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Long-Term Effects of Legalized Abortion on Female Education in Taiwan

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Increasing access to sex-selective abortions in societies with a male preference should, theoretically, increase investments and the level of care provided for girls who are not aborted. Existing literature finds that higher birth order sex selection increases following the legalization of abortion in Taiwan. This research presents evidence that the legalization of abortion in Taiwan improved educational outcomes for girls born at higher birth orders where sex selection is most common. Specifically, I find that girls born at higher birth orders after the legalization of abortion experience an improvement in their university attendance rates by approximately 4.5 percentage points. Moreover, a similar improvement in university attendance rates for higher birth order boys is not found. The findings in this analysis are robust to several specifications, and they extend existing literature by providing evidence of the substitution hypothesis for a later life economic outcome. (*JEL* J13, A22)

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1 Introduction

While the natural sex ratio at birth (henceforth SRB) is between 105 and 106 boys per 100 girls, a SRB as high as 110 has been observed in Taiwan (Chu and Yu, 2010). Sex selection is known to be the cause of unusually high male to female sex ratios in many Asian countries. Sex selection occurs either prenatally when there are gender-based abortions or postnatally when relatively worse care for infants results in higher death rates for children of the less preferred gender. Families in these societies prefer male children over female children for two reasons: (1) they desire to preserve the family name and (2) in many Asian countries, sons are more likely to financially support parents (Lin et al., 2003). Nations with particularly high SRB have expressed concerns regarding unbalanced sex ratios and, although often not strictly enforced, some have placed legal bans on the practice of sex selection. India, for example, banned sex detection tests in 1994, and similar policies are in place in China and South Korea (Vogel, 2012). While there are clear inefficiencies from high sex ratios such as marriage markets failing to clear, other implications of sex selection are not as obvious nor clearly deleterious. Studies have found improved early life female health outcomes after families are able to make endogenous gender-based abortion decisions (Lin et al., 2003; Dasgupta, 2010). This paper provides evidence that the legalization of abortion, hence sex selection, also improves later life educational outcomes for women in Taiwan.

The substitution hypothesis, as outlined by Daniel Goodkind in 1996, posits that increasing prenatal discrimination results in decreased postnatal discrimination, or that postnatal and prenatal discrimination are substitutes. Because many families who have low preferences for a girl will choose to abort, an average girl born after the legalization of abortion is born into a family who has a stronger desire for her in comparison to an average girl born before abortion is made legal. Although fewer in number, girls born in a sex-selective society with legalized abortion will be, on average, more desired than girls born before the legalization of abortion. Since boys are almost always desired in a society with male preference, the availability of sex-selective abortion does not drastically shift the composition of families who have boys towards those who desire them more. [1]

I investigate whether the ability to prenatally sex select through legal abortion and sex detecting technology in a society with a male preference improves investments in female children. I focus on university attendance in Taiwan as a measure of investment. Since university tuition is a cost often borne by parents, university attendance signals high investments in a child.[2] Moreover, children that attend a university are more likely to have had early life investments such as private schooling, more parental attention, and better health care.[3]

Following Lin et al. (2008), I exploit the legalization of abortion in Taiwan as a policy that exogenously increased sex selection at the highest birth orders in Taiwan. As in Lin et al. (2008), I use both the variation in sex selection across birth orders as well as the variation over time (before and after the legalization of abortion) to estimate a difference-in-difference model to determine whether abortion legalization improved the likelihood that higher birth order girls have ever attended a university. I find that abortion legalization in Taiwan increases the probability of attending a university for a second and higher birth

[1] If there are families that strictly have a preference for a girl and they abort male children to increase chances of a female birth, then a similar compositional shift would be observed for boys and an average boy born after abortion legalization will also be born into a family that desires him more. Given that the society under consideration has a male preference, the compositional change of families in favor of boys is small, if any at all. Results in this paper find this to be the case, and I do not find any evidence suggesting that such a compositional shift occurs for males.

[2] Costs of university tuition for 2003-2004 were $NT 58,714 for public universities and $NT 107,360 for private universities (approximately 24 percent and 44 percent of personal income per capita for public and private universities respectively). Source: Ministry of Education: Republic of China (Taiwan)

[3] Maluccio et al. (2009) find that an 8 year long nutrition intervention in Guatemala led to an increase in later life educational attainment for children who were under the age of 7 during the intervention. A quarter of a century later, women had completed 1.2 higher grades and both men and women had a quarter of a standard deviation higher scores in reading and comprehension tests.

order girl by about 4.5 percentage points, while no increase is observed for a second or higher birth order boy. The next section provides a brief overview of abortion policy in Taiwan.

2 Background

Taiwan legalized abortion on January 1st of 1985 under the *Eugenic Health Law* in response to a feminist movement which demanded the legalization of safe abortions (Lin et al., 2008). The law legalized abortions for fetal, maternal or social reasons during the first 6 months of gestation (Chiang, 2005). Only those born after the first 4 months of 1985 could be aborted, and 1986 was the first full year in which children could have been sex selected. Prior to 1985, abortion in Taiwan was only legal in cases of rape or if the fetus had a genetic disorder (Lin et al., 2008). At the time, contraception use was high and fertility rates were declining. In 1965, an extensive and highly effective family planning program under the Taiwan Provincial Institute of Family Planning was introduced, and by 1985, 95 percent of all married women in Taiwan had used some form of contraception (Chu and Yu, 2010). Ultrasound technology has also been present in Taiwan since its introduction worldwide in the early 1980s (Lin et al., 2008). The cost of abortion during the 1980s was roughly 1 percent of an average household's income (Lin et al., 2008).

