------------------------|---|--|--------------------------------------|
| | Started driver's training | Received license | Any driving in past month | Number of times left house in last 7 days | Share of trips made without male chaperone | No trips made without male chaperone |
| Treatment | 0.619*** (0.040) | 0.434*** (0.039) | 0.203*** (0.048) | 0.721 (0.478) | 0.084* (0.045) | -0.086* (0.048) |
| Observations | 469 | 469 | 489 | 470 | 461 | 461 |
| Mean: Control | 0.192 | 0.102 | 0.335 | 5.200 | 0.433 | 0.491 |

Notes: Column (5) and (6) outcomes are set to zero for 24 observations in which the respondent reported making no trips outside the home in the previous 7 days. Variations in sample size are due to drop-off from telephone survey; order of survey modules was randomized. All estimates include individual and household controls: age (in bins), indicators for each education level, indicators for employment status, marital status (indicators for married, never-married, and widowed), household size (number of members), and number of cars owned (indicators for one car and for more than one car), and randomization cohort fixed effects. SEs clustered at the household level. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$.

Table 2: Treatment effects on labor market outcomes and independent decision making

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---------------|----------|------------|-----------------------|----------------------|---|---|
| | Employed | Unemployed | Out of labor force | On the job search | Allowed to leave the house without permission | Allowed to make purchase without permission |
| Treatment | 0.074* | -0.097** | 0.023 | 0.033 | 0.054 | -0.090* |
| | (0.040) | (0.048) | (0.040) | (0.026) | (0.045) | (0.047) |
| Observations | 488 | 488 | 488 | 481 | 488 | 486 |
| Mean: Control | 0.210 | 0.569 | 0.221 | 0.072 | 0.344 | 0.484 |

Notes: The outcome in column (4) indicates whether the respondent is employed and applied for at least one job in the previous month (a more general measure of search beyond job applications was not collected for employed respondents). Results for unemployment are similar if we redefine unemployed to include only those who applied for at least one job in the previous month. The outcomes in columns 5 and 6 are constructed as follows: respondents were asked to rate their level of agreement (using a 5 point Likert scale from ‘completely disagree’ to ‘completely agree’) with the statements “If I wanted to meet with a friend outside of my home, I could do so without seeking approval / permission from anyone in my household first” and “I can make a purchase of 1000 SAR without needing to take permission from any member of my family” (1000 SAR is roughly equivalent to 265 USD, in 2021 dollars), respectively. The outcome variables are indicators for above-median response on the Likert scale for each statement response. Variations in sample size are due to drop-off from telephone survey; order of survey modules was randomized. All estimates include individual and household controls: age (in bins), indicators for each education level, indicators for employment status, marital status (indicators for married, never-married, and widowed), household size (number of members), and number of cars owned (indicators for one car and for more than one car), and randomization cohort fixed effects. SEs clustered at the household level. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$.

Table 3: Heterogeneous treatment effects on driving and mobility

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|------------------------------|---------------------|------------------------------|--|---|--|
| | Started driver's training | Received license | Any driving in past month | Number of times left house in last 7 days | Share of trips made without male chaperone | No trips made without male chaperone |
| β_1 : Treatment | 0.566*** (0.063) | 0.475*** (0.057) | 0.259*** (0.069) | 0.931 (0.712) | 0.176*** (0.064) | -0.164** (0.069) |
| β_2 : Has husband/co-parent | 0.038 (0.078) | 0.009 (0.065) | 0.118 (0.086) | 0.188 (0.818) | 0.104 (0.084) | -0.071 (0.090) |
| β_3 : Treatment x Has husband/co-parent | 0.086 (0.081) | -0.068 (0.076) | -0.104 (0.092) | -0.223 (0.955) | -0.191** (0.089) | 0.171* (0.098) |
| $\beta_1 + \beta_3$ | 0.653*** (0.051) | 0.407*** (0.053) | 0.155** (0.064) | 0.708 (0.640) | -0.014 (0.063) | 0.007 (0.068) |
| Observations | 450 | 450 | 470 | 452 | 444 | 444 |
| Mean: Control, no husband/co-parent | 0.211 | 0.118 | 0.321 | 5.114 | 0.448 | 0.468 |
| Mean: Control, has husband/co-parent | 0.182 | 0.091 | 0.337 | 5.161 | 0.434 | 0.495 |

Notes: 'Has husband/co-parent' is defined as (a) currently married or (b) divorced/separated with children under 18 in the household. 16 respondents are missing administrative data on children in the household and are excluded from these estimates; Appendix Table A12 alternatively assumes that such respondents do not have children, and results are very similar. Three additional observations are dropped due to missing baseline marital status, which are used as control variables and missing values are imputed using a dummy variable adjustment approach in Table 1; together these cause a difference in sample size between tables. Outcome variables are constructed as described in the notes to Table 1. Variations in sample size are due to drop-off from telephone survey; order of survey modules was randomized. All estimates include individual and household controls: age (in bins), indicators for each education level, indicators for employment status, marital status (indicators for married, never-married, and widowed), household size (number of members), and number of cars owned (indicators for one car and for more than one car), and randomization cohort fixed effects. Marital status dummies are not included as a control in this table because they are highly collinear with "has husband/co-parent". However, results are unchanged if we include individual indicators as controls for: married; divorced/separated with co-parent; and widowed (never married is the reference group). SEs are clustered at household level. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$.

Table 4: Heterogeneous treatment effects on labor market outcomes and independent decision making

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---------------------|---------------------|--------------------|-------------------|---|---|
| | Employed | Unemployed | Out of labor force | On the job search | Allowed to leave the house without permission | Allowed to make purchase without permission |
| β_1 : Treatment | 0.188*** (0.061) | -0.139** (0.069) | -0.050 (0.059) | 0.059 (0.042) | 0.060 (0.063) | 0.018 (0.068) |
| β_2 : Has husband/co-parent | 0.098 (0.077) | -0.037 (0.088) | -0.061 (0.078) | 0.003 (0.048) | 0.035 (0.082) | 0.153* (0.086) |
| β_3 : Treatment x Has husband/co-parent | -0.187** (0.082) | 0.035 (0.096) | 0.152* (0.081) | -0.045 (0.053) | -0.022 (0.089) | -0.186** (0.091) |
| $\beta_1 + \beta_3$ | 0.002 (0.053) | -0.104 (0.067) | 0.102* (0.055) | 0.014 (0.033) | 0.038 (0.064) | -0.168*** (0.064) |
| Observations | 469 | 469 | 469 | 463 | 469 | 467 |
| Mean: Control, no husband/co-parent | 0.190 | 0.583 | 0.226 | 0.071 | 0.267 | 0.419 |
| Mean: Control, has husband/co-parent | 0.213 | 0.574 | 0.213 | 0.065 | 0.404 | 0.527 |

Notes: ‘Has husband/co-parent’ is defined as (a) currently married or (b) divorced/separated with children under 18 in the household. 16 respondents are missing administrative data on children in the household and are excluded from these estimates; Appendix Table A12 alternatively assumes that such respondents do not have children, and results are very similar. Three additional observations are dropped due to missing baseline marital status, which are used as control variables and missing values are imputed using a dummy variable adjustment approach in Table 1; together these cause a difference in sample size between tables. Outcome variables are constructed as described in the notes to Table 1. Variations in sample size are due to drop-off from telephone survey; order of survey modules was randomized. All estimates include individual and household controls: age (in bins), indicators for each education level, indicators for employment status, marital status (indicators for married, never-married, and widowed), household size (number of members), and number of cars owned (indicators for one car and for more than one car), and randomization cohort fixed effects. Marital status dummies are not included as a control in this table because they are highly collinear with “has husband/co-parent”. However, results are unchanged if we include individual indicators as controls for: married; divorced/separated with co-parent; and widowed (never married is the reference group). SEs are clustered at household level. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$.

