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We investigate the impact of school starting age on teenage marriage and motherhood in Vietnam, where the rates of both are rising rapidly. We exploit a discontinuity in the age at which children start school and use regression discontinuity methods to identify the causal effect of school starting age on premature marriage—a first in the literature—and early motherhood. We find that girls who start school earlier and who are therefore younger relative to their classmates are significantly more likely to marry and/or give birth in their teenage years. We argue that the negative effects of starting school early are transmitted through adverse peer influences. We also determine that school starting age impacts are heterogeneous across girl subgroups. The significant effects of school starting age are concentrated among teenage girls who are members of ethnic minorities, whose mothers have relatively less education, whose households are relatively poor, and/or who live in rural areas. Girls that fall into these subgroups are more likely to benefit from starting school later. Finally, we present some preliminary and suggestive evidence that participation in extracurricular activities may help mitigate negative school-based peer effects.

Key words: school starting age, teenage marriage, teenage motherhood, peer influences, regression discontinuity, Vietnam

JEL: J12, J13, J16

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1. Introduction

An extensive literature examines the impact of school starting age (SSA) on child outcomes. In this framework, a broad range of effects has been considered, including, most frequently, those related to school performance, educational attainment, and post-education earnings (Bedard and Dhuey 2012; Chen 2017; Crawford et al. 2014; Datar 2006; Dobkin and Ferreira 2010; Elder and Lubotsky 2009; Fredriksson and Öckert 2014; Larsen and Solli 2017; Matta et al. 2016; McEwan and Shapiro 2008; Puhani and Weber 2007; Robertson 2011). Most, but not all, of these studies find that starting school earlier (later) leads to disadvantageous (favorable) outcomes.[1]

The key mechanism through which SSA effects are transmitted relates to the relative age of the student and associated peer effects (Larsen and Solli 2017). Students who are relatively younger in their class may be more likely to engage in risky behavior due to older peer influences and that conduct can have a negative influence on child outcomes. Negative peer effects may be especially important for school-age girls (Argys et al. 2006). On the other hand, those students that are relatively older may possess more self-esteem and leadership abilities, leading to positive impacts (Dhuey and Lipscomb 2008; Thompson et al. 2004).[2]

This paper examines the effects of SSA on teenage marriage and motherhood in Vietnam. Vietnam is an interesting case in this context. Eleven percent of Vietnamese girls are married (either formally or informally) before the age of 18. This rate is not high by (East Asia and the Pacific) regional or world standards—15 percent and 33 percent, respectively. However, the frequency of Vietnamese child marriage is increasing rapidly, doubling since 2006 (UNICEF 2016). World Bank data show that the adolescent fertility rate (births per one thousand girls) in Vietnam was 39 in 2015. This is about the same as that for middle-income countries as a

[1] Among the studies cited above, exceptions to the rule that SSA positively influences child outcomes include Dobkin and Ferreira (2010), who determine that SSA negatively affects completion of high school in US, and Chen (2017), who finds that SSA has a negative impact on child test scores in China.

[2] The vast bulk of studies find that being relatively older is beneficial in this context. See Crone and Whitehurst (1999), Lincove and Painter (2006), and Cobley et al. (2009), for examples of negative effects.

whole (40). However, the incidence of Vietnamese adolescent fertility has increased by more than 11 percentage points since 2002, whereas that for lower middle-income countries overall declined by 20 percentage points over the same period (WB 2018). For countries in East Asia and the Pacific, adolescent fertility rate held consistently at about 20 over this period. Worldwide, the adolescent fertility rate declined from 53 to 44 over the same period. Thus, early childbirth in Vietnam is changing in a divergent direction from that of its peers and growing rapidly. In summary, therefore, while the current rates of teenage marriage and motherhood in Vietnam are not alarmingly high by most standards, the recent rapid rise in both may be judged as potentially problematic. Research shows that growing teenage marriage and fertility rates are especially acute among ethnic minorities living in rural areas (Nguyen et al. 2016; UNICEF 2016).

Our examination makes three contributions to the SSA literature. First, to the best of the authors' knowledge this is the first study to investigate the impact of SSA on teenage marriage in any country. Second, we add to a very small literature on the impact of SSA on teenage motherhood. Only three other recent studies have examined SSA effects on fertility: Tan (2017), McCrary and Royer (2011) and Black et al. (2011). The two former investigations find that SSA has no statistically significant impact on the probability that women will become mothers or the age at which women give birth, in US.[3] The latter study determines that girls in Norway who start school earlier (later) are more (less) likely to become pregnant as teenagers than girls who start school at relatively older (younger) ages. Thus we weigh in on an important issue on which existing evidence is contradictory. Third, for the first time in the literature we make the link between SSA peer effects and child participation in extracurricular activities. While our analysis is preliminary and suggestive on this count, our study does demonstrate that this relationship offers a fruitful area for future research.

We find that SSA significantly affects teenage marriage and motherhood in Vietnam. We estimate that starting school later reduces teenage marriage and motherhood by 28 and 40 percent, respectively. We also determine that the impact of SSA is heterogeneous across subgroups as defined by girls': ethnicity, mothers' level of education, household wealth, and residential location. The significant effects of SSA are concentrated among teenage girls who

[3] Both Tan (2017) and McCrary and Royer (2011) use SSA as an instrument for educational attainment to examine the effects of the latter on fertility. As will be shown later, we find that SSA has a direct impact on teenage marriage and motherhood and therefore the use of SSA as an instrument for educational attainment to investigate the impact of education on our outcomes of interest would be improper. We investigate possible confounding effects of education (as proxied by years of schooling) on our estimated treatment effects later in the analysis.

are members of ethnic minorities, whose mothers have relatively less education, whose households are comparatively poor, and/or who live in rural areas. Otherwise put, girls classified as above are more likely to benefit from starting school later. Finally, we also present some tentative evidence that child participation in extracurricular activities may help mitigate negative school-based peer effects.

