MISSING WOMEN AND THE PRICE OF TEA IN CHINA: THE EFFECT OF SEX-SPECIFIC EARNINGS ON SEX IMBALANCE*

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Abstract

Economists have long argued that the sex imbalance in developing countries is caused by underlying economic conditions. This paper uses exogenous increases in sex-specific agricultural income caused by post-Mao reforms in China to estimate the effects of total income and sex-specific income on sex-differential survival of children. Increasing female income, holding male income constant, improves survival rates for girls; while increasing male income, holding female income constant, worsens survival rates for girls. Increasing female income increases educational attainment of all children, while increasing male income decreases educational attainment for girls and has no effect on boys' educational attainment. (*JEL* D1, J13, J16, J24, O13)

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1 Introduction

Many Asian populations are characterized by severe male-biased sex imbalances. For example, whereas 50.1% of the current populations in western European countries are female, only 48.4% are female in India and China.[1] Amartya Sen (1990, 1992) referred to this observed deficit as "missing women". Most of the world's missing women are in China and India, where an estimated thirty to seventy million women are missing, but the phenomenon cannot be dismissed as a problem of the past or as one that is isolated to poor countries. Rich Asian countries such as South Korea and Taiwan have the same sex imbalance as their poorer neighbors, China and India. Figure I shows that in China, for cohorts born during 1970-2000, when the economy grew rapidly, the fraction of males *increased* from 51% to 57%. The observed sex imbalance may be achieved in a variety of ways, from sex-selective abortion to neglect or even infanticide.

This paper explores whether changes in relative female income (as a share of total household income) affects the relative outcomes for boys and girls. Previous work on this subject has been impeded by identification problems: areas with higher female income may have higher income precisely because women's status is higher for other reasons, which makes it difficult to estimate the effect of female income on boys and girls.[2] I address this omitted variable bias problem by taking advantage of two post-Mao reforms in China. During the Maoist era, centrally planned production targets focused on staple crops. In the early reform era (1978-1980), reforms increased the returns to cash crops, which included tea and orchards. Women have a comparative advantage in producing tea whereas men have a comparative advantage in producing orchard fruits. Therefore, areas suitable for tea cultivation experienced an increase in female-generated income while areas suitable for orchard cultivation experienced an increase in male-generated income. This makes it possible to use a differences-in-differences (DID) strategy to identify the causal effect of an increase in sex-specific income on outcomes for boys and girls.

To estimate the effect of a change in sex-specific incomes, I compare sex imbalance for

[1] Source: WDI indicators (2005)

[2] Empirical studies by Ben Porath (1967, 1973, 1976); Burgess and Zhuang (2001); Clark (2000); Duflo (2003); Das Gupta (1987); Foster and Rosenzweig (2001); Rholf, Reed and Yamada. (2005); Rosenzweig and Schultz (1982) and Thomas, Strauss and Henriques (1991) have shown that female survival rates are correlated with relative adult female earnings. A relatively new strand of the literature have argued over whether the observed sex imbalance can be partially explained by biological factors completely unrelated to cultural or economic conditions. See studies by Norberg (2004); Lin and Luoh (2006); and Oster (2005). And a recent study by the Lin, Liu and Qian (2007) investigates the effect of access to sex-selective abortion on sex ratios at births and sex-specific survival rates.

cohorts born before and after the reforms, between counties that plant and do not plant sex-specific crops where the value of those crops increased because of the reform.[3] I first estimate the effect of an increase in adult female income on sex imbalance (holding adult male income constant) by comparing the fraction of males born in counties that plant tea to counties that do not, between cohorts born before and after the price increase. Then, I repeat the same strategy using orchard production to estimate the effect of an increase in relative male income (holding adult female income constant). These estimates together allow me to distinguish the effects of increasing sex-specific (relative) incomes from the effects of increasing total household incomes. Finally, using the same strategy with educational attainment as an outcome, I estimate the effects of increasing sex-specific incomes on the educational attainment of boys and girls.

The results show that an increase in relative adult female income has an immediate and positive effect on the survival rate of girls. In rural China, during the early 1980s, increasing annual adult female income by US$7.70 (10% of average rural annual household income) while holding adult male income constant increased the fraction of surviving girls by one percentage-point and improved educational attainment for both boys and girls by approximately 0.5 years. Conversely, increasing male income while holding female income constant decreased both survival rates and educational attainment for girls, and had no effect on educational attainment for boys. These results show that the effect of an increase in the value of sex-specific crops is due to the change in the relative share of income between men and women rather than the change in total household income. This is consistent with the additional finding that an increase in the value of all cash crops, most of which do not especially favor male or female labor, had no effects on either sex-specific survival rates or educational attainment.

The empirical results have several theoretical implications for household decision making. The effects on survival can be easily explained by either a model of intra-household bargaining or a unitary model of the household in which parents view children as a form of investment. The results on education favor a non-unitary model of household decision making. The implication for policy makers is straightforward: factors that increase the economic value of women are also likely to increase the survival rates of girls and to increase education investment in all children.

This study has several advantages over previous studies. A number of potentially confounding factors were fixed in China during this period. Migration was strictly controlled, little technological change occurred in tea production, sex-revealing technologies were unavailable to the vast majority of China's rural population (Zeng, 1993; Diao, Zhang and Somwaru, 2000), and stringent family planning policies largely controlled family size.

[3] This identification strategy is similar to Schultz's (1985) study of Swedish fertility rates in the late 19th century, which used changing world grain prices to instrument for changes in the female-to-male wage ratio.

The paper is organized as follows. First, I describe the empirical strategy and policy background. Then, I discuss the conceptual framework. Third, I present the empirical results. Fourth, I interpret the results. Finally, I offer concluding remarks.

2 Empirical Strategy

This paper uses the value of tea to proxy for female wages and the value of orchards to proxy for male wages. Tea is picked mainly by women in China.[4] Data on labor input by sex and crop are not available for examining sex specialization directly from the 1990 *Population Census*. Instead, I use household level survey data from the Ministry of Agriculture's RCRE National Fixed Point Survey (NFS) from 1993 to examine the correlation between the fraction of female laborers and the amount of tea sown.[5] Table I columns (1)-(4) show that the amount of tea sown per household and the fraction of arable land that is devoted to tea per household are both negatively correlated with the fraction of male laborers within households. Tea bushes are approximately 2.5 feet (0.76 meters) tall. Picking requires the careful plucking of whole tender leaves. This gives adult women absolute and comparative advantages over children and men. For China, women's specialization in tea picking may have been increased by strictly enforced household grain quotas that forced every household to plant grain. This means that in households that wished to produce tea after the reform, men continued to produce grain while women switched to tea production. Moreover, the monitoring of tea picking is made difficult by the fact that the quality and value of tea leaves increases greatly with the tenderness of the leaf. This decreases the desirability of hired labor.[6] In contrast, height and strength yields a comparative advantage for men in orchard-producing areas.[7] Columns (5)-(8) in Table I show that the amount of orchards sown per household and the fraction of a household's arable land devoted to orchards is positively correlated with the fraction of male laborers within a household. In the 1982 *Population Census*, 56% of laborers in tea production (which includes

[4] See Lu (2004) for a detailed anthropological analysis of the historical role of women in tea picking.

[5] Please see De Brauw and Giles (2006) and Padro, Qian and Yao (2007) for detailed descriptions of the RCRE data.

[6] Agricultural households in general rarely hired labor from outside the family. In 1997, 1 per 1000 rural households hired a worker from outside of the immediate family (Diao, Zhang and Somwaru, 2000). Because migration and labor market controls were more strict in the 1980s, it is most likely that the households studied in this paper hired even fewer non-family members. Plentiful cheap adult labor also would reduce the demand for child labor.

[7] Adult men have a comparative advantage in orchard production during both sowing and picking periods. Sowing orchard trees is strength-intensive, as it requires digging holes approximately 3 feet (0.91 meters) deep. The height of the trees means that adult males have advantages, both in pruning and picking, over adult females and children.

picking, pruning and drying) are male, whereas 62% of laborers in orchard production are male.[8] Since female comparative advantage is in picking, this six-percentage point difference should be interpreted as a lower-bound estimate of female comparative advantage in tea-picking. The magnitude of the advantage does not affect the internal validity of the empirical strategy.[9]

A simple cross-sectional comparison of the fraction of males in counties that plant no tea to counties that plant some tea shows that the latter has one percentage-point fewer males, or one percentage-point more females (see Table II). But these estimates do not prove that planting tea, or higher relative female earnings, has a positive causal effect on female survival. The main confounding factor is that regions that choose to plant tea may be regions with weaker boy preference. In this case, the cross-sectional comparison will not be able to disentangle the effect of planting tea from the effect of the underlying boy-preference. To address this, I take advantage of two post-Mao reforms that increased the value of planting tea and orchards relative to staple crops. Hence, in addition to the cross-sectional comparison of the fraction of males between regions that produce tea and regions that do not, I can examine the second difference between cohorts born before the reform with those born afterwards (i.e. differences-in-differences).

