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Guns and Gains: Effects of Exposure to Counterinsurgency Operations During School-Age Years

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Abstract

This paper examines the effects of exposure to hard-security counterinsurgency operations during school-age years on human capital and labor market outcomes in India. We exploit the 1989 introduction of the Greyhounds—a specialized commando force created to combat Naxalite insurgents—in Andhra Pradesh, as a natural experiment. Among all states affected by Naxalite violence, only Andhra Pradesh established such a force during that period. Difference-in-differences estimates suggest that exposure to the policy during school-age years led to increased educational attainment and, in adulthood, better labor market outcomes and improved socioeconomic status. We provide suggestive evidence that a plausible mechanism underlying these effects is increased household investment in education due to reduced uncertainty stemming from improved security. Our findings highlight the economic returns to peace and stability in conflict-affected regions.

Keywords

Childhood, Conflict, Counterinsurgency, Education, Employment, India

JEL Classifications

D74, H56, F52, O12, O15

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1 Introduction

Many low- and middle-income countries (LMICs) across South Asia, sub-Saharan Africa, and Latin America have long grappled with insurgency movements that have fueled persistent, and often violent, conflicts (Tekwani, 2020; Iyer and Ghani, 2010). These conflicts, particularly those involving long-standing insurgencies, jeopardize national security, disrupt development efforts, and can have lasting impacts on individuals and communities (Mueller et al., 2024; Batra, 2012). In particular, a growing body of interdisciplinary research shows that exposure to violence and instability during childhood and adolescence can profoundly undermine individuals’ long-term outcomes by disrupting the accumulation of human capital (see, e.g., Hidalgo-Aréstegui et al. (2025); Brück et al. (2019); Singhal (2019); Islam et al. (2016); Justino (2014)).

Against this backdrop, understanding the effects of counterinsurgency policies on individual economic trajectories is crucial, as governments in the affected countries strive to balance restoring order with fostering human development and economic growth. It is usually thought that these strategies help reduce the economic costs of conflict, benefiting individuals in both the short- and long-term. However, there is growing concern that counterinsurgency operations—particularly those led by the military, paramilitary, or police forces—can be counterproductive, risking civilian casualties, alienating local populations, and contributing to the long-term destabilization of affected regions (United Nations Assistance Mission in Afghanistan, 2020; Human Rights Forum, 2013). This raises critical questions about the effectiveness of such hard-security strategies, particularly in terms of their impacts on human capital and economic well-being.

This study examines the effects of exposure to a hard-security counterinsurgency policy during school-age years on human capital formation and labor market outcomes in adulthood. We leverage the rollout of a unique counterinsurgency policy in the Indian state of Andhra Pradesh (AP) as a natural experiment. The policy entailed the formation of an elite commando force—Greyhounds—to counter Naxalite (ultra-Left or Maoist) insurgency, which stands out as the most persistent one among all insurgencies in the country. The only prior study that exploits the rollout of the Greyhounds policy is Singhal and Nilakantan (2016), which documents its short-term macroeconomic effects, particularly increases in manufacturing output. Our study complements this work by shedding light on the policy’s microeconomic consequences. More broadly, our work extends the literature on economic impacts of counterinsurgency and counterterrorism (see, e.g., Mahmood and Jetter (2023); Dell and Querubin (2018); Amara (2012); Kocher et al. (2011)). To our knowledge, this is the first study to examine the effects of exposure to hard-security counterinsurgency operations

during formative years on individual-level economic outcomes.

The Naxalite movement, which began in 1967 in West Bengal—a major state in eastern India—sought to overthrow the Indian government and establish a communist regime (Gupta, 2007; Ramana, 2009). By the 1970s, it had fractured into numerous factions and spread beyond West Bengal, with AP emerging as a major stronghold. By the late 1980s and early 1990s, a large part of AP was affected, with insurgents engaging in widespread violence and high-profile assassinations—targeting not only security forces but also civilians perceived as collaborators or oppressors. Although the movement’s intensity declined in a few regions by the late 1990s, it escalated in others. By 2007, the insurgency had spread to 182 districts across 16 states, accounting for 91% of insurgent-related violence and 89% of conflict-related deaths. The movement continues to pose serious challenges; between 2014 and 2023 alone, government sources report 7,649 incidents and 2,020 deaths linked to Naxalite violence.¹

Among the states affected by Naxalite violence, AP established the first specialized police force to combat the insurgency in 1989 (Singhal and Nilakantan, 2016). This elite commando unit was trained in counterinsurgency methods, equipped with advanced resources, and supported by an intelligence network. While quantitative evidence on the Greyhounds’ impact on insurgency levels is limited, anecdotal reports suggest that the Greyhounds’ counterinsurgency tactics, including infiltration and intelligence gathering, weakened Naxalite activities in AP especially during the late 1990s (Shapiro et al., 2017). This success prompted the Greyhounds to train police forces in other states and even in Nepal since early 2000s.

We use data from the India Human Development Survey (IHDS) 2011-12, a nationally representative dataset that provides detailed information on a broad range of socioeconomic indicators, including education, labor market outcomes, household characteristics, and geographic identifiers such as state and district of residence. Crucially for our analysis, the IHDS records how long households have lived in their current location. Using this information, we construct a subsample of individuals whose geographic location during school-age years can be reliably identified—specifically, those who belong to households that report having lived in the same place forever. This group constitutes approximately 78% of the full sample, providing a fairly large base for our analysis of exposure to the Greyhounds operations during school-age years.

To estimate the effect of the Greyhounds counterinsurgency operations, we employ a difference-in-differences (DID) strategy. Specifically, we compare educational and labor market outcomes of cohorts who were in their school-age years (5-17 years) during the policy rollout in AP with older cohorts, and contrast these patterns with those in other Naxalite-affected states that did not implement similar policies until the end of 2000. By examining

¹<https://pib.gov.in/PressReleasePage.aspx?PRID=1942471>

differences in outcomes across these cohorts and states, we isolate the intent-to-treat (ITT) effect of exposure to the Greyhounds operations during school-age years. We also estimate an alternative specification that exploits within-state variation in insurgency intensity to identify the impact of Greyhounds.

Our ITT estimates indicate that exposure to the Greyhounds operations during school-age years led to improvements in both individual human capital and labor market outcomes in adulthood. Specifically, compared to their counterparts, individuals eligible for exposure to the policy during school-age years were more likely to achieve higher educational attainment—including higher literacy, more years of schooling and greater English proficiency—and were also more likely to secure salaried employment in adulthood. In addition, these individuals worked more days and hours and had higher earnings. These benefits extended beyond the individual level: households with such individuals reported higher total income, greater asset accumulation, and lower poverty rates. Importantly, these results hold consistently across a wide range of robustness checks and falsification tests.

We also examine whether the Greyhound counterinsurgency operations led to a measurable decline in actual violence. This is a critical link in our interpretation: if exposure to the policy during school-age years improved long-term educational and labor market outcomes, a key mechanism is likely a reduction in violence and an improvement in local security conditions. Using data from the Global Database of Events, Language, and Tone (GDELT) and Global Terrorism Database (GTD), which record conflict-related incidents across time and space, we provide suggestive evidence that conflict intensity and number of successful attacks in AP declined following the launch of Greyhound operations.

A reduction in exposure to conflict during school-age years can influence human capital accumulation and labor market outcomes through several pathways. We explore one key pathway in depth: increase in parental investment in human capital. Increased security during school-age years—by reducing school disruptions and stimulating local economic activity—may increase the perceived value of schooling, prompting households to invest more in their children’s education (Becker, 1962). We provide suggestive evidence supporting this mechanism. Using multiple rounds of data from the National Sample Survey (NSS), we find that households who were eligible for exposure to Greyhound operations reported significantly higher educational expenditure than their counterparts.

We also examine the impact of Greyhounds operations on school expansion using data from the Unified District Information System for Education (U-DISE). Our analysis reveals a notable increase in the number of private schools in AP following the implementation of the Greyhounds policy, relative to other Naxalite-affected states. To complement this, we use data from the NSS to study enrollment patterns and find that, compared to other

affected states, the likelihood of enrolling in private schools increased in AP after the policy’s introduction, while enrollment in public schools declined.

These results are broadly consistent with our main findings and interpretation. The expansion of private education likely reflects increased household demand for schooling, improved operating conditions, and more optimistic expectations among private providers in a more secure environment. This aligns with our argument that enhanced security during school-age years encouraged greater educational investment by reducing uncertainty and improving expectations about future returns. The shift from public to private schooling, often perceived as higher quality (Kingdon, 1996a,b, 2020; De Talancé, 2020) but also more costly, suggests that families were responding not just to immediate improvements in safety, but to a more optimistic assessment of their children’s future economic opportunities.

Finally, we consider and rule out several confounding factors that could potentially be driving our findings. These include general improvements in law and order in AP unrelated to the Greyhound operations, the disruptive anti-affirmative action agitations of the early 1990s in the comparison states, educational policies implemented in AP before or during the policy rollout, shifts in political leadership or governance during the study period, and the information technology (IT) boom in AP in the late 1990s.

1.1 Literature

Our work contributes to several strands of literature. First, it builds on research examining the impact of hard-security counterinsurgency on human development in conflict-affected areas. The findings of this literature have been mixed. Singhal and Nilakantan (2016) find that counterinsurgency operations in India positively influenced industrial growth and business activities. In contrast, Dell and Querubin (2018) and Kocher et al. (2011) study U.S. counterinsurgency during the Vietnam War and show that heavy aerial bombing intensified insurgent activity and eroded local governance. Mahmood and Jetter (2023) report similar findings in the context of U.S. counterinsurgency operations in Pakistan. Finally, Amara (2012) shows that while U.S. military stabilization efforts in Iraq, notably the 2007 surge, led to short-term improvements in security, they did not produce sustained economic development. We contribute to this literature by providing novel evidence of the effects of counterinsurgency operations on a range of individual-level economic outcomes. In addition, we explore a key mechanism, thereby shedding light on why hard-security interventions may

yield developmental dividends.²

Our work also relates to the broader literature on conflict and development, particularly studies examining the long-term effects of childhood exposure to violence on human capital and labor market outcomes. Several studies document persistent negative impacts: Brück et al. (2019) find that exposure to violence reduces academic performance; Hidalgo-Aréstegui et al. (2025) show that early-life exposure to political violence in Peru significantly lowers educational attainment; and Chamarbagwala and Morán (2011) and Galdo (2013) report lasting educational and economic setbacks among children affected by civil wars in Guatemala and Peru. Similarly, Islam et al. (2016) find that disruption to schooling during the Cambodian Civil War led to lower long-run labor productivity, while Justino et al. (2014) shows that prolonged exposure to high-intensity conflict in Timor-Leste reduced primary school completion rates, particularly for boys.³ Our findings complement this body of work by showing that improved security during school-age years—as a result of counterinsurgency—can help reverse some of these adverse effects, leading to gains in education, labor market outcomes, and socioeconomic status in adulthood.

Additionally, our work contributes to four further strands of the literature. First, it engages with the growing interdisciplinary literature on the Naxalite insurgency in India. Recent contributions in this area include Gomes (2015); Nandwani (2019); Vanden Eynde (2018); Gawande et al. (2017); Dasgupta et al. (2017); Mukherjee (2018), among others, who examine the causes, consequences, and policy responses to left-wing extremism. Second, our study relates to the literature on the impact of organized crime on economic development (Melnikov et al., 2020; Fenizia and Saggio, 2024). Third, it contributes to the extensive literature on the determinants of human capital formation in developing countries, as synthesized in the comprehensive reviews by Attanasio (2015) and Glewwe and Muralidharan (2016). Finally, we add to a well-established literature on how childhood conditions shape long-term economic outcomes in adulthood (see Almond et al. (2018) for a review). Our findings underscore the importance of peace and security during formative years as key inputs into human capital accumulation and labor market success in adulthood.

The rest of the paper unfolds as follows. Section 2 outlines the context. Section 3 discusses the data sources and presents the empirical strategy. Section 4 presents the findings. Section 5 explores potential underlying mechanisms. The last section concludes.

²A related literature explores the interaction between counterinsurgency operations and development programs. Key contributions include Berman et al. (2011), Miguel and Roland (2011), Berman et al. (2013), Kaila et al. (2020), Sexton (2016), Child (2019), and Beath et al. (2025). These studies generally conclude that development efforts can enhance the effectiveness of counterinsurgency by improving state legitimacy and reducing support for insurgents.

³See Verwimp et al. (2019) for a review of the broader literature on conflict in LMICs.

2 Context

2.1 The Naxalite Movement

The late 1960s were a period of intense political unrest across India, especially in the state of West Bengal—a region characterized by deep agrarian inequality, where tribal communities and poor peasants endured exploitation at the hands of powerful landlords. The first insurgent activity was reported in Naxalbari, a small village in West Bengal, in March 1967 (Ray, 2012) where landlord gentries attacked a tribal farmer over a land dispute. Subsequently, peasant committees began to confiscate land, food grains, and weapons from the landlord class, triggering violent confrontations. In response to the uprising, the government deployed police forces. During this period, a police inspector was killed by members of the peasant committees. In retaliation, on 25 May 1967, the police opened fire, killing nine women and one child.

This incident sparked an armed uprising led by revolutionaries of the Communist Party of India (Marxist), or CPI (M), across several Indian states, aiming to overthrow the state and establish a communist regime. This ‘peasants’ uprising’ against the feudal landlords sowed the seeds for insurgency in India (Banerjee, 1984).⁴ CPI(M) did not approve of the armed uprising, and all the leaders and a number of Calcutta sympathizers were expelled from the party. Subsequently, in response, this expelled faction formed the All India Coordination Committee of Communist Revolutionaries (AICCCR) in November 1967. The AICCCR later established the Communist Party of India (Marxist-Leninist), or CPI (ML), on April 22, 1969. Most Naxalite factions today trace their ideological roots to this organization.

By the 1970s, the Naxalite movement had splintered into various rival factions. Estimates suggest that by 1980, there were around 30 active groups with a total membership nearing 30,000. Formed in AP in 1980, the CPI (ML) People’s War, also known as the People’s War Group, was among the movement’s most prominent and violent factions. Since then, the movement has grown rapidly in the state. In fact, AP became the hub of Naxals (Kujur, 2006). As noted by Sahoo (2019), by the early 1990s, Naxalite activity had spread to much of AP, with at least some level of presence reported in 20 out of the state’s 23 districts. The movement had gained such strength in the state that its cadres were able to assassinate several senior political leaders—including the state’s Home Minister—and high-ranking police. Their targets extended beyond the security apparatus to include suspected police informers, village headmen, and landlords—individuals they accused of perpetuating oppression against the poor and marginalized. These violent activities created an atmosphere of deep social

⁴Also see <http://timesofindia.indiatimes.com/city/kolkata/Naxalbari-revisited/articleshow/4704>

instability and fear, exposing civilians to constant uncertainty and insecurity, and severely undermining governance and local development.

It is worth noting that, while during the 1980s and 1990s, the various factions rapidly built up their operational capabilities and actively confronted the state's security forces, there was little coordination across groups. However, in more recent years, there have been numerous mergers, leading to the formation of the largest organization, the Communist Party of India-Maoist, in 2004. For further details about the history and evolution of the Naxal movement in India, see Banerjee (1984), Gupta (2007), Kennedy and Purushotham (2012), and Singh (2015).

2.2 The Greyhounds Operations

AP's earlier efforts to counter the Naxalite insurgency were able to marginalize some level of insurgency, but failed to eliminate the guerrillas in various regions of the state (Kennedy and Purushotham, 2012). From 1980 onwards, with the rapid growth of the Naxalite insurgency, the AP's responses were inadequate. In 1989, AP established a specialized police unit, the Greyhounds, to combat the Naxalite insurgency.⁵ The members of this unit were selected from the regular state police force for a three-year tenure, after which they were supposed to reintegrate into their respective units.. This elite commando force was rigorously trained in jungle combat. They were well-compensated and equipped with advanced weaponry. They were supported by a network of paid village informers. This enabled them to locate, arrest and eliminate key Maoist leaders, disrupting the insurgent activities.⁶ They operated in agile units of 15–30 commandos, with limited involvement from the local police. To ensure operational secrecy and effectiveness, sometimes the local police were not even informed of Greyhound missions in advance.⁷

The Greyhounds have been widely recognized by security analysts and policymakers for their significant role in reducing Naxalite-related violence in AP (Shapiro et al., 2017). However, they have also sometimes been criticized on human rights grounds for indiscriminate killing and lack of oversight. Kennedy and Purushotham (2012) and Singhal and Nilakantan (2016) provide an overview of the insurgency and counterinsurgency response in AP.

It is important to note that the deployment of the Greyhounds was later complemented by the introduction of a surrender and rehabilitation policy in AP in 1997 (Shapiro et al., 2017). In 1999, the policy was further modified to allow Naxalites to surrender not only to the police

⁵Although the decision to establish the Greyhounds was announced on June 6, 1988 (Balagopal, 1988), the key changes in AP's police and counterinsurgency efforts began only after 1989.

⁶<https://archive.ph/20131130234633/http://news.outlookindia.com/items.aspx?artid=800000>

⁷<https://sundayguardianlive.com/news/7136-centre-s-greyhounds-strategy-pays-maoists-crippled>

but also to civilian authorities, addressing concerns that many were hesitant to approach the police. Although this policy was implemented toward the end of our study period—and we therefore do not expect it to be the primary driver of our results—we cannot entirely rule out the possibility that some of the estimated effects may reflect its influence. Accordingly, we acknowledge that our analysis cannot disentangle the independent effects of the Greyhounds operations and the surrender policy. Nonetheless, we focus on the Greyhounds, as it was the most crucial component of the anti-Naxalite approach taken by AP post-1989 (Singhal and Nilakantan, 2016).

2.3 Counterinsurgency Policies in Other States

State-level counterinsurgency strategies can typically be categorized into four broad categories: the formation of elite police forces, the implementation of surrender and rehabilitation programs, the initiation of peace talks, and the rollout of economic development initiatives. As documented in (Shapiro et al., 2017), none of the Naxalite-affected states—apart from AP—introduced any major counterinsurgency measures under these categories until the end of 2000. This suggests that until the late 1980s, AP and the other affected states followed a broadly similar policy trajectory in their response to the insurgency. After 1989, AP diverged from this common policy trajectory by forming the Greyhounds force.

The only notable exception to this general pattern was the state of Bihar. As discussed in Shapiro et al. (2017), Bihar, in collaboration with the central government, launched two initiatives—Operation Siddharth and Operation Rakshak—between 1988 and 1989. These programs aimed to combine development efforts with military operations to curb Naxalite activity. However, the effectiveness of these operations remains highly questionable. Several observers have described the initiatives as largely symbolic, with limited impact on the ground (Shapiro et al., 2017).⁸ Some accounts have gone so far as to label them ‘virtual non-events’.⁹ Nevertheless, we conduct a robustness check in which we exclude Bihar—and the present-day state of Jharkhand, which was carved out of Bihar in 2000—from our analytical sample and re-estimate our baseline specification.

⁸See also: <https://www.the-american-interest.com/2013/08/11/the-naxalite-rebellions/>

⁹See: <https://timesofindia.indiatimes.com/city/patna/scheme-to-root-out-naxal-influence/articleshow/3660647.cms>

3 Data and Empirics

3.1 Data

3.1.1 India Human Development Survey

We use data from the India Human Development Survey (IHDS) 2011-12 for our main analysis (Desai et al., 2018). The IHDS is a nationally representative, multi-topic household survey conducted by the National Council of Applied Economic Research (NCAER) in New Delhi and the University of Maryland. Designed to complement existing Indian household surveys, it brings together a wide range of socio-economic topics, enabling comprehensive analyses of relationships between various social and economic conditions. The second wave of the survey, conducted in 2011-12, covered 42,152 households across 1,420 villages and 1,042 urban neighborhoods across India. The Data Sharing for Demographic Research program hosts this publicly available data through the Inter-university Consortium for Political and Social Research (ICPSR).

The IHDS is particularly well-suited for our analysis for two key reasons. First, it provides essential data on the economic outcomes of individuals in the sampled households, including labor market participation and earnings, total income, assets, education levels, and poverty status. Second, and perhaps more importantly, it contains detailed information on the households’ duration of residence, which is critical for identifying individuals’ state of residence during school-age years. By focusing on households who report living in their current place of residence ‘forever’, we can infer the state of residence of individuals during school-age years. Notably, more than 78% of households indicated they have never moved, allowing us to retain nearly the entire sample for analysis. This rich dataset enables robust examination of long-term outcomes, contributing valuable insights to the study. Note, it is extremely common in studies examining exposure to treatment during school-age years to restrict the analysis to non-migrant individuals or ‘never-movers’ (e.g., von Der Goltz et al. (2020)).¹⁰

We use a rich set of educational and labor market outcomes from the India Human Development Survey (IHDS). Educational outcomes include indicators for literacy, school attendance, total years of schooling completed, completion of secondary and higher secondary education, attainment of a bachelor’s degree or higher, and self-reported proficiency in En-

¹⁰We acknowledge that children in ‘never-mover’ households in our sample may have moved out during the study period in response to the insurgency. However, our estimates remain unaffected as long as these moves occurred within the same state, consistent with our identification strategy (see Section 3.3). It is also worth noting that inter-district migration in India is extremely low, making inter-state migration even less likely.

glish. Labor market outcomes capture both participation and employment type. Specifically, we use indicators for whether an individual is engaged in any form of work, disaggregated into agricultural wage work (paid daily), non-agricultural wage work (paid daily), salaried employment (paid monthly or annually), family business activity, and family farm-related tasks. We also examine weekly working hours, total days worked in the past year, annual cash wages, and total annual earnings. In addition, we consider several household-level indicators of socioeconomic status, including household wage and salary income, total household income, per capita income, poverty status (based on official thresholds), and an asset score ranging from 0 to 30.¹¹

3.1.2 Global Database of Events, Language, and Tone

To examine potential mechanisms underlying our main findings, we draw on additional data sources. To measure patterns in violence and conflict intensity, we use geo-coded data from the Global Database of Events, Language, and Tone (GDELT) version 1.0. GDELT is a vast open database tracking global news in over 100 languages, with archives dating to 1979 and updates every 15 minutes. It provides daily, event-level information on political conflict, including protest and security incidents, allowing us to track local changes in conflict exposure over time. It excels in speed and coverage, coding conflict events in near real-time and providing source URLs for verification.¹²

3.1.3 Global Terrorism Database

To complement the GDELT data, we also draw on the Global Terrorism Database (GTD), compiled by the National Consortium for the Study of Terrorism and Responses to Terrorism (START) at the University of Maryland. The GTD is a comprehensive, open-source dataset that records over 200,000 terrorist incidents worldwide since 1970, providing detailed information on the date, location, nature, and outcome of each event. We use the GTD to track the number of successful attacks attributed to the Left-Wing Extremism (LWE) across the states affected by Naxalite insurgency.

3.1.4 National Sample Survey

We use the Household consumption expenditure surveys conducted by the National Sample Survey (NSS) organization to examine household investments in education before and after

¹¹It is computed by summing 30 dichotomous items measuring household possessions and housing quality.