The rest of the paper proceeds as follows. First, we provide some background on the education system and teenage marriage and motherhood in Vietnam. Second, we review the data and variables used in the analysis. Third, we outline our identification strategy. Fourth, we present and discuss the investigation's main empirical output and test the robustness of our results. Fifth, we examine the heterogeneity of treatment effects by ethnicity, mothers' schooling, household wealth, and residential location and explore the possible mitigating effects of SSA related to participation in extracurricular activities. Finally, we summarize and conclude.

2. Background: education and early marriage and motherhood in Vietnam

2.1. Education

Pre-tertiary education in Vietnam comprises three levels of schooling: primary (5 years), lower secondary (4 years), and upper secondary (3 years). Countrywide, there are about 29 thousand schools, including approximately 15 thousand for primary, 10 thousand for lower secondary, and about 2.5 thousand for upper secondary (GSO 2016). (The remainder are schools with combined levels of education.) There is at least one primary school in every commune and nearly every commune has a lower secondary school, as well. All districts have an upper secondary school (MOET 2015).[4] As of 2014, there were over 861 thousand teachers and 15.3 million students in the country.

In Vietnam, the school year starts in the first week of September (which it has done since 1945 when the nation became independent) and runs until the end of May the following year. Children start school (i.e. enter first grade) in September of the calendar year in which they turn six years of age. This implies that if students were to enter school as government regulations insist and progress without interruption or grade repetition, they would finish grade 12 in the year in which they turn 18 years old.

[4] The subnational public sector in Vietnam comprises provinces (63), districts (713), and communes (11,162).

The Ministry of Education and Training (MOET) has overall responsibility for managing education in Vietnam. Historically, administration of the education system has been quite centralized (Jonathan 2011). However, government began to decentralize some authority for the provision and financing of education (and other local services) in the early 2000s. Decree No. 10/2002 / ND-CP, for example, provided schools (and other public service delivery units) some limited autonomy in the allocation of financial resources. Four years later, the aforementioned decree was replaced by Decree No. 43/2006 / ND-CP, which awarded public schools (and other non-profit service providers) significant decision-making authority over organizational structure, financial management, salaries, and specific targeted outcomes.

Vietnamese government expenditure on education has grown rapidly in recent years and has become among the highest in Asia relative to GDP (MOET et al. 2016). Government spending on education was just one percent of GDP in 1990 (Jonathan 2011) and rose to 4.9 percent of GDP in 2008 and 5.7 percent of GDP in 2013[5]. In 2013, government education expenditure accounted for 20 percent of total spending, increasing from 16 percent in 2009.

Government and household spending on primary schooling makes up the largest share of education expenditure, amounting to about 25 percent of the total, followed by spending on lower secondary school and university, each of which accounts for roughly 20 percent of aggregate amount (MOET et al. 2016). Spending on pre-primary and upper secondary each comprises about 13 percent of the total. The remainder is allocated to technical education and others. Like other developing countries most public spending on education—about 80 percent of recurrent budgets—is allocated to teacher salaries (Jonathan 2011).

Vietnam achieved universal primary education in 2000. By 2013, the net primary school enrolment rate had risen to 97 percent (MOET 2015). Having realized universal primary schooling, government set its sights on increasing enrolments at the lower secondary level. Degree 88/2001/NĐ-CP of 2001 formally set a target of achieving universal access to lower secondary school by 2010. In the school year 2008-2009 all districts and provinces in the country reported that they had already reached the objective (MOET 2015). Net enrolment rates for lower secondary education have rapidly increased, from 30 percent in 1993 to 79 percent in 2008 (Jonathan 2011) and 88 percent in 2013 (MOET 2015). Net enrolment rates for upper secondary education have also grown quickly, surging to 54 percent in 2008 from

[5] https://data.worldbank.org/indicator/SE.XPD.TOTL.GD.ZS?locations=VN

seven percent in 1993 (Jonathan 2011). Some provinces have already initiated programs to reach universal upper secondary education (MOET 2015).

Despite these achievements related to improved schooling access, considerable inequalities exist in educational attainment, especially between children belonging to Kinh and non-Kinh ethnic groups, those living in urban and rural areas, and relative to household income status.[6] In 2010, differences in enrolment rates in primary, lower secondary, and upper secondary schools for Kinh and non-Kinh children were eight, 26, and 35 percent, respectively (GSO 2010). Net enrolment rates in urban and rural areas were 86 and 80 percent for lower secondary and 70 and 55 percent for upper secondary, respectively (GSO 2011). Similarly, educational gaps between children from low and high-income families exist across all age groups. More than a half of children who belong to the first income quintile drop out school when they are between 15 to 17 years of age, compared to just 16 percent of those in the fifth income quintile (Quyen 2011).

On a more positive note, Vietnam is moving rapidly towards achieving gender equality in education access. In 1990, the net secondary school enrolment for girls was five percent lower than that for boys. However, by 2010, enrolment rates for girls exceeded that for boys: 83 percent versus 80 percent for lower secondary education and 63 percent versus 54 percent for upper secondary school, for females and males, respectively (Jonathan 2011).

2.2. Early marriage and motherhood

Early Vietnamese culture was significantly influenced by Chinese doctrines and feudalism, which predominated in the country until independence in 1945. In this context, arranged early marriage and motherhood were common (Nguyen et al. 2016). After independence, the Vietnamese government attempted to raise the education levels of its people, with a view to breaking out of the feudal mind-set, starting with the implementation of a large literacy campaign (Nguyen and Nguyen 2008). Subsequently, in 1958, government issued a new Law on Marriage and Family, stipulating that the minimum age for marriage would be 18 for women and 20 for men. Rising literacy, the collapse of feudalism, and minimum marriage age requirements together led to significant declines in early marriage and parenthood. By the early 1960s, the number of births per 1,000 women aged 15-19 had stabilized at around 19 in

[6] Kinh is the dominant ethnic group in Vietnam, the members of which make up about 85 percent of the population. Fifty-three non-Kinh ethnic groups comprise the remaining 15 percent.

Vietnam, substantially lower than 48, the average figure for all Southeast Asia and Pacific countries (WB 2018).