The two reforms of interest to this paper are the increases in procurement prices of cash crops such as tea and orchards relative to staple crops and the *Household Production Responsbility System* (HPRS), which allowed households to take advantage of the price increases. Before 1978, Chinese agriculture was characterized by an intense focus on grain production, allocative inefficiency, lack of trade, lack of incentives for farmers and low rural incomes due to suppressed procurement prices (Perkins, 1966; Sicular, 1988a; Lin, 1988). Central planning divided crops into three categories. Category 1 included crops necessary for national welfare: grains, all oil crops, and cotton. In Category 2 were cash crops, including orchard products and tea (Sicular, 1988b). Category 3 included all other agricultural items (mostly minor local items). This last group was not under quota or procurement price regulation. The central government set procurement quotas for crops in Categories 1 and 2 that filtered down to the farm or collective levels. Quota production was purchased by the state at very low prices.

[8] This is the sample of adults who report living in rural areas and working in agriculture in the provinces of this study between the ages of 15-60. The data does not report hours worked. Due to problems of under-reporting girls at young ages due to the One Child Policy (1979/80), I cannot use the 1982 Census for the analysis in this paper.

[9] The magnitude of the advantage will affect the interpretation of the elasticity of demand for girls with respect to relative female earnings that underlies the reduced form effects estimated in this paper. For a given estimate of the effect of increasing tea prices on female survival, a smaller female advantage implies a larger elasticity.

These quotas were set so that farmers could retain enough food to meet their own needs but leave very little in surplus (Perkins, 1966). Non-grain producers produced grain and other food stuffs they needed for their own consumption.

Reforms in the post-Mao era (1978 and afterwards) focused on raising rural income, increasing deliveries of farm products to the state, and diversifying the composition of agricultural production by adjusting relative prices and profitability. Two sets of policies addressed these aims. The first set gradually reduced planning targets and represented a return to earlier policies that used procurement price as an instrument for controlling production (Sicular, 1988a). Although Category 1 crops benefited from the price increases, the increase in prices was greater for cash crops from Category 2. The second set of policies, the HPRS, was first enacted in 1980. It devolved all production decisions and quota responsibilities to individual households instead of production being a collective responsibility, and effectively allowed households to take full advantage of the increase in procurement prices by expanding production to cash crops when profitable (Johnson, 1996; Lin, 1988).[10] The two reforms contributed to diversification of agricultural production, greater regional specialization, and less extensive grain cultivation (Sicular, 1988a; Johnson, 1996). While agricultural households may not have viewed each specific reform as permanent, they were likely to have viewed the overall regime shift as permanent. Consequently, I only interpret this initial regime shift as plausibly exogenous.

Figure IIa shows that the reforms increased income from tea and orchards relative to income from Category 1 staple crops.[11] It also shows that income from tea did not exceed income from orchard production.[12] The increase in the relative value of Category 2 crops is also

[10]During the period of this study, there was no official market for buying or selling land. Agricultural land is allocated by the village to farmers based on characteristics such as the number of household members and land quality (Carter, Liu and Yao, 1995; Johnson, 1995; Kung, 1997; Kung and Liu, 1997; Rozelle and Li, 1998; Benjamin and Brandt, 2000; Jacoby, Li and Rozelle 2002; and Burgess, 2004). There is no evidence that the land allocation systematically differed between tea and non-tea producing regions.

[11]I use yearly data from the Ministry of Agriculture that reports output per standard day of labor by crop and procurment price data from the FAO. I assume that there are 257 labor days in a year and calculate for each crop:

$$yearlyinc = outputperday * 257 * price$$

[12]This addresses the possibility that the effect of income on sex ratio is not linear. An increase in income from tea translates into an increase in total household income as well as an increase in relative female income. I compare the effect of an increase in the value of tea to the effect of an increase in the value of orchard crops to discern whether sex ratios are responding to total income or to relative female income. However, if the income effect on sex ratio is non-linear, such that there exists some threshold income which must be met before income will affect sex ratio, then this strategy will only work if income from tea does not exceed income from orchard crops.

reflected in the disproportionate growth in their output relative to Category 1 crops. Figure IIb shows that although output for Category 1 crops increased, there was no change in the rate of increase. Figure IIc shows that the rate of increase for Cateogory 2 crops such as melons and orchard fruits accelerated after the reform, following the increases in procurement prices. Similar increases can be observed in Figure IId for tea.

The main effect of post-Mao reforms for tea production was increased picking, since most tea fields were sown during a rapid expansion program during the 1960s. The 50% increase in procurement price in 1979 was followed by extensive tending, pruning and picking (Etherington and Forster, 1994). Figures IIc and IId show the sudden increase in procurement prices and the corresponding increases in tea yield (output per hectare) and orchard production.

I estimate the effect of an increase in female labor on relative female survival by exploiting the variation in the price of tea caused by the post-Mao agricultural reforms. The reform increased the value of adult female labor in tea-producing regions. Hence, the intensity of treatment is positively correlated with the amount of tea sown. The increase should only affect individuals born close to and after the reform.[13] The date of birth and whether an individual is born in a tea planting region jointly determine whether she was exposed to the sex-specific income shock. I compare the fraction of males between counties that do and do not plant tea for cohorts born before and after the reform. Comparing the sex imbalance within counties across cohorts differences out time-invariant community characteristics. Comparing the sex imbalance within cohorts between tea planting and non-tea planting communities differences out changes over time that affect these regions similarly. I estimate the effect of increasing the value of adult male labor by exploiting the variation in prices of orchard fruits caused by the reforms. Finally, I estimate the effect of an increase in total household income without changing the relative shares of sex-specific incomes by exploiting the variation in prices for all cash crops where the vast majority of the crops are not known to favor either male or female labor.

Identification is based on the increase in the value of Category 2 crops relative to Category 1 crops, for which prices continued to be suppressed, and Category 3 crops, which were never regulated. Therefore, the effect of Category 1 and Category 3 crops on the fraction of males should not change after the reform. I test this by estimating the effect of Category 1 and Category 3 crops on the fraction of males by regressing the fraction of males on the interaction terms between the amount of Category 1 crops sown and birth year dummy variables and

[13] The exact timing of the response in sex ratios to the reform depends on the nature of sex selection. If sex selection was conducted via infanticide, then the reform should only affect sex ratios of cohorts born after the reform. However, if sex selection is conducted via neglect of young girls, then the reform also can affect sex ratios of children who were born a few years before it.

the interaction terms between the amount of Category 3 crops sown and birth year dummy variables.

$$sex_{ic} = \sum_{l=1963}^{1990} (cat1_i \times d_l)\beta_l + \sum_{l=1963}^{1990} (cat3_i \times d_l)\delta_l + Han_{ic}\zeta + \alpha + \psi_i + \gamma_c + \varepsilon_{ic} \qquad (1)$$

The fraction of males in county i, cohort c is a function of: the interaction terms between $cat1_i$, the amount of Category 1 crops planted for each county i, and d_l, a variable which indicates if a cohort is born in year l; the interaction terms between $cat3_i$, the amount of Category 3 crops planted in each county i, and d_l; γ_i, county fixed effects; and ψ_c, cohort fixed effects. The dummy variable for the 1962 cohort and all of its interactions are dropped. Figure IIIa plots the vector of coefficients for β_l and δ_l. It shows that the effects of planting Category 1 and Category 3 crops were close to zero before and after the reform.

The validity of the identification strategy does not rely on the assumption that only women pick tea. Tea is a proxy for female earnings. If men or children picked tea, the proxy for relative female income would exceed actual relative female income. Hence, the strategy would underestimate the true effect of relative female income on sex ratio. If there are any unobserved time-invariant cultural reasons that cause women to pick tea *and* affect the relative desirability of female children, then the effect will be differenced out by comparing cohorts born before and after the reform.

