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DIGGING FOR TROUBLE? MINING AND CRIMINAL BEHAVIOR OF YOUNG MALES

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Mining and Criminal Behavior of Young Males

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Abstract

This paper studies how a large, positive local economic shock affects criminal behavior. Better labor market opportunities raise the opportunity cost of crime, but higher incomes also relax the budget constraint for drugs and alcohol. I study both forces using the 2004 iron ore boom in northern Sweden, combining geocoded administrative data on all criminal convictions with a difference-in-differences design and causal forests. Among men aged 18–29 in mining municipalities, property crime convictions fall by 0.66 percentage points from a baseline of 1.25 percent (52 percent), concentrated among those with a prior conviction. Substance-related convictions rise by 0.46 percentage points from a baseline of 0.25 percent (181 percent), especially among prior offenders and workers in mining and manufacturing. Violent and traffic crimes are unaffected. The boom raised employment and earnings, while migration, policing, and income inequality do not explain the results. Among existing residents, local economic booms can reduce crime, but part of the gain is offset by a shift toward substance-related offending. (JEL R11, K42, Q33, O13)

Keywords: criminal behavior, economic opportunities, mining, Sweden

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1 Introduction

Improvements in local economic conditions are generally expected to reduce crime. The evidence broadly supports this, with worse labor market conditions increasing crime (Britto et al., 2022) and better conditions reducing it (Gould et al., 2002; Machin and Meghir, 2004). Most of this evidence is based on aggregate crime rates measured for places, which combine the behavior of existing residents with the arrival of new ones (Street, 2025), or on the timing of small and short-lived cash transfers (Foley, 2011; Carr and Packham, 2019). These designs identify the net effect of a shock on a place and the short-run response to a payment. This paper asks whether a large and lasting improvement in local economic conditions affects criminal behavior and moves different types of crime in opposite directions for the same individuals, and which of those individuals drive each response.

The reason to expect opposite signs is that each category of offense is governed by a different incentive. Property crime is subject to two opposing forces. Better legal earnings raise the opportunity cost of property crime and make it less attractive (Becker, 1968; Ehrlich, 1973), while higher local incomes and more available property make it more rewarding, an effect known as rapacity in the literature on commodity shocks and conflict (Dube and Vargas, 2013). The net response of property crime to an improvement in local economic conditions is therefore an empirical question. Substance-related offenses respond instead through consumption, since drugs and alcohol are goods whose use rises with income (Cotti et al., 2016). Interpersonal violence, unlike the organized violence studied in the conflict literature, is driven more by social conflict and norms than by material incentives, and is less likely to respond (Draca and Machin, 2015). Even the short-run evidence from government transfers shows that income changes shift the composition of crime and not only its level (Foley, 2011; Carr and Packham, 2019). Studies that pool offenses into one index, or examine one crime type in isolation, cannot separate these channels. A fall in property crime alone is consistent with either a strong opportunity cost effect or a weak rapacity effect, and a rise in total crime can hide offsetting movements. Separating these channels therefore requires estimating the response of each offense type for the same individuals.

Using the 2004 iron-ore boom in northern Sweden as quasi-experimental setting, this paper provides individual-level evidence on how a large and persistent improvement in local economic conditions changes criminal behavior, and shows that the same shock moves different crimes in opposite directions. The boom was driven by global commodity prices, and therefore plausibly exogenous to local conditions, and concentrated in two municipalities, Gällivare and Kiruna, where mining accounts for about 20 percent of employment. I link geocoded administrative registers covering all criminal convictions to individual-level information on employment, earnings, and residential location over the period 2000–2015. The outcome is conviction for a given offense type, so I measure the offending of identified individuals rather than crimes reported to the police. I focus on men aged 18–29, the

group with the highest conviction rates, the strongest response to changes in labor market conditions (Grogger, 1998; Gould et al., 2002), and the largest share of the mining workforce. Because I follow the same young men over time and across locations, I can separate changes in their behavior from changes in who lives in the mining towns, and compare property, substance, violent, and traffic offenses within the same population. The empirical design exploits spatial variation in exposure to mining activity and temporal variation induced by the global commodity price shock in a difference-in-differences framework, and the size and richness of the data allow me to characterize the heterogeneity of treatment effects across individuals using causal forests.

The 2004 mining boom is well suited to a quasi-experimental design for three reasons. First, the commodity price shock was generated by global demand, particularly China's rapid industrialization, and speculative dynamics in international markets, making it plausibly exogenous to local crime trends and labor market conditions in Sweden (Radetzki et al., 2008; Farooki and Kaplinsky, 2013; Singleton, 2014). Second, the location of iron ore deposits is determined by geological conditions rather than local economic or social characteristics (Brunnschweiler and Bulte, 2009). Third, the shock was large enough to generate meaningful first-stage labor market effects. Previous empirical evidence shows that mining booms improve the labor market, attract in-migrants to the areas, and increase local purchasing power (e.g., Black et al., 2005; Rodríguez-Puello, 2025). In northern Sweden, prior work documents substantial increases in employment and earnings among residents of the treated municipalities following the boom, driven by the mining sector as well as by spillovers into manufacturing, construction, and services (Rodríguez-Puello and Rickardsson, 2026).

Northern Sweden is an ideal context for this study for several reasons. First, previous evidence on resource booms and crime comes almost entirely from the United States (James and Smith, 2017; Komarek, 2018; Street, 2025) and from developing or middle-income countries (Corvalan and Pazzona, 2019; Axbard et al., 2021).[1] The United States is a global outlier among high-income democracies in crime rates, incarceration, and criminal justice institutions, which limits how far US estimates travel to other developed-country contexts. Second, the developing-country evidence, in turn, is confounded by armed conflict and weak state capacity (Collier and Hoeffler, 2005; Berman et al., 2017), so the economic channel cannot be separated from them. Sweden, as a stable democracy with strong institutions and a universal welfare state, allows me to isolate it. Finally, resource booms can raise local spending on policing and criminal justice, generating enforcement effects that are difficult to distinguish from behavioral responses. This concern is substantially attenuated in the Swedish

[1] Studies exploiting resource-driven booms often find increases in aggregate crime, especially in the context of oil and gas extraction and fracking in the United States (e.g., Raphael and Winter-Ebmer, 2001; Gould et al., 2002; James and Smith, 2017; Dix-Carneiro et al., 2018). There is also a relatively small literature specifically on mining booms and crime, where existing studies yield mixed results and most rely on aggregate outcomes (e.g., Carrington et al., 2011; Corvalan and Pazzona, 2019; Modrego et al., 2026).

context, where law enforcement is centrally and nationally funded and therefore insulated from local commodity cycles; I confirm this in Section 6.1, finding no evidence that the boom altered local police capacity or enforcement effort.

The mining boom produced opposing effects on different types of crime among young male residents aged 18–29. Property crime (e.g., theft, burglary, and fraud) convictions decline by 0.66 percentage points, a 52 percent drop relative to the pre-boom mean, while substance-related (e.g., narcotics possession and use) convictions increase by 0.46 percentage points, a 181 percent increase from a low baseline. Violent and traffic crimes are unaffected. Both effects are concentrated within 20 kilometers of the mines, with no significant changes for older men aged 30 and above. The magnitude of these effects is at the upper end of, but broadly consistent with, quasi-experimental estimates in the resource shocks literature (Appendix Figure B.1). The larger magnitudes partly reflect this focus on the group (young males aged 18–29) that is most active in crime and most responsive to economic incentives.[2] Both effects are driven by pre-boom residents, not by compositional change through migration, though migrants to mining municipalities also show higher substance crime relative to those settling elsewhere. Detection and police capacity do not account for these results, as Swedish policing is nationally funded and centrally allocated, so a local boom cannot expand local enforcement.

The recidivism decomposition and causal forest estimates together show that these opposing responses operate on different individuals. I define prior offenders as young men with at least one conviction in the pre-boom years 2000–2003. Both effects are largest among prior offenders, but they differ at the margin of entry. The decline in property crime is concentrated among prior offenders and among men with weak baseline labor market attachment, the groups for whom improved economic opportunities represent the largest change in the returns to legal activity; among non-prior offenders, the effect is close to zero. Substance-related crime, in contrast, rises among both groups: the increase is larger among prior offenders, but it is also positive among men without a prior conviction, and it is concentrated among men employed in mining and manufacturing and those with low education. This pattern points to an income channel: higher disposable income increases engagement in risky behaviors primarily by intensifying use among individuals already involved, while also drawing in some new offenders.

To assess the welfare implications of these opposing crime responses, I translate the estimated effects into implied social costs by combining my estimates with existing estimates of the costs of crime (Heeks et al., 2018; Alsan et al., 2025). Reductions in property crime

[2]While the percentage changes are large (a 52% decline in property crime and a 181% increase in substance crime), these reflect a combination of a low baseline mean in outcomes, a substantial income shock (approximately 25% higher earnings), and the high responsiveness of young males at the margin of crime. The implied earnings elasticity for property crime is approximately -2, in line with existing estimates (Gould et al., 2002; Machin and Meghir, 2004), suggesting the magnitude is consistent with prior findings once the size of the shock is accounted for.

generate sizable local welfare gains, amounting to SEK 17.8 million during the 2004–2015 boom period (12 years), and reflecting lower victimization costs and reduced criminal justice expenditures. These gains are partially offset by social losses of roughly SEK 14.1 million from increased substance-related crime, which impose social costs through enforcement, health, and broader social harms.

Mechanism analysis reveals important insights. The mining boom raised employment and labor income among young male residents by about 25 percent, in line with evidence for the same region in Rodríguez-Puello and Rickardsson (2026). In the economic model of crime, higher legal earnings raise the opportunity cost of property crime (legal wages now exceed criminal returns), and shift the cost-benefit calculation away from criminal activity (Becker, 1968). The decline in property crime, concentrated among men with weak pre-boom labor market attachment, indicates that this opportunity cost effect outweighs the rapacity effect of greater local wealth. For men employed in mining and manufacturing, by contrast, higher income relaxes the budget constraint for substance consumption, with the criminal margin concentrated in narcotics. This pattern is not specific to mining: workers in dangerous, physically demanding occupations show elevated substance use when income rises (Frone, 2006; Bush and Lipari, 2015).

A natural alternative explanation for the rise in substance crime among mining and manufacturing workers is occupational stress rather than income: if the boom made mining work more demanding, workers could turn to drugs and alcohol to cope. Administrative records capture what individuals do, but not the working conditions surrounding the boom. To test this channel, I assemble an original text dataset of Swedish newspaper articles mentioning the mining company LKAB from the press archive *Retriever Mediearkivet* and classify each article by topic. An interrupted time series around January 2004 no change in the share of coverage devoted to stress and mental health, and none in coverage of workplace accidents or labor relations. This pattern suggests that occupational stress is unlikely to be the primary driver of the rise in substance crime, although it cannot be fully excluded. Migration, income inequality, and police capacity also do not account for the results; I discuss each alternative mechanism in Section 6.1.

The results hold across a wide set of robustness checks. They are similar when distance rings are selected by a data-driven procedure, when exposure is measured by travel time by car rather than straight-line distance, and alternative definitions of treatment and control groups, such as when the control group is replaced by municipalities with positive synthetic control weights or by Swedish municipalities of similar population size. Moreover, a municipality-level synthetic control, which does not rely on parallel trends, reproduces the fall in property crime and the rise in substance crime. The effects are absent for men aged 30 and above, stable for upper age cutoffs between 24 and 35, and unchanged when the sample is restricted to birth cohorts observed both before and after the boom, when time-varying controls are

added, and when migrants are included.

Related literature. The closest paper to this one is Street (2025), to the best of my knowledge, the only individual-level study of crime and a resource boom to date. Studying the fracking boom in the US, the author finds that crime rose at the county level while the probability of a case being filed against households already living in the area fell, so the aggregate increase reflects the arrival of new residents rather than more offending among incumbents. That outcome pools all offense types into a single indicator of whether a case was filed, and therefore measures the net response of incumbent residents rather than its composition. This paper makes three contributions to the literature on crime and local economic shocks: it shows that a single positive shock moves different crimes in opposite directions and identifies who responds, it separates behavioral responses from migration by following the same individuals, and it provides causal evidence on mining booms and crime from a European country. The existing causal evidence on resource booms and crime relies almost entirely on aggregate crime outcomes (e.g., James and Smith, 2017; Corvalan and Pazzona, 2019), which are informative for assessing the net policy impact of a boom but cannot identify who responds, or through which mechanism.

First, this paper provides new evidence on the mechanisms behind, and heterogeneity in, crime responses to positive economic shocks. Studies using aggregate outcomes find that resource booms tend to increase overall crime (e.g., James and Smith, 2017; Komarek, 2018; Modrego et al., 2026), but aggregate crime indices mask offsetting responses across crime types (Cunningham et al., 2020). I show that the same shock simultaneously reduces economically motivated property crime and increases consumption-driven substance crime, reconciling the mixed findings in the resource-shocks literature and providing micro-level causal evidence consistent with the theoretical predictions of Becker (1968) and Ehrlich (1973). The implied earnings elasticity for property crime of approximately -2 is at the upper end of existing estimates (Gould et al., 2002; Machin and Meghir, 2004), suggesting the opportunity-cost channel is particularly strong in this setting. The substance-crime increase is consistent with an income channel established in the broader labor-and-health literature on workers in physically demanding, well-paid occupations (Frone, 2006; Bush and Lipari, 2015). It also connects the resource-shock literature to evidence that positive income shocks raise the consumption of goods that are complementary to crime, such as drugs and alcohol, documented for cash and in-kind transfers (Dobkin and Puller, 2007; Cotti et al., 2016; Carr and Packham, 2019). Relative to that literature, which identifies effects from the timing of relatively small and transitory transfers, such as the monthly payment dates of food stamps or disability benefits, I identify the response to a large and durable increase in local earnings, which allows the income channel to operate on the same population and over a longer horizon. Using causal forests (Athey and Imbens, 2016; Wager and Athey, 2018; Athey et al., 2019), an approach still rare in the literature on crime and economic shocks, I further show which individuals drive each response, rather than relying on average effects alone.

Second, I contribute to a growing literature that separates behavioral responses from changes in population composition when estimating the effects of local economic shocks. Individual-level designs have been used in the resource-boom literature mainly for labor market outcomes (e.g., Jacobsen et al., 2023; Kovalenko, 2023), with Street (2025) the exception for crime.[3] I complement this literature using a geocoded individual panel that tracks the same residents over time, cleanly separating incumbents' behavioral change from endogenous in-migration. Because the panel follows individuals across both time and residential location, it separates behavioral responses from the compositional shifts that influence place-level crime estimates, while also supporting granular analysis across crime types, economic sectors, and demographic groups. It also allows me to estimate the response of migrants as a group of interest in itself rather than as a source of bias.

Finally, to the best of my knowledge, this is the first study to examine the causal effects of a mining boom on crime in a European country. As discussed above, the existing causal evidence is concentrated in the United States and in developing or middle-income countries, and evidence from developed-country settings is particularly scarce (Komarek, 2018), where the institutional environment allows for a cleaner test of the economic channel.

Roadmap. The remainder of the paper is structured as follows. Section 2 describes the Swedish mining sector and the 2004 mining boom. Section 3 presents the data and sample. Section 4 presents the empirical framework and identification assumptions. Section 5 reports the main results, the heterogeneity analysis using causal forests, and the migrant analysis. Section 6 translates the estimates into welfare effects and discusses the mechanisms behind the results. Section 7 concludes.

2 Background and institutional setting

This section provides background on the mining boom, the mining sector in Sweden, and its local economic effects, including a summary of previous evidence on the labor market effects of mining booms in Sweden.

In the last two decades, resource-dependent countries and mining communities have experienced the economic and socioeconomic impacts of resource shocks in the form of price booms. These are characterized by large and persistent increases in international prices of minerals (Fleming and Measham, 2015; Álvarez et al., 2021). I analyze the global mining boom that started during the first years after the new millennium (2004), when international mining prices suddenly tripled (Baffes and Haniotis, 2010). I date the boom to 2004, the year iron-ore prices began their sustained rise (a 67% increase from 2004 to 2005) and mining employment turned from a decade of decline to sustained growth (Knobblock and Pettersson,

[3] A growing literature emphasizes the importance of separating individual behavioral responses from compositional changes when evaluating local economic shocks (Guettabi and James, 2020; Kovalenko, 2023; Jacobsen et al., 2023).

2010; SGU, 2014; Tano et al., 2016). This trend was accompanied by an increase in investment in the Swedish mining sector. The shock is well suited to a quasi-experimental design: it was large, time-limited, and generated outside Sweden, by China's rising commodity demand and by speculative investor flows into mineral markets rather than by shifts in Swedish supply (Radetzki et al., 2008; Farooki and Kaplinsky, 2013; Erten and Ocampo, 2013; Singleton, 2014). Because the demand shock is external to the Swedish mining industry, it lets me identify the causal effect of a local labor-market shock on criminal behavior.

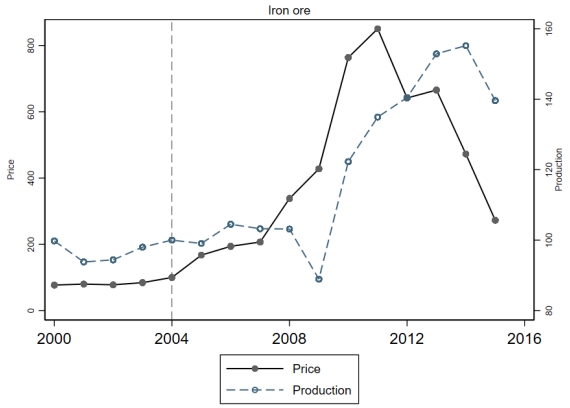
This boom is especially relevant for Sweden because the country has a long tradition of mining. During the mining boom, the main minerals and metals exploited in the country were iron ore, copper, zinc, and gold (Tano et al., 2016). I focus on iron ore because it is the most important mineral in the Swedish mining economy, in which the country is dominant at the European level, producing approximately 90% of the total iron ore production in the European Union (SGU, 2016). Figure 1, panel (a), shows the international prices and Swedish production of iron ore for the period 2000-2015. As can be seen, prices began to increase in 2004, reaching the maximum level in 2011. The price of iron ore started to increase from 2004 to 2005 and continued to grow rapidly in the following years (Tano et al., 2016). At the same time, observing the rise in prices, mining companies employed strategies to increase their production before a probable fall in prices. In addition, the start of the mining boom coincides with a dramatic increase in exploration activities and production in Northern Sweden due to high local and international investment in the sector and increasing demand for minerals and metals (Knobblock and Pettersson, 2010; SGU, 2014).

In addition to changes in prices and production, the mining boom was highly salient in the public debate. Using information from the newspaper archive *Retriever Mediearkivet*, I construct the annual number of articles in Swedish newspapers mentioning LKAB (Luossavaara–Kiirunavaara Aktiebolag), the state-owned iron ore company operating the major mines in Kiruna and Gällivare (Figure 1, panel (b)). Media coverage closely tracks the evolution of international iron ore prices, increasing sharply after 2004 and peaking during the height of the boom.[4] This pattern suggests that the mining boom was widely perceived and discussed at the national level, reinforcing the view that the shock was not only economically significant but also highly visible to local communities. A decomposition of the media corpus into seven thematic categories shows that the increase in coverage was concentrated in stories about production and economic output and toward employment and hiring (Appendix F.3).

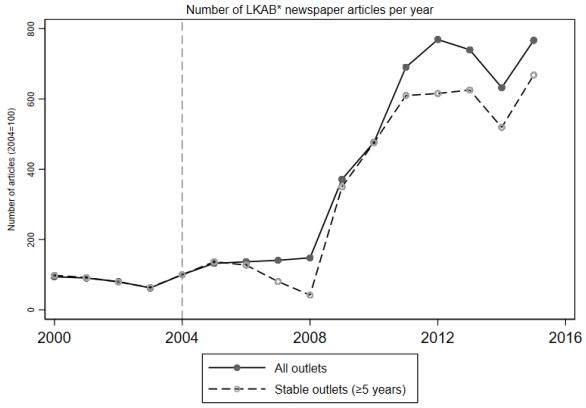
[4]A potential concern is that the rise in LKAB article counts after 2004 partly reflects the expansion of Retriever's coverage, where more outlets were indexed each year, mechanically inflating article totals. The fact that the number of articles was on a declining trajectory in the years before the boom speaks against this interpretation. However, I acknowledge that the rapid surge after 2008 is driven by both an increase in the number of articles per outlet and an increase in the number of distinct outlets covering LKAB. Finally, the dashed line in the Figure shows that restricting the sample to outlets present in at least five years of the panel leaves the upward trend essentially unchanged.

Figure 1: The 2004 iron ore mining boom in Sweden, 2000–2015

(a) Price and production of iron ore



(b) Media coverage of LKAB in newspapers



Notes: Both series are normalized to 2004 values (2004=100). The vertical dashed line marks the start of the mining boom (2004). *Panel (a):* The figure plots the international iron ore price (solid line, left axis) and Swedish iron ore production volumes (dashed line, right axis) over 2000–2015. The price series is from the International Monetary Fund; the production series is from the Geological Survey of Sweden (SGU, 2021). *Panel (b):* The solid line reports the annual number of newspaper articles mentioning "LKAB" normalized to 2004 values (2004=100). The dashed line restricts to outlets present in at least five distinct years of the panel. Articles are identified using the newspaper archive *Retriever Mediearkivet*. Following the literature, I search for articles containing the case-insensitive string "LKAB*", where the asterisk is used as a wildcard. The counts reflect exported and parsed articles. The vertical dashed line marks the start of the mining boom in 2004. LKAB is the main iron ore producer in Sweden and operates the large-scale mines in Kiruna and Gällivare. LKAB is Sweden's state-owned iron ore mining company.

In Sweden, mining activity is spatially concentrated in northern municipalities, with a few exceptions in the South of the country. The North of the country is part of the *Fennoscandinavian Shield*, a region considered rich in minerals (Nordregio, 2009; Haley et al., 2011). Most mines, mining jobs, and exploration are concentrated in the two northernmost counties: Norrbotten and Västerbotten (SGU, 2014), representing 93% of total mining employment in Sweden in 2013 (Moritz et al., 2017). There are mainly three large iron ore mines that were continuously operating during the mining boom period: the Malmberget mine located in Gällivare municipality and the Kirunavaara and Gruvberget mines in Kiruna municipality.[5] These are all existing mines, with Kirunavaara opening in the 1860s,

[5]There are other municipalities with mines during the boom period not included in the analysis. Other than Gällivare and Kiruna, these are Lycksele, Malå, Norsjö, Skellefteå, Sorsele, and Storuman in Västerbotten County, Askersund in Örebro County, and Hedemora in Dalarna County. The key distinction is that these municipalities extract polymetallic ores (copper, zinc, gold, and silver), whose price paths are governed by distinct demand shocks relative to the iron ore cycle that drove the 2004 boom. They also operate smaller mines with lower local employment shares, and are located in different demographic and labor market contexts from Norrbotten County. Appendix Table A.1 shows some basic information about the mines, municipalities, and their employment share in the mining sector. Tano et al. (2016) and SGU

and Malmberget in the 1820s. I focus on existing mines rather than mine openings or closures, since these were rare during this period and do not provide sufficient variation for empirical analysis.[6] Moreover, these mines are central to the labor market dynamics of these municipalities, employing a substantial share of the workforce.

Due to the lack of an official classification for mining and non-mining municipalities in Sweden, I identify treated municipalities as those highly specialized in mining, with a high mining employment share in 2003, focused on the exploitation of iron ore: Gällivare and Kiruna. Both municipalities had at least one operating iron ore mine during the boom period, with the mining sector representing more than 10% of total employment. Choosing treated units based on their employment share in the industry is a common approach in the empirical literature on resource booms (Black et al., 2005; Kumar, 2017; Jacobsen et al., 2023). To ensure that treated and control individuals are not only similar but also geographically close, I define the control group as individuals located in other municipalities within Norrbotten County.[7]

Gällivare and Kiruna are small municipalities with approximately 18,000 and 23,000 residents in 2015, respectively, characterized by very low population density and compact urban centers surrounded by sparsely populated land. In both municipalities, large-scale iron ore mines are located in close proximity to the urban centers, and local commuting patterns, employment opportunities, and economic activity are tightly linked to mining operations. Although neither municipality is a classic company town, mining represents the dominant economic sector in both Gällivare and Kiruna, directly and indirectly employing a substantial share of the local workforce. Both municipalities had experienced decades of population decline and disinvestment before the boom (Adjei et al., 2023), making the labor market shock particularly large relative to the local economic baseline. Moreover, prior to the mid-2000s mining boom, crime rates in both Gällivare and Kiruna were relatively stable and comparable to those in similar northern municipalities, with no pronounced differential trends relative to the control areas used in the analysis. Although the institutional environment of Swedish mining is specific, Gällivare and Kiruna share features with mining towns worldwide, such as copper-mining municipalities in northern Chile (Corvalan and Pazzona, 2019; Rodríguez-Puello, 2025), including geographic concentration of extraction activity, reliance on mainly one economic sector, and high sensitivity to global commodity price fluctuations, suggesting

(2021) provide a more detailed description of the mines opening and closing in Sweden, the public and private companies operating them, and the locations of the mines.

[6]This distinguishes the present study from much of the US resource-boom literature, which typically examines new extraction sites such as hydraulic fracturing counties where drilling began after 2005 (e.g., James and Smith, 2017; Komarek, 2018; Street, 2025). In those settings, concerns arise about whether site selection is endogenous to local economic conditions. Here, the location of mining is fixed by geology that has been exploited for over a century.

[7]Norrbotten County has 14 municipalities: Arjeplog, Arvidsjaur, Boden, Gällivare, Haparanda, Jokkmokk, Kalix, Kiruna, Luleå, Pajala, Piteå, Älvsbyn, Överkalix, and Övertorneå.

that comparative analyses across mining regions could help assess the external validity of the findings.

A growing body of evidence documents the labor market effects of mining booms in Sweden. Tano et al. (2016) and Moritz et al. (2017) find positive effects on employment, labor income, and population growth in Swedish mining municipalities. More recently, Rodríguez-Puello and Rickardsson (2026) use individual-level data to show that the mining boom significantly increased employment and earnings for residents in mining areas, with particularly strong effects in the mining sector. Moreover, the effects spread across space, sectors, and demographic characteristics. For example, the authors find that the mining boom affects the labor market conditions of individuals located as far as 27 km during the boom and 83 km in later years. Residents living near mines experienced around 5% higher annual earnings, equivalent to roughly 8,400 SEK in 2004. These findings provide the basis for examining whether improved labor market conditions constitute a channel through which the mining boom affects criminal behavior.

3 Data and sample

To examine the role of the mining boom on criminal behavior in Sweden, I combine several administrative registers linked through anonymized personal identifiers. Individual information on demographic and labor market characteristics comes from the Longitudinal Integrated Database for Health Insurance and Labour Market Studies (LISA), combined with geocoded residential location, both maintained by Statistics Sweden. The data are of yearly frequency, and the outcomes are measured in November each year. The dataset is rich and contains information on all individuals above the age of 16, including age, gender, education, region of origin, income, and household characteristics. The data also include information on employment, occupation, economic sector, and region of residence and work, and I focus on the period 2000–2015. The analysis ends in 2015 for two reasons. First, extending the sample well beyond the boom period risks introducing noise, as the effects of the shock are expected to dissipate, and other local economic shocks may confound the estimates. Second, Europe and Sweden experienced a migration crisis from around 2015, with refugees disproportionately placed in peripheral and rural areas (Wennström and Oner, 2019; Puschmann et al., 2019); extending the sample would risk confounding the effects of the mining boom with those of this demographic shock. These data have been linked to the Swedish Conviction Register (*Lagföringsregistret*), maintained by the National Council for Crime Prevention (Brottsförebyggande rådet, Brå), which records every individual found guilty of a criminal offense, together with the type and date of each offense. A single conviction may encompass multiple crimes, and I observe all crimes within a given conviction. The register excludes minor offenses such as speeding tickets, but includes offenses such as driving

without a license and driving under the influence (DUI).[8]

I restrict the sample to young males, defined as men aged 18-29 who appear in five or more consecutive annual observations.[9] Young males are an important population group to analyze the effect of economic opportunity on individuals' criminal behavior for several reasons. First, it is well established in previous empirical literature on crime that conviction rates are substantially higher among males than females, and they peak in early adulthood before declining steadily with age (e.g., Elonheimo et al., 2014; Epper et al., 2022). Importantly, young males are not only the group with the highest baseline crime rates, participating in a disproportionate amount of violent and property crimes (Komarek, 2018), but also the most responsive to changes in criminal incentives (Anker et al., 2021). Second, the mining sector worldwide and in Sweden is composed primarily of young and male workers (e.g., Kearney and Wilson, 2018; Chávez and Rodríguez-Puello, 2022). Finally, Rodríguez-Puello and Rickardsson (2026) finds evidence that the benefits from the mining boom in Sweden, through higher earnings and more employment opportunities, are large for males and young individuals located close to the mines.[10]

As described in Section 2, I define individuals located in Gällivare and Kiruna as treated and those in other Norrbotten municipalities as controls. Finally, I exclude individuals who moved to Norrbotten County in 2004 or later in the main specification, whom I call migrants. Therefore, the main analysis focuses on residents. I assume that those who migrated to

[8]Because the outcome is a conviction, I observe offenses for which an identified individual was found guilty, not all crimes reported to the police; crimes that are never cleared, which is common for thefts and burglaries, do not appear in the data. Convictions are therefore the relevant measure for studying how individuals respond to changing labor market conditions (as in Britto et al., 2022), but they would be misleading if the boom changed the probability that a given offense leads to a conviction. I find no evidence that the boom altered local police capacity or enforcement effort (Section 6.1). For narcotics offenses, I also use the Crime Suspicion Register (*Misstankeregistret*), maintained by Brå, which records individuals suspected of an offense regardless of whether the case leads to prosecution. The results are consistent with the conviction data (Section 5).