With the 1986 initiation of the "Doi Moi" economic reforms and the attendant rise in globalization, young Vietnamese began to be exposed to a broader set of international norms. The exposure influenced considerably their expectations and expressions regarding sexual life. As a result, the adolescent fertility rate (births per 1,000 girls) in Vietnam increased rapidly (Mestechkina et al. 2014; Ngo et al. 2008), reaching 34 in 1992 and peaking at 39 in 2015. The latter is substantially higher than the average rate of about 22 for all countries in East Asia and Pacific region (WB 2018). Results from national surveys on Vietnamese Youth conducted in 2003 and 2008 show that of 1,000 adolescent girls aged 14 to 19, 40 have experienced pregnancy (Nguyen et al. 2016). Similarly, the share of young population marrying before the legal age has increased significantly. In 2015, 11 percent of women aged 20 to 24 years married before they were 18 years old, a substantial increase from 5.4 percent in 2006. Among ethnic minority groups as a whole, the child marriage rate is around 30 percent (GSO and UNICEF 2015). The comparable figure for East Asian and Pacific region was 15 percent in 2016 (UNICEF 2016).

3. Data and variables

Data used in this paper derive from a 15 percent random sample drawn from the 2009 Vietnam Population and Housing Census, as provided by UPUMS International.[7] The data set contains information on the marriage status of household members aged 15 years and older and on the extent to which female members of the household between the ages of 15 and 49 have given birth, among others. The census date was 1 April 2009 and therefore the youngest age cohort for which data on marriage and fertility are available is that for girls born in March 1994, who would have been 15 years old at the time of the census.

We use sample information on marriage and fertility above to construct our two main dependent variables of interest in this study: teenage marriage and teenage motherhood. We define a dummy variable for teenage marriage that is equal to one if a female between the ages of 16 and 18 (inclusive) is part of a formal or informal marital union with a male, regardless of the latter's age, else zero. Our teenage motherhood variable is also a dummy, set equal to one

[7] Minnesota Population Center. *Integrated Public Use Microdata Series, International: Version 6.5* [dataset]. Minneapolis: University of Minnesota, 2017. http://doi.org/10.18128/D020.V6.5.

if a similarly aged female has given birth at least once, otherwise zero. The reason for focusing on girls between 16 and 18 years of age will become apparent below.

The sample also includes data on the month and year of birth of all household members. We use this information to define a variable that relates a girl's date of birth to the school entry cut-off date. As discussed above, the Education Law stipulates that the SSA is six years of age and that 1 January is the cuff-off for determining a child's school age. Accordingly, girls born before 1 January enter school one year earlier than those born on or after 1 January, thus leading to a discontinuity in SSA that we use to operationalize our RD design. We normalize date of birth as the number of months before and after the 1 January cut-off.

Normalized date of birth—the forcing variable in the RD analysis—ranges from negative six (for girls born in July) to six (for those born in June). Following Black et al. (2011) we redefine age cohorts to include females born from July to June rather than from January to December. As such, the youngest cohort of school-aged girls examined includes those born in June 1993; and the oldest cohort investigated comprises girls born in July 1990. The data available for analysis therefore become those on females 16 to 18 years of age. The final sample comprises approximately 441 thousand observations, including 152 thousand 16-year-old girls, 145 thousand 17-year-old girls and 144 thousand 18-year-old girls.

Finally, the data set also contains information on socio-economic characteristics of girls, their parents, and their households: ethnicity, presence of disabilities, parents' level of education, number of siblings, household wealth, and residential location. Ethnicity is a dummy variable, fixed at one if the girl is non-Kinh and zero if Kinh. Disability is a dummy with value of one if the girl is mentally or physically disabled, and zero otherwise. For parental education, we construct dummy variables for both mothers and fathers that indicate whether the parent had attained less than lower secondary education (=1) or equal to or more (=0). Number of siblings is the number of the girls' older or younger brothers and sisters, whether living in the household at the time of the census or not. We form an index of household wealth by taking the weighted average of 10 binary variables: whether the household has access to electricity and piped water (=0) or not (=1) and whether the household is in possession of (landline) phone, radio, television, computer, washing machine, refrigerator, air conditioner, and flush toilet (=0) or not

(=1).[8] Residential location is also a dummy, set equal to one if rural and zero if urban. Summary statistics for all variables used in the analysis are presented in Table 1.

[Table 1]

4. Identification

We use regression discontinuity (RD) methods to identify the causal effects of SSA on teenage marriage and motherhood in Vietnam. Under the assumption that girls start school at age six and the cut-off date for determining a child's school age is 1 January, we can use a girl's month and year of birth to establish when she should have entered school relative to the age cut-off. We term this relative age the normalized date of birth. In the RD model, a girl's normalized date of birth serves as the running variable and the specific date 1 January provides the threshold or cut-off. All girls born before 1 January would have started school one year earlier than girls who were born on or after 1 January. More to the point, girls born just before the cut-off are younger compared to their classmates and girls born immediately after the cut-off are comparatively older. In the language of RD, relatively older girls comprise the treatment group and relatively younger girls constitute the control group.[9]

Following Imbens and Lemieux (2008), define Y_i (0) and Y_i (1) to be potential marriage and motherhood outcomes for girl i where Yi (0) is the outcome for comparatively younger girls (i.e. those to the left of the cut-off) and Yi (1) is the outcome for relatively older girls (i.e. those to the right of the cut-off). In this case, the impact of SSA on marriage and motherhood is given by Y_i (1) - Y_i (0). Unfortunately, Y_i (0) and Y_i (1) cannot be observed simultaneously and so attention turns to the average effects of treatment, Y_i (1) - Y_i (0), across girl subgroups. Let D_i=0 if a girl is relatively younger in her class (and to the left of the cut-off) and D_i = 1 if she is older compared to her classmates (and to the right of the cut-off). Observed outcomes, Y_i, are therefore = Y_i (0) if D_i = 0 and = Y_i (1) if D_i = 1. The average causal effect of relative age, τ, at the cut-off, c, is given by:

$$\tau = \mathrm{E}\left[Y_i(1) - Y_i(0) \mid X_i = c\right] = \mathrm{E}\left[(Y_i(1) \mid X_i = c\right] - \mathrm{E}\left[Y_i(0) \mid X_i = c\right] \quad (1)$$

[8] We weight each variable as a function of its ability to explain household expenditure. Using data from 2010 VHLSS, we regress per capita household expenditure against the variables comprising the index and use the estimated coefficients to derive weights.

