# Guns and Gains: Human Capital Effects of Exposure to Counterinsurgency Operations*

Sourish Mustafi†
Shiv Nadar University, Delhi NCR, India

Bharti Nandwani‡
Indira Gandhi Institute of Development Research, Mumbai, India

Punarjit Roychowdhury§
Shiv Nadar University, Delhi NCR, India, GLO & CDES, Monash University

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## Abstract

This paper examines the effects of exposure to hard-security counterinsurgency operations during school-age years on human capital formation. We exploit the 1989 introduction of the Greyhounds—a specialized commando force created to combat Naxalite insurgents—in the Indian state of 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. 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.

**JEL Classification:** D74, H56, O12, O15
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†Department of Economics, School of Humanities and Social Sciences, Shiv Nadar University, NH91, Tehsil Dadri, Greater Noida 201314, Uttar Pradesh, India. Email: sm138@snu.edu.in

‡RB1-102, Indira Gandhi Institute of Development Research, Film City Road, Mumbai 400065, India. Email: bharti@igidr.ac.in

§Corresponding Author. Department of Economics, School of Humanities and Social Sciences, Shiv Nadar University, NH91, Tehsil Dadri, Greater Noida 201314, Uttar Pradesh, India. Email: punarjitroychowdhury@gmail.com.

# 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. 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 human capital formation.

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 the Greyhounds have occasionally been criticized for operating with limited oversight and employing heavy-handed tactics, anecdotal reports suggest that their counterinsurgency methods, 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 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

[1] https://pib.gov.in/PressReleasePage.aspx?PRID=1942471

did not implement similar policies until the end of 2000. By examining 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 significant improvements in human capital. 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. They are also more likely to finish secondary school and to obtain a bachelor's degree or higher. To assess the credibility of these findings, we conduct a battery of robustness checks and falsification tests. Among others, we account for treatment-effect heterogeneity using alternative estimators, implement exact randomization inference tests to rule out spurious correlations, introduce state-specific birth-year trends to flexibly capture differential economic trajectories, and exploit within-state variation in pre-policy insurgency intensity to mitigate concerns about state-level confounders. We also perform placebo analyses using pre-exposure birth cohorts. Across all these exercises, the estimated effects remain highly stable in both magnitude and significance, reinforcing the validity of our main results.

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 educational 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 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.

Further, we 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 alternative explanations that could plausibly account for our findings. These include general improvements in law and order in AP unrelated to the Greyhound operations, overall state-level economic growth, 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 effects of childhood exposure to violence on human capital. 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. La Mattina and Shemyakina (2024) show that early childhood exposure to armed conflict increases acceptance of domestic violence later in life, primarily through the deterioration of educational attainment and earlier marriage timing caused by conflict-related disruptions in Sub-Saharan Africa. Further, 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.

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).

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.

[2] A 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.

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

Section 5 explores potential underlying mechanisms. The last section concludes.

# 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.

[4] Also see `http://timesofindia.indiatimes.com/city/kolkata/Naxalbari-revisited/articleshow/4704`

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 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, 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. Human Rights Forum (2013) argues that the unit operated "without legislative oversight and scrutiny," and that it was "explicitly tasked to assault

[5] Although 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

and kill rather than apprehend suspects" (p. 47–48). 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 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

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

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

sample and re-estimate our baseline specification.

# 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. Note, it is extremely common in studies examining exposure to treatment during school-age years to restrict the analysis to non-migrant individuals (e.g., von Der Goltz et al. (2020)).[10]

We use a rich set of educational outcomes of individuals, including indicators of literacy, school attendance, total schooling years completed, indicators of completion of secondary

[10] We 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 migration in India is very low as per international standards and whatever limited migration that does occur is local in nature with inter-district migration accounting for only 24% and inter-state accounting for even lower 13% of internal migrants in India (Office of the Registrar General & Census Commissioner, India, 2001).

and higher secondary education, indicator of attainment of a bachelor's degree or higher, and self-reported proficiency in English.

### 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.[11]

### 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 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

[11] For further details, see: https://www.gdeltproject.org/data.html

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, Uttarkhand,

and West Bengal (see Figure 1).[12] 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. Also, note that in a robustness check (Appendix A.1), we construct an alternative comparison cohort by restricting it to individuals who were atleast 25 years old in 1989. We thus drop the 18-24 year olds from the comparison group as their college going decision could potentially be influenced by the counterinsurgency program. This also ensures that the comparison group is well past their schooling years.

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 counterinsurgency 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

[12] We 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).

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:

$$Y_{ist} = \alpha + \beta(YoungerCohort_t \times AP_s) + \gamma X_{ist} + \delta_t + \lambda_s + \epsilon_{ist} \tag{1}$$

where $Y_{ist}$ denotes the human capital 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; $\lambda_s$ 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 outcomes across cohorts ; $\delta_t$ denotes birth year fixed effects that partial out birth-year specific shocks to educational outcomes that are common across states; and $\epsilon_{ist}$ is the disturbance term. Our coefficient of interest, $\beta$, estimates the ITT of exposure to Greyhounds policy during school-age years on educational outcomes. We cluster standard errors at state of birth by cohort level.[13] Survey weights are used in all regressions.

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

[13]The 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'Haultfœuille (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.

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:

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

where $Yearbin_{\tau}$ is a dummy variable indicating whether individuals are born in the year bin $\tau$. Each coefficient $\beta_{\tau}$ captures the ITT of the counterinsurgency policy on the outcomes of individuals in the year bin cohort $\tau$. 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.[14] We expect the $\beta_{\tau}$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 $\beta_{\tau}$ in Figure 2. 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 evidence indicates that nearly all interaction coefficients in the pre-policy years are statistically insignificant, particularly in the period immediately preceding the counterinsurgency. While a small number of coefficients show significance in earlier years, these instances are limited.[15] To ensure that these few pre-trend deviations do not drive our findings, we re-estimate Equation 1 including state-birth year trends (Section 4.2.3), following Angrist and Pischke (2009). The results remain robust, reinforcing the validity of the parallel trends assumption.

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 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

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

[15] We also estimate year-specific effects without binning, and the resulting event-study plots remain consistent.

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 by classifying districts as high counterinsurgency and low/no counterinsurgency districts based on historical approximations

# 4 Results

## 4.1 Main Results

The ITT estimates of the effects of exposure to the Greyhounds operations during school-age years on educational outcomes are presented in Table 2. 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. increase[16] 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.

Overall, the results reveal a clear and consistent pattern—exposure to the Greyhounds operations during school-age years led to substantial gains in human capital.

[16] This represents a 0.1 unit (or 1.42 SD) increase in $Share$.

## 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. This approach not only estimates dynamic treatment effects but also enables placebo checks by comparing periods prior to treatment.

Figures 3 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 (Equation 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 in Fig. 4 . The figure shows that the distribution of most 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 Fig. 5. We also carry out this exercise for the individuals exposed (where we randomize the interaction $YC \times AP$) and obtain similar results.[17] 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 State- Birth Year Trends

We include interactions between birth year 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 year. 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 yearly trends or regional factors that might otherwise influence the outcomes we are studying. As shown in Table 3, the estimated ITT effects for human capital outcomes not only persist but in some cases increase in magnitude. These findings reinforce the robustness of our main results and suggest that they are not merely artifacts of unobserved regional dynamics.[18]

### 4.2.4 Falsification Test Using Pre-Exposure Birth Cohorts

We performed a falsification test using the older cohort (who were ineligible for exposure to the counterinsurgency during schoo-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 positive impact in a cohort that was never eligible for exposure to the policy during their school-age years. Our results (Table 4) confirm that the coefficients for the falsely treated younger cohort are largely statistically insignificant across key economic indicators. Notably, for Completed Years of Schooling and Bachelors completion, the coefficients are actually negative and weakly significant. These findings strengthen the inference that the estimated effects are attributable to the Greyhounds policy rather than broader external factors.

### 4.2.5 Exploiting Within-State Variation in Treatment Intensity

We exploit *within*-state variation in treatment intensity as an additional robustness check. This approach helps mitigate concerns about time-varying state-level confounders that might affect all districts within a state similarly. To identify districts in Andhra Pradesh that likely experienced high levels of Naxalite activity in the pre-policy period, we combine information

[17] The results are available upon request

[18] We also estimated an alternative specification that includes state-specific decade trends. The results remain robust and are available upon request.

from two historical maps and one detailed report. The first map was originally published in *Deshabrati*—a mouthpiece of the revolutionary faction within the CPI(M) and later the organ of the West Bengal unit of CPI(ML)—and is reproduced in Banerjee (1984). The second map, constructed by Borooah (2008), draws on Ministry of Home Affairs reports and open-source platforms such as the South Asia Intelligence Review to identify districts across ten states with documented Naxalite activity in the early 2000s. In addition, Sahoo (2019) identifies districts with the highest Naxalite activities in Andhra Pradesh during the 1980s. Combining these three sources provides a plausible approximation of the districts that experienced high pre-policy Naxalite intensity.[19]

Under the assumption that Greyhounds operations were more intensive in districts with higher pre-policy Naxalite activity, we construct an indicator, *AP High Intensity*, equal to 1 for these districts and 0 for all other districts in our sample. We then interact this indicator with birth cohorts whose school-age years overlapped with the Greyhounds period. This is our covariate of interest. We regress the human capital outcomes on this variable, individual level controls, and birth year and district fixed effects. The identifying assumption is that, in the absence of the counterinsurgency, high-intensity districts in Andhra Pradesh would have followed similar cohort trends as other districts in the state and districts in the comparison states.