Abortion in Taiwan is used to both control fertility and to sex select. Using data from Knowledge, Attitudes, and Practice of Contraception in Taiwan: Family and Fertility Survey (KAP Survey), Lin, Qian and Liu show that the percentage of women who have ever had an abortion increased from 23 percent in 1985 to approximately 27 percent in 1992. The KAP survey does not specify whether the abortions were performed for medical or other reasons, and this distinction cannot be determined from the data (Lin et al., 2008). This increase may not seem very large, but the KAP survey are self reported and it is possible that individuals underreport abortions. Additionally Lin, Liu and Qian also note that the number of doctors with registered ultrasound machines increased from 557 to 3027 from 1984 to 1989. Since abortion combined with ultrasound technology allows termination of pregnancy based on gender preferences, the legalization of abortion in Taiwan presents an exogenous shift in families' sex selection abilities. I exploit the variation created by the law change and investigate the effect of the legalization of abortion on gender-specific investment decisions in education for children. The next section discusses the data.

3 The Data

I use the Taiwan Family Income/Expenditure Survey, a nationally representative survey of randomly selected registered households in Taiwan. These data can be requested from Survey Research Data Archive (SRDA). My main analysis uses survey years 1996 to 2010 and focuses on children who are of college-age and born between 1978 and 1992. Between 13,000 to 15,000 households are surveyed each year. Although some households are repeated in different surveys, unique identifiers for households are not provided, hence the analysis treats the data as a cross-section over time. A household is defined as a group of individuals sharing a home. Additionally, individuals are considered part of a household if they contribute at least 50 percent of their income to the household or have at least 50 percent of their expenditures paid by the family. For example, college students who are financially supported by their families but no longer live at home are included, and financially independent children not living at home are not. Furthermore, for each member of the household, I observe age, sex, the relationship to the head of the household, and the highest level of education attempted. Using the year of the survey and the age of the individual, I extrapolate a year of birth for each individual. Using the age of the individual and their relationship to the head, I extrapolate the birth order of child. Due to the fact that some of the children are not observed in the sample, birth order is sometimes mis-specified. Details of birth order mis-specification are discussed in the next section.

My main analysis is limited to cohorts born within a 14-year window around the legalization of abortion in 1985. This sample is also limited to children between the ages of 18 and 24. This restriction is based on the fact that most of the children in the data (72 percent of them) who have ever attended a university are between the ages of 18 to 24. Table 1 provides summary statistics at the household level for children in the sample. The table is split for children born before (1978-1984) and after (1985-1992) the legalization of abortion. I also report average fertility rates in Taiwan. Fertility data come from the National Statistics of Republic of China's website.[4] The "pre" period reports the average fertility in Taiwan from the years 1981 to 1984 and the "post" period's fertility is the nation's average for the years 1985 to 1992.[5]

Of the children in the sample, those born after the legalization of abortion come from households with slightly younger and fewer children than those born prior to the legalization of abortion. Prior to the law change families averages 4.65 members, and 4.46 after the legalization. Mothers of children in the sample born prior to the reform are on average 46.26 years old at the time of the survey, while mothers of children in the sample are 46.81 at the time of the survey. Children born after the legalization of abortion come from families that have a higher income per capita and have heads who are slightly more educated. Since 2010 is the last survey year in the analysis, all of the children in the sample who are born in 1992 are 18 years old and only observed in the 2010 survey. Following similar logic for other birth years and survey year restrictions, children born post-legalization are mechanically a little younger than those born pre-legalization. All of the differences in means between the two periods are statistically significant at the 1 percent level. Fertility drops from 2.25

[4] http://eng.stat.gov.tw.

[5] 1981 is the first year the National Statistics of Republic of China's website provides the fertility rate.

in 1981-1984 to 1.76 in 1985-1992, and families who have a higher order child in a time of low fertility may be very different from families who have a higher order child prior to the legalization of abortion. For example, if at a time of lower fertility having more children is a luxury good, then higher investments in a higher birth order child could be independent of increased sex selection. In that case, however, the effect of abortion on investments in higher order children is independent of child's gender. To account for fertility differences, the main analysis adds additional controls for number of children in the family. I also add controls for the mother's age, to account for the age of the family. It was discussed that not all children are included in the data, and as a result, birth order is sometimes mis-specified. I explain this mis-specification in more detail in the following section.

4 Attrition

It is important to discuss the limitations of these data, since the nature of birth order mis-specification affects the research design. For example, birth order of a child may be incorrectly assigned if the oldest child from the family is financially independent and no longer lives at home. In this case, a younger child is assigned a birth order 1 even though he/she is of a higher birth order. Since most children not included in the survey are older and financially independent, the assigned birth order is likely a downward estimate of actual birth order.

In traditional Chinese culture, sons and daughters-in-law are expected to care for aging parents. Daughters are traditionally viewed as temporary members of the household who become part of their husbands' families after marriage. If families in Taiwan are very traditional, then attrition between the genders may be different. Although attrition in the sample is likely correlated with physically leaving home, it does not perfectly capture attrition in the sample. This is because attrition only occurs if the child doesn't live at home *and* no longer relies on financial assistance from the family.