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Appendix A Supplementary material

A.1 Additional Figures

Figure A1: Timeline

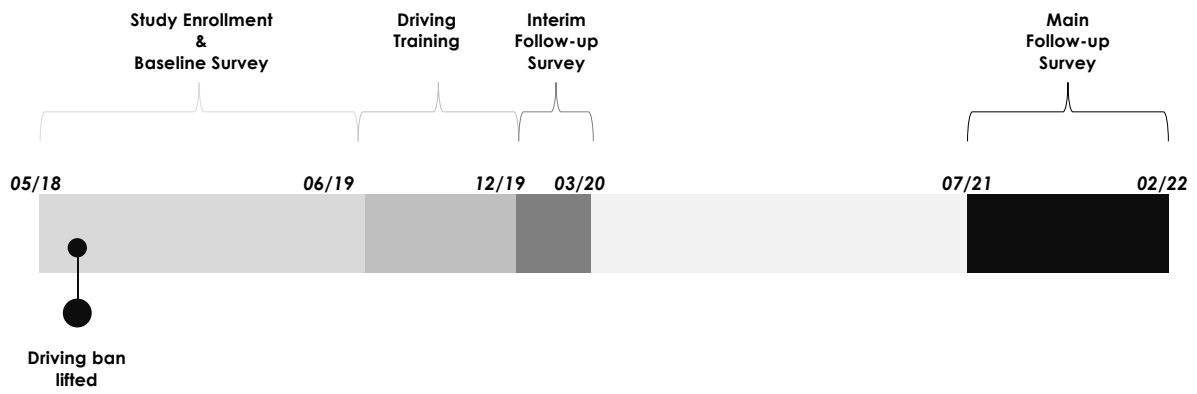
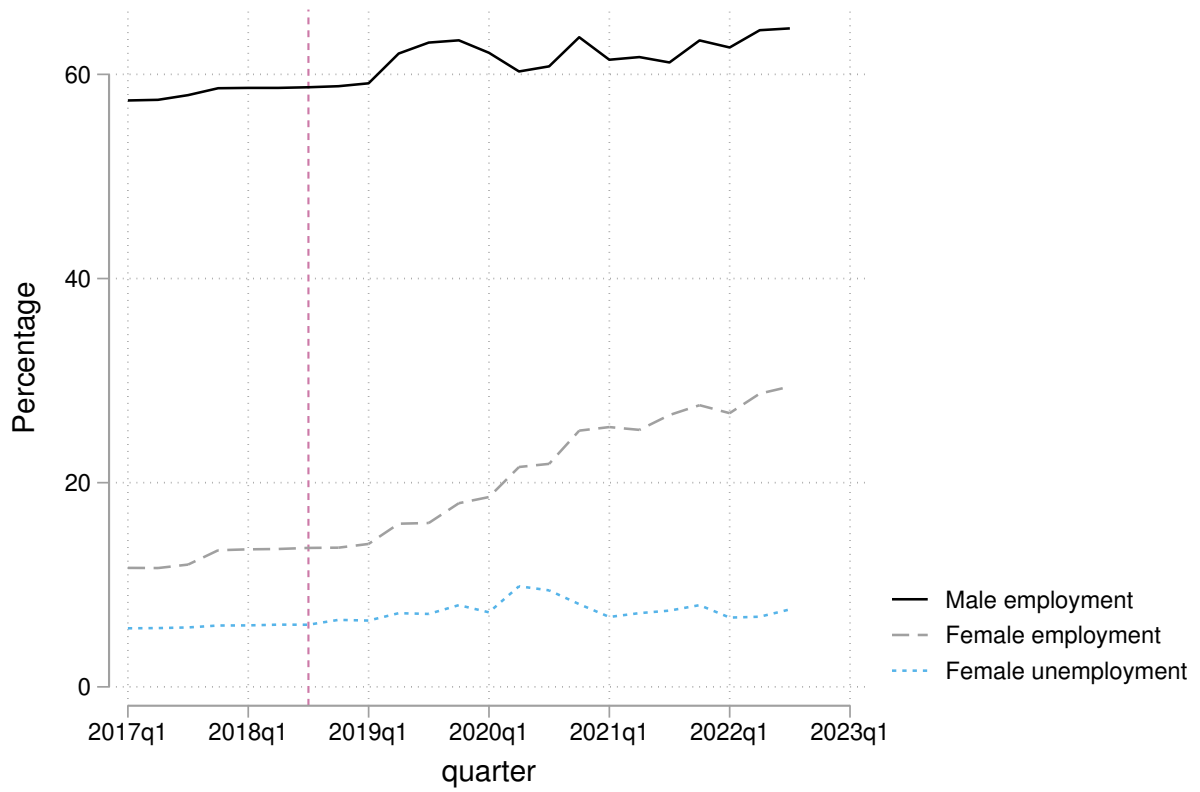


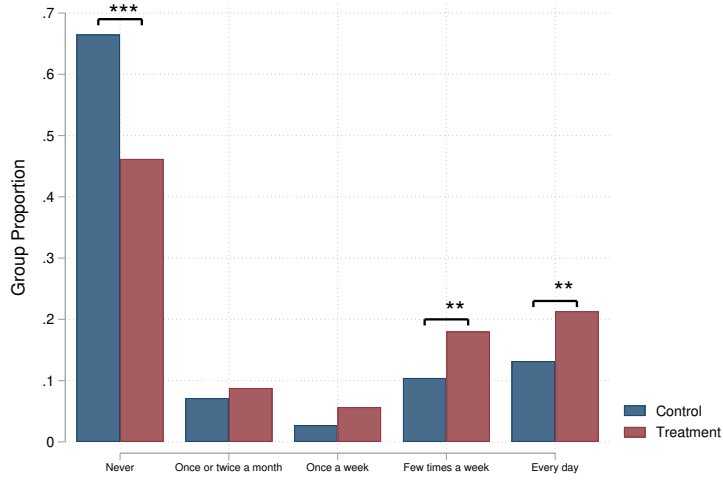
Figure A2: Female Labor Force Participation in Saudi Arabia



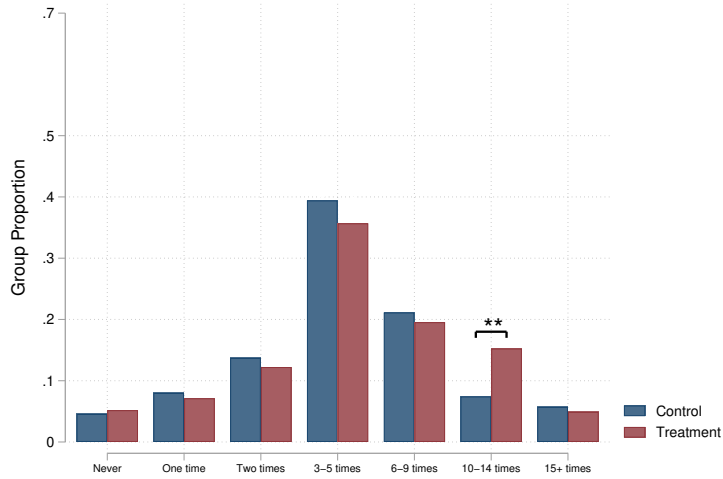
Notes: Source: Estimates from Saudi LFS - GASTAT. Red vertical line shows the date of the driving ban repeal.

Figure A3: Treatment effects on travel frequency

(a) Frequency of driving in previous month



(b) Frequency of leaving the house in previous 7 days



Notes: This figure shows the results of a series of estimates of equation 1 in which the outcome variables are mutually exclusive and exhaustive indicators for the frequency of travel reported by the respondent in the recall period. Each control group bar show the control group mean, while the treatment bar shows the sum of the control group mean and the ITT treatment effect β_1 . Regressions include individual and household controls: age (in bins), indicators for each education level, indicators for employment status, marital status (indicators for married, never-married, and widowed), household size (number of members), and number of cars owned (indicators for one car and for more than one car), and randomization cohort fixed effects. SEs clustered at the household level.
 * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Figure A4: Second Order Gender Attitudes (Control Group)



Notes: As part of the main follow up, respondents were asked to consider each of a series of statements and indicate what they believe the share of each second order group (their female social network, male family, and male social network) would agree with each statement. The statements presented to the respondent were: “On the whole, men make better business executives than women do”, “A woman’s priority should be in the home and with her family”, “When a mother works for pay, the children suffer”. Reported in this figure are responses given by the control group only, as percentages of the sample.

A.2 Additional Tables

Table A1: Legal rights of women by marital status

| | Never-married | Married | Divorced | Widowed |
|--|---------------|---------|--------------------|----------------------|
| Woman’s guardian | Father | Husband | Father/brother/son | Father/brother/son |
| Child’s legal guardian | N/A | Husband | Husband | In-laws ¹ |
| Head of children’s household (Article 91) ² | N/A | Husband | Husband | Woman |
| Physical custody of children (Article 30) ³ | N/A | N/A | Husband or woman | In-laws or woman |

Source: Embassy of the Kingdom of Saudi Arabia (2019). Red text highlights areas of influence by a husband or co-parent, while blue indicates areas where the woman herself or her blood relatives may hold the specified legal rights.

1. Guardianship of children is always granted to a male on the paternal side of the family, which is generally the deceased husband’s brother or father.
2. “Head of household” is a legal designation assigned to all children, which imbues that person with the authority to conduct government business on their children’s behalf. Legally speaking, the precise distinction for unmarried mothers is not between divorced and widowed women, but between divorced women with ex-husbands who are still alive, and widows or divorced women whose ex-husband has died, because as of a 2019 reform, the latter can become head of household for their minor children and unmarried daughters (Article 91).
3. In March 2018, women received the right to receive custody of children in divorce settlements, and the ruling that enforced their return to ex-spouses has been abolished. Article 30 states that default custody (place of residence) goes to father, but can be granted to women. Hence, while default physical custody by law remains with the husband and his family, physical custody of children can now go to mothers upon request. Hence, it is reasonable to assume that physical custody is more likely to go to widowed than divorced women because the children’s paternal uncle or grandfather would have a weaker motivation to retain custody than a father would.