[9] We do not have data on when girls actually started school, only on when they should have started school according to the rules. As such, we estimate intent-to-treat (ITT) treatment effects.

The key identifying assumption in this framework is that E [Y_i (1) | X_i] and E [Y_i (0) | X_i] are continuous in X, girls' normalized date of birth. This implies that all other unobserved determinants of marriage and motherhood, Y, are also continuously related to X (Imbens and Lemieux 2008). The implication allows one to use outcomes just below the cut-off as valid counterfactuals for those just above the cut-off (Cook and Kang 2016; de la Cuesta and Imai 2016; Imbens and Lemieux 2008) . The general form of the estimating equation is:

$$Y_i = \tau D_i + g(X_i) + \mu_i \quad (2)$$

In equation (2), g(X) is a polynomial function of normalized date of birth X_i; μ is the error term; and τ is the treatment effect, which is to be estimated.

In theory, equation (2) can be estimated by either non-parametric or parametric methods. Non-parametric estimation relies on continuously shrinking the bandwidth within which estimates are made and comparing observed outcomes just above the threshold with those just below. However, when the running variable is discrete, as is the case here, there are no observations just above or just below the threshold and therefore the needed comparison cannot be made in the manner described (Lee and Card 2008). In this case, suggested practice is to estimate a parametric regression of Y on lower order polynomials of X, where identification is achieved through extrapolation, as based on the estimated relationship between Y and X (Dong 2015).

Recent research argues for the use of lower order polynomials in regressions of Y on X (Gelman and Imbens 2017; Skovron and Titiunik 2015) and we employ polynomials of degree one in our analysis. We estimate the following equation.

$$Y_i = \alpha + \tau D_i + \beta_1 X_i + \beta_2 D_i X_i + \mu_i \quad (3)$$

where all variables and parameters have been previously defined.

We estimate equation (3) using ordinary least squares (OLS) within narrow windows (bandwidths) on each side of the cut-off as per usual practice. We use several of the narrowest possible bandwidths: two, three, and four months around the cut-off.[10] We use an inverted

[10] A bandwidth of two is our preferred bandwidth. This narrowest bandwidth will minimize treatment effects estimation bias. Although narrow bandwidths, in general, result in less precision because of comparatively fewer observations, this is not likely to be very problematic in our case since we have an abundance of observations even at a bandwidth of two.

distance weighting scheme, which places more weight on observations close to the cut-off in our regressions (Anderberg and Zhu 2014; Gibbons et al. 2013; Machin et al. 2011). Here, a girl born n months from the cut-off receives a weight of 1/n. So, for example, the weight for a girl born in January or December is one, in February or November it is one-half, in March and October it is one-third, and so on. We also control for fixed cohort effects in the regression, as is common practice in these types of analyses.[11] We cluster standard errors on the running variable, girl's relative age of birth, as is typical when the running variable is discrete.

Finally, we also adjust our OLS treatment effects estimates to address possible inconsistencies resulting from the use of a discrete and rounded running variable (Dong 2015). The adjusted treatment effect, τ_{adj} , can be written as follows.

$$\tau_{adj} = \hat{\tau} - \tfrac{1}{2}\hat{\beta}_2 \qquad (4)$$

where $\hat{\tau}$ and $\hat{\beta}_2$ are estimated parameters from equation (2). Standard errors of the adjusted estimated treatment effect are obtained by bootstrapping (Dong 2015).

The methods described here identify a local average treatment effect (Lee and Lemieux 2010). It is perhaps useful to emphasize the local character of estimated treatment effects. While the internal validity of effects estimated in the described manner is typically argued to be strong, external validity is usually thought to be relatively weak. This suggests that it may be unreasonable to generalize about the impact of SSA on marriage and motherhood at values of the running variable outside a narrow range around the cut-off.

5. Main empirical results and robustness tests

5.1. Main results

The treatment effects analysis begins by examining the standard RD plots. Figure 1 shows the RD plots for teenage marriage and teenage motherhood, both relative to normalized date of birth. Each dot in the figure represents the average value of the outcome in question for a data-driven selected range (bin) of girls, ordered by date of birth relative to the cut-off. Attention is

[11] Employing fixed cohort effects effectively controls for the absolute age of the girl, among other things, thus allowing us to interpret our estimated treatment effects as relative age effects. In any case, our estimation results do not change much if we exclude fixed cohort effects. We keep them in order to be consistent with usual practice in the literature.

drawn to variable relationships at the threshold. The plots show a distinct downward break in both teenage marriage and motherhood at the cut-off. This implies that girls who are to the right of the cut-off and older relative to their classmates are less likely to be married and/or have given birth than those who are to the left of the cut-off and relatively younger. The plots are just suggestive of SSA impacts, however; a firm conclusion can only be made after formal estimation of treatment effects.

[Figure 1]

We now provide formal empirical estimates of the impact of SSA on teenage marriage and teenage motherhood, by estimating equation (3). Table 2 provides the treatment effects output for both teenage marriage and teenage motherhood. The regressions confirm the expected impact of SSA on both outcomes, as illustrated in the RD plots. Specifically, the results suggest that the probability that a girl born after the cut-off marries is almost two percentage points lower than that for a girl born before the cut-off. The estimated treatment effects are consistent across the three bandwidths and they are all statistically significant at the one per cent level. Given that the share of girls marrying between 16 and 18 years of age in Vietnam is 7.2 percent (Table 1), starting school late reduces teenage marriage by about 28 percent (2/7.2).

