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Drivers of Change: Employment Responses to the Lifting of the Saudi Female Driving Ban[†]

By Chaza Abou Daher, Erica Field, Kendal Swanson, and Kate Vyborny*

We conduct a field experiment to quantify the impact of the lifting of the Saudi women's driving ban on women's employment by randomizing rationed spaces in driver's training. Treated women are 41 percent more likely to be employed yet are 19 percent less likely to be able to make purchases without family permission. Patterns of heterogeneous treatment effects reveal that these divergent impacts of access to driving are experienced by distinct subgroups of women. The results underscore the importance of intrahousehold responses that can counteract legal gains in women's freedoms. (*JEL* C93, D13, J16, J22, K38, O15, O17)

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 jobs and restrict their movement or work hours (World Bank 2022). These settings are also characterized by disproportionately low rates of female employment (Gonzales et al. 2015). But does reducing legal barriers to female economic participation necessarily generate significant increases in women's employment? Countries with gendered legal barriers are also likely to have strong norms against women's employment that shape women's preferences over work and curtail female work opportunities even when laws change. Indeed, female labor force participation (FLFP) remains low in many settings where legal restrictions have largely been abolished, such as India and Pakistan (Field and Vyborny 2016; Fletcher, Pande, and Troyer Moore 2018).

To shed light on this question, we study female employment responses to a sudden and dramatic change in women's rights that occurred in Saudi Arabia in 2018 when women were granted the legal right to drive after 61 years of prohibition. The

*Abou Daher: Alnahda Society (email: chaza.hks@gmail.com); Field: Duke University Department of Economics and NBER (email: field.erica@duke.edu); Swanson: Duke University Sanford School of Public Policy (email: kendal.swanson@duke.edu); Vyborny: World Bank South Asia Region Gender Innovation Lab (email: kvyborny@worldbank.org). Rema Hanna was the coeditor for this article. We appreciate helpful comments from participants in workshops at Duke University. We gratefully acknowledge funding support for the study was received from Uber's Masaruky Initiative, the Ministry of Human Resources and Social Development in Saudi Arabia, and Harvard's Kennedy School's Evidence for Policy Design (EPOD), and especially the support and collaboration throughout the research from Alnahda Society, particularly Jawaher Al-Sudairy. We appreciate excellent research assistance from Adam Soliman, Hussain al-Shammasi, Munira Alsharif and Ghida Ismail. The Duke University Campus IRB approved this study (protocol 2019-0505). This study is registered as AEARCTR-0005551 in the AEA RCT Registry (Field and Vyborny 2023).

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reform, which was pitched as an economic stimulus designed to encourage female employment, was received with excitement by human rights advocates but also skepticism about its potential impact given other first-order constraints to women's employment. For instance, until recently, guardianship rules restricted both female mobility and agency over work, and women faced legal restrictions on occupational choice (Sadek 2022; Alnahda Society 2019; Government of KSA 2017).

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 gender-progressive laws, FLFP increases (Hallward-Driemeier, Hasan, and Bogdana Rusu 2013; Hyland, Djankov, and Goldberg 2020; Gonzales et al. 2015). However, progressive legislation may arise in response 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 pin down the impact of any one particular reform.

In order to isolate the impact of lifting the driving ban on female employment, we conducted a randomized experiment in the immediate wake of the repeal that eased women's constraints on obtaining a 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: 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 percent of the average monthly salary of Saudi women and six times higher than the course fee for men (Gulf Business 2018). On account of these restrictions, two years after the ban was lifted, only 2 percent of women in the country had obtained a 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, over the proceeding two years 53 percent of respondents in the treatment group received a driver's license compared with only 10 percent of respondents in the control group. Moreover, access to training led to a dramatic and statistically significant increase in women's independent mobility. After two years, treated respondents were 61 percent more likely to have driven in the previous month and made 20 percent more of their trips without a male chaperone.

The change in access to a license was also accompanied by large and statistically significant effects on work: Treated respondents were 11 percentage points (19 percent) less likely to be unemployed or searching and 9 percentage points (41 percent) more likely to be employed at endline. Because our study sample overrepresents low-income women for whom access to employment is particularly valuable, we also estimate weighted treatment effects that account for sample differences in education and labor force status, which indicate that access to a license is associated with a 72–84 percent increase in employment even among a more representative sample of Saudi women. This finding provides new evidence on the economic cost of legal gender discrimination, adding rigorous experimental evidence to an existing literature that retrospectively evaluates employment responses to changes in women's legal rights with mixed results (see Doepke, Tertilt, and Voena 2012; Duflo, Kremer, and Robinson 2011 for reviews). More concretely, it demonstrates the potential for

reductions in commuting costs to increase female employment even in highly conservative settings, linking to a nascent literature on gendered constraints to physical mobility (Borker 2018; Velásquez 2020; Siddique 2022; Jacoby and Mansuri 2015; Kondylis et al. 2020).[1] While Saudi Arabia received global attention for placing legal prohibitions on women drivers, our results have relevance for a broader set of policies that could address nonlegal barriers to female driving in the many other settings in which we observe gender gaps in de facto access.[2] For example, in India only 6 percent of driver's licenses nationwide are held by women (Ministry of Road Transport and Highways (India) 2020).

Alongside these changes in women's work behavior, we also find evidence of a significant negative response among men to female family members learning to drive in terms of their willingness to grant them economic autonomy. In particular, despite being more likely to work for pay, treated women report significantly *less* decision-making power over spending and also report that men in their social network are significantly less supportive of women working.

Patterns of heterogeneous treatment effects reveal that these divergent impacts of access to driving are experienced by distinct subgroups of women. Employment gains are concentrated among younger women, while spending restrictions are concentrated among those above median age. Even more stark is the pattern by marital status: Only women who are either widowed or never married experience gains in employment, while those who are either married or divorced experience reductions in spending autonomy with no corresponding increase in the likelihood of working. This pattern suggests that male backlash is driven by husbands or co-parents, who are most likely to influence work and spending decisions of married and divorced women, and that backlash does not arise in response to women's higher earned income since those who experience it are no more likely to work.[3] In fact, the latter group is 9.5 percentage points *less* likely to be in the labor force as a result of access to driving, reflecting a shift from unemployment to nonparticipation. These findings contribute to a growing literature documenting social backlash in response to gender reforms and shed further light on why legally entrenched gender discrimination is so difficult to change in much of the world despite high potential economic returns.[4]

[1] This literature includes a subset of papers that directly test whether improved transport services increase women's economic participation (Borker 2021; Cheema et al. 2025; Burde and Linden 2013; Jacoby and Mansuri 2015; Christensen and Osman 2023; Muralidharan and Prakash 2017; Field and Vyborny 2022), as well as a burgeoning literature exploring how state intervention can shift women's economic participation in both developed countries (Blundell et al. 2016; Low et al. 2018; Borella, De Nardi, and Yang 2023; Guner, Kaygusuz, and Ventura 2020; Eissa and Liebman 1996; Schanzenbach and Strain 2020; Bick and Fuchs-Schündeln 2017; Smith et al. 2003; 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) and, to a lesser extent, developing countries, where more conservative social norms may constrain women's autonomy (Bertrand 2011; Jayachandran 2015, 2021).