For the DID estimate, I restrict the sample to the cohorts born during 1970-1986 and estimate the following equation where $post_c$ is a dummy variable that equals one if individuals are born after 1979.

$$\begin{aligned} sex_{ic} &= (tea_i \times post_c)\beta + (orchard_i \times post_c)\delta + (cashcrop_i \times post_t)\rho \\ &\quad + Han_{ic}\zeta + \alpha + \psi_i + \gamma_c + \varepsilon_{ic} \end{aligned} \qquad (2)$$

The fraction of males in county i, cohort $post_c$ is a function of: the interaction terms between tea_i, the amount of tea planted for each county i, and $post_c$, a dummy variable which indicates if an individual is born after 1979; the interaction terms between $orchard_i$, the amount of orchard planted for each county i, and $post_c$; the interaction terms between $cashcrop_i$, the amount of all cash crops planted for each county i, and $post_c$; Han_{ic}, the fraction that is ethnically Han; γ_i, county fixed effects; and ψ_c, cohort fixed effects. The reference group is comprised of individuals born during 1970-1979. It and all of its interaction terms are dropped. If the increase in value of tea improved female survival, then it should be reflected in a decrease in the fraction of males born after the reforms, $\beta < 0$. Conversely, if an increase in the value of orchards worsened female survival, we would expect $\delta > 0$.

One pitfall of the DID approach is that it may confound the effects of the reform with the effects of other changes that may have occurred during the pre or post-reform period. For example, tea producing regions may have been experiencing different pre-trends in sex ratios relative to other regions, which may cause the DID estimate to be capturing differences between tea and non-tea areas besides the increase in tea value. An illustration of the DID estimate shows that this is not the case. Figure IIIb plots the fraction of males in each birth year cohort for tea planting and non-tea planting counties. The vertical distance between the two lines shows that prior to the reform, tea counties had more males, whereas after the reform, tea counties consistently had fewer males. The DID estimate will be the difference in the average vertical distance before and after the reform. The figure shows clearly that before the reform, tea areas had more boys than non-tea areas, whereas after the reform, there was consistently fewer boys in tea areas. Hence, the DID estimate will not be capturing differences in pre-reform trends in sex ratios between tea and non-tea regions.

I can examine whether the effect of planting tea on sex ratios occured for the birth years close to the reform more rigorously by regressing the fraction of males by county and year of birth on the interaction terms of the amount of tea sown in the county of birth and birth year dummy variables for all birth years.

$$\begin{aligned} sex_{ic} \quad = \quad & \sum_{l=1963}^{1990}(tea_i \times d_l)\beta_l + \sum_{l=1963}^{1990}(orchard_i \times d_l)\delta_l + \\ & \sum_{l=1963}^{1990}(cashcrop_i \times d_l)\rho_l + Han_{ic}\zeta + \alpha + \psi_i + \gamma_c + \varepsilon_{ic} \end{aligned} \qquad (3)$$

The fraction of males in county i, cohort c is a function of: the interaction terms between tea_i, the amount of tea planted for each county i, and d_l, a dummy variable which indicates if a cohort is born in year l; the interaction terms between $orchard_i$, the amount of orchard planted for each county i, and d_l; the interaction terms between $cashcrop_i$, the amount of all cash crops planted for each county i, and d_l; Han_{ic}, the fraction that is ethnically Han; γ_i, county fixed effects; and ψ_c, cohort fixed effects. The dummy variable for the 1962 cohort and all of its interactions are dropped. β_l is the effect of planting tea on the fraction of males for cohort l. If increasing the price of tea improved female survival, then β_l should be constant until approximately the time of the reform, after which it should become negative. Similarly, δ_l is the effect of planting orchards on the fraction of males for cohort l. If increasing orchard prices worsened female survival, then δ_l should be constant until approximately the time of the reform, after which it should become positive.

Another problem of the empirical strategy is that if, at the time of the reforms, there is a change in the attitudes that drive sex preference in tea planting counties, then the estimate of the effect of planting tea will capture both the relative female income effect and the effect of the attitude change. Or, if the increase in the value of tea changed the reason for women to pick tea, then the pre-reform cohort will not be an adequate control group. While I cannot resolve the former problem, the latter is addressed by instrumenting for tea planting with time invariant geographic data.[14]

Tea grows in very particular conditions: on warm and semi-humid hilltops, shielded from wind and heavy rain. Therefore, hilliness is a valid instrument for tea planting if it does not have any direct effect on differential investment decisions and is not correlated with any other covariates in equation (5). I check this assumption by estimating the impact of planting tea on sex ratios for a sample containing only tea counties and those non-tea counties that share a boundary with tea counties. Hilliness varies gradually. County boundaries are straight lines drawn across spatial areas. The results for this restricted sample are similar to the estimate for the whole sample, although the precision is reduced due to the smaller sample size. This adds to the plausibility of the identification strategy, unless potentially confounding factors change discretely across county boundaries. Note that since the amount of orchards sown is also an endogenous regressor, the 2SLS specification does not separately control for it. The following equation estimates the first stage effects of hilliness on tea production after the reform.

$$\begin{aligned} tea_i \times post_c &= (slope_i \times post_c)\lambda + (cashcrop \times post_c)\varphi \\ &\quad + Han_{ic}\zeta + \alpha + \psi_i + post_c\gamma + \varepsilon_{ic} \end{aligned} \tag{4}$$

The second-stage regression is as follows.

$$\begin{aligned} sex_{ic} &= (tea_i \times post_c)\beta + (cashcrop \times post_c)\varphi \\ &\quad + Han_{ic}\zeta + \alpha + \psi_i + post_c\gamma + \varepsilon_{ic} \end{aligned} \tag{5}$$

3 Conceptual Framework

Since prenatal sex-revealing technology was not available for the most relevant period of this study, the observed sex imbalance is caused by differential neglect of girls or in some cases, female infanticide. The probability of a girl surviving will increase with the desirability of girls relative to boys, and also with the cost of sex selection.

Regarding relative survival rates for girls, increasing the price of tea can operate through four channels. First, it can increase the relative desirability of having a girl by increasing

[14] I also find that planting tea had no effect on sex ratios for non-agricultural households living in tea planting counties. This suggests that between-county comparison is unlikely to capture spillover effects between agricultural and non-agricultural households.

parents' perceptions of daughters' future earnings relative to that of sons. Second, the increase in total household income can increase the relative desirability of girls if for some reason daughters are luxury goods relative to sons. Third, increasing female-specific income can improve mothers' bargaining powers. This will increase relative female survival rates if mothers prefer girls more than fathers. Finally, increasing the value of adult female labor can raise the cost of sex-selection since pregnancies must be carried to term before the sex of the child is revealed. The first, second and last explanations are consistent with both the unitary and non-unitary models of household decision making. The third explanation most likely to be consistent with non-unitary model of the household.[15]

The unitary model makes the strong prediction that an increase in income should have the same effect on household consumption regardless of which member of the household brings home the additional income.[16] Hence, the second explanation can be ruled out if an increase in the price of orchard products does not have the same effect on female survival rates as an increase in the price of tea. The number of potential explanations can be further refined by comparing the effects of increases in the prices of tea and orchard products on the relative educational attainment of girls. In this case, the opportunity cost of the mother's time is not applicable. The first explanation of a unitary household with investment motives requires that the effect of increasing the relative value of women's labor on girls' educational attainment be symmetric to the effect of increasing the relative value of men's labor on boys' educational attainment.

In short, the empirical results will be able to shed light on several joint hypotheses. I test the joint hypotheses that households are unitary and parents view children as a form of *consumption* by examining whether an increase in tea prices has the same effect on sex imbalance as an increase in orchard product prices. I test the joint hypotheses that households are unitary and parents view children as a form of *investment* by examining whether an increase in tea prices has the same effect on educational attainment for girls as the effect of an increase in orchard product prices on educational attainment for boys. The latter test relies on the assumption that returns to education are positive in tea and orchard producing areas; and that the relationship between adult female wages and the returns to education for girls are the same as the relationship between adult male wages and the returns to education for boys. The data used in this study does not allow a direct test of the opportunity cost hypothesis. Instead,

[15] See Bourguignon et. al. (1993) and Browning and Chiappori (1998) for a detailed theoretical discussion about models of collective household decision making. Thomas (1994), Duflo (2003), Park and Rukumnuaykit (2004), and Ashraf (2006) for empirical evidence on non-unitary households.

[16] For simplicity, I assume that members of a unitary-decision making household pool their income. See Browning, Chiappori and Lechene (2004) for a detailed discussion of the conditions for which household members do not pool their incomes and are still unitary.

I present two sets of indirect evidence later in the section on Interpretation that suggest that opportunity cost is not likely to play a large role in explaining the empirical results.