Reassuringly, the results—reported in Table 5—are highly consistent with our main findings. An event-study version of this specification, reported in Fig. C.2, shows no evidence of differential pre-trends across high- and low-intensity districts, providing further support for the identifying assumption.

#### 4.2.6 Migration Concerns

Our estimation sample excludes households that did not reside in their current place of residence always. Consequently, households that may have migrated during or after the counterinsurgency period are not included in the analysis. This raises the concern that if the counterinsurgency *induced* displacement among households that were more adversely affected—such as those experiencing insecurity or livelihood disruption—then the excluded group may have had systematically lower educational outcomes.[20] In that case, removing these households would mechanically raise the average schooling of the exposed cohorts in AP, leading to an upward bias in the estimated effect.

[19]Many Andhra Pradesh districts classified as high-intensity in the early 2000s had already begun witnessing substantial insurgency activity in the 1980s.

[20]Migration that occurred before the counterinsurgency does not threaten identification. Pre-policy migration creates level differences, not differences in trends across exposed vs. unexposed cohorts, and DID fundamentally removes level differences.

However, it is likely that displacement-driven migration was limited in this context. The Greyhounds operations were characterized by short, targeted engagements rather than sustained territorial occupation or community-level uprooting. Contemporary accounts do not document widespread or systematic civilian flight due to Greyhound operations. Further, internal migration is anyway remarkably low in India by international standards (Bell et al., 2015) and whatever limited migration does occur, 87% if it is intra-state (census 2001). In the absence of large-scale displacement of vulnerable households, the scenario under which the exclusion of movers would lead to upward bias is unlikely. Therefore, while the direction of potential bias is theoretically clear, the empirical likelihood of a substantial upward distortion in our estimated effects is low.

#### 4.2.7 Additional Robustness Tests

We conduct a series of additional robustness checks and falsification tests which are discussed in Appendix A. Across these exercises, the estimated effects remain stable in both magnitude and statistical significance, closely mirroring the main results.

### 4.3 Additional Outcomes

To further assess the broader impacts of the Greyhounds policy, we examine its effects on labor market outcomes in adulthood, household socioeconomic status, and individuals' confidence in institutions.[21] The results, reported in Tables B.7–B.9 in the Appendix, indicate that exposure to Greyhounds operations during school-age years led to higher labor supply and earnings in adulthood. Moreover, households with such individuals reported higher total income, greater asset accumulation, and lower poverty rates in 2011. Finally, we find that exposure to Greyhounds operations increased confidence in the police and state government in adulthood. These findings suggest that the Greyhounds policy, in addition to human capital outcomes, had long-term positive effects on labor market outcomes, economic well-being, and institutional trust.

## 5 Discussion

### 5.1 Potential Mechanism

If exposure to the Greyhounds policy during school-age years improved educational outcomes, a key mechanism is likely a reduction in violence and an improvement in local security

[21] The relevant summary statistics are reported in Table B.6.

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

$$Y_{st} = \alpha + \beta(AP_s \times Post_t) + \gamma X_{st} + \delta_s + \lambda_t + \epsilon_{st} \tag{3}$$

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. $\delta_s$ and $\lambda_t$ are state fixed effects and year fixed effects, respectively; and $\epsilon_{st}$ 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 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.[22]

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%

[22] Since 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.

were from the Greyhounds.[23]

Improved security during school-age years can shape educational 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 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.[24] Specifically, we estimate the following specification:

$$\log(Y_{ist} + 1) = \alpha + \beta(AP_s \times Post_t) + \gamma X_{ist} + \delta_s + \lambda_t + \epsilon_{ist} \tag{4}$$

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; $\delta_s$ are state fixed effects; $\lambda_t$ are round fixed effects; and $\epsilon_{ist}$ 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 investments in education, providing a plausible pathway for the improvements in educational attainment 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

[23] https://www.thehindu.com/todays-paper/tp-national/tp-andhrapradesh/greyhounds-among-the-best-anti-insurgency-forces-experts/article17568627.ece

[24] See discussion in Section 3.1.4.

the Unified District Information System for Education (U-DISE). Specifically, we estimate a specification similar to Equation 3, 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.

In addition, we analyze enrollment patterns in AP during the period of Greyhound activity, using a specification analogous to Equation 4, where the outcome variable captures whether an individual is currently enrolled in a private or public school.[25] 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 1990s[26]—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 Alternative Explanations

Next, we examine and rule out several alternative explanations that could plausibly account for 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.

[25] However, the outcome variable is now used in levels rather than in logarithms.

[26] India currently has many 'low cost' private schools. But that is a relatively recent phenomenon. Such private schools were not very common before 2000s.

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. C.3 and C.4 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.

### 5.2.2 State-Level Economic Growth

It is plausible that divergent economic growth trajectories, rather than the counterinsurgency program, could be driving the observed effects. If AP was already on a distinct growth trajectory—independently increasing investment in human capital—our estimates could capture the effects of rising economic opportunities rather than improved security. To rule out this concern, we introduce a control for the state's share of national GDP in our main specification Eq. (1), for which the timing of the measure is critical [27]. There are two plausible specifications. The first, and arguably cleanest, approach is to control for the state's share of national GDP in the individual's birth year. This variable is pre-determined and captures the baseline economic condition of the state an individual was born into, well before the households made any educational or life-cycle decisions. This specification is unlikely to be affected by the counterinsurgency operations.

Alternatively, the contemporaneous economic condition during schooling might be more relevant than the birth-year baseline. A state's economic growth may shift, and the opportunities available during an individual's schooling years (ages 5-17) could be the more dominant factor. We construct a state-level measure of economic development experienced during schooling years by computing the average of State share of national Gross Domestic Product (GDP) over the period when each individual was aged 5–17. Our data on state share of national GDP is only available from the 1960-61 fiscal year onwards[28]. Hence, individuals born before 1960 are not included in these regressions.

We present the results from both specifications in Table B.10 in the Appendix. Panel I shows the estimates when controlling for the birth-year GDP share of the state, while Panel II shows the estimates when controlling for the school-age-average state GDP share. As is

[27] Data on state GDP is not available for initial years in our analysis which is why we use state share of national GDP.

[28] Our data for this measure is drawn from a working paper by the Economic Advisory Council to the Prime Minister (EACPM) for the decadal years 1960-61, 1970-71, 1980-81, 1990-91, and 2000-01 (https://eacpm.gov.in/wp-content/uploads/2024/09/State-GDP-Working-Paper_Final.pdf). Following the methodology of this source, the ratio for 1960-61 and 1970-71 is calculated as the state's Net State Domestic Product (NSDP) as a ratio of the sum of NSDP for all states. For the subsequent years, the measure is based on Gross State Domestic Product (GSDP).

evident, including these controls leaves our estimated treatment effects essentially unchanged, indicating that the results are not driven by concurrent state economic growth unrelated to improvements in security.

### 5.2.3 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 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 B.11, the results remain robust, lending further credibility to our main findings.

### 5.2.4 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.

### 5.2.5 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.[29]

### 5.2.6 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

[29] These states include Bihar, Uttar Pradesh, and Karnataka

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.

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 B.12).

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 B.13, are consistent with our main estimates suggesting the IT sector's growth is unlikely to be driving the observed gains in educational outcomes.[30]

Additionally, we drop Hyderabad, the state's primary economic hub and the nucleus of the later IT surge. To ensure that our estimates are not influenced by urban-specific growth dynamics, the results presented in Table B.14, remain highly consistent with our main findings.

# 6 Conclusion

We study the effects of exposure to counterinsurgency policy during school-age years on human capital formation in India. 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.

[30] It 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.

DID estimates suggest that exposure to the Greyhounds policy during school-age years led to significantly higher educational attainment. These results remain robust across a range of robustness checks and falsification tests. Further analysis suggests that a key driver of these effects is increased household investment in education, likely resulting from improved local security conditions. 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.

