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# Couples are Made of Four: Intergenerational Transmission of Within-household Allocations

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## Abstract

There is increasing evidence in favor of non-unitary models of the household. Moreover, gender norms and values have been shown to be transmitted across generations and to affect intra-household allocations. I lever a unique opportunity to observe each spouse's contributions to income, market, and home hours of parents and children (after forming their own household) in China and Australia to uncover a strong positive correlation between the female spouse's relative contributions across two generations in the absence of reverse causality. This is robust to the inclusion of a rich vector of controls and provincial fixed effects. Exploiting large exogenous changes in education brought along by the Chinese 1986 Compulsory Education Law, I find that the degree of intergenerational transmission was disrupted by the reform, and that this happened heterogeneously across groups with different parental relative contributions. I further show that this was driven by a change in the attitudes towards gender norms, which suggests that transmission occurs at least partly through socialization and that policies can have a multiplier effect both within and across generations.

**Keywords:** Intra-household Inequalities, Relative Spousal Contributions, Intergenerational Transmission, China, Australia.

**JEL Codes:** D10; I24; J16.

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# 1 Introduction

Gender norms, understood as the standards describing typical or desirable behavior (Tankard and Paluck, 2016), have emerged as an attractive explanation for the persistence in multiple gaps between socioeconomic groups. A prerequisite for their validity is that they are transmitted across generations. Early work already pointed at this direction by showing, for instance, that males whose mother participated in the labor force were more likely to marry a spouse who also participates (Fernández et al., 2004). More recent research abounds on this for East-Asian countries (e.g. Chen and Ge, 2018; Hyun, 2020).

These papers share the idea that individual characteristics or outcomes of own/spouse's parents are drivers of the decisions taken by the child once he/she forms his/her own household. However, such assumption obviates that intra-household allocations are made jointly between spouses<sup>1</sup> and that, potentially, one of their key determinants is how each spouse's *relative* contribution would look like across different scenarios of intra-household allocations. For example, the "male as a breadwinner" paradigm posits that males should contribute beyond 50% of total household income, whereas women are typically expected to contribute close to 100% of home production. These two instances of gender norms based on relative contributions share an emphasis on very specific cutoffs, which is attractive in that they yield clear empirical implications of what should happen around them. For instance, Bertrand et al. (2015), show that there is a sharp discontinuity in female behavior among those who would be more likely to marginally outperform their husbands in the labor market. However, one would expect that, given the underlying continuous nature of spousal relative contributions, they could matter beyond such strict thresholds. For instance, a household where the income split is 60-40 in favor of the male is unlikely to be perceived similarly to another one where the split is 80-20, even if both situations abide by the "male as a breadwinner" idea. This would be particularly relevant in non-unitary models of the household, where relative power of the spouses, and hence household decisions, may depend on individual income and past endogenous decisions (Chiappori and Mazzocco, 2017).<sup>2</sup>

The objective of my paper is to connect the literature on the role of relative spousal contributions in household's allocative choices and that on the intergenerational transmission of characteristics and economic attitudes by answering the following questions: i) is there evidence that

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<sup>1</sup>Recent evidence points towards the importance of bargaining power within the household in final allocative decisions. First, unitary models of the household, although still useful in a variety of contexts, have implications that do not seem to be supported by the data (e.g. Attanasio and Lechene, 2014). Second, work on collective models (Lise and Yamada, 2019) has shown that the Pareto weights of the spouses seem to change over time.

<sup>2</sup>For example, spouses' outside options might depend on past human capital accumulation achieved through experience or on-the-job training. The consequences of endogenous outside options on, for instance, efficiency is a relevant question that requires empirical investigation. Another example is that outside options may be affected by individual income, as suggested by the often-rejected income pooling restriction of unitary models that demand functions do not depend on the within-household distribution of income once total household income is controlled for.

the importance that spouses attach to relative female contributions, as rooted in identity norms, extend throughout the support of the distribution of relative contributions?; ii) if so, are these relative contributions persistent across generations?; and, if the answer is still affirmative, iii) what are the mechanisms behind such transmission?

The main challenge to address this set of novel questions is its intense data requirements: (1) one should observe individual-level outcomes (e.g. time use) for both spouses; (2) it requires the tracking of sons/daughters after moving out of their parents' household; (3) data should be collected for a sufficiently long period of time such that we observe both childhood conditions and post-cohabitation-with-partner ones. To the best of my knowledge, only two datasets satisfy all these requirements: the China Health and Nutrition Survey (CHNS) and the Household Income and Living Dynamics in Australia (HILDA). I make use of these two surveys to provide evidence that, both in China and Australia, not only are gender attitudes transmitted across generations, but that this is so to the extent that the relative spousal final allocations of parents and of children to three key economic outcomes (income, labor hours, and home production) are significantly positively correlated *throughout the support of relative parental contributions*. This is important for several reasons, two of which I outline now.

First, the availability of richer datasets has spurred the interest in exploring intra-household inequalities in addition to inter-household ones. For instance, aggregate measures of consumption or home hours, or the use of equivalence scales, are likely to mask large heterogeneity across household members and lead to distorted normative implications (Chiappori, 2016; Santaella-Llopis and Zheng, 2017; Hyun, 2020). Second, both welfare and intra-household inequalities are not invariant to the decision-maker (Lise and Seitz, 2011; Lise and Yamada, 2019). Indeed, if gender norms are such that they favor the perpetuation of relative spousal power (Pareto weights) within and across generations, for instance by limiting female's relative income or experience in the labor market, the potential reduction of intra-household inequalities linked to increased female labor force participation may be bounded from above. Future revisions of Pareto weights would then be prone to be, if present, predominantly male-favoring.<sup>3</sup> Given the marked differences in preferences displayed by males and females, notably in terms of investments on children in developing countries (e.g. Hoddinott and Haddad, 1995; Duflo, 2003), this has implications, for example, for the design of conditional cash programs (Attanasio and Lechene, 2014)<sup>4</sup>, the closely-related child development (Aizer, 2010) and, in the particular case of China, *missing women* (Qian, 2008, 2018).<sup>5</sup>

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<sup>3</sup>This would potentially exacerbate intra-household inequalities over the lifecycle. There may also be less tangible benefits from women gains in relative power. For instance, Aizer (2010) shows that, as women's relative income increases, they experience less domestic violence.

<sup>4</sup>This connects with the study of consumption inequality within households in collective settings as in Lise and Seitz (2011).

<sup>5</sup>More generally, the effects of the transmission of attitudes and preferences for within-household relative contributions is expected

Final economic outcomes — e.g. time allocation, as opposed to other characteristics that may or may not translate into actual behavior — are arguably the reason why exploring the intergenerational transmission of attitudes matters in the first place (Dohmen et al., 2012). I focus on them, but I make the distinct interpretation of spousal relative contributions to final outcomes as partly capturing attitudes/preferences towards the acceptable relative position of each spouse within a household. My results will provide evidence in favor of this interpretation as well as of the intergenerational transmission of these preferences for outcomes.<sup>6</sup>

This paper is, to the best of my knowledge, the first one to look at intergenerational correlations of relative spousal contributions, which emphasizes not only the extensive margin of spouses' participation decisions, but also the intensive one. I fill this gap providing cross-country evidence between two culturally-distinct nations. CNHS' focus on China is particularly appealing for my research question, as this country has been defined as "one of the most patriarchal family systems that ever existed" (Greenhalgh, 1985) and gender norms have been shown to matter for the extensive margin of female labor force participation and for home production (Xie, 2013; Chen and Ge, 2018). The fact that similar results extend to a more modern country as Australia hints at the external validity of my findings.

Apart from the novelty of the research question, there are a number of additional desirable features of my analysis compared to previous work on intergenerational correlations of *attitudes*. First, studies that explore the transmission of attitudes or risk preferences rely on subjective questions aimed at capturing these dimensions. However, it is not obvious that the items used succeed at this, nor that the framing of the question is irrelevant (Brenøe and Epper, 2019), nor that they translate into actual behavior, especially in unincentivized contexts (Hanaoka et al., 2018). An additional concern is that people provide information that may not be comparable even using the same scale (e.g. Kapteyn et al., 2007). Instead, my measures of income and time contributed by each member are more objective.

Second, by obtaining the relative contributions of each spouse contemporaneously to their actual provision I minimize the risk of measurement error or misreporting that could take place when grown-up children report childhood conditions long-time after experiencing them and in an indirect manner (i.e. when answering questions about his/her parents). Also, observing parental and child information years apart eliminates concerns of reverse causality that arise when contemporaneous measurements, usually of cohabiting individuals, are obtained.<sup>7</sup> Reverse causality is

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to have overarching effects on virtually any within-household decision — through its role on bargaining powers.

<sup>6</sup>Instead, the existing literature focuses on the transmission of mechanisms (e.g. risk attitudes) and then shows that these attitudes and/or characteristics predict final outcomes.

<sup>7</sup>Presence of reverse causality in any case unlikely given my outcomes of interest. For example, it would be hard to imagine that a child's values (not his/her presence) towards spousal time division could influence the final allocation chosen by his/her parents. Similarly, common shocks affecting both parental and child's allocation are unlikely to be driving the results as these ratios are not measured contemporaneously.

arguably one of the most pressing sources of endogeneity in the intergenerational transmission of attitudes (Dohmen et al., 2012). The main approach used in the literature to resolve this issue has been to follow the epidemiological literature initiated by Fernández et al. (2004) in using second-generation migrant's country of origin's attitudes to proxy for parental attitudes that should not be affected by the child (e.g. Ljunge, 2014). However, this approach relies on cross-country comparisons and on the comparability of migrants' conditions in the receiving country regardless of factors such as mother language or religion.<sup>8</sup> My analysis instead takes advantage of the timing of the survey to minimize potential reverse causality and exploits within-province variation in spousal relative contributions to increase the credibility of the comparisons made (Hwang et al., 2019).

Third, the existing literature on intergenerational transmission, specially of attitudes, has chiefly focused on developed countries (e.g. Farre and Vella, 2013; Lindquist et al., 2015) and little is known for developing ones — see Dhar et al. (2019) for a recent example from India. My focus is on China, a rapidly-developing country with strong social norms rooted in culture, where spousal inequalities are large, the welfare system and the social protection for children are weaker than in developed countries along multiple dimensions (health, education, nutrition...), and mothers have been shown to play a crucial role in improving child outcomes (Chen and Li, 2009; Cui et al., 2019).

My main result is that there is a strong and significant positive correlation between the relative contribution to income, home production, and market hours of the mother and the female spouse. In terms of magnitude, in the thought experiment of moving a child from a household where the mother contributes nothing to another one where she contributes the entirety would increase the female spouse's relative contribution in his/her own formed household by 20% of the mean female relative contribution for both income and market hours in the case of China, and by 10% for Australia. These results remain remarkably stable after the inclusion of a comprehensive set of socioeconomic controls of both spouses and parental characteristics (education, wealth, occupation, distribution factors, fertility and health, inter alia) as well as aggregate environmental aspects and provincial fixed effects.

These findings emphasize the importance of thinking of intergenerational transmissions of economic outcomes not only at the individual level, but also as a joint outcome for the two members of a couple. This has implications in the theoretical underpinnings of the formulation of household decisions.<sup>9</sup> Moreover, this would partly explain the presence not only of inter- but also of

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<sup>8</sup>Another limitation is that these studies cannot use parental data as it may be endogenous, so they proxy for it by country of origin characteristics which will not be perfectly correlated with the parents' actual measure if there is dispersion in the actual measure across households.

<sup>9</sup>My findings suggest that spousal work decisions and home production should be derived from utility functions that account for preferences for both parental and peers' relative contributions. Building upon this primitive, a choice of model of the household should

intra-household inequality under non-unitary models — particularly in the now widely-accepted limited commitment formulation where, for instance, relative spousal income may affect bargaining power through binding participation constraints (Mazzocco, 2007; Lise and Yamada, 2019). It also provides a reason for the persistence of gender norms (and the outcomes affected by them) across generations. One example would be the perpetuation of social class, proxied by family income, if households self-constrain the female spouse’s attainable income, which might translate into lower educational and health investment in the children.<sup>10</sup> This idea of lack of social mobility has been explored, among many others, by Chetty et al. (2014) and, for China, by Fan et al. (2020). Another one would be the persistence in gender gaps in labor market outcomes (Blau and Kahn, 2006; Bursztyn et al., 2017).

My work, which shows that the intergenerational correlations hold beyond those individuals who replicate the lack of labor force participation of their mothers, also constitutes an extension of Fernández et al. (2004) in that it demonstrates that simply acknowledging the transmission of preferences towards extensive margins of time allocations (and specifically of only one of the parents, the mother) is not necessarily enough to understand intra-household allocative choices and bargaining power, nor in the cross-section nor in terms of its dynamic evolution. This result, which is hard to reconcile with standard models predicting spousal specialization, also suggests that their analysis offers only a partial view and that a more accurate representation of reality would be to acknowledge the tendency to “marry your parents” rather than simply “marrying your mom”. Linking this to the discussion in the previous paragraph, my paper provides a novel explanation for why studies of intergenerational mobility typically find gradients in persistence throughout the whole distribution, not just at the tails (extensive margins) or at arguably relevant thresholds. The relevance of these findings is likely to extend to other fields. For instance, accounting for intensive time use margins as well as for both spouses’ individual decisions has recently been emphasized as crucial for matching macroeconomic aggregates (Borella et al., 2018).

Despite my focus being on the ultimate economic end, that is, on intra-household allocations, plainly documenting their persistence may still be regarded as a black box. More specifically, this could be the consequence of i) the transmission of values and norms through socialization; ii) similarities in the human capital acquired by parents and children and/or in the marriage market conditions experienced; and iii) inheritance of genetic characteristics that correlate with the outcomes. With this framework in mind, an important contribution of my paper is to go beyond most

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be made. While the unitary framework posits that relative spousal income contribution does not affect behavior (household members pool all income together and the household’s unique utility function does not depend on income nor on other distribution factors), non-unitary frameworks recognize that it can affect household decisions as a distribution factor. Indeed, this testable implication is often used to explore the validity of the unitary models’ assumptions by means of “income pooling” tests (Chiappori and Mazzocco, 2017).

<sup>10</sup>In Table C.1 in the appendix I document the high persistence in educational achievements for individuals born after 1970 using data from the China Family Panel Study.



existing literature in that I also address the mechanisms behind the persistence in gender norms and their relationship with final outcomes.<sup>11</sup>

In order to uncover whether socialization/contextual conditions play a role in this persistence, I exploit as a natural experiment the staggered implementation of a major reform, the Chinese Compulsory Education Law of 1986, which exogenously increased the compulsory years of education to show that, for the cohorts who were just young enough to be subject to the reform, the degree of transmission of relative contributions to home hours is significantly weaker than for those comparable cohorts just too old to be treated. In particular, those individuals brought up in more unequal households were no longer more likely than those exposed to more egalitarian partitions to replicate such unequal pattern. This suggests that a change in values and/or bargaining power within the couple influences the observed patterns, which therefore cannot solely be driven by genetic factors.

To further narrow down the sources of transmission I take advantage of another nationally-representative dataset, the China Family Panel Study (CFPS). I build upon the existing literature highlighting the minor, if existent, effects of the reform on labor market outcomes, and I provide novel evidence that there neither were any significant changes in the marriage market matching process, which is a largely unexplored aspect of the returns to education (Chiappori et al., 2018).

Importantly, there was a clear (self-reported) attitudinal shift towards less traditional gender norms (about the expected role of females in the household and in the society) for the cohorts just affected by the educational reform compared to those closely comparable ones that were not affected. Additional results on the portability of gender norms among cross-provincial migrants highlight the significant role that *policy* plays in norms-related outcomes. This finding, a relevant one within the literature on the non-pecuniary benefits of education<sup>12</sup> (Oreopoulos and Salvanes, 2011), is in line with recent findings by Bau (2019)<sup>13</sup>, (for non-gender-norms-related outcomes) by Lindquist et al. (2015) and Kosse et al. (2020) and, for the specific case of how education can lead to *rapid* changes in attitudes in China, Cantoni et al. (2017).

My work, which also speaks to the role of culture in economics, shows that policy has the potential to generate changes in values that translate into actual behavior. In particular, it implies that policies aiming at equalizing household allocations may not only be effective (something ex-

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<sup>11</sup>A recent work exploring the acquisition of social norms is Olivetti et al. (2020). They focus on a particular reference group, mothers (as opposed to fathers), and look at the intergenerational persistence in the *levels* of female market hours as a function of own and peers' maternal market hours.

<sup>12</sup>Potentially highlighting the room for attitudinal changes both through early childhood interventions (e.g. Heckman, 2006) and during teenager impressionable years (for instance, Shigeoka, 2019).

<sup>13</sup>She focuses on how policy (related to the pension system) can change the incentives to abide by gender norms. Instead, I focus on a context where policy changes the norms themselves (i.e. it now becomes less socially desirable to support traditional gender norms, irrespective of other factors.) Other papers exploring how culture can change are, for example, Bidner and Francois (2011) and Lowes et al. (2017).



ante not obvious) but also have a multiplier effect across generations. I find that children born to parents subject to the reform also display more female-favoring views than the children of similar, but unaffected, parents. This result contributes to our limited knowledge of the causal intergenerational spillovers of education-related outcomes in developing countries (Wantchekon et al., 2015; Agüero and Ramachandran, 2020), particularly in terms of attitudes, values, and preferences, which are typically much harder to be observed across two generations (plus exogenous variation in parental education) than other outcomes such as education or health. It further shows that cultural persistence not only is stronger when the environment is less changing (Giuliano and Nunn, 2017) but that a key source of such environmental rigidity is rooted within households.

The present work also falls within the field of economics of education (particularly on education-related programs and reforms), which traditionally focused on returns to education (e.g. Duflo, 2001), and shows that these reforms can have effects not only attitudes and final allocations within households, but also on their *intensity of transmission* across generations.<sup>14</sup>

The rest of the paper is organized as follows. Section 2 describes the main features of the data and of the empirical approach undertaken to explore the extent of intergenerational correlations in relative parental contributions. Section 3 measures the degree of persistence. Section 4 shows, by means of the Education Reform, that the strength of the intergenerational correlations can be affected by external forces and systematically addresses the potential channels at play. Section 5 argues for the robustness of the results. Section 6 concludes.

## 2 Data and Empirical Approach

### 2.1 CHNS and HILDA

The core of my analysis relies on two datasets. The CHNS is a close-to-nationally-representative collaborative project between economists, sociologists, and nutritionists from the University of North Carolina at Chapel Hill and the Chinese National Institute for Nutrition and Health. It started in 1989 and features ten waves, the last one dating from 2015.<sup>15</sup> While the structure of the data is longitudinal with households being the main unit of observation, individual information on income and time allocation of every household member is available. For more details, see Section B.1. HILDA is an annual survey from the University of Melbourne first undertaken in 2001 and modelled in the spirit of the United States' PSID.

Both datasets present the necessary attributes to undertake my project. Most notably, individual-

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<sup>14</sup>Erten and Keskin (2018) show that an educational reform in Turkey particularly increased female education and explore its consequences on domestic violence. Other extensions of the effects of increased schooling are, for instance, Martinez-Bravo (2017) on political outcomes and Bharati et al. (2018) on schooling as a countervailing force to early life shocks.

<sup>15</sup>Data was collected in 1989, 1991, 1993, 1997, 2000, 2004, 2006, 2009, 2011 and 2015.

level contributions to household income and time allocation are collected for both spouses. Crucially, they both follow split households, and new households are added to the main sample as separate units. I can therefore compute both the relative contribution of the female spouse when the individual was living in his/her parents' home, and the relative contribution of the wife in his/her own household once he/she forms one. Importantly, while HILDA participants are tracked irrespective of their new location, CHNS ones are so as long as they remain within the same geographic unit as the original household was located (I return to this below).

Two additional remarks about the data collection process are worth mentioning. First, individual-level information was provided by the relevant person. This took place in the form of one-to-one interviews in the CHNS<sup>16</sup> and of individual completion of separate booklets to then be returned by mail in HILDA. These procedures reduce the risk of measurement error compared to the surveys where a single person provides individual information about other household members, and reduce concerns of potential misreporting of information driven by social desirability.

In order to better understand the process of transmission of relative contributions, observing individuals' gender attitudes is informative. This information is available in HILDA, but not in CHNS. For this reason, I take advantage of a third dataset, the CFPS. This is a biennial, almost-representative, longitudinal survey launched by Peking University in 2010. It employs a multistage probability sampling procedure to cover around 15,000 households across 25 mainland provinces (hence has a wider geographical coverage than the CHNS) and its modelling was also inspired by the PSID. A more detailed description of the dataset can be found, for instance, in [Xie and Hu \(2014\)](#).

## 2.2 Empirical Approach

The core of the empirical methodology is based on the standard parametric approach in the inter-generational transmission of characteristics. I regress the relative female contribution in a child's own-formed household on the maternal contribution that he/she was exposed to while growing up. In particular, I estimate:

$$femaleshare_{icp}^{child} = \alpha + \beta_0 femaleshare_{icp}^{parent} + \beta_1 X_{icp}^{child} + \beta_2 X_{icp}^{parent} + \lambda_p^{child} + \gamma_c^{child} + \epsilon_{icp}, \quad (1)$$

where  $i$ ,  $c$ , and  $p$  refer to individual, cohort (year of birth,) and province (state in HILDA), respectively. When *femaleshare* is accompanied by "child" this indicates that I am referring to the time when an individual is observed as the head or as the head's spouse of his/her own household, whereas "parent" refers to the value obtained from his/her parents while he/she was growing up.  $X$ , a vector of individual- and household-level pre-determined characteristics featuring, among

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<sup>16</sup>Although there was no explicit requirement for individual interviews to be undertaken in private, it is believed that in practice most of them did follow this format. See page 7 in the 2006's Worker's Manual.

others, gender, education, and ethnic origin is introduced in order to rule out that the intergenerational transmission uncovered is driven by similarities in the controlled factors across generations.

Fixed differences at the provincial level and common effects across cohorts will be accounted for through provincial ( $\lambda$ ) and child year of birth ( $\gamma$ ) fixed effects. I also allow for potentially different trends across provinces/states by including province/state-specific linear trends in robustness checks. Therefore, while it may still be the case that my estimate of the intergenerational persistence partially captures the effects of an omitted variable, the results from my full-fledged model will show that there is a strong significant correlation between parents' and children's female relative contributions after accounting for cross-generational similarities along a wide range of individual and environmental characteristics.  $\epsilon$  is an error term.

