



## Regular Research Article

## Long-lasting consequences of being targeted

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## ABSTRACT

While numerous majority-controlled governments globally have enacted hostile policies targeting minority groups, the long-term consequences of these policies remain insufficiently explored. By exploiting policy changes directed at the Chinese ethnic minority in South Vietnam between 1956 and 1963, this paper investigates the long-lasting effects of *in utero* exposure to hostile policies on multigenerational outcomes and social mobility. The findings reveal that such exposure adversely affects education, labor market outcomes, family formation, and economic well-being, while significantly increasing women's fertility both intensively and extensively among directly impacted individuals. These hostile policies furthermore have intergenerational consequences, diminishing the next generation's human capital and hindering educational mobility across genera- tions.

## 1. Introduction

Throughout the evolution of human societies, minorities have frequently been subjected to hostile policies due to their ethnic, racial, and immigrant backgrounds. These policies, im- plemented by majority- controlled governments, have manifested in various forms, including legal discrimination, forced relocation, systemic violence, and genocide. Historical examples comprise the Roman Empire's persecution of Jews during ancient and medieval times (Simon 1996), the Spanish In- quisition's targeting of Jews and Muslims during the early modern period (Morris 2008), the United States' Indian Removal Act and Jim Crow laws (Black 2015; Thompson-Miller et al., 2014), and Nazi Ger- many's Holocaust against Jews in the 19th and 20th centuries (Bartov 1998; Longerich 2010). In more recent times, reports have documented human rights abuses against Uighur Muslims in China (Concepcion 2000; Stern 2021) and accusations of genocide against the Rohingya in Myanmar (Kipgen 2013; Ibrahim 2018). Over the past decade, the United Kingdom government has implemented measures aimed at creating a "hostile environment" for undocumented immigrants, thereby

restricting their access to employment, housing, public funds, health- care, and financial services (Griffiths and Yeo 2021; Qureshi et al. 2021). Understanding the consequences of such hostile policies is vitally important for explaining the persistent inequalities observed among various groups defined by ethnicity, race, and immigrant status, as extensively documented in the literature (van de Walle and Gune- wardena 2001; Alesina et al. 2016; Bayer and Charles 2018; Ravallion 2020; Gómez-Echeverry, 2024).<sup>1</sup> In this paper, I examine the long- lasting impacts of exposure to hostile policies during pregnancy.

A well-established body of literature in economics has demonstrated that early-life conditions significantly influence human capital forma- tion and subsequent socio-economic outcomes (Almond and Currie 2011; Currie and Almond 2011; Andalon et al. 2016). Children conceived in adverse social environments are more likely to encounter long- term disadvantages (Bhalotra and Rawlings 2013; Currie and Vogl 2013) as unfavorable circumstances during pregnancy are likely detri- mental to fetal development (DiPietro 2012; Graignic-Philippe et al. 2014), birth weight (De Bernabé et al. 2004; Camacho 2008; Nasir 2021) and maternal mental health (Persson and Rossin-Slater 2018; von Hinke

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<sup>1</sup> Previous studies have identified inequalities across ethnicities, races, and immigration statuses through various outcomes, such as schooling (Kirdar 2009; Panza 2020), labor market success (Barr and Oduro 2002; Goldsmith et al. 2006; Gómez-Echeverry 2024), wealth (Derenoncourt et al. 2023; Derenoncourt et al. 2024), access to economic resources (Alesina et al. 1999; De Luca et al. 2018; Fafchamps 2000), cognitive skills (Fryer Jr and Levitt 2013), and health (Alexander and Currie 2017; Alsan and Wanamaker 2018; Williams et al. 2019) among others.

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et al. 2022). These factors potentially mediate children's long-term outcomes (Black et al. 2007; Kim et al. 2017; Voit et al. 2022; Crump et al. 2024). This robust scientific foundation strongly motivates the examination of the long-lasting effects of exposure to hostile policies towards minorities during pregnancy. When policymakers implement such policies, they create adverse social environments that make the lives of the targeted groups more difficult through economic hardship, life uncertainty, depression, and isolation for the targeted groups. These conditions can be particularly harmful during critical developmental periods. Based on the *Fetal Origins Hypothesis*,<sup>2</sup> I hypothesize that individuals exposed to hostile policies *in utero* experience poorer long-term outcomes compared to those who were not exposed (Barker 1990; Ellison 2005; Arpino et al. 2018).

To test this hypothesis, I leverage changes in the policies of the government of the Republic of Vietnam (RVN) towards the Chinese ethnicity (also referred to as the *Hoa* ethnicity) that occurred in South Vietnam between 1956 and 1963. Through this approach, I aim to investigate the causal impacts of *in utero* exposure to hostile policies on the long-term schooling, family formation, and economic well-being outcomes of the directly affected generation, as well as on the human capital of the subsequent generation and intergenerational mobility. Prior to 1956, despite their status as an ethnic minority, the Chinese in South Vietnam enjoyed significant autonomy in their daily lives and work, experiencing relative economic prosperity (An 1967). However, in August 1956, President Ngo Dinh Diem of the RVN, renowned for his fervent Vietnamese nationalism, introduced a series of policy measures directly targeting the Chinese ethnicity (Amer 2011). These measures included imposing hostile restrictions on social life and economic activities such as excluding the Chinese from key business sectors, compelling them to obtain Vietnamese citizenship, and replacing the existing Chinese school system and curriculum with Vietnamese equivalents, among other stringent measures (An 1967). Historical evidence indicates that these hostile policies created an exceptionally adverse social environment for the Chinese population in South Vietnam during the period spanning from August 1956 to the assassination of President Diem in November 1963 (Ungar 1987). Employing a generalized difference-in-differences (DiD) research design, I compare the difference in the outcomes of interest between individuals who were and those who were not exposed to hostile policies *in utero* among the Chinese ethnicity (the treatment group) to the corresponding difference among other ethnicities (the comparison group). To produce precise estimates of the effects of interest, conditional on ethnicity and birth time fixed effects, I utilize restricted large-scale data from Vietnam's Population and Housing Census of 1999 and 2009. To validate my empirical identification of the causality of interest, I provide evidence of strong balances in predetermined characteristics, absence of pre-trends, and no fertility selection, among other considerations.<sup>3</sup>

I present several sets of findings for the long-lasting consequences of early life exposure to hostile policies. *In utero* exposure to these policies

<sup>2</sup> In addition to the factors proposed by the fetal origins hypothesis, other elements such as economic hardship, institutional discrimination, and the erosion of social capital likely play significant roles in shaping the adverse social environment during pregnancy, which can have long-term effects on child development. Economic hardship during pregnancy increases maternal stress, restricts access to prenatal care, and leads to poor nutrition, all of which impair fetal development and raise the risk of low birth weight, preterm birth, and long-term health issues (Kim et al. 2014; Miller 2017). Institutional discrimination further limits access to quality prenatal care, especially for marginalized groups, exacerbating maternal stress and health risks (Gajate-Garrido 2013; Mendez et al. 2014; van Daalen et al. 2022). The erosion of social capital—stemming from social isolation or lack of support networks—intensifies these issues, worsening maternal health and increasing the likelihood of developmental delays in children (Carter and Maluccio 2003; Scholmerich et al. 2014).