I perform a synthetic panel analysis to show that older children of the same birth year cohort are more likely to be assigned a smaller birth order. Cohorts born in a specific year are observed in different survey years at different ages, and within the sample, I observe a particular birth year cohort as it gets older. Equation 1 describes a regression that tests for mis-specification of birth order within a birth year cohort as it gets older.

$$Order3plus_i = \sum_{j=1}^{24} \beta_j Age_j + \sum_{j=1}^{24} \gamma_j (Age_j \times Girl_i) + \gamma_0 Girl_i + \epsilon_i \tag{1}$$

The dependent variable in Equation 1 is a dummy indicating whether child i is of third or higher birth order, and it is regressed on fixed effects for ages 1 to 24, with male children under the age of 1 as the omitted category. Also included in the regression are age fixed effects interacted with a fixed effect for being a girl. Equation 1 is estimated separately for each birth year. Then for example, the estimate of β_1 gives the difference in the ratio of children assigned birth order 3 or higher when they are 1 year old from the ratio of children assigned birth order 3 or higher when they are under the age of 1. If there are no deaths or attrition from leaving home within a sample of children of the same birth year (observed at different ages), then β_j should equal zero at any age. In other words, assuming the sample is representative and assuming all children are perfectly observed, the ratio of a cohort that is of birth order three or higher should not change as the birth year cohort ages.

Additionally, if female attrition rates are no different than male attrition rates, then the estimate of each γ_j should also equal zero for all ages. Table 2 presents the results from estimating Equation 1 for birth year cohorts 1981 to 1988.[6] The results indicate that there is obvious birth order mis-specification within the sample, and 18 to 24-year-old children

[6] Results from earlier years 1978-1980 and later years from 1989-1992 are similar.

born within their birth year are anywhere from 14 to 29 percentage points less likely to be assigned birth order 3 or higher than when they were under the age of 1.[7] Only 3 out of 53 of the relevant girl-specific age effects are statistically distinguishable from zero, further implying that within each birth year cohort, birth order mis-specification in the data does not differ across gender.[8]

In an effort to report a smaller table, the coefficient for younger ages' fixed effects are not reported, but in general when the birth year cohort is observed at much younger ages, the birth order mis-specification is much smaller and often indistinguishable from zero. For example, the estimated coefficient for age 1 is mostly zero, implying that children of the same birth year cohort observed at an age under 1 are no more or less likely to be assigned birth order 3 or greater than when the same birth year cohort is observed at age 1.

I find that the birth order mis-specification in the sample is substantial and that within the group of 18 to 24-year-old children, the ratio of children assigned birth order 3 or higher using the sample is 14 to 29 percentage points smaller than what it should be. Since the data are imperfect in assigning birth order and the assigned birth order is often smaller than the actual birth order, I do not investigate the effect for third or higher birth orders as in Lin et al. (2008), but instead exploit a more aggregate variation and investigate the effect for children assigned second or higher birth order. The next section discusses the

[7] Although the absolute value of the point estimates of the age effects for children born post-legalization is generally smaller, it is not implied that attrition is a lesser problem for children born after the legalization of abortion. Because fertility is lower in later years, a smaller effect in magnitude reflects the smaller baseline of children born at the third or higher birth order. Ratios of mis-specification for children born pre-legalization are 0.49, 0.54, 0.57, 0.63, 0.66, 0.69, and 0.74 for 18, 19, 20, 21, 22, 23, and 24 year old children respectively. Analogous rates for children born after the legalization of abortion are 0.41, 0.55, 0.55, 0.55, 0.68, and 0.73 respectively. To estimate these ratios, I run pooled regressions of Equation 1 for birth years 1978 to 1984 and 1985 to 1992 separately, and the ratios of mis-specification are defined to be $-\beta_j/\beta_0$ where β_0 is the ratio of children assigned birth order 3 or higher when under age 1.

[8] One may be concerned that gender-specific attrition varies across socioeconomic status, and that difference in attrition between socioeconomic statuses may drive the results. Performing attrition analysis for different income level families reveals that this is not the case. In addition, I find that families with above the median income per capita have a much lower fertility, and the results are largely driven by below the median income per capita families who have more higher birth order children to begin with.

nature of sex selection for the birth year cohorts considered.

5 The Effect of Abortion Legalization on Ratio of Boys

To show that sex selection occurs for the birth year cohorts considered, I estimate a simple difference-in-difference model.

$$Boy_{it} = \beta_1(Order2plus_{it} \times Post_t) + \beta_2 Order2plus_{it} + \beta_3 Post_t + \epsilon_{it} \tag{2}$$

The dependent variable in Equation 2 is whether or not child i born before or after the legalization of abortion period t is a boy. The independent variables are fixed effects for whether the child is of birth order 2 or higher, $Order2plus_{it}$, and its interaction with a dummy for being born after the legalization of abortion, $Order2plus_{it} \times Post_t$. The main effect of $Post_t$ is also included. Birth order 1 is the omitted category, and the coefficient on $Post_t$ captures the increase in sex ratios for first-born children born after the legalization of abortion. The result of this estimation is provided in Table 3. In specification 1, I limit the sample to young children for whom attrition and birth order mis-specification is a much smaller problem. The sample is limited to children with the same birth years as those in the main sample (1978 to 1992) but only when they are age 10 or younger. I find that the ratio of boys at birth orders 2 or higher increases by 0.0132 from the baseline of 0.52. The coefficient on $Post_t$ is statistically indistinguishable from zero, indicating that the legalization of abortion had no significant impact on the ratio of boys of birth order 1 for a sample of young children.[9]

[9] Repeating the analysis for children of age 10 or younger while investigating the effect of abortion legalization on sex ratios for birth order 3 and higher and birth order 2 separately finds that the legalization of abortion led to an increase in sex ratios by 0.0256 for children born at birth orders 3 or higher. No statistically significant effects are found for the first or the second birth order children. Lin et al. (2008) also finds sex ratios rise for birth orders 3 and higher following the legalization of abortion, while no significant increases are observed for smaller birth orders.

