Understanding Ethnic Inequality: The Long-Term Effects of *In-Utero* Exposure to Hostile Policies

Thang Dang

Centre for Fertility and Health, Norwegian Institute of Public Health Marcus Thranes Gate 2, 0473 Oslo, Norway (E-mail: thang.dang@thangdang.org).

Abstract

Although hostile policies are typically implemented to target ethnic and racial minority groups in many countries, their consequences for socio-economic inequalities have been rarely studied. Exploiting the heterogeneity in *in-utero* exposure to hostile policies among various ethnicities across birth time groups in South Vietnam between 1956 and 1963, we investigate the long-run and intergenerational effects of *in-utero* exposure to hostile policies. Exposure reduces schooling, labor market outcomes and economic well-being while increases fertility at both the intensive and extensive margins for the first generation. It also stunts intergenerational schooling mobility. Exposure further decreases children's human capital.

Keywords: Hostile policies; Ethnic inequality; Fetal origins; South Vietnam

JEL codes: I12, I25, J13

1. Introduction

Ethnic and racial minorities are typically the target of hostile policies from many governments. For instance, the United Kingdom's 2012 hostile environment policy and the Trump administration's 2017 immigration enforcement policies were strongly criticized by the public because these policies potentially impose negative impacts on British Asians in the United Kingdom and Hispanic Americans in the United States, who have a close link with immigrants (Cai 2020). Minority groups even encounter a worse situation from extremely oppressive policies in authoritarian regimes, for example, the Uyghurs people in the People's Republic of China and the Rohingya ethnicity in Myanmar. Understanding the consequences of hostile policies may provide crucial insights into the causes of persistent ethnic and racial disparities in socio-economic outcomes (Alesina et al. 2016; Bayer and Charles 2018; Chetty et al. 2020; Darity and Nembhard 2000; Elder and Zhou 2021; van de Walle and Gunewardena 2001).[1] Yet, the effects of such hostile policies have been rarely studied.

A well-established literature in economics has shown that early-life conditions likely affect human capital formation and subsequent socio-economic outcomes (Bharadwaj et al. 2018; Almond and Currie 2011; Currie and Almond 2011). Children who are conceived in an adverse social environment are more likely to bear long-run disadvantages (Bhalotra and Rawlings 2013; Currie and Vogl 2013). This is because adverse conditions during pregnancy are likely detrimental to fetal development (Conti et al. 2020), birth weight (Bundervoet et al. 2009; Camacho 2008; Quintana-Domeque and Ródenas-Serrano 2017), and maternal mental health (Persson and Rossin-Slater 2018; von Hinke et al. 2019), which all likely mediate children's long-run outcomes (Behrman and Rosenzweig 2004; Black et al. 2007; Bhalotra and Rawlings 2013; Case and Paxson 2008). In this study, we focus on exposure to hostile policies towards ethnic and racial minorities during pregnancy. When policymakers implement such policies, they are generating an adverse social environment which makes the life for targeting groups more difficult through economic hardship, life uncertainty, depression, and living isolation which

[1] Ethnic and racial inequalities have been revealed in previous studies through different outcomes, for example schooling (Kırdar 2009; Panza 2020), labor market outcomes (Barr and Oduro 2002; Goldsmith et al. 2006), access to economic resources (Alesina et al. 1999; De Luca et al. 2018; Fafchamps 2000), cognitive skills (Fryer and Levitt 2013), and health (Alexander and Currie 2017; Alsan and Wanamaker 2018; Williams et al. 2019) among others.

may be harmful to children in the critical period of child development. Based on the "Fetal Origins Hypothesis" (Almond and Currie 2011; Barker 1990; Currie 2011), it is arguably expected that children who are *in-utero* exposed to hostile policies would have poorer long-run outcomes compared to those who are not exposed.

To test this hypothesis, we exploit changes in policies towards the Chinese ethnicity (known as the *Hoa* ethnicity) the government of the Republic of Vietnam (RVN) implemented in South Vietnam between 1956 and1963 to investigate the impacts of *in-utero* exposure to hostile policies on the long-run schooling, family and economic well-being outcomes of the directly affected generation as well as the human capital of the next generation. Before 1956, despite being an ethnic minority, the Chinese people established a highly autonomous position of life and work and enjoyed comparable economic prosperity in South Vietnam (An 1967). In August 1956, President Ngo Dinh Diem of the RVN, who was well-known as a strong Vietnamese nationalist, introduced a series of new policies that directly targeted the Chinese ethnicity by imposing hostile punishments on social life and economic activities (Amer 2011). For example, these measures consisted of excluding the Chinese ethnicity from doing key business sectors, forcing the Chinese to take Vietnamese citizenship, and replacing the former Chinese school system with the Vietnamese system with Vietnamese curricula among other harsh measures (An 1967). Historical evidence shows that these hostile policies created a *strong* adverse social environment for the Chinese ethnicity in South Vietnam from August 1956 until the assassination of President Diem in November 1963 (Ungar 1987). Drawing on a difference-in-difference (DiD) research design, we compare the difference in an outcome (such as schooling years among others) between Chinese-ethnic individuals who were *in-utero* exposed to the hostile policies and older Chinese-ethnic those who were born before these policies with the corresponding difference in the outcome among other ethnicities. We make use of the restricted large-scale data from Vietnam's Population and Housing Census of 1999 and 2009 to produce precise estimates of the effects of interest conditional on ethnicity and birth time fixed effects. We provide evidence on no pre-trends and strong balances in pre-determined characteristics for our identifying ethnicity-by-birth time-level variation in exposure to the hostile policies to assure the validity of our DiD strategy.

Our results show that *in-utero* exposure to the hostile policies is detrimental to both the first and second generations of the Chinese ethnicity in South Vietnam. It particularly reduces the human capital of the directly affected generation (such as 0.4 years of schooling, and 5-7 percentage points for primary school and post-primary school completion). Working through a reduction in human capital formation, exposure lowers economic activity outside the agricultural sector and the probability of being married but increases women's fertility at both the extensive and intensive margins as well as household size. Exposure further has negative effects on household economic well-being by reducing housing and sanitation conditions, increasing the use of polluted cooking energy, and reducing household wealth. Interestingly, our gender heterogeneity results indicate that the negative effects are more pronounced among males than females for several outcomes, in particular schooling years and post-primary school completion, leading to 18-26-% decreases in the gender gaps in these outcomes which are considerably observed among pre-treated individuals. Importantly, exposure reduces intergenerational schooling mobility of the directly affected individuals. Finally, we detect the negative effects of *in-utero* exposure to the hostile policies on the next generation by reducing offspring's school enrollment and literacy skills for post-primary school-aged students.

Our paper makes three key contributions. First, it would contribute to a central question of studying *causal* determinants of ethnic and racial inequality. Large and persistent ethnic and racial disparities in socio-economic outcomes have been one of the most salient features facing social inequality in many countries (Alesina et al. 2016; Bayer and Charles 2018; Chetty et al. 2020; Darity and Nembhard 2000; Elder and Zhou 2021). Understanding related factors behind this inequality may provide insights for reducing large and persistent inequality and promoting the social inclusion of ethnic and racial minorities (Darity and Nembhard 2000; de Walle and Gunewardena 2001). Previous studies have put forward several explanations for the causes of ethnic and racial inequality including anti-discrimination legislation (Freeman 1973), and school quality (Card and Krueger 1992), and recently minimum wage (Derenoncourt and Montialoux 2021). We focus on governmental hostile policies targeting an ethnic or racial minority group as an additional explanation. The relationship between *in-*

utero exposure to hostile policies and ethnic inequality is proposed to work through the human capital formation of the directly affected individuals.[2]

Second, our paper supplements a well-documented body of literature studying the impacts of early-life conditions on children's long-run outcomes. When investigating the long-run consequences of *in-utero* exposure to an adverse social environment, previous studies mainly draw on extremely devastating events such as wars or armed conflicts (Akresh et al. 2012, 2021; Leon 2012; Phadera 2021; Singhal 2019), and other catastrophic circumstances of pandemics, terror attacks and natural disasters (Almond 2006; Karbownik and Wray 2019; Maccini and Yang 2009; Marcus 2021). To our knowledge, our paper is the first to exploit hostile policies which may be less severe compared to exceedingly disastrous events, but the costs of such hostile policies have been not fully understood. Our findings suggest that governmental hostile policies may generate considerable economic costs through their adverse impacts on the life trajectories of those exposed in the womb. We importantly expand our analysis to the impacts on the next generation which have been under-studied for the consequences of *in-utero* exposure to adverse shocks (Akresh et al. 2021; Brown 2020; Phadera 2021; Schwandt 2018).

Third, our paper relates to a growing literature studying *causal* determinants of intergenerational mobility. Previous research demonstrates the importance of neighbourhood environment (Chetty and Hendren 2018a, 2018b), higher education categories (Chetty et al. 2017), income shocks (Bütikofer et al. 2018), compulsory educational laws (Cornelissen and Dang 2021; Demirel and Okten 2020), public health policies (Bütikofer and Salvanes 2020), and parental income timing (Carneiro et al. 2021). Our findings uncover a harmful effect of hostile policies on schooling mobility across generations, exaggerating inequality of opportunity among ethnic and racial minorities.

The remainder of this paper is structured as follows. We start by providing historical background on ethnic Chinese and Ngo Dinh Diem's hostile policies in South Vietnam in Section 2. Section 3 describes the datasets used in this research, and Section 4 discusses our empirical strategy. Section 5

[2] While the human capital formation has been uncovered to be an important driver of long-run and intergenerational socio-economic success (Björklund and Salvanes 2011; Black and Devereux 2011; Black et al. 2005; Duflo 2001; Oreopoulos and Salvanes 2011), it can be negatively affected by *in-utero* exposure to adverse shocks (Akresh et al. 2012, 2021; Leon 2012; Karbownik and Wray 2019; Maccini and Yang 2009).

presents our results including the baseline effects and gender heterogeneity. Section 6 concludes the paper.

2. Historical background

2.1 The Chinese ethnicity in South Vietnam

The Chinese ethnicity comprises an important part of the ethnic minority population in South Vietnam. The establishment of the Chinese community in South Vietnam was originated from two mass migration flows from China. The first Chinese arrived in South Vietnam by the late 17th century through large migration flows from China to Southeast Asian countries for seeking new homes when the Ming dynasty collapsed and was replaced by the Manchu regime (Share 1994). The second flows had arrived during the French rule in Indochina by the late 19th and early 20th centuries (Feng 2017). Given the high demand for workers for running the colonial economy, the French encouraged immigrants from neighbouring countries to fulfil the shortage of labour supply provided by native Vietnamese (An 1967). A considerable number of Chinese people settled in South Vietnam to seek new economic opportunities. Cocochina especially Saigon-Cholon attracted the largest number of Chinese residents in South Vietnam. When Vietnam re-gained its independence from the colonists after World War II, Chinese immigrants and their next generations remained in South Vietnam making up a key component of ethnic minorities (An 1967). The Chinese population became the largest single minority group, amounting to about one million as of the middle of the 1950s (An 1967; Marsot 1993).

During colonial times, Chinese residents received considerable support from the French.[3] French colonists relied on Chinese immigrants to establish their bureaucracy in the initial days in Cochinchina and used the Chinese community's extensive and well-organized networks of business, transportation, and money-lending systems to proceed with economic transactions such as trading goods and services both domestically and internationally (Engelbert 2008). By offering economic opportunities to the

[3] The bilateral treaties between France and the Qian government in China (1886 Tientsin Treaty, 1930 Nanking Convention, and 1946 Chungking Treaty) allowed Chinese people to move, inhabit and participate in economic activities in French-ruled Indochina (Tran 1993). These treaties set the Chinese in a favored position in Indochina. Based on these treaties, the French treated the Chinese as their partners for colonial exploitation (Share 1994).