<sup>3</sup> See subsection 4.3 for details.

results in significant negative effects on the long-term achievements of directly affected individuals. Exposure particularly reduces overall human capital by 0.35 schooling years (−5.7 %) and decreases the likelihood of completing primary and post-primary education by 4 and 4.7 percentage points (−7% and −15 %), respectively. Labor market outcomes are also adversely affected, with a 1 percentage point reduction in the probability of working (−1.5 %) and a 4.2-percentage point decline in economic activity outside the agricultural sector (−5.2 %). Family formation is impacted, evidenced by a 3-percentage point decrease in the likelihood of being married (−3.9 %). Women's fertility increases both at the extensive and intensive margins, albeit modestly, with a 0.7-percentage point rise in the probability of having at least one child (0.8 %) and an increase of 0.3 children in completed fertility (7.2 %). Treated individuals also live in more crowded households, with an average of 0.34 more persons than untreated individuals (6 %). Exposure to hostile policies during pregnancy further diminishes household economic well-being. These effects include a 3.2-percentage point reduction in the likelihood of living in solid housing (−5.8 %), a 3.7-percentage point increase in the use of old cooking energy sources (16.7 %), a 1.6-percentage point decline in access to improved sanitation (−1.8 %), and a reduction in household wealth by 0.11 assets (−2.4 %).

In addition, early life exposure to hostile policies reduces the next generation's human capital and hinders social mobility across generations. The intergenerational spillovers are generally evident only for maternal exposure, not paternal exposure. Children aged 11–17 born to treated mothers have a 0.3-percentage point lower probability of school enrollment (−0.3 %) and a 1.5-percentage point lower probability of literacy (−1.7 %) compared to peers born to untreated mothers. Although the effect sizes are relatively small, these findings indicate the spillover of early life disadvantages across generations. Furthermore, *in utero* exposure to hostile policies increases the intergenerational persistence of education among the directly affected individuals. Interestingly, while exposure to hostile policies negatively affects both males and females, the effects were significantly more pronounced among males for almost all outcomes.

My paper contributes to three strands of literature. First, it adds to the body of research studying the impacts of early-life conditions on long-term outcomes. While previous studies predominantly focused on profoundly devastating events such as wars and armed conflicts (Schreier et al. 2011; Lee 2014; Akresh et al. 2023), pandemics, terror attacks, and natural disasters (Almond 2006; Camacho 2008; Fuller 2014; Li and Menon 2022; Noghanibehambari 2022), my paper examines hostile policies, which may have less immediate severity but whose long-term costs are not fully understood (Currie et al. 2022). Importantly, my paper extends the scope of the consequences beyond affected individuals by looking at the next generation, which have been relatively under-studied in the literature on the consequences of *in utero* exposure to adverse shocks (Cook et al. 2019; Brown 2020; Phadera 2021; Akresh et al. 2023; Doyle and Jernström, 2023), providing a comprehensive analysis of the multigenerational impacts of early-life exposure to hostile policies.

Second, my paper addresses a central question in the study of the causal determinants of inequalities related to ethnic, racial, and immigration backgrounds. My paper suggests government-hostile policies targeting ethnic or racial minority groups as an additional explanation. The relationship between *in utero* exposure to hostile policies and inequalities is posited to operate through the human capital formation of directly affected individuals. Human capital formation has been established as an important driver of long-term and intergenerational socioeconomic successes (Björklund and Salvanes, 2011; Black and Devereux 2011; Black et al. 2005; Duflo 2001; Oreopoulos and Salvanes 2011), but it can be negatively impacted by *in utero* exposure to adverse shocks (Akresh et al. 2012; Akresh et al. 2023; Leon 2012; Karbownik and Wray 2019; Maccini and Yang 2009).

Third, my paper contributes to the growing literature on the causal determinants of intergenerational mobility by exploring the role of

prenatal shocks in driving intergenerational mobility (O'Brien et al. 2018; Torche and Nobles 2024; Voit et al. 2024). My paper demonstrates the detrimental effect of exposure to hostile policies during pregnancy on social mobility across generations, thereby exacerbating inequality of opportunity among minorities.

## 2. Historical background

### 2.1. The Chinese ethnicity in South Vietnam

The Chinese community in South Vietnam was primarily established through several major immigration waves during the late 17th century (Share 1994) and the late 19th to early 20th centuries (An 1967; Feng 2017). By the mid-1950s, they constituted the largest minority group, numbering approximately one million individuals (An 1967; Marsot 1993). During the colonial period, the French relied on Chinese residents to build their colonial bureaucracy and economic activities (Share 1994; Engelbert 2008), reinforcing the Chinese people's economic dominance in South Vietnam (Schrock 1966; Engelbert 2008; Barton 1977). By the establishment of the RVN in 1955,<sup>4</sup> the Chinese controlled crucial economic sectors while overseeing a major share of the state's imports (Stern 1985).

Chinese residents in South Vietnam organized their lives within autonomous congregations (Khanh 1993; Engelbert 2008) and occupational associations (Engelbert 2008), establishing economic and social autonomy. This likely contributed to social isolation from Vietnamese natives while they maintained significant economic and political influence (Ungar 1987; Share 1994), potentially marginalizing Vietnamese natives and contributing to ethnic tensions (Khanh 1993; Share 1994).

### 2.2. Ngo Dinh Diem's hostile policies towards the Chinese ethnicity

President Ngo Dinh Diem, who was considered a strong Vietnamese nationalist, introduced the hostile *Chinh sach Hoa Kieu* policies in August 1956 (Share 1994). These policies aimed to weaken the economic influence of the Chinese community and force their cultural and social assimilation through coercive measures.

Diem's policies targeted four areas: citizenship, social and work organization, economic activities, and education (Share 1994). First, Ordinance Number 48 (August 1956) granted automatic Vietnamese citizenship to all Chinese-born individuals in the RVN, reclassifying them as Chinese Vietnamese (*ngươi Viet goc Hoa*). Non-citizens had to return to the PRC or Taiwan or face economic exclusion. Additionally, Chinese residents were required to adopt Vietnamese names under the threat of punitive taxation. Second, in June 1960, Diem abolished Chinese congregations and occupational associations, seizing their assets and requiring Chinese residents to follow RVN laws like other ethnic groups. Chinese males were also mandated to complete military service. Third, economic policies sought to shift resources from the Chinese community to Vietnamese natives. Ordinance Number 53 (September 1956) banned Chinese residents from 11 key industries, including retail, rice milling, and transportation (Stern 1985; Schrock 1966). Many Chinese businesses were forced to close or transfer ownership, and labor restrictions limited their employment in fields like medicine (Stern 1985). All households and firms had to pay taxes directly to the RVN government. Fourth, Chinese-managed schools were nationalized, with Vietnamese becoming the primary language of instruction (Fitzgerald 1970). Schools required Vietnamese principals, and the curriculum emphasized national history and culture. Admission to secondary education depended on a Vietnamese literature exam, while Chinese

<sup>4</sup> The 1954 Geneva Accords temporarily partitioned Vietnam into two states with differing political systems until its reunification in 1975. The RVN in the South was initially led by Ngo Dinh Diem, who emphasized high independence for Vietnam (Ungar 1987).

language instruction was reduced. Despite these changes, Chinese children's enrollment remained stable (Fitzgerald 1970).