[Table 2]

The results for teenage motherhood are similar in nature: girls born after the cut-off date, and thus who are relatively older than their classmates, are significantly less likely to become mothers compared to their counterparts born before the cut-off, who are relatively younger relative to the other girls in class. Specifically, the output indicates that the probability that a teenage girl in the former group becomes a mother is about one percentage point lower than that for a girl in the latter group. As before, the magnitude and statistical significance of the estimated treatment effects are robust across choice of bandwidth. In Vietnam, the share of teenage girls who become mothers is 2.4 percent (Table 1). As such, starting school late reduces teenage motherhood by about 40 percent (1/2.4).

In the debate between (Tan 2017), McCrary and Royer (2011) and Black et al. (2011) regarding the potential impact of SSA on teenage motherhood, we therefore side with the latter. Our estimated treatment effects for teenage motherhood are, however, somewhat smaller than those found by Black et al. (2011). The latter determine that a one-year increase in SSA leads to 1.8 percentage point decline in the probability of teenage motherhood. However, it must be kept

in mind that they estimate the impact of SSA on those girls who were, in fact, treated relative to those who were not, i.e. the so-called treatment on the treated (TOT) effects. We do not have information on the extent to which girls in our sample complied with school start age regulations and therefore we estimate intent to treat (ITT) effects. TOT impacts are simply scaled up ITT effects, where the scaling factor is a function of the compliance rate.[12] As such, it is to be expected that estimated TOT effects would be larger (in absolute value) than derived ITT effects, all else being equal. We therefore take our estimates of the impact of SSA on teenage motherhood to be broadly consistent with those found by Black et al. (2011).

5.2. Robustness of results

We investigate the robustness of our empirical results across three dimensions: running variable manipulation, covariate balance, and child years of schooling.

For the RD approach to be valid there must be no precise manipulation of the running variable, child month of birth, near the cut-off point. This would be the case, for example, if parents were to time the birth of their children so that they would be able to enter school either relatively early or late, depending on their preferences. It seems highly unlikely that this would be the case, although it may be judged as at least plausible. Figure 2 shows the density of child month of birth around the cut-off. The figure does not suggest any apparent, consistent discontinuities in birth month around the threshold, indicated by the vertical line at zero. We take this as evidence of no manipulation of the running variable.

[Figure 2]

The treatment effects analysis carried out here also assumes that other predetermined covariates (and/or placebo outcomes) are balanced around the threshold. If they were not balanced, then the validity of our identification strategy would be called into question. We test the balance assumption using several key predetermined covariates on which data are available: ethnicity (non-Kinh versus Kinh), disability (existence or not), mother's and father's level of education

[12] The relationship between TOT and ITT effects is given by:

$TOT = \frac{ITT}{\Pr(Treated \mid X \geq c) - \Pr(Treated \mid X < c)}$, where c is the cut-off (Angrist and Pischke 2008). The denominator of the ratio on the right-hand side of the equation is the probability that a unit is treated given assignment to the treatment group minus the probability that a unit is treated given assignment to the control group, i.e. the compliance rate.

(less than lower secondary education or above), number of siblings, the wealth index, and residential location (rural versus urban).

Table 3 supplies the results. The variables listed down the first column are the covariates of interest. Each covariate is used as the dependent variable in equation (3), in turn, where estimation follows the same procedures as earlier described. We show the output only for our preferred bandwidth of two months. As can be seen in the table, none of the treatment effects estimates is significantly different from zero. We take this output as evidence that covariates are balanced around the cut-off, which provides further support for the claim that our identification strategy is sound and that our main empirical results are robust.

[Table 3]

Our estimates of the impact of SSA on teenage marriage and motherhood comprise two separate effects: relative age and years of schooling. As already discussed, a girl born before the cut-off will be relatively younger compared to her classmates and a girl born after the cut-off will be relatively older. However, in any given year, a girl born before the school cut-off date will also have completed one more year of school than a girl born before the cut-off. The reverse holds for girls born after the cut-off. While our main concern in this study relates to the peer effects associated with relative age, it is also clearly the case that the amount of education—i.e. years of schooling—may also reduce risky behavior (Lochner and Moretti 2004) and therefore influence outcomes of interest.

More specifically, it might be expected that a girl to the left (right) of the cut-off with more (less) schooling would be less (more) likely to get married and/or become a mother in her teenage years. As such, given our estimation results above, the relative age and schooling effects should be off-setting. Because the two separate effects are likely to be opposing, our earlier derived treatment effects probably underestimate the impact of peer influences.

To isolate the relative age effects of SSA on teenage marriage and motherhood we re-estimate equation (3), now controlling for girls' years of schooling. Table 4 supplies the relevant output. As the table shows, SSA impact increases in magnitude for both outcomes of interest. This is expected given the likely counteracting effects of relative age and years of education as indicated above. In this case, entering school one year later leads to a decrease in the probabilities of teenage marriage and motherhood of 3.2 and 1.5 percentage points, respectively. Care must be taken in interpreting these results, however, since including covariates that are not balanced

around the cut-off (such as years of schooling in the present case) in an RD regression leads to biased treatment effects estimates (Calonico et al. 2016). We carry out this analysis only to confirm that relative age and years of schooling effects are opposing in direction and to get a general sense of the magnitude of relative age effects by themselves.

[Table 4]

6. Heterogeneity of SSA effects

Previous research has shown that SSA effects are strongest among relatively disadvantaged groups. Cook and Kang (2016), for example, find that the impact of SSA on adolescent risky behavior—crime in this case—is significantly larger for those individuals with mothers who have relatively limited education and for families that are comparatively poor. This motivates the examination of SSA effects among subgroups of the population as defined by mothers' level of education and household wealth here.[13] We also examine the heterogeneity of SSA impact across girl subgroups defined by ethnicity and residential location. The following two paragraphs stimulate our interest in impact heterogeneity across these latter subgroups.

As earlier mentioned, Kinh is the dominant ethnic group in Vietnam and its members make up approximately 86 percent of the population. Non-Kinh ethnic groups, which are 53 in number, comprise the rest. Non-Kinh Vietnamese are significantly more disadvantaged than their Kinh counterparts. The non-Kinh poverty rate in 2010 was about 66 percent, for example, while that for the Kinh majority was about 13 percent (Badiani et al. 2013). In 2012, the per capita income of non-Kinh households was just 50 percent of that of their Kinh counterparts (McCaig et al. 2015). Child marriage and pregnancy rates among ethnic minorities are significantly higher than those found among Kinh as well (GSO and UNICEF 2015).