When the sample is restricted to the population of interest, 18 to 24-year-old children with birth years between 1978 and 1992, I do not find a statistically significant increase in sex ratios for higher birth order children. This seemingly paradoxical result could be explained due to mis-specification of birth order for older children. Specification 2 of Table 3 presents the result from estimating Equation 2 for individuals in the sample of college-age individuals. When looking at older children, even birth order 1 children are mis-specified to be a smaller birth order than they actually are. The estimate for $Post_t$ being positive and statistically significant at the 10 percent level implies children assigned birth order 1 in my sample are 1.2 percentage points more likely to be a boy if they are born after abortion is made legal. In a sample of older children, sex selection gets picked up at the first order as well, which biases the estimate of the true effect for higher birth order children downward because the counterfactual is also sex-selected. Additionally, since ratio of boys is just the percentage of boys observed in the sample, it is impossible to distinguish between attrition in the sample (due to death or gaining independent status) and sex selection amongst older children. It must be emphasized that not picking up sex selection for higher birth order children in my particular sample of old children does not imply sex selection did not occur for them; when the same birth year cohorts are observed at younger ages, the data are able to distinctively capture significant increases in the proportion of boys for higher birth order children.

Nonetheless, I am unable to show that sex selection occurs disproportionately more for higher birth order children in my sample specifically. As a result, I cannot confidently rule out that a channel other than sex selection underlies the effects reported in this analysis. It is, however, difficult to imagine a mechanism other than the substitution hypothesis, that affects only the university enrollment of higher birth order girls and not the university enrollment of lower birth order girls or higher birth order boys as well. Next, I describe the main estimating equation to investigate the effect of abortion legalization on gender specific university enrollment.

6 Estimating Equation

I estimate the effect of the legalization of abortion on university attendance separately for boys and girls using a difference-in-difference (DD) specification described in Equation 3.

$$University_{ity} = \beta_1(Ord2plus_{ity} \times Post_t) + \beta_2 Ord2plus_{ity} + \gamma_y + \delta_t + \eta_{iy} + \Gamma X_{ity} + \epsilon_{ity} \quad (3)$$

Equation 3 exploits the fact that sex selection increased most dramatically at higher birth orders. $University_{ity}$ is a dummy variable for whether child i, born in year t, has ever attended a university (I exclude junior college from the definition of university) by survey year y. $Ord2plus_{ity}$ is a dummy for whether the child is of birth order 2 or higher and $Post_t$ is a dummy variable that equals one if the child is born in, or after, 1985 and is zero otherwise. In the equation are fixed effects for the survey year of observation, fixed effects for the birth year of child, controls for per capita household income and the age of the mother. Survey year fixed effects help capture overall trends in schooling that are increasing over time. Birth year fixed effects capture birth cohort effects, and in combination with survey year fixed effects, control for the age of the child. Since birth year fixed effects are perfectly collinear with the $Post_t$ variable, the main effect of $Post_t$ drops out of the model when birth year fixed effects are added.

Fertility declines over time lead to more one-child families. Children without siblings may be more likely to attend college since household resources are not spread over siblings, so I control for the number of children in the family. The full specification adds survey year-specific number of children fixed effects, η_{iy}. I use survey year-specific number of children fixed effects instead of just fixed effects for number of children because of declining fertility in Taiwan. It is likely that a family with 3 children before the legalization of abortion in a time of higher fertility is different from a family with 3 children after the legalization of abortion, in a time of much lower fertility. Including number of children fixed effects also controls for increased investments per child caused by the reduced financial burden of unwanted children post-legalization.

In Equation 3, birth order 1 represents the counterfactual and β_1 is the parameter of interest. As shown in Equation 4, β_1 is estimated by differencing out the mean effect of abortion policy for first born girls (boys) from the mean effect of abortion policy for girls (boys) at second or higher birth orders. A positive value of $\hat{\beta}_1$ indicates an improvement in the rate of university attendance for the second or higher birth order child beyond the improvement seen for the first birth order child. Differencing out the effect of abortion for the first birth order child from the effect of abortion for the higher birth order child removes any general trends in education common between the first and higher birth order children. In a sample of college-age children, birth order 1 children are not a perfect counterfactual as some children assigned birth order 1 are actually of higher birth order and have also been sex selected. Since some of the birth order one children also receive the "treatment" and may also experience the benefits explained by the substitution hypothesis, the estimated effect will be biased downward due to this mis-specification.

$$\hat{\beta}_1 = (\overline{Univ_{Ord2plus,post}} - \overline{Univ_{Ord2plus,pre}}) - (\overline{Univ_{Ord1,post}} - \overline{Univ_{Ord1,pre}}) \tag{4}$$

Causal interpretation of the effect relies on the assumption that without the legalization of abortion, trends in education would remain identical for the first and higher order births. To show that pre-treatment trends are identical for boys and girls of different birth orders prior to the reform, I estimate Equation 5 for the pre-legalization period.

$$Univeristy_{it} = \beta_1(Ord2plus_i \times BirthYear_t) + \beta_2 BirthYear_t + \beta_3 Ord2plus_i + \epsilon_{it} \quad (5)$$

The dependent variable is a dummy indicating whether child i born in year t has ever attended a university. It is regressed on a dummy for birth order 2 or higher, a linear birth year trend and a linear birth year trend interacted with a dummy for birth order 2 or higher for a sample of college-age children born prior to the legalization of abortion. Columns 1 and 2 of Table 4 limit the sample to only girls and boys respectively and present results from estimating Equation 5. For the validity of gender-specific difference-in-difference models, it is important that the coefficient on $Order2plus \times BirthYear$ is indistinguishable from zero for a sample of children born prior legalization. I find this to be the case for both college-age boys and girls born prior to the legalization of abortion.