While AP's experience with the Greyhounds was distinctive —and our findings should therefore be interpreted as evidence from a specific institutional context rather than as a general endorsement of similar policies elsewhere—they do suggest that counterinsurgency efforts, when effectively targeted and sustained, can potentially generate positive spillovers well beyond their immediate security objectives. This underscores the importance of viewing peacebuilding and development not as sequential but as 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 |
| | | | |
| **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: ITT Estimates of the Effects of the Greyhounds Operation on Human Capital

| | (1) Literacy | (2) Attended School | (3) Completed Years | (4) Secondary | (5) Higher Secondary | (6) Bachelors & above | (7) English Ability |
|---|---|---|---|---|---|---|---|
| **Panel A: Whether Exposed** | | | | | | | |
| YC X AP | 0.063*** | 0.052** | 0.593** | 0.036** | -0.001 | 0.031** | 0.061*** |
| | (0.021) | (0.025) | (0.287) | (0.015) | (0.009) | (0.012) | (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*** | 0.165*** | 2.146*** | 0.120*** | 0.006 | 0.099*** | 0.196*** |
| | (0.035) | (0.040) | (0.428) | (0.034) | (0.023) | (0.031) | (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: ITT Estimates of the Effects of the Greyhounds Operation on Human Capital with State-Specific Birth-Year Trends

| | (1) Literacy | (2) Attended School | (3) Completed Years | (4) Secondary | (5) Higher Secondary | (6) Bachelors & above | (7) English Ability |
|---|---|---|---|---|---|---|---|
| **Panel A: Whether Exposed** | | | | | | | |
| YC X AP | 0.073*** | 0.081*** | 0.958*** | 0.019 | 0.011 | 0.036** | 0.056** |
| | (0.028) | (0.022) | (0.294) | (0.014) | (0.012) | (0.016) | (0.025) |
| Observations | 45755 | 45755 | 45755 | 45755 | 45755 | 45755 | 45753 |
| R-squared | 0.253 | 0.267 | 0.295 | 0.048 | 0.040 | 0.058 | 0.100 |
| **Panel B: Share exposed** | | | | | | | |
| Share X AP | 0.215*** | 0.252*** | 3.435*** | 0.125*** | 0.035 | 0.134** | 0.238*** |
| | (0.042) | (0.036) | (0.680) | (0.038) | (0.027) | (0.054) | (0.057) |
| Observations | 45755 | 45755 | 45755 | 45755 | 45755 | 45755 | 45753 |
| R-squared | 0.253 | 0.267 | 0.296 | 0.049 | 0.040 | 0.058 | 0.101 |
| 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, state fixed effects, and state-specific linear trends in birth year. 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: Placebo ITT Estimates Using Pre-Exposure Birth Cohorts

| | (1) Literacy | (2) Attended School | (3) Completed Years | (4) Secondary | (5) Higher Secondary | (6) Bachelors & above | (7) English Ability |
|---|---|---|---|---|---|---|---|
| **Panel A: Whether Exposed** | | | | | | | |
| 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 B: Share exposed** | | | | | | | |
| Share X AP | -0.011 | -0.079 | -0.954 | 0.075** | -0.027 | -0.051** | -0.110 |
| | (0.091) | (0.086) | (0.743) | (0.032) | (0.058) | (0.021) | (0.082) |
| Observations | 5751 | 5751 | 5751 | 5751 | 5751 | 5751 | 5751 |
| R-squared | 0.279 | 0.290 | 0.271 | 0.101 | 0.025 | 0.037 | 0.108 |
| 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: ITT Estimates of the Effects of the Greyhounds Operation on Human Capital Assigning Treatment at the District-Level

| | (1) Literacy | (2) Attended School | (3) Completed Years | (4) Secondary | (5) Higher Secondary | (6) Bachelors & above | (7) English Ability |
|---|---|---|---|---|---|---|---|
| **Panel A: Whether Exposed** | | | | | | | |
| YC X AP High Intensity | 0.059** | 0.059*** | 0.418** | 0.026*** | -0.008 | 0.016 | 0.042*** |
| | (0.024) | (0.020) | (0.172) | (0.010) | (0.008) | (0.012) | (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 |
| **Panel B: Share exposed** | | | | | | | |
| Share X AP High Intensity | 0.179*** | 0.174*** | 1.515*** | 0.109*** | -0.016 | 0.045 | 0.155*** |
| | (0.050) | (0.042) | (0.481) | (0.028) | (0.017) | (0.043) | (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 |
| Demographics | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Birth year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| District FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

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

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

| | (1) Total Conflicts | (2) Specific Conflicts | (3) Number of Successful Attacks |
|---|---|---|---|
| Greyhounds | -253.051*** | -85.577*** | -3.992 |
| | (75.833) | (26.972) | (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: ITT Estimates of the Effects of the Greyhounds Operation on Household Education Expenditure

| | (1) Tuition fees (school/college) | (2) Other educational expenses | (3) Stationary articles | (4) Books & journals | (5) Total |
|---|---|---|---|---|---|
| **Panel A: Expenditure** | | | | | |
| AP X Post | 0.350*** | 0.170** | 0.132** | 0.071 | 0.286*** |
| | (0.113) | (0.076) | (0.061) | (0.057) | (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 | 0.010* | 0.018*** | 0.004 | 0.042*** |
| | (0.006) | (0.006) | (0.005) | (0.004) | (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 the Greyhounds Operation on Number of School Establishment and Enrollment

**Panel I: Number of Schools Established**

| | Public | Private | ln(Public) | ln(Private) |
|---|---|---|---|---|
| AP X Post | -24.80 | 183.5*** | 0.284 | 0.139* |
| | (289.5) | (63.06) | (0.181) | (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** | 0.0650* |
| | (0.0337) | (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

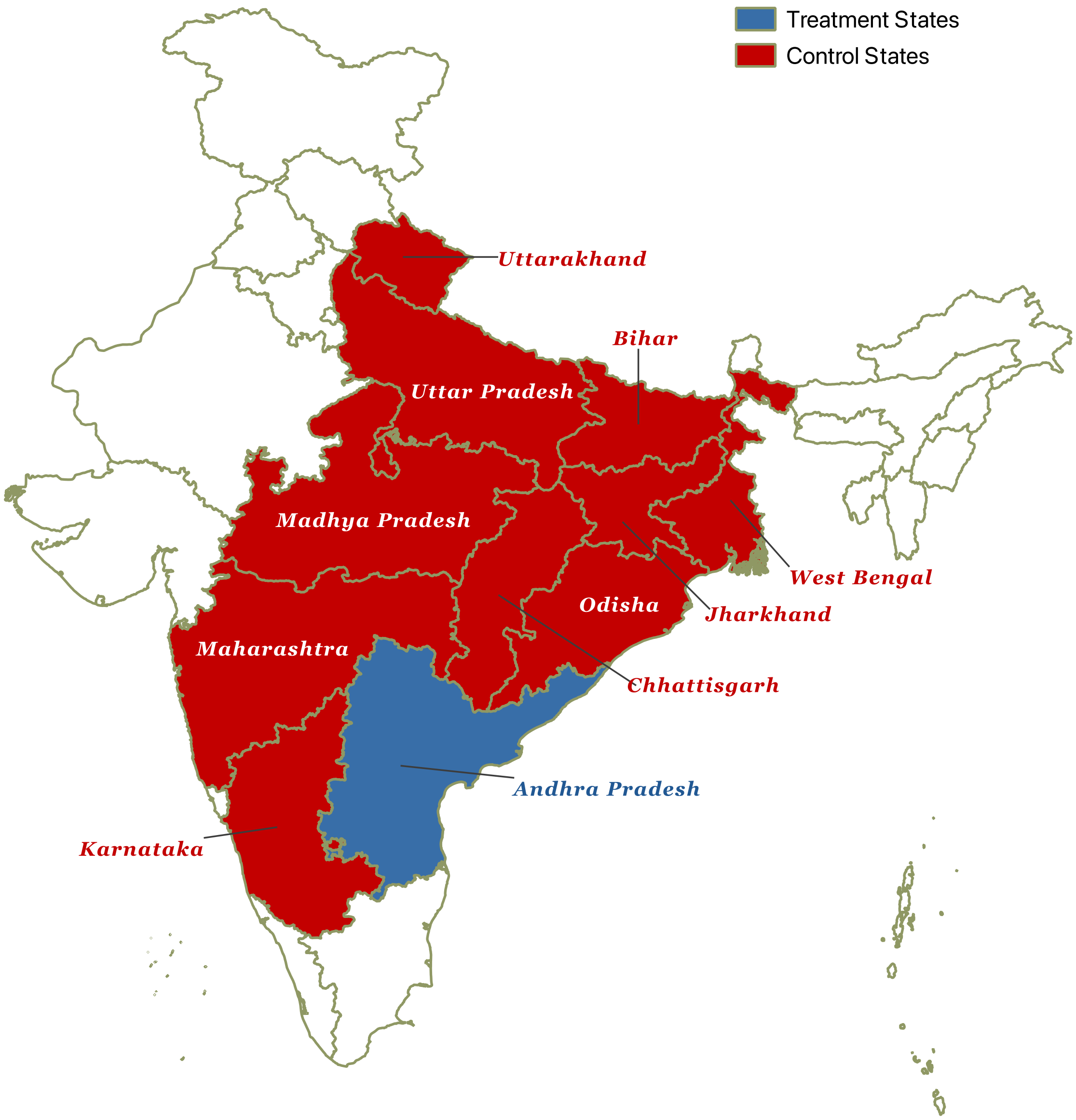


Figure 1: **States of India affected by Naxalite Insurgency Prior to 2000**
*Note:* Map constructed following Singhal and Nilakantan (2016). Chhattisgarh, Uttarakhand, and Jharkhand were created in 2000 from Madhya Pradesh, Uttar Pradesh, and Bihar respectively. Since IHDS reports these states separately, our analysis uses the post-2000 state boundaries.

