

CHOOSING ETHNICITY: THE INTERPLAY BETWEEN INDIVIDUAL AND SOCIAL MOTIVES

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Abstract

This paper studies how material incentives and social norms shape ethnic identity choices in China. Provincial policies give material benefits to minorities, which consequently affect the ethnicity choices for children in ethnically mixed marriages. We formalize the ethnic identity choice in a simple framework, which highlights the interaction of (i) material benefits stemming from ethnic policies, (ii) identity costs associated with breaking the norm of following the father's ethnicity, and (iii) social reputations altering the importance of identity costs. Consistent with the model, we find that ethnic policies increase the propensity to break the prevailing norm for mixed families with minority mothers. Moreover, the impact of ethnic policies is larger in localities where more such families follow the norm. More broadly, our study shows (1) how government policies can shape identity choices and (2) how one can allow for both complementarity and substitutability between individual and social motives in empirical analyses. (JEL: D10, H1, J1)

1. Introduction

What determines identity and to what extent is it fungible? Social scientists have long tried to understand these important questions.¹ Existing economic studies suggest different determinants of identity. Some of them show how social and intrinsic motives can support persistent choices,² while others show how material incentives can create individual motives for identity change.³

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1. Sociologists and political scientists contributed to the understanding of these questions earlier than economists. While that literature is too large to survey here, two examples are the pioneering theoretical research by Tajfel (1974) on identification with groups, and the pioneering empirical research by Bates (1974) and Vail (1989) on ethnic identities and tribalism in Africa.
2. Akerlof and Kranton (2000), Bisin and Verdier (2000), Bisin, Topa and Verdier (2001), and Fernandez and Fogli (2006) are pioneering studies along this line.
3. These studies include Botticini and Eckstein (2007), Cassan (2015) and Nix and Qian (2015).

In this paper, we study these questions in the context of ethnicity choices in China. In contrast to existing research, we emphasize that both individual material motives (shaped by government policies) and social motives (shaped by social norms) matter for identity choice. We formalize the interaction between these different motives in a simple model and confront the model predictions with microdata. Two findings stand out. Individuals do change their identity in response to the material incentives entailed in government policies. Moreover, the interplay between individual and social motives gives rise to a systematic non-linearity: the response to a policy change reflects whether individual choices are complements or substitutes, which – in turn – depends on the adherence to prevailing social norms.

China is an interesting testing ground to study ethnic identities. First, it is a multiethnic society with 55 officially recognized ethnicities beyond the dominant Han (about 91.5% of the population). Second, mixed ethnic couples are free to choose whichever of their two ethnicities for their children at birth, and we observe such choices at the individual level in China's microdata. Third, the government has given policy favors to minorities in the areas of family planning and education that vary by province. While these policies may not be intended to encourage parents to choose a minority status for their children, they do create material incentives to do so. Finally, the undisputed social norm in China's patriarchal society is to choose the father's ethnicity for the children. These four features allow us to study empirically both how material motives due to government policies affect ethnic choices and how the effect of these policies varies with the adherence to social norms.

For convenience, we refer to a mixed couple with a Han man and a minority woman as Han-minority and one with a minority man and a Han woman as minority-Han. To discipline our theoretical analysis, we start out from two facts on the ethnicity of children, which stand out in both aggregate and individual-level data: (1) the propensity to break the social norm of following the father's ethnicity – i.e., the share of children that instead follows the mother's ethnicity – is much higher in Han-minority families than in minority-Han families; and (2) the propensity to break the norm is increasing in Han-minority families after 1980, in tune with policy changes related to minority status. On top of these two facts, the extent of social-norm breaking amongst Han-minority families differs widely across localities (a closer account of these facts appears in Section 2).

Motivated by these facts, our theoretical analysis exploits a framework that was first suggested by Benabou and Tirole (2011). A key feature of this framework is that it allows for both complementarity and substitutability in individual decisions. In our setting, mixed couples make decisions on their children's ethnicity based on three interacting motives: material benefits (from policies favoring minorities), individual intrinsic costs (from picking an ethnicity against the social norm), and social reputations (depending on choices by other mixed couples in a peer group). For instance, a Han-minority family that chooses minority status for its child do obtain ethnic-policy favors, but suffer the intrinsic cost of breaking the norm. Meanwhile, the cost is amplified or mitigated by the social-reputation concerns.

In particular, the social-reputation concerns consist of two parts. A Han-minority family choosing minority for its child feels a stigma for breaking the norm, but this stigma declines if the rest of the community is more likely to choose minority. On the other hand, a Han-minority family that chooses Han for the child feels an honor for following the norm, an honor that rises if the rest of the community is more likely to choose minority – as foregoing the material benefits becomes more honorable. With ethnic policies inducing more families to break the norm, the race between changing stigma and changing honor determines the multiplier effect of social motives. When a small share breaks the norm, the stigma concern dominates because the stigma declines quickly. In this case, the positive impact of government policies on choosing minority is amplified by the social effect. By contrast, when a sizable share breaks the norm, the honor concern dominates as it increases quickly. In this case, the positive impact of government policies on choosing minority is dampened by the social effect.

This model delivers clear empirical predictions. In particular, material benefits from government ethnic policies increase the probability of choosing minority for the children for Han-minority families, but have little impact on minority-Han families (who do not face a tradeoff between material benefits and intrinsic costs). Thus, we focus on the prediction of the interplay between material benefits and social norms. Following the above logic, we expect to see the impact of material benefits on minority identity (for Han-minority families) differ by the share of families that breaks the norm. When this share is small (large), we expect to see a larger (smaller) impact of ethnic policies. Specific to the prediction of this model, our prediction calls for a non-linear specification: we expect to see different effects below and above a certain share of families breaking the norm.

To test the key predictions from the model, we study the ethnic identity of children in Han-minority families, using individual-level census data. We wish to examine how ethnicity varies by government policies and local social norms. There are at least three empirical challenges. One is how to measure the material benefit of policies favoring minorities. Since ethnic policies appear in a bundle of provincial regulations, it is not straightforward to quantify their regional variation over time. To ensure that our results are robust, we use three alternative measures: rollout of the one-child policy, additional fertility rates for minorities (relative to Han), and additional scores for minorities in college-entrance exams. While the first measure captures ex ante policies, the latter two reflect realized policy intensity.

A second challenge is to measure social-norm adherence. We exploit the variation across more than 300 prefectures (the administrative units one step below the province level) in the share of Han-minority families that break the social norm by giving the mother's identity to their children. However, individual and aggregate ethnic choices in the relevant peer group may be simultaneous, thus creating an instance of the reflection problem (Manski 1993). To avoid this problem, we define the share of children that follow their mother's identity by the choices in cohorts born already in 1970–1974 in the same prefecture – i.e., about a decade before the rollout of the one-child policy. This naturally leads to a difference-in-differences specification. The initial share becomes an ex ante classifier of social concerns, allowing us to compare the impacts of ethnic

policies in prefectures with low vs. high 1970–1974 norm-breaking shares. In addition to using the same prefecture as the peer group, we also employ alternative definitions by narrowing or broadening the peer groups.

A third challenge is omitted variables. Ethnic policies may be correlated with other variables that also affect ethnic choices. We check for omitted variables in two ways. We show that pre-trends are absent: changes in ethnic choices only occur after the implementation of the ethnic policies. On top of this, we collect a set of regional characteristics, allowing these to have different impacts on outcomes before and after the introduction of the ethnic policies – we then check if our estimates are stable when these characteristics are or are not considered.

Using individual census data from 1982, 1990, 2000, and 2005 (which can be linked via prefecture information), we document that policies raising the material benefits of minority children do indeed trigger a higher propensity to choose minority children in Han-minority families. For instance, when measuring material benefits via the rollout of the one-child policy, we find that the minority share of children goes up by 7.8 percentage points (19 percent of the pre-policy probability). Estimates with the difference-in-differences specification confirm that the effect of ethnic policies is indeed larger in prefectures with a share of norm-breakers below a certain cutoff. For example, with the one-child measure of material benefits, the increase in minority children is 10.3 (3.1) percentage points for prefectures where the share of norm-breakers in the 1970–1974 cohort is below (above) 50%. We also find support for additional predictions including non-linear effects for different quantiles that provide further support for our model setup.

Our framework has the advantage of delivering a very specific empirical specification, for which we find empirical support in the data. However, we are cautious about alternative explanations. We thus discuss, in detail, several theoretical and empirical alternative explanations for the results: different preference specifications, different definitions of social reputations or peer groups, the changing bargaining power of women, the minority-population share diluting material benefits or inducing social conflict, a kind of censoring, and changes in the number of children. While we find that some of these alternatives may contribute to the changing ethnicity of children of mixed couples, none of them alters our main empirical results on the interaction between individual and social motives. This suggests that our framework offers a useful perspective for guiding the empirical analysis.

Our study contributes to several lines of research. First, it adds to the existing studies we have already mentioned on the determinants of identity. Our study also complements a recent literature analyzing how social and economic factors shape the identity choice (e.g., Fouka 2019, Atkin, Colson-Sihra and Shayo 2019). We document that identity can be fungible: in our context, individuals change the ethnicity of their children in response to material benefits induced by government policies. Meanwhile, we show that the impact of these policies changes with the local strength of social-norm compliance.

Second, our study is related to a burgeoning literature on the interaction of individual and social motives when social-image concerns are critical (e.g., Bursztyn

et al. 2014, Bursztyn and Jensen 2017, Cantoni et al. 2018, Karing 2019).⁴ Even though Benabou and Tirole's (2011) framework permits social-image concerns to turn individual choices into strategic complements or strategic substitutes, few studies have formalized this flexible relationship empirically. Our study derives an empirical specification to formally test this insight.

While understanding the complex interaction between different motives certainly requires multiple frameworks, we believe that our method can be applied to a wide set of individual choices in the economic, political or social arena, where individual and social motives both play a substantial role. Subsequent to the first version of this paper, Besley, Jensen and Persson (2019) use an extension of the Benabou-Tirole model to derive predictions for their empirical study of the property-tax evasion in the UK, although they use council rather than individual data. Joensen and Skyt Nielsen (2015) also use a similar set up in an individual-level empirical analysis to compare how Danish girls and boys choose Math and Science majors. Chen (2016) exploits a similar framework to explain the extent to which the death penalty spurred absences in the British military during World War I.

The next section presents several facts and describes the relevant institutional background to our study. Section 3 formulates our model and spells out its predictions. Section 4 discusses which Chinese data can be used to test them. Section 5 demonstrates empirically that the main theoretical predictions are consistent with these data. Section 6 discusses alternative explanations for the patterns in the data and whether these explanations drive our main result. Section 7 provides a brief conclusion. To save space, we relegate some additional modeling, tables, and figures to an Online Appendix.

2. Background

China has 56 ethnic groups, the dominant Han plus 55 minorities. As of the year 2000, the combined population of minority groups stood at about 106 million, 8.5% of the total mainland population. The 55 minority groups vary widely in size. With a population of more than 15 million in the year 2000, the Zhuang was the largest minority, and the Lhoha with only 2,965 the smallest. Minority groups also vary greatly in culture, spoken language and religious practice – 53 minority groups speak languages of their own, 23 have their own written language, 10 groups are predominantly Muslim, and eight follow Tibetan Buddhism.

4. There is also evidence from experimental settings that often focuses on either complementarity or substitutability (e.g., Gneezy and Rustichini 2000, Fehr and List 2004, Ariely, Bracha and Meier 2009 among others). See Gneezy, Meier and Rey-Biel (2011) for an overview.

2.1. Salient Facts on Ethnic Choices

We begin by noting two salient facts on the ethnicity of children in mixed couples, which we will use to discipline the theoretical analysis.⁵ The first fact follows.

FACT F1. *The average share of children with their mother's ethnicity is much higher in Han-minority families than in minority-Han families. Han-minority families thus have a higher propensity to break the social norm.*

Figure 1(a) plots ethnicity shares over time in the aggregate data – by five-year birth cohorts – for the two types of mixed marriages. On average, the probabilities that children have their mother's ethnicity in minority-Han and Han-minority families are 6 and 47%, respectively. Naturally, these aggregate patterns can be confounded by regional characteristics and time trends. However, as discussed in Online Appendix A.1, differences of the same magnitude also hold at the individual level, when we control for prefecture fixed effects, birth-year fixed effects, and province-specific trends (province fixed effects times birth year).

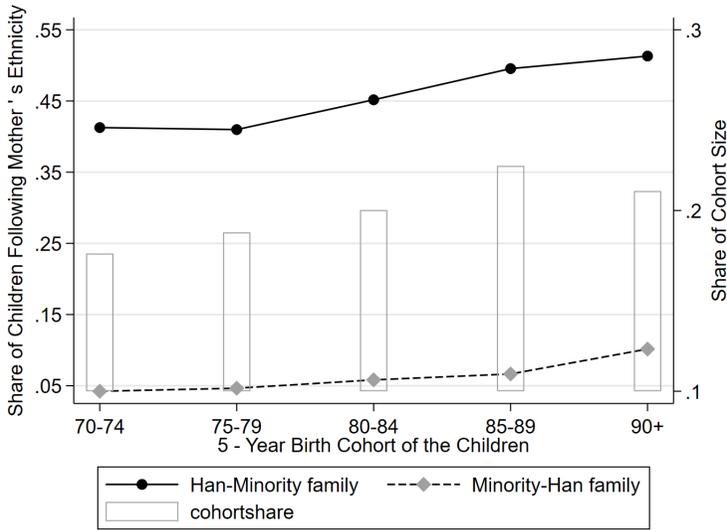
The second fact follows.

FACT F2. *The propensity to break the social norm – i.e., to give children their mother's ethnicity – is increasing in Han-minority families after 1980.*

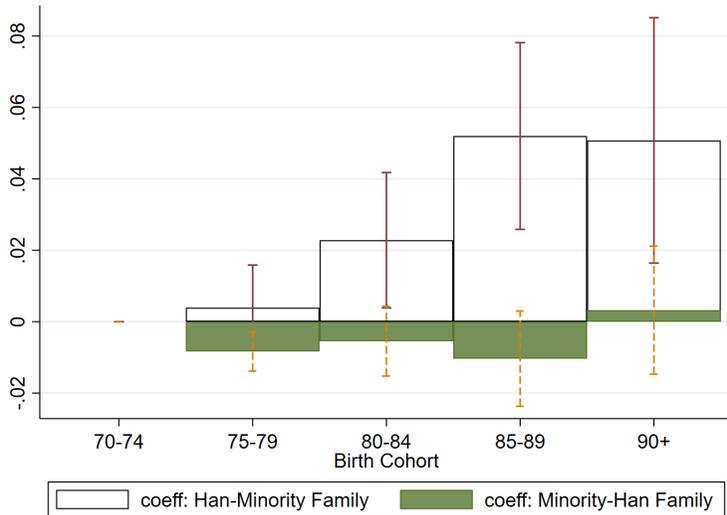
This pattern is clearly shown in panel (a) of Figure 1. At the aggregate level, the average share of children in Han-minority families with their mother's (minority) ethnicity is 41% in cohorts born before 1980, but 49% in cohorts born after 1980. Panel (b) shows that this pattern also holds at the individual level (after controlling for prefecture fixed effects and province-specific trends). Differently, we observe little change in ethnic choices by minority-Han families. In those families, the aggregate-level data suggests a slight increase in the average share of children with their mother's (Han) ethnicity, but this change is not significant in the individual-level data, once we control for province-specific trends (see the shaded area in panel (b)). If anything, the individual-level trend is weakly decreasing until the 1990s.

On top of these basic facts, we also find a wide variation across prefectures in the propensity for Han-minority families to break the social norm. Figure 2 plots the shares of children with their mother's ethnicity across prefectures in the two types of mixed families with children in the 1970–1974 cohort. These shares have a mean around 0.39 and a standard deviation of 0.33 for Han-minority families. By contrast, the variation is much smaller for minority-Han families.

5. We use the 1982, 1990, 2000 censuses and the 2005 mini-census. Our analysis focuses on the ethnicity of individuals born between 1970 and 2005, including those in two-generation and multiple-generation households. In one of the robustness checks, we restrict the data to two-generation households only.



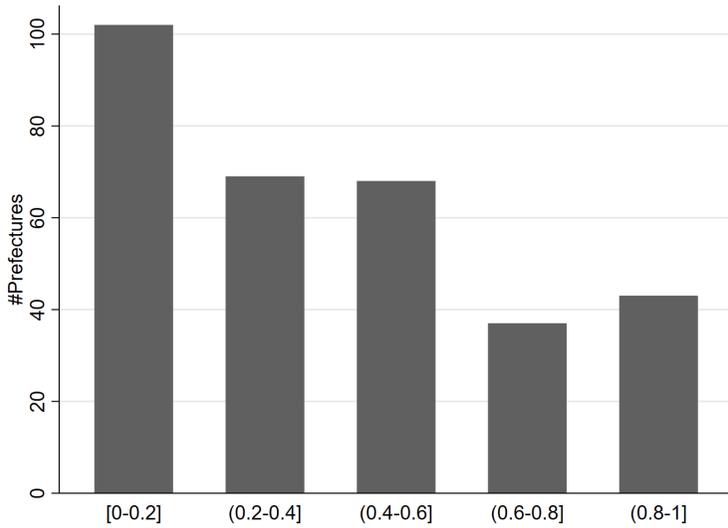
(a) Aggregate data: Share of children that break the norm.



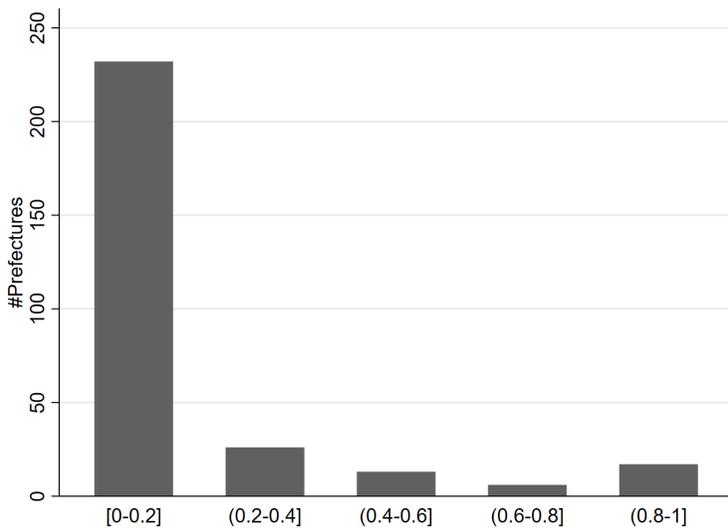
(b) Individual data: Probability of breaking the norm.

