Digging for Trouble?
Mining and Criminal Behavior of Young Males

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Abstract

This paper studies how a large, localized resource boom affects the criminal behavior of young males. I exploit the 2004 iron ore boom in Northern Sweden as an exogenous shock to local economic conditions and combine geocoded administrative data on all criminal convictions and demographics from 2000–2015 with a difference-in-differences design. Comparing young males living in the mining municipalities to young males of similar nearby municipalities, and exploiting fine-grained variation in distance to the mines, I identify the causal impact of improved local labor market opportunities on crime. The results show that the mining boom led to a large decline (52%) in property crime among young male residents aged 18–29, with no effects for older individuals. The reduction is concentrated within 20 kilometers of the mines and driven primarily by first-time offenders. In contrast, the probability of being convicted of substance-related crimes increases (181%) among young males, particularly among repeat offenders and individuals directly employed in the mining sector. There is no evidence of effects on violent or traffic crimes. Mechanism analysis shows that the boom substantially improved employment and earnings for local residents, while changes in migration patterns, policing, and income inequality do not explain the results. Overall, the findings provide new micro-level evidence that positive local labor market shocks can reduce economically motivated crime, while simultaneously increasing certain non-economic offenses. (JEL R11, K42, Q33, O13)

Keywords: criminal behavior, economic opportunities, mining, Sweden

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1 Introduction

Does a higher wage deter crime or help finance it? The canonical economic model of crime (Becker, 1968; Ehrlich, 1973) offers a clear prediction: better labor market opportunities raise the opportunity cost of illegal activity, pushing individuals toward legal work. A large empirical literature confirms this for negative shocks, finding that job loss and declines in wages increase criminal behavior (e.g., Britto et al., 2022), while higher employment and earnings reduce it (e.g., Gould et al., 2002; Machin and Meghir, 2004). Yet the same framework also allows for the opposite effect. Higher income can also complement criminal activity by increasing the returns to certain crimes or fuel demand for illegal goods (Draca and Machin, 2015; Cunningham et al., 2020). As a result, positive economic shocks may either substitute for crime or complement it, which is a question that remains largely unresolved in the empirical literature.

Importantly, these opposing forces are unlikely to affect all crimes equally. Property crimes, closely tied to economic incentives, are most likely to respond to changes in the opportunity cost of illegal activity and decline as legal opportunities improve. In contrast, substance-related offenses, linked to disposable income and consumption patterns, may instead increase with income. Violent crimes may respond to changes in stress, social interactions, or alcohol and drug use, while traffic offenses may reflect increased mobility and economic activity. Because different crimes are governed by different incentives, the net effect of a positive economic shock on criminal behavior is ultimately an empirical question, and aggregate crime measures can mask important heterogeneity in how economic conditions shape criminal behavior.

Resource booms provide a particularly clean laboratory to study this question. They generate large, sudden, and localized income and employment shocks, often driven by global commodity prices and therefore plausibly exogenous to local conditions. Yet empirical evidence from resource booms is mixed: many studies document increases in aggregate crime and social disorder following energy and mining booms (James and Smith, 2017; Couttenier et al., 2017; Komarek, 2018), while more recent micro-level work shows that crime among existing residents can decline when labor market opportunities improve (in line with Becker (1968)) (Axbard et al., 2021; Street, 2025). A key insight from this recent literature is that aggregate crime statistics may mask important heterogeneity across types of crime, across population groups, and between behavioral responses and compositional changes in the local population.

This paper studies how the criminal behavior of young males responds to positive local economic shocks, specifically, how a large, plausibly exogenous local economic boom affects individual criminal behavior, using the 2004 iron ore price boom in northern Sweden as a laboratory. This addresses the empirical challenges in establishing the causal effects of economic opportunities and criminal behavior, due to the difficulty in identifying plausibly exogenous variation in local economic opportunities. I ask how different types of crime respond to improved local labor market conditions, and which individuals are most affected. This allows me to identify the effect of economic opportunity on individuals' (young males) criminal behavior. Moreover, the mechanisms by which the boom may affect crime are analyzed, including the effect of the boom on local economies' labor market conditions and crime prevention capacity. In general, mining booms improve the labor market, attract in-migrants to the areas, and increase local purchasing power. In Sweden specifically, Rodríguez-Puello and Rickardsson (2026) find that individuals located close to the mines experienced higher employment and earnings after the boom, driven by the mining sector, but also by spillovers into manufacturing, construction, and services. I combine detailed geocoded administrative data on criminal convictions with rich individual-level information on employment, earnings, and residential mobility over the period 2000–2015. In addition to the administrative registers, I assemble an original text dataset from the Retriever Mediearkiv, consisting of newspaper articles mentioning the mining company LKAB over 2000–2015, which I use to construct monthly time series of crime- and stress-related media discourse. 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, allowing me to isolate how improved local economic conditions shape criminal behavior while explicitly accounting for residential mobility and spatial heterogeneity. Moreover, the size and richness of the data set allow me to characterize the heterogeneity of treatment effects across individuals using causal forests. I contribute to the literature by focusing on people rather than places, and estimating the effect more in depth across time, economic sectors, types of crime, and demographic groups.

Northern Sweden and the mining boom are ideal contexts for this study for several reasons. First, Sweden has a long tradition of iron ore mining, specifically in the North of the country (Nordregio, 2009; Haley et al., 2011; Tano et al., 2016).[1] The unexpected mining boom analyzed in this paper started circa 2004 when mining prices tripled (Baffes and Haniotis, 2010). The mining sector in the country is concentrated in a few municipalities in the north, which have been experiencing decades of disinvestment and population decline (Adjei et al., 2023). I focus on the cases of Gällivare and Kiruna municipalities, where the workers in the mining sector represent around 20% of the total employment. Research on the localized effect of a resource boom on criminal activity in a developed country is scarce (Komarek, 2018), especially in a context such as Northern Sweden, where boomtowns have these characteristics. This is despite crime being considered an obstacle to development and a serious threat to the well-being of individuals (The World Bank, 2011). Second, the shock was largely unforeseen and generated outside of Sweden. The mining boom is assumed to be plausibly exogenous

[1] Estimates indicate that in 2013, the mining industry contributed almost SEK 44 billion (1.3 percent) to Swedish GDP, and it is considered one of the most attractive mining countries in the world (Swedish Agency for Growth Policy Analysis, 2015).

since it was generated by global demand, such as China's increasing demand for commodities, and speculation in the stock markets, rather than shifts in the supply of minerals (Radetzki et al., 2008; Farooki and Kaplinsky, 2013; Singleton, 2014). In addition, empirical literature considers the location of natural resources as exogenous because it depends on local geology. Together, these support the assumption that the mining boom affected local labor markets for reasons unrelated to prior local conditions and individuals' behaviors, overcoming common critiques of the difference-in-differences research design (Besley and Case, 2000).

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.

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). Positive local economic shocks may influence crime via several mechanisms, which are not mutually exclusive.[2] First, improvements in the labor market conditions are expected to decrease crime for residents due to increases in the returns to legal activity. That is, individuals with higher wages or better employment opportunities experience an increase in the opportunity cost of engaging in criminal activity, reducing local crime levels for individuals residing in the local area (Komarek, 2018; Axbard et al., 2021). Intuitively, I expect that economically motivated crimes (property crimes) are likely to be better understood with this mechanism.[3]

[2]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.

[3]See Draca and Machin (2015) and Ferraz et al. (2022) for review articles on how economic incentives and economic shocks can affect crime.

Nevertheless, an additional mechanism also related to the improved labor market that suggests opposite effects, and often used in the literature to explain increases in crime due to resource shocks, is the increase in the payoff to commit crimes, known as the rapacity effect (Draca and Machin, 2015). Intuitively, the positive shock generates increases in earnings, providing individuals with more disposable income and criminals with higher incentives to commit crimes.

Second, there are indirect channels, such as changes in migration patterns, income inequality, and crime-prevention capacity, that may influence how a positive economic shock affects crime. Positive local economic shocks attract individuals looking for better economic opportunities (Wilson, 2022), especially those whose outside options are dominated by the expected gains from moving. By analyzing migrants to the mining areas, their characteristics, and how the combination of the boom and their relocation affects their criminal behavior, I provide insights into how different population groups respond to economic opportunities. Moreover, previous studies show that migrants, attracted by local economic shocks, are likely to be young and mobile (lower migration costs), low-skilled, and more risk-tolerant or with higher baseline crime returns (Dustmann and Glitz, 2011; Gröger, 2021; Wilson, 2022). Therefore, due to selection, migrants may have a higher average probability of committing crimes upon arrival, even if their own likelihood declines due to higher local wages. Finally, the probability of being caught depends on police resources and other area characteristics, affecting the final individual decision to commit crimes. This demonstrates the importance of an empirical analysis of the relationship between a positive local economic shock and local crime and the mechanisms behind it.

Although the analysis focuses on a specific mining boom in northern Sweden, 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 laboratory 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.

The main results show that the mining boom had 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 (52 percent relative to the pre-boom mean), while substance-related (e.g., narcotics possession and use) convictions increase by

0.46 percentage points (181 percent), especially possession and use rather than production or trafficking. These effects are concentrated within 20 kilometers of the mines, with no significant changes for violent or traffic crimes, or for older cohorts aged 30–39. The magnitude of these effects is comparable to other quasi-experimental estimates in the resource shocks literature (Online Appendix Figure B.1).

The richness of the administrative data allows me to go beyond average effects and investigate who is affected and why. The reduction in property crime is driven entirely by first-time offenders, suggesting that improved labor market opportunities deter initial entry into economically motivated crime. Consistent with this interpretation, mechanism analysis shows that the mining boom substantially increased employment and earnings among young male residents, shifting the cost-benefit calculation away from criminal activity. To systematically characterize the heterogeneity, I estimate conditional average treatment effects using flexible machine-learning methods such as causal forests algorithms (Athey et al., 2019), showing that the property crime reductions are largest among individuals with low education, weak labor market attachment, and low pre-boom earnings, which are precisely the groups for whom improved economic opportunities represent the largest change in the returns to legal activity. By contrast, the increase in substance-related crime is concentrated among repeat offenders, individuals employed in the mining sector, and those in the upper tail of the earnings distribution. This pattern points to an income and consumption channel: higher disposable income increases engagement in risky behaviors among individuals already involved in substance use, rather than inducing new entry. Complementary newspaper content analysis of LKAB coverage finds no statistically significant change in the share of articles mentioning occupational stress at the onset of the boom, providing no support for the stress channel and reinforcing the income effect as the primary explanation for the substance crime result. Importantly, changes in police presence, income inequality, and population composition through migration do not explain the results, reinforcing the role of labor market conditions as the primary channel. The estimates are also robust to several changes in assumptions and estimation.

I also examine the criminal behavior of young male migrants to the mining area, in order to assess the role of population mobility in shaping observed crime patterns. Migrants differ systematically from residents, with weaker pre-boom labor market attachment and higher baseline conviction rates. However, the mining boom does not generate broad increases in criminal behavior among migrants. Changes in crime following migration are similar across mining and non-mining municipalities for most crime types, suggesting they are largely driven by migration itself. An exception arises for substance-related offenses, where migrants to mining municipalities experience higher conviction rates relative to those settling in non-mining areas.

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). Reductions in property crime generate sizeable local welfare gains, amounting to SEK 17.8 million during the boom, 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.

Related literature. This paper contributes to several strands of literature on crime, labor markets, and local economic shocks. First, I provide micro-level causal evidence that positive local economic shocks have heterogeneous effects across types of crime. A large empirical literature, surveyed by Draca and Machin (2015) and Ferraz et al. (2022), studies the relationship between economic conditions and criminal behavior. 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).[4] More recent micro-level work, however, shows that improved labor market opportunities generated by resource shocks can reduce criminal behavior, consistent with rational models of crime (e.g., Axbard et al., 2021; Street, 2025). This paper helps reconcile these seemingly contradictory findings by showing that the same economic shock can simultaneously reduce economically motivated crime and increase substance-related offenses. The Swedish setting, a stable democracy with strong institutions, allows me to isolate the economic channels through which resource booms affect criminal behavior, without the confounding presence of armed conflict or weak state capacity that characterizes much of the developing-country evidence (e.g., Collier and Hoeffler, 2005; Berman et al., 2017).[5]

Second, the paper contributes to a relatively small literature 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; Axbard et al., 2021). By exploiting individual-level panel data and separating residents from migrants, I show that distinguishing people from places is essential for interpreting the crime effects of local economic shocks. Focusing on individual behavior reveals a clear pattern: property crime declines while substance crime increases, which is obscured in aggregate statistics. 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). By exploiting detailed migration histories, I show that changes in aggregate crime need not reflect changes in individual behavior, and that population mobility

[4] Stretesky and Grimmer (2020) provides a systematic review of the literature relating shale gas development and crime, concluding that most studies provide clear evidence that shale gas development increases crime, especially in the United States.

[5] See Vanden Eynde and Vargas (2025) for a recent review on the theoretical and empirical literature about how natural resource dynamics contribute to conflict.

plays an important role in shaping observed crime patterns following mining expansions. These findings complement Street (2025), who reaches a similar conclusion in the context of US fracking. These findings highlight how place-based analyses can be misleading when population mobility is ignored.

Third, the paper provides new evidence on heterogeneity in crime responses to economic opportunities. I document sharply different responses across crime types, demographic groups, and offender histories, showing that treating crime as a single outcome masks important variation in the social effects of economic shocks. To systematically characterize this heterogeneity, I use causal forest estimators (Athey et al., 2019), which allow the data to identify the relevant dimensions of treatment effect variation rather than relying on the researcher's discretion. This approach is still rare in the crime-and-economic-shocks literature and reveals that the individuals most responsive to improved economic opportunities are precisely those with the weakest prior labor market attachment, while substance crime increases are concentrated among those with higher incomes and pre-existing involvement. Additionally, I construct an original dataset of newspaper articles from the Retriever Mediearkiv (the comprehensive Swedish press archive) covering all mining company LKAB-related coverage from 2000 to 2015. Using a keyword dictionary, text analysis, and an interrupted time series (ITS) design, I find no statistically significant shift in the share of articles mentioning occupational stress or mental health at the onset of the boom. I assess this null result alongside the competing mechanisms (income effects, workplace culture, and employer drug testing) and discuss their consistency with the causal forest estimates of treatment effect heterogeneity.

Finally, the paper contributes to the literature on rational theories of crime (Becker, 1968; Ehrlich, 1973) by providing new causal evidence on the income channel. I estimate that a one percent increase in earnings reduces the likelihood of a property crime conviction by approximately two percent, an elasticity at the upper end of existing estimates (Gould et al., 2002; Machin and Meghir, 2004). More recent work, including Agan and Makowsky (2023), further supports the view that better access to economic opportunities can lower recidivism and overall criminal activity. At the same time, the results demonstrate that improved economic opportunities do not uniformly reduce all forms of criminal behavior, underscoring the importance of distinguishing economically motivated offenses from other types of crime when assessing the welfare consequences of labor market policies and place-based economic development.

Roadmap. The remainder of the paper is structured as follows. In Section 2, the background of the Swedish mining sector and the mining booms are presented. Section 3 presents the data and sample. Section 4 presents the empirical framework and identification assumptions. Section 5 reports the empirical results, including heterogeneity results using causal forest estimates of treatment effect heterogeneity. In Section 6.2 the relevant mechanisms are reviewed, with complementary interrupted time series evidence from an original newspaper dataset. Finally, Section 7 provides a discussion of the findings and conclusions.