Table A2: Comparison of Experimental Sample and Population Representative Statistics

| | Experimental sample (1) | Representative sample (Riyadh) (2) | Representative sample (National) (3) |
|--|-------------------------------|---|---|
| Ever employed | 0.393 | 0.540 | 0.427 |
| Currently employed | 0.185 | | 0.135 |
| Unemployed | 0.652 | | 0.062 |
| Average monthly household income (SAR) | 2,500 | 16,011 | 14,823 |
| <u>Age</u> | | | |
| 15-29 | 0.368 | | 0.422 |
| 30-44 | 0.412 | | 0.377 |
| 45+ | 0.221 | | 0.200 |
| <u>Marital status</u> | | | |
| Never married | 0.338 | | 0.215 |
| Married | 0.202 | | 0.665 |
| Divorced/Separated | 0.356 | | 0.052 |
| Widowed | 0.104 | | 0.067 |
| <u>Education</u> | | | |
| Less than primary | 0.061 | 0.202 | 0.232 |
| Elementary (1-5 years) | 0.298 | 0.303 | 0.296 |
| Highschool (6-12 years) | 0.340 | 0.286 | 0.261 |
| Vocational certificate (13-14 years) | 0.152 | 0.025 | 0.028 |
| College or above (16+ years) | 0.150 | 0.184 | 0.183 |

Notes: Column 1: Monthly household income is provided by administrative records from Alnahda beneficiary subsample only (64% of RCT responder sample). All other column (1) statistics are generated from a combination of administrative data provided by AlNahda and baseline survey responses for the RCT sample. Statistics reported for the subsample who started the online survey. Column 2 and 3: Statistics for Ever Employed, Age, and Marital Status are reported in KSA Ministry of Health (2019); Ever Employed is representative of the Riyadh region and Age and Marital Status are representative at the national level. Statistics for Currently Employed and Unemployed Searching are reported in GASTAT (2018b) and are representative at the national level. The statistics for Education levels and Average monthly household income in columns (2 and 3) are reported in GASTAT (2017) and GASTAT (2018a), respectively; Education is representative at the national level and Average monthly household income is representative at the Riyadh region level.

Table A3: Baseline balance among responders

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|-----|----------------|-------|-----|------------------|-------|------------|---------|
| | | <u>Control</u> | | | <u>Treatment</u> | | | |
| | N | Mean | SD | N | Mean | SD | Difference | P-value |
| Age | 188 | 34.78 | 11.00 | 315 | 35.26 | 11.19 | -0.337 | 0.72 |
| Married | 188 | 0.19 | 0.39 | 312 | 0.21 | 0.41 | 0.002 | 0.96 |
| Divorced or separated | 188 | 0.35 | 0.48 | 312 | 0.36 | 0.48 | -0.041 | 0.31 |
| Widowed | 188 | 0.14 | 0.35 | 312 | 0.08 | 0.27 | -0.020 | 0.40 |
| Never-married | 188 | 0.32 | 0.47 | 312 | 0.35 | 0.48 | 0.059 | 0.19 |
| Total number of household members | 185 | 6.71 | 3.22 | 303 | 6.25 | 2.36 | -0.512 | 0.08 |
| Number of household members under 18 | 185 | 2.97 | 2.40 | 303 | 2.60 | 2.11 | -0.429 | 0.08 |
| Number of household members 18+ | 185 | 3.75 | 2.19 | 303 | 3.64 | 1.97 | -0.083 | 0.71 |
| Cars owned by household | 183 | 0.78 | 0.96 | 308 | 0.78 | 0.88 | -0.018 | 0.85 |
| Likely to drive soon after ban is lifted | 187 | 0.68 | 0.47 | 305 | 0.67 | 0.47 | 0.046 | 0.29 |
| Highest edu: Less than primary | 183 | 0.05 | 0.22 | 311 | 0.07 | 0.25 | 0.017 | 0.45 |
| Highest edu: Elementary (1-5 yrs) | 183 | 0.28 | 0.45 | 311 | 0.31 | 0.46 | 0.007 | 0.86 |
| Highest edu: High school (6-12 yrs) | 183 | 0.36 | 0.48 | 311 | 0.33 | 0.47 | -0.008 | 0.86 |
| Highest edu: Any tertiary education (13+ yrs) | 183 | 0.31 | 0.46 | 311 | 0.30 | 0.46 | -0.015 | 0.73 |
| Out of the labor force | 188 | 0.15 | 0.36 | 315 | 0.17 | 0.37 | 0.015 | 0.65 |
| Unemployed | 188 | 0.69 | 0.47 | 315 | 0.63 | 0.48 | -0.043 | 0.33 |
| Employed | 188 | 0.16 | 0.37 | 316 | 0.20 | 0.40 | 0.028 | 0.47 |
| On-the-job search | 188 | 0.11 | 0.32 | 316 | 0.12 | 0.33 | -0.006 | 0.85 |
| Ever employed | 188 | 0.35 | 0.48 | 316 | 0.42 | 0.49 | 0.062 | 0.19 |
| Number of years experience ever employed | 61 | 3.15 | 2.53 | 124 | 2.70 | 2.45 | -0.413 | 0.32 |
| Last month salary <800 USD ever employed | 54 | 0.46 | 0.50 | 107 | 0.31 | 0.46 | -0.147 | 0.08 |
| Last month salary 800-1,330 USD ever employed | 54 | 0.48 | 0.50 | 107 | 0.61 | 0.49 | 0.131 | 0.11 |
| Last month salary 1,330-1,865 USD ever employed | 54 | 0.06 | 0.23 | 107 | 0.08 | 0.28 | 0.016 | 0.76 |

Notes: Data from administrative records and baseline survey. Statistics reported for the subsample who started the endline survey. Column 7 is estimated by regressing the baseline variable on an indicator for treatment and randomization cohort fixed effects, clustering standard errors at the household level; and Column 8 reports the P-value “Likely to drive soon after ban is lifted” is a binary variable based on the following scale for whether the respondent would be likely to drive once the ban on female driving would be lifted (it was lifted partway through the baseline): unlikely to drive, somewhat likely, likely but not at first, and likely; taking the value of 1 if ‘likely’ was reported. Responders includes 504 RCT participants who started the endline survey. Drop off is due to missing administrative or baseline data. Number of observations varies for “number of years experience” and “last month salary...” due to missing values for the following reasons: 198 respondents reported ever working; of these, 185 reported their years of experience, 11 respondents did not receive this question in the version of the baseline survey administered to them, and 2 did not provide a valid response. 161 respondents reported their monthly salary in their current or most recent job, 11 respondents did not receive this question in the version of the baseline survey administered to them, and 26 did not provide a valid response.

Table A4: Descriptive statistics on travel patterns in control group

| | N | Mean | SD | Min | Max |
|---|-----|-------|-------|-----|-----|
| Any trip yesterday | 149 | 0.51 | 0.50 | 0 | 1 |
| Trips Yesterday | 149 | 1.18 | 1.41 | 0 | 10 |
| Mean one-way trip duration mins any trip yesterday | 76 | 38.78 | 35.67 | 5 | 180 |
| One-way commute duration mins any commute to work yesterday | 16 | 41.31 | 22.72 | 10 | 90 |
| <u>Trip purpose Trip yesterday</u> | | | | | |
| Leisure to meet friends | 76 | 0.01 | 0.11 | 0 | 1 |
| Leisure to meet relatives | 76 | 0.16 | 0.37 | 0 | 1 |
| Leisure to park or movies | 76 | 0.01 | 0.11 | 0 | 1 |
| Leisure for meal | 76 | 0.05 | 0.22 | 0 | 1 |
| Errands - personal business | 76 | 0.01 | 0.11 | 0 | 1 |
| Errands - health | 76 | 0.11 | 0.31 | 0 | 1 |
| Errands - HH shopping | 76 | 0.16 | 0.37 | 0 | 1 |
| Errands - personal shopping | 76 | 0.21 | 0.41 | 0 | 1 |
| Pick or drop someone | 76 | 0.03 | 0.16 | 0 | 1 |
| University commute | 76 | 0.16 | 0.37 | 0 | 1 |
| Work commute | 76 | 0.21 | 0.41 | 0 | 1 |
| <u>Trip mode Trip yesterday</u> | | | | | |
| Bus provided by university or employer | 76 | 0.04 | 0.20 | 0 | 1 |
| Walking | 76 | 0.05 | 0.22 | 0 | 1 |
| Drove herself | 76 | 0.01 | 0.11 | 0 | 1 |
| Car - family member driving | 76 | 0.32 | 0.47 | 0 | 1 |
| Car with paid driver | 76 | 0.17 | 0.38 | 0 | 1 |
| Car pooling | 76 | 0.03 | 0.16 | 0 | 1 |
| Ride-hailing (e.g. Uber) | 76 | 0.24 | 0.43 | 0 | 1 |
| Taxi | 76 | 0.20 | 0.40 | 0 | 1 |
| Other mode | 76 | 0.13 | 0.34 | 0 | 1 |

Notes: Descriptive statistics from detailed travel diary collected as part of the interim follow-up. Control group sample only. Respondents may report multiple trips and/or multiple modes for each trip, so means for trip purposes and modes can sum to greater than 1.