¹²For further details, see: <https://www.gdeltproject.org/data.html>

the policy implementation. The surveys collect detailed information on household expenditures, including spending on education-related items, and are the main source of data for poverty and inequality statistics in India. The NSS employs a stratified multi-stage sampling design to collect nationally representative data through household interviews. Our analysis draws on pooled data from both pre- and post-policy periods: the 38th (1983) and 43rd (1987–88) rounds prior to the 1989 policy, and the 45th (1989–90), 46th (1990–91), 47th (1991), 48th (1992), and 49th (1993) rounds in the post-policy period.

Additionally, we use two rounds of Household social consumption on education surveys to examine enrollment patterns in public and private schools pre- and post-Greyhounds formation. The rounds used were conducted in 1987-88 and 1995-96, and are included in the NSS 43rd round and 52nd round respectively. These rounds provide detailed information on education particulars of school-going children.

3.1.5 Unified District Information System for Education

We also use the Unified District Information System for Education (U-DISE) 2012-13 dataset to calculate the total number of private and public schools established in our sample states between 1950-2000. U-DISE is an education management information system where all registered schools record information on school particulars, students' and teachers' details each academic year. This central platform was launched in 1995 and is maintained by the Ministry of Education under the Government of India. Thus, U-DISE provides a census of schools with detailed information on school particulars, including year of establishment, whether the school is public or private, teachers hired, infrastructure, and student enrollment, among others.

3.1.6 National Crime Records Bureau

For examining whether there was a general improvement in law and order in AP during the implementation of Greyhounds, we use crime data from the National Crime Records Bureau (NCRB). NCRB compiles annual statistics on cognizable offenses reported under the Indian Penal Code (IPC) across Indian states and union territories. We focus on serious crimes, using both disaggregated categories - such as murder, rape, kidnapping, dacoity, robbery, burglary, theft, riots, criminal breach of trust, cheating, and counterfeiting; as well as total crime rate. The NCRB provides data on population-adjusted crime rates, allowing us to make meaningful comparisons across regions and over time. We restrict our analysis to IPC crimes and exclude minor offenses recorded under Special and Local Laws.

3.2 Construction of Analytical Sample, Treatment and Control Groups

We construct our analytical sample following the approach of Singhal and Nilakantan (2016). Specifically, we restrict our sample to 11 states (as of the 2011 Indian Census) that were severely affected by Naxalite insurgency prior to 2000. These are: AP, Bihar, Chhattisgarh, Jharkhand, Karnataka, Madhya Pradesh, Maharashtra, Odisha, Uttar Pradesh, Uttarakhand, and West Bengal (see Figure 1).¹³ Note that Chhattisgarh, Uttarakhand, and Jharkhand were created in November 2000, having been carved out of Madhya Pradesh, Uttar Pradesh, and Bihar, respectively. We still include these states in our sample, as their parent states were among those affected by insurgency prior to the split.

Our treatment group is restricted to individuals born and residing in AP. We compare their outcomes with those from ten other states affected by the Naxalite insurgency but without counterinsurgency policies until 2000 (control states). For both the treatment and control groups, we create two cohorts: younger cohort and older cohort. The younger cohort includes individuals who were eligible for exposure to the Greyhounds policy during their school-age years (5-17 years). Given that the policy was effective from 1989, these are therefore the individuals who were born between 1972 and 1984. The older cohort, on the other hand, includes those individuals who were not eligible for exposure to the policy during their school-age years. These are therefore the individuals who were born before 1972. Note that individuals born after 1984 are excluded from our analysis. Since other Naxalite-affected states began implementing counterinsurgency policies only from the end of 2000, this ensures that our sample does not include individuals from these states who could have been exposed to such policies during their school-age years. In other words, the individuals included in our sample who could have been exposed to the counterinsurgency operations during their school-age years are only from AP.

Table 1 reports the summary statistics.

3.3 Estimation Strategy

We employ a DID strategy to estimate the impact of the Greyhounds counterinsurgency policy. Our approach exploits variation in exposure eligibility to the Greyhounds counterin-

¹³We also include Kerala in the analytical sample and conduct a robustness check. While Singhal and Nilakantan (2016) do not include Kerala, some observers note naxalite activities in small pockets in the state. However, as remarked by a former Naxal, Philip M Prasad in an interview, the naxal movement in Kerala was always an intellectual leadership activism, and even at their peak, the ultras or naxalites failed to exert significant impact in Kerala (see <https://www.newindianexpress.com/amp/story/states/kerala/2019/Nov/03/maoist-conundrum-in-kerala-2056379.html>).

surgency policy across cohorts within AP and compares their outcomes to similarly defined cohorts in other Naxalite-affected states that did not implement comparable policies during the same period. Specifically, we compare differences in outcomes between cohorts in AP who were eligible for exposure to the policy during their school-age years and those who were not, and examine whether these differences diverge from analogous cohort differences in other Naxalite-affected states.

The basic estimating equation is:

$$Y_{ist} = \alpha + \beta(\text{YoungerCohort}_t \times AP_s) + \gamma X_{ist} + \delta_t + \lambda_s + \epsilon_{ist} \quad (1)$$

where Y_{ist} denotes the long-term economic outcome of individual i residing in state s and born in year t ; YoungerCohort_t (or YC) is a dummy variable that takes a value of 1 if the individual belongs to the younger cohort, and 0 otherwise; AP_s is a dummy variable which takes a value 1 if the state in which the individual resides is AP and 0 if the individual resides in any other state affected by Naxalite violence (which include Bihar, Chattisgarh, Jharkhand, Karnataka, Madhya Pradesh, Maharashtra, Orissa, Uttar Pradesh, Uttarakhand and West Bengal); X_{ist} is the vector of individual and household level controls such as gender, caste, religion, household size; λ_s denotes state fixed effects which control for all time-invariant state-level characteristics—such as institutional quality, cultural norms, and geographic factors—that may lead to differential trends in education and labour market outcomes across cohorts; δ_t denotes birth year fixed effects that partial out birth-year specific shocks to labor market and education outcomes that are common across states; and ϵ_{ist} is the disturbance term. Our coefficient of interest, β , estimates the ITT of exposure to Greyhounds policy during school-age years on educational and labor market outcomes in adulthood. We cluster standard errors at state of birth by cohort level.¹⁴ Survey weights are used in all regressions. We acknowledge that the policy could have influenced labor market outcomes for individuals in AP who had crossed their school-age years during implementation—albeit likely to a lesser extent than for those exposed during school-age years—our ITT estimates may understate the full impact of the Greyhounds policy on labor market outcomes.

¹⁴The standard recommendation in the literature is to cluster standard errors at the level of treatment assignment, which in our case corresponds to the state of birth by cohort (see, e.g., de Chaisemartin and D’Haultfoeuille (2025)). Several other key studies, where treatment is assigned at the region of birth by cohort level, also cluster standard errors at the region-birth cohort level (e.g., Black et al. (2015); Doty et al. (2025)). An alternative would be to cluster at the state level. However, our analytical sample includes only 11 states, making this approach infeasible without using wild bootstrap clustering. However, as Hansen (2025) shows, in DID designs with only one treated cluster—as is the case in our study—the wild bootstrap tends to perform poorly, incorrectly estimating the true standard error. Hansen (2025) also demonstrates that jackknife methods are similarly uninformative in such settings, often producing standard errors that are nearly as large as the coefficient estimates, resulting in t-statistics close to one and a loss of statistical significance.

We also implement a regression specification in which, instead of $YoungerCohort_t$, the primary covariate of interest is $Share_t$, which is defined as the share of school-age years (5-17 years) that the individuals were eligible for exposure to the Greyhounds policy in AP. Clearly, for the older cohort, this value takes a zero value. For the younger cohort, this is calculated as $(Year\ they\ turn\ 17 - 1989)/17$.

The empirical approach is, therefore, to look at whether there is a break in any preexisting differences in the level or trend of the outcomes around the time of the introduction of the Greyhounds policy. The identifying assumption is that, absent Greyhounds, any pre-period differences would have continued on the same trends. While this assumption is inherently untestable, we provide suggestive evidence in support of it by examining whether there were any differential pre-trends in outcomes across cohorts before the policy was implemented. To do so, we estimate the following event-study specification:

$$Y_{ist} = \alpha + \sum_{\tau} \beta_{\tau}(AP_s \times Yearbin_{\tau}) + \gamma X_{ist} + \delta_t + \lambda_s + \epsilon_{ist} \quad (2)$$

where $Yearbin_{\tau}$ is a dummy variable indicating whether individuals are born in the year bin τ . Each coefficient β_{τ} captures the ITT of the counterinsurgency policy on the outcomes of individuals in the year bin cohort τ . We use year bins instead of years to reduce concerns related to low statistical power, a common challenge in event study estimates (Roth, 2022), while ensuring a more balanced distribution of observations across bins. Three-year bins are used for pre-1972 periods and two-year bins for post-1972 periods.¹⁵ We expect the β_{τ} s to be statistically insignificant for individuals born in years up to 1971 since they would have completed school-age in 1989 when the Greyhounds counterinsurgency unit was established.

We present the estimated coefficients β_{τ} in Figures 2, 3 and 4. Each point on the central solid line represents the coefficient of the interaction between the birth year bin variable and the AP_s dummy. The dashed lines above and below the solid line denote the 95% confidence intervals for these coefficients. The graphical representation provides suggestive evidence in favor of our identifying assumption.¹⁶

Despite this, some threats to identification remain. Most importantly, we must ensure that the estimated treatment effect is not conflated with the influence of time-varying, state-level confounders. Ideally, we would include state-by-birth cohort fixed effects to account for such factors. However, since our treatment varies at the state-cohort level, these fixed effects would absorb the treatment and prevent identification. To address this concern, we

¹⁵This binning structure was chosen to maintain a degree of uniformity in the number of observations across bins.

¹⁶We also estimate year-by-year effects without binning, and the event-study plots are consistent. The figures are available upon request.

conduct a series of robustness checks (see Section 4.2). Also, we investigate contemporaneous events around the time of the policy rollout that may have differentially affected AP and the comparison states, and we show that our results are not driven by such events (see Section 5.2).

Additionally, we present results from an alternative specification that exploits *within*-state variation in treatment intensity. Specifically, we identify districts in AP that likely had a high intensity of Naxalite activity in the pre-policy period. To do so, we combine information from two historical maps, and a report (Sahoo, 2019). The first map was originally published in *Deshabrati*—a mouthpiece of the revolutionary faction within the CPI(M), and later the official organ of the West Bengal unit of CPI(ML)—and is reproduced in Banerjee (1984). This map identifies Naxalite-affected districts during the 1970s. The second map is constructed by Borooah (2008), who, drawing on the Ministry of Home Affairs’ annual reports and open-source platforms such as the South Asia Intelligence Review, identifies districts across ten states with documented significant Naxalite activity in the early 2000s. The report by Sahoo (2019) identifies districts with the highest Naxalite activities in AP in the 1980s. The combination of the two maps along with the report likely offers an approximation of the districts that likely experienced high Naxalite activity in the pre-policy period.¹⁷ The final classification, based on this combined information, is shown in Figure B.1.

Under the assumption that the Greyhounds operations were more intensive in AP districts with a high intensity of Naxalite activities, we construct a binary variable, *AP High Intensity*, which equals 1 for such districts in AP and 0 for all other districts in our sample. We then estimate the following equation:

$$Y_{idt} = \alpha + \beta(\text{YoungerCohort}_t \times \text{APHighIntensity}_d) + \gamma X_{idt} + \delta_t + \theta_d + \epsilon_{idt} \quad (3)$$

where d indexes districts and θ_d district-fixed effects.¹⁸ The identifying assumption is that, absent Greyhounds, AP districts with a high intensity of Naxalite activities would have had similar cohort trends as other districts in AP and districts in other states. To provide suggestive evidence supporting this assumption, we also estimate an event study specification, replacing the younger cohort indicator with birth year bins, as described in Equation 2. The estimated β s from the event study specifications along with the 95% confidence intervals are presented in Figs. B.2 to B.4. The figures provide suggestive evidence in favor of the identifying assumption.

¹⁷Many of the AP districts designated as high-intensity in the early 2000s had already begun witnessing substantial insurgency activity by the 1980s.

¹⁸We also estimate a specification that uses *Share_t* in place of *YoungerCohort_t*.

It is worth noting, while a key advantage of this specification is that it mitigates concerns about time-varying state-level confounders (at least those that affect all districts within a state similarly), defining treatment at a finer geographic level such as the district introduces its own challenges—most notably, the risk of spillovers across neighboring districts and the possibility of selective intra-state migration in response to security operations. Moreover, the classification of high Naxalite-intensity districts is based on historical approximations, which raises the possibility of misclassification. These concerns are less salient when treatment is defined at the state level. For these reasons, while this alternative specification serves as a valuable robustness check, our preferred specification remains the baseline one, which defines treatment at the state-cohort level.

4 Results

4.1 Main Results

4.1.1 Educational Outcomes

We begin by assessing the ITT effects of exposure to the Greyhounds policy during school-age years on educational attainment estimated using Equation 1. Table 2 presents the results. Panel A reports estimates using the binary indicator, YC , which captures *whether* individuals were eligible for exposure to the policy during their school-age years. Panel B uses a continuous measure, $Share$, defined as the *share* of school-age years overlapping with the policy period. Each column corresponds to a different educational outcome.

The ITT estimates in Panel A indicate a clear and positive relationship between exposure to Greyhounds operations and educational achievement. Compared to their counterparts, individuals eligible for exposure during school-age years were 6.3 percentage points (p.p.) more likely to be literate, 5.2 p.p. more likely to have attended school, and had on average completed 0.59 more years of schooling. They were also 3.6 p.p. more likely to complete secondary education, 3.1 p.p. more likely to attain a bachelor’s degree or higher, and 6 p.p. more likely to report English proficiency. These effects are statistically significant at conventional levels and are meaningful in magnitude. For instance, the 6.3 p.p. increase in literacy corresponds to an 11% gain relative to the sample mean; the increase in completed years of schooling represents a 13.8% improvement.

Panel B corroborates these findings using a $Share$ as the measure of exposure eligibility. All the statistically significant ITT effects observed in Panel A remain significant. The magnitudes of the estimated coefficients are also economically meaningful—for instance, a

10 p.p. increase¹⁹ in the share of exposure eligibility is associated with a 1.75 p.p. increase in the likelihood of being literate, a 1.65 p.p. increase in school attendance, a 1 p.p. increase in completing secondary education, and a 1.96 p.p. increase in English proficiency.

Taken together, the results point to a consistent pattern: children who were eligible for exposure to the Greyhounds policy during their school-age years attained significantly better educational outcomes.

4.1.2 Labor Market Outcomes and Household Socioeconomic Status

The ITT estimates of the effect of counterinsurgency on workforce participation and labor supply estimated using Equation 1 are presented in Table 3. As in Table 2, Panel A reports results using *YC* as the exposure measure, while Panel B presents results using the *Share* as the measure of exposure.

The results in Panel A indicate that individuals who were eligible for exposure to the counterinsurgency policy during school-age years are 12.5 p.p. more likely to be engaged in any form of work compared to those not exposed—an increase of approximately 20% relative to the sample mean. Eligibility for exposure is also associated with a 7.7 p.p. increase in the likelihood of salaried employment and a 6.2 p.p. decrease in the likelihood of working in non-agricultural (but informal) jobs; both effects are statistically significant at conventional levels. In contrast, the estimated effects on employment in agriculture, business, and farm-related work are not statistically significant. The ITT estimates also suggest, exposure to Greyhounds during school-age years is linked to greater labor supply. On average, individuals eligible for exposure worked 28.2 more days per year and 201.5 more hours annually than individuals who were not eligible for exposure. These effects are economically meaningful: they represent a 19.8% increase in days worked and a 20.7% increase in total hours worked, relative to the sample means.

The Panel B estimates are broadly consistent with those in Panel A, reinforcing the finding that exposure to AP’s counterinsurgency operation during school-age years led to higher workforce participation, decreased agricultural employment, and greater engagement in salaried jobs. Additionally, the significant increase in workdays and hours worked highlights the lasting labor supply benefits of exposure to the policy.

In Table 4, we present the estimated ITT effects of exposure to the policy on individual wages and earnings, and indicators of household socioeconomic status. The results in Panel A indicate sizable and statistically significant gains for the cohort eligible for exposure to counterinsurgency. Individuals eligible for exposure to the policy during school-age years earned 17% higher cash wages and saw a 17.1% increase in total annual earnings. These

¹⁹This represents a 0.1 unit (or 1.42 SD) increase in *Share*.

gains extended to the household level as well: wage and salary income rose by 13%, and total household income increased by 15%. Household income per capita rose by 12.5%, suggesting that the benefits were broadly shared across family members.²⁰

The ITT estimates suggest, exposure to Greyhounds during school-age years also led to improvements in other measures of socioeconomic status. The likelihood of being officially classified as poor declined by 5.6 p.p. Additionally, household asset ownership rose by 0.56 points—a 4% increase relative to the sample mean. All effects are statistically significant at conventional levels. Panel B, which uses a continuous measure of exposure, yields similar results in both magnitude and significance, reinforcing the robustness of these findings.

Taken together, the results present a clear and consistent pattern: exposure to the Greyhounds policy during school-age years is linked to substantial improvements in educational attainment, labor market performance, and household economic conditions. Individuals eligible for exposure to the policy were more likely to attain higher levels of education, exhibit more English proficiency, hold salaried jobs, work longer hours and more days per year, and earn higher wages. These benefits also translate into broader household gains, including increased income, reduced poverty, and greater asset ownership. Importantly, the findings remain robust across alternative measures of exposure-eligibility.

4.2 Robustness Checks

In this section, we present a series of robustness checks and falsification tests to assess the credibility of our findings.

4.2.1 Treatment Effect Heterogeneity

Recent advances in causal inference highlight that standard two-way fixed effects (TWFE) estimators, such as the one used in this paper, may yield biased estimates in the presence of treatment effect heterogeneity (de Chaisemartin and D’Haultfœuille, 2020; Goodman-Bacon, 2021). In our setting, such heterogeneity may arise because the counterinsurgency policy affects individuals at different ages, potentially leading to variation in its impact across cohorts. To address this concern, we re-estimate our model using the estimator developed by (de Chaisemartin and D’Haultfœuille, 2020), which accounts for treatment effect heterogeneity and remains valid even when the parallel trends assumption is relaxed. This approach not only estimates dynamic treatment effects but also enables placebo checks by comparing periods prior to treatment.

²⁰Since the outcome variables are in logarithmic form, the estimated effects are interpreted as approximate percentage changes. To compute the exact percentage change, we use the transformation: $(e^{\hat{\beta}} - 1) \times 100$, where $\hat{\beta}$ is the estimated coefficient.

Figures B.5, B.6 and B.7 in the Appendix present the results. Two key patterns emerge. First, pre-treatment ITT effects are consistently small and statistically insignificant across all outcomes. Second, post-treatment ITT effects are stable and align with the main specification. These findings reinforce the credibility of the baseline estimates.

4.2.2 Exact Randomization Inference Test

We conduct an exact randomization inference test (Bharadwaj et al., 2014) to ensure that our estimated effects are not driven by spurious correlations. Specifically, we perform two types of placebo simulations by independently shuffling treatment assignments. In the first type, we randomly assign the state-level treatment status ($AP = 1$) to other states in our sample. In the second, we randomly assign cohort-level treatment status ($YC = 1$) to individuals based on birth year, while maintaining the original proportion of treated individuals. For each simulation, we re-estimate our main specification Eq. (1) using the new randomly assigned treatment variables and repeat this process 1,000 times. We expect distribution of the estimates to be centered around zero.

We plot the distribution of coefficients obtained from simulations based on state-level randomization for education, labor market, and wage-related outcomes in Figs. B.8 to B.10 . The figures show that the distribution of most all simulated coefficients are centered around zero and significantly different from the true estimated coefficients (indicated by a dashed black vertical line). We get similar results from the cohort-level treatment randomization shown in Figs. B.11 to B.13. We also carried out this exercise for the individuals exposed (where we randomize the interaction $YC \times AP$) and obtain similar results.²¹ These results reinforce that the observed effects are unlikely to be driven by chance, and thus strengthens the credibility of our identification strategy.

4.2.3 Inclusion of Birth Decade-State Trends

We include interactions between birth decade and state as controls. This means we account for any broad factors that are specific to people born in a particular state during a particular decade. These could include differences in economic conditions, state-level policies, education systems, public health programs, or broader political and social changes that occurred during that time. By doing this, we ensure that our ITT estimates are not biased by long-term trends or regional factors that might otherwise influence the outcomes we are studying. As shown in Table A.1, the estimated ITT effects for human capital outcomes not only persist but in some cases increase in magnitude. While the coefficients for labor market outcomes

²¹The results are available upon request

and household SES decline somewhat in size (and in some cases precision), they continue to remain economically significant. These findings reinforce the robustness of our main results and suggest that they are not merely artifacts of unobserved regional dynamics.²²

4.2.4 Alternative Comparison Cohort

We re-estimate our main specification using a more stringent definition of the comparison (older) cohort. In the baseline, we define the comparison group as individuals who were 18 years or older at the time of the Greyhounds policy introduction in 1989. In this alternative specification, we further restrict the comparison group to individuals who were at least 25 years old in 1989, dropping those who were between 18 and 24.

This approach ensures a cleaner separation between those eligible for exposure during school-age years and those whose school-age years were well behind them by the time the policy was implemented. The rationale is that individuals aged 18–24 at the time may have still been engaged in education (or transitioning into the labor market), and their outcomes could plausibly be influenced by the policy (albeit through different mechanisms than those experienced during school-age years). By excluding this intermediate age group, we reduce potential contamination of the comparison group and strengthen the contrast in exposure.

Table A.2 reports the results. Evidently, the results from this alternative specification are virtually identical to those from the baseline model, further reinforcing the robustness of our findings.

4.2.5 Bordering States Analysis

To further strengthen our findings, we limit the control group to only the states that share a border with AP. This choice is motivated by the idea that bordering states are more likely to share similar geographic, cultural, administrative and economic characteristics, and may also be exposed to common regional shocks.²³ By narrowing the control group in this way, we reduce the likelihood that our results are driven by unobserved differences between distant states and AP, strengthening the credibility of our findings. The results from this exercise, presented in Table A.3, remain consistent with our primary findings and provide additional support for the robustness of our estimates.

²²We also estimated an alternative specification that includes state-specific linear birth-year trends. The results remain robust and are available upon request.

²³Although our sample takes care of spillover through civilian migration

4.2.6 Falsification Test Using Unexposed Cohort

We performed a falsification test using the older cohort (who were ineligible for exposure to the counterinsurgency during school-age years), classifying younger individuals within this cohort as ‘falsely treated’. If the observed effects are genuinely attributable to the Greyhounds policy, then we should not observe a significant impact in a cohort that was never eligible for exposure to the policy during their school-age years. Our results (Table A.4 in the appendix) confirm that the coefficients for the falsely treated younger cohort are not statistically significant across key economic indicators. These findings strengthen the inference that the estimated effects are attributable to the Greyhounds policy rather than broader external factors.