4 The Data

The analysis of the sex imbalance uses the 1% sample of the 1997 *Chinese Agricultural Census*, the 1% sample of the 1990 *China Population Census* and GIS data from the Michigan China Data Center.[17] The data are aggregated and matched at the birth-year-county level. The number of individuals in each county-birth year cell is retained so that the regression analyses are all population weighted. The sample includes all 1,621 counties from all fifteen provinces of southern China. Any province that produced any tea in the 1997 Agricultural Census is included.[18] The 1990 Census contains data on sex, year of birth, educational attainment, sector and type of occupation, and relationship to the head of household. Because of the different family planning policies and market reforms experienced by urban and rural areas, I limit the analysis to rural households. The sample used in the empirical analysis contains individuals born during 1962-1990, who are living in a rural area in 1990.

To avoid confounding the estimates with the effects of migration, I further restrict the data to individuals who report having lived in the same county for over five years. The data does not include the county of birth. The main analysis assumes that the county of birth is the county of residence for those who report having been there for five years or longer. This is consistent with studies on migration which find that strict migration controls were well enforced until the late 1990s.[19] Entire families and children did not migrate because they had no access to government-controlled food rations, housing, schools and medical care once they left their registered homes. The first wave of rural migration did not occur until the 1990s, during the urban construction boom, and most of those migrants were young adult men. Consequently, it is highly unlikely that the results of this paper, which mainly examines individuals who were children in 1990, are confounded by migration. Using data from RCRE's NFS for 1986-90, I find that the probability of having a household member work away from the home village is very low and similar for regions that produce tea and regions that do not produce tea. There are almost no migrants under the age of 20, which is consistent with the finding from other

[17] This section describes the 1% sample of the 1990 *Population Census*. The analysis of sex ratios uses only the 1990 Census and the analysis of education uses only a 0.05% sample of the 2000 census described in Appendix Table A.2. The organization of the censuses is similar.

[18] These include Jiangsu, Zhejiang, Anhui, Fujian, Jiangxi, Shandong, Henan, Hubei, Hunan, Guangdong, Guangxi, Sichuan, Yunnan and Shaanxi.

[19] See West and Zhao (2000) and De Brauw and Giles (2006) for a detailed description of migration policies and outcomes in China.

studies that there is little migration of children. For robustness, I deliberately over estimate the number of migrants from tea areas to calculate the lower bound of the absolute value of the effect of planting tea on sex imbalance.

The empirical strategy compares the fraction of males for cohorts born before and after the reform. Hence, the identification for this paper comes from changes in the fraction of males over time (across cohorts). One cross-section would not be able to distinguish between the two hypotheses: 1) variation in the cross-section is driven by differences across age groups – e.g. there are sex-differential mortality rates during childhood such that more boys are born and higher mortality rates for boys cause the fraction of males to be negatively correlated with age (*age effect*); and 2) variation in the cross section is driven by differences across birth cohorts – e.g. the fraction of boys born is increasing each year (*cohort effect*). Figure I plots the fraction of males by birth year from China's 1982, 1990 and 2000 Population Censuses. It shows that the fraction of males for each birth cohort are stable over time. Hence, the cross-sectional variation in the fraction of males by age in the data for this study can be interpreted as increases in the fraction of males over time.[20]

Figure I also addresses concerns that there might be under-reporting of female births due to the One Child Policy. Comparing hospital-level data to the census data, Zeng et al. (1993) find misreporting to be present only for extremely young children who are easy to hide from the authorities. This is consistent with Figure I, which shows that the fraction of males by birth year is stable over time. For the DID estimates, I only use data for children four years of age and older.

Reliable data for procurement prices and output are not available for this period at the county level. For the sake of scope, accuracy and consistency between areas, this study uses county-level agricultural data on the sown area from the 1% sample of the 1997 *China Agricultural Census*. Using 1997 agricultural data to proxy for agricultural conditions in the early 1980s introduces measurement error. It is also possible that the counties that produced tea in 1997 are the counties that had stronger preferences for girls prior to the reform. In this case, comparing the fraction of males in counties that planted tea in 1997 to the fraction of males in counties that did not plant tea in 1997 will confound the effect of planting tea with the effect of underlying preferences for girls. However, as discussed earlier, the government emphasis on tea planting during the Cultural Revolution meant that the main determinant of whether a region had tea fields was geographic suitability, not sex preferences. Specifically, tea grows best on warm and humid hilltops. The population density of the Chinese countryside and the distribution of hills throughout southern China mean that counties that plant tea should not

[20] Ideally, these three censuses could be linked so that these two effects could be separately identified. However, changes in the geographic identifiers make linking at the county level difficult.

be very different in other respects from their neighboring counties that do not plant tea. For robustness, I address these problems by instrumenting for tea planting with natural conditions.

To assess whether counties that do not plant tea are good control groups for counties that do plant tea, I look for systematic differences between the treatment and control groups. The average demographic characteristics and educational attainment shown in Table II Panel A are very similar between counties that plant any tea and counties that plant no tea. The difference in ethnic composition is controlled for in the regression analysis. The descriptive statistics for sector of employment in Panel B show that in both types of counties, 94% of the population is involved in agriculture. Panel C shows that households in tea counties farm less total land on average, devote more land to rice and garden production, and devote less land to orchards. Agricultural households have very little farmable land, with an average of only 4.06-4.85 mu (0.20-0.32 hectares) per household.[21] Households in counties that plant tea average only 0.15 mu (0.02 hectares) of land for tea. Figure IVa shows the counties that plant tea. Darker shades correspond to more tea sown. It shows that tea producing counties are geographically dispersed, which helps to alleviate concerns that they are systematically different along unobservable characteristics (e.g., culture) from the control group.[22]

5 Empirical Results

5.1 Results for Survival Rates

The difference-in-differences estimates from equation (2) are shown in column (1) of Table III. It shows that planting one additional mu of tea decreased the fraction of males by 1.2 percentage-points; planting one additional mu of orchards increased the fraction of males by 0.5 percentage points; and planting cash crops in general had no effect. The estimates for planting tea and orchard are statistically significant at the 10% and 5% level, respectively.

The estimates for β_l, δ_l and ρ_l from equation (3) are shown in appendix Table A.1. The coefficients for β_l and δ_l are plotted in Figure V. They show that for cohorts born prior to the reform, the effects of planting tea and orchards on the fraction of males were similar to each other and constant across cohorts. The effects diverge for cohorts born around the time of the reform, when planting tea is associated with fewer males, while planting orchards is associated with more males. The differential effects persist over time. These results lend credibility to the interpretation that the effect of tea and orchard production on the fraction of males is attributable to the post-Mao agricultural reforms and not to other changes in these regions.

[21] Mu, the Chinese unit for measuring area, is 1/15th of a hectare.

[22] Similar geographic dispersion can be observed for orchard-producing counties.

Cohort fixed effects control for variation across cohorts that do not also vary across counties. They cannot control for county-varying cohort trends that may have occurred over the 29 years of this study. I address this issue by controlling for linear cohort trends at the county level (e.g. interaction terms of county dummy variable with linear time trends). In order to make the estimates comparable to the 2SLS estimates, I restrict the sample to only counties for which there is geographic data and estimate the same specification as the second stage of the 2SLS. This differences-in-differences specification does not explicitly control for orchards because planting orchards is likely to be endogenous. Column (2) in Table III shows the basic fixed effects estimates. Column (3) shows the estimate when I control for county-level cohort trends. The point estimates are similar. They show that planting tea decreased the fraction of males by 1.3 and 1.2 percentage-points. Estimates from both specifications are statistically significant at the 5% level. Thus, the OLS estimates are robust to differential linear changes across cohorts between counties.

Two problems motivate the use of instrumental variables. First, using 1997 agricultural data to proxy for agricultural conditions in earlier years introduces measurement error that may bias the estimate towards zero. Second, the OLS estimate will suffer from omitted variable bias if families that prefer girls switch to planting tea after the reform. In this case, the OLS estimate will overestimate the true effect of an increase in the value of female labor because it will confound the aforementioned effect with the sex-preferences of households that switched to planting tea. I address both problems by instrumenting for tea planting with the average slope of each county.

Figure IVb shows the slope variation in China, where the darker areas are steeper. The predictive power of slope for tea planting can be seen by comparing the tea planting counties in Figure IVa with the hilly regions in Figure IVb. I use the GIS data pictured in Figure IVb to calculate the average slope for each county and estimate the following first-stage equation, where both the amount of tea planted and the slope is time-invariant.