Table A5: Attrition from main follow-up

| | (1) | (2) | (3) |
|--------------------|----------------------------|---------------------------------|--------------------------------|
| | Started Mobility Module | Started Employment Module | Started Attitudes Module |
| Treatment | 0.036 (0.033) | 0.045 (0.034) | 0.026 (0.033) |
| Observations | 606 | 606 | 606 |
| Control Group Mean | 0.805 | 0.788 | 0.801 |

Notes: Dependent variables are indicators for whether the respondent began the respective module in the survey; the order of modules was randomized. All estimates include cohort FEs. SEs clustered at household level; * p < 0.1 ** p < 0.05 *** p < 0.01

Table A6: Treatment effects on second order beliefs

| | Index | Index Components | | |
|---------------------------------------|--------------------------------|--|---|--------------------------------|
| | (1) | (2) | (3) | (4) |
| | Gender Attitudes: 2nd Order | Women can be equally good business executives | It's ok for a woman to have priorities outside the home | Children OK if mother works |
| Panel A: Female Social Network | | | | |
| Treatment | -0.053 (0.098) | -0.054 (0.050) | 0.022 (0.044) | -0.012 (0.045) |
| Observations | 486 | 484 | 484 | 480 |
| Mean: Control | 0.000 | 0.461 | 0.315 | 0.328 |
| Panel B: Male Family | | | | |
| Treatment | -0.155 (0.099) | -0.089* (0.047) | -0.048 (0.047) | 0.003 (0.050) |
| Observations | 487 | 486 | 484 | 483 |
| Mean: Control | 0.000 | 0.354 | 0.425 | 0.427 |
| Panel C: Male Social Network | | | | |
| Treatment | -0.184* (0.101) | -0.078 (0.049) | -0.069 (0.049) | -0.047 (0.050) |
| Observations | 484 | 483 | 483 | 480 |
| Mean: Control | 0.000 | 0.536 | 0.456 | 0.475 |

Notes: Second order belief outcomes were constructed as follows: respondents were asked to think about each group (male family members, male members of social network, or female members of social network) and report what share of that group they think would 'somewhat' or 'completely' agree with the statement. Responses to each statement were then transformed into binary indicators for above median responses, which are reported in columns 2-4 of each panel. The outcome in column (1) of each panel is a weighted index of the standardized binary responses to each statement using the swindex command developed by Schwab *et al.* (2020). The command uses all available data (hence a higher N in Column 1) and assigns lower weight to index components with missing values. Variations in sample size among Columns 2-4 are due to drop-off from telephone survey; order of survey modules was randomized. All columns include individual and household controls: age (in bins), indicators for each education level, indicators for employment status, marital status (indicators for married, never-married, and widowed), household size (number of members), and number of cars owned (indicators for one car and for more than one car), and randomization cohort fixed effects. SEs clustered at the household level * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Table A7: Heterogeneous treatment effects on driving, mobility, labor market outcomes, and independent decision making by marital status

| Panel A: Driving and Independent Mobility | | | | | | |
|---|------------------------------|---------------------|------------------------------|--|---|--|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | Started driver's training | Received license | Any driving in past month | Number of times left house in last 7 days | Share of trips made without male chaperone | No trips made without male chaperone |
| β_1 : Treatment | 0.679*** (0.068) | 0.479*** (0.067) | 0.178** (0.082) | 0.490 (0.915) | 0.005 (0.082) | 0.033 (0.090) |
| β_2 : Married | 0.067 (0.087) | 0.128* (0.071) | 0.038 (0.102) | -0.954 (0.847) | 0.029 (0.107) | 0.016 (0.116) |
| β_3 : Never-married | -0.068 (0.106) | 0.018 (0.082) | -0.235** (0.111) | -0.263 (1.214) | -0.141 (0.108) | 0.125 (0.117) |
| β_4 : Widowed or divorced with no co-parent | 0.098 (0.110) | 0.171* (0.098) | 0.071 (0.127) | -1.139 (1.255) | 0.034 (0.127) | -0.038 (0.137) |
| β_5 : Treatment x Married | -0.065 (0.110) | -0.184* (0.105) | -0.045 (0.131) | 0.570 (1.354) | -0.044 (0.129) | -0.063 (0.141) |
| β_6 : Treatment x Never-married | -0.045 (0.100) | 0.120 (0.089) | 0.138 (0.112) | 0.207 (1.251) | 0.257** (0.111) | -0.270** (0.122) |
| β_7 : Treatment x Widowed or divorced with no co-parent | -0.263** (0.133) | -0.283** (0.127) | -0.050 (0.148) | 1.021 (1.558) | -0.029 (0.143) | -0.021 (0.154) |
| Observations | 450 | 450 | 470 | 452 | 444 | 444 |
| Mean: Control, divorced with co-parent | 0.182 | 0.091 | 0.333 | 5.569 | 0.415 | 0.491 |
| Mean: Control, married | 0.182 | 0.091 | 0.343 | 4.486 | 0.466 | 0.500 |
| Mean: Control, never-married | 0.200 | 0.080 | 0.246 | 4.962 | 0.386 | 0.537 |
| Mean: Control, widowed or divorced with no co-parent | 0.231 | 0.192 | 0.481 | 5.423 | 0.583 | 0.320 |
| p-val: $\beta_1 + \beta_5 = 0$ | 0.000 | 0.000 | 0.196 | 0.262 | 0.698 | 0.777 |
| p-val: $\beta_1 + \beta_6 = 0$ | 0.000 | 0.000 | 0.000 | 0.406 | 0.001 | 0.005 |
| p-val: $\beta_1 + \beta_7 = 0$ | 0.000 | 0.072 | 0.304 | 0.235 | 0.836 | 0.925 |

Panel B: Labor Market Outcomes and Independent Decision Making

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---------------------|---------------------|---------------------|---------------------|---|---|
| | Employed | Unemployed | Out of labor force | On the job search | Allowed to leave the house without permission | Allowed to make purchase without permission |
| β_1 : Treatment | -0.061 (0.067) | -0.034 (0.085) | 0.095 (0.073) | -0.007 (0.050) | -0.028 (0.085) | -0.170** (0.080) |
| β_2 : Married | -0.030 (0.089) | 0.183* (0.106) | -0.153** (0.078) | -0.079* (0.045) | -0.314*** (0.103) | -0.084 (0.113) |
| β_3 : Never-married | -0.031 (0.112) | -0.031 (0.120) | 0.062 (0.105) | 0.028 (0.074) | -0.184* (0.100) | -0.225** (0.111) |
| β_4 : Widowed or divorced with no co-parent | -0.241** (0.093) | 0.335*** (0.116) | -0.094 (0.097) | -0.141** (0.059) | -0.193 (0.128) | -0.058 (0.126) |
| β_5 : Treatment x Married | 0.144 (0.108) | -0.169 (0.134) | 0.025 (0.108) | 0.052 (0.056) | 0.183 (0.126) | 0.010 (0.135) |
| β_6 : Treatment x Never-married | 0.184* (0.100) | -0.010 (0.117) | -0.175* (0.103) | 0.017 (0.071) | 0.058 (0.109) | 0.291*** (0.110) |
| β_7 : Treatment x Widowed or divorced with no co-parent | 0.400*** (0.114) | -0.333** (0.140) | -0.067 (0.111) | 0.184** (0.082) | 0.183 (0.148) | -0.056 (0.147) |
| Observations | 469 | 469 | 469 | 463 | 469 | 467 |
| Mean: Control, divorced with co-parent | 0.237 | 0.492 | 0.271 | 0.103 | 0.534 | 0.561 |
| Mean: Control, married | 0.171 | 0.714 | 0.114 | 0.000 | 0.194 | 0.472 |
| Mean: Control, never-married | 0.246 | 0.474 | 0.281 | 0.105 | 0.190 | 0.293 |
| Mean: Control, widowed or divorced with no co-parent | 0.074 | 0.815 | 0.111 | 0.000 | 0.429 | 0.679 |
| p-val: $\beta_1 + \beta_5 = 0$ | 0.331 | 0.052 | 0.131 | 0.109 | 0.094 | 0.138 |
| p-val: $\beta_1 + \beta_6 = 0$ | 0.100 | 0.582 | 0.282 | 0.856 | 0.681 | 0.140 |
| p-val: $\beta_1 + \beta_7 = 0$ | 0.000 | 0.001 | 0.734 | 0.004 | 0.201 | 0.068 |