[9]Because the age window applies in every year, the sample is a rotating panel in which individuals enter at 18 and leave after 29 (see Section 5.5).

[10]Appendix Figure B.2 shows conviction rates for any crime by single year of age and gender, before and after the boom, as a descriptive pattern for the Swedish data. In both the pre-boom and boom periods, young males stand out as the group with the highest conviction rates, several times greater than those of women or older cohorts. Therefore, young males account for a disproportionate share of criminal activity, and most of the variation in convictions is concentrated in this group. For this reason, I do not estimate the effect for the full population. The difference-in-differences coefficient for a pooled sample is a weighted average of the group-specific effects, and adding large groups with low baseline conviction rates and no response to the boom would mostly dilute the estimate toward zero rather than reveal a different behavioral response. Appendix Figure C.1 shows the DID estimates for all males in age groups between 18 and 65 (in four brackets: 18–29, 30–39, 40–49, and 50–65), confirming that effects are concentrated exclusively in the main analytic sample, and none of the estimates for older men is statistically significant. I also examine males aged 30–39 as a falsification exercise, as shown in Appendix C. There are no statistically significant effects of the mining boom on criminal behavior for this older group across any crime type.

this area after the shock did so in response to improved labor market conditions. This is important because the results may be a combination of the effects of the mining boom on crime and endogenous movement decisions made by individuals who migrated to the mining areas (Winters et al., 2021). Nevertheless, for robustness, I also present the results with all individuals. Appendix Figure B.3 shows the spatial location of the treated and control municipalities.

Individuals residing closer to the mines may be more affected by the mining boom than those further away, even within the treated municipalities. To examine this spatial heterogeneity, I construct a measure of the distance in kilometers from each individual's residential location to the nearest mine, based on the coordinates of the grid where he or she is located. I consider the three large iron ore mines that were continuously operating during the mining boom period, as mentioned in Section 2. The grids in the data are 250 by 250 meters in size in urban areas and 1000 by 1000 meters in size in rural areas. Individuals are located in these grids according to their place of residence. I classify individuals into 20-kilometer rings based on their proximity to the nearest mine, obtaining five rings in total, where those located farther away serve as controls. This approach, commonly known as the "ring method" (e.g., Wilson, 2012; Benshaul-Tolonen et al., 2019; Rodríguez-Puello and Rickardsson, 2026), exploits within-municipality variation in exposure and allows me to test whether effects decay with distance from the mines, a pattern expected if the mining boom is the true driver of the results.[11]

In the main analysis, the main outcomes of interest that reflect the criminal behavior of individuals are: (1) being convicted of any crime, (2) property crime, (3) violent crime, (4) traffic crimes, and (5) substance-related crimes, per year.[12] Property crimes include theft, robbery, and other assaults, fraud and other misconduct, embezzlement and other faithlessness, offenses against creditors, and crimes of damage. Violent crimes include violations of life and health, violations of freedom and peace, defamation, sexual offenses, and crimes against family. Traffic crimes include a broad range of road- and maritime-traffic offenses such as reckless or negligent driving, unlawful driving, driving under the influence, hit-and-run, speeding, safety violations, and other traffic- and navigation-related offenses.[13] Substance-related crimes include convictions under Swedish legislation governing narcotics, doping substances, alcohol, tobacco, and nicotine products, covering offenses related to possession, use, production, and distribution of these substances.[14] Appendix Table A.2

[11] Appendix Figure B.4 shows the distribution of individuals in space according to their location and distance to the nearest mine and their distribution in the rings.

[12] Other types of crimes, such as crimes against the public, crimes against the state, and other special categories (e.g., smuggling, tax crimes, terrorist crimes), are excluded due to their low frequency.

[13] Traffic crime is identified using convictions under the Traffic and Maritime Offences Acts. The data do not allow for a consistent separation between driving under the influence and other traffic offenses within this category.

[14] The administrative conviction records do not allow for a consistent separation between possession, use,

presents summary statistics for treated and controls before (2000-2003) and after (2004-2015) the mining boom. In the pre-boom period the two groups are similar in age, labor income, disposable income, marital status, and children, and pre-boom conviction rates are close across all four crime types. The treated group is more concentrated in the primary sector, which is the definition of treatment rather than an imbalance, since treated municipalities are those specialized in mining, and somewhat more likely to hold only primary education. The main sample consists of 36,718 individuals (230,480 individual-year observations).

To complement the administrative registers and assess mechanisms that conviction records cannot directly capture, I construct an original corpus of Swedish newspaper articles covering LKAB, the state-owned iron ore producer operating the mines in Kiruna and Gällivare. Articles are identified from *Retriever Mediearkivet*, Sweden's largest newspaper archive, using a case-insensitive wildcard search for the string "LKAB*" over the period 2000–2015. The corpus is aggregated at monthly frequency and covers the full sample period, allowing analysis of how media coverage of the company evolved before and after the onset of the mining boom in 2004.[15] Article volume grows substantially following 2004, reflecting a growth in press attention to LKAB's operations during the boom years.[16] Full details on corpus construction, keyword dictionaries, and robustness checks are in Appendix F.

4 Empirical framework

Estimating the effect of economic opportunity from mining on criminal behavior is challenging because mine location and activity are not simply exogenous. While the geological location of deposits is fixed by nature and determined by local geology and therefore considered exogenous (Brunnschweiler and Bulte, 2008; Van der Ploeg and Poelhekke, 2010; Brunnschweiler and Poelhekke, 2021), the opening and scaling of mines depends on firm and government investment, and resource settlements differ systematically from other communities in ways correlated with both labor markets and crime (Cust and Harding, 2020; Asher and Novosad, 2023). A simple regression of crime on mining activity would therefore be biased by omitted variables and/or reverse causality. I address this by exploiting the price shock, which is external to Sweden, interacted with the individuals' exposure to mining activity determined by the geological location of deposits, as detailed below.

and distribution within narcotics offenses. As a result, substance-related crime is analyzed as a composite outcome.

[15] As robustness checks, outlets appearing fewer than five times in the corpus are excluded, to address concerns about noise introduced by rarely represented sources.

[16] A potential concern is that coverage of Kiruna's city relocation (*stadsomvandlingen*), announced in 2003 as a direct consequence of the mine's underground expansion, inflates the post-2004 article count. To assess this, I searched Retriever Mediearkivet for articles containing *stadsomvandling* or *stadsflytt* together with *Kiruna*. City-relocation articles account for less than 2% of the corpus on average across 2006–2015 and never exceed 5% in any single year, confirming that the post-2004 growth in coverage reflects mining in general and the mining boom rather than the relocation narrative.

I exploit the unexpected rise in iron ore prices that generated the global mining boom in 2004, coupled with variation in individuals' exposure to mining activity, driven by their geographical residential location. These provide a plausibly exogenous shock to local economic conditions, based on the assumption that the location of mines is exogenous because it depends on the local geology (Pelzl and Poelhekke, 2021; Christian and Barrett, 2024). Using a difference-in-differences (DID) approach that exploits both temporal and spatial variation in exposure, I compare the criminal behavior of treated individuals to residents in other municipalities in the county, before and after the mining boom, to identify the average treatment effect (ATE) on the treated in mining municipalities. Formally, I estimate the effects of local economic shocks on local residents' criminal behavior using the following linear probability model:[17]

$$Y_{ijmt} = \alpha_i + \alpha_j + \alpha_t + \beta(Post_t \times Treated_{imt}) + \epsilon_{ijmt} \tag{1}$$

where Y_{ijmt} is equal to 1 if individual i located in grid j and in municipality m in year t is convicted of a crime (property, violent, traffic, or substance crime). $Treated_{imt}$ is a binary variable that takes the value of 1 if individuals are located in the mining municipalities and 0 if individuals reside in other municipalities in the county (control).[18] $Post_t$ is a binary indicator equal to 0 before the mining boom (2000-2003) and 1 after (2004-2015). The coefficient of interest is the β, which identifies the difference-in-differences estimate (ATE) of the effects of the mining boom on the outcome Y_{ijmt}. I include α_i, α_j, and α_t, which are individual, grid, and time fixed effects, respectively, to account for omitted variables and isolate the effect of the event. Individual fixed effects account for any static differences in the propensity to commit a crime across individuals. Year fixed effects control for factors that affect the criminal behavior of all individuals in a given year, such as the Great Recession. Grid fixed effects account for any static differences in the propensity to commit a crime across geographical locations.[19] In

[17] While logit and probit models are also used for binary outcomes, they add their own assumptions, often don't have closed-form solutions, and their interpretation is more complex, especially with large amounts of fixed effects (Huntington-Klein, 2021).

[18] Treatment status is not fixed in a single year. $Treated_{imt}$ is assigned from the municipality of residence in year t, so an individual who moves between a mining municipality and a control municipality within Norrbotten changes status during the panel. Individuals who moved into the county after the boom are excluded from the main sample and are analyzed separately in Section 5.4. Because moving is itself a choice that may respond to the boom, I also estimate the model with treatment fixed at the 2003 municipality of residence, which holds status constant over the whole period. The estimates are nearly unchanged.

[19] In the robustness checks, I include time-varying individual-level controls, such as being married, having children under 18, education categories (primary, secondary, and tertiary), and economic sector which distinguishes between non-employed, primary (extraction and agriculture), secondary (manufacturing and construction), and tertiary (services, healthcare, public sector, and other). I do not include the control variables in the main specification because some of the controls could be endogenous to the mining boom (Allcott and Keniston, 2018; Pérez-Trujillo and Rodríguez-Puello, 2022; Rodríguez-Puello, 2025), becoming bad controls. The main results are robust.

all estimations, I cluster standard errors at the grid level, allowing for an arbitrary covariance structure over time within each grid, and accounting for the serial correlation in the error term (Bertrand et al., 2004; Miller, 2023). In addition to the municipality-level treatment, I exploit fine-grained spatial variation in distance to the mines to explore spatial heterogeneity in the ATE and examine whether the effects decay with proximity, as expected if the mining boom is truly driving the results.[20]

4.1 Identifying assumptions

The assumptions behind the DID approach are that, in the absence of the boom, residents' criminal behavior in mining and control municipalities would have evolved similarly (parallel trends) (Meyer, 1995), that behavior did not respond before 2004 (no anticipation), and that treatment of one individual does not affect another's outcome ("stable unit treatment value assumption", SUTVA). There are five threats on these assumptions in the form of non paralel trends, contemporaneous shocks specific to the treated municipalities, time-varying unobservables, spatial spillovers to the control group, and endogenous migration. I check these assumptions and threats in several ways.

First, regarding the parallel trends assumption, a violation of this assumption would imply that the observed effects might be a result of preexisting trends instead of the boom. I assess the validity of the "parallel trends" assumption via dynamic DID estimations.[21]

A second threat is that something other than the boom changed in Gällivare and Kiruna after 2004. Because the control group consists of other municipalities in the same county, shocks common to Norrbotten or to Sweden, including national crime policy, the business

[20] I classify individuals into 20-kilometer rings based on their proximity to the nearest mine, obtaining five rings in total, where those located farther away serve as controls. I obtain the following distance rings (in kilometers) from the nearest mine: $d \in \{(0, 20],\ (20, 40], (40, 60], (60, 80], (80, 236]\}$.

[21] To empirically assess the validity of the "parallel trends" assumption, I estimate the following dynamic DID equation:

$$Y_{ijmt} = \alpha_i + \alpha_j + \alpha_t + \sum_{t}^{T} \beta_t \times I_t \times Treated_{imt} + \epsilon_{ijmt} \tag{2}$$

where Y_{ijmt} is equal to 1 if individual i located in grid j and in municipality m in year t is convicted of a crime. $Treated_{ijmt}$ is a binary variable that takes the value of 1 if individuals are located in mining municipalities and 0 if individuals reside in other municipalities in the county (control). The I_t's represent each year, accounting for the dynamic nature of the approach. The coefficients of interest are the β_ts, which identify the per-period difference-in-differences estimate of the effects of mining on the outcome Y_{ijmt}. I normalize β_{2003} to zero; thus, all the coefficients are interpreted as changes relative to that year. In this dynamic DID approach, the first difference is between the reference period $t = 2003$ and the period $t + x$, while the second difference is between the treated and control individuals. The β_ts for $t > 2003$ capture the dynamic effects of the treatment. On the other hand, the β_ts for $t \leq 2003$ provide a placebo or falsification test for the parallel trend assumption. In this specification, I include the same fixed effects to account for omitted variables and isolate the effect of the event.

cycle, and the reorganization of policing, are differenced out. Therefore, it is highly unlikely that there is another shock specifically in 2004 that affects treated and control individuals differently. The sample ends in 2015 to avoid the migration crisis. The municipality-level synthetic control in Section 5.5 provides a further check, since it does not rely on parallel trends.

Third, I assume that there are no time-varying omitted variables at the treatment level correlated with the boom and the outcomes. Specifically, I assume that individuals in treated and control locations are similar in the time-varying evolution of observed and unobserved characteristics (Von der Goltz and Barnwal, 2019). The fact that there is little to no change in the results when including the control variables is suggestive in support for this assumption. Moreover, although the DID design only requires that treatment and control groups exhibit the same trends (not necessarily the same levels) in the absence of treatment, one could worry that the control group does not provide an adequate counterfactual in light of the level gap. Appendix Table A.2 describes the two groups in the pre-boom years. They are similar in age, income, and conviction rates, and differ mainly in sectoral composition, which follows from how treatment is defined and is absorbed by the individual fixed effects. Since the design requires common trends rather than common levels, the relevant evidence is Appendix Table A.3, which reports the change in each characteristic between 2000 and 2003 for treated against control individuals, and the mean difference test. For all four crime outcomes the differential change is small and statistically indistinguishable across groups, as it is for marital status, children, and non-employment. The two groups do differ in educational attainment and in the composition of employment. Specifically, young men in the treated municipalities move out of primary education and into the primary sector faster than their counterparts elsewhere. Since non-employment follows a common trend, this reflects where those who work are employed rather than how many work, and follows from the industrial structure of mining municipalities. Individual fixed effects absorb these differences in composition, and Section 5.5 shows that the estimates are unchanged when time-varying controls are added.

Fourth, if there are spillovers to neighboring control municipalities, the SUTVA assumption would be violated, and the parameters of interest in the main model would be biased toward zero. In other words, I assume that there is no interference between units, and the individuals in the control municipalities are not affected by the treatment via spatial spillover effects (Sinclair et al., 2012). As a robustness check, I remove residents located in the four neighboring municipalities, which are most prone to spillovers, and the main estimates are robust.[22] However, removing the four neighboring municipalities also removes the most comparable controls, so reducing spillover bias comes at the cost of comparability. Moreover, I do a distance-ring specification, which measures the gradient directly rather than imposing a treatment cutoff. The results show that the effects are concentrated within 20 kilometers

[22]The four neighboring municipalities are Jokkmokk, Pajala, Överkalix, and Boden.

of the mines and fade beyond, which bounds how far interference can extend.

Finally, an additional concern is endogenous self-selection and migration, where individuals may have chosen to migrate to the mining area, anticipating that the move would improve their living conditions, so that a change in the composition of residents would be mistaken for a change in behavior. To address this concern, the main specification excludes individuals who moved to the treated or control locations after 2004 (migrants) (Benshaul-Tolonen et al., 2019; Jacobsen et al., 2023), so the estimates follow the same individuals through the boom. Migrants are analyzed separately in Section 5.4 rather than pooled with residents.

5 Results: Mining boom and crime

5.1 Main results

I begin by estimating the overall effect of local economic shocks on the different types of crimes committed by residents. Table 1 reports the DID coefficients from equation (1) for 18-29-year-old male residents. All estimations include individual, year, and grid fixed effects, do not include control variables, and are the preferred specification since some controls could be endogenous to the mining boom and criminal behavior (Allcott and Keniston, 2018). All robustness tests, including controls and different fixed effects, among others, are in subsection 5.5.

Column (1) shows the results for being convicted of any crime, column (2) property crime, column (3) violent crime, column (4) traffic crimes, and column (5) substance-related crimes. Results for the combined "any crime" outcome show a statistically insignificant effect due to the mining boom, as the reduction in property crime and the increase in substance-related crime largely offset each other. This reinforces the importance of disaggregating by crime type, as aggregate measures obscure the opposing behavioral responses documented in this paper. Given this, the remainder of the analysis focuses on disaggregated crime types.

The results suggest a negative and significant reduction in the probability of being convicted of property crime after the mining boom for treated young residents. I observe a decline of 0.66 percentage points in the probability of being convicted of property crime among treated individuals relative to their non-treated counterparts. From a baseline sample mean of 0.012, this estimate translates to a 52% drop in individuals convicted and is statistically significant at the 5% level.[23] These declines are not confined to a single offense type: disaggregating by Criminal Code chapter shows broadly similar reductions across theft and appropriation offenses, breach-of-trust crimes such as embezzlement and misappropriation,

[23]This corresponds to approximately 16 fewer young males convicted of property crime per year in the mining municipalities. At the lower bound of the 95% confidence interval, property crime falls by at least 10% relative to the pre-boom mean.

and damage-related offenses including vandalism, with statistically significant declines in the latter two categories (Appendix Table A.4).[24] These findings are in line with recent micro-level evidence (e.g., Axbard et al., 2021; Street, 2025). For example, Axbard et al. (2021) find that increased mineral wealth in South Africa leads to less crime due to changes in employment opportunities, while Street (2025) documents a 14–17.5% drop in cases filed among residents in US fracking counties.

In line with expectations, and with evidence from commodity price shocks elsewhere (Corvalan and Pazzona, 2019; Modrego et al., 2026), there is no significant effect on violent crimes (Column (3)), suggesting that positive local economic shocks do not directly change interpersonal violence.[25] There is no effect on traffic-related crimes (Column (4)), and the coefficients are small and imprecise. The null for violent crime extends to gender-based violence: the mining boom has no statistically significant effect on suspected offenses in which the victim is female, consistent with the absence of a broader violent-crime response.[26]

[24]Chapter 8 (Column 1) of the Criminal Code covers theft, robbery, and other unlawful appropriation offenses, including shoplifting and burglary, which are most directly linked to economic incentives and short-run opportunity costs. Chapter 10 (Column 3) includes crimes involving breach of trust, such as embezzlement and misappropriation in organizational or employment settings, while Chapter 12 (Column 5) covers damage-related offenses, including vandalism and property destruction.

[25]Disaggregating violent crime by penal-code chapter (Appendix Table A.5) shows no significant effect on assault, threats, defamation, or family crimes. The only exception is sexual offenses, where the point estimate is positive and statistically significant, though estimated off a very low base rate (roughly two convictions per year at baseline). The direction is consistent with the boomtown externalities documented near resource-extraction sites (Wilson, 2012; Tynan et al., 2017; Beleche and Cintina, 2018).

[26]Gender-based violence is identified from the Crime Suspicion Register, which records the victim's gender. The outcome equals one if the individual is suspected of any of the following: assault against a woman, (attempted) murder of a woman, unlawful threat against a woman, stalking, molestation, sexual molestation, (attempted) rape of a woman or a person of unspecified gender, or violation of the restraining order. Such cases account for less than one percent of the young male sample, which limits statistical power.

Table 1: Impact of the mining boom on criminal behavior, 2000-2015

	(1) Any crime	(2) Property crime	(3) Violent crime	(4) Traffic crime	(5) Substance crime
Post*Treated	-0.0046	-0.0066**	0.0018	-0.0017	0.0046**
	(0.0039)	(0.0027)	(0.0016)	(0.0020)	(0.0018)
Individual FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Grid FE	Yes	Yes	Yes	Yes	Yes
Observations (N x T)	230480	230480	230480	230480	230480
N	14405	14405	14405	14405	14405
Mean dep. var (2000-03)	0.0251	0.0125	0.0060	0.0097	0.0025
Effect relative to the mean (%)	-18.25	-52.40	29.18	-17.06	181.24
Pre-trends (p-value)	0.7632	0.9194	0.9363	0.5220	0.3131
R-squared	0.3445	0.2929	0.2195	0.2708	0.3759
Within R-squared	0.0000	0.0001	0.0000	0.0000	0.0000

Notes: The table reports difference-in-differences estimates of the 2004 mining boom on the probability of criminal conviction among young male residents aged 18–29, comparing Gällivare and Kiruna (treated) with other Norrbotten County municipalities (control) before (2000–2003) and after (2004–2015) the boom. Outcomes are binary indicators for conviction of: any crime, property crime (theft, robbery, fraud, embezzlement, and property damage); violent crime (violence against persons, unlawful coercion, sexual offenses, and family crimes); traffic crime (Traffic Offences Act and Maritime Offences Act); substance crime (Narcotics Act and related legislation covering possession, use, production, and distribution of narcotics, doping substances, and alcohol/tobacco products). Individuals who moved to the area after 2003 are excluded; the sample comprises 14,405 individuals and 230,480 individual-year observations. All columns include individual, year, and grid fixed effects; standard errors (in parentheses) are clustered at the residential grid level. Data from administrative registers, Statistics Sweden (The longitudinal integrated database for health insurance and labour market studies, LISA). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Finally, there is a positive and significant increase in the probability of being convicted of a substance-related crime after the mining boom for treated young male residents (Column (5)).[27] I observe an increase of 0.46 percentage points in the probability of being convicted of a substance crime among treated individuals. From a baseline sample mean of 0.002, this estimate translates to a 181% increase in individuals convicted and is statistically

[27]The opposing signs of the estimates of property and substance crime are statistically distinguishable from each other. To test this, I stack both outcomes into a single dataset (each individual-year appears twice, once per crime type) and estimate a joint DID model that absorbs individual-by-crime-type, year-by-crime-type, and grid-by-crime-type fixed effects (Kling et al., 2007). The estimated difference between the substance and property crime coefficients is 0.0112 percentage points and is statistically significant, confirming that the two effects are statistically distinct.

significant at the 5% level.[28] Approximately 95% of substance-related convictions in the sample fall under the Narcotics Law, which includes offenses related to possession, use, production, and distribution of narcotic substances, and the conviction data does not allow for separation between them. I provide a decomposition of substance-related crimes using suspicion data. The increase reflects changes in personal consumption rather than supply-side activity. Appendix Table A.6 decomposes narcotics-related suspicions into holding/use versus production and distribution: the overall rise is concentrated in holding and use offenses, with negligible increases in production and distribution from very low baselines.[29] This pattern is consistent with increased local demand and market activity rather than widespread entry into drug production or trafficking. On the contrary, a very small share of convictions are alcohol-, tobacco-, and doping-related offenses. The increase is not driven by alcohol-related behavior: I find no significant effect of the mining boom on convictions or suspicions under alcohol legislation, nor on intoxicated driving identified from the Crime Suspicion Register; approximately 95 percent of substance-related convictions fall under the Narcotics Law.[30]

[28]This corresponds to approximately 11 additional young males convicted of a substance-related crime per year in the mining municipalities. At the lower bound of the 95% confidence interval, substance crime rises by at least 35% relative to the pre-boom mean.

[29]Narcotics suspicions are drawn from the Crime Suspicion Register (Brå). The decomposition into holding/use versus production and distribution provides the best available proxy for personal consumption versus market activity; minor personal-use offenses are classified as holding/use in the suspicion register. Base rates for production and distribution are very low; estimates for these sub-categories should be interpreted with caution. Importantly, the concentration in holding and use, rather than in distribution, also helps interpret the property crime results: an increase in drug distribution could, in principle, generate additional property crime through drug-market dynamics (e.g., dealers committing theft to fund supply or to enforce debt). The negligible changes in distribution offenses provide no evidence of this channel among resident young men, although convictions of this group are an incomplete measure of the size of the local market, which can also be supplied from outside the area. The pattern is therefore consistent with the property crime decline and substance crime increase operate through separate individual-level mechanisms (opportunity cost vs. income) rather than through drug-market externalities.

[30]Convictions under alcohol-related legislation include illegal production, possession, or sale of alcohol. Sweden's state alcohol retail monopoly (Systembolaget) limits availability by fixing outlet location, restricting the number and hours of retail outlets, imposing age verification, and setting prices above market-clearing levels. Alcohol offenses often result in fines rather than convictions; I use suspicion data as a robustness check and find no significant effect of the mining boom on convictions or suspicions under alcohol legislation, nor on intoxicated driving identified from the Crime Suspicion Register. However, using Systembolaget's publicly available county-level sales data, I show that total alcohol sales in Norrbotten County grew above the national average during 2010–2015 (Appendix Figure B.5). This provides suggestive evidence of a broader increase in risky substance consumption in the local area during the boom. These sales are measured at the county level and are not part of the identified individual-level effect. They are used as contextual evidence that risky substance consumption rose in the local area during the boom. The fact that alcohol consumption increased without generating alcohol convictions is consistent with Sweden's institutional setting, where Systembolaget channels virtually all legal alcohol purchases through a state monopoly, limiting the scope for alcohol-related offenses. The pattern across both substances points to an income effect that raised demand for risky goods broadly, with the criminal margin concentrated in narcotics rather than alcohol.

The increase in substance-related crime is consistent with a growing body of evidence that resource booms increase risky and consumption-related behaviors, such as an increase in the demand for various goods and services, including alcohol, narcotics, and entertainment activities provided by the adult entertainment industry (e.g., Wilson, 2012; Tynan et al., 2017; Beleche and Cintina, 2018; Cunningham et al., 2020).[31] There is no significant effect of the mining boom on young males between 30 and 39 years old (Appendix Table C.1).

Overall, these results reveal opposing crime responses to the mining boom among young males: a reduction in property crime consistent with the opportunity cost mechanism discussed in the introduction, and an increase in substance-related crime consistent with higher disposable income and consumption of risky goods. Together, these findings highlight that improved economic conditions do not uniformly reduce all forms of criminal behavior, and that the type of crime matters for understanding the social consequences of local economic shocks. I explore the mechanisms in more detail in Section 6.1.

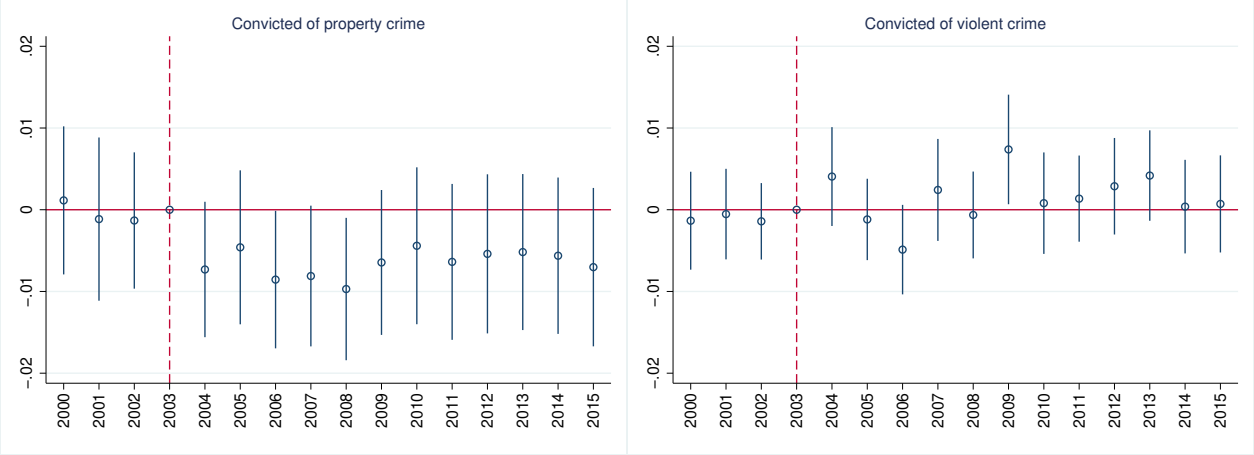
Development over time. The credibility of the DID estimation hinges crucially on the parallel trends assumption. That is, the pre-2004 time trends in the outcome follow the same trend over time between the residents in the treated and control municipalities until 2004, when the mining boom started. To validate the parallel trends assumption and analyze the temporal dynamics of criminal behavior after the mining boom, I estimate a dynamic DID (equation (2)). Figure 2 shows the dynamic treatment effect computed using the same specifications as Table 1, that is, the effect of the mining boom on the probability of being convicted of the different crime types by year. The coefficients for years 2000-2002 (before the shock) allow us to test the presence of parallel pre-trends. Importantly, these coefficients are not significantly different from zero, providing evidence supporting the identifying assumption that the treated and control individuals followed the same economic trajectory before the boom. Thus, they provide support for the use of a DID empirical strategy.

After 2004, I observe that the probability of being charged with a property crime decreases for individuals located in the mining municipalities compared to those in the control group. This effect disappears after 2009, becoming statistically insignificant, which coincides with the timing of the global financial crisis. Regarding substance crimes, I observe an increase in the probability of being charged with a substance-related crime after 2004, becoming insignificant after 2008. These findings complement the ones in Table 1 showing negative effects of the boom on property crimes and positive effects on substance crimes.[32]

[31] A related concern is whether the increase in substance crime could indirectly raise violent crime through drug-market dynamics. The evidence argues against this interpretation. First, approximately 95 percent of substance-related convictions fall under the Narcotics Law and reflect narcotics rather than alcohol, which is typically more closely linked to impulsive violence. Second, the substance crime increase is concentrated in holding and use offenses, not production or distribution. Drug distribution activity (the link most directly associated with violent market enforcement) does not increase. Moreover, there is no effect on gender-based violence, which is further consistent with this interpretation.

