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

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Abstract

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

Keywords: hostile policies; ethnic inequality; fetal origins; South Vietnam

JEL codes: I12, I25, J13

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1. Introduction

Ethnic and racial minorities are typically the target of the hostile policies of many governments. For instance, the United Kingdom's 2012 hostile environment policy and the Trump administration's 2017 immigration enforcement schemes were strongly criticized by the public because they potentially produced negative impacts for British Asians in the United Kingdom and for Hispanic Americans in the United States, who have a close link with immigrants (Cai 2020). Minority groups encounter worse outcomes when faced with the extremely oppressive policies of authoritarian regimes. The Uyghurs in the People's Republic of China and the Rohingya in Myanmar provide two salient examples. 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 those hostile policies have been rarely studied.

Well-established papers in economics have 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 a difficult social environment are more likely to encounter long-run disadvantages (Bhalotra and Rawlings 2013; Currie and Vogl 2013) because adverse conditions during pregnancy are likely to be 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 potentially 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 create an adverse social environment that makes the life of the targeted groups more difficult through economic hardship, life uncertainty, depression and isolation, which may be harmful

[1] Ethnic and racial inequalities have been identified in previous studies through different outcomes, such as schooling (Kırdar 2009; Panza 2020), the labor market (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.

to children in the critical period of their development. The "Fetal Origins Hypothesis" suggests that children who are exposed to hostile policies *in utero* have poorer long-run outcomes than those who are not (Almond and Currie 2011; Barker 1990; Currie 2011).

To test this hypothesis, we exploit changes in the policies of the government of the Republic of Vietnam (RVN) towards the Chinese ethnicity (known as the Hoa ethnicity) that were implemented in South Vietnam between 1956 and 1963. In this way, we investigate the impacts of *in utero* exposure to hostile policies on the long-run schooling, family and economic well-being outcomes of the generation that was affected directly as well as on the human capital of the one that followed it. Before 1956, despite being an ethnic minority, the Chinese were highly autonomous in their life and work and enjoyed relative economic prosperity in South Vietnam (An 1967). In August 1956, President Ngo Dinh Diem of the RVN, who was well known as a fervent Vietnamese nationalist, introduced a series of new policies that directly targeted the Chinese ethnicity by imposing hostile restrictions on social life and economic activities (Amer 2011). For example, these measures included excluding the Chinese from key business sectors, forcing them to become Vietnamese citizens and replacing the previous Chinese school system and its curriculum with Vietnamese ones as well as other harsh measures (An 1967). The historical evidence shows that these hostile policies created a highly adverse social environment for the Chinese in South Vietnam in the period between August 1956 and the assassination of President Diem in November 1963 (Ungar 1987). Adopting a difference-in-difference (DiD) research design, we compare the difference in outcomes (such as schooling years, among others) between ethnic Chinese individuals who were exposed to the hostile policies *in utero* and older members of the same minority who were born before those policies to the corresponding differences in outcomes among other ethnicities. We use 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 of no pre-trends and strong balances in pre-determined characteristics for our identification that is based on the ethnicity-by-birth-time-level variation in exposure to the hostile policies to ensure the validity of our DiD strategy.

Our results show that *in utero* exposure to hostile policies was detrimental to both the first generation of Chinese-ethnicity individuals in South Vietnam who were directly affected and to the second generation. Exposure reduced the human capital of the first generation (for example, by 0.4 schooling years and by 5-7 percentage points for primary-school and post-primary-school completion). Operating through a reduction in human capital formation, exposure lowered economic activity outside of the agricultural sector and the probability of being married but increased women's fertility at both the extensive and intensive margins, as well as household size. Exposure further had negative effects on the economic well-being of households, such as inferior housing and sanitation conditions, higher usage of polluted cooking energy, and lower household wealth. Interestingly, our results on gender heterogeneity indicate that the negative effects were more pronounced among males than among females for several outcomes, in particular schooling years and post-primary-school completion, leading to decreases of between 18% and 26% in the gender gaps for these outcomes, which are observed frequently among pre-treated individuals. Importantly, exposure reduced the intergenerational schooling mobility of the directly affected individuals. Finally, we identify the negative effects of *in utero* exposure to hostile policies on the next generation, which include lower school enrollment and literacy skills for post-primary-school-age students.

Our paper makes three key contributions. First, it contributes to a central question in the study of the causal determinants of ethnic and racial inequality. Ethnic and racial disparities in socio-economic outcomes that are large and persistent have been one of the most salient features of 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 the related factors that drive this inequality may provide insights for reducing it and for promoting the social inclusion of 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), school quality (Card and Krueger 1992) and, recently, the minimum wage (Derenoncourt and Montialoux 2021). We focus on governments' hostile policies that target an ethnic or racial minority group as an additional

explanation. The relationship between *in utero* exposure to hostile policies and ethnic inequality is argued to operate through the human capital formation of the individuals who are affected directly.[2]

Second, our paper supplements the voluminous body of literature that studies 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 have mainly drawn on extremely devastating events, such as wars and armed conflicts (Akresh et al. 2012, 2021; Leon 2012; Phadera 2021; Singhal 2019), and on other catastrophic occurrences, such as 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 that may be less severe in their impact. The costs of such hostile policies are however not fully understood. Our findings suggest that hostile policies may generate considerable economic costs through their adverse impacts on the life trajectories of those who are exposed to them in the womb. Importantly, we extend our analysis to the impact of the policies on the next generation, which have been under-studied in the literature on the consequences of *in utero* exposure to adverse shocks (Akresh et al. 2021; Brown 2020; Phadera 2021; Schwandt 2018).