Chinese community, the French were preventing Chinese residents from joining opposition movements against the French regime which were led by Vietnamese natives (Engelbert 2008). Thanks to French support combined with their vast capital, entrepreneurship and skilled manpower, the Chinese established an influential economic power until the RVN was established in 1955 in South Vietnam (Barton 1977; Engelbert 2008; Schrock 1966). By the middle of the 1950s, the Chinese dominated the domestic rice market and monopolized main industries such as milling and rice business as well as retail trades in urban areas. Chinese businessmen were also key players within banking, transportation, textile production, sugar milling and real estate industries. Furthermore, they controlled over nearly half of the total imports and a large share of the international trade volume of South Vietnam (Stern 1985).

In terms of social life, Chinese residents were closely connected within their families, clans and communities and kept social and cultural distinctions with Vietnamese natives as well other ethnic minorities (Chan 2018; Stern 1985). Under French rule, Chinese residents were given the autonomy to organize their society into five autonomous congregations (known as *bang*)[4] and several occupational associations (known as *hoi*).[5] Within congregations, Chinese communities constructed hospitals, markets, restaurants, media and press agencies, schools, and temples among other infrastructures, establishing their own 'state' within South Vietnam (Share 1994). Having highly autonomous organisations of life and work allowed the Chinese people to stay outside Vietnamese natives' political life and then to focus on making business profits and accumulating wealth. Making Chinese communities socially and culturally separated from and to some extent competitive with Vietnamese natives was a 'divide and rule' strategy the French used to control key population groups in Indochina (Ungar 1987).

[4] Five congregations included the Cantonese, Teochiu, Hokkien, Hakka, and Hainan (Tran 2018). These congregations played a role as key administrative units responsible for governing all life aspects of the Chinese community. These congregations were self-managed to select their heads who provided connecting roles between the French bureaucracy and Chinese residents (Engelbert 2008).

[5] Chinese people used occupational associations to manage their economic activities for different industries. These associations included the Chinese Chamber of Commerce, the Association of Overseas Chinese Rice Merchants, the Association of Importers and Exporters, and the Association of Chinese Grocers.

In summary, the Chinese ethnicity in South Vietnam enjoyed a comfortable living environment before August 1956 in which they were highly socially autonomous and held an influential economic power. However, Chinese residents were in a relatively opposite position with Vietnamese natives because they shared with the French the major part of the colonial economy's benefits, marginalizing Vietnamese natives into a disadvantaged position (Share 1994; Tran 1993). Therefore, Chinese residents became an obvious target of Vietnamese nationalists who hold political powers in their hands when the French left Vietnam. In August 1956, President Ngo Dinh Diem of the RVN introduced a series of *hostile* policies targeting the Chinese ethnicity, making adverse changes in Chinese residents' social environment.

2.2 Ngo Dinh Diem's hostile policies towards ethnic Chinese

The Geneva Accords of July 1954 had divided Vietnam into two states with different political regimes from 1955 until the country's reunification in 1975. The Republic of Vietnam (RVN) in South Vietnam was led by Ngo Dinh Diem, who strongly valued an independent Vietnam (Ungar 1987). Ngo Dinh Diem believed that attacking the Chinese ethnicity could help him gain support from the majority of Vietnamese natives whose attitudes of animosity towards the Chinese for the centuries and then strengthen his political image as a Vietnamese nationalist (Share 1994). In August 1956, Diem started a series of hostile policies targeting the Chinese ethnicity (which was known as '*Chinh sach Hoa Kieu*'). Diem's policies were designed not only to weaken the Chinese ethnicity's economic influence but also to force Chinese residents' cultural and social assimilation into Vietnamese society in a hostile manner.

It is important that both starting and ending timings of the policies were highly arbitrarily driven by exogenous changes in contemporaneous political situations both within and outside the RVN. First, the starting timing was strongly affected by an unexpected change in the PRC's new policy on the overseas Chinese people in 1955. Before 1955, the PRC by law accepted dual citizenship and '*citizenship by bloodline*' which automatically treated Chinese-originated people residing in other countries as Chinese citizens (Mitchison 1961). Therefore, the overseas Chinese including those living in the RVN were protected by the PRC (Han 2009). Such a citizenship policy of the PRC prevented small countries in Southeast Asia from making adverse policy changes towards Chinese residents

because of concerns about potential conflicts with the PRC (Suryadinata 2017). Yet, the situation was unexpectedly changed in April 1955 when Premier Zhou Enlai of the PRC declared at Bandung Conference that the PRC decided to end its '*citizenship by bloodline*' policies and called for the overseas Chinese to respect local customs and laws of countries of residence (Han 2009). By setting apart with the overseas Chinese communities, the PRC aimed to soothe anxieties of Chinese expansionism and thus helped the PRC construct trusty relations with Southeast Asian countries (Fredman 2014). This change in the PRC's citizenship policies gave Ngo Dinh Diem confidence to implement his policy reform on Chinese issues (Ungar 1987). Second, the ending timing of the policies was also highly exogenous. In November 1963, Ngo Dinh Diem was suddenly assassinated in an unexpected coup *d'état* made by a group of military generals of the RVN. The death of President Diem importantly generated a discontinuity in the implementation of his policies towards the Chinese ethnicity which was faded away under subsequent governments. These policies' highly unpredictable timings allow us to construct individuals' "as good as" randomly assigned exposure to the hostile policies during pregnancy among different birth cohorts (see Section 4.1 for the details).

Ngo Dinh Diem's policies focused on four main areas, including citizenship, life and work organizations, economic activities, and education (Share 1992). First, for the citizenship issue, Diem's Ordinance Number 48 promulgated in August 1956 required all Chinese people born in the RVN to automatically be granted Vietnamese citizenship. Chinese residents were called Chinese Vietnamese (known as '*Nguoi Viet goc Hoa*') rather than only Chinese as before. Individuals without Vietnamese citizenship were given a choice of returning to their countries of origin (the PRC or Taiwan) or being banned from participating in economic activities. Moreover, Chinese residents were forced to adopt Vietnamese names rather than using only their Chinese names as before or to pay higher taxes for not complying with the new rule.

Second, regarding life and work organisations, Diem abolished Chinese communities' five autonomous congregations and occupational associations in June 1960. The government seized large valued assets of these organisations and required all Chinese residents to directly follow the RVN's laws

for all aspects of their lives and work like other ethnicities in South Vietnam. Males were obligated to fulfil their military services.

Third, in terms of economic activities, Diem launched new regulations aimed to redirect economic resources which were traditionally dominated by Chinese residents before 1956 back to Vietnamese natives. Ordinance Number 53 issued in September 1956 prohibited Chinese residents from doing business in the eleven most profitable industries: pork and fish retail, retailed stores, coals, gasoline, sale of used items, textile, scrap metals, rice trade, transportation, rice milling, and moneylending (Stern 1985; Schrock 1966). This Ordinance also required Chinese-owned enterprises to transfer the ownership to Vietnamese people or to be closed (Stern 1985). Although Chinese businessmen tried to overcome the law by several measures for example hiring Vietnamese persons for ownership fronts, this regulation created turbulence in Chinese inhabitants' economic activities. Moreover, the government restricted the Chinese ethnicity's participation in other industries by implementing discrimination in labor markets. For instance, a Chinese doctor was less likely to be appointed in a hospital than a Vietnamese doctor although both held a similar qualification (Stern 1985). All Chinese households and firms were required to pay taxes directly to the RVN government.

The fourth focus of Diem's policies was on education. New policies reformed all schools established and managed by Chinese communities before. School ownership had to be transferred to the state; and school principals appointed had to be Vietnamese people, excluding Chinese people from holding managerial positions in schools (Fitzgerald 1972). At these schools, the Chinese language which was used as the main instructing language before 1956 was replaced by the Vietnamese language (Fitzgerald 1972). Furthermore, a new curriculum with a focus on Vietnamese history and culture was applied to educating Chinese children. A successful pass of *Vietnamese literature* at the end of primary school was one of conditions for the secondary education admission. The law also reduced the time for teaching the Chinese language. Despite these aggressive educational policies, Chinese children's school enrollment rates were still stable compared to the rates in the years before 1956 (Fitzgerald 1972).

2.3 Chinese ethnicity's responses and hostile environment

Chinese residents strongly reacted to Diem's policies. They particularly organized protests and large-sized market boycotts threatening to obstruct the economy (Amer 2011). Police forces used harsh measures such as beatings and thrashing to control Chinese protesters, leading to violent outbreaks between the government and Chinese residents. A very fearful and uncertain perspective faced Chinese communities when diplomatic efforts from other countries such as Taiwan to stop the RVN from using harsh actions towards Chinese residents in South Vietnam were failed.[6] Chinese residents also ended their economic activities to threaten the Diem administration about the collapse of South Vietnam's economy. For example, they withdrew their money from banks to devalue the Vietnamese currency; and they ceased trading transactions and flows of goods and services and closed retailed shops. These reactions to some extent created disorder within the economy and importantly the government's considerable loss of tax revenues. As a result, to keep the stability of the economy, Diem relaxed several economic punishments, for example giving Chinese inhabitants a limitation of 49% for business ownership (Ky 1963).

Although Chinese residents were almost successful in protecting their wealth, they encountered a chaotic social environment during 1956-1963 (Ungar 1987). The Chinese people's situation became extremely difficult under Diem's policies and Chinese inhabitants in South Vietnam were very insecure about their future. Several thousand Chinese households escaped from South Vietnam, but the majority of the Chinese decided to remain in South Vietnam (Share 1994). Their choice was to adapt to Diem's policies.

After Ngo Dinh Diem died in 1963, his hostile policies towards the Chinese ethnicity were not taken seriously by subsequent governments, making the social life of the Chinese more comfortable. The Chinese since then gradually restored their economic position to the way it was before 1956. By the early 1970s, the Chinese ethnicity again owned almost all commercial banks and they were returning to dominate other business industries in South Vietnam (Stern 1985). Diem's untimely death was a positive turning point for restoring the Chinese's economic position in South Vietnam.

[6] The Diem government declared that these actions were totally an internal affair of the RVN. For maintaining a relation with the RVN, Taiwan was then accepted to disregard the Chinese issue in South Vietnam (Ungar 1987).

In our analysis, we include those born between 1951 and 1962 into our sample because such a sample provides a treatment group (ethnic Chinese) and a comparison group (other ethnicities) with the best measure of *in-utero* exposure to an adverse social environment shock. Before 1956, all ethnicities in South Vietnam lived in highly similar social environments because no shock was affecting a particular ethnic group while ignoring other ethnic groups (Taylor 2013). Yet, the Chinese ethnicity faced a harsh social environment induced by Diem's hostile policies while the social environment of other ethnic groups remained unchanged between 1956 and 1963 (Taylor 2013).

3. Data

Our analysis draws on two large-scale and random samples of the 1999 and 2009 Population and Housing Census of Vietnam. While the 1999 sample includes about 25 million individuals (equivalent to 33% of the total population), the 2009 sample consists of about 14 million individuals (equivalent to 15% of the total population). These are both the restricted versions of these censuses and the largest micro datasets available for Vietnam which contain information about demographics, schooling, employment, mortality, wealth, and housing of the respondents across the country's regions. Our treatment ethnicity group is the Chinese ethnicity which only accounts for about 1% of the total population. Using household surveys that typically contain a small number of ethnic Chinese individuals would prevent us from producing precise estimates of effects of interest conditional on birth groups and ethnicity fixed effects. Large-scale samples from these censuses would therefore help us overcome the drawback of using small samples from household surveys.

We use these samples to create our variables of interest in which depending on the outcomes, one of these censuses or both censuses are used. We first use birth year and birth month information to create 9-month birth groups which help us to identify the groups of *in-utero* exposure to the policies. We further extract information on ethnicity to construct dummies for various ethnic groups. Based on the sizes of ethnicities from these census samples, we group all 54 ethnicities living in Vietnam to construct 23 ethnicity groups in which the *Kinh* ethnicity (the majority) and other large minorities (including the

Chinese ethnicity) are classified a single group and the rest of other minor minorities are classified into one group.[7] We then construct a set of outcomes for both the first and second generations.