I focus on three particularly relevant household outcomes: relative contributions to (after-tax) income, home production<sup>17</sup>, and market hours. They are measured as a proportion (ratio of female contribution to the sum of the contributions of the two spouses, hence ranging between 0 and 1). The rationale for choosing these three main dependent variables is clear. Market<sup>18</sup> and home hours are key time-use categories that reflect the main economic activities of the individuals<sup>19</sup> (e.g. market hours are crucial for a person's individual income), within-household inequalities in the use of private time (e.g. the complement of market and home hours is leisure), as well as the potential effects of gender norms about how each member should allocate his or her time. Market hours are measured as the number of hours worked in all income-generating activities (hence they could include multiple activities) in the year prior to the interview. Home production includes time buying and preparing food, ironing and washing clothes, and taking care of kids and the elderly (if any) during the week prior to the interview<sup>20</sup>. These categories are in line with the ones used in [Aguiar and Hurst \(2007\)](#). The fact that the questions ask about time use over a relatively long time span (a year and a week) contributes to reducing measurement error compared to surveys based on shorter time frames (e.g. ATUS records time use for a single day.) Finally, income is a crucial component of household resources and (various forms of it, such as permanent and individual income) have been used as key measures of spouses' bargaining power in non-unitary models (e.g. [Thomas, 1990](#)).

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<sup>17</sup>I use the terms "home hours/production" and "chores" interchangeably.

<sup>18</sup>I also consider whether there is transmission of labor force participation (LFP) in the extensive margin but it does not seem to be the case in my sample. This is not surprising given that China had a female LFP close to 90% until the mid 1990s in line with Communist mandates ([Maurer-Fazio et al., 2011](#)).

<sup>19</sup>Home hours at the aggregate level represent a sizable fraction of economic production (e.g. [Bridgman, 2016](#)).

<sup>20</sup>For the CHNS, I follow [Fan et al. \(2018\)](#) in not including time cleaning the house in my main analysis, as this variable is often missing. They show that results change little when recoding missing values as zero and including cleaning the house as an additional component of home production. For HILDA, total home production is computed as the sum of hours devoted to housework and household errands.

## 2.3 Sample Selection

The sample selection is straightforward: I only keep individuals who I observe both as son/daughter and as household head/spouse.<sup>21</sup> For these individuals I compute their maternal relative contribution and the female spouse's relative contribution in their own household. In the presence of more than one observation per role I obtain the average value across all observations in an individual-role cell.

While this procedure yields a well-balanced estimating sample for HILDA, the patrilocal aspects of Chinese culture lead the CHNS sample to over-represent males. In particular, 90% of the individuals that I am able to match are of that gender. In Sections B.3, B.4, and B.5 I report a formal analysis providing strong evidence that my restricted sample does not present systematic differences from the non-selected, original one, other than in terms of its gender composition.

In order to be able to undertake my analysis I need to identify those individuals who are not missing any key covariate and have information on both the parental and own relative contributions for at least one of the three outcomes of interest. There are 1,736 in HILDA and 474 in CHNS<sup>22</sup>.

Although this could, in principle, be my estimating sample, for the Chinese case I further exclude individuals born prior to 1962, as these people lived in potentially different conditions due to the Great Chinese Famine (1959-1961) and the disruption in schooling brought along by the Cultural Revolution<sup>23</sup>. This leaves me with 414 individuals.<sup>24</sup> In the main analysis I discard 30 additional individuals born in or after 1980 to be consistent with the sample used in the schooling reform exercise undertaken in Section 4. The number of observations changes slightly across specifications depending on whether the key outcome variables and/or additional controls are missing. For example, there are 371 available observations for income, 365 for home hours and 341 for market hours. I choose to allow this changing sample size given that the baseline number of observations is already not very large.<sup>25</sup>

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<sup>21</sup>See Section B.2 for a brief description on the treatment of household head information in the CHNS. In HILDA I exclusively consider a "child" to be a household head if he/she forms a separate household with a partner.

<sup>22</sup>Unlike for HILDA, for CHNS I do not distinguish between the children who move out to a new household and those who take over the responsibility of being the household head from their parents in order to not decrease the sample size any further. In robustness checks I show that this choice does not affect my results.

<sup>23</sup>The Cultural revolution ended in 1976. Therefore, none of the individuals in my main estimating sample were over 15 years of age by the time of the end of the Cultural Revolution, which should increase the homogeneity of my sample.

<sup>24</sup>This reduction in units of observation is close to the one that takes place when undertaking a somewhat similar sample selection in the Panel Study of Income Dynamics (Vosters, 2018).

<sup>25</sup>Although in order to belong to these close-to-400 individuals I only require to observe the female relative contributions once as a child and once in their own household, these individuals are often observed multiple times in each role. For example, Table C.2 in the Appendix shows that, in the CHNS, for income, individuals are observed, on average, 3.6 times in their parents' household and 2.85 in their own household, accounting for a total of 2,588 person-round observations. The fact that information on the same individual is collected around 6.5 times on average and that I obtain the mean value across the observations within a given role significantly contributes to reducing the noise in our measurements and attenuation biases that have plagued intergenerational studies (Mazumder, 2005). I verify that there is a strong persistence of relative contributions for households across rounds through t-test of

## 2.4 Sample Characteristics

**Descriptive Statistics.** We begin by providing summary statistics of the main variables in Table 1. For the CHNS, these are computed for the whole estimating sample prior to imposing the year of birth restrictions. The CHNS sample has the particular feature that the vast majority of individuals observed in both roles are males (90%). As addressed in Sections B.3-B.5 these individuals are representative of the male child population and my main results would best be interpreted under the lenses of the transmission of attitudes to sons. 15% of my sample belongs to an ethnic minority<sup>26</sup> (as in the baseline sample). Turning to the relative contributions of each spouse we note the persistence in the average ratios for parents and children: while for income and market hours they are around 45%, for home hours it stays close to 80%. These mean values are important when interpreting the size of the intergenerational correlation parameter. One key aspect to note about these ratios, albeit at an aggregate level, is the striking similarity in their levels for parents and children (e.g. 0.800 and 0.804 for home hours, respectively). Among Australians, the most notable aspects are that there is an increase in the equality of splits in all three outcomes among the children's generation, and that home production is significantly less concentrated among females.

## 3 Main Results

### 3.1 Baseline Results

**Average Effects.** Building upon the positive raw correlations arising from plotting the child's household female spouse relative contribution against the child's maternal relative contribution in Figure 1, Table 2 formally presents the baseline results. Column 1 shows the raw intergenerational correlations, only controlling for the child's gender. There is a strong and positive correlation in both samples and for all outcomes, the magnitude being larger for China. Column 2 adds standard pre-determined controls of the child to account for individual heterogeneity: own and spousal education, ethnic background, an indicator for rural/urban location (only for CHNS), and year of birth fixed effects. Point estimates decrease slightly and remain significant. In column 3 I further introduce provincial/state fixed effects, which accounts for fixed characteristics of the region and its inhabitants. While the main results do not change, the correlation for home production in China, albeit still positive, is imprecisely estimated due to the lower degree of variation available within provinces. Column 4 adds a richer set of controls, that slightly differ across datasets. For the CHNS I add: a wealth index, household composition, occupational indicators so as to reduce concerns that our results are purely driving by persistence in labor market conditions or occupa-

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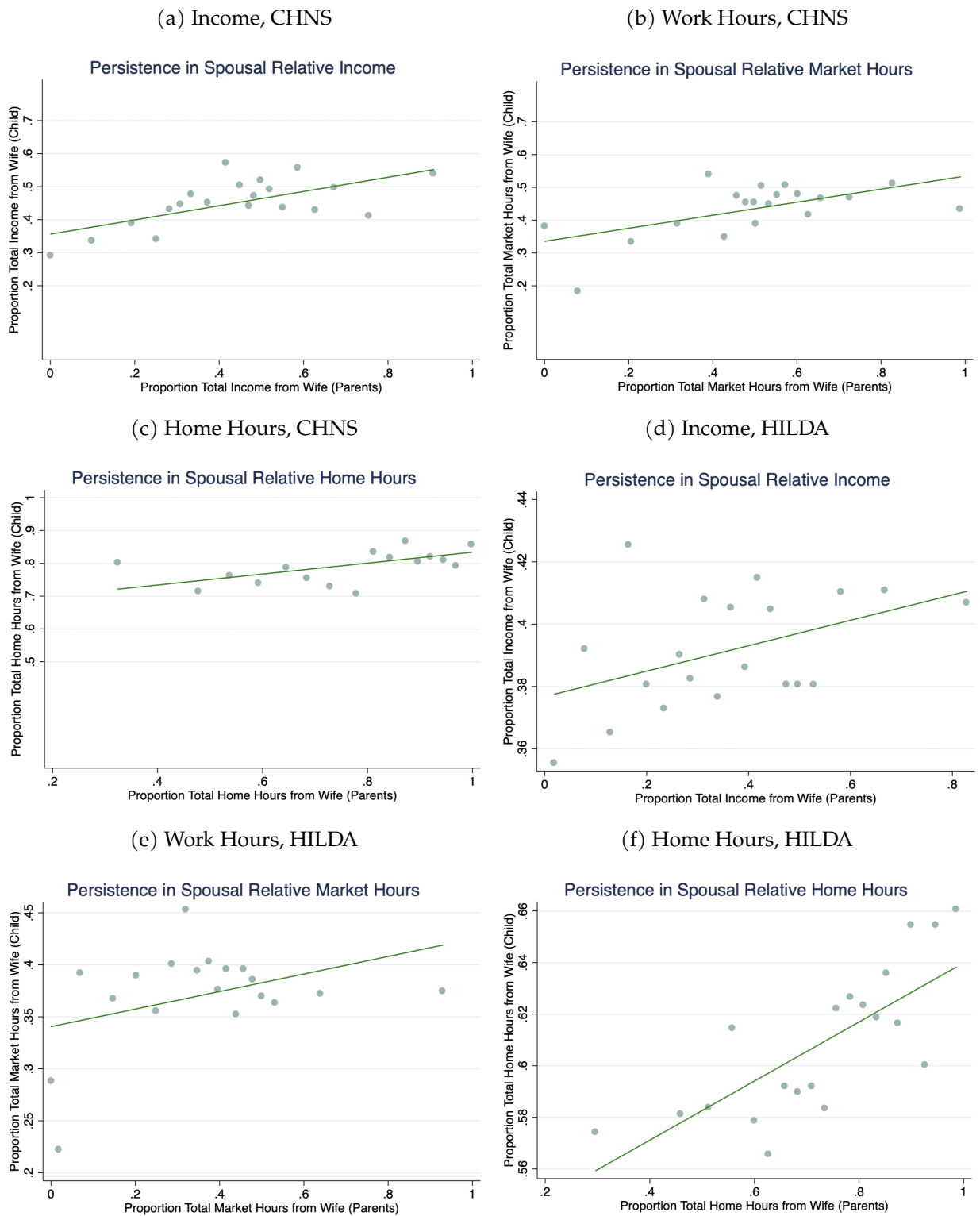
equality (unreported).

<sup>26</sup>An individual is defined as belonging to an ethnic minority if (s)he is non-Han.

Table 1: Descriptive Statistics of Main Variables

	(1)	(2)	(3)	(4)	(5)
	Mean	Standard Deviation	Min	Max	Count
<b>CHNS</b>					
Female (proportion)	0.099	0.299	0	1	474
Rural (proportion)	0.783	0.413	0	1	474
Ethnic Minority (proportion non-Han)	0.152	0.359	0	1	474
Father's Education (years)	4.723	4.664	0	19	471
Mother's Education (years)	2.493	3.937	0	16	474
Child's Education (years)	9.703	3.179	0	16	474
Spouse's Education (years)	8.920	3.601	0	19	474
Ratio Female Total Income (parents)	0.438	0.224	0	1	459
Ratio Female Total Market Hours (parents)	0.496	0.238	0	1	422
Ratio Female Total Home Hours (parents)	0.800	0.210	0	1	465
Ratio Female Total Income (child)	0.449	0.251	0	1	464
Ratio Female Total Market Hours (child)	0.428	0.238	0	1	453
Ratio Female Total Home Hours (child)	0.804	0.182	0.103	1	454
<b>HILDA</b>					
Female (proportion)	0.497	0.500	0	1	1,736
Ratio Female Total Income (parents)	0.361	0.199	0	1	1,736
Ratio Female Total Market Hours (parents)	0.339	0.228	0	1	1,540
Ratio Female Total Home Hours (parents)	0.729	0.177	0	1	1,536
Ratio Female Total Income (child)	0.391	0.159	0	1	1,736
Ratio Female Total Market Hours (child)	0.365	0.200	0	1	1,542
Ratio Female Total Home Hours (child)	0.613	0.173	0	1	1,448

Figure 1: Binscatter PLOTS of Child's Household Female Relative Contribution as a Function of Maternal Relative Contribution



tional segregation, and, as distribution factors, the gap in years of education of the spouses and the sex ratio at birth in 2000 (Lise and Yamada, 2019). For HILDA I add a variable measuring



long-term physical health limitations as well as the average age that an individual is observed in my data. All controls also include parallel measures for parental and own spousal characteristics. The inclusion of this rich set of controls does not affect the findings. Finally, in column 5 I make sure that the results are not driven by extensive margin decisions by conditioning the sample to using only individuals whose mother contributed at least 10% to total income or market hours and not more than 90% in home production. While this restriction makes the intergenerational correlation of market hours no longer significant for HILDA, all other results remain.<sup>27</sup>

### 3.2 Potential Sources of the Intergenerational Correlations: Discussion

In order to systematically explore the sources of the intergenerational persistence uncovered, it is useful to briefly formalize a theoretical framework characterizing the static collective model of the household. This model is sufficient for our purposes as it can account for the allocation of resources and choices made across household members and the role played by spousal bargaining power in such decisions. Under the assumption of efficiency in household decisions, the problem becomes the maximization of a weighted sum of spouses' utilities subject to budget and home production constraints.

For the standard case where there are  $N$  public goods (e.g. a tidy house, represented by the vector  $\mathbf{Q}$ ) produced using market goods ( $\mathbf{X}$ )<sup>28</sup> and individual time ( $D^1$  and  $D^2$ ),  $n$  private goods ( $\mathbf{q}$ ) also produced using market goods ( $\mathbf{x}$ ) and individual time ( $d^1$  and  $d^2$ ), and the spouses enjoy their private leisure ( $l$ ), the problem reads:

$$\max_{\mathbf{x}, \mathbf{X}, d^1, D^1, d^2, D^2, l^1, l^2} \mu^1 U^1(\mathbf{Q}, \mathbf{q}^1, l^1; \mathbf{t}^1) + \mu^2 U^2(\mathbf{Q}, \mathbf{q}^2, l^2; \mathbf{t}^2) \quad (2)$$

subject to

$$\mathbf{p}' \left( \sum_{k=1}^N X_k + \sum_{h=1}^n x_h \right) + \sum_{i=1}^2 w^i \left( l^i + \sum_{k=1}^N D_k^i + \sum_{h=1}^n d_h^i \right) = Y, \quad (3)$$

$$Q_k = F_k(X_k, D_k) \text{ for all } k, \quad (4)$$

$$\sum_i q_h^i = f_h(x_h, d_h) \text{ for all } h, \quad (5)$$

where  $w$  are wages,  $\mathbf{p}$  are goods' prices,  $F$  and  $f$  are well-behaved production functions<sup>29</sup>, and  $Y$  is the household's total potential income. The partition of bargaining power between the spouses

<sup>27</sup>For the Chinese case I verify that the results hold when exclusively focusing on sons and when not imposing year of birth restrictions. For both countries the findings are robust to interacting provincial fixed effects with a linear trend in year of birth. All these results are available upon request.

<sup>28</sup>Note that  $\mathbf{X}$  contains each spouse (indexed by 1 and 2)'s vector of market goods used:  $\mathbf{X}^1$  and  $\mathbf{X}^2$ .

<sup>29</sup>One could enrich these production technologies by allowing for the presence of production shifters.

Table 2: Intergenerational Correlation of Relative Contributions

<b>Panel A: Ratio Maternal Income (CHNS)</b>					
	(1)	(2)	(3)	(4)	(5)
Ratio Female Income (child)	0.226*** (0.058)	0.178*** (0.059)	0.144** (0.062)	0.162** (0.064)	0.167** (0.077)
Observations	372	371	371	361	335
R-squared	0.054	0.180	0.208	0.237	0.226
<b>Panel B: Ratio Maternal Market Hours (CHNS)</b>					
	(1)	(2)	(3)	(4)	(5)
Ratio Female Market Hours (child)	0.201*** (0.058)	0.153*** (0.056)	0.127** (0.056)	0.146** (0.059)	0.215*** (0.073)
Observations	342	341	341	332	304
R-squared	0.058	0.230	0.270	0.302	0.313
<b>Panel C: Ratio Maternal Home Hours (CHNS)</b>					
	(1)	(2)	(3)	(4)	(5)
Ratio Female Home Hours (child)	0.164*** (0.059)	0.163*** (0.061)	0.061 (0.068)	0.047 (0.065)	0.088 (0.109)
Observations	366	365	365	354	205
R-squared	0.033	0.138	0.196	0.265	0.294
<b>Panel D: Ratio Maternal Income (HILDA)</b>					
	(1)	(2)	(3)	(4)	(5)
(mean) ratio_parent_income	0.042** (0.019)	0.035* (0.020)	0.037* (0.020)	0.051** (0.021)	0.074*** (0.026)
Observations	1,736	1,576	1,576	1,304	1,186
R-squared	0.005	0.053	0.067	0.091	0.107
<b>Panel E: Ratio Maternal Market Hours (HILDA)</b>					
	(1)	(2)	(3)	(4)	(5)
(mean) ratio_parent_market	0.084*** (0.022)	0.065*** (0.022)	0.063*** (0.022)	0.047** (0.022)	-0.026 (0.027)
Observations	1,375	1,375	1,375	1,225	991
R-squared	0.010	0.099	0.120	0.133	0.132
<b>Panel F: Ratio Maternal Home Hours (HILDA)</b>					
	(1)	(2)	(3)	(4)	(5)
(mean) ratio_parent_chores	0.116*** (0.026)	0.118*** (0.027)	0.121*** (0.027)	0.115*** (0.025)	0.110*** (0.028)
Observations	1,298	1,297	1,297	1,265	1,052
R-squared	0.015	0.086	0.096	0.100	0.110

Notes. For each of the three outcome variables, the table provides OLS estimates from regressions of the proportion of the total spousal dependent variable provided by the female on the same measure for when the individual lived with his/her parents. Standard errors in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

is captured by  $\mu$ , the Pareto weight. In its general form it depends on prices, wages, distribution factors ( $\mathbf{z}$ ), and taste shifters ( $\mathbf{t}$ ). More specifically, spousal outside options are assumed to play a role in choices through taste shifters (characteristics that affect preferences, e.g. number of children or *stigma costs for deviating from identity norms*) and distribution factors (i.e. variables that do not affect preferences nor the budget set but still affect choices by only modifying spouses' bargaining power, e.g. sex ratio in the local marriage market, relative spousal income, or the spousal age gap<sup>30</sup>). Formally:

$$\mu = \mu(\mathbf{p}, \mathbf{w}, Y, \mathbf{z}, \mathbf{t}). \quad (6)$$

Therefore, optimal household allocations ( $\mathbf{x}, \mathbf{X}, d^1, D^1, d^2, D^2, l^1, l^2$ ) will all be functions of prices, wages, potential income, and Pareto weights. I now proceed to discuss what the available empirical evidence can say about how these determinants of the demand functions may persist across generations in a way that they lead to the observed persistence in relative contributions.

**Transmission of Gender Attitudes.** The strong positive associations of intra-household allocations across generations is robust to the inclusion of controls featuring, among others, wealth, health, and family composition. This suggests that the transmission of relative contributions holds beyond the passing on of key characteristics such as social status, education or health, so it would be reasonable to believe that their presence is indeed due to attitudes acquired while growing up based on the observed relative position of each parent, as predicted by models of attitude transmission (e.g. [Bisin and Verdier, 2000](#)). This possibility is captured in  $\mathbf{t}$ .

In order to look at its empirical plausibility, I turn to my secondary dataset, the CFPS, and take advantage of a series of questions aiming at capturing gender norms-related values collected during its 2014 wave. In particular, respondents rank, on a five-point scale, their agreement with the following statements (higher values indicate stronger agreement): i) Men should focus on career, while women should focus on family; ii) Marrying well is more important for women than doing well; iii) Men should do half of the housework.

I interpret these questions as eliciting the strength of traditional gender norms in terms of the role of females both within the family and in the society as a whole. For the present analysis, I construct binary variables taking the value of 1 for a high degree of agreement to the statement (if a respondent grades it with a 4 or a 5) and zero otherwise.

Table 3 explores the child-parent correlations among these three variables and shows strong positive relationships. These questions are only asked in the adult questionnaire, i.e. respondents had to be above 15 years old, which therefore constrains the set of children whose outcomes I can use in the regressions. In columns 1-3 I focus on individuals still living with their parents who were between 15 and 20 years of age at the time of the 2014 round of the CFPS. This choice places

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<sup>30</sup>[Browning et al. \(2014\)](#) provides a detailed discussion.

the focus on the most impressionable years for attitude formation (Heckman and Kautz, 2013; Shigeoka, 2019).

The results show strong positive correlations between both parents' and child agreement to the various statements, the correlations being stronger with respect to the mother. All the regressions control for gender, age, education level, household registration status, and province of residence fixed effects of the respondent as well as for the maternal and paternal level of education. Moreover, in order to account for further child-related attitudinal characteristics of the parents not directly related to gender norms but that could be partially driving the correlations, I also include controls for the maternal and parental degrees of agreement to whether children "should treat their parents well however bad they are treated by parents."

One should note, however, that because these "children" still live in their parents' household, it may be the case that some degree of reverse causality is in place. For this reason, in column 4 I make use of the z-score of the three variables<sup>31</sup> only for individuals belonging to households that have split-off from the original one. Similarly to the main analysis, this tempers reverse causality issues and shows that the findings hold.

In Table 4 I proceed similarly with the HILDA data. Panel A shows that there is a strong positive correlation between the child's gender attitudes and both the father's and, particularly, the mother's views. This holds for the five dimensions considered: (i) "home hours should be split equally if both spouses work"; (ii) "if money is not needed, mothers should not work"; (iii) "children do as well if it is the mother who earns money"; (iv) "it is better if it is the male who earns money"; (v) "it is not good for a relationship if the mother earns more". Panel B (relationship between the child and his/her spouse's views) and C (relationship between the child's mother's and father's views) show that there is assortative matching in attitudes. Panel D accounts for the endogeneity of household formation by instrumenting child's views with his/her father's and mother's views, the outcome being the child's spouse's views.

Overall, undertaking this exercise is informative in that the gender norms that are expected to affect relative intra-household allocations are indeed correlated between parents and their offspring, and also provides novel evidence about the importance of accounting for intergenerational transmissions when evaluating the effectiveness and the welfare effects of a policy (Daruich, 2018).