Third, our paper is related to the growing literature on the causal determinants of intergenerational mobility. Previous research has demonstrated the importance of neighborhood environments (Chetty and Hendren 2018a, 2018b), higher education categories (Chetty et al. 2017), income shocks (Bütikofer et al. 2018), compulsory schooling 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 reveal the harmful effect of hostile policies on schooling mobility across generations, which exacerbates the inequality of opportunity that ethnic and racial minorities face.

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

[2] While human capital formation has been revealed 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).

strategy. Section 5 presents our results, including those on 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 of South Vietnam. The Chinese community in South Vietnam was established as a consequence of two mass migration flows from China. The first Chinese arrived in South Vietnam by the late 17th century as part of a large flow from China to Southeast Asian countries, where they sought new homes after the collapse of the Ming dynasty and its replacement by the Manchu regime (Share 1994). The second flow arrived during the period of French rule in Indochina in the late 19th and early 20th century (Feng 2017). Given the high demand for workers in the colonial economy, the French encouraged immigrants from neighboring countries to fill the shortage of native Vietnamese labor (An 1967). A considerable number of Chinese individuals settled in South Vietnam to seek new economic opportunities. Cochinchina, especially Saigon-Cholon, attracted the largest number of Chinese residents in South Vietnam. When Vietnam regained its independence from the colonists after World War II, the Chinese immigrants and their descendants remained in South Vietnam, accounting for a large proportion of the country's ethnic minorities (An 1967). The Chinese, amounting to about one million individuals in the middle of the 1950s, became the single largest minority group (An 1967; Marsot 1993).

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

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

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

In terms of social life, Chinese residents were closely connected to their families, clans and communities and maintained social and cultural distinctions from the native Vietnamese as well as from other ethnic minorities (Chan 2018; Stern 1985). Under French rule, Chinese residents were given the right to organize their society into five autonomous congregations (known as "*bang*")[4] and several occupational associations (known as "*hoi*").[5] The Chinese communities established hospitals, markets, restaurants, media outlets, news agencies, schools and temples, among other types of infrastructure, within these congregations, forming their own 'state' in South Vietnam (Share 1994). Highly autonomous means of organizing life and work allowed the Chinese minority to remain outside of the political life of the Vietnamese natives and to focus on business profits and accumulating wealth. Keeping Chinese communities socially, culturally and, to some extent, competitively separate from the Vietnamese natives was a divide-and-rule strategy that the French used to control key population groups in Indochina (Ungar 1987).

[4] The five congregations included the Cantonese, the Teochiu, the Hokkien, the Hakka, and the Hainan (Tran 2018). These congregations played a role as key administrative units that were responsible for governing all aspects of the life of the Chinese community. These congregations selected their own leaders, who intermediated between the French bureaucracy and Chinese residents (Engelbert 2008).

[5] Chinese individuals used occupational associations to manage their economic activities in 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 (Engelbert 2008).

In summary, the Chinese ethnicity in South Vietnam enjoyed a comfortable living environment before August 1956. They were highly autonomous socially and held considerable economic influence. However, there was some tension between Chinese residents and Vietnamese natives because the former shared a large part of the benefits of the colonial economy with the French, marginalizing the latter (Share 1994; Tran 1993). Therefore, Chinese residents became an obvious target for the Vietnamese nationalists who assumed political power when the French left Vietnam. In August 1956, President Ngo Dinh Diem of the RVN introduced a series of hostile policies targeting the Chinese, changing their social environment adversely.

2.2 Ngo Dinh Diem's hostile policies towards the ethnic Chinese

The Geneva Accords of July 1954 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 valued an independent Vietnam highly (Ungar 1987). Ngo Dinh Diem believed that attacking the Chinese ethnicity could help him to gain support from the majority of Vietnamese natives, who had treated the Chinese with animosity for the centuries, and then to strengthen his political image as a Vietnamese nationalist (Share 1994). In August 1956, Diem initiated a series of hostile policies targeting the Chinese ethnicity (which were known as '*Chinh sach Hoa Kieu*'). Diem's policies were designed not only to weaken the economic influence of the Chinese but also to force them to assimilate culturally and socially into Vietnamese society through hostile means.

It is important that the starting point and the end point of the policies were determined arbitrarily by exogenous changes in contemporaneous political conditions, both within and outside of the RVN. The starting point was strongly affected by an unexpected change in the People's Republic of China (PRC) policy on Chinese individuals overseas in 1955. Before 1955, PRC law recognized dual citizenship and 'citizenship by bloodline,' which meant that individuals with Chinese origins who were residing in other countries were treated as Chinese citizens (Mitchison 1961). Therefore, the Chinese overseas, including those living in the RVN, were protected by the PRC (Han 2009). That citizenship policy prevented small countries in Southeast Asia from introducing policies that were adverse towards their Chinese residents because of concerns about potential conflicts with the PRC (Suryadinata 2017).