Column (4) of Table III shows the first-stage estimate from equation (4). The estimate for the correlation between hilliness and planting tea, λ, is statistically significant at the 5% level. Column (5) shows the 2SLS estimate from equation (5). The estimate is larger than the OLS estimate and statistically significant. Column (6) shows the 2SLS estimate after controlling for county-level cohort trends. The estimate is similar in magnitude to the OLS estimate, but no longer statistically significant. The estimates with and without trends are not statistically different from each other. The estimate without trends is larger but also less precisely estimated. The 2SLS estimate in column (6) shows that conditional on county-level cohort time trends, the OLS estimate is not biased. Furthermore, the OLS and 2SLS estimates in columns (3) and (6) are almost numerically identical to the initial OLS estimate in column

(1). These results give confidence to the robustness of the initial OLS estimates.

5.2 Results on Educational Attainment

This analysis uses county-birth-year-level data from a 0.05% sample of the 2000 *Population Census*. The sample is selected based on the same criteria as the main sample from the 1990 *Population Census*. In order to confine the sample to children who had completed their education, I restrict it to cohorts born between 1962 and 1982. Individuals in the sample should not be affected by the Cultural Revolution because disruptions to schools generally were isolated to urban areas.[23] I use cohorts that were born before 1976 and thus had not yet reached public preschool age at the beginning of the reforms as the pre-reform control.[24]

The empirical strategy is the same as before. I estimate equation (2) with years of education as the dependent variable to examine the effect of planting tea, orchards, and all Category 2 cash crops on educational attainment for all individuals. I repeat the estimation for the sample of girls, the sample of boys, and the difference in education between boys and girls. This is first done with dummy variables indicating whether any tea, orchard or cash crops are planted in a county, and then with continuous variables for the amount of each crop that is planted. The estimates in Panel A of Table IV show that planting any tea at all increased all, female, and male educational attainment by 0.2, 0.25 and 0.15 years, respectively. On the other hand, planting any orchards at all decreased female educational attainment by 0.23 years and had no effect on male educational attainment. These estimates are statistically significant at the 1% level. Planting orchards had no effect on male educational attainment. The estimates in Column (4) show that planting tea decreased the male-female difference in educational attainment, whereas planting orchards increased the difference. The latter is statistically significant at the 1% level. The sample size for the estimate in Column (4) is smaller than the sample size for the estimate in Column (1) because not every county-birth year cell contains both males and females. The estimates for all category 2 cash crops are close to zero and statistically insignificant.

I repeat this estimate with continuous variables for the amount of tea and orchards planted in each county i. Columns (5)-(8) of Table IV show that the estimates have the same signs as the estimates with the dummy variables in columns (1)-(4). The estimates show that one additional mu of tea planted increases female educational attainment by 0.38 years and male educational attainment by 0.5 years, whereas one additional mu of orchards decreases female

[23] For robustness, I repeat the experiment on a sample of cohorts born after 1967 who did not begin primary school until after 1974 when schools were re-opened. The results are similar and statistically significant.

[24] Children enter public preschools at age 4 or 5 in China during this period. Public nursery schools, targeted at children age 1-4, are not available to most rural populations.

educational attainment by 0.12 years and has no effect on male educational attainment.

To observe the timing of the effect of tea on educational attainment, I examine the the effect of planting tea by birth year. I estimate equation (3) with years of education as the dependent variable to examine the the effect of planting tea by birth year. The dummy variable for the 1962 cohort and all its interactions are dropped. I plot the three-year-moving averages for the estimated coefficients for each cohort l in vectors β_l and δ_l in Figure VI. This shows that female educational attainment was similar between tea and orchard areas until 1976, after which it increased in the former and decreased in the latter.

5.3 Robustness

5.3.1 Family Planning Policies

If the enforcement of family planning policies systematically varied between tea planting and non-tea planting regions, the empirical strategy will confound the effects of planting tea with the effects of family planning policies. Family planning policies in China began with a four-year birth spacing law in the early 1970s. The One Child Policy was introduced in 1979/80. Enforcement in rural areas was phased in during the early 1980s. The One Child Policy applied to all healthy parents of the Han ethnicity, which comprises of 92% of China's total population.[25] Qian (2006) shows that for rural areas, the four-year birth spacing law combined with the One Child Policy meant that the unanticipated One Child Policy was, in practice, binding for cohorts born in 1976 and later. Hence, the effective date of the One Child Policy does not coincide with the increase in the price of tea in 1979. I can also investigate the correlation between tea and orchard production and family planning policies directly. The 1989 China Health and Nutritional Survey (CHNS) reports data on local enforcement of family planning policies. Matching this data to the data used in this study, I find no correlation. The sample of matched counties are too few to use for statistical analysis controlling for family planning laws.

I perform two additional robustness checks using the fact that non-Han ethnic minorities are largely exempt from family planning restrictions. First, I control for the interaction term between the fraction that is Han and birth year dummies. Next, I re-estimate equation (2) using a sample containing only ethnic minorities. In both cases, the estimates are similar to the main results, suggesting that they are not confounded by family planning policies. These estimates are not reported in the paper for the sake of brevity.

[25] See Qian (2006) for a detailed description of the One Child Policy, exemptions, and later relaxations.

5.3.2 Migration

If migration patterns differed significantly between tea and non-tea areas, then the OLS estimates could be capturing the effects of migration rather than those of income changes. In particular, suppose that females born before 1979 are disproportionately likely to leave tea planting areas relative to non-tea areas, whereas out-migration patterns are the same in both areas for later cohorts. Then the empirical strategy would incorrectly attribute changes in the sex imbalance to changes in sex-specific survival rates rather than changes in migration. The same reasoning would apply to changes in male migration from orchard areas.

To address migration more directly, I deliberately over-estimate the number of female migrants from tea areas. Recall that the DID estimate presented earlier used a sample of individuals who were four to twenty years old in 1990. Using the 2000 *Population Census*, which reports whether an individual is currently living in his/her county of birth, I assume that *all* individuals under 20 years old living away from his/her county of birth are *females* born in tea planting regions before 1979 (even for example, fifteen year old men). Then, I add these migrants into the 1990 data and re-estimate equation (2). This is even more conservative than it first appears, since migration rates were approximately an order of magnitude greater in 2000 than they were in 1990. Even with this extremely conservative approach, the resulting DID estimate scarcely changes. These results are not reported in the paper.

6 Interpretation

This section discusses the empirical results and their theoretical implications. The results for survival rates show that the increase in the value of tea improved female survival. Data on agricultural income by crop is not widely available for the time period of this study. If the data on agricultural income used by Etherington and Forster's (1994) anthropological study of Chinese tea plantations are representative of the average tea planting household, then the findings imply that augmenting annual household income by 10%, and giving it all to women, increases the fraction of girls by 1.3 percentage points. This would increase educational attainment for boys and girls by approximately 0.2 years. Roughly speaking, this suggests that increasing household income by 20% and giving it all to women would have brought China's sex imbalance in the early 1980s to about the level of Western Europe. These calculations, provided for illustrative purposes, assume that the elasticity of demand for girls relative to boys with respect to sex-specific earnings is linear, whereas they are likely to be highly concave in reality.

The empirical results have several theoretical implications. The findings for both survival and education reject the joint hypotheses that households are unitary (under the assumption that unitary households are income-pooling) and parents view girls as luxury goods relative to boys. An alternative explanation for the results within the unitary framework is that parents view children as a form of investment. This is consistent with the results for survival. However, if parents view returns from having children in the same way as returns from children's education, then this hypothesis cannot easily explain the results for educational attainment. To see why the empirical results favor a non-unitary framework, I consider the two scenarios. First, if there are zero returns to education, the explanation can obviously not be about investment motives. Second, if there are positive returns to education, then the results on education are only consistent with an unitary model with investment motives if there are positive returns to producing tea for boys and girls, no returns to producing orchards for boys and negative returns for girls. It is difficult to think of why this would be the case. A model with intra-household bargaining provides a simple alternative explanation. If mothers value education more than fathers and face a higher cost of neglecting children of either sex, then increasing mothers' bargaining power will lead to the equalization in treatment of boys and girls, which will in turn be reflected in the data as an increase in relative female survival rates.