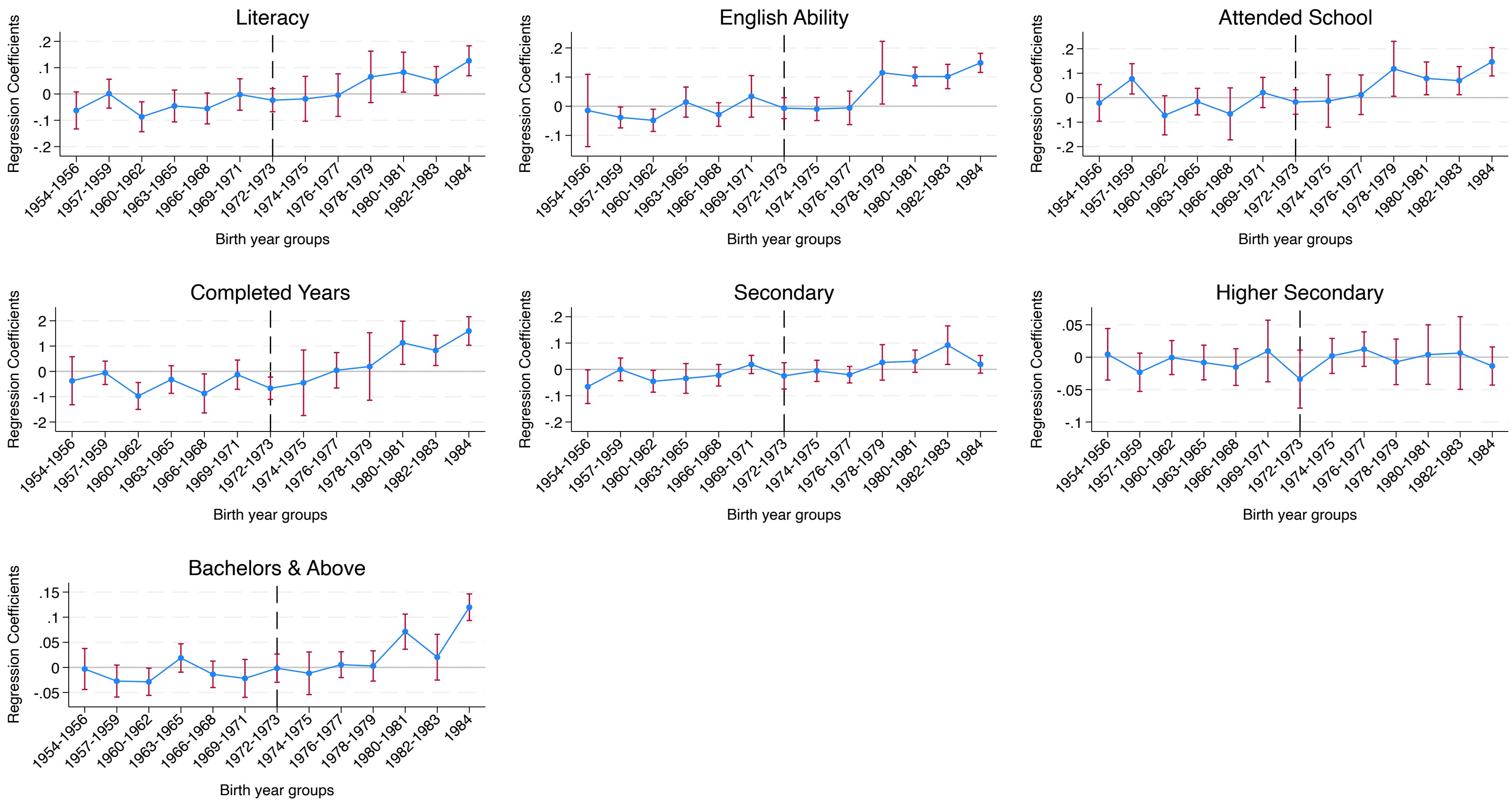


Figure 2: **ITT Estimates of the Effects of the Greyhounds Operation on Human Capital, Event Study Plots**
*Note:* This figure shows the estimated impact of exposure to the 1989 Greyhounds counterinsurgency policy on education, using the event study specification outlined in equation 2. Estimates are 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.

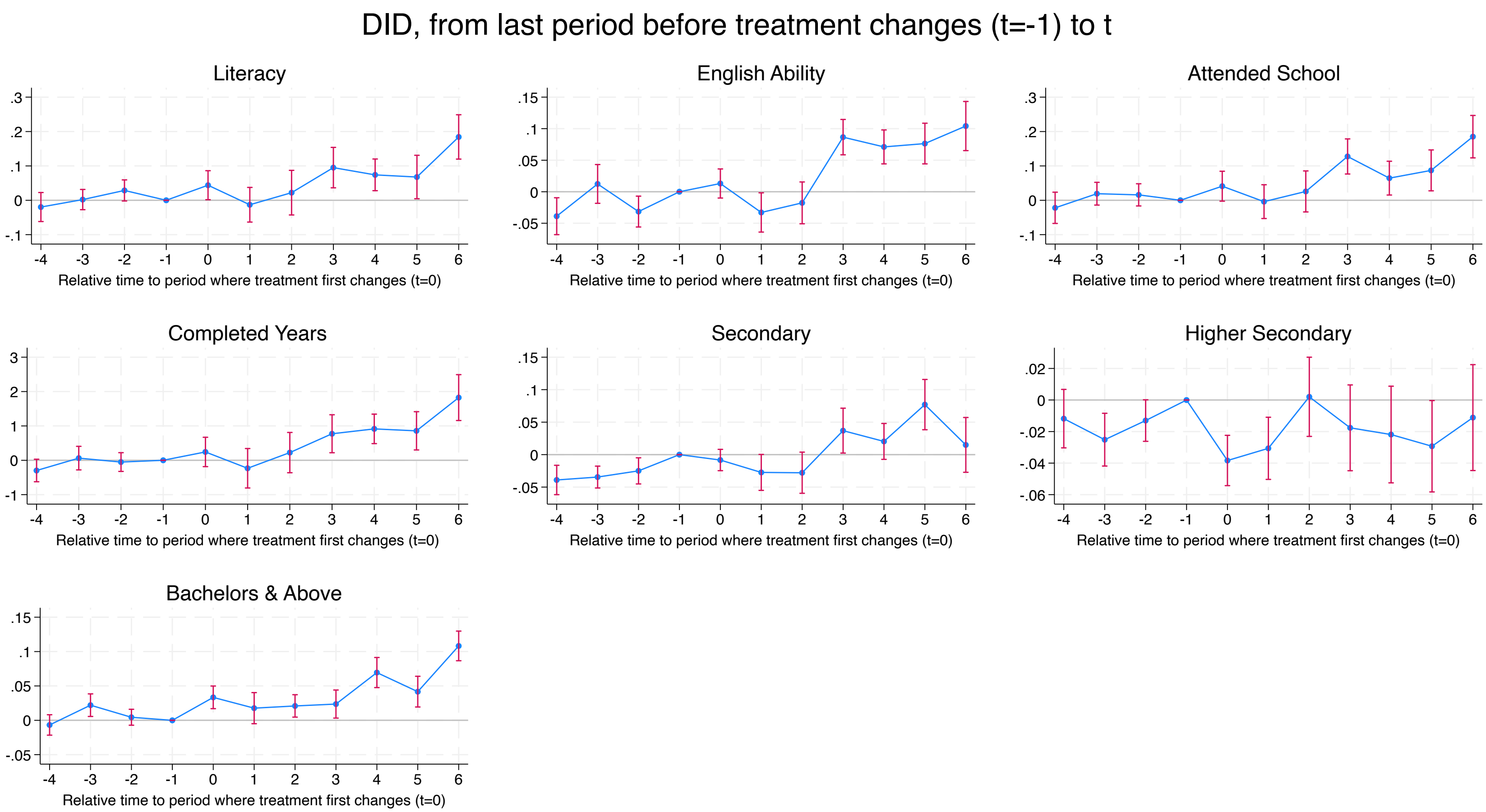


Figure 3: **ITT Estimates of the Greyhounds Operation on Human Capital, Robust to Treatment-Effect Heterogeneity (Using the de Chaisemartin–D'Haultfoeuille Estimator)**
*Note:* The figure reports estimates and confidence intervals estimated using the estimator proposed by de Chaisemartin and D'Haultfœuille (2020), implemented via the `did_multiplegt` Stata command, available from the SSC repository

