Cash Transfers and Violent Crime in Indonesia

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Abstract

We study the impact of Indonesia's flagship conditional cash transfer (CCT) program on violent crime. Using RCT and nationwide program rollout data, we show that access to CCTs led to substantial increases in violent crime. We also document significant increases in idleness among young men within beneficiary households, which is likely the primary driver of the rise in crime: crime effects are concentrated among non-employed offenders, occur on workdays, and attenuate in stronger labor markets. In contrast, we find no evidence of crime increasing through wealth, inequality, or justice channels. Within-household spillovers could explain the observed rise in violent crime.

JEL codes: D62, H53, I38, O12

Keywords: conditional cash transfer, crime, youth, Indonesia

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1 Introduction

Conditional Cash Transfer (CCT) programs have become a cornerstone of social and economic policy. By conditioning benefits on school attendance and health checkups, CCTs effectively boost human capital investments (Millán et al., 2019) and improve long-term economic outcomes (Behrman et al., 2010; Parker and Vogl, 2023). As they are costly, researchers increasingly examine their broader societal impacts, including growing evidence that these programs may reduce crime (Camacho and Mejia, 2013; Chioda et al., 2016; Machado et al., 2018; Borraz and Munyo, 2020; Attanasio et al., 2021). These effects are particularly relevant, given their implications for human capital formation and long-run economic growth. However, the mechanisms linking CCTs to crime remain incompletely understood.

This paper investigates the impact of a CCT program on local crime and explores intra-household mechanisms that could drive such effects. Our setting is the rollout of Indonesia's flagship anti-poverty program, PKH (*Program Keluarga Harapan*), the world's second-largest CCT, which targets poor families with children under 16 years and reaches over 10 million households. Leveraging annual violent crime data (2005–2014), a randomized controlled trial (RCT), and administrative records from PKH's nationwide rollout, we estimate short- and medium-run causal effects of the program on violent crime. We document that PKH increased community-level violent crime by 0.6 to 3.2 percentage points (10–33%). These effects are robust across specifications, including alternative data sources (newspaper articles vs. victimization surveys), definitions of crime (at intensive vs. extensive margins), estimators (OLS, IV, TWFE, generalized synthetic controls), and samples (contrasting RCT vs. nationwide roll-out locations). We also rule out confounding factors such as selective migration or general equilibrium effects.

We explore potential mechanisms two to four years post-implementation using RCT and nationwide household survey data. First, we assess whether PKH increased the potential victimization of beneficiaries through higher accumulation of lootable assets or inequality-driven motives. However, we find no evidence that PKH households accumulated more assets, that local inequality rose, or that violent crime was linked to mistargeting.

Second, we examine intra-household resource reallocations: PKH led to increased idleness (neither working, attending school, nor performing household chores) among young male siblings (18–25 years) in beneficiary households. Placebo checks reveal no such effects among young males in non-poor households or those not eligible for PKH due to their family composition, suggesting that the mechanism stems specifically from intra-household spillovers of the program rather than a broader deterioration in youth employment conditions. Parental attention patterns further corroborate this mechanism. CCT-receiving parents report allocating more time to rearing younger, school-age children (targeted by program conditionality) and less time to older siblings not in school. The reduced parental monitoring of older children aligns with findings linking diminished supervision to increased youth crime (Akee et al., 2010). Given strong evidence that male youth idleness predicts violent crime (BPS, 2013; POLRI, 2019; Allan and Steffensmeier, 1989; Britt, 1994; Ivaschenko et al., 2017; Nahuel et al., 2022), we posit idleness as a plausible causal channel.

Third, we substantiate the idleness hypothesis through further analyses. A back-of-the-envelope calculation yields an upper-bound estimate of a 3.8% PKH-induced crime rate among newly idle youth, which appears to be a plausible magnitude. Exploiting crime timing and perpetrator characteristics, we find that PKH-linked crime increases disproportionately occur on workdays, when idle individuals face fewer constraints, and do not rise on weekends or idiosyncratically timed public holidays. Furthermore, such increases do not materialize among perpetrators with reported employment. Finally, regions with stronger labor markets exhibit somewhat more muted crime effects, consistent with the idleness-driven mechanism.

Our paper contributes to the literature in three key ways. First, we engage with research examining how welfare programs influence criminal activities among juveniles and young adults. Existing studies focus on policies that directly target youth through improved access to schooling, work opportunities, or stricter enforcement (Machin et al., 2011; Gelber et al., 2015; Hjalmarsson et al., 2015; Bratsberg et al., 2019; Bell et al., 2022; Sviatschi, 2022) and welfare policies which indirectly impact youth through employment and neighborhood mechanisms (Britto et al., 2022; Chin, 2018; Cohen, 2024; Corman et al., 2017; Dave et al., 2021; Dustmann et al., 2024; Jacob et al., 2014; Khanna et al., 2021; Kling et al., 2005; Ludwig et al., 2001; Khanna et al., 2023). Our study identifies a novel mechanism: intra-household spillovers, where education and health conditionality, combined with income effects, shift parental attention toward younger children, increasing idleness among older juveniles and young adults. This idleness, we argue, could plausibly drive the observed rise in violent crime.

We further contribute to the emerging literature on intra-household spillover effects of CCTs. While CCTs are widely recognized for improving recipients' welfare (Millán et al., 2019), they can distort household resource allocation (Kazianga et al., 2014; Suarez and

Maitra, 2021; Barrera-Osorio et al., 2019; Bryan et al., 2023) and generate unintended consequences for non-targeted children's education and labor supply (Barrera-Osorio et al., 2011; Ferreira et al., 2017; Hoop et al., 2019; Chuan et al., 2021). Our findings extend this body of work by demonstrating that PKH-induced spillovers increased idleness among young men, thereby potentially fueling violent crime.

Last, we enhance the external validity of studies linking welfare programs to crime. Many prior analyses rely on single crime data sources (e.g., police reports) and a single causal identification strategy (e.g., difference-in-differences). Given crime data's inherent noise and incompleteness, we strengthen external validity by using two independent data sources—newspaper articles and self-reported victimization surveys—to measure crime. Additionally, we address methodological concerns about necessary assumptions (Banerjee et al., 2017; Muralidharan and Niehaus, 2017; Bold et al., 2018; Vivalt, 2020; Galiani and Quistorff, 2022) by employing two distinct identification strategies: local average treatment effect (LATE) estimates based on an RCT and (2) difference-in-differences (DiD) estimates using an event-study design with staggered nationwide rollout data.

The paper proceeds as follows. Section 2 provides background on PKH and violent crime in Indonesia. Section 3 details our data sources and construction. Section 4 outlines the empirical strategy, presents main results, and conducts robustness checks. Section 5 examines potential mechanisms underlying our findings, focusing on victimization and idleness. Section 6 concludes.

2 Background

Crime in Indonesia Indonesia has the eighth largest prison population in the world, with 271,000 people incarcerated in 2020 (Fair and Walmsley, 2021; UNODC, 2021). Driven by penal code revisions targeting drug offenses, the prisoner population tripled from 2002 to 2020. Administrative data show 70% of inmates are incarcerated for drug-related crimes, 26% for robbery and theft, and 3% for homicides (Mutiarin et al., 2019). Police reports since the mid-2000s reveal that thefts and physical assaults account for about 80% of all crimes. Perpetrators are predominantly male (95%), with 73% having less than nine years of schooling, and many from lower socioeconomic backgrounds. Nearly half (50%) of offenders are under 25 (BPS, 2013; POLRI, 2019). Unemployed are overrepresented among offenders: in 2018, 28% of criminals were unemployed versus Indonesia's 3.6% official unemployment rate (POLRI, 2019).

The conditional cash transfer program PKH is Indonesia's flagship anti-poverty program managed by the Ministry of Social Affairs (MoSA). It targets poor households with specific demographic structures: those with at least one child under 16 or a pregnant woman. Conditionality requires participation in health screenings for pregnant mothers and children under seven, and school attendance of children aged 7–15 (Cahyadi et al., 2020).[1] Households receive annual cash transfers of 83–290 USD per year (in 2012 prices), constituting about 15% of average beneficiary expenditures (Nazara and Rahayu, 2013; World Bank, 2012a). Transfers are typically disbursed to women (mothers).

Empirical evidence highlights PKH's success in improving education and health outcomes. While early impacts on schooling and child labor were mixed (Alatas et al., 2011), sustained gains in educational and health outcomes emerged over time (Cahyadi et al., 2020). PKH has also spurred research on targeting mechanisms (Alatas et al., 2016, 2016b; Banerjee et al., 2020) suicide spillovers (Christian et al., 2019), and health care supply responses (Triyana, 2016).

When introduced in 2007, PKH covered about 500,000 households. It expanded gradually to 3.2 million households by 2014 and 10 million by 2020 (MoSA, 2020). Initially concentrated in urban areas of Java and Sumatra (cf. Appendix Figure A.1), the program later expanded to rural and more remote regions. By 2014, PKH covered nearly all Javanese sub-districts and significantly expanded in Sumatra, Sulawesi, Kalimantan, and Eastern Indonesia. Expansion occurred at the sub-district and community level, though not all communities within a sub-district were included.[2] Once introduced, PKH remained active in sub-districts and communities for the duration of the study period.

3 Data

3.1 Data sources and samples

NVMS crime data Our primary crime data source is the National Violence Monitoring System (NVMS), covering 16 of Indonesia's 33 provinces. Initially launched in 1998 with 10 conflict-prone provinces, it expanded to 16 provinces by 2005 (cf. Appendix Figure A.2).[3] While non-representative nationally, these provinces span major island groups

[1]See Appendix Tables B.1 and B.2 for PKH's conditionality criteria and payment structures (2007–2015).
[2]We define communities as rural villages (*desa*) or urban precincts (*kelurahan*).
[3]The 16 provinces include Aceh, Central Kalimantan, Central Sulawesi, DKI Jakarta, East Java, East Nusa Tanggara, Lampung, Maluku, North Maluku, North Sulawesi, North Sumatra, Papua, South Sulawesi, West Kalimantan, West Nusa Tenggara, and West Papua.

and cover 54% of Indonesia's population in 2014. The dataset systematically codes print newspaper archives to capture daily crime incidences at high spatial resolution, which we aggregate annually from 2000 to 2014. Spanning 75 newspapers (2000–2004) and 123 newspapers (2005–2014), it includes 2 million coded articles. Incidents were coded based on standardized procedures based on motives, with categories including violent crime and conflict (see Barron et al., 2009, 2014; Bazzi and Gudgeon, 2021; Bazzi et al., 2022, for data quality details).

Our study uses NVMS data on *violent crime*, defined as "an act of violence that occurs without any prior dispute between parties (due to monetary and/or non-monetary motives)." Violent crime is categorized into 13 subtypes, including assault, robbery, vandalism, and fights. Most cases involve assaults and robberies.[4] NVMS data excludes petty crime; only cases reported in provincial or national newspapers are included in the data. To our knowledge, NVMS is the only Indonesian dataset enabling consistent annual crime indicators over time and at sub-provincial administrative units (below admin-1). Provincial-level police records (2007–2014) correlate positively but not very strongly with NVMS (with a correlation coefficient of 0.38). Other sources, like the village census PODES, are more sporadic in coverage and subject to definition changes.

Crime victimization data We complement our findings using self-reported crime victimization data from Indonesia's National Household Survey ('Survei Sosial Ekonomi Nasional', SUSENAS, 2007–2011). Conducted by Statistics Indonesia (BPS), SUSENAS annually surveys about 250,000 households, serving as Indonesia's primary source for socio-economic data. Since 2007, the survey includes questions on self-reported crimes experienced in the past year. Our analysis focuses on 2007–2011 due to questionnaire design and data release policies. From 2012, BPS omitted village/sub-district identifiers, precluding sub-district-level matching with PKH rollout data.[5]

Administrative CCT rollout data We obtained PKH rollout data from MoSA, detailing the year PKH launched in each given community and annual household enrollment numbers. The data covers the period from 2007 (PKH's initial launch year) until 2014. Regulatory changes after 2014 restrict access to later administrative records.

RCT data The World Bank conducted a large-scale RCT evaluation of PKH's impact on child education and health outcomes during its initial rollout (2007–2009), with baseline and endline surveys (Alatas et al., 2011; Cahyadi et al., 2020). The evaluation used a

[4]See Appendix Figure A.3 for a Word Cloud from crime descriptions, Appendix Table A.1 for selected crime case examples, and Appendix Table A.2 for descriptive statistics by crime type.