[2] World Bank (2024) documents that, although gendered restrictions on driving a vehicle are rare, 30 percent of economies still have some form of legal restrictions on women's freedom of movement, many of which consist of legal restrictions on women leaving the marital home without a husband's permission.

[3] Other work on spousal opposition to female employment includes Field et al. (2021) and Mckelway (2018).

[4] Several recent studies have demonstrated how policy and legal changes can lead to backlash when they threaten existing identities, norms, or interests, including Blumenstock, Dube, and Hussain (2022); Fouka (2020); Wheaton (2022); Abdelgadir and Fouka (2020); Gottlieb (2016); Mehmood, Naseer, and Chen (2022); Bursztyn, González, and Yanagizawa-Drott (2020); Andrew et al. (2022); Brulé (2018); and Anukriti, Erten, and Mukherjee (2022). This stands in contrast to evidence of similar changes leading to more progressive views among men or reductions in domestic violence (Beaman et al. 2009; Delaporte and Pino 2022; Kuipers 2020). Similarly, several studies show that challenges to existing gender norms can subject women to backlash in the

I. Setting

Our study takes place in Riyadh, in the Kingdom of Saudi Arabia (KSA). Saudi Arabia has consistently ranked near the bottom of global gender parity measures, standing one hundred forty-second out of 144 countries analyzed in the 2017 World Economic Forum's Global Gender Gap Report in female economic participation and opportunity (World Economic Forum 2017). Male family members have substantial leverage over women in Saudi Arabia. Every woman has a legal male guardian: her father and then her husband, while divorced and widowed women come under the guardianship of their fathers, sons, or brothers. Historically, guardians have had extensive legal say over women's lives, including whether they travel and where and with whom they live, and children's guardians maintain official status as "head of household" for government administrative processes such as birth registration and children's school enrollment (KSA Bureau of Experts 1986, 2000; US Department of State 2022; AlRiyadh 2019).[5] During the period of our study, unmarried women were not legally allowed to live alone. Although women could legally obtain employment without the approval of a guardian, many employers continued to seek written approval of the guardian before offering a woman a job (Debees n.d.).

Legal restrictions have also severely curtailed Saudi women's bargaining power in marriage, particularly as conceptualized by the notion of "threat point." Supplemental Appendix Table A1 lays out details of women's guardianship and head of household rights by marital status. Women in Saudi Arabia have little ability to threaten to leave a marriage because they cannot initiate no-fault divorce and face substantial financial costs and uncertainty in initiating fault divorce.[6] Moreover, divorced men retain a high degree of bargaining power over ex-wives even once they lose official guardianship status because only fathers have the right to guardianship over children. Thus, a divorced father can determine where children will live and make financial decisions related to them. The father can also take back physical custody if the mother remarries or if a court determines that the mother is incapable of raising the child in accordance with the appropriate religious standards, which could potentially be influenced by a mother's employment (Nihal 2022; Morley 2023; Sadek 2022).

While guardians and ex-husbands are likely to have substantial leverage over women's decisions to work, certain norms curtail their control over women's earned income, which may influence male preferences over women working. In

form of marital friction, dissolution, or violence (Bertrand, Kamenica, and Pan 2015; Folke and Rickne 2020; Ashraf, Field, and Lee 2014; Bobonis 2011; Angelucci 2008; Bobonis, González-Brenes, and Castro 2013; Heath 2014; Erten and Keskin 2021; Bulte and Lensink 2019; Luke and Munshi 2011; Tankard, Paluck, and Prentice 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 (Roy et al. 2019; Angelucci 2008; Haushofer et al. 2019; Heath, Hidrobo, and Roy 2020; Bobonis, González-Brenes, and Castro 2013; Hidrobo and Fernald 2013; Hidrobo, Peterman, and Heise 2016).

[5] This section describes the legal environment at the onset of our study. Some of these restrictions were lifted midway through the study, including laws granting women the legal right to live alone and take a job without male approval (Al Amir 2021). However, our assessment is that these changes are too recent to have impacted behavior by the time of our follow-up, and hence the legal environment at the onset of the study is most relevant for circumscribing early responses to the reform.

[6] Less than 1 percent of divorces are wife initiated (Daqaeq 2022). While a man can unilaterally divorce his wife—until 2020 he could do so without her knowledge (US Department of State 2022)—a woman must go through the *khul'* process, which requires a financial payment and court approval, or seek an annulment by proving that she suffered harm (Al-Sharif 2022; Nihal 2012).

particular, the religious doctrine of *nafaqah* 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; Sadek 2022). This is widely interpreted as implying that, if a woman works, her income is her own and she has no responsibility to contribute to household financial needs (Ali Khan 2005; Khan 2021; Amini 2011; Schatzmiller 2019; Khan 2005). Additionally, employers in Saudi Arabia are legally required to pay workers via direct deposit (International Labour Organisation 2020), and 74 percent of working women in Saudi Arabia reported having an individual bank account in 2021 (Demirgüç-Kunt et al. 2021).