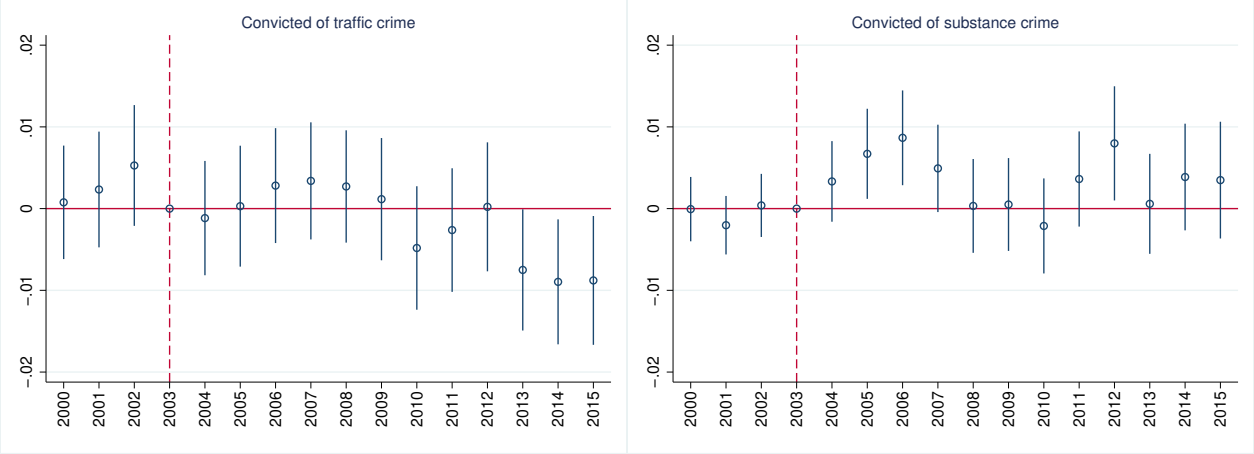
[32] In addition, I estimate a dynamic DID to validate the parallel trends assumption for the spatial

Figure 2: Event study of the impact of the mining boom on criminal behavior, 2000-2015



(a) Property crime

(b) Violent crime



(c) Traffic crime

(d) Substance crime

Notes: The figure plots year-by-year DID coefficients from a dynamic version of the main model, estimated separately for each crime type. Each coefficient measures the difference in conviction probability between treated and control residents relative to the reference year 2003 (normalized to zero). Coefficients for 2000–2002 serve as placebo tests for the parallel trends assumption. 95% confidence interval shown. Design and sample identical to Table 1. Estimations include individuals, grid, and time fixed effects. The sample excludes the migrants to the mining area. Standard errors are clustered at the grid level.

Table 1 shows no statistically significant effects of the mining boom on violent or traffic crimes, with small and imprecise coefficients. For the remainder of the analysis, I therefore focus on property crime and substance-related crime, which are the two outcomes that respond significantly to the boom and drive the opposing mechanisms documented in this

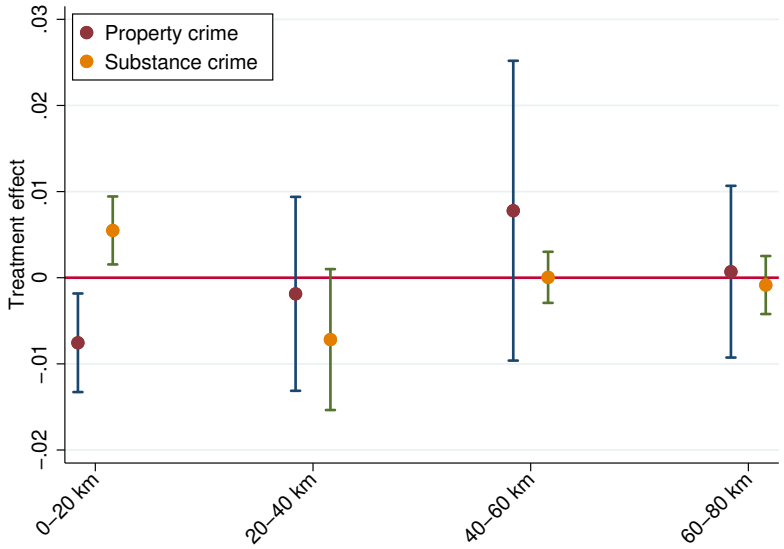
heterogeneity treatment (Table A.7), which is in the Appendix Figure B.6. Importantly, the coefficients for years 2000-2002 (before the shock), which allow us to test the presence of parallel pre-trends, are not significantly different from zero. Moreover, after 2004, I observe that the probability of being charged with a property crime decreases and increases for substance crimes for individuals located within 20 kilometers of the nearest mine compared to those in the control ring.

paper. Estimates for violent and traffic crimes across all subsequent specifications are reported in Appendix A for completeness.

Spatial heterogeneity. To examine whether these effects are spatially concentrated near the mines, as expected if the mining boom is the driving force, I exploit variation in the distance from each individual's residential location to the nearest mine. I classify individuals into 20-kilometer rings and estimate the treatment effect for each ring relative to those living beyond 80 kilometers. Figure 3 reports the results.[33] I observe a large spatial heterogeneity in crime responses. There is a negative and significant reduction in the probability of being convicted of property crime after the mining boom for young male residents located within 20 kilometers of the mines. There is no significant effect for those individuals located farther away from the mines. These results provide evidence of the large spatial localization of the mining boom effects. Additionally, the increase in substance-related crimes is also concentrated among those young male residents located within 20 kilometers of the mines. The specification indicates a decline of 0.76 percentage points in the probability of being convicted of property crime among treated individuals. From a baseline sample mean of 0.012, this estimate translates to a 61% drop in individuals convicted of property crime and is statistically significant at the 1% level. This spatial gradient is consistent with Rodríguez-Puello and Rickardsson (2026), who document that the labor market effects of the mining boom spread up to 27 kilometers during the boom, and with the broader finding that the local effects of economic shocks decay sharply with distance (e.g., Feyrer et al., 2017). The alignment between the spatial reach of the labor market channel and the crime response strengthens the interpretation that improved economic opportunities are driving the observed changes in criminal behavior.

[33]The coefficient estimates, standard errors, and sample descriptives corresponding to the figure are reported in the Appendix Table A.7.

Figure 3: Treatment effects by distance to the nearest mine, 2000-2015



Notes: The figure shows DID estimates by 20-kilometer distance ring from the nearest iron ore mine (Kirunavaara/Gruvberget in Kiruna and Malmberget in Gällivare), with residents beyond 80 km as the reference category. Only property and substance crime are shown, as violent and traffic crime effects are not statistically significant (Table 1). Horizontal lines show 95% confidence intervals. Underlying coefficient estimates for all four crime types are in Appendix Table A.7. Design and sample identical to Table 1.

For the remainder of the analysis, I use the municipality-based treatment definition. The two measures are highly overlapping in practice: over 87% of Gällivare and Kiruna residents live within 20 kilometers of the nearest mine, and the average distance from the mine for Gällivare residents in the sample is approximately 8 kilometers. The municipality definition is more transparent and easier to interpret while capturing essentially the same population.

As noted in Section 1, Street (2025) provides the closest individual-level comparison. For households already resident in US fracking counties, the probability of a crime case being filed fell by 17.5% during the production period, pooling all offense types, while cases filed at the county level rose by 44.7%. The pooled estimate in Column (1) of Table 1 is a decline of 18.25% relative to the pre-boom mean. The two point estimates are therefore similar, and mine lies well inside the confidence interval reported in Street (2025), although the pooled estimate here is not statistically significant. Two different countries and two different resource booms deliver the same answer for incumbent residents. Regarding different types of crime, property cases filed fall by 35%, against the 52% decline in property convictions in Column (2). Driving cases fall by 15%, close to the 17% decline in traffic convictions in Column (4), and neither estimate is statistically significant. Assault cases show no effect, as do violent convictions in Column (3). However, the two papers diverge on substance crime. Drug cases filed by incumbent residents are essentially unchanged during the production period, while substance convictions here rise by 181%.[34]

[34]It is important to note that there are two differences in measurement between this paper and Street

The disaggregated estimates show what that pooled outcome conceals. Property crime convictions fall by 52% while substance-related convictions rise by 181%, so a single pooled indicator reports a net effect close to zero and understates both responses. Two differences in measurement also remain. The outcome here is a conviction rather than a case filed, which is a later stage of the justice process, and the sample is men aged 18 to 29 rather than households. Both should raise the estimated percentage effects in this setting, since convictions are rarer than filings and young men are the most responsive group.

5.2 Entry into crime and continued offending

A central question for policy is whether improved economic opportunities deter entry into crime or only reduce reoffending among individuals already involved. The two margins have different welfare implications: deterrence at the extensive margin, among individuals with no prior record, avoids the long-run consequences of a first criminal record, while a response concentrated at the intensive margin, among individuals already involved in crime, points instead to changes in the returns to continued offending. The panel structure of the data, combined with detailed conviction information, lets me distinguish individuals with and without a pre-boom conviction, and separate convictions resulting in a prison sentence from those that do not.

To capture the nature and intensity of criminal activity, I follow two strategies (e.g., Britto et al., 2022; Grenet et al., 2024). First, to separate these two margins, I classify individuals by their criminal history measured before the boom. Using the pre-boom period 2000–2003, I define an indicator equal to one for individuals with at least one conviction before the boom (prior offenders) and zero otherwise (non-prior offenders). I then estimate a triple-difference specification that interacts the post-by-treated term with this pre-boom indicator, allowing the boom effect to differ between the extensive margin (non-prior offenders, capturing entry into crime) and the intensive margin (prior offenders, capturing continued offending). I define prior-offender status on any conviction in 2000–2003.[35] Second, to separate convictions resulting in a prison sentence from those that do not, three additional outcomes split convictions by whether they result in a prison sentence: convicted-without-prison, convicted-with-prison, and post-prison reoffense (any conviction

(2025). The outcome here is a conviction rather than a case filed, which is a later stage of the justice process, and the sample is men aged 18 to 29 rather than households. Both should raise the estimated percentage effects in this setting, since convictions are rarer than filings and young men are the most responsive group.

[35] Formally, I estimate

$$Y_{ijmt} = \beta_1 \left(\text{Post}_t \times \text{Treated}_m\right) + \beta_2 \left(\text{Post}_t \times \text{Treated}_m \times \text{Prior}_i\right) + \alpha_i + \gamma_t + \delta_j + \varepsilon_{ijmt}, \tag{3}$$

where Prior_i equals one for individuals with any conviction in the pre-boom period 2000–2003, and α_i, γ_t, and δ_j are individual, year, and grid fixed effects. Lower-order terms in Prior_i are absorbed by the individual fixed effects. The coefficient β_1 is the extensive-margin effect (non-prior offenders); $\beta_1 + \beta_2$ is the intensive-margin effect (prior offenders). Standard errors are clustered at the grid level.

in a year following a previous prison sentence). Convicted-without-prison represents the majority of convicted individuals. Convicted-with-prison serves as a proxy for more serious offenses or incapacitation. Post-prison reoffense isolates patterns of reentry into criminal activity post-incarceration.[36] Individuals with no convictions across all years constitute the reference group; the outcomes are mutually exclusive within each category.

The results reveal important heterogeneity (Table 2). The reduction in property crime is concentrated among prior offenders (the intensive margin), whereas the effect on non-prior offenders is essentially zero and statistically insignificant. Improved labor market conditions therefore reduce property offending among young men already involved in crime, the group with the highest baseline offending and the largest change in the returns to legal work, rather than deterring initial entry among those with no prior record, who have a near-zero baseline and little scope to respond. This is consistent with Britto et al. (2022), who find that the recidivism margin responds to labor market shocks, and with an opportunity-cost mechanism operating on the population already at risk. It also aligns with Eren and Owens (2024), who find that the fracking boom reduced ex-offenders' recidivism through higher young-adult wages, even as it raised aggregate crime, underscoring that the recidivism margin responds to local labor demand. For substance-related crimes, the boom raises offending on both margins: the effect among non-prior offenders shows a small increase, while the intensive-margin effect among prior offenders is larger and more precise. The increase is thus concentrated among individuals with pre-existing involvement, consistent with an income channel, while also raising substance-related offending among non-prior offenders. This asymmetry between property and substance crime, where the property decline falls on offenders already at risk while substance offending rises on both margins, is consistent with the view that different crime types respond to distinct economic incentives. While improved labor market opportunities raise the opportunity cost of economically motivated crime among those with the most to gain (Becker, 1968; Draca and Machin, 2015), substance-related offenses appear to be driven by income effects, reinforcing existing behavior and inducing some new use (Cunningham et al., 2020).

[36] Post-prison reoffense is an "ever reoffended after prison" measure within the panel window, not a rate conditional on release timing or sentence length. Therefore, I do not observe the post-prison reoffense of those individuals who are imprisoned late in the sample period, because I only observe a few years afterward.

Table 2: Impact of the mining boom on crime by pre-boom offender status (any-crime definition, triple DiD), 2000-2015

	(1) Property crime	(2) Substance crime
Post*Treated (non-prior)	-0.0001	0.0023*
	(0.0013)	(0.0012)
Post*Treated*Prior offender	-0.0260*	0.0185*
	(0.0158)	(0.0102)
Intensive-margin effect (prior)	-0.0261*	0.0201**
	(0.0157)	(0.0102)
Individual FE	Yes	Yes
Year FE	Yes	Yes
Grid FE	Yes	Yes
Observations (N x T)	230480	230480
N	14405	14405
Control post-period mean, non-prior	0.0123	0.0098
Extensive effect relative to that mean (%)	-0.45	22.91
R-squared	0.2987	0.3761
Within R-squared	0.0083	0.0002

Notes: The table reports a triple-difference decomposition of property (Column 1) and substance (Column 2) crime convictions by pre-boom offending history. A prior offender is an individual with at least one conviction in the pre-boom period 2000–2003. Post*Treated is the extensive-margin effect, that is, the effect on non-prior offenders; Post*Treated*Prior offender is the differential effect for prior offenders; and the intensive-margin effect is the sum of the two, giving the effect on prior offenders. The control post-period mean is the mean of the outcome among non-prior offenders in control municipalities after the boom, used as the baseline for the extensive effect because the pre-boom mean of the non-prior group is zero by construction. Design and sample identical to Table 1 (14,405 individuals, 230,480 person-years). Standard errors (in parentheses) clustered at the grid level. Data from the Swedish Conviction Register (Brå) and administrative registers, Statistics Sweden (LISA). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3 corroborates the previous findings. The reduction in property crime is concentrated among those convicted without receiving a prison sentence (Column 1), with no effect on convictions resulting in prison or post-prison reoffense. Similarly, the increase in substance crimes is concentrated among convictions without prison (Column 4).[37] Overall, these results suggest that local economic shocks, such as the mining boom, reduce new and low-severity criminal activity, while persistent criminal behavior among those with prior

[37]Post-prison reoffense (Columns 3, 6) is estimated on the full sample; individuals never sentenced to prison are coded zero in all years. Individual fixed effects absorb their time-invariant contribution, so coefficient estimates are unaffected, but baseline means reflect the full-sample share rather than a conditional reoffense rate and should be interpreted accordingly.

incarceration is less elastic to local labor market conditions. This is in line with Agan and Makowsky (2023), who find that improved access to economic opportunities primarily reduces lower-level offending. It also complements the findings of Britto et al. (2022), where job loss increases criminal convictions predominantly among individuals without prior incarceration, suggesting that the incarceration margin represents a threshold beyond which labor market conditions have limited influence on criminal behavior.

Table 3: Impact of the mining boom on criminal behavior by incarceration status, 2000-2015

	(1) Convicted + no prison Property crime	(2) Convicted + in prison Property crime	(3) Post-prison reoffense Property crime	(4) Convicted + no prison Substance crime	(5) Convicted + in prison Substance crime	(6) Post-prison reoffense Substance crime
Post*Treated	-0.0060**	-0.0002	-0.0004	0.0026*	0.0004	0.0015
	(0.0026)	(0.0005)	(0.0009)	(0.0014)	(0.0004)	(0.0012)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Grid FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations (N x T)	230480	230480	230480	230480	230480	230480
N	14405	14405	14405	14405	14405	14405
Mean dep. var (2000-03)	0.0113	0.0006	0.0006	0.0019	0.0003	0.0004
Effect relative to the mean (%)	-53.20	-24.92	-66.84	142.47	150.95	384.75
R-squared	0.2390	0.1617	0.3567	0.3215	0.1418	0.3586
Within R-squared	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000

Notes: The table decomposes property and substance crime convictions by incarceration status. Convicted + no prison (Columns 1, 4) = convicted without a custodial sentence in that year. Convicted + in prison (Columns 2, 5) = convicted and sentenced to prison in the same year, a proxy for more serious offenses. Post-prison reoffense (Columns 3, 6) = any conviction following a prior prison sentence within the panel (note: reoffenses of individuals imprisoned late in the panel are not fully observed). Individuals with no convictions are the reference group. Post-prison reoffense (Columns 3, 6) is estimated on the full sample; individuals never sentenced to prison are coded zero in all years. Therefore, baseline means reflect the full-sample share rather than a conditional reoffense rate. Design and sample identical to Table 1 (14,405 individuals, 230,480 person-years). Standard errors (in parentheses) clustered at the grid level. Data from the Swedish Conviction Register (Brå) and administrative registers, Statistics Sweden (LISA). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

5.3 Heterogeneous treatment effects using causal forests

The average treatment effects reported above mask significant heterogeneity in how the mining boom affected individual criminal behavior. To systematically characterize this heterogeneity, I use causal forest estimators via machine learning (Athey and Imbens, 2016; Wager and Athey, 2018; Athey and Imbens, 2019), which rely on data-driven sample splits to identify the relevant dimensions of treatment effect variation, rather than relying on the researcher's discretion when selecting subgroups (Britto et al., 2022). I estimate Conditional Average Treatment Effects (CATEs) for each individual based on baseline levels of observed characteristics included in the registry data (educational level, earnings, employment status, and economic sector).[38] The CATEs are average treatment effects (ATEs) conditional on a set of variables for which the treatment effects may vary. The method produces individual average treatment effects (IATEs) and group average treatment effects (GATEs). Appendix D provides additional details on the estimation procedure.

I find substantial heterogeneity in treatment effects. Appendix Figure B.7 presents the distribution of IATEs in deciles and compares the causal forest ATE to the DID estimates from Table 1. For property crime, the causal forest ATE is larger in magnitude than the DID estimate, indicating that the average effect understates the reduction experienced by the most affected individuals. For substance crime, the causal forest ATE is similar to the DID estimate and statistically significant, with a mean of 0.004. The individual effects range between -0.02 and 0.04, and the distribution of IATEs by decile ranges from a 1.3 percentage point decline in the first decile to a 0.03 percentage point increase in the last decile, indicating that while the average effect is positive, some individuals actually experience a reduction in substance crime. While these estimates have limited precision due to the relatively small sample, the systematic patterns across subgroups are informative about who is most affected by the boom.

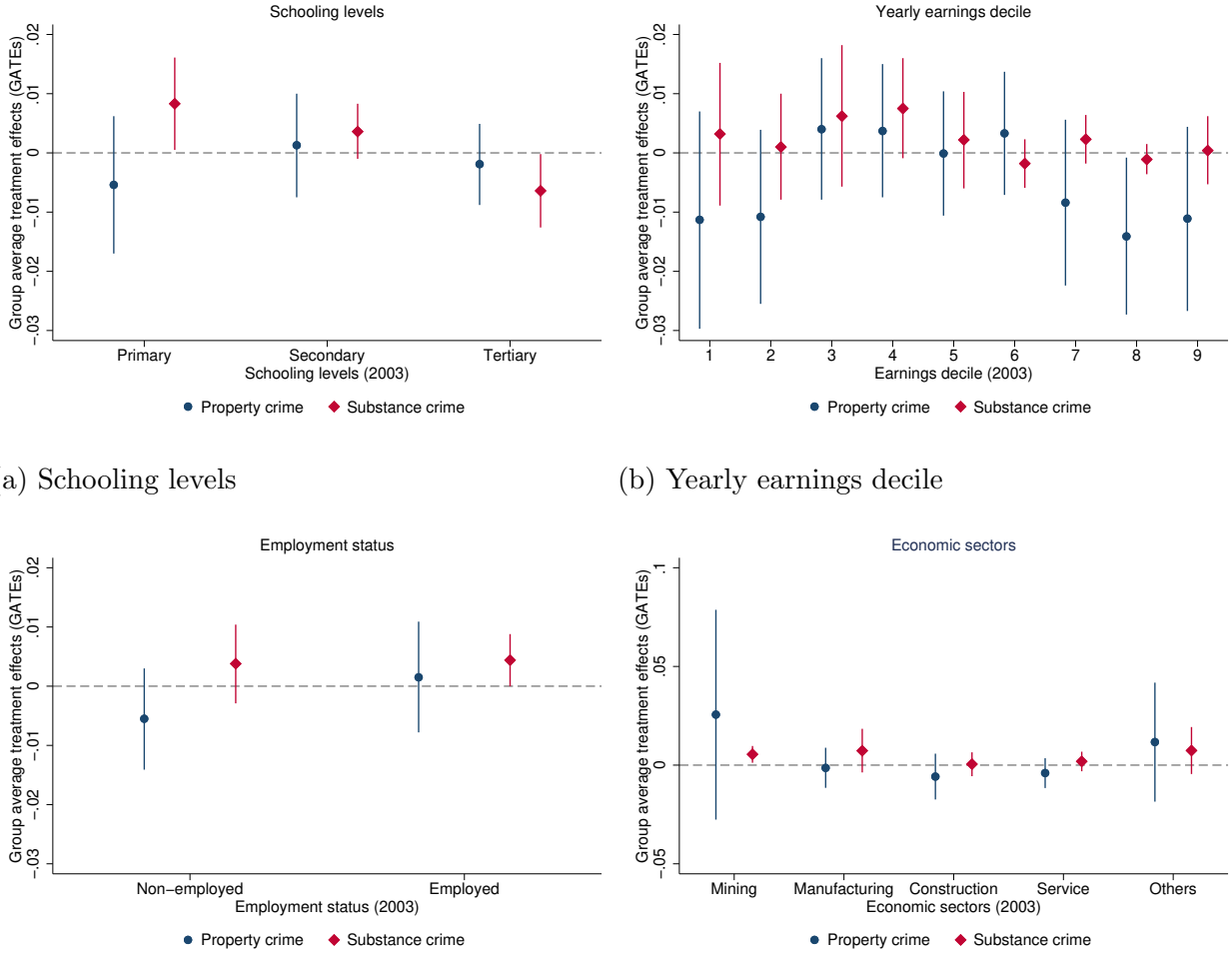
Figure 4 shows the GATEs for property and substance crimes by individual characteristics:

[38] To avoid endogenous movement across categories, individuals are classified in their education level, earnings decile, employment status, and economic sector according to their information in 2003. I analyze heterogeneity in educational levels (primary, secondary, and tertiary), across the income distribution, among employed or unemployed, and regarding employed individuals, whether those who are affected are those in sectors that are directly related to mining extraction, or whether these effects (positive or negative) are experienced in other sectors as well. This analysis is important since previous literature has found significant spillover effects of resource shocks in terms of earnings and employment to other sectors of the economy (Feyrer et al., 2017). I classify economic sectors into mining, manufacturing, construction, services, and others (including agriculture, public, and healthcare). Since the sample includes individuals as young as 18, the youngest cohorts may not have completed their education by 2003, potentially conflating low educational attainment with age. To address this concern, I replicate the education dimension of the heterogeneity analysis, restricting to individuals aged 23 or older, by which age the vast majority of Swedes have completed secondary education. The results are qualitatively unchanged, confirming that the pattern reflects educational attainment rather than age (Appendix Figure B.8).

educational level, income distribution, employment status, and economic sector. On the one hand, the results for property crime suggest concentration among young males with primary educational levels, those who are non-employed, and those in the low tail of the earnings distribution in 2003, though individual group differences are not statistically significant. This pattern is consistent with the economic model of crime: individuals with low educational attainment and weak labor market attachment have lower opportunity costs of crime at baseline and are therefore most responsive to improvements in economic conditions (Becker, 1968; Gould et al., 2002; Machin and Meghir, 2004). The concentration of effects among low-skilled individuals also aligns with Axbard et al. (2021), who find that the crime-reducing effects of mining wealth in South Africa operate through employment opportunities in sectors accessible to low-skilled workers.

On the other hand, the statistically significant increase in substance-related convictions is concentrated among young males with primary educational levels and those directly employed in the mining and manufacturing sectors. Point estimates are positive across most other employed sectors, but are not precisely estimated. There is a statistically significant reduction in the probability of being convicted of substance crimes for young males with tertiary educational levels. The concentration among low-education individuals and mining-sector workers is consistent with an income effect channel, where higher disposable income increases consumption of risky goods (James and Smith, 2017; Cunningham et al., 2020), rather than an opportunity cost mechanism.

Figure 4: Group average treatment effects (GATEs) by characteristics for property and substance crimes, 2000-2015



(a) Schooling levels

(b) Yearly earnings decile

(c) Employment status

(d) Economic sectors

Notes: The figure shows Group Average Treatment Effects (GATEs) for property and substance crimes from a causal forest estimator (Wager and Athey, 2018). GATEs are estimated using causal forest algorithms. Each panel plots the mean predicted treatment effect for groups defined by a pre-boom characteristic measured in 2003: schooling level (panel a), earnings decile (panel b), employment status (panel c), and economic sector (panel d). Characteristics are fixed at 2003 values to avoid post-treatment endogeneity. Bars show 95% confidence intervals. Property crime in navy (circles); substance crime in cranberry (diamonds). The individual IATE distribution is in Appendix Figure B.7; estimation details in Appendix D.

Taken together, the causal forest results reveal that the same economic shock produces opposing crime responses through distinct channels operating on different individuals. Property declines are concentrated among young males with weak baseline labor market attachment (primary education, non-employment, and the lower tail of the earnings distribution), precisely the groups for whom improved opportunities represent the largest change in the returns to legal activity. Substance increases, in contrast, are concentrated

among individuals employed in the mining sector and in low educational levels, consistent with an income effect that relaxes the budget constraint for risky consumption. This separation reinforces the interpretation that property crime responds to opportunity costs while substance crime responds to income effects. Appendix Table A.8 confirms this directly: the statistically significant increase in substance crime is concentrated among individuals with no pre-boom property crime record, while the coefficient among prior property offenders is indistinguishable from zero, ruling out crime-type switching as a mechanism. A complementary descriptive check confirms the same pattern from the supply side: property crime declines are largest among individuals who newly enter mining employment post-2004, while substance crime rises among those already employed in mining before the boom, precisely the group that experienced large earnings gains without a change in employment status (Appendix Table A.9).[39]

5.4 Effects on migrants

Local economic shocks attract individuals looking for better labor market opportunities (Black et al., 2005; Komarek, 2016; Wilson, 2022). The consequences of positive economic shocks may be exploited by migrants rather than residents (Guettabi and James, 2020; Winters et al., 2021; Wilson, 2022). Moreover, as the mining sector is predominantly composed of young male individuals (a more crime-prone demographic) (James and Smith, 2017; Pérez-Trujillo and Rodríguez-Puello, 2022), improved labor market conditions may lead to a shift in population composition in mining municipalities, as a resource boom attracts workers, which could impact criminal behavior. Therefore, I describe the demographic characteristics of migrants to the mining area and empirically analyze the effects of the mining boom on the criminal behavior of young male migrants. Since migration reasons are unobserved, I define migrants as individuals who moved to Norrbotten County in 2004 or later and assume they relocated in response to improved labor market conditions.

There are notable differences in demographic characteristics between residents and migrants to the mining area (Appendix Table A.10).[40] First, I find that a higher share of migrants to the county are convicted of crimes, compared to residents in the mining municipalities. This is descriptive and a rough estimate, but it can be thought of as a conservative estimate of the difference in criminal propensity between groups. Second, migrants are, on average, less likely to be married and have higher educational attainment. Their employment rates and earnings are also lower compared to those of residents, particularly before 2004, suggesting more limited economic opportunities. These findings are similar to those found on migrants to US states due to fracking, who are primarily young, male, unmarried, and white (Wilson, 2022).

While the main specification estimates the effect of the mining boom on the criminal

[39] Since sector transitions are endogenous to the boom, this exercise is descriptive rather than causal.

[40] The sample for this descriptive table includes male residents and migrants aged 18-39.

behavior of residents, Table 4 shifts focus to young male migrants and shows the effects of the mining boom on their probability of being convicted of any crime. The table presents results from several model specifications that differ in comparison groups and estimation approaches. Columns (1)–(2) estimate conventional two-way fixed-effects regressions comparing migrants to the mining municipalities or the control municipalities to themselves before migration (the estimation includes two post-migration interaction dummies, with "pre-migration years" as the omitted category). Columns (3)–(4) contrast migrants to the mining municipalities with migrants to the control municipalities. These specifications allow me to estimate the combined impact of migration and the mining boom on individual criminal behavior.