[5]Appendix C.2 includes results for 2013–2014 and 2017–2019, where households reported their PKH-recipient status directly. In these samples, we use matching techniques to estimate treatment effects.

clustered randomized design across 360 sub-districts in six provinces (DKI Jakarta, East Java, East Nusa Tenggara, Gorontalo, North Sulawesi, and West Java), with 180 control and 180 treatment sub-districts. We utilize this RCT in two ways: (1) to identify PKH's impact on violent crime using its random assignment, and (2) to explore mechanisms relying on baseline and endline household survey data.

Sample construction Our main analysis uses two samples. The first, the RCT sample, links NVMS crime data to sub-districts in the World Bank's PKH impact evaluation. Out of the 360 sub-districts in the RCT, 250 are covered by NVMS (123 control, 127 treatment sub-districts, within DKI Jakarta, East Java, East Nusa Tenggara, and North Sulawesi). The sample spans 2005–2010, with 10,980 observations across 1,830 communities over six years. Observations post-2010 are excluded because control areas began adopting PKH by then. As the original RCT ensured covariate balance across control and treatment locations within the 360 sub-districts (Alatas et al., 2011; Cahyadi et al., 2020), our RCT sample with NVMS coverage also retains balance at baseline (cf. Appendix Tables B.3 and B.4).

The second sample, the Roll-out sample, links NVMS crime data with administrative PKH rollout records. It spans 2005–2014, consisting of 288,730 observations across 28,873 communities. As of 2014, Indonesia had 82,190 communities.[6] Our sample only includes communities that adopted PKH by 2014, reducing the risk of omitted variable bias (communities without PKH access might differ systematically from those in the early rollout period). Of all Indonesian communities, about 54% are covered by NVMS. Appendix Table A.3 shows that NVMS-covered communities are smaller and more rural, consistent with NVMS's focus on violence-prone regions. In robustness checks, we relax these restrictions and (i) analyze all NVMS communities, regardless of PKH adoption timing, and (ii) assess PKH's crime impact nationwide using victimization surveys (including non-NVMS areas).

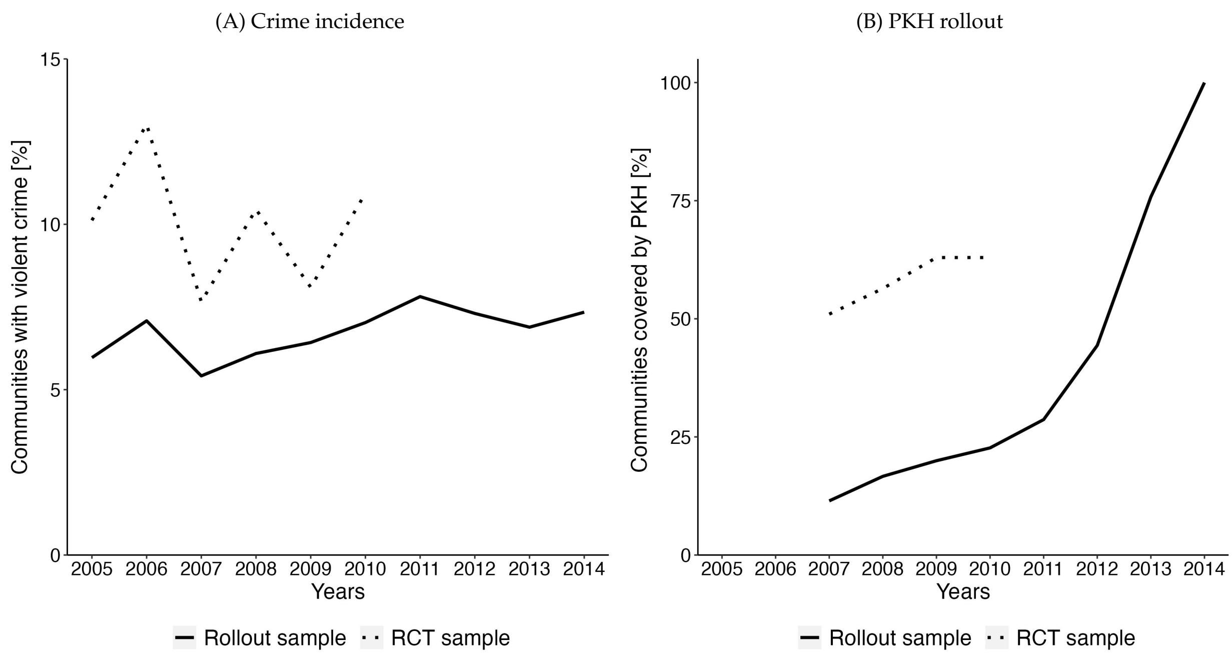
Other data To explore mechanisms, we use additional data beyond the RCT survey. The national household survey SUSENAS expands coverage beyond the RCT by including both eligible and ineligible households. We leverage SUSENAS data on time use, employment, PKH recipient status, and parental practices. We also use PODES data, the Indonesian village (or community) census, which interviews local officials every 3–4 years to collect information on local economies, infrastructure, and population.

[6] Indonesia experienced administrative decentralization between 2005–2014, creating new communities, sub-districts, and districts. Our analysis uses 2014 administrative boundaries and identifiers, matched via a crosswalk based on RAND (2022).

3.2 Variable construction

Violent crime Using NVMS data, we construct annual community-level measures of violent crime, defined by a binary indicator variable (1 if a community recorded at least one violent crime in a year, 0 otherwise). Figure 1 (Panel A) shows crime trends in the RCT sample and Roll-out sample. In the Roll-out sample, violent crime cases rose moderately over time: 2,800 communities reported incidents in 2005, increasing to 3,500 by 2014 (averaging 6% of all communities annually). The RCT sample shows a higher incidence (around 10%), likely due to its more centrally located communities, which may reduce underreporting of crime. Violent crime remains rare in both samples, with fewer than 2% of communities in the Roll-out sample reporting more than one crime annually and only 0.2% reporting more than 5 crimes. Thus, our empirical models focus on the extensive margin: on whether crime occurs rather than its frequency.

Figure 1: Community-level crime incidence and PKH rollout



Note: The Roll-out sample includes 28,873 communities that received PKH between 2007 and 2014. The RCT sample covers 1,830 communities that were part of the RCT, ending in 2010.

Treatment indicators PKH coverage expanded rapidly across Indonesia, growing from 4,000 communities in 358 sub-districts in 2007 to 56,000 communities in 4,800 sub-districts by 2014. Our primary treatment indicator tracks PKH's actual implementation at the community level (using MoSA administrative data), indicating whether PKH was active in a community in a given year. Figure 1 (Panel B) shows rising PKH coverage over time in both the RCT and Roll-out samples.

For an intent-to-treat (ITT) analysis in the RCT sample, we construct a binary indicator denoting original RCT treatment assignment (1 if a community is in a treatment sub-district; 0 otherwise). As shown in Cahyadi et al. (2020) and Appendix Table B.5, PKH implementation in the RCT did not fully align with the intended assignment. While treatment-group compliance was nearly perfect (99.8%), control-group compliance eroded over time as the national rollout progressed. By 2010, 33% of RCT control communities adopted PKH. We use RCT treatment assignment as an instrument for actual program uptake to identify local average treatment effects.

To analyze crime victimization data and explore further mechanisms, we draw on the nationwide household survey, SUSENAS, that contains no community, only sub-district identifiers. Due to this constraint, we define two alternative treatment variables: the proportion of communities implementing PKH and a binary indicator for sub-districts with at least 100 PKH-enrolled households.

4 The impact of the CCT on violent crime

4.1 Econometric framework

RCT sample We estimate three distinct econometric models to identify causal effects in the *RCT sample*. First, we use a two-way-fixed effects (TWFE) model, linking community-level violent crime to PKH implementation:

$$Crime_{ckdt} \quad = \quad \eta \, PKH\text{-}Treat_{ckdt} + \lambda_c + \theta_{dt} + \epsilon_{ckdt}, \tag{1}$$

where $Crime_{ckdt}$ is a binary variable indicating violent crime occurrence in community c, sub-district (*kecamatan*) k, district (*kabupaten* or *kota*) d in year t. $PKH\text{-}Treat_{ckdt}$ is a binary indicator that denotes the actual implementation of the program. Community fixed effects, λ_c, control for time-invariant community characteristics affecting crime propensity. District-year fixed effects, θ_{dt}, account for economic and political district-level dynamics. Standard errors are clustered at the sub-district level to address serial correlation and align with the RCT's group-wise treatment design (Angrist and Pischke, 2008; Abadie et al., 2022).

Second, we estimate ITT effects by regressing crime on the original treatment assignment:

$$Crime_{ckdt} = \alpha\, PKH\text{-}Assign_{kd} \times Post_t + \lambda_c + \theta_{dt} + \epsilon_{ckdt}, \tag{2}$$

where $PKH\text{-}Assign_{kd}$ is the original treatment assignment, $Post_t$ indicates the post-treatment years, and α is the ITT effect.

Third, we address potential biases in ITT estimates stemming from imperfect compliance (see Appendix Table B.5), as some control communities also adopted PKH. To correct for this, we use an instrumental variable approach. The instrument is the interaction between original treatment assignment, $PKH\text{-}Assign_{kd}$, and a post-treatment indicator. LATE estimates are obtained using two-stage least squares (2SLS):

$$\begin{aligned} PKH\text{-}Treat_{ckdt} &= \beta\, PKH\text{-}Assign_{kd} \times Post_t + \pi_c + \phi_{dt} + u_{ckdt}, \\ Crime_{ckdt} &= \gamma\, PKH\text{-}Treat_{ckdt} + \lambda_c + \theta_{dt} + \epsilon_{ckdt}. \end{aligned} \tag{3}$$

Roll-out sample We compare RCT estimates with large-scale quasi-experimental results using the staggered national roll-out of PKH between 2005 and 2014. In the Roll-out sample, we estimate treatment effects via equation (1) using standard TWFE specifications and event-study designs robust to heterogeneous treatment effects (cf. Borusyak et al., 2024; Sun and Abraham, 2021; Hazlett and Xu, 2018).

The staggered rollout requires assumptions of parallel trends (conditional on district-year fixed effects) and constant treatment effects (Goodman-Bacon, 2021). To analyze factors driving early program access, Appendix Table A.4 regresses the first year of PKH introduction on baseline community characteristics using 2008 PODES data (collected shortly after PKH's 2007 launch, avoiding program influence). Results show that PKH prioritized populous, urban, and poorer communities initially. However, supply-side preparedness indicators like health facilities and schools show mixed associations: kindergartens are associated with earlier rollout, while health facilities are linked to delayed access. Importantly, however, these correlations vanish when district fixed effects are included (joint F-statistic drops from 28.6 to 1.4). This suggests that within-district rollout timing is largely idiosyncratic. Thus, our preferred specifications always include district-year fixed effects.

4.2 Main results

Panel A of Table 1 reports results from the RCT sample. TWFE estimates (columns 1–2) show a positive, statistically significant effect of PKH on violent crime incidence. Controlling for district-year dynamics (column 2) strengthens precision, with significance at the 5% level or below. ITT estimates (column 3) align with TWFE results, while LATE estimates (column 4) reveal even larger crime increases. The LATE estimate indicates a 3.2 percentage point rise in the likelihood of violent crime occurrence at the extensive margin, or a 32% increase relative to non-PKH communities.[7]

Table 1: The impact of the CCT program on violent crime

	Panel A: RCT sample (2005–2010)			
Estimation	TWFE		TWFE (ITT)	IV (LATE)†
	(1)	(2)	(3)	(4)
PKH treatment	0.021*	0.026***	0.027***	0.032***
	(0.011)	(0.010)	(0.010)	(0.012)
Mean (control)	0.101	0.101	0.101	0.101
Community FE, year FE	Yes	Yes	Yes	Yes
District-year FE		Yes	Yes	Yes
Adj. R-squared (F-stat^{F})	0.206	0.221	0.447	1735.0^{F}
Observations (clusters)	10,980 (250)	10,980 (250)	10,980 (250)	10,980 (250)
	Panel B: Roll-out sample (2005–2014)			
Estimation	TWFE		BJS‡	TJBAL¶
	(1)	(2)	(3)	(4)
PKH treatment	0.004*	0.007***	0.015***	0.014***
	(0.002)	(0.003)	(0.004)	(0.002)
Mean (control)	0.060	0.060	0.060	0.054
Community FE, year FE	Yes	Yes	Yes	
District-year FE		Yes	Yes	
Adj. R-squared	0.307	0.321		
Observations (clusters)	288,730 (2,335)	288,730 (2,335)	244,270 (2,335)	259,857

Note: The dependent variable is a binary indicator that takes the value one if NVMS reported at least one violent crime incident in a community in a given year. The RCT sample (Panel A) is restricted to 1,830 communities. The Roll-out sample (Panel B) includes at most 28,873 communities. In the TWFE and IV models, PKH treatment measures actual program access by at least one household. In the ITT model, PKH treatment stands for the original treatment assignment status at the sub-district level. In the IV model, PKH access is instrumented by PKH assignment. †The first-stage coefficient of PKH treatment on PKH assignment is 0.844 (0.020); ^{F}Kleibergen-Paap F-statistic. ‡Borusyak et al.'s (2024) imputation-based estimator. ¶Hazlett and Xu's (2018) mean trajectory balancing estimator; dynamic estimates are shown in Appendix Figure A.5. Robust standard errors are clustered at the sub-district level and reported in parentheses. TJBAL's standard error is based on bootstrapping over 100 iterations. */**/*** denote significance levels at 10/5/1% respectively.