FIGURE 1. Share of children that break the norm (i.e., with mother’s ethnicity) by type of mixed marriage and birth cohort. This figure shows two facts using aggregate and individual data:

- a. As in F1, children are more likely to have their mother’s ethnicity in Han-Minority families;
- b. As in F2, an increasing share of children with their mother’s ethnicity in Han-Minority families after 1980.
- c. Figure (b) visualizes the results in columns (3) and (6) of Appendix Table A.2. It shows the probability of having a minority child in two types of mixed marriages over time, using those born during 1970–1974 as the comparison group. The bars indicate 95% confidence intervals.



(a) Share of children with mother's (minority) ethnicity in Han-minority marriages.



(b) Share of children with mother's (Han) ethnicity in minority-Han marriages.

FIGURE 2. Distribution of the share of children with their mother's ethnicity, in mixed marriages across prefectures (for those born in 1970–1974). Our empirical analysis focuses on Han-minority families.

Panel (a) shows that the share of children with their mother's ethnicity varies a great deal across regions for Han-minority marriages.

Panel (b) shows that the children of minority-Han couples in most prefectures rarely have their mother's ethnicity.

2.2. Benefits and Costs of Minority Children

Anecdotal Evidence. Little research exists on ethnic choices for children in China. However, one finds numerous discussions online among parents, which reflect the motives for alternative ethnicity choices for their children.

One example of a suggestive discussion [in our own translation from Chinese] appears at the website babytree.com – see Online Appendix A.2 for the original discussions:

Anonymous asked: \If the father is a Han and the mother is a minority, could the child be a minority?"

Linyibaobeixuan answered: \Generally it should follow that of the father. But following that of the mother has benefits of ethnic favors."

Yuer2011 answered: \The child usually follows the father's ethnicity. It is also fine if you insist on following that of the mother."

Sankouzhijiatu answered: \The child should follow the father's ethnicity. Only the children of a live-in husband will follow that of the mother."

Xixi1011 answered: \You can follow that of the mother. A minority has the option of having a second child."

This dialogue suggests that material benefits due to ethnic policies – especially the option for a minority child of having more than one child as an adult – motivate Han-minority families to give children their mother's ethnicity. The costs of having a minority child are primarily social and intrinsic: the social norm is that children adopt the fathers' ethnicity and the opposite choice imposes costs on a father. For instance, a Han man with a minority child may be stigmatized – as the wife usually goes to live in the husband's family, only lower-status men will consider becoming live-in husbands, known as \uxorilocal marriage" in social anthropology. This first example illustrates the stigma side of social reputations, in that men with children of a different ethnicity can be considered to be of low social status.

Another illustrative discussion [also in our own translation from Chinese] is found at the website jzb.com (jzb means parents' helper in Chinese) – see Online Appendix A.2 for the original discussions:

Zhongermen said: \I went to register the birth of my child a while ago. I am a Han man and my wife is a minority. I told the police that I want my child to be a Han. The police kindly suggested that I should choose minority for the child. She said that one score lower implies an extra playground of competitors in the high-school entrance exam and that I should be responsible for my child's future. But I insisted on choosing Han in the end. I hope that my child's future will rely on his own ability, not ethnic favors."

fh2315 remarked: \Choosing minority is not a big deal if the minority does not practice religion."

claetitia remarked: \Well, if you despise the ethnic favor for extra scores, minorities can at least have more children!"

Magua remarked: \I am a minority and my child follows my ethnicity. The reason is simple. Even though I belong to a minority group whose population size is large, I am proud of my ethnicity. So I hope that my child is also [proud of my ethnicity]. This has nothing to do with extra scores."

Once again, these arguments reflect the trade-off between material individual benefits and intrinsic or social motives. But this example illustrates the honor, rather than the stigma, side of social reputations. For instance, the Han man who starts the discussion feels honorable to assign his own ethnicity to his child despite the favors toward ethnic minorities. The first commentator suggests that the cost may be higher for adopting the mother's minority ethnicity when this is associated with religious practice. As already mentioned, our theoretical framework will allow for such interaction effects. The last commentator argues that he chooses his own ethnicity for his child, because he is proud of his ethnicity and hopes the child will share this pride. Relatedly, our theory will highlight the interplay between social stigma and social honor.

The Choice of Ethnicity. No legal barriers exist for mixed marriages between any two ethnic groups. At the birth of a child, a mixed couple has to choose one of their own ethnicities for their children. This ethnic identity appears in almost every context, including the birth certificate and all forms that have to be filled out at school. This information is particularly salient when someone gets benefits due to his or her ethnicity status. This means that the chosen ethnicity can be considered public information to peers. As already suggested by the data, an ethnic choice is more than a label. If it were merely a label, all parents would presumably choose minority status in view of the preferential policies.

Minority identity brings both benefits and costs for the child and hence, indirectly for the parents. According to government regulation, couples with the same ethnicity cannot choose any other ethnicity for their children. As for switches later in life, children from mixed marriages can apply to change the ethnicity they were given at birth before the age of 20. However, the applications have to be made by the parents for those younger than 18. Since these applications are costly and approval is uncertain, the impact of policy interventions on switches later in life should be much less important than the ethnicity choices by parents at birth. Unfortunately, no systematic data is available to shed light on this. As an indirect check, we examine the correlation of ethnicities by prefecture-birth year across censuses. For instance, the correlation for the 1982 and 1990 samples is around 0.96, suggesting that switching of ethnicities does exist but cannot be very frequent.

Benefits of Minority Children. The benefit side reflects various policies. Since the beginning of the People's Republic of China (1949-), the government has introduced various benefits to ethnic minorities to promote positive Han-minority relationships. Such policies exist in three areas:

(i) *Family planning.* When family-planning policies started in the 1960s, minorities were more favorably treated than the Han majority. Over time, the treatment of different minorities has varied by region. As detailed in Section 4, family-planning policies became much more stringent in the years around 1980 with the implementation of the one-child policy, rendering the advantages of minorities more salient.

In our analysis, we use two measures: one is on the rollout of family-planning policies across provinces; the other is the completed fertility ratio of minority vs. Han mothers (aged 40 and above). Section 4 describes this measurement in detail.

(ii) *Entrance to higher education.* Since the restoration of entrance exams in 1977, minorities have enjoyed additional points in the exams deciding upon educational entries, especially to high school and college. These benefits also vary by province. In our analysis, we measure this benefit by average extra scores for minorities by province in the National College Entrance Exam. Once again, Section 4 details data and measurement.

(iii) *Employment.* The national ethnic policy states that minorities should have a favorable treatment in employment. However, explicit benefits are rare. As minorities can be discriminated against in employment, it is unclear that this policy would make people tend to choose a minority identity for their children. For instance, Hasmath and Ho (2015) find that minorities perceive that they are disadvantaged in job search, even though estimated Han-minority wage differentials demonstrate little evidence for this.

Costs of Minority Children. The cost side has two main aspects, as explained below.

(i) *Discrimination.* Minorities may face discrimination in the labor market, even when they have the same educational background as Han. However, two facts suggest that material benefits are likely to dominate the potential discrimination costs in the labor market. First, in the anecdotal discussions, discrimination is never mentioned when parents make ethnicity choices for newborns. Second, almost all minority-Han couples choose minority ethnicity for their children – one would expect more Han children if discrimination were a dominant concern. In any case, our model below has a basic level of net material benefits, which can be positive or negative – what matters for our predictions is the change in net benefits due to family-planning and education policies. Our framework thus allows for the existence of discrimination.

(ii) *Identity costs.* As highlighted by the anecdotal discussions, a main disadvantage of having a minority child for Han-minority families is an identity cost, especially for Han fathers. In patriarchal China, children are expected to follow the ethnicity of the father. Additionally, the identity costs are likely to be affected by prevailing social norms and by the choices of relevant peer groups. Therefore, a Han man will face a trade-off between material benefits (on behalf of the child) and social status. By contrast, the problem for a minority father does not involve a trade off: having a child of his own ethnicity does not only bring material benefits to the child, but also conforms with the prevalent social norm.

Given these considerations, our model in Section 3 incorporates three different motives: individual material benefits, intrinsic benefits or costs, and social reputations. We also build in the asymmetry for Han and minority men. Before presenting the model, we describe the patterns of mixed marriages which provide the context of ethnic choices for the children.

2.3. Mixed Marriages and Demographics

Besides ethnic choices, a few other demographic characteristics vary over time with family-planning policies. First, the population share of minorities is increasing: from 8.7% for those born in 1970–1974 to 10.1 percent for those born in 1990–1994. This is a consequence of the ethnic choices studied here, as well as different fertilities among Han and minorities. Second, family-planning policies raise the male-to-female sex ratios and, consequently, affect the marriage market. For instance, the sex ratio goes up from 1.01 for those born in 1970–1974 to 1.15 for those born in 1990–1994.

While we do not focus on the marriage-market consequences in this paper, these also respond to ethnic policies. For instance, the probability of marrying a Han man for minority women born in 1940–44 (married in the 1960s) was 14%, whereas it went up to 21% for minority women born in 1970–1974 (married in the 1990s). This hike is likely related to policies favoring minority children – no specific policy favors mixed marriages as such during the period we study, but the ethnic benefits to children do affect the relative value of entering a Han-minority marriage vs. a Han-Han marriage.⁶

Later in our analysis, we will show that our findings are not affected by marriage patterns. Nevertheless, it is useful to know the prevalence of mixed marriages. In Online Appendix A.3, we present descriptive patterns of the four possible types of marriages, as well as education and age differences between husband and wife. Compared to couples of the same ethnicity, the education differences among mixed couples are slightly lower, suggesting a somewhat more assortative matching by education. The age differences between husband and wife are not substantive across marriage types. Among the married couples that appear in our four censuses, 17% of the minority men marry Han women, while 18% of the minority women marry Han men.⁷

3. Theory

We employ the framework in Benabou and Tirole (2011) to model the ethnicity choice for children as a trade-off involving individual (material and intrinsic), as well as social (norms-related) payoffs. The distinctive feature is that prevailing norms do not only imply a social stain (stigma) when the norm is broken, but also a social esteem (honor) when the norm is adhered to. As illustrated by the anecdotal discussions in Section 2, mixed couples choosing ethnicity for their children have both stigma and honor in mind. This framework allows us to analyze the interplay between the two, where individual behaviors can be either complements or substitutes, depending on the behavior of others.

6. Huang and Zhou (2016) argue that the one-child policy has raised the probability of mixed marriages in China due to the material benefits.

7. This gender-ethnicity difference is much less striking than the corresponding difference in US black-white marriages, where 6% of black men marry white women while 2.9% of black women marry white men around 2000 (Fryer 2007).

As the main role of the model is to derive empirical predictions, we only include the prospective determinants of ethnicity choices that can be measured – or proxied – with some confidence (see Section 4). The model is certainly highly stylized. However, it is not only consistent with the facts presented in Section 2 but also yields additional and testable predictions. In particular, the model clearly predicts how material benefits and social motives interact, which is our main issue of interest.

3.1. Setup

Consider a region – a prefecture, to be concrete – with a continuum of households (couples) in a given cohort. There are two ethnicities $J \in \{H, M\}$, where H denotes Han and M minority. Children yield the same basic benefit v for every household. Each household has a single binary decision to make: to break the norms by assigning the mother’s ethnicity to their child, $m = 1$, or not, $m = 0$. In line with China’s social situation, we assume that (i) the choice primarily reflects the father’s preferences (see Section 6 for a model of bargaining and related implications), and (ii) the socially acceptable choice is to pass on the fathers’s ethnicity to the child. We focus on the decisions by mixed couples (H, M) or (M, H) , where the first entry is the ethnicity of the father. (Non-mixed couples are obliged to pick their joint ethnicity for their child.)

Han-minority Mixed Couples. Consider a typical H, M couple. All such couples in the prefecture belong to the same *peer group*. They have a preference function

$$u^{H,M} = v + (b - e(H) - \varepsilon)m + \mu E(\varepsilon | m), \quad (1)$$

where b is the net *material individual* benefit of having a minority child. This could differ across regions or time, due to different policies favoring minority children (recall Section 2). Further $e(H) + \varepsilon$ is the *intrinsic individual* cost of a child’s ethnicity being different from the father’s ethnicity (recall Section 2). Its first component $e(H)$ is the average cost perceived by households when their child has a different ethnicity than the Han father – this is common and deterministic to all peer-group members, but could differ across groups. The second component ε captures the variation in intrinsic cost, the only source of household heterogeneity. We assume that ε is distributed across couples with mean $E(\varepsilon) = 0$, c.d.f. $G(\varepsilon)$, and continuous, differentiable, and single-peaked p.d.f. $g(\varepsilon)$. By these individual motives alone, households with a high ε would have a child with the father’s Han ethnicity, while those with a low value would have a child with the mother’s minority ethnicity.

The final term in (1) captures the social motive: the household’s *social reputation* (or self image) – how the peer group views the mixed couple (or the couple views itself) – given its ethnicity decision. This assessment varies with the actions and the norms of the mixed couple’s peer group, which explains the specification as a conditional expectation. Taken literally, the model assumes that the choices of m are perfectly observable by everybody in the peer group. In reality, observability is realistic since the ethnic choice follows the child through life, as discussed in Section 2. The assumption can easily be relaxed to allow for stochastic observation – in that case, one

part of parameter μ reflects the probability that m is observed (see Besley, Jensen, and Persson 2019).

As high-value ε households make the socially accepted choice, we assume that the household's social reputation is increasing in its "expected type" $E(\varepsilon | m)$, the conditional mean of ε of those in the peer group who make the same choice as does the couple. Parameter μ is the relative weight on this social motive. (In the Online Appendix, we consider alternative preference structures without social reputations, as well as alternative formulations for the social reputations.)

It is useful to define the difference

$$\Delta = E(\varepsilon | m = 0) - E(\varepsilon | m = 1). \tag{2}$$

The value of Δ is the couple's *gain* in social reputation within its peer group when it conforms to the social norm to give the father's (Han) ethnicity to the child. The first term is the social honor of giving a child the father's ethnicity – the socially accepted choice – and the second term is the social stigma of a child with the mother's ethnicity – the socially non-accepted choice.

An Equilibrium Cutoff Rule. With this notation, it follows from (1) and (2) that the mixed couple is indifferent about the child's identity when

$$b - e(H) - \varepsilon_H^* = \mu\Delta(\varepsilon_H^*). \tag{3}$$

Since social reputations depend on the behavior of other couples in the peer group, this equality implicitly defines an equilibrium cutoff value ε_H^* . For the marginal couple, the net individual benefit of a child with the mother's ethnicity (the LHS) is equal to the gain in social reputation of a child with the father's ethnicity (the RHS). Children of couples with an ε below ε_H^* get the mother's ethnicity and those with an ε above ε_H^* get the father's ethnicity. Consequently, the share of children with the mother's (minority) ethnicity in the peer group is given by $G(\varepsilon_H^*)$. By (3), ε_H^* is a function of b, e and μ . Given the cutoff rule, the equilibrium gain in social reputation becomes

$$\Delta(\varepsilon_H^*) = E(\varepsilon | \varepsilon > \varepsilon_H^*) - E(\varepsilon | \varepsilon < \varepsilon_H^*) > 0. \tag{4}$$

By definition of truncated means (of a mean-zero variable), the first term is always positive and the second term is always negative. Hence, $\Delta(\varepsilon_H^*)$ is always positive. By the results in Jewitt (2004), the single peak of g implies that Δ has a unique interior minimum.⁸

Comparative Statics. From the cutoff condition (3), we can derive how the share of children with the mother's ethnicity changes with the material benefits of such children

8. Note that, for the whole peer group, social reputation is like a zero-sum game: under a veil of ignorance about ε , the *ex ante* expected value of $\mu E(\varepsilon | m)$ is zero (as the unconditional mean of ε is zero).

b. The implicit function theorem implies

$$\frac{\partial G(\varepsilon_H^*(b, e, \mu))}{\partial b} = g(\varepsilon_H^*(b, e, \mu)) \frac{1}{1 + \mu \frac{d\Delta(\varepsilon_H^*(b, e, \mu))}{d\varepsilon^*}} > 0. \quad (5)$$

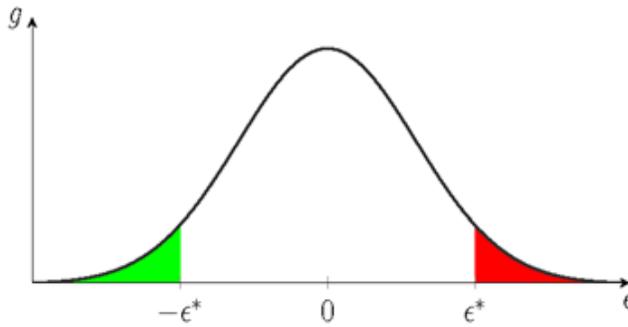
Higher material benefits for minorities raise the share of children with (the mother's) minority ethnicity: the density is positive and so is the "social multiplier" – if we follow Benabou and Tirole (2011) and assume that $1 + \mu d\Delta(\varepsilon_M^*(b, e, \mu))/d\varepsilon^* > 0$ (which implies that μ is not large enough to create multiple equilibria). The social multiplier reflects the interaction between individual and social motives in the model and the properties of the comparative statics depend on the sign and size of $d\Delta(\varepsilon_H^*)/d\varepsilon$, i.e., how the gain in social reputation from a Han child changes with the behavior of others.

As ε_H^* rises with b , more couples follow the mother's minority ethnicity. Then, both the honor and the stigma terms in (4) go up in value. When more children get the mother's minority ethnicity (i.e., breaking the norm), this makes following the norm more honorable. At the same time, having a minority child becomes less stigmatizing. What matters for the sign of $d\Delta(\varepsilon_H^*)/d\varepsilon$ is whether the honor goes up by more or less than the stigma goes down ("goes down" here and below means that a negative number becomes closer to zero).