**Transmission of Characteristics beyond Attitudes.** While the transmission of attitudes is likely to explain at least part of the above results, parents transmit multiple other characteristics that matter for household formation (e.g. health, wealth). If there was little geographical and social mobility, similarities in the relative characteristics of the spouses in the parents' and child's

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<sup>31</sup>I first compute the individual-specific mean agreement among the three dimensions — after reversing the scale of question iii). Then I transform them into a province-specific z-score.

household could be the root of the transmission of relative contributions. At this point, whether the correlations in relative contributions are due to preferences or to persistence in matching are observationally equivalent. In the next section I use the educational reform as an exogenous shifter of education, which I show to have significantly reduced gender norms traditionalism but to not have had an impact on marriage and other relevant outcomes for relative contributions, such as female labor force participation. This allows me to provide further evidence that the intergenerational transmission occurs, at least partly, through socialization.

**Persistence in Local Conditions.** An alternative, final, explanation is that parents and children are exposed to similar local conditions, such as prices and aggregate distribution factors like gender sex ratios in the marriage market. Particularly relevant are labor market conditions (e.g. similar occupational composition, similar female wage gaps). The inclusion of provincial fixed effects and the rural location indicator are a first attempt to account for this possibility ([Attanasio and Kaufmann, 2017](#)). Moreover, the results held when explicitly accounting for potential persistence in local labor markets by controlling for parental and child occupation. This is also the case when controlling for community/sampling unit fixed effects, which are significantly smaller geographical units than province/states (unreported).

## 4 Changing the Degree of Transmission: the Role of the Education Reform

Given the above results, one important consideration is whether the strength of this transmission is persistent or if it can be influenced by policy or other events. This will also speak to whether the transmission is purely genetic or socialization plays a role. In this section I explore if, and how, the implementation of a major policy, the 1986 Chinese Educational Reform, could have influenced the transmission process.

### 4.1 The Reform: Characteristics and Empirical Approach

**Context.** Aiming at equating the Chinese educational standards to those in the West, the 1986 Compulsory Education Law was a turning point in China's nation-wide educational policy. It replaced the until-then prevailing 4-6 years of compulsory education, depending on the province, by a nationally-unified requirement of 9 obligatory years starting from age 6 (as opposed to directly establishing a legally-acceptable minimum age to quit schooling).<sup>32</sup> This requirement effectively amounted to the prohibition of dropping out of the system prior to age 15. Hence, previewing the empirical strategy, I can exploit the fact that individuals above 15 years of age at the time of the

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<sup>32</sup>Another important clause was to forbid child labor.

Table 3: Intergenerational Correlations in Gender Norms: CFPS

	(1)	(2)	(3)	(4)	(5)
	Men should focus on career (child)	Marrying well is key for females (child)	Men should do half housework (child)	z-score	z-score
Men focus career, women housework (father)	0.051* (0.030)				
Men focus career, women housework (mother)	0.076** (0.030)				
Marrying well key for females (father)		0.058** (0.024)			
Marrying well key for females (mother)		0.107*** (0.024)			
Men should do half of housework (father)			0.051* (0.029)		
Men should do half of housework (mother)			0.105*** (0.030)		
Father's z-score				0.099* (0.054)	
Father Treated by Reform					-0.200** (0.086)
Sample Children in Same HH	Yes	Yes	Yes	No	Yes
Sample Children in Split-off HH	No	No	No	Yes	No
Observations	1,377	1,369	1,377	690	1,005
R-squared	0.123	0.077	0.064	0.224	0.150

*Notes.* The child's (aged 15-20) agreement (dichotomous) to the following three statements — i) Men should focus on career, while women should focus on family; ii) Marrying well is more important for women than doing well; iii) Men should do half of the housework — is regressed on his/her mother's and father's dichotomous agreement. Column 4 uses instead z-scores and all children living in split-off households from the original, parental one. Column 5 explores the effects of the education reform on gender norm attitudes of the next generation. It divides the children in two groups according to their age: 16-17 and 18-20, and compute the province-age-block-specific z-score. This is then regressed on indicators of parental treatment by the reform. All regressions control for gender, age, education level (except column 5), household registration status and province of residence fixed effects of the respondent. Moreover, paternal age, parental level of education, and parental degree of agreement to whether child should respect parents irrespective of the treatment received are also controlled for. Robust standard errors in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

passing were not subject to the reform, while younger ones were. If the adolescent had left school but was below 15 at the time of the implementation, she was required to return to school until that age.

Three aspects should be highlighted. First, the implementation of the reform was staggered across provinces. All provinces had to abide by the new Law within a relatively short period of time (there is a gap of five years between the first and last province to implement the regulation), but they were granted some flexibility for the exact timing of implementation (and the strictness of the enforceability), as the central Government recognized that the different provinces were not

Table 4: Intergenerational Correlations in Gender Norms: HILDA

	Home Hours Should Shared Equally if Both Work	If money not needed, mothers shouldn't work	Children do as well if mother earns money	Better if male earns money	Not good for relationship mother earns more
<b>Panel A</b>	(1)	(2)	(3)	(4)	(5)
Father's View	0.110*** (0.019)	0.051* (0.024)	0.135** (0.046)	0.143*** (0.033)	0.100** (0.038)
Mother's View	0.139*** (0.026)	0.111*** (0.021)	0.129*** (0.023)	0.129*** (0.023)	0.061** (0.024)
Observations	1,276	1,275	1,271	1,272	826
R-squared	0.084	0.137	0.108	0.173	0.136
<b>Panel B</b>	(1)	(2)	(3)	(4)	(5)
Own View	0.113*** (0.032)	0.241*** (0.026)	0.233** (0.068)	0.333*** (0.048)	0.259*** (0.032)
Observations	1,307	1,307	1,307	1,308	1,306
R-squared	0.042	0.114	0.085	0.150	0.092
<b>Panel C</b>	(1)	(2)	(3)	(4)	(5)
Mother's View	0.196*** (0.016)	0.387*** (0.021)	0.250*** (0.017)	0.342*** (0.038)	0.245*** (0.049)
Observations	1,427	1,426	1,423	1,424	940
R-squared	0.076	0.223	0.114	0.204	0.147
<b>Panel D</b>	(1)	(2)	(3)	(4)	(5)
Own View	0.006 (0.186)	0.454** (0.176)	0.283* (0.149)	0.734*** (0.130)	0.818** (0.353)
Observations	1,102	1,100	1,097	1,099	715
R-squared	0.029	0.092	0.105	0.018	-0.058

Notes. Panel A regresses child views on maternal and paternal views. Panel B regresses the child's spouse's views on the child's views. Panel C regresses paternal views on maternal ones. Panel D regresses child's spouse's views on child's views instrumented by his/her mother's and father's views. Standard errors clustered at the state level. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

equally prepared to immediately enforce the new law. This is reminiscent of the context in [Cantoni et al. \(2017\)](#). While this leeway adds some non-randomness in the implementation timing, such differences were rooted in predated fundamentals that can be accounted for by my empirical strategy, which chiefly requires for identification that provincial trends in education and the outcomes of interest were evolving in parallel, which is both plausible and verifiable.

Second, the reform would be expected to predominantly bind for females and less-developed provinces, whose education levels lagged behind their respective counterparts. While [Fang et al. \(2012\)](#) already described this heterogeneity using CHNS data (I verify that this holds in my sample



as well) I complement it by showing that this also shows up in the CFPS data, where females increased their education in 0.7 years on average while males did so by 0.32 (Garcia-Brazales, 2020). This is therefore, at least in principle, a women-empowering reform. However, its intra-household consequences are ex-ante unclear as, among other reasons, educational improvements may not translate into labor market gains (returns to education in China were remarkably low in the early stages of the Opening Up period Fleisher and Wang, 2004; Li et al., 2005) and males may attempt to preserve their relative position in the household through, for instance, force or coercion (Bloch and Rao, 2002; Bobonis et al., 2013).

Third, it is pertinent to note that 1986 was a point in time in which no other major contemporaneous policies or events occurred that could be confounding the results presented below.<sup>33</sup> The Cultural Revolution finished two decades before, and the 1995 Labor Reform and the end of the 1990's privatization phase lied far (and unexpectedly) ahead.<sup>34</sup> However, it may be possible that the time variation exploited still captures the effects of other minor policies implemented in the 1980s. Huang (2015) makes a convincing case that this is unlikely to be a big concern.<sup>35</sup>

**Empirical Approach.** The above discussion naturally leads one to think that this exogenous increase in education may have had an effect on the transmission of contributions across generations, be it because it led to a change in gender norms, to a change in bargaining power within the household or to a combination of both. The strategy to explore the effects of the Law is straightforward: I compare individuals belonging to the earliest cohorts that were affected by the reform with the last ones just old enough not to be subject to it.<sup>36,37</sup> If the educational reform mitigated the intergenerational transmission, we should find a significant interaction between the parental cat-

<sup>33</sup>Under a difference-in-differences (DID) setting, a usual concern is that other events happened at the same time as the change of interest. For this to be a concern in my setting, the roll out across provinces of such a potential program would have had to be similar to the one for the educational reform.

<sup>34</sup>One policy studied in the Chinese context is the 1998 reform on *hukou* status bequeaths, which ceased to be matrilineal (Han and Shi, 2019). My empirical strategy to identify the effects of the educational reform, presented below, can account for this class of policy. Moreover, the fall in the attractiveness of female urban *hukou* holders arising from this specific policy runs counter to the female-favoring effects of the Education Law discussed later.

<sup>35</sup>Specifically, he exploits the fact that the education reform requires *nine* years of compulsory education to show that the reform had large effects on final achievement up to that threshold, but not beyond. Had the increase in education been driven by other factors of policies, there should be no reason to expect that the effects should stop at the 9-year cutoff.

<sup>36</sup>For the provinces participating in the CHNS, the actual implementation dates were as follows (Fang et al., 2012): July 1, 1986 in Heilongjiang and Liaoning; September 9, 1986 in Jiangsu; September 12, 1986 in Shandong; October 1, 1986 in Henan; March 1, 1987 in Hubei; January 1, 1988 in Guizhou; and September 1, 1991 in Hunan and Guangxi.

For all provinces, the dates are reported in Table C.8. In order to gain a spatial perspective, Figure D.4 reports the provincial time of implementation, where lighter colors indicate an earlier implementation (plain white indicates no information). Importantly, these differences in “preparedness” are likely to be both historically-rooted and unlikely to lead to systematic differences in the *changes* in outcomes across cohorts. I will provide further evidence for this throughout the text.

<sup>37</sup>The CHNS lacks information on the province of residence during junior high school. Using the current location might introduce measurement error in the treatment variable if individuals migrated after the reform and get misclassified as treated or untreated based on their current location. However, this is unlikely to pose significant problems because inter-provincial migration is very limited (as described in Section B.3 and in Fang et al., 2012) and because the provincial differences in the time of actual implementation are small, which limits the room for potential misclassification. Indeed, I later use the CFPS when exploring the mechanisms, which does have information of the province of residence at age 12, and the results are virtually unchanged.

egory and a post-reform indicator going in the opposite direction to the main transmission effect. Formally, I estimate the following specification<sup>38</sup>:

$$y_{icp}^{child} = \alpha + \beta_0 y_{icp}^{parent} + \beta_1 Treat_{icp} + \beta_2 Treat_{icp} * y_{icp}^{parent} + \mu_1 X_{icp}^{child} + \mu_2 X_{icp}^{parent} + \lambda_p + \gamma_c^{child} + \epsilon_{icp}, \quad (7)$$

where the main difference with respect to the baseline specification is the inclusion of *Treat*, a measure of individual-specific treatment intensity described in more detail below. The crucial time variation determining treatment status is highlighted by the *c* subscript, indicating the cohort (natural year) when the individual was born. The coefficient of interest is  $\beta_2$ , which will inform us about whether the reform had differential effects in the transmission of relative allocations depending on the maternal contribution beyond the common transmission captured in  $\beta_0$ .

The proposed econometric approach accounts for fixed differences across provinces (which mattered for the non-random timing of the implementation of the program) and across cohorts (e.g. a tendency towards modernization and less traditional norms) through the provincial and year of birth fixed effects, respectively. It may still be, however, that there are different trends in the evolution of the outcomes across provinces and/or that there are unobserved changes correlated with the implementation of the law, with educational achievement or with the outcomes of interest across provinces (Stephens Jr and Yang, 2014). In robustness checks I exploit the fact that I have multiple cohorts pre- and post-reform to also allow for these trends to be different across provinces by including province\*cohort trends. Hence, in those specifications the effects of the reform are estimated out of sharp deviations from province-specific trends so that the remaining (unlikely) source of bias are time-varying factors that affect cohorts across provinces not only differently but, importantly, non-smoothly.

Additional tests of the validity of my approach, which I defer to the robustness checks in Section 5.2, include: i) undertaking placebo tests for cohorts that should either be all affected or all unaffected by the regulation. The lack of differential effects after the placebo reform date also helps in building confidence that positive trends in overall educational achievement (which might correlate to other societal changes that make the pre- and post-reform groups no longer comparable) are not likely to be present; ii) considering instead province-specific windows around their implementation time; iii) verifying the comparability of the treatment and control groups through a balance check of observables around the implementation of the reform.

Furthermore, in order to be as restrictive as possible, I focus on the sample of children born between 1967 and 1979. This window is intended to ensure that, within every province, there are some individuals that were treated and others that were not (the first treated cohort in any

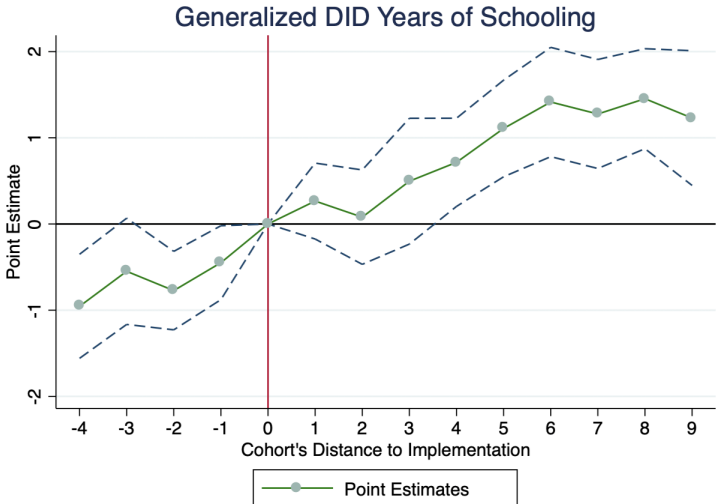
<sup>38</sup>See Roland and Yang (2017) for a similar approach.

province is that from 1971 and the youngest cohort from the last province to implement the law is that from 1976) who all grew up under relatively similar contexts while, at the same time, not excessively decreasing the sample size. The results are insensitive to using wider windows.

**First-stage.** Before discussing the results from estimating Equation 7 I provide evidence for a first-stage effect of the reform on school attainment. Figure 2 plots the estimated coefficients from a generalized DID specification where I regress an individual’s years of education on the distance of an individual’s age at the province-specific date of implementation from the 15 years of age threshold while controlling for a female indicator and provincial and year of birth fixed effects. Any cohort to the left of the red line was too old to be affected by the reform. The omitted category (0) comprises the individuals who turn 15 on the year of the implementation.

One can see that, as expected, educational achievement prior to the reform was significantly lower and without a strong trend. The effects relative to the first treated cohort increase over time as cohorts were subject to the reform for longer time periods. In particular, the differences start being significant for those individuals aged 11-12 (x-axis at 3-4), which is when enrollment into junior high school takes place. Moreover, the effects seem to eventually flatten out for individuals aged 9 and below (x-axis at 6 and beyond), as consistent with these cohorts being close to fully-treated. This satisfies the testable implications, reminiscent of Duflo (2001), arising from the differences in treatment intensity by age-at-reform-implementation and is consistent with the body of literature pointing at the success of the 1986 reform in increasing education levels (e.g. Fang et al., 2012; Huang, 2015; Rawlings et al., 2015).

Figure 2: Educational Trends by Distance to the Reform’s Implementation



**Measure of Treatment Intensity.** In my empirical approach I exploit the fact that different cohorts received different intensities of treatment. In particular, I follow Huang (2015) by taking into account both that individuals aged below but close to fifteen at the time of the implementation

were exposed to the reform with lower intensity (the reform was binding for them for less years) than slightly younger cohorts and that the reform was more influential in provinces where the junior high-school achievement prior to the reform was lower.<sup>39</sup> I operationalize this by interacting the province-specific (time-invariant) proportion of students who did not complete junior high-school in the year prior to the reform obtained — for the nine provinces in CHNS, proportions range from 0.29 in Hubei to 0.48 in Guizhou, see Table C.8 — with a measure of exposure to treatment that assumes linear effects of the policy to extrapolate its intensity among the partially-treated. In particular, I define as fully treated (value 1) those individuals who were up to age 12 at the time of the implementation of the reform — this is a conservative choice; Figure 2 shows that increases in education were still significantly pronounced up to age 9 and Huang (2015) even allows for the possibility that the effects were increasing up to age 6, the time of first enrollment. For those between 12 and 15 I assign linear intensities based on the distance to age 15 (namely 0.75, 0.5, and 0.25 to those aged 13, 14, and 15, respectively.)<sup>40</sup>

## 4.2 The Reform: Effects on Intergenerational Transmission

The results for income and market hours are reported in Table 5 where, to demonstrate robustness to different specifications, even-numbered columns show the results for ordered probits with ten categories of the dependent variable based on its deciles. Compared to the baseline results, the main effects of the parental ratios continue being positive, significant, and with similar point estimates. The interactions with the post-reform indicator for income are not large nor significant. The same is true for market hours. Overall, this suggests that the reform did not bring about significant changes in the transmission of these two outcomes.

Turning to the analysis for home hours in Table 6, the story is different. While the main effects continue being significant and in the expected direction (positive), the interaction terms display the opposite sign. This suggests that, after the reform, the degree of intergenerational transmission

<sup>39</sup>Figure D.5 documents the trends for average years of education achieved separately for low-intensity provinces (those above the median value of junior high-school completion prior to the reform) and high-intensity ones (those above the median, hence expected to have been more affected). The horizontal axis measures the distance of a given cohort to the implementation of the policy at her specific province, which occurs in the period denoted by 5. The vertical axis measures the average amount of education achieved by that cohort. The lines provide quadratic fits for both sides around the cutoff. While high intensity provinces were initially faring worse in terms of average years of education (1.5 less), as expected by construction, their trends were remarkably parallel (and continue being so after the implementation of the regulation.) Right at the regulation we observe a raise in the level of education and an increasing trend onward. One may also notice that the jump at the cutoff seems to be larger for the low intensity regions, as expected, which reduces to some extent the existing gap between the two groups. Apart from these common trends in education, our identification also relies in parallel trends in our outcomes of interest.

<sup>40</sup>The proposed approach provides two differences: the comparison of cohorts born closer and further from the province-specific implementation and between high- (where the exogenous changes in education should be larger due to the Law being more likely to bind) and low-intensity provinces. The results are robust to not exploiting any time variation and relying solely on the fixed-ranking provided by the cross-sectional variation in dropout rates across provinces prior to the reform. Hence, this approach, which has the flavor of the analysis in, for instance, (Card, 1992) and (Goodman-Bacon, 2016), reduces potential biases from differential trends as long as the measurement of dropout rates is not correlated with changes in outcomes across cohorts.

Table 5: Intergenerational Correlations of Relative Contributions after the Educational Reform: Income and Market Hours

	(1)	(2)	(3)	(4)
	Ratio Female Income (child)	10 Category Female Income (child)	Ratio Female Market Hours (child)	10 Category Female Market Hours (child)
Treatment Intensity	0.080 (0.316)	0.115 (1.342)	-0.090 (0.319)	-0.462 (1.551)
Ratio Female Income (parents)	0.169* (0.089)	0.867** (0.397)		
Ratio Female Income (parents)*Treatment Intensity	-0.000 (0.410)	0.096 (1.800)		
Ratio Female Market Hours (parents)			0.113 (0.077)	0.765* (0.397)
Ratio Female Market Hours (parents)*Treatment Intensity			-0.171 (0.452)	-1.550 (2.238)
Observations	302	302	280	280
R-squared	0.178		0.259	

Notes. Columns 1 and 3 display OLS results for a continuous outcome variable while 2 and 4 for ordered probit ones with ten categories of the dependent variable constructed from its deciles. Controls are: individual's gender, indicators for belonging to an ethnic minority and for being located in a rural area, and provincial and year of birth fixed effects. I also control for the age gap between the spouses, household size, spousal education, and the provincial sex ratio at birth in 2000. Robust standard errors in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

of relative contributions decreases. This is true both when using a continuous outcome (columns 1 and 2, the latter does not exclude individuals based on year of birth and clusters standard errors at the 5-year cohort level) or indicators for the female spouse contributing less than the relevant percentage of home hours in columns 3-5 (hence the expected signs of the estimated coefficients should be reversed).

In a nutshell, the reform allowed individuals who grew up in more traditional households to be more likely than otherwise to form a relatively egalitarian households in terms of home production. More specifically, after the reform, individuals brought up at any point in the parental distribution do not show statistical differences in their average female spouse contributions in their own households (no difference after summing  $\beta_0$  and  $\beta_1$ ).<sup>41</sup>

These results provide important evidence suggesting that the transmission of relative contributions can be affected by external forces that do not mechanically/directly affect such contributions. This finding is particularly encouraging because home hours are strongly driven by social norms that have proved to be difficult to change (e.g. [Chi and Li, 2014](#); [Chen and Ge, 2018](#)). A detailed discussion on the relevance and sources of this finding is provided in the next section.

<sup>41</sup>Table C.9 completes the picture by showing that, after the reform, the degree of transmission of relative leisure hours was also disrupted, as consistent with the effects found for home hours and the lack of significant changes in market hours.