4.2.7 Excluding Bihar and Jharkhand, including Kerala

As noted in Section 2.3, Bihar, in collaboration with the central government, launched two counterinsurgency initiatives—Operation Siddharth and Operation Rakshak—between 1988 and 1989. While several observers have described the initiatives as largely symbolic, with limited impact on the ground (Shapiro et al. (2017)), nevertheless, we conduct a robustness check in which we exclude Bihar—and the present-day state of Jharkhand—from our analytical sample and re-estimate our baseline specification. In an additional robustness check, we include Kerala in our analytical sample and reestimate our baseline regressions since there are reports suggestive of Naxalite activities in small pockets in the state.

The results of these two robustness checks are reported in Table A.5 and Table A.6. As evident, the results are broadly in line with our baseline findings.

4.2.8 Rural Sample

We restrict our analysis to the rural sample within Naxalite-affected districts. Since the Naxalite insurgency was primarily concentrated in rural areas, we would expect to observe effects in this subsample if they are indeed driven by counterinsurgency efforts. Conversely, if the estimated effects are economically negligible in this setting, it would raise concerns that our baseline results may be confounded by other factors. The results of this analysis are presented in Table A.7. As shown, the estimated coefficients remain economically meaningful and broadly consistent with our baseline findings, although some estimates lose statistical significance—likely due to the smaller sample size.

4.2.9 District-level Treatment

Table A.8 presents results from Equation 3, where we identify the effect of Greyhounds operations by comparing outcomes across cohorts within high-insurgency (and thus high-counterinsurgency) districts in AP to other districts in AP and districts in other Naxalite-affected states. The estimates closely mirror our baseline findings, providing strong reassurance that our results are not being driven by time-varying, state-level confounders.

Together, the robustness checks and falsification tests confirm that our results are fairly robust.

4.3 Additional Outcomes

To further assess the broader impacts of the Greyhounds policy, we examine its effects on individuals' confidence in institutions in adulthood using the same estimating equation as in Equation 1. Table 5 presents the results for a range of institutional confidence variables, including confidence in politicians, the military, police, state government, news media, and panchayats.²⁴ The ITT estimates suggest that exposure to Greyhounds led to an increase in confidence in police and state government. Specifically, those eligible for exposure to the Greyhounds policy during school-age years were 3.7 p.p. more likely to report confidence in the police and 4.2 p.p. more likely to express confidence in the state government than their counterparts. Although the estimated effects on confidence in politicians are not statistically significant, they are economically meaningful. These results highlight the potential for counterinsurgency policies to influence institutional confidence.

5 Discussion

5.1 Potential Mechanism

If exposure to the Greyhounds policy during school-age years improved educational outcomes and, in adulthood, labor market outcomes and socioeconomic status, a key mechanism is likely a reduction in violence and an improvement in local security conditions. This is a critical link in our interpretation. We examine whether the Greyhound operations led to a measurable decline in actual violence.

Assessing the effect of Greyhounds on insurgency is a significant challenge due to lack of comprehensive data on Naxalite insurgency in the states before 1989 and during 1989-2000. Nevertheless, we use the GDELT 1.0 database and analyze the number of conflicts

²⁴See Table A.9 for the summary statistics of these variables.

reported for the Naxalite-affected states between 1980 and 2000. We estimate the following specification:

$$Y_{st} = \alpha + \beta(AP_s \times Post_t) + \gamma X_{st} + \delta_s + \lambda_t + \epsilon_{st} \quad (4)$$

where Y_{st} denotes the number of conflict related incidents reported in state s and year t for the GDELT database, while for the GTD database, it captures the number of successful attacks. AP_s is an indicator for the state being AP; $Post_t$ equals one for post-policy period and zero otherwise; X_{st} includes the total state population, measured using decadal Census data. δ_s and λ_t are state fixed effects and year fixed effects, respectively; and ϵ_{st} is the error term.

Our results reveal a significant decline in conflict intensity following the Greyhounds' formation. Specifically, as shown in columns (1) and (2) of Table 6, we find negative and statistically significant coefficients for both total reported conflicts and specific conflict categories. This provides suggestive evidence that the Greyhounds' counterinsurgency efforts played a role in reducing overall conflict intensity in the region, reinforcing the argument that improved security conditions contributed to the observed economic and human capital outcomes.

We also use the Global Terrorism Database (GTD) to estimate the effect of Greyhounds on the number of successful attacks (perpetrated by insurgent and terrorist groups). We estimate the same regression equation. The coefficient reported in column (3) of Table 6, although imprecisely estimated, is negative and economically significant suggesting that Greyhounds operation were associated with a fall in the number of successful attacks. We caution against placing too much weight on this result, as we lack data for AP prior to 1995—that is, before the rollout of the policy.²⁵

It also worth mentioning that there are media reports that highlight reduction in insurgent activities in AP, although limited. From 1995 to 2016, AP police recorded over 1,780 Maoist fatalities, with the Greyhounds responsible for nearly 80% of these encounters. In comparison, police casualties were much lower, with 163 officers killed, of which about 20% were from the Greyhounds.²⁶

Improved security during school-age years can shape educational and labor market outcomes through several interconnected pathways. One key pathway is increased parental investment in human capital. A decline in violence helps ensure the regular functioning of

²⁵Since AP has no pre-treatment observations, the estimator is essentially identifying the effect by comparing post-treatment outcomes in AP relative to the (post-minus-pre) changes in other states.

²⁶<https://www.thehindu.com/todays-paper/tp-national/tp-andhrapradesh/greyhounds-among-the-best-anti-insurgency-forces-experts/article17568627.ece>

schools and reduces the risk of disruptions to learning. In addition, a more stable environment can stimulate local economic activity, as shown by Singhal and Nilakantan (2016) which may raise the perceived returns to education. Together, these factors enhance the value households attach to schooling and can motivate greater investment in children’s education.

To investigate the pathway, we compare household expenditure on education-related items between AP and other Naxalite-affected states before and after the enactment of the counterinsurgency policy (i.e., pre- and post-1989) using pooled rounds of NSS data.²⁷ Specifically, we estimate the following specification:

$$\log(Y_{ist} + 1) = \alpha + \beta(AP_s \times Post_t) + \gamma X_{ist} + \delta_s + \lambda_t + \epsilon_{ist} \quad (5)$$

where Y_{ist} denotes household i ’s expenditure on education-related items in state s and round t ; AP_s is an indicator for the state being AP; $Post_t$ equals one for post-policy rounds, and zero otherwise; X_{ist} includes household-level controls such as household type (rural/urban), household size, social group, and logarithm of total monthly household expenditure; δ_s are state fixed effects; λ_t are round fixed effects; and ϵ_{ist} is the error term. Additionally, we estimate a specification in which we exclude logarithm of total monthly household expenditure from the set of covariates and use logarithm of household i ’s expenditure on education-related items as a share of total expenditure as outcome variables.

We find that households in AP exhibit a relatively stronger increase in education-related spending such as tuition fees, stationery, and other educational expenses after 1989 compared to the control states (Panel A, Table 7). Results using the share of household expenditure allocated to education, (shown in Panel B) yield similar findings. These trends support that enhanced security conditions enabled households to reallocate resources toward long-term investments in education, providing a plausible pathway for the improvements in educational attainment and labor market outcomes we observe in our main results. This shift in household behavior—driven not by direct schooling interventions but by changes in the broader security and economic landscape—underscores how counterinsurgency efforts can have powerful, indirect effects on human capital formation.

We also examine the impact of Greyhound operations on school expansion using data from the Unified District Information System for Education (U-DISE). Specifically, we estimate a specification similar to Equation 4, where the outcome variable denotes the number of schools of a given type (private or public) established in a state in a given year. Results are reported in Table 8, Panel A. We find a notable increase in the number of private schools in areas exposed to the policy.

²⁷See discussion in Section 3.1.4.

In addition, we analyze enrollment patterns in AP during the period of Greyhound activity, using a specification analogous to Equation 5, where the outcome variable captures whether an individual is currently enrolled in a private or public school.²⁸ As shown in Table 8, Panel B, the likelihood of enrolling in private schools increased relative to other Naxalite-affected states, while enrollment in public schools declined.

These findings are consistent with our broader interpretation. The growth of private schooling likely reflects increased household demand for private education, improved operating conditions, and greater confidence among private providers in a more secure environment. This supports our argument that enhanced security during school-age years encouraged higher educational investment by reducing uncertainty and raising expectations about future returns. The shift from public to private schooling—often perceived as higher quality (Kingdon, 1996a,b; De Talancé, 2020; Kingdon, 2020; Gruijters et al., 2021)) but also more expensive, especially in the 1980s and 1990s²⁹—suggests that families were responding not only to immediate safety gains but also to improved long-term prospects for their children.

5.2 Ruling Out Confounding Factors

Next, we consider and rule out several confounding factors that could plausibly explain our findings.

5.2.1 Improvements in Law Enforcement

A key alternative explanation is that the observed effects may stem from broader improvements in law enforcement unrelated to the counterinsurgency strategy. If in response to insurgency, there was general strengthening of law and order in AP, there should have been an decline in non-insurgency crime rates, in addition to insurgency crimes. On the other hand, if the outcomes are driven by the policy specifically targeted to suppress insurgency rather than general crime activities, there should be no significant decline in non-insurgency crime rates.

To examine this, we use data on different crime rates from the National Crime Records Bureau (NCRB) on offenses registered under the Indian Penal Code (IPC). Consistent with this expectation, our event study estimates in Figs. B.14 and B.15 indicate no significant decline in overall crime levels following the policy’s implementation. This indicates that the observed effects are unlikely to be driven by broader improvements in law and order unrelated to the Greyhounds.

²⁸However, the outcome variable is now used in levels rather than in logarithms.

²⁹India currently has many ‘low cost’ private schools. But that is a relatively recent phenomenon. Such private schools were not very common before 2000s.

5.2.2 Anti-Mandal Agitations

The anti-Mandal agitations of 1989–90 were triggered by the Indian government’s decision to implement the recommendations of the Mandal Commission, which proposed reserving a significant share of public sector jobs and educational opportunities for Other Backward Classes (OBCs). This announcement sparked intense protests and widespread civil unrest, particularly in the northern states of India, including Uttar Pradesh and Bihar (Balagopal (1990)). To ensure that our results are not confounded by these contemporaneous disruptions—which could independently affect educational or labor market outcomes—we conduct a robustness check excluding the most affected states. Specifically, we drop Uttar Pradesh (including present-day Uttarakhand) and Bihar (including present-day Jharkhand) from our sample of control states. This allows us to isolate the effects of the counterinsurgency policy in AP from the broader civil disturbances related to the Mandal agitation. As shown in Table A.10, the results remain robust, lending further credibility to our main findings.

5.2.3 Role of State Education Policy

An important alternative explanation for our results is that they may have been driven not by the counterinsurgency policy, but by contemporaneous changes in educational policy in AP. If the state government had placed greater emphasis on education during the same period, improvements in outcomes could plausibly stem from enhanced educational investment rather than the counterinsurgency intervention itself.

To examine this possibility, we turn to trends in public spending and budgetary allocations for education. Public investment in education is a key driver of improvements in literacy and schooling outcomes, and budget allocations reflect the government’s prioritization of the sector.

Reddy and Rao (2003) analyze these trends for AP between 1980–81 and 1995–96. The findings show that public expenditure on education as a share of Net State Domestic Product (NSDP) remained stagnant at around 3 percent. Moreover, education’s share in the total state budget declined by nearly 2 percentage points—from 18.4 percent to 16.6 percent—over this period. Notably, AP’s budgetary allocation to education was not only lower than the national average, but also lagged behind other southern states.

Further analysis of the education budget reveals that primary education consistently received a smaller share of the state budget compared to central government allocations; further, the share declined from 8.5 per cent to 6.9 per cent between 1985 and 1995. Additionally, within the state’s education budget, the share allocated to primary education declined after 1985–86 (from 46.1 per cent to 41.7 per cent). Per-student expenditure, both

overall and at the elementary level, also showed a downward trend beginning in the mid-1980s.

These patterns strongly suggest that the state's education policy was not undergoing a significant positive shift during the relevant period. Therefore, it is unlikely that our findings are driven by an expansion of educational investment or prioritization by the AP government.

5.2.4 Political Power Shifts

Another mechanism that could be driving our results is political power shifts in AP. The Congress defeating the Telugu Desam Party (TDP) marked a significant political shift in the 1989 Loksabha and Assembly elections. While it might seem that this political transition may have affected education and employment outcomes, there are reasons to rule it out as a potential channel. The fall of TDP was largely due to anti-people policies, dissatisfaction among government employees, and growing unrest among unemployed youth, rather than a strong education or job creation agenda of the Congress. Although young voters played a role in the election outcome, their support for Congress was more of a protest than an endorsement of any specific education or employment policy (Rao and Ram, 1990). Further, as discussed in Rao (1998), AP's economy was in serious crisis until the mid-1990s, as indicated by all standard macroeconomic indicators implying that the shift in political power in 1989 was not associated with improvement of the state economy. In fact, some observers note, the economic distress experienced by AP in the early 1990s contributed to the rise of the TDP as a political force opposing the Congress Party, eventually leading to the TDP's return to power in 1994 (see, e.g., (Rao, 1998)). Finally, if political change had been the key factor, we would expect similar changes in individual outcomes across other Naxalite-affected states, where Congress or Janata Dal also came to power in 1989.³⁰

5.2.5 IT Boom

As noted above, TDP returned to power in 1994, and on 1 September 1995, N. Chandrababu Naidu was elected as the national president of the TDP. Under his leadership as Chief Minister from 1995 to 2004, AP underwent a significant technological transformation. In the late 1990s and early 2000s, the state emerged as a hub of the IT revolution, spurred by the development of HITEC City in Hyderabad (inaugurated in November 1998), strategic investments in infrastructure, a business-friendly environment, and a growing emphasis on IT skills, supported by a rapid expansion of engineering colleges.

³⁰These states include Bihar, Uttar Pradesh, and Karnataka

While this transformation is noteworthy, it occurred toward the end of our study period—by which point even the youngest cohort in our treatment group had completed most of their school-age years. Thus, it is unlikely to be a major confounding factor. Nevertheless, to address this concern more rigorously, we exclude individuals born after 1982 from our sample and re-estimate our baseline models. These individuals would have completed their school-age years by 1998–1999, before the IT boom could plausibly influence household decisions related to education. The results remain robust and are not meaningfully different from our main estimates (see Table A.11).

To further strengthen this argument, we conduct an additional test by restricting our control group to Karnataka, which is widely recognized as the epicenter of India’s IT revolution during the 1990s Oster and Steinberg (2013). Bengaluru, in particular, emerged as the leading destination for IT firms and professionals well before similar developments gained momentum in AP. Since both states were exposed to the broader IT-led economic changes—and if anything, Karnataka was ahead of AP—the estimated treatment effect cannot be attributed to the IT boom. The results, reported in Table A.12, are consistent with our main estimates suggesting the IT sector’s growth is unlikely to be driving the observed gains in educational and labor market outcomes.³¹

6 Conclusion

We study the effects of exposure to counterinsurgency policy during school-age years on human capital and labor market outcomes in India. While the negative consequences of growing up in conflict zones are well established, there is limited evidence on how state responses to insurgency shape long-term economic trajectories.

To address this gap, we focus on the 1989 introduction of the Greyhounds—an elite commando force created to fight Naxalite insurgents—in AP. At the time, AP was the only Naxalite-affected state to implement such a focused counterinsurgency strategy. DID estimates suggest that exposure to the Greyhounds policy during school-age years led to significantly higher educational attainment, along with improved labor market outcomes and household socioeconomic status in adulthood. These results remain robust across a range of robustness checks and falsification tests. Further analysis suggests that a key driver

³¹It is also important to emphasize that our earlier findings on household education expenditure, the likelihood of private school enrollment, and the number of new private schools—each of which reflects changes in household and provider expectations—are based on data from the period before 1996. These outcomes, therefore, could not have been influenced by the subsequent growth of the service sector, further reinforcing the interpretation that improved security, rather than broader economic changes, drove the observed increases in educational investment.

of these effects is increased household investment in education, likely resulting from improved local security conditions.

Our findings demonstrate that counterinsurgency efforts, when effectively targeted and sustained, can generate positive spillovers well beyond their immediate security objectives. By reducing the threat of violence during critical periods of childhood and adolescence, the Greyhounds policy appears to have enabled greater household investment in education, with lasting benefits for individuals' economic mobility.

This underscores the importance of viewing peacebuilding and development not as sequential but mutually reinforcing goals. In conflict-affected settings, efforts to restore order and build institutional capacity can directly shape long-term human development outcomes. As such, security policies should be designed with an eye toward their developmental consequences, particularly for young children whose life trajectories are highly sensitive to the conditions they experience while growing up.

The study also offers insights relevant to United Nations' Sustainable Development Goal (SDG) 16, which calls for promoting peaceful and inclusive societies and building effective, accountable institutions. Our results suggest that targeted interventions to restore public order and state capacity can contribute to these goals, not only by reducing violence but by creating conditions for inclusive human development. Future work should explore whether these findings hold across different contexts and how such gains can be sustained over time.

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Tables

Table 1: Summary Statistics

	N	Mean	SD
Education			
Literacy (=1 if literate)	45755	0.53	0.50
Attended school (=1 if yes)	45755	0.54	0.50
Completed years	45755	4.27	4.82
Secondary (=1 if yes)	45755	0.08	0.27
Higher secondary (=1 if yes)	45755	0.05	0.21
Bachelors & above (=1 if yes)	45755	0.05	0.22
English ability (=1 if yes)	45753	0.16	0.37
Labor Market Outcomes			
Any work (=1 if yes)	45755	0.60	0.49
Agriculture work (=1 if yes)	28842	0.27	0.44
Non-agriculture work (=1 if yes)	28842	0.25	0.43
Salary work (=1 if yes)	28842	0.13	0.34
Business work (=1 if yes)	28842	0.14	0.35
Farm work (=1 if yes)	28842	0.49	0.50
Work days /year	45755	142.28	132.09
Work hours /year	45755	973.84	1069.30
ln(Cashwages)	19296	9.62	1.25
ln(Earnings)	19300	9.73	1.21
Household Socioeconomic Status			
ln(HH wage salary)	32819	10.54	1.17
ln(Income)	45055	11.16	1.08
ln(Income per capita)	45055	9.32	1.00
Poverty (=1 if yes)	45733	0.27	0.44
Total household assets	45735	13.06	5.71
Exposure Eligibility			
Younger Cohort (=1 if yes)	45755	0.03	0.17
Share	45755	0.01	0.07
Demographics			
Male (=1 if yes)	45755	0.49	0.50
Married (=1 if yes)	45755	0.82	0.39
Age	45755	47.59	14.380
Brahmin & other Upper Caste (=1 if yes)	45755	0.26	0.44
OBC (=1 if yes)	45755	0.45	0.50
SC (=1 if yes)	45755	0.20	0.40
ST (=1 if yes)	45755	0.08	0.27
Hindu (=1 if yes)	45755	0.85	0.36
Muslim (=1 if yes)	45755	0.13	0.33
Other Religion (=1 if yes)	45755	0.03	0.16
Household size	45755	7.07	3.64

Note: This table reports sample means and standard deviations (SD) for key variables, using authors' own calculations. For binary indicators, the mean represents the proportion of the sample with value 1. Continuous variables such as years of schooling, work days/year, work hours/year, household assets, share, age, and household size are reported in their natural units. Logged variables (ln(Cashwages), ln(Earnings), ln(Household wage salary), ln(Income), and ln(Income per capita)) are in natural logarithms. Sample sizes vary due to item-specific nonresponse.

Table 2: Effects of Greyhounds on Education

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Literacy	Attended School	Completed Years	Secondary	Higher Secondary	Bachelors & above	English Ability
Panel A: Whether Exposed							
YC X AP	0.063*** (0.021)	0.052** (0.025)	0.593** (0.287)	0.036** (0.015)	-0.001 (0.009)	0.031** (0.012)	0.061*** (0.021)
Observations	45755	45755	45755	45755	45755	45755	45753
R-squared	0.251	0.264	0.290	0.046	0.039	0.057	0.098
Panel B: Share exposed							
Share X AP	0.175*** (0.035)	0.165*** (0.040)	2.146*** (0.428)	0.120*** (0.034)	0.006 (0.023)	0.099*** (0.031)	0.196*** (0.025)
Observations	45755	45755	45755	45755	45755	45755	45753
R-squared	0.251	0.264	0.290	0.047	0.039	0.057	0.098
Demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Demographics include Gender, Caste, Religion, Marital Status, Household size. Birth year fixed effects, and State fixed effects are included in all regression specifications. Survey weights are used in all regressions. Robust standard error in parenthesis, corrected for clustering at the state-by-birth year level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Effects of Greyhounds on Workforce Participation and Labor Supply

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Any work	Agriculture	Non-agriculture	Salary work	Business work	Farm work	Work Days	Work Hours
Panel A: Whether Exposed								
YC X AP	0.125*** (0.021)	-0.050** (0.025)	-0.062*** (0.023)	0.077*** (0.020)	0.001 (0.013)	0.034 (0.022)	28.187*** (5.424)	201.538*** (44.572)
Observations	45755	28840	28840	28840	28840	28840	45755	45755
R-squared	0.325	0.139	0.107	0.046	0.051	0.131	0.346	0.343
Panel B: Share exposed								
Share X AP	0.243*** (0.047)	-0.150*** (0.040)	-0.081 (0.050)	0.185*** (0.038)	0.007 (0.034)	0.044 (0.040)	60.641*** (10.927)	425.242*** (96.146)
Observations	45755	28840	28840	28840	28840	28840	45755	45755
R-squared	0.325	0.139	0.107	0.046	0.051	0.131	0.346	0.343
Demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Demographics include Gender, Caste, Religion, Marital Status, Household size. Birth year fixed effects, and State fixed effects are included in all regression specifications. Survey weights are used in all regressions. Robust standard error in parenthesis, corrected for clustering at the state-by-birth year level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Effects of Greyhounds on Labor Market Earnings and Household Socioeconomic Status

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	ln (Cash Wages)	ln (Earnings)	ln (HH Wage & Salary)	ln (Total Income)	ln (Income Per Capita)	Poverty	HH Assets
Panel A: Whether Exposed							
YC X AP	0.157** (0.064)	0.159** (0.064)	0.122*** (0.041)	0.140*** (0.047)	0.119*** (0.046)	-0.056*** (0.012)	0.560*** (0.175)
Observations	19295	19299	32818	45055	45055	45733	45735
R-squared	0.258	0.250	0.120	0.189	0.092	0.096	0.261
Panel B: Share exposed							
Share X AP	0.401*** (0.120)	0.407*** (0.123)	0.174** (0.087)	0.236*** (0.088)	0.163* (0.091)	-0.088** (0.038)	0.725** (0.354)
Observations	19295	19299	32818	45055	45055	45733	45735
R-squared	0.258	0.250	0.120	0.188	0.092	0.096	0.261
Demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Demographics include Gender, Caste, Religion, Marital Status, Household size. Birth year fixed effects, and State fixed effects are included in all regression specifications. Survey weights are used in all regressions. Robust standard error in parenthesis, corrected for clustering at the state-by-birth year level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Effects of Greyhounds on Confidence in Institutions