2 Background and institutional setting

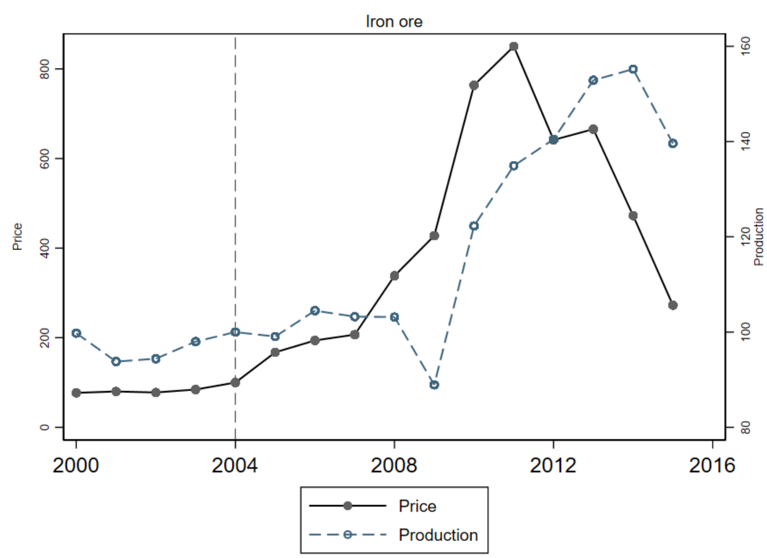
This section provides background on the mining boom, the mining sector in Sweden, and its local economic effects, including a summary of previous evidence on the labor market effects of mining booms in Sweden.

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). It is difficult to choose the timing of the mining boom because of the complex fluctuations of international prices of different minerals (Rossen, 2015). Following Tano et al. (2016), I use 2004 as the starting point because it is the year when the price of minerals started to rapidly increase; for example, the price of iron ore increased by 67% from 2004 to 2005. In addition, the number of mining jobs had a negative trend until 2003, started to increase in 2004, and continued to grow over the coming years (SGU, 2014; Knobblock and Pettersson, 2010). This trend was accompanied by an increase in investment in the Swedish mining sector. According to the literature, this shock can be considered a quasi-experiment and plausibly exogenous if it fulfills four conditions: large, variable, temporary, and generated outside an industry or country. It was generated by China's increasing demand for commodities (Radetzki et al., 2008; Farooki and Kaplinsky, 2013) and speculation in stock markets that generated investor flow (Singleton, 2014; Erten and Ocampo, 2013), rather than shifts in the supply of minerals. Therefore, it was generated outside of the country. In addition, it must be large and variable enough to affect municipalities' local conditions and temporary to identify the phases and years in which it occurred. Since this external demand shock is exogenous to the Swedish mining industry, it allows me to identify causal effects of labor market shocks on criminal behavior. Moreover, being able to track individuals over a long period provides a unique setting to examine how criminal behavior responds to changes in local economic conditions.

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 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 increased by 67% 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, showing some changes in production between those same years after the increase 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 (Petterson and Knobblock, 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 (Online Appendix Figure B.2). Media coverage closely tracks the evolution of international iron ore prices, increasing sharply after 2004 and peaking during the height of the boom. 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.

Figure 1: Price and production values for iron ore in overall Swedish production, 2000–2015



Notes: Price and production are normalized to 2004 values (2004=100). The vertical dashed line shows the year of the start of the mining boom (2004). Data are obtained from SGU (2021) and International Monetary Fund.

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.[6] These are all existing mines, with Kirunavaara opening in the 1860s, and Malmberget in the 1820s. I focus on existing mines instead of the opening or closing of mines since that was rare during this period and does not provide sufficient variation for empirical analysis. 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 consider those municipalities highly specialized in mining, with a high mining employment share in 2003, which can be classified as industrial mining and focused on the exploitation of iron ore: Gällivare and Kiruna. Choosing treated units based on their high share of employment in the industry is a common approach in the empirical literature about resource booms (Black et al., 2005; Kumar, 2017; Jacobsen et al., 2023). Therefore, I consider individuals living in Gällivare and Kiruna (mining municipalities) as treated, which are the municipalities expected to be more affected by the mining boom.

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

[6]There are other small mines in other municipalities, not considered in the study due to their size and because they are located in different parts of Sweden in terms of demographics and labor market. Other than Gällivare and Kiruna, the other eight municipalities that have mines during the mining boom period are Lycksele, Malå, Norsjö, Skellefteå, Sorsele, and Storuman in Västerbotten County, Askersund in Örebro County, and Hedemora in Dalarna County. Online 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 (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.

findings.

A growing body of evidence documents the labor market effects of mining booms in Sweden. Tano et al. (2016) finds positive effects on employment and labor income in Swedish mining municipalities among mining and construction workers, while Moritz et al. (2017) documents population growth and improved labor market conditions in these areas. Haikola and Anshelm (2020) highlights how the volatility of global iron ore prices influenced local attitudes toward state involvement and economic policy in mining communities. More recently, Rodríguez-Puello and Rickardsson (2026) uses 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 rely on geocoded register data that originate from various administrative registers managed by Statistics Sweden. The data is 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 includes information on employment, occupation, economic sector, and region of residence and work, and I focus on the period 2000-2015.[7] These data have been linked to the Swedish Conviction Register, maintained by the National Council for Crime Prevention (Brottsförebyggande rådet - BRÅ). These data contain comprehensive details concerning criminal convictions at the individual level during this period. It includes information on the type of crime and the date of the crime, among other information. A single conviction may encompass multiple crimes, and I observe all crimes within a given conviction. It excludes minor offenses such as speeding tickets, but includes offenses such as driving without a license and DUI.

I restrict the sample to young males: males older than 18 years and under 29 years

[7]The analysis ends in 2015 for several reasons. First, statistics of reported crimes might not be entirely comparable for a large period of time due to changes in the counting and judicial system. In addition, at about this time, Europe and Sweden experienced the start of a migration crisis (Puschmann et al., 2019; Gamalerio et al., 2023), where refugees were disproportionately placed in peripheral and rural areas (Wennström and Oner, 2019); therefore, including this period in the analysis could lead to confusion about the impact of the mining boom and the migration crisis.

who appear in five or more annual observations consecutively in the sample. Young males are the demographic group with the highest baseline conviction rates and the strongest responsiveness to local labor market conditions.[8] Moreover, I consider individuals located in Gällivare and Kiruna municipalities, in Norrbotten County, as treated due to the high presence of mining in the territory and labor market, and because they had at least one operating iron ore mine during the mining boom period, representing more than 10% of employment in the mining sector. To ensure that individuals in the treated and control groups are not only similar but also geographically close, I define the control group as those located in Norrbotten County. Therefore, all individuals located in municipalities other than Gällivare and Kiruna in the county are considered controls.[9] 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 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. Online Appendix Figure B.4 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

[8]Online Appendix Figure B.3 shows the conviction rates by gender and age as a descriptive pattern for the Swedish data, and all types of crimes. 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 are disproportionately responsible for overall crime levels, and most variation in criminal activity is concentrated in this demographic. I also examine males aged 30–39 as a falsification exercise, as shown in Online Appendix C. There are no statistically significant effects of the mining boom on criminal behavior for this older group across any crime type.

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

is the true driver of the results.[10]

In the main analysis, the main outcomes of interest that reflect the criminal behavior of individuals are: (1) being convicted of property crime, (2) violent crime, (3) traffic crimes, and (4) substance-related crimes, per year. 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.[11] 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.[12] See Table A.2 in the Online Appendix for a detailed description of each category and subcategory of crime.[13] Online Appendix Table A.3 presents summary statistics for treated and controls before (2000-2003) and after (2004-2015) the mining boom. The two groups are balanced in terms of demographics and job characteristics, confirming the expectations about the similarity of individuals in the treatment and control groups before the boom. The summary statistics show similar crime levels among treated and controls. The main sample consists of 27,525 individuals (440,402 individuals-year observations). Moreover, I identify 15,108 migrants (241,725 migrant-year observations).

4 Empirical framework

Understanding the effects of economic opportunity from mining on criminal behavior is challenging from an empirical standpoint. Empirical literature often argues that resource endowments are exogenous because they occur due to chance and not so much to the political and economic environment in the host country. Therefore, according to Brunnschweiler and Bulte (2008) and Van der Ploeg and Poelhekke (2010), they are considered good measures of exogenous variation in resource wealth. Nevertheless, recent literature challenges this view. Although the location of natural resources is considered exogenous since it is determined by local geology and natural characteristics (Brunnschweiler and Poelhekke, 2021), the opening

[10] Online Appendix Figure B.5 shows the distribution of individuals in space according to their location and distance to the nearest mine and their distribution in the rings.

[11] 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.

[12] The administrative conviction records do not allow for a consistent separation between possession, use, and distribution within narcotics offenses. As a result, substance-related crime is analyzed as a composite outcome.

[13] 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.

of mines and finding these resources is not completely exogenous, as it often relies on foreign firms to provide capital and expertise or on local governments' investment in exploration (Cust and Harding, 2020; Brunnschweiler and Poelhekke, 2021). Moreover, minerals are typically found in places that are remote and rugged; therefore, settlements driven by natural resources are highly heterogeneous from other communities (Asher and Novosad, 2023). Those features are not orthogonal to the labor market conditions or crime; hence, regressions of such outcomes on mining availability may be biased due to omitted variables and/or reverse causality.

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 generalized difference-in-differences framework 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:[14]

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

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). $Post_t$ 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.[15] In

[14]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).

[15]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

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.[16]

4.1 Identifying assumptions

The assumptions behind the DID approach are that, in the absence of the mining boom, residents' criminal behavior in mining municipalities would have changed similarly over time with residents' criminal behavior in control municipalities (parallel trends) (Meyer, 1995), pre-periods are not affected by treatment (no anticipation), and an individual's treatment status does not affect the potential outcome of another ("stable unit treatment value assumption", SUTVA). I check these assumptions 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. 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

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.

[16] 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: $d \in \{(0, 20], (20, 40], (40, 60], (60, 80], (80, 236]\}$.

specification, I include the same fixed effects to account for omitted variables and isolate the effect of the event.

Second, 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.[17] 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 supports 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. Online Appendix Table A.3 shows that individuals in the pre-boom years are close to each other in observed characteristics. More importantly, in terms of trends, Online Appendix Table A.4 shows the changes in individual characteristics between 2000 and 2003 for treated individuals compared with control individuals and the mean difference test. I do not find any economically meaningful differences in trends across groups, and only a few characteristics have p-values less than 0.05. Finally, an additional concern is endogenous self-selection, where individuals may have chosen to migrate to the mining area, anticipating that the move would improve their living conditions. 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).

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 5.6.

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

Column (1) shows the results for being convicted of property crime, column (2) violent crime, column (3) traffic crimes, and column (4) substance-related crimes.[18] 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. These findings are in line with recent micro-level evidence (e.g., Axbard et al., 2021; Street, 2025). For example, Axbard et al. (2021) finds 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, there is no significant effect on violent crimes (Column (2)), suggesting that positive local economic shocks do not directly change interpersonal violence. There is no effect on traffic-related crimes (Column (3)), and the coefficients are small and imprecise.

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 (4)). I observe an increase of 0.46 percentage points in the probability of being convicted of a substance crime among treated individuals relative to their non-treated counterparts. From a baseline sample mean of 0.002, this estimate translates to a 181% increase in individuals convicted and is statistically significant at the 5% level. 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 in subsection 5.5. On the contrary, a very small share of convictions are alcohol-, tobacco-, and doping-related offenses. 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). There is no significant effect of the mining boom on young males between 30 and 39 years old (Online Appendix Table C.1).[19]

[18]Results for a combined "any crime" outcome show a small and statistically insignificant effect, as the reduction in property crime and the increase in substance-related crime largely offset each other. The results for this outcome are available upon request. This reinforces the importance of disaggregating by crime type, as aggregate measures obscure the opposing behavioral responses documented in this paper.

[19]In an earlier version of the paper, I used the number of crimes reported to the police per 100,000 inhabitants in Gällivare municipality, and the synthetic control method to consider the relationship between mining booms and crime rates. I found similar results, but less precise and significant: the mining boom improves the labor market conditions of mining municipalities, which translates to reductions in total crime at the end of the sample period (2013, 2014, and 2015). However, using aggregate data may introduce bias

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

	(1) Property crime	(2) Violent crime	(3) Traffic crime	(4) Drug crime
Post*Treated	-0.0066**	0.0018	-0.0017	0.0046**
	(0.0027)	(0.0016)	(0.0020)	(0.0018)
Individual FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Grid FE	Yes	Yes	Yes	Yes
Nxt	230480	230480	230480	230480
N	14405	14405	14405	14405
Mean dep. var (2000-03)	0.0125	0.0060	0.0097	0.0025
Effect relative to the mean (%)	-52.40	29.18	-17.06	181.24
R-squared	0.2929	0.2195	0.2708	0.3759
Within R-squared	0.0001	0.0000	0.0000	0.0000

Notes: Two-way fixed effects panel data regression. Standard errors (in parentheses) are clustered at the grid level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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 6.2.

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 pretrends. 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

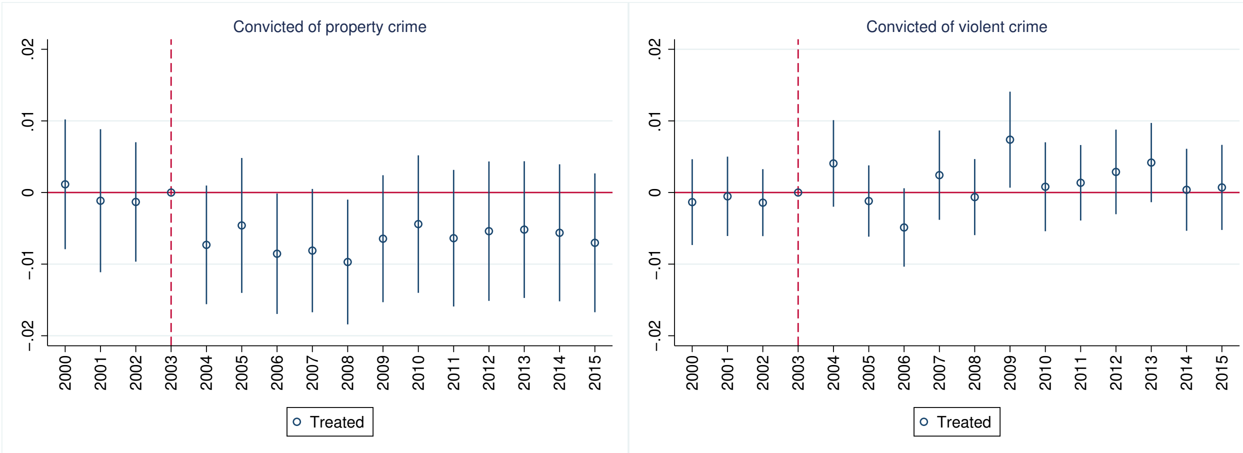
to the results, such as measurement error in crime reports, unobserved omitted factors, given the large heterogeneity between mining municipalities and other municipalities considered for the synthetic control, and compositional changes due to migration. This reinforces the benefit of using detailed administrative data on criminal convictions, which allows addressing several identification challenges and analyzing in depth both mechanisms and treatment effect heterogeneity. Results are available in Rodríguez-Puello (2024).

boom. Thus, they provide support for the use of a DID empirical strategy.