It is important to note that the timings of Diem's policies were arguably shaped by exogenous changes in contemporaneous political conditions. The unexpected 1955 shift in the People's Republic of China (PRC) citizenship policy, abandoning 'citizenship by bloodline' for overseas Chinese, significantly influenced this starting point (Han 2009). This removed prior PRC protection (Mitchison 1961; Suryadinata 2017), bolstering Diem's confidence to target Chinese people in South Vietnam (Ungar 1987; Fredman 2014). The endpoint was largely determined by Diem's unexpected assassination in November 1963, disrupting and eventually fading these policies under subsequent administrations. The unpredictable nature of these events allows us to consider exposure to these policies during pregnancy as randomly assigned across birth cohorts (see subsection 4.1 for details).

Facing Diem's policies, Chinese residents mounted strong opposition through protests and boycotts, risking economic disruption (Ungar 1987; Amer 2011), which was met with harsh police measures and violent clashes. To regain control, Diem eased restrictions such as permitting up to 49% Chinese ownership in businesses (Ky 1963). Although Chinese residents largely preserved their wealth, they faced social turmoil between 1956 and 1963 (Ungar 1987). Diem's policies created severe hardships, leaving them uncertain about their future (Share 1994). After Ngo Dinh Diem's death in 1963, his anti-Chinese policies were not enforced by subsequent governments, improving the social environment for the Chinese in South Vietnam (Stern 1985).

## 3. Data

### 3.1. Data sources and sample selection

My 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 micro datasets available in Vietnam. The data contains information about demographic traits, education, employment, mortality, wealth, and housing of the respondents across the country's regions. The 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 researchers 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.

To establish the analytical sample for the first generation, I restrict the individuals to those born between May 1951 and July 1962 in South Vietnam, which consists of Chinese ethnic individuals as a treatment group and individuals of other ethnicities as a comparison group. For the sample of the next generation, I limit the individuals to the children (aged between 11 and 17) who were born to those of the first generation's sample who were born between May 1951 and July 1962.

### 3.2. Construction of variables and summary statistics

I exploit various information to create the variables of interest. Depending on the information availability, a specific variable is relied on by one or both censuses. Summary statistics of these outcome variables for the total sample and the sub-samples of Chinese and non-

**Table 1**  
Summary statistics.

		All			Chinese			Non-Chinese		
		Obs	Mean	SD	Obs	Mean	SD	Obs	Mean	SD
<b>Panel A. First generation (Individuals born in May 1951-July 1962)</b>										
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	0.677	0.467
Post-primary school (dummy)	Census (1999, 2009)	1,989,296	0.328	0.470	44,075	0.279	0.448	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
Working (dummy)	Census (1999)	1,652,173	0.806	0.395	39,579	0.700	0.458	1,612,594	0.809	0.393
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	2,361,969	0.869	0.338
Women's having a child (dummy)	Census (1999)	875,741	0.917	0.276	21,188	0.904	0.296	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, 2009)	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
<b>Panel B. Children's generation (children aged 11–17 of parents born in May 1951-July 1962)</b>										
<i>Children of fathers</i>										
School enrollment	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	Census (1999, 2009)	408,704	0.884	0.320	5,282	0.911	0.284	403,422	0.884	0.321
<i>Children of mothers</i>										
School enrollment (dummy)	Census (1999, 2009)	1,212,893	0.966	0.182	17,116	0.975	0.155	1,195,777	0.966	0.183
Literacy skills (dummy)	Census (1999, 2009)	409,901	0.885	0.320	5,183	0.894	0.308	404,718	0.885	0.321

Chinese observations are presented in [Table 1](#).<sup>5</sup>

I first use the information about individuals' year and month of birth to define 9-month birth groups to identify the groups that were exposed to the policies *in utero*. I further extract information on ethnicity to construct dummies for various ethnic groups. Based on the sizes of the ethnicities from the census samples, I group all ethnicities into 23 ethnicity groups, in which each major ethnicity (including the Kinh, and the Chinese among others) is classified as one separate group, and all remaining very tiny ethnicities as one group.<sup>6</sup> I then construct a set of outcomes for both the first generation that was directly exposed to hostile policies and their children.

**Human capital.** I focus on educational success as a measure of individuals' human capital. I extract the information about individuals' highest levels of formal education from both censuses to construct several schooling outcomes. *Schooling years* is measured as the number of completed years of schooling. In addition, I construct three indicators for the completion of key educational levels: primary school, post-primary school, and college. *Primary school* is a dummy taking a value of 1 if one person completed primary education, and 0 otherwise. *Post-primary school* is a dummy taking a value of 1 if one person completed any level of education beyond primary school including lower secondary school, upper secondary school, and tertiary education, and 0 otherwise. *College* is a dummy taking a value of 1 if one person has a college degree, and 0 otherwise.

**Labor market and family.** I extract information on employment and

family to construct a range of labor market and family outcomes. The variables for the labor market outcomes include indicators for working and non-agricultural work. *Working* is a dummy taking a value of 1 if one person has a paid job, and 0 otherwise. *Non-agricultural work* is a dummy taking a value of 1 if one individual has a paid job outside the agriculture industry, and 0 otherwise. The variables for family outcomes include indicators for being married, women's fertility, and household size. *Married* is a dummy taking a value of 1 if one person is married, and 0 otherwise. *Women's one child* is a dummy taking a value of 1 if a woman has at least one child, and 0 otherwise. *Women's fertility* is measured as the total number of children one female has. *Household size* is measured by the total number of family members who live within the same household.

**Family living standards.** I construct some proxy outcomes for family living standards which convey the information about households' living conditions: solid house, old cooking energy, improved sanitation, and asset index. *Solid house* is an indicator that takes a value of 1 if a home has a solid structure that is made from masonry units and reinforced concrete, or a value of 0 if the house is not solid or if the home is constructed from wood and wood materials. *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 energy. *Improved sanitation* is an indicator that takes a value of 1 if the household has one of several types of an improved toilet<sup>7</sup> or a value of 0 if the household has an unimproved toilet.<sup>8</sup> Finally, *asset index* is defined as the sum of 8 durable assets that are the most valuable in a

<sup>5</sup> To maximize the number of observations, the sample size is determined based on the specific outcome of interest. For each outcome, I include all individuals with non-missing data and exclude those with missing values. As a result, the number of observations varies across the different outcome groups.

<sup>6</sup> These 23 ethnicity groups include Kinh, Tay, Thai, Kho-me, Hoa (Chinese), 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.

<sup>7</sup> 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.

<sup>8</sup> 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 latrines and no facilities.

typical Vietnamese household. The list of such assets particularly consists of television, radio, telephone, computer, washing machine, refrigerator, air conditioner, and motorcycle.