Panel B of Table 1 presents results from the nationwide PKH rollout until 2014. Consis-

[7]This effect size lies within a reasonable range. At the median, 38 households received cash transfers within RCT communities during this time period, corresponding to an increase in yearly crime propensity of 0.83% per CCT household ((0.032/0.101)/38), or one additional major crime event per about 120 CCT households per year (1/0.0083).

tent with RCT findings, TWFE estimates (columns 1–2) show that the CCT program increased violent crime, though effect sizes are smaller (0.4–0.7 percentage points). Adding time fixed effects interacted with initial community characteristics (as in Appendix Table A.4) in column 2 has little impact on the estimates. To address biases in staggered designs with time-varying treatment effects, columns 3–4 report results from two estimators: the imputation-based BJS estimator and the synthetic control TJBAL estimator (Borusyak et al., 2024; Hazlett and Xu, 2018).[8] Both estimators confirm a positive link between PKH coverage and violent crime, with larger and more precisely estimated coefficients (25–26% increases relative to a baseline of 5.4–6%). This magnitude aligns with the RCT sample's relative crime increases. Overall, results indicate PKH access substantially raised violent crime rates in Indonesian communities.

4.3 Robustness checks

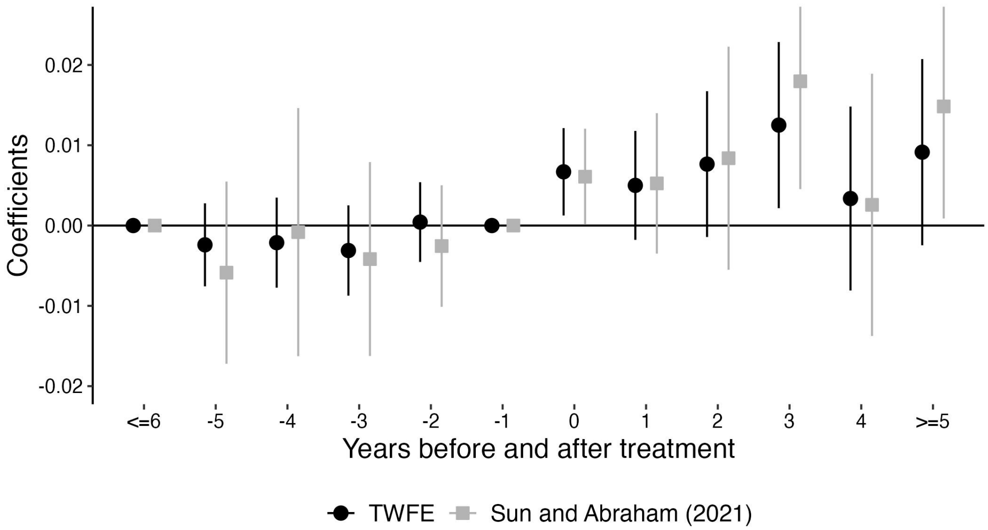
An event study design tests the parallel trend assumption for the Roll-out sample and examines dynamic treatment effects. We replace the static treatment indicator in equation (1) with a set of dynamic indicators $\textit{PKH-Treat}_{ckd,t+\tau}$ with $\tau \in \{\text{-}5, \text{-}4, \text{-}3, \text{-}2, 0, 1, 2, 3, 4, 5^{+}\}$, which take one in $t+\tau$ years before and after PKH was introduced in a community and zero otherwise. The baseline omitted category is $t-1$.[9] Results in Figure 2 show that pre-treatment coefficients are near zero and statistically insignificant, supporting the parallel trends assumption in DiD models. Post-treatment effects grow gradually and persist for at least three years. These dynamics align with rising PKH household participation in treated communities, potentially amplifying spillover effects on local crime rates.

Two concerns arise regarding the identification of our main treatment effects. First, the staggered nature of our DiD specifications may lead to treatment effects reflecting period- and group-specific weighting biases rather than true average effects. We address this using Sun and Abraham's (2021) interaction-weighted estimator, which allows to efficiently control for district-year fixed effects. While results become less precisely estimated, the qualitative dynamics remain consistent. Second, unobservable time-varying factors might influence treatment and violent crimes simultaneously at the community

[8] Both estimators assume linearity in outcomes, projecting potential untreated outcomes using pre-treatment data (Hazlett and Xu, 2018). The BJS estimator projects fixed effects using 'not-yet-treated' observations. TJBAL extends the generalized synthetic control method (Xu, 2017) by balancing on more pre-treatment outcome principal components, making it efficient for short pre-treatment periods and multiple treatment timings.

[9] As all sample communities were treated by 2014, we include a second (binned) omitted category for pre-treatment years beyond five years before program start and assume constant effects after four post-treatment years (cf. Schmidheiny and Siegloch, 2023; Borusyak et al., 2024).

Figure 2: Staggered DiD in the roll-out sample



Note: The figure presents dynamic PKH effect estimates using the actual PKH implementation indicator (equation 1) in the Roll-out sample. It includes 28,873 communities exposed to PKH between 2007 and 2014 with full NVMS data coverage. The dependent variable is a binary indicator (1 if NVMS reported at least one violent crime in a community-year, 0 otherwise). Regressions include community, year, and district-year fixed effects. 90% confidence intervals are reported based on robust standard errors clustered at the sub-district level.

level, even if pre-treatment coefficients are insignificant (cf. Roth, 2022). The sensitivity analysis following Rambachan and Roth (2023) yields wide confidence intervals under moderate deviations from parallel trends (cf. Figure A.4). To mitigate this, we relax the linear parallel trends assumption by adopting Hazlett and Xu's (2018) synthetic control method with trajectory balancing. Results in Appendix Figure A.5 reaffirm our main findings: PKH effects increase by 1–3% in the first four years before declining.

Causal identification in DiD relies on the stable unit treatment value assumption (SUTVA), which assumes no spillovers between treatment and control units. Given our spatial units of analysis (communities), the primary concern is spatial spillovers. Negative spillovers, displacing crime from control to treatment communities due to program rollout (e.g., criminals shifting their activities to PKH communities), could bias estimates upward. To assess the relevance of such spatial spillovers, we first examine heterogeneous treatment effects by Euclidean distance between treatment and control communities (Crost et al., 2016). Second, we re-estimate our main results using Clarke's (2017) 'spillover-robust double difference estimator,' which accounts for how spillover effects vary with distance between communities. Findings in Appendix Table A.5 show no evidence of spillover effects on neighboring communities.

To strengthen the credibility of our causal estimates, we conduct additional sensitivity checks. Appendix Table A.6 randomly assigns treatment status in the Roll-out sample across and within years (columns 1–2). Results show no evidence of a structural overestimation (10% of estimates are significant at the 10% level), confirming that our main results are not driven by unobserved factors such as spuriously coinciding trends (Athey and Imbens, 2017). Results also hold when using randomization-inference p-values for RCT sample estimates (column 3). For standard error adjustments, Appendix Table A.7 tests clustering at the district level (next administrative tier) and Conley (1999) standard errors within a 50-km radius to account for spatial correlation. Program effects remain significant at the 1% level in both the Roll-out sample and RCT sample. Finally, Appendix Table A.8 examines robustness to sample construction by extending the RCT sample to include all 3,323 communities within RCT sub-districts (irrespective of baseline/endline data availability), aggregating all data to the sub-district level (as treatment assignment was sub-district based), extending the Roll-out sample to 2000–2014 (including early years with worse NVMS coverage), and broadening the sample to include all NVMS-covered communities (47,680 communities, not limited to PKH-adopting areas by 2014). Results remain consistent across all specifications.

Lastly, we assess sensitivity to measurement issues across several dimensions. First, we analyze the intensive margin of crime by regressing the number of violent incidents per community-year. As variation comes predominantly from the extensive margin, we use an inverse hyperbolic sine transformation to retain zero values. Results in Panel A of Appendix Table A.9 show significant crime increases in both the RCT and Roll-out samples. The scale-invariant (Chen and Roth, 2023) Poisson Pseudo-Maximum Likelihood model (PPML) (panel B) yields insignificant results on average, but rural communities—where both estimation strategies yield highly significant effects—experience robust crime increases. Second, we examine treatment intensity in the Roll-out sample by splitting the treatment indicator based on the number of CCT households per community. Appendix Table A.10 confirms significant crime increases only when more than a few households receive transfers. Third, we test whether migration patterns (e.g., delayed migration of young individuals) drive results. Using PODES data, we find no evidence that PKH altered community population levels (Appendix Table A.11), consistent with prior findings on household attrition in the RCT (Cahyadi et al., 2020). Fourth, we assess whether violent crime might reflect conflict spillovers rather than genuine crime increases (Crost et al., 2016; Premand and Rohner, 2023). Replacing our crime measure with NVMS-coded conflict indicators (disaggregated by type) and re-estimating effects (Appendix Table A.12) yields no link between PKH and local conflict in Indonesia.

4.4 Additional evidence from crime victimization surveys

Crime victimization surveys (from SUSENAS) address two key concerns about NVMS crime data. First, NVMS data may suffer from non-random measurement error if newspapers focus their attention on CCT-receiving communities. Second, NVMS covers only the more violence-prone (remote, less wealthy) half of Indonesia, limiting external validity. Victimization surveys mitigate these issues by providing an alternative crime measure. While self-reported and subject to biases, responses are likely independent of newspaper reporting, addressing the first concern. Additionally, surveys also cover non-NVMS regions, enabling nationwide validity checks.

We link household-level SUSENAS data to sub-district-level CCT rollout data. We restrict our sample to 2007–2011 due to the inclusion of crime questions and sub-district identifiers. We include only sub-districts surveyed at least twice and estimate the following TWFE model at the household level:

$$\textit{Crime victim}_{jkdt} = \eta \, \textit{PKH-Treat}_{kdt} + X'_{jkdt}\gamma + \kappa_k + \theta_{dt} + \epsilon_{jkdt}, \tag{4}$$

where $\textit{Crime victim}_{jkdt}$ indicates whether household j in sub-district k of district d reported a theft or robbery in year t.[10] $\textit{PKH-Treat}_{kdt}$ is defined as either the share of communities in the sub-district k covered by the program or an indicator for whether at least 100 households in sub-district k accessed PKH in year t. Controls X_{jkdt} include urban status, household head's education, age, marital status, and household size quintiles. To align with the difference-in-differences specifications, we include sub-district fixed effects, κ_k, and district-year effects, θ_{dt}, focusing on sub-district-level changes in crime victimization over time. As before, we restrict the sample to sub-districts exposed to CCTs by 2014.

Table 2 shows results consistent with those in Table 1: program rollout increased self-reported crime nation-wide. In column 1, full sub-district PKH coverage raises the probability of being a crime victim by 0.9 percentage points (24%). Similar effects emerge when at least 100 households access PKH (column 4).[11] Effect magnitudes align with our main NVMS-based results, reducing concerns that findings reflect newspaper reporting biases rather than actual crime changes.[12] Crucially, coefficients mirror those in Table 1, confirming our results are not confined to NVMS-covered areas. This strengthens exter-

[10] Thefts and robberies dominate crime reporting in the SUSENAS victimization surveys. Results hold when analyzing *all* reported crimes instead (cf. Panel B of Appendix Table A.13).

[11] Appendix Table A.14 shows significant crime increases only when PKH reaches many households.

[12] Using PODES data on local police stations, Appendix Table A.15 helps rule out improved local policing as an alternative explanation.