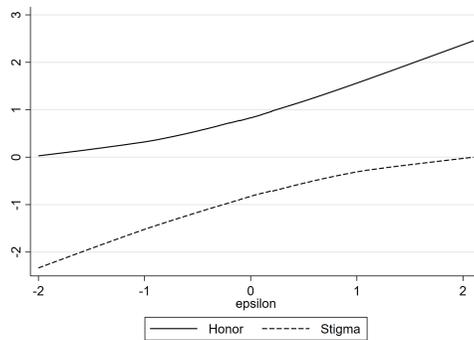
The Race between Honor and Stigma. Panel (a) of Figure 3 illustrates two different possibilities. Suppose first that $\varepsilon_H^* = -\varepsilon < 0$ in the left tail of the ε distribution, so the share of minority children is small. In this case, the effect on the honor – the (truncated) mean of ε to the right of $-\varepsilon$ – is relatively small. But the effect on the stigma (the mean of ε in the green tail to the left of $-\varepsilon$) is relatively large. As the stigma of a minority child goes down faster than the honor of a Han child goes up, the gain in social reputation from having a Han child goes down. That is $d\Delta(\varepsilon_H^*)/d\varepsilon^* < 0$, so even more people have minority children. In this case, the decisions of different couples are strategic complements and the social multiplier is larger than 1.

The alternative equilibrium in Figure 3(a) has $\varepsilon_H^* = \varepsilon > 0$ in the right-hand tail of the distribution, where many couples break the social norm and have minority children. In this case, the honor of a Han child (the mean of ε in the red tail to the right of ε) goes up faster than the stigma of a minority child goes down, so the gain in social reputation from having a Han child rises, which dampens the rise in the share of minority children. That is, $d\Delta(\varepsilon_H^*)/d\varepsilon^* > 0$, decisions of different couples are strategic substitutes, and the social multiplier is smaller than 1.

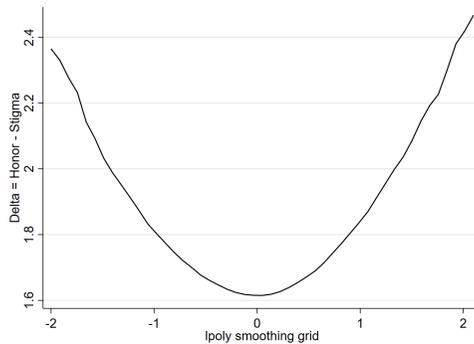
Panel (b) of Figure 3 illustrates this race between honor and stigma in a numerical example with a symmetric distribution. It shows that both the honor of respecting the norm goes up and the stigma of breaking the norm goes down (its negative value approaches zero) with a higher ε^* and that the (positive) honor always exceeds the (negative) stigma. Moreover, the honor goes up faster when many Han-minority couples have minority children, whereas the stigma falls faster when few couples have minority children. This difference generates the pattern in panel (c), where $\Delta(\varepsilon_H^*)$ decreases in ε^* when few children among Han-minority families have the mother's ethnicity but increases in ε^* when many such children have the mother's ethnicity.



(a) An illustration



(b) Stigma and honor



(c) $\Delta = \text{Honor} - \text{Stigma}$

FIGURE 3. The race between honor and stigma. This figure presents a numerical example assuming that ε has a (0,1) normal distribution.

Panel (a) illustrates the social stigma (the green area) and the social honor (the non-green area) for a low equilibrium cutoff, and the stigma (non-red area) and the honor (the red area) for a high cutoff.

Panel (b) illustrates how both stigma and honor change together with ε .

Panel (c) shows that the gain in social reputation (Honor minus Stigma) has a minimum and changes with the equilibrium share of children breaking the norm (i.e. following the mother's ethnicity).

Under a relatively mild assumption on the ε -distribution, the second derivative of $\Delta(\varepsilon_H^*)$ is everywhere positive $d^2\Delta(\varepsilon_H^*)/d\varepsilon^{*2} > 0$. Once we make that assumption, the multiplier monotonically decreases as the initial equilibrium ε_H^* (and the share of children following mother’s ethnicity) travels from low values to high values.

Minority-Han Mixed Couples. A M, H mixed couple has preferences analogous to (1) viz.:

$$u^{M,H} = v + (1 - m)b - m(e(M) + \varepsilon) + \mu E(\varepsilon | m). \tag{6}$$

Now, $e(M)$ and ε represent the average and idiosyncratic intrinsic cost of having a child with the mother’s ethnicity (Han in this case) – the social norm now prescribes passing minority identity to the child. We specifically assume that the distribution function G for ε and the weight on social reputation μ are the same in the two types of couples in the same locality.⁹

The M, H couple will have a Han child when $-(e(M) + \varepsilon) + \mu E(\varepsilon | m = 1) > b + \mu E(\varepsilon | m = 0)$. Defining the gain in social reputation as before – i.e., Δ is the honor of following the father’s minority ethnicity, $\mu E(\varepsilon | m = 0)$ minus the stigma of following the mother’s Han ethnicity, $\mu E(\varepsilon | m = 1)$ – we can write the indifference condition for a Han child as

$$-b - e(M) - \varepsilon_M^* = \mu \Delta(\varepsilon_M^*). \tag{7}$$

Thus, minority-Han households with ε smaller (larger) than ε_M^* will have Han (minority) children. Because Δ is always positive, it follows that $\varepsilon_M^* < 0$. The share of children with the mother’s Han ethnicity within this peer group is thus $G(\varepsilon_M^*)$.

In the same manner as for H, M couples, we can derive the comparative statics for a change in b to get:

$$\frac{\partial G(\varepsilon_M^*(b, e, \mu))}{\partial b} = -g(\varepsilon_M^*(b, e, \mu)) \frac{1}{1 + \mu \frac{d\Delta(\varepsilon_M^*(b, e, \mu))}{d\varepsilon^*}} < 0. \tag{8}$$

3.2. Consistency with Motivating Facts

In this subsection, we show that the model is consistent with the facts presented in Section 2.

Choices across Mixed Marriages – Fact F1. In terms of the model, F1 requires that (in the majority of prefectures) $G(\varepsilon_M^*) < G(\varepsilon_H^*)$. This follows from (3) and (7) plus the fact that $1 + \mu d\Delta/d\varepsilon^* > 0$.

9. This is a strong assumption, although one can think of arguments why μ , say, could be either higher or lower among minorities than majorities – the former may be more eager to fit in or more eager to preserve their identities. We do not pursue this issue any further, however. The main argument is measurement: since proxies for μ and the distributions of ε would be very hard to find in available data, theoretical predictions would be empirically empty.

The intuition is straightforward: on average, minority men do not only experience material benefits, but also intrinsic benefits and a higher social reputation of a child with their own ethnicity. Compared to Han men, more of them thus choose their own identity for their children. Because H, M mixed couples trade off material benefits against intrinsic and social reputation costs, they are more likely to cross the paternal ethnic cutoff.

The Effect of Material Benefits – Fact F2. Expressions (5) and (8) show how the two types of couples react to higher material benefits, b . These expressions reveal that higher b raises the probability of a child with the mother's ethnicity for Han-minority families, everything else equal, consistent with F2. By contrast, the model predicts an opposite pattern for minority-Han families. As the probability of picking the mother's ethnicity is already very low, this leaves little variation to exploit statistically.

Our model is thus consistent with facts F1 and F2. We now turn our interest to the new predictions of the model, which are those that we will test empirically.

3.3. Main Prediction

Our most important prediction concerns the interaction between individual (material) motives and social motives. We focus on the effects on Han-minority families and state the model predictions in two alternative ways.

Comparing High and Low Initial Cutpoints. What is the testable prediction of the effects of a change in material benefits, depending on the initial equilibrium? Expression (5) says that material benefits are crowded in by social reputation – a social multiplier $1/(1 + \mu d\Delta(\varepsilon_H^*(b, e, \mu))/d\varepsilon^*)$ larger than 1 – when few Han-minority couples break the social norm and their choices are strategic complements ($d\Delta(\varepsilon_H^*(b, e, \mu))/d\varepsilon^* < 0$). When many couples break the social norm and choices are strategic substitutes ($d\Delta(\varepsilon_H^*(b, e, \mu))/d\varepsilon^* > 0$), the benefits are instead crowded out – a social multiplier smaller than 1. This difference between crowding in with low shares of norm breakers, and crowding out with high shares of norm breakers, is the essence of our model.

But the effect in (5) of a change in benefits also includes the density $g(\varepsilon_H^*)$ at the initial cutpoint. When considering this channel, we impose the condition that the distribution of ε has a (weakly) positive skew.¹⁰ Specifically, we assume that the median ε_{50} (and the mean) of the distribution lies (weakly) to the right of the mode. Suppose that we compare two prefectures with cutpoints at percentiles equidistant from – and not too far from – the median, i.e., ε_{50+n}^* and ε_{50-n}^* . Because of the positive skew, we have $g(\varepsilon_{50-n}^*) \geq g(\varepsilon_{50+n}^*)$. The larger effects of material benefits due to the higher social multiplier at ε_{50-n}^* compared to ε_{50+n}^* are thus reinforced by a higher density.

10. This assumption can be weakened to say that the distribution of ε does not have too much negative skew.

We can now repeat this comparison for every other twin percentile cutpoints above and below the median. If the cutpoints in the prefectures we observe in the data are continuously distributed along the support of ε , we can conclude that the average effect of material benefits in regions with cutpoints ε_H^* below the median must be higher than the average effect in regions with cutpoints above the median.

Naturally, we do not observe the cutpoints ε_H^* in different prefectures directly, only the shares of Han-minority households who get minority children $G(\varepsilon_H^*)$. However, in the model the cutpoints and shares are one-to-one. Based on the argument above, we can therefore state:

PREDICTION P1. *Consider peer groups of Han-minority families who face the same increase in minority benefits within a province. Then, the effect on the share of minority children should be larger in peer groups with an existing share of minority children below a cutoff, compared to peer groups above that cutoff.*

In the data, we will evaluate Prediction P1 by difference in differences, comparing prefectures and cohorts above and below a cutoff share, near the median, of children with the mother's ethnicity, before and after the shift in policy.

Comparing Initial Cutpoints In Different Quartiles. Prediction P1 compares the effects of changing benefits above and below a cutoff share. Our discussion about the combined effect of a decreasing social multiplier and the density of a single-peaked distribution suggests that we *cannot* state P1 as a linear interaction between the initial share and the change in benefits. When the initial share of minority kids is in the left part of the distribution, an upward shift in ε^* has an ambiguous local effect due to the countervailing effects of a higher density and a lower social multiplier, whereas the effect becomes unambiguous for initial shares on the other side of the median.

We now illustrate this ambiguity and derive an alternative form of our main prediction. For this purpose, we consider the comparative statics at different segments of the share distribution $G(\varepsilon^*)$ observed in the data, corresponding to different quartiles of cutpoints ε^* . Let ε_q^* , $q = 1, 2, 3, 4$ denote cutpoints located in the middle of the quartiles of the ε distribution. The (weak) positive skew of the distribution implies that $g(\varepsilon_4^*) \leq g(\varepsilon_1^*)$ and $g(\varepsilon_4^*) \leq g(\varepsilon_3^*) \leq g(\varepsilon_2^*)$. Moreover, under the assumption that $d^2\Delta/d\varepsilon^{*2} > 0$, the first derivatives of the social multiplier are monotonically ordered as: $d\Delta(\varepsilon_1^*)/d\varepsilon^* < d\Delta(\varepsilon_2^*)/d\varepsilon^* < 0 < d\Delta(\varepsilon_3^*)/d\varepsilon^* < d\Delta(\varepsilon_4^*)/d\varepsilon^*$. Using these facts in (5), we obtain an alternative testable prediction:

PREDICTION P1'. *Suppose all peer groups in a province experience the same increase in benefits, due to a provincial policy. Then, the effect on the probability of Han-minority couples having minority children is (i) larger in the first, second and third quartile than in the fourth quartile of the share distribution, (ii) larger in the second than in the third quartile and (iii) ambiguous when we compare the first and second quartiles, or the first and third quartiles.*

The third part of P1' shows why we cannot use a simple interaction between the initial share and a policy indicator to test the theory. A cutoff in the first quartile is associated with a lower density but a higher social multiplier than a cutoff in the second quartile, and a higher social multiplier but a lower (or higher density) than a cutoff in the third quartile. Comparing the effects in the first quartile to those in the second or third quartile thus leads to ambiguous results, which differ from a (weakly) linear impact. We return to this point when discussing our empirical findings.

Auxiliary Prediction. Our paper focuses on the interaction between individual (material) motives and social motives, as stated in P1 and P1'. An additional testable prediction concerns the interaction effect of material benefits and intrinsic costs. Do the (average) intrinsic costs $e(H)$ of minority children modify the effect of material benefits b for Han-minority couples? Yes, as long as the share of minority children in the peer group is relatively small, material benefits have a smaller effect on the probability of minority children in Han-minority families when the intrinsic costs are high. This intuitive prediction may not be specific to our model; other models could also deliver a similar prediction. But a test of it does provide a sanity check on our setup, which we consider in Online Appendix D.

4. Measurement

This section discusses how to measure the variables and parameters in the model. We also provide more background information for each variable. Outcomes and some controls are measured at the individual level, whereas the individual and social motives are measured at the prefecture, residency, education-group, or ethnicity level.

Linking of Data and Samples. We draw on two sources of data: the 1-percent samples of the 1982, 1990 and 2000 censuses – and the 2005 population survey, also covering about 1 percent of the population. As in the model, we gauge the husband-wife-children structure of households. These can be identified by the head-spouse-child label in the censuses, and by the gender of the head and spouse. We can directly identify children in the 2000 and 2005 data. The 1982 and 1990 censuses do not distinguish between children and children-in-law. To identify children in these earlier data sets, we limit ourselves to unmarried children who still live with their parents. The results we report below are robust to using the 2000 census and the 2005 mini-census only. Below, we also consider composition effects by separating families with one child and multiple children.

Our data includes both two-generation and multiple-generation households. We always focus on the head-spouse-child structure. Specifically, if a household head lives with his parents and children, we measure the ethnicity of his children (rather

than his own ethnicity).¹¹ If a head lives with his children and grandchildren, we measure the ethnicity of his children (rather than his grandchildren). Later, we also conduct an analysis by restricting the data to two-generation households only.

After linking different datasets, our sample of mixed marriages has around 235,000 children born between 1970 and 2005. Some 125,000 of these come from Han-minority families and 110,000 from minority-Han families. We start from 1970 because a small share (13%) of the children in the linked data were born before 1970, and we need a representative cohort in the initial period to define the initial shares of children with their mother's ethnicity.¹²

We use administrative units defined by four-digit census codes and always call these prefectures. While all four waves provide information on prefectures, we can only access county-level (six-digit census code) information for 2000. Thus, we focus on prefecture-level norms in our baseline analysis and use county-level information (for 2000) in our supplementary analysis.

Ethnicity Outcomes (m in the Model). Censuses always report gender, birth year and ethnicity for each individual, which provides our measure of ethnicity outcomes. However, the data do *not* report household names, or locations at a finer level than prefectures (or counties). As shown by the summary statistics in Table 1, 47 percent of the children in Han-minority families have their mother's ethnicity, whereas only 6% do in minority-Han families. This is Fact F1 in the introduction. The low shares with the mother's ethnicity in minority-Han families is associated with little variation across time, as well as across space. This is consistent with our model predictions discussed in Section 3.2 and our focus on children in Han-minority families.

11. We make this choice for two reasons. First, the head-spouse-child structure is the most common in the data. Second, we cannot tell parents from parents-in-law, which makes it difficult to examine the ethnicity of household heads.

12. Including those born before 1970 does not alter the main results.

TABLE 1. Summary Statistics for Han-Minority Families

	(1) Mean	(2) s.d.	(3) #Obs.
a) Individual Characteristics			
Child with Mother's Ethnicity: All	0.465	0.499	124,940
Child with Mother's Ethnicity: Born before 1980	0.411	0.492	42,140
Child with Mother's Ethnicity: Born after 1980	0.492	0.5	82,800
Husband's education (1-4)	2.094	0.616	124,940
Wife's education (1-4)	1.878	0.654	124,940
Urban husband	0.261	0.439	59,278
Urban wife	0.261	0.422	59,278
b) Ethnic Benefits Measures (b)			
Post policy (rollout of family-planning)	0.581	0.493	121,908
Lagged extra Minority fertility (for women aged 40+)	0.059	0.129	107,903
Extra exam scores (relative to cutoff score for colleges)	0.014	0.015	124,938
c) Proxy for Intrinsic Costs (e)			
Son	0.531	0.499	124,940
Religious wife	0.185	0.388	124,940
d) Prefecture Characteristics			
Minority prob. of those born in 1970-74	0.388	0.31	319
Borderland	0.273	0.446	319
Minority pop. share in 1982	13.295	23.405	272
Share of pop. with high school or above in 1982	7.89	4.655	272
#children for a minority in 1982 (women aged 40+)	5.255	1.279	261

Notes: This table reports the summary statistics for the key variables in our analysis.

In the following analysis, we treat mixed marriages as given. It is possible that some regions are more open to mixed marriages, as well as to minority ethnicities for children. To take this into consideration, we always control for prefecture fixed effects and non-parametric provincial trends (province-by-year fixed effects) in our econometric specifications. In Section 5, we also discuss whether endogenous mixed marriages could explain our main results, and present empirical estimates suggesting they cannot.

Material Benefits (b in the Model). Since ethnic material benefits for minority children arise from a bundle of provincial regulations (recall Section 2), it is not straightforward to quantify the regional variation over time. To check that our results are robust, we therefore use three different measures. As mentioned above, these ethnic policies vary at the province level by law, which does not rule out variation at more local levels (e.g., across prefectures) due to different implementations of the same law. However, since we ask how ethnic policies shape people's choices, it is reasonable to use legislated provincial-level policies.

1. *Rollout of the one-child policy.* Family-planning policies favored minorities already in the 1960s. But these policies became more generous and more salient in

the 1980s, with the switch to a stricter one-child policy. However, minorities enjoy family-planning flexibility. By giving them minority status, parents can thus create an option for their children to have a second child.

To measure its rollout, we employ the timing for 27 provinces used in Edlund et al. (2013).¹³ As explained in that paper and earlier work on family planning (Peng 1996), the one-child policy is an umbrella for a raft of policies. Edlund et al. (2013) consider three programs: (i) family-planning science and technology-research institutes, (ii) family-planning education centers, and (iii) family-planning associations. Since all programs make the one-child policy salient, we set the first year any of them is legislated in a province as the first year of the one-child policy. These starting dates range from 1976 (in Jiangsu) to 1984 (in Guangxi).

This measure has the advantage of being staggered across provinces, but the disadvantage of a binary classification that cannot distinguish between potentially different material benefits across provinces. A related measure of the one-child policy is the fines for extra children used in Ebenstein (2010), which we employ as an alternative to the rollout of policies.