**Children’s human capital.** I exploit information on educational attainment and literacy from both censuses to construct two proxy variables for the human capital of the children’s generation: school enrollment and literacy skills. *School enrollment* is a dummy taking a value of 1 if the child is currently enrolled in school, and 0 otherwise. *Literacy skills* is a dummy taking a value of 1 if the child has a full ability to write and read, and 0 otherwise.

**Control variables.** I construct a set of variables capturing individual characteristics which are included in regressions. These variables consist of age, age squared, and indicators for individuals’ gender (male) and religious affiliations (Buddhism, other religions, and no religious affiliation). I also include in the set of control variables indicators for residing in rural versus urban areas.

#### 4. Methodology

##### 4.1. Definition of treatment and exposure groups

My empirical strategy relies on variation in *in utero* exposure to hostile policies among different ethnicity groups as well as across groups of individuals with various birth times.<sup>9</sup> Because the Chinese were the only ethnicity receiving the treatment, I use them as the treatment group, and I use other ethnicities as the comparison group.

To provide preliminary evidence on the potential consequences of hostile policies toward individuals of Chinese ethnicity, I present a series of graphs in Supplementary Appendix Fig. A.1–Fig. A.3, which track key outcomes across birth cohorts using raw census data. These figures allow visual inspection of trends before and after the implementation of discriminatory policies, focusing on individuals born between 1951 and 1962. A consistent pattern emerges across all outcomes—schooling (Fig. A.1), labor market and family formation (Fig. A.2), and family living standards (Fig. A.3). For cohorts born between 1951 and 1955—who were unaffected by hostile policies—outcome trends between Chinese and non-Chinese individuals are highly parallel. However, starting from the 1956 birth cohort, corresponding to increasing exposure to discriminatory policies, the outcomes for Chinese individuals begin to diverge from the trends of their non-Chinese counterparts. In many cases, this divergence reflects a worsening relative position for Chinese cohorts, suggesting policy-driven disadvantages. For example, post-1955 Chinese cohorts show slower educational advancement, declining labor force participation, and deteriorating housing and asset conditions, while non-Chinese cohorts steadily progressed. These patterns not only confirm parallel trends in the pre-reform period but also document a clear shift in outcomes among Chinese cohorts following policy exposure, providing suggestive evidence of the adverse effects of discriminatory policies.

To identify those who were *in utero* exposed to hostile policies, I create nine-month birth groups. I treat those who were born in July 1956 or before as the no-exposure birth groups because they were born before the commencement of the policies in August 1956. Among those who were exposed to hostile policies *in utero*, I 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 policies for 9 months as the full-exposure birth group.

I restrict the analytical sample to those born between May 1951 and July 1962. Fig. 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  $t$ . I use individuals born between May 1951

<sup>9</sup> 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; Akresh et al. 2023).

and July 1956 (birth time  $t = -7, \dots, -1$ ) as the no-exposure birth time groups. I 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 birth time groups with full exposure ( $t = 1, \dots, 7$ ).

##### 4.2. Estimation methods

To estimate the effects of *in utero* exposure to hostile policies, I perform a generalized DiD regression which draws comparisons across ethnicities and birth groups, controlling for ethnicity and birth time fixed effects. The specifications respectively used for estimating the effects on the first generation and the children’s generation are as below.

**Effects on the first generation.** I 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  $s$ :

$$Y_{ietps} = \alpha_0 + \alpha_1 \text{Chinese}_{ie} \times \text{PartialExposure}_{it} + \alpha_2 \text{Chinese}_{ie} \times \text{FullExposure}_{it} + \gamma \mathbf{X}' + \varphi_e + \omega_t + \pi_p + \theta_s + \epsilon_{ietps} \quad (1)$$

where  $\text{Chinese}_{ie}$  is a dummy variable that indicates that ethnicity  $e$  of individual  $i$  is Chinese.  $\text{PartialExposure}_{it}$  is a dummy variable that indicates that individual  $i$  belongs to the partial-exposure birth time group ( $t = 0$ ).  $\text{FullExposure}_{it}$  is a dummy variable that indicates that individual  $i$  belongs to the full-exposure birth time group ( $t = 1, \dots, 7$ ).  $\mathbf{X}'$  is a control vector for individual characteristics, such as dummies for male gender and religious affiliations (Buddhism, other religions, and no religion). The terms  $\varphi_e$ ,  $\omega_t$ ,  $\pi_p$  and  $\theta_s$  are the fixed effects of ethnicity, birth time, province and survey year.  $\epsilon_{ietps}$  is the error term. The ethnicity fixed effect  $\varphi_e$  controls for time-invariant ethnic characteristics, such as culture, way of life, and other socio-economic and demographic traits. The birth time fixed effect  $\omega_t$  controls for birth-group trends in the outcome of interest. The inclusion of the province fixed effect  $\pi_p$  allows us to control for potential differences in provincial characteristics. The survey-year fixed effect  $\theta_s$  controls for potential time trends across survey years, and it is dropped in specifications that use datasets that consist of a single survey wave.

**Effects on the children’s generation.** To explore how *in utero* exposure to the hostile policies affects the next generation, I 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  $s$ :

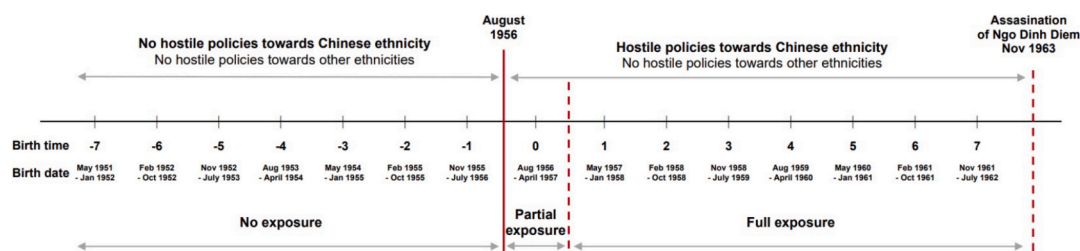
$$Y_{jkeps} = \beta_0 + \beta_1 \text{Chinese}_e^k \times \text{PartialExposure}_t^k + \beta_2 \text{Chinese}_e^k \times \text{FullExposure}_t^k + \rho \mathbf{X}'_t + \varphi_e^k + \omega_t^k + \pi_p + \theta_s + \epsilon_{jkeps} \quad (2)$$

where  $Y_{jkeps}$  is an outcome for child  $j$ .  $\mathbf{X}'$  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, other religions, and no religion).  $\epsilon_{jkeps}$  is the error term. The definitions of the other terms are similar to those used in Eq. (2).

I cluster standard errors at the ethnicity level in both regression equations. The joint inclusion of ethnicity and birth time fixed effects turns both equations into a generalized DiD regression. The parameters of interest are  $\alpha_2$  and  $\beta_2$ , which indicate the causal effects of *in utero* full exposure to hostile policies on the first and second generations, respectively. In addition to reporting these coefficients, I also show the coefficients  $\alpha_1$  and  $\beta_1$  for the baseline results to know whether partially exposed to hostile policies affects children in the long run.