Notes: Outcome variables are constructed as described in the notes to Table 1. Four observations are dropped due to missing baseline marital status, which are used as control variables with missing values imputed using a dummy variable adjustment approach in Table 1, causing a difference in sample size between tables. Results are similar if we follow the same imputation method. Variations in sample size are due to drop-off from telephone survey; order of survey modules was randomized. OLS regressions; all estimates include individual and household controls: age (in bins), indicators for each education level, indicators for employment status, household size (number of members), number of cars owned (indicators for one car and for more than one car), and randomization cohort fixed effects. SEs clustered at the household level. * p < 0.1 ** p < 0.05 *** p < 0.01

Table A8: Comparison of baseline characteristics across family structure among responders

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) | (13) |
|---|--------------------|-------|------|---------|-------|------|--------|-------|------|---------|-------|------|---------|
| | Divorced/separated | | | Married | | | Single | | | Widowed | | | |
| | n | mean | sd | n | mean | sd | n | mean | sd | n | mean | sd | P-value |
| Age | 178 | 40.11 | 9.21 | 101 | 40.49 | 7.65 | 169 | 24.14 | 5.65 | 51 | 43.29 | 9.47 | 0.00 |
| Total number of household members | 166 | 6.27 | 2.72 | 101 | 6.59 | 2.57 | 166 | 6.71 | 2.66 | 52 | 5.79 | 3.17 | 0.03 |
| Number of household members under 18 | 166 | 3.03 | 2.22 | 101 | 3.55 | 2.25 | 166 | 2.07 | 2.06 | 52 | 2.40 | 2.11 | 0.00 |
| Number of household members 18+ | 166 | 3.23 | 2.13 | 101 | 3.04 | 1.89 | 166 | 4.64 | 1.70 | 52 | 3.38 | 2.13 | 0.00 |
| Cars owned by household | 176 | 0.66 | 0.94 | 100 | 0.85 | 0.67 | 163 | 0.87 | 1.00 | 49 | 0.76 | 0.92 | 0.00 |
| Likely to drive soon after ban is lifted | 174 | 0.66 | 0.47 | 99 | 0.56 | 0.50 | 164 | 0.73 | 0.44 | 52 | 0.71 | 0.46 | 0.31 |
| Highest edu: Less than primary | 177 | 0.06 | 0.23 | 99 | 0.13 | 0.34 | 163 | 0.00 | 0.00 | 51 | 0.14 | 0.35 | 0.00 |
| Highest edu: Elementary (1-5 yrs) | 177 | 0.38 | 0.49 | 99 | 0.51 | 0.50 | 163 | 0.07 | 0.25 | 51 | 0.37 | 0.49 | 0.00 |
| Highest edu: High school (6-12 yrs) | 177 | 0.29 | 0.45 | 99 | 0.26 | 0.44 | 163 | 0.47 | 0.50 | 51 | 0.25 | 0.44 | 0.01 |
| Highest edu: Any tertiary education (13+ yrs) | 177 | 0.28 | 0.45 | 99 | 0.10 | 0.30 | 163 | 0.47 | 0.50 | 51 | 0.24 | 0.43 | 0.00 |
| Out of the labor force | 178 | 0.20 | 0.40 | 101 | 0.22 | 0.41 | 169 | 0.06 | 0.24 | 52 | 0.27 | 0.45 | 0.00 |
| Unemployed | 178 | 0.57 | 0.50 | 101 | 0.51 | 0.50 | 169 | 0.82 | 0.38 | 52 | 0.63 | 0.49 | 0.00 |
| Employed | 178 | 0.23 | 0.42 | 101 | 0.27 | 0.44 | 169 | 0.12 | 0.32 | 52 | 0.10 | 0.30 | 0.03 |
| On-the-job search | 178 | 0.17 | 0.38 | 101 | 0.16 | 0.37 | 169 | 0.07 | 0.25 | 52 | 0.04 | 0.19 | 0.03 |
| On-the-job search | 178 | 0.17 | 0.38 | 101 | 0.16 | 0.37 | 169 | 0.07 | 0.25 | 52 | 0.04 | 0.19 | 0.03 |
| Ever employed | 178 | 0.40 | 0.49 | 101 | 0.54 | 0.50 | 169 | 0.33 | 0.47 | 52 | 0.33 | 0.47 | 0.07 |
| Number of years experience ever employed | 59 | 3.12 | 2.77 | 54 | 3.13 | 2.43 | 55 | 2.02 | 2.11 | 17 | 3.68 | 2.16 | 0.00 |
| Last month salary <800 USD ever employed | 49 | 0.31 | 0.47 | 50 | 0.42 | 0.50 | 47 | 0.30 | 0.46 | 15 | 0.53 | 0.52 | 0.36 |
| Last month salary 800-1,330 USD ever employed | 49 | 0.61 | 0.49 | 50 | 0.50 | 0.51 | 47 | 0.64 | 0.49 | 15 | 0.40 | 0.51 | 0.35 |
| Last month salary 1,330-1,865 USD ever employed | 49 | 0.08 | 0.28 | 50 | 0.08 | 0.27 | 47 | 0.06 | 0.25 | 15 | 0.07 | 0.26 | 0.96 |

Notes: Data from administrative records and baseline survey. Column 13 is the p-value estimated by testing the difference in means after regressing the baseline variable on marital status with randomization cohort fixed effects and clustering standard errors at the household level. "Likely to drive soon after ban is lifted" is a binary variable based on the following scale for whether the respondent would be likely to drive once the ban on female driving would be lifted (it was lifted partway through the baseline): unlikely to drive, somewhat likely, likely but not at first, and likely; taking the value of 1 if 'likely' was reported. Responders includes 504 RCT participants who started the endline survey. Drop off is due to missing administrative or baseline data. Number of observations varies for "number of years experience" and "last month salary..." due to missing values for the following reasons: 198 respondents reported ever working; of these, 185 reported their years of experience, 11 respondents did not receive this question in the version of the baseline survey administered to them, and 2 did not provide a valid response. 161 respondents reported their monthly salary in their current or most recent job, 11 respondents did not receive this question in the version of the baseline survey administered to them, and 26 did not provide a valid response.

Table A9: Robustness of heterogeneity results to treatment interactions with baseline characteristics

| | Employed | |
|--|----------------------|----------------------|
| | (1) | (2) |
| β_1 : Treatment | 0.188*** (0.0609) | 0.158 (0.148) |
| β_2 : Has husband/co-parent | 0.0980 (0.0771) | 0.160* (0.0867) |
| β_3 : Treatment x Has husband/co-parent | -0.187** (0.0818) | -0.318*** (0.106) |
| Observations | 469 | 469 |
| Mean: Control, no husband/co-parent | 0.190 | 0.190 |
| p-val: $\beta_1 + \beta_3 = 0$ | 0.971 | 0.410 |
| Treatment x Education | | X |
| Treatment x Age | | X |
| Treatment x Number of children < 18 in household at baseline | | X |

Notes: Column 2 repeats the estimation in Column 1 but additionally controls for treatment interacted with the respondent's education, age, and the number of children under 18 years old in the household at baseline. 'Has husband/co-parent' is defined as (a) currently married or (b) divorced/separated with children under 18 in the household. 16 respondents are missing administrative data on children in the household and are excluded from these estimates; results are similar if we alternatively assume that such respondents do not have children. Three additional observations are dropped due to missing baseline marital status; this causes a difference in sample size to Table 1. Both columns include individual and household controls: age (in bins), indicators for each education level, indicators for employment status, household size (number of members), and number of cars owned (indicators for one car and for more than one car), and randomization cohort fixed effects. Marital status dummies are not included as a control in this table because they are highly collinear with "has husband/co-parent". However, results are unchanged if we include individual indicators as controls for: married; divorced/separated with co-parent; and widowed (never married is the reference group). SEs are clustered at household level. * p < 0.1 ** p < 0.05 *** p < 0.01