Starting in 2016, the government announced a series of reforms (the "Vision 2030" plan) with an explicit goal of increasing FLFP from 22 percent to 30 percent by 2030 (Government of Saudi Arabia 2016; Alshuwaikhat and Mohammed 2017). Individual announcements about specific reforms followed gradually, 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; civic and labor laws to protect women's rights in employment and criminalize gender pay discrimination; 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). In September 2017, the government further issued a decree lifting the Kingdom's 61-year ban on women's driving, effective June 2018.[7]

The driving reform was framed with the goal of enabling women to commute to work and was heralded as a major step toward female economic inclusion. However, there was also skepticism about whether granting women access to driving would increase female employment due to other notable barriers. First, social norms and stigma may reduce women's comfort with both driving and 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). Relatedly, family disapproval may pose a binding constraint on driving or working, particularly under the guardianship system. More than half of our sample reported 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." Safety and harassment concerns might also discourage women from driving or working even when they are legally allowed to, especially at early stages of policy reform (Ali et al. 2021). Finally, demand-side factors, including discrimination, limited job opportunities that match women's skills, and the cost of adapting the physical workplaces for required gender segregation may also prevent Saudi women from entering the labor force en masse (Miller, Peck, and Seflek 2022; Eger et al. 2022).

II. Experimental Design

A key complication in investigating the impact of any nationwide policy change, including the lifting of the driving ban, is identifying a comparison group of women unaffected by the reform. To do so, we experimentally vary whether women in our

[7] The Vision 2030 announcement was widely expected to result in a lifting of the ban, but the timing was uncertain.

study have access to a driver's license. Our study began 12 months after the reform was enacted (Supplemental Appendix Figure A1). At that time there were only seven licensed driving schools for women nationwide and just one in Riyadh, which had a wait list of several months. Obtaining a license required 30 hours of instruction and a fee of 3,000 SAR ($800 USD), half the average monthly salary for women and six times the fee for men (Gulf Business 2018; Saudi Arabia General Authority for Statistics 2020).

This supply constraint enables us to identify the causal impact of gaining access to a driver's license by randomizing and subsidizing rationed and costly spots in driver's training. With cooperation from the Saudi government, we designed an intervention that granted free and immediate access to driver's education for a randomly chosen set of women who expressed interest in driving, including training, license exam and fees, and prearranged travel to and from training. While this treatment does not perfectly mimic the legal reform of granting the right to drive, the rationing and high fees of driver's training for women arguably constituted a "partial" implementation of the reform by the government, possibly as an attempt to actively slow the pace of de facto change. Hence, we interpret our intervention as shifting a randomly chosen set of women into a "full reform" environment.[8] The intervention was implemented by Alnahda Society, a Riyadh-based nonprofit organization dedicated to improving the well-being of Saudi women. Women in our study were recruited from among the beneficiaries of social programs offered through Alnahda and two sister organizations. These NGOs target needy families for financial support, skills training, counseling, and legal aid. As such, women in our sample are disproportionately poor, in households with an average monthly income of 2,500 SAR (667 USD), less than a quarter of the national median (King Khalid Foundation 2013).

Supplemental Appendix Table A2 compares our experimental sample with Saudi population statistics. Study participants have a similar age profile to the national population but are much more likely to be divorced or widowed. Women in our sample are also more likely to be in the labor force, reflecting our partner organizations' emphasis on assisting women in economic hardship. Most households have a car but share access to it among multiple adults (Supplemental Appendix Table A3).[9] Interim survey data from a subsample of the control group confirm that few walk or take the bus, and most women are driven or rely on taxis and ride hailing for everyday commuting needs (Supplemental Appendix Table A5). Thus, access to a license has the potential to be a binding constraint for this population.

Given the difference in socioeconomic status (SES), treatment effects might be larger for our study population than they would be among wealthier women, for whom access to employment is less valuable. We address this issue of external validity at the estimation stage by reweighting observations to account for sample differences in the distributions of education and labor force status. Additionally, as is often the case in impact evaluations, participation in our study selects on women who express interest in obtaining a license, at least when it is offered with assistance. Hence, the

[8] Because the treatment offered participants access to a course with limited slots but also offered it at no cost, the treatment could induce an income effect. In general, this should be expected to reduce labor supply, so if anything it should attenuate our results on employment.

[9] The 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.

correspondence between our estimates and the population-level impact will also depend on the fraction of women interested in driving. However, population-level data indicate extremely high interest: In a 2017 national poll, 80 percent of women reported a desire to drive pending the change in law (Flanagan 2018), and in baseline eligibility screening 83 percent of Alnahda beneficiaries indicated interest in driving.

Recruitment took place in six rounds from May 2018 until August 2019. In total, 375 participants (62 percent) were allocated to the treatment group and 231 (38 percent) to the control group.[10] Randomization was stratified into six recruitment cohorts in which prospective participants were enrolled into the study over time and across partner organizations.[11] Because the study allowed enrollment of more than one family member, randomization was conducted at the household level to minimize spillovers.[12] Participants assigned to the treatment group were offered driver's training in one of two cycles, July–September 2019 and October–December 2019.

The main follow-up survey was conducted between July 2021 and February 2022, 1.5–2.5 years after the intervention (Supplemental Appendix Figure A1).[13] In light of concerns over in-person surveying in the wake of the COVID-19 pandemic, the survey was administered by phone and was kept brief (with a target length of ten minutes) to maximize response rates.[14] Although data collection was organized by our partner organization, to minimize social desirability bias, interviews were conducted by short-term interns.[15] The final response rate was 83 percent, and both attrition rates and baseline characteristics are balanced across treatment arms (Supplemental Appendix Tables A6 and A3).[16]

[10] The sample size in the control group is smaller than the sample size in the treatment group because the number of current beneficiaries available from the participating NGOs was small relative to the number of slots available in driver's training based on the amount of grant funding the NGO received and the logistical cooperation with the official government driving school.

[11] We further incorporated an additional level of stratification by age group, car ownership, and above-median anticipated likelihood of driving, giving us 52 substrata at the stage of randomization. As detailed in Supplemental Appendix B, our preferred specification uses fixed effects for the larger strata based on recruitment cohort only because of a large number of singletons in the substrata; however, Supplemental Appendix Table B10 shows our key results are unchanged by including fixed effects for substrata.

[12] Alnahda enrolled all interested women within all its beneficiary households such that 23 percent of households in our sample include more than one respondent. Controlling for this household characteristic does not change our results.

[13] We carried out an interim follow-up survey a few months after completion of the intervention; Supplemental Appendix B presents outcomes from the interim survey.

[14] Phone ownership is nearly universal even for low-income women in Saudi Arabia (UN ITU DataHub 2024). To maximize respondent privacy, calls were conducted during daytime hours when male family members were likely to be out of the house.

[15] We also do not expect strong social desirability bias given that a large fraction of respondents were no longer active beneficiaries of Alnahda and so should have little potential to perceive a gain from strategic responses to an Alnahda survey. This is also evidenced by the fact that multiple follow-up calls were necessary to convince participants to complete the survey, which is difficult to reconcile with strong social desirability bias.