Seventy percent of the Vietnamese population lives in rural areas. There is a considerable gap in living standards between rural and urban populations. Per capita expenditure of rural households is only half that of those in urban areas (Thu Le and Booth 2014). The share of rural households with hygienic toilet facilities is just 39 percent, compared to 88 percent in towns and cities (GSO 2011). Rural girls tend to marry earlier than urban girls. The average age at first marriage for

[13] To better focus the analysis here, we restrict our attention to mother's level of education and do not consider father's education, as the former is widely recognized as the more important variable in determining child outcomes of various kinds (Ahmed and Iqbal 2016; Chen and Li 2009; Emerson and Souza 2002; Nguyen 2018).

girls in rural areas is 22, while that in urban places it is 24 (GSO 2010).

Table 5 presents the results of the heterogeneity analysis. We estimate treatment effects for subgroups along the dimensions indicated above in the same manner as previously done and show the output for our preferred bandwidth of two months in order to save space. As the table demonstrates, the estimated treatment effects are consistently larger for disadvantaged groups compared to their advantaged counterparts for both teenage marriage and teenage motherhood outcomes. The lone exception is that for non-Kinh versus Kinh teenage marriage, where the treatment effect is larger for the former but not statistically significant at the five percent level. (It is significant at the 10 percent level, however, with a p value of 0.066) All other cases conform to the described pattern: SSA effects are more pronounced for disadvantaged groups, i.e. among girls from ethnic minorities, whose mothers have comparatively little education, and who live in relatively poorer households and/ or in rural areas. Girls in these disadvantaged subgroups would benefit most from starting school later.

[Table 5]

A reasonable question to ask based on the above analysis concerns the extent to which household wealth may drive outcomes for those groups not explicitly defined as a function of wealth. That is, since non-Kinh, low education, and rural households are all relatively economically underprivileged it might be argued that the larger estimated SSA effects for those groups are merely a reflection of their relatively lower wealth and nothing more.

In Table 6, we estimate the heterogeneous effects of SSA across subgroups defined by ethnicity, mother's education, and residential location as above, but we do so separately for groups of girls whose families are positioned in the lower and upper halves of household wealth (as defined by the median level of our index), respectively. As before, we present only the estimation outcomes for our preferred bandwidth, two months. The results demonstrate that, in general, the patterns previously observed hold across both relatively low and high wealth classifications. That is, the relatively larger SSA impacts for disadvantaged groups as defined by ethnicity, mother's education, and location obtain regardless of household wealth. The main exception to the rule again relates to teenage marriage for non-Kinh and Kinh groups, as Panel A shows. But the expected results are observed for all other disadvantaged classes and outcomes. We conclude that although household wealth obviously matters in conditioning SSA impacts, other factors unrelated to economic status are important as well.

[Table 6]

What drives the heterogeneous impacts across such a broad range of girl subgroups? Alternatively put, what factors may dampen negative peer influences among relative younger school girls who are not disadvantaged as defined above? Although we cannot be certain given a lack of data, we conjecture that one such element concerns the differential availability of and participation in extracurricular activities for girls. Research from psychology shows that adolescent extracurricular involvement in US can mitigate negative peer influences and outcomes. Outside school activities broaden the peer groups of participating girls and boost young people's self-esteem and confidence, making them less susceptible to undesirable peer pressures during the school day (Fredricks and Eccles 2005; Mahoney and Cairns 1997). It seems reasonable therefore to conjecture that relatively young Vietnamese girls who engage in extracurricular activities might also be less likely to suffer from undesirable peer effects and avoid risky behaviors.

Data from 2010 Vietnam Living Standard Survey (2010 VHLSS) and the 2009 Young Lives Survey show that the difference in the extra class participation rate between Kinh and non-Kinh girls is indeed stark—51 percent for the former and 12 percent for the latter. A similar participation gap—25 percent—is found between girls whose mothers' have lower secondary education and above and those whose mothers have primary school education and below. The percentage of school-age girls living in urban areas taking classes outside school is 59 percent, compared to 36 percent in rural areas. This descriptive evidence is at least consistent with our conjecture that so-called advantaged girls may be able to escape negative peer effects due to participation in activities outside school.

To get a better idea of the extent to which the advantaged/disadvantaged dichotomy consistently influences extracurricular class participation we regress a dummy variable indicating if the girl joined such classes (=1) or not (0) against a collection of dummy variables that specify girl membership in the collection of disadvantaged groups, as defined by ethnicity, mother's level of education (as proxied by the highest level of education attained by any female in the household aged 19 or more), household wealth, and residential location. We also control for the child's age (which varies between seven and 18 years) in the regression. Estimation is by probit with robust standard errors.

Regression results (based on 2010 VHLSS data) are presented in Table 7. As the table shows, all

disadvantaged (=1)/advantaged (=1) dummy variables are highly significant determinants of the probability that girls participate in extracurricular classes. The regression output implies that disadvantaged girls, as defined across all indicators, are relatively less likely to engage in extracurricular activities. The results here also parallel the treatment effects estimations carried out above in that non-wealth related variables are significant even after controlling for wealth. The outcome broadly supports our conjecture that advantaged girls are more likely to engage in extracurricular activities and that, according to received theory, such engagement may help explain why these girls are less likely to suffer the ill effects of negative peer pressure, which, in turn, decreases the likelihood of their early marriage and motherhood.

[Table 7]

The above results should be considered suggestive and tentative. More rigorous analysis of the issue is beyond the scope of the current exercise. Further examination of the extent to which such extracurricular and related activities might dampen deleterious school-based peer influences among relatively younger girls would seem to constitute a fruitful avenue for future research.

7. Summary and conclusions

Although the impact of SSA on child education and labor force outcomes is widely acknowledged, effects on teenage marriage and childbearing are still relatively little understood, given the limited and inconsistent research heretofore undertaken. This paper examines the impact of SSA on teenage marriage—a first in the literature—and teenage motherhood in Vietnam. The current rates of child marriage and motherhood in Vietnam are not alarmingly high, but the recent rapid rise in both is concerning. This concern motivates the research in this paper.