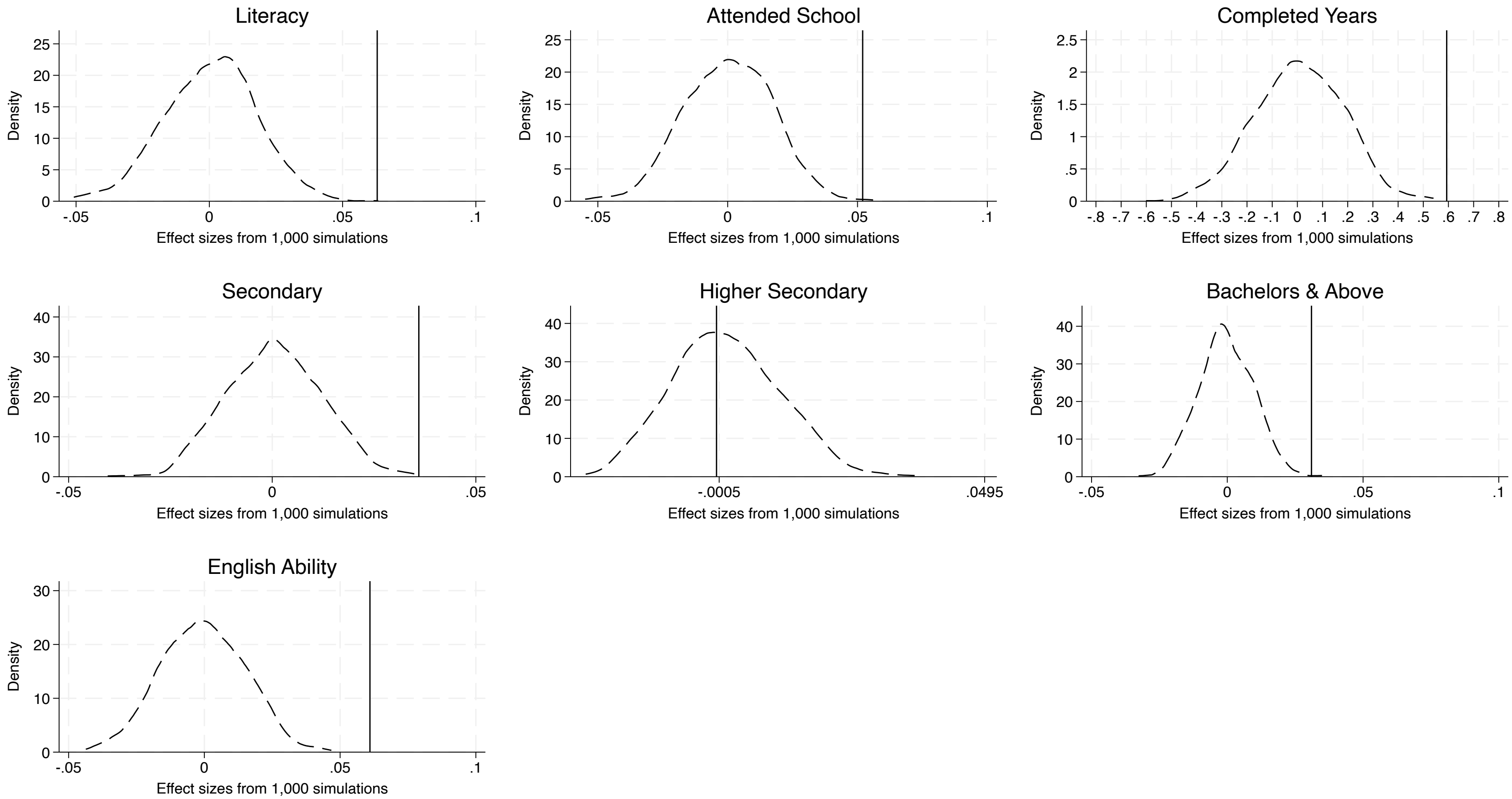


Figure 4: **Exact Randomization Test based on State**

*Note:* We plot the distribution of estimated coefficients of the exposure variable (Younger Cohort × 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.

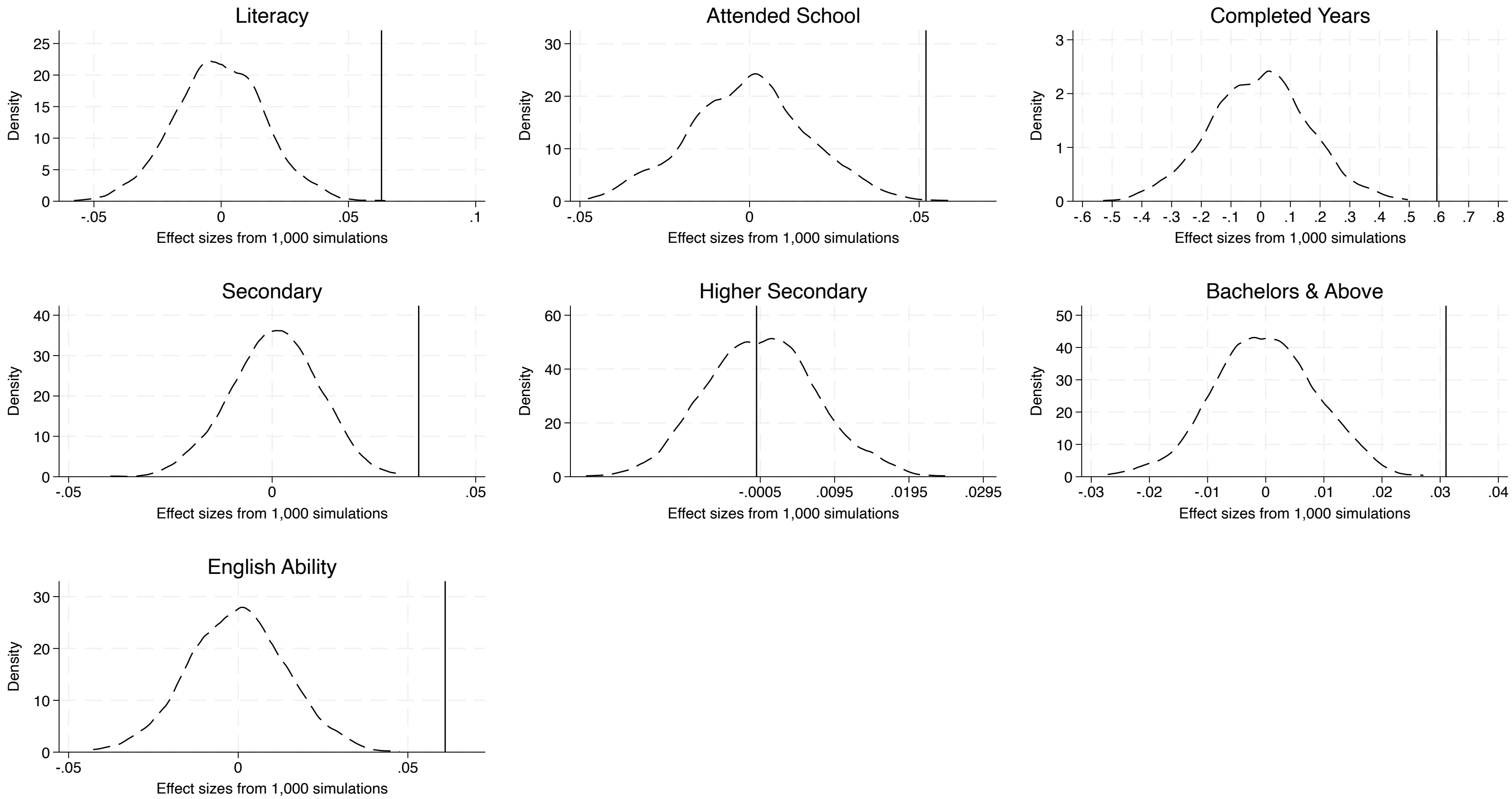


Figure 5: **Exact Randomization Test based on Cohort**

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

# Appendix

## Guns and Gains: Human Capital Effects of Exposure to Counterinsurgency Operations

# A Additional Robustness Checks

## A.1 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, 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 B.1 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.

## A.2 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.[31] 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 B.2, remain consistent with our primary findings and provide additional support for the robustness of our estimates.

## A.3 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

[31] Although our sample takes care of spillover through civilian migration

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 B.3 and Table B.4. As evident, the results are broadly in line with our baseline findings.

## A.4 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 B.5. 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.

# B Appendix - Tables

Table B.1: ITT Estimates of the Effects of the Greyhounds Operation on Human Capital excluding 18-24 Years Cohort

| | (1) Literacy | (2) Attended School | (3) Completed Years | (4) Secondary | (5) Higher Secondary | (6) Bachelors & above | (7) English Ability |
|---|---|---|---|---|---|---|---|
| **Panel A: Whether Exposed** | | | | | | | |
| YC X AP | 0.061*** | 0.042* | 0.471* | 0.038** | -0.002 | 0.028** | 0.062*** |
| | (0.022) | (0.025) | (0.284) | (0.016) | (0.009) | (0.012) | (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 |
| **Panel B: Share exposed** | | | | | | | |
| Share X AP | 0.172*** | 0.152*** | 2.011*** | 0.125*** | 0.006 | 0.096*** | 0.201*** |
| | (0.036) | (0.038) | (0.422) | (0.035) | (0.023) | (0.032) | (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 |
| 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 B.2: ITT Estimates of the Effects of the Greyhounds Operation on Human Capital using Bordering States of Andhra Pradesh