The empirical findings cannot directly test the hypothesis that female survival is increasing because the opportunity cost of a woman's time is increasing. However, there are several reasons to think that this is not the primary explanation. First, following the strategy used in Pitt and Sigle (1998), I investigate the possibility that fertility decisions are responsive to the cost of women's time by using data on the month of birth to examine whether fertility decisions are timed according to planting or harvesting seasons. I find no evidence of fertility timing. Second, I examine the effect of planting tea on female survival during the late 1980s, when ultrasound B, the technology that enables pre-natal sex-detection, began to diffuse through China making sex-selective abortion possible. There is no reason to believe that the technology diffused differentially through tea and non-tea areas. The cost of sex selection should have decreased in all areas. If the effect of the increase in tea prices was to improve female survival mainly by increasing the opportunity cost of sex selection in tea areas relative to other areas, then the decrease in the cost of sex selection would have diminished this relative difference in opportunity cost. Consequently, we should observe the effect of planting of tea decreasing in magnitude in the late 1980s. Figure V shows that the effect of planting tea persisted over time. In another study, I investigate the impact of a reduction in the cost of sex selection on sex imbalance by examining the effects of the introduction of sex-selective abortion.[26]

[26] Lin, Liu and Qian (2007) examines the impact of sex-selective abortion on sex ratios at birth.

7 Conclusion

This paper addresses the long-standing question of whether economic conditions affect outcomes for girls relative to boys. Methodologically, it addresses the problem of joint determination in estimating the effects of changes in adult income on the survival rate of girls. It does this by exploiting changes in sex-specific incomes caused by post-Mao reforms in rural China during the early 1980s. The empirical findings provide a clear affirmative answer: both sex imbalance and educational attainment respond quickly to changes in sex-specific incomes. In addition, increasing total household income without changing the relative shares of female and male income has no effect on either survival rates or education investment. Several past studies have found that China's gender wage gap increased by over 100% from 1976 to 1984.[27] The findings of this paper suggest that this increase in the female wage disadvantage could be an important source of the growth of missing women in China during this period. Similarly, the increase in the gender wage gap may be one of the causes of the observed decline in rural school enrollment during the early 1980s.[28] The policy recommendation from these results is clear. One way to reduce excess female mortality and to increase overall education investment in children is to increase the relative earnings of adult women.

[27] Many studies estimate China's gender wage gap to have increased by over 100% since 1976. Before the reform, compensation for workers was set according to education, experience and skill. There was no official differentiation between sexes (Rozelle, Dong, and Zhang, 2002; Cai, Park and Zhao, 2004).

[28] See Hannum and Park (2005) for a description of the decrease in schooling in rural China during the early reform period.

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Table I – The Correlation between Sex Ratios of Adult Laborers and Tea and Orchard Production

Coefficients of the fraction of males amongst adult laborers per household

	Dependent Variables							
	Tea Land Sown (Mu=1/15 Hectare)		Tea Land/Total Arable Land		Fruit Land Sown (Mu=1/15 Hectare)		Fruit Land/Total Arable Land	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
# Male/# Total Labor in HH	-0.115	-0.086	-0.040	-0.010	0.0002	0.065	0.005	0.015
	(0.056)	(0.055)	(0.021)	(0.022)	(0.106)	(0.037)	(0.016)	(0.004)
Village Fixed Effects	N	Y	N	Y	N	Y	N	Y
Observations	3488	3488	3457	3457	3488	3488	3457	3457
R-squared	0.00	0.14	0.00	0.18	0.00	0.06	0.00	0.05

Standard errors are clustered at the village level.
Data for land sown are from the 1997 China Agricultural Census.
Data source: RCRE 1993 Household Survey

Table II – Descriptive Statistics

The Matched Dataset of the 1% Sample of the 1990 Population Census and the 1% Sample of the 1997 Agricultural Census

	I. Counties that Plant No Tea			II. Counties that Plant Some Tea		
	Obs	Mean	Std. Err.	Obs	Mean	Std. Err.
A. Demographic Variables						
Fraction male	41665	0.51	(0.0003)	10101	0.52	(0.0007)
Age	41665	14.00	(0.0410)	10101	14.00	(0.0833)
Han	41665	0.95	(0.0009)	10101	0.88	(0.0027)
De-collectivized	41665	0.99	(0.0002)	10101	0.99	(0.0004)
Household size	41665	5.22	(0.0132)	10101	5.16	(0.0261)
Married	23641	0.62	(0.0002)	7164	0.62	(0.0004)
Years of Education	32785	6.63	(0.0095)	7996	6.38	(0.0205)
(Female)	37653	4.70	(0.0082)	9465	4.39	(0.0148)
(Male)	37618	6.01	(0.0072)	9465	5.69	(0.0130)
Father's Education	40647	6.17	(0.0067)	10043	5.82	(0.0127)
Mother's Education	40655	4.53	(0.0082)	10054	4.33	(0.0146)
School Enrollment (Female)	40781	0.24	(0.0018)	10009	0.22	(0.0036)
School Enrollment (Male)	40636	0.27	(0.0019)	9977	0.25	(0.0038)
B. Industry of Occupation of Household Head						
Agricultural	41665	0.94	(0.0006)	10101	0.94	(0.0013)
Industrial	41665	0.04	(0.0005)	10101	0.04	(0.0009)
Construction	41665	0.01	(0.0001)	10101	0.00	(0.0002)
Commerce, etc.	41665	0.01	(0.0001)	10101	0.01	(0.0002)
C. Agricultural production and Land Use (Mu = 1/15 Hectare)						
Farmable land per household	23018	4.87	(0.0150)	10101	4.06	(0.0211)
Rice Sown Area	23018	1.66	(0.0106)	10101	2.55	(0.0106)
Garden Sown Area	23018	0.23	(0.0029)	10101	0.34	(0.0047)
Tea Sown Area	41665	0.00	(0.0000)	10101	0.15	(0.0034)
Orchard Sown Area	23018	0.20	(0.0029)	10101	0.16	(0.0034)

Sample of those born in during 1962-1990.

Data for land area sown are from the 1997 China Agricultural Census.
Observations are birth year x county cells.
Cell size: Mean=89, Median=68.

Table III – OLS and 2SLS Estimates of The Effect of Planting Tea and Orchards on Sex Ratios Controlling for County Level Linear Cohort Trends

Coefficients of the Interactions between Dummies Indicating Whether a Cohort was Born Post Reform and the Amount of Tea Planted in the County of Birth

	Dependent Variables					
	Fraction of Males			Tea*Post	Fraction of Males	
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	OLS	1st	IV	IV
Tea * Post	-0.012	-0.013	-0.012		-0.072	-0.011
	(0.007)	(0.006)	(0.005)		(0.031)	(0.007)
Orchard * Post	0.005					
	(0.002)					
Slope * Post	-0.002			0.26		
	(0.002)			(0.057)		
Linear Trend	No	No	Yes	Yes	No	Yes
Observations	28349	37756	37756	37756	37756	37756

All regression include county and birth year fixed effects and controls for Han, and cashcrop*post.

All standard errors are clustered at the county level.

In column (1), the sample includes all individuals born during 1970-1986.

In columns (2)-(6), the sample includes all individuals born during 1962-1990.

Post=1 if birthyear>1979.

Data for land area sown are from the 1997 China Agricultural Census.

Table IV – The Effect of Planting Tea, Orchards and Category 2 Cash Crops on Education Attainment
Coefficients of the Interactions between Dummies Indicating Whether a Cohort was Born Post Reform and the Amount of Tea, Orchard or Cash Crops Planted in the County of Birth