Nothing suggests that the rollout of family-planning institutions is related to the ethnic choices of children in mixed marriages. In the data, the p -value is 0.759 for the correlation between the year of adopting the instructions and the 1970–1974 cohort share of minority children in Han-minority families. We also check empirically for pre-trends.

2. *Extra fertility of minorities.* To capture the intensity of the family-planning policy and its variation over provinces and time, we calculate a second measure, namely the extra fertility for minorities (relative to Han) after the policy rollout. This measure is allowed to differ by province and by 5-year mother-birth cohort. Specifically, we gauge (close to) completed fertility based on the number of children of Han and minority women aged 40 and above. After the policy has been implemented, minority mothers do, on average, have 0.11 more children than Han mothers.

Moreover, the extra fertility measure varies widely across provinces. Panel (a) in Online Appendix B.1 presents a map of the ratio of the completed fertilities for minority and Han mothers born in 1955–59 (as of 1995–99). As shown, this ratio is higher in the western provinces.

When matching this measure with the ethnicity-choice data, we use the extra fertility in the previous 5-year cohort of mothers, which is less likely to suffer from endogeneity.

3. *Extra exam scores of minorities.* To proxy the variation in education benefits for minorities across provinces, we use the extra scores for minorities in the 2000 National

13. Beijing, Shanghai, Tianjin and Chongqing are not included. We thank Lena Edlund for providing this data.

College Entrance Exam.¹⁴ We normalize these extra scores by the cutoff score for four-year universities in a province.¹⁵ Different from the time-varying measures on fertility, this measure – which ranges from 0 to 6% – is only available for the cross section of provinces. Based on administrative data from 2000, a three-percent extra score is associated with a 2.3 percentage point increase in the college admission probability.¹⁶ When matching this measure with the ethnic-choice data, we assume it to be 0 before the introduction of the National College Entrance Exam in 1977, and use the cross-sectional measure for the entire period afterwards.

Panel (b) in Online Appendix B.1 presents a map of the extra-exam scores by province. Unlike the extra-fertility scores, the extra-exam scores matter more for southwestern provinces. At the cross-sectional level, the correlation between Measures 2 and 3 is low, with an insignificant correlational coefficient of 0.06. Thus, the two measures can be considered as two independent policy experiments to examine responses by mixed-marriage couples. Therefore, we look at the two separately rather than aggregating them into an index.

In sum, the one-child policy has a staggered introduction across provinces, but assumes that (once introduced) the material benefits to minorities are the same across provinces. The extra-college scores do instead have a cross-sectional variation after their (simultaneous) introduction, while the extra-fertility score has cross-sectional as well as time-series variation. We use all three measures of material benefits in our analysis. The number of observations varies slightly due to data availability.

Peer Groups for Social Motives (related to $d\Delta/\varepsilon^$ in the Model).* Following the discussion about crowding in or crowding out in Section 3 (the sign of $d\Delta/d\varepsilon^*$), we measure social motives by the shares of children with the mother's ethnicity in mixed marriages. To avoid the reflection problem discovered and discussed by Manski (1993), we want to treat the social motives for a particular cohort as predetermined by previous choices in the peer group.

Because we cannot observe the relevant peer group directly and our data reflect a population sample, we define the peer group relevant for the social motives in different ways in the hope of avoiding biased estimates. In line with the model, where people are influenced by others who make the same decisions, we use a choice-based definition of peer groups. In particular, we associate each Han-minority couple with a set of couples in a certain location who have the opportunity to choose the same ethnicities

14. We cannot access the data for a long period, but at least during 1999-2003, we find the extra scores for minorities to be very stable.

15. There are separate cutoffs for first-tier and second-tier universities in a province. We normalize the extra scores by the second-tier cutoff. The results are robust to using the first-tier cutoff (since the two cutoffs are highly correlated).

16. This calculation is based on how logged exam scores affect the probability of college admission: we find that a 1% increase in exam scores is associated with an increase of 0.76 percentage points in the college admission rate.

for their children.¹⁷ In addition, we also allow for a wider peer group by considering all families who can choose to follow the mother's ethnicity or not.

1. *The 1970–1974 cohort from Han-minority families in the same prefecture.* We exploit the variation across prefectures in the 1970–1974 birth cohort – i.e., in the initial cohort unambiguously before the start of the dramatic changes in ethnic policies. This treats the social motives as predetermined over the period of changing policies (and also allows us to examine the dynamic impacts of social motives over time).

2. *The 1970–1974 cohort in the same prefecture from Han-minority families, subdivided by residence, education, or wife's ethnicity.* The measure in 1. only uses the husband's ethnicity, the wife's minority status, and prefecture to define the peer group. But we also consider a number of finer peer groups. A. The first group also conditions on urban or rural residence and defines the peer group at the prefecture-ethnicity-cohort-residency level. Specifically, we distinguish between urban and rural based on the husband's *Hukou* (legal residence). Definition 2.A implies smaller groups in each cell than definition 1. by disaggregation and by the fact that we rely on rural/urban information in the 2000 and 2005 censuses.¹⁸ B. Peer groups may plausibly be formed by people with similar levels of education. Our second refinement thus conditions on father's education: those who have completed high-school or above. This way, we define the peer group at the prefecture-ethnicity-cohort-education level. Since material benefits (especially education benefits) can differ by education groups, this definition also helps us check heterogeneity by education levels. C. The relevant peer group for a Han man and a minority woman may be limited to other couples, where the wife has the same minority ethnicity. We consider this possibility as well, by defining the peer group at the prefecture-cohort-(female)ethnicity level.

3. *The 1970–1974 cohort from Han-minority families in the same county.* Instead of prefectures, we also use a narrower definition. But we can only access the county information for 2000. Thus, this exercise can be taken a check to show that our finding holds when using only one census.

4. *All mixed couples in the 1970–1974 cohort in the same prefecture are able to make an ethnic choice.* All peer group definitions under 2. are refinements of definition 1. To check for robustness, we also consider a broader peer group, namely the share of children with their mother's ethnicity, among both Han-minority couples and minority-Han couples.

17. The prospective econometric problems of estimating the influence of unobserved peer groups in a sample from the population appear related to the biases due to measurement error when estimating peer effects for members of partially sampled networks (Chandrasekhar and Ellis, 2011).

18. Rural/urban information was asked for in the 1990 but not in the 1982 census, which makes it absent from the merged 1982–90 data by IPUMS. It is possible to identify it based on separate information for 1990. We choose not to do so to keep consistency with the IPUMS merged data. This also serves as a check on whether our findings hold with the 2000 and 2005 censuses only.

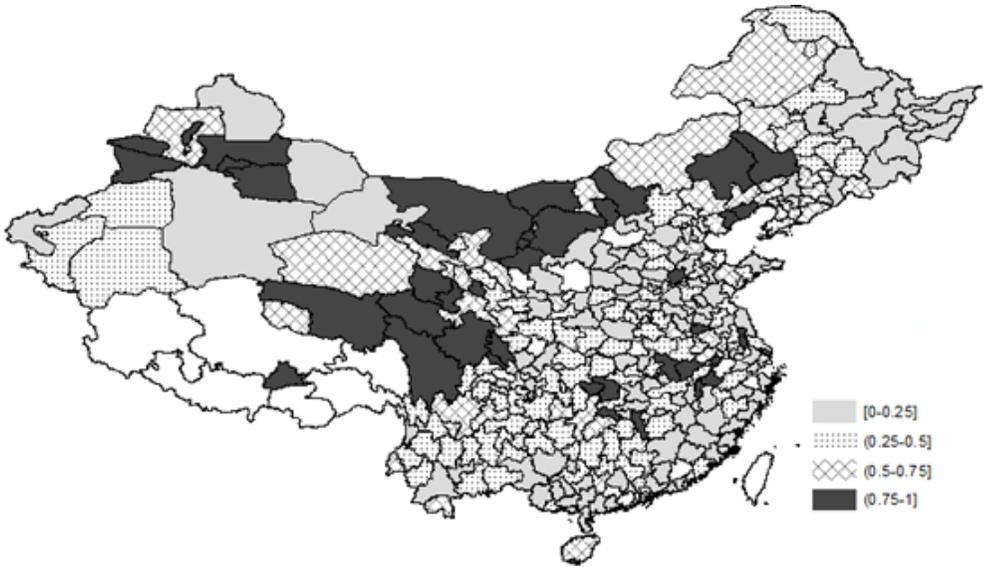


FIGURE 4. Spatial variation in the share of children with mother’s ethnicity in Han-minority marriages (for those born in 1970–1974). This figure maps the average probability of children with their mother’s ethnicity born during 1970–1974 in Han-minority families across prefectures.

- a. A set of province fixed effects explains about 36% of the variation across prefectures. Our empirical analysis exploits only within-province variation.
- b. We also control for province-by-year fixed effects to allow for flexible (non-parametric) time trends across provinces.

Pre-policy Variation in the Share of Children with their Mother’s Ethnicity ($G(\varepsilon^)$ in the Model).* As already discussed, Figure 2 shows a great deal of variation across prefectures for Han-minority mixed families. In terms of the model, this dispersion reflects the joint distribution of parameters b , $e(H)$, μ leading to different cutoffs ε^* and the mapping from these cutoffs into shares via distribution G . By contrast, for minority-Han mixed families, most prefectures are concentrated at the left end, leaving little variation across prefectures. We therefore focus on the social motives for Han-minority families.

Figure 4 maps the spatial distribution across China of ethnicity choices by Han-minority couples (in the 1970–1974 cohort). It suggests that the social motives vary considerably across prefectures, and that this variation is not strongly geographically clustered. For instance, province fixed effects only explain about a third of the variation across prefectures.

For Han-minority families, our model predicts a strategic complementarity $d\Delta/d\varepsilon^* < 0$ for low values of cutoff ε^* (low norm-breaking shares) and a strategic substitutability $d\Delta/d\varepsilon^* > 0$ for high values of ε^* (high norm-breaking shares). If the distribution of ε were symmetric, the sign would flip at a critical cutoff of $\varepsilon_{50}^* = 0$, corresponding to a share of minority kids at 0.5. We also allow for a non-symmetric distributions in two ways. First, we check how the estimates behave as we vary the

assumption about the critical cutoff when testing Prediction P1. Second, we look at the estimates in different quartiles, when testing Prediction P1'.

Other Prefecture and Individual Characteristics. We control for a set of prefecture and individual variables. At the prefecture level, we include characteristics that might affect ethnic choices: whether a prefecture is a borderland, its minority population share in the 1982 census, its population share with high-school education or higher in the 1982 census, as well as the number of children for minority women (aged 40 and above) in the 1982 census. The direct level effect of these characteristics is absorbed by prefecture fixed effects. To rule out that the identifying variation in the (pre-policy) share of children with their mother's ethnicity is systematically related to the change in ethnic policies, we allow the prefecture controls to have different impacts before and after the introduction of the ethnic policies (interacting them with our measures of ethnic benefits).

We present correlations between these characteristics and our baseline measure of social motives (pre-policy share of minority children in Han-minority families) in Online Appendix B.2. It is worthwhile pointing out that the correlation between our measure of social motives and the share of minority population is weakly positive. This correlation rejects a "scarcity" effect, whereby children are less likely to be a minority in regions with a higher minority population share because a more or less fixed set of material benefits becomes diluted by many takers.

At the individual level, we include education-level fixed effects and 5-year birth-cohort fixed effects for both father and mother. We unify the categorical education levels across censuses into four groups: 1=less than completed primary school, 2=completed primary school, 3=completed secondary (high) school, and 4=some college education or more. As shown in Table 1, the average husband has more education than the average wife. To rule out that our estimated effects are driven by omitted individual variables, we allow the impacts of these individual characteristics on ethnic choices to differ before and after any policy shifts.

Migration. The variation across prefectures and provinces discussed in this section is based on residency at census time. However, migration may make residency different than birth place. Only the 2000 census includes information on whether an individual's birth place and current residency coincide (the 1982 and 1990 censuses spell out if people lived in the same county five years ago, while the 2005 mini-census only has that information one year ago). Based on the 2000 census, over 85% of individuals were born in the same county as their current residence, while 94% were born in the same province. Given that prefecture is the administrative level in between county and province, these facts suggest that migration is unlikely to make a major difference for our main results. Moreover, Frijters, Gregory and Meng (2013) document that rural-urban migration did not take off until 1997. Nevertheless, we conduct robustness checks by omitting the (most recent) 2005 census from the sample, and by excluding individuals whose birth and residence counties are different. This should minimize the potential impact of migration. In addition, we show that the interaction of material

benefits and social motives (our main effect) has no impact on migration outcomes in the 2000 census.

5. Empirical Evidence

The most important new predictions from our model are P1 (and P1') on the interactions between individual and social motives. To the best of our knowledge, no similar prediction has been studied in the existing literature. This section confronts these predictions with data.

5.1. Testing P1 and P1'

Our model of the interactions between individual and social motives predicts a larger effect of higher material benefits in peer groups where the initial share of breaking the social norm (with the mother's ethnicity) is smaller. This is because individual motives driven by material benefits are crowded in rather than crowded out by prevailing social motives. Empirically, Prediction P1 relies on a comparison of the effects above and below a cutoff share.

Main Specification. To test Prediction P1, we ask whether β_b is positive in the difference-in-differences specification:

$$CME_{i,p,t} = \beta_b I(\leq V)_p \times b_{r,t} + b_{r,t} + birthyear_t + pref_p + ethn_g + \gamma \mathbf{X}_{i,p} + \gamma' \mathbf{X}_{i,p} \times b_{r,t} + prov_r \times year_t + \varepsilon_{i,p,t}, \quad (9)$$

where the dependent variable $CME_{i,p,t}$ is a binary indicator for child i – with a Han father and a minority mother of ethnic group g , in prefecture p (belonging to province r), and birth year t – having the mother's ethnicity.

The material benefits from ethnic policy $b_{r,t}$ are measured in the three ways discussed in Section 4. Thus, $b_{r,t}(\text{Post Policy})$ is a dummy for whether province r has implemented the one-child policy, $b_{r,t}(\text{Extra Fertility})$ is the extra fertility for minorities in the province post the one-child policy, and $b_{r,t}(\text{Extra Scores})$ is the extra score for minorities in the provincial national college-entrance exam.

$I(\leq V)_p$ is an indicator for whether the peer group – by Definition 1 in Section 4, i.e., Han-minority couples with children in the 1970–1974 birth cohort in the same prefecture – has a minority-children share below some critical value V between 0 and 1. This value corresponds to the theoretical borderline between crowding in and crowding out. Thus, the parameter of interest β_b measures the interaction between material benefits and social reputations: the difference in the effect of material benefits in prefectures below and above the cutoff.

To allow time-invariant, or slowly changing, prefecture characteristics – such as attitudes to mixed marriages – to influence ethnicity choices, we include prefecture fixed effects ($pref_p$). To hold constant factors that affect China-wide ethnicity choices

by different cohorts (like the average effects of post-policy material benefits), we include birth-year fixed effects ($birthyear_t$). To control for time-invariant or slowly changing ethnicity-specific factors, we include (a set of 55) ethnicity fixed effects ($ethn_g$). For example, some minority groups may have a stronger preference that the child maintains the father's ethnicity. As we focus on the children of Han-minority couples, these fixed effects refer to the wife's particular minority ethnicity.

$\mathbf{X}_{i,p}$ is the set of individual and prefecture characteristics presented in Section 4 and we include $\mathbf{X}_{i,p} \times b_{r,t}$ to allow their impacts on individual behavior to change with the material benefits.

Finally, we include province-by-calendar-year non-parametric trends ($prov_r \times year_t$) to control for different evolutions across provinces, such as the direct effects of different provincial policies, or different evolutions of discrimination against minorities. We cluster the standard errors at the prefecture level and present those clustered at the province level as a robustness check.

Baseline Results. We start with a cutoff share $V = 0.5$ and $b_{r,t}(\text{Post Policy})$ as the material benefits in Table 2A. Columns (1)-(2) of the table only include prefecture fixed effects. Column (1) shows that the average effect of $b_{r,t}(\text{Post Policy})$ is around 0.078 – i.e., an additional 7.8 percentage points of Han-Minority couples choose to have a minority child after the introduction of the one-child policy, which is around 19% of the pre-policy mean (41 percentage points). Column (2) presents the interaction effect of interest, on $I(\leq 0.5)_p \times b_{r,t}(\text{Post Policy})$, which shows that the effect of material incentives is indeed significantly larger when the share of children following the mother's ethnicity is smaller than 0.5. The estimated interaction effect is quantitatively large, at least on the order of the average effect in column (1). This is consistent with Prediction P1 which benefits from having a larger effect in peer groups where few mixed households break the social norm, as this leads to a strategic complementarity (and a social multiplier above 1) rather than a strategic substitutability (and a social multiplier below 1). For example, given the estimates in column (2), the average effect of the introduction of the one-child policy is around 10 percentage points below the 0.5 cutoff and 3 percentage points above the cutoff.¹⁹

19. Instead of examining an interaction effect, one can also evaluate the effect of $Post_{r,t}$ in separate samples with prefectures below and above the cutoff (the difference between the specifications is from which samples the fixed effects are estimated). The results are very similar.

TABLE 2A. Baseline Results for P1: The Impact of Material Benefits and Social Motives on the Prob. of Mixed-marriage Children with Mother's Ethnicity
 Results using pre-post family planning policy to proxy material benefits b
 Dependent Variable: Child with Mother's Ethnicity=0/1 (mean: 0.47)

	(1)	(2)	(3)	(4)	(5)	(6)
I(≤ 0.5) \times b(Post Policy)		0.072*** (0.019) [0.030]	0.069*** (0.019) [0.031]	0.071*** (0.020) [0.031]	0.074*** (0.022) [0.027]	0.070*** (0.014) [0.024]
b(Post Policy)	0.078*** (0.011) [0.019]	0.031** (0.014) [0.014]	0.035** (0.013) [0.013]			
Prefecture FE	Y	Y	Y	Y	Y	Y
Wife Ethnicity FE			Y	Y	Y	Y
Birth Year FE				Y	Y	Y
Controls*b					Y	Y
Province FE*Year FE						Y
Observations	121,908	121,908	121,908	121,908	108,914	108,914
R-squared	0.276	0.277	0.290	0.292	0.299	0.334

Notes: This table shows the results using the provincial roll-out of family-planning policies to measure material benefits. The data comes from three censuses and a mini census during 1982-2005.

a. The cutoff (0.5) is defined by the share of minority children in Han-minority families in the 1970–1974 birth cohort.

b. Controls include couples' characteristics (education-level fixed effects and 5-year birth-cohort fixed effects, for both husband and wife) and prefecture characteristics (listed in panel (d) of Table 1).

c. Standard errors in parentheses are clustered at the prefecture level, while those in brackets are clustered at the province level. Significance: ***, 1%, **, 5%, *, 10%.