Table 2: *Alternative measure:* CCT access and the probability of being a victim of crime (2007–2011)

PKH rollout:	Village share in sub-district			At least 100 households in sub-district		
Household sample:	All	Poor	Non-poor	All	Poor	Non-poor
	TWFE	TWFE	TWFE	TWFE	TWFE	TWFE
	(1)	(2)	(3)	(4)	(5)	(6)
PKH rollout	0.009***	0.026***	0.008**	0.008**	0.021***	0.007*
	(0.004)	(0.008)	(0.004)	(0.003)	(0.006)	(0.003)
Mean	0.037	0.029	0.038	0.037	0.029	0.038
Sub-district FE	Yes	Yes	Yes	Yes	Yes	Yes
District-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,095,130	95,500	999,630	1,095,130	95,500	999,630

Note: Estimates are restricted to households in sub-districts exposed to the program by 2014. The dependent variable is a binary indicator (1 if the household reported at least one theft or robbery in a given year). The first treatment variable (columns 1–3) measures the share of communities within a sub-district receiving PKH; the second treatment variable (columns 4–6) equals 1 if at least 100 households in the sub-district received PKH. Controls include education level, age, and marital status of the household head, household size quintiles, and urban status. Standard errors (clustered at the sub-district level) are in parentheses. */**/*** denote significance at 10/5/1% levels.

nal validity by showing that the effects hold nationwide. Split sample estimates (columns 2–3 and 5–6 for poor vs. non-poor households) show crime victimization rises for both poor and non-poor households.[13] CCT coverage is associated with a 0.8 percentage point (21%) increase in victimization among non-poor households (ineligible for transfers), but effects are substantially larger among poor households, ranging from 2.1 to 2.6 percentage points (72–90%). Appendix C.2 presents results analyzing crime victimization from 2013–2019 using SUSENAS data (cf. Table C.2). Since administrative program rollout data cannot be matched to the survey for these years, we use households' self-reported PKH receipt (available for selected years). Propensity score matching shows CCT-recipients report crime victimization more frequently than comparable eligible poor households.

A potential rival explanation—increased victimization might reflect changes in crime-reporting behavior—is tested by examining the share of crimes reported to the police relative to all experienced crimes. Appendix Table C.3 finds no evidence that CCT recipients report crimes to the police more often than non-receiving households.

[13]Household poverty status is determined using per capita expenditures relative to Indonesia's rural and urban provincial poverty lines. Focusing on the smaller RCT sample, Appendix Table A.13 shows increases in crime among poor households but finds no significant increases for non-poor households.

5 Potential mechanisms

Our findings indicate that the CCT program increased community-level violent crime. Potential mechanisms fall into two broad categories: effects increasing beneficiaries' victimization and effects increasing their likelihood of perpetrating crime. First, beneficiary households may hold more unprotected cash or acquire durable assets, making them targets for crime (Draca and Machin, 2015; Draca et al., 2018). Victimization could also rise due to weakened social cohesion, program-related discontent, or related general-equilibrium effects. Second, the CCT may prompt households to reallocate time and resources among members upon receiving cash transfers. School attendance requirements could boost enrollment and redirect parental attention toward school-age children, reducing younger members' criminal activities through incapacitation effects (cf. Attanasio et al., 2021; Bratsberg et al., 2019). However, cash transfers may reduce older siblings' need to work, increasing unemployment among this group (unbound by program conditions). Without additional parental focus, their free time may be channeled into risky or violent activities (Ludwig et al., 2001; Fougère et al., 2009; Bratsberg et al., 2019). This mechanism may disproportionately affect male youth, as economic crime literature highlights their heightened vulnerability to unemployment-driven criminality (Phillips et al., 1972; Myers Jr., 1983; Gelber et al., 2015; Freedman and Owens, 2016; Bell et al., 2018). Our analysis assesses these mechanisms as well as further related explanations.

The primary data in this section draws from the baseline and endline surveys of the World Bank's PKH impact evaluation.[14] We estimate treatment effects using 2SLS (following Cahyadi et al., 2020):

$$\begin{aligned} \textit{PKH-Beneficiary}_{jk} &= \gamma\ \textit{PKH-Assign}_k + X'_{(i)j0}\phi + \theta Y_{(i)jk0} + \alpha_d + \epsilon_{(i)jk} \\ Y_{(i)jk} &= \beta\ \textit{PKH-Beneficiary}_{jk} + X'_{(i)j0}\lambda + \omega Y_{(i)jk0} + \pi_d + \mu_{(i)jk} \end{aligned} \tag{5}$$

where $Y_{(i)jk}$ denotes the outcome variable for individual i or household j in sub-district k at the endline survey, while $Y_{(i)jk0}$ denotes its baseline value. The treatment variable, $\textit{PKH-Beneficiary}_{jk}$, is a binary indicator of PKH transfers at the household level. The instrument $\textit{PKH-Assign}_k$ reflects original treatment assignment at the sub-district level. The vector $X_{(i)j0}$ includes baseline controls such as respondent demographics (age, gender, marital status), household head characteristics (education, occupation), household size,

[14] As shown in Cahyadi et al. (2020), the evaluation achieved balanced covariates, high compliance with treatment assignment, and minimal attrition. We replicate these findings in Appendix Tables B.3 to B.8 for our sample.

and dwelling features (e.g., water and sanitation, construction materials). District fixed effects are denoted by α_d and π_d, error terms by $\epsilon_{(i)js}$ and $\mu_{(i)js}$. Standard errors are clustered at the sub-district level (randomization unit).

5.1 Victimization potential

Lootable assets While the CCT is designed to fund education, healthcare, and food for young children, households may use the transfers to acquire valuable, easily looted assets like cellphones, TVs, or motorbikes (Borraz and Munyo, 2020). Such purchases could increase a community's asset base, raising incentives for theft and violent crime.[15] However, as shown in Table 3 and consistent with Cahyadi et al. (2020), we find no evidence of increased asset accumulation among beneficiaries. This null result might stem from non-random measurement error in asset/wealth variables due to selective underreporting by households seeking to retain eligibility. However, Banerjee et al. (2020) find such underreporting is not a major issue in the Indonesian PKH context.

Table 3: The short-run impact of the CCT program on assets and expenditures (*LATE estimates*)

	Panel A: Total and fixed assets			
	ln Assets	Radio	TV	Antenna
PKH beneficiary	−0.002	0.033	−0.005	0.011
	(0.051)	(0.103)	(0.027)	(0.010)
	Panel B: Mobile assets			
	Bicycle	Motorcycle	Car	Cell
PKH beneficiary	−0.027	0.011	0.007	0.000
	(0.121)	(0.030)	(0.005)	(0.041)
	Panel C: Expenditures			
	ln Total Exp.	*ln* Food Exp.	*ln* Non-food exp.	*ln* Transport Exp.
PKH beneficiary	−0.008	−0.003	−0.018	−0.117
	(0.032)	(0.033)	(0.047)	(0.092)

Note: Results are reported for 12,929 households, based on 2SLS estimates (LATE) from the RCT's impact evaluation, using the baseline and endline surveys. All regressions use district fixed effects and a set of controls. The abbreviation 'Exp.' refers to per capita expenditure values in logs. Robust standard errors in parentheses are clustered at the sub-district level. */**/*** denote significance levels at 10/5/1% respectively.

[15] Alternatively, such assets might enhance information access and mobility, thereby enabling criminal activities (Glaeser et al., 1996). However, the null findings do not support this mechanism.

CCT beneficiaries' spending on essentials like food, healthcare, and education could indirectly boost non-recipients' wealth through local economic spillovers (Angelucci and De Giorgi, 2009; Cunha et al., 2019; Filmer et al., 2021; Egger et al., 2022; Muralidharan et al., 2023). For instance, increased demand for goods and services might boost community-level economic activity, benefiting non-participating households. However, using PODES data and estimating equation 1, Appendix Table A.11 shows no significant increase in community wealth proxies (e.g., access to services, electricity, or markets) following program implementation.

Inequality and injustice motivations The motivations for criminal behavior extend beyond monetary gains to include psychological drivers such as perceptions of fairness, retribution, and accomplishment. Government programs that inadvertently exacerbate local inequality, particularly through the exclusion of eligible poor households, may trigger such reactions (Fajnzylber et al., 2002; Cameron and Shah, 2014). While mistargeting of transfers to non-poor households remains a documented issue in PKH implementation (World Bank, 2012b; Alatas et al., 2019), our analysis of baseline and endline RCT survey data (Appendix Table A.16) finds no evidence that the CCT program increased inequality among the poor in treated communities. Extending Cameron and Shah's (2014) framework, we further examine whether implementation errors (undercoverage and leakage) correlate with violent crime increases. Using SUSENAS 2014 data, which first captures household-level beneficiary status, we define undercoverage as the proportion of eligible households excluded from PKH and leakage as the share of ineligible households receiving benefits. Combining these metrics with RCT and Roll-out sample data (Appendix Table A.17), we find no robust association between implementation errors and heightened violent crime. If anything, tentative evidence suggests undercoverage negatively correlates with crime increases, implying that violent crime rises only when a critical mass of eligible poor households receive benefits—a finding inconsistent with the hypothesis that exclusion drives retributive behavior.

5.2 Intra-household spillovers and youth idleness

RCT sample evidence We analyze the CCT program's effects on time allocation by gender and age, focusing on four main activities reported by respondents when asked "What was a person's main activity over the last week?": market work, household chores, school attendance, and idleness (non-participation in the first three, see Appendix B.3 for details). LATE estimates from the RCT in Table 4 (using equation 5) are stratified by gender and three age cohorts: (1) the program-targeted group (7–15 years), (2) non-targeted youth (18–25 years) potentially exposed to intra-household spillovers, and (3) adults (26–35 years), who are typically more attached to the labor market and thus less likely to adjust their labor supply in response to transfers. We exclude 16–17-year-olds due to partial program eligibility during our observation period. Their results (Appendix Table A.20) are insignificant for most outcomes.

Table 4: *RCT:* Short-run effects of the CCT program on work, schooling, and idleness by cohort

	Men			Women		
Age group:	7–15 (1)	18–25 (2)	26–35 (3)	7–15 (4)	18–25 (5)	26–35 (6)
Working	−0.014	−0.098**	−0.030	−0.004	−0.074*	0.010
	(0.009)	(0.049)	(0.025)	(0.008)	(0.063)	(0.046)
Household chores	0.0001	0.002	0.004	−0.004	0.078	−0.006
	(0.002)	(0.008)	(0.006)	(0.004)	(0.050)	(0.045)
Attending school	0.078***	0.013	0.001	0.035**	−0.031	−0.0001
	(0.018)	(0.024)	(0.005)	(0.017)	(0.026)	(0.003)
Staying idle	−0.063***	0.086*	0.025	−0.025***	0.032	0.003
	(0.016)	(0.050)	(0.024)	(0.014)	(0.044)	(0.012)
Observations	9,296	3,583	4,438	8,545	3,170	5,607
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: The dependent variables reflect the main activity during the last week before the survey, capturing whether individuals primarily engaged in (i) work outside the household, (ii) household chores, (iii) school attendance, or (iv) remained idle (not participating in work, household chores, or school). Estimates use the RCT sample, with 2SLS models instrumenting community-level treatment with sub-district level treatment assignment. Age categories encode a person's age at the endline survey. Controls include urban residence, quintiles of household size, and indicators for the household head's marital status, education level, and age. Standard errors in parentheses are clustered at the sub-district level. */**/*** denote significance at 10/5/1% level.

The CCT program effectively increased school enrollment among the targeted cohort (7–15 years), consistent with findings in Cahyadi et al. (2020). As expected, school attendance among non-targeted age groups in beneficiary households remained unchanged. However, we observe statistically significant reductions in market work participation among young adults (18–25 years) in transfer-receiving households. Notably, while the program reduced idleness in favor of schooling within the targeted age group, it coincided with a marginally significant 8.6 percentage point rise in idleness among young men (18–25 years), representing a 28% increase relative to the 30.3% baseline idleness rate in this cohort (see Appendix Table B.4). This increase in youth idleness likely stems from intra-household spillovers affecting non-targeted young adults in beneficiary households. One alternative explanation—households strategically reducing older children's earnings to maintain eligibility (as proposed in Khanna et al., 2023)—is implausible here: eligibility criteria are not re-verified post-enrollment, and households could easily underreport income or assets if re-evaluated.[16]

Official data on criminal demographics in Indonesia highlights that violent crimes are disproportionately committed by young men with low educational attainment and lower socio-economic status (BPS, 2013; POLRI, 2019). Given this profile, the observed rise in idleness among male youth in CCT households may have channeled their time toward risky behaviors, including violent crime. To further validate this mechanism, we examine complementary evidence linking program-induced idleness to crime outcomes.