The situation changed unexpectedly in April 1955, when Premier Zhou Enlai of the PRC declared at the Bandung Conference that the PRC had decided to end its 'citizenship by bloodline' policies and called for the Chinese overseas to respect local customs and the laws of their countries of residence (Han 2009). By setting itself apart from the overseas Chinese communities, the PRC aimed to soothe anxieties about Chinese expansionism and thus construct trusting relations with Southeast Asian countries (Fredman 2014). This change in the citizenship policies of the PRC instilled Ngo Dinh Diem with the confidence to implement his policy reform on Chinese issues (Ungar 1987).

The end point of the policies was also highly exogenous. In November 1963, Ngo Dinh Diem was suddenly assassinated in an unexpected coup d'état led by a group of RVN generals. Importantly, the death of President Diem created a discontinuity in the implementation of his policies towards the Chinese ethnicity, which faded away under subsequent governments. The highly unpredictable timing of the policies allows us to treat exposure to them during pregnancy as if it was randomly assigned to different birth cohorts (see Section 4.1 for details).

Diem's policies focused on four areas, including citizenship, the organization of life and work, economic activities and education (Share 1992). First, as far as the citizenship issue was concerned, Diem's Ordinance Number 48, promulgated in August 1956, required all Chinese individuals born in the RVN to be granted Vietnamese citizenship automatically. Chinese residents were called 'Chinese Vietnamese' (known as '*Nguoi Viet goc Hoa*') rather than 'Chinese.' Individuals without Vietnamese citizenship were given a choice between returning to their country of origin (the PRC or Taiwan) or being banned from participating in economic activities. Moreover, Chinese residents were forced to adopt Vietnamese names on pain of punitive taxation.

Second, turning to the organization of life and work, Diem abolished the five autonomous congregations and the occupational associations of the Chinese communities in June 1960. The government seized the high-value assets of these organizations and required all Chinese residents to follow the RVN's laws on all aspects of their lives and work, like other ethnicities in South Vietnam. Males were obligated to complete their military service.

Third, in terms of economic activity, Diem launched new regulations, which aimed to return the economic resources that had been held by Chinese residents before 1956 back to Vietnamese natives. Ordinance Number 53, issued in September 1956, prohibited Chinese residents from doing business in the 11 most profitable industries: pork and fish retail, retail stores, coal, gasoline, the sale of used items, textile, scrap metal, rice, transportation, rice milling and moneylending (Stern 1985; Schrock 1966). The ordinance also required Chinese-owned enterprises to be transferred to Vietnamese individuals or to close (Stern 1985). Although Chinese businessmen tried to circumvent the law through several techniques, such as hiring Vietnamese individuals as nominee owners, the regulation disrupted the economic activities of the Chinese community. Moreover, the government restricted Chinese participation in other industries by implementing discriminatory labor-market policies. For instance, a Chinese doctor was less likely to be appointed to a hospital than a Vietnamese one, even if both held similar qualifications (Stern 1985). All Chinese households and firms were required to pay taxes directly to the RVN government.

Fourth, Diem's policies also focused on education. New policies reformed all of the schools that had been established and managed by Chinese communities. School ownership had to be transferred to the state, and school principals had to be Vietnamese, excluding Chinese individuals from holding managerial positions (Fitzgerald 1972). At these schools, the Chinese languages, which had been the main language of instruction before 1956, was replaced by Vietnamese (Fitzgerald 1972). Furthermore, a new curriculum, with a focus on Vietnamese history and culture, was adopted to educate Chinese children. Passing an exam in Vietnamese literature at the end of primary school became one of the conditions for admission to secondary education. The law also reduced the time allotted to teaching the Chinese languages. 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 Response of Chinese minority and hostile environment

Chinese residents reacted to Diem's policies strongly. In particular, they organized protests and large-scale boycotts, threatening to obstruct the economy (Amer 2011). Police forces used harsh measures, such as beatings and thrashings, to control Chinese protesters, leading to violent confrontations between

the government and Chinese residents. When diplomatic efforts from other countries, such as Taiwan, to stop the RVN from its use of harsh measures against the Chinese in South Vietnam failed, the Chinese communities faced a fearsome and uncertain outlook.[6] Chinese residents ended their economic activities in order to threaten the Diem administration with the collapse of the South Vietnamese economy. For example, they withdrew their money from banks to devalue the Vietnamese currency, they ceased trading and interrupted the flow of goods and services, and they closed retail outlets. To some extent, these reactions created disorder in the economy and, importantly, caused the government to suffer a significant loss of tax revenue. As a result, to keep the economy stable, Diem relaxed several economic measures. For example, Chinese residents were allowed to own 49% of businesses (Ky 1963).

Although the Chinese residents were almost successful in protecting their wealth, they encountered a chaotic social environment between 1956 and 1963 (Ungar 1987). Their situation had become extremely difficult under Diem's policies, and the Chinese inhabitants of 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 (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 have since then gradually returned to the economic position that they occupied before 1956. By the early 1970s, the Chinese ethnicity again owned almost all commercial banks, and they had re-established their dominance over other industries in South Vietnam (Stern 1985). Diem's untimely death was a positive turning point for the economic position of the Chinese in South Vietnam.