TABLE 2B. Baseline Results for P1: The Impact of Material Benefits and Social Motives on the Prob. of Mixed-marriage Children with Mother’s Ethnicity
 Results using additional fertility and additional exam scores for minorities to proxy material benefits b
 Dependent Variable: Child with Mother’s Ethnicity=0/1 (mean: 0.47)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
I(≤ 0.5) \times b (Extra Fertility)		0.027*** (0.009)	0.026*** (0.009)				0.022** (0.009)
b (Extra Fertility)	0.034*** (0.005)						
I(≤ 0.5) \times b (Extra Scores)					0.052*** (0.009)	0.034*** (0.009)	0.022*** (0.008)
b (Extra Scores)				0.044*** (0.007)			
Prefecture FE	Y	Y	Y	Y	Y	Y	Y
Wife Ethnicity FE	Y	Y	Y	Y	Y	Y	Y
Birth Year FE		Y	Y		Y	Y	Y
Controls*b			Y			Y	Y
Province FE*Year FE			Y			Y	Y
Observations	107,903	107,903	96,874	124,938	124,938	111,944	96,873
R-squared	0.277	0.295	0.341	0.269	0.287	0.329	0.342

Notes: This table reports the results using two proxies for individual material incentives: extra fertility for minority in the previous birth cohort and extra scores for minority (relative to the provincial college acceptance cutoffs) in the college entrance exam. Both proxies are standardized.

a. The cutoff (0.5) is defined by the share of minority children in Han-minority families in the 1970–1974 birth cohort.

b. Controls include couples’ characteristics (education-level fixed effects and 5-year birth-cohort fixed effects, for both husband and wife) and prefecture characteristics (listed in panel (d) of Table 1).

c. Standard errors in parentheses are clustered at the prefecture level. Significance: ***, 1%, **, 5%, *, 10%.

Column (3) adds wife-ethnicity fixed effects. Column (4) further includes birth-year fixed effects – as 82% of the variation in the policy measure are absorbed by these birth-year fixed effects, the coefficient on $b_{r,t}(\text{Post Policy})$ is omitted from the results (but still appears in the regression). These columns both display an estimate of β_b similar to that in column (2). Column (5) shows that the column (2) results are little affected by including prefecture and individual characteristics and their interactions with $b_{r,t}(\text{Post Policy})$. Column (6) further shows that the pattern is robust to non-parametric provincial trends (province-by-calendar-year fixed effects).

The standard errors in parentheses are clustered at the prefecture level and those in brackets are clustered at the province level. The estimate of β_b is significantly different from zero, regardless of the levels of clustering. As this is also true for all results to follow, we only show the results for prefecture-level clustering in the subsequent tables.

Alternative Policy Measures. In Table 2B, we employ $b_{r,t}(\text{Extra Fertility})$ and $b_{r,t}(\text{Extra Scores})$ as measures of material benefits. We limit ourselves to three specifications for each policy measure, namely those in columns (3), (4) and (6) of Table 2B. To facilitate the comparison, we present the impacts of a one-standard-deviation (1σ) increase of the measures. As shown in column (1), a 1σ extra fertility is associated with a 3.4 percentage point increase in the probability of having a child with the mother's ethnicity. By columns (2)-(3), the difference in impact below and above the cutoff is in the order of the average effect. Columns (5)-(6) present the results for $b_{r,t}(\text{Extra Scores})$, which exhibit a similar pattern as extra fertility. Column (7) presents a specification which includes both these policy measures, and shows that their impacts are comparable. As mentioned in Section 4, the variations in the two measures are only weakly correlated, so they can be considered as largely independent interventions.

In addition, another related policy measure in the existing literature is the amounts of penalties associated with additional children (Ebenstein 2010). Replacing our policy rollout measure with the penalties, we obtain a similar pattern (presented in Online Appendix C.1).

Alternative Cutoffs. We further examine the impacts of b for different cutoffs. For the same specification as column (6) of Table 2A, we visualize the corresponding interaction estimates and their 95% confidence intervals for all cutoffs V between 0.1 and 0.9 (see Online Appendix C.2). Each estimate represents the different effects of $b_{r,t}$ on individuals in prefectures below and above cutoff V . For $b_{r,t}(\text{Post Policy})$ and $b_{r,t}(\text{Extra Fertility})$, the positive impact is significant for all cutoff values from 0.3 and upwards; for $b_{r,t}(\text{Extra Scores})$, it is significant for all cutoff values from 0.4 and upwards. As discussed next, a lower point estimate at the lowest cutoffs is consistent with the model.

Quartile Results (P1'). In Section 3, Prediction P1' stated the predicted effects of interacting individual and social motives in different quartiles. To test this prediction,

we replace $I(\leq 0.5)_p$ in (9) with three indicators for the early 1970s cohort share of minority children in quartiles: $I(0-0.25)_p$, $I(0.25-0.50)_p$, and $I(0.50-0.75)_p$ thus leaving the fourth quartile as the reference group.

Prediction P1' is confirmed by the results in Table 3, again for the peer group of all Han-Minority couples in the same prefecture with children in the 1970–1974 cohort. Columns (1)–(3) present the estimates when the three quartile indicators are interacted with $b_{r,t}(\text{Post Policy})$: column (1) includes only prefecture fixed effects; column (2) adds ethnic fixed effects (for the minority wife); column (3) further adds birth-year fixed effects, the interaction effects of prefecture and individual characteristics and province-by-calendar-year fixed effects. Columns (4)–(9) present analogous results using $b_{r,t}(\text{Extra Fertility})$ and $b_{r,t}(\text{Extra Scores})$ to measure the material benefits. Consistent with Prediction P1', the effect of material benefits is significantly larger in the first, second and third quartile compared to the fourth quartile. Also consistent with the prediction, the point estimates for the second quartile are indeed significantly higher than those for the third quartile (a p -value smaller than 0.05 in all specifications with all controls).

TABLE 3. Baseline Results for P1': The Interaction Effects by Quartiles on the Prob. of Mixed-marriage Children with Mother's Ethnicity
 Dependent Variable: Child with Mother's Ethnicity=0/1 (mean: 0.47)

Proxy for benefits b	(1) Rollout of Family Planning Post Policy =0/1	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
				Extra Fertility for Minority (lagged cohort)			Extra Exam Scores for Minority (share of the cutoff score)		
I(0-0.25)×b	0.097*** (0.024)	0.092*** (0.023)	0.094*** (0.029)	0.030** (0.014)	0.033** (0.013)	0.031*** (0.011)	0.052*** (0.016)	0.047*** (0.016)	0.040** (0.016)
I(0.25-0.5)×b	0.145*** (0.027)	0.143*** (0.026)	0.119*** (0.027)	0.043** (0.017)	0.043** (0.017)	0.032*** (0.009)	0.077*** (0.015)	0.075*** (0.016)	0.063*** (0.014)
I(0.25-0.75)×b	0.079*** (0.025)	0.078*** (0.024)	0.052* (0.027)	0.031*** (0.009)	0.032*** (0.009)	0.009 (0.010)	0.030 (0.018)	0.028 (0.019)	0.029* (0.016)
b	-0.020 (0.018)	-0.017 (0.016)	-0.242*** (0.075)	0.007 (0.005)	0.007 (0.005)	-0.023 (0.032)	-0.014 (0.013)	-0.011 (0.013)	-0.174*** (0.045)
Prefecture FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Wife Ethnicity FE		Y	Y		Y	Y		Y	Y
Birth Year FE			Y			Y			Y
Controls*b			Y			Y			Y
Province FE*Year FE			Y			Y			Y
p-value (2nd vs. 3rd)	0.014	0.020	0.000	0.514	0.219	0.048	0.003	0.000	0.0003
p-value (1st vs. 2nd)	0.064	0.080	0.132	0.523	0.720	0.987	0.040	0.005	0.011
Observations	121,908	121,908	108,914	107,903	107,903	96,874	124,938	124,938	111,944
R-squared	0.278	0.291	0.334	0.277	0.291	0.341	0.270	0.283	0.329

Notes: According to Prediction P1', the interaction effects of material benefits and social motives estimated for the first three quartiles should be larger than that for the fourth quartile. Further, the effect for the second quartile should be larger than that for the third quartile.

a. Controls include couples' characteristics (education-level fixed effects and 5-year birth-cohort fixed effects, for both husband and wife) and prefecture characteristics (listed in panel (d) of Table 1).

b. Standard errors in parentheses are clustered at the prefecture level. Significance: ***, 1%, **, 5%, *, 10%.

As in the test of Prediction P1, these effects are large: the difference in the effects of higher material benefits, say, in the first vs. the fourth quartile, is in the order of the average effect estimated in Table 2A. This corresponds to the theoretical prediction of a social multiplier above 1 in the first quartile – due to crowding in – and a social multiplier below 1 in the fourth quartile – due to crowding out. Another indication of a substantial variation in the social multiplier is that the empirically estimated effect in the first quartile is everywhere larger than that in the third quartile. As stated in Prediction P1', this relative effect is theoretically ambiguous, as the social multiplier is higher but the density is lower when we compare the first and the third quartile.²⁰ Empirically, we find that the first quartile effect is weakly smaller than the second quartile effect (with p -values reported in Table 3). This finding rejects the alternative hypothesis that the effect is (weakly) decreasing in the share measure and provides further support for the theoretical prediction of a non-linear effect.

5.2. Robustness

This subsection asks if our baseline results are robust to a pre-trends analysis, potential mis-measurement due to migration or multiple-generation households, and endogenous mixed marriages.

Dynamic Impacts and Pre-trends. Our baseline specification highlights the average effect of material benefits. A more flexible way of examining the impact of $b_{r,t}$ (Post Policy) is to allow the effect to vary by birth cohort:

$$CME_{i,p,t} = \sum_{\tau} \beta_{b,\tau} Cohort_{r,\tau} \times I(\leq 0.5)_p + \sum_{\tau} Cohort_{r,\tau} + birthyear_t + pref_p + ethn_g + \gamma \mathbf{X}_{i,p} + \gamma' \mathbf{X}_{i,p} \times Post_{r,t} + prov_r \times year_t + \varepsilon_{i,p,t}. \quad (10)$$

Here, the birth cohort 1 year before the family-planning policy (i.e., $\tau = -1$) is the reference group and τ refers to different periods relative to the policy year. In particular, we consider 1 year, 2 years, 3 years, 4 years, 5-10 years, more than 10 years before and after the policy implementation, as well as the policy year.

If the estimates of $\beta_{b,\tau < -1}$ are different from zero, prefectures with different social motives were different already before the introduction of ethnic policies. In effect, the estimates of $\beta_{b,\tau < -1}$ are insignificant, indicating that pre-trends are not critical. The estimation results are presented in Table 4 and visualized in Figure 5 (where the bars indicate 95% confidence intervals for standard errors clustered at the prefecture level).

We find that the interaction between individual and social motives becomes significantly positive only after the policy shift. Moreover, the size of this interaction effect grows over time. These results are consistent with a dynamic extension of the

20. Moreover, Figure 2 suggests that the empirical density of the equilibrium prefectural cutoffs is skewed with a relatively high density in the first quartile,

TABLE 4. Checking Pre-trends
 Dependent Variable: Child with Mother's Ethnicity=0/1 (mean: 0.47)

	(1)	(2)	(3)
I(≤0.5)×10 years+ pre policy	-0.037* (0.020)	-0.028 (0.020)	-0.027 (0.018)
I(≤0.5)×5-10 years pre policy	-0.029* (0.017)	-0.018 (0.018)	-0.013 (0.019)
I(≤0.5)×4 years pre policy	-0.006 (0.017)	0.010 (0.017)	0.009 (0.018)
I(≤0.5)×3 years pre policy	-0.004 (0.020)	0.009 (0.020)	-0.002 (0.020)
I(≤0.5)×2 years pre policy	-0.031* (0.018)	-0.023 (0.019)	-0.018 (0.017)
I(≤0.5)×policy year	0.031* (0.019)	0.043** (0.020)	0.041** (0.020)
I(≤0.5)×1 year post policy	0.009 (0.018)	0.033* (0.018)	0.033 (0.020)
I(≤0.5)×2 years post policy	0.011 (0.020)	0.027 (0.020)	0.035 (0.022)
I(≤0.5)×3 years post policy	0.020 (0.020)	0.028 (0.020)	0.029 (0.020)
I(≤0.5)×4 years post policy	0.039* (0.024)	0.048** (0.022)	0.049** (0.023)
I(≤0.5)×5-10 years post policy	0.054** (0.023)	0.068*** (0.022)	0.069*** (0.019)
I(≤0.5)×10+ years post policy	0.091*** (0.029)	0.105*** (0.028)	0.100*** (0.023)
Prefecture FE	Y	Y	Y
Wife Ethnicity FE	Y	Y	Y
Birth Year FE		Y	Y
Controls*Post Policy			Y
Province FE*Year FE			Y
Observations	121,908	121,908	108,914
R-squared	0.286	0.287	0.323

Notes: This table shows that there are no similar positive effects before the policy, and the effect is increasing over time, using 1 year before the introduction of the policy as the reference group.

a. Controls include couples' characteristics (education-level fixed effects and 5-year birth-cohort fixed effects, for both husband and wife) and prefecture characteristics (listed in panel (d) of Table 1).

b. Standard errors in parentheses are clustered at the prefecture level. Significance: ***, 1%, **, 5%, *, 10%.

model presented in Online Appendix C.3. Specifically, if the social motives of each cohort are tied to the behavior of the previous cohort, equation (3) still defines a steady state value for ε_H^* . However, the equilibrium adjusts towards the new steady state according to the non-linear difference equation:

$$b - e(H) - \varepsilon_{H,t}^* = \mu \Delta(\varepsilon_{H,t-1}^*). \tag{11}$$

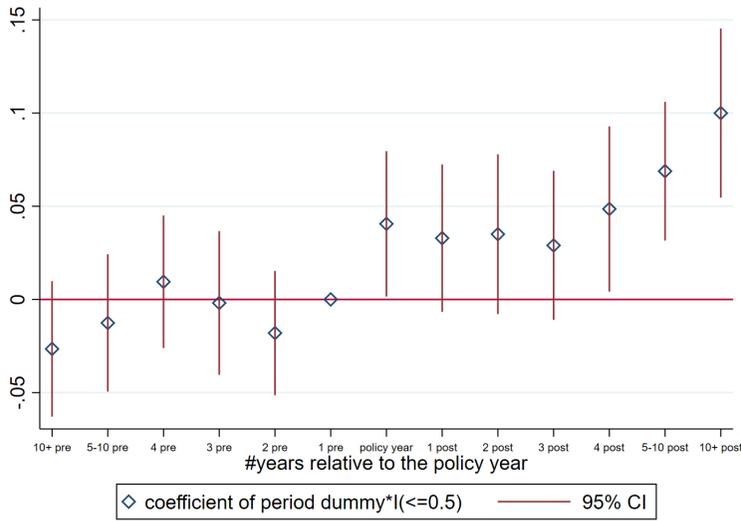


FIGURE 5. The dynamic effects of material benefits (b) social motives on the probability of mixed-marriage children with their mother’s ethnicity. These figures plot the dynamic impacts of material benefits (measured by the rollout of family-planning policy) interacting with social motives (measured by the 1970–1974 share of children with mother’s ethnicity falling below a 0.5 cutoff).

- a. The reference period is 1 year before the introduction of the policy. The diamonds indicate the estimates in column (3) in Table 4 and the bars indicate 95% confidence intervals.
- b. The figure shows that (1) there are no similar positive effects before the policy, and (2) the effect is increasing over time, which is consistent with the dynamic extension of the model in Online Appendix C.3.

In this dynamic setting, the dynamic adjustment – impulse response – to the same b shock predicts that the difference between peer groups with low and high initial shares should go up over time, as it does in the data.

Migration. To deal with mis-measurement due to migration, we re-estimate the baseline results, dropping all post-2000 data as well as individuals whose birth and residency counties are different in the 2000 census. The results, presented in columns (1)–(6) in the table of Online Appendix C.4, entail estimates similar to those in Table 2A. Columns (7)–(9) of the same table show that $I(\leq 0.5)_p \times b_{r,t}$ has no significant impact on being a migrant in the 2000 census. These results suggest that our main finding is unlikely to be driven by migration per se.

Multiple Generations. While most individuals we study live in two-generation households, we include those living in multiple-generation households for completeness. This way of data construction does not affect our finding. As shown in Online Appendix C.5, our results are similar if we restrict the data to those in two-generation households only (column (1)–(3)). In addition, we can limit our data

to those born during 1970 and 1990 whose household structure are relatively more similar, where we obtain a similar pattern (columns (4)-(6)).

Endogenous Mixed Marriages. An important concern about our analysis is that Han-minority marriages may be simultaneous to the ethnic choice of children. If unobserved factors drive the incidence of mixed marriages as well as the ethnicity choices in such marriages, our findings in Tables 2A and 2B may just proxy for those omitted drivers rather than capture an interaction between individual and social motives. However, an increase in Han-minority mixed couples can only explain a higher share of children with their mother's ethnicity, if the additional mixed couples are more likely to break the social norm. Our analysis provides a specific answer as to why couples who were married after the ethnic policies do so. But in a broader context, mixed marriages are certainly endogenous. In fact, we are currently doing additional research on the incidence of mixed marriages. While we leave the question about the drivers of mixed marriages to an accompanying paper, it is important to examine whether these marriages can explain our main findings.

The sharpest way of dealing with this issue would be to examine choices within the same family, before and after the implementation of ethnic policies. At the aggregate level, the share of mixed couples (with more than one child) with children of different ethnicities goes up from 3.6% to 4.5% after the implementation of the one-child policy. To conduct an individual-level within-family analysis, however, we must limit the sample to Han-minority families with some children born before and some children born after the policy was implemented. Since this restriction preserves only 9% of the mixed couples in our sample, we do not have enough power for a within-family analysis at the prefecture level – family fixed effects explain over 97% of the variation in children's ethnicity.

A less satisfactory approach, which still provides a useful check, is to consider the subsample of couples who were married before the introduction of ethnic policies. The marriage decisions of these couples are very unlikely affected by the ethnic policies. The restriction to early marriages, plus the fact that marriage-year information is available only in the 2000 and 2005 censuses, considerably cuts the sample from that in our baseline estimates.