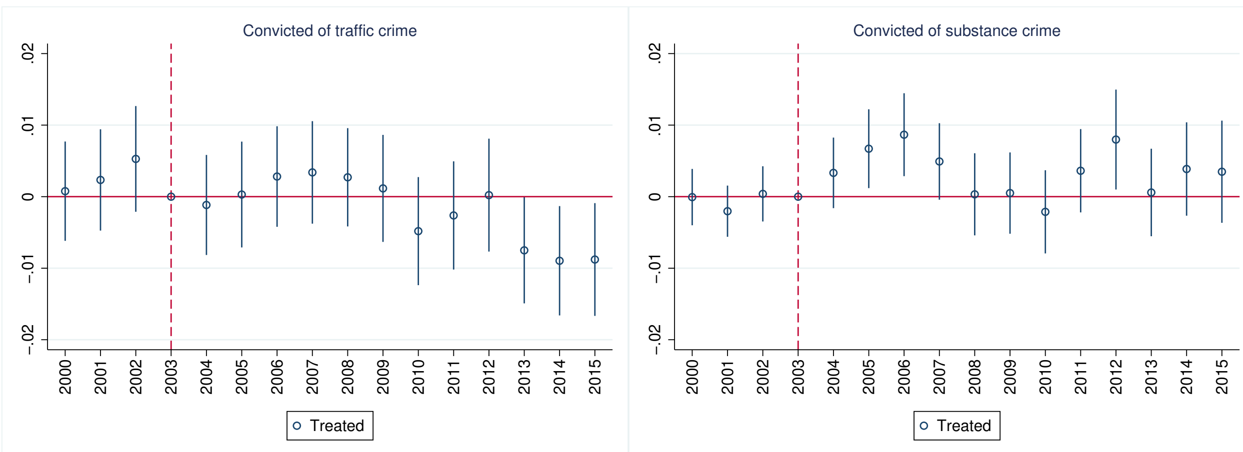
After 2004, I observe that the probability of being charged with a property crime decreases for individuals located in the mining municipalities compared to those in the control group. This effect disappears after 2009, becoming statistically insignificant, which coincides with the timing of the global 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.[20]

[20] In addition, I estimate a dynamic DID to validate the parallel trends assumption for the spatial heterogeneity treatment (Table A.5), which is in the Online Appendix Figure B.6. Importantly, the coefficients for years 2000-2002 (before the shock), which allow us to test the presence of parallel pretrends, 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.

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

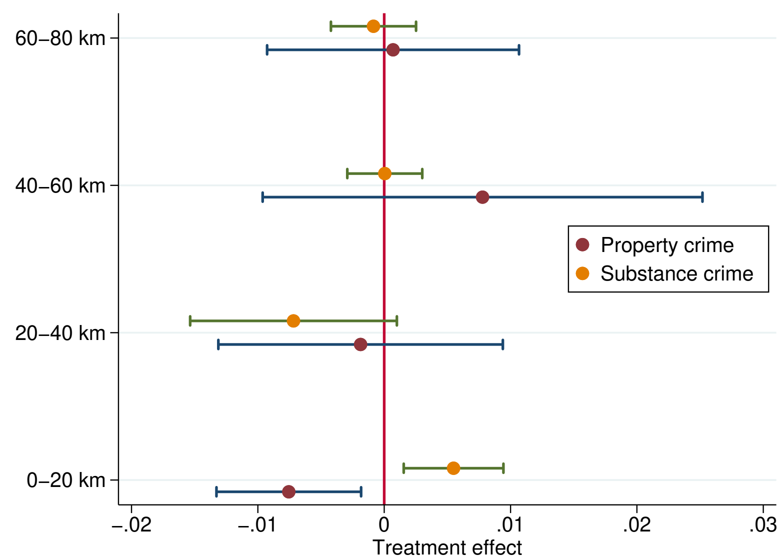
Notes: Year 2003 is the reference. 95% confidence interval shown. Estimations include individuals, grid, and time fixed effects. The sample excludes the migrants to the mining area. Standard errors are clustered at the grid level.

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.[21] 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

[21]The coefficient estimates, standard errors, and sample descriptives corresponding to the figure are reported in the Online Appendix Table A.5.

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 relative to their non-treated counterparts. 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.

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



Notes: Two-way fixed effects panel data regression. Each point represents the estimated DID coefficient for residents in the indicated distance ring, with those living beyond 80 km as the reference category. 95% confidence intervals shown. Estimations include individual, grid, and time fixed effects. Standard errors are clustered at the grid level.

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.

5.2 Detailed criminal behavior

I take advantage of the panel structure of the data and the detailed criminal information to construct additional outcomes that reflect more in detail the criminal behavior of young males as a response to the mining boom (Table 2). I focus on property crimes and substance-related crimes. I construct two distinct binary outcomes capturing different types of criminal behavior (e.g., Britto et al., 2022; Grenet et al., 2024). First, I classify individuals as first-time offenders, which is an indicator equal to one in the first year in which an individual is convicted, with no prior convictions in the panel. Second, re-offense captures subsequent convictions following an earlier conviction and reflects persistent or repeated criminal behavior. Individuals with no convictions across all years constitute the reference group for these outcomes. These outcomes allow for a richer analysis of how the mining boom affects the nature and intensity of criminal activity, differentiating between initial criminal engagement and repeat offending.

The results reveal important heterogeneity (Table 2). The reduction in property crime convictions is concentrated among first-time offenders (Column 1), with no significant effect on re-offense (Column 2). This suggests that improved labor market conditions deter initial entry into economically motivated crime, while individuals with prior convictions are less responsive to changes in local economic conditions. This result is contrary to Britto et al. (2022), who find that crime increases for both first-time offenders and re-offenders after a job loss. For substance-related crimes, the pattern is reversed: the increase is concentrated among re-offenders (Column 4), suggesting that the boom intensifies substance-related criminal activity among individuals with pre-existing involvement rather than inducing new entry. This asymmetry between property and substance crime, where entry into property crime decreases but repeated substance offending intensifies, 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, deterring initial engagement (Becker, 1968; Draca and Machin, 2015), substance-related offenses appear to be driven by income and consumption effects that reinforce existing behavioral patterns (Cunningham et al., 2020).

Table 2: Impact of the mining boom on detailed criminal behavior, 2000-2015

	(1) First-time convicted Property crime	(2) Re-offense Property crime	(3) First-time convicted Substance crime	(4) Re-offense Substance crime
Post*Treated	-0.0060**	-0.0006	0.0012	0.0034**
	(0.0025)	(0.0014)	(0.0012)	(0.0014)
Individual FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Grid FE	Yes	Yes	Yes	Yes
Nxt	230480	230480	230480	230480
N	14405	14405	14405	14405
Mean dep. var (2000-03)	0.0102	0.0023	0.0020	0.0006
Effect relative to the mean (%)	-58.87	-24.94	62.27	586.46
R-squared	0.1605	0.3358	0.1710	0.3654
Within R-squared	0.0001	0.0000	0.0000	0.0000

Notes: Two-way fixed effects panel data regression. Standard errors (in parentheses) are clustered at the grid level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Next, I create three additional binary outcomes capturing different types of criminal behavior and the role of incarceration (e.g., Britto et al., 2022; Grenet et al., 2024). First, a binary indicator reflecting those individuals convicted of any crime and not sentenced to prison, which represents the majority of convicted individuals. Second, conviction with prison indicates individuals who are convicted and simultaneously receive a prison sentence in that year, serving as a proxy for more serious offenses or incapacitation. And third, post-prison reoffense identifies individuals who reoffend in any year following a previous prison sentence, isolating patterns of reentry into criminal activity post-incarceration.[22] Individuals with no convictions across all years constitute the reference group for these outcomes, which are mutually exclusive.

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). 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 incarceration is less elastic to local labor market conditions. This is in line with Agan and Makowsky (2023), who finds 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.

[22]It is important to note that this measure of recidivism is an "ever recidivist" 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 3: Impact of the mining boom on detailed criminal behavior (the role of prison), 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
Nxt	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: Two-way fixed effects panel data regression. Standard errors (in parentheses) are clustered at the grid level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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).[23] 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). Online Appendix D provides additional details on the estimation procedure.

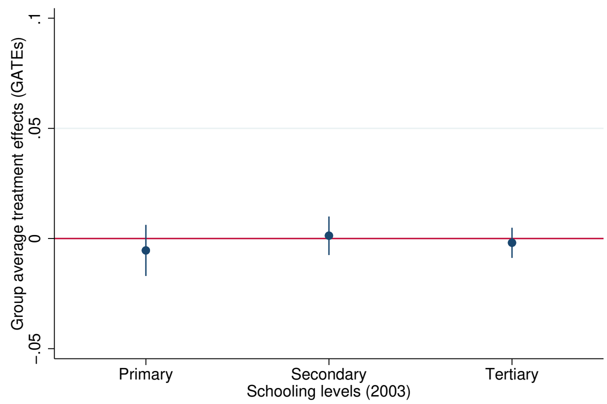
I find substantial heterogeneity in treatment effects. Online Appendix Figure B.7 presents the distribution of IATEs in deciles and compares the causal forest ATE to the DID estimates from Table 1. For property crime, the causal forest ATE is larger in magnitude than the DID estimate, indicating that 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 crime by individual characteristics: educational level, income distribution, employment status, and economic sector. The reduction in property crime is concentrated among young males with primary educational levels, those who are non-employed, and those in the low tail of the earnings distribution in 2003. This pattern is consistent with the economic model of crime: individuals with low educational

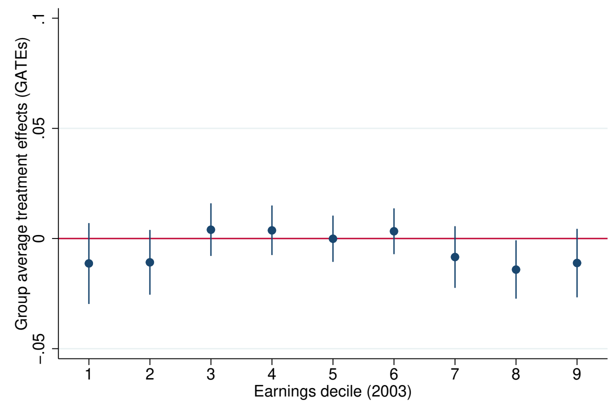
[23] To avoid endogenous movement across categories, individuals are classified in their education level, earnings decile, employment status, and economic sector according to their information in 2003. I analyze heterogeneity in educational levels (primary, secondary, and tertiary), across the income 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).

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 finds that the crime-reducing effects of mining wealth in South Africa operate through employment opportunities in sectors accessible to low-skilled workers.

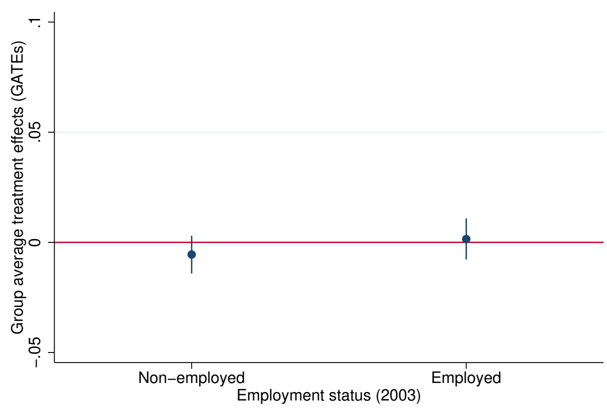
Figure 4: Group average treatment effects (GATEs) by characteristics for property crime, 2000-2015



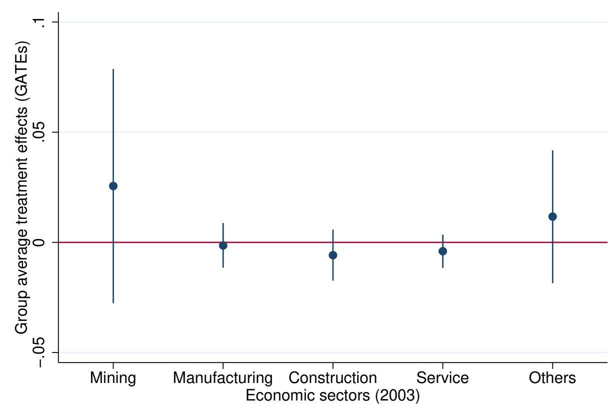
(a) Schooling levels



(b) Yearly earnings decile



(c) Employment status



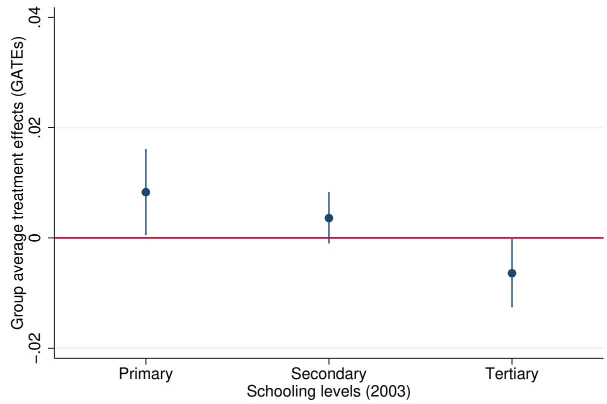
(d) Economic sectors

Notes: This figure shows the mean predicted Conditional Average Treatment Effects (CATE) over individual-level characteristics. GATEs are estimated using causal forest algorithms. 95% confidence interval shown.

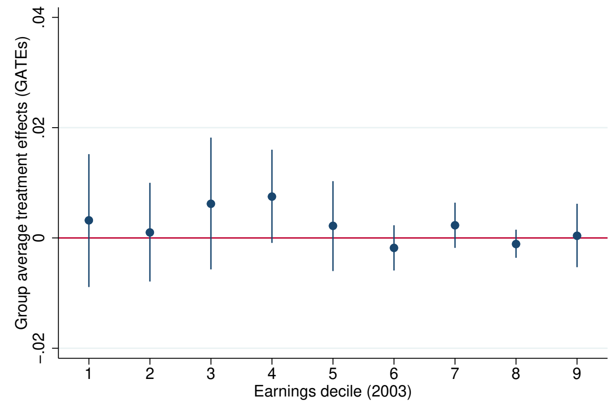
Figure 5 presents the corresponding GATEs for substance crime. In contrast to property crime, the increase in substance-related convictions is concentrated among young males with primary educational levels, employed directly in the mining sector, and in the high tail of the earnings distribution in 2003. There is a reduction in the probability of being convicted of substance crimes for young males with a tertiary educational level. The concentration among those employed in mining and in the upper part of the earnings distribution 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.

Taken together, the causal forest results reveal that the same economic shock produces opposing crime responses through distinct channels. The individuals who reduce property crime (those with the weakest labor market attachment) are not the same individuals who increase substance use (those with higher earnings and direct mining employment). This separation reinforces the interpretation that property crime responds to opportunity costs while substance crime responds to income and consumption effects.

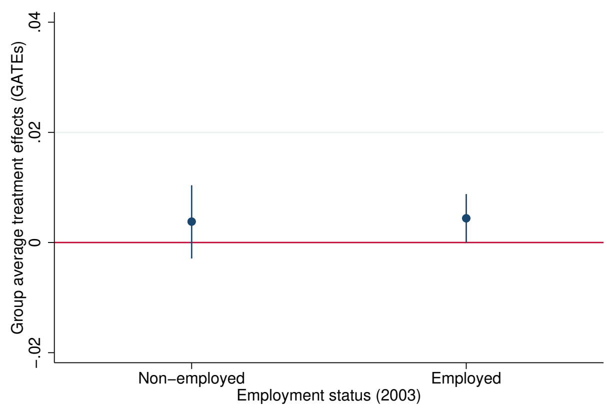
Figure 5: Group average treatment effects (GATEs) by characteristics for substance crime, 2000-2015



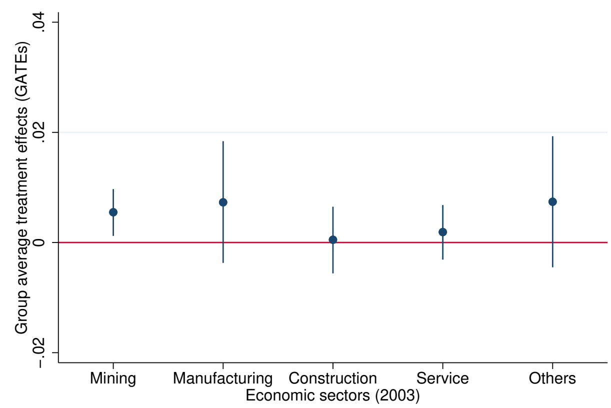
(a) Schooling levels



(b) Yearly earnings decile



(c) Employment status



(d) Economic sectors

Notes: This figure shows the mean predicted Conditional Average Treatment Effects (CATE) over individual-level characteristics. GATEs are estimated using causal forest algorithms. 95% confidence interval shown.