| | (1) Literacy | (2) Attended School | (3) Completed Years | (4) Secondary | (5) Higher Secondary | (6) Bachelors & above | (7) English Ability |
|---|---|---|---|---|---|---|---|
| **Panel A: Whether Exposed** | | | | | | | |
| YC X AP | 0.016 | 0.005 | -0.134 | 0.015 | -0.018** | 0.025** | 0.030* |
| | (0.016) | (0.019) | (0.231) | (0.013) | (0.008) | (0.011) | (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 |
| **Panel B: Share exposed** | | | | | | | |
| Share X AP | 0.059* | 0.053* | 0.623 | 0.071** | -0.039* | 0.099*** | 0.138*** |
| | (0.030) | (0.031) | (0.413) | (0.036) | (0.020) | (0.027) | (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 |
| 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 B.3: ITT Estimates of the Effects of the Greyhounds Operation on Human Capital excluding Bihar (and present-day Jharkhand)

| | (1) Literacy | (2) Attended School | (3) Completed Years | (4) Secondary | (5) Higher Secondary | (6) Bachelors & above | (7) English Ability |
|---|---|---|---|---|---|---|---|
| **Panel A: Whether Exposed** | | | | | | | |
| YC X AP | 0.061*** | 0.050** | 0.512* | 0.036** | 0.000 | 0.028** | 0.062*** |
| | (0.020) | (0.024) | (0.275) | (0.016) | (0.009) | (0.011) | (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 |
| **Panel B: Share exposed** | | | | | | | |
| Share X AP | 0.166*** | 0.158*** | 1.895*** | 0.117*** | 0.008 | 0.089*** | 0.191*** |
| | (0.034) | (0.037) | (0.440) | (0.034) | (0.022) | (0.031) | (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 |
| 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 B.4: ITT Estimates of the Effects of the Greyhounds Operation on Human Capital including Kerala as a Control State

| | (1) Literacy | (2) Attended School | (3) Completed Years | (4) Secondary | (5) Higher Secondary | (6) Bachelors & above | (7) English Ability |
|---|---|---|---|---|---|---|---|
| **Panel A: Whether Exposed** | | | | | | | |
| YC X AP | 0.067*** | 0.057*** | 0.536** | 0.033*** | -0.003 | 0.027 | 0.049*** |
| | (0.021) | (0.017) | (0.229) | (0.011) | (0.011) | (0.018) | (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 |
| **Panel B: Share exposed** | | | | | | | |
| Share X AP | 0.183*** | 0.175*** | 2.044*** | 0.114*** | 0.004 | 0.090* | 0.177*** |
| | (0.043) | (0.038) | (0.603) | (0.027) | (0.022) | (0.051) | (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 |
| 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 B.5: ITT Estimates of the Effects of the Greyhounds Operation on Human Capital using only Rural Sample

| | (1) Literacy | (2) Attended School | (3) Completed Years | (4) Secondary | (5) Higher Secondary | (6) Bachelors & above | (7) English Ability |
|---|---|---|---|---|---|---|---|
| **Panel A: Whether Exposed** | | | | | | | |
| YC X AP | 0.051** | 0.035 | 0.236 | 0.030* | -0.001 | 0.011 | 0.043** |
| | (0.021) | (0.026) | (0.262) | (0.016) | (0.011) | (0.010) | (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 |
| **Panel B: Share exposed** | | | | | | | |
| Share X AP | 0.149*** | 0.126** | 1.269*** | 0.106*** | 0.009 | 0.046* | 0.155*** |
| | (0.040) | (0.050) | (0.416) | (0.036) | (0.032) | (0.025) | (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 |
| 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 B.6: Additional Summary Statistics

| | N | Mean | SD |
|---|---|---|---|
| **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 |
| | | | |
| **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 B.7: ITT Estimates of the Effects of the Greyhounds Operation on Workforce Participation and Labor Supply

| | (1) Any work | (2) Agriculture | (3) Non-agriculture | (4) Salary work | (5) Business work | (6) Farm work | (7) Work Days | (8) Work Hours |
|---|---|---|---|---|---|---|---|---|
| **Panel A: Whether Exposed** | | | | | | | | |
| YC X AP | 0.125*** | -0.050** | -0.062*** | 0.077*** | 0.001 | 0.034 | 28.187*** | 201.538*** |
| | (0.021) | (0.025) | (0.023) | (0.020) | (0.013) | (0.022) | (5.424) | (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*** | -0.150*** | -0.081 | 0.185*** | 0.007 | 0.044 | 60.641*** | 425.242*** |
| | (0.047) | (0.040) | (0.050) | (0.038) | (0.034) | (0.040) | (10.927) | (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 B.8: ITT Estimates of the Effects of the Greyhounds Operation 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** | 0.159** | 0.122*** | 0.140*** | 0.119*** | -0.056*** | 0.560*** |
| | (0.064) | (0.064) | (0.041) | (0.047) | (0.046) | (0.012) | (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*** | 0.407*** | 0.174** | 0.236*** | 0.163* | -0.088** | 0.725** |
| | (0.120) | (0.123) | (0.087) | (0.088) | (0.091) | (0.038) | (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 B.9: ITT Estimates of the Effects of the Greyhounds Operation 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 | -0.005 | 0.037** | 0.042** | 0.006 | 0.003 |
| | (0.025) | (0.009) | (0.016) | (0.017) | (0.014) | (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 | -0.011 | 0.075** | 0.054 | 0.011 | -0.008 |
| | (0.052) | (0.019) | (0.037) | (0.034) | (0.041) | (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 B.10: ITT Estimates of the Effects of the Greyhounds Operation on Human Capital Including Control for State Share of National GDP

| | (1) Literacy | (2) Attended School | (3) Completed Years | (4) Secondary | (5) Higher Secondary | (6) Bachelors & above | (7) English Ability |
|---|---|---|---|---|---|---|---|
| **Panel I: Controlling for State GDP Share at Birth Year** | | | | | | | |
| **A. Whether Exposed** | | | | | | | |
| YC X AP | 0.082*** | 0.084** | 0.992*** | 0.039** | 0.005 | 0.036*** | 0.067*** |
| | (0.024) | (0.033) | (0.325) | (0.018) | (0.010) | (0.013) | (0.024) |
| Observations | 30428 | 30428 | 30428 | 30428 | 30428 | 30428 | 30427 |
| R-squared | 0.199 | 0.213 | 0.247 | 0.036 | 0.037 | 0.054 | 0.089 |
| **B. Share Exposed** | | | | | | | |
| Share X AP | 0.206*** | 0.218*** | 2.915*** | 0.131*** | 0.021 | 0.109*** | 0.216*** |
| | (0.038) | (0.048) | (0.434) | (0.039) | (0.025) | (0.033) | (0.026) |
| Observations | 30428 | 30428 | 30428 | 30428 | 30428 | 30428 | 30427 |
| R-squared | 0.199 | 0.213 | 0.248 | 0.037 | 0.037 | 0.055 | 0.090 |
| **Panel II: Controlling for Average State GDP Share during Schooling (Ages 5-17)** | | | | | | | |
| **A. Whether Exposed** | | | | | | | |
| YC X AP | 0.060*** | 0.063** | 0.604** | 0.021 | -0.012 | 0.039*** | 0.049** |
| | (0.023) | (0.032) | (0.284) | (0.017) | (0.009) | (0.012) | (0.021) |
| Observations | 30428 | 30428 | 30428 | 30428 | 30428 | 30428 | 30427 |
| R-squared | 0.199 | 0.213 | 0.248 | 0.037 | 0.037 | 0.054 | 0.089 |
| **B. Share Exposed** | | | | | | | |
| Share X AP | 0.149*** | 0.167*** | 1.881*** | 0.082** | -0.031 | 0.126*** | 0.173*** |
| | (0.040) | (0.048) | (0.431) | (0.040) | (0.025) | (0.032) | (0.027) |
| Observations | 30428 | 30428 | 30428 | 30428 | 30428 | 30428 | 30427 |
| R-squared | 0.199 | 0.213 | 0.248 | 0.038 | 0.037 | 0.055 | 0.090 |
| 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. Panel I regressions include the state share of national GDP from the individual's birth year. Panel II regressions include the average state share of national GDP during the individual's schooling years (ages 5-17). 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 B.11: ITT Estimates of the Effects of the Greyhounds Operation on Human Capital excluding States with Major Anti-Mandal Agitations

| | (1) Literacy | (2) Attended School | (3) Completed Years | (4) Secondary | (5) Higher Secondary | (6) Bachelors & above | (7) English Ability |
|---|---|---|---|---|---|---|---|
| **Panel A: Whether Exposed** | | | | | | | |
| YC X AP | 0.042** | 0.029 | 0.251 | 0.027** | -0.006 | 0.029** | 0.053*** |
| | (0.018) | (0.022) | (0.253) | (0.013) | (0.008) | (0.012) | (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 |
| **Panel B: Share exposed** | | | | | | | |
| Share X AP | 0.127*** | 0.112*** | 1.443*** | 0.091*** | -0.012 | 0.104*** | 0.182*** |
| | (0.035) | (0.036) | (0.433) | (0.033) | (0.020) | (0.028) | (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 |
| 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 B.12: ITT Estimates of the Effects of the Greyhounds Operation on Human Capital excluding Cohort Exposed to the IT Boom