TABLE 5. Considering the Effects of Endogenous Marriages
 Dependent Variable: Child with Mother's Ethnicity=0/1 (mean: 0.47)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Marriage Before the Policy						All		
I(≤ 0.5) \times b(Post Policy)		0.034* (0.020)					0.065*** (0.015)		
b(Post Policy)	0.022*** (0.008)								
I(≤ 0.5) \times b(Extra Fertility)				0.013 (0.018)				0.023*** (0.008)	
b(Extra Fertility)			0.013** (0.007)						
I(≤ 0.5) \times b(Extra Scores)						0.028* (0.015)			0.029** (0.011)
b(Extra Scores)					0.012** (0.006)				
I(≤ 0.5) \times HM Marriage Share							0.003 (0.003)	0.002 (0.003)	0.004 (0.003)
Prefecture FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Wife Ethnicity FE		Y		Y		Y	Y	Y	Y
Birth Year FE		Y		Y		Y	Y	Y	Y
Controls*b		Y		Y		Y	Y	Y	Y
Prov. FE*Year FE		Y		Y		Y	Y	Y	Y
Observations	18,814	15,584	18,583	15,397	21,206	17,976	108,914	96,874	111,944
R-squared	0.304	0.319	0.304	0.320	0.285	0.300	0.334	0.341	0.329

Notes: Columns (1)-(6) present estimation results in a subsample of only those couples married before the policy. The results show that the baseline pattern is robust to the concern of endogenous marriages.

a. Controls include couples' characteristics (education-level fixed effects and 5-year birth-cohort fixed effects, for both husband and wife) and prefecture characteristics (listed in panel (d) of Table 1).

b. Standard errors in parentheses are clustered at the prefecture level. Significance: ***, 1%, **, 5%, *, 10%.

Estimation results for this smaller sample are presented in columns (1)-(6) of Table 5 for our three measures of material benefits. Since we exclude all couples who were married after the policy was implemented, most children in the sample were born before 1985. The resulting post-policy period is thus very short, which explains why the average effects of higher material benefits in columns (1), (3) and (5) are smaller than in the full sample – recall the dynamic pattern in Figure 5. However, the interaction effects with the social motive in columns (2), (4) and (6) are positive and similar in magnitude to the average effect, precisely as our baseline estimates in Table 2A. The magnitude of $b_{r,t}(\text{Post Policy})$ is also similar to the results in Table 4 for the interaction effect 1-3 years after the policy.

Another way of dealing with the concern of endogenous mixed marriages is to re-estimate our baseline specification in Table 2A, but add the mixed-marriage share and its interaction with the share indicator $I(\leq 0.5)_p$ in the same prefecture. Columns (7)-(9) of Table 5 show that this does only very marginally alter the estimates of the central interaction effects in Tables 2A and 2B. The bottom line is thus that our baseline pattern on the interactions between individual and social motives appears to hold up in the wake of endogenous mixed marriages.

5.3. Alternative Peer Groups

Peer groups play a key role in our model. The empirical estimates we have shown so far rely on the assumption that an earlier cohort of Han-minority couples in the same prefecture makes up the relevant peer group for ethnicity decisions. As peer groups are not observable, it is important to consider alternatives. In particular, our definition may be too wide if a certain Han-minority couple is more influenced by other such couples who live under similar conditions, have the same education, or a wife from exactly the same minority ethnicity. Below, we consider these three possibilities.

One may also argue that our definition is too narrow. Would Han-minority couples also be influenced by minority-Han and minority-minority couples who can choose to break the norm? We also consider this possibility.

Narrower Peer Groups. Panel (a) of Table 6 presents separate results for rural-resident and urban-resident members of the same ethnicity-prefecture-cohort (peer-group definition 2A in Section 4). Although based on a considerably smaller sample, the estimates of the interaction between individual and social motives deliver a similar message as the prefecture-cohort-level results in Table 2A and Table 2B. The effects are generally larger for urban residents, consistent with the fact that family-planning policies are more strictly enforced in cities. These results also show that our main finding in Tables 2A and 2B is unlikely to be driven by different perceived values of ethnic benefits (b in the model). Another possibility is that rural and urban residents put a different weight on social reputations (μ in the model). We do not attempt to empirically disentangle the impacts of μ and b .

Another more narrow peer-group definition subdivides each mixed-couples cohort by educational background. In particular, we split the sample (according to definition

TABLE 6. Results Using Alternative Peer Groups
 Dependent Variable: Child with Mother's Ethnicity=0/1

	(1)	(2)	(3)	(4)	(5)	(6)
(a)	Rural (mean: 0.49)			Urban (mean: 0.66)		
$I(\leq 0.5) \times b(\text{Post Policy})$	0.043*			0.121***		
	(0.023)			(0.025)		
$I(\leq 0.5) \times b(\text{Extra Fert.})$		0.050***			0.056***	
		(0.014)			(0.015)	
$I(\leq 0.5) \times b(\text{Extra Score})$			0.019			0.083***
			(0.014)			(0.026)
(b)	Below High School (mean: 0.44)			High School + (mean: 0.60)		
$I(\leq 0.5) \times b(\text{Post Policy})$	0.058***			0.071***		
	(0.014)			(0.027)		
$I(\leq 0.5) \times b(\text{Extra Fert.})$		0.024***			0.021	
		(0.009)			(0.015)	
$I(\leq 0.5) \times b(\text{Extra Score})$			0.027***			0.037
			(0.009)			(0.023)
(c)	By Precise Ethnicity of the Wife			(d) By County (also county FEs)		
$I(\leq 0.5) \times b(\text{Post Policy})$	0.106***			0.150***		
	(0.015)			(0.019)		
$I(\leq 0.5) \times b(\text{Extra Fert.})$		0.045***			0.099*	
		(0.008)			(0.013)	
$I(\leq 0.5) \times b(\text{Extra Score})$			0.045***			0.098***
			(0.007)			(0.019)
(e)	Using HM&MH families					
$I(\leq 0.5) \times b(\text{Post Policy})$	0.069***					
	(0.014)					
$I(\leq 0.5) \times b(\text{Extra Ferti})$		0.027***				
		(0.009)				
$I(\leq 0.5) \times b(\text{Extra Score})$			0.034***			
			(0.009)			
Prof FE, Birth YearFE	Y	Y	Y	Y	Y	Y
Wife Ethnicity FE	Y	Y	Y	Y	Y	Y
Controls*b	Y	Y	Y	Y	Y	Y
Prov. FE*Year FE	Y	Y	Y	Y	Y	Y

Notes: This table shows that the baseline results hold up under alternative definitions of peer groups.
 a. Panel (a) separates rural/urban couples based on the 2000 and 2005 censuses; panels (b) and (c) define peer groups relevant for social reputation by education and minority groups; panel (d) defines peer groups by county; and panel (e) defines these peer groups as both types of mixed families.
 b. Controls include couples' characteristics (education-level fixed effects and 5-year birth-cohort fixed effects, for both husband and wife) and prefecture characteristics (listed in panel (d) of Table 1).
 c. Standard errors in parentheses are clustered at the prefecture level. Significance: ***, 1%, **, 5%, *, 10%.

2B) into those with less than a high-school education, and those with high-school or more. The results are presented in panel (b) of Table 6. On average, families with more education are more likely to break the norm and choose minority for their children. Multiple channels can account for this difference: the better educated may

have better information, perceive higher benefits of education, or attach less importance to tradition. Our focus, however, is still on the interplay between material motives and social motives. As shown, the estimated interaction effect between individual material motives and social motives appears to be comparable for these two groups.

The estimates presented so far assume that all mixed couples with a Han man and a minority wife – no matter which minority – belong to the relevant peer group. What if we restrict the peer group to each specific minority group of the wife (definition 2C)? To check this, we allow each cohort in a prefecture to make up 55 different peer groups. In practice, the average number is much smaller due to the regional dispersion of minorities. The central estimates, presented in panel (c) of Table 6, are slightly larger than the baseline estimates in Tables 2A and 2B.

In addition, we use the county information (available in census 2000) to define peers. Once again, we find a similar pattern (presented in panel (d)). If anything, the coefficients are larger than in our baseline. Altogether, the results in panels (a)-(d) show that our results are robust to narrower peer groups.

A Wider Peer Group. As opposed to the refinement in panels (a)-(d) of Table 6, panel (e) presents results for a wider peer group: both Han-minority and minority-Han couples, as both can choose whether to break the norm. Using the same specification as in panels (a)-(d), panel (e) shows a similar pattern for this wider peer group. The findings in Table 6 show that our main result is unlikely to be driven by the specific peer-group definition in our baseline specification.

In summary, the results reported in Section 5.1-5.3 constitute solid and robust evidence that peer-group dependent social motives help shape the effect of individual material benefits on individual ethnicity choices. As in the theory, stronger material motives are either crowded in or crowded out by social motives, depending on the initial equilibrium in the peer group. The interaction between individual and social motives appears to not only be statistically significant but also quantitatively significant.

As mentioned in Section 3, our model also predicts that when the intrinsic costs are high, material benefits have a smaller effect on the probability of minority children in Han-minority families. In Online Appendix D, we present supportive evidence for this sanity check, when intrinsic costs are proxied by male gender of the child and the religiosity of the wife's ethnicity.

6. Alternative Explanations

Our model is consistent with motivating Facts F1-F2 in Chinese micro data. Its central predictions P1 and P1' are also borne out by the data. Our model is thus a plausible framework for understanding the interaction of individual and social motives for identity choice. But could our findings be explained by other theoretical and empirical mechanisms? In this section, we discuss two alternative ways of specifying the model and four alternative ways of interpreting the empirical findings. The bottom line of this discussion will be that some alternative explanations may indeed help us think about

the patterns in the data. But they are unlikely to drive our main results on individual-social interactions. Other alternatives can be ruled out *a priori*, on either theoretical or empirical grounds. We summarize the key conceptual issues for each explanation and present detailed modeling and/or estimations in Online Appendix E.

Nonlinear Utility. In our version of the Benabou-Tirole model, couples have preferences, which are linear in material benefits b and intrinsic costs $e + \varepsilon$, but nonlinear in social reputations $\mu E(\varepsilon | m)$. What if we got rid of the social-reputation term, but made the preferences concave in individual benefits and convex in intrinsic costs. Could this reproduce the prediction that higher benefits would have a larger effect on the share of children breaking the social norm (by adopting the mother's ethnicity) when the share is smaller? As we show in Online Appendix E.1, the answer is no. Intuitively, due to the convexity of the cost function, the effect of b is smaller at lower values of ε^* (lower norm-breaking shares), which contradicts Prediction P1.

Social Interactions. How particular are our theoretical predictions and empirical results to the assumed form of social interactions? The latter has two dimensions: how the social motive enters the household's preferences, and which social peer group is the relevant for the household. We have examined alternative peer groups in Section 5. In this subsection, we discuss the specific functional-form assumption and show that the most natural alternatives cannot explain our findings.

As we have emphasized, our version of the Benabou-Tirole model produces either crowding in or crowding out, because people do not only take into account the stigma of breaking the social norm but also the honor of obeying it, given how others in their peer group behave. Many papers in the literature only consider the stigma of breaking the norm, assuming that it becomes smaller if more people do so. But this is equivalent to assuming strategic complementarity, and hence crowding in *a priori*.

How important is the model's form for social reputation, namely that people choose the identity for their children to signal their expected type, given how everybody else in the peer group behaves? One could think of other ways of modelling social reputation. The most natural alternative is to assume that the honor of a child with the father's ethnicity and the stigma of a child with the mother's ethnicity are given by the *shares* of norm-followers and norm-breakers in the peer group. As we show in Online Appendix E.2, such an assumption would typically lead to strategic complements only. The attractiveness of our social-reputation model defined over expected types is that it delivers non-trivial and testable predictions about the interaction between individual and social motives without overly strong functional-form assumptions.

Bargaining Power. Bargaining between husbands and wives, with conflicting preferences, is an alternative mechanism behind some of the patterns in the data. Consider Facts F1 and F2 in Figure 1. Assume that women's bargaining power has gone up over time so that a higher number of Han-minority couples choose the mother's minority ethnicity for their children. It may be further argued that this mechanism may have become more powerful post ethnic policies, due to social and economic factors,

like unbalanced and increasing sex ratios – more men per woman – among the Han. However, a bargaining mechanism cannot explain our main finding on Prediction P1, neither in the theory nor in the data. In Online Appendix E.3, we first sketch a bargaining model. Intuitively, to explain P1, one has to make the very specific assumption that the bargaining power has risen over time and more so in peer groups with lower shares of minority children.

Empirically, we can proxy bargaining power by three measures: education differences, or age differences, between husband and wife, as well as sex ratios of children. In all cases, we find no empirical evidence that the bargaining power rises more in peer groups with lower shares of minority children. Moreover, our main results are robust to considering these measures and their interactions with the cutoff indicator in Tables 2A-2B. These estimation results are presented and discussed in Online Appendix E.3.

Minority Population Share. Our findings on Prediction P1 cannot really be explained by the variation in the share of minorities in the prefecture population. First, this prediction is about the share of minority children in Han-minority families rather than the share of minorities itself. As discussed in the prefecture-level correlations, however, these two measures are weakly positively correlated. This suggests that our findings are unlikely to be driven by a "scarcity" effect, whereby children are less likely to be minority in some regions because a more or less fixed set of material benefits become diluted by a larger minority population. Such scarcity is also unlikely *a priori*, as ethnic policies are not set by fixed quotas.

One may also argue that a higher minority population share works in the same direction as the share of minority children in Han-minority families. In particular, if the minority population share in a prefecture is small, ethnic conflict is less likely to be a social problem and parents may feel safer to break the norm and choose minority for their children. Tables 2A-2B already show that our findings hold conditional on the minority-population share and its interaction with the ethnic policies $b_{r,t}$. Moreover, the coefficients on this interaction term are not significant in those specifications (with a p -value of 0.46 in column (6) of Table 2A). Thus, our findings are not affected by the share of the minority population and there is no empirical evidence for a scarcity or conflict effect.

Censoring. Another possible concern is that our main result could be mechanically explained by a kind of upward censoring. Specifically, our finding of a larger policy effect in prefectures with a small minority-children share could reflect that there is little room to respond when this share is large and approaching one. To check for this, we restrict the estimation sample to prefecture-cohorts with a minority-children share between 0.3 and 0.7. In this interval, there should be enough room for Han-minority couples in every prefecture-cohort to respond without hitting a constraint. As shown in Online Appendix E.4, the estimates from the restricted sample are similar to the baseline estimates from the full sample in Table 2A. Upward censoring does thus not drive our main findings. Moreover, the evidence supporting P1' (the effects by

quartiles) also suggests that censoring is unlikely to be the main driver. In fact, we find that the effect for the second quartile is typically larger than that for the first quartile.

Quantity of Children. Finally, the results could conceivably capture another mechanical effect. Specifically, some Han-minority couples may always have children with the father's Han ethnicity and others may always have children with the mother's minority ethnicity. Suppose now that after the one-child policy, couples who choose the mother's minority ethnicity have a larger number of children than those who choose the father's Han ethnicity. Such composition effects could explain our results without any behavioral change (other than regarding the number of children).

To check this, in Online Appendix E.5 we present separate results for families with a single child (in columns (1)-(3)) and for those with multiple children (in columns (4)-(6)). As these estimates show, the results for both types of households are similar to the baseline findings in Tables 2A and 2B. If anything, the pattern is slightly stronger for single-child families. Our baseline results on Prediction P1 are thus not driven by any composition effects. This finding also suggests that our main insight is not affected by the quantity of children.

7. Conclusion

In this paper, we provide theoretical and empirical analyses of ethnicity choices for children in China's interethnic marriages. Drawing on earlier work by Benabou and Tirole (2011), we present a model which is consistent with motivating facts in the data. The model also delivers additional predictions, which are supported by empirical tests on Chinese census data. Our study adds to the effort of understanding the determinants of identity choice. While scholars have realized that both individual and social motives drive identity decisions, few have formalized the interplay among individual material incentives, individual intrinsic motives, and social reputations. Our study may open avenues for future research on identity choice in other settings.

Characterizing the interplay between individual and social motives is also relevant in many other contexts. Our methodology for using micro data to investigate how social reputations modify the effect of individual incentives complements recent experimental studies. One can apply a similar approach to observational data on a set of different issues – where both individual and social motives are likely to be important – e.g., in tax evasion, political participation, fertility, or environment-friendly investments.

Finally, we hope that our study makes a contribution specific to China, where the economics and politics of ethnicity have been important issues, yet rarely studied with economic methods. In future work, we hope to extend our empirical analysis to predictions from a model of directed marriage search, asking which individuals end up in mixed couples in the first place. Then, the ethnic choices for children analyzed in this paper would help determine the continuation value from the marriage stage. In addition, the recent abolition of the one-child policy provides another testing ground for our design, which we have to leave to future work.

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**Online Appendix:
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Appendix A: Additional Results on the Background

A.1. Individual-level Evidence for F1 and F2

The two facts F1 and F2 in Figure 1 also hold at the individual level. Below, the two tables report the estimates with different sets of controls.

Table A1(a) Fact F1: HM-Families versus MH-Families

	(1)	(2)	(3)	(4)
		following mother's ethnicity = 0/1		
HM-Marriage	0.400*** (0.030)	0.420*** (0.029)	0.417*** (0.029)	0.416*** (0.029)
Prefecture FE		Y	Y	Y
Birth Year FE			Y	Y
Provincial Trends				Y
Observations	235,930	235,930	235,930	235,930
R-squared	0.199	0.298	0.302	0.315

Table A1(b) Fact F2: Ethnicity of Children by Cohorts

	(1)	(2)	(3)	(4)	(5)	(6)
		following mother's ethnicity = 0/1				
		HM-Marriage		MH-Marriage		
Born 1975-79	-0.002 (0.009)	0.017*** (0.005)	0.003 (0.006)	0.004* (0.002)	0.002 (0.002)	-0.008*** (0.003)
Born 1980-84	0.040** (0.015)	0.048*** (0.008)	0.020** (0.010)	0.016*** (0.003)	0.015*** (0.003)	-0.005 (0.005)
Born 1985-90	0.086*** (0.017)	0.089*** (0.011)	0.048*** (0.013)	0.024*** (0.004)	0.020*** (0.004)	-0.010 (0.007)
Born 1990+	0.108*** (0.024)	0.109*** (0.015)	0.047*** (0.018)	0.059*** (0.006)	0.047*** (0.005)	0.003 (0.009)
Prefecture FE		Y	Y		Y	Y
Provincial Trends			Y			Y
Observations	124,940	124,940	124,940	110,020	110,020	110,020
R-squared	0.008	0.272	0.277	0.007	0.082	0.086

Notes: Provincial trends indicate provincial-birth year linear trends. Standard errors are clustered at the prefecture level. Significance: ***, 1%, **, 5%, *, 10%.