[16] Supplemental Appendix Tables A7–A8 test whether participants differentially attrit by treatment status and several key baseline covariates; we do not find statistically significant evidence of differential attrition on the dimensions of heterogeneity we study. Moreover, we regress attrition status on treatment interacted with the vector of all covariates and test for the joint significance of the treatment-covariate interactions; the p-value of this test is 0.530 (result in replication package). Results with the conservative Lee (2009) bounds adjustment are included in Supplemental Appendix Table A9; because our sample, and thus strata, are small, Lee bounds are unstable with the strata and control variables in our preferred specification, so this table includes the main point estimate and the bounds estimated with no controls or fixed effects. While the bounded estimates are less precise given our small sample, the bounds are relatively narrow around the main point estimates for key outcomes including employment.

The survey, found in Supplemental Appendix D, elicited information on a respondent's driving behavior, including whether she had started and completed driver's training, whether she had received a license, and her frequency of driving in the last month. To gauge independent mobility, respondents were asked about chaperoned and unchaperoned (i.e., with or without a male *mahrem*) trips outside the house in the last week. To measure labor force status, respondents were asked if they were employed, whether they were searching for a job, and the number of job applications in the last month. To capture intrahousehold constraints, respondents were asked to report whether they agree with the following 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 (approximately USD 265) without needing to take permission from any member of my family." In order to characterize participants' gender attitudes, women were asked to rate their level of agreement with the following statements: "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," "I think it's OK to sometimes put my own needs above those of my family," and "The government should allow a national women's soccer team." They were also asked to report the ideal age for a woman to have her first child. The first three of these questions were also used to capture participants' second-order beliefs about the attitudes of male family members and men and women in their social network. Specifically, participants were asked how many people in each group would "somewhat" or "completely" agree with each of the three statements.[17] Finally, we collected measures of social and civic engagement, including the number of people the respondent had met and spoken to on the phone in the previous week, whether she planned to vote in the upcoming election, and whether she was interested in signing up for Alnahda volunteering and leadership programs.[18]

Descriptive statistics from the control group reveal a population with limited mobility and agency at the time of follow-up. While 19 percent of respondents had started training by endline, only 10 percent had received a license. Despite this, 34 percent reported driving in the past month.[19] However, they did not leave the house very often. Women in the bottom quartile of mobility reported leaving the house fewer than three times in the last week, and 5 percent had not left the house at all. Only those in the top quartile are mobile enough for regular commuting: 34 percent reported leaving the house six or more times in the last week. Independent

[17] Response options were "none," "a minority," "about half," "a majority," or "all of them."

[18] We transform Likert scale outcomes to indicators for above/below median throughout the paper rather than using raw Likert scale points. Avoiding the use of scale points avoids the problem of implicitly assuming that intervals between the points are equal (i.e., that a change from "strongly disagree" to "disagree," for example, means the same as a change from "disagree" to "neutral"), and it allows for more interpretable estimates: "the average of 'fair' and 'good' is not 'fair-and-a-half'" (Blaikie 2003; Jamieson 2004).

[19] Some women did drive without a license prior to the ban being lifted, and driving without a license is also observed in the control group at follow-up. The latter fact suggests that for some women, the need to travel outweighed the possible consequences of being caught by police. Anecdotally, driving without a license is only common for short trips within the neighborhood since traffic police patrol and check licenses regularly on main roads, such that not having a license is still likely to be a binding constraint on most employment.

mobility is substantially lower: Half the respondents reported that they had not left the house without a chaperone even a single time in the last week.[20]

In terms of employment, 21 percent reported being employed and 57 percent were unemployed and searching. Notably, women in the control group experienced an upward trend in employment from a baseline of 16 percent, consistent with generalized employment responses to the broad set of gender reforms discussed in Section I. Correspondingly, as shown in Supplemental Appendix Figure A2, there is substantial variation in first- and second-order beliefs about female employment 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. Women in this setting also reported a low degree of control over movement and spending. Among women in the control group, 51 percent "completely disagree" that they could leave the house to meet a friend without permission, and 31 percent "completely disagree" that they could make a purchase without permission. Note that the latter measure does not simply proxy for women's individual income: Within the control group, 50 percent of working women and 48 percent of those who are not working report they can make an independent expenditure decision.

III. Empirical Estimation

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

$$Y_{ij} = \beta_0 + \beta_1 TREAT_i + \boldsymbol{\gamma}' \mathbf{X}_i + \mu_j + \epsilon_{ij}, \tag{1}$$

where Y_{ij} is an outcome of interest for respondent i recruited in cohort j; $\mathbf{X}_i$ is a vector of baseline controls prespecified in the PAP, and μ_j are fixed effects for the randomization strata. Standard errors are clustered at the household level, the unit of randomization. Results without controls are presented in Supplemental Appendix Table A10; they are nearly identical and in some cases more precise than the results with controls.

A pre-analysis plan (PAP) was registered on the AEA RCT Registry (Field and Vyborny 2023) in two stages: a first stage before the short-term follow-up survey and a second stage before the main follow-up survey. The paper presents outcomes from the main survey following the second stage of the PAP. Supplemental Appendix B describes the PAP in more detail, including the following variations between the PAP and our main analysis: (i) In addition to our main treatment, we also cross-randomized a light-touch treatment informing respondents of the availability of a government subsidy for ride-hailing costs (Uber). Because commuting data from our interim survey revealed very minimal take-up of the subsidy, the second-stage PAP focused on testing only the effects of the main treatment. In the

[20] Such limited mobility also occurs in other settings in which women's labor force participation is low and mobility is restricted by norms or safety concerns. While data with the same seven-day recall is not available from other contexts, Andrew and Smurra (2024) report that 45 percent of married women in the India Time Use Survey did not leave the house at all in the previous 24 hours.

current analysis, we verify that our main results are robust to a fully interacted specification. (ii) Due to time constraints imposed by our local partner, we were required to shorten the main follow-up survey partway through fieldwork. As a result, we dropped from the survey a subset of questions on attitudes, social contact, and membership in community groups. (iii) Labor supply was a key outcome in both the preliminary and final PAPs. However, ex ante we anticipated that the proximate outcome, job search, would be more feasible to observe than employment within the time span of our study, particularly in light of the COVID-19 pandemic, which we expected (incorrectly) to dramatically lower transition rates into new jobs. In fact, by the time of follow-up, many treated women had already found work and thus stopped searching; hence we are better positioned to observe impacts on employment than job search at follow-up, and so we emphasize the former in the analysis. (iv) We employ fixed effects by a larger stratum than prespecified because of a large number of singleton strata in the initial groupings. The results are unchanged when we estimate with the smaller strata, but we choose to showcase the specification that does not drop observations. (v) We do not present results from a 2SLS specification described in the initial PAP because of concerns about instrument validity.