Nationwide evidence We examine whether program-induced increases in idleness among male youth persist at scale using Indonesia's National Household Survey (SUSENAS, for years with sub-district information, 2004–2011). The survey further enables placebo tests by assessing idleness changes in households ineligible for the CCT, a group excluded from the RCT sample by design. We first focus on PKH-eligible households—those below provincial poverty lines with at least one child aged 0-15—and estimate individual-level pooled models with sub-district-year fixed effects and controls (equation (4)). Dependent variables measure primary activities over the prior week: market work, household chores, schooling, or idleness (non-participation in the first three). Given the lack of community identifiers in the survey, treatment exposure is operationalized as the proportion of villages within a sub-district participating in PKH. Nationwide results in Table 5 mirror the RCT findings (Table 4), though with smaller relative effect sizes. Among targeted children (7–15 years), program exposure reduces work and idleness in favor of school attendance. For non-targeted youth (18–25 years), idleness rises significantly among males as PKH expands within sub-districts. In a sub-district where all communities implement PKH, male youth idleness increases by 2.8 percentage points—a 13% rise relative to the 21.9% baseline.

Placebo checks in Table 6 show that while idleness rises significantly among males in PKH-eligible poor households (column 1), no such increases emerge for young males in non-eligible poor households (column 2) or non-poor households (columns 3–4). This rules out spurious correlations between program expansion and regional labor market trends unrelated to the CCT, reinforcing that observed effects stem specifically from program exposure rather than broader economic shifts affecting poor youth. Further, within

[16]Our results contrast with Banerjee et al. (2020), who found no labor supply effects of PKH. Re-estimating their ITT models, we confirm no average impact on labor supply for adults (18–60/65 years) but uncover reduced labor participation among younger adults (e.g., under 26 years), reinforcing that the CCT induced labor supply adjustments among non-targeted youth.

Table 5: *Rollout:* Country-wide effects of the CCT program on work, schooling and idleness by cohort

	Men			Women		
Age group:	7–15	18–25	26–35	7–15	18–25	26–35
	(1)	(2)	(3)	(4)	(5)	(6)
Working	−0.025***	−0.030*	0.008	−0.021***	−0.049***	−0.024
	(0.007)	(0.017)	(0.009)	(0.006)	(0.018)	(0.015)
Household chores	−0.021**	−0.028**	−0.018	−0.056***	−0.031*	−0.005
	(0.010)	(0.013)	(0.015)	(0.014)	(0.019)	(0.007)
Attending school	0.036***	−0.004	−0.001	0.038***	0.006	0.000
	(0.009)	(0.009)	(0.001)	(0.010)	(0.011)	(0.001)
Staying idle	−0.016**	0.028*	−0.007	−0.014**	0.018	0.003
	(0.007)	(0.015)	(0.008)	(0.006)	(0.012)	(0.003)
Observations	82,730	41,357	38,087	75,546	33,493	59,525
Sub-district FE, year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: The dependent variables reflect the main activity during the last week before the survey, capturing whether individuals primarily engaged in (i) work outside the household, (ii) household chores, (iii) school attendance, or (iv) remained idle (not participating in work, household chores, or school). Appendix Table A.19 reports dependent variable means. The treatment variable measures the share of PKH recipient communities within a sub-district-year. Results are based on pooled cross-sections of SUSENAS (2004–2011), restricted to sub-districts that received the program by 2014. Only households below the provincial poverty line and with at least one child of eligible age are included. Controls include urban residence, quintiles of household size, and indicators for the household head's marital status, education level, and age. Standard errors in parentheses are clustered at the sub-district level. */**/*** denote significance at 10/5/1% level.

eligible households, idleness increases exclusively for the household head's children (column 5) and not for other young relatives (column 6), underscoring intra-household spillovers as the primary mechanism—likely benefiting older siblings who may reduce labor supply in response to transfers. Additional analyses (Appendix Table A.20) confirm that the idleness effect peaks among 18–20-year-olds in the nationwide sample, diminishing in magnitude and significance for older youth (21–25). Notably, the youngest non-eligible cohort (16–17 years) shows no idleness increases in SUSENAS data but exhibits marginally higher schooling participation, possibly reflecting delayed enrollment adjustments not captured in the RCT. These patterns align with the hypothesis that CCT spillovers disproportionately affect targeted and adjacent age groups within beneficiary households.

Parental attention CCTs may reshape parental time allocation by incentivizing parents to prioritize activities tied to program conditionality, such as monitoring children's health and schooling. Drawing on Akee et al. (2010), who found that income shocks from casino openings increased parental monitoring among Native American families, we ex-

Table 6: *Placebo tests:* Middle-run effects of PKH on idleness by PKH eligibility

Sample:	Young men (aged 18–25)					
PKH eligibility criteria:						
Poverty status:	Poor	Poor	Non-poor	Non-poor	Poor	Poor
Children (< 16) in hh.:	Yes	No	Yes	No	Yes	Yes
Relation to hh. head:	All	All	All	All	Child	Non-child
	(1)	(2)	(3)	(4)	(5)	(6)
Staying idle	0.028*	–0.017	–0.007	–0.005	0.034**	–0.001
	(0.015)	(0.018)	(0.007)	(0.005)	(0.017)	(0.037)
Observations	41,357	23,915	156,236	219,933	34,095	5,305
Sub-district FE, year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: The dependent variable, 'Staying idle', refers to the last week before the survey and indicates whether individuals were reported as not engaging in work, household chores, or school. The treatment variable measures the share of PKH recipient communities within a sub-district-year. Results are based on pooled cross-sections of SUSENAS (2004–2011), restricted to sub-districts that received PKH by 2014. Results distinguish between young men living in PKH-eligible and non-eligible households (defined by per capita household expenditures w.r.t. the poverty line and the presence of children of PKH-eligible age). Controls include urban residence, quintiles of household size, and indicators for the household head's marital status, education level, and age. Standard errors in parentheses are clustered at the sub-district level. */**/*** denote significance at 10/5/1% level.

amine whether PKH alters parenting behaviors. Our analysis uses cross-sectional data from the 2018 SUSENAS Socio-Cultural and Education Module, the only round including PKH beneficiary status. This module captures parental involvement in ten recommended child-rearing practices (e.g., shared reading, homework assistance, playtime, and prayer) for children aged 0–17. We construct a child-specific parenting quality index ranging from 0 (no activities) to 10 (all activities performed). To estimate PKH's effects given the data's cross-sectional nature, we employ propensity score matching, comparing PKH recipient households to similar non-recipients. Robustness checks using a principal component index or excluding education-focused activities yield comparable results (available upon request).

Table 7 reveals that PKH recipient households exhibit marginally higher child-specific parenting inputs overall (column 1). Age-disaggregated results (columns 2–4) highlight a targeted shift: parenting investments rise notably for school-age children within the CCT's eligibility criteria (aged 7–15, column 3) but decline for older children outside this range (column 4). Further analysis (Appendix Table A.22) shows that parental attention for younger children intensifies with school attendance, with PKH recipients demonstrating a disproportionate response to attendance compared to non-recipients (column 1). For older children, parental attention correlates strongly with school enrollment and diminishes with idleness (columns 2–3, Appendix Table A.22). This pattern, combined with the documented rise in idleness among non-targeted youth, suggests that PKH induces a reallocation of parental focus: attention shifts from older, idle youth toward younger, school-eligible children under program conditionality.

Table 7: Impact of PKH on index of parental attention: PSM estimates

Child's age in years:	Aged < 18	Aged < 7	Aged 7–15	Aged 16–17
	(1)	(2)	(3)	(4)
PKH	0.327***	-0.014	0.615***	-0.322***
	(0.029)	(0.062)	(0.027)	(0.064)
Mean dependent	2.43	2.20	2.58	2.50
Observations	92,504	33,785	49,134	9,585
Province FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Notes: The dependent variable is a composite index, summing up ten binary parenting indicators. PKH is a binary indicator for PKH recipient households. The sample is derived from the Parenting Module of SUSENAS 2018 and includes only individuals below the age of 18. Treated and control individuals are selected using a propensity score matching (PSM), matching 5 nearest neighbours, based on child's age, gender, and marital status, quintiles of household size, # children below the age of 12 in the household, # children between age 13 and 18 in the household, household wealth deciles, education of the household head, and rural residence status as covariates. Robust standard errors are in parentheses. */**/*** denote significance at 10/5/1 percent level.

These findings highlight how CCTs may inadvertently increase violent crime. Since parental supervision deters delinquency, the combined effect of changing time use (increased idleness) and diminished oversight exacerbates risks for male youth, who are overrepresented in crime statistics. This underscores unintended household spillovers of conditional aid.

5.3 The plausibility of the idleness-crime link

The PKH program's rollout, as evidenced by both RCT and nationwide data, raised local crime rates and idleness among youth in beneficiary households. With no dominant other mechanisms identified, a connection between increased idleness and criminal behavior appears plausible. While the lack of individual-level data on the perpetrators of crime precludes establishing a causal chain from PKH to idleness-driven crime, the suggestive evidence warrants further scrutiny. We proceed by (1) assessing the plausibility of the idleness-crime link in terms of effect magnitudes; (2) analyzing perpetrator demographics and crime timing; (3) examining PKH effects on idleness and crime under varying labor market conditions; and (4) investigating the public perception of poor, young, and idle men.

Plausibility of the estimated magnitudes We estimate an upper bound for crime incidence assuming all additional crimes stem from PKH-induced idleness in male youth (see Appendix D for details). Nationwide rollout estimates indicate a 1.4 percentage point rise in community-level crime annually (cf. Table 1, column 4), affecting approximately 779 local communities yearly. According to administrative records, combined with survey data, about 725 thousand young men aged 18–25 lived in PKH recipient households by 2014. The 2.8 percentage point increase in idleness due to PKH rollout (cf. Table 5) translates to about 20 thousand additional young men staying without an occupation. If each additional crime involved only one newly idle male, this would imply a 3.8% crime incidence rate among affected youth—a figure broadly consistent with our estimates of youth crime rates under varying assumptions (0.6%–14.5%) (based on BPS, 2014, see Appendix D). This supports the quantitative plausibility of the mechanism.

Perpetrators and timing of crime For youth idleness to be a mechanism of PKH increasing crime, the effect should neither be concentrated among employed, nor among schoolchildren. We use NVMS's rich contextual information to differentiate between the nature and timing of criminal activities and re-estimate our main specifications for the RCT and Roll-out samples. Results are shown in Table 8. Columns 1 and 2 report for both samples statistically insignificant changes for perpetrators explicitly listed as employed and significant increases for all others.[17] Coefficients for employed are smaller and are imprecisely estimated, indicating that employed people are not the dominant force.

An alternative explanation relates to the role of pupils and schools. While CCTs have been shown to reduce crimes through binding children to school (incapacitation effect), crimes could increase due to a productivity effect. This would be the case if schools would function as information, network, and coordination hubs for criminal activities (Jacob and Lefgren, 2003). If true, PKH should be increasing crime during periods when pupils have more time to commit crimes, such as weekends and public holidays. In contrast, results in columns 3 to 5 show that most of the crime effects arise during the workweek, with small and insignificant estimates during weekends and public holidays. Idle time at home is more likely to lead to criminal activity during workdays, when parental absence reduces monitoring.

Idleness, crime, and the labor market If youth idleness arises because CCT-receiving households withdraw their children from marginal jobs with limited economic prospects,

[17] About one-fifth of all crime events in NVMS are perpetrated by individuals with an explicitly reported occupation (either as private or government employees). The rest are mainly encoded as people without a clear affiliation.