We restrict the census data to the individuals born in May 1951-July 1962 to construct our sample of the first generation. We particularly extract both censuses to construct educational outcomes, including schooling years and dummies for completing primary school, post-primary school, and a dummy for a college degree. We further extract a range of labor market and family outcomes. These outcomes consist of a dummy for non-agricultural work (using the 1999 census), a dummy for being married (using both censuses), a dummy for having a child and number of children for females (using the 1999 census), and household size which is measured by the number of persons (using the 1999 census).

We use the 2009 census to construct several proxy outcomes for family living standards including solid house, old cooking energy, improved sanitation, and asset index. *Solid house* is an indicator that takes a value of 1 if the house has a solid structure made from masonry and reinforced concrete or a value of 0 if the house is constructed with an unsolid or wooden structure. *Old cooking energy* is an indicator that takes a value of 1 if the household uses charcoal, firewood, or kerosene or a value of 0 if the household uses gas or electricity for cooking energy. *Improved sanitation* is an indicator that takes a value of 1 if the household has one of the kinds of improved toilets[8] or a value of 0 if the household has one of the types of unimproved toilets.[9] Finally, *asset index* is the sum of 8 durable assets (television, radio, telephone, computer, washing machine, refrigerator, air conditioner, and motorcycle).

To estimate the effect of parental *in-utero* exposure to Diem's policies on the next generation, we restrict the sample to children aged 11-17 of the parents who were born between May 1951-July 1962. We exploit information on education from both censuses to construct two proxy variables for the human capital of the next generation. These include a dummy for school enrollment and a dummy for literacy

[7] These 23 ethnicity groups include Kinh, Tay, Thai, Kho-me, Hoa, Nung, Gia-rai, Ede, Bana, Cham, Coho, Xo dang, Hre, Ra glai, Mnong, Bru Van Kieu, Co Tu, Gie Trieng, Co, Muong, Xtieng, Ma, and other ethnicities.

[8] Improved toilets include flush toilet, connection to a piped sewer system, connection to a septic system, flush or pour-flush to a pit latrine, pit latrine with slab, ventilated improved pit latrine, or composting toilet.

[9] Unimproved toilets include public or shared latrine, flush or pour flush to elsewhere (not into a pit, septic tank, or sewer), pit latrine without slab, bucket latrines, hanging toilet or latrine, or no facilities.

skills. Moreover, we construct a set of control variables that are included in our regressions. These controls consist of age, age squared (for the next generation estimation), dummies for males, rural areas, and religious affiliations of Buddhism, other religious affiliations, and no religious affiliation. Table A.1. in Supplementary Appendix presents summary statistics of the outcome variables for the total sample, ethnic Chinese and non-Chinese samples.

4. Empirical strategy

4.1 Definition of treatment and exposure groups

Our empirical strategy relies on variation in *in-utero* exposure to Diem's policies for different ethnicity and birth time groups.[10] Because the Chinese were the only ethnicity receiving the treatment, we use them as the treatment group whereas we use other ethnicities as the comparison group.

To identify individual groups of *in-utero* exposure to Ngo Dinh Diem's adverse policies, we create 9-month birth groups. We treat those who were born in July 1956 and backwards as no exposure birth groups because they were born before the commencement of the policies in August 1956. Among those who were exposed to Diem's policies during pregnancy, we classify individuals who had less than 9 months of being exposed to Diem's policies as the partial exposure birth group and those who had a 9-month period of *in-utero* exposure to Diem's policies as the full exposure birth group.

-Figure 1-

We restrict our baseline sample to those born between May 1951 and July 1962. Figure 1 demonstrates the timings of Diem's policies and *in-utero* exposure to the policies of various birth time groups. In total, we have15 birth time groups indexed by t. We use individuals born between May 1951 and July 1956 (the birth time $t = -7, \dots, -1$) as the *no exposure* birth time group. We treat individuals who were born between August 1956 and April 1957 as the *partial exposure* birth time group ($t = 0$), and those born between May 1957 and July 1962 with a *full exposure* ($t = 1, \dots, 7$).

[10] Several previous studies' identification relies on the heterogeneity in exposure to an event (such as a war) across ethnicities and birth cohorts (see Akresh et al. 2012, 2021).

-Table 1-

We perform balancing tests to check the degree to which the identifying ethnicity-by-birth time variation in exposure to Diem's policies correlates with trends in observables. Using an aggregate dataset at the ethnicity-by-birth time level constructed from the 1999 and 2009 censuses, we, therefore, regress the likelihood of having an *in-utero full exposure* to Diem's policies on specific observables, conditioning on ethnicity, birth time, and province and survey year fixed effects. We focus on observable characteristics that are unlikely to be driven by *in-utero* exposure to Diem's policies as reported in Table 1. Columns 1-3 in Table 1 show that an ethnicity-by-birth time cell's probability of full exposure does not correlate to gender composition (% of male) and religious affiliations (% of Buddhism, other religious affiliations, and no religious affiliation) both separately (columns 1-2) and jointly (column 3, *p*-value for a joint significance is 0.479). We further uncover that regional composition (% of different key Vietnamese regions) is not statistically related to the ethnicity-by-birth time probability of full exposure both separately (column 4) and jointly with gender and religion composition (column 5, *p*-value for a joint significance is 0.755). These results allow us to establish that our identifying ethnicity-by-birth time variation in exposure to Diem's policies is unrelated to trends in observables.

4.2 Estimation methods

To estimate the effects of *in-utero* exposure to the hostile policies, we perform generalized DiD regressions that compare across ethnicities and birth groups, controlling for ethnicity, birth time, and province fixed effects, and letting birth time effects vary across ethnicities. The implementation of the regression is different according to whether we estimate effects on the generation directly affected by Diem's policies or the next generation. The two respective approaches are the following.

Effects on the first generation

For an outcome Y_{ietps} of individual i of the directly affected generation, belonging to ethnicity e, being born to birth time t, living in province p, and being surveyed in year s, we run the regression:

$$Y_{ietps} = \alpha_0 + \alpha_1 Chinese_{ie} \times PartialExposure_{it} + \alpha_2 Chinese_{ie} \times FullExposure_{it} + \gamma \boldsymbol{X'}_i + \varphi_e + \omega_t + \pi_p + \theta_s + \chi_t \times e + \varepsilon_{ietps} \quad (1).$$

$Chinese_{ie}$ is a dummy variable indicating whether ethnicity e of individual i belongs to the Chinese ethnicity. $PartialExposure_{it}$ is a dummy variable indicating if individual i belongs to the birth time group of being partially *in-utero* exposed to the hostile policies ($t = 0$). $FullExposure_{it}$ is a dummy variable indicating whether individual i belongs to the birth time groups of full exposure to the hostile policies during pregnancy ($t = 1, \dots, 7$). $\boldsymbol{X'}_i$ is a control vector for individual characteristics, such as dummies for gender (male) and religious affiliations (Buddhism, other religions and no religion). φ_e, ω_t, π_p and θ_s are the fixed effects for the ethnicity, birth time, province, and survey year. ε_{ietps} is the error term.

The ethnicity fixed effect φ_e controls for time-invariant ethnic characteristics such as culture, ways of life among other socioeconomic and demographic traits. The birth time fixed effect ω_t controls for birth group trends in the outcome of interest. The inclusion of the province fixed effect π_p allows us to control for potential differences in provincial characteristics. The survey year fixed effect θ_s controls for potential time trends across survey years, and it is dropped in specifications using a dataset consisting of only one survey wave. We include the interaction of birth time with ethnicity dummies $\chi_t \times e$, which allows trends across cohorts to vary in unrestricted ways across ethnicities. The joint inclusion of ethnicity and cohort fixed effects turns Equation (1) into a generalized difference-in-differences regression, in which α_2 is the effect of *in-utero* exposure to hostile policies on the outcome of interest.

Effects on the next generation

To explore how *in-utero* exposure to the hostile policies affects the next generation, in child j born to parent k who belongs to ethnicity e, was born in birth time t living in province p in survey s, we run regressions of the following type:

$$\begin{aligned} Y_{jketps} = \quad & \beta_0 + \beta_1 Chinese_e^k \times PartialExposure_t^k + \beta_2 Chinese_e^k \times FullExposure_t^k \\ & + \rho \boldsymbol{X'}_j + \varphi_e^k + \omega_t^k + \pi_p + \theta_s + \chi_t^k \times e^k + \epsilon_{jketps} \end{aligned} \quad (2).$$

Y_{jketps} is an outcome of child j. $\boldsymbol{X'}_j$ is a set of controls for the child characteristics such as age, age squared, and dummies for the child's gender (male) and religious affiliations (Buddhism, other religions and no religion). ϵ_{jketps} is the error term. Other terms are similarly defined as in Equation (1).

Causal interpretation and parallel assumption

Following Akresh et al. (2021), we cluster standard errors at the ethnicity-by-birth time level in both regression equations. Our parameters of interest are α_2 and β_2, which indicate the causal effects of *in-utero* exposure to the adverse social environment on the first and second generations, respectively. To interpret α_2 and β_2 as the causal effects, the parallel trends assumption is required to be held. In other words, the estimates of α_2 and β_2 of the effects on the outcomes of interest in the absence of Diem's policies should be close to zero. To check if this assumption is held, we perform an event-study analysis for the human capital outcomes of the first generation. The regression equation is now modified from Equation (1) in which we replace the interaction terms of $Chinese \times PartialExposure$ and $Chinese \times FullExposure$ with the interaction terms of $Chinese \times BirthTime$, where $BirthTime$ is a dummy for the birth time t ($t = -7, \dots, 7$). We omit the *partial exposure* birth time of $t = 0$ as the reference group. Other terms included in our even-study regressions are similar to those in Equation (1).

-Figure 2-

Figure 2 plots the point estimates of $Chinese \times BirthTime$ from the event study regression for the primary outcome of human capital: schooling years. This figure shows that the coefficients are close to zero for the birth time of no exposure but gradually reduce from the first birth time of full exposure to Diem's policies onwards ($t = 1, \dots, 7$). A gradual decrease in the estimates reflects the gradual increase in exposure to the adverse social environment among birth time groups with full exposure. The pattern in these graphs shows no evidence of pre-trends, confirming the validity of our identification strategy.

5. Results

In this section, we present empirical results for the effects on both the first and second generations in Tables 2-10. We will present the baseline estimates which are produced using regression Equation (1) for the first generation, or Equation (2) for the second generation. We further show gender heterogeneity of the effects which are produced using baseline regression equations with additional interaction effects by gender to break the overall effects up into the effect components for males and females. To check if

the baseline estimates are robust to different specifications and sub-samples, we perform two additional estimations by using another specification that excludes province fixed effects and using a sub-sample that excludes observations from Ho Chi Minh City. These results are present in Supplementary Appendix. Dropping province fixed effects from the baseline specification allows us to check if the effects change. By excluding observations from Ho Chi Minh City, which is the most developed city and comprises 50% of the Chinese community lives in South Vietnam, we want to see if the baseline effects are driven by the effects from Ho Chi Minh City.

5.1 Effects on human capital formation

Baseline estimates

We start with presenting the estimates on the effects of *in-utero* exposure to the hostile policies on the outcomes of schooling of the first generation in Table 2. The exposure has negative effects on all outcomes of schooling as shown in columns 1-4. These effects are strongly statistically significant at the 1% level. Column 1 indicates that the exposure reduces about 0.4 years of completed schooling. Compared to the mean of 6.1 schooling years for pre-treated birth time groups, this effect is equivalent to a reduction of nearly 7% in completed schooling years, suggesting a sizable effect. Columns 2-3 further show that the exposure decreases the probability of completing various educational levels, including 6.8 percentage points for primary school, and 5 percentage points for post-lower secondary school. These effects are also substantial as they create an equivalent reduction of 10-19% when comparing to the pre-treatment mean values. As shown in column 4, the largest adverse effect is on the probability of achieving a college degree with a 0.4-percentage point reduction, equivalent to a decrease of 67% when comparing to pre-treated birth time groups.