IV. Results

Our empirical analysis of follow-up survey data first presents treatment effects on women's driving and mobility, followed by the corresponding (ITT) effects on employment and financial control. We then investigate heterogeneous treatment effects across key demographic characteristics.

A. *Individual Responses to Driving*

As shown in Table 1, panel A, granting women free and immediate access to driver's training had a large first-stage effect on female driving and mobility. The intervention led to a dramatic and persistent shift in the probability of both enrolling in driver's training and obtaining a license: At follow-up, only 19 percent of respondents in the control group have started driver's training compared to 81 percent of those in the treatment group (column 1), 53 percent of the treatment group have received a license compared to only 10 percent of the control group (column 2), and treated women are 61 percent more likely to have driven in the previous month (20.3 percentage points, $p = 0.000$, column 3).[21] The incomplete take-up in the treatment group is striking given that the RCT sample was restricted to women who had indicated interest in driving at baseline, and it provides immediate evidence of constraints to women's driving beyond cost and individual openness to getting a license.[22]

We observe corresponding impacts on women's physical mobility beyond driving. Women in the control group report leaving the house on average five times in

[21] Note that the probability of completion conditional on starting the training is also higher at 65 percent in the treatment group versus 53 percent in the control group, which likely reflects the fact that women in the control group who started training by the time of follow-up likely started the course later than those in the treatment group.

[22] Anecdotally, 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.

Table 1—Treatment Effects on Individual Outcomes and Intrahousehold Responses

	Started driver's training (1)	Received license (2)	Any driving in past month (3)	Number of times left house in last 7 days (4)	Share of trips made without male chaperone (5)	Always travels with male chaperone (6)
Panel A. Driving and independent mobility						
Treatment	0.618	0.426	0.203	0.739	0.087	−0.091
	(0.039)	(0.039)	(0.047)	(0.475)	(0.045)	(0.048)
Observations	467	467	489	470	461	461
Control mean	0.192	0.102	0.335	5.200	0.433	0.491
β/control mean	3.219	4.176	0.606	0.142	0.201	−0.185
p-value $\beta = 0$	0.000	0.000	0.000	0.121	0.051	0.058

	Employed (1)	Unemployed (2)	Out of labor force (3)	On the job search (4)	Index: own attitudes toward women working (5)	Index: social contact (6)
Panel B. Labor, individual attitudes, and social interactions						
Treatment	0.086	−0.106	0.019	0.031	0.110	0.058
	(0.043)	(0.049)	(0.041)	(0.026)	(0.097)	(0.111)
Observations	488	488	488	483	490	474
Control mean	0.210	0.569	0.221	0.072	0.000	0.000
β/control mean	0.410	−0.186	0.086	0.431	–	–
p-value $\beta = 0$	0.045	0.032	0.643	0.236	0.259	0.602

(*continues*)

the last seven days and report leaving without a male chaperone on average 43 percent of the time. Treatment effects on frequency of travel outside the home are imprecisely estimated but suggest a 14 percent increase in trips taken, the equivalent of one trip per 10–11 days ($p = 0.121$, column 4); treatment had a large and statistically significant impact on the share of trips that women make without a male chaperone, which increases by 20 percent (8.7 percentage points, $p = 0.051$, column 5). As shown in Supplemental Appendix Figure A3, treatment shifts 19 percent 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 in the last week compared to only 40 percent in the treatment group, a decrease of 19 percent (9.1 percentage points, $p = 0.058$, column 6).

Table 1, panel B explores treatment effects on women's economic and social inclusion, including labor force outcomes, attitudes toward work, and social contact. Overall, treatment induces a large and significant increase in women's rate of employment. Specifically, treated women are 41 percent (column 1, 8.6 percentage points, $p = 0.045$) more likely to be employed at follow-up relative to the control group, most of which appears to be a shift out of unemployment, which falls by 19 percent (column 2, −10.6 percentage points, $p = 0.032$). If we assume that access to driver's training only affects employment through obtaining a license, this implies that receiving a license nearly doubles women's chances of being employed. The employment results align with the observed effects on patterns of driving: As shown in Supplemental Appendix Figure A4, panel A, treatment shifts women from never driving to driving a few times a week or daily, consistent with an increase in regular commuting.

Table 1—Treatment Effects on Individual Outcomes and Intrahousehold Responses (*continued*)

	Agreement with the following statements		Indices: second-order attitudes toward women working	
	Allowed to leave house w/o permission (1)	Allowed to make purchase w/o permission (2)	Female social network (3)	Male social network (4)
Panel C. Permission to leave house and to make a purchase, second-order attitudes				
Treatment	0.057	−0.093	−0.046	−0.200
	(0.045)	(0.047)	(0.099)	(0.100)
Observations	488	486	486	487
Control mean	0.344	0.484	0.000	−0.000
β/control mean	0.166	−0.192	–	–
p-value $\beta = 0$	0.207	0.051	0.642	0.045