Table 8: The impact of PKH by actors and timing of violent crime

Dependent:	Crime committed by		Crime committed on		
	employed	others	work*days*	week*ends*	public holidays
	(1)	(2)	(3)	(4)	(5)
	Panel A: RCT sample (2005–2010) ITT				
PKH assignment	0.007	0.019**	0.021**	−0.001	−0.001
	(0.006)	(0.009)	(0.009)	(0.008)	(0.005)
Mean (control)	0.031	0.066	0.078	0.038	0.021
	Panel B: Roll-out sample (2005–2014) TWFE				
PKH	0.002	0.005**	0.006**	0.002	−0.001
	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)
Mean (control)	0.016	0.044	0.047	0.020	0.011
Community & year FE	Yes	Yes	Yes	Yes	Yes
District-year FE	Yes	Yes	Yes	Yes	Yes

Note: The Roll-out sample includes 28,873 communities, and the RCT sample comprises 1,830 communities. In both samples, results are based on TWFE, yielding ITT estimates for the RCT. The dependent variable is a binary indicator that takes the value one if NVMS reported at least one incident of violent crime of a given type in a community in a given year. Columns 1 and 2 distinguish between violence committed by actors who are employed as private, non-government, or government employees and those without employment. Columns 3 to 5 distinguish between crimes that occurred on workdays, during the weekend (excluding public holidays), or on public holidays. Standard errors in parentheses are clustered at the level of sub-districts. */**/*** denote significance levels at 10/5/1% respectively.

the effects of the CCT on idleness should be less pronounced in places where the labor market is strong and young household members can also find a better occupation. At the same time, if the CCT-induced idleness is truly responsible for the increase in crime, we should expect lower crime increases in places where labor market opportunities are strong. To assess these explanations, we define a local labor market at the district level as particularly strong if the large majority of adult males is non-marginally employed, that is, if at least two-thirds of adult males (aged 25 to 55) work at least 35 hours per week. Relying on these cutoffs, we assign the upper quintile (one-fifth) of the Indonesian population to places with a relatively stronger labor market. Results in Appendix Table A.23 show only weak evidence for the labor-market link for idleness: Although the interaction between CCT access and strong labor markets is negative and of substantial size, it remains insignificant. In contrast, Table A.24 provides some evidence for crime: In the RCT sample, the baseline effect of CCT access on crime disappears in places with a strong labor market. For the national rollout, no comparable differences are observed. Results are indicative, although they should be interpreted with caution, as the use of aggregate-level indicators unavoidably splits the data along correlated, unobserved characteristics.

Further mechanisms and general equilibrium effects Appendix E discusses a set of further results, none of which point towards alternative possible mechanisms behind an increase in crime. Among other factors, results show no evidence for changes in community cohesion, which could have explained reduced community-level monitoring of the social piece, no evidence for differential migration, or changing patterns of marriage formation.

Perceptions of the idleness–poverty–crime link Finally, we assess the perceptions of the proposed idleness-poverty-crime relationship. Specifically, we examine whether a depiction of criminals as predominantly young, male, idle, and poor in police reports (BPS, 2013; POLRI, 2019) is corroborated by Indonesians. To this end, we conducted an online factorial vignette experiment in early 2023 with about 1,800 Indonesians, designed to causally link individuals' perception of guilt (i.e., whether person X committed a crime) to person X's traits (being poor, being idle). The survey experiment is described in Appendix F. We find that Indonesians are more likely to consider a young male guilty of theft if he is described as both idle and poor, but less likely to do so if he is described as poor only. Although vignettes can trigger stereotypes and contain representation bias, they are often based on a 'kernel of truth' (Bordalo et al., 2016).[18] Thus, participants may overestimate the effects of poverty and idleness on crime, though the direction likely reflects real-world patterns.

6 Conclusion

This paper demonstrates that welfare programs can inadvertently increase local violent crime. Focusing on the nationwide rollout of Indonesia's conditional cash transfer program, PKH, which covered approximately three million households by 2014, we find that the program raised the community-level incidence of violent crime by up to 30% at the extensive margin. These results hold across multiple econometric identification strategies and robustness checks addressing potential measurement concerns.

Our analysis further identifies mechanisms underlying this relationship. First, PKH induced within-household spillovers that increased idleness among non-targeted young

[18]The sociological crime literature suggests that individuals tend to be good at correctly identifying actual crime offenders, as there is often a sizeable positive correlation between people's perceived level of guiltiness of a person and whether the person has indeed committed a certain crime (Dressel and Farid, 2018; Lin et al., 2020).

male siblings aged 18–25, potentially freeing some time for violent criminal activity. Second, the CCT's education-focused conditionality led parents to reallocate attention and resources toward younger, school-aged children (the program's primary beneficiaries), which reduced oversight of older, non-targeted siblings. This shift in parental investment likely amplified the impact of idleness on criminal behavior among older siblings. Importantly, we find no direct evidence that the program increased violent crime by enhancing victimization among program beneficiaries. The CCT did not boost household asset accumulation, exacerbate local peer-group inequality, nor were crime increases linked to targeting errors.

Our study highlights that even beneficial policies like CCTs can generate unintended negative spillovers. To mitigate such effects, policymakers must address the potential role of youth idleness in driving the program's link to local violent crime. While few existing policies directly target the idleness-crime nexus, interventions can broadly be categorized into two complementary approaches: "carrot" strategies that reduce idleness through opportunity creation, and "stick" measures that contain unwanted behaviors. The carrot approach acknowledges that structural barriers (e.g., information gaps, limited networks, or low aspirations) often hinder employment prospects for young men, even with CCT financial support. To address this, policymakers could integrate beneficiary households into active labor market programs, such as workfare initiatives or vocational training, to improve labor market integration. The stick approach, in contrast, targets behavioral constraints (e.g., self-control issues) by imposing stricter constraints or penalties. For instance, CCTs could expand eligibility criteria for schooling or training to include older children and adults beyond age 15, mandate labor market participation for older household members, or reduce cash transfers if household members engage in criminal activities. Though seemingly stringent, such measures may help maintain public support for CCTs by addressing societal concerns (Gelbach and Pritchett, 2001; Baute et al., 2021). Notably, Indonesia's 2018 PKH reforms reflect both strategies: they extended conditionality requirements to include school attendance for children up to age 21, thereby addressing idleness through both expanded educational incentives and stricter oversight.

Finally, we note several limitations of our study. First, the generalizability of our findings to other welfare programs and country contexts is unclear. Second, our results focus on short- to medium-term impacts, which we expect to be transitory. As the program improved educational attainment among young children, its effect on violent crime may diminish over time when this cohort enters the labor market. Third, we lack micro-level data directly linking young adults in CCT households to increased criminal behavior, leaving our evidence circumstantial. Lastly, our key outcome variable, violent crime, cannot be directly compared to other studies. While crime rates from newspaper reports and victimization surveys offer advantages over police data (e.g., being less influenced by variations in policing intensity), we would have preferred to validate our findings using spatially disaggregated police administrative data. Unfortunately, such data remains inaccessible to researchers in Indonesia.

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A Online Appendix: Additional Figures and Tables

A.1 Tables

Table A.1: Examples of violent crimes committed by youth

No.	Bahasa	English
1	25/7/2007. wakidan di pukuli pemuda labuhan ratu, melihat wakidan di pukuli, sejumlah rekan wakidan yang masih berada di sekitar lokasi mendekati dan balas memukul pemuda labuhan ratu, namun tiba-tiba terdengar latusan pistol berkali-kali mende	7/25/2007. Wakidan was beaten by Labuhan Ratu youth, seeing Wakidan being beaten, a number of Wakidan's colleagues who were still around the location approached and hit the Labuhan Ratu youth back, but suddenly a gunshot was heard repeatedly de
2	Di Desa Negeri Lima, Kec. Leihitu, Kab. Maluku Tengah, terjadi penganiayaan yang dilakukan oleh dua pemuda Desa Negeri Lima terhadap tiga warga Desa Seith yang hendak pulang ke kampungnya dengan melewati Negeri Lima. Tidak diketahui motif penga	In Negeri Lima Village, Leihitu District, Central Maluku Regency, there was an assault carried out by two young men from Negeri Lima Village against three residents of Seith Village who were about to return to their village via Negeri Lima. The motive for the assault is unknown.
3	27-8-2011,2 pemuda bersenjata samurai melakukan perampokan di kawasan kel. kunciran, kec. pinang, kota tangerang. seorang pelaku juga memperkosa nn si pemilik rumah. 2 pelaku berhasil menggasak sejumlah perhiaan emas, dan sebuah hp milik korban	August 27, 2011, 2 young men armed with samurai weapons carried out a robbery in the Kunciran Village area, Pinang District, Tangerang City. One perpetrator also raped nn, the homeowner. The 2 perpetrators managed to steal a number of gold jewelry and a cellphone belonging to the victim.
4	25/7/2007. wakidan di pukuli pemuda labuhan ratu, melihat wakidan di pukuli, sejumlah rekan wakidan yang masih berada di sekitar lokasi mendekati dan balas memukul pemuda labuhan ratu, namun tiba-tiba terdengar latusan pistol berkali-kali mende	7/25/2007. Wakidan was beaten by Labuhan Ratu youth, seeing Wakidan being beaten, a number of Wakidan's colleagues who were still around the location approached and hit the Labuhan Ratu youth back, but suddenly a gunshot was heard repeatedly de
5	Di Desa Negeri Lima, Kec. Leihitu, Kab. Maluku Tengah, terjadi penganiayaan yang dilakukan oleh dua pemuda Desa Negeri Lima terhadap tiga warga Desa Seith yang hendak pulang ke kampungnya dengan melewati Negeri Lima. Tidak diketahui motif penga	In Negeri Lima Village, Leihitu District, Central Maluku Regency, there was an assault carried out by two young men from Negeri Lima Village against three residents of Seith Village who were about to return to their village via Negeri Lima. The motive for the assault is unknown.
6	27-8-2011,2 pemuda bersenjata samurai melakukan perampokan di kawasan kel. kunciran, kec. pinang, kota tangerang. seorang pelaku juga memperkosa nn si pemilik rumah. 2 pelaku berhasil menggasak sejumlah perhiaan emas, dan sebuah hp milik korban	August 27, 2011, 2 young men armed with samurai weapons carried out a robbery in the Kunciran Village area, Pinang District, Tangerang City. One perpetrator also raped nn, the homeowner. The 2 perpetrators managed to steal a number of gold jewelry and a cellphone belonging to the victim.
7	Di Desa Tarus, Kec. Kupang Tengah, Kab. Kupang, NTT, Terjadi perkelahian antara pemuda RT 10 dengan pemuda RT 13. Perkelahian terjadi karena dendam antar dua pemuda, dimana pada minggu sebelumnya di acara off road kedua pemuda terlibat perkela	In Tarus Village, Kupang Tengah District, Kupang Regency, NTT, there was a fight between young men from RT 10 and young men from RT 13. The fight occurred because of a grudge between the two young men, where the previous week at an off-road event the two young men were involved in a fight
8	2.10.06 saat hasri seorang oknum polusu hendak menjemput istrinya ia melihat 7 pemuda menodongkan pisau meminta uang kepada seorang ibu hasri pun melerai ketiganya namun mereka malah menganiaya hasri dan memukulnya dengan batu kejadian berlangs	2.10.06 when Hasri, a police officer, was about to pick up his wife, he saw 7 young men pointing a knife and asking for money from a woman. Hasri tried to break up the three of them, but instead they abused Hasri and hit him with stones. The incident took place
9	14 januari 2008. di babara. 4 pemuda menganiaya 2 pemuda karena dendam, 1 tewas akibat luka bacok dan 1 cidera.	January 14, 2008. in Babara. 4 young men abused 2 young men out of revenge, 1 died from a slash wound and 1 was injured.
10	Di Hutan Kuyun, Desa Kuyun, Kec. Celala, Kab. Aceh Tengah, terjadi pencabulan yang dilakukan oleh 6 orang pemuda asal Kuyun terhadap 3 orang gadis ABG warga Takengon. Saat itu salah satu pelaku yakni W memaksa R rekan ketiga korban untuk berhub	In Kuyun Forest, Kuyun Village, Celala District, Central Aceh Regency, there was an indecent act carried out by 6 young men from Kuyun against 3 teenage girls from Takengon. At that time, one of the perpetrators, namely W, forced R, the friend of the three victims, to have sex

Note: NVMS reported incidents come with a description limited to 244 characters. Violent crimes related to youth are selected by searching for keywords related to youth (Bahasa: *pemuda* | *anak* | *muda*; engl.: *youth* | *child* | *young person*).