-Table 2-

We find the baseline estimates are highly robust to estimating another specification that excludes province fixed effects as shown in Supplement Appendix Table A.2. However, the effect size of the estimates using this alternative specification is larger than that of the baseline estimates for all outcomes. Table A.3. in Supplementary Appendix reports the estimates when running the baseline specification

but using a sub-sample that excludes observations from Ho Chi Minh City. We also find statistically significant and negative estimates for all outcomes. Yet, these estimates are relatively larger than the baseline estimates for schooling years, primary and post-primary school completion. The effect on achieving a college degree is in contrast smaller than the baseline estimate.

In general, we find negative considerable effects of *in-utero* exposure to the hostility on the human capital of the directly affected generation. These effects equal half of the effects of the 1991 compulsory schooling reform in Vietnam (although the effect direction is opposite), which raised 0.9 years of education and about 10 percentage points for primary school completion in Vietnam (Cornelissen and Dang 2021).

Gender heterogeneity

Table 3 reports the heterogeneity of the baseline estimates by gender for the effects on the human capital formation of the first generation. The point estimates in the first two rows of Table 3 show detrimental effects for both genders in all outcomes (strongly statistically significant at the 1% level, except for female's college with the 5% level). However, the negative effects on males are considerably larger than those on females. For schooling years (column 1), the effect for males is greater than that for females by a factor of 1.96. For the probability of completing educational levels, these differences consist of a factor of 1.26 for primary school (column 2), a factor of 2.65 for post-primary school (column 3), and a factor of 2 for a college degree (column 4).

-Table 3-

The third row of coefficients in Table 3 reports the differences in the effects between males and females. Accordingly, we do reject the hypothesis that the effects are equal between males and females for schooling years (statistically significant at the 5% level) and the probability of completing post-primary education (statistically significant at the 1% level). The effect gaps for primary school and college are not statistically significant. The fourth row of coefficients in Table 3 shows that the (unconditional) gaps for *no exposure* birth time groups are skewed in favour of males in all outcomes. Among individuals who were not exposed to the hostile policies during pregnancy, males have

considerably better outcomes than females, for example, 1.5 years of schooling and 16.6 percentage points of completing post-primary education. Comparing these two rows, we can recognize that exposure would have strongly reduced the gender differences against males, leading to significant reductions in the gender gaps, in particular 18% for schooling years and 26% for post-primary education. These findings are interesting because they reveal that

5.2 Effects on labour market and family outcomes

Baseline estimates

Table 4 presents the baseline effects on labour market and family outcomes. We use non-agricultural work as a proxy for an improved outcome of the labour market status because working in a non-agriculture section is highly likely to link with a highly skilled occupation of the non-traditional economy. In column 1, we find that the hostile exposure during pregnancy reduces the probability of having a non-agricultural job by 7.9 percentage points (statistically significant at the 1% level). Comparing with the pre-treatment mean (81%), this effect is equivalent to a reduction of 9.8%. This finding suggests that the individuals of *in-utero* being exposed to the hostility have a lower labour market status than those without such exposure.

-Table 4-

The remaining columns in Table 4 present the estimates for family outcomes. The estimate in column 2 indicates that the hostility exposure causes a reduction in the probability of being married by 5.7 percentage points (highly statistically significant at the 1% level). This effect is translated to a fall of 7.5% in being married compared to the pre-treatment mean.

Columns 3 and 4 show the effects on fertility for women. We find that hostility exposure during pregnancy has a positive effect on fertility at both extensive and intensive margins. In particular, the effect on the probability of having a child is 0.8 percentage points (statistically significant at the 1% level, column 3). This fertility effect at the extensive margin equals nearly an increase of 9% in having a child when comparing with the pre-treatment mean. At the intensive margin, the hostility exposure

raises the number of children by 0.3 (statistically significant at the 1% level, column 4), which is translated to a 12-% increase in the number of children.

Finally, column 5 shows the effect on household size. The individuals of exposure to the hostility during pregnancy live in a larger family by 0.49 persons than those without such exposure (statistically significant at the 1% level). This effect is corresponding to an increase of 8.7% compared to the pre-treated individuals. This finding provides suggestive evidence that the *in-utero* hostility exposure is likely to raise the likelihood of co-residing within an extended family.

Gender heterogeneity

Table 5 reports the gender heterogeneity of the effects on several labour market and family outcomes. The first two rows of the coefficients indicate that the effects are heterogeneous by gender for all outcomes. The effects are damaging on both genders as same as the overall baseline effects in Table 4. The estimates are all statistically significant at the 1% level.

-Table 5-

Overall, the point estimates for males are only slightly larger than those for females. The male-female ratio of the effect is only 1.05 for non-agricultural work and being married and 1.14 for household size. However, as shown in the third row of the estimates in Table 5, we cannot reject the hypothesis that the effects are equal between genders for these outcomes. Therefore, there is no evidence on gender differences in the effects of the hostility exposure on the labour market and family outcomes.

5.3 Effects on family living standards

Baseline estimates

Table 6 reports the results for the effects on several outcomes of family living standards. The estimates suggest that the individuals who were fully exposed to the hostility during pregnancy, in general, have poorer living conditions compared to those without such exposure. All the estimates are statistically significant at the 1% level. Column 1 shows that they are less likely to live in a solid house by 3.4 percentage points. This effect is equivalent to a 6-% reduction in high-quality housing compared to the pre-treatment mean.

-Table 6-

Column 2 shows that the households of those with the *in-utero* hostility exposure have a higher likelihood of using the old cooking energy by 5.5 percentage points (corresponding to an increase of 24.9%). Furthermore, the households of the fully exposed individuals also have a lower chance to use improved sanitation by 6.5 percentage points (equivalent to a decrease of 7.3%) than the counterpart households as shown in column 3. Finally, column 4 shows that the fully exposed individuals are also poorer than those who were not exposed to hostility in terms of household wealth. The effect on the asset index is a reduction of 0.23 assets (equivalent to a reduction of 4.9%).

Gender heterogeneity

Table 7 presents the results of the effect heterogeneity by gender. The two first rows of the coefficients show that the effects on both men and women are in the same direction with the baseline estimates (strongly statistically significant at the 1% level).

-Table 7-

Again, the effects are to some extent larger for males than those for females. The male-female ratios for solid house (column 1), old cooking energy (column 2), improved sanitation (column 3), and asset index (column 4) are respectively 1.3, 1.22, 1.06, and 1.36. However, the coefficients for gender differences are not statistically significant as shown in the third row, suggesting no evidence on gender differences in the effects on family living standards.

5.4 Effects on ethnic inequality across generations

In this sub-section, we address the question of whether prenatal hostility exposure exacerbated inequality of opportunity and then reduced intergenerational mobility. We, therefore, test whether the exposure would affect the correlation in educational attainment between the directly affected generation and their parents. Using both the 1999 and 2009 censuses, we estimate an augmented version of Equation (1) with years of schooling of an individual of the directly affected generation on the left-hand side. The right-hand side is extended by adding parental years of schooling ($ParentalSchooling$) and its interactions with $Chinese \times FullExposure$, $Chinese \times PartialExposure$, $Chinese$, $FullExposure$, and

$PartialExposure$. Other terms remain as same as in Equation (1). While the coefficient for $ParentalSchooling$ suggests the baseline intergenerational elasticity (IGE) of schooling, the coefficient for $Chinese \times FullExposure \times ParentalSchooling$ points to how the *in-utero* hostility exposure to the hostile policies affects IGE.

Table 8 presents the coefficients on the intergenerational persistence of schooling. We estimate for the overall samples of child-father pairs (panel A) and mother pairs (panel B), and the sub-samples broken up by child genders within each panel.[11] The first row of the coefficients in each panel reports the baseline IGE estimates. Generally, the baseline IGE estimates are strongly statistically significant and around 0.4 years of schooling for both child-father and child-mother pairs. These IGE coefficients are in line in terms of both the direction and the IGE size with other studies for Vietnam (for example Cornelissen and Dang 2021).

-Table 8-

The second row of the coefficients in each panel of Table 8 shows the interaction effects. The estimates, in general, show that the *in-utero* exposure to the hostility enlarges the dependence of schooling of the directly affected generation on their parents' schooling. For the overall samples, the exacerbating effects consist of about 0.16 for child-father pairs (statistically significant at the 1% level, column 1, panel A) and 0.15 for child-mother pairs (statistically significant at the 1% level, column 1, panel B). The estimates using sub-samples by child genders shows that daughters have a stronger dependence on their parents' schooling than sons. These effects are 0.25 for daughter-father pairs (statistically significant at the 5% level, column 3, panel A) and 0.2 for daughter-mother pairs (weakly statistically significant at the 10% level, column 3, panel B). For sons, we only find a statistically significant worsening effect of *in-utero* exposure to the hostility for son-mother pairs (weakly statistically significant at the 10% level, column 2, panel B).

Despite evidence on the damaging effect of prenatal exposure to hostility on schooling years is stronger for males than that for females as demonstrated in Table 3, the effects of the hostility exposure

[11] It is important to bear in mind that child/son/daughter mentioned in this sub-section belongs to the first generation. Father and mother are the parents of the first generation.

on the intergenerational persistence of education are still larger for females than males. This is likely because schooling of Chinese-ethnic girls was traditionally low compared to that of boys because of strong son reference culture. The male-female difference in the negative effect on schooling years in favour of males is not large enough to upset the traditional gap in the intergenerational schooling persistence between males and females, making larger adverse effects on IGE for females than that for males.

In summary, these results demonstrate that *in-utero* exposure to the hostile policies remarkably raises the intergenerational schooling persistence. This finding provides suggestive evidence on a negative impact of being targeted during pregnancy on the educational mobility across generations.

5.5 Effects on the next generation's human capital

Baseline estimates

The final discussion of the results is the effects of *in-utero* exposure to hostility on the human capital of the second generation. We use the 1999 and 2009 censuses to construct a sample of children aged between 11 and 17 of the individuals who were born between May 1951 and July 1962. We focus on school enrollment and literacy skills as two main proxies for the human capital of post-primary school-aged children. While school enrollment is a proxy measure for the quantity of schooling among school-aged children, literacy skills can be inferred as the quality of schooling because children aged 11 onwards who typically complete primary education in Vietnam are expected to have full abilities to read and write. The baseline estimates produced separately for paternal and maternal exposure using Equation (2) are present in Table 9.[12]

-Table 9-

Overall, we find that *in-utero* exposure to hostility hurts children's human capital. Columns 1 and 2 show paternal exposure reduces children's school enrollment and literacy skills by 1 percentage point and 4.9 percentage points (both statistically significant at the 5% level). These effects are equivalent to

[12] Fathers and mothers in this sub-section are those belonging to the first generation.

a reduction of 1% and 5.5% in school enrollment and literacy skills, respectively. Maternal exposure has slightly larger negative effects on these outcomes than paternal exposure. As shown in columns 3 and 4, the effects of maternal exposure include 1.3 percentage points for children's school enrollment (statistically significant at the 1% level) and 5.8 percentage points for children's literacy skills (statistically significant at the 5% level). These effects can be translated to a reduction of 1.3% and 6.5% in offspring's school enrollment and literacy skills, respectively.

Child gender heterogeneity

The results of heterogeneity by child genders are present in Table 10. For paternal exposure, we only find a statistically significant effect on sons' school enrollment (1.3 percentage points, statistically significant at the 5% level, column 1) but not on that of females as shown in column 1. The difference in these adverse effects is statistically significant in favour of sons (0.7 percentage points, statistically significant at the 10% level, column 1). Paternal exposure has statistically significant and negative effects on literacy skills for both sons and daughters as shown in column 2. Yet, the son-daughter difference in the effects on literacy skills for paternal exposure is not statistically significant.