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 I do not observe the reasons for migrating, I make a series of conservative assumptions to analyze migrants. As mentioned above, I define migrants as those individuals who moved to Norrbotten County in 2004 or later. I assume that those who migrated to this area after the boom did so in response to improved labor market conditions.

There are notable differences in demographic characteristics between residents and migrants to the mining area (Online Appendix Table A.6).[24] 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 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)–(4) 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 (5)–(8) 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.

The results indicate that migration is associated with changes in criminal behavior, but the role of the mining boom differs across crime types. Columns (1)–(4) show that, following migration, young male migrants experience an increase in property crime convictions both in mining and in control municipalities. The magnitude is slightly larger in mining municipalities, but a statistically significant increase is also present for migrants to the control areas, suggesting that the rise in property crime is largely driven by migration itself rather than by exposure to the mining boom. For violent crime, post-migration convictions increase for migrants in the control municipalities. A similar pattern emerges for substance crime: post-migration convictions decrease for migrants in the control municipalities, while no significant change is observed in mining municipalities. This suggests no meaningful differential effect attributable to mining activity. For traffic crimes, there is no evidence of systematic changes

[24]The sample for this descriptive table includes residents and migrants between 18-39-year-old males.

following migration in either type of municipality.

Columns (5)–(8) directly compare migrants who move to mining municipalities with those who move to non-mining municipalities, isolating the additional effect of the mining boom. These estimates show no statistically significant differential effect of mining exposure on property, violent, or traffic crime convictions among young male migrants. In contrast, column (8) reveals a positive and statistically significant effect on substance-related crime convictions: migrants relocating to mining municipalities are more likely to be convicted of substance crimes relative to migrants settling in control municipalities. While the baseline probability of substance convictions among migrants is low, the relative effect is sizeable, suggesting that mining-driven local conditions amplify substance-related criminal activity among migrants.

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

	(1) Property crime	(2) Violent crime	(3) Traffic crime	(4) Substance crime	(5) Property crime	(6) Violent crime	(7) Traffic crime	(8) Substance crime
Post*Migrants (Mining mun.)	0.0107**	0.0041	-0.0084	-0.0006	0.0017	-0.0039	-0.0081	0.0105*
	(0.0052)	(0.0033)	(0.0052)	(0.0051)	(0.0058)	(0.0039)	(0.0058)	(0.0057)
Post*Migrants (Control mun.)	0.0093***	0.0070***	0.0010	-0.0078***				
	(0.0033)	(0.0022)	(0.0028)	(0.0029)				
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Grid FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Nxt	155753	155753	155753	155753	50603	50603	50603	50603
N	9735	9735	9735	9735	3163	3163	3163	3163
Mean dep. var (2000-03)	0.0616	0.0205	0.0446	0.0466	0.0169	0.0063	0.0108	0.0058
Effect relative to the mean, Treated (%)	17.32	19.82	-18.78	-1.20	9.92	-61.02	-74.81	181.32
Effect relative to the mean, Control mun. (%)	15.12	34.27	2.26	-16.69	0.00			
R-squared	0.4892	0.3494	0.4901	0.5258	0.4455	0.3270	0.3891	0.4634
Within R-squared	0.0001	0.0001	0.0000	0.0000	0.0000	0.0000	0.0001	0.0001

Notes: Two-way fixed effects panel data regression. Migrants before the move are the references. Standard errors (in parentheses) are clustered at the grid level. Columns (1)-(4) compare migrants to the mining municipalities or the control municipalities to themselves before the migration event. Columns (5)-(8) compare migrants to the mining municipalities to migrants to the control municipalities. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Overall, these results imply that the mining boom does not broadly increase criminal behavior among migrants across all crime categories. Instead, its effects are concentrated in substance-related offenses, consistent with mechanisms related to increased local income, demand, or illicit market activity in mining areas, rather than generalized increases in criminality driven by migration alone. Regarding other types of crime, the evidence suggests that the economic benefits associated with the boom, such as increased employment and earnings opportunities as documented in Rodríguez-Puello and Rickardsson (2026), mitigate potential criminal behavior among migrants to the mining municipalities. These results underscore the importance of distinguishing between individual- and aggregate-level analyses in evaluating the impacts of local economic shocks, and complement Street (2025), who shows that aggregate crime increases in US fracking areas are largely driven by compositional changes from in-migration, while crime among pre-existing residents declines.

5.5 Additional evidence

Further evidence on property crime. Disaggregating property crime by legal category reveals broadly similar declines across several types of property-related offenses (Online Appendix Table A.7). 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. While the estimate for these theft and appropriation offenses is negative but not statistically significant, the magnitude is comparable to those observed for other property crime categories. 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. For both of these categories, the estimates indicate statistically significant declines during the mining boom. Taken together, the results point to a broad reduction in property-related criminal behavior rather than effects concentrated in a single offense type, and are consistent with improved labor market conditions reducing a range of property-related offenses through increased economic opportunities.

Alcohol-related offenses and intoxicated driving. Given the observed increase in substance-related crime, a natural question is whether the mining boom also affected alcohol-related criminal behavior (Tynan et al., 2017). I separately analyze convictions under alcohol-related legislation, which include illegal production, possession, or sale of alcohol. The results show no statistically significant effects of the mining boom on alcohol-related convictions or alcohol-related suspicions.[25] Moreover, alcohol-related offenses account for only a very small

[25] These measures using conviction registers might be reflecting data limitations rather than an actual absence of behavior. For instance, alcohol offenses often result in fines rather than convictions. Therefore, I also use suspicion data to check the robustness of these results. Suspicion data is closer to what police observe on the ground, capturing disorderly conduct that never reaches the proof required for a court conviction. The

share of substance-related convictions, with the vast majority falling under the Narcotics Law (approximately 95%). I also examine intoxicated driving, a related behavior that might be affected by the mining boom. Traffic crime convictions do not separate driving under the influence from other driving-related offenses, so I use information from the Crime Suspicion Register to identify suspected drunk driving or aggravated drunk driving under the influence of alcohol and/or drugs. The results show no statistically significant effects on the probability of being suspected of intoxicated driving. Together, these findings indicate that the increase in substance-related crime is not driven by alcohol-related offenses or intoxicated driving, but instead reflects changes in narcotics-related convictions.

Decomposing narcotics-related crime. To further understand the increase in narcotics-related crime, I use information from the Crime Suspicion Register, maintained by the National Council for Crime Prevention. I decompose narcotics-related suspicions into production, selling, and holding/use offenses.[26] Online Appendix Table A.8 shows that the overall increase is driven primarily by holding and use, with smaller increases in distribution and production from very low baseline levels. This pattern is consistent with increased local demand and market activity rather than widespread entry into drug production or trafficking. This interpretation aligns with existing evidence that positive resource shocks can increase engagement in risky or addictive behaviors, even as they reduce economically motivated crimes such as property offenses (e.g., James and Smith, 2017; Cunningham et al., 2020; Axbard et al., 2021).

Gender-based violence. Given the increase in substance-related crime and the highly male-dominated nature of the mining sector, a concern is whether the boom translated into higher levels of gender-based violence. Theoretically, the expected effects are ambiguous: substance use could raise the risk of violence, while improved labor market opportunities could reduce stress and lower violent behavior. I use information from the Crime Suspicion Register, which provides information on victim gender, allowing identification of suspected offenses involving violence against women.[27] I find no statistically significant effects of the mining boom on suspected violence against women. Such cases are relatively rare in the sample, accounting for less than one percent of young males, which limits statistical power.

Health-related outcomes. An additional concern is whether the mining boom affected

null results are robust.

[26] The outcomes are based on police suspicion records and capture suspected involvement in narcotics-related offenses. Base rates for production and distribution are very low; estimates should be interpreted with caution.

[27] This approach is necessary because the conviction register does not consistently identify the gender of the victim. A limitation is that the data do not allow for a more precise separation of intimate partner violence from other forms of violence against woman. The outcome is equal to one if the individual is suspected of the following crimes: assault against a woman, (attempted) murder of woman, unlawful threat against a woman, stalking of a woman, molestation of a woman, sexual molestation of a woman, (attempted) rape of a woman or a person of unspecified gender, and violation of the restraining order act.

health-related outcomes, including mental health or risky behavior that may not result in criminal convictions. For example, Shandro et al. (2011) finds 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.[28] The analysis reveals no statistically significant effects of the mining boom on the incidence of long-term sick leave or sickness benefit payments among young male residents. While the findings suggest no detectable impact of the boom on severe health-related work absences, richer health data would be required to assess more immediate or short-term health responses to local economic expansions.

5.6 Robustness checks

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

By treated municipality. As the first alternative specification, I estimate the main results individually for Gällivare and Kiruna (Online Appendix Table A.9). This is important because, while both municipalities are heavily dependent on mining, they are different in other aspects. For instance, in Kiruna, in 2004, the government made a plan to move the city of Kiruna 4 kilometers east, a process that started in 2013. The main reason was the security of the population because years of mining had caused the town to sink into the ground. This policy may affect individuals' behavior, the labor market, and crime in the municipality. It may have created substantial social and demographic disruption, including population displacement, new housing construction, and temporary inflows of workers. Therefore, I expect weaker or even opposite effects relative to Gällivare, as the relocation may offset the effects of improved labor market opportunities. There is no significant effect on the criminal behavior among young male residents in Kiruna municipality due to the mining boom. The effect observed is concentrated among the residents of Gällivare.

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).[29] It estimates a curve that represents the effect as a function of

[28] 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.

[29] According to Butts (2023), this method is similar to using the distance to the nearest mine as a continuous

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 Online Appendix Table A.10, show that the findings are robust to this empirical strategy.

Travel time treatment. Next, as an alternative to defining treated individuals based on geographical distance, I redefined treatment using travel time by car, measured with OpenStreetMap data using 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. Online Appendix Table A.11 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.

Triple difference-in-differences. I estimate a triple difference-in-differences (DDD) model to further account for unobserved municipality-level shocks (Online Appendix Table A.12). This approach compares changes in criminal behavior before and after the mining boom between treated and non-treated areas, and additionally across employment sectors (public vs. non-public). The triple interaction term isolates whether the mining boom had a differential effect on crime for individuals employed in the public sector relative to others, netting out common time trends, area-specific shocks, and baseline sectoral differences. Following Rodríguez-Puello and Rickardsson (2026), workers in the public sector do not experience any labor market effect from the mining boom, providing a good group for this placebo analysis. As expected, there is no evidence that public-sector workers in mining-exposed areas experienced crime changes that differ systematically from those of non-public workers. The absence of a triple-difference effect implies that public-sector employees are insulated from the mining shock, consistent with greater income stability and weaker exposure to local labor market fluctuations.

Additional robustness checks. Online Appendix Table A.13 shows the robustness checks for the main results of young males (18-29 years old). Column (1) reports the results from the baseline specification for reference, focusing only on the sample of young male residents. In Column (2), I limit the movement of individuals across treated and control municipalities by defining their treated/control status on the municipality of residence in 2003. This change

measure to estimate the "dosage-response" function proposed by Callaway et al. (2024) in the difference-in-differences approach with continuous treatment.

has little effect on the coefficient estimates. In Column (3), 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. In Column (4), I estimate the results by also including migrants. As noted above, by separating the effect for residents and migrants, I can exclude crimes committed in the mining municipalities by new individuals who migrated to the relatively stronger labor markets looking for better opportunities. In this way, I can distinguish the effect of the economic shock from the impact of the changing demographics on overall crime rates. The inclusion of migrants in the analysis of the individuals' behavioral change in crime does not change the results.

In Column (5), I restrict the sample to a balanced panel to improve the stability across time in the sample size and follow individuals throughout the whole period. The results are robust to this restriction. In Column (6), I exclude residents located in the four neighboring municipalities, which are most prone to spillovers, to check for the SUTVA assumption. The results remain robust, providing evidence of no spillover effects to neighboring municipalities. A possible reason is that population density in northern Sweden is low, and the municipalities cover large geographical areas. Finally, in column (7), grid fixed effects are replaced with municipality fixed effects to account for possible confounding omitted variables at the municipality level, and the results remain robust.

6 Contextualization and mechanisms

6.1 Social cost effects

Online Appendix Table A.14 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 boom, obtaining the aggregate social cost effect (total welfare implication) of the mining boom.

The results show a mixed picture. Consistent with the regression estimates, reductions in property crimes translate into sizeable 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 underscore that the mining boom had heterogeneous welfare implications. On balance, the largest and most robust effects come from reductions in property crimes, which dominate the aggregate social savings. At the same time, the rise in substance-related crime partly offsets these benefits.

6.2 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. To illustrate the potential pathways linking the mining boom to changes in criminal behavior, Online Appendix Figure B.8 presents a Directed Acyclic Graph (DAG) that maps out the main hypothesized mechanisms. 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 distinguish across them or rule out the possibility that there are other intermediating variables at work.[30]

6.2.1 Property crime: the opportunity cost channel

Labor market opportunity cost. Established literature shows that local communities exposed to resource shocks tend to experience improvements in labor market conditions (Corden and Neary, 1982; Sachs and Warner, 2001; Allcott and Keniston, 2018).[31] Several empirical papers find positive effects on employment (Black et al., 2005; Pérez-Trujillo and

[30] 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.

[31] When there is a mining boom, due to an increase in international prices, revenues in the resource sector will increase, generating a shift from the nontradable sector to the export-oriented tradable (resource) sector. This economic movement would cause a positive shift in the demand for labor in the resource sector. As a result, employment, wages, and earnings are expected to increase in local communities affected by the boom (Corvalan and Pazzona, 2019; Chávez and Rodríguez-Puello, 2022), especially in the resource sector. Due to spillover effects between economic sectors, the boom may affect sectors beyond extraction. For example, sectors directly linked to the extractive sector as input providers would eventually experience an increase in demand due to the higher employment in the area, leading to an overall positive effect on the labor market of residents extended throughout the local economy.

Rodríguez-Puello, 2022) and earnings (Weber, 2012; Chávez and Rodríguez-Puello, 2022). As discussed in Section 2, previous research has documented significant positive labor market effects of the Swedish mining boom (Tano et al., 2016; Rodríguez-Puello and Rickardsson, 2026). The link between labor market conditions and crime has also been explored (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). Therefore, labor market conditions constitute a natural channel through which a mining boom may have affected crime. If individuals face improved labor markets, the returns to legal activity increase, and individuals should substitute away from illegal activities.

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). Online Appendix Figure B.9 provides a visual overview of the labor market trends in mining versus non-mining municipalities over the study period. 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.[32] Consistent with previous work, the mining boom 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.[33] The results show that a mining boom has positive effects on labor market conditions in the Swedish case, as noted in previous literature (Tano et al., 2016; Rodríguez-Puello and Rickardsson, 2026). This may shift the cost-benefit calculation away from criminal activity, thereby reducing local crime levels (Draca and Machin, 2015; Edmark, 2005; Axbard et al., 2021). This mechanism seems to be dominating over the one that suggests that a mining boom that increases earnings generates higher benefits to committing crimes because now people are wealthier, increasing the payoff of crime.

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

[33] Rodríguez-Puello and Rickardsson (2026) finds 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.

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. It is important to note that 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.

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 (8) and (9) of Table 5 show that the mining boom significantly reduces the probability of being in the bottom earning tercile (Column (6), significant at the 5% level) and weakly increases the probability of being in the top earning tercile (Column (7), 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 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.