Table 6: Intergenerational Correlation of Relative Contributions after the Educational Reform: Home

	(1)	(2)	(3)	(4)	(5)
	Ratio Female Home Hours (child)	Ratio Female Home Hours (child)	< 80 Ratio Female Home Hours (child)	< 60 Ratio Female Home Hours (child)	< 50 Ratio Female Home Hours (child)
Ratio Female Home Hours (parents)	0.185* (0.100)	0.088 (0.056)	-0.163 (0.221)	-0.373** (0.163)	-0.210 (0.138)
Treatment Intensity	0.562 (0.378)	0.413 (0.289)	-0.347 (1.069)	-0.626 (0.635)	-0.511 (0.462)
Ratio Female Home Hours (parents)*Treatment Intensity	-0.727* (0.427)	-0.592* (0.337)	0.182 (1.114)	1.180* (0.707)	0.952* (0.523)
Observations	294	444	294	294	294
R-squared	0.203	0.204	0.140	0.184	0.155

Notes. Controls are: individual's gender, indicators for belonging to an ethnic minority and for being located in a rural area, and provincial and year of birth fixed effects. I also control for the presence of any male child, the age gap between the spouses, household size, spousal education, and the provincial sex ratio at birth in 2000. Column 2 does not impose year-of-birth restrictions and clusters standard errors at the province\*(5-year block) cohort level (35 clusters.) Columns 3-5 display LPM results for dummy variables taking the value 1 if the female spouse contributed less than the different percentages to home hours. Robust standard errors in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

### 4.3 Effects of the Educational Reform: Discussion on Mechanisms

The above findings are important in that they highlight that: (i) policy can affect transmission, which will in turn have long lasting effects through further transmissions across time; (ii) intra-household allocations are not fully driven by genetic characteristics<sup>42</sup>.

However, the reform might have operated through a variety of mechanisms, and better understanding which one is likely to be the most relevant is of key importance for policy and for economic modelling. These are: (1) changes in attitudes towards traditional gender norms; (2) changes in marriage matches (e.g. changes in the relative characteristics of the spouses); (3) changes in labor market conditions/outcomes (e.g. increased female labor force participation); (4) changes in non-attitudinal characteristics or outcomes (e.g. better health), and (5) changes in skills (e.g. differential ability for undertaking home production). In the main body of the paper I discuss channel (1) at length, which is the ex ante most plausible explanation, and I show that the remaining channels are unlikely to be of importance in Online Appendix A.2.

**Changes in Gender Norms.** Increased education may open a new perspective on the role of females, their capabilities, and the extend to which each spouse should be expected to contribute to home hours.<sup>43</sup> This view seems particularly well-fitted given that the effects of the reform were largely concentrated in home hours, the archetypal outcome related to traditional gender norms. As in section 3.2, I refer back to the CFPS and take advantage of the set of questions aiming at

<sup>42</sup>Robalino and Robson (2013) reach the conclusion that both genetic and cultural elements are featured in the transmission process.

<sup>43</sup>A recent example of how schooling (in particular, the curricula taught) can affect political attitudes is Cantoni et al. (2017). While that paper seems to suggest that beliefs (expectations) are relatively easy to change, this may be harder for values (preferences), at least through propaganda. My work finds that such change in values can potentially be achieved through policy (increased compulsory schooling).

capturing gender norms values. These were: i) Men should focus on career, while women should focus on family; ii) Marrying well is more important for women than doing well; iii) Men should do half of the housework.

Recall also that I interpret these questions as eliciting the strength of traditional gender norms in terms of the role of females both within the family and in the society as a whole — I flip the ordering of responses in iii) as it instead captures the degree of agreement with less-traditional views. I combine them by first obtaining their simple average for each individual and then standardizing them within provinces to have a mean of zero and a standard deviation of 1. The results are not sensitive to using different windows of cohorts around the province-specific year of implementation of the regulation.

The nature and the roll-out of the implementation of the policy have implications on its expected effects on our outcomes of interest. In particular, the evolution of our attitudinal measure should follow a similar pattern as the increase in average years of education, which should be monotonic for younger cohorts at the time of implementation and eventually flatten out for all fully-treated cohorts. A generalized DID (introducing dummies for the exact distance to age 15 for each cohort at the date of provincial implementation as in Equation 8 rather than using a unique indicator for treatment) is provided in Figure 3, where the first year of implementation is the omitted category (x-axis at 0). One can appreciate the clear parallelism in the cohort estimates between Figures 2 and 3.

First, attitudes do not appear to have been experiencing an upward nor a downward trend prior to the reform. Second, there is a clear fall already for the first treated cohort (x-axis at zero). Indeed, all pre-treatment periods display positive z-scores (i.e. are above the full timespan's average values) and all post-treatment periods feature negative z-scores (i.e. are below the period's average values.) The fact that the views on gender norms experienced such clear discontinuity aligns well with my proposed explanation<sup>44</sup>. Third, there is evidence that the effects are overall non-decreasing in the intensity of the treatment (the monotonicity of effects will be clear in the formal econometric approach). Given the nature of my outcome of interest (individual attitudes), which one would expect to change smoothly across cohorts, the fact that there is such a clear discontinuity around the cutoff provides a good indication that the educational reform may indeed have had a causal effect on this dimension.

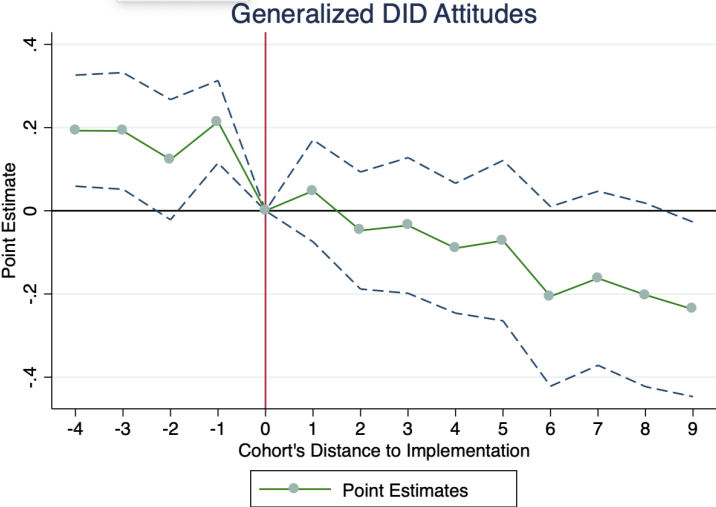
In order to further explore this issue I turn to a regression framework in Table 7. All estimated regressions include provincial and year of birth fixed effects and control for gender, current house-

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<sup>44</sup>One point to highlight is that, while under the new reform there were compulsory courses such as Chinese and mathematics, there were no explicit modifications to the curricula related to views on gender norms. Hence, such a causal effect of increases in education that do not target attitudes is in line with recent evidence from Turkey (Erten and Keskin, 2018)



Figure 3: Generalized DID for Support for Traditional Gender Norms by Distance to Province-specific Date of Education Reform Implementation (95% CI reported)



hold registration, and ethnicity.<sup>45</sup> In columns 1-3 I look at each of the three survey questions separately. There are negative effects of the reform for questions i) and ii), indicating that individuals exposed to the reform (in a dichotomous manner) agree significantly less with the statement that there is a clear divide between men and women in terms of personal careers (market vs. homework).<sup>46</sup> The effect on the extent of agreement with an equal division of home production also increases significantly, which seems closely linked to the shift in relative home production after the reform.

To gauge an overall effect, instead of looking at these highly correlated outcomes separately I use the averaged, and then standardized, score across these three questions in columns 4-8. Column 4 strengthens the evidence from columns 1-3 in that it shows that individuals affected by the reform are above a tenth of a standard deviation less likely to agree with our measure of traditional gender norms constructed by combining the three dimensions previously studied separately. Up to this point, I have assigned treatment intensity based on the province of birth (as I did with the CHNS data in Section 4.2) both for comparability reasons and because the fraction of individuals who lived outside their province of birth by age 12 is very small (5%). In column 5 I exploit a particularly attractive feature of the CFPS by which I have information on the province of residence of each individual at age 12, the crucial moment in a youth’s schooling period when the regulation would be binding, to compute a treatment intensity for each person that would not be subject to measurement error due to migration. As expected, results do not change much and, if anything, get slightly larger and less noisy. Column 6 includes province-specific time trends.

<sup>45</sup>I estimate:  $attitude_{icp} = \alpha + \beta BPR_{icp} + \mu X_{icp} + \lambda_p + \gamma_c + \epsilon_{icp}$ .

<sup>46</sup>These results are in line with modernization theory.

Table 7: Effects of the Reform on Gender Norms

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Marrying well more important for women than doing well	Men should focus on career while women should focus on family	Men should do half of the housework	z-score	z-score	z-score	z-score	z-score
Treated	-0.041 (0.087)	-0.100* (0.054)	0.119** (0.045)	-0.127*** (0.043)	-0.146*** (0.042)	-0.102** (0.047)	-0.155*** (0.055)	
Placebo Treated								0.077 (0.087)
Observations	5,267	5,279	5,280	5,281	5,283	5,281	5,279	5,245
R-squared	0.047	0.080	0.029	0.041	0.040	0.045	0.027	0.044

*Notes.* All regressions include provincial and year of birth fixed effects and controls for gender, current household registration and ethnicity. Columns 1-3 look at each question separately, while 4-8 used the averaged and standardized, one. Column 5 defines treatment exposure based on the province of residence at age 12. Column 6 controls for province-specific linear trends. Column 7 controls for the interaction of pre-reform (1985) provincial GDP per capita and year of birth fixed effects and clusters standard errors at the province\*year of birth level. Column 8 undertakes our placebo exercise of shifting the actual year of implementation to one year before. The sample is restricted to individuals born between 1962 and 1980. Standard errors clustered at the province level in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

In order to further account for the non-random time implementation of the reform in column 7 I also include the interaction of pre-reform (1985) provincial GDP per capita (recall the stage of development of the province was the most important determinant of the timing) and year of birth fixed effects and cluster standard errors at the province\*year of birth level. As an additional check of the validity of the identifying assumption of lack of pre-trends, column 8 performs a placebo test in which the actual provincial date of implementation is shifted one year ahead. The fact that the new “treatment” indicator is no longer significant reinforces, once again, the idea that an underlying trend was not present.<sup>47</sup>

As a final test, I collapse my attitudinal score to its mean for each bin indicating the distance in years from the 15 years threshold at the province-specific implementation and look for *unknown* structural breaks in the time series for 30 cohorts around the enforcement date (Quandt, 1960; Gershoni and Low, 2017). Such breaks are found to be exactly at 0, the first-affected cohort and also at 6, as consistent with Figures 2 and 3.

With these results at hand, it is pertinent to emphasize that one strength of my proposed empirical approach in Equation 7 is that it allows me to compute the reduced-form effects of the reform, which are fully informative when *distinguishing between the socialization vs. genetic channels* without relying on an exclusion restriction assumption. One would like to know, however, if education per se is the causal driver of this change in attitudes. In Section A.5 I provide evidence that this seems to indeed be the case.

<sup>47</sup>It is important to highlight how two set of results already provided fit together. The first one is that the findings in Table 7 show that the reform indeed shifted the values of the individuals affected by it. The second is that Table C.10 suggests that the *gaps* in values between spouses did not change. To fully rationalize these two sets of results, in unreported regressions similar to those performed for Table C.10 I show that females affected by the reform are indeed married to husbands favoring less traditional gender roles (i.e. we do observe the expected change in levels, even if the gaps did not change).

**Additional Evidence on the Validity of Gender Norms as Main Mechanism.** In Online Appendix A.3 I provide further evidence in favor of gender norms as being the root of the decline in the persistence of relative contributions to home production. In particular, I show that: (1) the educational reform did not induce selective migration that could bias my estimates; (2) households with a female spouse that was treated by the reform display lower *levels* of female relative contributions to home production (i.e. not only the transmission was disruptive), and (3) the findings are indeed likely driven by norms since relative contributions to home production are portable. In particular, cross-provincial migrants originally from more traditional provinces are more likely to display more skewed contributions to home production even in their new location.

## 5 Robustness of Baseline Results and Extensions

In this section I perform a battery of checks to verify the robustness of my main results. I first provide two types of placebo tests. In the first one I randomly allocate parents to children within their provinces and show that my baseline results would occur with extremely low frequency were relative contributions purely random. The second one restricts my estimating sample to either only children who were all too old to be affected by the educational reform, or were all young enough to be treated. The idea is that, if my interaction term between parental ratios and post-reform was capturing some trend or differential characteristics generated around the time of the policy, we should find an effect in at least one of these two subsamples, but I will show that it is not the case.

### 5.1 Robustness to Placebo Tests

**Randomization Based Inference** I turn now to the two placebo tests. In the first falsification test, I use the original parental and child information and I reshuffle the children across households within their same province without replacement. This randomization based inference<sup>48</sup> has the additional benefit of allowing the distribution of individual characteristics to differ across provinces. Figure 4 shows how our estimated coefficient of interest (called “beta”) is centered around zero, while the p-values obtained for it are fairly uniformly distributed between 0 and 1 (unreported). While this uniformity is not surprising (it should be true any time a randomization takes place), the fact that the betas are distributed around zero is a particularity of my context and points towards the lack of an statistically significant effect in this placebo setting.

To be more specific, the way to interpret the graph for the betas is the following: given the point estimates that I found in my baseline specification (columns 1 in Table 2), if the world was purely random, the effect found (or a higher one) would occur P percent of the time, where P reflects the

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<sup>48</sup>See, for instance, [Athey and Imbens \(2017\)](#).

proportion of replications that yielded a higher point estimate than my original one. I find that the cases to the right of the vertical line are extremely scarce. Therefore, this exercise reinforces the idea that the correlations shown between parental and children ratios are indeed capturing within-household characteristics and are not driven by other common contextual aspects at the provincial level.

**Placebo for Post-reform Outcomes.** Next, I proceed with the placebo exercise for the analysis of the moderating effects of the educational reform. The idea is to consider subsamples in which all individuals should share the same actual treatment (or lack thereof) to see if the disruptive effects of the reform might simply be a mechanic result due to changing trends in the sample over time. In Table C.27 I focus on the individuals born between 1962 (the earliest cohort considered in my baseline sample) and 1971 (the last unaffected cohort in the first province to implement the reform). I arbitrarily define as “treated” individuals born on or after 1966, and non-treated the ones born before. The interactions between the quintiles and the post reform indicators do not show any statistical significance, which suggests that there is an absence of a differential trend over time that may be confounding the results. This is true for market hours in column 1 (which I show for completeness, since we did not find an effect of the reform on them in the first place) and for home hours and leisure in columns 2 and 3. An alternative placebo exercise is to focus on the subsample where all individuals are truly treated, and assign them a year of treatment. I do not report the results as the sample size is small given the few individuals in my sample born after 1980, but the results again do not point towards our main results potentially being confounded.<sup>49</sup>

## 5.2 Balance Checks Around the Date of Implementation of the Educational Reform

In this section I further explore the validity of a key assumption in the pseudo-regression discontinuity framework: treatment and control groups should be comparable prior to the reform. To test this I regress individual and parental characteristics that are pre-determined with respect to the reform on the same treatment indicator used in the main analysis. Table C.28 confirms that there are no significant differences between the two groups in key variables such as ethnicity, parental education, presence of sons among the first two children born to the parents (which could affect intra-household allocations), age gap between the parents (a traditional distribution factor), female relative contribution to home hours nor in parental attitudes (at least those captured by the degree of agreement towards the question that income is very important in life).

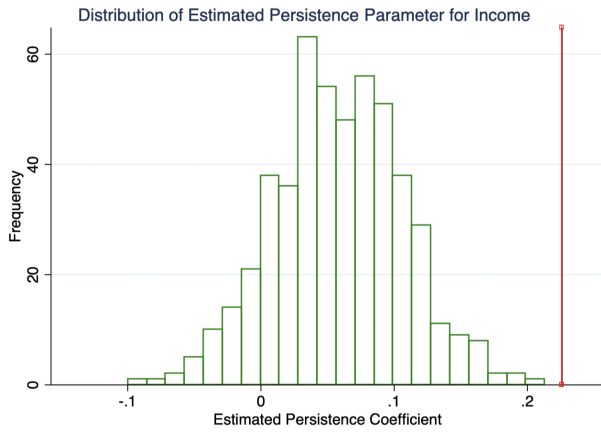
Additionally, Figure D.6 provides evidence in favor of the parallel trends assumption for the z-

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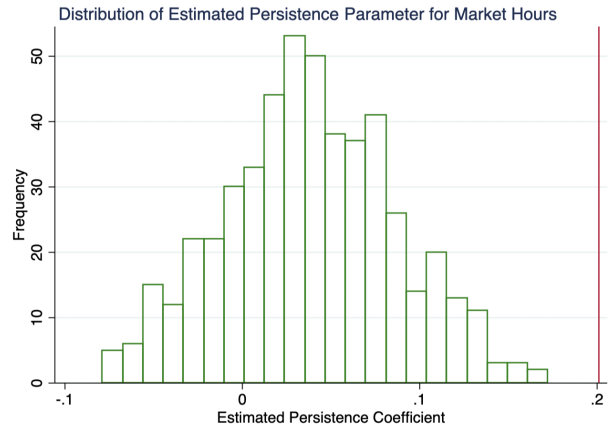
<sup>49</sup>To increase sample size I consider all individuals treated with respect to the actual date of implementation in their province (rather than using a common year in which all provinces in my sample are already treated). Still, this leaves me with 113 observations. Also note that, when restricting the sample to only-treated individuals, the “intensity” of the treatment varies, as some should only be affected for one academic years while others are affected throughout their whole academic trajectory.

Figure 4: Placebo Within-province Reshuffling of Individuals

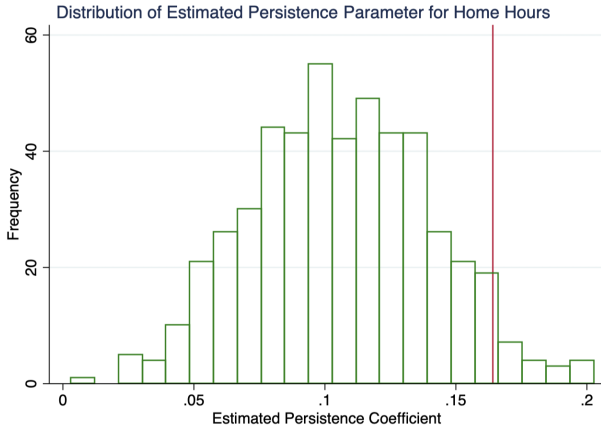
(a) Income, CHNS



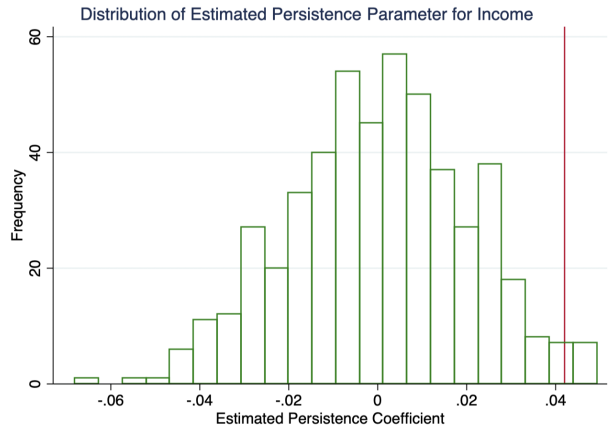
(b) Work Hours, CHNS



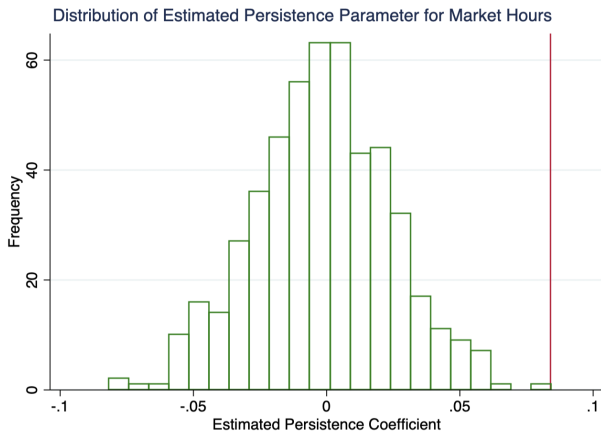
(c) Home Hours, CHNS



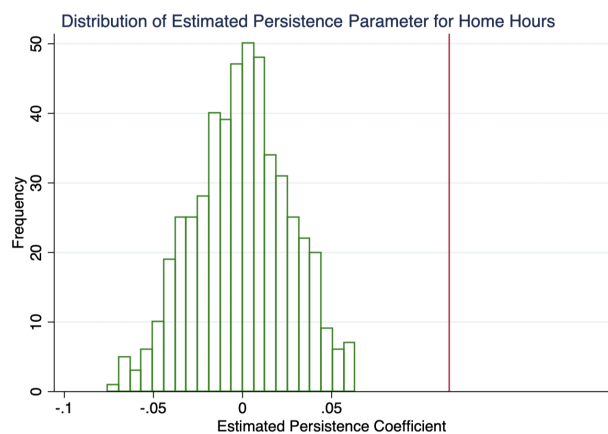
(d) Income, HILDA



(e) Work Hours, HILDA



(f) Home Hours, HILDA



Notes: Distribution of the Intergenerational Correlation Parameter of Within-Household Relative Contributions from the Placebo Tests of Random Parent-Child Allocations (500 Replications).

score measure of agreement towards traditional gender norms between high- and low-intensity regions. I deepen this result by exploring whether trends in gender norms differed across provinces

with different degrees of junior high school dropout rates (which was correlated with the timing of the implementation of the reform and led to different degrees of binding potential for the Law) were present. I consider the interaction of pre-reform dropout rates with both linear cohort trends and cohort fixed effects for cohorts that were not treated at all in any province (born between 1963 and 1970). The results in Table C.29 show no sign of such trends. This result for gender norms complements the common trends found for education in Figure D.5 and in other attitudes such as trust or views towards inequality that I uncover in complementary work (Garcia-Brazales, 2020). The lack of systematic differences in levels and trends for all these dimensions suggests that differences in unobservables are unlikely to be present either, which supports the validity of the “too-old” cohort acting as a counterfactual for the treated group in our empirical strategy.

## 6 Conclusion

Exploiting a unique feature of the CHNS and HILDA datasets by which I observe the relative maternal contribution to income and to home and market production an individual is exposed to while growing up and those that he/she displays in adulthood, I provide the first evidence showing that these relative contributions are strongly positively correlated across generations. This result holds after the inclusion of a rich set of controls and provincial fixed effects and after a battery of robustness checks. Removing the variation arising from the most plausible alternative explanations, such as the transmission of education, wealth, occupation, or health offers support to the hypothesis that preferences towards replicating childhood relative position of the spouses may be driving the results. This is true above and beyond the transmission of extensive margin decisions, and therefore extends the notion of persistence in female spouse’s labor force participation as emphasized in the influential work by Fernández et al. (2004). My estimates are unlikely to suffer from one of the most pressing concerns in the intergenerational transmission of attitudes as is reverse causality given the time gap in the collection of parental and child relative contributions provided by the tracking of households over three decades. This distinctive feature renders the interpretation of the correlations as causal plausible.