##### 4.3. Validity of empirical identification

There are several important concerns related to diverging pre-trends, the exogeneity of ethnicity-by-birth time variation in the treatment exposure, and fertility selection among others, which may be potential



**Fig. 1.** Timeline of hostile policies and *in-utero* exposure by birth groups. *Notes:* This figure shows the timeline for the starting and endpoint of Ngo Dinh Diem’s hostile policies towards the Chinese ethnicity in South Vietnam and exposure during pregnancy by birth groups.

threats to my interpretation of  $\alpha_2$  and  $\beta_2$  as causal effects. In this section, I conduct several tests to validate my empirical identification.

First, I 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 aggregate dataset at the ethnicity-by-birth time level that is constructed from the 1999 and 2009 censuses, I regress the likelihood of being fully exposed to the hostile policies during pregnancy on specific observables, conditional on ethnicity, birth time, province, and survey-year fixed effects. I focus on observable characteristics that are unlikely to have been driven by *in utero* exposure to hostile policies. The estimates presented in Supplementary Appendix Table A.1, columns (1)-(3) indicate 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)); *p*-value of 0.479 for joint-significance). I 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)); *p*-value for a joint significance is 0.755). These results allow us to establish that my identification of ethnicity-by-birth time variations in exposure to Diem’s hostile policies is unrelated to trends in the observables.

Second, I conduct an event-study analysis to check for diverging pre-trends for all outcomes. The regression equation is now a modified version of Eq. (1), in which I replace the interaction terms *Chinese* × *PartialExposure* and *Chinese* × *FullExposure* with a set of interaction terms *Chinese* × *BirthTime*, where *BirthTime* is a dummy for birth time *t* (*t* = − 7, ..., 7). I omit the partial-exposure birth time of *t* = 0 and treat it as a reference group. The other terms in my event-study regressions are similar to those in Eq. (1). In Supplementary Appendix Fig. A.4–Fig. A.7, I plot the point estimates of *Chinese* × *BirthTime* from the event-study regressions respectively for the groups of outcomes: schooling, labor market and family outcomes, family living standards, and children’s human capital. Across all outcomes, there are relatively small differences between treatment and comparison groups across the non-exposure birth groups (*t* = − 7, ..., 1). The coefficients for these non-exposed birth groups are close to 0 and importantly pre-trend differences are mostly statistically insignificant at the 10 % level. The pattern in these graphs supports my approach regarding there is no evidence of systematic differences in pre-trends of the outcomes I have examined.

Third, I conduct a check for a possible sample selection bias which may stem from fertility selection. One can argue that the hostile policies might affect childbearing decisions in the years of policy implementation leading to potential bias in my estimates. To check if there is such selection in my analytical sample, I regress the log of the number of people at the birth time-by-ethnicity level of observations on the *Chinese* × *FullExposure* interaction and other terms such as fixed effects for ethnicities, birth time groups, provinces and survey years. The results presented in Supplementary Appendix Table A.2 indicate that there is a small reduction in the log of the number of people by 2.8–5.8 log points among the treated group relative to the untreated one. However, as

these estimates are all statistically insignificant, I eliminate a potential bias source from selection into childbearing decisions.

Fourth, one concern for the intergenerational effect of parental exposure to the hostile policies during pregnancy on children’s outcomes is that it may be driven by differences in children’s ages. If Diem’s hostile policies made parents to delay or to speed up their childbearing decisions, the children born to treated versus untreated parents would have a different age measured in my data which may drive my results because children’s human capital outcomes may vary by their age. The estimates in Supplementary Appendix Table A.3 allow me to rule out this possibility by showing no effect of parental exposure on children’s age for both paternal exposure (column (1)) and maternal exposure (column (2)).

Fifth, I conduct an additional test for whether the effects on my outcomes of interest are also existent for other ethnicities rather than Chinese. This test aims to examine a concern that the policies may also affect other minor ethnicities rather than Chinese. I specifically regress the outcomes of interest on 22 ethnicity dummies interacted with *Full-Exposure* and other terms as used in Eq. (1). The interactive term for the Kinh ethnicity (*Kinh* × *FullExposure*) is omitted and used as the reference group. The coefficient for each minor ethnicity allows us to detect if there was favoritism or discrimination towards that ethnicity by the policies. The estimates reported in subsection 6.1 demonstrate that while I find strongly robust estimates relative to the baseline estimates for the treated Chinese ethnicity, I do not find systematic effects of the policies on all other minor ethnicities. Although there are some estimates being statistically significant for several ethnic groups outside the Chinese ethnicity, the corresponding effects are relatively small in terms of effect size in comparison with those on the Chinese ethnicity, showing less economic meaning.

## 5. Results

### 5.1. 13 Direct effects on the first generation

I start to discuss the results by showing the estimates for the effects on the directly affected generation. The estimates are based on the baseline specifications from Eq. (1) for the first generation. I focus on three sets of outcomes, including human capital, labor market and family, and family living standards.

**Human capital.** The estimates for the effects on the schooling outcomes of the first generation are presented in Table 2. I find that exposure to hostile policies during pregnancy has a negative effect on educational outcomes. These estimates are strongly statistically significant at the 1 % level for all outcomes. It particularly reduces completed schooling years by 0.35 as shown in column (1). 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 5.7 %. Columns (2)-(3) further show that exposure decreases the probability of attaining various levels of education, including 4.7 and 4 percentage points for completing primary and post-primary education, respectively. These effects are also substantial because they create an equivalent reduction of 7 % and 15 % when compared to the pre-treatment means. As shown

**Table 2**  
Effects on schooling.

	(1)	(2)	(3)	(4)
	Schooling	Primary	Post- primary	College
	years	school	school	
Chinese x Partial exposure	-0.165*	-0.025*	-0.015	-0.002
	(0.100)	(0.015)	(0.011)	(0.002)
Chinese x Full exposure	-0.346***	-0.047***	-0.040***	-0.004***
	(0.029)	(0.003)	(0.004)	(0.000)
Pre-treatment mean	6.076	0.668	0.259	0.006
% change	-5.695	-7.036	-15.444	-66.667
Observations	1,989,296	1,989,296	1,989,296	1,989,296

Notes: Specifications include fixed effects for birth time, ethnicity, province, 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 ethnicity level are in parentheses.

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

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

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 %.

Partial exposure to hostile policies during pregnancy appears to have a modest negative impact on schooling outcomes. Specifically, individuals who were partially exposed experienced a reduction of nearly 0.17 years in schooling, a statistically significant effect at the 10 % level. Partial exposure decreases the likelihood of completing primary school by

2.5 Percentage points, also significant at the 10 % level. However, there is no statistically significant effect on post-primary school attendance (-1.5 percentage points) or college attendance (-0.2 percentage points), as these estimates are not distinguishable from zero. These findings suggest that while partial exposure may have slightly hindered overall educational attainment, its impact was most pronounced in early schooling stages and did not significantly affect progression to higher education. In comparison with these effects for partial exposure, the negative effects of full exposure are notably larger and more statistically significant, suggesting that prolonged exposure to hostile policies had a much stronger impact on educational attainment.