Table 5: Mechanisms: impact of the mining boom on different mechanisms, 2000-2015

	(1) Disposable income	(2) Labor income	(3) Lab. inc. employed	(4) Employment	(5) Employment mining	(6) Bottom earning tercile	(7) Top earning tercile	(8) Police occupation	(9) Police industry
Post*Treated	17.9232***	28.3618***	26.1973***	0.0186**	0.0869***	-0.0108**	0.0071*	0.0033	0.0026
	(1.4464)	(2.2079)	(2.2928)	(0.0094)	(0.0068)	(0.0054)	(0.0040)	(0.0023)	(0.0021)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Grid FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Nxt	230480	230480	152308	230480	230480	124267	124267	187619	230480
N	14405	14405	9519	14405	14405	7767	7767	11726	14405
Mean dep. var (2000-03)	107.6874	114.5652	172.4796	0.6225	0.0219	0.4136	0.2197	0.0019	0.0052
Effect relative to the mean (%)	16.64	24.76	15.19	2.99	396.29	-2.62	3.23	170.34	50.07
R-squared	0.6161	0.7432	0.7152	0.5618	0.6831	0.9091	0.9176	0.5546	0.5200
Within R-squared	0.0007	0.0025	0.0024	0.0000	0.0083	0.0001	0.0001	0.0001	0.0000

Notes: Two-way fixed effects panel data regression. Disposable income and labor income expressed in 1000 SEK and in real values with the base year 2000. Standard errors (in parentheses) are clustered at the grid level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

6.2.2 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 concentrated among repeat offenders and young males employed directly in the mining sector, not among first-time offenders or individuals in the lower tail of the earnings distribution. This pattern points away from the opportunity cost channel and toward income and consumption effects operating among individuals already embedded in substance use.

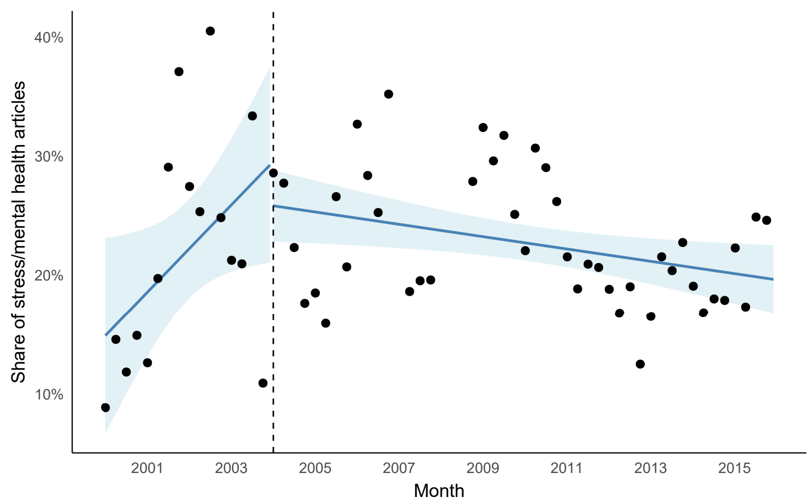
Four mechanisms could plausibly explain this pattern: (i) an income effect, whereby higher earnings relax the budget constraint for drug and alcohol purchases; (ii) workplace culture specific to male-dominated extractive industries; (iii) occupational stress from the rapid expansion of operations; and (iv) increased employer drug testing and occupational health screening, which would raise the detection rate of substance offenses without any behavioral change. I cannot distinguish all four channels cleanly, but I provide suggestive evidence on the income and occupational stress channels.

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 simultaneously relaxes the budget constraint for 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). The first-stage evidence supports this channel directly. Table 5, Columns (1) and (2) show that the boom raised disposable income by 17 percent relative to the pre-boom mean, and labor income by 24.8 percent among treated young male residents. The causal forest results in Figure 5 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.

Occupational stress. A second channel operates through occupational stress. Workers in extractive industries face physically demanding conditions, elevated injury risk, and the psychological pressures of a high-intensity work environment. These are well-documented conditions that increase substance use as a coping response, even among individuals with rising incomes (Shandro et al., 2011). I provide suggestive evidence on the 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 Online 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 6 plots the series alongside the fitted values. The share was rising in the pre-boom period, averaging approximately 15 percent in 2000 and reaching roughly 22 percent by late 2003. At the onset of the boom, however, there is no significant level shift: the estimated immediate level shift is $\hat{\beta}_1 = -0.037$, indicating no detectable change in the share of articles mentioning occupational stress or mental health. The pre-boom trend is positive ($\hat{\beta}_2 = 0.003$), while the post-boom slope is negative ($\hat{\beta}_3 = -0.003$), implying a reversal of the pre-boom trend after 2004. The null result is robust to weighting by article count (Online Appendix Table A.15). I interpret this as providing no support for an occupational stress channel. However, more research is needed in this area.

Figure 6: Interrupted time series of stress/mental health newspaper articles, 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. 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 Online Appendix Table A.15 for coefficient estimates and Online Appendix F for data and estimation details.

Among the four mechanisms, the income effect is the most coherent given the overall pattern of results: property crime falls (the opportunity cost of crime rises with employment and earnings), while substance-related crime rises (purchasing power directly enables drug purchases). The absence of a detectable increase in stress-related media coverage further narrows the set of plausible channels, as it provides no corroborating evidence for the occupational stress hypothesis. The detection channel remains the hardest to rule out: as LKAB's operations scaled up after 2004, stricter safety protocols and mandatory drug testing may have mechanically increased recorded substance offenses. I acknowledge this as an important caveat to the substance crime result.

6.2.3 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 (8) and (9) Table 5).[34] I find no significant change in police employment or resources in the mining municipalities following the boom. 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. A local mining boom, therefore, cannot generate additional police capacity, regardless of its fiscal effects. The existing literature confirms that policing and detection do affect crime when they are meaningfully increased. For instance, Anker et al. (2021) finds that expanding DNA database registration reduces recidivism by around 40%, and Kirchmaier et al. (2020) finds 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.

[34] 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%.

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 and consumption channel: raising earnings relaxes the budget constraint for drug purchases, concentrated among individuals already involved in substance use and directly employed in the mining sector. The occupational stress channel receives no support from the newspaper text analysis. 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.

7 Conclusions

The present study provides evidence of the local effects of a mining boom that started in 2004 on criminal behavior in Sweden. Sweden is a developed country with a long tradition of mining, especially in the North of the country, and, therefore, is subject to both the positive and negative effects of commodity price volatility. The Becker (1968) and Ehrlich (1973) economic theory of crime, and the discussed mechanisms, suggest that there are competing effects that could result in an increase, decrease, or null changes (canceling each other) in the criminal behavior of residents in mining municipalities as a result of the mining boom. These competing theoretical predictions highlight the importance of the empirical analysis of the relationship between a mining boom and local criminal behavior.

More specifically, I exploit the boom in iron ore prices in northern Sweden as a plausibly exogenous shock to local economic conditions, which is similar to local stimulus from large construction or manufacturing projects. Using detailed geocoded administrative data on all criminal convictions and demographics in Sweden from 2000 to 2015, I estimate the effect of improvement in labor market conditions on the criminal behavior of young males using difference-in-differences and event study models. An important strength of this study is that by focusing the analysis on residents already living in the area before the boom, I distinguish the effect of improved economic opportunity from the effect of population inflows on aggregate crime, as Street (2025). Moreover, I contribute by focusing on people rather than places, and estimating the effect more in-depth in different types of crime and demographic sectors, allowing me to separate behavioral responses from compositional changes driven by population mobility.

Results indicate that local young male residents (18-29 years old) experience a decrease in criminal activity during the mining boom. Specifically, I find a decline of 0.66 percentage points in the probability of being convicted of property crimes among treated young males relative to their non-treated counterparts. From a baseline sample mean of 0.012, this estimate translates to a 52% drop in individuals convicted. These results are consistent with recent micro-level evidence. In addition, 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. I observe an increase of 0.46 percentage points in the probability of being convicted of a substance crime among treated individuals relative to their non-treated counterparts. From a baseline sample mean of 0.002, this estimate translates to a 181% increase in individuals convicted. 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.

In addition, I take advantage of the panel structure of the data and the detailed criminal information to construct additional outcomes that reflect in more detail the criminal behavior of young males as a response to the mining boom. Results show that the reduction in property crimes for young males due to the mining boom is concentrated among first-time offenders, suggesting that improved labor market conditions through increased opportunity costs may deter individuals from engaging in crime for the first time. On the contrary, there is no effect on the probability of re-offending, suggesting no broader behavioral responses that include repeat offenders, and individuals with prior convictions are less responsive to local economic changes. Regarding substance crimes, I observe the opposite pattern. The increase in substance crime convictions for young males due to the mining boom is concentrated among re-offenders, suggesting that the boom primarily intensifies criminal activity among individuals with pre-existing involvement in substance-related offenses, rather than inducing new entry. The observed reductions in property crime are consistent with improved labor market opportunities reducing the net returns to economically motivated crime, and in line with previous literature.

To understand this result, the analysis of mechanisms suggests that the mining boom had a direct, significant effect on the labor market, improving the labor market conditions for individuals living in the Swedish mining municipalities. On the other hand, I find no evidence that changes in the population composition due to migration and the government's crime-prevention capacity (police force) drive the crime results. Although the mining boom significantly reduces the probability of being in the bottom-earning tercile and weakly increases the probability of being in the top, this pattern is more consistent with a broad improvement in economic conditions than with a widening of income inequality that could drive criminal behavior. Taken together, these results are consistent with economic opportunities reducing economically motivated crimes. I also observe an increase in substance-related crimes among mining workers, which may be driven by at least three channels: an income effect that relaxes budget constraints for drug purchases, occupational stress from rapid workforce expansion, and increased employer drug testing that raises the detection rate of substance offenses without any behavioral change. Complementary text analysis of LKAB newspaper coverage finds no statistically significant change in stress-related media discourse at the onset of the boom, providing no support for an occupational stress channel.

The income effect remains the most coherent explanation given that property crime falls while substance crime rises, a pattern consistent with the opportunity-cost logic of economic booms, whereby employment raises the cost of criminal activity while higher earnings independently enable substance purchases.

While this study focuses on a developed-country mining context, the results highlight general mechanisms through which local economic shocks affect criminal behavior. The focus on the mining boom as a laboratory to study the effects of economic conditions on criminal behavior is an important natural experiment that works as an opportunity to address concerns about economic shocks in general. Natural experiments classified as exogenous and that occurred in clearly specified local areas are difficult to find, but the mining boom is one such case. The mining boom provides a useful laboratory for studying how localized economic shocks affect criminal behavior, similar to other resource-dependent communities worldwide. 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	Skelleftea	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: Description of crime variables

Crime category	Description
Total violations of the criminal code	Violent crimes Property crimes Crimes against the public Crimes against the state
Violent crimes	1+2+3+4+5
1. Violations of life and health	Completed murder, manslaughter, or assault with fatal outcome. Attempt, preparation, and branding for murder or manslaughter. Child killing.
2. Violations of freedom and peace	Kidnapping, human trafficking, human exploitation. Illegal restraint. Child welfare violation. Unlawful coercion. Serious breach of peace, serious breach of women's peace, unlawful persecution. Unlawful threats. Unlawful use of identity. Illegal invasion of privacy. Molestation. Urge to commit suicide. Reckless solicitation of suicide. Data breach, illegal wiretapping.
3. Defamation	Crime of defamation. Slander, insult, slander of the deceased.
4. Sexual offenses	Rape incl. Bearish. Negligent rape. Rape against children incl. Bearish. Sexual assault incl. gross, negligent sexual assault. Sexual exploitation of children under the age of 18. Sexual assault incl. violently against children under the age of 18. Intercourse with offspring or siblings. Purchase of sexual services, pimping of persons 18 years and older. Exploitation of children for sexual posing, purchase of sexual act of children under 18 years. Sexual harassment, exhibitionism. Contact to meet a child for sexual purposes.
5. Crimes against family	Bigamy, illicit marriage; Undue influence in the adoption of children adoption of children; Distortion of family status.
Property crimes	1+2+3+4+5
1. Theft, robbery and other assault	Theft of motor-driven means of transport. Theft of non-motorized means of transportation. Theft (including burglary). Theft by burglary. Theft without breaking and entering. Robbery without a firearm. Robbery with a firearm.

	Other offenses against the Criminal Code.
2. Fraud and other misconduct	Fraud, petty fraud, gross fraud, gross debt fraud. Other offenses against the Criminal Code.
3. Embezzlement and other faithlessness	Embezzlement, petty embezzlement, gross embezzlement; Wrongful disposal; Misdemeanor; Breach of trust; Abuse of authority.
4. Offenses against creditors, etc.	Misconduct against creditors, gross misconduct against creditors; Aggravation of bankruptcy and executive proceedings; Negligence against creditors; Undue favoring of creditor.
5. Crime of damage	Damage, minor damage, injury, serious damage: on motor vehicles, car fire or other motor vehicle fire, by fire, against state, municipality, county council, other manage, graffiti against public transport.

Notes: Own elaboration using Brå (2023) as a base. For a detailed description of the types of crimes and the Swedish criminal code, consult Brå (2023).

Table A.3: 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	0.009	0.009	0.009	0.011	0.009	0.010
	(0.092)	(0.097)	(0.093)	(0.102)	(0.094)	(0.101)
Convicted violent crime	0.005	0.005	0.005	0.006	0.006	0.006
	(0.069)	(0.068)	(0.069)	(0.076)	(0.076)	(0.076)
Convicted substance crimes	0.002	0.002	0.002	0.007	0.007	0.007
	(0.047)	(0.042)	(0.047)	(0.086)	(0.084)	(0.086)
Convicted traffic crimes	0.008	0.010	0.008	0.009	0.010	0.009
	(0.088)	(0.098)	(0.090)	(0.094)	(0.097)	(0.094)
Married	0.184	0.150	0.178	0.158	0.125	0.153
	(0.387)	(0.357)	(0.383)	(0.365)	(0.331)	(0.360)
Children under 18	0.416	0.411	0.416	0.390	0.375	0.387
	(0.493)	(0.492)	(0.493)	(0.488)	(0.484)	(0.487)
Primary education	0.493	0.602	0.511	0.341	0.390	0.349
	(0.500)	(0.490)	(0.500)	(0.474)	(0.488)	(0.477)
Secondary education	0.409	0.348	0.398	0.551	0.553	0.551
	(0.492)	(0.476)	(0.490)	(0.497)	(0.497)	(0.497)
Tertiary education	0.099	0.050	0.090	0.108	0.057	0.100
	(0.298)	(0.218)	(0.287)	(0.311)	(0.232)	(0.299)
Non-employed	0.270	0.232	0.263	0.251	0.162	0.236
	(0.444)	(0.422)	(0.440)	(0.433)	(0.369)	(0.425)
Primary sector	0.024	0.205	0.054	0.028	0.244	0.064
	(0.152)	(0.404)	(0.226)	(0.164)	(0.429)	(0.244)
Secondary sector	0.268	0.190	0.255	0.269	0.232	0.263
	(0.443)	(0.392)	(0.436)	(0.444)	(0.422)	(0.440)
Tertiary sector	0.439	0.373	0.428	0.452	0.362	0.437
	(0.496)	(0.484)	(0.495)	(0.498)	(0.481)	(0.496)
Nxt	100410	20184	120594	266136	53672	319808

N	25102	5046	30148	22178	4473	26651

Notes: The table shows mean and standard deviations in parentheses. Treated: Gällivare and Kiruna, control: municipalities in Norrbotten County. Individuals who moved to Norrbotten County after 2004 are excluded. 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).

Table A.4: Mean differences of changes (2000-2003) comparing treated and control individuals

	Treated	Control
Convicted property crime	0.00	0.00
Convicted violent crime	0.00	0.00
Convicted substance crime	0.00	0.00
Convicted traffic crime	0.00	0.00
Married	0.03	0.05***
Children under 18	0.03	0.03
Primary education	-0.06	-0.06
Secondary education	0.05	0.01***
Tertiary education	0.02	0.05***
Non-employed	-0.07	-0.06
Primary sector	0.03	-0.00***
Secondary sector	-0.01	0.02***
Tertiary sector	0.05	0.05

Notes: Each value represents a change between 2000 and 2003. 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). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.5: 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
Nxt	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: Two-way fixed effects panel data regression. Standard errors (in parentheses) are clustered at the grid level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.6: 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 shows mean and standard deviations in parentheses. The full sample (18-39-year-old males) is included in this table. Residents in Gällivare and Kiruna. Migrants to Norbotten County in columns 3 and 4, and migrants to the mining municipalities in columns 5 and 6. 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).