	(1)	(2)	(3)	(4)	(5)	(6)
	Politicians	Military	Police	State Govt.	News	Panchayats
Panel A: Whether Exposed						
YC X AP	0.023 (0.025)	-0.005 (0.009)	0.037** (0.016)	0.042** (0.017)	0.006 (0.014)	0.003 (0.012)
Observations	45755	45755	45755	45755	45755	45755
R-squared	0.041	0.028	0.057	0.040	0.013	0.029
Panel B: Share exposed						
Share X AP	0.067 (0.052)	-0.011 (0.019)	0.075** (0.037)	0.054 (0.034)	0.011 (0.041)	-0.008 (0.026)
Observations	45755	45755	45755	45755	45755	45755
R-squared	0.041	0.028	0.057	0.040	0.013	0.029
Demographics	Yes	Yes	Yes	Yes	Yes	Yes
Birth year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes

Note: Demographics include Gender, Caste, Religion, Marital Status, Household size. Birth year fixed effects, State fixed effects and State-specific linear trends in birth year decade are included in all regression specifications. Survey weights are used in all regressions. Robust standard error in parenthesis, corrected for clustering at the state-by-birth year level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Effects of Greyhounds on Reported Number of Conflicts and Insurgencies

	(1)	(2)	(3)
	Total Conflicts	Specific Conflicts	Number of Successful Attacks
Greyhounds	-253.051*** (75.833)	-85.577*** (26.972)	-3.992 (5.478)
Mean of dependent variable	422.190	159.846	13.374
Observations	210	208	283
R-squared	0.789	0.794	0.555
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Note: Total Conflicts includes all type of conflicts events reported within the states. Specific conflicts include conflicts related to Insurgents, communist party, government, police forces, officers, criminal investigative units, protective agencies, criminals, civilians, military, radical, rebels, refugees, unidentified armed force (UAF), unidentified state actors (UIS). Total state population, year fixed effects and state fixed effects are included in all regression specifications. Robust standard errors clustered at state-year level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Effects of Greyhounds on Household Expenditure on Education

	(1)	(2)	(3)	(4)	(5)
	Tuition fees (school/college)	Other educational expenses	Stationary articles	Books & journals	Total
Panel A: Expenditure					
AP X Post	0.350*** (0.113)	0.170** (0.076)	0.132** (0.061)	0.071 (0.057)	0.286*** (0.056)
Observations	27008	31042	52886	34382	145198
R-squared	0.415	0.247	0.335	0.190	0.422
Panel B: Share of Expenditure					
AP X Post	0.007 (0.006)	0.010* (0.006)	0.018*** (0.005)	0.004 (0.004)	0.042*** (0.011)
Observations	27008	31041	52884	34380	145193
R-squared	0.186	0.048	0.086	0.065	0.192
Controls	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes
Sub Round FE	Yes	Yes	Yes	Yes	Yes

Note: All dependent variables are in natural logarithm. Controls include caste, household size, residence (rural/urban), total monthly household expenditure. Column 5 reports results for total education expenditure, calculated as the sum of household spending on books, stationery, tuition fees, and other education-related items (Columns 1–4). Results using the share of total household expenditure allocated to education are qualitatively similar and available upon request. All specifications include state fixed effects and NSS sub-round fixed effects. Robust standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Effects of Greyhounds on Number of School Establishment and Enrollment

Panel I: Number of Schools Established				
	Public	Private	ln(Public)	ln(Private)
AP X Post	-24.80 (289.5)	183.5*** (63.06)	0.284 (0.181)	0.139* (0.0721)
Mean of Dependent Variable	856.3	206.9	6.331	4.212
R-squared	0.382	0.535	0.510	0.929
Observations	561	561	561	558
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Panel II: Enrolment by School-Type				
	Public	Private		
AP X Post	-0.139** (0.0337)	0.0650* (0.0213)		
Mean of Dependent Variable	0.639	0.265		
R-squared	0.174	0.190		
Observations	98171	98171		
Controls	Yes	Yes		
State FE	Yes	Yes		
Sub Round FE	Yes	Yes		

Notes: Controls include gender, caste, household size, age and rural/urban. State and time (year and sub round) fixed effects are included in all regressions. Survey weights are used in the regressions in panel B. Robust standard error in parentheses, corrected for clustering at the state-year level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figures

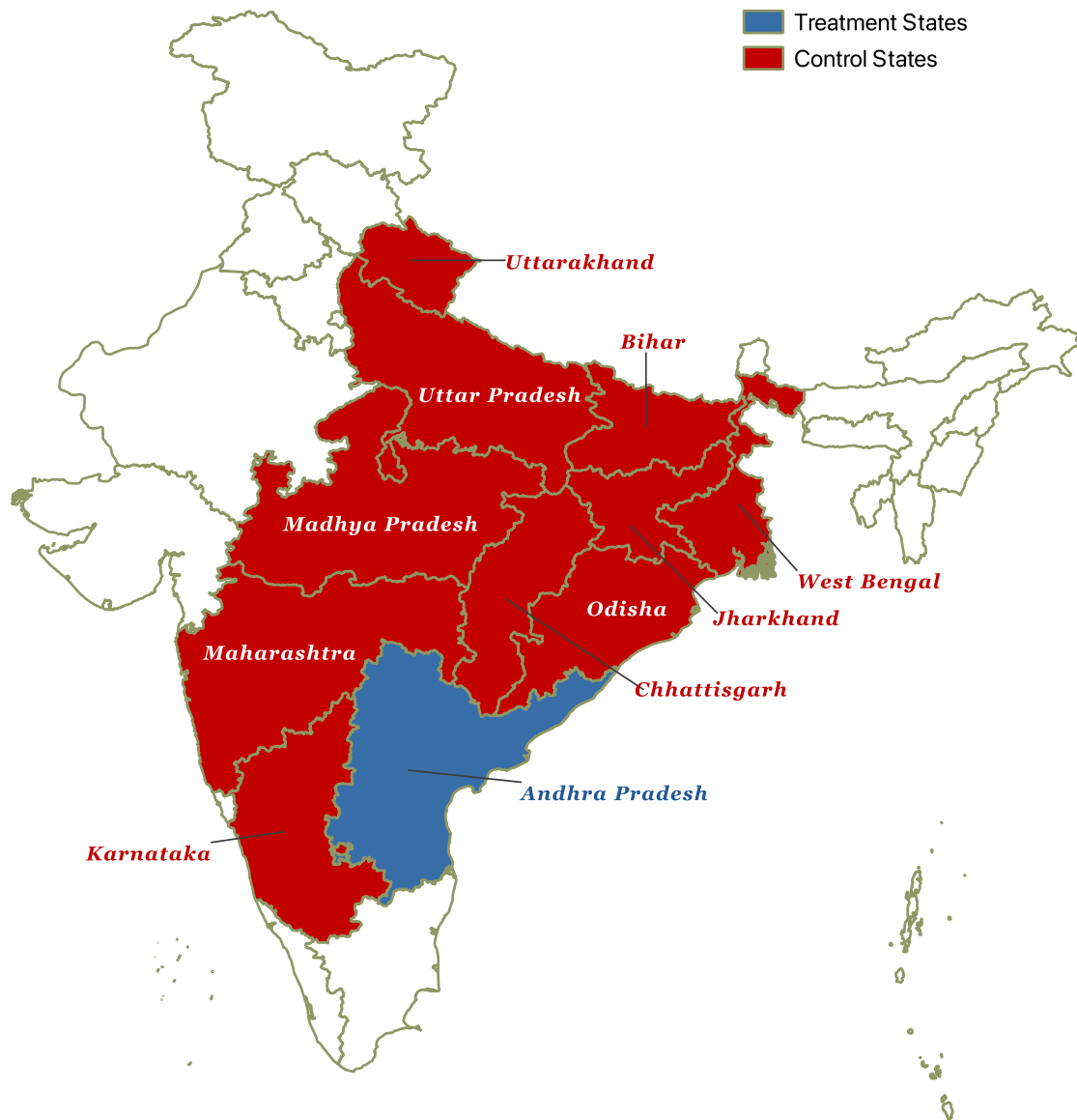


Figure 1: States of India affected by Naxalite Insurgency Prior to 2000

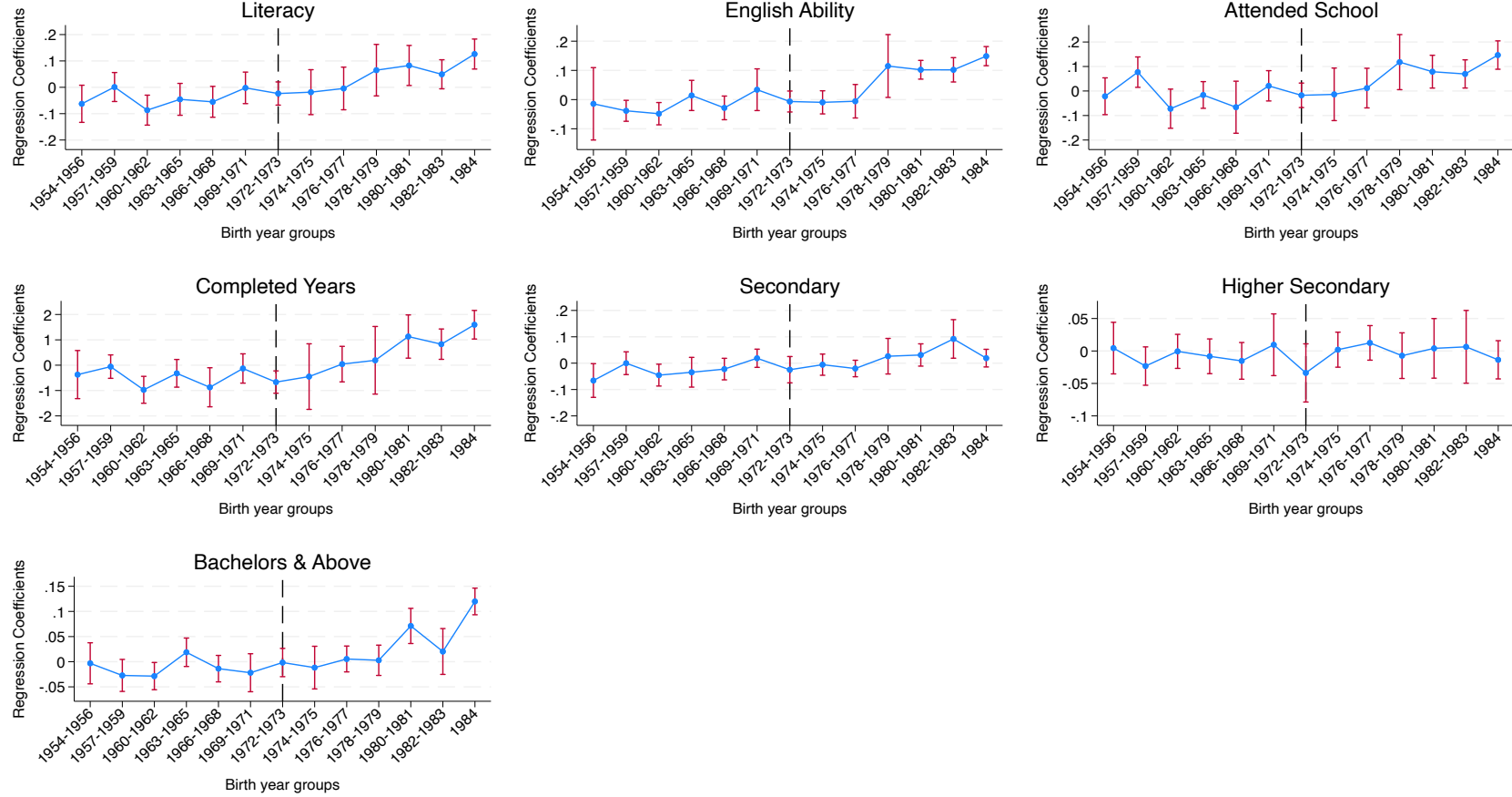


Figure 2: **Event Study Plot: Education**

Note: This figure shows the estimated impact of exposure to the 1989 Greyhounds counterinsurgency policy on education, using the event study specification outlined in equation 2. Estimates are plotted by birth year bins relative to the oldest bin, with 95% confidence intervals. Individuals are classified as treated if they are born between 1972 and 1984, capturing those potentially affected by the policy during schooling years. The control group comprises individuals who were born before 1972 and thus not exposed to the policy during school-age year.

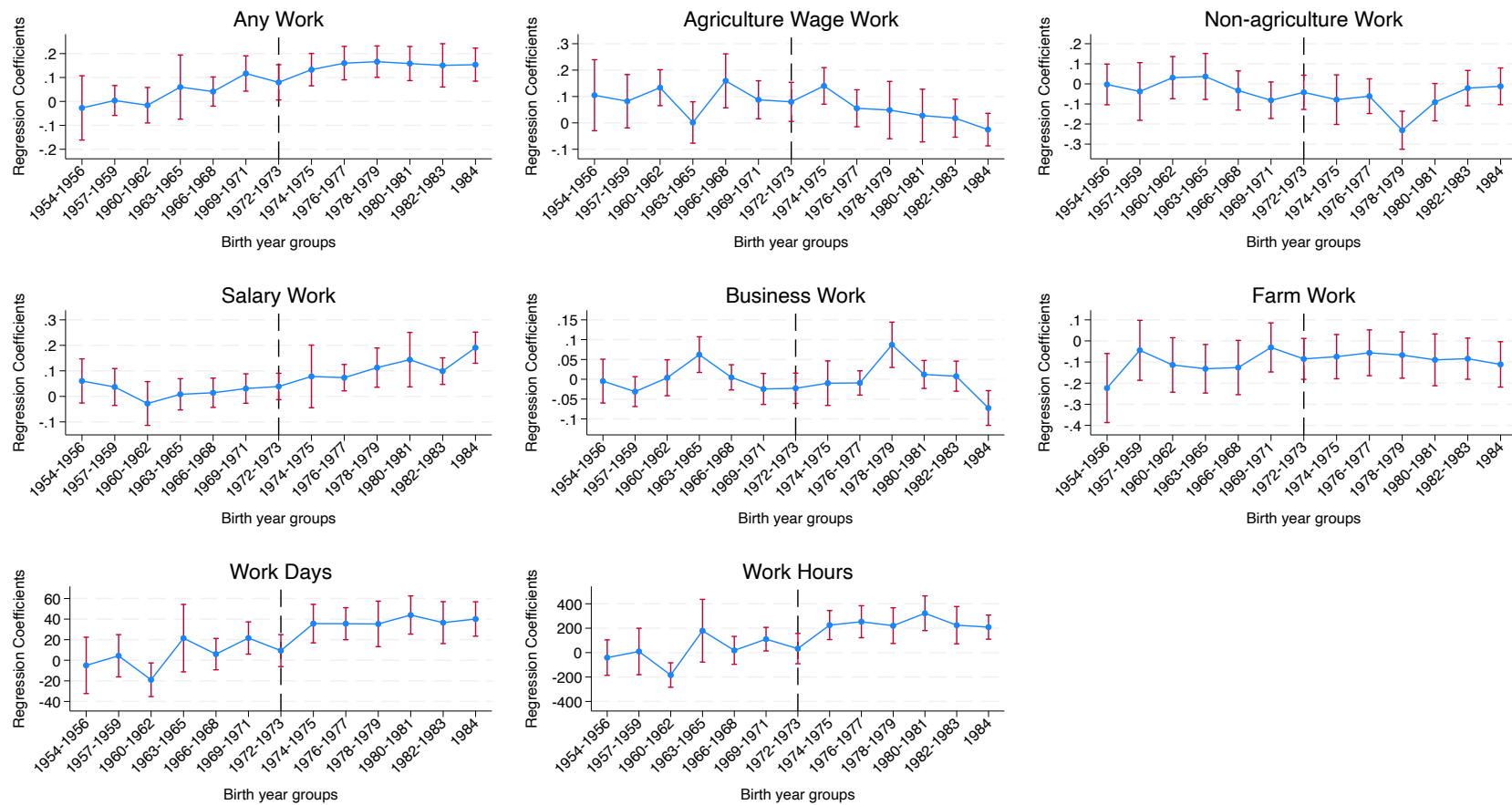


Figure 3: Event Study Plot: Workforce Participation and Labor Supply

Note: This figure shows the estimated impact of exposure to the 1989 Greyhounds counterinsurgency policy on workforce participation and labor supply, using the event study specification outlined in equation 2. Estimates are plotted by birth year bins relative to the oldest bin, with 95% confidence intervals. Individuals are classified as treated if they are born between 1972 and 1984, capturing those potentially affected by the policy during schooling years. The control group comprises individuals who were born before 1972 and thus not exposed to the policy during school-age year.

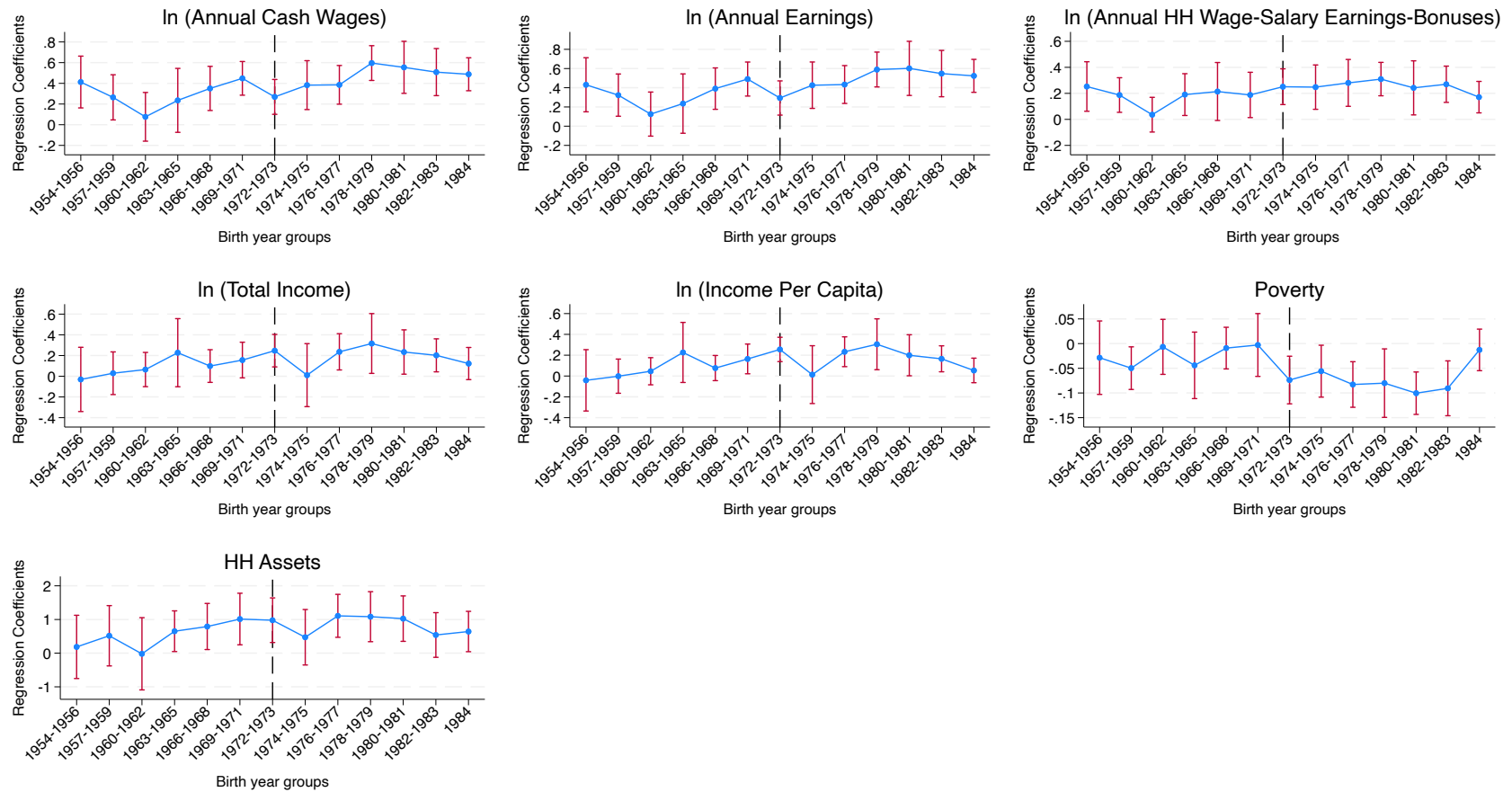


Figure 4: **Event Study Plot: Labor Market Earnings and Household Socioeconomic Status**

Note: This figure shows the estimated impact of exposure to the 1989 Greyhounds counterinsurgency policy on labor market earnings and household socioeconomic status, using the event study specification outlined in equation 2. Estimates are plotted by birth year bins relative to the oldest bin, with 95% confidence intervals. Individuals are classified as treated if they are born between 1972 and 1984, capturing those potentially affected by the policy during schooling years. The control group comprises individuals who were born before 1972 and thus not exposed to the policy during school-age year.