Table A10: Treatment effects on first order beliefs

| | Index | Index Components | | | | | |
|---|-----------------------------|---|---|-----------------------------|--|---|--|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| | Gender attitudes: 1st order | Women can be equally good business executives | It's ok for a woman to have priorities outside the home | Children OK if mother works | Ok to put own needs above those of my family | Ideal age for a woman to have her first child | Government should allow a national women's soccer team |
| Treatment | 0.116 (0.099) | -0.022 (0.047) | 0.046 (0.047) | -0.017 (0.049) | 0.075 (0.048) | 0.097 (0.294) | 0.024 (0.047) |
| Observations | 490 | 487 | 486 | 482 | 490 | 473 | 481 |
| Mean: Control | 0.000 | 0.434 | 0.392 | 0.469 | 0.348 | 25.090 | 0.382 |
| β_1 : Treatment | 0.135 (0.152) | -0.055 (0.069) | 0.041 (0.066) | -0.093 (0.075) | 0.057 (0.069) | 0.433 (0.427) | 0.110 (0.067) |
| β_2 : Has husband/co-parent | -0.095 (0.194) | 0.015 (0.090) | -0.028 (0.083) | -0.212** (0.089) | -0.047 (0.084) | -0.441 (0.565) | 0.142 (0.090) |
| β_3 : Treatment x Has husband/co-parent | -0.005 (0.195) | 0.066 (0.097) | 0.010 (0.089) | 0.137 (0.095) | 0.046 (0.094) | -0.540 (0.577) | -0.146 (0.096) |
| Observations | 471 | 468 | 467 | 463 | 471 | 454 | 462 |
| Mean: Control, no husband/co-parent | 0.086 | 0.453 | 0.400 | 0.518 | 0.356 | 25.537 | 0.378 |
| p-val: $\beta_1 + \beta_3 = 0$ | 0.307 | 0.863 | 0.436 | 0.486 | 0.116 | 0.791 | 0.594 |

Notes: The gender attitudes index was constructed as follows: respondents were asked to rate their own level of agreement (using a 5 point Likert scale from 'completely disagree' to 'completely agree') for each statement. Responses to each statement were then transformed into binary indicators for above-median responses, which are reported in columns 2-7. The outcome in column (1) is a weighted index of the standardized binary responses to each statement using the swindex command developed by Schwab *et al.* (2020). The command uses all available data (hence a higher N in Column 1) and assigns lower weight to index components with missing values. 'Has husband/co-parent' is defined as (a) currently married or (b) divorced/separated with children under 18 in the household. All estimates include individual and household controls: age (in bins), indicators for each education level, indicators for employment status, household size (number of members), and number of cars owned (indicators for one car and for more than one car), marital status (indicators for married, never-married, and widowed), and randomization cohort fixed effects. Variations in sample size among Columns 2-7 are due to drop-off from telephone survey; order of survey modules was randomized. SEs clustered at the household level. * p < 0.1 ** p < 0.05 *** p < 0.01

Table A11: Treatment Effects on Other Measures of Women’s Attitudes and Behaviors

| Panel A | | | | |
|----------------|--|---|--|---|
| | Social Networks | | Stated Approval of Gender Policy | |
| | (1) | (2) | (3) | (4) |
| | Number of people spoken to on phone in past 7 days | Number of different people met in person in past 7 days | Government is working fast enough to give women same rights as men | Feels the impact of changes that government is making to give women same rights |
| Treatment | 0.520 (1.295) | 0.926 (1.463) | -0.039 (0.041) | -0.038 (0.036) |
| Observations | 471 | 460 | 464 | 484 |
| Mean: Control | 7.358 | 8.265 | 0.805 | 0.877 |

| Panel B | | | | | |
|----------------|--------------------------------|--|---|------------------------------------|---|
| | Civic Engagement | | | | |
| | (1) | (2) | (3) | (4) | (5) |
| | Will vote in the next election | Expressed interest in signing up for volunteer program | Expressed interest in signing up for leadership program | Leadership program: Anyone clicked | Leadership program: Number people clicked |
| Treatment | -0.007 (0.048) | 0.016 (0.048) | 0.050 (0.047) | 0.029 (0.045) | -0.003 (0.173) |
| Observations | 481 | 504 | 504 | 504 | 504 |
| Mean: Control | 0.452 | 0.644 | 0.559 | 0.340 | 0.585 |

Notes: Outcomes in Panel A, Columns 3-4 were constructed as follows: respondents were asked to rate their level of agreement (using a 5 point Likert scale from ‘completely disagree’ to ‘completely agree’) with the statements “I think the government is working enough/working fast enough to make changes to give women the same rights as men.” and “In my day to day life, I feel the impact of the changes that the government is making to give women the same rights as men”. We then generated dummies at the median response on the Likert scale for each statement response. The wording of the statement “I think the government is working enough/working fast enough to make changes to give women the same rights as men” was modified after data collection began due to sensitivity of the original wording. It was updated to “I think the pace of social changes that Saudi society has been witnessing is fast enough to give women the same rights as men and doesn’t need to move faster.” We combine responses from both versions to create the outcome in column (3), and include an indicator for question version as a control in that model. Outcomes in Panel B, columns 2-3 are indicators for whether the respondent expressed interest in signing up for a given program during the survey. We also sent respondents a text message with a link, tied to their survey ID, to a prompt that provided further information about the program and where to apply. The text message also asked respondents to forward the link to any of their friends or family whom they thought might also be interested in the program. Column 4 is an indicator for whether anyone clicked on the link (respondent or friend), and column 5 is a measure of the number of people who clicked the link for more information. These outcomes are estimated for all respondents who started the survey, with the outcome for those who did not respond to that question or respond to the invitation coded as zero. All columns include individual and household controls: age (in bins), indicators for each education level, indicators for employment status, marital status (indicators for married, never-married, and widowed), household size (number of members), and number of cars owned (indicators for one car and for more than one car), and randomization cohort fixed effects. SEs clustered at the household level. Variations in sample size are due to drop-off from telephone survey; order of survey modules was randomized. * p < 0.1 ** p < 0.05 *** p < 0.01

Table A12: Robustness of heterogeneity results to alternative variable construction for ‘has husband/co-parent’

| Panel A: Driving and Independent Mobility | | | | | | |
|---|---------------------------|---------------------|---------------------------|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | Started driver's training | Received license | Any driving in past month | Number of times left house in last 7 days | Share of trips made without male chaperone | No trips made without male chaperone |
| β_1 : Treatment | 0.586*** (0.061) | 0.469*** (0.055) | 0.241*** (0.068) | 0.705 (0.727) | 0.194*** (0.062) | -0.190*** (0.067) |
| β_2 : Has husband/co-parent | 0.053 (0.076) | -0.002 (0.063) | 0.093 (0.085) | -0.010 (0.827) | 0.119 (0.082) | -0.092 (0.089) |
| β_3 : Treatment x Has husband/co-parent | 0.067 (0.079) | -0.062 (0.074) | -0.082 (0.091) | 0.027 (0.971) | -0.208** (0.088) | 0.196** (0.096) |
| $\beta_1 + \beta_3$ | 0.653*** (0.051) | 0.406*** (0.052) | 0.158** (0.064) | 0.732 (0.639) | -0.014 (0.063) | 0.007 (0.068) |
| Observations | 465 | 465 | 485 | 466 | 457 | 457 |
| Mean: Control, no husband/co-parent | 0.203 | 0.114 | 0.333 | 5.244 | 0.432 | 0.488 |
| Mean: Control, has husband/co-parent | 0.182 | 0.091 | 0.337 | 5.161 | 0.434 | 0.495 |
| Panel B: Labor Market Outcomes and Independent Decision Making | | | | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | Employed | Unemployed | Out of labor force | On the job search | Allowed to leave the house without permission | Allowed to make purchase without permission |
| β_1 : Treatment | 0.158*** (0.061) | -0.103 (0.068) | -0.055 (0.058) | 0.052 (0.042) | 0.059 (0.062) | -0.003 (0.066) |
| β_2 : Has husband/co-parent | 0.078 (0.077) | -0.008 (0.087) | -0.070 (0.076) | 0.004 (0.049) | 0.020 (0.081) | 0.126 (0.084) |
| β_3 : Treatment x Has husband/co-parent | -0.153* (0.082) | -0.003 (0.095) | 0.156** (0.079) | -0.036 (0.054) | -0.017 (0.088) | -0.165* (0.090) |
| $\beta_1 + \beta_3$ | 0.005 (0.053) | -0.105 (0.067) | 0.101* (0.055) | 0.016 (0.033) | 0.041 (0.064) | -0.168*** (0.064) |
| Observations | 484 | 484 | 484 | 477 | 484 | 482 |
| Mean: Control, no husband/co-parent | 0.207 | 0.563 | 0.230 | 0.080 | 0.281 | 0.438 |
| Mean: Control, has husband/co-parent | 0.213 | 0.574 | 0.213 | 0.065 | 0.404 | 0.527 |

Notes: ‘Has husband/co-parent’ is defined as (a) currently married or (b) divorced/separated with children under 18 in the household. 16 respondents are missing administrative data on children in the household; this table assumes that such respondents do not have children, presenting a robustness check to the main analysis in Table 3 in which such cases are excluded from analysis. Outcome variables are constructed as described in Table 1. Three additional observations are dropped due to missing baseline marital status, which are used as control variables and missing values are imputed using a dummy variable adjustment approach in Table 1. Results are similar if we follow the same imputation method. All estimates include individual and household controls: age (in bins), indicators for each education level, indicators for employment status, household size (number of members), and number of cars owned (indicators for one car and for more than one car), and randomization cohort fixed effects. SEs clustered at the household level. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$.