Notes: Panel A, columns 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 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); five individuals responded to work status but not to the applications measure, leading to the variation in sample size between columns. 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 and in panel C, columns 3 and 4 are weighted indices of sets of standardized outcomes described as follows using the swindex command developed by Schwab et al. (2020). For panel B, column 5, respondents were asked to rate their own level of agreement (using a 5 point Likert scale) for the following statements: "Women can be equally good business executives," "It's ok for a woman to have priorities outside the home," "Children are OK if a mother works," "It's OK to put my own needs above those of my family," and "The Government should allow a national women's soccer team." Responses were transformed into binary indicators for above median response. Respondents were also asked what the ideal age is for a woman to have her first child. These outcomes are reported in Supplemental Appendix Table A12, panel A. For panel B, column 6, women were asked about the number of people they spoke with and met in the previous 7 days. These outcomes are reported in Supplemental Appendix Table A12, panel B. For panel C, columns 1 and 2, respondents were asked to rate their level of agreement (on a 5 point Likert scale) with the following 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 US$(2021)265), respectively. Responses were transformed into binary indicators for above median response. For panel C, columns 3 and 4, respondents were asked to think about a group and report what share of that group ("none," "a minority," "about half," "a majority," or "all") they think would "somewhat" or "completely" agree with the following statements: "Women can be equally good business executives," "It's ok for a woman to have priorities outside the home," and "Children are OK if a mother works." Responses were transformed into binary indicators for above median response. Second order beliefs questions are indexed for one female reference group (female community members) in column 3 and two male reference groups: male family members and male community members in column 4. The components of the second-order attitudes indices are reported in Supplemental Appendix Table A16; in panel B and C of that table, we additionally report the indices separately for male family members and male community. Variations in sample size are due to drop-off from telephone surveys; the order of survey modules was randomized. All estimates include individual and household controls: age (above median dummy), education level (less than a high school degree), marital status (indicators for married, never married, and widowed), household size (number of members), number of cars owned (indicators for one car and for more than one car), an indicator for baseline labor force participation, and strata fixed effects. SEs are clustered at household level. We replace missing control values with zero and include missing dummies for each.

There is no detectable change in the rate of nonparticipation (column 3, 8.6 percent or 1.9 percentage point increase, $p = 0.643$), which indicates that access to a license did nothing to move work behavior among women who had already opted out of the labor force. There is suggestive evidence of an increase in on-the-job search, but the estimate is not statistically significant (column 4, 43 percent or 3.1 percentage point increase, $p = 0.236$). [23]

[23] The fact that treatment increases the proportion of women employed but not the proportion searching could occur for several reasons. First, treated women's searches might become more productive because they are in a

Given that our study sample is drawn from a lower-SES subpopulation, we use administrative data from Saudi Arabia GASTAT (2017) and Saudi Arabia GASTAT (2018) to estimate population-level impacts on employment by reweighting observations to account for sample differences in the distributions of education (by age group) and labor force participation. As reported in Supplemental Appendix Table A11, these estimates indicate that the implied labor market impacts are even larger in the general population in which access to driver's training is associated with a 15–17 percentage point increase in employment (panels B and C, column 1, $p = 0.003$ and 0.011, respectively). Driving may be particularly impactful for higher-SES women if, for instance, poor households are less likely to own one or more vehicles (only 16 percent of our sample own more than one car).

The observed impacts on female employment indicate that access to a driver's license significantly reduced women's commuting costs, which is sensible in this setting. Even accounting for the cost of a vehicle, driving is the lowest cost option for many commutes in Riyadh given limited public transportation options and the high cost of private sector alternatives such as ride hailing (Field et al. 2018). Even if a woman does not have regular access to a car, the ability to drive could affect her willingness to take a job if she anticipates negotiating access to a shared car or purchasing one in the future. 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. The large treatment effects on employment further imply that the expected returns to lower commuting costs outweigh any increases in the value of women's leisure that arise from their greater ability to travel unaccompanied more generally, which should weakly reduce incentives to work.

An alternative interpretation is that female employment rises because the possibility of driving increases women's bargaining power in the household by improving their outside options in the event of divorce (Aizer 2010). Greater bargaining power could enable a woman to negotiate favorable employment outcomes (which could be either an increase or a decrease in labor supply, depending on her preferences) with family members once she has obtained a license even if she does not ultimately drive herself to work. However, because of the legal barriers to female autonomy and divorce discussed in Section I, we deem this a less plausible mechanism for employment responses in the Saudi setting. On the other hand, it is possible that a woman's employment response to driving is either magnified or attenuated by her anticipated gains in autonomy over spending once employed. As discussed in Section I, in the Saudi context this could arise out of the cultural norm of *nafaqah*, which stipulates that men must pool earned income, but women should not be asked to do so.[24] If a woman is not expected to pool her earnings, the autonomy of spending she gains from earned income increases her personal incentives to work while decreasing the value of her work for other household members.

better position to take up employment in a larger set of potential locations. Alternatively, it is possible that searches in fact increased at an earlier point in time, and, by the time of our follow-up survey, this had subsided since many women had indeed found jobs.

[24] A similar argument could be made based on the fact that, if a woman drives herself to shop, it is more difficult for household members to monitor her spending.

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. We test for treatment effects on women's gender attitudes using the survey data described in Section II, reported in Table 1, panel B, column 5. The estimated treatment effect on an index of progressive gender attitudes is a 0.11 SD increase, but the estimate is imprecise (column 5, $p = 0.259$), and treatment effects on individual index components vary in direction (Supplemental Appendix Table A12). Hence there is no clear evidence that employment responses are driven by changes in women's own attitudes or preferences. It is also possible that driving broadens women's social networks, which could have an indirect effect on employment behavior, for instance, if networks are an important source of job referrals. However, the estimated treatment effect on the index of social contact is also small and imprecisely estimated (column 6, 0.06 SD, $p = 0.602$). 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.

Following our PAP, we also estimate treatment effects on women's political attitudes and civic engagement (Supplemental Appendix Tables A13 and A14). We do not detect significant effects of treatment on either category of outcomes. However, an important caveat is that our measures of engagement and attitudes were significantly reduced from their original scope, as described in Section III, hence we relegate both outcomes to the Supplemental Appendix.

B. *Intrahousehold Responses to Women Driving*

We next examine how access to a driver's license changes household restrictions placed on women's mobility and economic independence. Rather than simply prohibiting women from working, household members may respond to women's increased opportunity to drive by imposing new restrictions—such as monitoring spending—that reduce women's incentive or opportunity to work, particularly if women lack a credible outside option.

Panel C, columns 1–2 of Table 1 present treatment effects on women's decision-making power over mobility and expenditure. Given that significantly more treated women both work for pay and drive alone, we would expect positive impacts on both independent mobility and spending autonomy, all else equal. Indeed, the estimated effect on women's ability to leave the house without permission is positive and large but statistically insignificant (column 1, 17 percent or 5.7 percentage point increase, $p = 0.207$). In contrast, treatment 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 drops by 19 percent (column 2, -9.3 percentage points, $p = 0.051$), consistent with the idea that women's newfound ability to travel independently and earn and spend without direct observation led family members to substitute toward alternative forms of expenditure monitoring and control.[25]

[25] Supplemental Appendix Table A15 shows the results for spending autonomy reweighting observations to account for sample differences in the distributions of education (by age group) and labor force participation. In each case, the negative effect on spending autonomy is similar or larger than that in our main results.