-Table 10-

In columns 3 and 4, we find that maternal exposure has a slightly larger effect on sons rather than daughters for both school enrollment and literacy skills. Maternal exposure to the hostile policies during pregnancy reduces school enrollment by 1.6 and 1.2 percentage points for males and females, respectively (both statistically significant at the 5% level). It also decreases literacy skills by 6.6 and 5.6 percentage points for males and females, respectively (both statistically significant at the 5% level). These point estimates, therefore, suggest that sons tend to be slightly worse than daughters when their parents were exposed to hostile policies during pregnancy. However, as the estimates in the third row are statistically insignificant, we can accept the hypothesis that the effects are similar between child genders.

6. Concluding remarks

Using large-scale census data from Vietnam, we have studied the long-run and intergenerational effects of *in-utero* exposure to hostile policies. We first discover that exposure to the hostile policies in the womb has damaging impacts on the human capital formation of the directly affected generation, measured by schooling years, primary and post-primary school completion, and a college degree. These results are in line with previous evidence showing that exposure to adverse shocks early in life likely hurts human capital (Akresh et al. 2012, 2021; Leon 2012; Karbownik and Wray 2019; Maccini and Yang 2009).

Given the importance of human capital as a key driver of long-run and intergenerational socio-economic success (Björklund and Salvanes 2011; Black and Devereux 2011; Black et al. 2005; Duflo 2001; Oreopoulos and Salvanes 2011), we treat these effects on human capital formation as our main channels linking *in-utero* exposure to hostile policies to other labour market, family and economic outcomes of the directly affected generation and the children's generation. We next find that through a reduction in human capital, *in-utero* exposure to hostile policies reduces economic activities outside the traditional economy and the likelihood of being married, increases fertility, and decreases household living standards of the directly affected generation. We further uncover negative impacts of prenatal exposure to hostile policies on the next generation's human capital measured by school enrollment and literacy skills.

Our paper shows that hostile policies that governments implement to target specific ethnic and racial minorities may be a driver of persistent ethnic and racial inequalities in socio-economic outcomes which have been observed in many countries. For that reason, considering potential consequences induced hostile policies is of importance in doing public policies in many countries, especially in those with a weak institution, for reducing ethnic and racial inequalities.

References

Akresh, R., Bhalotra, S., Leone, M., & Osili, U. O. (2012). War and stature: Growing up during the Nigerian civil war. American Economic Review, 102(3), 273-77.

Akresh, R., Bhalotra, S., Leone, M., & Osili, U. O. (2021). First and second generation impacts of the Biafran war. Journal of Human Resources, 0118-9272R1.

Alesina, A., Baqir, R., & Easterly, W. (1999). Public goods and ethnic divisions. The Quarterly Journal of economics, 114(4), 1243-1284.

Alesina, A., Michalopoulos, S., & Papaioannou, E. (2016). Ethnic inequality. Journal of Political Economy, 124(2), 428-488.

Alexander, D., & Currie, J. (2017). Is it who you are or where you live? Residential segregation and racial gaps in childhood asthma. Journal of Health Economics, 55, 186-200.

Almond, D. (2006). Is the 1918 influenza pandemic over? Long-term effects of in utero influenza exposure in the post-1940 US population. Journal of Political Economy, 114(4), 672-712.

Almond, D., & Currie, J. (2011). Killing me softly: The fetal origins hypothesis. Journal of economic perspectives, 25(3), 153-72.

Almond, D., Edlund, L., & Palme, M. (2009). Chernobyl's subclinical legacy: prenatal exposure to radioactive fallout and school outcomes in Sweden. The Quarterly journal of economics, 124(4), 1729-1772.

Alsan, M., & Wanamaker, M. (2018). Tuskegee and the health of black men. The Quarterly Journal of Economics, 133(1), 407-455.

Amer, R. (2011). Examining the demographic developments relating to the ethnic Chinese in Vietnam since 1954. In Migration, Indigenization and Interaction: Chinese Overseas and Globalization (pp. 171-229).

An, T. S. (1967). The Overseas Chinese in South Vietnam: A Note. Vietnam Perspectives, 2(4), 13-19.

Barker, D. J. (1990). The fetal and infant origins of adult disease. BMJ: British Medical Journal, 301(6761), 1111.

Barr, A., & Oduro, A. (2002). Ethnic fractionalization in an African labour market. Journal of Development Economics, 68(2), 355-379.

Barton, C. G. (1977). Credit and Commercial Control Strategies and Methods of Chinese Businessmen in South Vietnam? Unpublished Ph.D. Dissertation, Cornell University.

Bayer, P., & Charles, K. K. (2018). Divergent paths: A new perspective on earnings differences between black and white men since 1940. The Quarterly Journal of Economics, 133(3), 1459-1501.

Behrman, J. R., & Rosenzweig, M. R. (2004). Returns to birthweight. Review of Economics and statistics, 86(2), 586-601.

Bhalotra, S., & Rawlings, S. (2013). Gradients of the intergenerational transmission of health in developing countries. Review of Economics and Statistics, 95(02), 660-672.

Bharadwaj, P., Lundborg, P., & Rooth, D. O. (2018). Birth weight in the long run. Journal of Human Resources, 53(1), 189-231.

Björklund, A., & Salvanes, K. G. (2011). Education and family background: Mechanisms and policies. In Handbook of the Economics of Education (Vol. 3, pp. 201-247). Elsevier.

Black, S. E., & Devereux, P. J. (2011). Recent Developments in Intergenerational Mobility. Handbook of Labor Economics, 4, 1487-1541.

Black, S. E., Devereux, P. J., & Salvanes, K. G. (2005). Why the apple doesn't fall far: Understanding intergenerational transmission of human capital. American Economic Review, 95(1), 437-449.

Black, S. E., Devereux, P. J., & Salvanes, K. G. (2007). From the cradle to the labor market? The effect of birth weight on adult outcomes. The Quarterly Journal of Economics, 122(1), 409-439.

Brown, R. (2020). The intergenerational impact of terror: did the 9/11 tragedy impact the initial human capital of the next generation? Demography, 57(4), 1459-1481.

Bundervoet, T., Verwimp, P., & Akresh, R. (2009). Health and civil war in rural Burundi. Journal of human Resources, 44(2), 536-563.

Bütikofer, A., & Salvanes, K. G. (2020). Disease control and inequality reduction: Evidence from a tuberculosis testing and vaccination campaign. The Review of Economic Studies, 87(5), 2087-2125.

Bütikofer, A., Dalla-Zuanna, A., & Salvanes, K. G. (2018). Breaking the links: Natural resource booms and intergenerational mobility. NHH Dept. of Economics Discussion Paper, 19.

Cai, C. (2020). Tightening Immigration Policy: Unintended Consequences on Hispanic U.S. Citizens' Health Behavior and Status.

Camacho, A. (2008). Stress and birth weight: evidence from terrorist attacks. American Economic Review, 98(2), 511-15.

Card, D., & Krueger, A. B. (1992). School quality and black-white relative earnings: A direct assessment. The quarterly journal of Economics, 107(1), 151-200.

Carneiro, P., García, I. L., Salvanes, K. G., & Tominey, E. (2021). Intergenerational mobility and the timing of parental income. Journal of Political Economy, 129(3), 757-788.

Case, A., & Paxson, C. (2008). Stature and status: Height, ability, and labor market outcomes. Journal of political Economy, 116(3), 499-532.

Chan, Y. W. (2018). 'Vietnam is my country land, China is my hometown': Chinese communities in transition in the south of Vietnam. Asian Ethnicity, 19(2), 163-179.

Chetty, R., & Hendren, N. (2018a). The impacts of neighborhoods on intergenerational mobility I: Childhood exposure effects. The Quarterly Journal of Economics, 133(3), 1107-1162.

Chetty, R., & Hendren, N. (2018b). The impacts of neighborhoods on intergenerational mobility II: County-level estimates. The Quarterly Journal of Economics, 133(3), 1163-1228.

Chetty, R., Friedman, J. N., Saez, E., Turner, N., & Yagan, D. (2017). Mobility report cards: The role of colleges in intergenerational mobility (No. w23618). National Bureau of Economic Research.

Chetty, R., Hendren, N., Jones, M. R., & Porter, S. R. (2020). Race and economic opportunity in the United States: An intergenerational perspective. The Quarterly Journal of Economics, 135(2), 711-783.

Cornelissen, T., & Dang, T. (2021). The multigenerational impacts of educational expansion: Evidence from Vietnam. Working paper.

Conti, G., Hanson, M., Inskip, H., Crozier, S., Cooper, C., & Godfrey, K. M. (2020). Beyond birthweight: The origins of human capital.

Currie, J. (2011). Inequality at birth: Some causes and consequences. American Economic Review, 101(3), 1-22.

Currie, J., & Almond, D. (2011). Human capital development before age five. In Handbook of labor economics (Vol. 4, pp. 1315-1486). Elsevier.

Currie, J., & Vogl, T. (2013). Early-life health and adult circumstance in developing countries. Annu. Rev. Econ., 5(1), 1-36.

Darity, W., & Nembhard, J. G. (2000). Cross-national comparisons of racial and ethnic economic inequality. American Economic Review 90, Papers and Proceedings of the One Hundred Twelfth Annual Meeting of the American Economic Association, 308-11.

de Chaisemartin, Clément, and Xavier D'Haultfœuille. 2020. Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects. American Economic Review, 110 (9): 2964-96.

De Luca, G., Hodler, R., Raschky, P. A., & Valsecchi, M. (2018). Ethnic favoritism: An axiom of politics?. Journal of Development Economics, 132, 115-129.

Derenoncourt, E., & Montialoux, C. (2021). Minimum wages and racial inequality. The Quarterly Journal of Economics, 136(1), 169-228.

Duflo, E. (2001). Schooling and labor market consequences of school construction in Indonesia: Evidence from an unusual policy experiment. American economic review, 91(4), 795-813.

Duncan, B., Mansour, H., & Rees, D. I. (2017). It's just a game the super bowl and low birth weight. Journal of Human Resources, 52(4), 946-978.

Elder, T., & Zhou, Y. (2021). The Black-White Gap in Noncognitive Skills among Elementary School Children. American Economic Journal: Applied Economics, 13(1), 105-32.

Engelbert, T. (2008). Vietnamese-Chinese Relations in Southern Vietnam during the First Indochina Conflict. Journal of Vietnamese Studies, 3(3), 191-230.

Fafchamps, M. (2000). Ethnicity and credit in African manufacturing. Journal of Development economics, 61(1), 205-235.

Feng, M. M. (2017). Chinese Newspapers in Chợ Lớn, 1930–1975. SOJOURN: Journal of Social Issues in Southeast Asia, 32(3), 766-782.

Fitzgerald, S. (1972). China and the Overseas Chinese (pp. 6-9). Cambridge.

Fredman, Z. (2014). "The specter of an expansionist China": Kennedy administration assessments of Chinese intentions in Vietnam. Diplomatic History, 38(1), 111-136.

Freeman, R. B. (1973). Decline of labor market discrimination and economic analysis. The American Economic Review, 63(2), 280-286.

Freeman, R. B. (1973). Decline of labor market discrimination and economic analysis. The American Economic Review, 63(2), 280-286.

Fryer Jr, R. G., & Levitt, S. D. (2013). Testing for racial differences in the mental ability of young children. American Economic Review, 103(2), 981-1005.

Goldsmith, A. H., Hamilton, D., & Darity Jr, W. (2006). Shades of discrimination: Skin tone and wages. American Economic Review, 96(2), 242-245.

Goodman-Bacon, Andrew, "Difference-in-Differences with Variation in Treatment Timing," 2018. NBER Working Paper No. 25018.

Han, X. (2009). Spoiled guests or dedicated patriots? The Chinese in North Vietnam, 1954-1978. International Journal of Asian Studies, 6(1), 1.