Table A.7: 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
Nxt	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: Two-way fixed effects panel data regression. Standard errors (in parentheses) are clustered at the grid level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.8: 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
Nxt	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: Two-way fixed effects panel data regression. Standard errors (in parentheses) are clustered at the grid level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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

	(1) Property crime	(2) Violent crime	(3) Traffic crime	(4) Substance crime
Panel A: Gällivare				
Post*Gällivare	-0.0101**	0.0033	-0.0048	0.0073**
	(0.0043)	(0.0024)	(0.0031)	(0.0031)
Nxt	209232	209232	209232	209232
N	13077	13077	13077	13077
Mean dep. var (2000-03)	0.0125	0.0060	0.0097	0.0025
Effect relative to the mean (%)	-80.42	54.93	-49.08	287.80
R-squared	0.2976	0.2219	0.2783	0.3821
Within R-squared	0.0001	0.0000	0.0000	0.0001
Panel B: Kiruna				
Post*Kiruna	-0.0037	0.0005	0.0011	0.0024
	(0.0028)	(0.0021)	(0.0024)	(0.0020)
Nxt	213263	213263	213263	213263
N	13329	13329	13329	13329
Mean dep. var (2000-03)	0.0125	0.0060	0.0097	0.0025
Effect relative to the mean (%)	-29.79	7.56	11.47	93.33
R-squared	0.2918	0.2203	0.2731	0.3725
Within R-squared	0.0000	0.0000	0.0000	0.0000
Individual FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Grid FE	Yes	Yes	Yes	Yes

Notes: Two-way fixed effects panel data regression. Standard errors (in parentheses) are clustered at the grid level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.10: 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
Nxt	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: Two-way fixed effects panel data regression. Ring 1: 0.00 km-2.74 km, ring 2: 2.75 km-3.37 km, ring 3: 3.38 km-3.82 km, ring 4: 3.83 km-4.26 km, ring 5: 4.27 km-4.86 km, ring 6: 4.87 km-18.34 km, ring 7: 18.35 km-73.67 km, ring 8: 73.68 km-102.47 km, ring 9: 102.48 km-125.37 km, and ring 10: 125.38 km-236.00 km. Standard errors (in parentheses) are clustered at the grid level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.11: 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*$\leq$ 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
Nxt	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: Two-way fixed effects panel data regression. Treated: 20-kilometer rings using travel time duration by car to the nearest mine. Standard errors (in parentheses) are clustered at the grid level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.12: Impact of the mining boom on criminal behavior using DDD approach, 2000-2015

	(1) Property crime	(2) Violent crime	(3) Traffic crime	(4) Substance crime
Post*Treated (DID)	-0.0053**	0.0029*	-0.0012	0.0055***
	(0.0026)	(0.0017)	(0.0022)	(0.0020)
Post*Treated*Public (DDD)	0.0027	-0.0019	-0.0023	-0.0004
	(0.0084)	(0.0049)	(0.0056)	(0.0057)
Individual FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Grid FE	Yes	Yes	Yes	Yes
Nxt	124267	124267	124267	124267
N	7767	7767	7767	7767
Mean dep. var (2000-03)	0.0125	0.0060	0.0097	0.0025
R-squared	0.2992	0.2084	0.2816	0.3560
Within R-squared	0.0001	0.0000	0.0000	0.0002

Notes: Two-way fixed effects panel data regression. Standard errors (in parentheses) are clustered at the grid level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.13: 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
Nxt	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: Two-way fixed effects panel data regression. Controls include marital status, having children under 18, educational levels, and economic sectors. Standard errors (in parentheses) are clustered at the grid level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.14: 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). 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). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

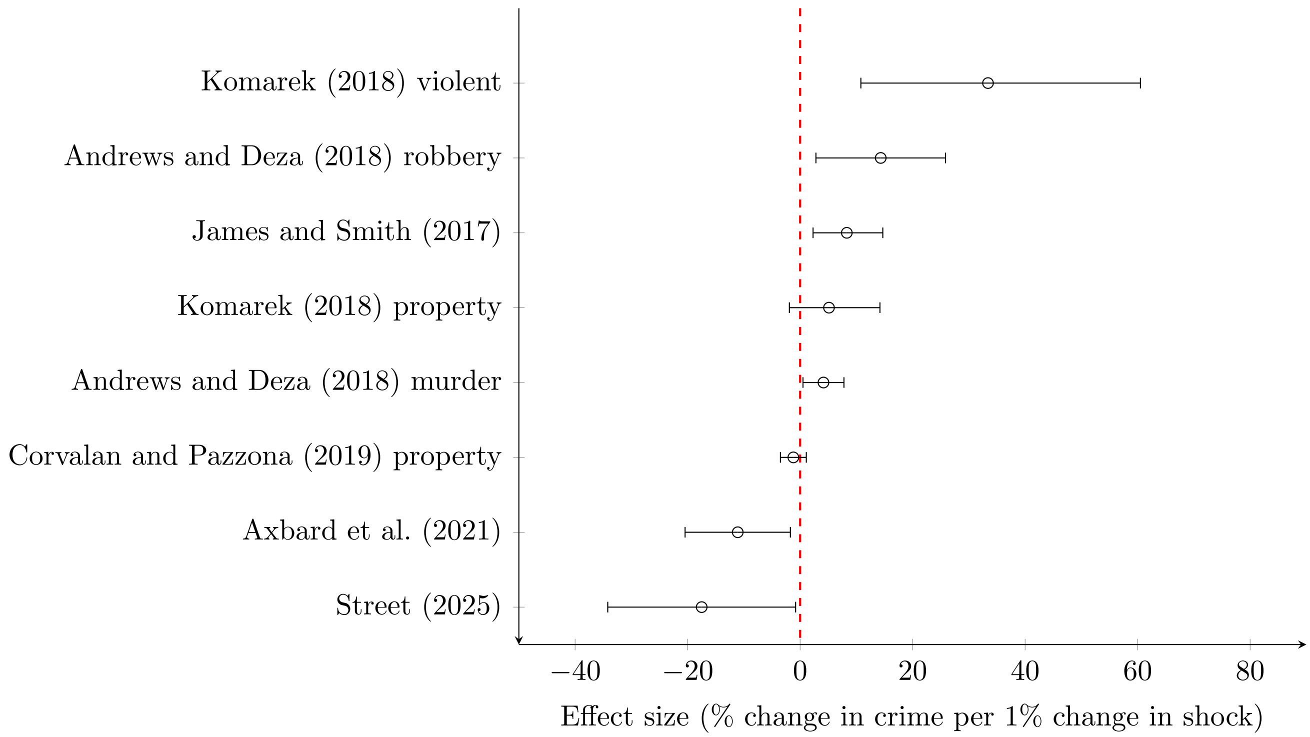
Table A.15: Interrupted Time Series: Share of LKAB articles mentioning stress/mental health terms, 2000-2015

	(1) Baseline	(2) Quadratic trend	(3) Weighted
Post-period indicator (β_1)	-0.037	-0.061	-0.013
	(0.054)	(0.059)	(0.055)
Time in months (β_2)	0.003*	0.003*	0.002*
	(0.002)	(0.002)	(0.001)
Time^2		-0.000	
		(0.000)	
Post-period slope (β_3)	-0.003**	-0.002	-0.003**
	(0.002)	(0.002)	(0.001)
Constant	0.295***	0.292***	0.290***
	(0.049)	(0.048)	(0.048)
Observations	175	175	175
R-squared	0.066	0.078	0.080

Notes: Newey-West standard errors (lag=12) in parentheses. Column (3) weights observations by the number of articles per month. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

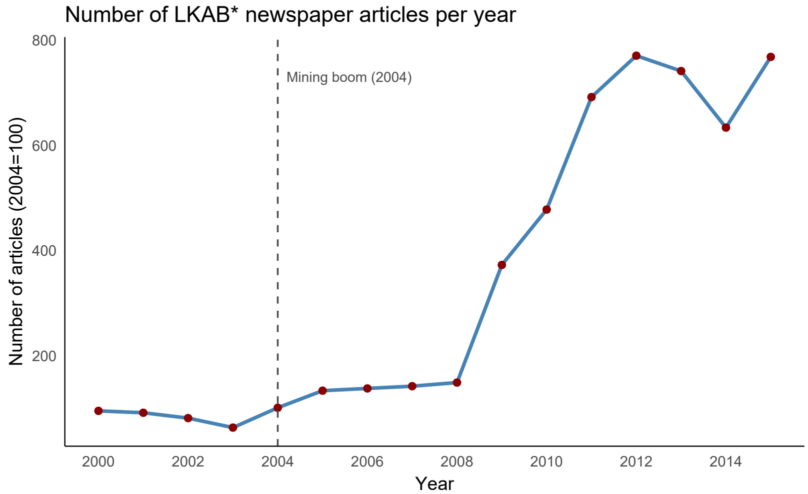
B Appendix: Additional figures

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



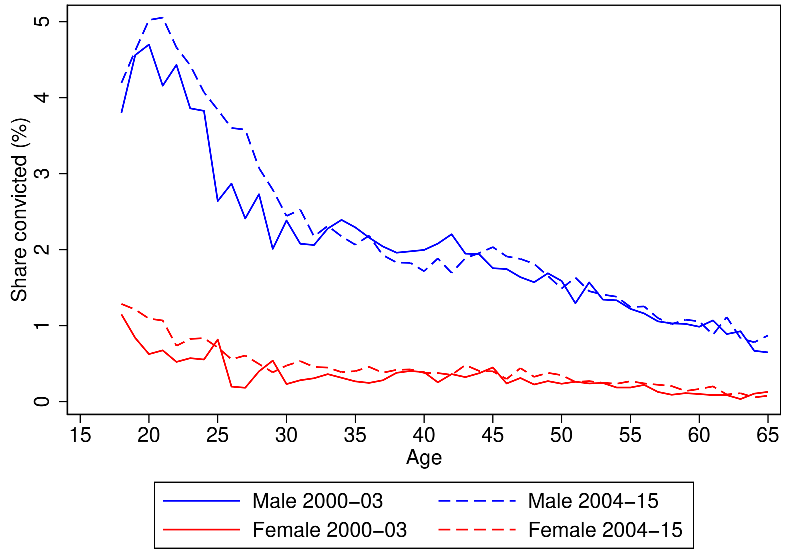
Notes: This figure compares estimated treatment effect sizes in the literature. Each dot shows the estimated effect size (%) with 95% confidence intervals. The figure compares the effect of different resource shocks exposure, such as mining and fracking booms, on criminal behavior. See Online Appendix 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.

Figure B.2: Media coverage of LKAB in Swedish newspapers, 2000–2015



Notes: The figure reports the annual number of newspaper articles mentioning "LKAB" normalized to 2004 values (2004=100). 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. 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.

Figure B.3: Conviction rates of any crime by age and gender, before vs after



Notes: The sample excludes the migrants to the mining area. Convictions include all types of crimes.

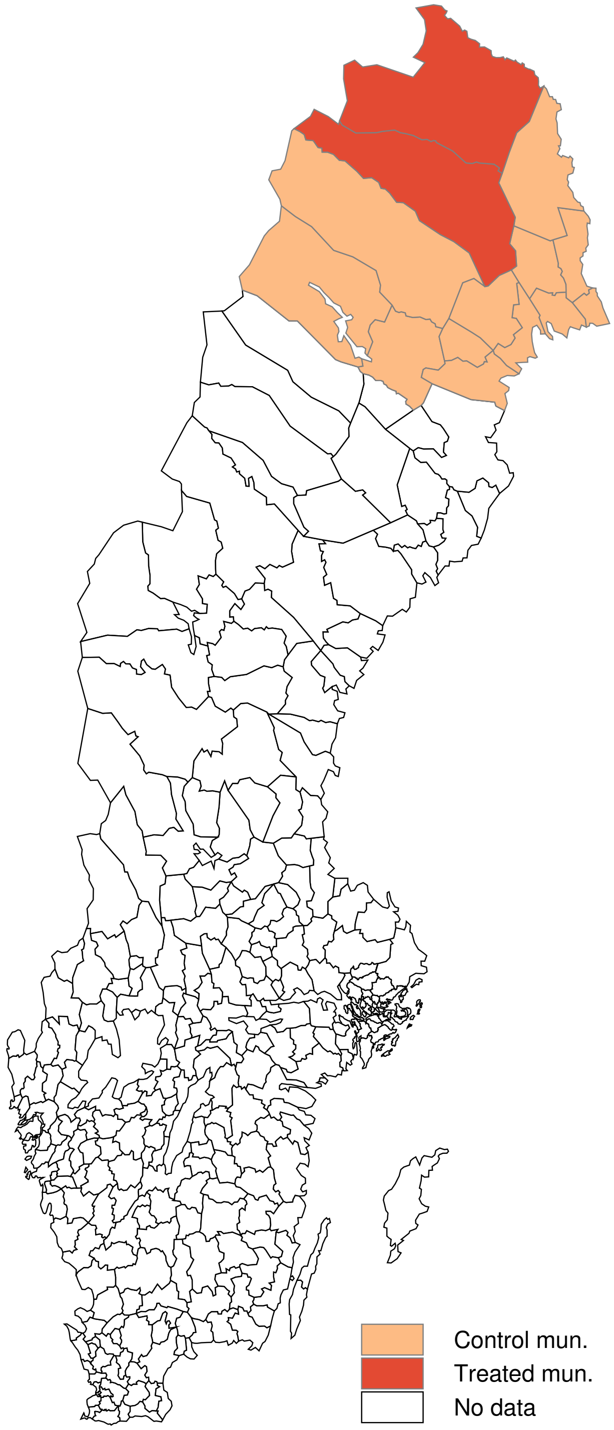


Figure B.4: Treated and control municipalities

Notes: This map shows the spatial location of the treated (Gällivare and Kiruna) and control municipalities. The rest of the municipalities in white are excluded from the sample.