| | (1) Literacy | (2) Attended School | (3) Completed Years | (4) Secondary | (5) Higher Secondary | (6) Bachelors & above | (7) English Ability |
|---|---|---|---|---|---|---|---|
| **Panel A: Whether Exposed** | | | | | | | |
| YC X AP | 0.053** | 0.037 | 0.433 | 0.024* | 0.004 | 0.023** | 0.046** |
| | (0.021) | (0.025) | (0.300) | (0.014) | (0.009) | (0.010) | (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 |
| **Panel B: Share exposed** | | | | | | | |
| Share X AP | 0.172*** | 0.138*** | 2.035*** | 0.100*** | 0.039* | 0.090*** | 0.183*** |
| | (0.047) | (0.052) | (0.607) | (0.026) | (0.022) | (0.022) | (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 |
| 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 B.13: ITT Estimates of the Effects of the Greyhounds Operation on Human Capital using only Karnataka as a Control State

| | (1) Literacy | (2) Attended School | (3) Completed Years | (4) Secondary | (5) Higher Secondary | (6) Bachelors & above | (7) English Ability |
|---|---|---|---|---|---|---|---|
| **Panel A: Whether Exposed** | | | | | | | |
| YC X AP | 0.038** | 0.017 | 0.205 | 0.011 | -0.018** | 0.033*** | 0.039*** |
| | (0.016) | (0.016) | (0.218) | (0.010) | (0.008) | (0.011) | (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 |
| **Panel B: Share exposed** | | | | | | | |
| Share X AP | 0.091*** | 0.057** | 1.151*** | 0.038 | -0.027 | 0.120*** | 0.146*** |
| | (0.026) | (0.023) | (0.384) | (0.028) | (0.025) | (0.020) | (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 |
| 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 B.14: ITT Estimates of the Effects of the Greyhounds Operation on Human Capital excluding Hyderabad

| | (1) Literacy | (2) Attended School | (3) Completed Years | (4) Secondary | (5) Higher Secondary | (6) Bachelors & above | (7) English Ability |
|---|---|---|---|---|---|---|---|
| **Panel A: Whether Exposed** | | | | | | | |
| YC X AP | 0.061*** | 0.048* | 0.447* | 0.034** | 0.000 | 0.017 | 0.057*** |
| | (0.020) | (0.025) | (0.269) | (0.015) | (0.009) | (0.010) | (0.021) |
| Observations | 45580 | 45580 | 45580 | 45580 | 45580 | 45580 | 45578 |
| R-squared | 0.252 | 0.265 | 0.291 | 0.046 | 0.039 | 0.056 | 0.098 |
| **Panel B: Share exposed** | | | | | | | |
| Share X AP | 0.172*** | 0.161*** | 1.754*** | 0.116*** | 0.007 | 0.063** | 0.186*** |
| | (0.035) | (0.039) | (0.397) | (0.032) | (0.025) | (0.027) | (0.025) |
| Observations | 45580 | 45580 | 45580 | 45580 | 45580 | 45580 | 45578 |
| R-squared | 0.252 | 0.266 | 0.291 | 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

# C Appendix - Figures

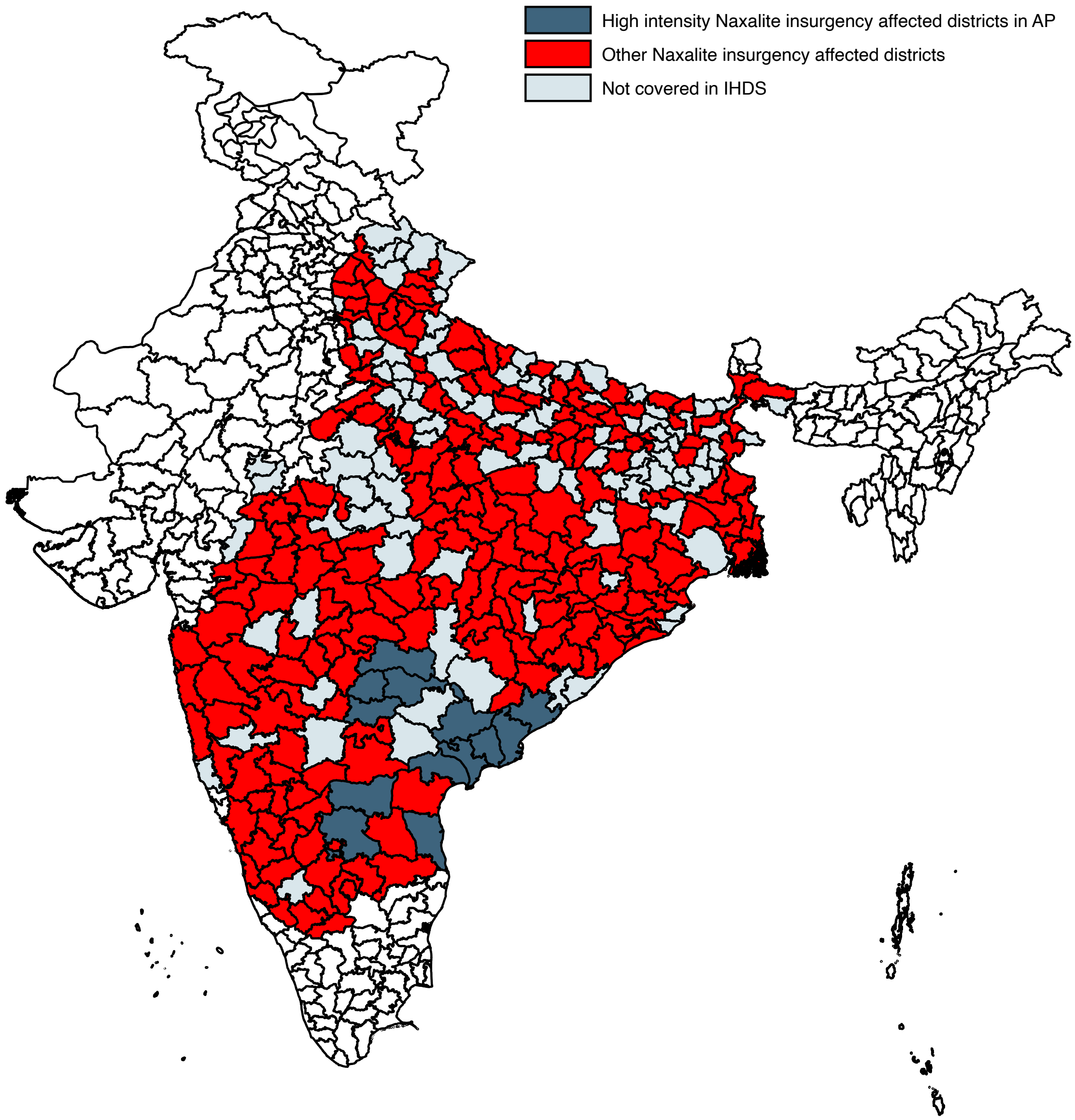


Figure C.1: **Districts affected by Naxalite Insurgency**
*Note:* The classification is based on three sources: a map from the 1970s reproduced in Banerjee (1984), originally published in Deshabrati; a map compiled by Borooah (2008); and district-level information from Sahoo (2019) on Naxalite activity in the 1980s.