Building upon these correlations, I then turn to the Chinese Compulsory Education Law of 1986 and use it as a natural experiment to show that, after the reform, the degree of intergenerational transmission for individuals who were just young-enough to be affected by it was disrupted. In particular, the reform decreased the correlation for individuals whose mother was contributing more to home hours — those individuals whose mothers were more responsible of home production were no longer more likely to replicate this pattern in their own households if affected by the reform. While this comes together with a clear shift among treated cohorts towards less

traditional gender norms (as stated in explicitly-related self-reported survey questions), no significant changes took place in the type of matches arising from the marriage market (which might explain outcomes through spousal bargaining power or through changes in spousal specialization of tasks) nor on female relative labor market outcomes (hence suggesting the absence of an income/bargaining power channel). These findings emphasize the key role of human and social capital in determining intergenerational mobility<sup>50</sup>, and complement our understanding of the role of educational reforms beyond their impacts on years of education and market returns by showing that policy can affect intra-household allocations, likely through attitudinal changes, and that this can, in turn, have a multiplier effect through intergenerational transmission.

These results also highlight that persistence in relative contributions is not purely driven by the transmission of genetic factors that determine individual and household allocations. Additional findings on i) the portability of relative contributions among migrants; ii) positive assortative matching (consistent with theories of attitude transmission); together with iii) placebo analyses consisting of randomly allocating children to households within the same province that show that these synthetic households no longer display significant intergenerational correlations, reinforce the argumentation in favor of the important role of socialization, both at the household level and through peers.

My work therefore contributes to our understanding on the presence and, importantly, the mechanisms of transmission of gender norms. I highlight the potential to affect norms through policy, even in a rapid manner, especially across heterogeneous subgroups of the population. My finding that the transmission of preferences about relative spousal contributions goes beyond the extensive margin points at a novel force for the perpetuation of Pareto weights across generations, for their divergence within couples over time (potentially by affecting both outside options and the distribution of income within the household), as well as for the wide range of outcomes affected by these relative powers (e.g. within-household inequality in resource allocation, child development, etc.) Apart from the more tangible policy implications arising from this result — which suggest a central role for increases in education as well as for targeted attempts to shift gender attitudes towards more egalitarian ones in order to maximize the returns of increased female wages in terms of within household bargaining and more equal final allocations, from a theoretical perspective it also points at the importance of incorporating relative transmissions across generations as well as neighbor effects when modelling individual preferences and intra-household decisions, particularly in non-unitary models, whose validity has significantly been strengthened by recent studies.

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<sup>50</sup>Chiappori (2019) has recently emphasized the key role of human capital, particularly combined with increasing positive assortative matching (which jointly create what he terms “inequality spiral”) for explaining current and future low intergenerational mobility in the US.



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## A Extended Analyses (For Online Publication)

### A.1 Extending BKP's Analysis: The Chinese Case

One of the pillars of this paper is the work by BKP who find a sharp discontinuity in the density of female relative contribution for income right after the 0.5 divide.<sup>51</sup> Figure D.1 shows that this fact is also present in my sample. In order to formally explore the presence of this discontinuity, I perform a McCrary test that rejects the null hypothesis of the absence of a discontinuity at the 1% level.<sup>52</sup>

Taking intra-household allocations as the final outcome of interest is relevant in its own right because, among other reasons, it reflects inequalities among household members. In this section, I follow BKP to show that relative allocations between spouses may actually arise as a voluntary decision with foundations in gender norms. My results suggest that females who would be more likely to outperform their male counterparts in terms of income generation disproportionately choose not to enter the labor market in the first place and, if they nevertheless do so, they do not exploit their full potential. Hence, this constitutes a fundamental argument for motivating the interest in looking at relative contributions across generations. These findings are also valuable in that the results from BKP for the US seem to also apply to a country with a very different culture such as China.

More specifically, the authors explore whether the pattern discussed above is due to women who have the potential to earn more than their husbands restraining either their labor market participation or their earnings so as to conform with gender norms<sup>53</sup>. In order to address this possibility, they focus on females married to a working man and construct a measure of the probability that the wife would earn more than the husband if she entered the market (for her given sociodemographic cell based on age, education, ethnicity and province).<sup>54</sup> After computing certain percentiles of the empirical distribution of female income, the probability that the wife would earn more than the husband is approximated based on where the husband's observed income lies

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<sup>51</sup>This 50% threshold is based on the male breadwinner perspective that males should provide more income than females. Other work exploring this idea are [Sayer et al. \(2011\)](#) and [Ishizuka \(2018\)](#).

<sup>52</sup>Since one may think that the large fall could be more of a reporting bias/deficiency in the survey framing, I conduct the test for two alternative definitions: self-reported hourly wage (the one in the reported picture) and estimated hourly wages out of the information for hours spent last year working for pay and total wage income (not reported). The results are very similar. A graphical representation of the discontinuity in the density at the 0.5 divide is provided in Figure D.2 in the Appendix.

<sup>53</sup>Note that, although when females have lower wage rates than their husbands they are more likely to prevail (do more than their husbands) at household chores (75%) than when they earn higher wages than their husbands (70%), most of the higher-earning wives still prevail in chores time: among the cases where the female earns higher wages than their husbands, only 25% of them do less chores than their husbands.

<sup>54</sup>I restrain my sample to urban individuals between 24 and 54 years old and I do not require observing two generations of individuals, which considerably increases the sample size. I construct three blocks for age (10 years each) and three blocks for education. This is because finer measures would run into very few observation per gender-age-education-minority-province-wave cell. Within these cells, I keep those groups for which I observe at least 20 individuals in the wave. The percentiles that I use are either 15-30-45-60-75-90 or 10-20-30-40-50-60-70-80-90. Finer detail is, again, inadvisable due to sample size.

in the estimated income distribution for females (I find the average probability of a wife earning more than the husband to be around 30%, slightly above BKP's 20%).<sup>55</sup>

Then, individual females' labor force participation in the previous year — measured in two different ways: i) having supplied positive wage hours in the previous year (hence including both part-time and full-time workers); ii) having worked over 1800 hours (working below 35 hours per week is considered to be part-time work as it is done in Meng (2012) times 52 weeks) in the previous year in wage activities — is regressed on the measure of the probability that the wife would earn more than the husband. Additional controls are the usual sociodemographics plus provincial and round fixed effects as well as (polynomials of) the natural log of the husband's income. Additionally, I look at whether the income gap, measured as the ratio of (wife's income — wife's potential income) / wife's potential income, where wife's potential income is the mean of the empirical distribution of female income for the relevant sociodemographics, is also affected by the wife's earnings potential.<sup>56</sup>

Table C.6 reports a strongly significant negative sign, suggesting that women tend to purposefully restrain their earnings so as to reduce their lead over their husbands' earnings. They do so both by not participating at all or participating less in the market (columns 1 and 2) and by settling for outcomes further from their highest potential conditional on participation (column 3). In terms of magnitude, the fall in the probability of participation in the labor force of about 10% participation is similar to the one found in BKP whereas, for those women who do participate, the gap relative to their potential earnings is close to 0.3, bigger than the 0.2 in BKP. This is consistent with the fact that the probability that the female earns more than the husband is higher in my sample than in BKP's.<sup>57</sup>

## A.2 Alternative Mechanisms Behind the Reform's Effects

### A.2.1 Changes in Marriage Matches.

First, the reform's effects both on individual education and on its distribution might have changed the type of matching occurring in the marriage market (Zha, 2019). Unreported, I first check in

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<sup>55</sup>Formally: for selected percentiles  $p$  (there are  $P$  of them), we compute  $\omega^p$  as the  $p^{th}$  percentile of the observed income distribution constructed from the working females in her specific sociodemographic group, and we compute the probability that the woman earns more than the husband as:  $\frac{1}{P} \sum_p 1\{\omega^p > income\ of\ husband\}$ , where 1 is an indicator function.

<sup>56</sup>To deal with the concern that there may be unobserved variables that explain why females who marry males whose income is lower than their potential one may be more likely to stay out of the labor force, BKP show the consistency of their results to the inclusion of additional controls, including a measure of relative income between the spouses at marriage.

<sup>57</sup>As a complement to the BKP analysis, in Figure D.3 in the Appendix I report the relative female contributions to income, market and home hours (of households headed by a 25-50 years-old individual) over a two-decade period, together with the evolution of the labor force participation by gender. While relative income and market hours follow the patterns in labor force participation (particularly the larger decline in female participation following the privatization measures undertaken at the end of the 1990s), the relative contribution in home hours seems independent of labor market conditions, as consistent with the presence strong gender norms in this realm. The Chinese context, where female labor force participation has decreased over time, is in contrast to the British one examined in Lise and Seitz (2011), which led to a significant catching up in relative female contribution to the household's total income.

the CHNS whether, on average across all individuals or heterogeneously based on relative maternal home production (where we found effects from the reform), treated individuals experience changes in their age of marriage, in the age gap with the spouse, and/or in their values towards the importance of income. I do not find evidence for any of this. I do not find differences in the spousal gap in *predicted* wages<sup>58</sup> after the reform either, which I interpret as a meaningful indicator of spousal similarity and is also a potentially important distribution factor (Lise and Yamada, 2019).<sup>59</sup> This suggests that the marriages that took place right after the reform were not significantly different from those happening prior to it.

To gain further confidence that the marriage matching process was not significantly affected, I turn again to the CFPS, which also collects extensive sociodemographic data but is richer in terms of attitudinal information about both spouses. Although I cannot use this dataset to explore intergenerational correlations in relative contributions, I can still rely on the educational reform to explore whether the matches that occur for just-affected females are different from those for just-unaffected ones. For this I define a “spousal gap” as the difference between the husband’s and the wife’s values for any given variable. Then, I look into whether individuals affected by the reform (for expositional clarity I focus on the women’s side, but the results hold when looking at males, as mentioned below) are systematically matching differently than comparable individuals not affected by it through the estimation of Equation 8:

$$gap_{icp} = \alpha + \sum_{l=-5}^{-1} \beta_l \delta_{icpl} + \sum_{l=1}^5 \beta_l \delta_{icpl} + \mu X_{icp} + \lambda_p + \gamma_c + \epsilon_{icp}, \quad (8)$$

where the outcome of interest are the above-mentioned gaps in spousal characteristics.  $\delta_{icpl}$  are indicators for the person being  $l$  years away from age 15 at the time of the implementation of the reform at her province. The omitted category is the cohort that was only affected for a year. The remaining notation is as before. I cluster the standard errors by province and year of birth.

Table C.10 in the Appendix reports the results from the modified version where, for simplicity, I only distinguish between affected and unaffected individuals instead of the full set of distance dummies.<sup>60</sup> It shows that the reform did not significantly affect the gaps in spouses’ characteristics along a very wide range of pre-determined and, importantly, attitudinal characteristics and total

<sup>58</sup>To be specific, I predict wages based on the 1991 (the first CHNS round with wage information) as a function of a polynomial in age, gender, rural/urban indicator, education, ethnicity, marital status, household size and provincial fixed effects. I then compute the gap in predicted wages between the spouses and regress it on a treatment indicator, provincial and year of birth fixed effects. The point estimate is 0.001 with p-value 0.785.

<sup>59</sup>Additionally, I also estimate the wage residual for working individuals. The gap in this variable does not change either after the reform, which provides evidence that the *unobservable* factors that predict wages for the spouses does not change either (results available upon request).

<sup>60</sup>More specifically, in the reported results I use an indicator for whether the *female* spouse was affected by the reform. Given that the male spouse is older in the vast majority of marriages, if the male spouse is treated so will be the female spouse. Because the converse is not true, I verify (available upon request) that the qualitative results hold when using an indicator for “any spouse” being affected while controlling for both spouses’ year of birth fixed effects.

fertility.<sup>61</sup> For example, there were no changes in the age gap between the spouses or in the age of first marriage (Panel A's columns 1 and 2,) which are dimensions frequently available in standard datasets. My analysis is particularly rich in that I also explore multiple attitudinal dimensions, such as potential differences in the perception of certain outcomes to be achieved in life, in the determinants and scope of control of multiple dimensions, in views on inequality, etc. Moreover, column 7 in Panel C shows that total fertility, an interesting outcome of education in its own right, was not affected by the reform (nor was the age at first childbearing nor the *desired* number of children, which are available upon request). This is not only reassuring given the intergenerational nature of our analysis of interest (Goodman-Bacon, 2016) but also discards a potential change in a notable taste shifter: the presence of children.<sup>62</sup>

Overall, there is strong evidence of a lack of disruption in the matches taking place.<sup>63</sup> In particular, these findings do not support the idea that the lower transmission of female relative contributions to home hours is because the relationship between spouses' characteristics and their relative position in newly formed couples are different from those for individuals not subject to the reform. They also provide a first pass that specialization theories based on spouses' complementarity of skills may not account for the full transmission of relative contributions to home production. I return to this point below.<sup>64</sup>

Additionally, one may be interested in exploring not only the role of a change in an individual's education on matching patterns but also the corresponding change in the *distribution* of education within the society. In this section I build upon recent work by Zha (2019) and aim at estimating the following equation:

$$y_{icp} = \alpha + \beta_0 JHS_{icp} + \beta_1 PJHS_{cp} + \mu X_{icp} + \lambda_p + \gamma_c + \epsilon_{icp}, \quad (9)$$

where  $JHS_{icp}$  is an indicator for whether individual  $i$ , born in cohort  $c$ , at province  $p$  completed junior high school (the 9 years of education promulgated by the reform) and  $PJHS$  is the cohort-province attainment rate.

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<sup>61</sup>In unreported regressions I also check if the spousal *gap* in occupations — whether measured by Ganzeboom et al. (1992)'s International Socio-Economic Index (ISEI) or the Standard International Occupational Prestige Scale, a more subjective occupational ranking (Ganzeboom and Treiman, 1996) — differ after the reform. This is not the case, nor for the *level* of husband's occupation.

<sup>62</sup>This also reduces concerns about whether the *composition* of home production after the reform might have changed, which might have affected the split of production (e.g. if males were more prone to spend time with children and fertility had increased due to the reform there might be a shift towards more male home production). In order to provide further support that this compositional story is not present I undertake the same exercise as in Table 6 but where I exclude time spent in childcare. The qualitative results remain (available upon request).

<sup>63</sup>I additionally check if divorce became more prevalent after the reform, which could be a sign of changes in outside options. Given the extremely low divorce rates even at the time of the 2010 CFPS wave, this possibility is ex-ante likely not very relevant quantitatively. Results available upon request show no statistical change after the Law. Moreover, there were no changes in the probability of never marrying (or, alternatively, to ever marrying).

<sup>64</sup>Given the multiple testing nature of this exercise, one could consider performing p-value adjustments such as Bonferroni (1936). This would simply decrease the number of cases of false positives, which would further reinforce the lack of effects on marriage outcomes.

Because of likely endogeneity concerns, one may aggregate outcomes at the province-cohort level and instrument the added JHS and PJHS with the reform (in particular, with the interaction of an indicator of being less than 15 at the time of the reform and the provincial junior high school graduation rate prior to the reform.) Results for the effects on spousal age gap and age at first marriage are reported in Table C.11. While the effective F-statistic from Montiel-Pflueger weak instrument tests, which are relevant given my clustering of standard errors, for the proposed instrument satisfies the standard rule of thumb proposed by Andrews and Stock (2018) above which estimation by two-stage least squares is recommended, we see once more that the overall academic achievement at the province-cohort level did not affect marriage market outcomes

One can then separate the individual and the province-cohort average levels of education (which are jointly estimated as a sum of the separate coefficients in the above instrumented regression), by simply noticing that the individual effects can simply be estimated as the expected difference in outcomes conditional on the level of education (having achieved at least junior school or not). The mean difference in female spouse's age at first marriage is 2.4 (24.4 for junior school-educated and 22 for not) while the average age gap between the spouses is smaller for the more educated (-1.78 vs. -2.11).

These numbers imply that one can isolate the effects of increasing the female province-cohort-specific average education by 10% (while controlling for the female's education) on the two outcomes of interest as follows: i) for age at first marriage:  $(-3.694-2.4)/10 \approx -0.61$ ; ii) for gap spousal age:  $(0.331-(-0.33))/10 \approx 0.066$ , neither of them being statistically significant.

### A.2.2 Effects through the Labor Market

Second, the reform might have led to better employment opportunities for women, which would increase their incentives to work as well as their bargaining power. This does not seem to be supported by the data: females are not significantly more likely to work nor to earn more income (in levels or relative to their partner) after the reform.

Indeed, one reason why I rely on the 1986 Reform is because, given that the literature has emphasized the importance of disentangling the effects of increased education from those of increased income (Oreopoulos and Salvanes, 2011; Huebener, 2017), finding a context where the income channel is weakly operational is particularly attractive for isolating attitudinal changes. This is the case for the 1986 Law: Cui et al. (2019), using the CFPS, find positive but inconsistently significant higher employment rates and family income for females affected by the educational reform. These weak effects on employment and earnings are in line with the fall in the intergenerational transmission of relative income that I uncover in Table 5 for Q3, but that was not large in size and not statistically significant.



I complement the existing literature by considering whether, at the time of the 2014 CFPS wave, treated females were more likely to have *ever* participated in the labor force. Table C.12 shows that this is not the case (column 1) nor that they are more likely to be employed at the time of the survey (column 2). Also, for completeness (i.e. not limiting myself to the exploration of the effects of the reform), in Section A.4 in the Appendix I look into the extent to which broader labor market conditions, in particular the gender wage gap, play a role in relative income contributions.

A final important piece of evidence to take into account comes from Fang et al. (2012), who employ the reform as an instrument for years of education and show, using the CHNS, that returns to schooling seem to be substantially higher for males than for females, at least among the subpopulation of compliers. I confirm this by exploring the returns to education in terms of occupational and prestige classifications of workers, as measured by the International Socio Economic Index (ISEI) and the Standard International Occupational Prestige Scale (SIOPS), respectively.<sup>65</sup> Under a standard limited commitment intertemporal framework, only large enough changes in outside options will result in updates in the Pareto weights. One would therefore not expect the female-favoring change in relative distributions to have been driven by better female labor market potential. What is more, improved outside options would play a role only as long as they can be credibly used when threatening divorce<sup>66</sup>. However, with a current divorce rate at 0.32% (and smaller in the past), this does not seem to be the case.

### A.2.3 Changes in Non-attitudinal Characteristics

Third, it may be possible that certain individual outcomes are affected by the increase in educational levels (and potentially so in an asymmetric manner across genders). For example, it may have led to improved health conditions which can, in turn, affect the ability/productivity of work. A now relatively large set of papers (e.g. Rawlings et al., 2015; Huang, 2015; Ma, 2017) have followed Fang et al. (2012) in making use of the 1986 educational reform to instrument for years of education and have carefully explored the credibility of the exclusion restriction (i.e. that the reform did not affect their outcomes of interest through other means than education). Apart from their results being robust to different placebo tests and to accounting for the presence of other contemporaneous policies, these papers have shown that key aspects such as health, nutrition, and the quality of education were not significantly affected.

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<sup>65</sup>I explore this issue, conditional on labor force participation, in companion work (Garcia-Brazales, 2020). The heterogeneity of the effects by gender are available upon request. For details on the occupational measures, see Ganzeboom et al. (1992) and Ganzeboom and Treiman (1996).

<sup>66</sup>Alternatively, one could think of the spouses outside option as the one resulting from non-cooperation while remaining married.

#### A.2.4 Changes in Skills

Fourth, as previously discussed, the persistence in relative contributions could be due to the transmission of certain characteristics that matter for the way spousal tasks are split rather than through the role of preferences for specific relative contributions. For instance, male children who grow up in more traditional households might develop particularly low skills for home production which may put him at a comparative disadvantage with respect to his future partner. While the reform might have disproportionately changed the exposure to home hours for females (who increased schooling more than males) and hence made them relatively less productive at home production, this explanation is unlikely to be the one behind the disruption in the transmission of home hours based on the exploration of certain testable implications that each of these two explanations entail.

First, if the human capital mechanism was behind the results, we would expect to have observed changes in the relative productivity of the spouses after the reform. However, as mentioned, there was no change in the predicted wage gap between spouses, which suggests that females did not narrow down the male's comparative advantage for market production after the reform.<sup>67</sup> Indeed, the disruption in relative home hours is not accompanied by a parallel change in market hours, which would be intimately related if relative productivities was the sole driver of decisions. Second, there is strong evidence in favor of the transmission of preferences. I have already shown that attitudes and preferences towards gender norms are causally transmitted across generations in Table 3 and that they matter for female labor force participation (Table C.6). Moreover, under the preferences channel, children of mothers who work more hours should display less traditional gender norms (Olivetti et al., 2020). In Table C.14 I use CFPS data and regress maternal weekly working hours in 2010 on their children's agreement in 2014 to whether men should do half of the housework (column 1) and whether men should focus on their professional career while females on family (column 2). The results indicate that, indeed, children brought up by women working more hours show significantly less traditional gender views. The next point provides further evidence in favor of the key role of preference transmission.

### A.3 Additional Evidence on the Validity of Gender Norms as Main Mechanism

The previous section shows the presence of a clear shift in attitudes towards gender norms driven by the reform that was not accompanied by changes in dimensions related to the alternative channels contemplated. In particular, the fact that labor market outcomes and the quality of education were not significantly affected reduces potential concerns about general equilibrium effects.

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<sup>67</sup>While it may still be the case that more educated (thanks to the reform) females decrease their home productivity given that they may have less exposure to chores due to schooling, the lack of changes in other indicators of relative productivity in the labor market such as the spousal education gap (available upon request) did not change. This suggests that matching in the marriage market based on one's productivity is, if anything, only one of multiple drivers for the persistence in relative contributions.



The above findings therefore complement recent research highlighting that (non-gender-norm-related) attitudes in China are susceptible to change due to major events experienced during adolescence (e.g. [Chen and Yang, 2015](#); [Gong et al., 2015](#)). In this section I provide further evidence to reinforce the claim that the disruption in the transmission of relative home hours is indeed rooted in the experienced changes in the views towards gender norms.

**Non-selective Migration.** Existing correlational analyses suggest that, in China, low-educated rural individuals are more likely to migrate ([Du et al., 2005](#)). While this likely reinforces my main results ([Table 2](#)) given that less educated individuals are more prone to replicate traditional gender norms, it could also bias my sample for the educational reform exercise if this policy had rendered affected individuals more likely to remain in their original location (together with changing their attitudes towards gender norms.)

In [Table C.15](#) in the Appendix I show, using data from the CFPS, that individuals affected by the reform are not more likely than those unaffected to reside in 2010 in a different province (column 1) nor in a different county (column 2) to the one in which they lived at age 12. This suggests that the sample used in my post-reform analysis is unlikely to be a selected one, as I further discuss in the robustness checks of [Section 5.2](#).<sup>68</sup>

I now provide two additional analyses to reinforce the importance of the role of gender norms transmission and of the role of the Law in disrupting relative contributions.