Appendix B Additional Details and Specifications from Pre-Analysis Plan

We registered the outcome variables presented in the paper in a Pre-Analysis Plan; the full PAP document is ungated on the AEA RCT Registry (AEARCTR-0005551). In this appendix, we present additional details related to the PAP.

First stage PAP with interim followup data and cross-randomized subsidy information

We registered a first stage PAP and carried out an interim follow-up telephone survey immediately following the training, between January and early December of 2020. This survey collected key initial outcomes, to evaluate short-term impacts on take-up of the training course, completion of the training course, issuing the driving license and driving. However, the team faced differential attrition in this interim follow-up survey. Thus, the paper presents outcomes from the main followup survey, following the second stage of the PAP. For completeness, we present here the estimates registered in the first stage of the PAP.

In addition to our main treatment, we also cross-randomized a light-touch information treatment informing respondents of the availability of a government subsidy for rideshare costs (Uber). This subsidy was available for women with three or fewer years of experience, employed in the private sector at a salary up to SAR 8,000 (USD 2,133). This subsidy was worth 80% of the total cost of each commute to and from work in the Riyadh area, up to a cap of SAR 800 per month, for up to one year. Women would be eligible to apply for the subsidy after taking up employment in a private sector firm, and eligible to receive it for up to one year. Survey data indicate that 86% of our sample would potentially be eligible for the subsidy (at a current job or if they took up employment), based on their experience and salary history; this intervention was intended to make eligible respondents aware of the program. 49% of respondents were not yet aware of the program at the time of the information treatment. The cross-randomization allows us to test for substitution or complementarity between driving and other forms of transport to work.

Tables B1 - B2 present the results from the *first stage* of the PAP using the interim followup survey, and incorporating the interacted specification outlined in the first stage PAP.

Consistent with the main follow-up results in Table 1, we find strong effects of the driver’s training treatment on completion of the official Saudi driver’s training and receipt of license (Table B1). Treated women are significantly more likely to have driven in the previous month, and they are driving more often.

Providing information to respondents about the availability of the subsidy program might be expected to lead to a substitution effect, as women plan around using cheap ride-hailing rather than driving themselves to work. (Table A4 shows that a quarter of trips reported by women in the control group were taken on ride-hailing.)

However, at the interim follow-up we find little evidence that the subsidy information shifted outcomes

of interest, in particular respondents' expectations over ride-hailing costs as a result of the subsidy, which is the most immediate outcome that the information treatment should have affected to have any impact on downstream outcomes (Table B1, Panel A, Column 3), driving takeup, or other measures. This could occur because respondents did not understand or remember the message, or because they did not believe they would in fact be eligible.

In addition, at the time of the short-term interim follow-up, 1-3 months after treatment, we did not detect treatment effects of either the main driving treatment or the subsidy information on job search outcomes (Table 1, Panel C). It is likely that the impacts of treatment on employment occurred over a longer time horizon and were thus only detected in our main followup survey, 1.5-2 years after treatment.

Thus, because of limited evidence of effects on both immediate outcomes (expected commute costs) or downstream outcomes at the time of our short-term interim follow-up survey, we registered our second stage pre-analysis plan with a simple specification testing for effects of the main driver's training treatment, as shown in the draft, rather than the cross-randomized treatment.

Going *beyond* the pre-specified estimates, we further confirm that our main results are unchanged in a fully interacted model with the rideshare subsidy (Table B3).

Variables not collected in final version of main follow-up survey

For the main followup survey, we pre-registered variables based on a full length survey. However, initial waves of the followup survey faced substantial attrition. Therefore, to combat attrition, we completed the main follow-up survey with a substantially shorter survey instrument. A smaller number of indicators in each pre-registered outcome family was collected. The following variables were cut from the completed shorter version of the main follow-up survey and thus excluded from the analysis:

- Stated first-order attitudes on the following statements:
 - “The government should make all laws apply to men and women the same way.”
 - “Women can be good politicians and should be encouraged to stand in elections.”
 - “As citizens it is our responsibility to hold leaders accountable for their decisions.”
 - “As Muslims, we should be more active in examining the guidance of Imams and cultivate our own understanding of Islam.”
- Stated first- and second-order attitudes on the statement “A university education is more important for a boy than for a girl”.
- Education and work aspirations for daughter / granddaughter
- Interactions with other people via text and social media
- Group membership (savings groups, volunteering, hobby/recreational groups, parent/school associations, religious groups) and attendance

Additional details on specifications

The PAP indicated we would incorporate fixed effects at the level of the randomization stratum. Randomization was stratified within the six recruitment cohorts; within these it was stratified by age group, car ownership, and self-assessed likelihood of driving. However, because of the small sample size, this resulted in a substantial number of singletons. In addition, some strata are very small, such that there are further singletons in interaction specifications with family structure. Therefore, our preferred specification employs fixed effects for the larger group within which randomization was stratified, the recruitment cohort. Table B4 shows that our main results are unchanged when we incorporate fixed effects for the smaller strata.

Finally, the pre-analysis plan also details an additional 2SLS specification in which treatment assignment would be used to instrument for takeup of a driver's license. Further consideration suggests that the assumptions required for this instrument to be valid may be too strong in this experiment; thus we have not presented such estimates. Of course, as with all 2SLS estimation, this would simply rescale our ITT estimates to account for incomplete takeup of the intervention.

Table B1: PAP Part I specifications: Treatment effects on driving training, license, expected commute cost, and mobility measured in interim follow-up

| Panel A: driving training, license, expected commute cost, and driving frequency | | | | | | |
|---|---|---------------------------------|--|-----------------------------|--|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | Completed driver's training | Received license | Expected cost of commute on e-hailing including any discount | Drove in the previous month | Driving frequency: estimated number of trips per month | Expected likelihood of driving in the future |
| β_1 : Driving training | 0.702*** (0.043) | 0.407*** (0.051) | 2.789 (3.187) | 0.311*** (0.070) | 0.830*** (0.193) | 0.089 (0.066) |
| β_2 : Rideshare subsidy | 0.053 (0.042) | 0.019 (0.043) | -4.771 (3.326) | -0.014 (0.075) | 0.023 (0.212) | 0.102 (0.074) |
| β_3 : Driving training x Rideshare subsidy | 0.014 (0.069) | 0.127* (0.074) | 0.794 (4.152) | -0.007 (0.097) | 0.019 (0.285) | -0.102 (0.088) |
| Observations | 395 | 395 | 314 | 394 | 394 | 394 |
| Control Group Mean | 0.058 | 0.022 | 34.714 | 0.168 | 0.445 | 0.803 |
| Panel B: Trips taken in previous 24 hours | | | | | | |
| | Type of trip taken in previous 24 hours | | | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | Drove yesterday | Trip taken within last 24 hours | Without any family member accompanying | Visit relatives | Visit friends | Any destination other than work/study commute |
| β_1 : Driving training | 0.005 (0.026) | 0.002 (0.081) | 0.040 (0.068) | -0.017 (0.052) | 0.016 (0.036) | 0.065 (0.082) |
| β_2 : Rideshare subsidy | -0.016 (0.020) | 0.090 (0.087) | 0.096 (0.083) | -0.009 (0.057) | -0.014 (0.034) | 0.147* (0.084) |
| β_3 : Driving training x Rideshare subsidy | 0.053* (0.031) | -0.067 (0.109) | -0.033 (0.100) | 0.031 (0.067) | 0.020 (0.048) | -0.077 (0.103) |
| Observations | 410 | 391 | 390 | 389 | 390 | 390 |
| Control Group Mean | 0.007 | 0.635 | 0.270 | 0.096 | 0.051 | 0.679 |