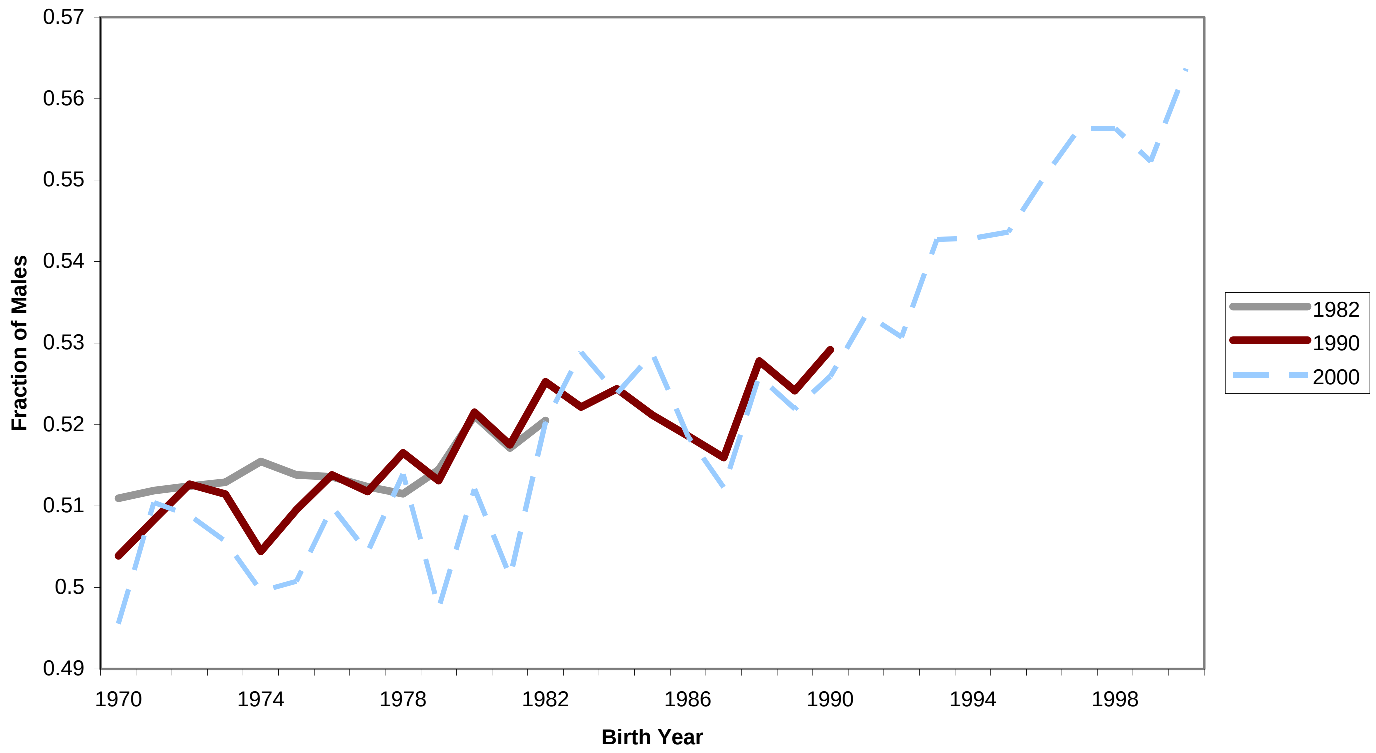
	Dependent Variable: Years of Education							
	A. Dummy Variable for Crops Sown				B. Continuous Variable for Amount of Crops Sown			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All	Female	Male	Diff	All	Female	Male	Diff
Tea * Post	0.199	0.247	0.149	-0.069	0.449	0.383	0.501	-0.097
	(0.043)	(0.057)	(0.049)	(0.063)	(0.107)	(0.133)	(0.146)	(0.218)
Orchard * Post	-0.124	-0.226	-0.029	0.174	-0.021	-0.119	0.054	0.118
	(0.037)	(0.050)	(0.040)	(0.056)	(0.056)	(0.071)	(0.064)	(0.086)
Cat2 * Post	-0.036	-0.024	-0.037	-0.020	-0.065	-0.040	-0.074	-0.012
	(0.026)	(0.032)	(0.028)	(0.040)	(0.032)	(0.041)	(0.035)	(0.050)
Observations	68522	33538	34984	58314	68522	33538	34984	58314
R-squared	0.37	0.48	0.34	0.14	0.37	0.48	0.34	0.14

All regressions include controls for Han, county fixed effects and birth year fixed effects.

All standard errors clustered at the county level.

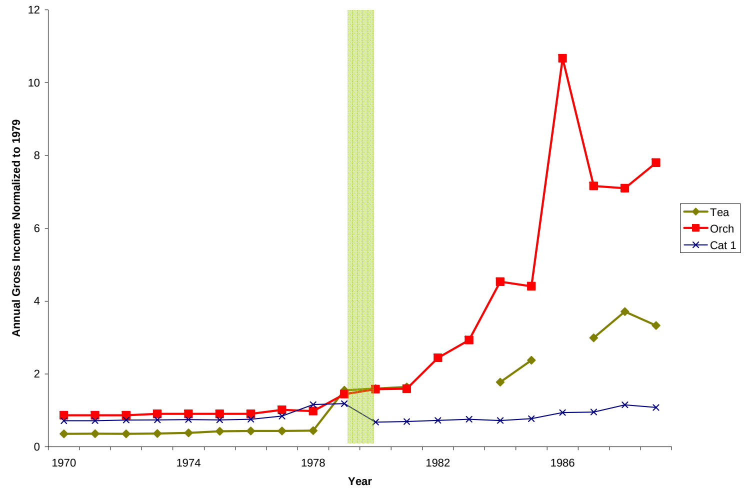
Post = 1 for cohorts born after 1976.

Figure I – Sex Ratios by Birth Year in Rural China



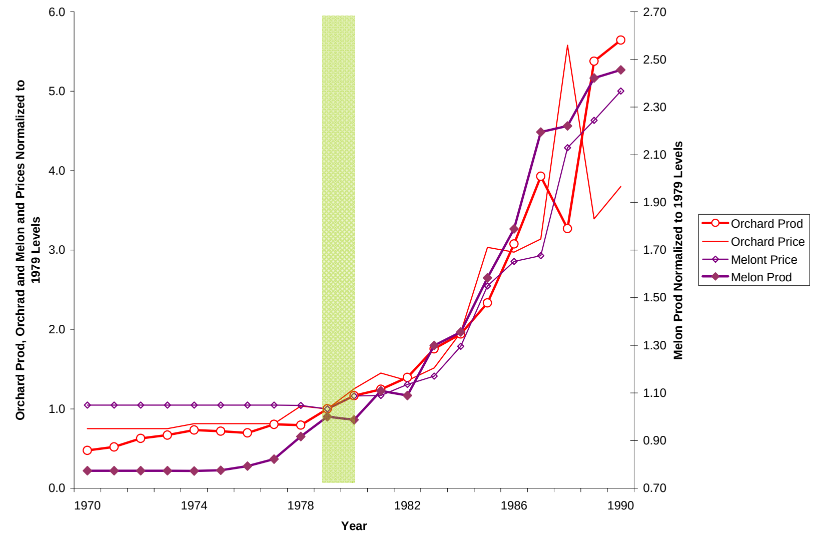
Source: The 1% Sample of the 1982 and 1990 *China Population Censuses* and the 0.05% Sample of the 2000 *China Population Census*

Figure IIa – Gross Agricultural Incomes from Producing Tea and Category 1 Crops



Source: FAO and Ministry of Agriculture of China; Note: the missing data points reflect years for when labor output data is missing.

Figure IIc – Category 2 Production: Orchard and Melon Production and Procurement Prices



Source: FAO

Figure IIb – Category 1 Production: Grains

Source: FAO

Figure IId – Tea Yield and Tea Procurement Price

Source: FAO

Figure IIIa – The Effect of Category 1 and 3 Crops on Sex Ratios

Coefficients of the Interactions Birth Year * Amount of Category 1 Crops Planted and Birth Year * Amount of Category 2 Crops Controlling for Year and County of Birth FE

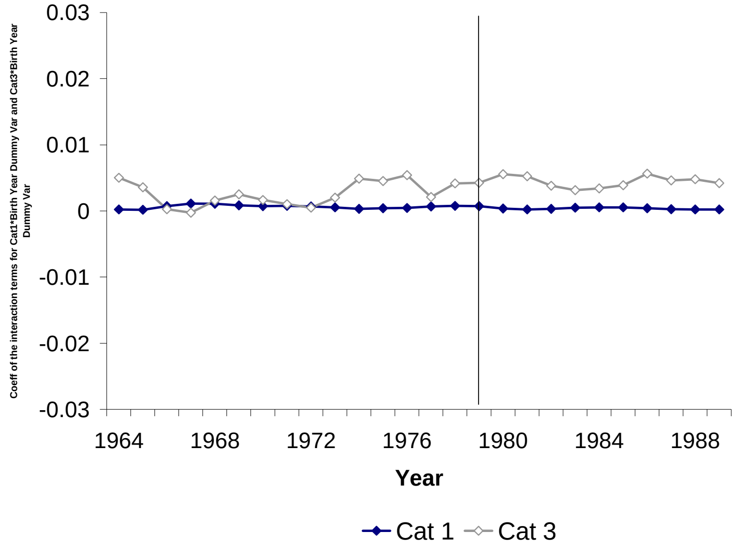
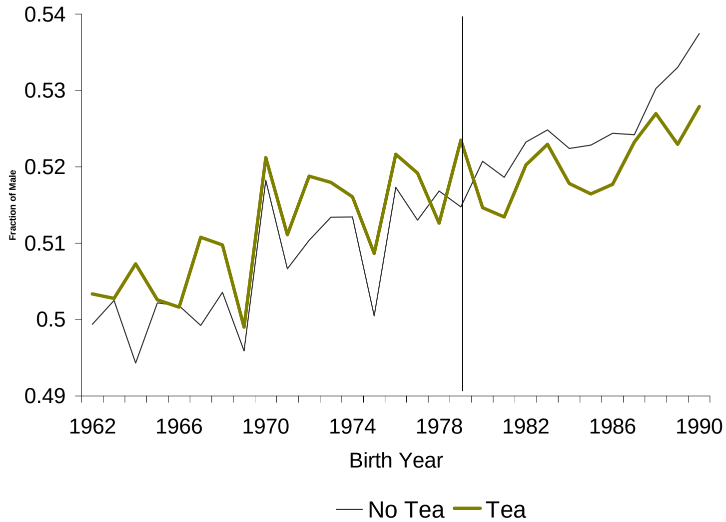