**Effects of the Reform on Levels of Relative Contributions.** In [Table C.17](#) I make use of *all* households in the CHNS headed by an individual born between 1967 and 1979 and explore whether there was a reduced-form effect of the reform on the *levels* of relative contributions (rather than on the transmission as I have done so far.) As expected, while the relative contributions to income and market hours are positive but small in economic size and not statistically significant, households headed by an individual who was subject to the reform feature a female spouse contribution to home hours that is 6 percentage points lower than those headed by comparable individuals who were not affected by the reform. This provides further support for the income/bargaining power channel being relatively unimportant and for the change in attitudes uncovered in [Table 7](#) to have translated into actual behavior within the households. It also reinforces the idea that the results on the disruption of intergenerational transmissions obtained from the subsample of individuals observed in both roles indeed reflects the changes in the overall population.

I provide further evidence for the robustness of this important result. In column 4 I additionally allow for province-specific trends across cohorts by interacting provincial fixed effects and cohort linear trends, and I cluster the standard errors at the province\*year-of-birth level. The re-

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<sup>68</sup>As a complementary analysis (also to that in [Table C.10](#)) I additionally explore whether the probability of marrying someone from a different province changed for treated individuals. This was not the case (results available upon request).

sults are virtually unchanged. In columns 5-7 I emphasize the absence of pre-trends in several ways. I first delay and advance the province-specific date of implementation two years in columns 5 and 6, respectively, and show that the treatment indicator is no longer significant. Then, in column 7 I provide the estimates from a generalized DID where, instead of using an indicator for post-treatment, I report the cohort-specific effects based on distance to age 15 at the time of the implementation. The omitted category is the last cohort unaffected by the reform. One can see that, while there are no differences across unaffected cohorts, already the first affected cohort shows signs of lower female relative contributions to home hours, as consistent with Figure 3.<sup>69</sup>

**Portability of Gender Norms Among Cross-provincial Migrants.** Second, if relative contributions are truly embedded in gender norms, one would expect that couples who migrate across provinces may continue displaying these preferences. I exploit the subsample of cross-provincial migrants within the CHNS (those whose household head was not born in the current province of residence) to check whether those migrants coming from more traditional provinces (as measured by the provincial gender sex ratio at birth in the year 2000<sup>70</sup>) are also more likely to feature higher female relative contributions to home hours even within their destination province (i.e. featuring province of residence fixed effects) and controlling for the type of employment (Table C.18). The province-of-origin sex ratio *for migrants* is expected to only capture the role of cultural preferences in determining our outcomes of interest, even if it could also be the product of province-of-origin characteristics or institutions (Luttmer and Singhal, 2011).<sup>71</sup>

The portability of culture is evident both when directly comparing the levels of relative contributions among all same-destination migrants as standard in the economics of culture literature (columns 1-6) and when looking at the likelihood that the migrant household's female relative contribution is higher than the province-and-wave-specific mean contribution among *non-migrant* households — who face similar economic conditions but were brought up under a different intensity of traditionalism — in columns 7 and 8.<sup>72</sup> Column 9 expands the sample to also include

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<sup>69</sup>A further check that I perform is to predict relative home hours based on exogenous characteristics (education, ethnicity, age, rural/urban location and provincial fixed effects) and then check that there is no correlation between the predicted relative home production and the treatment indicator. This suggests that affected cohorts were not systematically different from the unaffected ones.

<sup>70</sup>I build upon the standard approach in the literature of using sex ratios at birth as a measure of gender-inequitable beliefs (e.g. Xue, 2016; Hwang et al., 2019). This is particularly attractive in the Chinese context since we exploit within-country regional variation in sex-ratios in an otherwise fairly culturally homogeneous nation. By not relying on cross-country variation, as usually done in the epidemiological literature, concerns about omitted variables such as linguistic or institutional differences partially accounting for the results are reduced.

<sup>71</sup>The emphasis is on whether household relative contributions have a cultural root. For this reason I take province-of-origin average relative contributions as exogenous and I do not aim at exploring its determinants.

<sup>72</sup>Details on the exact specification are on the Table's notes. Results hold when controlling for household head's occupation, which could potentially differ between migrants and non-migrants (and between immigrants of different types), household size, years since residing in the current location (to account for a potential slow understanding of the new economic context in the migration destination rather than true cultural preferences), and relative female income, and for key aspects about the province of origin (i.e. graduation rate from junior high school and GDP per capita at 1985, a short period before the start of data collection for the first round of the CHNS) which could have been driving the effects captured by sex ratios. Such a rich inclusion of controls significantly reduce concerns of selective migration based on economic reasons. The other type of potential selection issue of migrant destination is a preference-

non-migrant households to show that, within a province, individuals who grew up in more traditional provinces are prone to feature significantly higher than the local household's mean female relative contribution to home hours. These exercises, which are similar in spirit to [Atkin \(2016\)](#), further reinforce the finding that gender norms play a substantial role in relative female contributions to home hours beyond local conditions and current economic environments (e.g. labor market), and that socialization through peers is also likely to contribute to the determination of final relative allocations.

#### **A.4 Effects of Labor Market Conditions on Relative Spousal Contributions to Income**

The main analysis has shown that the educational reform did not have large effects on the relative female contribution to income. This was part of my strategy to isolate the effects of gender norms about home production, since they were affected by the reform while previous research had shown small effects on income.

In this section I aim at further exploring the extent to which income contributions are hard to change. In particular, I exploit exogenous longitudinal variation in the labor market conditions experienced by females over two decades (1989-2009) based on [Meng \(2012\)](#)'s estimates of the gender gap in urban wages<sup>73</sup> to investigate whether relative female contribution to income is affected by average the contemporaneous gender wage gap.

Column 1 in [Table C.13](#) reports a positive correlation between a more favoring female gender wage gap and relative female income contribution. In columns 2-6 I show that, while these female-favoring conditions allow women to be more likely to contribute up to 49%, as expected, the effects are no longer significant above the 50% threshold.

Splitting the sample between provinces with less traditional gender norms, proxied by below-median sex ratios at birth, [Panel B](#) shows that they are the drivers of these results (very strong and significant effects on the probability of reaching 40% and 45% and on the overall relative levels), while these effects are much weaker, if present at all, for households in more traditional provinces. This reinforces the idea that, while within provinces relative contributions tend to persist across generations, cross-provincial lower traditionalism also paves the ground towards the malleability of accepted/preferred relative contributions.

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based explanation. Biases arising from potential selection in who migrates in the first place are expected to go against finding an effect, as these individuals are likely to feel less attached to the prevailing views in their province of origin. Moreover, if individuals chose to migrate to provinces with similar degrees of traditionalism, this would bias downwards the estimated effects.

Additionally, I check the robustness of the results by replicating the analysis using the 2010 CFPS survey, which collects information of both spouses' hours of home production and province of residence at age 12. The qualitative results are very similar to the ones from CHNS and are available upon request.

<sup>73</sup>She finds the gap to be increasing over the period of interest, with the female dummy in a regression with log annual wages as dependent variable going from -0.09 in 1989 to -0.22 in 2009.

## A.5 Causal Effects of Education on Gender Norms

In order to explore the causal effects of education on gender norms I continue using the CFPS and instrument years of education with my measure of treatment intensity (the effective F-statistic from Montiel-Pflueger weak instrument tests is 25). The validity of the exclusion restriction assumption has been abundantly discussed in existing work estimating the returns to education from quasi-natural variation from the 1986 reform (e.g. [Huang, 2015](#); [Garcia-Brazales, 2020](#)).

The results in Table [C.16](#) provide evidence that, indeed, education causally changes attitudes about gender roles (towards a more egalitarian view). This is an important finding as higher average schooling achievement is on the way in many developing countries and is a key aspect in a country's modernization process whose effects spillover across generations.

## A.6 Additional Robustness Checks to Baseline Analysis

### A.6.1 Inference: Robustness to Alternative Clusterings of Standard Errors

To test the sensitivity of my inference to various treatments of the standard errors I replicate in Table [C.21](#) the results reported in the even columns of Table [2](#) both clustering at the province level (9 clusters) and applying wild-bootstrapping in columns 1-3 and at the province\*cohort level (35 clusters, columns 4-6). In the latter case I define a cohort as a five-year window, but the results hold with smaller windows, such as three-year ones.

### A.6.2 Robustness to Alternative Measures of Relative Contributions

Connected with the discussion on the use of median relative contributions in Table [C.20](#), one issue with the estimated persistence parameters is that they may be biased, likely downwards, due to measurement error. [Clark \(2015\)](#) argued that, when multiple measures of related dimensions are available, they can be combined and alleviate this potential bias. In my context I compute both the average relative contribution of parents and children across income, market and home hours (to make the direction of the measure of home hours consistent with those for income and market hours in that higher values indicate a more female-favoring situation, I use  $(1 - \text{female relative contribution to home hours})$ ) and I conduct principal component analysis to predict the first component of these three dimensions<sup>74</sup>. I replicate the analysis from Table [2](#) with the new measures in Table [C.22](#). While the point estimates increase slightly and remain very strongly significant, they do not support the idea that the baseline estimates were severely biased downwards. [Vosters and Nybom \(2017\)](#) and [Vosters \(2018\)](#) reach similar conclusions.

Additionally, in order to complement the measures in levels and the quantiles used throughout

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<sup>74</sup>Note that the estimates under PCA are independent of using relative home hours or its complement (1-relative contribution).

the main analysis, one may want to be broader in scope and ask whether individuals who grew up in households where the mother did not reach a significant threshold of relative contributions are more likely to also feature similar divisions in their own household. This not only helps dealing with potential measurement error (as it relies less of the exact level of relative contributions) but also provides a different angle to the exploration of their transmission.

In particular, I set this relevant threshold to 35% for income and market hours and 80% for home hours. Columns 1 and 2 in Table C.23 show that individuals risen up in households with a large female participation in income and in the labor market are significantly less likely to form households where the female spouse does not reach 20% of relative contribution (columns 1 and 2), and that their levels of relative contributions are higher (columns 3 and 4, where I use instead an indicator for maternal contributions not reaching 35%). These results are robust to removing from the estimation those individuals whose mother contributed below 5% to income or to market hours. This, once again, suggests that the results are not driven by an intergenerational transmission of extensive labor decisions. Similarly, columns 5 and 6 show that individuals with mothers who contributed over 80% to home hours are significantly more likely to have own households where the wife contributes more that 70 and 80%, respectively.

### A.6.3 Robustness to Model Specification

While the inclusion of the extensive set of controls outlined above was theory-driven, their large number and the possibility that their functional relationship with my outcomes of interest is non-linear leaves the door open to potential improvements in model specification as well as for further robustness checks of the baseline results in terms of selection on observables. In order to deal with these aspects I undertake both a standard lasso approach for Equation 1 and Belloni et al. (2014)'s double machine learning selection algorithm (which implements two lassos: one for the main independent variable, i.e. relative maternal contribution, and one for the outcome variable, i.e. relative female contribution in the child's household) to perform my variable selection, which will inform the set of controls to be included when re-estimating Equation 1. My starting point for the lasso is to consider all the controls included at some point throughout the test, their squared terms, and their interaction with a year of birth time trend.<sup>75</sup>

Table C.24 shows, for the sample without age restrictions, that the baseline results are robust to including the controls selected from the two lasso approaches enumerated above. In particular, odd-numbered columns include those variables selected from the double selection algorithm while even-numbered columns use the controls selected from using a standard lasso only to the outcome variable of interest.

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<sup>75</sup>Results are similar when allowing for pairwise interactions between all the control variables.

#### A.6.4 Robustness to Modifications of the Estimating Sample

In this section I show that the main results hold under further restrictions of my estimating sample (Table C.25). First, even though information at the individual level is provided individually by the person herself and with little or no influence exerted by the spouse, it may still be the case that social desirability leads the person to voluntarily distort her response. Let me first mention that this is unlikely to be driving the results since each parent should individually provide his/her (potentially distorted) answer in a way that the proportion between the spouses will positively correlate with the (potentially distorted) responses of their child some years later. Still, if this were the case, we would expect that, following Bertrand et al. (2015), females would aim at making their relative contribution slightly below that of the male. Hence, I rerun my basic analysis excluding individuals who either themselves or their parents have relative values between 0.48 and 0.52 (using wider windows does not affect the results either). Note in passing that this also partly deals with the potential concern that the correlations found are driven by co-working couples in their own businesses, who may then report split earnings at 0.5.

Second, as mentioned, an important finding of my work is that the persistence in relative distributions are not purely driven by extensive margin decisions. To strengthen this claim, columns 3 and 4 are estimated excluding those individuals brought up in households where the mother contributed below 5% or above 95% of the total income or market hours. The results show that, indeed, the strong correlations persist even after conditioning on the mother being a provider of income and market hours.

Third, although I have argued that reverse causality is very unlikely, it may still be possible that children are able to influence the relative contributions of their parents while they are still living in the household if they do not move out until an advanced age. This would be the case, for example, if the child, despite not being the household head, earns an important share of the household's total income or has old parents with precarious health, which might give him/her a saying in family allocations. To deal with this I proceed in two manners: i) I focus on individuals who form their own household (i.e. do not take over their parents' household); ii) I restrict the sample to children whose latest age observed as a son/daughter in the household is less than 30 (the results hold for more stringent age requirements, such as moving out of home before 23, but the sample size is significantly reduced to around 100 observations). Once again, the baseline results are remarkably robust.<sup>76</sup>

Fourth, in order to further homogenize the estimating sample I undertake the following (unreported) checks: i) I add indicators for the last CHNS wave in which the individual is observed

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<sup>76</sup>An additional check in the spirit of homogenizing the age range is to restrict the observations to individuals observed up to age 50 as a parent and 26 as a child (the average marriage rate for males). Results hold.

as a child and the last one in which he/she is observed as a household head/spouse; ii) I restrict my sample to individuals who were last observed as a child prior to the 2009 round. These two modifications should reduce potential concerns about the fast changes in the Chinese economic conditions.<sup>77</sup> No changes in our point estimates nor in their significance arise.

Fifth, I further control for selection on observables by re-estimating my main effects using propensity score matching. In essence, I compare individuals that only differ in the degree of maternal contribution to income, market, and home hours (I proceed separately for each of them) without imposing strong functional form assumptions, as it is the case in ordinary least squares (Imbens, 2015). I operationalize this approach by distinguishing individuals whose mother contributed above 35% for income and market hours, and 95% for home hours. The results in Table C.26 in the Appendix show that, after successfully generating a balanced matched sample (columns 2-4), the main results of the paper continue to hold (column 1).

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<sup>77</sup>Results are generally robust to restricting the sample to earlier waves than the 2009. This also helps in assuaging concerns about migration outside the original province, as this phenomenon did not become sizable until the end of the 1990s (Xie and Hu, 2014). To further strengthen the robustness of the results to selection-into-the-sample concerns arising from selective migration I also experiment restricting my sample to individuals first observed in their role of parent at or before the 2004 round of the CHNS. This is to strike a balance between choosing a period where migration is relatively less common and not substantially decreasing sample size. The main results hold (available upon request.)



## B Additional Aspects (For Online Publication)

### B.1 Details on the CHNS

The CHNS has been a very relevant data source for multidisciplinary analysis along China's Opening-Up period. Although the sample was not conceived to be nationally representative, significant efforts were devoted to cover a wide range of economic, sociodemographic, cultural and climatic conditions within China (Popkin et al., 2010). Originally, households from eight different provinces were surveyed. The sample was extended in 2011 to include three megacities (Shanghai, Beijing and Chongqing).<sup>78</sup> The selection process followed a multistage cluster randomization by which cities and counties were first stratified by income, then four counties and two cities within each province were randomly selected, and finally twenty households were drawn among randomly-chosen communities and neighborhoods.<sup>79</sup> On average, about 4,000 households are interviewed per round amounting to roughly 16,000 individuals.<sup>80</sup> Around 70% of the sample is located in a rural area. One strength of the data is that, despite the long time span of the project and the large changes experienced in China during this transitional period, attrition has been kept relatively low.<sup>81</sup>

### B.2 Treatment of Relationships with Household Head

There are some instances in which there has clearly been a coding problem in the relationship-with-head variable — this is also noted by Santaaulalia-Llopis and Zheng (2018). I manually correct the following cases based on information for multiple rounds, which allows me to better gauge the true household composition: i) households without a change in the household head but whose relationship with some of the cohabiting members does change in an unjustified way (e.g. a person goes from being the sibling to being the spouse of the household head); ii) household without a current household head are assigned one; iii) if a household is observed multiple times and the head changes in one round just to go back to the previous head in the next I assume that there was a reporting error in the unusual round. These modifications are marginal (less than 1% of the total sample). In any case, I replicate my analysis without applying my corrections and the results are virtually unchanged.

### B.3 Data Caveats

Being able to exploit such unique information comes at the cost of working with a selected sample. In this section I enumerate a series of caveats that should be kept in mind when interpreting the

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<sup>78</sup>In 1997 a ninth province, Heilongjiang, was also enrolled.

<sup>79</sup>To be more specific, within counties, a subset of villages and townships were then randomly selected, and then the 20 households would be chosen. A similar process was followed for cities, but the subunits there are termed "urban" and "suburban" neighborhoods.

<sup>80</sup>To counter the effects of attrition, replacements of households so as to keep a consistent cross-sectional sample size have taken place since 1997 (Zhang et al., 2014).

<sup>81</sup>By 2006, around 70% of individuals who had participated in the 1989 round were still available (Popkin et al., 2010).



main results of this paper. However, I argue that these caveats are not likely to limit the external validity of my findings.

First, in order to restrict the survey's additional operational costs, only individuals who form their household within the same city/county are tracked. Although this may seem to be a restrictive requirement, the particularities of China make it less so. Specifically, mobility is severely restricted through the household registration system, even between urban and rural areas within the same province. In my sample, less than 10% of individuals were not born in the same province where they currently reside, but even smaller figures have been found in other studies.<sup>82</sup> Exploiting rich information on the history of individual mobility available in the 2010 round of the China Family Panel Study I find that, among the individuals born prior to 1980, 94% of them still lived in their community of birth at age 12, and 75% at the time of the interview (i.e. respondents were at least 30 years of age)<sup>83</sup>. In Section B.5 I show that this large geographical persistence, which is beneficial for my study, is largely captured by the tracking practices of CHNS: 85% of children remain within the tracking area. Given such high figures of locational persistence, one would expect that the sample that I use is not composed of individuals who are particularly inclined to stay around their original households and, by extension, may also be more likely to preserve their intra-household allocations. The one margin through which selection is still possible is that those split-off children who choose to remain being part of the CHNS are more prone to replicate parental behavior. I formally show that this is not the case when conducting very detailed balance checks. Finally, one should further note that, despite its significant rise since the early 2000s, migration in China is overwhelmingly temporary, as transitioning from rural to urban *hukou* status is very rare regardless of the duration of the staying at an urban location (Wang, 2004).

Second, and connected with the above point, we only observe the parental intra-household allocations for one of the spouses. This is a shortcoming since one would ideally observe the home conditions for both spouses because, apart from offering a more detailed picture, for example in terms of assortative mating, it would also allow us to provide an assessment of the relative importance of the background conditions of the wife and the husband. In any case, I will still explore this issue superficially when I look into potential heterogeneity by gender. At this point I simply emphasize that around 90% of the children that I observe are males. Although I do not have enough information to clearly identify the origin of this difference, females do not significantly differ from males in their permanent migration rates so a plausible explanation turns to the patrilocal nature of the Chinese culture, which leads females to join their husband's family. Because that family was not originally participating in the CHNS one would expect the probability of that

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<sup>82</sup>For instance, 3% for the 1990-2000 period (Fan, 2005).

<sup>83</sup>This number is higher for males (81%) than for females (72%). Another relevant figure is that, at the time of the interview, 92% of the respondents lived in the same province as they were born in (males were slightly above one percentage point more likely to have remained in their province of birth).

family to voluntarily join the CHNS to be low. Hence, the main discussion should be regarded as mostly reflective of the intergenerational transmission of relative contributions to sons (which in Section B.4 I show not to be a selected group among the pool of sons). This focus is in line with the existing literature, which has stressed the key role of the husband's parents.<sup>84</sup>

Third, as usual with intergenerational studies, observing parents and children at different points in their lifecycle may affect the transmission parameters obtained (Jenkins, 1987). My analysis is not immune to this, but I take several measures to minimize concerns. First, I exploit the three-decade tracking of households to limit the use of information to cases where the household head is over 21 and less than 52 years of age (I experiment with other ranges and results do not change). On average, the oldest age that I observe the parents' generation is 40, whereas that for the child's generation is 30. The fact that I observe both generations around midlife (results hold with a more compressed age range) is a valuable and atypical feature of my dataset since it allows us to better deal with the evolution (likely widening) of gaps along the lifecycle (Stuhler et al., 2018). Second, in one robustness check I focus on individuals who move out of their households prior to age 30 (other age limits are explored, as explained when relevant). Since individuals are tracked in the next wave after moving out, this increases the homogeneity in the stage of the lifecycle that they are observed. Third, in the analysis of the effects of the reform I employ a pseudo-regression discontinuity design for which I exclusively look at individuals born in a twelve-year window. Fourth, I control for household composition (as, for instance, the presence of children may contribute to the withdrawal of females from the labor force).

#### **B.4 Comparison with the Original Sample**

I turn now to the comparison of the characteristics of the sons/daughters that we use in our estimating sample (i.e. the ones that we also observe in their role of household head/spouse) and those of the sons/daughters that I never get to observe in such role. If there are no significant differences between the original and the restricted samples this will increase our confidence that our results are obtained from a subsample that inherits the representativity of the original one.

I proceed as follows. First, I restrict my sample to individuals that I observe as son/daughter in a household and are aged between 21 and 40. This age restriction is imposed so that we are able to compare the final outcomes (e.g. highest education level achieved) of individuals that we use in our intergenerational analysis and those that we do not. The results are robust to including younger individuals when reducing the lower bound to 18. Then, I regress individual- and family-level characteristics on an indicator of whether the child satisfied the conditions to be included in

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<sup>84</sup>One may think that the transmission of gender norms is stronger for males, as the context faced by women tends to change more rapidly, hence potentially making them less prone to reproduce their mother's reality. The fact that males are likely to abide more by these norms should also temper concerns about the unobservability of the female spouse's childhood conditions, and particularly so in the case of China, "one of the most patriarchal family systems that ever existed" (Greenhalgh, 1985).

the intergenerational analysis, while only controlling for basic exogenous characteristics: provincial, year of birth, and round fixed effects, and a rural indicator<sup>85</sup>. If this dummy is significant, it will point towards differences in means between the two groups. Because the estimating sample is predominantly composed of males, I also include a gender indicator.<sup>86</sup>

Table C.3 clearly shows the lack of statistical differences between the two groups along a large set of different dimensions. It includes both parental characteristics (measured by father and mother's working status and log income per capita<sup>87</sup>), and individual ones (full or part time worker, amount of hours worked in the market last week (wage hours or any other type of income generating activity), health status, ethnic minority status, education).<sup>88</sup> Importantly, there are no differences either in the ratios of income, home and market hours provided by the mothers, nor in terms of whether they were affected by the educational reform that I exploit in Section 4 (column 18.) This lack of statistical significance is accompanied by a small economic size of the point estimates and tight standard errors. The same results (i.e. the presence of only a statistical difference for household size) holds when splitting the sample into males and females and repeating the analysis (available upon request). This highlights that the lack of selection into my sample holds for both genders even if the proportion of retained females is low.