We include those born between 1951 and 1962 into our sample, which contains a treatment group (the ethnic Chinese) and a comparison group (other ethnicities), because doing so provides the optimal measure of *in utero* exposure to an adverse shock in the social environment. Before 1956, all ethnicities in South Vietnam lived in highly similar social environments—no shock was affecting one ethnic group

[6] The Diem government declared that these actions were an internal affair of the RVN. To maintain relations with the RVN, Taiwan then agreed to disregard the Chinese issue in South Vietnam (Ungar 1987).

while leaving others unscathed (Taylor 2013). Between 1956 and 1963, the Chinese ethnicity inhabited the harsh social environment that Diem's hostile policies created, while the social environment of other ethnic groups remained unchanged (Taylor 2013).

3. Data

Our analysis draws on two large and random samples from 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). Both are the restricted versions of these censuses and the largest Vietnamese micro datasets. They contain information about the demographics, schooling, employment, mortality, wealth and housing of the respondents across the country's regions. Our treatment group is the Chinese ethnicity, which only accounts for about 1% of the total population. Using household surveys, which typically contain a small number of ethnic Chinese individuals, would prevent us from producing precise estimates of the effects of interest that are conditional on birth group and ethnicity fixed effects. The large census samples therefore help us overcome the drawbacks of using small samples from household surveys.

We employ these samples to create our variables of interest, in which one or both censuses are used, depending on outcomes. We first use birth-year and birth-month information to create nine-month birth groups, which help us to identify the groups that were exposed to the policies *in utero*. We further extract information on ethnicity to construct dummies for various ethnic groups. Based on the sizes of the ethnicities from the 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 residents) are classified as one group and other small minorities as another.[7] We then construct a set of outcomes for both the first generation that was exposed to the policies and the one that followed it.

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

We restrict the census data to individuals born between May 1951 and July 1962 to construct our sample for the first generation. In particular, we extract data from both censuses to construct educational outcomes, including schooling years and dummies for primary-school, post-primary-school and college qualifications. We further extract data on 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 childbearing and the number of children for females (using the 1999 census) and household size, which is measured by the number of individuals (using the 1999 census).

We use the 2009 census to construct several proxy outcomes for family living standards, including housing, cooking energy, sanitation and assets. *Solid house* is an indicator that takes a value of 1 if a home has a solid structure, made from masonry units and reinforced concrete, or a value of 0 if the house is not solid or if it is made from wood. *Old cooking energy* is an indicator that takes a value of 1 if the household uses charcoal, firewood or kerosene and a value of 0 if the household uses gas or electricity for cooking. *Improved sanitation* is an indicator that takes a value of 1 if the household has one of several types of improved toilet[8] or a value of 0 if the household has an unimproved toilet.[9] Finally, *asset index* is the sum of eight durable assets (television, radio, telephone, computer, washing machine, refrigerator, air conditioner and motorcycle).

To estimate the effect of *in utero* exposure to Diem's policies on the next generation, we restrict the sample to the children (aged between 11 and 17) of parents who were born between May 1951 and 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 skills. Moreover, we construct a set of control variables to include in our regressions. These controls consist of age, age squared (for the estimates for the next generation), dummies for male gender and rural areas as well as religious affiliation to Buddhism, other religions and no religious affiliation.

[8] Improved toilets include flush toilets, toilets that are connected to a piped sewer system, a septic system, toilets that flush or pour-flush to a pit latrine, a pit latrine with a slab, ventilated improved pit latrines and composting toilets.

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

Table A.1 in the Supplementary Appendix presents summary statistics for the outcome variables in the total sample, the ethnic Chinese sample and the non-Chinese sample.

4. Empirical strategy

4.1 Definition of treatment and exposure groups

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

To identify individual groups' *in utero* exposure to Diem's policies, we create nine-month birth groups. We treat those who were born in July 1956 or before 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 *in utero*, we classify individuals who were exposed to the policies for less than nine months as the partial-exposure birth group and those who were exposed to the polices for nine months as the full-exposure birth group.

-Figure 1-

We restrict our baseline sample to those born between May 1951 and July 1962. Figure 1 displays the timing of Diem's policies and the *in utero* exposure of various birth-time groups to the policies. In total, 15 birth-time groups are indexed by t. We use individuals born between May 1951 and July 1956 (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 as the full-exposure birth-time group ($t = 1, \dots, 7$).

-Table 1-

We perform balancing tests to ascertain the degree to which the identifying ethnicity-by-birth-time variation in exposure to Diem's policies correlates with trends in the observables. Using an

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

aggregate dataset at the ethnicity-by-birth-time level that is constructed from the 1999 and 2009 censuses, we therefore regress the likelihood of full *in utero* exposure on specific observables, conditional on ethnicity, birth-time, province and survey-year fixed effects. We focus on observable characteristics that are unlikely to have been 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 (male) and religious affiliation (Buddhism, other religious affiliations and no religious affiliation), either separately (columns 1-2) or jointly (column 3; joint-significance *p*-value of 0.479). We further discover that regional composition (based on Vietnamese economic regions) is not statistically related to the ethnicity-by-birth-time probability of full exposure either separately (column 4) or jointly with gender and religious composition (column 5; joint-significance *p*-value of 0.755). These results allow us to establish that our identification of ethnicity-by-birth-time variations in exposure to Diem's policies is unrelated to trends in the observables.