For additional evidence supporting parallel trends, Figure 1 shows university enrollment trends by birth year, birth order, and gender. Solid lines represent trends for girls and the dashed lines represent university attendance trends for boys. Gender-specific trends appear to be parallel across birth orders. Figure 1 also shows a closing birth order gap in university attendance for girls after the legalization of abortion in 1985, but, the gap does not narrow for boys.[10] It is worth pointing out that girls, even prior to the legalization of abortion, generally have higher university attendance rates than boys. Higher female college enrollment is observed in almost all OECD economies and in most rich countries.

[10] Given that the sample considers 18 to 24-year-old children born between 1978 and 1992 and only surveys up to 2010 are available, later surveys mechanically only include younger children. For example children born in 1992 are only included in the sample at 18 years of age. I limit Figure 1 to years with identical age distribution of children, and as a result 1986 is the last year in the graph. Younger children are less likely to have ever attended a university since they have not been given the same amount of time to attend a university, and changing age distribution confounds the graph for later birth years. This is not an issue in the regression analysis, because I include both birth year and survey year fixed effects, which essentially help control for age.

Several reasons are cited for this including changes in societal values, changes in future female employment, and behavioral differences between males and females. Also, in most estimates, the return of education on wage is estimated to be larger for females than for males (Goldin et al., 2006).

A fully interacted difference-in-difference-in-difference (DDD) model in which all of the terms on the right hand side of Equation 3 are also included with their interactions with a fixed effect for the child being a girl directly tests whether the effect of abortion legalization for girls is statistically different from the effect for boys. A well identified DDD model, in this case, requires that birth order demeaned time trends in education for boys and girls are identical prior to the reform. That is, we need the gap between low and high birth order girls to be moving at a similar rate as the gap between low and high birth order boys. Although girls have a steeper trend in education relative to boys starting in the early 1980s, this does not imply that the DDD assumption fails because it is the gap between low and high birth orders that is of relevance for the DDD model. The assumption for parallel birth order demeaned trends is tested empirically by estimating an equation similar to Equation 5 where all of the right hand side variables are also included with their interactions with a dummy variable for a child being a girl. Column 3 of Table 4 presents the result from this estimation. For the validity of the DDD model, it is important that the coefficient on $Order2plus \times BirthYear \times Girl$ is statistically indistinguishable from zero. I find this to also be the case.

7 Empirical Results

Table 5 presents the results from estimating Equation 3. Identical specifications are estimated for a sample of girls and boys in Panels A and B respectively, and Panel C presents the fully interacted DDD estimated effect of abortion legalization for high birth order girls.

Column 1 is the basic difference-in-difference model and does not account for important time trends, birth year effects, household income, or number of children effects. Results of Column 1 in Panel A imply that the increase in university enrollment for a second and higher birth order girl born after the legalization of abortion is 4.86 percentage points greater than the increase in university enrollment for a first birth order girl born after the legalization of abortion. As shown in Column 2, the point estimate is robust to the inclusion of time trends and it moves down slightly due to a general increase in university enrollment over time. Column 3 adds birth year fixed effects. The coefficient for second or higher birth order girl born after the legalization of abortion is robust when accounting for birth year fixed effects and remains at a 4.58 percentage point increase in university attendance. Since richer families can afford tuition for college more readily and because families in the more recent survey years are younger, Column 4 adds controls for income and mother's age and the effect does not change much. The number of children limit a family's ability to afford tuition for a particular child, and Column 5 adds survey year-specific number of children fixed effects. The estimate for the effect of abortion legalization on the likelihood of ever attending a university for a second or higher order girl remains a 4.29 percentage point increase for the preferred specification of Column 5.

Panel B repeats the identical analysis for boys. In all of the specifications, the coefficient on birth order 2 and higher post-legalization is statistically insignificant from zero. Moreover, all estimated effects for birth orders 2 and higher are smaller in magnitude for boys than their respective estimates for girls. Panel C presents the DDD estimate of $Order2plus \times BirthYear \times Girl$ for each specification. Indicative of having 2 valid DD models, the estimated effects using a DDD for higher birth order girls are comparable to the simple DD estimates for high birth order girls of Panel A. The statistical significance of the DDD estimates implies that the reported effects for boys and girls are not the same.