In Vietnam, children start school in the year they turn six, and the cut-off date for school entry is 1st January. This regulation leads to a discontinuity in the age that children start school, e.g. children born in December enter primary school one year earlier than those born in January of the following year. We exploit this discontinuity and employ regression discontinuity methods to identify and estimate the causal effects of SSA on teenage marriage and motherhood.

We find that girls born before the cut-off date are more likely to get married and/or give birth between the ages of 16 and 18 than those girls born after the cut-off. Girls who enter school

early are two percentage points and one percentage point more likely to experience teenage marriage and motherhood, respectively, than girls who start late. This implies that starting school late reduces teenage marriage and motherhood by about 27 and 40 percent in Vietnam, respectively. These are substantial effects. We also find that SSA impacts are heterogenous across subgroups. The harmful SSA effects associated with starting school early are more pronounced for relatively disadvantaged girls, especially those from minority ethnic groups, whose mothers have relatively limited education, whose households are relatively poor, and who live in rural areas. Finally, we also provide some tentative and preliminary evidence that engagement in extracurricular activities may help mitigate negative peer influences that confront relatively younger girls. The impact of outside school activities on dampening deleterious peer effects merits additional research.

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Table 1. Summary statistics

Variables	Mean	Standard Deviation
Teenage marriage	0.072	0.259
Teenage motherhood	0.024	0.153
Non-Kinh ethnic minority	0.255	0.436
Disability	0.017	0.123
Mother's education below lower secondary level	0.653	0.476
Father's education below lower secondary level	0.639	0.480
Number of siblings	2.816	1.299
Wealth index	0.356	0.191
Rural area	0.768	0.422

Source: Authors' calculation from the 2009 Vietnam Population and Housing Census

Table 2. Effect of SSA on teenage marriage and teenage motherhood

	h =2	h =3	h =4
Teenage marriage	-1.944**	-1.917**	-1.898**
	(0.452)	(0.306)	(0.256)
Teenage motherhood	-1.065**	-0.995**	-0.966**
	(0.264)	(0.183)	(0.158)
Observations	131,182	216,355	291,017

Notes: h is the bandwidth in months; Estimated coefficients and standard errors times 100; * $p<0.05$; ** $p<0.01$; Bootstrapped standard errors with 1000 replications in parentheses

Table 3. Covariate balance test

	τ_{adj}
Non-Kinh ethnic minority	0.247 (0.888)
Disability	0.086 (0.237)
Mother's education below lower secondary level	0.169 (0.750)
Father's education below lower secondary level	-1.452 (0.833)
Number of siblings	-4.432 (2.718)
Wealth index	0.595 (0.376)
Rural	-0.059 (0.865)

Notes: Bandwidth is 2 months; * $p<0.05$; ** $p<0.01$; Bootstrapped standard errors with 1000 replications in parentheses

Table 4. Effect of SSA on teenage marriage and teenage motherhood, controlling for years of schooling

	h =2	h =3	h =4
Teenage marriage	-3.171**	-3.189**	-3.256**
	(0.429)	(0.289)	(0.247)
Teenage motherhood	-1.587**	-1.535**	-1.543**
	(0.259)	(0.179)	(0.153)
Observations	131,182	216,355	291,017

Notes: h is the bandwidth in months; Estimated coefficients and standard errors times 100; * $p<0.05$; ** $p<0.01$; Bootstrapped standard errors with 1000 replications in parentheses

Table 5. Heterogeneous effects of SSA among subgroups of girls

		A. Ethnicity
Teenage marriage	Non-Kinh	-2.345
		(1.274)
	Kinh	-1.810**
		(0.376)
Teenage motherhood	Non-Kinh	-2.635**
		(0.861)
	Kinh	-0.509**
		(0.176)
		B. Mother's education
Teenage marriage	Below lower secondary level	-2.562**
		(0.639)
	Lower secondary level and above	-0.267
		(0.176)
Teenage motherhood	Below lower secondary level	-1.511**
		(0.454)
	Lower secondary level and above	-0.067
		(0.086)
		C. Wealth index
Teenage marriage	Below median	-2.811**
		(0.750)
	Above median	-1.387**
		(0.390)
Teenage motherhood	Below median	-1.955**
		(0.507)
	Above median	-0.448
		(0.243)
		D. Residential Location
Teenage marriage	Rural	-2.428**
		(0.522)
	Urban	-0.739
		(0.635)
Teenage motherhood	Rural	-1.497**
		(0.307)
	Urban	0.116
		(0.382)

Notes: Bandwidth is 2 months; Bootstrapped standard errors with 1000 replications in parentheses; Estimated coefficients and standard errors times 100; * $p<0.05$; ** $p<0.01$.

Table 6. Heterogeneous effects of SSA among subgroups of girls by wealth index

		Wealth index	
		Below Median	Above median
		A. Ethnicity	
Teenage marriage	Non-Kinh	-3.656	-0.356
		(1.921)	(1.905)
	Kinh	-2.407**	-1.496**
		(0.695)	(0.406)
Teenage motherhood	Non-Kinh	-3.606**	-1.259
		(0.994)	(1.018)
	Kinh	-0.926*	-0.276
		(0.394)	(0.219)
		B. Mother's education	
Teenage marriage	Below lower secondary level	-3.413**	-1.925*
		(1.074)	(0.752)
	Lower secondary level and above	-0.569	-0.156
		(0.427)	(0.207)
Teenage motherhood	Below lower secondary level	-2.445**	-0.667
		(0.604)	(0.435)
	Lower secondary level and above	-0.172	-0.029
		(0.180)	(0.106)
		C. Residential location	
Teenage marriage	Rural	-3.129**	-1.838**
		(0.934)	(0.646)
	Urban	-0.493	-0.722
		(2.101)	(0.624)
Teenage motherhood	Rural	-2.279**	-0.727*
		(0.537)	(0.342)
	Urban	0.743	0.030
		(1.420)	(0.322)

Notes: Bandwidth is 2 months; Bootstrapped standard errors with 1000 replications in parentheses; Estimated coefficients and standard errors times 100; * $p<0.05$; ** $p<0.01$.