Similar to the aggregate pattern, the propensity of breaking the norm of following father's ethnicity is much higher in Han-minority families at the individual level. Column (1) in Table (a) compares the probability without any controls. Columns (2) and (3) present the results after including prefecture fixed effects and birth year fixed effects. Column (4) further allows for provincial-specific trends. The estimates are very similar to those in column (1).

Table (b) presents estimation results for F2 at the individual level, using those born in 1970-74 as the reference group. Columns (1)-(3) show the results for Han-minority families and columns (4)-(6) for minority-Han families. Again, these results show the increase in the propensity of breaking the norm after 1980.

A.2. Anecdotal Evidence on the Benefits and Costs of a Minority Child

The discussion in Example 1 below comes from <http://www.babytree.com/ask/detail/3690549>, which shows that parents are thinking about both social motives and ethnic policies (especially the option of having more children for their child if they choose minority for their child).

Figure A2(a) Ethnic Policies, Social Motives: Example 1

父亲是汉族，母亲是少数民族，那孩子的民族怎么决定？可以随母亲吗？

父亲是汉族，母亲是少数民族，那孩子的民族怎么决定？可以随母亲吗？wsjssc.com

"If the father is a Han and the mother is a minority, could the child be a minority?"

 **林芝宝贝轩** 来自：网页 2012-08-28
 小 Generally should follow the father's. But following the mother's has the benefits of ethnic favors.
 一般都随父亲，听说这样的孩子聪明，不过随母亲有好处，少数民族人家有照顾  0

 **郭冠冠** 来自：网页 2012-08-28
 小 4岁2个月
 Ok. I have a friend who followed the mother's.
 可以的。 . . . 我有朋友就随母亲的  2

 **青儿2011** 来自：网页 2012-08-28
 小 6岁9个月
 Generally should follow the father's. It is fine if you insist on following the mother's.
 一般都是随父亲的，你一定要随母亲应该问题也不大  0

 **海角湾妈咪** 来自：网页 2012-08-28
 小 3岁7个月
 The parent can make a choice. It is fine following the mother's.
 父母可以自己决定，随母亲也行的  0

 **三口之家兔** 来自：网页 2012-08-28
 小 3岁
 The child should follow the father's ethnicity. Only the children of a live-in husband will follow the mother's.
 亲这样的话，孩子的民族一般是跟着孩子的父亲的，如果是跟着母亲的话，一般是上门女婿才可以的啊。  0

 **西西1011** 来自：网页 2012-09-02
 小
 You can follow the mother's. A minority has the option of having a second child.
 可以随母亲，当个少数民族，然后还可以再生一个。  0

The discussion in Example 2 comes from <http://jzb.com/bbs/thread-335421-1-1.html?action=printable>, which shows that both honor and stigma are discussed in making the ethnic choices for the children.

Figure A2(b) Ethnic Policies, Social Motives: Example 2

作者: 中二门 时间: 2010-5-18 13:17

标题: 我为孩子选择了汉族, 放弃了少数民族

前一阵去给孩子办户口, 我是汉族, 孩子她妈是少数民族, 我对警察说孩子入汉族。

警察大姐好好把我教育了一番, 说中考的时候少一分就多一操场人, 她家孩子要是有6分, 稳上四中了, 说要对孩子负责。

最后我还是坚持入了汉族。我是想孩子成长过程中要靠自己的实力, 而不是靠特权和特殊的照顾。我可以花钱让她去上好的培训班, 也可以给她辅导, 而不是直接把分给她。希望孩子长大后能理解她的父母。

Zhongermen: "I went to register the birth of my child a while ago. I am a Han man and my wife is a minority. I told the police that I want my child to be a Han. The police kindly suggested that I should choose minority for the child. She said that one score lower implies an extra playground of competitors in the high-school entrance exam and that I should be responsible for my child's future. But I insisted on choosing Han in the end. I hope that my child's future will rely on his own ability, not ethnic favors."

作者: fh2315 时间: 2010-5-18 13:27

我觉得无所谓

如果没有什么特别的信仰的话。

fh2315: "Not a big deal if the minority is not religious."

作者: claitia 时间: 2010-5-18 15:49

告诉你吧加分什么的你要是瞧不上的话

至少少数民族还可以生2个孩子

claitia: "Well, if you despise the ethnic favor for extra scores, minorities can at least have more children!"

作者: 麻爪 时间: 2010-5-19 00:04

我是少数民族, 女儿随我, 原因很简单, 姓随爸爸, 民族就随妈妈吧, 虽然是人数多的少数民族但我还是以自己的民族为骄傲的, 所以希望女儿也是, 与加分无关

Magua: "I am a minority and my child follows my ethnicity. The reason is simple...Even though I belong to a minority group whose population size is large, I am proud of my ethnicity. So I hope that my child is also [proud of my ethnicity]. This has nothing to do with extra scores."

A.3. Descriptive Patterns of the Four Types of Marriages

The following table describes the marriage patterns among all married couples in the four censuses (1982, 1990, 2000 and 2005). This sample includes all the couples in the data, while our analysis on mixed marriages focuses on those with children born between 1970 and 2005. Among married couples that appear in our four censuses, 17% of minority men marry Han women, while 18% of minority women marry Han men.

Table A3: Differences across Marriages

	HH	MM	HM	MH
#Couples	6,436,486	417,089	90,704	81,570
Share in total marriages	91.60%	5.90%	1.30%	1.20%
HM Share for a minority woman				$1.3/(1.3+5.9)=18\%$
MH Share or a minority man				$1.2/(1.2+5.9)=17\%$
Husband Edu-Wife Edu	0.27	0.26	0.21	0.23
Husband Age-Wife Age	2.41	2.72	2.8	2.48

Appendix B: Additional Results on the Measurement

B.1. Spatial Variation in Ethnic Policies

The two figures below present the cross-sectional variation in our measures of ethnic policies: extra fertility and extra scores. They show that the two types of benefits are not closely correlated at the cross-sectional level, with an insignificant correlational coefficient of 0.06. The data for extra scores in Tibet are not available.

Figure B1(a) Total fertility ratio between Minority and Han women born in 1955-59

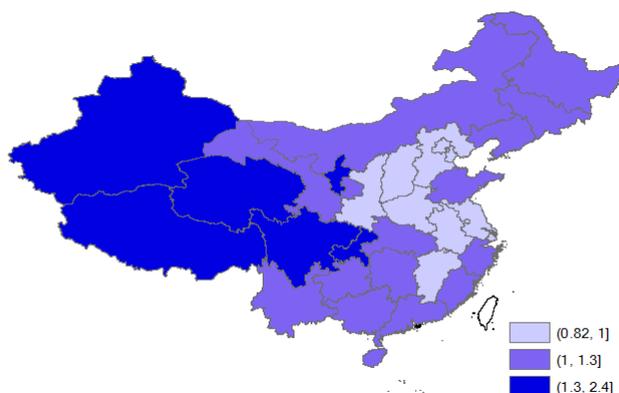
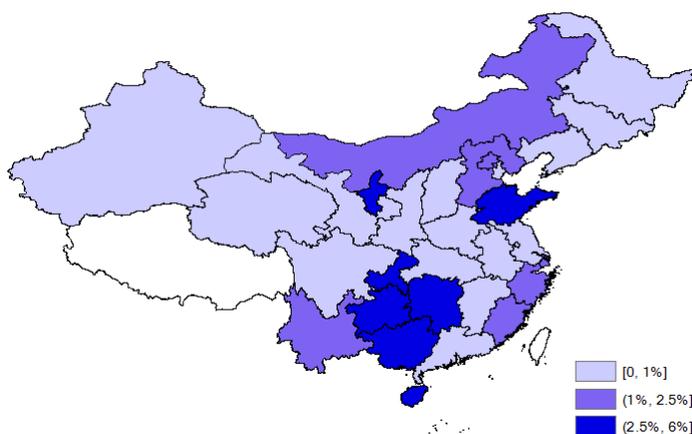


Figure B1(b) Extra scores (relative to provincial cutoff) for minorities in 2000



B.2. Correlation b/w the Norms and Prefecture Characteristics

The following table reports the correlations between our measure of norms and other prefecture characteristics. As mentioned in the main text, it is worthwhile pointing out that correlation between our measure of social norms and the share of minority population is weakly positive. This correlation rejects a scarcity effect, whereby children are less likely to be minority in regions with a higher share of minority population because a more or less fixed set of material benefits get diluted by population.

Table B2: Correlation b/w the Norms and Other Prefecture Characteristics

	(1)	(2)
	Share of children following mother's ethnicity in the 1970-74 cohort	
Minority Population Share 1982	0.004*** (0.001)	0.002 (0.001)
Share of Pop. with high school + 1982	0.934*** (0.340)	0.854** (0.361)
Borderland Prefecture	-0.130*** (0.042)	-0.088** (0.044)
#children for a minority 1982 (women aged 40+)	0.018 (0.018)	-0.003 (0.020)
Province fixed effects		Y
Observations	261	261
R-squared	0.110	0.415

Notes: Standard errors are clustered at the prefecture level. Significance: ***, 1%, **, 5%, *, 10%.

Appendix C: Additional Results on the Main Prediction

C.1. Using Fines as an Alternative Policy Measure

An additional policy measure in the existing literature is the amounts of penalties associated with additional children as (Ebenstein 2010). Replacing our policy rollout measure with the penalties, we obtain a similar pattern:

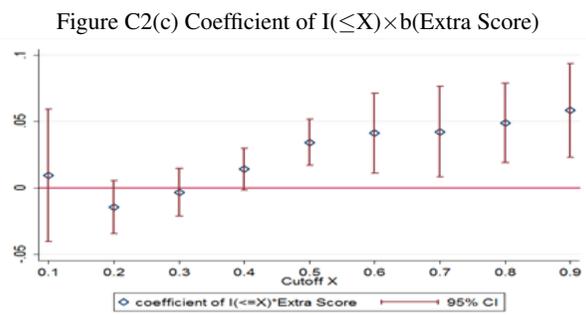
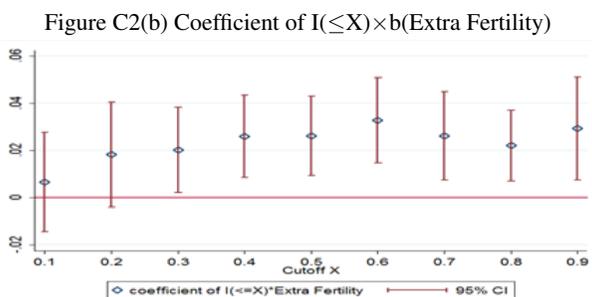
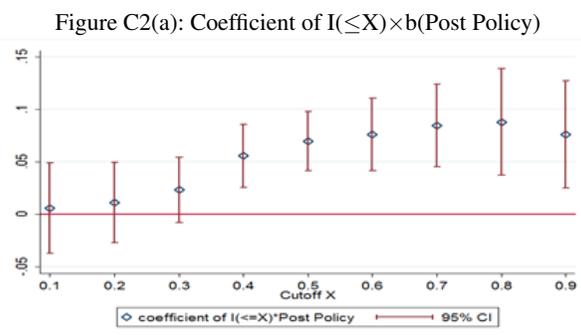
Table C1: Results Using Fines

	(1)	(2)	(3)
$I(\leq 0.5) \times b(\text{Fines})$		0.065*** (0.015)	0.059*** (0.016)
Fines	0.052*** (0.009)	-0.012 (0.024)	-0.348*** (0.133)
Prefecture FE	Y	Y	Y
Wife Ethnicity FE		Y	Y
Birth Year FE		Y	Y
Controls*b			Y
Province FE*Year FE			Y
Observations	124,042	124,042	111,089
R-squared	0.268	0.288	0.330

Notes: Standard errors are clustered at the prefecture level. Significance: ***, 1%, **, 5%, *, 10%.

C.2. Varying Cutoff Values

This figure plots the results for testing prediction P1 while using different cutoff values for the share of minority children, ranging from 0.1 to 0.9. The econometric specification is the same as that in column (6) of Table 2A. The diamonds indicate the estimates and the bars through each dot indicates 95% confidence intervals.



C.3. Dynamic Extensions of the Model

One can extend the model to get a dynamic adjustment to a new steady state after a one time shock, which deliver a prediction consistent with the pattern discussed in C.3 above.

Introducing dynamics. Suppose that the social-reputation motives of the parents in a given birth cohort (where a cohort could, e.g., be defined as a year) are tied to the behavior of the parents in the previous birth cohort. Specifically, the cutoff entering the gain in social reputation for Han-minority couples with birth cohort t is tied to the behavior of the Han-minority couples with birth cohort $t - 1$. One rationale for this assumption could be that the behavior of other couples is only observed with a period's lag. This assumption is similar to the one made by Besley, Jensen and Persson (2015) in their analysis of tax evasion in a dynamic version of the Benabou-Tirole model.

Drawing on their results, equation (3) in the main text still defines a steady-state value for ε_H^* . As long as other parameters, b and $e(H)$ are constant, the equilibrium cutoff (and therefore the share of children following mother's ethnicity) adjusts gradually towards the new steady state according to the non-linear difference equation:

$$b - e(H) - \varepsilon_{H,t}^* = \mu \Delta(\varepsilon_{H,t-1}^*)$$

The steady state is stable under the assumption we have already made that $1 + \mu \frac{d\Delta(\varepsilon_H^*(b,e,\mu))}{d\varepsilon^*} \geq 0$. This guarantees the root on non-linear difference equation above is less than 1 in absolute value.

A Shift in b . Consider now an upward shift in benefits b that occurs in period 1. Consider two peer groups L and H with low and high initial shares $\varepsilon_{H,0}^{*L} < \varepsilon_{H,0}^{*H}$ of children following mother's ethnicity. In the dynamic setting, the steady-state shift in the minority share is going to be larger in group L than in group H , in the same way as in the static model. But the impact effect of the shift in b in period 1 is the same in the two groups, as the behavior by the previous cohort $\varepsilon_{H,0}^{*L}$ is given at the time of the shock. However, the cutoff starts changing from birth cohort 1 and onwards. Because the share of children following the mother's ethnicity in the group L is adjusting more than the one of group H , its share will become progressively higher as we go forward in time from period 2. This is precisely what we see in Figure 5 in the main text and in the corresponding regression estimates presented in C.3 above.

C.4. Dealing with Migration

Our baseline estimation is robust to considering migration. Columns (1)-(6) of the following table present the results after excluding all data after the 2000 census as well as individuals whose birth county and residency county are different. Controls include couples' characteristics (education level fixed effects and 5-year birth-cohort fixed effects, for both husband and wife) and prefecture characteristics (listed in panel (d) of Table 1). Columns (7)-(9) show no similar interaction effect on migration as an outcome. Note that columns (1)-(6) focus on Han-Minority families while columns

(7)-(9) consider all types of families, which show that migration is not correlated with the ethnic policies.

Table C4: Results Robust to Considering Migration

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Excluding migrants: following mother's ethnicity=0/1						Migration in 2000 (0/1)		
I(\leq) \times b(Post Policy)		0.068*** (0.014)					0.006 (0.006)		
b(Post Policy)	0.078*** (0.011)								
I(\leq 0.5) \times b(Extra Fertility)				0.024*** (0.008)				-0.000 (0.002)	
b(Extra Fertility)			0.033*** (0.005)						
I(\leq) \times b(Extra Score)						0.032*** (0.009)			0.002 (0.002)
b(Extra Score)					0.042*** (0.007)				
Prefecture FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Wife Ethnicity FE		Y		Y		Y			
Birth Year FE		Y		Y		Y			
Controls*b		Y		Y		Y			
Prov. FE*Year FE		Y		Y		Y			
Observations	113,343	101,546	102,216	92,012	115,796	103,999	89,741	71,271	93,287
R-squared	0.285	0.344	0.284	0.348	0.278	0.339	0.064	0.064	0.070

Notes: Standard errors are clustered at the prefecture level. Significance: ***, 1%, **, 5%, *, 10%.

C.5. Dealing with Multiple Generations

While most of the individuals we study live in two-generation households, we include those living in multiple-generation households for completeness. This way of data construction does not affect our finding.

Table C5: Considering Multiple Generations

	(1)	(2)	(3)	(4)	(5)	(6)
	Two-generation households only			Born during 1970-1990 only		
I(\leq 0.5) \times b(Post Policy)		0.080*** (0.021)	0.077*** (0.016)		0.048*** (0.017)	0.046*** (0.013)
Post Policy	0.082*** (0.011)	0.031** (0.015)	-0.197** (0.076)	0.066*** (0.009)	0.034*** (0.012)	-0.119* (0.071)
Prefecture FE	Y	Y	Y	Y	Y	Y
Wife Ethnicity FE			Y			Y
Birth Year FE			Y			Y
Controls*b			Y			Y
Province FE*Year FE			Y			Y
Observations	95,457	95,457	85,253	98,488	98,488	89,152
R-squared	0.280	0.281	0.338	0.279	0.280	0.341

Notes: Standard errors are clustered at the prefecture level. Significance: ***, 1%, **, 5%, *, 10%.

As shown above, our results are similar if we restrict data to those in two-generation households only (column (1)-(3)). In addition, we can limit our data to those born during 1970 and 1990. Consistent with our earlier results, we find that the average effect of policies is smaller. Nevertheless, the interaction effect between policy and social motives is similar (compared with the average effect).

Appendix D: Prediction on Material Incentives \times Intrinsic Costs

An intuitive prediction of our model is that higher intrinsic costs weaken the effect of material incentives. Here, we provide supportive evidence of this hypothesis.

D.1. Measurement

We use two measures to proxy intrinsic costs. Our first measure is whether the child is a son or a daughter. Given that China is a typical patriarchal society, we assume that the intrinsic costs of having a child with different ethnicity than the father are higher for a son than a daughter. This measure, however, may capture heterogeneity in material benefits. In particular, ethnic benefits may be more important for sons who are perceived to play a more important role in providing old-age support and enjoy more advantages in education. For instance, in the administrative data for 2000, boys accounted for 57 percent of the college entrance exam takers and 55.5 percent of those accepted by colleges. Note that this channel alone would predict an opposite pattern (i.e., sons are more likely to be minorities for given ethnic policies). This observation matters for interpreting our estimation results below.