Figure IIIb – Fraction of Males in Counties which Plant Some Tea and Counties which Plant No Tea



Source: 1% Sample of 1990 Population Census; Note: Tea counties are defined as all counties that plant some tea

Figure IVa - Tea Planting Counties in China
Darker shades correspond to more tea planted per household.

Figure IVb - Hilliness
Darker shades correspond to steeper regions.

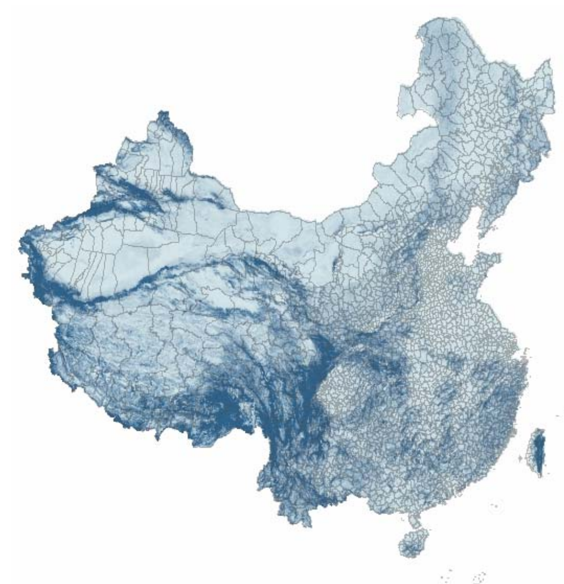


Figure V – The Effect of Planting Tea and Orchards on Sex Ratios

Coefficients of the Interactions of Birth Year * Amount of Tea Planted & Birth Year * Amount of Orchards Planted Controlling of Year and County of Birth FEs

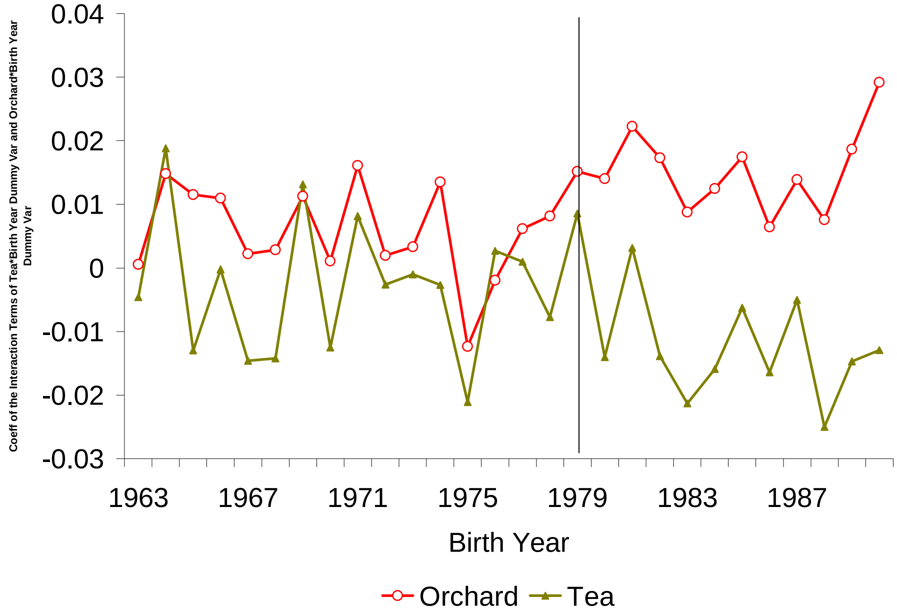


Figure VI – The Effect of Planting Tea and Orchards on Girls' Education Attainment

Coefficients of the Interactions Birth Year * Amount of Tea Planted and Birth Year * Amount of Orchards Planted Controlling for Year and County of Birth FEs

Table A.1 – The Effects of Tea, Orchards and Cash Crops on Fraction of Males
Coefficients of the Interactions Between Dummies Indicating Birth Year and the Amount of Tea, Orchards and Category 2 Cash Crops Planted in the County of Birth Controlling for Year and County of Birth FEs

Dependent Variable: Fraction of Males						
	Tea (1)		Orchards (2)		Cat 2 Cash Crops (3)	
Birth Year	Coeff.	Std. Error	Coeff.	Std. Error	Coeff.	Std. Error
1963	-0.005	(0.016)	0.001	(0.009)	0.000	(0.002)
1964	0.019	(0.026)	0.015	(0.010)	-0.001	(0.002)
1965	-0.013	(0.016)	0.012	(0.009)	-0.003	(0.002)
1966	0.000	(0.016)	0.011	(0.009)	-0.001	(0.002)
1967	-0.015	(0.018)	0.002	(0.009)	0.000	(0.002)
1968	-0.014	(0.017)	0.003	(0.009)	-0.003	(0.002)
1969	0.013	(0.018)	0.011	(0.009)	-0.001	(0.002)
1970	-0.013	(0.019)	0.001	(0.010)	-0.004	(0.002)
1971	0.008	(0.014)	0.016	(0.011)	-0.002	(0.002)
1972	-0.003	(0.014)	0.002	(0.010)	-0.003	(0.002)
1973	-0.001	(0.013)	0.003	(0.010)	-0.004	(0.002)
1974	-0.003	(0.017)	0.014	(0.010)	-0.003	(0.002)
1975	-0.021	(0.016)	-0.012	(0.011)	-0.002	(0.002)
1976	0.003	(0.023)	-0.002	(0.012)	-0.002	(0.002)
1977	0.001	(0.021)	0.006	(0.009)	-0.002	(0.002)
1978	-0.008	(0.016)	0.008	(0.009)	-0.004	(0.002)
1979	0.009	(0.014)	0.015	(0.010)	-0.001	(0.002)
1980	-0.014	(0.017)	0.014	(0.009)	-0.004	(0.002)
1981	0.003	(0.018)	0.022	(0.010)	-0.004	(0.002)
1982	-0.014	(0.014)	0.017	(0.010)	0.000	(0.002)
1983	-0.021	(0.018)	0.009	(0.008)	-0.002	(0.002)
1984	-0.016	(0.021)	0.012	(0.009)	-0.005	(0.002)
1985	-0.006	(0.019)	0.017	(0.009)	-0.003	(0.002)
1986	-0.016	(0.017)	0.006	(0.009)	-0.004	(0.002)
1987	-0.005	(0.018)	0.014	(0.009)	-0.001	(0.002)
1988	-0.025	(0.015)	0.008	(0.009)	-0.005	(0.002)
1989	-0.015	(0.022)	0.019	(0.009)	-0.005	(0.002)
1990	-0.013	(0.023)	0.029	(0.011)	-0.002	(0.002)
Observations			49082			
R-Squared			0.14			

All regressions include controls for Han, and county and birth year fixed effects.

Standard errors clustered at county level.

Table A.2 – Descriptive Statistics of the 0.05% Sample of the 2000 Population Census

	Counties that Plant No Tea			Counties that Some Tea		
	Obs	Mean	Std. Err.	Obs	Mean	Std. Err.
Fraction of Male	81774	53.31%	0.0017	25290	53.56%	0.0031
Fraction of Han	81774	93.47%	0.0008	25290	86.05%	0.0019
Years of Education	81774	7.14	0.0110	25290	6.89	0.0198
Male-Female Education	58590	0.55	0.0071	18034	0.55	0.0141
Fraction with Tap Water	81441	31.39%	0.0012	25182	37.60%	0.0021

Sample of individuals born 1962-1986

Observations are Birth Year x County Cells