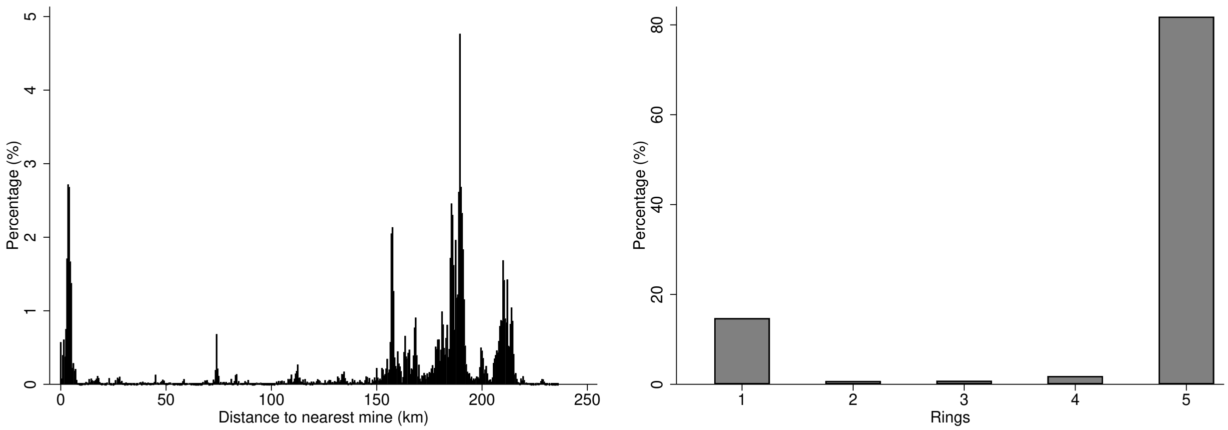
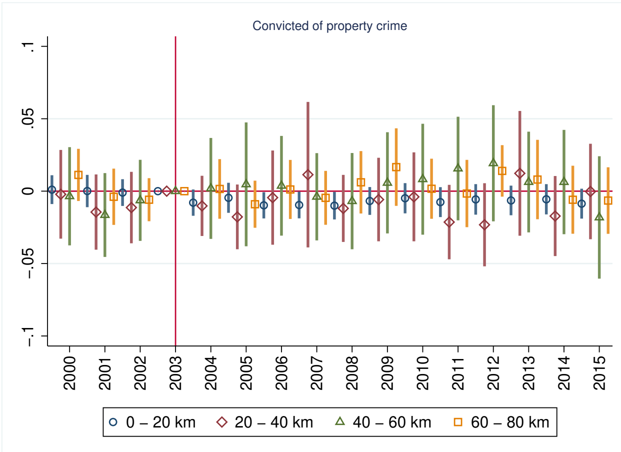


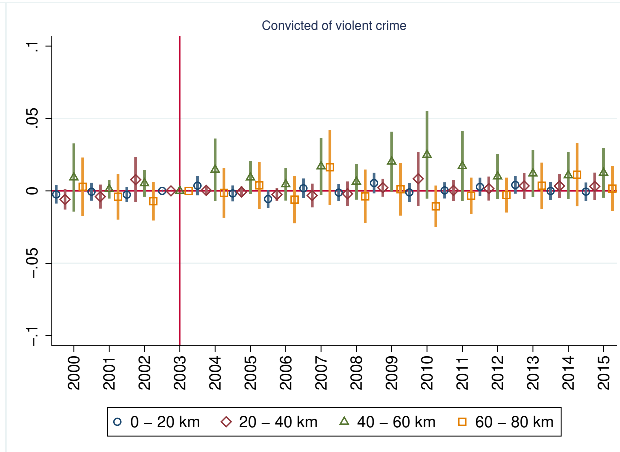
Figure B.5: Distribution of individuals according to their distance to the nearest mine and in the rings

Notes: The figure shows the distribution of individuals according to their distance to the nearest mine and in the rings.

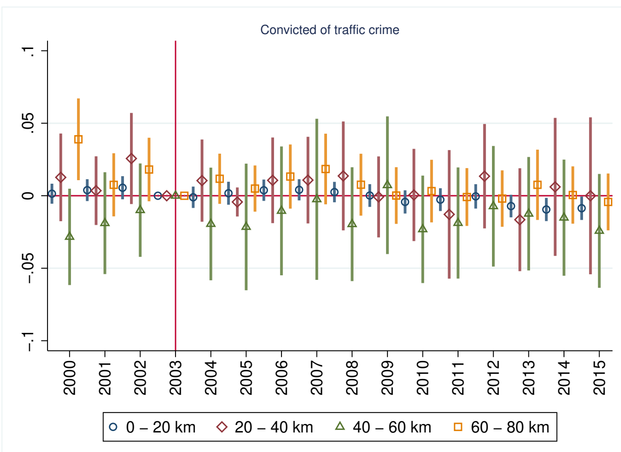
Figure B.6: Event study of the impact of the mining boom on criminal behavior by rings, 2000-2015



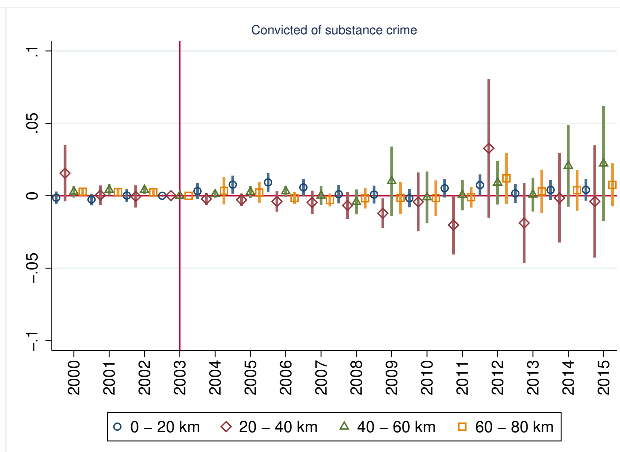
(a) Property crime



(b) Violent crime



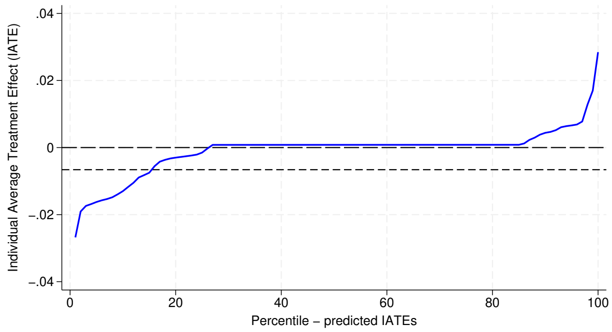
(c) Traffic crime



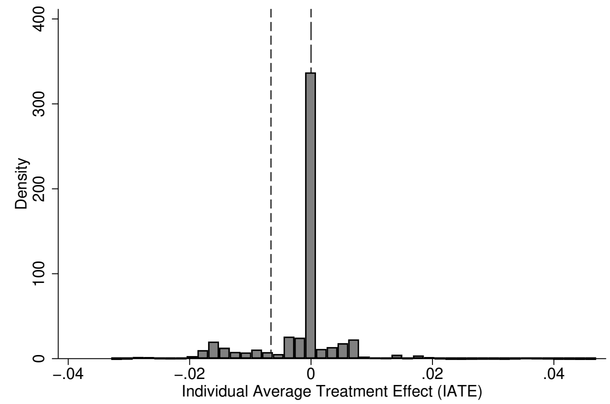
(d) Substance crime

Notes: Year 2003 and > 80 km are the references. is the reference. 95% confidence interval shown. 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.

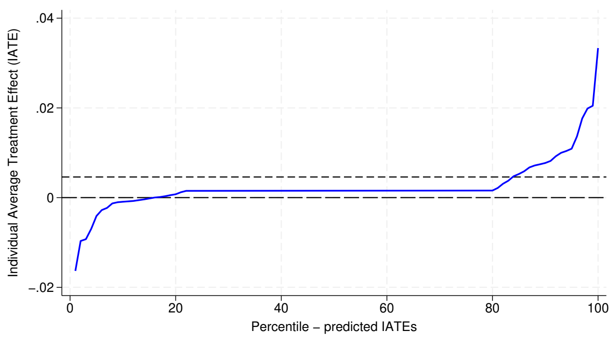
Figure B.7: Distribution predicted individual average treatment effects



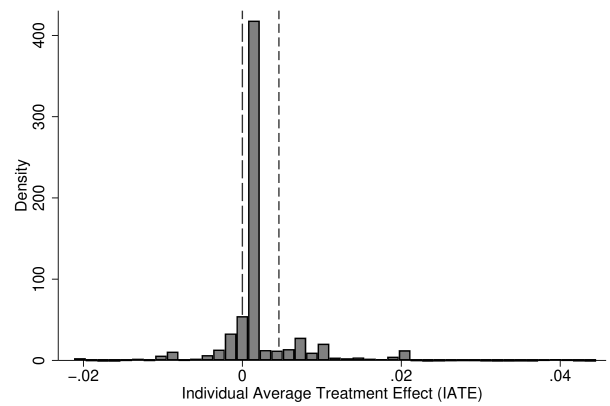
(a) Property crime



(b) Property crime



(c) Substance crime



(d) Substance crime

Notes: The figure shows how the predicted Individual Average Treatment Effect (IATE) varies over its rank, aggregated over percentiles (panel a) and its distribution (panel b). A causal forest is implemented to estimate the CATE. Long dash lines show the 0 in both figures. Dash lines show the Average Treatment Effect (ATE) in both figures.

Figure B.8: Directed Acyclic Graph (DAG) of mechanisms linking mining booms to crime

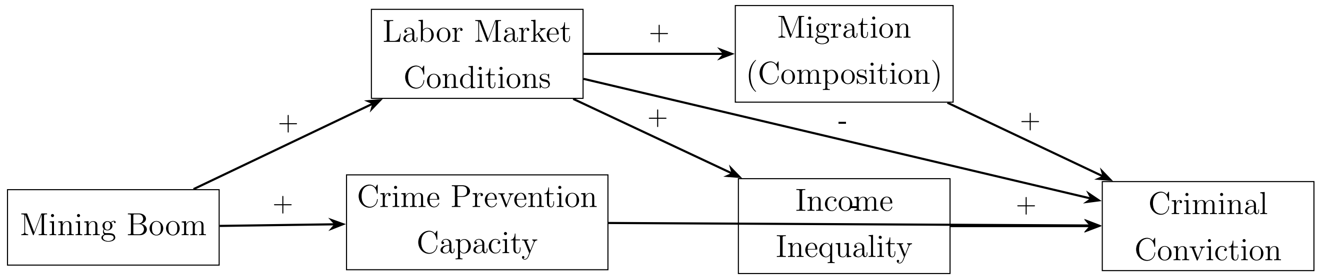
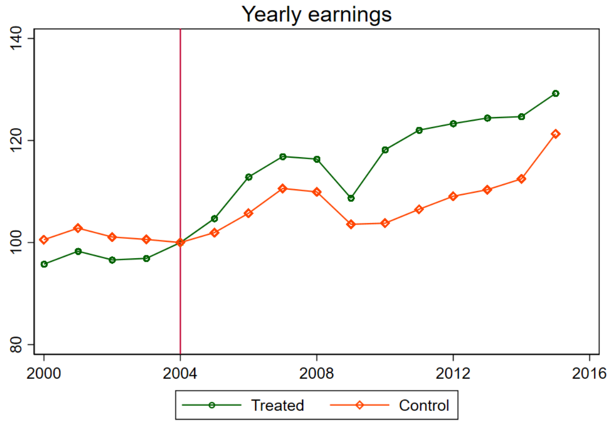
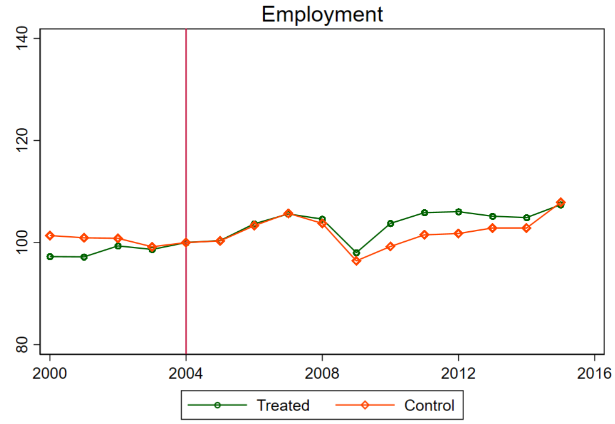


Figure B.9: Earnings and employment evolution for mining and comparison municipalities, 2000-2015



(a) Yearly earnings



(b) Employment

Notes: Treated: Gällivare and Kiruna. Earnings and employment are normalized to 2004 values (2004=100). The vertical line shows the year of the start of the mining boom (2004).

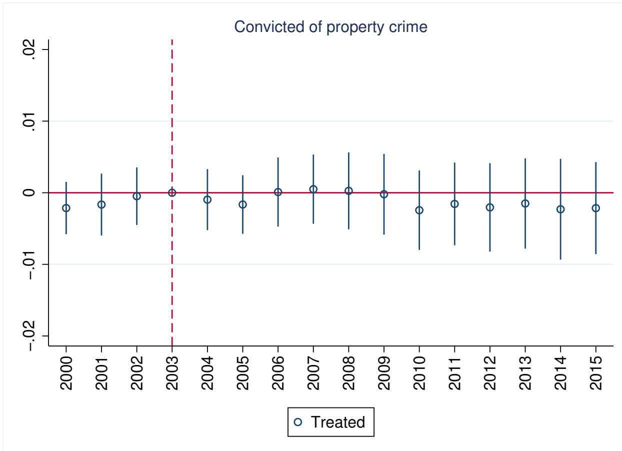
C Appendix: Results for the 30-39-year-old males sample

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

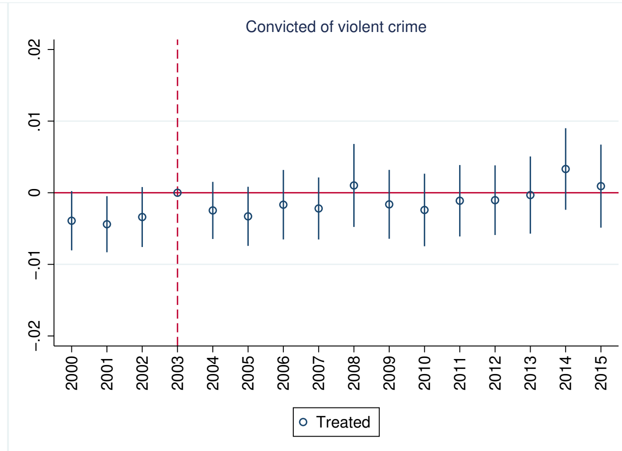
	(1) Property crime	(2) Violent crime	(3) Traffic crime	(4) Drug crime
Post*Treated	0.0002	0.0004	0.0008	-0.0008
	(0.0012)	(0.0011)	(0.0015)	(0.0006)
Individual FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Grid FE	Yes	Yes	Yes	Yes
Nxt	209922	209922	209922	209922
N	13120	13120	13120	13120
Mean dep. var (2000-03)	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: Two-way fixed effects panel data regression. Standard errors (in parentheses) are clustered at the grid level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

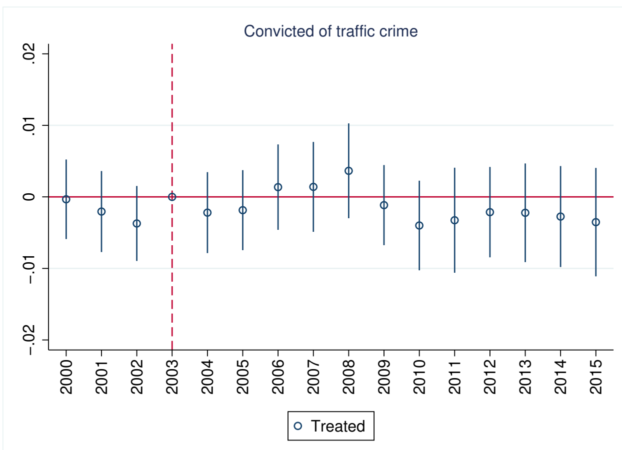
Figure C.1: Event study of the impact of the mining boom on criminal behavior of 30-39-year-old males, 2000-2015



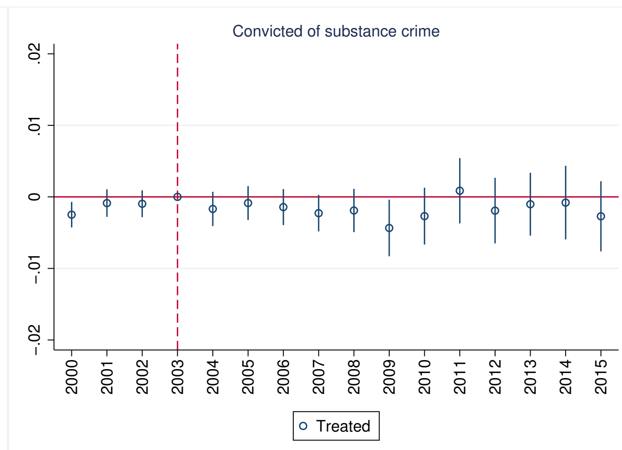
(a) Property crime



(b) Violent crime



(c) Traffic crime



(d) Substance crime

Notes: Year 2003 is the reference. 95% confidence interval shown. 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 C.2: Impact of the mining boom on detailed criminal behavior for 30-39 years old, 2000-2015