4.2 Estimation methods

To estimate the effects of *in utero* exposure to the hostile policies, we perform generalized DiD regressions that draw comparisons across ethnicities and birth groups, controlling for ethnicity, birth-time and province fixed effects while letting birth-time effects vary across ethnicities. The implementation of the regression differs, depending on whether we are estimating effects on the generation that was affected directly by Diem's policies or on the next generation. The two approaches are described in the paragraphs that follow.

Effects on the first generation

We run the following regression for outcome Y_{ietps} of individual i who is from the directly affected generation, belongs to ethnicity e, was born at birth time t, lives in province p and was surveyed in year s:

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

$Chinese_{ie}$ is a dummy variable that indicates that the ethnicity e of individual i is Chinese. $PartialExposure_{it}$ is a dummy variable that indicates that individual i belongs to the partial-exposure birth-time group ($t = 0$). $FullExposure_{it}$ is a dummy variable that indicates that individual i belongs to the full-exposure birth-time group ($t = 1, \dots, 7$). $\boldsymbol{X'}_i$ is a control vector for individual characteristics, such as dummies for male gender and religious affiliation (Buddhism, another religion and no religion). φ_e, ω_t, π_p and θ_s are the fixed effects of 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, way of life and other socio-economic 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 that use datasets that consist of a single survey wave. We include the dummies for the interaction between birth time and ethnicity $\chi_t \times e$, which allows trends across cohorts to vary in unrestricted ways across ethnicities. The joint inclusion of ethnicity and birth-time fixed effects turns Equation (1) into a generalized DiD 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, we run regressions of the following type for a child j who was born to parent k at birth time t, belongs to ethnicity e, lives in province p and was surveyed in year s:

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

Y_{jketps} is an outcome for child j. $\boldsymbol{X'}_j$ is a set of controls for child characteristics, such as age and age squared, as well as dummies for male gender and religious affiliation (Buddhism, another religion and no religion). ϵ_{jketps} is the error term. The definitions of the other terms are similar to those used 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. For α_2 and β_2 to be interpreted as causal effects, the parallel-trends assumption must hold. In other words, the estimates of α_2 and β_2 for the effects on the outcomes of interest in the absence of Diem's policies should be close to 0. To check if this assumption holds, we perform an event-study analysis of the human capital outcomes of the first generation. The regression equation is now a modified version of Equation (1), in which we replace the interaction terms $Chinese \times PartialExposure$ and $Chinese \times FullExposure$ with the interaction term $Chinese \times BirthTime$, where $BirthTime$ is a dummy for birth time t ($t = -7, \dots, 7$). We omit the partial-exposure birth time of $t = 0$ to form a reference group. The other terms in our event-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 0 for the no-exposure birth time but decline gradually from the first full-exposure birth time onwards ($t = 1, \dots, 7$). The gradual decrease in the estimates reflects the steady deterioration of the social environment encountered by full-exposure birth-time groups. 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 the empirical results for the effects experienced by both the first and the second generation in Tables 2-10. We also present the baseline estimates, which are produced using Equation (1) for the first generation and Equation (2) for the second generation. We further show the gender heterogeneity of the effects, which is produced using baseline regression equations, modified with the inclusion of the additional interaction effects of gender to generate 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 a sub-sample that excludes observations from Ho Chi Minh City. These results are presented in the Supplementary Appendix. Excluding province fixed effects from the baseline specification allows us to determine whether the effects are sensitive to controls for geographic location. By excluding observations from Ho Chi Minh City, which is the most developed city and host to 50% of the Chinese community in South Vietnam, we seek to ascertain whether the baseline effects are driven by the effects of living in Ho Chi Minh City.

5.1 Effects on human capital formation

Baseline estimates

We start by presenting estimates of the effects of *in utero* exposure to hostile policies on the schooling outcomes of the first generation in Table 2. Exposure has negative effects on all schooling outcomes, as shown in columns 1-4. These effects are strongly statistically significant at the 1% level. Column 1 indicates that exposure leads to a fall in completed schooling of about 0.4 years. Compared to the mean of 6.1 schooling years for pre-treated birth-time groups, this effect is equivalent to a sizable reduction of nearly 7%. Columns 2-3 further show that exposure decreases the probability of achieving various levels of education, including 6.8 percentage points for primary schooling and 5 percentage points for post-lower-secondary schooling. These effects are also substantial because they create a reduction that is equivalent to between 10% and 19% when compared to pre-treatment means. 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 compared to pre-treated birth-time groups.

-Table 2-

The baseline estimates are highly robust to estimating another specification that excludes province fixed effects, as shown in Table A.2 of the Supplementary Appendix. However, when this alternative specification is used, the effect size of the estimates is larger than that of the baseline estimates for all outcomes. Table A.3 in the Supplementary Appendix reports the estimates when the baseline specification is ran 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 and primary- and post-primary-school completion. The effect on college qualification, in contrast, is smaller than the baseline estimate.