Appendix

A Appendix - Tables

Table A.1: Effects using Trends

Panel I: Education								
	Literacy	Attended School	Completed Years	Secondary	Higher Secondary	Bachelors+	English Ability	
A. Whether Exposed								
YC X AP	0.079*** (0.027)	0.064** (0.027)	0.085** (0.041)	1.038*** (0.389)	0.029 (0.020)	0.006 (0.013)	0.043*** (0.013)	
Observations	45755	45753	45755	45755	45755	45755	45755	
R-squared	0.253	0.099	0.266	0.294	0.048	0.040	0.058	
B. Share Exposed								
Share X AP	0.216*** (0.043)	0.239*** (0.030)	0.248*** (0.057)	3.375*** (0.502)	0.133*** (0.044)	0.025 (0.029)	0.137*** (0.037)	
Observations	45755	45753	45755	45755	45755	45755	45755	
R-squared	0.253	0.100	0.267	0.295	0.048	0.040	0.058	
Panel II: Workforce Participation and Labor Supply								
	Any work	Agri	Non-agri	Salary	Business	Farm	Work Days	Work Hours
A. Whether Exposed								
YC X AP	0.065** (0.027)	-0.027 (0.045)	-0.016 (0.031)	0.084*** (0.026)	-0.022 (0.019)	-0.013 (0.036)	22.246*** (8.110)	140.768** (66.268)
Observations	45755	28840	28840	28840	28840	28840	45755	45755
R-squared	0.329	0.140	0.109	0.046	0.052	0.133	0.349	0.345
B. Share Exposed								
Share X AP	0.104** (0.046)	-0.145** (0.064)	0.054 (0.052)	0.204*** (0.046)	-0.027 (0.044)	-0.064 (0.055)	45.868*** (12.320)	284.629*** (109.827)
Observations	45755	28840	28840	28840	28840	28840	45755	45755
R-squared	0.329	0.140	0.109	0.047	0.052	0.133	0.349	0.345
Panel III: Labor Market Earnings and Household Socioeconomic Status								
	ln(Cash Wages)	ln(Earnings)	ln(HH Wage+Salary)	ln(Total Income)	ln(Income Per Cap)	Poverty	HH Assets	
A. Whether Exposed								
YC X AP	0.029 (0.080)	0.023 (0.080)	0.035 (0.065)	0.036 (0.059)	0.047 (0.060)	-0.064*** (0.017)	-0.141 (0.252)	
Observations	19295	19299	32818	45055	45055	45733	45735	
R-squared	0.259	0.250	0.121	0.189	0.092	0.097	0.262	
B. Share Exposed								
Share X AP	0.233 (0.153)	0.230 (0.156)	-0.050 (0.106)	-0.001 (0.108)	-0.044 (0.111)	-0.067 (0.047)	-0.842** (0.377)	
Observations	19295	19299	32818	45055	45055	45733	45735	
R-squared	0.259	0.251	0.121	0.189	0.092	0.097	0.262	

Notes: All regression specifications include demographic controls (Gender, Caste, Religion, Marital Status, Household Size), birth year fixed effects, state fixed effects, and state-specific linear trends in birth year decade. Survey weights are used in all regressions. Robust standard error in parentheses, corrected for clustering at the state-by-birth year level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.2: Effects excluding 18-24 years cohort

Panel I: Education								
	Literacy	Attended School	Completed Years	Secondary	Higher Secondary	Bachelors+	English Ability	
A. Whether Exposed								
YC X AP	0.061*** (0.022)	0.042* (0.025)	0.471* (0.284)	0.038** (0.016)	-0.002 (0.009)	0.028** (0.012)	0.062*** (0.021)	
Observations	38353	38353	38353	38353	38353	38353	38351	
R-squared	0.260	0.274	0.298	0.048	0.039	0.056	0.101	
B. Share Exposed								
Share X AP	0.172*** (0.036)	0.152*** (0.038)	2.011*** (0.422)	0.125*** (0.035)	0.006 (0.023)	0.096*** (0.032)	0.201*** (0.025)	
Observations	38353	38353	38353	38353	38353	38353	38351	
R-squared	0.260	0.275	0.298	0.048	0.039	0.057	0.102	
Panel II: Workforce Participation and Labor Supply								
	Any work	Agri	Non-agri	Salary	Business	Farm	Work Days	Work Hours
A. Whether Exposed								
YC X AP	0.154*** (0.020)	-0.049** (0.025)	-0.086*** (0.025)	0.075*** (0.023)	0.006 (0.014)	0.056** (0.026)	33.411*** (5.470)	240.936*** (43.758)
Observations	38353	23302	23302	23302	23302	23302	38353	38353
R-squared	0.330	0.135	0.109	0.050	0.049	0.135	0.350	0.346
B. Share Exposed								
Share X AP	0.276*** (0.050)	-0.150*** (0.037)	-0.098* (0.055)	0.177*** (0.043)	0.014 (0.036)	0.065 (0.043)	66.944*** (11.502)	473.430*** (99.279)
Observations	38353	23302	23302	23302	23302	23302	38353	38353
R-squared	0.330	0.135	0.109	0.051	0.049	0.135	0.350	0.346
Panel III: Labor Market Earnings and Household Socioeconomic Status								
	ln(Cash Wages)	ln(Earnings)	ln(HH Wage+Salary)	ln(Total Income)	ln(Income Per Cap)	Poverty	HH Assets	
A. Whether Exposed								
YC X AP	0.247*** (0.070)	0.249*** (0.070)	0.154*** (0.042)	0.182*** (0.055)	0.157*** (0.051)	-0.055*** (0.013)	0.810*** (0.178)	
Observations	15416	15420	27207	37767	37767	38335	38337	
R-squared	0.264	0.256	0.122	0.194	0.091	0.095	0.258	
B. Share Exposed								
Share X AP	0.514*** (0.130)	0.521*** (0.132)	0.207** (0.090)	0.285*** (0.102)	0.203** (0.102)	-0.081** (0.039)	1.026*** (0.389)	
Observations	15416	15420	27207	37767	37767	38335	38337	
R-squared	0.264	0.257	0.121	0.194	0.091	0.095	0.258	

Notes: All regression specifications include demographic controls (Gender, Caste, Religion, Marital Status, Household Size), birth year fixed effects, and state fixed effects. Survey weights are used in all regressions. Robust standard error in parentheses, corrected for clustering at the state-by-birth year level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.3: Effects using bordering states of Andhra Pradesh

Panel I: Education								
	Literacy	Attended School	Completed Years	Secondary	Higher Secondary	Bachelors+	English Ability	
A. Whether Exposed								
YC X AP	0.016 (0.016)	0.005 (0.019)	-0.134 (0.231)	0.015 (0.013)	-0.018** (0.008)	0.025** (0.011)	0.030* (0.017)	
Observations	23633	23633	23633	23633	23633	23633	23632	
R-squared	0.260	0.278	0.305	0.049	0.047	0.047	0.096	
B. Share Exposed								
Share X AP	0.059* (0.030)	0.053* (0.031)	0.623 (0.413)	0.071** (0.036)	-0.039* (0.020)	0.099*** (0.027)	0.138*** (0.027)	
Observations	23633	23633	23633	23633	23633	23633	23632	
R-squared	0.260	0.278	0.305	0.050	0.047	0.048	0.097	
Panel II: Workforce Participation and Labor Supply								
	Any work	Agri	Non-agri	Salary	Business	Farm	Work Days	Work Hours
A. Whether Exposed								
YC X AP	0.083*** (0.018)	-0.062*** (0.023)	-0.053** (0.021)	0.077*** (0.018)	0.004 (0.013)	0.000 (0.020)	17.769*** (4.744)	130.533*** (40.839)
Observations	23633	15822	15822	15822	15822	15822	23633	23633
R-squared	0.308	0.116	0.090	0.047	0.054	0.145	0.326	0.323
B. Share Exposed								
Share X AP	0.157*** (0.044)	-0.144*** (0.043)	-0.084* (0.047)	0.181*** (0.038)	0.020 (0.030)	-0.025 (0.036)	38.976*** (10.585)	289.637*** (94.343)
Observations	23633	15822	15822	15822	15822	15822	23633	23633
R-squared	0.308	0.116	0.090	0.048	0.054	0.145	0.326	0.323
Panel III: Labor Market Earnings and Household Socioeconomic Status								
	ln(Cash Wages)	ln(Earnings)	ln(HH Wage+Salary)	ln(Total Income)	ln(Income Per Cap)	Poverty	HH Assets	
A. Whether Exposed								
YC X AP	0.176*** (0.057)	0.173*** (0.057)	0.097** (0.038)	0.105** (0.042)	0.083** (0.041)	-0.028** (0.014)	0.518*** (0.186)	
Observations	11047	11048	17644	23201	23201	23612	23614	
R-squared	0.316	0.294	0.177	0.218	0.075	0.154	0.250	
B. Share Exposed								
Share X AP	0.372*** (0.100)	0.375*** (0.105)	0.106 (0.082)	0.180** (0.077)	0.125 (0.076)	-0.036 (0.034)	0.535 (0.352)	
Observations	11047	11048	17644	23201	23201	23612	23614	
R-squared	0.316	0.294	0.177	0.217	0.075	0.154	0.250	

Notes: All regression specifications include demographic controls (Gender, Caste, Religion, Marital Status, Household Size), birth year fixed effects, and state fixed effects. Survey weights are used in all regressions. Robust standard error in parentheses, corrected for clustering at the state-by-birth year level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.4: Falsification Test

Panel I: Education								
	Literacy	Attended School	Completed Years	Secondary	Higher Secondary	Bachelors+	English Ability	
Falsely Treated X AP	-0.041	-0.067	-0.627*	0.020	-0.026	-0.018*	-0.052	
	(0.038)	(0.041)	(0.373)	(0.014)	(0.025)	(0.011)	(0.039)	
Observations	5751	5751	5751	5751	5751	5751	5751	
R-squared	0.280	0.290	0.272	0.101	0.026	0.037	0.108	
Panel II: Workforce Participation and Labor Supply								
	Any work	Agri	Non-agri	Salary	Business	Farm	Work Days	Work Hours
Falsely Treated X AP	0.014	0.010	-0.014	-0.355	0.105	0.256	-6.590	20.236
	(0.040)	(0.251)	(0.127)	(0.247)	(0.079)	(0.162)	(9.918)	(74.497)
Observations	5751	1293	1293	1293	1293	1293	5751	5751
R-squared	0.184	0.219	0.098	0.076	0.137	0.189	0.200	0.152
Panel III: Labor Market Earnings and Household Socioeconomic Status								
	ln(Cash Wages)	ln(Earnings)	ln(HH Wage+Salary)	ln(Total Income)	ln(Income Per Cap)	Poverty	HH Assets	
Falsely Treated X AP	0.473	0.540	0.158	-0.138	-0.108	0.034	-0.003	
	(0.449)	(0.451)	(0.171)	(0.171)	(0.149)	(0.030)	(0.509)	
Observations	576	576	3415	5657	5657	5745	5750	
R-squared	0.280	0.277	0.156	0.199	0.081	0.101	0.255	

Notes: All regression specifications include demographic controls (Gender, Caste, Religion, Marital Status, Household Size), birth year fixed effects, and state fixed effects. Survey weights are used in all regressions. Robust standard error in parentheses, corrected for clustering at the state-by-birth year level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.5: Effects excluding Bihar (and present-day Jharkhand)

Panel I: Education								
	Literacy	Attended School	Completed Years	Secondary	Higher Secondary	Bachelors+	English Ability	
A. Whether Exposed								
YC X AP	0.061*** (0.020)	0.050** (0.024)	0.512* (0.275)	0.036** (0.016)	0.000 (0.009)	0.028** (0.011)	0.062*** (0.019)	
Observations	41240	41240	41240	41240	41240	41240	41239	
R-squared	0.250	0.264	0.293	0.046	0.039	0.056	0.096	
B. Share Exposed								
Share X AP	0.166*** (0.034)	0.158*** (0.037)	1.895*** (0.440)	0.117*** (0.034)	0.008 (0.022)	0.089*** (0.031)	0.191*** (0.025)	
Observations	41240	41240	41240	41240	41240	41240	41239	
R-squared	0.250	0.265	0.294	0.046	0.039	0.057	0.096	
Panel II: Workforce Participation and Labor Supply								
	Any work	Agri	Non-agri	Salary	Business	Farm	Work Days	Work Hours
A. Whether Exposed								
YC X AP	0.121*** (0.021)	-0.052** (0.024)	-0.063*** (0.022)	0.077*** (0.021)	-0.001 (0.013)	0.031 (0.022)	27.943*** (5.415)	194.963*** (43.713)
Observations	41240	26446	26446	26446	26446	26446	41240	41240
R-squared	0.324	0.133	0.098	0.042	0.057	0.140	0.337	0.337
B. Share Exposed								
Share X AP	0.236*** (0.049)	-0.147*** (0.040)	-0.089* (0.049)	0.189*** (0.042)	-0.000 (0.033)	0.039 (0.041)	59.716*** (11.481)	407.569*** (95.333)
Observations	41240	26446	26446	26446	26446	26446	41240	41240
R-squared	0.324	0.134	0.098	0.042	0.057	0.139	0.337	0.337
Panel III: Labor Market Earnings and Household Socioeconomic Status								
	ln(Cash Wages)	ln(Earnings)	ln(HH Wage+Salary)	ln(Total Income)	ln(Income Per Cap)	Poverty	HH Assets	
A. Whether Exposed								
YC X AP	0.145** (0.064)	0.147** (0.064)	0.110** (0.046)	0.134*** (0.046)	0.110** (0.045)	-0.051*** (0.013)	0.526*** (0.177)	
Observations	17774	17777	29835	40573	40573	41218	41220	
R-squared	0.258	0.250	0.125	0.191	0.082	0.098	0.237	
B. Share Exposed								
Share X AP	0.352*** (0.118)	0.364*** (0.122)	0.135 (0.091)	0.218** (0.088)	0.141 (0.091)	-0.079* (0.040)	0.610* (0.345)	
Observations	17774	17777	29835	40573	40573	41218	41220	
R-squared	0.258	0.250	0.125	0.191	0.082	0.097	0.237	

Notes: All regression specifications include demographic controls (Gender, Caste, Religion, Marital Status, Household Size), birth year fixed effects, and state fixed effects. Survey weights are used in all regressions. Robust standard error in parentheses, corrected for clustering at the state-by-birth year level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.6: Effect including Kerala as a control state

Panel I: Education								
	Literacy	Attended School	Completed Years	Secondary	Higher Secondary	Bachelors+	English Ability	
A. Whether Exposed								
YC X AP	0.067*** (0.021)	0.057*** (0.017)	0.536** (0.229)	0.033*** (0.011)	-0.003 (0.011)	0.027 (0.018)	0.049*** (0.018)	
Observations	48823	48823	48823	48823	48823	48823	48820	
R-squared	0.264	0.277	0.307	0.049	0.040	0.057	0.116	
B. Share Exposed								
Share X AP	0.183*** (0.043)	0.175*** (0.038)	2.044*** (0.603)	0.114*** (0.027)	0.004 (0.022)	0.090* (0.051)	0.177*** (0.042)	
Observations	48823	48823	48823	48823	48823	48823	48820	
R-squared	0.264	0.277	0.307	0.050	0.040	0.057	0.116	
Panel II: Workforce Participation and Labor Supply								
	Any work	Agri	Non-agri	Salary	Business	Farm	Work Days	Work Hours
A. Whether Exposed								
YC X AP	0.125*** (0.020)	-0.049 (0.032)	-0.060** (0.026)	0.071*** (0.021)	0.004 (0.017)	0.034 (0.024)	27.752*** (4.179)	196.262*** (38.759)
Observations	48823	30229	30229	30229	30229	30229	48823	48823
R-squared	0.331	0.141	0.107	0.051	0.051	0.143	0.350	0.347
B. Share Exposed								
Share X AP	0.243*** (0.044)	-0.148** (0.057)	-0.077 (0.050)	0.175*** (0.054)	0.013 (0.038)	0.042 (0.042)	59.830*** (9.622)	417.465*** (79.504)
Observations	48823	30229	30229	30229	30229	30229	48823	48823
R-squared	0.330	0.142	0.107	0.051	0.051	0.143	0.350	0.347
Panel III: Labor Market Earnings and Household Socioeconomic Status								
	ln(Cash Wages)	ln(Earnings)	ln(HH Wage+Salary)	ln(Total Income)	ln(Income Per Cap)	Poverty	HH Assets	
A. Whether Exposed								
YC X AP	0.150*** (0.052)	0.152*** (0.055)	0.114** (0.047)	0.133*** (0.036)	0.113*** (0.037)	-0.055*** (0.010)	0.554*** (0.183)	
Observations	20404	20408	35173	48101	48101	48797	48799	
R-squared	0.298	0.288	0.168	0.196	0.112	0.099	0.299	
B. Share Exposed								
Share X AP	0.384*** (0.120)	0.390*** (0.115)	0.158* (0.082)	0.223** (0.090)	0.150* (0.087)	-0.085*** (0.024)	0.716* (0.432)	
Observations	20404	20408	35173	48101	48101	48797	48799	
R-squared	0.298	0.288	0.168	0.196	0.112	0.099	0.298	

Notes: All regression specifications include demographic controls (Gender, Caste, Religion, Marital Status, Household Size), birth year fixed effects, and state fixed effects. Survey weights are used in all regressions. Robust standard error in parentheses, corrected for clustering at the state-by-birth year level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.7: Effects using only Rural Sample

Panel I: Education								
	Literacy	Attended School	Completed Years	Secondary	Higher Secondary	Bachelors+	English Ability	
A. Whether Exposed								
YC X AP	0.051** (0.021)	0.035 (0.026)	0.236 (0.262)	0.030* (0.016)	-0.001 (0.011)	0.011 (0.010)	0.043** (0.020)	
Observations	35599	35599	35599	35599	35599	35599	35598	
R-squared	0.254	0.269	0.297	0.051	0.042	0.048	0.095	
B. Share Exposed								
Share X AP	0.149*** (0.040)	0.126** (0.050)	1.269*** (0.416)	0.106*** (0.036)	0.009 (0.032)	0.046* (0.025)	0.155*** (0.034)	
Observations	35599	35599	35599	35599	35599	35599	35598	
R-squared	0.254	0.269	0.297	0.052	0.042	0.048	0.095	
Panel II: Workforce Participation and Labor Supply								
	Any work	Agri	Non-agri	Salary	Business	Farm	Work Days	Work Hours
A. Whether Exposed								
YC X AP	0.133*** (0.027)	-0.025 (0.027)	-0.075*** (0.026)	0.063*** (0.018)	0.003 (0.012)	0.058** (0.025)	26.396*** (6.639)	178.319*** (50.610)
Observations	35599	23714	23714	23714	23714	23714	35599	35599
R-squared	0.317	0.162	0.126	0.035	0.039	0.138	0.333	0.328
B. Share Exposed								
Share X AP	0.277*** (0.065)	-0.096** (0.048)	-0.081 (0.061)	0.137*** (0.033)	0.017 (0.028)	0.113** (0.050)	59.566*** (15.211)	392.418*** (120.202)
Observations	35599	23714	23714	23714	23714	23714	35599	35599
R-squared	0.317	0.162	0.125	0.036	0.039	0.138	0.333	0.328
Panel III: Labor Market Earnings and Household Socioeconomic Status								
	ln(Cash Wages)	ln(Earnings)	ln(HH Wage+Salary)	ln(Total Income)	ln(Income Per Cap)	Poverty	HH Assets	
A. Whether Exposed								
YC X AP	0.100 (0.072)	0.110 (0.073)	0.100* (0.054)	0.153*** (0.059)	0.132** (0.057)	-0.063*** (0.015)	0.536*** (0.198)	
Observations	15831	15835	25705	34937	34937	35584	35586	
R-squared	0.247	0.236	0.114	0.185	0.078	0.096	0.269	
B. Share Exposed								
Share X AP	0.279* (0.155)	0.302* (0.159)	0.109 (0.132)	0.293** (0.143)	0.208 (0.149)	-0.118*** (0.039)	0.552 (0.404)	
Observations	15831	15835	25705	34937	34937	35584	35586	
R-squared	0.248	0.236	0.114	0.185	0.078	0.095	0.269	

Notes: All regression specifications include demographic controls (Gender, Caste, Religion, Marital Status, Household Size), birth year fixed effects, and state fixed effects. Survey weights are used in all regressions. Robust standard error in parentheses, corrected for clustering at the state-by-birth year level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.8: District-level treatment

Panel I: Education								
	Literacy	Attended School	Completed Years	Secondary	Higher Secondary	Bachelors+	English Ability	
A. Whether Exposed								
YC X AP High Intensity	0.059** (0.024)	0.059*** (0.020)	0.418** (0.172)	0.026*** (0.010)	-0.008 (0.008)	0.016 (0.012)	0.042*** (0.014)	
Observations	45755	45755	45755	45755	45755	45755	45753	
R-squared	0.300	0.310	0.352	0.065	0.058	0.097	0.174	
B. Share Exposed								
Share X Hi-Intensity AP Districts	0.179*** (0.050)	0.174*** (0.042)	1.515*** (0.481)	0.109*** (0.028)	-0.016 (0.017)	0.045 (0.043)	0.155*** (0.044)	
Observations	45755	45755	45755	45755	45755	45755	45753	
R-squared	0.300	0.310	0.352	0.065	0.058	0.097	0.174	
Panel II: Workforce Participation and Labor Supply								
	Any work	Agri	Non-agri	Salary	Business	Farm	Work Days	Work Hours
A. Whether Exposed								
YC X AP High Intensity	0.130*** (0.021)	-0.044 (0.029)	-0.056** (0.028)	0.052*** (0.020)	-0.002 (0.018)	0.071*** (0.022)	26.545*** (4.412)	184.796*** (38.322)
Observations	45755	28840	28840	28840	28840	28840	45755	45755
R-squared	0.359	0.220	0.154	0.127	0.133	0.246	0.383	0.384
B. Share Exposed								
Share X AP High Intensity	0.248*** (0.049)	-0.117** (0.055)	-0.056 (0.050)	0.119*** (0.045)	0.010 (0.040)	0.122*** (0.040)	58.048*** (10.247)	395.845*** (84.137)
Observations	45755	28840	28840	28840	28840	28840	45755	45755
R-squared	0.358	0.220	0.154	0.127	0.133	0.246	0.383	0.384
Panel III: Labor Market Earnings and Household Socioeconomic Status								
	ln(Cash Wages)	ln(Earnings)	ln(HH Wage+Salary)	ln(Total Income)	ln(Income Per Cap)	Poverty	HH Assets	
A. Whether Exposed								
YC X AP High Intensity	0.101 (0.063)	0.107 (0.066)	0.070 (0.059)	0.121*** (0.040)	0.100*** (0.038)	-0.058*** (0.011)	0.603*** (0.211)	
Observations	19295	19299	32818	45055	45055	45733	45735	
R-squared	0.380	0.362	0.268	0.280	0.192	0.193	0.439	
B. Share Exposed								
Share X AP High Intensity	0.280** (0.114)	0.302*** (0.115)	0.070 (0.079)	0.131 (0.085)	0.061 (0.080)	-0.106*** (0.022)	0.580 (0.481)	
Observations	19295	19299	32818	45055	45055	45733	45735	
R-squared	0.380	0.362	0.268	0.279	0.192	0.193	0.439	

Notes: All regression specifications include demographic controls (Gender, Caste, Religion, Marital Status, Household Size), birth year fixed effects, and district fixed effects. Survey weights are used in all regressions. Robust standard error in parentheses, corrected for clustering at the district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.9: Additional Summary Statistics

	N	Mean	SD
Confidence in Institutions			
Politician (=1 if yes)	45755	0.50	0.50
Military (=1 if yes)	45755	0.97	0.16
Police (=1 if yes)	45755	0.75	0.43
State Government (=1 if yes)	45755	0.80	0.40
News (=1 if yes)	45755	0.91	0.29
Panchayats (=1 if yes)	45755	0.79	0.40

Note: Authors' own calculations using sampling weights available in IHDS 2012.