8 Robustness Checks

Perhaps the most convincing way to determine that factors other than the legalization of abortion are not underlying the effect is to look within a very small window around the legalization of abortion and examine whether an effect still exists. When investigating the effect of abortion legalization on 18 to 24-year-olds born between 1978 and 1992, there could be several unobservables that change over time. In an attempt to minimize the number of varying unobservables, I estimate Equation 3 for a sample of girls born within a smaller window of birth years near the legalization of abortion. Column 1 in Table 6 presents the results from the most preferred specification, which includes all of the fixed effects and controls of Table 5, for girls born in a 6 year window between 1982 and 1987. As in Table 5, Panel A of Table 6 also presents estimation results for a sample of girls while Panel B presents analogous estimation results for a sample of boys. The estimated effect of the legalization of abortion on higher birth order girls born right around the policy change is a 4.35 percentage points increase in university attendance. This is very close to the 4.29 percentage points effect estimated in the full sample.

One may also be concerned that it is not the number of children in a family that matters, but the composition of children in a family. For example, a family with several college-age children may find it difficult to afford tuition for all of the children, while a family with young children and one college-age child may find it easier to afford tuition for the one child who is of college-age. Column 2 of Table 6 reports the estimates of Equation 1 with all of the fixed effects and controls, but replaces survey year-specific number of children fixed effects with detailed survey year-specific family composition fixed effects. For each survey year, the specification adds fixed effects for number of daughters under the age of 18, number of sons under the age of 18, number of college-age daughters between the ages of 18 and 24, number of college-age sons between the ages of 18 and 24, number of daughters over 24, and number of sons over 24. The coefficient for girls at the second or higher order remains around 4.34 percentage points and is statistically significant at the 1 percent level.

Additionally, girls and boys may have differing opportunity costs of attending a university in Taiwan and may enter a university at different ages. Limiting the sample to older children helps account for the different opportunity costs associated with delayed university enrollment. Column 3 presents the results from limiting the sample to older girls that are between 20 and 24 years old. Within the sample of older girls between the ages 20 and 24, higher birth order girls born after the legalization of abortion are 5.16 percentage points more likely to attend a college. This effect is statistically significant at the 1 percent level.

Because all children born in 1985 were not born before the legalization of abortion, an argument can be made for using either 1985 or 1986 as the post-legalization period. Column 4 presents the results from redefining 1986 and after as the "post" period. Redefining the post-treatment period in such a way does not yield a much different result for a sample of girls.

Limiting the sample to children born near the time of legalization does not rule out the possibility that the increase in higher order female university attendance was caused by an existing trend and not due to abortion legalization. One way to test whether a general trend of improving educational levels for higher birth order girls existed in Taiwan is to investigate whether an effect existed before the legalization of abortion. In column 5, I limit the sample to girls born before the legalization of abortion in the years 1978-1984. I define 1981 as the year that the pseudo treatment occurs. The reported magnitude of the effect of the pseudo treatment is -0.0047 for higher birth order girls. It is not only statistically indistinguishable from zero, it is also negative in magnitude.[11] The lack of an

[11] Investigating an effect for treatment years 1980 and 1982 for girls born before the legalization of

effect for the placebo test provides additional evidence that the preferred specification is not just capturing a general trend of a shrinking education gap between high and low birth order girls.

Panel B presents results from limiting the sample to boys. In specifications 1-4 of panel B, I do not find a statistically significant effect for higher birth order boys. Also consistent with a lack of a general trend in improving educational outcomes for higher birth order boys, no effect is found for the pseudo treatment defined in specification 5.[12] To verify that the effects reported are statistically different for boys and girls, I present the DDD estimates for high birth order girls in Panel C. I am unable to reject that the effect for the pseudo treatment of specification 5 is statistically different for boys and girls as the DDD estimate is statistically insignificant. For all other robustness checks, the reported effects for boys and girls are statistically different from each other at the 10 or lower percent level.

9 Conclusion

I find evidence supporting the substitution hypothesis that prenatal gender discrimination reduces postnatal discrimination for girls later in life. Once abortion is made legal, families with a strong preference for a boy at a higher birth order (or strong distaste for a girl at a higher birth order) choose to abort the higher order female fetus. Hence, the girls born at higher birth orders after the legalization of abortion are born into families with, on average, higher preferences for girls. I find results consistent with this compositional change, with abortion legalization leading to an increase in university attendance of higher birth order girls by about 4.5 percentage points. Consistent with no shift in preferences for boys for

abortion yields an estimated effect of 0.00701 and -0.0132 respectively and both effects are statistically indistinguishable from zero.

[12]Investigating an effect for treatment years 1980 and 1982 for boys born before the legalization of abortion yields an estimated effect of 0.00791 and 0.00159 respectively and both effects are statistically indistinguishable from zero.

families who continue having boys, I find that boys at the second or higher birth order born after legalization of abortion are not significantly more likely to attend a university. Additionally, I can reject the hypothesis that the two effects for boys and girls are the same. Somewhat surprising, I show that gender discrimination exists in an arena (education) for which girls are advantaged relative to boys to begin with. Results in the analysis are evidence that there indeed is discrimination against girls in Taiwan, but the discrimination is specific to higher birth order girls.

While these results are unable to speak to how sex-selective abortion compares to other policies aimed to reduce gender discrimination, they shed light on the implications of endogenizing the gender composition of children decision through sex-selective abortions. As families are shown to substitute across prenatal and postnatal discrimination, placing bans against sex-selective abortions do not provide the solution. In hopes of eliminating both prenatal and postnatal sex selection, policies that remove the underlying male preference should be implemented instead.