Notes: OLS regressions; SEs clustered at household level; strata FEs. All columns include individual and household controls: age (in bins), indicators for each education level, indicators for employment status, marital status (indicators for married, never-married, and widowed), household size (number of members), and number of cars owned (indicators for one car and for more than one car). The outcome in Panel A, Column 6 was constructed as follows: respondents who reported not driving in the previous month were asked “will you drive in the future? How likely are you to drive?” with a Likert response scale. We then create a dummy variable for likeliness differentiated at the median response. This was also coded as 1 if the respondent reported driving in the previous month. All outcomes reported in this table were collected during the interim follow-up. Variations in sample size are due to drop-off from telephone survey. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Table B2: PAP Part I specifications: Treatment effects on job search measured in interim follow-up

| | (1) | (2) | (3) | (4) | (5) |
|--|----------------------|---|--|--|--|
| | Looking for a job | Attended a career fair in last 3 months | Proportion of job search activities taken in the last month | Travel to search (visited a job center or employers in person) | Self-reported reservation wage |
| β_1 : Driving training | -0.009 (0.071) | -0.063 (0.042) | -1.927 (3.999) | -0.011 (0.075) | -136.014 (217.906) |
| β_2 : Rideshare subsidy | 0.045 (0.078) | -0.035 (0.049) | -1.060 (4.494) | -0.070 (0.084) | -1.394 (234.975) |
| β_3 : Driving training x Rideshare subsidy | -0.007 (0.096) | 0.136** (0.057) | 1.977 (5.380) | 0.131 (0.103) | 1.610 (307.517) |
| Observations | 405 | 404 | 405 | 405 | 289 |
| Control Group Mean | 0.746 | 0.106 | 28.991 | 0.359 | 3717.308 |
| | (1) | (2) | (3) | (4) | (5) |
| | Job applications | Interview invitations | Interviews attended | Willing to take a job for 3000 SAR 15 minutes away | Willing to take a job for 3000 SAR 30 minutes away |
| β_1 : Driving training | 0.205 (0.463) | 0.004 (0.055) | 0.001 (0.052) | 0.030 (0.084) | -0.083 (0.081) |
| β_2 : Rideshare subsidy | 0.591 (0.642) | 0.033 (0.061) | 0.007 (0.058) | 0.063 (0.092) | 0.019 (0.091) |
| β_3 : Driving training x Rideshare subsidy | -0.403 (0.803) | 0.034 (0.076) | 0.028 (0.072) | -0.060 (0.113) | -0.035 (0.111) |
| Observations | 405 | 405 | 405 | 394 | 394 |
| Control Group Mean | 1.894 | 0.155 | 0.141 | 0.474 | 0.328 |

Notes: OLS regressions; SEs clustered at household level; strata FEs. All columns include individual and household controls: age (in bins), indicators for each education level, indicators for employment status, marital status (indicators for married, never-married, and widowed), household size (number of members), and number of cars owned (indicators for one car and for more than one car). All outcomes reported in this table were collected during the interim follow-up. Variations in sample size are due to drop-off from telephone survey; order of survey modules was randomized. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Table B3: Treatment effects on driving, mobility and labor market outcomes: robustness to fully interacting rideshare subsidy and driver’s training

| Panel A: Driving and Independent Mobility | | | | | | |
|---|---------------------------|---------------------|---------------------------|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | Started driver’s training | Received license | Any driving in past month | Number of times left house in last 7 days | Share of trips made without male chaperone | No trips made without male chaperone |
| β_1 : Driving training | 0.621*** (0.056) | 0.399*** (0.058) | 0.183** (0.073) | 0.770 (0.695) | 0.085 (0.067) | -0.085 (0.073) |
| β_2 : Rideshare subsidy | 0.086 (0.064) | 0.040 (0.056) | 0.037 (0.075) | 0.516 (0.698) | 0.011 (0.073) | -0.019 (0.079) |
| β_3 : Driving training x Rideshare subsidy | 0.005 (0.079) | 0.075 (0.077) | 0.042 (0.095) | -0.059 (1.024) | -0.002 (0.090) | -0.003 (0.096) |
| Observations | 469 | 469 | 489 | 470 | 461 | 461 |
| Control Group Mean | 0.192 | 0.102 | 0.335 | 5.200 | 0.433 | 0.491 |
| P-val: $\beta_1 + \beta_3 = 0$ | 0.000 | 0.000 | 0.000 | 0.315 | 0.169 | 0.166 |
| P-val: $\beta_2 + \beta_3 = 0$ | 0.058 | 0.035 | 0.180 | 0.507 | 0.858 | 0.691 |
| Panel B: Labor Market Outcomes and Independent Decision Making | | | | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | Employed | Unemployed | Out of labor force | On the job search | Allowed to leave the house without permission | Allowed to make purchase without permission |
| β_1 : Driving training | 0.126** (0.057) | -0.136* (0.073) | 0.009 (0.062) | 0.059 (0.040) | 0.139** (0.067) | -0.033 (0.070) |
| β_2 : Rideshare subsidy | 0.074 (0.059) | -0.024 (0.075) | -0.050 (0.062) | 0.005 (0.038) | 0.097 (0.070) | 0.096 (0.076) |
| β_3 : Driving training x Rideshare subsidy | -0.098 (0.078) | 0.075 (0.095) | 0.023 (0.080) | -0.052 (0.053) | -0.159* (0.088) | -0.103 (0.094) |
| Observations | 488 | 488 | 488 | 481 | 488 | 486 |
| Control Group Mean | 0.210 | 0.569 | 0.221 | 0.072 | 0.344 | 0.484 |
| P-val: $\beta_1 + \beta_3 = 0$ | 0.604 | 0.325 | 0.524 | 0.836 | 0.727 | 0.031 |
| P-val: $\beta_2 + \beta_3 = 0$ | 0.651 | 0.392 | 0.589 | 0.190 | 0.270 | 0.901 |

Notes: OLS regressions; SEs clustered at household level. All columns include individual and household controls: age (in bins), indicators for each education level, indicators for employment status, marital status (indicators for married, never-married, and widowed), household size (number of members), and number of cars owned (indicators for one car and for more than one car), and randomization cohort fixed effects. Panel A, Column (5) and (6) outcomes are set to zero for 24 observations in which the respondent reported making no trips outside the home in the previous 7 days. The outcome in Panel B, column (2) indicates the respondent reports she is not working but is searching for a job. The outcome in Panel B, column (4) indicates whether the respondent is employed and applied for at least one job in the previous month (a more general measure of search beyond job applications was not collected for employed respondents). Results for unemployment are similar if we redefine unemployed to include only those who applied for at least one job in the previous month. The outcomes in Panel B columns 5 and 6 are constructed as follows: respondents were asked to rate their level of agreement (using a 5 point Likert scale from ‘completely disagree’ to ‘completely agree’) with the statements “If I wanted to meet with a friend outside of my home, I could do so without seeking approval / permission from anyone in my household first” and “I can make a purchase of 1000 SAR without needing to take permission from any member of my family” (1000 SAR is roughly equivalent to 265 USD, in 2021 dollars), respectively. The outcome variables are indicators for above-median response on the Likert scale for each statement response. Variations in sample size are due to drop-off from telephone survey; order of survey modules was randomized. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Table B4: Robustness to fixed effects for small group strata: Treatment effects on driving, mobility and labor market outcomes

| Panel A: Driving and Independent Mobility | | | | | | |
|--|---------------------------|---------------------|---------------------------|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | Started driver's training | Received license | Any driving in past month | Number of times left house in last 7 days | Share of trips made without male chaperone | No trips made without male chaperone |
| Treatment | 0.600*** (0.042) | 0.426*** (0.041) | 0.185*** (0.049) | 0.700 (0.511) | 0.090* (0.048) | -0.087* (0.051) |
| Observations | 456 | 456 | 478 | 460 | 450 | 450 |
| Control Group Mean | 0.195 | 0.104 | 0.339 | 5.173 | 0.429 | 0.494 |
| Panel B: Labor Market Outcomes | | | | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | Employed | Unemployed | Out of labor force | On the job search | Allowed to leave the house without permission | Allowed to make purchase without permission |
| Treatment | 0.071* (0.041) | -0.101** (0.050) | 0.030 (0.041) | 0.036 (0.026) | 0.053 (0.048) | -0.091* (0.049) |
| Observations | 476 | 476 | 476 | 470 | 477 | 475 |
| Control Group Mean | 0.208 | 0.567 | 0.225 | 0.068 | 0.350 | 0.480 |

Notes: OLS regressions; SEs clustered at household level; strata FEs. All columns include individual and household controls: age (in bins), indicators for each education level, indicators for employment status, marital status (indicators for married, never-married, and widowed), household size (number of members), and number of cars owned (indicators for one car and for more than one car). Panel A, Column (5) and (6) outcomes are set to zero for 24 observations in which the respondent reported making no trips outside the home in the previous 7 days. The outcome in Panel B, column (2) indicates the respondent reports she is not working but is searching for a job. The outcome in Panel B, column (4) indicates whether the respondent is employed and applied for at least one job in the previous month (a more general measure of search beyond job applications was not collected for employed respondents). Results for unemployment are similar if we redefine unemployed to include only those who applied for at least one job in the previous month. The outcomes in Panel B columns 5 and 6 are constructed as follows: respondents were asked to rate their level of agreement (using a 5 point Likert scale from 'completely disagree' to 'completely agree') with the statements "If I wanted to meet with a friend outside of my home, I could do so without seeking approval / permission from anyone in my household first" and "I can make a purchase of 1000 SAR without needing to take permission from any member of my family" (1000 SAR is roughly equivalent to 265 USD, in 2021 dollars), respectively. The outcome variables are indicators for above-median response on the Likert scale for each statement response. Variations in sample size are due to drop-off from telephone survey; order of survey modules was randomized. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$