Table A.10: Effects excluding States with Major Anti-Mandal Agitations

Panel I: Education								
	Literacy	Attended School	Completed Years	Secondary	Higher Secondary	Bachelors+	English Ability	
A. Whether Exposed								
YC X AP	0.042** (0.018)	0.029 (0.022)	0.251 (0.253)	0.027** (0.013)	-0.006 (0.008)	0.029** (0.012)	0.053*** (0.019)	
Observations	33148	33148	33148	33148	33148	33148	33147	
R-squared	0.239	0.255	0.285	0.045	0.039	0.051	0.089	
B. Share Exposed								
Share X AP	0.127*** (0.035)	0.112*** (0.036)	1.443*** (0.433)	0.091*** (0.033)	-0.012 (0.020)	0.104*** (0.028)	0.182*** (0.028)	
Observations	33148	33148	33148	33148	33148	33148	33147	
R-squared	0.239	0.256	0.285	0.046	0.039	0.052	0.091	
Panel II: Workforce Participation and Labor Supply								
	Any work	Agri	Non-agri	Salary	Business	Farm	Work Days	Work Hours
A. Whether Exposed								
YC X AP	0.092*** (0.018)	-0.054** (0.023)	-0.058*** (0.021)	0.075*** (0.019)	0.007 (0.012)	0.005 (0.020)	19.218*** (4.494)	146.795*** (38.188)
Observations	33148	21811	21811	21811	21811	21811	33148	33148
R-squared	0.325	0.106	0.086	0.045	0.055	0.135	0.335	0.329
B. Share Exposed								
Share X AP	0.166*** (0.044)	-0.135*** (0.043)	-0.087* (0.049)	0.179*** (0.039)	0.015 (0.029)	-0.008 (0.038)	38.562*** (9.577)	289.524*** (83.609)
Observations	33148	21811	21811	21811	21811	21811	33148	33148
R-squared	0.325	0.107	0.085	0.045	0.055	0.135	0.335	0.329
Panel III: Labor Market Earnings and Household Socioeconomic Status								
	ln(Cash Wages)	ln(Earnings)	ln(HH Wage+Salary)	ln(Total Income)	ln(Income Per Cap)	Poverty	HH Assets	
A. Whether Exposed								
YC X AP	0.146** (0.058)	0.144** (0.057)	0.094** (0.037)	0.116*** (0.042)	0.099** (0.041)	-0.030** (0.013)	0.509*** (0.185)	
Observations	15220	15223	24711	32591	32591	33126	33128	
R-squared	0.274	0.258	0.155	0.217	0.075	0.125	0.252	
B. Share Exposed								
Share X AP	0.337*** (0.100)	0.339*** (0.101)	0.112 (0.076)	0.185** (0.078)	0.138* (0.077)	-0.044 (0.034)	0.632* (0.353)	
Observations	15220	15223	24711	32591	32591	33126	33128	
R-squared	0.274	0.258	0.155	0.217	0.075	0.125	0.252	

Notes: All regression specifications include demographic controls (Gender, Caste, Religion, Marital Status, Household Size), birth year fixed effects, and state fixed effects. Survey weights are used in all regressions. Robust standard error in parentheses, corrected for clustering at the state-by-birth year level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.11: Effects excluding cohort exposed to the IT Boom

Panel I: Education								
	Literacy	Attended School	Completed Years	Secondary	Higher Secondary	Bachelors+	English Ability	
A. Whether Exposed								
YC X AP	0.053** (0.021)	0.037 (0.025)	0.433 (0.300)	0.024* (0.014)	0.004 (0.009)	0.023** (0.010)	0.046** (0.021)	
Observations	43018	43018	43018	43018	43018	43018	43017	
R-squared	0.250	0.263	0.290	0.048	0.040	0.057	0.099	
B. Share Exposed								
Share X AP	0.172*** (0.047)	0.138*** (0.052)	2.035*** (0.607)	0.100*** (0.026)	0.039* (0.022)	0.090*** (0.022)	0.183*** (0.038)	
Observations	43018	43018	43018	43018	43018	43018	43017	
R-squared	0.250	0.263	0.290	0.048	0.040	0.057	0.099	
Panel II: Workforce Participation and Labor Supply								
	Any work	Agri	Non-agri	Salary	Business	Farm	Work Days	Work Hours
A. Whether Exposed								
YC X AP	0.129*** (0.022)	-0.038 (0.025)	-0.071*** (0.024)	0.068*** (0.020)	0.005 (0.013)	0.037* (0.022)	28.487*** (5.856)	212.854*** (48.693)
Observations	43018	27044	27044	27044	27044	27044	43018	43018
R-squared	0.325	0.141	0.108	0.043	0.052	0.130	0.345	0.340
B. Share Exposed								
Share X AP	0.328*** (0.048)	-0.124** (0.053)	-0.149** (0.076)	0.181*** (0.052)	0.036 (0.030)	0.071 (0.052)	79.243*** (11.987)	608.072*** (99.365)
Observations	43018	27044	27044	27044	27044	27044	43018	43018
R-squared	0.325	0.141	0.108	0.043	0.052	0.129	0.345	0.341
Panel III: Labor Market Earnings and Household Socioeconomic Status								
	ln(Cash Wages)	ln(Earnings)	ln(HH Wage+Salary)	ln(Total Income)	ln(Income Per Cap)	Poverty	HH Assets	
A. Whether Exposed								
YC X AP	0.164** (0.068)	0.166** (0.068)	0.139*** (0.042)	0.149*** (0.051)	0.136*** (0.048)	-0.064*** (0.012)	0.622*** (0.179)	
Observations	17976	17979	30747	42353	42353	42998	43001	
R-squared	0.257	0.249	0.122	0.190	0.092	0.098	0.262	
B. Share Exposed								
Share X AP	0.559*** (0.124)	0.570*** (0.128)	0.293*** (0.099)	0.342*** (0.114)	0.288** (0.113)	-0.156*** (0.028)	1.155** (0.474)	
Observations	17976	17979	30747	42353	42353	42998	43001	
R-squared	0.258	0.249	0.122	0.190	0.092	0.098	0.262	

Notes: All regression specifications include demographic controls (Gender, Caste, Religion, Marital Status, Household Size), birth year fixed effects, and state fixed effects. Survey weights are used in all regressions. Robust standard error in parentheses, corrected for clustering at the state-by-birth year level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.12: Effects using only Karnataka as a control state

Panel I: Education								
	Literacy	Attended School	Completed Years	Secondary	Higher Secondary	Bachelors+	English Ability	
A. Whether Exposed								
YC X AP	0.038** (0.016)	0.017 (0.016)	0.205 (0.218)	0.011 (0.010)	-0.018** (0.008)	0.033*** (0.011)	0.039*** (0.015)	
Observations	11830	11830	11830	11830	11830	11830	11830	
R-squared	0.220	0.248	0.251	0.053	0.047	0.042	0.090	
B. Share Exposed								
Share X AP	0.091*** (0.026)	0.057** (0.023)	1.151*** (0.384)	0.038 (0.028)	-0.027 (0.025)	0.120*** (0.020)	0.146*** (0.023)	
Observations	11830	11830	11830	11830	11830	11830	11830	
R-squared	0.220	0.248	0.252	0.053	0.046	0.045	0.092	
Panel II: Workforce Participation and Labor Supply								
	Any work	Agri	Non-agri	Salary	Business	Farm	Work Days	Work Hours
A. Whether Exposed								
YC X AP	0.076*** (0.016)	-0.048* (0.026)	-0.048** (0.018)	0.062*** (0.016)	0.027** (0.011)	-0.011 (0.019)	12.645*** (4.595)	92.742** (41.543)
Observations	11830	7984	7984	7984	7984	7984	11830	11830
R-squared	0.283	0.104	0.057	0.055	0.068	0.132	0.307	0.306
B. Share Exposed								
Share X AP	0.160*** (0.034)	-0.132*** (0.042)	-0.062 (0.040)	0.170*** (0.028)	0.053** (0.021)	-0.088*** (0.032)	31.850*** (8.543)	259.907*** (81.943)
Observations	11830	7984	7984	7984	7984	7984	11830	11830
R-squared	0.283	0.105	0.057	0.056	0.068	0.133	0.307	0.307
Panel III: Labor Market Earnings and Household Socioeconomic Status								
	ln(Cash Wages)	ln(Earnings)	ln(HH Wage+Salary)	ln(Total Income)	ln(Income Per Cap)	Poverty	HH Assets	
A. Whether Exposed								
YC X AP	0.184*** (0.055)	0.187*** (0.054)	0.115*** (0.041)	0.097** (0.044)	0.064 (0.043)	-0.031** (0.014)	0.695*** (0.183)	
Observations	5877	5877	9104	11487	11487	11813	11811	
R-squared	0.226	0.223	0.116	0.144	0.027	0.141	0.135	
B. Share Exposed								
Share X AP	0.428*** (0.088)	0.433*** (0.087)	0.150** (0.071)	0.195*** (0.072)	0.131* (0.071)	-0.036 (0.035)	0.922*** (0.291)	
Observations	5877	5877	9104	11487	11487	11813	11811	
R-squared	0.227	0.223	0.115	0.144	0.027	0.140	0.134	

Notes: All regression specifications include demographic controls (Gender, Caste, Religion, Marital Status, Household Size), birth year fixed effects, and state fixed effects. Survey weights are used in all regressions. Robust standard error in parentheses, corrected for clustering at the state-by-birth year level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

B Appendix - Figures

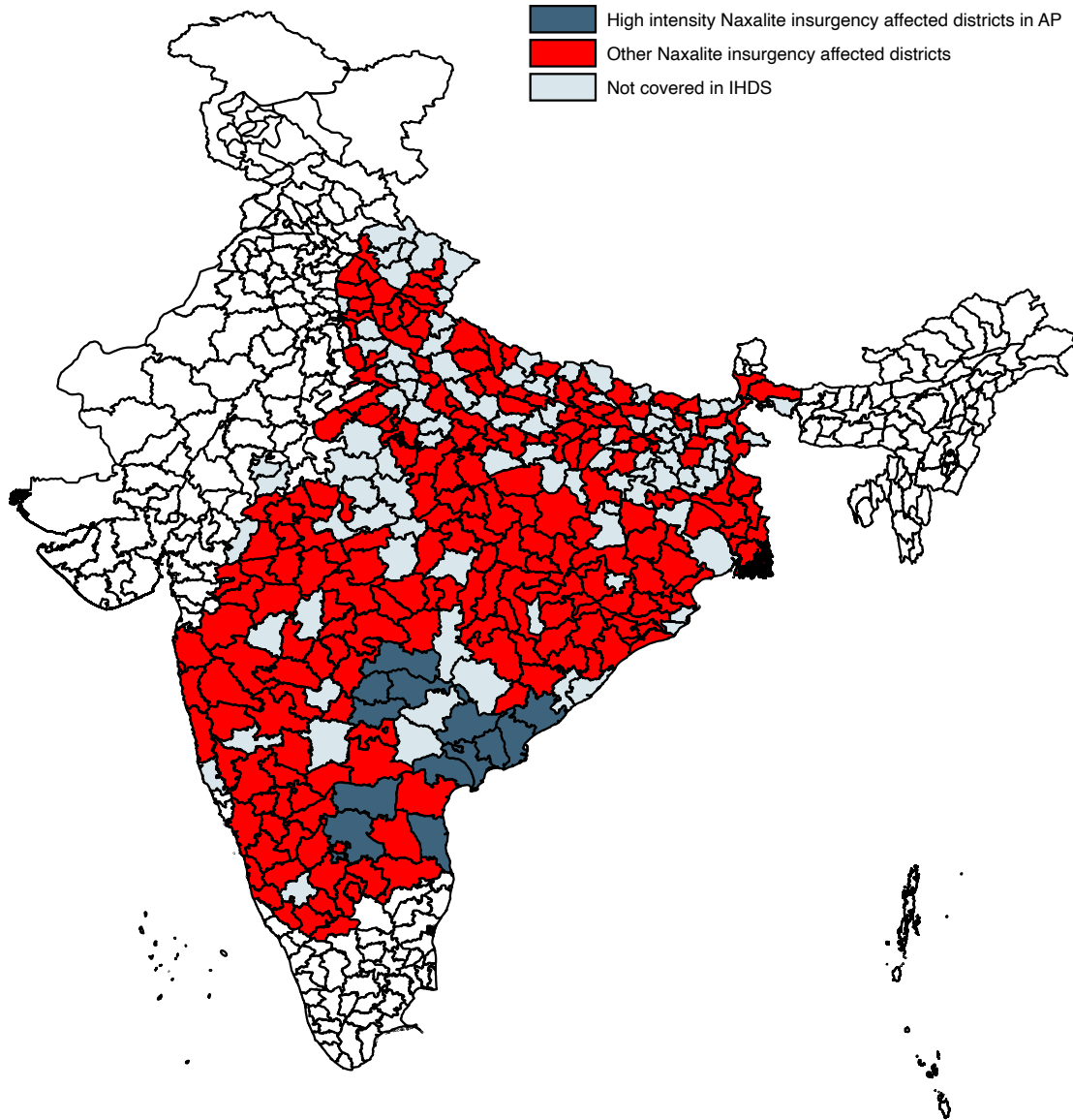


Figure B.1: **Districts affected by Naxalite Insurgency**

Note: The classification is based on three sources: a map from the 1970s reproduced in Banerjee (1984), originally published in Deshabrati; a map compiled by Borooah (2008); and district-level information from Sahoo (2019) on Naxalite activity in the 1980s.

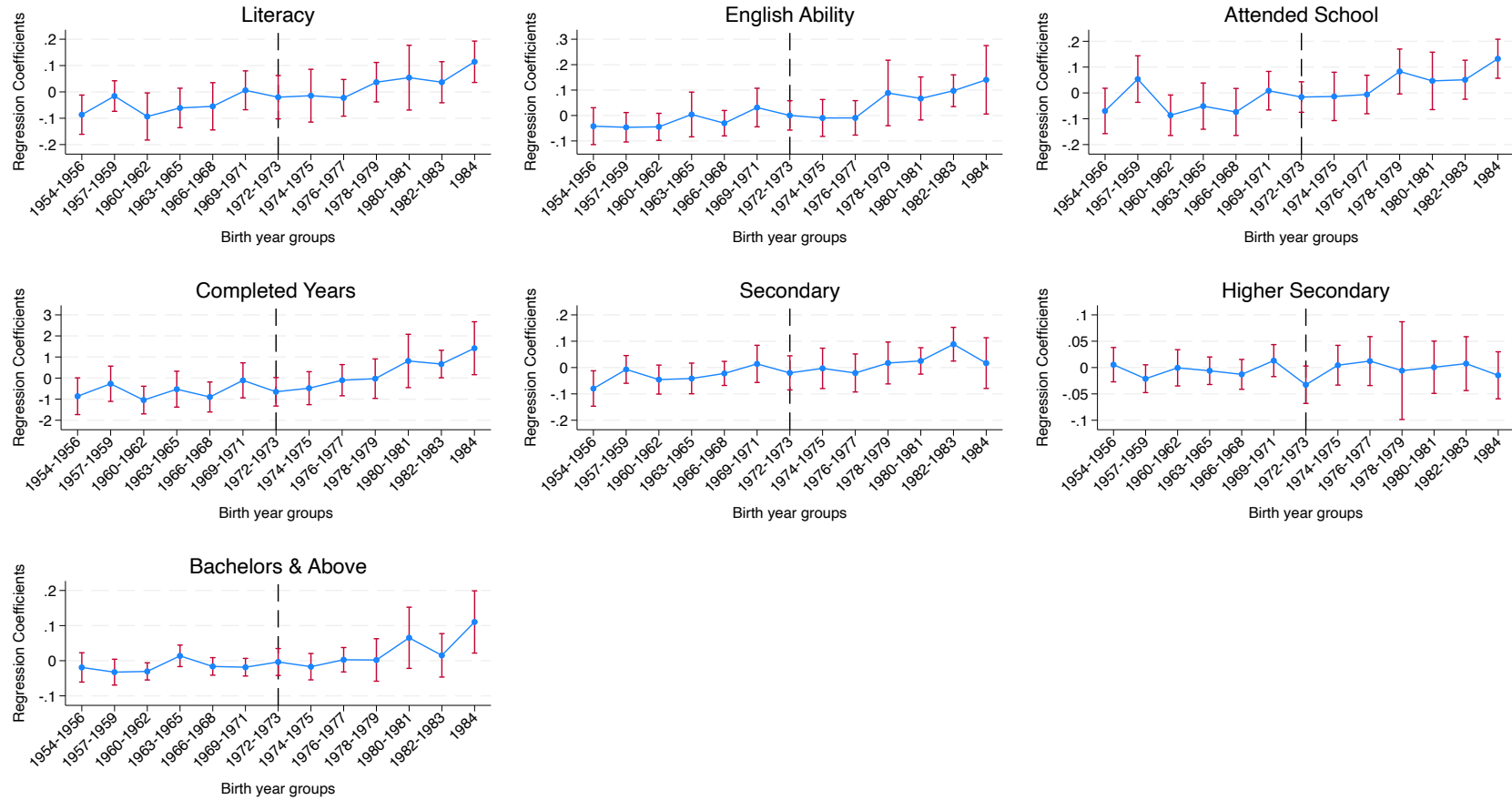


Figure B.2: Event Study Plot for High Intensity AP Districts: Education

Note: This figure shows the estimated impact of exposure to the 1989 Greyhounds counterinsurgency policy on education, using the event study specification outlined in Eq. (2). Estimates are plotted by birth year bins relative to the oldest bin, with 95% confidence intervals. Individuals are classified as treated if they are born between 1972 and 1984, capturing those potentially affected by the policy during schooling years. The control group comprises individuals who were born before 1972 and thus not exposed to the policy during school-age year.

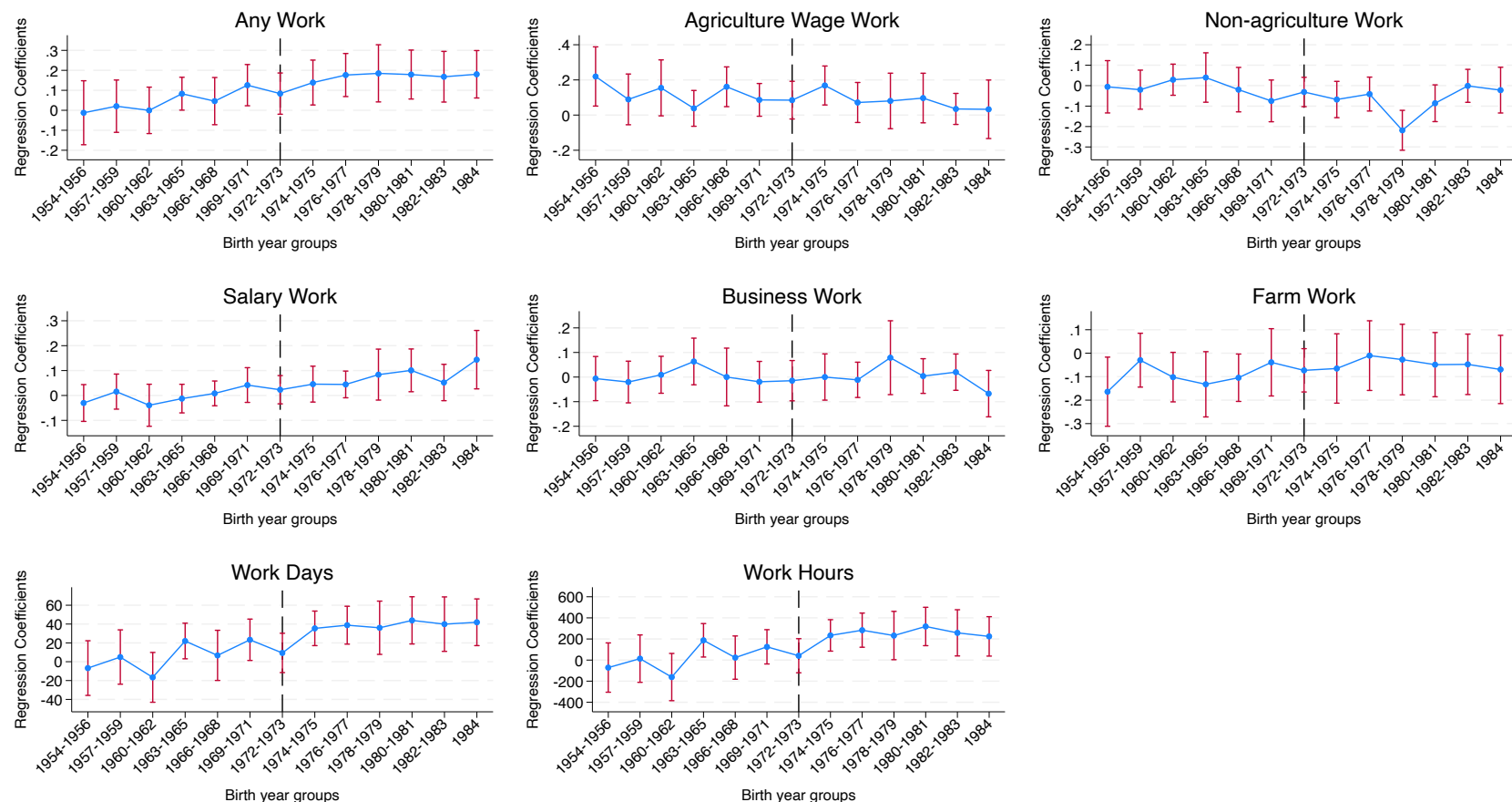


Figure B.3: Event Study Plot for High Intensity AP Districts: Workforce Participation and Labor Supply

Note: This figure shows the estimated impact of exposure to the 1989 Greyhounds counterinsurgency policy on education, using the event study specification outlined in Eq. (2). Estimates are plotted by birth year bins relative to the oldest bin, with 95% confidence intervals. Individuals are classified as treated if they are born between 1972 and 1984, capturing those potentially affected by the policy during schooling years. The control group comprises individuals who were born before 1972 and thus not exposed to the policy during school-age year.

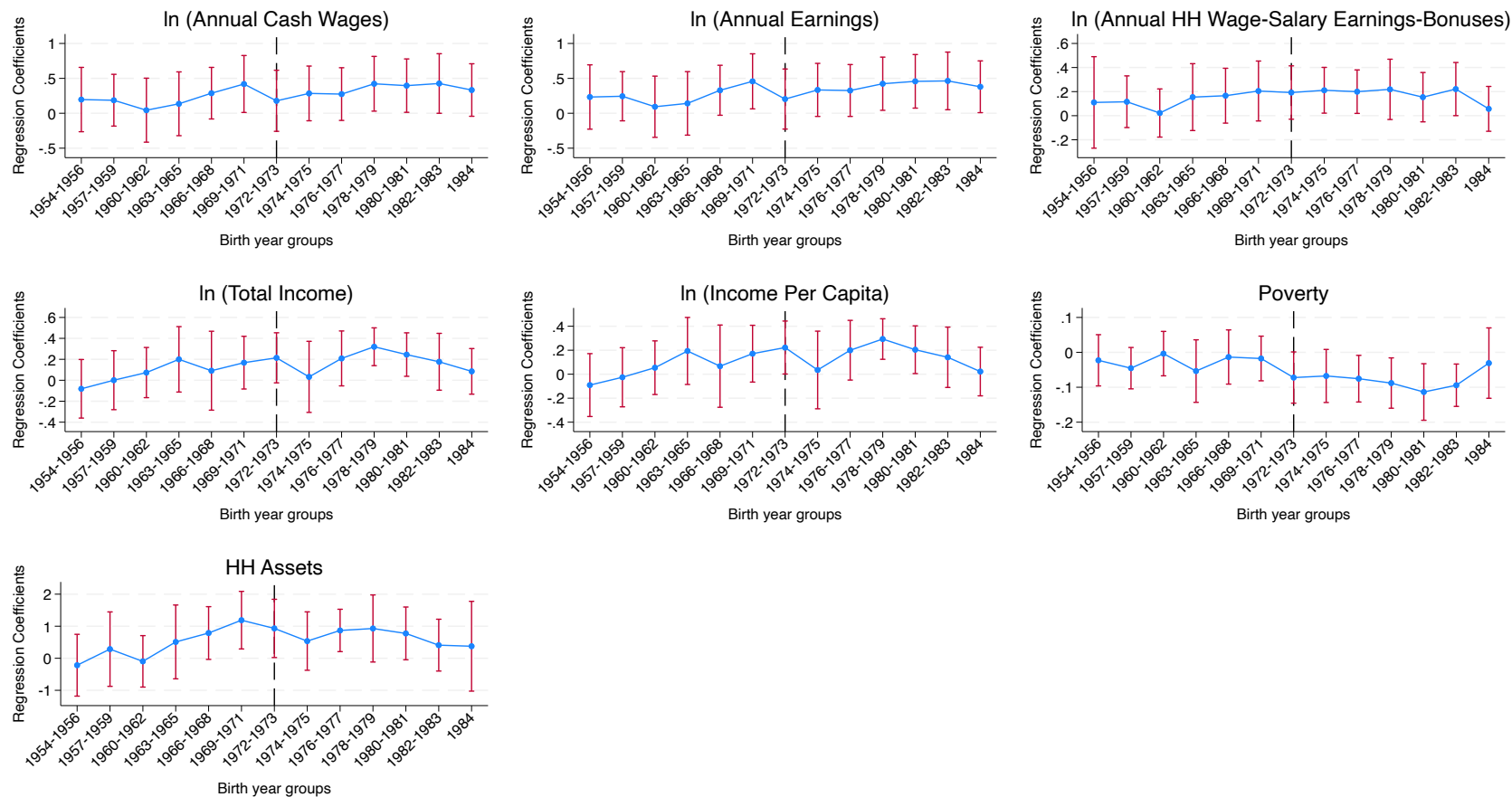


Figure B.4: Event Study Plot for High Intensity AP Districts: Labor Market Earnings and Household Socio-economic Status

Note: This figure shows the estimated impact of exposure to the 1989 Greyhounds counterinsurgency policy on education, using the event study specification outlined in Eq. (2). Estimates are plotted by birth year bins relative to the oldest bin, with 95% confidence intervals. Individuals are classified as treated if they are born between 1972 and 1984, capturing those potentially affected by the policy during schooling years. The control group comprises individuals who were born before 1972 and thus not exposed to the policy during school-age year.