Karbownik, K., & Wray, A. (2019). Long-run consequences of exposure to natural disasters. Journal of Labor Economics, 37(3), 949-1007.

Kırdar, M. G. (2009). Explaining ethnic disparities in school enrollment in Turkey. Economic Development and Cultural Change, 57(2), 297-333.

Ky, L. N. (1963). The Chinese in Vietnam: a Study of Vietnamese-Chinese Relations with Special Attention to the Period 1862-1961 (Doctoral dissertation).

Leon, G. (2012). Civil conflict and human capital accumulation the long-term effects of political violence in perú. Journal of Human Resources, 47(4), 991-1022.

Maccini, S., & Yang, D. (2009). Under the weather: Health, schooling, and economic consequences of early-life rainfall. American Economic Review, 99(3), 1006-26.

Marcus, M. (2021). Going beneath the surface: Petroleum pollution, regulation, and health. American Economic Journal: Applied Economics, 13(1), 1-37.

Marsot, A. G. (1993). The Chinese community in Vietnam under the French. Edwin Mellen Press.

Mitchison, L. (1961). The Overseas Chinese. Bodley Head.

Oreopoulos, P., & Salvanes, K. G. (2011). Priceless: The nonpecuniary benefits of schooling. Journal of Economic perspectives, 25(1), 159-84.

Panza, L. (2020). The impact of ethnic segregation on schooling outcomes in Mandate Palestine. Journal of Development Economics, 146, 102514.

Persson, P., & Rossin-Slater, M. (2018). Family ruptures, stress, and the mental health of the next generation. American Economic Review, 108(4-5), 1214-52.

Phadera, L. (2021). Unfortunate moms and unfortunate children: Impact of the Nepali civil war on women's stature and intergenerational health. Journal of Health Economics, 76, 102410.

Quintana-Domeque, C., & Ródenas-Serrano, P. (2017). The hidden costs of terrorism: The effects on health at birth. Journal of Health Economics, 56, 47-60.

Schrock, J. L. (1966). Minority groups in the Republic of Vietnam (Vol. 550, No. 105). Headquarters, Department of the Army.

Schrock, J. L. (1966). Minority groups in the Republic of Vietnam (Vol. 550, No. 105). Headquarters, Department of the Army.

Schwandt, H. (2018). The lasting legacy of seasonal influenza: In-utero exposure and labor market outcomes.

Share, M. (1994). The Chinese community in South Vietnam during the second Indochina War. Journal of Third World Studies, 11(2), 240-265.

Singhal, S. (2019). Early life shocks and mental health: The long-term effect of war in Vietnam. Journal of Development Economics, 141, 102244.

Smith, J. P. (1995). Racial and ethnic differences in wealth in the Health and Retirement Study. Journal of Human resources, S158-S183.

Stern, L. M. (1985). The Overseas Chinese in Vietnam, 1920-75: Demography, Social Structure, and Economic Power. Humboldt Journal of Social Relations, 1-30.

Suryadinata, L. (2017). The rise of China and the Chinese overseas: A study of Beijing's changing policy in Southeast Asia and beyond. ISEAS-Yusof Ishak Institute.

Taylor, K. W. (2013). A History of the Vietnamese. Cambridge University Press.

Tran, K. (1993). The ethnic Chinese and economic development in Vietnam. Institute of Southeast Asian.

Ungar, E. S. (1987). The struggle over the Chinese community in Vietnam, 1946-1986. Pacific Affairs, 596-614.

Van de Walle, D., & Gunewardena, D. (2001). Sources of ethnic inequality in Viet Nam. Journal of Development Economics, 65(1), 177-207.

von Hinke, S., Rice, N., & Tominey, E. (2019). Mental Health around Pregnancy and Child Development from Early Childhood to Adolescence.

Williams, D. R., Lawrence, J. A., & Davis, B. A. (2019). Racism and health: evidence and needed research. Annual Review of Public Health, 40, 105-125.

Figure 1. Timeline of and *in-utero* exposure to hostile policies

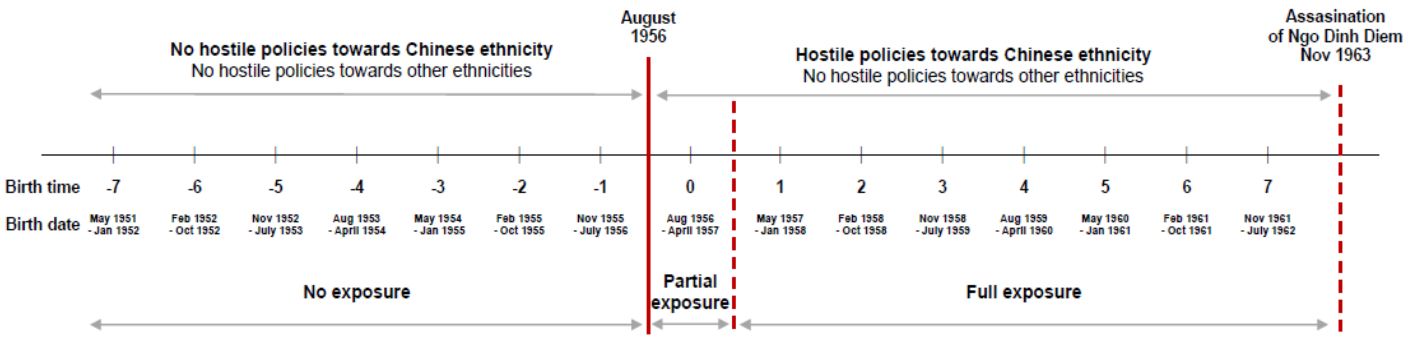
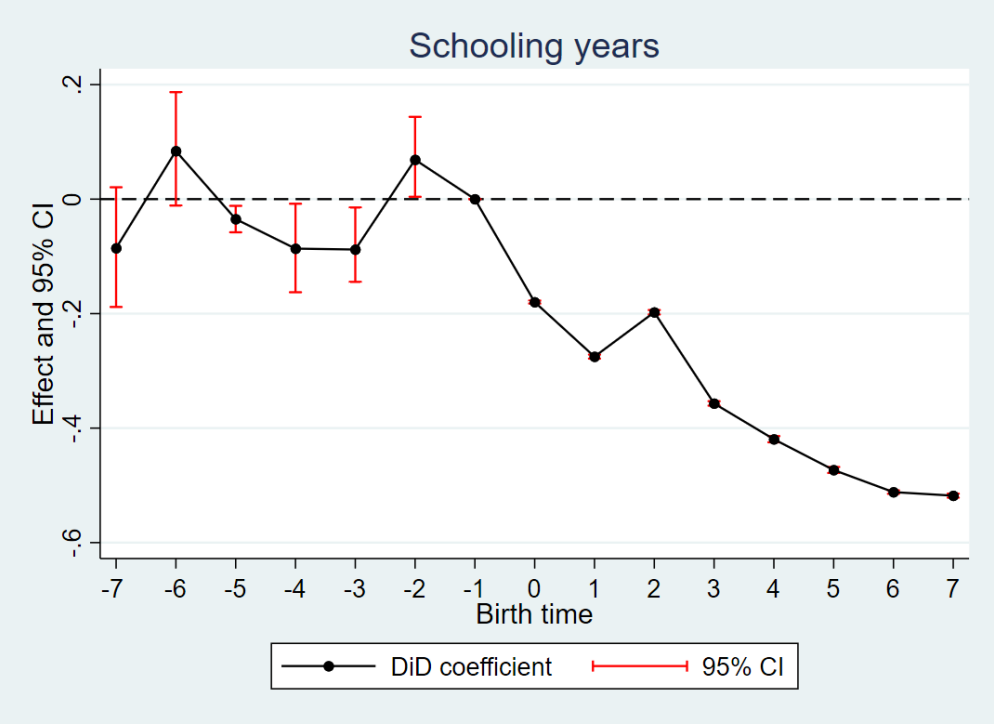


Figure 2. Event-study graphs



Notes: This figure shows the results of running an event-study. Pre-treatment effects are close to zero and statistically equal to zero. Post-treatment effects are negative and statistically different from zero.

TABLES

Table 1. Balance tests

Dep. var: Full exposure to the hostile policies (dummy)	(1)	(2)	(3)	(4)	(5)
Male (percent)	0.002		0.002		0.002
	(0.001)		(0.001)		(0.001)
Buddhism affiliation (percent)		-0.003	-0.003		-0.003
		(0.004)	(0.004)		(0.004)
Other religious affiliations (percent)		-0.001	-0.001		-0.001
		(0.001)	(0.001)		(0.001)
No religious affiliation (percent)		Reference	Reference		Reference
North and South Central Coast (percent)				-0.026	-0.025
				(0.026)	(0.026)
Central Highlands (percent)				-0.026	-0.024
				(0.026)	(0.026)
Southeast (percent)				-0.026	-0.025
				(0.026)	(0.026)
Mekong River Delta (percent)				Reference	Reference
Constant	0.034	0.248**	0.086	2.759	2.526
	(0.180)	(0.117)	(0.186)	(2.618)	(2.630)
p -value for joint significance of covariates			0.479		0.755
Dummies for birth time groups	Yes	Yes	Yes	Yes	Yes
Dummies for ethnicities	Yes	Yes	Yes	Yes	Yes
Dummies for provinces	Yes	Yes	Yes	Yes	Yes
Dummies for survey year	Yes	Yes	Yes	Yes	Yes
Observations	690	690	690	690	690

Notes: Observations are at the birth time-by-ethnicity level. Data is aggregated for individuals born in May 1951-July 1962. Standard errors are in parentheses.

* $p<0.1$, ** $p<0.05$, *** $p<0.01$

Data sources: All estimates are produced using Census (1999, 2009)

Table 2. Effects on schooling: Baseline estimates

	(1)	(2)	(3)	(4)
	Schooling years	Primary school	Post-primary school	College
Chinese x Full exposure	-0.432***	-0.068***	-0.050***	-0.004***
	(0.003)	(0.000)	(0.000)	(0.000)
Pre-treatment mean	6.076	0.668	0.259	0.006
% change	-7.110	-10.180	-19.305	-66.667
Observations	1,989,296	1,989,296	1,989,296	1,989,296

Notes: All specifications include fixed effects for birth time, ethnicity, province, birth time-by-ethnicity, and survey year. Further control variables included into these specifications consist of dummies for gender (male), rural area, and religious affiliations (Buddhism, others and no religious affiliation). The samples include individuals born in May 1951-July 1962. Robust standard errors clustered at the birth time-by-ethnicity level (345 clusters) are in parentheses.

* p <0.1, ** p <0.05, *** p <0.01

Data sources: All estimates are produced using Census (1999, 2009)

Table 3. Effects on schooling: Gender heterogeneity

	(1)	(2)	(3)	(4)
	Schooling years	Primary school	Post-primary school	College
Chinese x Full exposure x Female	-0.278***	-0.058***	-0.026***	-0.003**
	(0.065)	(0.007)	(0.007)	(0.001)
Chinese x Full exposure x Male	-0.546***	-0.073***	-0.069***	-0.006***
	(0.064)	(0.007)	(0.007)	(0.001)
Effect difference in full exposure	-0.268**	-0.014	-0.043***	-0.003
(Male - Female)	(0.130)	(0.0138)	(0.014)	(0.002)
Unconditional difference in no exposure	1.485***	0.185***	0.166***	0.010***
(Male - Female)	(0.008)	(0.001)	(0.001)	(0.000)
Observations	1,989,296	1,989,296	1,989,296	1,989,296

Notes: All specifications include fixed effects for birth time, ethnicity, province, birth time-by-ethnicity, and survey year. Further control variables included into these specifications consist of dummies rural area and religious affiliations (Buddhism, others and no religious affiliation). The samples include individuals born in May 1951-July 1962. Robust standard errors clustered at the birth time-by-ethnicity level (345 clusters) are in parentheses.