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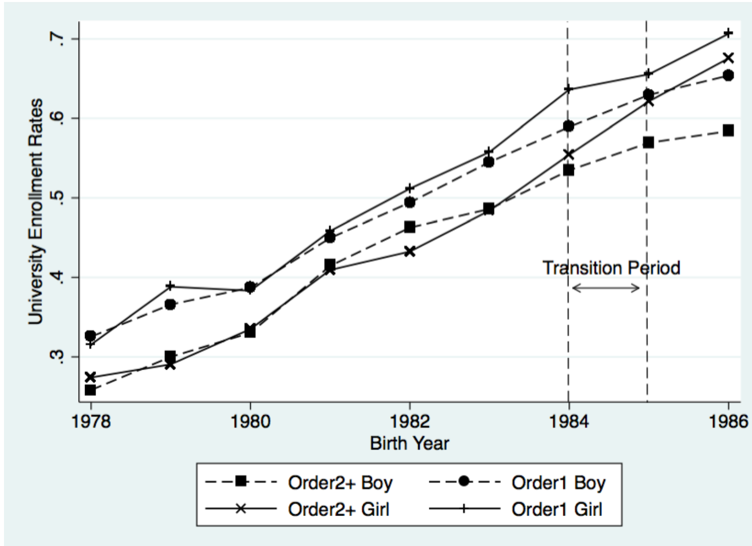

Figure 1: Birth order and gender-specific university enrollment trends

Table 1: Summary statistics by birth year

Birth Years	Pre-reform 1978-1984	Post-reform 1985-1992	
Variable	Mean	Mean	Diff
Mean age of children	13.84	13.22	-0.59***
	(0.01)	(0.02)	(0.02)
No. of children	2.48	2.33	-0.15***
	(0.01)	(0.01)	(0.01)
No. of people	4.65	4.46	-0.19***
	(0.01)	(0.01)	(0.01)
Mother's age	46.26	46.81	0.56***
	(0.03)	(0.03)	(0.04)
Income per capita $NT	303,660	314,700	10,273***
	(1,084)	(1,372)	(1,720)
Head went to a university	0.09	0.12	0.03***
	(0.00)	(0.00)	(0.00)
Head is male	0.83	0.79	-0.03***
	(0.00)	(0.00)	(0.00)
Average Fertility Rate	2.25	1.76	

Standard errors in parentheses. Sample weights used.
Fertility data from http://eng.stat.gov.tw. Since 1981 is the first year with reported fertility, only years 1981-1984 are used to estimate the pre-reform average fertility. Post-reform fertility is the average fertility in Taiwan from 1985-1992.
*** p<0.01, ** p<0.05, * p<0.1

Table 2: Attrition- Gender-specific birth order mis-specification for older children

Order3plus ? [0,1]	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Birth Year	1981	1982	1983	1984	1985	1986	1987	1988
Age 1	-0.00	0.00	-0.00	0.02	-0.01	-0.04	-0.06**	-0.02
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
.	.	.	.	.	.	.	.	.
.	.	.	.	.	.	.	.	.
Age 18	-0.22***	-0.22***	-0.23***	-0.19***	-0.15***	-0.14***	-0.20***	-0.15***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Age 19	-0.22***	-0.20***	-0.25***	-0.17***	-0.18***	-0.21***	-0.24***	-0.19***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Age 20	-0.25***	-0.23***	-0.24***	-0.20***	-0.17***	-0.18***	-0.22***	-0.19***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Age 21	-0.27***	-0.16***	-0.27***	-0.24***	-0.20***	-0.17***	-0.19***	-0.22***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Age 22	-0.25***	-0.24***	-0.23***	-0.24***	-0.22***	-0.21***	-0.24***	-0.24***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Age 23	-0.28***	-0.22***	-0.25***	-0.23***	-0.22***	-0.21***	-0.27***	
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	
Age 24	-0.29***	-0.26***	-0.27***	-0.25***	-0.21***	-0.22***		
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)		
Age 1 × Girl	-0.05	0.05	-0.02	-0.05	-0.02	0.02	0.01	-0.00
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
.	.	.	.	.	.	.	.	.
.	.	.	.	.	.	.	.	.
Age 18 × Girl	0.01	0.05	0.10**	-0.03	-0.02	-0.04	0.06	-0.02
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.05)	(0.04)
Age 19 × Girl	0.04	0.03	0.05	-0.04	0.01	0.04	0.04	0.01
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Age 20 × Girl	0.04	0.05	0.02	-0.04	0.01	-0.02	0.06	0.07
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Age 21 × Girl	0.06	-0.05	0.08*	-0.01	-0.02	-0.04	-0.02	0.07*
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Age 22 × Girl	0.01	0.03	0.00	-0.02	0.01	0.00	0.01	0.02
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Age 23 × Girl	0.03	-0.03	-0.02	-0.00	0.04	0.00	0.06	
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	
Age 24 × Girl	0.03	-0.00	-0.01	0.04	-0.01	0.03		
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.05)		
Girl	-0.00	-0.03	-0.02	0.03	0.00	-0.00	-0.03	-0.03
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Constant	0.36***	0.34***	0.35***	0.30***	0.30***	0.28***	0.31***	0.30***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Observations	28,474	26,805	24,468	22,980	20,829	17,666	17,401	18,479

Robust standard errors in parentheses. Sample weights used. Each regression is estimated for a specific birth year cohort and includes age fixed effects for ages 0-24 and age interacted with girl fixed effects. Omitted category is age 0 boy.
*** p<0.01, ** p<0.05, * p<0.1.