Backlash from 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 perceptions of the gender attitudes of men and women in their social network. We report impacts on indices of second-order beliefs in panel C, columns 3 and 4 of Table 1, and impacts on individual index components are reported in Supplemental Appendix Table A16.[26] We observe no effect of driving on beliefs about other women's attitudes (column 3, −0.05 SD, $p = 0.642$) but find negative and significant effects on respondents' beliefs about men's gender attitudes (column 4, −0.2 SD, $p = 0.045$), consistent with the backlash interpretation of treatment effects on spending control. These results may reflect a reactionary change in men's attitudes as they witness female contacts driving and working and either update their beliefs or seek to rationalize their entrenched resistance to women joining the labor force, but they could also be explained by women learning about men's preexisting attitudes by experiencing negative reactions to driving and working. Either case is consistent with a negative sentiment among men regarding women's opportunity to drive.

C. *Heterogeneous Treatment Effects*

An open question is whether family members imposing spending restrictions on women when they are given access to a driver's license reflects a response to increased employment among female family members or, conversely, a means of deterring them from seeking a job. We can gain insight into this by examining whether changes in employment and spending autonomy are experienced by the same subgroup.

To better understand which subgroups of women are benefiting from the opportunity to drive and which are experiencing backlash, we look at heterogenous treatment effects across a range of baseline dimensions, including age, education, marital status, and labor supply (Table 2 and Table 3). Certain patterns emerge that paint a more complex picture of household responses to lower commuting costs and provide suggestive evidence that employment and backlash responses are operating on distinct margins. First, we observe reductions in spending autonomy in responses to driving only for the following subgroups of women: Those who are above median age, those without a high school degree, and those who were ever married (column 4 in Table 2). Meanwhile, employment responses appear to be concentrated among younger and single women but also widowed women (column 2, Table 2, panels A and C). Interestingly, women with a high school degree are no more likely to become employed than are less educated women as a result of driving despite the fact that they are significantly more likely to receive a license (Table 2, panel B, columns 1 and 2).

But why do younger, single women respond to the opportunity to drive by working while older, married and divorced women experience backlash? While the effects could be driven by any number of unobservable differences across these groups, an important clue might come from the observed impacts of driving on widows

[26] Index items are coded such that positive values reflect more progressive attitudes toward women's rights and roles.

TABLE 2—HETEROGENEOUS TREATMENT EFFECTS

	Received license (1)	Employed (2)	Not in LF (3)	Allowed to make purchase w/o permission (4)
Panel A				
β_1: Treatment	0.523	0.141	−0.005	0.038
	(0.055)	(0.063)	(0.059)	(0.070)
β_2: Above median age	0.162	−0.024	0.093	0.279
	(0.060)	(0.074)	(0.066)	(0.091)
β_3: Treatment × above median age	−0.176	−0.103	0.046	−0.245
	(0.073)	(0.081)	(0.077)	(0.092)
$\beta_1 + \beta_3$	0.347	0.038	0.041	−0.207
	(0.052)	(0.055)	(0.054)	(0.062)
Observations	467	488	488	486
Mean: control, below median age	0.092	0.247	0.188	0.329
Panel B				
β_1: Treatment	0.497	0.078	0.009	−0.024
	(0.052)	(0.055)	(0.048)	(0.059)
β_2: Less than HS	−0.007	−0.104	0.064	0.063
	(0.057)	(0.059)	(0.068)	(0.080)
β_3: Treatment × less than HS	−0.206	0.038	0.025	−0.191
	(0.078)	(0.078)	(0.085)	(0.094)
$\beta_1 + \beta_3$	0.291	0.116	0.034	−0.215
	(0.058)	(0.060)	(0.074)	(0.075)
Observations	459	479	479	477
Mean: control, completed HS	0.129	0.265	0.186	0.451
Panel C				
β_1: Treatment	0.430	−0.033	0.065	−0.220
	(0.060)	(0.070)	(0.070)	(0.072)
β_5: Treatment × married	−0.133	0.141	0.049	0.050
	(0.100)	(0.112)	(0.105)	(0.131)
β_6: Treatment × never married	0.162	0.184	−0.164	0.345
	(0.085)	(0.105)	(0.101)	(0.108)
β_7: Treatment × widowed	−0.226	0.294	−0.027	0.031
	(0.142)	(0.135)	(0.114)	(0.161)
Observations	463	484	484	482
Mean: control, divorced	0.083	0.250	0.266	0.597
Mean: control, married	0.091	0.171	0.114	0.472
Mean: control, never married	0.080	0.246	0.281	0.293
Mean: control, widowed	0.208	0.080	0.120	0.654

Notes: Variations in sample size are due to drop-off from telephone surveys; the order of survey modules was randomized. Outcomes are defined as described in Table 1. In panel C the omitted marital status category is divorced women. All estimates include individual and household controls: age (above median dummy), education level (less than a high school degree), marital status (indicators for married, never married, and widowed), household size (number of members), number of cars owned (indicators for one car and for more than one car), an indicator for baseline labor force participation, and strata fixed effects. SEs are clustered at household level. We replace missing control values with zero and include missing dummies for each, except for the interaction control. As such, some observations are lower relative to Table 1. Ten respondents are missing values for education level at baseline, with some overlap in respondents who are also missing values for outcomes. Four respondents are missing values for marital status.

who are above median age and below median education but experience employment gains comparable to young, educated, and single women. One potentially important common denominator is the absence of a husband or co-parent with whom they

Table 3—Heterogeneous Treatment Effects, Continued

	Received license (1)	Employed (2)	Not in LF (3)	Allowed to make purchase w/o permission (4)
Panel A				
β_1: Treatment	0.467	0.206	−0.060	0.012
	(0.058)	(0.063)	(0.061)	(0.070)
β_2: Has husband/co-parent	0.005	0.151	−0.125	0.167
	(0.066)	(0.073)	(0.076)	(0.085)
β_3: Treatment × has husband/co-parent	−0.066	−0.217	0.155	−0.197
	(0.076)	(0.084)	(0.080)	(0.092)
$\beta_1 + \beta_3$	0.401	−0.011	0.095	−0.184
	(0.051)	(0.056)	(0.056)	(0.062)
Observations	463	484	484	482
Mean: control, no husband/co-parent	0.118	0.190	0.226	0.419
Panel B				
β_1: Treatment	0.366	0.164	0.083	−0.187
	(0.067)	(0.072)	(0.112)	(0.120)
β_2: In LF at BL	0.111	0.139	−0.123	−0.053
	(0.047)	(0.058)	(0.097)	(0.106)
β_3: Treatment × in LF at BL	0.071	−0.092	−0.076	0.112
	(0.080)	(0.086)	(0.120)	(0.127)
$\beta_1 + \beta_3$	0.437	0.072	0.007	−0.075
	(0.045)	(0.048)	(0.044)	(0.050)
Observations	466	487	487	485
Mean: control, out of LF at BL	0.000	0.037	0.370	0.536