* p <0.1, ** p <0.05, *** p <0.01

Data sources: All estimates are produced using Census (1999, 2009)

Table 4. Effects on labor maket and family outcomes: Baseline estimates

	(1)	(2)	(3)	(4)	(5)
	Non-agricultural work	Married	Women's having a child	Women's fertility	Household size
Chinese x Full exposure	-0.079***	-0.057***	0.008***	0.304***	0.485***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
Pre-treatment mean	0.807	0.765	0.893	2.504	5.574
% change	-9.789	-7.451	0.896	12.141	8.701
Observations	1,325,405	2,414,083	875,741	875,741	1,652,224

Notes: All specifications include fixed effects for birth time, ethnicity, province, and birth time-by-ethnicity. Further control variables included into these specifications consist of dummies for gender (male), rural area, and religious affiliations (Buddhism, others and no religious affiliation). The samples include individuals born in May 1951-July 1962. Robust standard errors clustered at the birth time-by-ethnicity level (345 clusters) are in parentheses.

* p <0.1, ** p <0.05, *** p <0.01

Data sources: All estimates are produced using Census (1999) for columns 1, 3 and 4, and Census (1999, 2009) for columns 2.

Table 5. Effects on labor maket and family outcomes: Gender heterogeneity

	(1)	(2)	(3)	(4)	(5)
	Non-agricultural work	Married	Women's having a child	Women's fertility	Household size
Chinese x Full exposure x Female	-0.077***	-0.057***	-	-	0.456***
	(0.003)	(0.007)	-	-	(0.027)
Chinese x Full exposure x Male	-0.081***	-0.060***	-	-	0.519***
	(0.002)	(0.008)	-	-	(0.031)
Effect difference in full exposure	-0.004	-0.003	-	-	0.063
(Male - Female)	(0.006)	(0.015)	-	-	(0.058)
Unconditional difference in no exposure	-0.010***	0.183***	-	-	0.117***
(Male - Female)	(0.001)	(0.001)	-	-	(0.006)
Observations	1,325,405	2,414,083	875,741	875,741	1,652,224

Notes: All specifications include fixed effects for birth time, ethnicity, province, and birth time-by-ethnicity. Further control variables included into these specifications consist of dummies for rural area and religious affiliations (Buddhism, others and no religious affiliation). The samples include individuals born in May 1951-July 1962. Robust standard errors clustered at the birth time-by-ethnicity level (345 clusters) are in parentheses.

* $p<0.1$, ** $p<0.05$, *** $p<0.01$

Data sources: All estimates are produced using Census (1999)

Table 6. Effects on family living standards: Baseline estimates

	(1)	(2)	(3)	(4)
	Solid house	Old cooking energy	Improved sanitation	Asset index
Chinese x Full exposure	-0.034***	0.055***	-0.065***	-0.225***
	(0.000)	(0.000)	(0.000)	(0.001)
Pre-treatment mean	0.551	0.221	0.895	4.607
% change	-6.171	24.887	-7.263	-4.884
Observations	762,139	762,352	762,352	762,352

Notes: All specifications include fixed effects for birth time, ethnicity, province, and birth time-by-ethnicity. Further control variables included into these specifications consist of dummies for gender (male), rural area, and religious affiliations (Buddhism, others and no religious affiliation). The samples include individuals born in May 1951-July 1962. Robust standard errors clustered at the birth time-by-ethnicity level (345 clusters) are in parentheses.

* p <0.1, ** p <0.05, *** p <0.01

Data sources: All estimates are produced using Census (2009)

Table 7. Effects on family living standards: Gender heterogeneity

	(1)	(2)	(3)	(4)
	Solid house	Old cooking energy	Improved sanitation	Asset index
Chinese x Full exposure x Female	-0.030***	0.051***	-0.064***	-0.183***
	(0.005)	(0.005)	(0.004)	(0.023)
Chinese x Full exposure x Male	-0.039***	0.062***	-0.068***	-0.249***
	(0.006)	(0.006)	(0.005)	(0.024)
Effect difference in full exposure	-0.008	0.010	-0.004	-0.066
(Male - Female)	(0.012)	(0.011)	(0.009)	(0.046)
Unconditional difference in no exposure	-0.0002	-0.012***	0.002	0.165***
(Male - Female)	(0.002)	(0.002)	(0.002)	(0.007)
Observations	762,139	762,352	762,352	762,352

Notes: All specifications include fixed effects for birth time, ethnicity, province, and birth time-by-ethnicity. Further control variables included into these specifications consist of dummies for rural area and religious affiliations (Buddhism, others and no religious affiliation). The samples include individuals born in May 1951-July 1962. Robust standard errors clustered at the birth time-by-ethnicity level (345 clusters) are in parentheses.

* $p<0.1$, ** $p<0.05$, *** $p<0.01$

Data sources: All estimates are produced using Census (2009)

Table 8. Effects on intergenerational schooling persistence: Baseline estimates

	(1)	(2)	(3)
	Panel A. Child-father pairs		
	Child-father	Son-father	Daughter-father
Parental schooling	0.417***	0.374***	0.456***
	(0.011)	(0.013)	(0.011)
Chinese x Full exposure x Parental schooling	0.162***	0.085	0.249**
	(0.053)	(0.068)	(0.101)
Observations	62,529	31,352	31,177
	Panel B. Child-mother pairs		
	Child-mother	Son-mother	Daughter-mother
Parental schooling	0.451***	0.409***	0.493***
	(0.014)	(0.016)	(0.012)
Chinese x Full exposure x Parental schooling	0.152***	0.129*	0.197*
	(0.058)	(0.074)	(0.109)
Observations	108,918	57,929	50,989

Notes: All specifications include fixed effects for birth time, ethnicity, province, birth time-by-ethnicity, and survey year. Further control variables included into these specifications consist of dummies for gender (male), rural area, and religious affiliations (Buddhism, others and no religious affiliation). The samples include individuals born in May 1951-July 1962. Robust standard errors clustered at the birth time-by-ethnicity level (345 clusters) are in parentheses.

* $p<0.1$, ** $p<0.05$, *** $p<0.01$

Data sources: All estimates are produced using Census (1999, 2009)

Table 9. Effects on the next generation's human capital: Baseline estimates

	(1)	(2)	(3)	(4)
	Fathers' exposure		Mothers' exposure	
	School enrollment	Literacy skills	School enrollment	Literacy skills
Chinese x Full exposure	-0.010**	-0.049**	-0.013***	-0.058**
	(0.004)	(0.021)	(0.005)	(0.024)
Pre-treatment mean	0.977	0.897	0.976	0.888
% change	-1.024	-5.463	-1.332	-6.532
Observations	1,121,277	408,704	1,212,893	409,901

Notes: All specifications include fixed effects for birth time, ethnicity, province, birth time-by-ethnicity, and survey year. Further control variables included into these specifications consist of child's age and age squared, dummies for child gender (son), rural area, and child's religious affiliations (Buddhism, others and no religious affiliation). The samples include children aged 11-17 of parents born in May 1951-July 1962. Robust standard errors clustered at the birth time-by-ethnicity level (345 clusters) are in parentheses.

* p <0.1, ** p <0.05, *** p <0.01

Data sources: All estimates are produced using Census (1999, 2009)

Table 10. Effects on the next generation's human capital: Gender heterogeneity

	(1)	(2)	(3)	(4)
	Fathers' exposure		Mothers' exposure	
	School enrollment	Literacy skills	School enrollment	Literacy skills
Chinese x Full exposure x Female	-0.006	-0.047**	-0.012**	-0.056**
	(0.004)	(0.021)	(0.005)	(0.028)
Chinese x Full exposure x Male	-0.013**	-0.040*	-0.016**	-0.066***
	(0.005)	(0.024)	(0.006)	(0.026)
Effect difference in full exposure	-0.007*	0.007	-0.004	-0.010
(Male - Female)	(0.004)	(0.015)	(0.005)	(0.017)
Unconditional difference in no exposure	-0.004***	-0.016***	-0.004***	-0.018***
(Male - Female)	(0.001)	(0.002)	(0.001)	(0.002)
Observations	1,121,277	408,704	1,212,893	409,901

Notes: All specifications include fixed effects for birth time, ethnicity, province, birth time-by-ethnicity, and survey year. Further control variables included into these specifications consist of child's age and age squared, dummies for rural area and child's religious affiliations (Buddhism, others and no religious affiliation). The samples include children aged 11-17 of parents born in May 1951-July 1962. Robust standard errors clustered at the birth time-by-ethnicity level (345 clusters) are in parentheses.

* $p<0.1$, ** $p<0.05$, *** $p<0.01$

Data sources: All estimates are produced using Census (1999, 2009)

SUPPLEMENTARY APPENDIX (NOT FOR PUBLICATION)

Table A.1. Summary statistics of key outcomes

Outcome	Data source	All			Chinese			Non-Chinese		
		Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.
Panel A. First generation (Individuals born in May 1951-July 1962)										
Schooling years (completed years)	Census (1999, 2009)	1,989,296	6.587	3.466	44,075	6.244	3.191	1,945,221	6.595	3.472
Primary school (dummy)	Census (1999, 2009)	1,989,296	0.678	0.467	44,075	0.689	0.463	1,945,221	0.678	0.467
Post-primary school (dummy)	Census (1999, 2009)	1,989,296	0.328	0.470	44,075	0.279	0.449	1,945,221	0.329	0.470
College (dummy)	Census (1999, 2009)	1,989,296	0.018	0.133	44,075	0.006	0.077	1,945,221	0.018	0.134
Non-agricultural work (dummy)	Census (1999)	1,325,405	0.418	0.493	27,684	0.802	0.399	1,297,721	0.409	0.492
Married (dummy)	Census (1999, 2009)	2,414,083	0.866	0.340	52,434	0.764	0.425	2,361,649	0.869	0.338
Women's having a child (dummy)	Census (1999)	875,741	0.917	0.276	21,188	0.904	0.295	854,553	0.917	0.276
Women's fertility (number of children)	Census (1999)	875,741	2.394	1.377	21,188	2.589	1.773	854,553	2.389	1.365
Household size (persons)	Census (1999)	1,652,224	5.367	2.339	39,580	5.675	2.965	1,612,644	5.360	2.321
Solid house (dummy)	Census (2009)	762,139	0.370	0.483	12,894	0.519	0.500	749,245	0.367	0.482
Old cooking energy (dummy)	Census (2009)	762,352	0.555	0.497	12,898	0.231	0.421	749,454	0.561	0.496
Improved sanitation (dummy)	Census (2009)	762,352	0.582	0.493	12,898	0.893	0.309	749,454	0.577	0.494
Asset index (number of assets)	Census (2009)	762,352	3.243	1.858	12,898	4.539	1.985	749,454	3.221	1.848
Panel B. Next generation (Children aged 11-17 of parents born in May 1951-July 1962)										
Children of fathers										
School enrollment (dummy)	Census (1999, 2009)	1,121,277	0.964	0.186	16,081	0.978	0.147	1,105,196	0.964	0.187
Literacy skills (dummy)	Census (1999, 2009)	408,704	0.884	0.320	5,282	0.911	0.284	403,422	0.884	0.320
Children of mothers										
School enrollment (dummy)	Census (1999, 2009)	1,212,893	0.960	0.195	17,116	0.975	0.155	1,195,777	0.960	0.196
Literacy skills (dummy)	Census (1999, 2009)	409,901	0.863	0.344	5,183	0.894	0.308	404,718	0.863	0.344

Table A.2. Effects on schooling: Robustness (excluding province fixed effects)

	(1)	(2)	(3)	(4)
	Schooling years	Primary school	Post-primary school	College
Chinese x Full exposure	-0.535***	-0.071***	-0.061***	-0.006***
	(0.002)	(0.000)	(0.000)	(0.000)
Pre-treatment mean	6.076	0.668	0.259	0.006
% change	-8.805	-10.629	-23.552	-100.000
Observations	1,989,296	1,989,296	1,989,296	1,989,296

Notes: All specifications include fixed effects for birth time, ethnicity, birth time-by-ethnicity, and survey year. Further control variables included into these specifications consist of dummies for gender (male), rural area, and religious affiliations (Buddhism, others and no religious affiliation). The samples include individuals born in May 1951-July 1962. Robust standard errors clustered at the birth time-by-ethnicity level (345 clusters) are in parentheses.