Table A.2: *NVMS data:* Number of incidents by the type of violent crime

	RCT Sample Total cases	*Roll-out sample* Total cases
All	2,487	31,189
Group Clash	35	762
Fight	173	1,888
Lynching	214	3,540
Vandal.	162	2,338
Assault	2,254	23,848
Sweeping	1	12
Kidnap.	7	237
Robbery	361	7,289
Others	7	345

Note: Each incident included in the "All" total can be classified under up to two categories listed above. The RCT sample comprises 1,830 communities, while the Roll-out sample includes 28,873 communities. Data spans 2005–2010 (RCT sample) and 2005–2014 (Roll-out sample), and is restricted to provinces covered by the NVMS. Violent crime is defined according to the NVMS framework: "Violent crime comprises acts of violence that occur without any prior dispute between parties. The motivation behind a criminal act can be monetary, for example, robbery or abduction; or personal pleasure, for example, rape or serial killings. In contrast, violence in the context of conflict occurs due to pre-existing disputes between those involved such as disputes over land, election, religion or other such matters. As such, in the NVMS system, an act of killing can be coded as 'Conflict' if there is a dispute behind it, e.g., in a killing of a certain group figure by other groups, or can be coded as 'Crime' if there is no pre-existing dispute between parties, for example, serial killings.", (NVMS, 2014)). The "Others" category includes demonstrations, blockades, riots and terror attacks.

Table A.3: Sample selection due to NVMS coverage

Samples:	National roll-out (until 2014)			RCT sample			
Crime database coverage:	None (1)	NVMS (2)	Diff. (2–1)	None (3)	NVMS (4)	Diff. (4–3)	Diff. (4–2)
Socio-economic variables in 2008:							
Population [in 1,000]	4.159	2.977	−1.182***	6.671	3.215	−3.456***	0.278***
	(0.027)	(0.026)	(0.038)	(0.115)	(0.052)	(0.116)	(0.081)
Urban	0.136	0.102	−0.034***	0.076	0.068	−0.008	−0.038***
	(0.002)	(0.001)	(0.002)	(0.009)	(0.004)	(0.009)	(0.005)
% Househ. w. electricity	0.832	0.750	−0.082***	0.939	0.851	−0.088***	0.115***
	(0.001)	(0.001)	(0.002)	(0.004)	(0.004)	(0.009)	(0.005)
Community market available	0.197	0.178	−0.019***	0.112	0.174	0.062***	−0.003
	(0.002)	(0.002)	(0.003)	(0.010)	(0.006)	(0.014)	(0.007)
Community hospital available	0.023	0.020	−0.003**	0.019	0.015	−0.004	−0.005**
	(0.001)	(0.001)	(0.001)	(0.004)	(0.002)	(0.004)	(0.002)
No. communities (max.)	26,657	28,654		872	3,317		

Note: The Roll-out sample includes communities where PKH was introduced by 2014. It is divided into those within the 16 NVMS-covered provinces (analytical sample), and the remaining 18 provinces without NVMS coverage. Similarly, the RCT sample is divided between communities in the 16 NVMS provinces and the remaining provinces. For differences in columns 4–2, the Roll-out sample excludes communities overlapping with the RCT sample. Statistical significance is assessed via t-tests. All variables are sourced from the 2008 PODES dataset. */**/*** denote significance at 10/5/1% level.

Table A.4: Rollout determinants: Explaining year of PKH rollout

Sample	Roll-out (2007–2014) (1)	(2)
Village characteristics		
Population (ln)	–0.448***	–0.042
	(0.049)	(0.034)
Rural	–0.660***	0.094
	(0.133)	(0.102)
Electricity access (hh %)	–0.892***	0.077
	(0.135)	(0.092)
Poverty cards (ln)	–0.070**	–0.024
	(0.029)	(0.019)
Roll-out criteria: Health facilities		
Hospital	0.120	–0.047
	(0.126)	(0.070)
Sub-hospital	0.503***	–0.011
	(0.044)	(0.019)
Puskesmas (health station)	0.523***	–0.006
	(0.049)	(0.022)
Roll-out criteria: Education facilities		
Kindergarten	–0.460***	–0.057
	(0.081)	(0.052)
Primary school	0.022	0.040
	(0.090)	(0.070)
High-school	–0.054	0.011
	(0.081)	(0.269)
District FE	No	Yes
Observations	26,597	26,597
Adj. R^2	0.078	0.594
F-statistic	28.60	1.35

Note: The dependent variable is the year of rollout; the cross-sectional sample includes all communities in our national Roll-out sample with full data in the village census PODES 2008. Robust standard errors are clustered at the sub-district level and reported in parentheses. */**/*** denote significance levels at 10/5/1% respectively.

Table A.5: *Robustness:* Assessing the presence of spillover effects

Sample	Roll-out					
Estimator	TWFE			Spillover-robust double difference estimator (Clarke, 2017)		
	(1)	(2)	(3)	(4)	(5)	(6)
PKH treatment	0.006*	0.007*	0.008*	0.010***	0.007*	0.012**
	(0.003)	(0.004)	(0.004)	(0.003)	(0.004)	(0.005)
Close to community with PKH treatment						
$0-10km$	0.000			0.004	0.001	0.006
	(0.000)			(0.003)	(0.004)	(0.005)
$0-10km$ × PKH	0.000					
	(0.000)					
$10-20km$		0.000			–0.003	0.001
		(0.000)			(0.003)	(0.004)
$10-20km$ × PKH		–0.000				
		(0.000)				
$20-30km$			0.000			0.007
			(0.000)			(0.004)
$20-30km$ × PKH			–0.000			
			(0.000)			
Community FE, year FE	Yes	Yes	Yes	Yes	Yes	Yes
District-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	288,730	288,730	288,730	288,340	288,340	288,340

Note: The Roll-out sample includes 28,873 communities that ever received PKH during the period 2007–2014 and is restricted to communities with full NVMS coverage and population information. Additional controls indicate the number of communities with PKH access within k kilometers of the community (centroid-based). In columns 4–6 neighboring indicators are set to zero in case the community was treated itself. Robust standard errors clustered at the sub-district level are reported in parentheses. */**/*** denote significance levels at 10/5/1% respectively.

Table A.6: *Robustness:* Results from randomization inference

Sample	Roll-out		RCT
Randomization	Across year	Within years	
	(1)	(2)	(3)
Mean PKH effect	0.000	0.000	
Mean standard error	(0.002)	(0.002)	
Median standard error	(0.002)	(0.002)	
Share of significant estimates ($p \leq 0.1$)	10.2%	9.5%	
PKH assignment			0.027***
			(0.010)
Rand. Inference p-value			0.00
Community FE, year FE	Yes	Yes	Yes
District-year FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
No. repetitions	10,000	10,000	1,000
Mean Adj. R^2	0.221	0.221	

Note: The Roll-out sample includes 28,873 communities, while the RCT sample comprises 1,830 communities. The dependent variable is a binary indicator that takes the value one if NVMS reported at least one violent crime incident in a community in a given year.

The table shows statistics on TWFE estimates after randomly distributed PKH treatments for 10,000 repetitions. Randomization across years assigns PKH to start randomly across all years after 2006, whereas within-year randomization assigns the yearly PKH beginning randomly within the same year. For randomization inference p-values reported in column 3, the Stata package *ritest* (Heß, 2017) was used with $N = 1,000$ resampling iterations. Standard errors in parentheses are clustered at the level of sub-districts. */**/*** denote significance levels at 10/5/1% respectively.

Table A.7: *Robustness:* PKH effects after adjusting standard errors

Sample	RCT			Roll-out		
Standard Errors	Basic	District	Conley	Basic	District	Conley
	(1)	(2)	(3)	(4)	(5)	(6)
PKH treatment	0.032***	0.032***	0.032***	0.007**	0.007**	0.007***
	(0.012)	(0.011)	(0.010)	(0.003)	(0.003)	(0.002)
Community FE, year FE	Yes	Yes	Yes	Yes	Yes	Yes
District-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	10,980	10,980	10,956	288,730	288,730	287,940

Note: The Roll-out sample includes 28,873 communities, while the RCT sample comprises 1,830 communities. For Conley SEs, several communities are excluded due to missing GPS coordinates. The dependent variable is a binary indicator that takes the value one if NVMS reported at least one incident of violent crime in a community in a given year. Robust standard errors reported in parentheses are clustered at the sub-district level in columns 1 and 4, clustered at the district level in columns 2 and 5 and (Conley-)clustered within a 50km threshold around respective community centroids in columns 3 and 6. Alternative thresholds at 100km or 200km do not alter the estimated SEs significantly. Results are available from the authors upon request. */**/*** denote significance levels at 10/5/1% respectively.

Table A.8: *Robustness:* PKH effects by sample construction

Sample	RCT		Roll-out	
	Extended	Sub-district	Ever-teated	All
	2005-2010	2005-2010	2000-2014	2005-2014
	(1)	(2)	(3)	(4)
PKH	0.018**	0.023**	0.007**	0.004**
	(0.010)	(0.012)	(0.003)	(0.002)
Community FE, year FE	Yes	Yes	Yes	Yes
District-year FE	Yes	Yes	Yes	Yes
Observations	19,950	1,500	206,985	476,800

Note: The sample in column 1 is restricted to 3,323 communities that were included in the extended RCT of the PKH program. Column 2 displays sub-district level estimates for the RCT sample consisting of 250 sub-districts with data coverage. Column 3 includes only those 13,779 communities that ever received PKH in 2007–2014 and had NVMS coverage already in 2000. Column 4 covers all Indonesian communities with NVMS coverage. The dependent variable is a binary indicator that takes the value one if NVMS reported at least one incident of violent crime within the community in a given year in columns 1, 3, and 4, and it is the share of communities within the sub-district that experience at least one conflict in column 2. PKH treatment indicates whether any household within a given community received transfers from the CCT program in a given year. Robust standard errors are clustered at the sub-district level and reported in parentheses. */**/*** denote significance levels at 10/5/1% respectively.

Table A.9: *Robustness:* PKH effect on the intensive margin of violent crime

Sample	*RCT sample*			*Roll-out sample*		
	All	Rural	City	All	Rural	City
PANEL A	Dependent: *asinh* Number of violent crime events					
	TWFE (1)	TWFE (2)	TWFE (3)	TWFE (4)	TWFE (5)	TWFE (6)
PKH	0.028**	0.032***	−0.104	0.009***	0.007**	0.046
	(0.013)	(0.012)	(0.170)	(0.003)	(0.003)	(0.046)
Mean dependent	0.128	0.074	1.249	0.074	0.053	0.367
Observations	10,980	10,476	504	288,730	269,730	19,000
PANEL B	Dependent: Number of violent crime events					
	PPML (1)	PPML (2)	PPML (3)	PPML (4)	PPML (5)	PPML (6)
PKH	0.097	0.492***	−0.194	0.076	0.119**	−0.009
	(0.148)	(0.163)	(0.224)	(0.065)	(0.049)	(0.161)
Mean dependent	0.690	0.306	3.382	0.332	0.224	0.981
Observations	3,605	3,155	450	93,863	80,462	13,401
Community FE, year FE	Yes	Yes	Yes	Yes	Yes	Yes
District-year FE	Yes	Yes	Yes	Yes	Yes	Yes

Note: The dependent variable captures the number of NVMS-reported violent crime incidents within a community in a given year. It is transformed by the inverse hyperbolic sine and estimated by simple two-way fixed effects in panel A. It is estimated as a count variable by PPML in panel B. The displayed mean of dependent variable refers to the full sample. Robust standard errors are clustered at the sub-district level and reported in parentheses. */**/*** denote significance levels at 10/5/1% respectively.

Table A.10: *Robustness:* PKH effects by access intensity

PKH recipient households per village	< 3 or ≥ 3	< 5 or ≥ 5	< 10 or ≥ 10	< 30 or ≥ 30
	TWFE	TWFE	TWFE	TWFE
	(1)	(2)	(3)	(4)
PKH low intensity	0.004	0.004	0.005	0.007**
	(0.004)	(0.004)	(0.003)	(0.003)
PKH high intensity	0.007***	0.007***	0.008***	0.007**
	(0.003)	(0.003)	(0.003)	(0.003)
Mean (control)	0.060	0.060	0.060	0.060
Sub-district FE	Yes	Yes	Yes	Yes
District-year FE	Yes	Yes	Yes	Yes
Observations	288,730	288,730	288,730	288,730

Note: The dependent variable is a binary indicator that takes the value one if NVMS reported at least one violent crime incident in a community in a given year. All estimates are restricted to sub-districts that received access to the program until 2014. The treatment variable turns to one if the indicated number of households within the sub-districts received PKH, where *low* intensity refers to the lower, and *high* intensity to the upper indicated threshold. Robust standard errors in parentheses are clustered at the level of sub-districts. */**/*** denote significance levels at 10/5/1% respectively.