Notes: Outcomes are defined as described in Table 1. "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 (above median dummy), education level (less than a high school degree), household size (number of members), number of cars owned (indicators for one car and for more than one car), an indicator for baseline labor force participation, and strata fixed effects. SEs are clustered at household level. Marital status dummies are not included as a control in panel A because they are highly collinear with "has husband/co-parent." However, results are unchanged if we include individual indicators as controls for never married, married, and widowed (divorced/separated is the reference group). We replace missing control values with zero and include missing dummies for each, except for the interaction control. As such, observations are lower relative to Table 1. Four respondents are missing values for marital status (and therefore missing values for whether they have a husband or co-parent), and one respondent is missing a value for labor force participation at baseline. Variations in sample size are due to drop-off from telephone surveys; the order of survey modules was randomized.

must negotiate employment and spending, which could matter if husbands and ex-husbands have systematically different preferences over female autonomy than fathers and brothers. Indeed, the patterns are especially stark when we group women according to this characteristic, comparing women who do versus women who do not have a husband or co-parent (i.e., never married, widowed, or childless divorcees, Table 3, panel A). For the latter category, employment rates in the treatment group jump to 40 percent relative to the control mean of 19 percent, a 110 percent increase (column 2). In contrast, women who are either married or divorced with children are 44 percent more likely to be out of the labor force once they gain access to driving (column 3, 9.5 percentage points, $p = 0.091$), a different indication of potential backlash within the family. We also observe that the decrease in financial control is experienced only by women who have husbands and co-parents, who are 34 percent less likely to be allowed to make purchases without permission

(column 4, 18.4 percentage points, $p = 0.003$). If we interpret this pattern to be driven by differences in intrahousehold bargaining environments, it suggests that fathers are more willing to grant women autonomy over work and spending than are husbands or co-parents. While speculative, there are a number of reasons this could arise. For instance, individuals may exhibit differential altruism toward blood relatives, as in Doepke and Tertilt (2009) and Case (2001), or husbands of working wives may experience disproportionate stigma (Bernhardt et al. 2018).

It is also possible that the observed patterns of labor market responses are driven by differences in preferences across the two groups, for example if married women prefer to use their mobility to spend more time in activities supporting their families rather than working for pay. While we cannot fully rule that out, it is difficult to reconcile this class of explanations with the consistent patterns across married and divorced women and in particular with the result on reduced spending autonomy. Another possible interpretation is that withdrawing from the labor force leaves married and divorced women with less spending autonomy simply because they have lost their source of independent income. However, this subgroup exits the labor force out of unemployment rather than employment (column 3, panel B, Table 3) and so experiences no loss in earned income. Moreover, it is worth noting that the two groups of women have identical patterns of labor force participation and employment in the control group. In addition, this heterogeneous treatment effect is robust to including controls for treatment interacted with education, age, and presence of children (Supplemental Appendix Table A17).

While patterns by baseline labor force participation are imprecisely estimated, if anything, increases in employment are stronger for women who were not in the labor force at baseline (Table 3, panel B, columns 2–3), which suggests that commuting costs are an important barrier to FLFP in this setting. Meanwhile, patterns of backlash mirror the employment results, consistent with spending restrictions increasing for baseline nonparticipants in response to greater earnings power. Taken together, the overall patterns suggest that backlash in this context is driven by two forces: Some women respond to lower commuting costs by working and then experience new restrictions that curtail their potential gains in financial autonomy, while others experience restrictions on financial autonomy that discourage them from working at all.

V. Conclusion

Results from our experiment reveal that granting women access to driving in Saudi Arabia led to a substantial increase in women's employment, which implies that commuting costs are a binding constraint on women's work even in settings with very strong norms against female economic participation and where women face significant barriers to economic agency beyond the right to drive. This underscores the importance of transport constraints for women's labor force outcomes in a wide variety of contexts. While we study the impact of granting women access to the legal right to drive, the results also pertain to the economic and social returns to addressing more commonly encountered constraints to female mobility such as safety concerns and financial commuting costs. Our findings also provide suggestive evidence of the potential for intrahousehold responses to curtail the economic gains

of gender reforms, particularly in societies in which women have very low bargaining power in marriage.

It would be difficult to predict the impact of implementing our intervention at scale—giving free and immediate access to all Saudi women interested in obtaining a license—given potential general equilibrium effects on employment.[27] However, we can use our results to calculate how much of the steady increase in Saudi women's employment postreform (Supplemental Appendix Figure A5) can be attributed to the observed increase in female driver's licenses. As of 2020, only 2 percent of Saudi women nationwide had received a license. Extrapolating our population-reweighted treatment effect estimates implies that access to a license explains only 12 percent of the total 8.6 percentage point increase in female employment over the same period. As an extreme upper bound, if all women nationwide who received licenses were constrained from working only by lack of a driving license, access to licenses would explain only 23 percent of the total increase.

This discrepancy is unsurprising given that our experimental estimates are net of a number of channels through which the lifting of the driving ban is likely to have increased female employment for drivers and nondrivers alike. For instance, the announcement of the reform may have changed expectations around female mobility and potentially increased firms' willingness to hire women. The government's endorsement of women's independent mobility may itself have shifted social norms around FLFP, potentially lowering the stigma or discrimination working women face. Moreover, as discussed in Section I, a large number of labor market reforms that occurred over this period are likely to have encouraged female employment. Hence, while our study was not designed to measure these influences, back-of-the-envelope calculations based on our experimental estimates indicate that factors other than putting women behind the wheel are responsible for the majority of the recent increase in Saudi women's employment.

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[27] In particular, if demand for women's labor in Saudi Arabia is inelastic, then the effect of the intervention at scale would be smaller than implied by the RCT estimate.

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