Our second measure of intrinsic costs is whether the spouse belongs to a religious minority group. The idea is that the cost of giving the child the mother's ethnicity may be higher, if that ethnicity is practicing religion (recall the online dialogue in A.2). To clarify, this is a measure at the ethnic-group rather than the individual level. We define a wife as religious if she belongs to one of the 18 minority groups that practice Islam or Tibetan Buddhism. Men who marry religious women constitute a selected sample, but our question concerns how a religious wife shapes the effect of material benefits on ethnic choice for children, rather than the effect of a religious wife itself. Table 1 shows that the share of Han-minority mixed families with a religious wife is about 19 percent.

D.2. Empirical Results

We thus examine whether the impact of material benefits on ethnic choices is smaller for sons and for couples with religious wives. The estimation results are presented in the table of this subsection. The effect of fertility-related material benefits are indeed smaller when the child is a son. The estimates for our three measures of material benefits are displayed in columns (1)-(6). Columns (1), (3) and (5) report the results with prefecture fixed effects. Columns (2), (4) and (6) include additional fixed effects

and controls. Having a son decreases the impact of a 1σ increase in extra fertility by 0.006, around 16% of the mean effect. However, we find no strong pattern related to education. A possible reason is related to the heterogeneous material benefits in education for sons mentioned above, which would provide a channel working toward the opposite direction.

Having a religious wife also cuts the effect of material benefits. Columns (7)-(12) show the results on the effect on Han men with religious minority wives. Having a religious wife decreases the impact of a 1σ increase in extra fertility by 0.026, around 60% of the mean effect.

Table D2: Material Benefits×Intrinsic Costs on the Prob. of Following Mother's Ethnicity

	(1)	(2)	(3)	(4)	(5)	(6)
b(Post Policy)×Son	-0.017*** (0.005)	-0.008 (0.005)				
b(Extra Fertility)×Son			-0.006*** (0.002)	-0.005* (0.003)		
b(Extra Score)×Son					0.000 (0.003)	0.003 (0.003)
Son	-0.000 (0.004)	-0.009** (0.004)	-0.009*** (0.003)	-0.013*** (0.003)	-0.011*** (0.003)	-0.014*** (0.003)
b	0.087*** (0.011)	-0.099 (0.075)	0.037*** (0.005)	0.015 (0.034)	0.043*** (0.007)	-0.148*** (0.041)
	(7)	(8)	(9)	(10)	(11)	(12)
b(Post Policy)×Religious Wife	-0.045*** (0.015)	-0.026** (0.013)				
b(Extra Fertility)×Relig. Wife			-0.026*** (0.006)	-0.017*** (0.006)		
b(Extra Score)×Relig. Wife					-0.036*** (0.011)	-0.061*** (0.010)
Religious Wife	0.203 (0.206)	0.125 (0.284)	0.064*** (0.013)	0.122 (0.277)	0.053*** (0.013)	-0.026 (0.214)
b	-0.006 (0.013)	-0.114 (0.073)	0.042*** (0.009)	-0.013 (0.033)	0.046*** (0.007)	-0.175*** (0.038)
Prefecture FE	Y	Y	Y	Y	Y	Y
Wife Ethn. FE		Y		Y		Y
Birth Year FE		Y		Y		Y
Controls*b		Y		Y		Y
Province FE*Year FE		Y		Y		Y

Notes: Standard errors are clustered at the prefecture level. Significance: ***, 1%, **, 5%, *, 10%.

Thus, these results provide supportive evidence for our model setup and also shed light on additional factors that can affect ethnic choices.

Appendix E: Additional Results on Alternative Explanations

E.1. Nonlinear Utility

In our version of the Benabou-Tirole model, the preference function of couples is linear in material benefits b and intrinsic costs $e + \varepsilon$, but nonlinear in the social-reputation term $\mu E(\varepsilon | m)$. Suppose we got rid of the social-reputation term, but made preferences nonlinear in the individual benefits and costs. Perhaps this alternative setting could reproduce the prediction that the effect on the share of children following mother's ethnicity of a change in benefits is larger when the share is smaller. In this subsection, we show that nonlinear utility actually contradicts Prediction P1.

An alternative model. Assume that the utility function of a Han-minority couple is

$$u^H = v + m[u(b) - c(e + \varepsilon)], \quad (\text{E.1})$$

where u and c are nonlinear functions. The natural assumption is that the utility in material benefits u is concave, with decreasing marginal benefits ($u' > 0$ and $u'' < 0$) and the intrinsic cost c is convex, with increasing marginal costs in the type ($c' > 0$ and $c'' > 0$).¹ The indifference condition for having a child following mother's ethnicity now becomes

$$u(b) - c(e + \varepsilon^*) = 0,$$

which defines a cutoff value $\varepsilon^*(b, e)$ as an increasing function of b and a decreasing function of e – at higher average intrinsic costs the share of minority children is lower.

Comparative statics. Straightforward comparative statics imply

$$\frac{\partial \varepsilon^*}{\partial b} = \frac{u'(b)}{c'(e + \varepsilon^*)} > 0.$$

Suppose ε^* is lower because e is higher. How does this alter the effect of material benefits? The answer is given by:

$$\frac{\partial^2 \varepsilon^*}{\partial b \partial e} = -\frac{c''(e + \varepsilon^*)u'(b)}{(c'(e + \varepsilon^*))^2} < 0.$$

That is to say, at lower ε^* (higher e) – and a lower share of minority children – the effect of b is lower. This contradicts our empirical results from the tests of P1. Thus, the alternative model without a social reputation term can help us understand some aspects of the data (e.g., the effect of b), but does not offer an alternative explanation for our central result.

1. The results of this section largely hold up also in the case where the preferences are linear in the intrinsic costs and in the type.

E.2. Alternative Ways of Modeling Social Interactions

How important is the model's assumed functional form for social reputation, namely that people decide on the identity choice for their children to signal their *expected type*, given how everybody else in the peer group behaves? One could think of other ways of modelling social reputation. The most natural alternative is to assume that the honor of a child with father's ethnicity and the stigma of a child with mother's ethnicity are given by the *shares* of norm-followers and norm-breakers in the peer group. Under that alternative relative-numbers assumption, we would write the gain in social reputation as

$$\Delta(\varepsilon^*) = h(1 - G(\varepsilon^*)) - sG(\varepsilon^*) = h - (h + s)G(\varepsilon^*),$$

where h and s are some positive constants.

In this case, we get $\frac{d\Delta}{d\varepsilon^*} = -(h + s)g(\varepsilon^*)$, such that choices would always be strategic complements, with maximal complementarity at the single peak of the p.d.f. for ε . This would deliver quite different predictions than our model, predictions that would not be supported by the data. In particular, we would not predict a larger effect of b on G , when ε^* is low and the share of kids following mother's ethnicity $G(\varepsilon^*)$ is high, unless we made very specific and strong assumptions about (unobservable) distribution G .

The attractiveness of our social-reputation model defined over expected types is that it delivers non-trivial and testable predictions about the interaction between individual and social motives without overly strong functional-form assumptions.

E.3. Bargaining Power: Model and Empirics

Bargaining is an alternative mechanism behind some of the patterns in the data. Consider facts F1 and F2 in Figure 1. Assume that women's bargaining power have gone up over time so that a higher number of Han-minority couples chose the mother's minority ethnicity for their children. One may further argue that this mechanism may have become more powerful post ethnic policies, due to social and economic factors, like unbalanced and increasing sex ratios – more men per woman – among the Han. In this subsection, we show that a bargaining mechanism cannot explain our main finding on Prediction P1, both in the theory and in the data.

A simple bargaining model. Let us sketch a simple bargaining model, without any social reputations. Suppose the Han man has a similar utility function as in (E.1), namely:

$$u^H = v + m[u(b) - (e + \varepsilon)].$$

The minority woman has an analogous utility function:

$$u^M = v + m[u(b) + (e + \varepsilon)],$$

except that the intrinsic cost for the Han man of a child with minority ethnicity is an intrinsic benefit for the minority woman. In these expressions for u^H and u^M , ε is

an idiosyncratic couple-specific shock to the intrinsic cost drawn after the couple is formed.² We assume that these utility functions are linear in the intrinsic cost since this allows aggregation. An efficient bargaining solution maximizes

$$(1 - \alpha(\mathbf{z}))u^H + \alpha(\mathbf{z})u^M = v + m[u(b) - (1 - 2\alpha(\mathbf{z}))(e + \varepsilon)],$$

where $\alpha(\mathbf{z}) < 0.5$ is the relative bargaining power of the minority woman and \mathbf{z} a vector of variables that affects it. The indifference condition for a minority child becomes:

$$u(b) - (1 - 2\alpha(\mathbf{z}))(e + \varepsilon^*) = 0.$$

Predictions. The effect of material benefits on the share of Han-minority couples with minority children is proportional to:

$$\frac{\partial \varepsilon^*}{\partial b} = \frac{u'(b)}{(1 - 2\alpha(\mathbf{z}))} > 0.$$

The effect of *changing bargaining power* for minority women can be determined from:

$$\frac{\partial \varepsilon^*}{\partial \alpha(\mathbf{z})} = \frac{2(e + \varepsilon^*)}{(1 - 2\alpha(\mathbf{z}))} > 0.$$

Intuitively, higher bargaining power of the minority wife – a rise in $\alpha(\mathbf{z})$ – raises ε^* and the share of minority children. An alternative explanation for F2 – or a complementary explanation to the increase in b – is thus that the bargaining power of minority women in mixed marriages went up over time. However, to explain our results of testing P1 in Table 2, $\alpha(\mathbf{z})$ would not only have to rise over time, but also have to rise by more in peer groups with a low ε^* .

In the remainder of this subsection, we check this possibility for three plausible proxies for \mathbf{z} , the determinants of minority women's bargaining power. The results are presented in Table E3 below.

2. Having two independent shocks ε^H and ε^M revealed before the marriage would make the analysis more difficult. To say something useful about this case, we would need a marriage matching model.

Table E3: Examining Bargaining Power

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	edu difference		following mother's	age difference		following mother's	sex ratio		following mother's	following mother's
I(≤ 0.5) \times b(Post Policy)		0.009 (0.015)	0.064*** (0.015)		0.369*** (0.110)	0.067*** (0.015)		0.018 (0.011)	0.062*** (0.014)	0.064*** (0.014)
b(Post Policy)	-0.116*** (0.007)			-0.465*** (0.069)			0.022*** (0.007)			
I(≤ 0.5) \times (Husb–Wife Edu.)			-0.007 (0.007)							-0.007 (0.007)
Husband–Wife Edu.			-0.001 (0.005)							-0.001 (0.005)
I(≤ 0.5) \times (Husb–Wife Age)						0.003** (0.001)				0.003** (0.001)
Husband–Wife Age						-0.003*** (0.001)				-0.003*** (0.001)
I(≤ 0.5) \times Sex Ratio									0.000 (0.056)	-0.002 (0.055)
Sex Ratio									0.046 (0.040)	0.049 (0.040)
Prefecture FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Wife Ethnicity FE		Y	Y		Y	Y		Y	Y	Y
Birth Year FE		Y	Y		Y	Y		Y	Y	Y
Pref. Characteristics*b		Y	Y		Y	Y		Y	Y	Y
Province FE*Year FE		Y	Y		Y	Y		Y	Y	Y
Observations	121,908	108,914	108,914	121,908	108,914	108,914	120,094	108,688	108,688	108,688
R-squared	0.036	0.047	0.326	0.066	0.076	0.326	0.373	0.404	0.326	0.326

Notes: Standard errors are clustered at the prefecture level. Significance: ***, 1%, **, 5%, *, 10%.

Education differences. A proxy for one component of \mathbf{z} is the education gap between husband and wife. Realistically, the spouse with higher education (and income) has more bargaining power. We calculate the gap based on the 1-4 levels of education (used as control variables in the baseline specification). The education difference between husbands and wives is around 0.2, such that the average minority woman marries a Han man with more education. Moreover, column (1) of the table below shows that the education gap decreases by 0.1 after the one child policy, consistent with the idea that bargaining power of minority women went up. Thus, higher bargaining power of minority women can help explain fact F2.

But can it also explain the results in our tests of P1? To approach that question, we first use the education difference as an outcome. If this difference decreases with $I(\leq 0.5)_p \times b_{r,t}$ ($b_{r,t}$ refers to $b_{r,t}$ (Post Policy) in this table), the change in minority women's bargaining power goes in the same direction as our baseline findings. However, as shown in column (2), $I(\leq 0.5)_p \times b_{r,t}$, is not significantly correlated with education differences. Thus, the data does not support the idea that education differences decrease faster after the one-child policy in peer groups where the share of minority children is initially low.

As a further check, we add the education difference – and its interaction with the share indicator $I(\leq 0.5)_p$ – to specifications similar to those underlying Table 2A. The results are presented in column (3). After controlling for education differences and its interaction with $I(\leq 0.5)_p$, the estimated interaction coefficient of $I(\leq 0.5)_p \times b_{r,t}$ is very close to that in Table 2A. Therefore, this measure of bargaining power cannot drive the interaction between individual and social motives.

Age differences. A proxy for another component of \mathbf{z} is the age difference between husband and wife, where a smaller age difference presumably raises the wife's bargaining power. The average age difference between husband and wife is 2.6 years. Moreover, as shown in column (4) of the table above, the age gap decreases by 0.46 years after the one-child policy, consistent with increasing bargaining power of minority women. Thus this factor too may have contributed to the trend summarized in F2. Can it also explain the results on our tests of P1?

Column (5) estimates how age differences correlate with material benefits interacted with the initial share of minority children. We see that $I(\leq 0.5)_p \times b_{r,t}$ is positively correlated with the age gap. So if minority women's bargaining power due to age were an important factor behind the choice of identity, we should see minority children chosen less often where the initial share of children is small – the opposite to Prediction P1 in our model. Similar to the estimates for education differences, column (6) presents the results when we include the age difference between husband and wife and its interaction with the share indicator $I(\leq 0.5)_p$. Again, the magnitude of the estimated individual-social interactions is very close to those in Table 2A.

Sex ratios. A third candidate to measure bargaining power is the (male to female) sex ratio in the husband's birth cohort of Han men within the same prefecture. Here it is plausible to assume that a higher sex ratio raises the bargaining power of

the wife.³ Once again, the result in column (7) is consistent with the previous findings using education and age gaps: sex ratios increase over time.

Column (8) shows that the increase is weakly larger in prefectures with a lower share of minority children, which goes in the same direction as our prediction on the effect of $I(\leq 0.5)_p \times b_{r,t}$. However, as shown in column (9), our coefficient estimate on $I(\leq 0.5)_p \times b_{r,t}$ is only marginally affected by controlling for sex ratio and its interaction with $I(\leq 0.5)_p$, while the interaction between sex ratio and $I(\leq 0.5)_p$ is insignificant. Even though the sex ratio measure of bargaining power is positively correlated with our policy variable and may help explain F2, it is unlikely to drive our baseline estimate.

Finally, column (10) presents the results when we include all three measures of bargaining power. The results using $b_{r,t}$ (Extra Fertility) and $b_{r,t}$ (Extra Scores) to measure material benefits are similar and are not further discussed. As the estimates show, bargaining power may help us understand F2, the increase of children following mother's ethnicity after the introduction of the one-child policy, but it cannot explain our main findings on Prediction P1 on the interaction between social and individual motives.

E.4. Censoring

To check for the possibility of censoring, we restrict the estimation sample to prefecture-cohorts with a share of minority children between 0.3 and 0.7. In this interval, there should be enough room for Han-minority couples in every prefecture-cohort to respond without hitting a constraint. As shown in the table below, the estimates from the restricted sample are similar to the baseline estimates from the full sample in Table 2A. In other words, upward censoring does not drive our main findings when testing Prediction P1 on individual-social interactions.

3. The shortage of Han women becomes more pronounced in recent years. One implication of our findings is that mixed-couple children increase with the shortage of Han women.

Table E4: Considering the Possibility of Censoring

	(1)	(2)	(3)	(4)	(5)	(6)
	following mother's ethnicity = 0/1					
I(≤ 0.5) \times b(Post Policy)		0.076*** (0.017)				
b(Post Policy)	0.108*** (0.015)					
I(≤ 0.5) \times b(Extra Fertility)				0.023 (0.015)		
b(Extra Fertility)			0.045*** (0.010)			
I(≤ 0.5) \times b(Extra Score)						0.028*** (0.010)
b(Extra Score)					0.052*** (0.010)	
Prefecture FE	Y	Y	Y	Y	Y	Y
Wife Ethnicity FE		Y		Y		Y
Birth Year FE		Y		Y		Y
Controls*b		Y		Y		Y
Province FE*Year FE		Y		Y		Y
Observations	54,387	48,522	47,327	42,299	56,922	51,057
R-squared	0.093	0.195	0.104	0.201	0.107	0.194

Notes: Standard errors are clustered at the prefecture level. Significance: ***, 1%, **, 5%, *, 10%.

E.5. Quantity of Children

Finally, to check whether the composition of children drives our findings, we present separate results for families with a single child in columns (1)-(3) in the table below, and for those with multiple children in columns (4)-(6). As the estimates show, the results for both types of households are similar to the baseline results in Table 2A. If anything, the pattern is slightly stronger for the single-child families. These findings imply that our baseline result on Prediction P1 are not driven by composition effects. This finding also suggests that our main insight is not acted by the quantity of children.

Table E5: Considering Composition Effects

	(1)	(2)	(3)	(4)	(5)	(6)
	following mother's ethnicity = 0/1					
	Single-child Family			Multiple-children family		
I(≤ 0.5) \times b(Post Policy)	0.074*** (0.017)			0.060*** (0.016)		
I(≤ 0.5) \times b(Extra Fertility)		0.037*** (0.010)			0.021* (0.011)	
I(≤ 0.5) \times b(Extra Score)			0.062*** (0.019)			0.023*** (0.010)
Prefecture FE	Y	Y	Y	Y	Y	Y
Wife Ethnicity FE	Y	Y	Y	Y	Y	Y
Birth Year FE	Y	Y	Y	Y	Y	Y
Controls*b	Y	Y	Y	Y	Y	Y
Province FE*Year FE	Y	Y	Y	Y	Y	Y
Observations	30,910	23,932	32,561	78,004	72,942	79,383
R-squared	0.290	0.297	0.284	0.357	0.363	0.354

Notes: Standard errors are clustered at the prefecture level. Significance: ***, 1%, **, 5%, *, 10%.