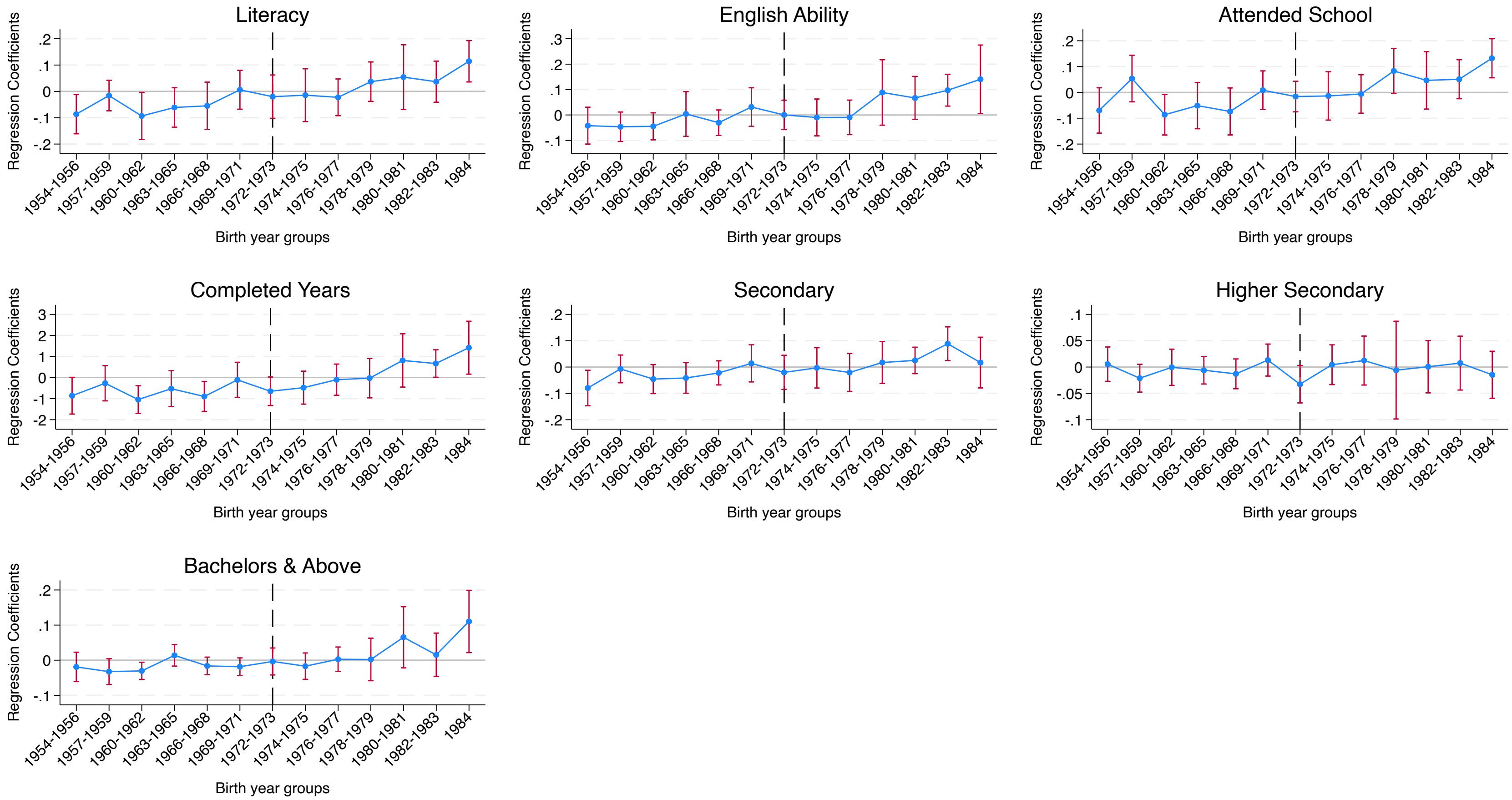


Figure C.2: **ITT Estimates of the Effects of the Greyhounds Operation on Human Capital, Event Study Plot for High Intensity AP Districts**

*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.

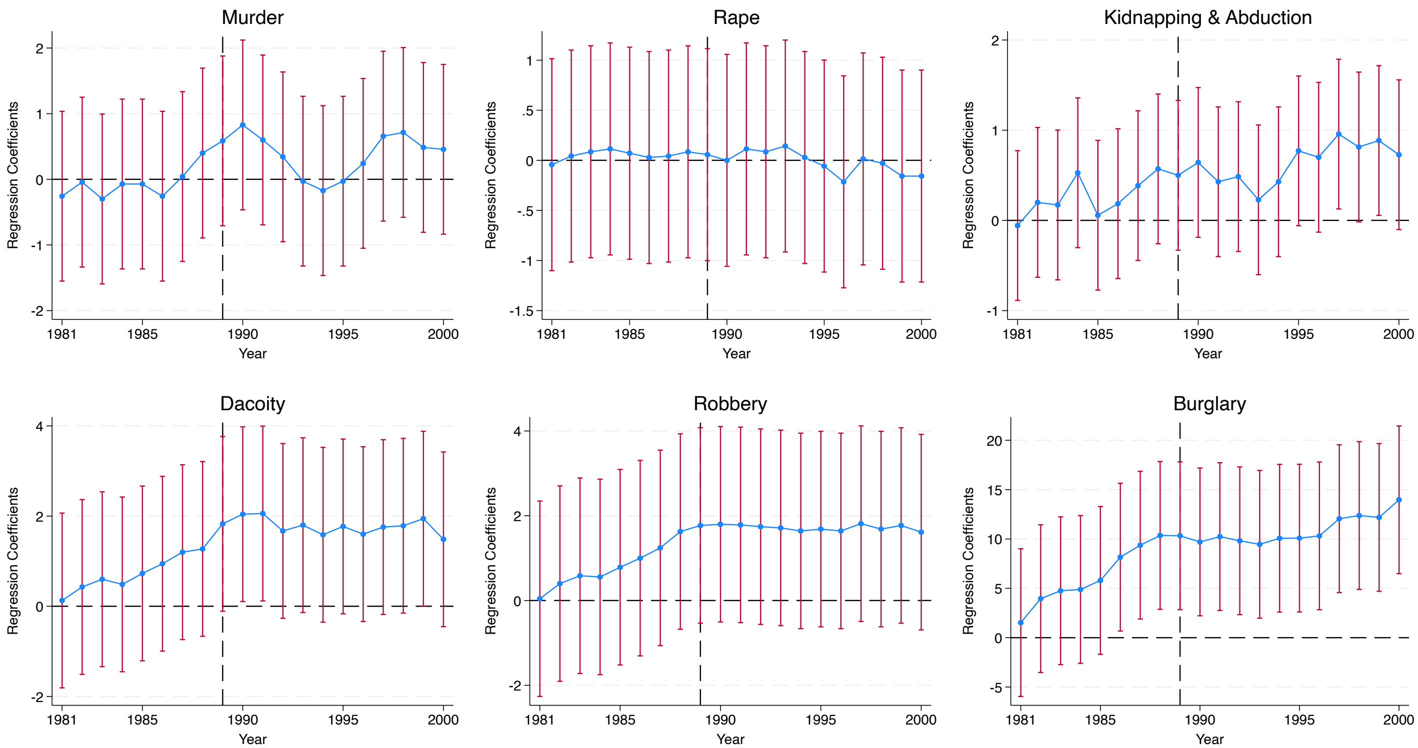


Figure C.3: **Effects of the Greyhounds Operation on Other crime rates**
*Note:* The figure presents the estimates and confidence intervals of the effect of the 1989 counterinsurgency policy on general crime rates using an event study specification.

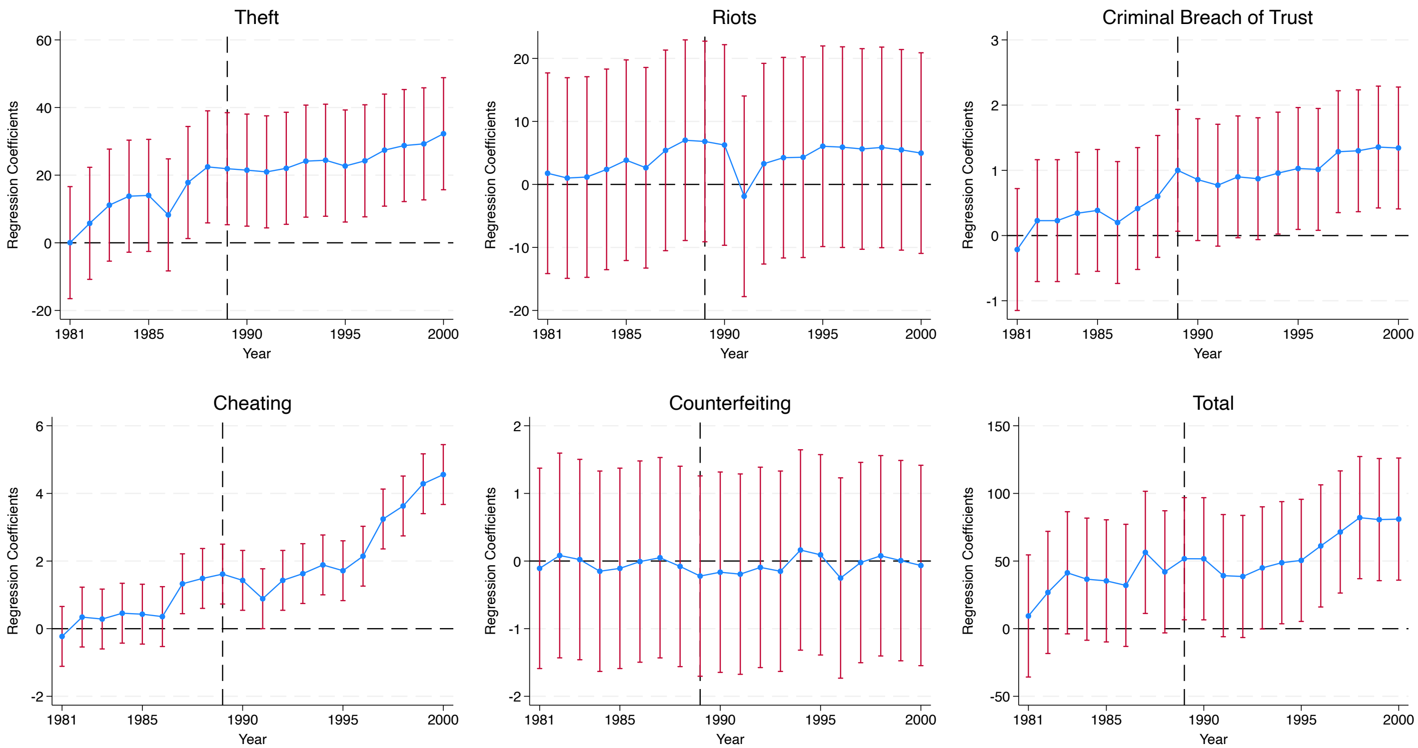


Figure C.4: **Effects of the Greyhounds Operation on Other crime rates (continued)**
*Note:* The figure presents the estimates and confidence intervals of the effect of the 1989 counterinsurgency policy on general crime rates using an event study specification.