## B.5 Documenting Location Patterns of Children

In order to provide a more detailed perspective on the geographical location of children relative to their parents, I exploit a unique feature of the CFPS by which adults are asked about the location of the current residence of *all* their children (together with certain characteristics such as their age, gender, education level, etc.) In particular, children may: i) co-reside with the parents; ii) live next door; iii) live in the same street; iv) live in the same county/district; v) live in a different city within the same province; vi) live in a different province; vii) live overseas.

In Table C.4 I focus on parents aged 55 to 75 and restrict the attention to their children aged 25-50. Column 1 shows that 23% of the children continue living with their parents. Importantly 85% of all children remain within the same prefecture-level city, which corresponds with the geographical unit to which the CHNS would track split-off households. This figure further reinforces the representativity of my estimating sample. Columns 2 and 3 describe the behavior for females and males, respectively. As expected, male children are more likely to remain in close proximity to

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<sup>85</sup>As mentioned, accounting for the Chinese urban-rural divide is important. Moreover, the children that play both roles are slightly more rural (78%) than the original sample (70%).

<sup>86</sup>Given this gender-of-the-child asymmetry, I also explore whether, even if there are no statistical differences in the average characteristics across observed and unobserved "children", this could be hiding heterogeneity across genders. For this, I interact the "Intergenerational Sample" dummy with a female indicator. Such interaction does not turn out to be significant across the different variables. This suggests that, although one may initially want to be cautious and interpret the main results of the paper as reflective of intergenerational transmission to male children, it would be reasonable to consider them to be representative as well of their female counterparts.

<sup>87</sup>I trim the income measures by removing the observations below the first and above the 99<sup>th</sup> percentiles of the (separate) rural and urban samples.

<sup>88</sup>The provincial composition is also remarkably similar, with a slight over-representation of households from Guangxi.

their parents (up to within the same county or district) but are almost equally likely to remain in the same prefecture-level city (83% vs. 86%) and equally like to live in the same province (91%).

I complement the above unconditional descriptive figures by exploring in Table C.5 whether certain key observables are correlated with the geographical location of a child's household relative to his/her parent's. To be specific, I regress an indicator for the child living in categories v-vii above on key variables: marital status, and indicator for having achieved at least a high-school degree, holding an managerial position or having been affected by the educational reform, while controlling for family and year of birth fixed effects. As consistent with previous results, only gender significantly predicts child location: males are 3.4% less likely to live far away from their parents.

## C Additional Tables (For Online Publication)

Table C.1: Transition Matrices of Educational Categories

<b>Child's Education (columns)</b>	(1) Below Primary	(2) Primary	(3) Junior	(4) Senior	(5) College	(6) Count
Below Primary (father)	22.35	26.99	35.60	10.60	4.47	1,208
Primary (father)	6.06	24.52	45.81	16.65	6.97	1,550
Junior (father)	2.34	13.35	45.53	25.75	13.03	2,225
Senior (father)	1.03	9.37	35.05	30.93	23.62	1,067
College (father)	0	3.28	29.10	31.97	35.66	244

*Notes.* This table provides the transition matrices from parent's education level (rows) to children's (columns) for individuals born after 1970. Probabilities are expressed in percentages.

Table C.2: Number of Observations per Person and Role

	(1) Mean	(2) Standard Deviation	(3) Min	(4) Max	(5) Total
Relative Income (as a child)	3.603	1.623	1	8	1,445
Relative Income (as a parent)	2.850	1.721	1	9	1,143
Relative Home Hours (as a child)	2.595	1.365	1	7	960
Relative Home Hours (as a parent)	2.678	1.573	1	8	991
Relative Market Hours (as a child)	3.359	1.531	1	7	1,327
Relative Market Hours (as a parent)	2.534	1.460	1	8	1,001

*Notes.* This table provides descriptive statistics on the number of times that each individual in my main sample is observed for each role (i.e. as a child is his/her parents' home and as a household head/spouse in his/her own formed household). Everybody in the sample is observed at least once in each role (as show in in column 3.) For any given outcome (e.g. relative income) the total number of observations used to correlate relative contributions as a child and as a household head (as when estimating Equation 1) is computed as the sum of the cells in column 5 (i.e. for income:  $1,445+1,143=2,588$ ).

Table C.3: Comparison of Individuals in the Estimating Sample and the Discarded Ones (CHNS)

	(1) HH Head Currently Working	(2) Head's Spouse Currently Working	(3) Age Difference Spouses	(4) Household Size	(5) Log HH Income per Capita
Intergenerational Sample	0.022 (0.023)	0.009 (0.026)	-0.212 (0.223)	0.292*** (0.091)	-0.034 (0.056)
Observations	5,845	5,031	5,058	6,065	5,930
R-squared	0.257	0.242	0.025	0.178	0.387
	(6) Full-time Worker Any Job	(7) Part-time Worker Any Job	(8) Total Hrs. Worked Any Job	(9) Total Hrs. Worked Wage Job	
Intergenerational Sample	0.005 (0.024)	0.264 (0.185)	-0.002 (0.018)	-0.010 (0.018)	
Observations	5,005	5,005	5,528	4,937	
R-squared	0.117	0.026	0.118	0.105	
	(10) Poor Health	(11) Log Own Income	(12) Ethnic Minority	(13) Upper Middle School or Below	
Intergenerational Sample	0.005 (0.024)	0.264 (0.185)	-0.002 (0.018)	-0.010 (0.018)	
Observations	4,057	4,850	5,879	6,054	
R-squared	0.073	0.312	0.295	0.268	
	(14) 3-Day Average Protein (g)	(15) Ratio Female Spouse Income	(16) Ratio Female Spouse Market Hours	(17) Ratio Female Spouse Home Hours	(18) Affected by Educational Reform
Intergenerational Sample	-0.678 (1.388)	-0.002 (0.020)	0.009 (0.021)	0.001 (0.019)	0.004 (0.014)
Observations	3,444	4,495	3,264	4,272	6,103
R-squared	0.126	0.045	0.084	0.086	0.877

Notes. OLS regressions of the outcome variable on an indicator of belonging to my estimating sample (i.e. being observed in both roles), a female indicator, provincial, round and year of birth fixed effects and an indicator of being located in a rural area. The sample for these regressions is limited to individuals born after 1962 and aged 22-40 when they are observed as son/daughter in their parents' household. Robust standard errors in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Table C.4: Geographic Location of Children Relative to Parental Domicile (China)

	(1) All	(2) Females	(3) Males
At parents' home	22.80	8.27	36.12
Next door	4.32 (27.11)	1.24 (9.51)	7.14 (43.25)
Same street	17.54 (44.65)	13.84 (23.35)	20.93 (64.19)
Different street in same county/district	28.93 (73.58)	45.10 (68.45)	14.10 (78.28)
Different county/district in same prefecture-level city	11.16 (84.74)	14.83 (83.28)	7.79 (86.08)
Different city in the same province	6.63 (91.37)	7.94 (91.22)	5.42 (91.50)
Different province	8.19 (99.56)	8.10 (99.32)	8.27 (99.78)
Overseas	0.44 (100)	0.68 (100)	0.22 (100)
Count	11,980	5,730	6,249

*Notes.* The table collects the percentage of children aged 25-50 living at each location with respect to their 55-75 year old parents. Column 1 uses all individuals in the CFPS that satisfy these age restrictions. Columns 2 and 3 condition on child gender. Cumulative percentages in parenthesis.

Table C.5: Exploring Differences in Observables by Distance of Location to Parental Domicile (China)

	(1)	(2) Child Lives in Different City	(3)	(4)	(5)
Married	-0.008 (0.014)				
High-school and above		0.033 (0.028)			
Male			-0.034** (0.014)		
Administrative position				0.013 (0.046)	
Treated					0.015 (0.024)
Observations	8,614	8,602	8,741	7,316	8,237
R-squared	0.577	0.578	0.573	0.620	0.595

*Notes.* The table explores, using data from the 2010 wave of the CFPS, whether certain observables, including alternatively as indicated by the rows, can predict whether an individual will live in a different city (either in the same province, in a different one, or overseas). Year of birth and household fixed effects are included. Standard errors clustered at the province level in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

Table C.6: Gender Norms' Effects on Female Labor Market-related Decisions

	(1) Female Over 1800 Wage Hours Last Year	(2) Female Part- or Full Time Worker	(3) Female Income Gap
Prob. Female Earns More Than Husband	-0.110** (0.0515)	-0.0878* (0.0504)	-0.294** (0.120)
Round FE	Yes	Yes	Yes
Province FE	Yes	Yes	Yes
Wife and Husband Education Controls	Yes	Yes	Yes
Wife and Husband Age Controls	Yes	Yes	Yes
Percentiles Female Income Distribution	Yes	Yes	Yes
Polynomials Ln Male Income	Yes	Yes	Yes
Observations	2,657	2,657	1,875
R-squared	0.203	0.201	0.123

*Notes.* OLS regressions where the dependent variable are: 1) an indicator that the female spouse worked at least 1800 hours in a wage job over the last 365 days; 2) an indicator that the female spouse is employed either as a part- or full-time worker; 3) a measure of the gap between potential and actual income earned by the female spouse as explained in the text. The sample is restricted to two-earner couples within the CHNS. No further restrictions (e.g. the sample selection for my main analysis) are applied. Additional controls are: the natural log of male spouse's income (and its squared transformation), nine deciles of the empirical female income distribution, indicators for age and education cells of the husband and the wife, ethnic minority indicator and provincial and round fixed effects. Robust standard errors in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$



Table C.7: Similarities in Marriage Matching across Generations

<b>Panel A</b>				
	(1) Partner's Years Education	(2) Partner's Years Education	(3) Child-Spouse's Age Gap	(4) Wealth Index (Child)
Years Education (child)	0.501*** (0.064)			
Mother > 0.5 Education		0.774** (0.329)		
Age Gap (parents)			0.101** (0.045)	
Wealth Index (parents)				0.391*** (0.047)
Observations	386	386	386	381
R-squared	0.356	0.380	0.120	0.314
<b>Panel B</b>				
	(1) Male's High Priority Income	(2) Mother's Extreme Priority Income	(3) Female's Extreme Priority Income	(4) Female's Extreme Priority Income
Father's High Priority Income	0.161* (0.082)	0.289*** (0.071)		
Mother's High Priority Income				0.097* (0.054)
Male's High Priority Income			0.138*** (0.039)	
Observations	119	90	324	123
R-squared	0.180	0.380	0.127	0.219

*Notes.* Column 1 in Panel A explores the correlation in years of education of the child and his/her partner. Column 2 correlates an indicator for high relative maternal education (the ratio of maternal over paternal education is at least 0.5) on partner's education. Columns 3 and 4 look at the correlations between the difference in age and the wealth index of parents and of children. Panel B looks at values towards the importance of income. Column 1 shows a transmission between father and child. Column 2 shows the similarity in father's and mother's perceptions, while column 3 does so for the husband's and the wife's. Column 4 suggests that maternal and wife's values are also correlated. Controls are: individual's gender, indicators for belonging to an ethnic minority and for being located in a rural area. Robust standard errors in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

Table C.8: Time of Implementation and Pre-Reform Achievement Variation by Province

(1) Province	(2) Implementation Year	(3) Junior High-School Non-completion Rate
Beijing	1986	0.053
Tianjin	1987	0.285
Hebei	1986	0.401
Shanxi	1986	0.394
Liaoning	1986	0.352
Jilin	1987	0.487
Heilongjiang	1986	0.385
Shanghai	1987	0.220
Jiangsu	1987	0.306
Zhejiang	1986	0.249
Anhui	1987	0.302
Fujian	1989	0.790
Jiangxi	1986	0.672
Shandong	1987	0.392
Henan	1987	0.358
Hubei	1987	0.288
Hunan	1991	0.357
Guangdong	1987	0.382
Guangxi	1991	0.381
Chongqing	1986	0.226
Sichuan	1986	0.318
Guizhou	1988	0.475
Yunnan	1987	0.499
Shaanxi	1988	0.409
Gansu	1991	0.577
Xinjiang	1988	0.581

*Notes.* Junior high-school non-completion proportions refer to the year prior to the province-specific implementation, as reported by [Huang \(2015\)](#), whose original source was the provincial education yearbooks.

Table C.9: Effects of the Reform on Relative Leisure Hours

	(1) Ratio Female Leisure Hours (child)
Ratio Female Leisure Hours (parents) Q2	0.022 (0.019)
Ratio Female Leisure Hours (parents) Q3	0.008 (0.019)
Ratio Female Leisure Hours (parents) Q4	0.006 (0.018)
Ratio Female Leisure Hours (parents) Q5	0.016 (0.025)
Ratio Female Leisure Hours (parents) Q2*Treated	-0.079** (0.036)
Ratio Female Leisure Hours (parents) Q3*Treated	-0.055* (0.033)
Ratio Female Leisure Hours (parents) Q4*Treated	-0.050 (0.031)
Ratio Female Leisure Hours (parents) Q5*Treated	-0.064 (0.039)
Observations	203
R-squared	0.162

*Notes.* Similar specification to Table 6's column 1 using quintiles of relative leisure hours (computed as the residual after accounting for home and market hours). Robust standard errors in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

Table C.10: Effects of the Education Reform on the Gaps in Spousal Characteristics

Panel A							
	(1) Gap Spousal Age	(2) Age at Marriage	(3) Gap Paternal Education	(4) Gap Maternal Education	(5) Gap Importance Money	(6) Gap Importance Not Being Disliked	
Wife Treated	0.149 (0.328)	0.038 (0.345)	0.002 (0.102)	-0.013 (0.080)	-0.114 (0.104)	-0.138 (0.105)	
Observations	2,706	2,693	2,574	2,739	2,749	2,749	
R-squared	0.064	0.145	0.017	0.021	0.017	0.021	
Panel B							
	(1) Gap Importance Having Fun	(2) Gap Importance Having Success	(3) Gap Importance Being Missed After Death	(4) Gap Importance Happiness	(5) Gap Importance Lineage	(6) Gap Importance Child's Success	
Wife Treated	0.062 (0.113)	-0.230** (0.090)	-0.144 (0.152)	0.063 (0.072)	-0.045 (0.129)	-0.046 (0.072)	
Observations	2,749	2,749	2,749	2,749	2,749	2,749	
R-squared	0.018	0.019	0.022	0.014	0.024	0.021	
Panel C							
	(1) Gap Income Gap Needed for Prosperity	(2) Gap Fairness is Needed for Harmony	(3) Gap Wealth Reflects Achievement	(4) Gap Hard Work is Rewarded	(5) Gap Intellect is Rewarded	(6) Gap Network Prevails over Ability	(7) Number of Children
Wife Treated	0.071 (0.090)	0.031 (0.075)	-0.018 (0.080)	0.034 (0.101)	0.114 (0.073)	0.010 (0.094)	-0.061 (0.049)
Observations	2,218	2,426	2,435	2,644	2,611	2,539	2,750
R-squared	0.030	0.023	0.030	0.021	0.021	0.022	0.329
Panel D							
	(1) Gap Importance HH Social Status for Child's Achievement	(2) Gap Richer Children More Likely Succeed	(3) Gap More Educated More Likely to Succeed	(4) Gap Talent Key for Success	(5) Gap Effort Key for Success	(6) Gap Luck Key for Success	(7) Gap Network Key for Success
Wife Treated	0.018 (0.080)	-0.099 (0.080)	0.087 (0.076)	0.013 (0.077)	0.060 (0.070)	-0.070 (0.090)	0.052 (0.094)
Observations	2,521	2,610	2,622	2,482	2,598	2,614	2,569
R-squared	0.026	0.021	0.021	0.028	0.011	0.022	0.015

Notes. The gap in the spouses' value of each of the outcome variables is regressed on an indicator of the wife having been affected by the reform, her ethnicity, her parental education (except when this is the outcome of interest), her household registration status, and provincial and year of birth fixed effects. The sample is restricted to individuals born between 1967 and 1979. Standard errors clustered at the province\*year-of-birth level in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Table C.11: Effects of Distribution of Education on Marital Outcomes

	(1) Gap Spousal Age	(2) Age at Marriage
PJHS	0.331 (2.210)	-3.694 (3.008)
Effective F-statistic:	10.801	10.801
Observations	210	210
R-squared	0.518	0.313

Notes. Regressions of average outcomes (at the province\*cohort level) on the province\*cohort proportion of junior high school completion, provincial and cohort fixed effects. I define cohorts as individuals born every two years in order to guarantee a reasonable number of individuals within each province-cohort cell. Effective F-statistics are obtained from the Montiel-Pflueger robust test for weak instruments. Standard errors clustered at the province\*cohort level in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Table C.12: Effects of the Reform on Female Spouse Labor Force Participation

	(1) Ever Worked	(2) Currently Working
Wife Treated	0.005 (0.014)	0.033 (0.036)
Observations	2,340	2,298
R-squared	0.046	0.072

*Notes.* Regressions of an indicator of a person reporting ever having been employed by the time of the 2014 CFPS wave (column 1) and of contemporaneously being employed at that time (column 2) on an indicator for the female spouse having been affected by the reform. Controls are: parental education, ethnicity, and province and year of birth fixed effects. The sample is restricted to female individuals born between 1967 and 1979. Standard errors clustered at the province level in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

Table C.13: Gender Wage Gap and Relative Female Spouse's Income Contribution

Panel A						
	(1) Ratio Female Spouse Income	(2) Above 40%	(3) Above 45%	(4) Above 49%	(5) Above 50%	(6) Above 55%
Female-favoring Gender Gap	0.005** (0.002)	0.011*** (0.004)	0.010*** (0.004)	0.006* (0.004)	0.005 (0.004)	0.002 (0.003)
Observations	14,502	14,502	14,502	14,502	14,502	14,502
R-squared	0.431	0.430	0.423	0.413	0.400	0.384
Panel B						
	(1) Above 40%	(2) Above 40%	(3) Above 45%	(4) Above 45%	(5) Ratio Female Spouse Income	(6) Ratio Female Spouse Income
Female-favoring Gender Gap	0.013*** (0.005)	0.011* (0.005)	0.014*** (0.005)	0.006 (0.006)	0.007*** (0.003)	0.002 (0.003)
Traditional Provinces	No	Yes	No	Yes	No	Yes
Observations	7,637	6,488	7,637	6,488	7,637	6,488
R-squared	0.455	0.403	0.445	0.403	0.453	0.410

*Notes.* Column 1 uses the level of female relative contribution to income as dependent variable. Columns 2-6 use indicators for whether the female contributed more than the respective threshold. All specifications control for household size, indicators for the household head being currently employed, a female, and for not being present in the household. I also include household head's age and household fixed effects together with province-specific linear time trends. The sample is restricted to households headed by individuals up to age 50 and to the CHNS wages 1989-2009. Panel B proceeds similarly but splits the sample into traditional provinces (above median male sex ratio at birth) and less traditional ones (below median) as indicated in the table. Standard errors clustered at the household level in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

Table C.14: Relationship between Maternal Work Hours and Child Gender Attitudes

	(1) Men Should Do Half of the Housework	(2) Men Should Focus on Work and Females on House
2010 Mother Market Hours	0.004** (0.002)	-0.003** (0.001)
Observations	1,247	1,247

*Notes.* Ordered probit regressions of the 5-category degree of agreement to the two attitudes questions for children aged 15-20 in the 2014 CFPS's round on the number of hours per week worked by their mother in any type of job in 2010. Controls are: gender, registration type, BMI of the child, education of the child and the parents, parental degree of agreement to the statement that children should respect their parents under any circumstance, and age, year of birth, and provincial fixed. Standard errors clustered at the province-at-age-12 level in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

Table C.15: Effects of the Education Reform on Out-Migration

	(1) Moved Provinces	(2) Moved Counties
Treated	0.001 (0.013)	0.015 (0.024)
Observations	9,328	9,328
R-squared	0.094	0.134

*Notes.* An indicator of whether the individual does not currently live (as of the 2010 CFPS wave) in the same province (column 1) or county (column 2) where she resided at age 12 is regressed on a gender and urban location indicators, her parental education, and year of birth and province of residence at age 12 fixed effects. The sample is restricted to individuals born between 1962 and 1980. Standard errors clustered at the province-at-age-12 level in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

Table C.16: Instrumented Effects of Education on Gender Norms

	(1) z-score
Years of Education	-0.105** (0.046)
Effective F-statistic:	24.817
Observations	11,716
R-squared	0.068

*Notes.* The z-score described in Table 7 is regressed on (instrumented) years of education while controlling for gender, parental education, ethnicity, and province and year of birth fixed effects. The sample is restricted to individuals born after 1962. Effective F-statistics are obtained from the Montiel-Pflueger robust test for weak instruments. Standard errors clustered at the province of birth and cohort level in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Table C.17: Effects of the Education Reform on Levels of Relative Contributions

	(1) Income	(2) Market Hours	(3) Home Hours	(4) Home Hours
Treated	0.008 (0.034)	-0.024 (0.041)	-0.059** (0.018)	-0.052** (0.026)
Observations	1,647	1,540	1,484	1,484
R-squared	0.048	0.062	0.064	0.066
	(5)	(6) Ratio Female Home Hours	(7)	
Placebo (2 years after actual date)	0.033 (0.043)			
Placebo (2 years before actual date)		0.005 (0.022)		
Affected (cohort -3)			0.016 (0.023)	
Affected (cohort -2)			0.013 (0.019)	
Affected (cohort -1)			0.019 (0.034)	
Affected (cohort 1)			-0.060** (0.018)	
Affected (cohort 2)			-0.085** (0.027)	
Affected (cohort 3)			-0.072** (0.031)	
Observations	1,484	1,484	1,484	
R-squared	0.065	0.065	0.072	

*Notes.* In columns 1-3, the relative contribution to each of the three dimensions is regressed on an indicator of being affected by the reform, a rural location indicator, spousal education, and provincial and year of birth fixed effects for all individuals born between 1967 and 1979 observed at least two times, irrespective of whether they are retained for my intergenerational analysis. Column 4 includes province-specific linear trends and province\*year-of-birth clustered standard errors. For all other columns the standard errors are clustered at the provincial level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Table C.18: Migrants' Relative Home Hours