In the comparison of migrants with their own pre-migration years (Panel A), property-crime convictions rise for movers to both mining and control municipalities, indicating that the increase reflects migration itself rather than the mining boom. The direct comparison of mining-destination with control-destination migrants (Panel B) confirms this, as there is no differential property-crime effect of mining exposure (column (3)). Substance crime, by contrast, is the one margin where mining exposure matters for migrants. Mining-destination migrants are more likely to be convicted of a substance crime than control-destination migrants (column (4)), consistent with the income channel documented for residents, though the baseline probability among migrants is low.

Table 4: Impact of the mining boom on criminal behavior of migrants, 2000-2015

	A. Migrants vs. own pre-migration years		B. Mining- vs. control-destination migrants	
	(1) Property crime	(2) Substance crime	(3) Property crime	(4) Substance crime
Post*Migrants (Mining mun.)	0.0107**	-0.0006	0.0017	0.0105*
	(0.0052)	(0.0051)	(0.0058)	(0.0057)
Post*Migrants (Control mun.)	0.0093***	-0.0078***		
	(0.0033)	(0.0029)		
Individual FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Grid FE	Yes	Yes	Yes	Yes
Observations (N x T)	155753	155753	50603	50603
N	9735	9735	3163	3163
Mean dep. var (2000-03)	0.0616	0.0466	0.0169	0.0058
Effect relative to the mean, Treated (%)	17.32	-1.20	9.92	181.32
Effect relative to the mean, Control mun. (%)	15.12	-16.69		
R-squared	0.4892	0.5258	0.4455	0.4634
Within R-squared	0.0001	0.0000	0.0000	0.0001

Notes: The table estimates the mining boom's effect on criminal behavior among young male migrants aged 18–29. Migrants are individuals who relocated their registered municipality of residence to Norrbotten County in 2004 or later, assumed to have moved in response to improved labor market conditions; they are excluded from the main resident sample (Table 1). Columns (1)–(2) compare migrants against themselves before migration, separately for those who moved to mining municipalities (Gällivare/Kiruna) versus control municipalities (N = 9,735 individuals, 155,753 person-years); pre-migration years are the reference. Columns (3)–(4) directly compare mining-destination migrants with control-destination migrants, isolating the additional effect of mining exposure conditional on migration (N = 3,163 individuals, 50,603 person-years). Crime definitions are in Table 1. All columns include individual, year, and grid fixed effects; standard errors (in parentheses) clustered at the grid level. Data from the Swedish Conviction Register (Brå) and administrative registers, Statistics Sweden (LISA). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Overall, the mining boom does not broadly increase criminal behavior among migrants: effects are concentrated in substance-related offenses, consistent with income and local-market mechanisms, while other crime types show no differential impact relative to control-area migrants. This complements Street (2025), who finds that aggregate crime increases in US fracking areas are driven by in-migration rather than by pre-existing residents, a pattern my individual-level results corroborate.

5.5 Robustness checks

The estimated impacts of the mining boom on criminal behavior are robust to various alternative specifications and robustness checks.

Data-driven rings. The spatial analysis in Section 5.1 classifies individuals into 20-kilometer rings to examine distance decay. A natural concern is whether the results are sensitive to this choice of cutoff. According to Butts (2023), the wrong choice of cutoff biases the results, while the correct identification of the cutoff enables an enhanced understanding of the spatial propagation of the treatment effects. I use an alternative nonparametric estimator that provides a more complete picture of how the shock affects units at different distances, proposed by Butts (2023).[41] It estimates a curve that represents the effect as a function of distance by using many rings. In addition, it selects the rings in a data-driven procedure, eliminating the need to specify a cutoff where the treatment effects become zero to estimate the average treatment effect (Cattaneo et al., 2019), thereby avoiding potential specification searching (Andrews and Kasy, 2019). Using this method, I obtain ten rings grouping the young males sample according to their distance to the nearest mine. The results, in Appendix Table A.11, show that the findings are robust to this empirical strategy.

Travel time treatment. Next, in the characterization of the spatial decay using distance to the nearest mine, straight-line distance may mischaracterize actual accessibility in northern

[41] According to Butts (2023), this method is similar to using the distance to the nearest mine as a continuous measure to estimate the "dosage-response" function proposed by Callaway et al. (2024) in the difference-in-differences approach with continuous treatment.

Sweden, where road infrastructure is sparse, and routes between settlements are often circuitous. As a robustness check, I redefine treatment using travel time by car, measured with OpenStreetMap data via the Open Source Routing Machine (OSRM). I classify individuals into 20-minute rings, obtaining a total of five rings, where individuals located farther away serve as controls. Appendix Table A.12 reports the results. The estimated effects for being convicted of property and substance-related crime are virtually unchanged, compared to those of Table 1, confirming that the findings are robust to this alternative treatment definition. Specifically, there is a negative and significant reduction in the probability of being convicted of property crime after the mining boom for young male residents located within 20 minutes by car of the mines, and an increase in substance crimes for the same treated individuals.

Synthetic control. Although the main analysis is at the individual level, all treatment variation comes from only two municipalities. The synthetic control method (Abadie et al., 2010) provides aggregate-level validation of the individual results, constructing a municipality-level counterfactual that does not rely on the parallel trends assumption. I follow Galiani and Quistorff (2017) and pool Gällivare and Kiruna into a single composite treated unit, constructing a weighted combination of control municipalities that minimizes pre-treatment prediction error over 1996–2003.[42] Appendix Figure B.9 shows the synthetic control tracks pre-boom crime trends closely for both property crime (panel a) and substance crime (panel b). The post-boom divergence mirrors the main DiD estimates: property crime falls below the counterfactual from 2004 onward while substance crime rises above it, corroborating the causal interpretation at the aggregate level.

Age sample robustness. Four complementary exercises address potential concerns about the age restriction used in the main analysis. First, Appendix C replicates the main results for males aged 30–39 as a falsification test, and the estimated effects are small and statistically insignificant across all crime types (Appendix Table C.1), confirming that the findings are specific to young males rather than a general consequence of the boom. Second, Appendix Figure C.1 plots DID estimates separately for four ten-year age groups spanning 18–65: the reduction in property crime and increase in substance crime are concentrated entirely in the 18–29 group and absent for all older cohorts, independently motivating the upper age cutoff. Third, Appendix Figure C.2 shows the main DID estimates as the upper age cutoff varies from 24 to 35 in one-year increments: the effects are stable across the full range, confirming the choice of 29 does not drive the results. Finally, because the sample is restricted to males aged 18–29 in each year, it constitutes a rotating panel: individuals enter when they turn 18 and exit when they turn 30. Within-person identification of the DID coefficient therefore relies on birth cohorts observed in both the pre-boom (2000–2003) and post-boom (2004–2015) periods, corresponding to cohorts born 1975–1985 (aged 18–28 in 2003). Cohorts turning

[42] The donor pool excludes municipalities with a small mine, those in the Stockholm, Gothenburg, and Malmö metropolitan regions, and those with high steel industry employment, the same exclusions as the main analysis.

18 after 2003 appear only post-boom and do not contribute to within-person identification; cohorts turning 30 before 2004 appear only pre-boom. Appendix Table C.2 restricts the sample to the balanced birth cohorts (born 1975–1985) who contribute directly to within-person identification: the estimates are virtually unchanged.

Alternative control groups. To address concerns about the comparability of the residents in control municipalities, I estimate the main results under three alternative control group definitions (Appendix Table A.13). First, I restrict the control group to residents in municipalities that received positive weights in the synthetic control analysis, using only those municipalities that the SCM identifies as the best pre-boom comparators for Gällivare and Kiruna. Second, I replace the Norrbotten control group with residents in all Swedish municipalities of comparable population size, defined as municipalities with between 15,000 and 35,000 residents in 2003 (Gällivare and Kiruna had 19,251 and 23,407 residents, respectively). Third, I exclude residents in Pajala municipality from the control group because it opened the Kaunisvaara iron ore mine in 2012, which could introduce a confounding treatment. The substance crime increase is robust across all three alternative control group definitions. The property crime reduction is robust; however, it loses precision in the SCM-weighted specification due to the much smaller sample.

Additional robustness checks. Appendix Table A.14 reports seven robustness exercises for the main results of young males: (1) baseline, (2) treatment defined by 2003 municipality of residence, (3) inclusion of time-varying controls, (4) inclusion of migrants, (5) balanced panel, (6) excluding four neighboring spillover-prone municipalities, (7) municipality FE in place of grid FE. Coefficients are stable across all specifications.

6 Contextualization and mechanisms

6.1 Why does the mining boom affect crime?

Generally, the estimates are reduced-form effects that encompass multiple potential mechanisms. However, I claim that the estimates are consistent with an opportunity cost mechanism from an improvement in labor market conditions.[43] The results reveal two opposing crime responses that operate through distinct channels, which I discuss in turn. In this section, I use the same empirical design described in Section 4 (using the variables capturing these mechanisms as outcomes) to explore the first-stage effect for the mechanisms via which a mining boom might affect criminal behavior, even though I cannot definitively

[43]While this paper builds on the economic model of crime, sociological theories emphasize complementary channels, such as social integration, cultural conflict, social inequality, and a breakdown of social control (Cohen et al., 1981; Pratt and Cullen, 2005). Some of these channels, for instance, the routine activity theory, which contends that criminals act when there is a convergence of likely offenders, suitable targets, and an absence of capable guardians against crime (Cohen and Felson, 1979), may overlap with the economic mechanisms discussed here, particularly regarding crime prevention capacity and population composition.

distinguish across them or rule out the possibility that there are other intermediating variables at work.[44]

6.1.1 Property crime: the opportunity cost channel

Labor market opportunity cost. If the mining boom improved local labor market conditions, it would raise the returns to legal work and shift the cost-benefit calculation away from criminal activity (Becker, 1968; Ehrlich, 1973).[45] As documented in Section 2, resource booms of this type raise local employment and earnings significantly (Black et al., 2005; Allcott and Keniston, 2018), including in the Swedish case (Tano et al., 2016; Rodríguez-Puello and Rickardsson, 2026), making labor market conditions a natural channel through which the boom may have affected crime.[46] At the same time, higher earnings also make potential victims wealthier, raising the expected payoff to property crime, the so-called rapacity effect (Draca and Machin, 2015). This channel operates through the local environment rather than through a person's own earnings. Because property crime convictions fall on net, rapacity does not dominate in this setting, although its separate magnitude is not identified.

I start by examining how the mining boom affects the labor market conditions of young male residents in the mining municipalities and discuss its relative importance in explaining the changes in crime as a result of the resource shock (Table 5).[47] Columns (1), (2), (3), (4), and (5) show the results for disposable income, labor income (earnings), labor income conditional on being employed, employment overall, and employment in the mining sector, respectively.[48] Consistent with Rodríguez-Puello and Rickardsson (2026), the mining boom

[44] Another approach to evaluate mechanisms in the literature is to include the variable on the right-hand side as a control to see how the main treatment effect changes and test mediation or partial channeling. Nevertheless, the main concern why I do not apply it is that mediators (e.g., earnings) are bad controls and bias the treatment effect.

[45] Theoretically, individuals are rational economic agents that choose between legal work and criminal activity by comparing the legal work wage in the labor market and the expected payoff to crime (which depends on the expected gain from crime minus the cost, which is the product of the probability of being caught and the associated punishment), choosing crime whenever the former exceeds the legal wage (Becker, 1968; Ehrlich, 1973). Therefore, individuals with higher wages or better employment opportunities experience an increase in the opportunity cost of engaging in criminal activity, reducing local crime levels.

[46] The link between labor market conditions and crime has also been explored in the literature (e.g., Raphael and Winter-Ebmer, 2001; Edmark, 2005; Öster and Agell, 2007; Fougère et al., 2009; Nordin and Almén, 2017; Dix-Carneiro et al., 2018; Ferraz et al., 2022).

[47] Unlike Rodríguez-Puello and Rickardsson (2026), who study the broader resident population, I estimate these outcomes for exactly the same sample and design as the crime results in Table 1, so they serve as the first stage of the opportunity-cost channel.

[48] Disposable income is the sum of all incomes, including other benefits (e.g., child allowances, social benefits, and housing benefits) minus final tax. All income variables are adjusted to real values with the base year 2000 using the national CPI. To avoid typical problems of zeros in the outcome variables (Chen and Roth, 2024; Mullahy and Norton, 2024), I measure income in levels. Therefore, the coefficients can be

raises labor market opportunities on both the extensive and intensive margins. Specifically, the mining boom has a positive effect on the labor market conditions of young male residents in the mining municipalities, with a significant increase in disposable and labor income, and the probability of being employed, especially directly in the mining sector. Yearly disposable income increases by 17,923 SEK for treated young male residents after the mining boom compared with control residents. This represents a 17% increase from the baseline mean. The observed increase in labor income is higher. While there is no clear effect on employment due to different effects for different economic sectors, a substantial increase is observed in the probability of being employed in the mining sector.[49] These labor market improvements raise the returns to legal activity, shifting the cost-benefit calculation away from property crime (Draca and Machin, 2015; Edmark, 2005; Axbard et al., 2021).[50]

Table 5: Mechanisms: impact of the mining boom on labor market outcomes, 2000-2015

	(1) Disposable income	(2) Labor income	(3) Lab. inc. employed	(4) Employment	(5) Employment mining
Post*Treated	17.9232***	28.3618***	26.1973***	0.0186**	0.0869***
	(1.4464)	(2.2079)	(2.2928)	(0.0094)	(0.0068)
Individual FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Grid FE	Yes	Yes	Yes	Yes	Yes
Observations (N x T)	230480	230480	152308	230480	230480
N	14405	14405	9519	14405	14405
Mean dep. var (2000-03)	107.6874	114.5652	172.4796	0.6225	0.0219
Effect relative to the mean (%)	16.64	24.76	15.19	2.99	396.29
R-squared	0.6161	0.7432	0.7152	0.5618	0.6831
Within R-squared	0.0007	0.0025	0.0024	0.0000	0.0083

Notes: The table estimates the mining boom's effect on labor market outcomes using the same design and sample as Table 1. Disposable income (Col. 1) and labor income (Cols 2-3) are in 1,000 SEK, adjusted to real 2000 values using the national CPI. Labor income, employed (Col. 3), is labor income conditional on employment (9,519 individuals). Employment (Col. 4) and mining employment (Col. 5) are binary indicators. Standard errors (in parentheses) clustered at the grid level. Data from administrative registers and LISA, Statistics Sweden. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

To examine the wage rate component of the annual earnings increase, I supplement the LISA annual earnings measure with employer-reported full-time equivalent monthly salaries

interpreted as the effect on income as measured directly in 1000 Swedish krona (in 1000 SEK).

[49] Rodríguez-Puello and Rickardsson (2026) find that the mining boom in Sweden increased employment in mining and manufacturing, while there is a reduction in service employment due to high competition for workers.

[50] This operates through both higher returns to legal work and a direct time-use effect: employment occupies hours that would otherwise be available for criminal activity, reinforcing the opportunity cost mechanism (Cohen and Felson, 1979; Lochner, 2004).

from the Structure of Earnings Survey (Lönestrukturstatistik, LSS) by Statistics Sweden.[51] Because the LSS wage measure is already standardized to full-time equivalents, it captures the wage rate per se rather than total compensation. The mining boom raises the monthly wage rate by approximately 3.4 percent for young male individuals in treated municipalities (Appendix Table A.15). The annual earnings effect reported in Table 5 is larger, mainly because it reflects both the monthly wage rate and the number of months employed during the year.

To benchmark the magnitude of these effects, I compute an implied elasticity of property crime with respect to earnings by dividing the estimated effect on property crime by the 24.8 percent increase in labor income (Column 2, Table 5), yielding an elasticity of approximately -2. That is, a one percent increase in earnings is associated with a two percent decrease in property crime conviction probability. This is at the upper end of existing estimates in the literature, which find implied elasticities roughly between -1 and -2.5 (e.g., Gould et al., 2002; Machin and Meghir, 2004; Agan and Makowsky, 2023), and substantially larger than the -0.58 estimated by Britto et al. (2022) using job loss as a shock. This elasticity should not be interpreted as causal, as the mining boom may affect criminal behavior through channels other than earnings, including crime prevention capacity, migration, and income inequality, which I discuss below.

To link earnings and crime directly, I estimate a two-stage least squares (2SLS) model that instruments individual labor income with the mining boom ($Post \times Treated$), avoiding the bad-controls problem of placing post-treatment earnings on the right-hand side. The first stage is strong ($F = 118$). Appendix Table A.16 reports the results. A one thousand SEK increase in income lowers the probability of a property crime conviction by 0.015 percentage points, an implied earnings elasticity of about -1.4, in line with the -1 to -2.5 range in the literature (e.g., Gould et al., 2002; Machin and Meghir, 2004; Agan and Makowsky, 2023). The sign reverses for substance crime, where higher income raises the probability of conviction. These opposing signs are evidence for the two channels: earnings gains deter property crime through the opportunity cost of time, while raising substance crime through the income channel.[52]

[51] The Lönestrukturstatistik is an annual survey conducted by SCB covering the entire Swedish economy. It is based on a stratified sample of enterprises with ten or more employees, with near-complete enumeration of the public sector. Employers report wages and salary components directly; the monthly wage measure used here is standardized to full-time equivalents, making it interpretable as the wage rate independently of individual working-time arrangements. The matched sample linked to my existing administrative panel covers 3,251 individuals and 52,020 person-year observations over 2000–2015. Because the survey targets larger enterprises, coverage in small northern municipalities is sparser than in LISA, and results should be interpreted with this limitation in mind.

[52] Two caveats apply to this specification. The instrument is the boom itself, so the exclusion restriction requires that the boom affect crime only through an individual's own labor income. Several mechanisms discussed in this section, including the wealth of potential victims, the size of the local illicit market, and changes in the local population, violate that restriction. The 2SLS estimates should therefore be read as the

Income inequality. According to the economics literature on crime, there are rational incentives to commit crimes when there are lower-income people near high-income people in a community (Deller and Deller, 2010), and the economic gains of a mining boom may be concentrated among specific population groups, such as extraction workers rather than other residents (Hardy and Kelsey, 2015). There is empirical literature linking resource booms with income inequality (e.g., Reeson et al., 2012; Loayza et al., 2013) and income inequality with crime and violence (e.g., Kelly, 2000; Bourguignon et al., 2003; Neumayer, 2005). Therefore, combining both pieces of evidence, a mining boom that increases local income inequality may indirectly generate incentives to commit crime. While I do not observe property crime increases due to the mining boom, only increases in substance-related crimes, it is important to examine this mechanism to discard its role in the main results.

Measuring income inequality at the individual level is a challenge. I examine the effect of the mining boom on the probability of moving into (or out of) the top and bottom of the income distribution. Specifically, using the labor income in 2003, I classify individuals by year and municipality of residence into terciles, and use binary outcomes equal to one if the individual is in the first tercile (bottom of the income distribution) or the third tercile (top of the income distribution). The results in Columns (1) and (2) of Table 6 show that the mining boom significantly reduces the probability of being in the bottom earning tercile (Column (1), significant at the 5% level) and weakly increases the probability of being in the top earning tercile (Column (2), significant at the 10% level). These results suggest that the boom shifts the income distribution upward, benefiting individuals at both ends: lifting some out of the lowest-income group while moving others into the highest-income group. Importantly, the stronger and more precisely estimated effect at the bottom of the distribution indicates that the income gains from the boom are not solely concentrated among top earners. This pattern is more consistent with a general improvement in economic conditions than with a widening of income inequality. Therefore, there is no compelling evidence suggesting that income inequality played a role in the changes in criminal behavior. This result is contrary to James and Smith (2017), who find descriptive evidence for this mechanism in the case of the impact of an energy boom on regional crime in the United States, where the resource shock increased crime rates in shale-rich counties, and this coincided with a rise in income inequality.

reduced-form effect expressed in earnings units, which makes the magnitudes comparable with the elasticities reported in the literature, rather than as the causal effect of an individual's own earnings.

Table 6: Mechanisms: impact of the mining boom on different mechanisms, 2000-2015

	(1) Bottom earning tercile	(2) Top earning tercile	(3) Police occupation 18–29	(4) Police industry 18–29	(5) Police occupation 18–65	(6) Police industry 18–65
Post*Treated	-0.0108**	0.0071*	0.0033	0.0026	0.0005	-0.0004
	(0.0054)	(0.0040)	(0.0023)	(0.0021)	(0.0006)	(0.0005)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Grid FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations (N x T)	124267	124267	187619	230480	936784	1138200
N	7767	7767	11726	14405	58549	71138
Mean dep. var (2000-03)	0.4136	0.2197	0.0019	0.0052	0.0053	0.0088
Effect relative to the mean (%)	-2.62	3.23	170.34	50.07	8.82	-4.31
R-squared	0.9091	0.9176	0.5546	0.5200	0.7228	0.7363
Within R-squared	0.0001	0.0001	0.0001	0.0000	0.0000	0.0000

Notes: The table estimates the mining boom's effect on different mechanisms using the same design and sample as Table 1. Bottom/top earning tercile (Cols 1–2) indicate whether the individual falls in the bottom or top third of the annual labor income distribution within municipality and year (7,767 individuals with positive income). Police occupation (Col. 3) and police industry (Col. 4) are binary indicators for employment in police-related occupations or the justice sector (11,726 individuals). Columns (5)–(6) replicate the police analysis for the full 18–65 working-age male population. Standard errors (in parentheses) clustered at the grid level. Data from administrative registers and LISA, Statistics Sweden. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

6.1.2 Substance crime: income and occupational stress channels

The increase in substance-related offenses follows a different logic from the property crime decline. As documented in Sections 5.2 and 5.3, the rise is larger among prior offenders but also present among non-prior offenders, and it is concentrated among young males employed in the mining and manufacturing sectors rather than among individuals in the lower tail of the earnings distribution. This pattern points away from the opportunity cost channel and toward income effects.

Four mechanisms could plausibly explain this pattern: (i) an income effect, meaning that higher earnings relax the budget constraint for drug and alcohol purchases; (ii) workplace culture specific to male-dominated extractive industries; (iii) occupational stress from the rapid expansion of operations; and (iv) increased employer drug testing and occupational health screening, which would raise the detection rate of substance offenses without any behavioral change. Mechanisms (i) and (iii) are addressed through regression evidence and newspaper text analysis below, and mechanism (iv) through institutional documentation of LKAB's testing policy. Mechanism (ii) cannot be separated from the income effect with the data available, since both predict an increase in substance crime concentrated among mining workers, so it is discussed as a possibility rather than tested.

Income channel. The most coherent of the four is the income effect. Higher earnings raise the purchasing power of individuals already inclined toward substance use, enabling greater consumption of drugs and alcohol as normal or even luxury goods (James and Smith, 2017; Cunningham et al., 2020). This is the demand-side counterpart to the opportunity-cost argument: the same income gain that makes property crime less attractive also relaxes the budget constraint on risky consumption. Consistent with this, a large literature documents that positive income shocks can increase engagement in addictive or health-risky behaviors even as they reduce economically motivated crime (Draca and Machin, 2015; Cunningham et al., 2020). In the language of the transfers-and-crime literature, they are complements to crime: positive income shocks that deter acquisitive offending can simultaneously raise consumption-driven offending (Dobkin and Puller, 2007; Cotti et al., 2016; Carr and Packham, 2019). The narcotics offenses decomposition by type above is consistent with this channel: the increase in substance crime is concentrated in holding and use offenses rather than in distribution, pointing to personal consumption rather than supply-side market entry. The first-stage evidence in Table 5 directly supports this channel. The causal forest results in Figure 4 reinforce this reading: the increase in substance crime is concentrated among individuals in the upper earnings distribution in 2003, those who gained most in absolute income terms from the boom.

Since the causal forest conditions on 2003 characteristics, this concentration among high earners points to pre-boom mining workers, those who gained most in absolute terms, as the primary margin of the income effect. The sector transition analysis in Section D corroborates this: the substance crime increase is concentrated among treated residents who did not newly enter mining post-2004, that is, those already employed in the sector before the boom. New mining entrants show no significant increase in substance crime. This suggests that the income effect on substance consumption operates primarily through the earnings gains of established mining workers rather than through the initial employment margin of boom entrants. This income-driven pattern is not specific to mining, as industries combining physically demanding work with above-average wages, including construction, oil and gas extraction, and transportation, systematically exhibit higher substance use rates than white-collar occupations (Frone, 2006; Bush and Lipari, 2015), consistent with a compensating-differential channel in which higher earnings expand the budget available for risky consumption.

Workplace culture. A second channel, distinct from pure income effects, operates through the social norms and behavioral culture of male-dominated extractive industries, where intense physical labor, long shifts, and social isolation create conditions for collective substance use in off-hours (Shandro et al., 2011; Arratia-Solar and Paredes, 2023). Higher earnings would facilitate this pattern by relaxing financial constraints. This channel is observationally equivalent to the income effect in my data, because both predict an increase in substance crime concentrated among mining workers, so I draw no conclusion about its

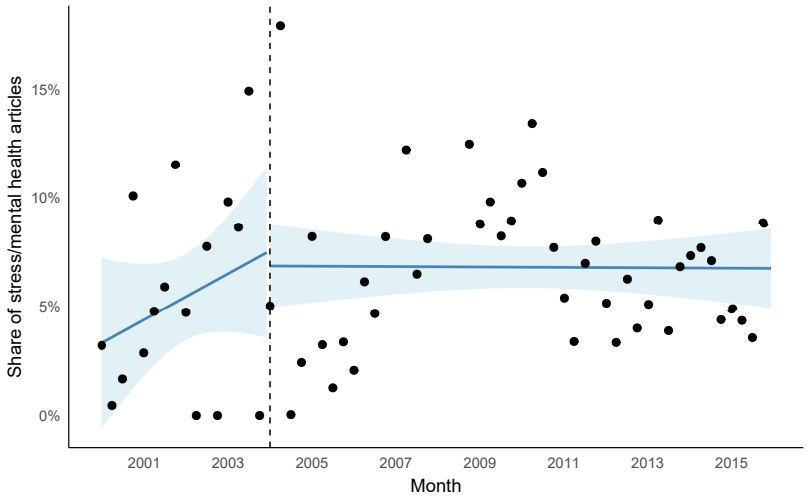
role. Separating the two would require information the registers do not contain, such as shift schedules, co-worker networks, or within-firm variation in exposure to the boom. I note the channel because the literature on extractive industries treats it as important.

Occupational stress. A second channel operates through occupational stress. The mining boom shifted the sectoral composition of employment in treated municipalities. Table 5, Column (5) shows that the probability of being employed in the mining sector increased among treated young male residents, while Rodríguez-Puello and Rickardsson (2026) document that the boom raised employment in mining and manufacturing while reducing service sector employment as competition for workers intensified. Mining and extractive industries are among the most physically demanding and hazardous occupational environments, characterized by elevated injury risk, irregular shift schedules, and the psychological pressures of high-intensity underground operations (Shandro et al., 2011; Arratia-Solar and Paredes, 2023). These are well-documented conditions that can increase substance use as a coping response, even among individuals with rising incomes. In Sweden, however, official statistics from Arbetsmiljöverket indicate that the mining sector records fewer fatal occupational accidents than construction, transport, or agriculture, pointing to a relatively safe working environment by Swedish standards (Arbetsmiljöverket, 2025).

I provide suggestive evidence on the occupational stress channel using a text analysis of Swedish newspaper coverage of LKAB and interrupted time series (ITS) models over 2000–2015. LKAB is the main iron ore producer in Sweden and operates the large-scale mines in Kiruna and Gällivare. Full details are in Appendix F; I summarize the main findings here.

I construct the monthly share of LKAB-related articles mentioning stress or mental health terms and estimate an ITS model with a structural break at January 2004. Figure 5 plots the series alongside the fitted values. The share of stress-related articles is low throughout the period, averaging approximately 3 percent in 2000 and around 7 percent by late 2003. At the onset of the boom, there is no significant level shift: the estimated immediate level shift is $\hat{\beta}_1 = -0.007$, indicating no detectable change in the share of articles mentioning occupational stress or mental health. The pre-boom trend ($\hat{\beta}_2 = 0.001$) and post-boom slope change ($\hat{\beta}_3 = -0.001$) are both statistically insignificant, providing no evidence of systematic trends in stress-related coverage before or after the mining boom. The null result is robust to weighting by article count (Appendix Table F.4) and to dropping months with fewer than five total LKAB articles, a restriction that addresses concerns about inflated share variance in sparse pre-boom months. In the latter sample, the estimated level shift is $\hat{\beta}_1 = 0.008$, and neither the pre-boom trend nor the post-boom slope change is statistically significant (Appendix Table F.5). I interpret this as providing no support for occupational stress as the primary driver. The evidence points against this channel, though I cannot rule out that stress played some residual role.

Figure 5: Interrupted time series of stress/mental health newspaper articles, 2000–2015



Notes: The figure plots a quarterly binscatter of the monthly share of LKAB*-related newspaper articles mentioning stress or mental health terms (keyword dictionary in Appendix Table F.3) over 2000–2015. LKAB is Sweden's state-owned iron ore company, operating the mines in Kiruna and Gällivare. Fitted lines show estimated pre- and post-boom linear trends from an interrupted time series (ITS) regression with a structural break at January 2004 (vertical dashed line). Articles are identified using the newspaper archive *Retriever Mediearkivet*. Following the literature, I search for articles containing the case-insensitive string "LKAB*", where the asterisk is used as a wildcard. Newey-West standard errors (lag = 12). None of the ITS coefficients is statistically significant, providing no evidence of a systematic increase in occupational stress coverage following the boom. Coefficient estimates in Appendix Table F.4; full details in Appendix F.