DID, from last period before treatment changes ($t=-1$) to t

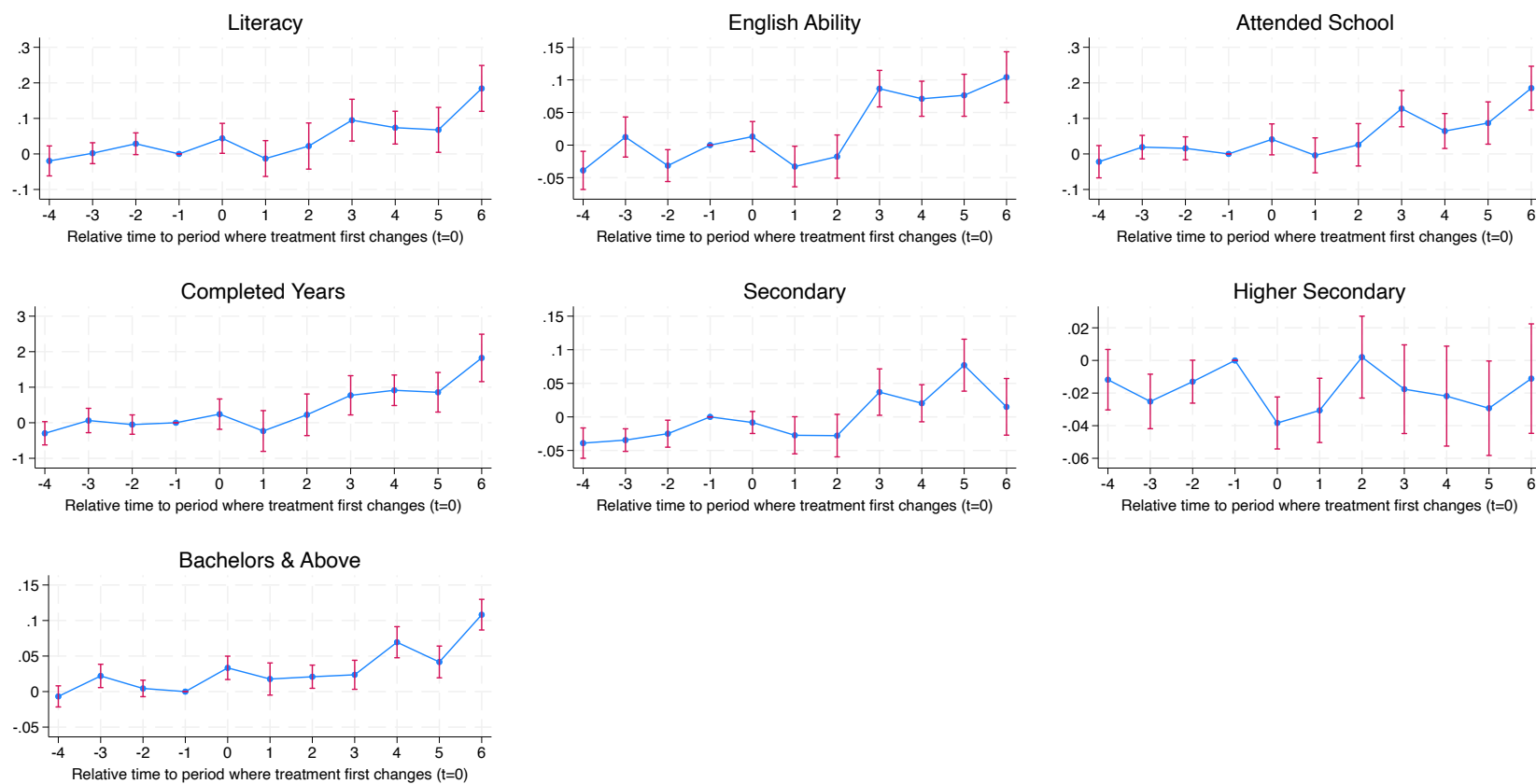


Figure B.5: **Robustness to Chaisemartin: Education**

Note: The figure reports estimates and confidence intervals estimated using the estimator proposed by de Chaisemartin and D'Haultfœuille (2020), implemented via the `did_multiplegt` Stata command, available from the SSC repository

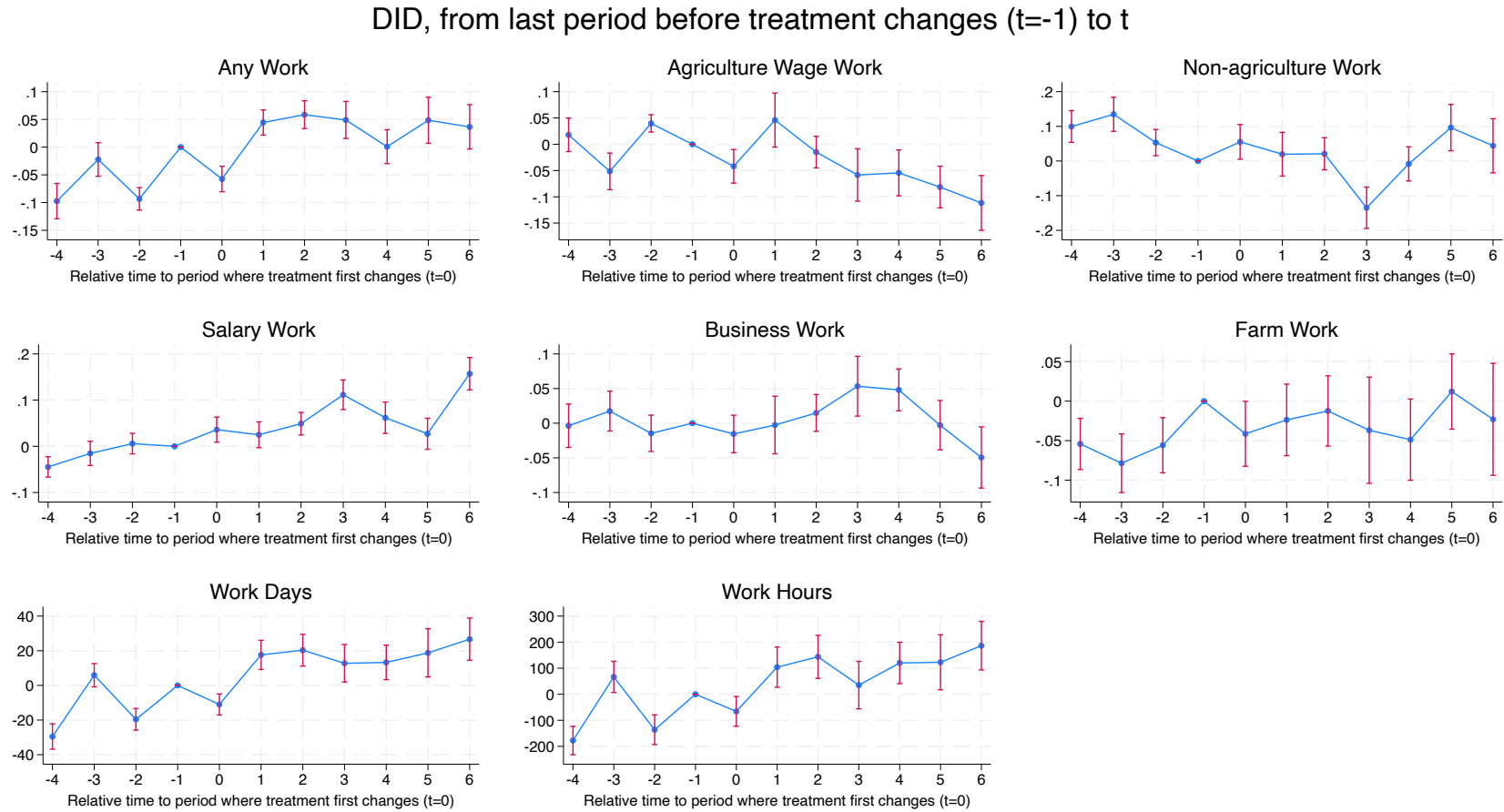


Figure B.6: **Robustness to Chaisemartin: Workforce Participation and Labor Supply**

Note: The figure reports estimates and confidence intervals estimated using the estimator proposed by de Chaisemartin and D'Haultfœuille (2020), implemented via the `did_multiplegt` Stata command, available from the SSC repository

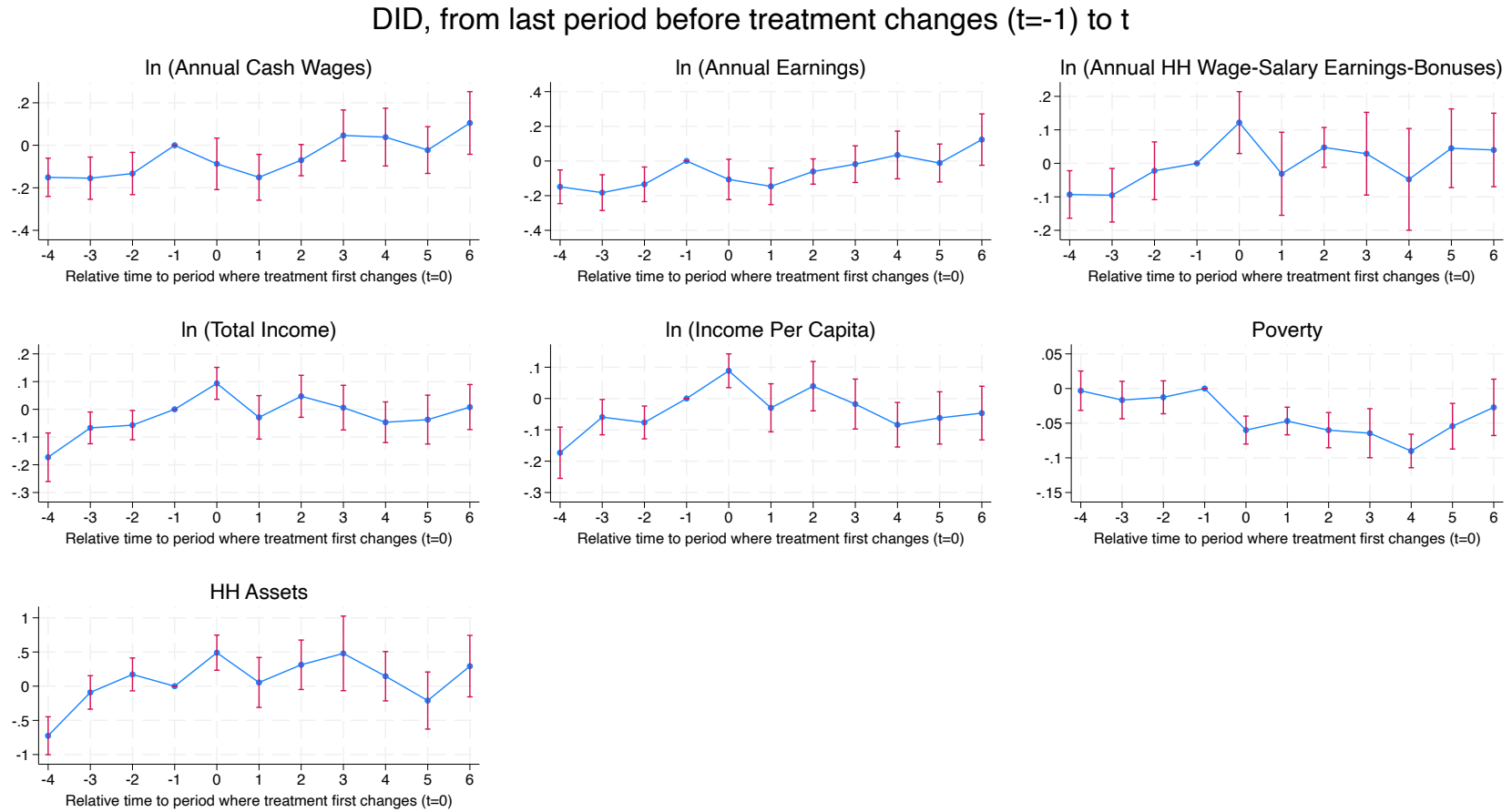


Figure B.7: **Robustness to Chaisemartin: Labor Market Earnings and Household Socioeconomic Status**

Note: The figure reports estimates and confidence intervals estimated using the estimator proposed by de Chaisemartin and D'Haultfœuille (2020), implemented via the `did_multiplegt` Stata command, available from the SSC repository

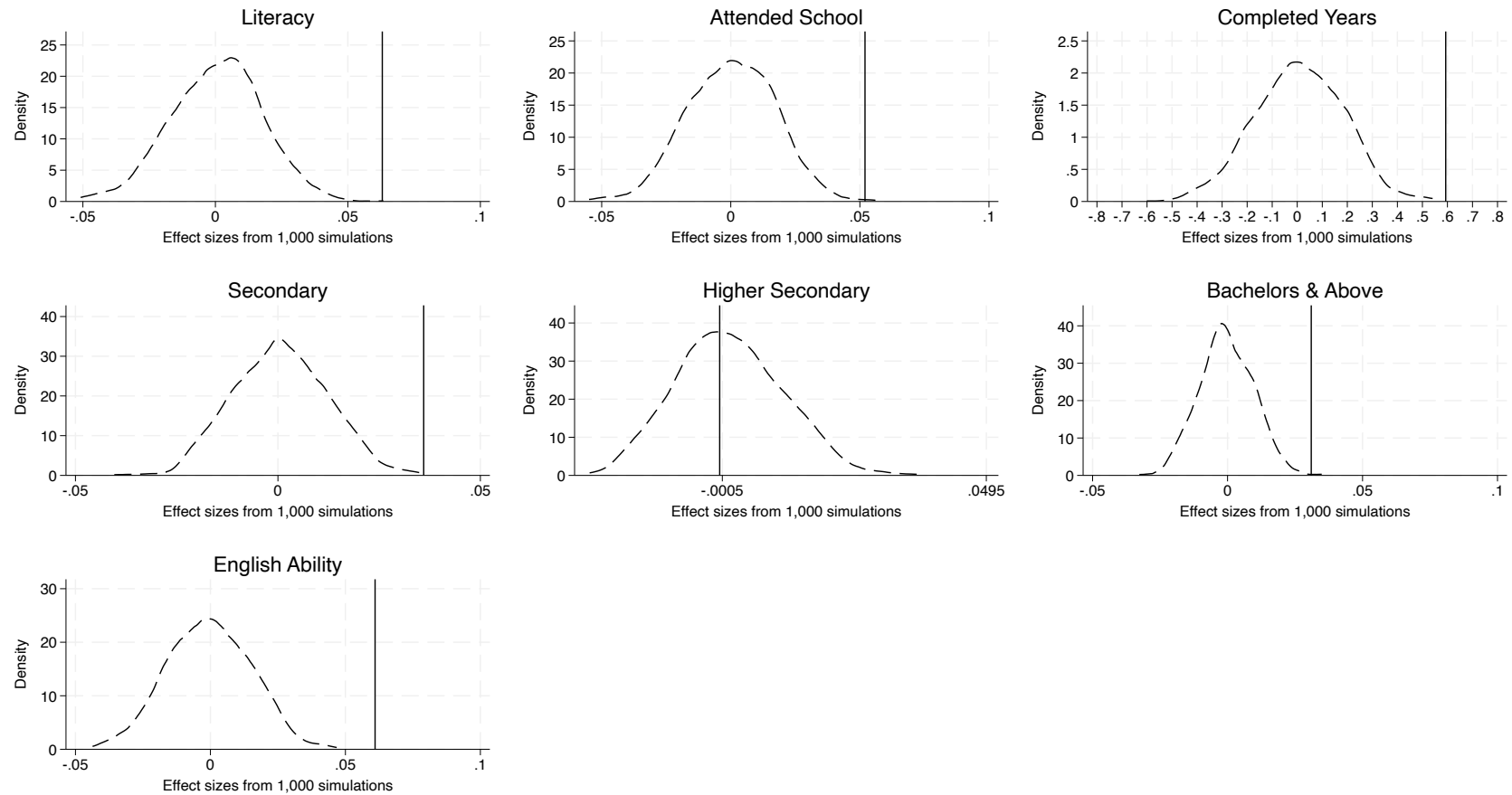


Figure B.8: **Exact Randomization Test based on State: Education Outcomes**

Note: We plot the distribution of estimated coefficients of the exposure variable (Younger Cohort $\times$ AP) from 1,000 simulations where the AP is randomly reassigned. Each simulation re-estimates the baseline specification. The vertical line indicates the true estimate from the original analysis.

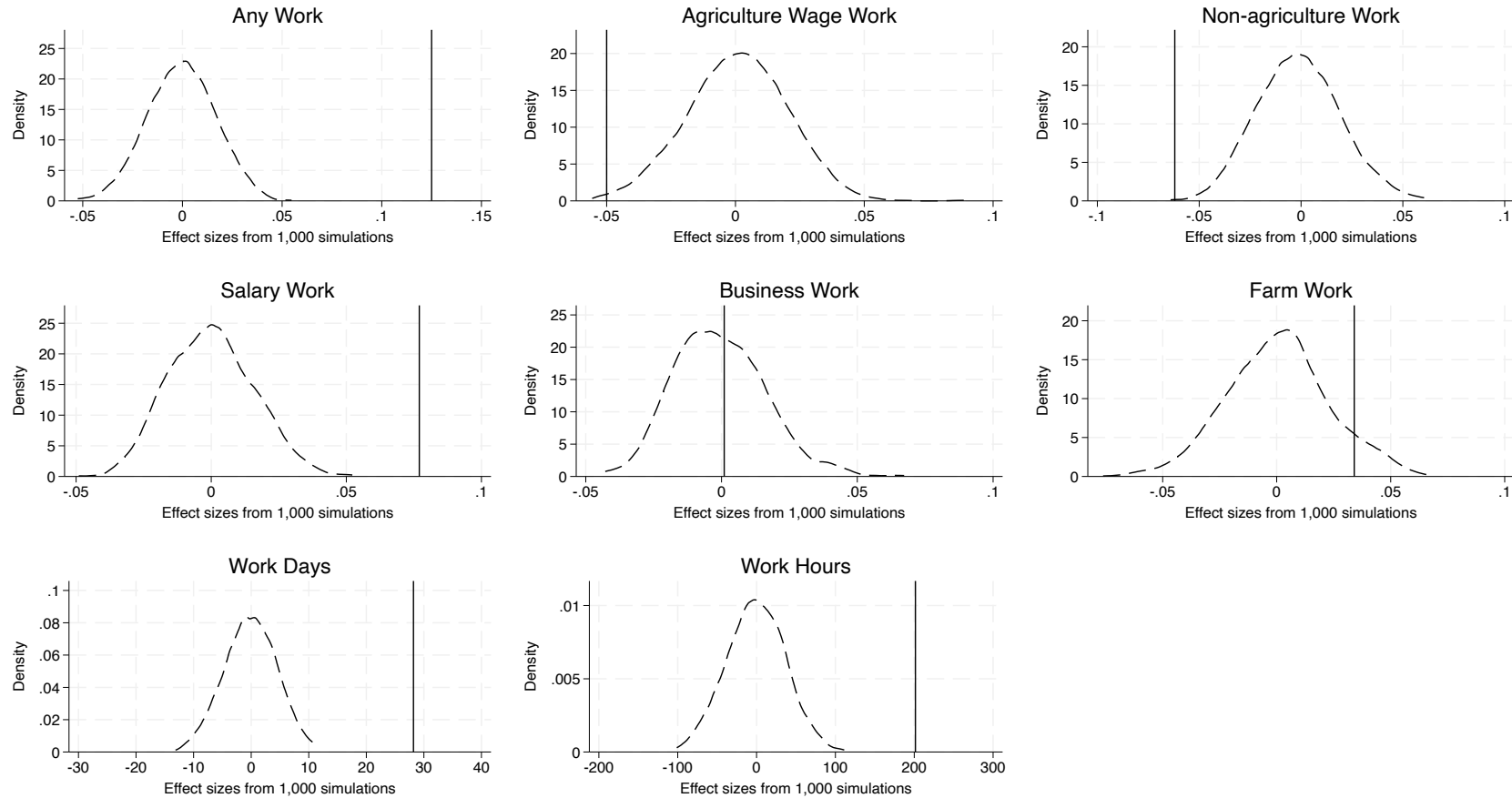


Figure B.9: **Exact Randomization Test based on State: Workforce Participation and Labor Supply**

Note: We plot the distribution of estimated coefficients of the exposure variable (Younger Cohort $\times$ AP) from 1,000 simulations where the AP is randomly reassigned. Each simulation re-estimates the baseline specification. The vertical line indicates the true estimate from the original analysis.