In general, I find that *in utero* exposure to hostile policies has considerable negative effects on the human capital formation of the directly affected generation. These effects are nearly 40 % of the effects of exposure to a compulsory schooling reform in Vietnam (although the direction is opposite), which precipitated an increase of 0.9 years of overall education and around 10 percentage points in primary school completion (see Cornelissen and Dang 2022).

**Labor market and family.** The estimates in columns (1) and (2) of Table 3 show the baseline effects on labor market outcomes: the likelihood of working and the possibility of having a non-agricultural work. While the former indicates the extensive margins of labor market participation, the latter shows the quality of employment one individual has for their labor market status. This is because working in a non-agricultural sector is highly likely associated with high-skilled occupations in a non-traditional economy. The results suggest a negative impact on these labor market outcomes for being exposed to the hostile policies during pregnancy, with a reduction of 1 percentage point for the possibility of working and 4.2 percentage points for having a non-agricultural job (both statistically significant at the 1 % level). Compared with the pre-treatment mean values, these effects are respectively equivalent to a reduction of 1.5 % and 5.2 %.

The remaining columns in Table 3 present the estimates for the effects on family outcomes. The estimate in column (3) indicates that

exposure to hostile policies affects affected individuals' family formation by causing a reduction in the probability of being married of 3 percentage points (statistically significant at the 1 % level). This effect translates into a decline of 3.9 %. Columns (4)–(5) show the effects on female fertility. I find that exposure has a positive effect on fertility at both the extensive and intensive margins. The effect on the probability of having a child is at 0.7 percentage points (statistically significant at the 1 % level). However, this extensive margin effect equals a relatively small increase of only 0.8 %. At the intensive margin, *in utero* exposure increases the number of children by 0.3 (statistically significant at the 1 % level), which is equivalent to a 7.2-% increase. These findings on the positive effects of fertility can be plausibly explained by reduced educational attainment, which was shaped during individuals' school-age years. This interpretation aligns with a well-established body of literature documenting a strong negative association between education and fertility (see Handa 2000; Basu 2002; Keng and Sheu 2011). Lower levels of education are often linked to earlier family formation and higher fertility rates, suggesting that disruptions to educational pathways may play a key role in driving these fertility outcomes.

Finally, column (6) shows the effect of exposure on household size. Directly affected individuals live in larger families in terms of the number of people living within the same household by 0.34 persons, relative to those who were not exposed to hostile policies (statistically significant at the 1 % level). This effect corresponds to an increase of 6 %. An increase in household size is likely a consequence of lower private housing affordability due to worsened economic status and/or reduced geographical mobility for work or education. Also, it is reasonable to establish a hypothesis that an increase in women's fertility could contribute to a larger household size.

Partial exposure to hostile policies during pregnancy had mixed effects on labor market and family outcomes. It slightly reduces the probability of working by 0.3 percentage points, significant at the 10 % level, and lowers the likelihood of being married by 1.4 percentage points, also significant at the 10 % level. However, it has no significant effect on non-agricultural employment. In terms of family outcomes, partial exposure slightly increases the probability of having a child by 0.2 percentage points and fertility by 0.1 children, both statistically significant. The effect on household size was positive (0.13) but not statistically significant. Overall, these findings suggest that partial exposure has a modest negative impact on labor market participation and marriage while slightly increasing fertility, though its effects are smaller and less significant than those of full exposure.

**Family living standards.** Table 4 reports the estimates for the effects on family living standards. Individuals who were fully exposed to the hostile policies *in utero* generally experience poorer living conditions than those who were not exposed. The estimates are all statistically significant at the 1 % level. Column (1) shows that exposed individuals are less likely to live in a solid house by 3.2 percentage points. This effect is equivalent to a 5.8-% reduction in high-quality housing.

Column (2) shows that households of those directly exposed to the hostile policies *in utero* have a higher likelihood of using an old form of cooking energy than untreated households by 3.7 percentage points (corresponding to an increase of 16.7 %). An increase in using traditional cooking energy may be linked to an increase in exposure to indoor air pollution which likely affects the health of family members among treated households (Duflo et al. 2008; Ezzati and Kammen 2002; Pillarisetti et al. 2022).

Furthermore, the probability that treated individuals' households use improved sanitation is also lower than that of untreated individuals by 1.6 percentage points (equivalent to a decrease of 1.8 %) as shown in column (3). Finally, the estimate in column (4) shows that fully exposed individuals are poorer than those who were not exposed: a negative effect on the asset index with a reduction of 0.11 assets (equivalent to a decline of 2.4 %). These findings indicate a harmful effect of *in utero* exposure to hostile policies on the household welfare of directly affected individuals during adulthood.

**Table 3**  
Effects on labor market and family outcomes.

	(1) Working	(2) Non-agri. work	(3) Married	(4) Women's a child	(5) Women's fertility	(6) Household size
Chinese x Partial exposure	-0.003* (0.001)	-0.031 (0.020)	-0.014* (0.008)	0.002** (0.001)	0.104* (0.063)	0.129 (0.080)
Chinese x Full exposure	-0.010*** (0.001)	-0.042*** (0.002)	-0.030*** (0.001)	0.007*** (0.001)	0.180*** (0.002)	0.335*** (0.005)
Pre-treatment mean	0.674	0.807	0.765	0.893	2.504	5.574
% change	-1.484	-5.204	-3.922	0.784	7.188	6.010
Observations	1,652,173	1,325,405	2,414,083	875,741	875,741	1,652,224

Notes: Specifications include fixed effects for birth time, ethnicity, province, 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 ethnicity level are in parentheses.

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

Data sources: All estimates are produced using Census (1999) for columns 1–2 and 4–6, and Census (1999, 2009) for column 3.

**Table 4**  
Effects on family living standards.

	(1) Solid house	(2) Old cooking energy	(3) Improved sanitation	(4) Asset index
Chinese x Partial exposure	-0.017 (0.015)	0.051 (0.033)	-0.012* (0.007)	-0.134 (0.082)
Chinese x Full exposure	-0.032*** (0.000)	0.037*** (0.002)	-0.016*** (0.001)	-0.112*** (0.003)
Pre-treatment mean	0.551	0.221	0.895	4.607
% change	-5.808	16.742	-1.788	-2.385
Observations	762,139	762,352	762,352	762,352

Notes: Specifications include fixed effects for birth time, ethnicity, and province. 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 ethnicity level are in parentheses.

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

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

Partial exposure to hostile policies during pregnancy has minimal effects on family living standards. It slightly reduces the probability of living in a solid house (1.7 percentage points) and access to improved sanitation (1.2 percentage points), though only the latter is statistically significant at the 10 % level. It has no significant effect on the use of old cooking energy (5.1 percentage points) or the asset index (-0.134). In contrast, full exposure has significantly larger negative effects on all outcomes, indicating a stronger deterioration in living standards.

## 5.2. Spillover effects on the next generation

One important question is whether *in utero* exposure to hostile policies causes spillover effects on the children's generation. To answer this question, I use the 1999 and 2009 censuses to construct a sample of children aged between 11 and 17 who were born to those directly affected by hostile policies of the first generation's sample (born between May 1951 and July 1962). I focus on *school enrollment* and *literacy skills* as two primary 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 to a large extent indicative of educational quality among children aged from 11 who typically complete their primary education and are expected to have full abilities to read and write in Vietnam.