Two additional results corroborate this reading. First, I find a null result on long-term sick leave: the mining boom generated no statistically significant increase in sick-leave spells exceeding two weeks or sickness-benefit payments among young male residents in treated municipalities.[53] Second, I provide a complementary test using employer-reported data on contracted work scope, a measure of working time as a share of full-time equivalents that incorporates overtime adjustments (Appendix Table A.15). The overall effect is negative and statistically significant, driven primarily by workers outside the mining sector whose contracted hours declined. For workers employed in the mining sector in 2003, the estimated effect is small and statistically insignificant. However, pre-boom mining workers were already contracted at essentially full-time scope (mean of 99.7 out of 100), leaving no room to detect additional work intensity through this measure. Uncontracted overtime or informal work pressure would not be captured here. Together, these results mean the contracted work scope analysis is uninformative about stress specifically for mining workers, and I rely on the newspaper text evidence as the primary basis for the conclusion that occupational stress

[53]This is motivated by Shandro et al. (2011), who find increases in pregnancies, sexually transmitted infections, and mine-related injuries during booming mine activities. While I do not observe detailed health outcomes such as hospital admissions or mental health diagnoses, I explore this margin using employer-paid sick leave spells exceeding two weeks, which capture more serious or prolonged health episodes available in the administrative data. Although sick leave of more than two weeks is an imperfect proxy for health and mental health, and does not capture day-by-day sickness related to substance use, prior work has used extended sickness absence as an indicator of underlying health shocks.

is unlikely to be the primary driver of the substance crime increase. Taken together, if the boom had induced widespread occupational stress or health deterioration, one would expect the share of stress-related media coverage, the incidence of severe health-related absences, and the overtime work to increase. Together, these results substantially narrow the scope for occupational stress as the primary driver of the increase in substance crime, even though the sectoral composition of employment shifted toward more physically demanding industries.

Detection channel. The concern that employer drug testing mechanically inflates recorded substance convictions without any behavioral change is substantially limited by the institutional structure of LKAB's testing policy. LKAB's published workplace policy documents that a positive drug test result leads to revocation of the individual's site access permit, not to a police report or criminal referral; the police are contacted only when a vehicle operator tests positive in a breath alcohol test, which constitutes a traffic offense (LKAB, 2023). Since the conviction records used in this paper require police involvement and prosecution, internal LKAB testing cannot mechanically generate entries in the outcome variable. This institutional structure reflects Swedish labor law more broadly, as employer drug testing is legally restricted to safety-sensitive roles, and a positive test result is routed to occupational health assessment and rehabilitation rather than criminal proceedings. This applies to both direct LKAB employees and contracted workers operating on LKAB's sites. For the detection channel to explain the substance crime results, police would need to have independently increased enforcement in mining municipalities after 2004, which is a hypothesis addressed and rejected in the police capacity analysis below (Section 6.1.3).

6.1.3 Government's crime prevention capacity

Changes in policing represent a competing explanation for both results. Becker (1968) highlights that the probability of detection is an important factor to consider when examining factors influencing an individual's decision to commit a crime. Increasing the probability of being caught and/or the resulting punishment may reduce crime according to theory. Previous literature has shown that crime decreases when there is an increase in police presence (Di Tella and Schargrodsky, 2004; Machin and Marie, 2011). Therefore, a concern in interpreting the main results is that the changes in crime may be due to improvements in the government's crime prevention capacity. If the mining boom generated additional local tax revenue that funded police resources, increased police presence could explain the property crime decline through a deterrence channel rather than an opportunity cost channel (Foley, 2011; Axbard et al., 2021). Conversely, an expansion of police activity could mechanically increase recorded substance offenses without any behavioral change, by raising the probability that existing substance use is detected and prosecuted.

As an approximation to this mechanism and as a proxy of the government's crime prevention capacity, I test for changes in the police force by examining the effect of the mining boom on the probability of young male residents becoming a police worker (Columns (3) and

(4) Table 6).[54] I find no significant change in police employment or resources in the mining municipalities following the boom. This null result holds across a broader age range. Columns (5) and (6) of Table 6 replicate the analysis for all 18–65-year-old residents and confirm no significant effect on police employment under either classification, with coefficients close to zero.

This null is expected in the Swedish institutional context: police resources are funded by the state alone, not by the state and local authorities as it is in other countries (Lindström, 2015), and it does not depend on the crime level or economic conditions in each municipality. During the sample period, Swedish policing was organized into 21 county-level police authorities and was the responsibility of the National Police Board, not of local governments, and was not tied to local economic conditions (Klockare, 2001). Norrbotten County had its own authority, covering both the treated municipalities (Gällivare, Kiruna) and all control municipalities within the county. The National Police Board sets budget allocations, staffing levels, and operational priorities through annual appropriation letters issued by the central government following a parliamentary appropriations decision. Municipalities had no authority to increase police funding, assign officers, or direct enforcement priorities. A local mining boom, therefore, cannot generate additional police capacity, regardless of its fiscal effects. Moreover, since the county police authority covered the entire county, any national-level changes in staffing or resources in Norrbotten would apply equally to treated and control municipalities and would be differenced out in the DID design. The existing literature confirms that policing and detection do affect crime when they are meaningfully increased. For instance, Anker et al. (2021) find that expanding DNA database registration reduces recidivism by around 40%, and Kirchmaier et al. (2020) find that a targeted policing operation reduced metal theft by around 35%. However, neither condition held here. Consistent with this, James and Smith (2017) and Axbard et al. (2021) find that changes in fracking activities and mining value did not affect police operations, ruling out policing as a driving mechanism in similar settings.

Overall, the evidence points to two distinct behavioral channels operating simultaneously. The reduction in property crime is consistent with improved labor market conditions, raising the opportunity cost of economically motivated criminal activity (Becker, 1968). The increase in substance-related crime is consistent with an income channel: raising earnings relaxes the

[54]There are two ways of classifying residents as police workers using data from Statistics Sweden. Since neither of them is a perfect classification, I use both to compare the results. I classify as police those individuals working in the security sector using the Swedish Standard Industrial (SNI) Classification from 2007, specifically classified in the codes 74900, 80100, 80200, 80300, and 84240. I use this data because they are available for the whole period of analysis. As a comparison, I use data from the "Swedish Occupational Register with Statistics" (Statistics Sweden) for the period 2001-2015. The data are available only after 2001, and those for the years 2014 and 2015 are not comparable. As police officers, I consider patrol officers, criminal investigators, and community police officers (Lindström, 2015). The correlation of the police per capita variables for the period 2001-2013 among the two measures is 90%.

budget constraint for drug purchases, concentrated among prior offenders and individuals directly employed in the mining sector, while also reaching men with no prior conviction. The newspaper text analysis finds no detectable increase in stress-related coverage at the boom's onset, pointing away from occupational stress as a primary driver. Changes in policing do not account for either result. Selective migration and income inequality do not drive the results. Disentangling the remaining channels further would require data on employer testing policies and workplace health records not available in the current analysis.

6.2 Social cost effects

To convert these opposing crime responses into monetary terms, I translate the estimates in Table 1 into social costs, applying the unit costs of crime in Heeks et al. (2018) to the treated young male population over the 2004–2015 boom period, following Alsan et al. (2025). Full unit-cost definitions and the detailed calculation are in Appendix E (Appendix Table A.17). Over these twelve years of the boom (2004-2015), the property-crime reduction generates roughly SEK 17.8 million in social savings, partly offset by about SEK 14.1 million in additional costs from the substance-crime increase, for a net crime-related gain of approximately SEK 3.7 million.[55] Two features of this accounting matter for interpretation. First, it is deliberately order-of-magnitude: the unit costs are drawn from UK estimates applied to Sweden as the closest available high-income, welfare-state comparator, and the substance-crime figure likely understates long-run costs if addiction persists after the boom, so the net figure is best read as a lower bound on the benefit of the property-crime decline. Second, and more important, these are only the crime-related consequences: the same boom raised labor income by approximately SEK 28,000 per treated young male resident per year, an aggregate income gain that far exceeds the crime effects, so the near break-even in crime costs should not be read as the boom being welfare-neutral overall.

7 Conclusions

This paper asks whether a positive local economic shock substitutes for or complements crime. The 2004 iron-ore boom in northern Sweden, combined with geocoded administrative registers on all criminal convictions and demographics in Sweden from 2000 to 2015, allows a clean answer: it does both. Among young male residents aged 18–29 in mining municipalities, property crime convictions decline by 52% and substance-related convictions rise by 181%, with no effect on violent or traffic crimes. These effects are driven by existing residents in the area, rather than in-migrants, and are concentrated among young males located within 20 km of the mines. The two responses operate on different individuals through different channels: property declines reflect the rising opportunity cost of crime among those with weak

[55] The calculation covers convictions of young men who lived in the mining municipalities before the boom, so it is not the change in the total social cost of crime in those municipalities, which also reflects offending by people who moved there during the boom.

baseline labor market attachment, while substance increases reflect an income effect among prior offenders and individuals directly employed in the mining sector, while also reaching men with no prior conviction. The evidence does not point to changes in the population composition due to migration, income inequality, occupational stress, or the government's crime-prevention capacity as primary drivers of the crime results, though I cannot formally rule out a residual role for stress.

The findings carry several implications for policy in resource-dependent regions. First, the evidence supports labor market access through improved employment and earnings as the primary lever for reducing property crime among existing residents, without any targeted criminal justice intervention. This implies that place-based employment programs, active labor market policies, and industrial development that reaches the young male residential population can serve simultaneously as crime-prevention tools. Resources devoted to property crime enforcement may be partially displaced as labor market conditions improve, and could instead be reallocated toward the areas where the boom generates new social costs. Second, the flip side of this finding is that positive economic shocks create new demands on narcotics enforcement and public health infrastructure. Rather than expanding criminal justice responses to substance offenses, the evidence is more consistent with public health interventions in the sense that substance use among employed, high-income young males in a high-pressure work environment may be better addressed through occupational health programs, addiction prevention, and accessible treatment services than through increased policing. The detection channel analysis confirms that LKAB's internal testing policy already routes positive drug test results to rehabilitation rather than criminal referral.

Although the analysis focuses on a specific mining boom in northern Sweden, a developed-country mining context, the mechanisms studied in this paper are not unique to this context. The theoretical channels linking economic opportunities to criminal behavior, through changes in labor market returns, opportunity costs, disposable income, and population mobility, are central to a wide range of local economic shocks. Mining booms provide a particularly clean setting to study these mechanisms because they generate large, localized, and plausibly exogenous changes in economic conditions. However, similar dynamics arise in other settings characterized by rapid local economic expansion, including energy booms, infrastructure investments, manufacturing expansions, and large place-based development programs. At the same time, institutional features such as labor market regulation, social insurance, and law enforcement capacity may shape how these mechanisms operate, implying that the magnitude of the effects may vary across contexts even when the underlying behavioral responses are similar. Future work could examine how these mechanisms operate in settings with different institutional environments, such as developing countries or regions with weaker labor market protections and enforcement capacity.

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Online Appendix

Digging for Trouble?
Mining and Criminal Behavior of Young Males

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A Appendix: Additional tables

Table A.1: Mining municipalities, mines and mining employment share

County	Municipality	Mine(s) and main product(s)	Population 2015	Mining employment share 2003	2010	2015
Norrbotten	Gällivare	Malmberget (Iron ore) and Aitik (Copper)	18,123	17.44%	20.89%	22.56%
Norrbotten	Kiruna	Kirunavaara (Iron ore) and Gruvberget (Iron ore)	23,178	13.94%	16.51%	18.44%
Västerbotten	Lycksele	Kristineberg (Copper/zinc) and Svartliden (Gold)	12,177	1.50%	1.97%	1.70%
Västerbotten	Malå	Storliden (Zinc/copper)	3,109	4.93%	6.10%	7.64%
Västerbotten	Norsjö	Maurliden (Copper/zinc) and Maurliden Ö (Copper/zinc)	4,176	2.92%	2.68%	4.69%
Västerbotten	Skellefteå	Björkdal (Gold) and Renström (Copper/zinc)	72,031	1.82%	1.88%	2.61%
Västerbotten	Sorsele	Blaiken (Zinc)	2,516	0.52%	1.37%	0.99%
Västerbotten	Storuman	Svartliden (Gold) and Blaiken (Zinc)	5,943	0.75%	0.80%	1.07%
Örebro	Askersund	Zinkgruvan (Zinc)	11,151	7.24%	7.39%	7.75%
Dalarna	Hedemora	Garpenberg (Zinc)	15,235	3.24%	3.35%	4.40%

Notes: Information from Statistics Sweden, Nordregio (2009), SGU (2014), Tano et al. (2016), and SGU (2021). Following Tano et al. (2016), municipalities are considered if they had an operating mine during the mining boom ranging from 2004 to 2010. Only individuals located in Norrbotten County are included in the paper, either as treated or control. Employment in the mining sector via the Swedish Standard Industrial (SNI) Classification 2002 includes the codes 10100, 10200, 10301, 10302, 12000, 13100, 13200, 14110, 14120, 14130, 14210, 14220, 14300, 14400, 14500, 29520, and 51820.

Table A.2: Summary statistics, 2000-2003 and 2004-2015

	Control 2000-2003 Mean SD	Treated 2000-2003 Mean SD	Total 2000-2003 Mean SD	Control 2004-2015 Mean SD	Treated 2004-2015 Mean SD	Total 2004-2015 Mean SD
Convicted property crime	0.012	0.016	0.013	0.014	0.012	0.014
	(0.108)	(0.125)	(0.111)	(0.119)	(0.108)	(0.117)
Convicted violent crime	0.006	0.006	0.006	0.008	0.007	0.007
	(0.078)	(0.075)	(0.077)	(0.086)	(0.082)	(0.086)
Convicted substance crimes	0.002	0.003	0.003	0.011	0.010	0.011
	(0.049)	(0.055)	(0.050)	(0.104)	(0.100)	(0.104)
Convicted traffic crimes	0.009	0.013	0.010	0.011	0.011	0.011
	(0.096)	(0.111)	(0.098)	(0.105)	(0.105)	(0.105)
Age	23.595	23.532	23.585	23.117	23.086	23.112
	(3.564)	(3.646)	(3.577)	(3.427)	(3.409)	(3.424)
Disposable income	107.236	110.195	107.687	122.236	160.612	128.817
	(197.649)	(62.286)	(183.584)	(83.022)	(99.422)	(87.264)
Labor income	112.798	124.394	114.565	130.981	195.209	141.995
	(100.580)	(102.910)	(101.024)	(117.406)	(142.861)	(124.523)
Married	0.046	0.035	0.044	0.036	0.042	0.037
	(0.209)	(0.184)	(0.206)	(0.187)	(0.202)	(0.190)
Children under 18	0.267	0.292	0.271	0.261	0.283	0.265
	(0.442)	(0.455)	(0.444)	(0.439)	(0.450)	(0.441)
Primary education	0.375	0.468	0.389	0.307	0.336	0.312
	(0.484)	(0.499)	(0.488)	(0.461)	(0.472)	(0.463)
Secondary education	0.559	0.501	0.550	0.638	0.641	0.638
	(0.497)	(0.500)	(0.497)	(0.481)	(0.480)	(0.480)
Tertiary education	0.066	0.031	0.061	0.055	0.023	0.049
	(0.249)	(0.174)	(0.239)	(0.228)	(0.151)	(0.217)
Non-employed	0.384	0.342	0.378	0.350	0.218	0.327
	(0.486)	(0.475)	(0.485)	(0.477)	(0.413)	(0.469)
Primary sector	0.019	0.135	0.037	0.027	0.203	0.057
	(0.137)	(0.342)	(0.188)	(0.161)	(0.402)	(0.232)
Secondary sector	0.219	0.191	0.214	0.239	0.251	0.241
	(0.413)	(0.393)	(0.410)	(0.427)	(0.434)	(0.428)
Tertiary sector	0.378	0.331	0.371	0.384	0.329	0.375
	(0.485)	(0.471)	(0.483)	(0.486)	(0.470)	(0.484)
Observations (N x T)	46988	8450	55438	145027	30015	175042
N	11747	2112	13860	12086	2501	14587

Notes: The table shows means and standard deviations for the treated (Gällivare/Kiruna) and control (other Norrbotten County) groups separately for the pre-boom (2000–2003) and post-boom (2004–2015) periods. Economic sector follows SNI 2002: primary = extraction/agriculture; secondary = manufacturing/construction; tertiary = services, healthcare, public sector, and other. Marital status is a dummy variable equal to 1 if married. Education is categorized as primary, secondary, and tertiary. Sample: young male residents aged 18–29, migrants excluded. Data from the Swedish Conviction Register (Brå) and administrative registers, Statistics Sweden (LISA).

Table A.3: Mean differences of changes (2000-2003) comparing treated and control individuals

	Treated	Control	Difference	p-value
Convicted property crime	0.0028	0.0000	-0.0028	0.515
Convicted violent crime	0.0000	-0.0005	-0.0005	0.875
Convicted substance crime	0.0028	0.0031	0.0002	0.925
Convicted traffic crime	-0.0007	-0.0013	-0.0006	0.885
Married	0.0255	0.0322	0.0066	0.215
Children under 18	-0.0362	-0.0412	-0.0050	0.716
Primary education	-0.1387	-0.1057	0.0329***	0.000
Secondary education	0.1137	0.0456	-0.0681***	0.000
Tertiary education	0.0250	0.0602	0.0352***	0.000
Non-employed	-0.1539	-0.1351	0.0188	0.217
Primary sector	0.0525	-0.0008	-0.0532***	0.000
Secondary sector	0.0028	0.0329	0.0301***	0.008
Tertiary sector	0.0986	0.1029	0.0043	0.771

Notes: The table shows the change in each individual characteristic between 2000 and 2003 for treated and control individuals, used to assess pre-boom parallel trends in observable characteristics. Stars indicate the statistical significance of the difference in these pre-boom changes between groups. Marital status is a dummy variable equal to 1 if married. Education is categorized as primary, secondary, and tertiary. The economic sectors are divided into primary (extraction and agricultural sector), secondary (manufacturing and construction), and tertiary (services, healthcare, public sector, and others). Data from the Swedish Conviction Register (Brå) and administrative registers, Statistics Sweden (LISA). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.4: Impact of the mining boom on property crimes by offense type, 2000-2015

	(1) Theft crimes	(2) Fraud crimes	(3) Embezzlement crime	(4) Crimes to creditors	(5) Vandalism crime
Post*Treated	-0.0023	-0.0007	-0.0027**	-0.0000	-0.0025*
	(0.0019)	(0.0009)	(0.0011)	(0.0006)	(0.0015)
Individual FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Grid FE	Yes	Yes	Yes	Yes	Yes
Observations (N x T)	230480	230480	230480	230480	230480
N	14405	14405	14405	14405	14405
Mean dep. var (2000-03)	0.0067	0.0024	0.0016	0.0008	0.0031
Effect relative to the mean (%)	-33.93	-29.66	-170.07	-2.78	-78.45
R-squared	0.2924	0.2122	0.1685	0.1561	0.1945
Within R-squared	0.0000	0.0000	0.0001	0.0000	0.0000

Notes: The table disaggregates the property crime result from Table 1 into five legal subcategories: theft and robbery (Ch. 8), fraud (Ch. 9), embezzlement and breach of trust (Ch. 10), offenses against creditors (Ch. 11), and property damage/vandalism (Ch. 12). Each outcome is a binary conviction indicator. Design and sample identical to Table 1 (14,405 individuals, 230,480 person-years). Standard errors (in parentheses) clustered at the grid level. Data from the Swedish Conviction Register (Brå) and administrative registers, Statistics Sweden (LISA). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.5: Impact of the mining boom on violent crimes by offense type, 2000-2015

	(1) Crimes against life and health	(2) Crimes against liberty and peace	(3) Crimes against family and Defamation	(4) Sexual offenses
Post*Treated	0.0016	0.0013	-0.0000	0.0007**
	(0.0014)	(0.0010)	(0.0001)	(0.0003)
Individual FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Grid FE	Yes	Yes	Yes	Yes
Observations (N x T)	230480	230480	230480	230480
N	14405	14405	14405	14405
Mean dep. var (2000-03)	0.0045	0.0023	0.0002	0.0002
Effect relative to the mean (%)	35.85	57.62	-26.66	339.59
R-squared	0.2229	0.2083	0.1715	0.1670
Within R-squared	0.0000	0.0000	0.0000	0.0000

Notes: The table disaggregates the violent crime result from Table 1 into five legal subcategories of the Swedish Penal Code (Brottsbalken), grouped into four columns: crimes against life and health (Ch. 3; assault, aggravated assault, manslaughter), crimes against liberty and peace (Ch. 4; unlawful threats, coercion, harassment, stalking), crimes against family and defamation (Ch. 7 and Ch. 5, combined due to their low base rates), and sexual offenses (Ch. 6; rape, sexual assault, sexual harassment). Each outcome is a binary conviction indicator. Design and sample identical to Table 1 (14,405 individuals, 230,480 person-years). Standard errors (in parentheses) clustered at the grid level. Data from the Swedish Conviction Register (Brå) and administrative registers, Statistics Sweden (LISA). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.6: Impact of the mining boom on narcotics-related suspicions by offense type, 2000-2015

	(1) Narcotic suspicion	(2) Narcotic production	(3) Narcotic distribution	(4) Narcotic use-holding
Post*Treated	0.0055**	0.0009**	0.0014*	0.0049**
	(0.0023)	(0.0004)	(0.0007)	(0.0023)
Individual FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Grid FE	Yes	Yes	Yes	Yes
Observations (N x T)	230480	230480	230480	230480
N	14405	14405	14405	14405
Mean dep. var (2000-03)	0.0045	0.0000	0.0006	0.0017
Effect relative to the mean (%)	122.92	2603.78	209.61	290.00
R-squared	0.4056	0.1691	0.2194	0.4025
Within R-squared	0.0000	0.0000	0.0000	0.0000

Notes: The table disaggregates narcotics-related criminal activity using data from the Crime Suspicion Register (BRÅ) rather than the conviction records used in the main analysis. Suspicion data capture police-recorded incidents regardless of prosecution outcome and allow decomposition into: any narcotics suspicion (Col. 1), production (Col. 2), distribution/selling (Col. 3), and holding/use (Col. 4). Base rates for production and distribution are very low; estimates should be interpreted with caution. Design and sample identical to Table 1. Standard errors (in parentheses) clustered at the grid level. Data from the BRÅ Crime Suspicion Register and administrative registers, Statistics Sweden. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.7: Impact of the mining boom on criminal behavior by distance to the mines, 2000-2015

	(1) Property crime	(2) Violent crime	(3) Traffic crime	(4) Substance crime
Post*≤ 20 km	-0.0076***	0.0015	-0.0018	0.0055***
	(0.0029)	(0.0017)	(0.0021)	(0.0020)
Post* 20 - 40 km	-0.0019	-0.0002	-0.0046	-0.0072*
	(0.0057)	(0.0036)	(0.0116)	(0.0042)
Post*40 - 60 km	0.0078	0.0090	-0.0024	0.0000
	(0.0089)	(0.0060)	(0.0125)	(0.0015)
Post*60 - 80 km	0.0007	0.0027	-0.0046	-0.0008
	(0.0051)	(0.0039)	(0.0078)	(0.0017)
Individual FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Grid FE	Yes	Yes	Yes	Yes
Observations (N x T)	230480	230480	230480	230480
N	14405	14405	14405	14405
Mean dep. var (2000-03)	0.0125	0.0060	0.0097	0.0025
R-squared	0.2929	0.2195	0.2708	0.3759
Within R-squared	0.0001	0.0000	0.0000	0.0001

Notes: The table reports the coefficient estimates underlying Figure A.7. Treatment is defined by 20-kilometer distance rings from the nearest mine; residents beyond 80 km are the reference category. Crime definitions are in Table 1. Design and sample identical to Table 1 (14,405 individuals, 230,480 person-years). Standard errors (in parentheses) clustered at the grid level. Data from the Swedish Conviction Register (Brå) and administrative registers, Statistics Sweden (LISA). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.8: Impact of the mining boom on substance crime by pre-boom property crime history, 2000-2015

	(1) Had property crime pre	(2) No property crime pre
Post*Treated	0.0230	0.0036**
	(0.0252)	(0.0015)
Individual FE	Yes	Yes
Year FE	Yes	Yes
Grid FE	Yes	Yes
Observations (N x T)	5080	225400
N	318	14088
Mean dep. var (2000-03)	0.0471	0.0008
Effect relative to the mean (%)	48.78	432.42
R-squared	0.4931	0.3667
Within R-squared	0.0003	0.0000

Notes: The table tests whether the increase in substance crime reflects crime switching by the same individuals who reduced property crime. The sample is split by pre-boom property crime history: Column (1) includes individuals with at least one property crime conviction in 2000–2003 (N = 318); Column (2) includes those with no pre-boom property crime convictions (N = 14,088). If switching drives the results, the substance crime increase should be concentrated in Column (1). The small sample in Column (1) limits statistical power. Design otherwise identical to Table 1. Standard errors (in parentheses) clustered at the grid level. Data from the Swedish Conviction Register (Brå) and administrative registers, Statistics Sweden (LISA). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.9: Impact of the mining boom on criminal behavior by mining sector transition, 2000-2015

	(1) Property crime (entrants)	(2) Property crime (non-entrants)	(3) Substance crime (entrants)	(4) Substance crime (non-entrants)
Post*Treated	-0.0107**	-0.0045	0.0000	0.0057**
	(0.0048)	(0.0027)	(0.0019)	(0.0022)
Individual FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Grid FE	Yes	Yes	Yes	Yes
Observations (N x T)	196544	225951	196544	225951
N	12284	14122	12284	14122
Mean dep. var (2000-03)	0.0121	0.0124	0.0024	0.0026
Effect relative to the mean (%)	-88.47	-36.06	0.95	216.20
R-squared	0.2959	0.2947	0.3782	0.3769
Within R-squared	0.0000	0.0000	0.0000	0.0001

Notes: The table splits treated individuals by whether they newly entered the mining sector after the boom. Entrants (Cols 1, 3) were not in mining in 2003 but worked in mining in at least one year during 2004–2015. Non-entrants (Cols 2, 4) did not newly enter mining post-2004, a group that includes those already in mining before the boom. The full control group is included in all columns. Because sector entry after 2004 is endogenous to the boom, these estimates are descriptive and should not be interpreted causally. Design otherwise identical to Table 1. Standard errors (in parentheses) clustered at the grid level. Data from the Swedish Conviction Register (Brå) and administrative registers, Statistics Sweden (LISA). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.10: Summary statistics of residents and migrants, 2000-2003 and 2004-2015

	Residents 2000-2003	Residents 2004-2015	Migrants (county) 2000-2003	Migrants (county) post-migration-2015	Migrants (Treated mun.) 2000-2003	Migrants (Treated mun.) post-migration-2015
	Mean/SD	Mean/SD	Mean/SD	Mean/SD	Mean/SD	Mean/SD
Convicted of property crime	0.01	0.01	0.07	0.04	0.01	0.01
	(0.10)	(0.09)	(0.25)	(0.19)	(0.11)	(0.11)
Convicted of violent crime	0.00	0.01	0.02	0.01	0.00	0.00
	(0.07)	(0.08)	(0.15)	(0.11)	(0.06)	(0.07)
Convicted of traffic crime	0.01	0.01	0.05	0.03	0.02	0.01
	(0.10)	(0.10)	(0.22)	(0.16)	(0.13)	(0.09)
Convicted of substance crime	0.00	0.01	0.05	0.04	0.00	0.01
	(0.04)	(0.08)	(0.22)	(0.19)	(0.05)	(0.10)
Disposable income(100SEK)	1439.84	1886.59	1080.36	1372.35	1015.39	1907.15
	(792.69)	(1244.01)	(3148.50)	(1321.92)	(1096.85)	(1059.97)
Yearly earnings(100SEK)	1701.11	2278.91	978.01	1367.14	1000.87	2281.41
	(1172.99)	(1453.96)	(1317.49)	(1521.95)	(954.14)	(1487.67)
Employment	0.77	0.84	0.51	0.60	0.59	0.82
	(0.42)	(0.37)	(0.50)	(0.49)	(0.49)	(0.38)
Age	30.13	28.26	27.31	27.57	24.85	29.03
	(6.44)	(6.65)	(6.20)	(5.53)	(5.78)	(5.44)
Married	0.15	0.13	0.11	0.12	0.06	0.14
	(0.36)	(0.33)	(0.31)	(0.33)	(0.23)	(0.34)
Children under 18	0.41	0.37	0.24	0.23	0.30	0.28
	(0.49)	(0.48)	(0.43)	(0.42)	(0.46)	(0.45)
Primary education	0.60	0.39	0.47	0.26	0.34	0.22
	(0.49)	(0.49)	(0.50)	(0.44)	(0.47)	(0.41)
Secondary education	0.35	0.55	0.44	0.52	0.60	0.53
	(0.48)	(0.50)	(0.50)	(0.50)	(0.49)	(0.50)
Tertiary education	0.05	0.06	0.10	0.22	0.06	0.25
	(0.22)	(0.23)	(0.29)	(0.42)	(0.23)	(0.43)
Non-employed	0.23	0.16	0.49	0.40	0.41	0.18

	(0.42)	(0.37)	(0.50)	(0.49)	(0.49)	(0.38)
Primary economic sector	0.21	0.24	0.01	0.02	0.04	0.17
	(0.40)	(0.43)	(0.10)	(0.13)	(0.21)	(0.38)
Secondary economic sector	0.19	0.23	0.14	0.13	0.12	0.17
	(0.39)	(0.42)	(0.35)	(0.34)	(0.32)	(0.38)
Tertiary economic sector	0.37	0.36	0.36	0.45	0.43	0.48
	(0.48)	(0.48)	(0.48)	(0.50)	(0.49)	(0.50)

Notes: The table compares demographic and labor market characteristics across three groups: residents in Gällivare/Kiruna (Cols 1–2), all migrants to Norrbotten County in 2004 or later (Cols 3–4), and migrants to the mining municipalities specifically (Cols 5–6). For residents, pre/post refers to 2000–2003 and 2004–2015; for migrants, pre/post refers to before and after the move. Unlike the main analysis, this table uses the full 18–39-year-old male sample to provide a broader picture of migration patterns. Migrants are assumed to have moved in response to the mining boom's improved labor market conditions. Marital status is a dummy variable equal to 1 if married. Education is categorized as primary, secondary, and tertiary. The economic sectors are divided into primary (extraction and agricultural sector), secondary (manufacturing and construction), and tertiary (services, healthcare, public sector, and others). Data from the Swedish Conviction Register (Brå) and administrative registers, Statistics Sweden (LISA).