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

Gender heterogeneity

Table 3 reports the gender heterogeneity of the baseline estimates of the effects on human capital formation in the first generation. The point estimates in the first two rows of Table 3 show detrimental effects on all outcomes for both genders (strongly statistically significant at the 1% level, except for female college attainment at the 5% level). However, the negative effects on males are considerably larger than those on females. For schooling years (column 1), the effect on males is greater than that on females by a factor of 1.96. For the probability of completing a level of education, the increase is by 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 college (column 4).

-Table 3-

The third row of coefficients in Table 3 reports the differences in the effects on males and females. Accordingly, we reject the hypothesis that the effects on males and females for schooling years (statistically significant at the 5% level) and for the probability of completing post-primary education (statistically significant at the 1% level) are equal. 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 in no-exposure birth-time groups are skewed in favor of males for all outcomes. Among individuals who were not exposed to the hostile policies *in utero*, males have considerably more favorable outcomes than females, including, for example, an additional 1.5 years of schooling and 16.6 percentage points of post-

primary-education completion. The comparison between these rows reveals that exposure would have reduced the gender differences to the strong detriment of males, leading to significant reductions in the gender gaps for schooling years (18%) and post-primary education (26%).

5.2 Effects on labor-market and family outcomes

Baseline estimates

Table 4 presents the baseline effects for labor-market and family outcomes. We use non-agricultural work as a proxy for improved labor-market outcomes because working in a non-agricultural sector is very likely to be associated with highly skilled occupations in the non-traditional economy. In column 1, we find that *in utero* exposure to hostile policies reduces the probability of having a non-agricultural job by 7.9 percentage points (statistically significant at the 1% level). Compared with the pre-treatment mean (81%), this effect is equivalent to a reduction of 9.8%. This finding suggests that the individuals who were exposed to the policies *in utero* hold a lower status in the labor market than those who were not.

-Table 4-

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

Columns 3 and 4 show the effects of exposure on female fertility. We find that exposure has a positive effect on fertility at both the extensive and the intensive margin. The effect on the probability of having a child is at 0.8 percentage points (column 3; statistically significant at the 1% level). At the extensive margin, this fertility effect nearly equals an increase of 9% relative to the pre-treatment mean. At the intensive margin, *in utero* exposure increases the number of children by 0.3 (column 4; statistically significant at the 1% level), which is equivalent to a 12% spike.

Finally, column 5 shows the effect of exposure on household size. The exposed individuals live in families that are larger by 0.49 persons, relative to those who were not exposed to the hostile policies

(statistically significant at the 1% level). This effect corresponds to an increase of 8.7%, compared to the pre-treated individuals. This finding provides indicative evidence that *in utero* exposure to hostility is likely to increase the likelihood of cohabitation within an extended family.

Gender heterogeneity

Table 5 reports the gender heterogeneity of the effects of exposure on labor-market and family outcomes. The first two rows of coefficients indicate that the effects are gender heterogeneous for all outcomes. The effects are damaging for both genders, similarly to the overall baseline effects presented 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 marital status and 1.14 for household size. However, as shown in the third row of estimates in Table 5, we cannot reject the hypothesis that the effects on these outcomes are equal for the two genders. Therefore, there is no evidence of gender differences in the effects of exposure to hostile policies on labor-market and family outcomes.

5.3 Effects on family living standards

Baseline estimates

Table 6 reports the results for the effects of exposure on several family-living-standards outcomes. The estimates suggest that individuals who were fully exposed to the hostie policies *in utero* generally experience poorer living conditions than those who were not exposed. All estimates are statistically significant at the 1% level. Column 1 shows that exposed individuals 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, relative to the pre-treatment mean.

-Table 6-

Column 2 shows that the households of those who were exposed to the adverse policies *in utero* have a likelihood of using old forms of cooking energy that is 5.5 percentage points higher

(corresponding to an increase of 24.9%). Furthermore, the probability that the households of the fully exposed individuals use improved sanitation is also lower by 6.5 percentage points (equivalent to a decrease of 7.3%) than that of their counterparts, as shown in column 3. Finally, column 4 shows that the fully exposed individuals are also poorer than those who were not exposed: the asset index reveals a reduction of 0.23 assets (equivalent to 4.9%).

Gender heterogeneity

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

-Table 7-

Again, the effects are to some extent larger for males than 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 1.3, 1.22, 1.06 and 1.36, respectively. However, the coefficients for gender differences are not statistically significant, as shown in the third row, suggesting that there is no evidence of gender differences in the effects of exposure on family living standards.

5.4 Effects on ethnic inequality across generations

In this sub-section, we address the question of whether prenatal exposure to hostile policies exacerbates inequality of opportunity, thus reducing intergenerational mobility. We therefore inquire whether exposure affected the correlation between the levels of educational attainment of the directly affected generation and their parents. Using both the 1999 and 2009 censuses, we estimate an augmented version of Equation (1), with a directly affected individual's years of schooling on the left-hand side. The right-hand side is extended by adding a term for parental years of schooling ($ParentalSchooling$) and its interactions with $Chinese \times FullExposure$, $Chinese \times PartialExposure$, $Chinese$, $FullExposure$, and $PartialExposure$. The other terms remain the same as in Equation (1). While the coefficient for $ParentalSchooling$ indicates the baseline intergenerational elasticity (IGE) of schooling, the

coefficient for $Chinese \times FullExposure \times ParentalSchooling$ suggests the degree to which *in utero* exposure to the hostile policies affects IGE.