**Table 5**  
Effects on children's human capital.

	(1) Father's exposure School enrollment	(2) Mother's exposure Literacy skills	(3) School enrollment	(4) Literacy skills
Chinese x Partial exposure	0.005 (0.004)	-0.002 (0.002)	-0.002** (0.001)	-0.016 (0.010)
Chinese x Full exposure	-0.001 (0.001)	-0.003 (0.008)	-0.003*** (0.001)	-0.015*** (0.005)
Pre-treatment mean	0.977	0.897	0.976	0.888
% change	-0.102	-0.334	-0.307	-1.689
Observations	1,121,277	408,704	1,212,893	409,901

Notes: Specifications include fixed effects for birth time, ethnicity, province, 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 ethnicity level are in parentheses.

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

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

The baseline estimates, which were produced separately for paternal and maternal exposure using Eq. (2), are presented in Table 5.<sup>10</sup> I find that *in utero* exposure to hostile policies adversely affects children's human capital for maternal exposure but not for paternal exposure. The estimates in columns (3) and (4) document the negative effects of maternal exposure on children's human capital with a 0.3-percentage point decline in school enrollment (statistically significant at the 1 % level) and a 1.5-percentage point decline in literacy skills (statistically significant at the 5 % level). These effects however relatively small with the corresponding reductions of 0.3 % and 1.7 % in school enrollment and literacy skills, respectively, relative to the pre-treatment mean values.

Partial exposure to hostile policies during pregnancy has minimal effects on children's human capital. It has no significant impact on school enrollment or literacy skills when considering fathers' exposure. However, for mothers' exposure, partial exposure slightly reduces the probability of school enrollment by 0.2 percentage points, a statistically significant effect at the 5 % level, while it has no significant impact on literacy skills. These findings suggest that partial exposure has less pronounced negative effects relative to full exposure as described above.

The small effect sizes of the intergenerational effects are expected.

<sup>10</sup> The fathers and mothers to whom this sub-section refers are the individuals of the first generation who were directly exposed to hostile policies.

One possible explanation for this is that children living in the current society benefit from improved social environments, such as exposure to contemporary schooling reforms, which may buffer them from the long-term effects of their parents' exposure to hostile policies. These modern interventions likely play a significant role in mitigating the transmission of disadvantages across generations (Cheng et al. 2016).

## 6. Robustness and heterogeneity

### 6.1. Robustness

In this section, I provide the results for some robustness checks to further validate my baseline estimates as shown in section 5. In these checks, I make some changes in the specification or estimate a subsample to observe how the baseline estimates change. First, I exclude province-fixed effects from the baseline specification to check whether the baseline effects are sensitive to controls for geographic location at the provincial level. The estimates for this robustness check presented in Supplementary Appendix Table A.4, Table A.5 and Table A.6 for the first generation's outcomes, and Table A.7 for the human capital of the children's generation are in line with the corresponding baseline estimates.

Second, I exclude the observations from Ho Chi Minh City, which is the most developed city and the home of 50 % of the Chinese population in South Vietnam to ascertain whether the baseline effects are driven by the spatial concentration of the Chinese ethnicity in Ho Chi Minh City. The results are presented in Supplementary Appendix Table A.8, Table A.9 and Table A.10 for the first generation's outcomes, and Table A.11 for the human capital of the children's generation. Although the estimates are to some extent larger than the baseline estimates, the direction and levels of statistical significance for the effects of these estimates remain the same as the baseline estimates.

Third, I add to the estimation specification dummies for other ethnic groups interacted with an indicator of full exposure to the hostile policies to check whether the effects are also existent among other ethnic groups rather than only the Chinese ethnicity. The coefficients for the parameters of interest are presented in Supplementary Appendix Table A.12, Table A.13 and Table A.14 for the first generation's outcomes, and Table A.15 for the human capital of the children's generation. The results show that the effects of exposure to hostile policies on the Chinese ethnicity exist. The coefficients remain statistically significant at standard levels and the effect sizes are highly similar to the baseline estimates. Although some of the coefficients for other ethnicity groups are statistically significant, the magnitudes are relatively small giving trivial economic meaning.

## 7. Heterogeneity by gender

This section presents the results for effect heterogeneity by gender, which is produced by estimating an augmented version of the baseline specification of Eq. (1). In the specification, I particularly include additional interaction effects with genders to generate effect components for males and females.

**Human capital.** The point estimates in the first two rows in Supplementary Appendix Table A.16 demonstrate detrimental effects on schooling outcomes for both genders. All the estimates are strongly statistically significant at the 1 % level. However, the effects among males are considerably larger than those among females. For schooling years (column (1)), the effect on males is greater than that on females by a factor of 2.22. For the probability of completing a level of education, the male–female factor is 1.38 for primary school (column (2)), 3.1 for post-primary school (column (3)), and 2.5 for a college degree (column (4)).

The third row of coefficients in Table A.16 reports the male–female differences in the effects. Accordingly, I reject the hypothesis that the differences in these effects on males versus females for all schooling

outcomes are equal to zeros (statistically significant at the 1 % level). The fourth row of coefficients in Table A.16 shows that the (unconditional) gaps in no-exposure birth time groups are skewed in favor of males for all outcomes. Males have considerably higher education than females with a gap of 1.5 years of schooling, and 10–18.5 percentage points for completing (post-)primary school and college. The estimates in these rows therefore suggest that *in utero* exposure to hostile policies would result in significant reductions in the gender gaps in education by for example 18 % for schooling years, 8–25 % for (post-)primary education, and 30 % for a college degree.

**Labor market and family.** Supplementary Appendix Table A.17 reports the gender heterogeneity of the effects 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, similar to the overall baseline effects presented in Table 3. The estimates are all statistically significant at the 1 % level.

Overall, the point estimates for males are to some extent larger than those for females. The male–female ratios of these effects are 1.43 for working, 1.1 for non-agricultural work and married, and 1.22 for household size. Given the estimates in the third row of Table A.17, I can reject the hypothesis that the effects on these outcomes are equal for both genders.

**Family living standards.** Supplementary Appendix Table A.18 presents the results for 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).

Again, the effects are statistically different between males and females. The male–female ratios are 1.32, 0.87, 1.07, and 0.78 respectively for solid house (column (1)), old cooking energy (column (2)), improved sanitation (column (3)), and asset index (column (4)).

**Children's human capital.** The estimates for the effects on children's human capital by child's gender are reported in Supplementary Appendix Table A.19. I find that the effects are statistically significant for sons but not for daughters. Father's exposure reduces son's school enrollment by 0.2 percentage points and mother's exposure reduces son's school enrollment and literacy skills by 0.4 and 1.6 percentage points, respectively.

These point estimates suggest that sons tend to fare slightly worse than daughters when their parents are exposed to hostile policies during pregnancy. However, due to the coefficients for the son–daughter effect differences being not statistically significant at standard levels as shown in the third row of Table A.19, I thus cannot reject the null hypothesis of equal impacts between sons and daughters.