Table A.11: Impact of the mining boom on criminal behavior by rings, 2000-2015

	(1) Property crime	(2) Violent crime	(3) Traffic crime	(4) Substance crime
Post*Ring 1	-0.0105	0.0009	-0.0062	0.0091*
	(0.0086)	(0.0032)	(0.0042)	(0.0049)
Post*Ring 2	-0.0106**	-0.0019	-0.0046	0.0041
	(0.0045)	(0.0049)	(0.0055)	(0.0046)
Post*Ring 3	0.0037	0.0068*	0.0019	0.0061***
	(0.0042)	(0.0039)	(0.0039)	(0.0021)
Post*Ring 4	-0.0097	0.0037	0.0017	0.0050
	(0.0060)	(0.0042)	(0.0033)	(0.0041)
Post*Ring 5	-0.0070	-0.0045	-0.0055	0.0053
	(0.0046)	(0.0033)	(0.0051)	(0.0058)
Post*Ring 6	-0.0148**	0.0038	0.0034	0.0018
	(0.0069)	(0.0025)	(0.0045)	(0.0033)
Post*Ring 7	0.0019	0.0054*	-0.0028	-0.0032**
	(0.0040)	(0.0029)	(0.0074)	(0.0016)
Post*Ring 8	-0.0029	0.0015	-0.0004	0.0009
	(0.0058)	(0.0035)	(0.0055)	(0.0021)
Post*Ring 9	-0.0010	-0.0042	-0.0055	-0.0004
	(0.0057)	(0.0041)	(0.0063)	(0.0017)
Individual FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Grid FE	Yes	Yes	Yes	Yes
Observations (N x T)	230480	230480	230480	230480
N	14405	14405	14405	14405
Mean dep. var (2000-03)	0.0125	0.0060	0.0097	0.0025
R-squared	0.2929	0.2195	0.2708	0.3759
Within R-squared	0.0001	0.0001	0.0000	0.0001

Notes: The table replicates the distance analysis using ten granular rings based on the actual distance distribution rather than fixed 20-kilometer intervals. Ring boundaries (km): Ring 1: 0.00–2.74; Ring 2: 2.75–3.37; Ring 3: 3.38–3.82; Ring 4: 3.83–4.26; Ring 5: 4.27–4.86; Ring 6: 4.87–18.34; Ring 7: 18.35–73.67; Ring 8: 73.68–102.47; Ring 9: 102.48–125.37; Ring 10: 125.38–236.00 km (reference). Design and sample identical to Table 1. Standard errors (in parentheses) clustered at the grid level. Data from the Swedish Conviction Register (Brå) and administrative registers, Statistics Sweden (LISA). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.12: Impact of the mining boom on criminal behavior using time duration for treatment, 2000-2015

	(1) Property crime	(2) Violent crime	(3) Traffic crime	(4) Substance crime
Post*≤ 20 min	-0.0128***	0.0038	-0.0048	0.0083**
	(0.0046)	(0.0026)	(0.0031)	(0.0035)
Post* 20 - 40 min	0.0056	-0.0008	-0.0025	-0.0061
	(0.0070)	(0.0042)	(0.0120)	(0.0048)
Post*40 - 60 min	-0.0027	-0.0002	-0.0003	0.0029
	(0.0029)	(0.0021)	(0.0026)	(0.0021)
Post*60 - 80 min	0.0025	0.0076	-0.0130	-0.0053***
	(0.0097)	(0.0086)	(0.0166)	(0.0017)
Individual FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Grid FE	Yes	Yes	Yes	Yes
Observations (N x T)	230480	230480	230480	230480
N	14405	14405	14405	14405
Mean dep. var (2000-03)	0.0125	0.0060	0.0097	0.0025
R-squared	0.2929	0.2195	0.2708	0.3759
Within R-squared	0.0001	0.0000	0.0000	0.0001

Notes: The table replicates Table A.7 using driving time by car to the nearest mine (in minutes) as the spatial treatment measure, rather than straight-line distance in kilometers. Rings are defined in 20-minute intervals: ≤20, 20–40, 40–60, and 60–80 minutes, with those beyond 80 minutes as the reference. Design and sample identical to Table 1 (14,405 individuals, 230,480 person-years). Standard errors (in parentheses) clustered at the grid level. Data from the Swedish Conviction Register (Brå) and administrative registers, Statistics Sweden (LISA). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.13: Robustness: alternative control groups, 2000–2015

	SCM-weighted controls		Excl. largest cities		Excl. Pajala	
	(1) Property crime	(2) Substance crime	(3) Property crime	(4) Substance crime	(5) Property crime	(6) Substance crime
Post*Treated	-0.0001	0.0059***	-0.0054**	0.0058***	-0.0065**	0.0046**
	(0.0031)	(0.0021)	(0.0026)	(0.0017)	(0.0026)	(0.0018)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Grid FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations (N x T)	93177	93177	2034065	2034065	221129	221129
N	5912	5912	128670	128670	14077	14077
Mean dep. var (2000-03)	0.0139	0.0038	0.0119	0.0062	0.0126	0.0026
Effect relative to the mean (%)	-0.68	158.36	-45.31	94.30	-51.78	178.04
R-squared	0.2983	0.3943	0.2918	0.3756	0.2934	0.3765
Within R-squared	0.0000	0.0001	0.0000	0.0000	0.0001	0.0000

Notes: Each column pair presents a robustness variant of the main result in Table 1, using an alternative control group definition. The sample is restricted to 18–29-year-old male residents, excluding migrants who arrived after 2003. The specification includes individual, year, and grid fixed effects; standard errors (in parentheses) are clustered at the grid level. Crime definitions as in Table 1. Data from the Swedish Conviction Register (Brå) and administrative registers, Statistics Sweden (LISA). Cols. (1)–(2): Control group restricted to municipalities that received positive weights in the synthetic control analysis (Appendix Figure B.9), drawn from across Sweden: Mullsjö, Bjuv, Laxå, Hällefors, Åre, Dorotea, Arjeplog, Överkalix, Pajala, and Älvsbyn. The reduced sample size (93,177 observations) reflects the small number of SCM-weighted donor units. Cols. (3)–(4): Control group consists of all Swedish municipalities with a 2003 population between 15,000 and 35,000 residents, comparable to Gällivare (19,251) and Kiruna (23,407). Cols. (5)–(6): Control group excludes Pajala municipality because the Kaunisvaara iron ore mine opened there in 2012, which could introduce a confounding treatment through the same mechanism studied here. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.14: Robustness checks: impact of the mining boom on criminal behavior, 2000-2015

	(1) Baseline Residents	(2) Residents (treated 2003)	(3) Including controls	(4) Residents and migrants	(5) Balanced panel	(6) Exclude neigh. municipalities	(7) Municipality fixed-effect
Panel A: Property crime							
Post*Treated	-0.0066**		-0.0060**	-0.0052**	-0.0061*	-0.0074***	-0.0061**
	(0.0027)		(0.0027)	(0.0024)	(0.0037)	(0.0027)	(0.0025)
Post*Treated (2003)		-0.0045*					
		(0.0027)					
Mean dep. var (2003)	0.0125	0.0125	0.0125	0.0127	0.0139	0.0123	0.0125
Effect relative to the mean (%)	-52.40	-35.86	-48.08	-41.41	-44.04	-60.26	-48.45
R-squared	0.2929	0.2991	0.2932	0.3020	0.2800	0.2951	0.2710
Within R-squared	0.0001	0.0000	0.0005	0.0000	0.0001	0.0001	0.0000
Panel B: Violent crime							
Post*Treated	0.0018		0.0019	0.0014	0.0026	0.0019	0.0014
	(0.0016)		(0.0016)	(0.0015)	(0.0019)	(0.0016)	(0.0016)
Post*Treated (2003)		0.0019					
		(0.0017)					
Mean dep. var (2003)	0.0060	0.0060	0.0060	0.0061	0.0066	0.0059	0.0060
Effect relative to the mean (%)	29.18	31.34	31.30	23.53	39.85	31.77	22.53
R-squared	0.2195	0.2084	0.2196	0.2213	0.2104	0.2219	0.1975
Within R-squared	0.0000	0.0000	0.0001	0.0000	0.0000	0.0000	0.0000
Panel C: Traffic crime							
Post*Treated	-0.0017		-0.0020	-0.0026	-0.0037	-0.0024	-0.0016
	(0.0020)		(0.0020)	(0.0018)	(0.0030)	(0.0020)	(0.0019)
Post*Treated (2003)		-0.0010					
		(0.0020)					
Mean dep. var (2003)	0.0097	0.0097	0.0097	0.0097	0.0112	0.0089	0.0097
Effect relative to the mean (%)	-17.06	-10.45	-20.16	-26.61	-32.71	-27.19	-16.16
R-squared	0.2708	0.2815	0.2711	0.2735	0.2697	0.2744	0.2480
Within R-squared	0.0000	0.0000	0.0004	0.0000	0.0000	0.0000	0.0000

Panel D: Substance crime							
Post*Treated	0.0046**		0.0048***	0.0042**	0.0019	0.0047**	0.0039**
	(0.0018)		(0.0018)	(0.0017)	(0.0020)	(0.0018)	(0.0017)
Post*Treated (2003)		0.0046**					
		(0.0018)					
Mean dep. var (2003)	0.0025	0.0025	0.0025	0.0027	0.0028	0.0023	0.0025
Effect relative to the mean (%)	181.24	179.33	188.73	154.33	68.23	202.28	154.28
R-squared	0.3759	0.3560	0.3764	0.3769	0.3662	0.3765	0.3600
Within R-squared	0.0000	0.0001	0.0009	0.0000	0.0000	0.0001	0.0000
Observations (N x T)	230480	124267	230480	263626	49698	193151	230480
N	14405	7767	14405	16477	3106	12072	14405
Controls	No	No	Yes	No	No	No	No
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Grid FE	Yes	Yes	Yes	Yes	Yes	Yes	No
Municipality FE	No	No	No	No	No	No	Yes

Notes: Each column presents a robustness variant of the main result in Table 1, organized in four panels by crime type. Col. (1): Baseline (= Table 1). Col. (2): Treatment status assigned using the 2003 municipality. Col. (3): Adds time-varying individual controls (marital status, children under 18, education, economic sector); controls are excluded from the main specification because some may be endogenous to the boom. Col. (4): Includes migrants alongside residents (16,477 individuals). Col. (5): Balanced panel — individuals observed in all 16 years (3,106 individuals). Col. (6): Excludes the four Norrbotten municipalities nearest to treated areas (Jokkmokk, Pajala, Överkalix, Boden) to address spillover concerns. Col. (7): Replaces grid fixed effects with municipality fixed effects. Standard errors (in parentheses) clustered at the grid level. Crime definitions as in Table 1. Data from the Swedish Conviction Register (Brå) and administrative registers, Statistics Sweden (LISA). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.15: Mechanisms: impact of the mining boom on wages and work intensity, 2000-2015

	(1) Monthly wage	(2) Log monthly wage	(3) Work scope overall	(4) Work scope mining sec.
Post*Treated	933.0368***	0.0342***	-2.3489***	-0.1315
	(153.2177)	(0.0074)	(0.9019)	(0.5263)
Individual FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Grid FE	Yes	Yes	Yes	Yes
Observations (N x T)	52020	52020	52020	1689
N	3251	3251	3251	106
Mean dep. var (2000-03)	18065.5247	9.7804	89.7336	99.6585
Effect relative to the mean (%)	5.16	0.35	-2.62	-0.13
R-squared	0.8184	0.8300	0.6649	0.4634
Within R-squared	0.0015	0.0011	0.0003	0.0000

Notes: The table reports DID estimates using employer-reported data from the Lönestrukturstatistik (LSS), Statistics Sweden's Structure of Earnings Survey. Monthly wage (Col. 1) and log monthly wage (Col. 2) are full-time equivalent monthly salary, capturing the wage rate independently of working-time arrangements. Work scope (Col. 3) is contracted working time as a percentage of full-time (0–100), and Col. 4 uses the work scope for mining workers in 2003 as the outcome; a positive effect would indicate increased work intensity. Because the LSS targets enterprises with ten or more employees, the matched sample is smaller than the main analysis (3,251 individuals, 52,020 person-years). Design otherwise identical to Table 1. Standard errors (in parentheses) clustered at the grid level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.16: Mechanisms: impact of the mining boom on labor market outcomes using IV, 2000-2015

	(1) First stage	(2) Property crime	(3) Substance crime
Post*Treated	28.60434***		
	(3.32094)		
Labor income		-0.00015*	0.00013*
		(0.00009)	(0.00007)
Individual FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Grid FE	Yes	Yes	Yes
Observations (N x T)	205342	209232	209232
N	12834	13077	13077
Mean dep. var (2000-03)	114.5652	0.0125	0.0025
Kleibergen-Paap F		117.98	117.98

Notes: The table reports two-stage least squares (2SLS) estimates of the effect of labor income on the probability of criminal conviction among young male residents aged 18–29, comparing Gällivare and Kiruna (treated) with other Norrbotten County municipalities (control) before (2000–2003) and after (2004–2015) the boom. Labor income (in 1000 SEK) is instrumented by the mining boom (Post × Treated); Column 1 reports the first stage, and Columns 2 and 3 report second-stage estimates for property and substance crime, respectively. The Kleibergen-Paap F statistic confirms a strong first stage. Individuals who moved to the area after 2003 are excluded. All columns include individual, year, and grid fixed effects; small differences in N across columns reflect singleton dropping in the fixed-effects estimator. Standard errors (in parentheses) are clustered at the grid level. Data from the Swedish Conviction Register (Brå) and administrative registers, Statistics Sweden (LISA). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

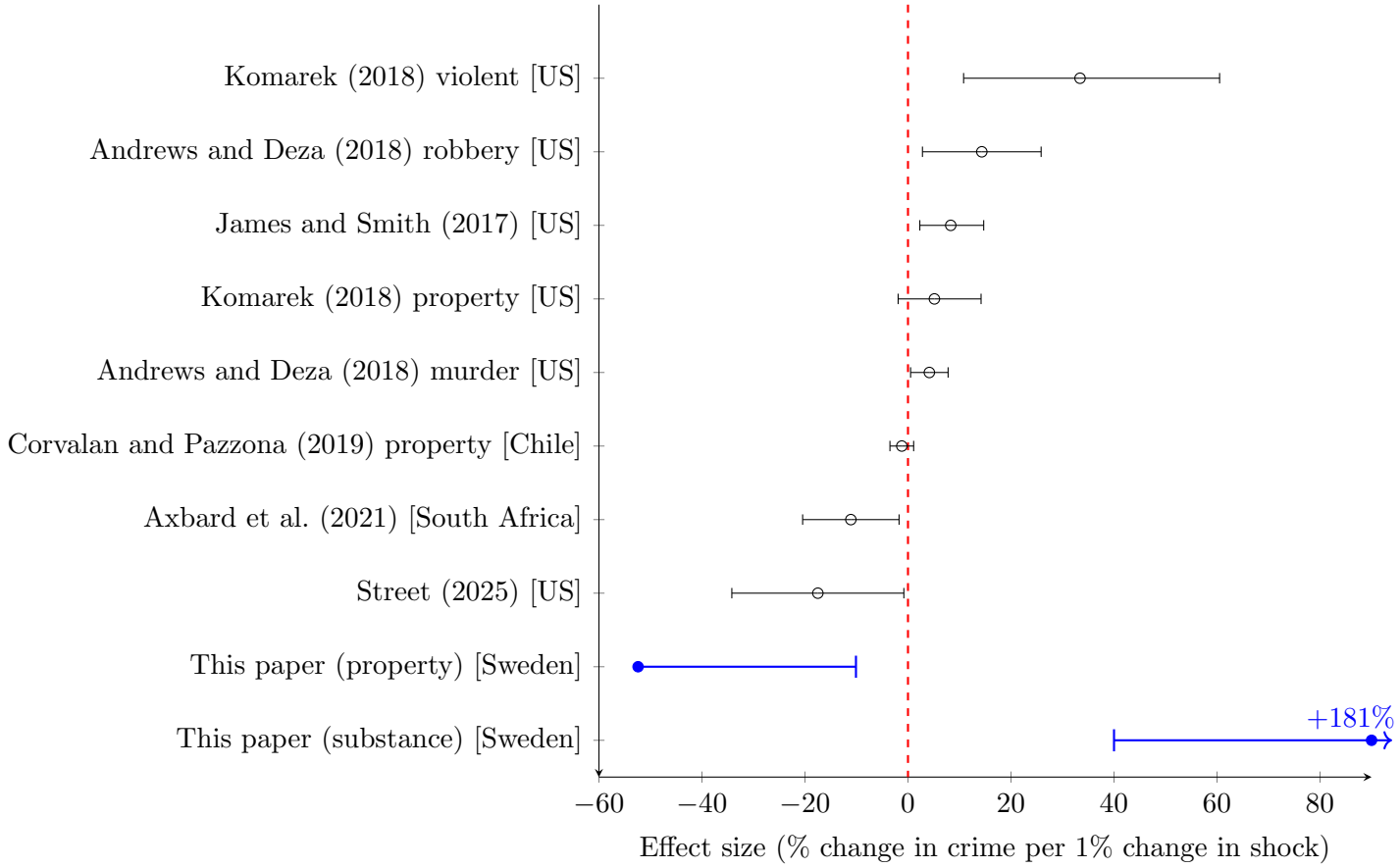
Table A.17: Social cost effects of the mining boom, 18-29-year-old male sample

	Property crime	Violent crime	Traffic crime	Substance crime
	Total social cost effect (1000s SEK)			
Post	-17866.61**	11345.70	-1921.11	14086.18**
	(7223.97)	(10332.34)	(2327.19)	(5473.34)

Notes: The table shows the social costs of effects computed using the DID estimates of the effect of the mining boom on different types of crime for young males. I take the total unit cost for each crime category for the UK using 2015/2016 prices and convert it to SEK using the 2004 exchange rate (1 GBP = 13.45 SEK). The total unit costs for property and violent crimes include the costs for anticipation (e.g., defensive expenditure), consequence (e.g., physical and emotional harm), and response (e.g., police costs) to the crime. The source for the crime costs is Heeks et al. (2018), which includes response costs (police, courts, probation) within each crime category. Incarceration costs are not reported as a separate row because the crime effects are concentrated among convictions without prison sentences. For property crimes, I use the estimated cost of 79,794 SEK for domestic burglary and dwelling. For violent crimes, I use the estimated cost of 189,057 SEK for violence with injury. For the traffic crimes, I use the estimated cost of 33,936 SEK, which accounts for a damage-only accident. For substance crimes, I use the estimated cost of 89,523 SEK, which only accounts for the cost of arrest. I multiply the estimated coefficients of Table 1 by the cost of crime and add it by the number of treated young male individuals during the boom, obtaining the aggregate social cost effect for this population (welfare implication). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

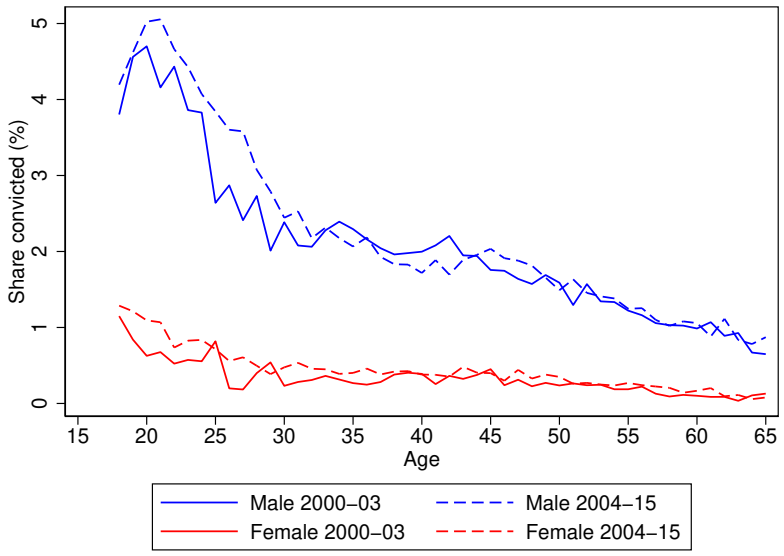
B Appendix: Additional figures

Figure B.1: Literature comparisons: resource shocks and crime



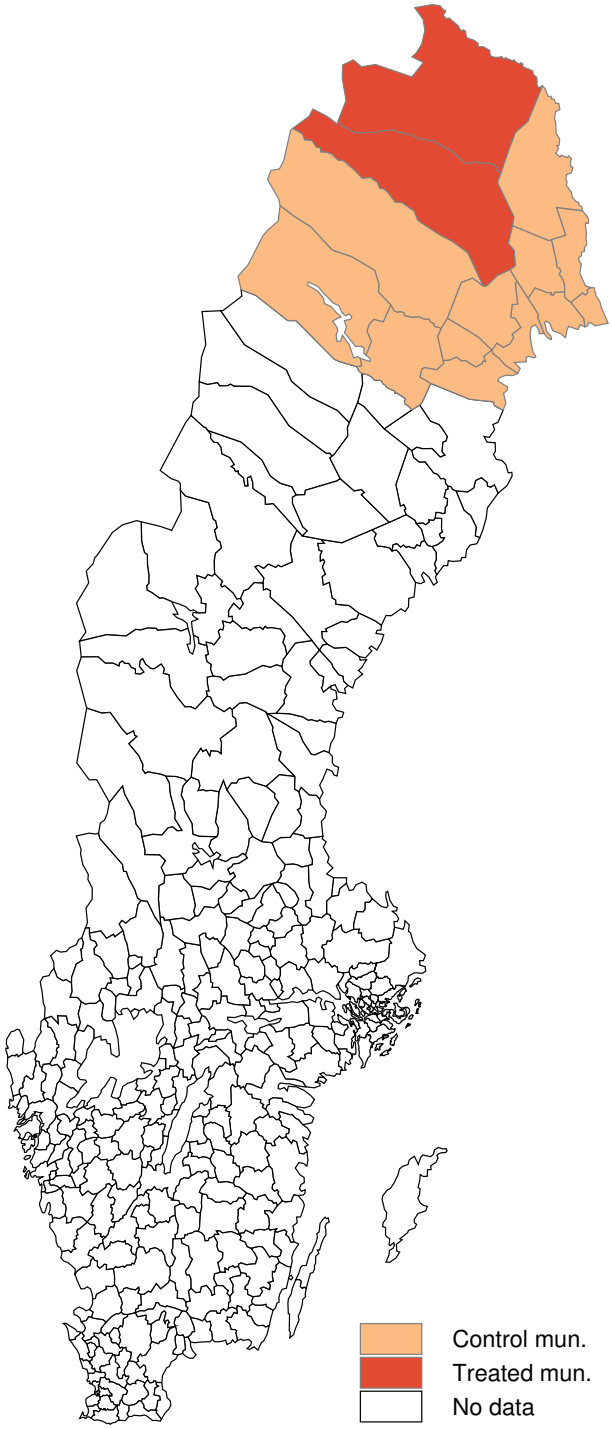
Notes: This figure compares estimated treatment effect sizes in the literature. Each dot shows the estimated effect size (%) with 95% confidence intervals. The figure compares the effect of different resource shocks exposure, such as mining and fracking booms, on criminal behavior. See Appendix E for details on the papers and effect size construction. Each point indicates the estimated effect of treatment (direct percent change) on criminal behavior for treated areas or individuals relative to controls as a percent of the control mean. When not specified, the outcome in the paper is all types of crime. This paper's estimates refer to young males aged 18–29; larger effect sizes relative to studies using broader samples are expected given this group's higher baseline crime rates and greater responsiveness to labor market conditions.

Figure B.2: Conviction rates of any crime by age and gender, before vs after



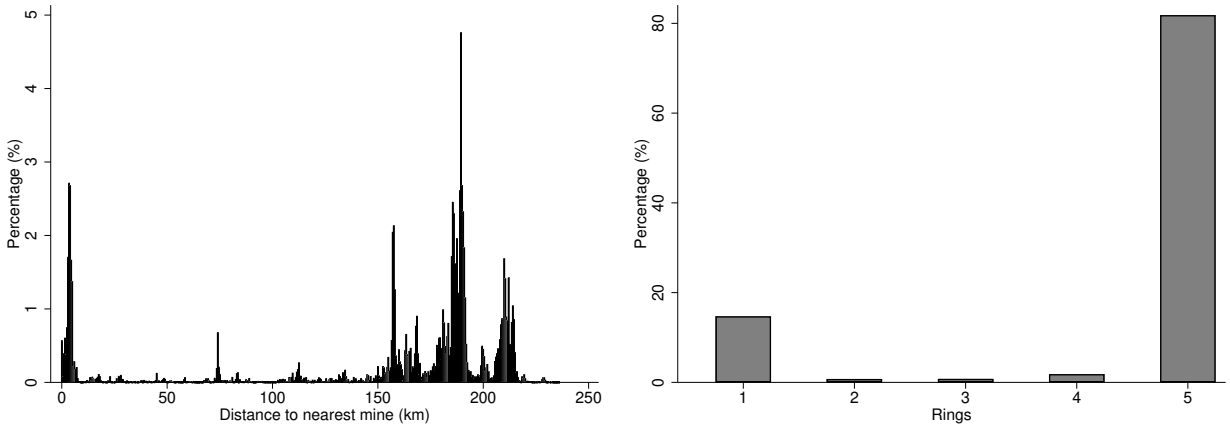
Notes: The figure plots the share of individuals convicted of any crime by single year of age (15–65) and gender, separately for the pre-boom (2000–2003) and post-boom (2004–2015) periods. The figure is descriptive and illustrates the age-crime profile motivating the focus on young males. Migrants are excluded. Data from the Swedish Conviction Register (Brå) and administrative registers, Statistics Sweden (LISA).

Figure B.3: Treated and control municipalities



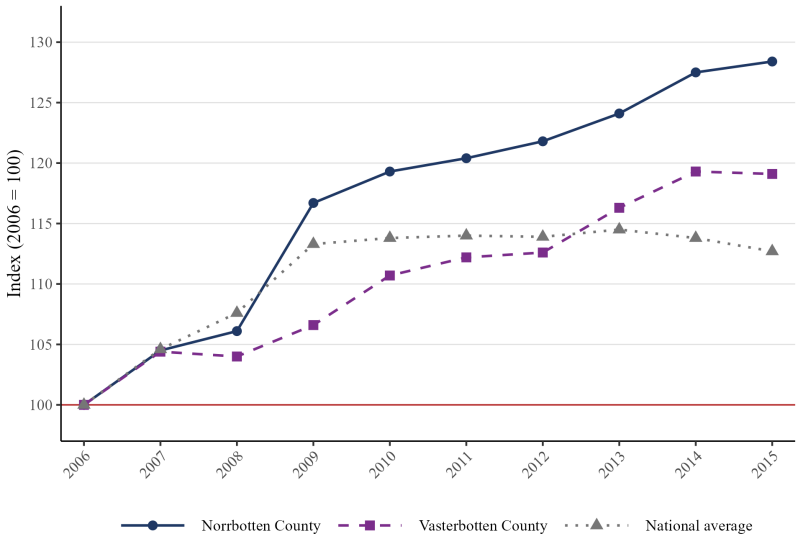
Notes: The map shows the spatial location of treated municipalities (Gällivare and Kiruna, darker shading) and control municipalities (other Norrbotten County municipalities, lighter shading). Municipalities in white are outside Norrbotten County and are excluded from the empirical sample. Data from Statistics Sweden.