	(1) First-time convicted Property crime	(2) Re-offense Property crime	(3) First-time convicted Substance crime	(4) Re-offense Substance crime
Post*Treated	0.0002	-0.0000	-0.0006	-0.0002
	(0.0011)	(0.0007)	(0.0006)	(0.0004)
Individual FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Grid FE	Yes	Yes	Yes	Yes
Nxt	209922	209922	209922	209922
N	13120	13120	13120	13120
Mean dep. var (2000-03)	0.0045	0.0010	0.0014	0.0005
Effect relative to the mean (%)	4.15	-4.84	-40.26	-49.88
R-squared	0.1874	0.3943	0.2161	0.4678
Within R-squared	0.0000	0.0000	0.0000	0.0000

Notes: Two-way fixed effects panel data regression. Standard errors (in parentheses) are clustered at the grid level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.3: Impact of the mining boom on detailed criminal behavior (the role of prison) for 30-39 years old, 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	-0.0000	-0.0000	0.0002	-0.0007	0.0001	-0.0001
	(0.0011)	(0.0004)	(0.0006)	(0.0005)	(0.0003)	(0.0004)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Grid FE	Yes	Yes	Yes	Yes	Yes	Yes
Nxt	209922	209922	209922	209922	209922	209922
N	13120	13120	13120	13120	13120	13120
Mean dep. var (2000-03)	0.0041	0.0008	0.0007	0.0011	0.0004	0.0004
Effect relative to the mean (%)	-1.13	-3.56	37.07	-69.08	13.07	-28.58
R-squared	0.2311	0.2264	0.4059	0.3273	0.2387	0.4908
Within R-squared	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000

Notes: Two-way fixed effects panel data regression. Standard errors (in parentheses) are clustered at the grid level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.4: 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
Nxt	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: Two-way fixed effects panel data regression. Standard errors (in parentheses) are clustered at the grid level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.5: Impact of the mining boom on criminal behavior of migrants for 30-39-year-old males, 2000-2015

	(1) Property crime	(2) Violent crime	(3) Traffic crime	(4) Substance crime	(5) Property crime	(6) Violent crime	(7) Traffic crime	(8) Substance crime
Post*Migrants (Mining mun.)	0.0088	-0.0119*	-0.0145	-0.0094	-0.0142	-0.0166*	-0.0306**	-0.0049
	(0.0135)	(0.0068)	(0.0127)	(0.0063)	(0.0162)	(0.0086)	(0.0149)	(0.0065)
Post*Migrants (Control mun.)	0.0178**	0.0069	0.0133**	-0.0056				
	(0.0080)	(0.0055)	(0.0064)	(0.0062)				
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Grid FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Nxt	85972	85972	85972	85972	25977	25977	25977	25977
N	5373	5373	5373	5373	1624	1624	1624	1624
Mean dep. var (2000-03)	0.0797	0.0242	0.0691	0.0537	0.0186	0.0096	0.0173	0.0058
Effect relative to the mean, Treated (%)	11.06	-49.25	-20.96	-17.60	-76.60	-172.59	-176.94	-84.80
Effect relative to the mean, Control mun. (%)	22.35	28.64	19.24	-10.37				
R-squared	0.5680	0.4011	0.5384	0.5718	0.5183	0.3622	0.4389	0.5082
Within R-squared	0.0001	0.0000	0.0001	0.0000	0.0001	0.0002	0.0004	0.0000

Notes: Two-way fixed effects panel data regression. Migrants before the move are the references. Standard errors (in parentheses) are clustered at the grid level. Columns (1)-(4) compare migrants to the mining municipalities or the control municipalities to themselves before the migration event. Columns (5)-(8) compare migrants to the mining municipalities to migrants to the control municipalities. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.6: Impact of the mining boom on criminal behavior by treated municipality for of 30-39-year-old males, 2000-2015

	(1) Property crime	(2) Violent crime	(3) Traffic crime	(4) Substance crime
Panel A: Gällivare				
Post*Gällivare	0.0005	-0.0011	-0.0022	-0.0012
	(0.0021)	(0.0015)	(0.0022)	(0.0012)
Nxt	189753	189753	189753	189753
N	11860	11860	11860	11860
Mean dep. var (2000-03)	0.0055	0.0036	0.0067	0.0019
Effect relative to the mean (%)	9.64	-30.66	-32.49	-65.92
R-squared	0.3141	0.2439	0.3173	0.4444
Within R-squared	0.0000	0.0000	0.0000	0.0000
Panel B: Kiruna				
Post*Kiruna	0.0001	0.0016	0.0030*	-0.0005
	(0.0012)	(0.0014)	(0.0018)	(0.0006)
Nxt	194700	194700	194700	194700
N	12169	12169	12169	12169
Mean dep. var (2000-03)	0.0055	0.0036	0.0067	0.0019
Effect relative to the mean (%)	1.71	42.72	44.94	-29.11
R-squared	0.3103	0.2397	0.3049	0.4397
Within R-squared	0.0000	0.0000	0.0000	0.0000
Individual FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Grid FE	Yes	Yes	Yes	Yes

Notes: Two-way fixed effects panel data regression. Standard errors (in parentheses) are clustered at the grid level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.7: Impact of the mining boom on criminal behavior by rings for 30-39-year-old males, 2000-2015

	(1) Property crime	(2) Violent crime	(3) Traffic crime	(4) Substance crime
Post*Ring 1	0.0031	0.0004	-0.0049*	-0.0009
	(0.0036)	(0.0022)	(0.0027)	(0.0024)
Post*Ring 2	0.0010	-0.0016	-0.0036	-0.0041**
	(0.0033)	(0.0030)	(0.0029)	(0.0020)
Post*Ring 3	-0.0039	0.0060**	0.0055	-0.0008
	(0.0030)	(0.0030)	(0.0037)	(0.0006)
Post*Ring 4	-0.0019	-0.0001	-0.0007	0.0002
	(0.0020)	(0.0018)	(0.0028)	(0.0006)
Post*Ring 5	-0.0018	-0.0002	0.0056	0.0018
	(0.0029)	(0.0022)	(0.0042)	(0.0017)
Post*Ring 6	0.0017	-0.0035	0.0076*	-0.0002
	(0.0018)	(0.0033)	(0.0046)	(0.0008)
Post*Ring 7	-0.0001	0.0004	-0.0047	-0.0015
	(0.0026)	(0.0020)	(0.0041)	(0.0016)
Post*Ring 8	0.0008	-0.0000	0.0069	0.0006
	(0.0030)	(0.0037)	(0.0047)	(0.0011)
Post*Ring 9	-0.0009	-0.0040	-0.0035	0.0004
	(0.0033)	(0.0042)	(0.0040)	(0.0019)
Individual FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Grid FE	Yes	Yes	Yes	Yes
Nxt	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	0.3072	0.4398
Within R-squared	0.0000	0.0001	0.0001	0.0000

Notes: Two-way fixed effects panel data regression. Ring 1: 0.00 km-2.74 km, ring 2: 2.75 km-3.37 km, ring 3: 3.38 km-3.82 km, ring 4: 3.83 km-4.26 km, ring 5: 4.27 km-4.86 km, ring 6: 4.87 km-18.34 km, ring 7: 18.35 km-73.67 km, ring 8: 73.68 km-102.47 km, ring 9: 102.48 km-125.37 km, and ring 10: 125.38 km-236.00 km. Standard errors (in parentheses) are clustered at the grid level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.8: Impact of the mining boom on criminal behavior using time duration for treatment for 30-39-year-old males, 2000-2015

	(1) Property crime	(2) Violent crime	(3) Traffic crime	(4) Substance crime
Post*$\leq$ 20 min	0.0002	-0.0007	-0.0018	-0.0013
	(0.0023)	(0.0016)	(0.0024)	(0.0014)
Post* 20 - 40 min	-0.0002	-0.0003	-0.0087*	-0.0034
	(0.0053)	(0.0039)	(0.0051)	(0.0037)
Post*40 - 60 min	-0.0003	0.0009	0.0032*	-0.0003
	(0.0013)	(0.0014)	(0.0019)	(0.0005)
Post*60 - 80 min	-0.0053	0.0006	-0.0055	-0.0016
	(0.0065)	(0.0006)	(0.0044)	(0.0010)
Individual FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Grid FE	Yes	Yes	Yes	Yes
Nxt	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: Two-way fixed effects panel data regression. Treated: 20-kilometer rings using travel time duration by car to the nearest mine. Standard errors (in parentheses) are clustered at the grid level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.9: Impact of the mining boom on criminal behavior using DDD approach for 30-39 years old, 2000-2015

	(1) Property crime	(2) Violent crime	(3) Traffic crime	(4) Substance crime
Post*Treated (DID)	-0.0001	0.0004	0.0013	-0.0010
	(0.0013)	(0.0012)	(0.0016)	(0.0007)
Post*Treated*Public (DDD)	0.0020	-0.0005	-0.0029	0.0006
	(0.0027)	(0.0020)	(0.0032)	(0.0010)
Individual FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Grid FE	Yes	Yes	Yes	Yes
Nxt	195335	195335	195335	195335
N	12208	12208	12208	12208
Mean dep. var (2000-03)	0.0055	0.0036	0.0067	0.0019
R-squared	0.3013	0.2311	0.3044	0.4445
Within R-squared	0.0000	0.0000	0.0000	0.0000

Notes: Two-way fixed effects panel data regression. Standard errors (in parentheses) are clustered at the grid level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.10: Robustness checks: impact of the mining boom on criminal behavior for 30-39-year-old males, 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	0.0002		0.0002	-0.0000	0.0000	0.0002	-0.0006
	(0.0012)		(0.0012)	(0.0012)	(.)	(0.0012)	(0.0012)
Post*Treated (2003)		0.0006					
		(0.0012)					
Mean dep. var (2003)	0.0055	0.0055	0.0055	0.0055	.	0.0053	0.0055
Effect relative to the mean (%)	3.11	10.02	3.36	-0.81	.	3.18	-10.41
R-squared	0.3088	0.3013	0.3092	0.3183	0.3708	0.3080	0.2772
Within R-squared	0.0000	0.0000	0.0005	0.0000	0.0000	0.0000	0.0000
Panel B: Violent crime							
Post*Treated	0.0004		0.0003	0.0003	0.0000	0.0006	0.0002
	(0.0011)		(0.0011)	(0.0011)	(.)	(0.0011)	(0.0011)
Post*Treated (2003)		0.0003					
		(0.0011)					
Mean dep. var (2003)	0.0036	0.0036	0.0036	0.0037	.	0.0036	0.0036
Effect relative to the mean (%)	11.27	8.11	9.17	9.16	.	15.60	4.36
R-squared	0.2396	0.2311	0.2397	0.2405	0.2781	0.2477	0.2053
Within R-squared	0.0000	0.0000	0.0001	0.0000	0.0000	0.0000	0.0000
Panel C: Traffic crime							
Post*Treated	0.0008		0.0010	0.0008	0.0000	0.0010	0.0010
	(0.0015)		(0.0015)	(0.0015)	(.)	(0.0015)	(0.0014)
Post*Treated (2003)		0.0005					
		(0.0015)					
Mean dep. var (2003)	0.0067	0.0067	0.0067	0.0067	.	0.0064	0.0067
Effect relative to the mean (%)	12.66	7.76	14.95	11.60	.	16.14	15.42
R-squared	0.3071	0.3044	0.3072	0.3094	0.3126	0.3050	0.2801
Within R-squared	0.0000	0.0000	0.0002	0.0000	0.0000	0.0000	0.0000

Panel D: Substance crime							
Post*Treated	-0.0008		-0.0008	-0.0009	0.0000	-0.0008	-0.0009
	(0.0006)		(0.0006)	(0.0006)	(.)	(0.0007)	(0.0006)
Post*Treated (2003)		-0.0010					
		(0.0006)					
Mean dep. var (2003)	0.0019	0.0019	0.0019	0.0019	.	0.0017	0.0019
Effect relative to the mean (%)	-42.73	-51.23	-42.76	-48.96	.	-44.33	-50.23
R-squared	0.4397	0.4445	0.4400	0.4351	0.4154	0.4443	0.4062
Within R-squared	0.0000	0.0000	0.0005	0.0000	0.0000	0.0000	0.0000
Nxt	209922	195335	209922	230440	39031	176669	209922
N	13120	12208	13120	14402	2439	11042	13120
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: Two-way fixed effects panel data regression. Standard errors (in parentheses) are clustered at the grid level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.11: Mechanisms: impact of the mining boom on different mechanisms for 30-39-year-old males, 2000-2015

	(1) Disposable income	(2) Labor income	(3) Lab. inc. employed	(4) Employment	(5) Employment mining	(6) Bottom earning tercile	(7) Top earning tercile	(8) Police occupation	(9) Police industry
Post*Treated	8.5549***	18.5580***	19.0213***	0.0087	0.0295***	-0.0023	0.0071	0.0004	0.0005
	(1.2340)	(1.5654)	(1.4816)	(0.0059)	(0.0040)	(0.0031)	(0.0050)	(0.0011)	(0.0014)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Grid FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Nxt	209922	209922	180910	209922	209922	195335	195335	174442	209922
N	13120	13120	11307	13120	13120	12208	12208	10903	13120
Mean dep. var (2000-03)	168.0663	196.5386	231.6293	0.8338	0.0476	0.2217	0.4723	0.0047	0.0087
Effect relative to the mean (%)	5.09	9.44	8.21	1.04	62.12	-1.06	1.50	7.98	5.24
R-squared	0.5079	0.8284	0.7976	0.6802	0.9087	0.9474	0.9081	0.7833	0.8025
Within R-squared	0.0001	0.0017	0.0021	0.0000	0.0030	0.0000	0.0000	0.0000	0.0000

Notes: Two-way fixed effects panel data regression. Disposable income and labor income expressed in 1000 SEK and in real values with the base year 2000. Standard errors (in parentheses) are clustered at the grid level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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:

$$CATE \equiv \tau(x) = E[Y_{1i} - Y_{0i}|X_i = x] \tag{3}$$

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).[35] 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

[35] 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:

$$IATE \equiv \tau(x) = E\{treatment(1) - treatment(0)|x\} \tag{4}$$

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:

$$GATE \equiv \tau(g) = E\{treatment(1) - treatment(0)|G = g\} \tag{5}$$

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

E Appendix: 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 Online 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]).

F Appendix: Newspaper content analysis: Stress coverage of LKAB

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 B.2 shows the rapid increase in annual article counts following the mining boom, reflecting the company's growing newsworthiness.

F.2 Outcome variable and keyword dictionaries

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 stress discourse became a larger fraction of the conversation. Matching is case-insensitive substring matching, which is important in Swedish because of compound-word formation (*arbetsmiljöproblem* would be missed by exact-word matching). Table F.1 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.

F.3 Econometric model

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

$$S_t = \alpha + \beta_1 \text{Post}_t + \beta_2 t + \beta_3 (t \times \text{Post}_t) + \varepsilon_t, \tag{6}$$

where S_t is the share of articles classified as mentioning stress and mental health 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,

Table F.1: 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
press	pressure	Psychological pressure
belastning / arbetsbelastning	load / workload	Physical or psychological burden
depression	depression	Clinical mental health condition
ångest	anxiety	Co-occurs with stress and substance use
välmående / välbefinnande	wellbeing	Worker wellbeing

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 for the mechanism test. 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 stress discourse or changes in newspaper coverage of mental health 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.4 Results

Online Appendix Table A.15 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 $\hat{\beta}_1 = -0.037$, 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 is positive ($\hat{\beta}_2 = 0.003$, while the post-boom slope change is $\hat{\beta}_3 = -0.003$, implying a reversal of the upward pre-boom trend after 2004. 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 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 6 (in the main text) presents the binscatter of quarterly shares alongside the ITS fitted trends of Column (1).

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