Table 8 presents the coefficients for the intergenerational persistence of schooling. We provide estimates for whole samples of child-father (panel A) and child-mother pairs (panel B) as well as sub-samples that are divided into child genders within each panel.[11] The first row of coefficients in each panel reports baseline IGE estimates. Generally, the baseline IGE estimates are strongly statistically significant and amount to around 0.4 years of schooling for both child-father and child-mother pairs. The size and the direction of these IGE coefficients accord with results reported in other studies of Vietnam (for example Cornelissen and Dang 2021).

-Table 8-

The second row of coefficients in each panel of Table 8 shows interaction effects. The estimates, in general, show that *in utero* exposure to hostile policies increases the dependence of the schooling of the directly affected generation on parental schooling. In the whole samples, the exacerbating effects are at about 0.16 for child-father pairs (column 1, panel A; statistically significant at the 1% level) and at 0.15 for child-mother pairs (column 1, panel B; statistically significant at the 1% level). The estimates from the child-gender sub-samples show that the schooling of daughters depends on parental schooling to a higher extent than that of sons. These effects are at 0.25 for daughter-father pairs (column 3, panel A; statistically significant at the 5% level) and at 0.2 for daughter-mother pairs (column 3, panel B; weakly statistically significant at the 10% level). For sons, we only find a statistically significant worsening of the effect of *in utero* exposure for son-mother pairs (column 2, panel B; weakly statistically significant at the 10% level).

Although the evidence of the damaging effect of prenatal exposure to hostile policies on schooling years is stronger for males than for females, as demonstrated in Table 3, the effects of exposure on the intergenerational persistence of education are still larger for females. One likely explanation is that schooling was traditionally less prevalent among Chinese-ethnicity girls than among Chinese-ethnicity

[11] It is important to bear in mind that the children, sons and daughters described in this sub-section belong to the first generation. The fathers and the mothers to whom the text refers are the parents of the first generation.

boys because of the strength of son preference. The gender difference in the negative effect of exposure on schooling years, which favors males, is not large enough to overcome the traditional gender gap in the persistence of intergenerational schooling, meaning that the effects of exposure on IGE are larger for females than for males.

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

5.5 Effects on the human capital of the next generation

Baseline estimates

The final part of the discussion of the results concerns the effects of *in utero* exposure to hostile policies on the human capital of second-generation children. We use the 1999 and 2009 censuses to construct a sample of the children (aged between 11 and 17) of individuals who were born between May 1951 and July 1962. We focus on school enrollment and literacy as the two main proxies of the human capital of post-primary-school-age children. While school enrollment is a proxy of the quantity of schooling that school-aged children receive, literacy skills are indicative of its quality—children older than 11, who typically complete their primary education in Vietnam, are expected to know how to read and write. The baseline estimates, which were produced separately for paternal and maternal exposure using Equation (2), are presented in Table 9.[12]

-Table 9-

Overall, we find that *in utero* exposure to hostile policies affects children's human capital adversely. Columns 1 and 2 show that paternal exposure reduces school enrollment and literacy skills by 1 percentage point and 4.9 percentage points, respectively (both statistically significant at the 5% level). These effects are equivalent to reductions of 1% and 5.5%. Maternal exposure has slightly larger negative effects on these outcomes than paternal exposure. As shown in columns 3 and 4, the effects of

[12] The fathers and mothers to whom this sub-section refers are from the first generation.

maternal exposure include a 1.3-percentage-point decrease in school enrollment (statistically significant at the 1% level) and a 5.8-percentage-point fall in literacy skills (statistically significant at the 5% level). These effects translate into reductions of 1.3% and 6.5% in school enrollment and literacy skills, respectively.

Children's gender heterogeneity

The results on children's gender heterogeneity are presented in Table 10. For paternal exposure, we only find a statistically significant effect on the school enrollment rates of sons (column 1; 1.3 percentage points, statistically significant at the 5% level) but not on that of daughters, as shown in column 1. The difference in these adverse effects is statistically significant and favors sons (column 1; 0.7 percentage points, statistically significant at the 10% level). Paternal exposure has statistically significant and negative effects on literacy skills for both sons and daughters, as shown in column 2. This said, for paternal exposure, the son-daughter difference in literacy skills is not statistically significant.

-Table 10-

As is evident from columns 3 and 4, we find that maternal exposure has a slightly larger effect on both school enrollment and literacy skills for sons than for daughters. 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 suggest that sons tend to fare slightly worse than daughters when their parents have been exposed to hostile policies during pregnancy. However, as the estimates in the third row are statistically insignificant, we can accept the hypothesis that children's gender effects are similar.