Figure B.4: Distribution of individuals according to their distance to the nearest mine and in the rings



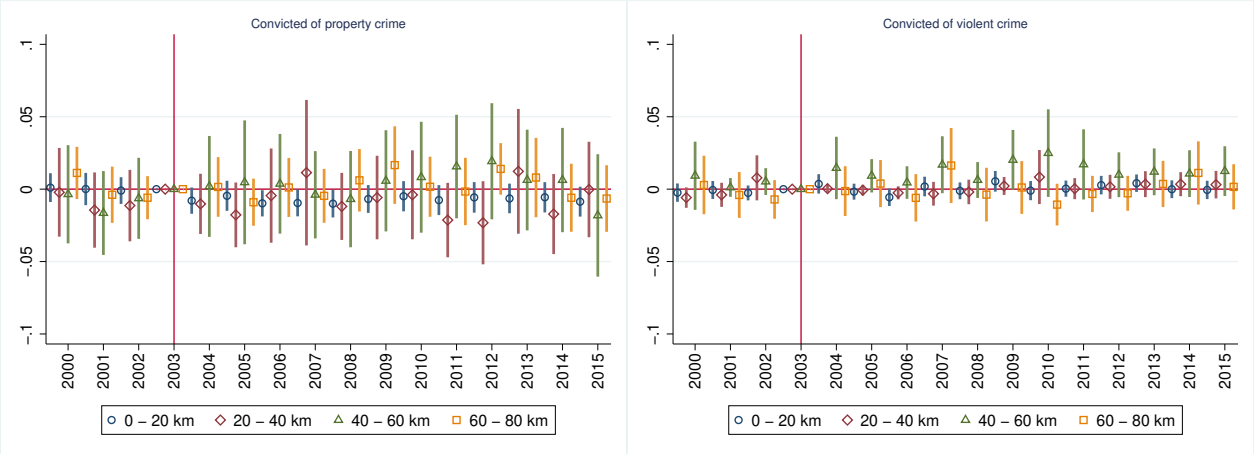
Notes: The left panel shows the distribution of sample individuals by distance (km) to the nearest of the three operating iron ore mines. The right panel shows the percentage in each of the five 20-kilometer rings used in Table A.7 and Figure 3. The majority of treated individuals live within 20 km of the nearest mine. Sample: young male residents aged 18–29, migrants excluded. Data from administrative registers and geocoded grid coordinates, Statistics Sweden.

Figure B.5: Systembolaget alcohol sales per capita by county, 2006–2015



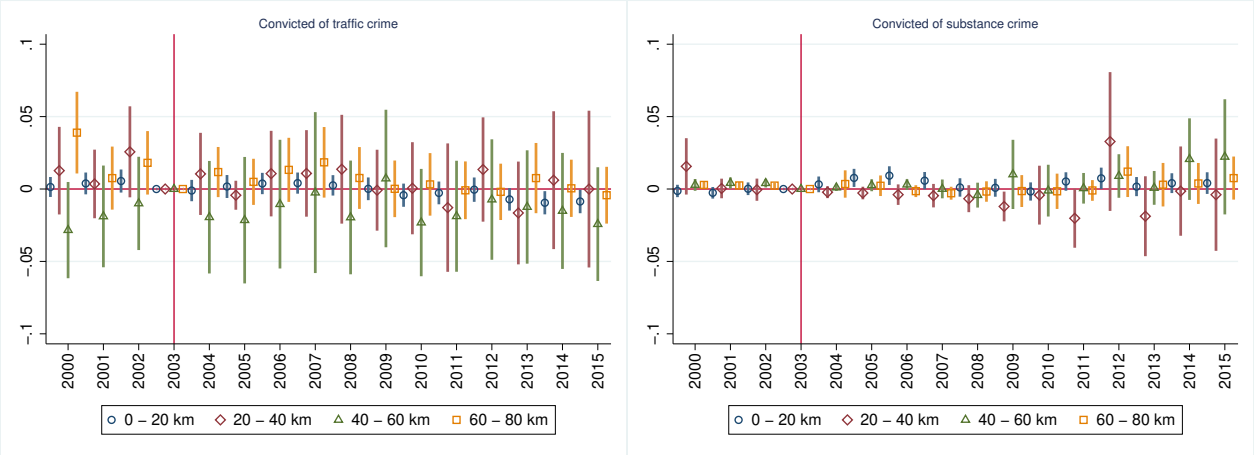
Notes: The figure plots an index of annual alcohol sales per capita (2006 = 100) for Norrbotten County, Västerbotten County, and the national average. For 2006–2009, per capita figures are taken directly from Systembolaget's annual county reports (sum of spirits, wine, cider, and beer; non-alcoholic beverages excluded). For 2010–2015, per capita figures are computed from Systembolaget's quarterly cumulative county volumes divided by Statistics Sweden (SCB) county population estimates. Data are at the county level; Gällivare and Kiruna, the two treated municipalities, are two of fourteen municipalities in Norrbotten County. The 2004 mining boom occurred before the availability of Systembolaget county-level data, so a pre-boom baseline comparison is not available.

Figure B.6: Event study of the impact of the mining boom on criminal behavior by rings, 2000-2015



(a) Property crime

(b) Violent crime

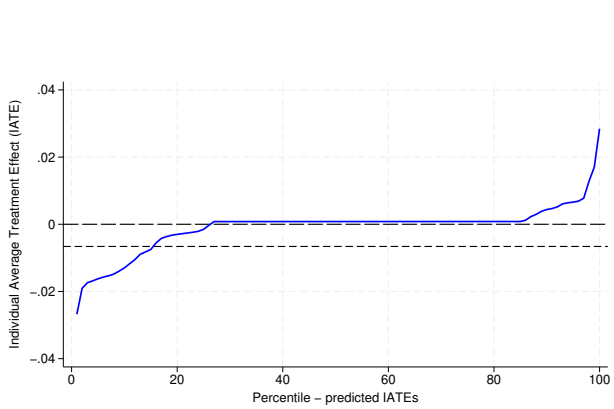


(c) Traffic crime

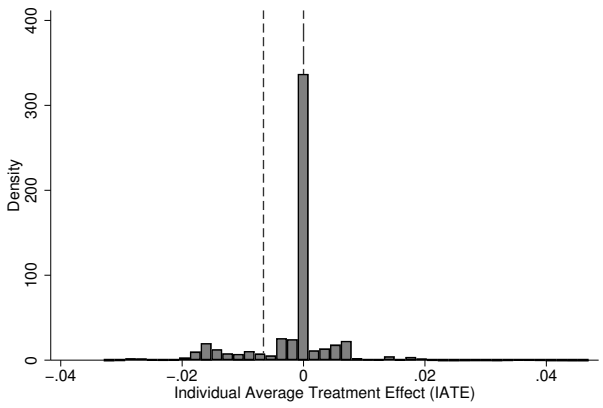
(d) Substance crime

Notes: The figure plots year-by-year DID coefficients from the dynamic model, estimated separately for each 20-kilometer distance ring. Each line traces the annual treatment effect for the indicated ring relative to residents beyond 80 km (reference) and the reference year 2003. 95% confidence interval shown. Pre-2004 coefficients test parallel trends within each ring. Design and sample identical to Table 1. Standard errors clustered at the grid level.

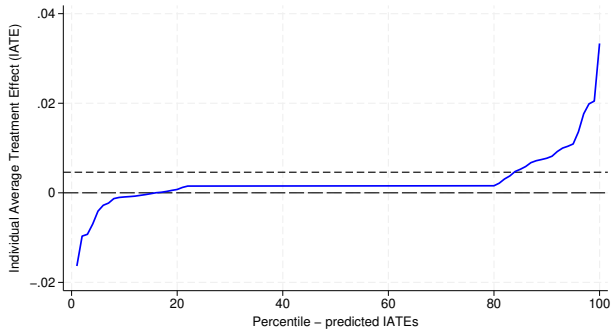
Figure B.7: Distribution predicted individual average treatment effects



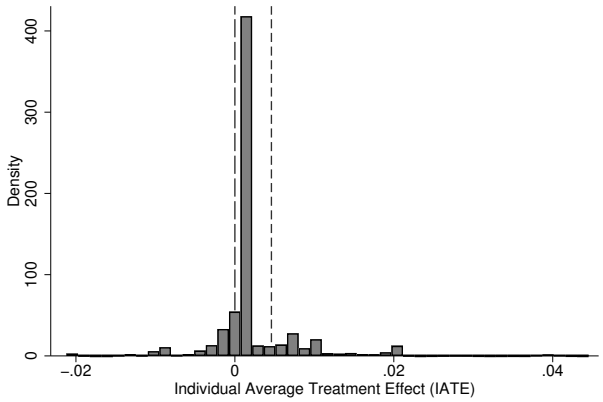
(a) Property crime



(b) Property crime



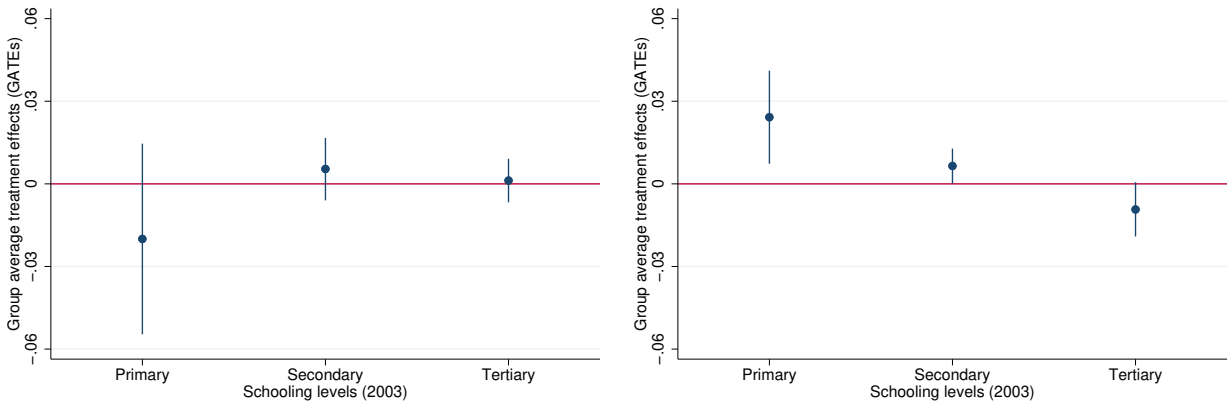
(c) Substance crime



(d) Substance crime

Notes: The figure shows the distribution of Individual Average Treatment Effects (IATEs) from the causal forest, separately for property crime (panels a–b) and substance crime (panels c–d). Panels (a) and (c) plot IATEs ranked by percentile; panels (b) and (d) show the kernel density. The long-dashed line marks zero; the short-dashed line marks the causal forest ATE, which can be compared with the DID ATE from Table 1. Sample identical to Table 1; estimation details in Appendix D.

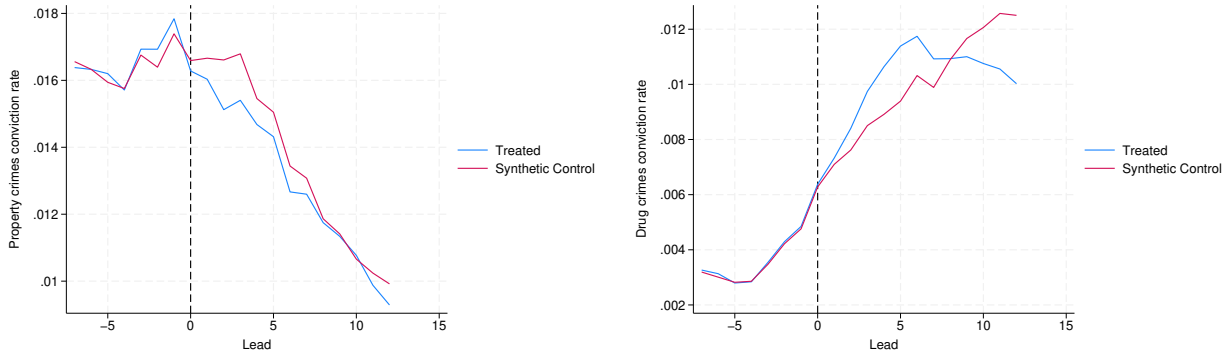
Figure B.8: Group average treatment effects (GATEs) by schooling levels, 2000-2015



(a) Property crimes (b) Substance crimes

Notes: The figure replicates the schooling dimension of Figure 4a, restricting the sample to individuals aged 23–29 to separate low educational attainment from incomplete schooling among the youngest cohorts. GATEs for property crime (panel a) and substance crime (panel b) are shown by schooling level (primary/secondary/tertiary), measured in 2003. Bars show 95% confidence intervals. Estimation details in Appendix D.

Figure B.9: Synthetic control: Property and substance crime convictions

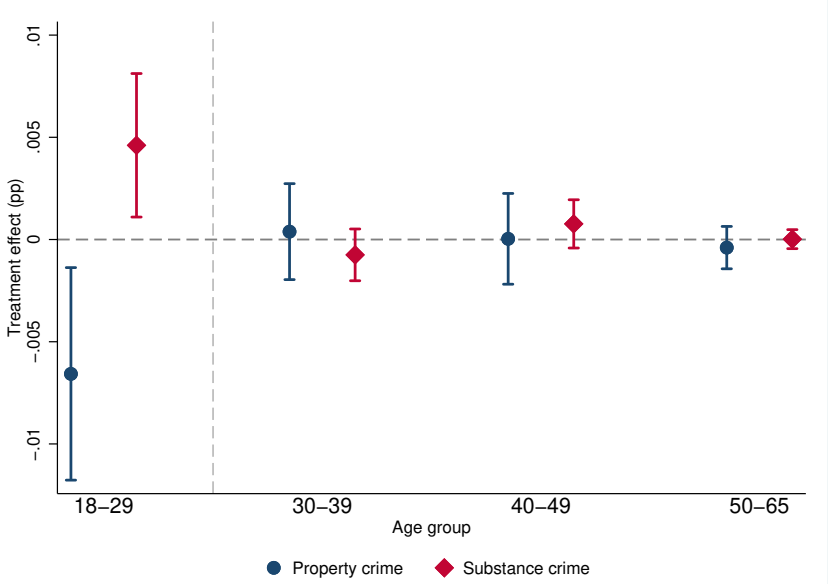


(a) Property crimes (b) Substance crimes

Notes: The figure plots actual and synthetic control crime trajectories for the two treated mining municipalities (Gällivare and Kiruna), pooled into a single composite treated unit following Galiani and Quistorff (2017). The outcome is the 3-year centered moving average of the annual conviction rate, defined as the share of young males aged 18–29 convicted of the relevant crime type in a given municipality and year, collapsed from individual-level register data. The synthetic control is constructed as a weighted combination of control municipalities that minimizes the pre-treatment mean squared prediction error (MSPE) over 1996–2003, matching on all eight pre-treatment lags of the outcome variable. The donor pool excludes other mining municipalities, municipalities belonging to the Stockholm, Gothenburg, and Malmö metropolitan regions, and municipalities with steel industry employment shares above 10% in 2003. The horizontal axis shows years relative to treatment onset; the dashed vertical line marks the last pre-treatment year (2003).

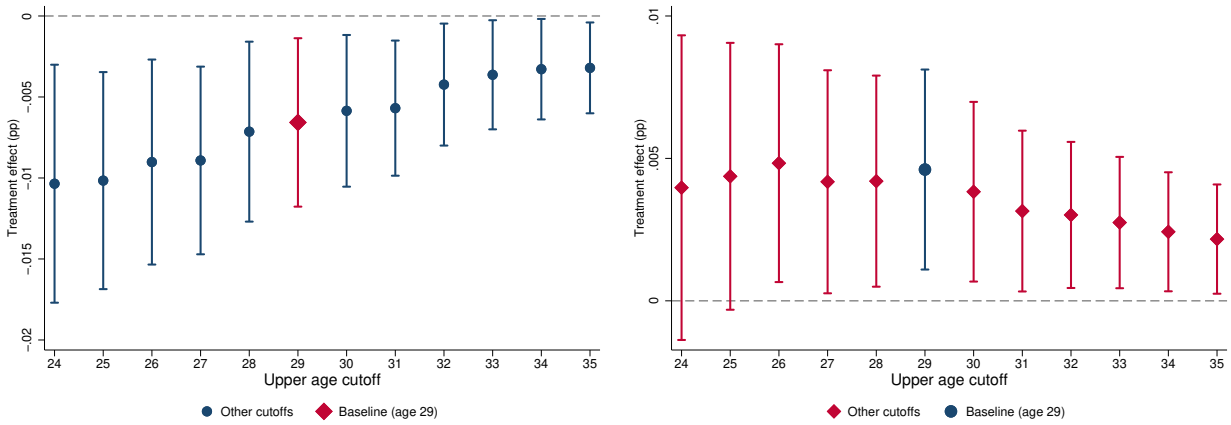
C Appendix: Age sample robustness and results for the 30-39-year-old males sample

Figure C.1: Difference-in-differences estimates by age group, property and substance crime, 2000-2015



Notes: Each point shows the DID estimate of the mining boom on the probability of conviction, separately for property crime (navy circles) and substance crime (cranberry diamonds), estimated within each age group: 18–29, 30–39, 40–49, and 50–65. Each point shows the coefficient on Post×Treated from a separate regression, restricted to males in the indicated age group. The specification includes individual, year, and grid fixed effects and standard errors clustered at the grid level. The sample excludes individuals who migrated to Norrbotten County in 2004 or later. The dashed vertical line separates the main analytic sample (18–29) from the broader population. Horizontal bars show 95% confidence intervals.

Figure C.2: Robustness to upper age cutoff, 2000–2015



(a) Property crimes

(b) Substance crimes

Notes: The figure plots the DID coefficient on Post×Treated from the main specification as the upper age cutoff varies from 24 to 35 in one-year increments, for property crime (left panel) and substance crime (right panel). The lower age cutoff is fixed at 18. The baseline cutoff (age 29) is highlighted with a diamond marker. Bars show 95% confidence intervals. Standard errors clustered at the grid level.

Table C.1: Impact of the mining boom on criminal behavior for 30-39 years old, 2000-2015

	(1) Property crime	(2) Violent crime	(3) Traffic crime	(4) Substance crime
Post*Treated	0.0002	0.0004	0.0008	-0.0008
	(0.0012)	(0.0011)	(0.0015)	(0.0006)
Individual FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Grid FE	Yes	Yes	Yes	Yes
Observations (N x T)	209922	209922	209922	209922
N	13120	13120	13120	13120
Mean dep. var (2000-03)	0.0055	0.0036	0.0067	0.0019
Effect relative to the mean (%)	3.11	11.27	12.66	-42.73
R-squared	0.3088	0.2396	0.3071	0.4397
Within R-squared	0.0000	0.0000	0.0000	0.0000

Notes: The table replicates Table 1 for males aged 30–39 as a falsification exercise. Older males have stronger pre-existing labor market attachment and are expected to be less responsive to the boom than the main sample. The design is identical to Table 1 with the sample replaced by 30–39-year-old male residents (13,120 individuals, 209,922 person-years). Crime definitions as in Table 1. Standard errors (in parentheses) clustered at the grid level. Data from the Swedish Conviction Register (Brå) and administrative registers, Statistics Sweden (LISA). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.2: Robustness check: impact of the mining boom on criminal behavior among balanced birth cohorts (born 1975-1985), 2000-2015

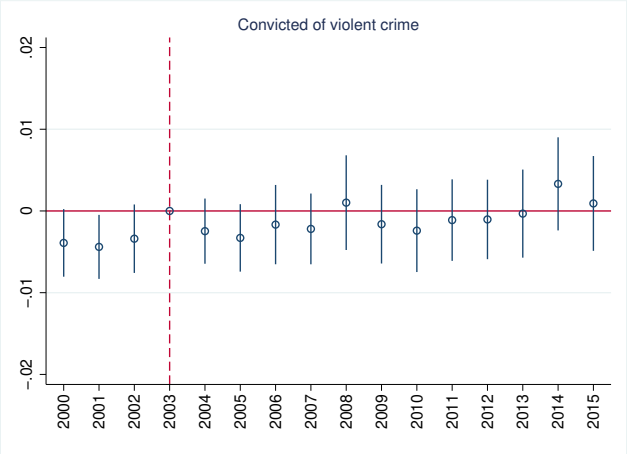
	(1) Any crime	(2) Property crime	(3) Violent crime	(4) Traffic crime	(5) Substance crime
Post*Treated	-0.0039	-0.0047**	0.0025*	-0.0020	0.0037***
	(0.0031)	(0.0020)	(0.0014)	(0.0017)	(0.0014)
Individual FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Grid FE	Yes	Yes	Yes	Yes	Yes
Observations (N x T)	179000	179000	179000	179000	179000
N	11188	11188	11188	11188	11188
Mean dep. var (2000-03)	0.0284	0.0146	0.0067	0.0109	0.0031
Effect relative to the mean (%)	-13.83	-31.99	36.80	-18.63	118.73
R-squared	0.2912	0.2556	0.1616	0.2318	0.3310
Within R-squared	0.0000	0.0001	0.0000	0.0000	0.0001

Notes: The table replicates Table 1 restricting the sample to birth cohorts observed in both the pre-boom (2000–2003) and post-boom (2004–2015) periods, corresponding to males born 1975–1985 (aged 18–28 in 2003). These are the cohorts that contribute directly to within-person identification of the DID coefficient; cohorts entering the sample after 2003 (born after 1985) and those exiting before 2004 (born before 1975) appear in only one period and do not contribute to within-person variation. The design otherwise is identical to Table 1 (11,188 individuals, 179,000 person-years). Crime definitions as in Table 1. Standard errors (in parentheses) clustered at the grid level. Data from the Swedish Conviction Register (Brå) and administrative registers, Statistics Sweden (LISA). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure C.3: Event study of the impact of the mining boom on criminal behavior of 30-39-year-old males, 2000-2015



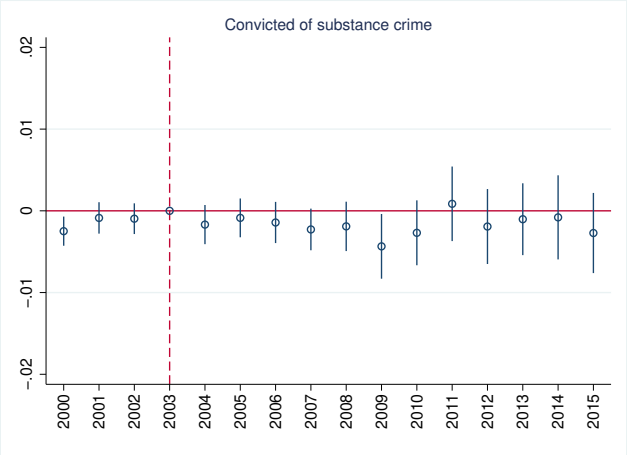
(a) Property crime



(b) Violent crime



(c) Traffic crime



(d) Substance crime

Notes: The figure replicates Figure 2 for males aged 30–39 as a falsification exercise. Year-by-year DID coefficients are plotted for each crime type, with 2003 as the reference year. Pre-2004 coefficients test the parallel trends assumption for this cohort. Design and sample identical to Table C.1 (13,120 individuals, 209,922 person-years). 95% confidence interval shown. Standard errors clustered at the grid level.

Table C.3: Impact of the mining boom on criminal behavior by distance to the mines for 30-39 years old, 2000-2015

	(1) Property crime	(2) Violent crime	(3) Traffic crime	(4) Substance crime
Post*$\leq$ 20 km	-0.0002	0.0004	0.0012	-0.0007
	(0.0013)	(0.0011)	(0.0016)	(0.0007)
Post* 20 - 40 km	0.0018	-0.0029	-0.0074	-0.0033
	(0.0053)	(0.0027)	(0.0055)	(0.0038)
Post*40 - 60 km	0.0007	-0.0014	0.0032	-0.0005
	(0.0011)	(0.0023)	(0.0060)	(0.0004)
Post*60 - 80 km	-0.0003	-0.0017	-0.0034	-0.0003
	(0.0029)	(0.0026)	(0.0046)	(0.0004)
Individual FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Grid FE	Yes	Yes	Yes	Yes
Observations (N x T)	209922	209922	209922	209922
N	13120	13120	13120	13120
Mean dep. var (2000-03)	0.0055	0.0036	0.0067	0.0019
R-squared	0.3088	0.2396	0.3071	0.4397
Within R-squared	0.0000	0.0000	0.0000	0.0000

Notes: The table replicates Table A.7 for males aged 30–39. Treatment is defined by 20-kilometer distance rings from the nearest mine, with residents beyond 80 km as the reference. Design identical to Table A.7 with the sample replaced by 30–39-year-old male residents (13,120 individuals, 209,922 person-years). Crime definitions as in Table 1. Standard errors (in parentheses) clustered at the grid level. Data from the Swedish Conviction Register (Brå) and administrative registers, Statistics Sweden (LISA). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

D Appendix: The causal forest approach for heterogeneous treatment effects

Using the causal forest method, I estimate the Conditional Average Treatment Effects (CATE) of the form:

$$CATE \equiv \tau(x) = E[Y_{1i} - Y_{0i}|X_i = x] \tag{4}$$

where Y_{1i} and Y_{0i} are the potential outcomes of interest for the *ith* individual when treated and untreated, respectively, and X is a vector of observable characteristics. The causal forest approach is a form of supervised machine learning techniques that is used for predicting heterogeneity in causal treatment effects (Athey and Imbens, 2016; Wager and Athey, 2018).[56] I follow the generalized random forest implementation developed by Athey et al. (2019). By using these methods, I rely on data-driven sample splits, thus limiting the researcher's discretion when selecting the relevant dimensions of heterogeneity. Given that I have a difference-in-differences setting (e.g., Davis and Heller, 2017; Britto et al., 2022), which is different than most applications based on RCTs, I run the causal forest over first differences, comparing pre- and post-boom averages. By doing this, the unconfoundedness assumption, explained in Wager and Athey (2018), holds because the treatment indicator is orthogonal to the covariates.

The method estimates conditional average treatment effects (CATEs), which are average treatment effects (ATEs) conditional on a set of variables for which the treatment effects may vary. I focus on two different estimates: individual average treatment effects (IATEs) and group average treatment effects (GATEs). IATEs are treatment effects conditional on observation-level characteristics, and there is one IATE for each observation in the sample. GATEs are treatment effects conditional on prespecified groups, and there is a treatment effect for each group. The approach fits an outcome model and a treatment-assignment model. I fit these models using cross-fitting via random forest. The CATEs are estimated using a partialing-out (PO) estimator via random forest. The algorithm randomly partitions the data across a large number of trees to flexibly capture heterogeneity in treatment effects without imposing a parametric structure. By default, the sample is randomly split into two parts ("honest" estimation): one half is used to determine the tree structure (e.g., how the data are partitioned into leaves), and the other half is used to estimate treatment effects within those leaves. This approach prevents overfitting and ensures unbiased estimation of treatment effects. The final CATE prediction for each observation is obtained by averaging over all trees in the forest. The default settings use 2000 trees, with subsampling and minimum leaf sizes chosen automatically by the algorithm to balance bias and variance. In addition, inference

[56] See Athey and Imbens (2019) for a review and discussion on recent machine learning (ML) literature for economics and econometrics.

and confidence intervals are computed using the bootstrap of little bags proposed in Athey et al. (2019).

In my specific case, the main outcomes are the probability of criminal conviction for property or substance-related crimes. The algorithm starts by building trees defined by data-driven sample splits characterizing leafs, which are followed by a prediction of the causal effect over the characteristics X. I believe that the treatment effect of the mining boom could vary based on schooling, earnings, employment status, and economic sectors, which I denote as x. Treatment(1) represents the potential outcomes of being treated, and Treatment(0) represents the potential outcomes of not being treated. I estimate the effects of the mining boom on criminal behavior conditional on the variables x:

$$IATE \equiv \tau(x) = E\{treatment(1) - treatment(0)|x\} \tag{5}$$

As x refers to individual characteristics, this version of the CATE is also known as IATEs. In this approach, I do not assume any functional form of $\tau(x)$, therefore, the data tells us what this function looks like.

If I want to know how the ATEs vary across population groups, I estimate the GATEs. Specifically, if G is a group variable (e.g., schooling levels) and g is a specific level of the group variable (e.g., primary education), I estimate the ATE conditional on belonging to group g, that is:

$$GATE \equiv \tau(g) = E\{treatment(1) - treatment(0)|G = g\} \tag{6}$$

where the function $\tau(g)$ is referred to as the GATE function.

E Appendix: Literature comparisons and social cost

E.1 Literature comparisons, resource shocks and crime

I compare the baseline estimated effect of the mining boom on criminal behavior with the effects of other resource shocks evaluated in the literature. To benchmark the findings, I calculate the effect sizes for related work against the control complier mean, the complier mean, the control mean, or the mean value of the criminal behavior measure, in that order of priority based on availability. I apply the same transformations to the confidence intervals. When the outcome is in log points, I interpret the effect as $100 \times (e^{\beta} - 1)$. Below, I detail this calculation for each paper included in the literature comparison plots in Appendix Figure B.1.

1. Andrews and Deza (2018) studies how a change in oil reserves in Texas impacts the crime in counties that have reserves. The authors exploit plausibly exogenous changes in the value of reserves and estimate reduced form models to capture the relationship between changes in the value of oil reserves and criminal activity in a given Texas county. As the independent variable of interest is an interaction between the oil price in the previous year and the amount of time-invariant reserves in million barrels of oil in any given county, I use the 26% increase in value of reserves reported in the paper to convert the results into comparable elasticities to the other papers using a DID. As outcomes, the authors have several types of crime. For simplicity, I focus on murder and robbery as proxies for violent and property crimes. The authors find that a 1% increase in the value of oil reserves increases murder by 0.16% and robbery by 0.55%. Using the 26% increase in the value of oil reserves to convert the results, there is a 4.1% (95% CI: 0.5% to 7.8%) increase in murder ([Table 2, Column 1]). Moreover, there is a 14.3% (95% CI: 2.8% to 25.9%) increase in robbery ([Table 2, Column 1]).

2. James and Smith (2017) studies how the energy boom of oil and shale gas in the United States affected regional crime rates throughout the country. The authors use a difference-in-differences design comparing counties for which the geographic center lies above one of the major play formations (treated) against controls, and exploiting the national temporal variation in shale energy production. They find positive effects on rates of various property and violent crimes in shale-rich counties. Focusing on all crimes, the authors find that the shock increased crime in treated counties by 0.080, significant at the 1% level ([Table 3, Column 6]. As the outcome is in log points, I interpret that there is an 8.3% (95% CI: 2.3% to 14.7%) increase in all crime.