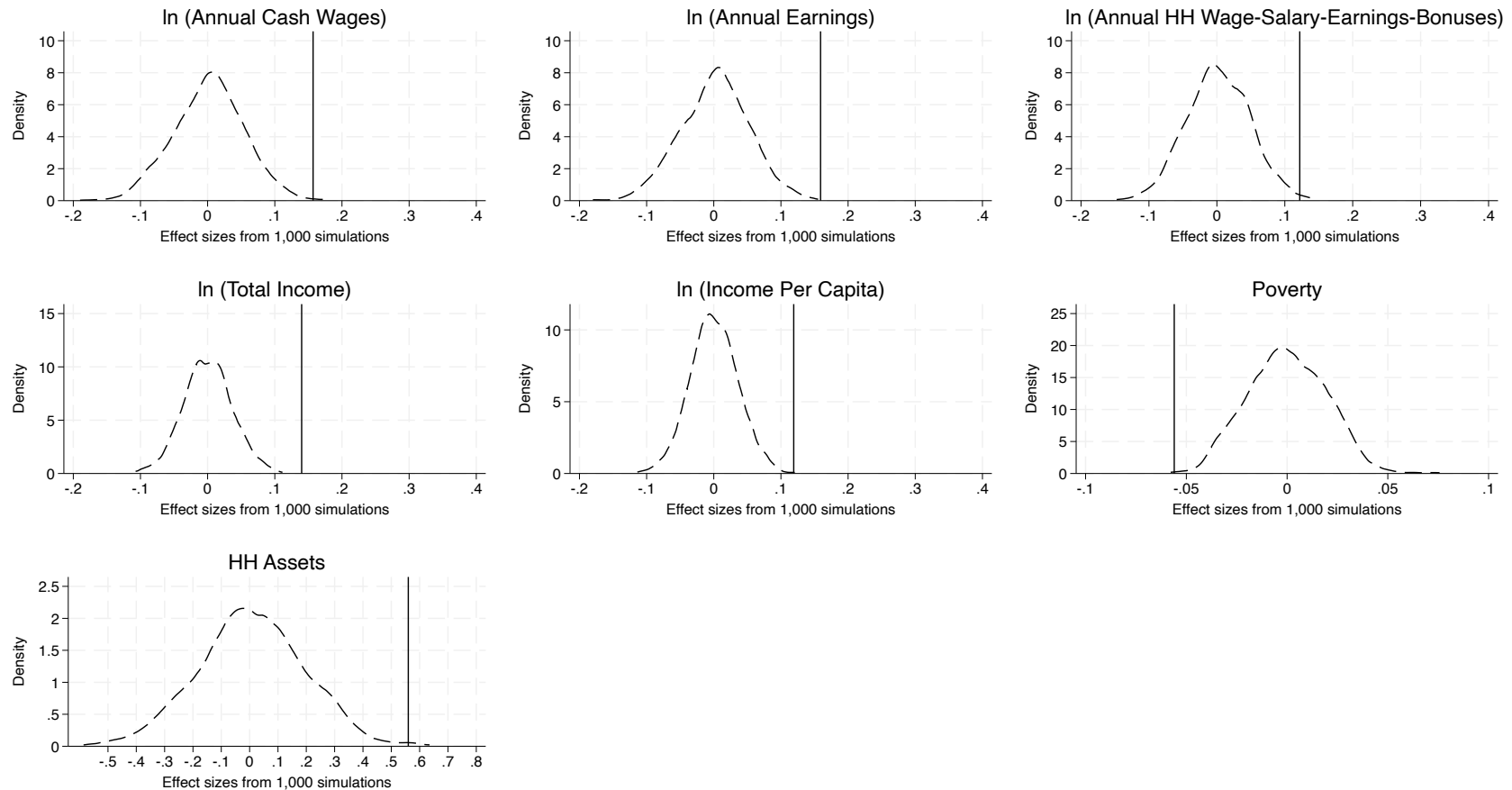


Figure B.10: **Exact Randomization Test based on State: Wages, Income, Poverty & Assets**

Note: We plot the distribution of estimated coefficients of the exposure variable (Younger Cohort $\times$ AP) from 1,000 simulations where the AP is randomly reassigned. Each simulation re-estimates the baseline specification. The vertical line indicates the true estimate from the original analysis.

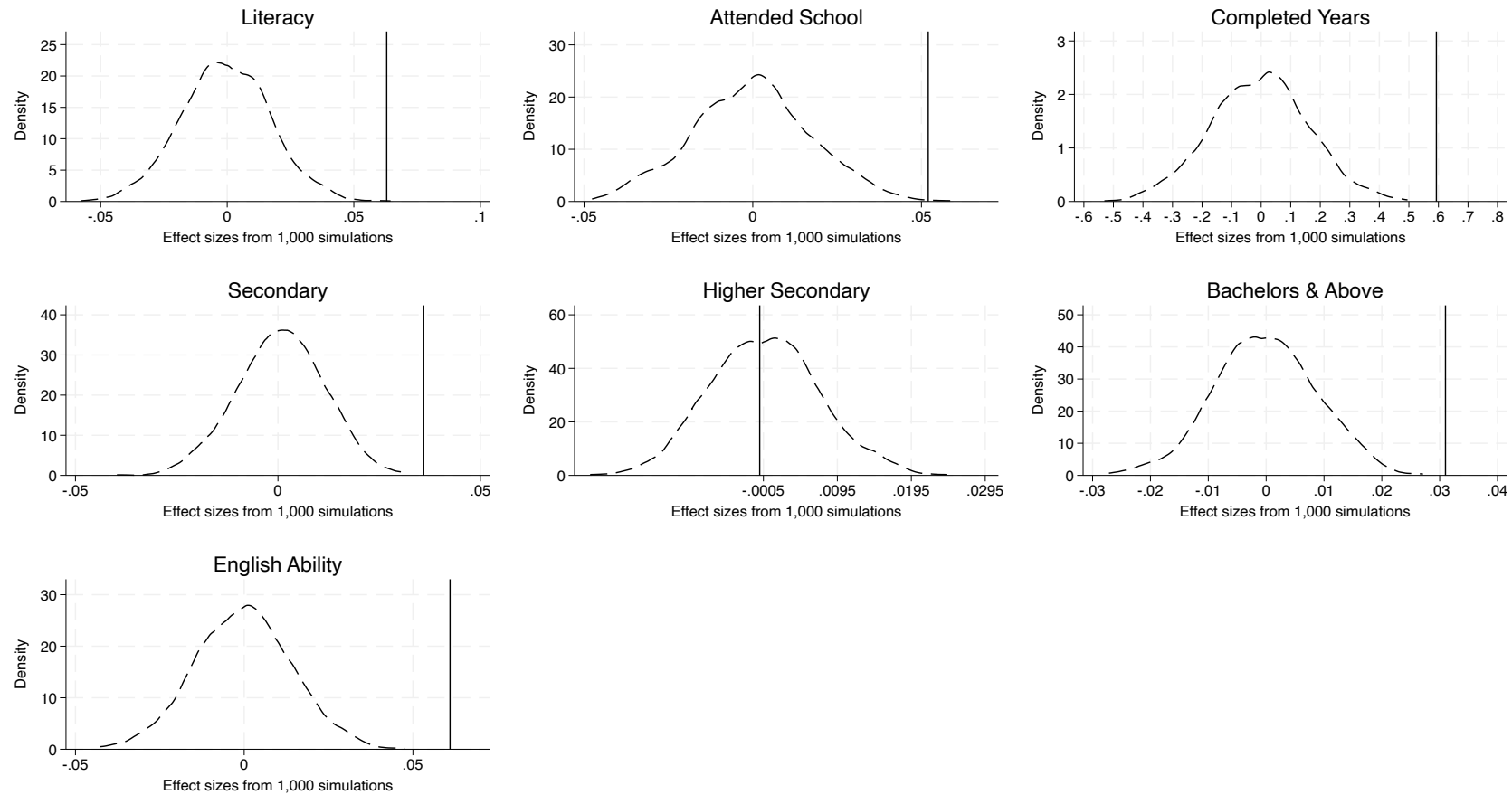


Figure B.11: **Exact Randomization Test based on Cohort: Education Outcomes**

Note: We plot the distribution of estimated coefficients of the exposure variable (Younger Cohort $\times$ AP) from 1,000 simulations where the AP is randomly reassigned. Each simulation re-estimates the baseline specification. The vertical line indicates the true estimate from the original analysis.

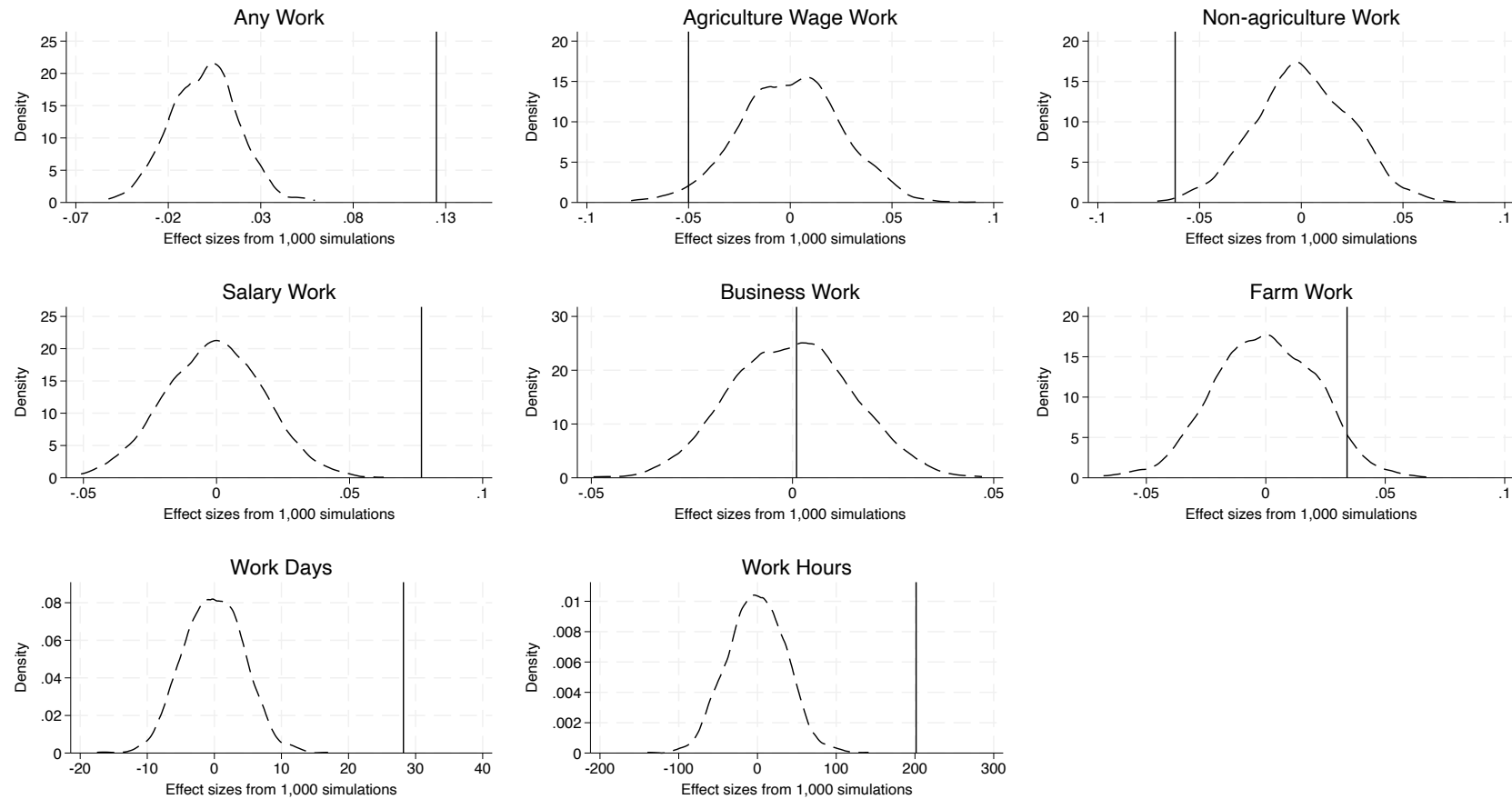


Figure B.12: **Exact Randomization Test based on Cohort: Workforce Participation and Labor Supply**

Note: We plot the distribution of estimated coefficients of the exposure variable (Younger Cohort $\times$ AP) from 1,000 simulations where the AP is randomly reassigned. Each simulation re-estimates the baseline specification. The vertical line indicates the true estimate from the original analysis.

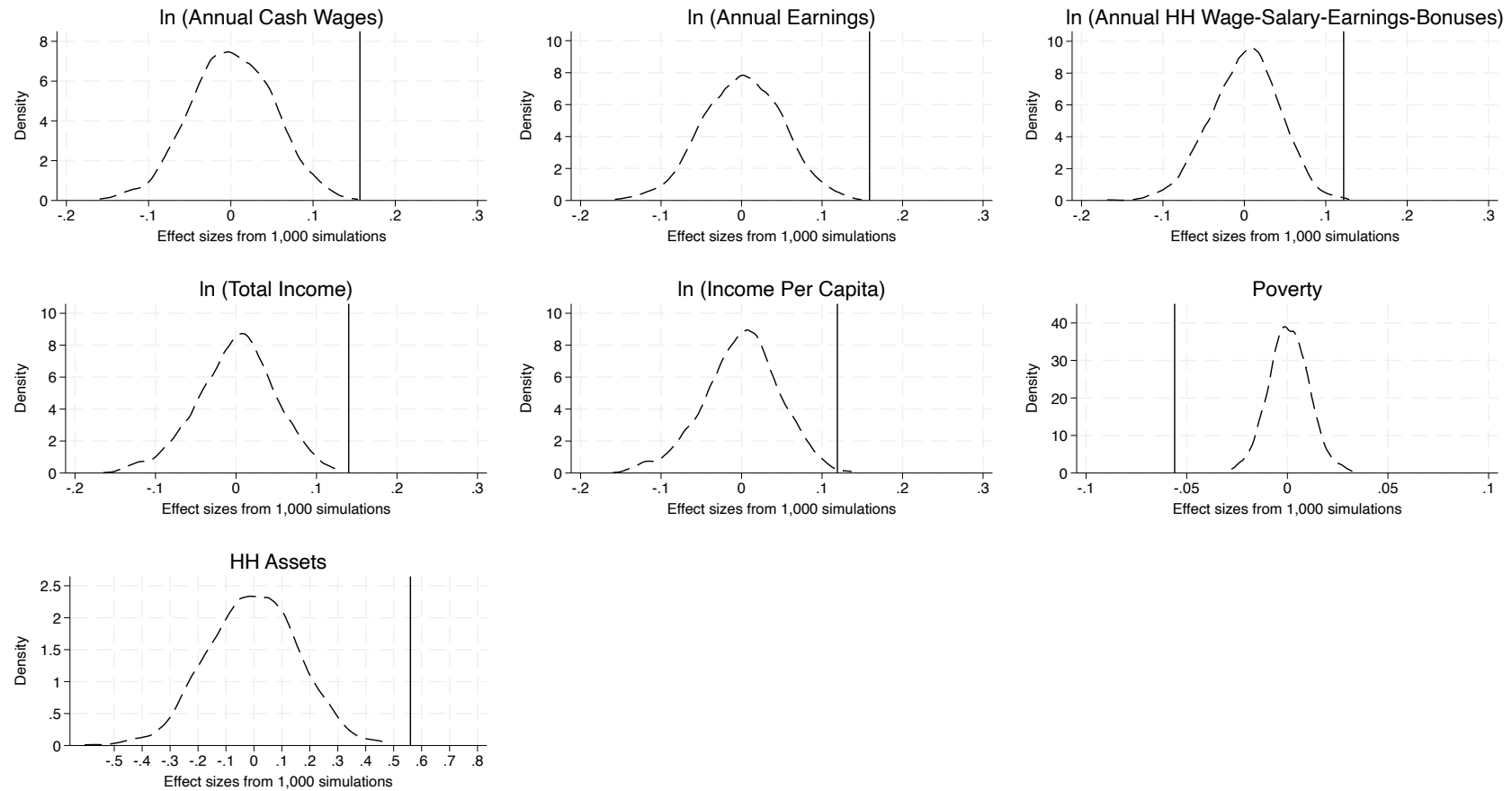


Figure B.13: **Exact Randomization Test based on Cohort: Wages, Income, Poverty & Assets**

Note: We plot the distribution of estimated coefficients of the exposure variable (Younger Cohort $\times$ AP) from 1,000 simulations where the AP is randomly reassigned. Each simulation re-estimates the baseline specification. The vertical line indicates the true estimate from the original analysis.

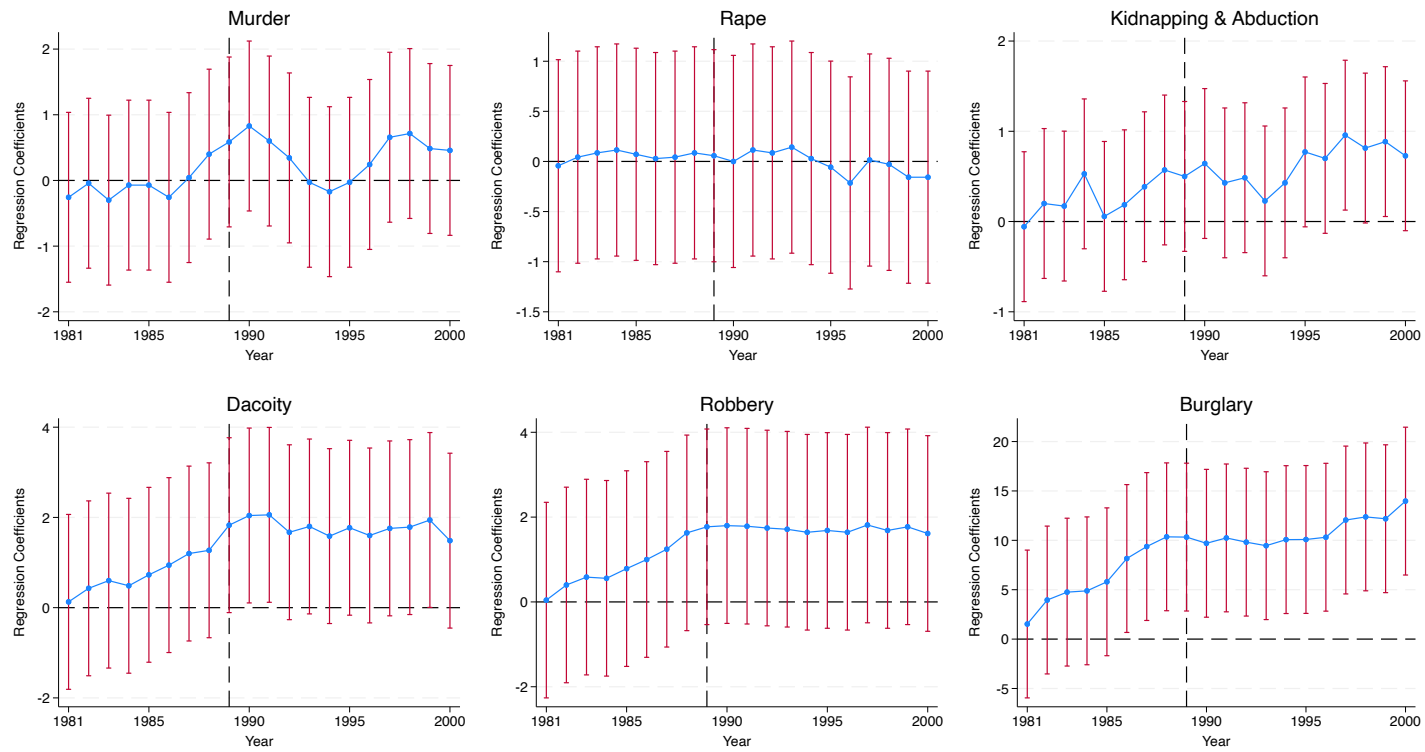


Figure B.14: **Other crime rates**

Note: The figure presents the estimates and confidence intervals of the effect of the 1989 counterinsurgency policy on general crime rates using an event study specification.

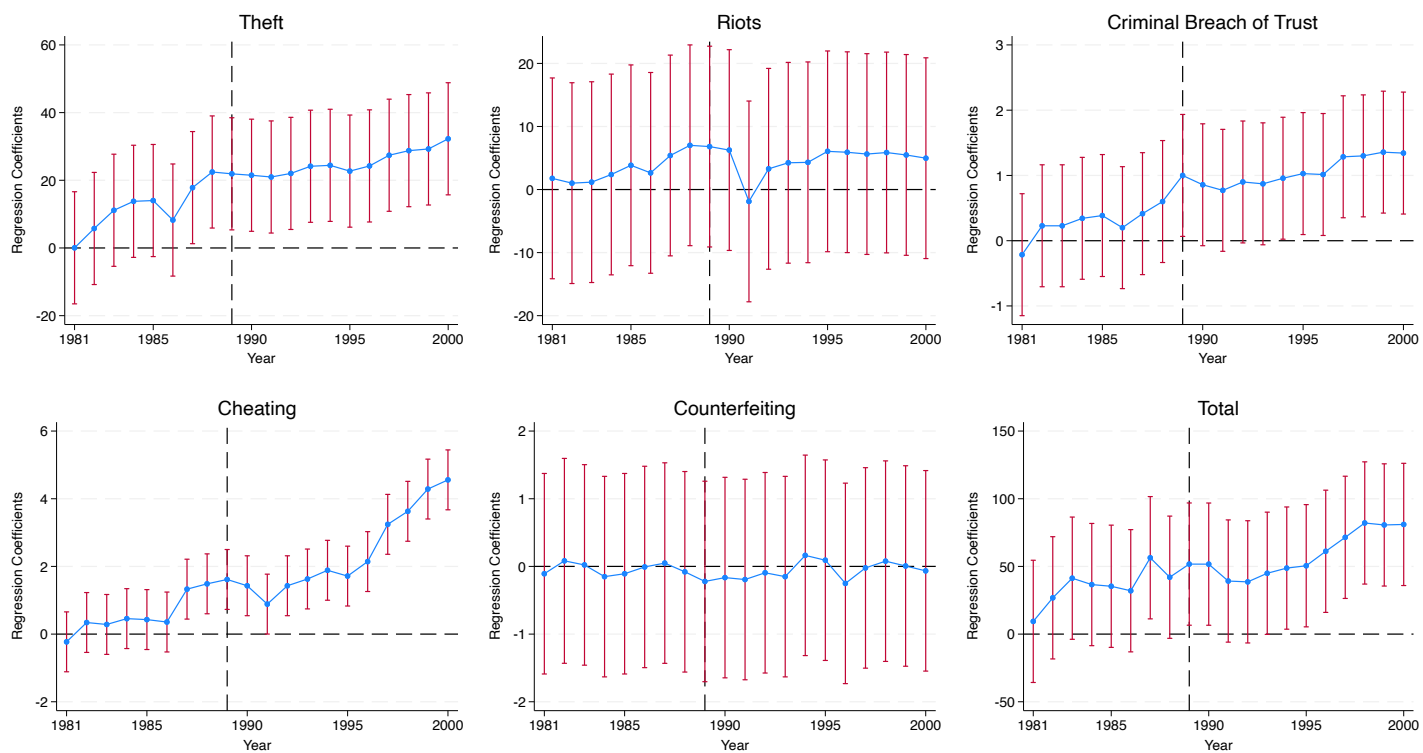


Figure B.15: **Other crime rates (continued)**

Note: The figure presents the estimates and confidence intervals of the effect of the 1989 counterinsurgency policy on general crime rates using an event study specification.