6. Concluding remarks

Using large-scale census data from Vietnam, we studied the long-run and intergenerational effects of *in utero* exposure to hostile policies. We first discovered that exposure to hostile policies in the womb has damaging impacts on human capital formation in the directly affected generation, measured by schooling years, primary- and post-primary-school completion and college qualifications. These results

are in line with previous evidence, which shows that exposure to adverse shocks early in life likely reduces human capital (Akresh et al. 2012, 2021; Leon 2012; Karbownik and Wray 2019; Maccini and Yang 2009). Given the importance of human capital to 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 treated these effects as the main channels that link *in utero* exposure to hostile policies and labor-market, family and economic outcomes in the generation that is affected directly and in the generation of their children. We also found that, by reducing human capital, *in utero* exposure to hostile policies depresses economic activity outside of the traditional economy and lowers the likelihood of being married, that it increases fertility and that it decreases living standards among households in the generation that is affected directly. In addition, we uncovered the negative impacts of prenatal exposure to hostile policies on the next generation's human capital, as measured by school enrollment and literacy skills.

Our paper shows that the hostile policies that governments implement to target specific minorities may drive the persistent ethnic and racial inequalities in socio-economic outcomes that have been observed across the globe. For this reason, considering the potential consequences of hostile policies is important for public policies in reducing ethnic and racial inequality.

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

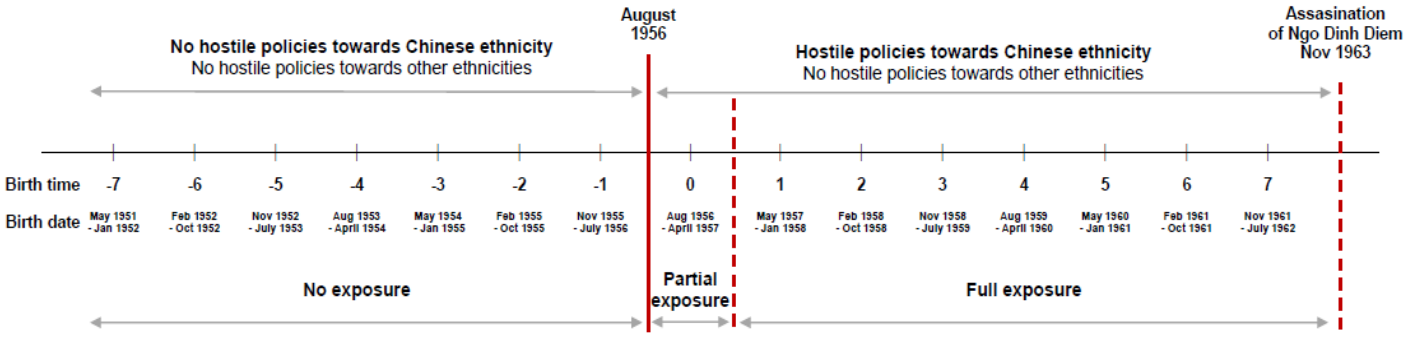
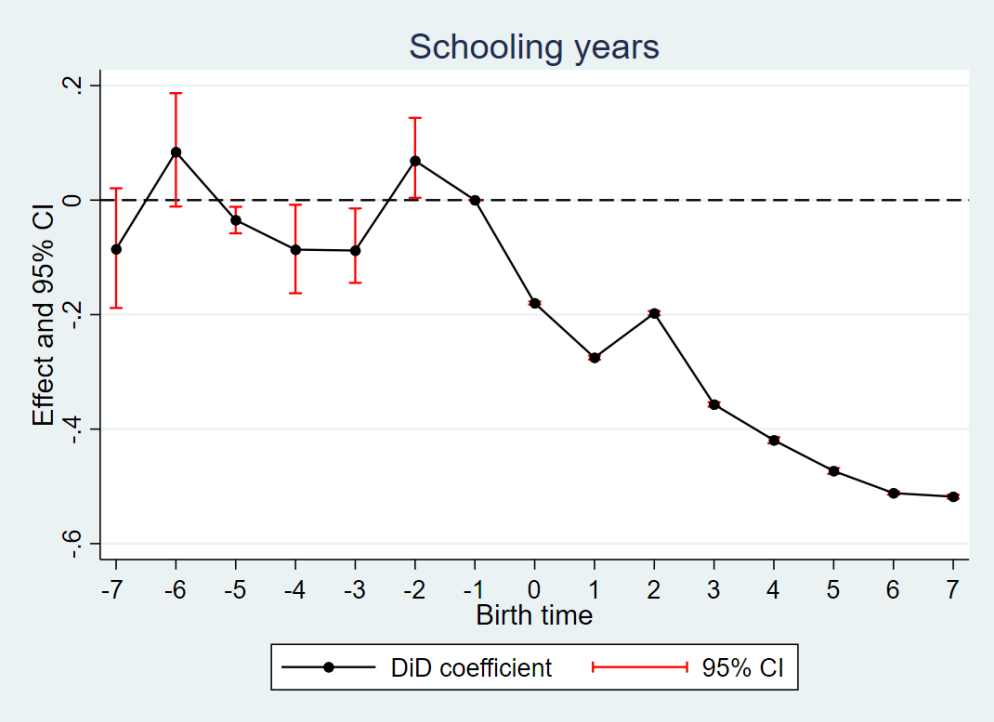


Figure 2. Event-study graphs



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)