3. Corvalan and Pazzona (2019) studies the short- and medium-run effects that an increase in copper price had on the local economy and on criminal activity in Chile. The authors compute the current value of the copper production in the year 2000 in each

municipality, in billions of Chilean pesos, and multiply it by the current price of copper in billions of Chilean pesos. Then, by comparing mining and non-mining municipalities, the authors find that, after a decade of high prices, mining municipalities did not exhibit lower crime rates compared to non-mining municipalities. As an outcome, the authors focus on property crimes and use the number of crime reports to the authorities per 100,000 inhabitants. As the independent variable of interest is an interaction between the copper production in the year 2000 and the price, I use the 400% increase in the international price of copper reported in the paper to convert the results into comparable elasticities to the other papers using a DID. The authors find that a 1 billion CLP increase in the value of copper production reduces property crime by 0.98 per 100,000. Using the 400% increase in the price to convert the results, there is a 1.2% (95% CI: -3.5% to 1.1%) reduction in property crime, which is not statistically significant ([Table 3, Column 6]).

4. Axbard et al. (2021) studies the impact of natural resource wealth on criminal activity in South Africa. The authors exploit price fluctuations in 15 internationally traded minerals as exogenous variation and compare mining police precincts against controls. The outcome of interest is the inverse hyperbolic sine transformed total number of crimes. As the independent variable of interest is the inverse hyperbolic sine transformation of the mineral value, I use the 154% increase in mining value reported in the paper to convert the results into comparable elasticities to the other papers using a DID. The authors find that increased mineral wealth leads to less crime. Specifically, the authors find that a 10% increase in the value of mineral production reduces the total number of crimes by about 0.7% (significant at the 5%-level). Using the 154% increase in mining value to convert the results, there is a 11.1% (95% CI: -20.4% to -1.7%) crime reduction ([Table 1, Column 1]).

5. Komarek (2018) studies the effect of resource extraction on local crime using the fracking boom as a natural experiment in the Marcellus region in the United States. The author uses a difference-in-differences model, exploiting variation in both the timing of fracking activity in a county and the moratorium on fracking natural gas in the State of New York. That is, counties in Pennsylvania can receive the treatment of fracking activity, while similar counties in New York can only serve as controls due to the policy. He finds that areas experiencing a natural gas extraction boom suffer an increase in overall violent crimes, while property crimes remain similar to non-boom areas. Specifically, the author finds that the shock increased violent crime in treated counties by 0.288, significant at the 1% level ([Table 2, Column 2]). For property crimes, the author finds that the shock increased property crime in treated counties by 0.050, which is not statistically significant ([Table 2, Column 4]). As the outcome is transformed using the inverse hyperbolic sine transformation of the number of crimes

per 100,000 residents, I interpret that there is a 33.4% (95% CI: 10.8% to 60.5%) increase in violent crimes and a 5.1% (95% CI: -3.2% to 14.2%) increase in property crimes.

6. Street (2025) studies the effect of the fracking boom in North Dakota, both at the individual and aggregate levels, on criminal behavior. The author uses a generalized difference-in-differences framework, comparing the criminal behavior of resident households in counties within the shale play to residents in counties outside the shale play, before and after the fracking boom. The author considers two periods: leasing (2004–2008) and production (2008–2017). I compare my effects with the effects of the production period. At the aggregate level, the outcome is aggregate cases and charges filed per household population for each county-year, and the author finds large increases in charges and cases filed during the production period. Specifically, there is a 0.0371 percentage point increase in cases per household during the production period, translating to a 44.7% (95% CI: 13.3% to 75.9%) increase, using the baseline mean of 0.083, significant at the 5% level ([Table 3, Column 1]). At the individual level, the outcome is a binary indicator for whether a case was filed for the household each year, and evidence shows a modest decrease in crime for treated individuals. Specifically, there is a 0.35 percentage point decrease during the production period in the probability of having a case filed for treated individuals, translating to a 17.5% (95% CI: -34.2% to -0.8%) decrease, using the baseline mean of 0.02, significant at the 5% level ([Table 2, Column 2]).

E.2 Social cost calculation

Appendix Table A.17 translates the main crime effect estimates into estimates of the effect of the mining boom on social costs of crime for young males, as Alsan et al. (2025). Specifically, I calculate the total unit cost for each crime category, which includes the costs for anticipation (e.g., defensive expenditure), consequence (e.g., physical and emotional harm), and response (e.g., police costs) to the crime, as reported in Heeks et al. (2018). I multiply the estimated coefficients of Table 1 by the cost of crime and add it by the number of treated young male individuals during the 12 post-boom years (2004–2015), obtaining the aggregate social cost effect (total welfare implication) of the mining boom.

The cost estimates draw on Heeks et al. (2018), who construct unit costs for each crime category using three distinct methods. Anticipation costs are derived from survey data on defensive expenditures, including spending by potential victims on security measures, locks, alarms, and insurance premiums. Consequence costs combine willingness-to-pay estimates from victim surveys, capturing physical injury, psychological distress, and quality-of-life losses. Response costs are computed from administrative records on police investigation, prosecution, court proceedings, and incarceration per crime type. A natural

concern is whether these UK-based estimates transfer to the Swedish context. No analogous comprehensive cost study exists for Sweden, but the United Kingdom is a closer institutional comparator than the United States: both are high-income welfare states with broadly comparable criminal justice structures, crime category definitions, and income levels. The estimates should therefore be read as order-of-magnitude welfare calculations rather than precise accounting, because differences in incarceration rates, healthcare costs, and victim compensation schemes introduce measurement error of uncertain direction.

The results show a mixed picture. Consistent with the regression estimates, reductions in property crimes translate into sizable social savings, amounting to SEK 17.8 million during the boom. These savings are meaningful at the local level, even if modest relative to national figures. However, other categories reveal offsetting costs. The effects on violent and traffic crimes are statistically insignificant. Substance crimes increase significantly during the boom, leading to social losses of roughly SEK 14.1 million. This pattern mirrors the conviction results and suggests that mining-driven shocks may have unintended spillovers into illicit substance activity. Taken together, the social cost estimates show that the mining boom's crime-related welfare effects are positive on net: reductions in property crime generate SEK 17.8 million in social savings, partly offset by SEK 14.1 million in additional costs from the substance crime increase, for a net crime-related gain of approximately SEK 3.7 million over the boom period.

Two caveats are necessary before drawing policy conclusions from these estimates. First, the social cost figures capture only the crime-related welfare consequences of the boom. The boom also raised labor income by approximately SEK 28,000 per treated young male resident per year, generating aggregate income gains that substantially exceed the crime-related effects in magnitude. The net social gain from the boom is therefore substantially larger than the crime cost accounting suggests, and the crime-specific near break-even result should not be read as evidence that the boom was welfare-neutral overall. Second, the social cost of substance crime is difficult to measure precisely because it encompasses health costs, productivity losses, and criminal justice expenditures that extend well beyond the boom period. The SEK 14.1 million figure likely understates the long-run social burden of increased substance convictions, particularly if addiction persists after the boom ends. These limitations suggest that the social cost calculation is best understood as a lower bound on the full welfare consequences of the substance crime increase, and a lower bound on the net benefits of the property crime reduction.

F Appendix: Newspaper content analysis

F.1 Data and search procedure

I use the Retriever Mediearkiv, a comprehensive Swedish newspaper archive that covers the large majority of Swedish daily and regional newspapers and is the standard archive in Swedish media research (accessible through the BIBSAM consortium university license). I search for all articles containing the string `LKAB*`, where the wildcard captures inflected forms such as *LKABs* (genitive) and compound words such as *LKAB-anställda* (LKAB employees). The search covers January 2000 to December 2015, yielding 16 years of coverage that span four pre-boom years (2000–2003) and twelve post-boom years (2004–2015).

Because Retriever limits each export to 500 articles, articles were downloaded in monthly or bi-monthly batches for high-volume years and combined. After restricting to articles with a valid publication date and non-empty body text, the final sample contains approximately 16,569 articles. Figure 1, panel (b), shows the rapid increase in annual article counts following the mining boom, reflecting the company's growing newsworthiness.

F.2 Outcome variable and econometric model

For each calendar month t, I construct the share of articles mentioning at least one term from a given dictionary:

$$S_t \;=\; \frac{\text{articles mentioning keyword(s) in month } t}{\text{total LKAB articles in month } t}.$$

Using a share rather than a count controls for the large post-boom increase in total LKAB coverage, isolating whether the specific topic became a larger fraction of the conversation. Matching is case-insensitive substring matching, which is important in Swedish because of compound-word formation (e.g., *arbetsmiljöproblem* would be missed by exact-word matching).

I estimate a monthly interrupted time series (ITS) model of the form:

$$S_t \;=\; \alpha + \beta_1 \text{Post}_t + \beta_2 t + \beta_3 (t \times \text{Post}_t) + \varepsilon_t, \tag{7}$$

where S_t is the share of articles classified in a specific topic in month t, the variable $\text{Post}_t = 1$ for $t \geq$ January 2004 indicates the post-boom period, and t is a running variable measured in months, equal to zero at January 2004, negative before, and positive after. I include $t \times \text{Post}_t$ to allow for potential change in the trend following January 2004, which captures the slope specific to the post-boom period. The coefficients are interpreted as follows: β_1 represents the immediate level shift in the share in January 2004, which is the primary coefficient of interest. β_2 is the pre-boom monthly time trend, β_3 is the change in trend after the boom, and the post-boom slope is $\beta_2 + \beta_3$. α is the baseline share in January 2004. The error term is captured by ε_t. Standard errors follow Newey and West (1987) with 12 lags, which is the

standard choice for monthly data (Ghazarian, 2025), and correct for both serial correlation and heteroskedasticity.

Identification. The ITS design relies on the assumption that, absent the mining boom, the share S_t would have continued along its pre-2004 trend. This "parallel trend in time" assumption cannot be tested directly. The design has no control group; therefore, concurrent national trends in the discourse or changes in newspaper coverage of the specific topic more broadly are not differenced out. Results should therefore be interpreted as descriptive and suggestive rather than causal. Additional caveats include the short pre-period (48 monthly observations), keyword false positives arising from compound words, and potential changes in the mix of newspapers covering LKAB over time.

F.3 Topic decomposition of LKAB coverage

General topic coverage of LKAB. To characterize what the mining boom looked like in public discourse, I perform the keyword analysis on seven broad topic categories: production and economic output, employment and hiring, accidents and safety, environment and community, labor relations, stress and mental health, and crime. For each article, I flag membership in each topic category using case-insensitive substring matching; an article may be assigned to multiple topics. Table F.1 lists the keyword dictionary.

Table F.1: Topic keyword dictionary (Swedish)

Topic	Swedish keywords	English meaning
Production & economy	produktion, järnmalm, export, investering, vinst, pris, marknad, efterfrågan, expansion, utbyggnad, gruvdrift	production, iron ore, export, investment, profit, price, market, demand, expansion, mine operations
Employment & hiring	anställa, rekrytera, arbetstillfälle, lediga jobb, nyanställd, nyanställning, arbetskraft, bemanning	hire, recruit, job opportunity, job opening, new hire, workforce, staffing
Accidents & safety	olycka, olyckor, skadad, dödsolycka, arbetsolycka, gruvras, bergras, jordskalv, marksättning, driftstopp, driftstörning, produktionsstopp	accident, injured, fatal accident, mine collapse, rockslide, earthquake, ground subsidence, shutdown
Environment & community	miljö, utsläpp, damm, buller, samhällsomvandling, stadsomvandling, marksubsidenser, renar, samer, sameby	environment, emissions, dust, noise, urban transformation, city relocation, reindeer, Sami
Labor relations	strejk, konflikt, fack, löneförhandling, varsel, uppsägning, nedskärning	strike, conflict, union, wage negotiation, redundancy notice, dismissal, cutback
Stress & health	stress, utbränd, utmattning, sjukskrivning, psykisk, arbetsmiljö, depression, ångest, ohälsa, välmående	stress, burnout, exhaustion, sick leave, psychological, work environment, depression, anxiety, wellbeing
Crime	brott, kriminalitet, stöld, narkotika, polisen, dömd, fängelse, rättegång, åtalad, häktad	crime, theft, narcotics, police, convicted, prison, trial, prosecuted, detained

Note: The table lists the Swedish-language terms used to classify LKAB-related newspaper articles by topic. An article is assigned to a topic if it contains at least one term from that topic's dictionary. Matching is case-insensitive substring matching to capture Swedish compound words. Terms excluded to avoid false positives: *kiruna*, *gällivare*, *malmberget* (place names appearing in virtually all LKAB articles); *gruvan* (generic "the mine"); *personal* (generic "staff/personnel"); *ras* (homonym: also means "race/breed"); *seismisk* (ubiquitous for mine subsidence); *evakuering* (Kiruna city relocation, present in a large share of all articles); *stopp* (too broad). An article may be assigned to multiple topics.

Table F.2 reports the ITS estimates for the different topics alongside pre- and post-boom mean shares. Figure F.1 plots, for each topic, the estimated level shift in the monthly share of LKAB articles at January 2004, that is, how much more (or less) of the coverage each topic captured after the boom onset relative to the pre-boom period. The topic composition is

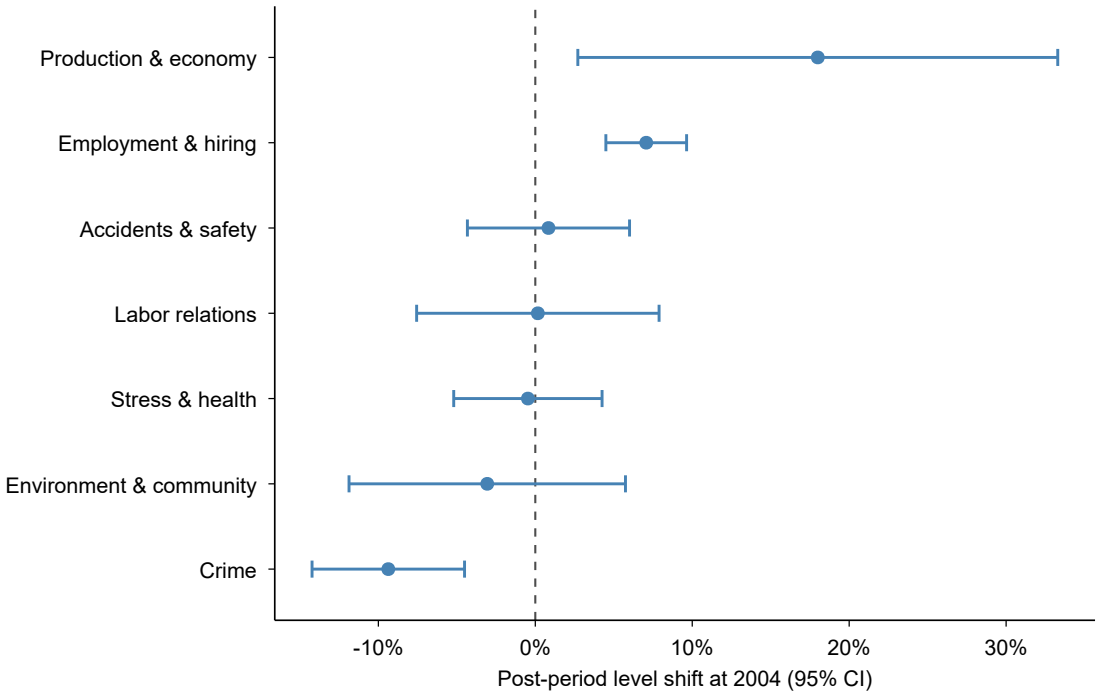
informative about how the boom was perceived locally beyond prices and output: as a story of jobs and expansion, or of strained working conditions, accidents, and conflict.n Figure F.2 plots the pre- and post-boom ITS fitted trends for all seven topics. The only topics showing statistically significant level shifts at the boom onset are employment and hiring and production and economy, consistent with boom-driven news centered on expansion and hiring. By contrast, stress and health, accidents and safety, and labor relations show no significant level shift. The share of LKAB articles mentioning crime falls. Because each topic is measured as a share of all LKAB articles, this decline can reflect the surge in production and hiring stories rather than fewer crime-related articles.

Table F.2: Topic decomposition of LKAB newspaper articles, 2000–2015

	Pre-boom (2000–2003)	Post-boom (2004–2015)	Post-period indicator ($\hat{\beta}_1$)	p-value
Production & economy	61.9%	62.4%	0.180**	0.022
			(0.078)	
Employment & hiring	7.0%	12.8%	0.071***	0.000
			(0.013)	
Accidents & safety	6.1%	8.1%	0.008	0.750
			(0.026)	
Environment & community	19.2%	33.1%	-0.031	0.497
			(0.045)	
Labor relations	24.2%	15.9%	0.002	0.967
			(0.039)	
Stress & health	5.0%	6.5%	-0.005	0.845
			(0.024)	
Crime	16.9%	14.4%	-0.094***	0.000
			(0.025)	

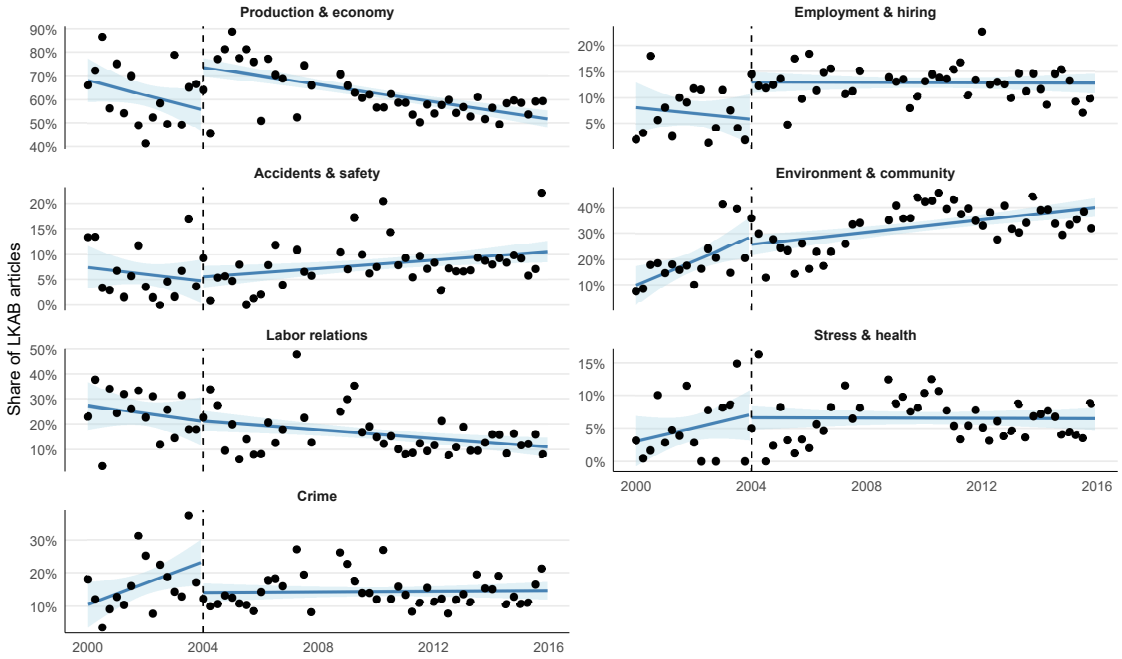
Notes: The table reports estimates from the interrupted time series (ITS) model for the different topics. The outcome is the monthly share of LKAB*-related newspaper articles mentioning each topic. The ITS model estimates three parameters: an immediate level shift at January 2004, a pre-boom monthly linear trend, and a change in slope after the boom. The table shows the immediate level shift at January 2004. The sample covers 175 monthly observations (2000–2015). Newey–West standard errors (in parentheses), lag = 12. Data from Retriever Mediearkivet. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure F.1: Topic composition of LKAB newspaper coverage, 2000–2015



Notes: Each point is the estimated post-period level shift ($\hat{\beta}_1$) from an interrupted time series regression of the monthly share of LKAB*-related newspaper articles mentioning keywords from the indicated topic category, with a structural break at January 2004. Horizontal lines show 95% confidence intervals based on Newey–West standard errors (lag = 12). Topics are ordered by point estimate. The keyword dictionary is reported in Appendix Table F.1; full regression estimates in Appendix Table F.2; quarterly trends in Appendix Figure F.2. Articles are identified using the newspaper archive *Retriever Mediearkivet*. LKAB is Sweden's state-owned iron ore company operating large-scale mines in Kiruna and Gällivare.

Figure F.2: Interrupted time series, topic decomposition of LKAB newspaper articles, 2000–2015



Notes: Each panel plots a quarterly binscatter of the monthly share of LKAB*-related articles mentioning keywords from the indicated topic category over 2000–2015. The fitted lines represent the estimated pre- and post-boom linear trends from the interrupted time series regression. The vertical dashed line marks January 2004, which corresponds to the beginning of the mining boom. Articles are identified using the newspaper archive *Retriever Mediearkivet*. Following the literature, I search for articles containing the case-insensitive string "LKAB*", where the asterisk is used as a wildcard. LKAB is Sweden's state-owned iron ore company and operates the large-scale mines in Kiruna and Gällivare. Newey–West standard errors (lag = 12).

Stress coverage of LKAB. To test the occupational stress channel, I perform the keyword analysis in more detail for the stress and mental health topic. Table F.3 shows the stress and mental health dictionary, listing the terms used to classify articles as mentioning this topic and the rationale for why they are used.

Table F.4 reports the ITS estimates for the stress series under three specifications: a baseline linear trend (column (1)), a robustness check that adds a quadratic time term (column (2)), and a specification that weights each monthly observation by the total number of articles in that month (column (3)). In the baseline specification, the estimated level shift at January 2004 is $\hat{\beta}_1 = -0.007$, indicating no statistically significant change in the share of LKAB articles mentioning stress or mental health terms at the onset of the boom. The pre-boom trend ($\hat{\beta}_2 = 0.001$ and the post-boom slope change ($\hat{\beta}_3 = -0.001$) are both positive and negative, respectively, but neither is statistically significant, providing no evidence of a systematic trend in stress-related coverage before or after the boom. Column (2) adds a quadratic time term; the level shift remains negative and statistically insignificant. I follow Ghazarian (2025) in using this quadratic robustness check as a standard test of the functional form assumption on the pre-period trend specification. Finally, Column (3) weights

Table F.3: Stress and mental health keyword dictionary (Swedish)

Swedish term	English meaning	Rationale
stress / stressad	stress / stressed	Core term
utbränd / utbrändhet	burned out / burnout	Clinical work-related exhaustion
utmattning	exhaustion / fatigue	Physical and mental exhaustion
sjukskrivning	sick leave (certified)	Objective indicator of worker health
sjukfrånvaro	sick absence	Absence due to illness
psykisk	mental / psychological	Modifier for health/illness
psykosocial	psychosocial	Occupational health technical term
ohälsa	ill-health / poor health	General health problems
arbetsmiljö	work environment	Central to labor law discussions
arbetsbelastning	load / workload	Work overload (compound only)
depression	depression	Clinical mental health condition
ångest	anxiety	Co-occurs with stress and substance use
välmående / välbefinnande	wellbeing	Worker wellbeing

Note: The table lists the Swedish-language terms used to classify LKAB-related newspaper articles as mentioning occupational stress or mental health, with English translations and rationale. Matching is case-insensitive substring matching to capture Swedish compound words. Two terms are excluded to avoid false positives: "press" (Swedish for "media/press", appearing in virtually all news articles) and standalone "belastning" ("load/strain", common in mining engineering contexts); the compound "arbetsbelastning" ("work overload") is retained as it is specific to occupational health. The monthly share variable is the fraction of LKAB articles in a given month containing at least one of these terms.

observations by the total number of articles per month (Ghazarian, 2025); the result is again insignificant, suggesting that the baseline result is not driven by months with sparse coverage. Taken together, the three specifications provide no evidence of a structural break in stress-related media coverage at the onset of the mining boom. Figure 5 (in the main text) presents the binscatter of quarterly shares alongside the ITS fitted trends of Column (1).

Table F.4: Interrupted Time Series: Share of LKAB articles mentioning stress/mental health terms, 2000-2015

	(1)	(2)	(3)
	Baseline	Quadratic trend	Weighted
Post-period indicator (β_1)	-0.007	-0.023	0.031
	(0.024)	(0.029)	(0.025)
Time in months (β_2)	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)
Time^2		-0.000	
		(0.000)	
Post-period slope (β_3)	-0.001	0.000	-0.001*
	(0.001)	(0.001)	(0.001)
Constant	0.075***	0.073***	0.066***
	(0.019)	(0.019)	(0.014)
Observations	175	175	175
R-squared	0.024	0.043	0.051

Notes: The table reports estimates from the interrupted time series (ITS) model underlying Figure 5. The outcome is the monthly share of LKAB*-related newspaper articles mentioning stress or mental health terms (keyword dictionary in Table F.3). The ITS model estimates three parameters: an immediate level shift at January 2004, a pre-boom monthly linear trend, and a change in slope after the boom. Column (1) is the baseline; Column (2) adds a quadratic time trend as a functional form check; Column (3) weights observations by article count per month. The sample covers 175 monthly observations (2000–2015). None of the coefficients is statistically significant in any specification. Newey–West standard errors (in parentheses), lag = 12. Data from Retriever Mediearkivet. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

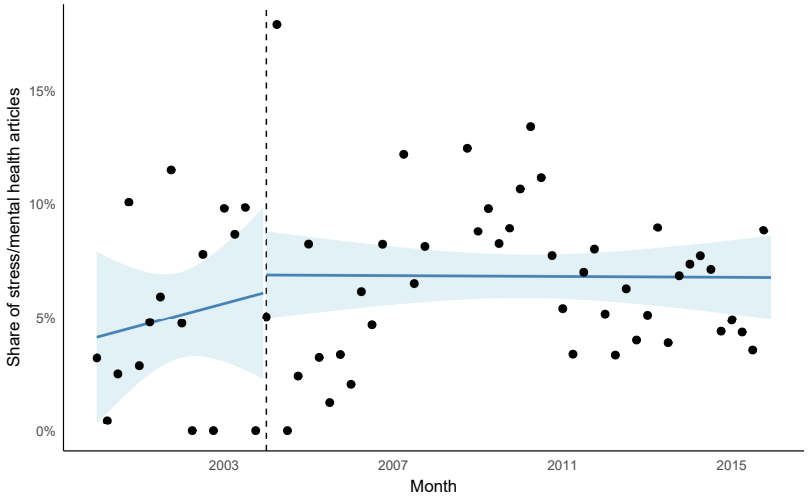
As a robustness check, I implement a minimum monthly article threshold. Pre-boom months contain substantially fewer total LKAB articles than post-boom months. In months with very few articles, a single article mentioning a stress term can shift the monthly share substantially, inflating pre-boom variance. To assess whether this drives any results, I drop months with fewer than five total articles and re-estimate the ITS. Table F.5 reports the results and Figure F.3 plots the series. The estimated level shift at January 2004 is $\hat{\beta}_1 = 0.008$, and neither the pre-boom trend ($\hat{\beta}_2 = 0.000$) nor the post-boom slope change ($\hat{\beta}_3 = 0.000$) is statistically significant. The standard errors on β_1 shrink from 0.024 (full sample) to 0.021 (restricted sample), confirming that dropping sparse months increases precision without affecting the null result.

Table F.5: Interrupted Time Series: Share of LKAB articles mentioning stress/mental health terms (N≥5 robustness), 2000-2015

	(1)	(2)	(3)
	Baseline	Quadratic trend	Weighted
Post-period indicator (β_1)	0.008	-0.009	0.035
	(0.021)	(0.026)	(0.024)
Time in months (β_2)	0.000	0.000	0.001
	(0.000)	(0.000)	(0.001)
Time^2		-0.000	
		(0.000)	
Post-period slope (β_3)	-0.000	0.001	-0.001
	(0.000)	(0.001)	(0.001)
Constant	0.061***	0.058***	0.063***
	(0.014)	(0.013)	(0.014)
Observations	173	173	173
R-squared	0.021	0.041	0.052

Notes: The table replicates Table F.4 after excluding months with fewer than five total LKAB articles, addressing concerns that sparse pre-boom months (where a single stress-related article can inflate the share) may drive results. The restricted sample covers 173 monthly observations. Column specifications are identical to Table F.4; all coefficients remain statistically insignificant. Newey–West standard errors (in parentheses), lag = 12. Data from Retriever Mediearkivet. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure F.3: Interrupted time series of stress/mental health newspaper articles, months with $N \geq 5$ (robustness), 2000–2015



Notes: The figure plots a binscatter of the quarterly share of LKAB* articles mentioning stress or mental health terms from 2000 to 2015. Restricted to months with at least five total LKAB articles. Months with fewer than five articles are concentrated in the pre-boom period (2000–2003). The fitted lines represent the estimated pre- and post-boom linear trends from the interrupted time series regression. The vertical dashed line marks January 2004, which corresponds to the beginning of the mining boom. Articles are identified using the newspaper archive *Retriever Mediearkivet*. Following the literature, we search for articles containing the case-insensitive string "LKAB*", where the asterisk is used as a wildcard. LKAB is the main iron ore producer in Sweden and operates the large-scale mines in Kiruna and Gällivare. LKAB is Sweden's state-owned iron ore mining company. Newey-West standard errors (lag = 12). See Appendix Table F.5 for coefficient estimates and Appendix F for data and estimation details.

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