	(1) Above Median	(2) Above Median	(3) Above Median	(4) Below 90%	(5) Below 80%	(6) Below 70%	(7) Above Provincial Mean	(8) Above Provincial Mean	(9) Distance to Provincial Mean
Sex Ratio at Birth	0.005* (0.003)						0.006** (0.003)		0.003*** (0.001)
Sex Ratio at Birth: 107-112		0.206 (0.165)	0.248*** (0.049)	-0.243 (0.155)	-0.263* (0.153)	-0.250 (0.189)		0.234 (0.153)	
Sex Ratio at Birth: 112-120		0.272* (0.158)	0.242*** (0.053)	-0.308** (0.150)	-0.290** (0.143)	-0.254 (0.184)		0.242* (0.139)	
Sex Ratio at Birth: >120		0.359** (0.164)	0.268*** (0.050)	-0.347** (0.157)	-0.414*** (0.150)	-0.363* (0.191)		0.352** (0.140)	
Sample	Migrants	Migrants	CFPS+CHNS Migrants	Migrants	Migrants	Migrants	Migrants	Migrants	All households
Observations	297	297	676	297	297	297	298	298	5,898
R-squared	0.174	0.181	0.092	0.182	0.154	0.156	0.090	0.092	0.037

Notes. The sample is restricted to households whose head was not born in the current province of residence (for columns 1-8) and his/her age is 22-55 at the time of the interview. Column 1, 2 and 3's dependent variables are an indicator taking the value of 1 if the household's female relative contribution is higher than the median relative home hours (86%) among my restricted sample. Column 3 combines information from the CHNS and CFPS, includes an indicator for the household coming from the CHNS dataset and clusters standard errors at the provincial level. Columns 4-6 use instead indicators for whether the female relative contribution is below each of the three thresholds. Columns 7 and 8 use an indicator for whether the relative contribution of the household is above the province-round specific mean contribution among the *non-migrant* households. Column 9 uses the household-specific *distance* to the provincial mean (positive if the household has a higher relative female contribution than the provincial average) and includes both migrant and non-migrant households. Provincial sex ratios at the household head's province of birth for the year 2000 are obtained from the World Bank. Columns 1, 7 and 9 use the continuous variable while columns 2-6 and 8 use its categorical version. The omitted category is an indicator for having a male-female ratio below 107 (i.e. the most equal category). Maintained controls are: household head's education, ethnic minority status, occupational information, and a quadratic in age. Current province of residence fixed effects are also included. Standard errors clustered at the community level in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Table C.19: Robustness to the Inclusion of Controls for Labor Market Conditions

	(1) Ratio Female Income (child)	(2) Ratio Female Market Hours (child)	(3) Ratio Female Income (child)	(4) Ratio Female Market Hours (child)	(5) Ratio Female Income (child)	(6) Ratio Female Market Hours (child)
Ratio Female Income (parents)	0.152** (0.068)		0.169** (0.067)		0.158** (0.066)	
Ratio Female Market Hours (parents)		0.169*** (0.065)		0.151** (0.059)		0.130** (0.061)
Gender Gap Controls	Yes	Yes	No	No	No	No
Predicted Wages Controls	No	No	Yes	Yes	No	No
Regional Employment Rate and Education	No	No	No	No	Yes	Yes
Observations	335	309	353	328	361	332
R-squared	0.294	0.319	0.272	0.327	0.289	0.352

Notes. I build upon the most extensive set of controls in Table 2 and I add in columns 1 and 2 for the average gender wage gap experienced by the parents while the child is still at home and by the child's couple after moving out as estimated by Meng (2012). Columns 3 and 4 control instead for the average spousal gap in *predicted* wages at each year. Columns 5 and 6 control for the average employment rate and years of education at the *community* level both when individuals are observed in their parents' and in their own household. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Table C.20: Robustness to Using Median Contributions

	(1) Median Ratio Female Income (child)	(2) Median Ratio Female Market Hours (child)	(3) Median Ratio Female Income (child)	(4) Median Ratio Female Market Hours (child)
Median Ratio Female Income (parents)	0.171*** (0.063)		0.163** (0.070)	
Median Ratio Female Market Hours (parents)		0.117* (0.060)		0.153** (0.061)
Observations	361	332	368	339
R-squared	0.210	0.280	0.247	0.317

Notes. The median female contribution among all observations of a given person as a household head is regressed on the median contribution of his mother while growing up. Controls are: individual's gender, indicators for belonging to an ethnic minority, being located in a rural area, partner's (categorical) education, parental and child's wealth, father's and mother's education, household size, presence of time saving appliances, the age differences between the spouses (parents and child), province-of-birth sex ratio at birth and year of birth fixed effects. Columns 3 and 4 proceed similarly but weighting by the number of observations used to construct the median maternal contribution. The sample is restricted to individuals born between 1962 and 1980. Standard errors clustered at the province-at-age-12 level in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1



Table C.21: Robustness to Alternative Clusterings of Standard Errors

	(1) Ratio Female Income (child)	(2) Ratio Female Market Hours (child)	(3) Ratio Female Home Hours (child)	(4) Ratio Female Income (child)	(5) Ratio Female Market Hours (child)	(6) Ratio Female Home Hours (child)
Ratio Female Income (parents)	0.142* (0.072)			0.142** (0.059)		
Ratio Female Market Hours (parents)		0.145* (0.059)			0.145** (0.060)	
Ratio Female Home Hours (parents)			0.060 (0.360)			0.060 (0.076)
Observations	371	341	365	371	341	365
R-squared	0.179	0.193	0.160	0.179	0.193	0.160

Notes. This table replicates the estimations in the even columns of Table 2 when clustering standard errors at the province level (columns 1-3; 9 provinces) and at the province\*cohort level (columns 4-6; 35 clusters). The sample is restricted to individuals born between 1962 and 1980. The computed p-values after wild-bootstrapping the standard errors are provided in parentheses in columns 1-3. In columns 4-6 the numbers in parenthesis are standard errors clustered at the province\*cohort level. Column \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Table C.22: Robustness to Alternative Measures of Relative Contributions: Combining Relative Contributions to Income, Home, and Market Hours

	(1) Average Female Contribution (child)	(2) PCA Female Contribution (child)
Average Female Contribution (parents)	0.170** (0.070)	
PCA Female Contribution (parents)		0.175*** (0.065)
Observations	327	327
R-squared	0.249	0.252

Notes. This table replicates the estimations in the even columns of Table 2 when using the average relative contribution across income, market and home hours (column 1) and their predicted first component (column 2). The sample is restricted to individuals born between 1962 and 1980 for whom we observe the three relative contributions both in their parents' and their own households. Robust standard errors in parenthesis. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Table C.23: Robustness to Alternative Measures of Relative Contributions: Indicators for Location Relative to Relevant Thresholds

	(1) Female Spouse <20% Income (child)	(2) Female Spouse <20% Market Hours (child)	(3) Ratio Female Income (child)	(4) Ratio Female Home Hours (child)	(5) Female Spouse >70% Home Hours (child)	(6) Female Spouse >80% Home Hours (child)
Female Spouse >35% Income (parents)	-0.115*** (0.044)					
Female Spouse >35% Market Hours (parents)		-0.199*** (0.064)				
Female Spouse <35% Income (parents)			-0.065** (0.027)			
Female Spouse <35% Market Hours (parents)				-0.104*** (0.031)		
Female Spouse >80% Home Hours (parents)					0.127** (0.056)	0.118* (0.064)
Observations	371	341	371	341	365	365
R-squared	0.147	0.167	0.179	0.202	0.129	0.138

Notes. This table replicates the estimations in the even columns of Table 2 when using instead as main explanatory variables indicators of belonging to a family where the female contributed above 35% of total contributions to income and market hours (columns 1 and 2), below 35% (columns 3 and 4) and above 80% to home hours (columns 5 and 6). The dependent variables are either indicators for reaching the level of relative contributions indicated in columns 1, 2, and 5 or the level of relative contributions (columns 3 and 4). The sample is restricted to individuals born between 1962 and 1980. Robust standard errors in parenthesis. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Table C.24: Robustness to Model Specification

	(1) Ratio Female Income (child)	(2) Ratio Female Income (child)	(3) Ratio Female Market Hours (child)	(4) Ratio Female Market Hours (child)
Ratio Female Income (parents)	0.143*** (0.053)	0.127** (0.052)		
Ratio Female Market Hours (parents)			0.115** (0.051)	0.119** (0.052)
Double Selection	Yes	No	Yes	No
Observations	451	451	405	405
R-squared	0.024	0.041	0.085	0.043

Notes. Column 1 controls for an indicator for Shandong province. Column 2 controls for indicators of belonging to Shandong or Guangxi and for squared spousal education. Column 3 controls for an indicator for Liaoning province. Column 4 controls for indicators of belonging to Guizhou or Guangxi and for squared spousal education. Robust standard errors in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Table C.25: Robustness Checks: Different Estimating Samples

	(1) Ratio Female Market Hours (child)	(2) Ratio Female Income (child)	(3) Ratio Female Market Hours (child)	(4) Ratio Female Income (child)	(5) Ratio Female Market Hours (child)	(6) Ratio Female Income (child)	(7) Ratio Female Market Hours (child)	(8) Ratio Female Income (child)
Ratio Female Market Hours (parents) Q2	0.136*** (0.048)		0.137*** (0.045)		0.106* (0.055)		0.095 (0.064)	
Ratio Female Market Hours (parents) Q3	0.127* (0.070)		0.125*** (0.046)		0.105** (0.053)		0.112** (0.057)	
Ratio Female Market Hours (parents) Q4	0.140*** (0.042)		0.129*** (0.043)		0.102** (0.047)		0.097* (0.052)	
Ratio Female Market Hours (parents) Q5	0.179*** (0.051)		0.179*** (0.053)		0.095* (0.057)		0.079 (0.062)	
Ratio Female Income (parents) Q2		0.110** (0.048)		0.115** (0.047)		0.068 (0.053)		0.115** (0.058)
Ratio Female Income (parents) Q3		0.162*** (0.049)		0.156*** (0.046)		0.173*** (0.052)		0.176*** (0.055)
Ratio Female Income (parents) Q4		0.123** (0.050)		0.130*** (0.048)		0.075 (0.048)		0.131** (0.055)
Ratio Female Income (parents) Q5		0.117** (0.046)		0.111** (0.047)		0.108** (0.048)		0.099* (0.054)
Observations	242	293	299	341	219	228	203	214
R-squared	0.221	0.156	0.150	0.113	0.168	0.204	0.186	0.187

Notes. Columns 1 and 2 restrict the sample to individuals for whom the relative contribution of neither their parents nor themselves lies between 0.48 and 0.52. Columns 3 and 4 restrict it to those for whom the mother contributed above 5% and below 95%. Columns 5 and 6 limit attention to individuals who form their own household rather than taking over the household head role in their original household from their parents. Columns 7 and 8 focus on individuals who are up to 30 years old the last round that they are still part of their parents' household. Controls are: individual's gender, indicators for belonging to an ethnic minority, being located in a rural area, and parental education. The sample is restricted to individuals born between 1962 and 1980. Robust standard errors in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Table C.26: Robustness to Estimation by Propensity Score Matching

	(1) ATT	(2) Treatment	(3) Control	(4) p-value t-test
<b>Panel A: Income</b>				
Treated	0.097 (2.53)			
Above 6 Years of Education		0.961	0.953	0.664
Rural		0.851	0.870	0.523
Household Size		4.565	4.538	0.739
Age Difference (parents)		-1.902	-2.102	0.604
Wealth Index (parents)		0.241	0.158	0.212
Observations	401			
<b>Panel B: Market Hours</b>				
Treated	0.119 (2.05)			
Above 6 Years of Education		0.979	0.986	0.524
Rural		0.885	0.895	0.689
Household Size		4.643	4.616	0.695
Age Difference (parents)		-1.643	-0.920	0.014
Wealth Index (parents)		0.234	0.240	0.925
Observations	370			
<b>Panel C: Home Hours</b>				
Treated	0.069 (2.69)			
Above 6 Years of Education		0.960	0.952	0.759
Rural		0.848	0.840	0.862
Household Size		4.300	4.415	0.889
Age Difference (parents)		-1.592	-2.016	0.379
Wealth Index (parents)		0.143	0.099	0.631
Observations	395			

*Notes.* Treated is an indicator for the mother having contributed above 35% of total income and market hours (Panels A and B) and above 95% for home hours (Panel C). Column 1 reflects the average treatment on the treated among the matched sample with common support. The corresponding t-statistic is reported in brackets. Columns 2 and 3 describe the average characteristics of the treatment and control groups along the matching dimensions and column 4 the p-value of the t-test of equality of these quantities. All individual born after 1962 are used.

Table C.27: Robustness of Educational Reform Results: Placebo Test

	(1) Ratio Female Market Hours (child)	(2) Ratio Female Home Hours (child)	(3) Ratio Female Leisure (child)
Post 1966	0.054 (0.073)	-0.122 (0.120)	0.005 (0.072)
Ratio Female Market Hours (parents)	0.113 (0.111)		
Post 1966*Ratio Female Market Hours (parents)	0.048 (0.127)		
Ratio Female Home Hours (parents)		0.003 (0.115)	
Post 1966*Ratio Female Home Hours (parents)		0.059 (0.142)	
Ratio Female Leisure (parents)			0.062 (0.139)
Post 1966*Ratio Female Leisure (parents)			0.011 (0.156)
Observations	203	225	162
R-squared	0.189	0.204	0.043

*Notes.* Robustness check where the sample is restricted to those born between 1962 and 1971 and where treatment takes the value one for cohorts born on or after 1966. Controls are: individual's gender, indicators for belonging to an ethnic minority, being located in a rural area, and (categorical) education. Robust standard errors in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Table C.28: Robustness of Educational Reform Results: Balance Check

<b>Panel A</b>					
	(1) Ethnic Minority	(2) Age Gap Parents	(3) Any Male Child Parents	(4) Father's Education	(5) Mother's Education
Treated	0.021 (0.039)	-0.337 (0.418)	0.061 (0.050)	-0.052 (0.568)	-0.269 (0.502)
Observations	312	313	252	312	313
R-squared	0.239	0.019	0.093	0.129	0.232
<b>Panel B</b>					
	(1) Ratio Parental Education	(2) Ratio Maternal Home Hours	(3) Maternal Below 80% Home Hours	(4) Maternal Below 60% Home Hours	(5) Parent's Judgement Importance of Income
Treated	-0.038 (0.033)	0.002 (0.019)	0.005 (0.055)	-0.018 (0.041)	0.036 (0.142)
Observations	312	310	310	310	101
R-squared	0.071	0.326	0.314	0.208	0.074

*Notes.* Results of OLS regressions of the dependent variables indicated in each column on the same treatment indicator used in Table 5, indicators for gender and rural location and provincial fixed effects. The sample is restricted to individuals born between 1967 and 1979. Robust standard errors in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

Table C.29: Robustness of Educational Reform Results: Lack of Pretends in Gender Norms

	(1)	(2)
	z-score	
1964 Cohort*Pre-reform Dropout Rate	-0.384 (0.566)	
1965 Cohort*Pre-reform Dropout Rate	-0.929** (0.381)	
1966 Cohort*Pre-reform Dropout Rate	0.066 (0.811)	
1967 Cohort*Pre-reform Dropout Rate	-1.189 (0.710)	
1968 Cohort*Pre-reform Dropout Rate	0.201 (0.375)	
1969 Cohort*Pre-reform Dropout Rate	0.104 (0.510)	
1970 Cohort*Pre-reform Dropout Rate	-0.401 (0.498)	
Continuous Measure Cohort*Pre-reform Dropout Rate		0.022 (0.058)
Observations	4,442	4,442
R-squared	0.036	0.034

*Notes.* The specifications estimated are based on those in Table 7 but focus on the subsample of individuals born between 1963 — the omitted category — and 1970 (i.e. before any cohort in any province was first treated). Hence, instead of actual treatment, I am interested in the cohort trends in gender norms across provinces with different junior high school non-completion rates. Standard errors clustered at the provincial level in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

## D Additional Figures (For Online Publication)

Figure D.1: Distribution of Within-Household Relative Contributions to Home Hours and Wage Income (Urban Households with Household Head Aged 24-54, CHNS)

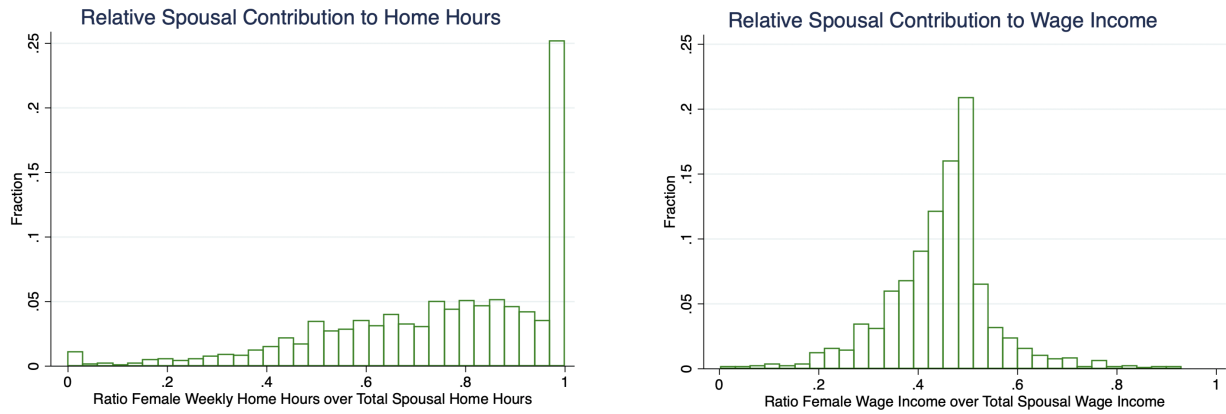


Figure D.2: McCrary Test for Relative Spousal Contribution to Total Wage Income (CHNS)

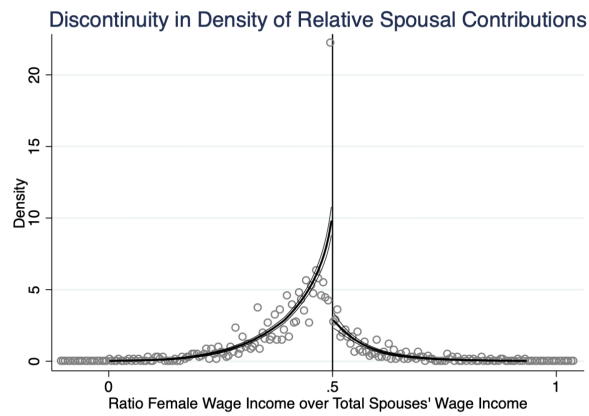


Figure D.3: Trends in Relative Contributions and Labor Market Participation (China)

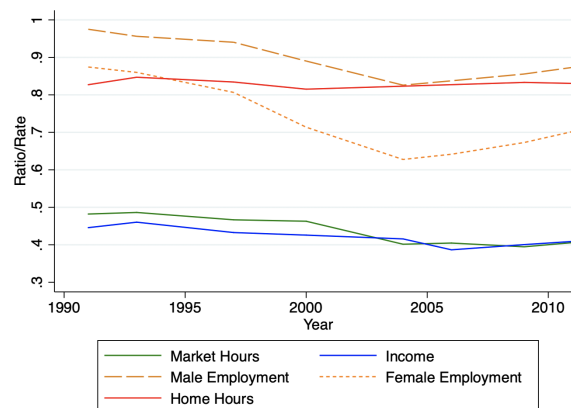


Figure D.4: Geographical Variation in the Timing of the Reform's Implementation



Figure D.5: Educational Trends Before and After Reform Enforcement by Provincial Intensity

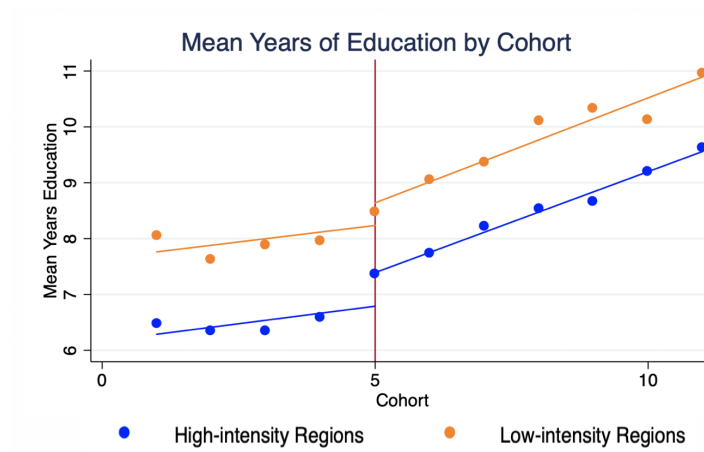
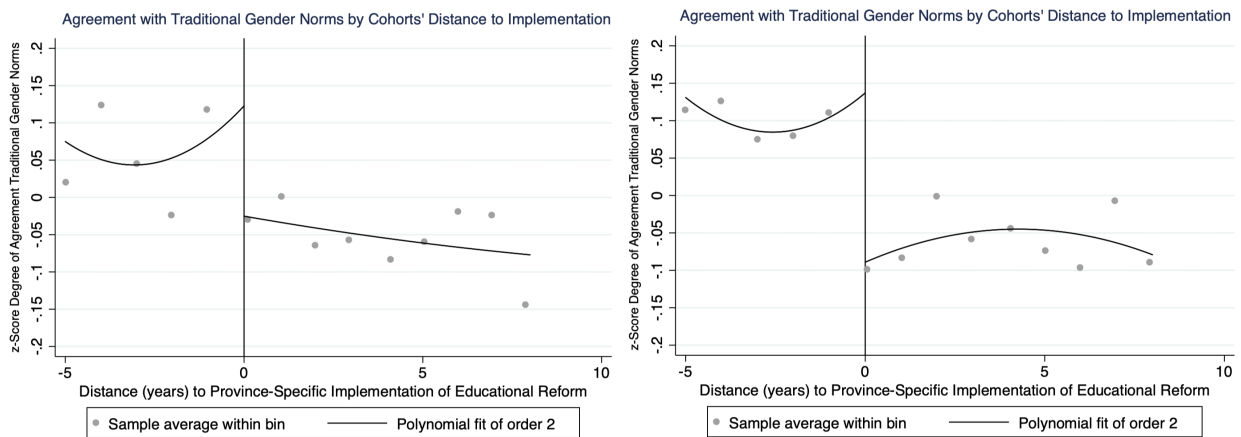


Figure D.6: Trends of Gender Norms Before and After the Reform Enforcement by Provincial Intensity



(a) High Intensity Provinces

(b) Low Intensity Provinces