Son preferences in East Asian cultures, such as in Vietnam and China, may explain why the effects of *in utero* exposure to hostile policies are more pronounced for males. In these societies, sons are often prioritized due to their role in carrying on the family name and supporting parents in old age. This cultural bias leads to greater social and economic pressure on male children, making them more vulnerable to adverse effects when exposed to discriminatory policies. As sons are expected to succeed in the labor market and uphold family honor, any disruptions during their early development, such as those caused by hostile policies, can have more severe long-term impacts on their outcomes, including education and employment, compared to females, who may not face the same level of societal expectation (see Das Gupta et al. 2003; Guilimoto 2012).

### 7.1. Effect on social mobility across generations

It is very important to ask a question that whether prenatal exposure to the hostile policies exacerbates the inequality of opportunity and then drives social mobility across generations. In this section, I inquire whether exposure affected the correlation between the levels of educational attainment of the directly affected generation and their parents.

Using both censuses, I estimate an augmented version of Eq. (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 (indexed by *ParentalSchooling*) and its interactions with *Chinese* × *FullExposure*, *Chinese* × *PartialExposure*, *Chinese*, *FullExposure*, and *PartialExposure*. Other terms are similarly defined as in Eq. (1). While the coefficient for *ParentalSchooling* indicates the baseline intergenerational elasticity (IGE) of education, the coefficient for *Chinese* × *FullExposure* × *ParentalSchooling* suggests the degree to which *in utero* exposure to the hostile policies affects IGE.

Table 6 reports the coefficients for the intergenerational persistence of schooling. I provide estimates for whole samples of child-father pairs (panel A) and child-mother pairs (panel B) as well as sub-samples that are divided into child genders.<sup>11</sup> The first row of coefficients in each panel reports baseline IGE estimates. Generally, the coefficients on parental schooling in column (1) indicate a strong dependence of a child's schooling on the schooling of their father (panel A) and mother (panel B) with the IGE estimates being around 0.4 years of schooling. These IGE coefficients accord with the estimates in terms of both the size and direction reported in other studies using the same Vietnamese data (for example, see Cornelissen and Dang 2022).

The second row of the coefficients in each panel of Table 6 shows interaction effects. Overall, the estimates suggest that *in utero* exposure to the hostile policies increases the dependence of the schooling of the directly affected generation on parental schooling. The estimates are strongly statistically significant at the 1 % level. The exacerbating effects are 0.15 for the overall sample of child-father pairs (column (1), panel A) and 0.16 for child-mother pairs (column (1), panel B). The estimates from the sub-samples divided by the child gender in columns (2)-(3) also are statistically significant, and interestingly the effects of *in utero* exposure to the hostile policies on the intergenerational persistence of education are stronger among daughters than sons. Exposure increases the dependence on the father's education by 0.07 for sons and 0.23 for

**Table 6**  
Effects on intergenerational schooling persistence.

	(1)	(2)	(3)
<b>Panel A. Child-father pairs</b>			
Parental schooling	0.416*** (0.006)	0.373*** (0.004)	0.455*** (0.007)
Chinese x Full exposure x Parental schooling	0.148*** (0.003)	0.074*** (0.003)	0.231*** (0.001)
Observations	62,529	31,352	31,177
<b>Panel B. Child-mother pairs</b>			
Parental schooling	0.452*** (0.008)	0.409*** (0.006)	0.494*** (0.009)
Chinese x Full exposure x Parental schooling	0.157*** (0.002)	0.135*** (0.003)	0.205*** (0.004)
Observations	108,918	57,929	50,989

Notes: Specifications include fixed effects for birth time, ethnicity, province, 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 ethnicity level are in parentheses.

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

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

<sup>11</sup> It is important to bear in mind that the child observations 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.

daughters (columns (2)-(3), panel A), and it increases the dependence on the mother's education by 0.14 for sons and 0.21 for daughters (panel B).

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 2, the effects of such exposure on the intergenerational persistence of education are still larger for females than for males. One likely explanation is that schooling was traditionally less prevalent among Chinese girls than among Chinese boys because of strong son preference in the setting of East Asian culture in Vietnam (Haughton and Haughton 1998; den Boer and Hudson 2017). 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 schooling across generations, 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 the hostile policies increases the persistence of schooling across generations. My finding would therefore provide suggestive evidence of the negative impact of being targeted by the hostile policies *in utero* on intergenerational educational mobility.

## 8. Conclusion

Using large-scale census data from Vietnam, I have studied the long run and intergenerational effects of *in utero* exposure to hostile policies. I 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, and (post-)primary school completion and a college degree. 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; Akresh et al. 2023; 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), these effects could be acted 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. I also found that *in utero* exposure to hostile policies depresses economic activity outside of the traditional economy and lowers the likelihood of being married, increases fertility, and decreases living standards among households in the generation of being affected directly. In addition, I 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 although the effects are particularly pronounced among maternal exposure and among sons. Moreover, *in utero* exposure to hostile policies negatively affects social mobility across generations by exacerbating intergenerational persistence of schooling.

This paper does not only provide a comprehensive analysis of the multigenerational impacts of early life exposure to hostile policies but also furnishes significant relevant policy implications for reducing inequalities. 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.

While this paper offers valuable insights into the role of hostile policies in perpetuating ethnic and racial inequalities, it does have certain limitations. First, I cannot disentangle the sub-effects on the outcomes of interest which work via the specific sub-policies. The policy package directed at the Chinese ethnicity by Ngo Dinh Diem encompassed various measures such as the prohibition of Chinese education, economic constraints, and citizenship restrictions. Understanding how each of these sub-policies influences the outcomes of affected individuals is crucial. Unfortunately, the simultaneous implementation of

these sub-policies makes it challenging to isolate their individual effects from the collective impact of the entire policy package. Thus, I acknowledge the need for further research to delve into this intricate matter. This leaves the task of unraveling the distinct effects of each sub-policy as an open question, ripe for exploration in future studies. Similarly, due to data limitations, I am unable to provide evidence on the key mechanisms through which exposure to hostile policies during pregnancy affects children's long-term outcomes. Potential pathways may include maternal mental health, parental income, and economic hardship, among others. Second, I cannot analyze heterogeneity based on family background at the time individuals were exposed to Diem's hostile policies due to the unavailability of data on parental education and/or income. Third, it is important to consider that some individuals in the control group may have been indirectly exposed to hostile policies. This indirect exposure could introduce a conservative bias in the estimates. If the control group experienced any of these secondary effects—such as reduced economic opportunities or psychological stress—it could attenuate the observed differences between the treated and control groups, leading to an underestimation of the true impact of direct exposure.

### CRedit authorship contribution statement

**Thang Dang:** Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Software, Resources, Project administration, Methodology, Investigation, Funding acquisition, Formal analysis, Data curation, Conceptualization.

### Declaration of competing interest

The author declares that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

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### Appendix A. Supplementary data

Supplementary data to this article can be found online at <https://doi.org/10.1016/j.worlddev.2025.107115>.

### Data availability

The authors do not have permission to share data.

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