Table 7. Probit model of participation in extracurricular activities

	Marginal effect
Non-Kinh ethnic minority	-0.294**
	(0.019)
Mother's education below lower secondary level	-0.180**
	(0.014)
Wealth index below median	-0.089**
	(0.016)
Rural area	-0.074**
	(0.018)
Age	0.006**
	(0.002)
Observations	3,827

Source: Data from 2010 VHLSS; Robust standard errors in parentheses; * $p<0.05$; ** $p<0.01$.

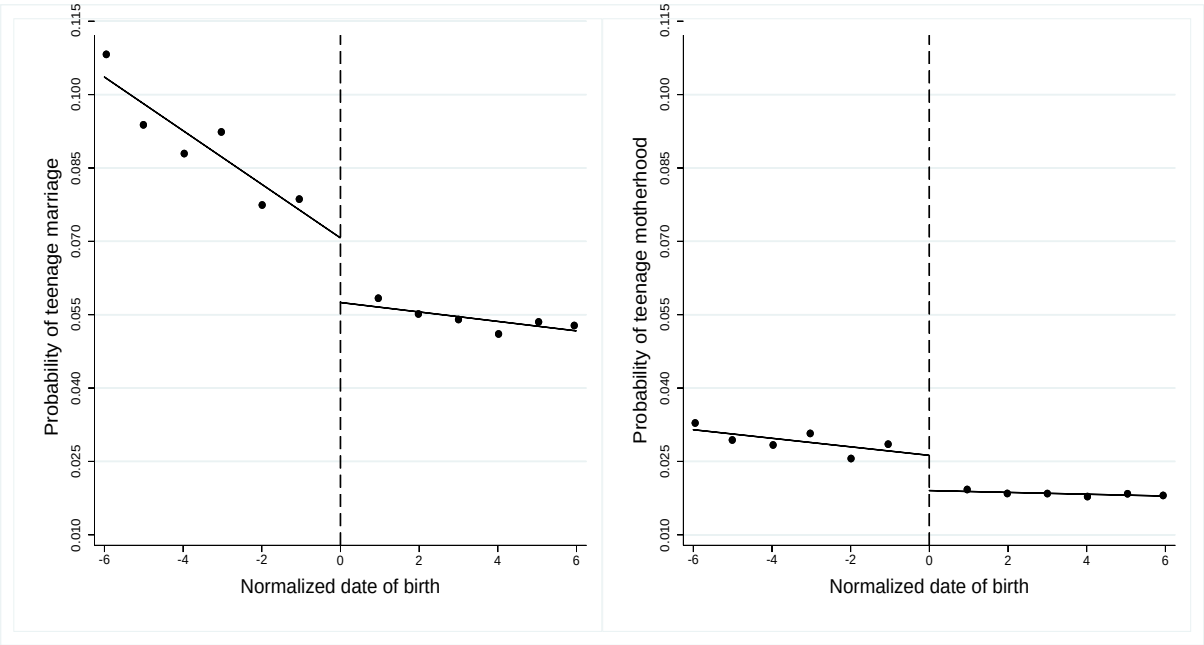

Figure 1. RD plots for teenage marriage and teenage motherhood

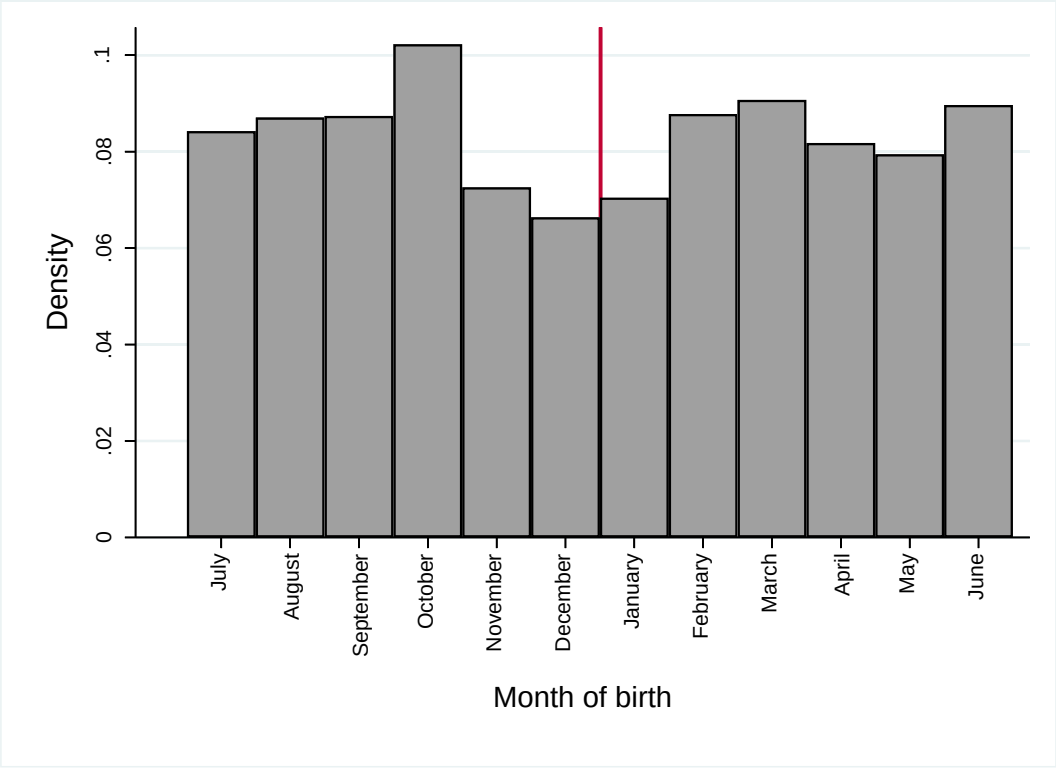

Figure 2. Density of month of birth