Table 3: Effect of abortion legalization on the ratio of boys

Dep var: Boy? [0,1]	(1)	(2)
Order2plus× Post	0.0132***	-0.0104
	(0.00509)	(0.00989)
Order2plus	0.000316	0.000328
	(0.00333)	(0.00614)
Post	0.000322	0.0124*
	(0.00402)	(0.00655)
Constant	0.519***	0.504***
	(0.00268)	(0.00407)
Ages	0-10	18-24
Observations	176,379	47,549

Robust standard errors in parentheses. Sample weights used. Sample restricted to children with birth years 1978 to 1992.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 4: Pre-reform university attendance differentials by birth order and gender

Dep Var: Ever attend a University? [0,1]	(1) Girls	(2) Boys	(3) All
Order2plus×Birth Year×Girl	–	–	-0.0028
	–	–	(0.00587)
Birth Year×Girl	–	–	0.00302
	–	–	(0.00395)
Order2plus×Girl	–	–	5.53
	–	–	(11.63)
Girl	–	–	-5.966
	–	–	(7.822)
Order2plus×BirthYear	-0.00249	0.000308	0.000308
	(0.00420)	(0.00410)	(0.0041)
BirthYear	0.0507***	0.0477***	0.0477***
	(0.00283)	(0.00275)	(0.00275
Order2plus	4.870	-0.660	-0.66
	(8.329)	(8.114)	(8.114)
Constant	-100.0***	-94.04***	-94.04***
	(5.608)	(5.452)	(5.452)
Observations	14,450	14,801	29,251

Robust standard errors in parentheses. Sample restricted to children of ages 18-24 born between 1978-1992. Sample weights used.
*** p<0.01, ** p<0.05, * p<0.1.

Table 5: The effect of abortion legalization on university attendance for girls and boys

Panel A: Girls	(1)	(2)	(3)	(4)	(5)
Dep var:					
Ever attend a University? [0,1]					
Order2plus×Post	0.0486***	0.0423***	0.0458***	0.0487***	0.0429***
	(0.0135)	(0.0131)	(0.0130)	(0.0131)	(0.0136)
Order2plus	-0.0678***	-0.0545***	-0.0516***	-0.0799***	-0.0927***
	(0.00863)	(0.00823)	(0.00824)	(0.00839)	(0.00904)
Post	0.221***	-0.0737***	–	–	–
	(0.00898)	(0.0127)			
Observations	23,369	23,369	23,369	22,551	22,551
Panel B: Boys					
Order2plus×Post	0.00889	-0.00576	0.00320	0.00148	-0.00761
	(0.0135)	(0.0131)	(0.0132)	(0.0132)	(0.0139)
Order2plus	-0.0532***	-0.0339***	-0.0325***	-0.0652***	-0.0840***
	(0.00853)	(0.00820)	(0.00821)	(0.00838)	(0.00920)
Post	0.210***	-0.0533***	–	–	–
	(0.00885)	(0.0124)			
Observations	24,180	24,180	24,180	23,211	23,211
Panel C: Fully interacted DDD					
Order2plus×Post×Girl	0.0397**	0.0480***	0.0426**	0.0472**	0.0505***
	(0.0191)	(0.0186)	(0.0185)	(0.0186)	(0.0194)
Observations	47,549	47,549	47,549	45,762	45,762
Survey Year FE	no	yes	yes	yes	yes
Birth Year FE	no	no	yes	yes	yes
No. of Children × survey yr FE	no	no	no	no	yes
Additional Controls	no	no	no	yes	yes

Table reports results from separate regressions for girls and boys in Panel A and Panel B respectively. Panel C provides DDD estimates for girls from a fully interacted model. Sample weights used. Robust standard errors in parentheses. Sample restricted to children of ages 18-24 with birth years 1978-1992.
*** p<0.01, ** p<0.05, * p<0.1

Table 6: Robustness Checks: The effect of abortion legalization on university enrollment for girls and boys

Panel A: Girls	(1)	(2)	(3)	(4)	(5)
Dep Var:					
Ever attend a University? [0,1]					
Order2plus×Post	0.0435**	0.0434***	0.0516***	0.0415***	-0.0047
	(0.0195)	(0.0133)	(0.0175)	(0.0142)	(0.0169)
Observations	10,288	22,551	13,541	22,551	13,988
Panel B: Boys					
Order2plus×Post	-0.0167	-0.00484	-0.0118	0.0081	0.0016
	(0.0197)	(0.0134)	(0.0183)	(0.0146)	(0.0170)
Observations	10,762	23,211	13,158	23,211	14,234
Panel C: Fully interacted DDD					
Order2plus×Post×Girl	0.0602**	0.0482**	0.0634**	0.0335*	-0.0063
	(0.0278)	(0.0189)	(0.0253)	(0.0204)	(0.0240)
Observations	21,050	45,762	26,699	45,762	28,222
Survey Year FE	yes	yes	yes	yes	yes
Birth Year FE	yes	yes	yes	yes	yes
No. of Children× yr FE	yes	no	yes	yes	yes
Comp. of Children × yr FE	no	yes	no	no	no
Additional Controls	yes	yes	yes	yes	yes
Age Group	18-24	18-24	20-24	18-24	18-24
Birth Years	1982-1987	1978-1992	1978-1992	1978-1992	1978-1984
Treatment Year	1985	1985	1985	1986	1981

Table reports results from separate regressions for girls and boys in Panel A and Panel B respectively. Panel C provides DDD estimates for girls from a fully interacted model. Robust standard errors in parentheses. Sample weights used.
*** p<0.01, ** p<0.05, * p<0.1