* $p<0.1$, ** $p<0.05$, *** $p<0.01$

Data sources: All estimates are produced using Census (1999, 2009)

Table A.3. Effects on schooling: Robustness (excluding Ho Chi Minh City)

	(1)	(2)	(3)	(4)
	Schooling years	Primary school	Post-primary school	College
Chinese x Full exposure	-0.708***	-0.108***	-0.077***	-0.002***
	(0.004)	(0.001)	(0.000)	(0.000)
Pre-treatment mean	6.213	0.678	0.280	0.007
% change	-11.395	-15.929	-27.500	-28.571
Mean of dep. variable	6.305	0.650	0.295	0.016
Observations	1,711,231	1,711,231	1,711,231	1,711,231

Notes: Observations from Ho Chi Minh City are excluded from the samples. All specifications include fixed effects for birth time, ethnicity, province, birth time-by-ethnicity, and survey year. Further control variables included into these specifications consist of dummies for gender (male), rural area, and religious affiliations (Buddhism, others and no religious affiliation). The samples include individuals born in May 1951-July 1962. Robust standard errors clustered at the birth time-by-ethnicity level (345 clusters) are in parentheses.

* p <0.1, ** p <0.05, *** p <0.01

Data sources: All estimates are produced using Census (1999, 2009)

Table A.4. Effects on labor maket and family outcomes: Robustness (excluding province fixed effects)

	(1)	(2)	(3)	(4)	(5)
	Non-agricultural work	Married	Women's having a child	Women's fertility	Household size
Chinese x Full exposure	-0.063***	-0.049***	0.003***	0.298***	0.510***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.002)
Pre-treatment mean	0.807	0.765	0.893	2.504	5.574
% change	-7.807	-6.405	0.336	11.901	9.150
Observations	1,325,405	2,414,083	875,741	875,741	1,652,224

Notes: All specifications include fixed effects for birth time, ethnicity, and birth time-by-ethnicity. Further control variables included into these specifications consist of dummies for gender (male), rural area, and religious affiliations (Buddhism, others and no religious affiliation). The samples include individuals born in May 1951-July 1962. Robust standard errors clustered at the birth time-by-ethnicity level (345 clusters) are in parentheses.

* p <0.1, ** p <0.05, *** p <0.01

Data sources: All estimates are produced using Census (1999)

Table A.5. Effects on labor maket and family outcomes: Robustness (excluding Ho Chi Minh City)

	(1)	(2)	(3)	(4)	(5)
	Non-agricultural work	Married	Women's having a child	Women's fertility	Household size
Chinese x Full exposure	-0.091***	-0.052***	0.026***	0.280***	0.286***
	(0.000)	(0.000)	(0.000)	(0.001)	(0.002)
Pre-treatment mean	0.580	0.803	0.916	2.545	5.682
% change	-15.690	-6.476	2.838	11.002	5.033
Mean of dep. variable	0.335	0.875	0.921	2.381	5.353
Observations	1,140,536	2,086,176	736,999	736,999	1,391,449

Notes: Observations from Ho Chi Minh City are excluded from the samples. All specifications include fixed effects for birth time, ethnicity, province, and birth time-by-ethnicity. Further control variables included into these specifications consist of dummies for gender (male), rural area, and religious affiliations (Buddhism, others and no religious affiliation). The samples include individuals born in May 1951-July 1962. Robust standard errors clustered at the birth time-by-ethnicity level (345 clusters) are in parentheses.

* $p<0.1$, ** $p<0.05$, *** $p<0.01$

Data sources: All estimates are produced using Census (1999)

Table A.6. Effects on family living standards: Robustness (excluding province fixed effects)

	(1)	(2)	(3)	(4)
	Solid house	Old cooking energy	Improved sanitation	Asset index
Chinese x Full exposure	-0.067***	0.070***	-0.039***	-0.121***
	(0.000)	(0.000)	(0.000)	(0.001)
Pre-treatment mean	0.551	0.221	0.895	4.607
% change	-12.16	31.674	-4.358	-2.626
Observations	762,139	762,352	762,352	762,352

Notes: All specifications include fixed effects for birth time, ethnicity, and birth time-by-ethnicity. Further control variables included into these specifications consist of dummies for gender (male), rural area, and religious affiliations (Buddhism, others and no religious affiliation). The samples include individuals born in May 1951-July 1962. Robust standard errors clustered at the birth time-by-ethnicity level (345 clusters) are in parentheses.

* $p<0.1$, ** $p<0.05$, *** $p<0.01$

Data sources: All estimates are produced using Census (2009)

Table A.7. Effects on family living standards: Robustness (excluding Ho Chi Minh City)

	(1)	(2)	(3)	(4)
	Solid house	Old cooking energy	Improved sanitation	Asset index
Chinese x Full exposure	-0.081***	0.112***	-0.117***	-0.365***
	(0.001)	(0.001)	(0.001)	(0.002)
Pre-treatment mean	0.541	0.351	0.800	3.997
% change	-14.972	31.909	-14.625	-9.132
Mean of dep. variable	0.356	0.601	0.543	3.046
Observations	694,710	694,896	694,896	694,896

Notes: Observations from Ho Chi Minh City are excluded from the samples. All specifications include fixed effects for birth time, ethnicity, province, and birth time-by-ethnicity. Further control variables included into these specifications consist of dummies for gender (male), rural area, and religious affiliations (Buddhism, others and no religious affiliation). The samples include individuals born in May 1951-July 1962. Robust standard errors clustered at the birth time-by-ethnicity level (345 clusters) are in parentheses.

* p <0.1, ** p <0.05, *** p <0.01

Data sources: All estimates are produced using Census (2009)

Table A.8. Effects on intergenerational schooling persistence: Robustness (excluding province fixed effects)

	(1)	(2)	(3)
	Panel A. Child-father pairs		
	Child-father	Son-father	Daughter-father
Parental schooling	0.436***	0.393***	0.475***
	(0.012)	(0.015)	(0.011)
Chinese x Full exposure x Parental schooling	0.147***	0.066	0.235**
	(0.052)	(0.063)	(0.099)
Observations	62,529	31,352	31,177
	Panel B. Child-mother pairs		
	Child-mother	Son-mother	Daughter-mother
Parental schooling	0.475***	0.427***	0.525***
	(0.013)	(0.015)	(0.011)
Chinese x Full exposure x Parental schooling	0.132**	0.108	0.168
	(0.057)	(0.066)	(0.110)
Observations	108,918	57,929	50,989

Notes: All specifications include fixed effects for birth time, ethnicity, birth time-by-ethnicity, and survey year. Further control variables included into these specifications consist of dummies for gender (male), rural area, and religious affiliations (Buddhism, others and no religious affiliation). The samples include individuals born in May 1951-July 1962. Robust standard errors clustered at the birth time-by-ethnicity level (345 clusters) are in parentheses.

* $p<0.1$, ** $p<0.05$, *** $p<0.01$

Data sources: All estimates are produced using Census (1999, 2009)

Table A.9. Effects on intergenerational schooling persistence: Robustness (excluding Ho Chi Minh City)

	(1)	(2)	(3)
	Panel A. Child-father pairs		
	Child-father	Son-father	Daughter-father
Parental schooling	0.413***	0.368***	0.453***
	(0.011)	(0.012)	(0.011)
Chinese x Full exposure x Parental schooling	0.155**	0.087	0.234**
	(0.063)	(0.093)	(0.105)
Observations	61,311	30,777	30,534
	Panel B. Child-mother pairs		
	Child-mother	Son-mother	Daughter-mother
Parental schooling	0.454***	0.409***	0.499***
	(0.016)	(0.018)	(0.015)
Chinese x Full exposure x Parental schooling	0.154***	0.174**	0.164
	(0.054)	(0.071)	(0.112)
Observations	105,777	56,449	49,328

Notes: Observations from Ho Chi Minh City are excluded from the samples. All specifications include fixed effects for birth time, ethnicity, province, birth time-by-ethnicity, and survey year. Further control variables included into these specifications consist of dummies for gender (male), rural area, and religious affiliations (Buddhism, others and no religious affiliation). The samples include individuals born in May 1951-July 1962. Robust standard errors clustered at the birth time-by-ethnicity level (345 clusters) are in parentheses.

* $p<0.1$, ** $p<0.05$, *** $p<0.01$

Data sources: All estimates are produced using Census (1999, 2009)

Table A.10. Effects on the next generation's human capital: Robustness (excluding province fixed effects)

	(1)	(2)	(3)	(4)
	Fathers' exposure		Mothers' exposure	
	School enrollment	Literacy skills	School enrollment	Literacy skills
Chinese x Full exposure	-0.025***	-0.056**	-0.034***	-0.071***
	(0.004)	(0.023)	(0.005)	(0.027)
Pre-treatment mean	0.977	0.897	0.976	0.888
% change	-2.559	-6.243	-3.484	-7.995
Observations	1,121,277	408,704	1,212,893	409,901

Notes: All specifications include fixed effects for birth time, ethnicity, birth time-by-ethnicity, and survey year. Further control variables included into these specifications consist of child's age and age squared, dummies for child gender (son), rural area, and child's religious affiliations (Buddhism, others and no religious affiliation). The samples include children aged 11-17 of parents born in May 1951-July 1962. Robust standard errors clustered at the birth time-by-ethnicity level (345 clusters) are in parentheses.

* p <0.1, ** p <0.05, *** p <0.01

Data sources: All estimates are produced using Census (1999, 2009)

Table A.11. Effects on the next generation's human capital: Robustness (excluding Ho Chi Minh City)

	(1)	(2)	(3)	(4)
	Fathers' exposure		Mothers' exposure	
	School enrollment	Literacy skills	School enrollment	Literacy skills
Chinese x Full exposure	-0.008**	-0.046**	-0.012**	-0.054**
	(0.004)	(0.020)	(0.006)	(0.024)
Pre-treatment mean	0.975	0.876	0.975	0.874
% change	-0.821	-5.251	-1.231	-6.178
Mean of dep. variable	0.964	0.881	0.960	0.860
Observations	1,110,903	398,335	1,204,664	401,675

Notes: Observations from Ho Chi Minh City are excluded from the samples. All specifications include fixed effects for birth time, ethnicity, birth time-by-ethnicity, and survey year. Further control variables included into these specifications consist of child's age and age squared, dummies for child gender (son), rural area, and child's religious affiliations (Buddhism, others and no religious affiliation). The samples include children aged 11-17 of parents born in May 1951-July 1962. Robust standard errors clustered at the birth time-by-ethnicity level (345 clusters) are in parentheses.

* p <0.1, ** p <0.05, *** p <0.01

Data sources: All estimates are produced using Census (1999, 2009)

Table A.12. Effects on age of the next generation's human capital

	(1)	(2)
	Fathers' exposure	Mothers' exposure
Chinese x Full exposure	0.077	-0.081
	(0.081)	(0.079)
Mean of dep. variable	14.074	14.225
Observations	1,121,295	1,212,910

Notes: All specifications include fixed effects for birth time, ethnicity, birth time-by-ethnicity, and survey year. Further control variables included into these specifications consist of dummies for child's gender (son), rural area, and child's religious affiliations (Buddhism, others and no religious affiliation). The samples include children aged 11-17 of parents born in May 1951-July 1962. Robust standard errors clustered at the birth time-by-ethnicity level (345 clusters) are in parentheses.

* p <0.1, ** p <0.05, *** p <0.01

Data sources: All estimates are produced using Census (1999, 2009)