Table A.11: PKH effects on community-level socio-economic development

Variable	HH share	Bank access		Availability of				Access to				ln Popu-
	electricity	Bank 1	Bank 2	Bar	Soccer	Clinic	Doctor	Phone	Internet	Market 1	Market 2	lation
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(8)	(9)	(10)	(11)
Panel A: RCT sample												
PKH	−0.025*	0.030	0.015	−0.016	−0.060	−0.012	−0.014	0.000	0.016	0.023	−0.006	0.006
	(0.013)	(0.061)	(0.034)	(0.011)	(0.037)	(0.014)	(0.038)	(0.010)	(0.010)	(0.020)	(0.017)	(0.016)
Observations	3,650	1,826	1,826	1,826	1,826	3,650	3,650	1,824	1,824	3,650	3,650	3,650
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: Roll-out sample												
PKH	0.007	−0.003	−0.013	0.002	0.005	−0.009	0.014	0.003	0.019**	−0.023	−0.031**	−0.005
	(0.005)	(0.009)	(0.008)	(0.002)	(0.010)	(0.006)	(0.022)	(0.005)	(0.008)	(0.016)	(0.012)	(0.005)
Observations	85,782	57,610	57,610	57,610	57,610	85,782	85,782	56,338	56,338	85,782	85,782	85,782
Community FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: 'Bank 1' refers to the number of commercial banks in a community, while 'Bank 2' refers to the number of public credit banks. The remaining dependent variables are binary and refer to the availability of entertainment places (bars/karaoke), soccer fields, a health clinic, a medical doctor, a landline phone connection, internet cafes, markets with (market 1) and without (market 2) a permanent building in a given community. All variables are based on PODES. The Roll-out sample includes up to 28,873 communities for the years 2007, 2010 and 2013. The RCT sample comprises up to 1,830 communities and includes observations for the years 2007 and 2010 only. It does not include a community fixed effect because RCT assignment begins only in 2007. For both samples, some variables are available for 2010 and 2013 only. Results are estimated using equation 1. PKH treatment captures actual treatment by indicating whether any household in a given community received PKH in a given year. Robust standard errors are clustered at the sub-district level and reported in parentheses. */**/*** denote significance levels at 10/5/1% respectively.

Table A.12: *Robustness:* PKH effects on conflict

Dependent:	Violence by type				
	All conflict excl. crime	Domestic violence	Popular justice	Law enforcement	Other conflict
	(1)	(2)	(3)	(4)	(5)
	Panel A: RCT sample (2005–2010) ITT				
PKH assignment	0.005	–0.001	0.002	–0.0001	0.005
	(0.009)	(0.007)	(0.005)	(0.004)	(0.007)
Mean (control)	0.070	0.032	0.026	0.011	0.039
Observations	10,980	10,980	10,980	10,980	10,980
	Panel B: Roll-out sample (2005–2014) TWFE				
PKH	–0.002	–0.002	0.002	–0.002*	0.002
	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)
Mean (control)	0.048	0.015	0.020	0.007	0.032
Observations	288,730	288,730	288,730	288,730	288,730
Community FE, year FE	Yes	Yes	Yes	Yes	Yes
District-year FE	Yes	Yes	Yes	Yes	Yes

Note: The Roll-out sample includes 28,873 communities, while the RCT sample comprises 1,830 communities. The dependent variable is a binary indicator that takes the value one if NVMS reported at least one incident of conflict (i.e. cases that are not categorized as "violent crime") in a community in a given year. Robust standard errors are clustered at the sub-district level and reported in parentheses. */**/*** denote significance levels at 10/5/1% respectively.

Table A.13: *Alternative measure:* CCT access and the probability of being a victim of crime (2007–2011): Varying samples and crime definitions

PKH roll-out:	Village share in sub-district			At least 100 households in sub-district		
Household sample:	All	Poor	Non-poor	All	Poor	Non-poor
	TWFE	TWFE	TWFE	TWFE	TWFE	TWFE
	(1)	(2)	(3)	(4)	(5)	(6)
PANEL A:	*Probability of being victim of theft and robbery* Based on sub-districts included in the *RCT*					
PKH roll-out	0.007	0.045**	0.004	0.004	0.040***	0.001
	(0.013)	(0.018)	(0.014)	(0.012)	(0.016)	(0.013)
Mean	0.054	0.049	0.055	0.054	0.049	0.055
Observations	65,076	6,368	58,699	65,076	6,368	58,699
PANEL B:	*Probability of being victim of any type of crime* Based on sub-districts with a *Roll-out* until 2014					
PKH roll-out	0.009***	0.030***	0.008**	0.008**	0.025***	0.006*
	(0.004)	(0.008)	(0.004)	(0.003)	(0.007)	(0.004)
Mean (control)	0.046	0.037	0.047	0.045	0.037	0.047
Observations	1,095,130	95,340	999,630	1,095,130	95,321	999,630
Sub-district FE	Yes	Yes	Yes	Yes	Yes	Yes
District-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: The dependent variable is a binary indicator that takes the value one if a household reported being a victim of at least one crime in a given year. It includes thefts and robberies in panel A and thefts, robberies, homicides, rapes and other types of crime in panel B. The first treatment variable (columns 1–3) measures the share of CCT recipient communities within a sub-district; the second treatment variable (columns 4–6) turns to one if at least 100 households within a sub-district received PKH. Controls include indicators for the completed education, age and marital status of the household head, the quintiles of household size, and urban status. Standard errors in parentheses are clustered at the level of sub-districts. */**/*** denote significance levels at 10/5/1% respectively.

Table A.14: *Alternative measure:* CCT access intensity and the probability of being a victim of crime (2007–2011)

Intensity threshold	< 50 or ≥ 50	< 100 or ≥ 100	< 200 or ≥ 200	< 300 or ≥ 300
	TWFE	TWFE	TWFE	TWFE
	(1)	(2)	(3)	(4)
PKH low intensity	0.001	0.001	0.002	0.005
	(0.004)	(0.004)	(0.003)	(0.003)
PKH high intensity	0.009***	0.009***	0.009***	0.008**
	(0.003)	(0.003)	(0.003)	(0.003)
Mean (control)	0.043	0.043	0.043	0.043
Sub-district FE	Yes	Yes	Yes	Yes
District-year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	1,095,130	1,095,130	1,095,130	1,095,130

Note: All estimates are restricted to households living in sub-districts that received access to the program until 2014. The dependent variable is a binary indicator that takes the value one if a household reported being a victim of at least one crime (theft or robbery) in a given year. The treatment variable turns to one if the indicated number of households within the sub-districts received PKH, where *low* intensity refers to the lower, and *high* intensity to the upper indicated threshold. Controls include indicators for the completed education, age and marital status of the household head, the quintiles of household size, and urban status. Standard errors in parentheses are clustered at the level of sub-districts. */**/*** denote significance levels at 10/5/1% respectively.

Table A.15: PKH effects on the community-level presence of police stations

Dependent:	Village Police Station (0/1)			
Sample:	RCT (LATE)		Roll-out	
	(1)	(2)	(3)	(4)
PKH treatment	0.015	0.014	0.003	-0.001
	(0.015)	(0.011)	(0.003)	(0.003)
Community FE, year FE	No/Yes	No/Yes	Yes	Yes
District-year FE		Yes		Yes
Observations	3,650	3,650	85,782	85,782

Note: The Roll-out sample includes 28,873 communities, while the RCT sample comprises 1,830 communities. Columns 1 and 2 only include observations for the years 2007 and 2010 and do not include community fixed effect because RCT assignment begins only in 2007. Columns 3 and 4 only include 2007, 2010, and 2013 given data availability reasons. PKH treatment captures actual treatment by indicating whether any household within a given community received PKH in a given year. Robust standard errors are clustered at the sub-district level and reported in parentheses. */**/*** denote significance levels at 10/5/1% respectively.

Table A.16: *RCT:* PKH effects on peer-group inequality

	ITT			LATE		
Inequality index:	Gini	Theil	CV	Gini	Theil	CV
	(1)	(2)	(3)	(4)	(5)	(6)
PKH Assignment	-0.003	-0.002	-0.008			
	(0.003)	(0.002)	(0.007)			
PKH Treatment				-0.004	-0.003	-0.011
				(0.004)	(0.003)	(0.010)
Observations	2,212	2,212	2,212	2,212	2,212	2,212
District FE	Yes	Yes	Yes	Yes	Yes	Yes

Note: Inequality measures are calculated based on household-level expenditures per capita, taking into account all households included in the RCT sample. In the analysis, only communities with at least 5 household observations per survey round were used. 'CV' refers to 'Coefficient of Variation'. Additional controls: Baseline inequality measure. LATE estimates are obtained from 2SLS. Standard errors in parentheses are clustered at the sub-district level. */**/*** denote significance levels at 10/5/1% respectively.

Table A.17: PKH effects on crime: The role of community-level targeting inequality

Sample	RCT sample		Roll-out sample	
	(1)	(2)	(3)	(4)
PKH	0.079*	−0.059	0.015***	−0.010
	(0.040)	(0.063)	(0.006)	(0.010)
PKH × Undercoverage	−0.222		−0.035*	
	(0.159)		(0.019)	
PKH × Leakage		0.132		0.025
		(0.098)		(0.016)
Community FE, year FE	Yes	Yes	Yes	Yes
District-year FE	Yes	Yes	Yes	Yes
Observations	10,980	10,980	267,680	267,680

Note: The Roll-out sample includes 28,873 communities, while the RCT sample comprises 1,830 communities. The dependent variable is a binary indicator that takes the value one if NVMS reported at least one incident of violent crime in a community in a given year. Undercoverage/leakage measure the share of PKH non-receiving eligible/receiving ineligible households in surveyed households at the district level (based on SUSENAS 2014). Standard errors in parentheses are clustered at the level of sub-districts. */**/*** denote significance levels at 10/5/1% respectively.

Table A.18: *Mechanisms:* The short-run impact of the CCT program on community involvement and drug consumption, (*LATE estimates*)

	Panel A: Community involvement			
	Engage 1	Engage 2	Engage 3	Engage 4
PKH beneficiary	0.012	0.012	−0.057	−0.080
	(0.027)	(0.027)	(0.119)	(0.100)
	Panel B: Expenditures on alcohol and drugs			
	ln Alcohol Exp.	*ln* Drug Exp.	*ln* Alc.& Drug Exp.	
PKH beneficiary	−0.384	−0.054	−0.019	
	(0.239)	(0.203)	(0.202)	

Note: Results are reported for 12,929 households, based on 2SLS estimates (LATE) from the pilot's impact evaluation, using the baseline and endline surveys. All regressions use district fixed effects and a set of controls. 'Engage 1' captures whether a household is a member of any type of community organization. 'Engage 2' refers to the number of community organizations a household has joined. 'Engage 3' refers to the number of household members that have joined community organizations. 'Engage 4' captures the number of times any household member has joined a community organization meeting over the three months preceding the survey. Robust standard errors in parentheses are clustered at the sub-district level. */**/*** denote significance levels at 10/5/1% respectively.

Table A.19: *Mechanisms:* Dependent variable means for work, schooling and idleness by cohort

	Men			Women		
Age group:	7–15 (1)	18–25 (2)	26–35 (3)	7–15 (4)	18–25 (5)	26–35 (6)
Working	0.094	0.659	0.925	0.056	0.347	0.438
	(0.291)	(0.474)	(0.263)	(0.229)	(0.476)	(0.450)
Household chores	0.089	0.108	0.148	0.224	0.658	0.938
	(0.285)	(0.310)	(0.355)	(0.417)	(0.475)	(0.241)
Attending school	0.821	0.093	0.001	0.835	0.099	0.001
	(0.384)	(0.292)	(0.041)	(0.371)	(0.300)	(0.033)
Staying idle	0.096	0.219	0.059	0.063	0.120	0.013
	(0.295)	(0.413)	(0.235)	(0.247)	(0.325)	(0.115)
Observations	82,730	41,357	38,087	75,546	33,493	59,525

Note: The dependent variables refer to the last week before the survey and indicate whether an individual reported having engaged in any (i) work activities outside the household, (ii) household chores, (iii) school attendance, or (iv) stayed idle (not engaged in work, household chores, or school). Standard deviations in parentheses.

Table A.20: *Mechanisms:* Short- and middle-run effects of PKH on idleness by cohort

Dependent:	Staying idle (by age group)			
Age group:	18-25	16-17	18-20	21-25
	(1)	(2)	(3)	(4)
PANEL A	*RCT sample: LATE*			
PKH beneficiary	0.086*	0.103*	0.113*	0.054
	(0.050)	(0.059)	(0.066)	(0.059)
Mean dependent	0.169	0.281	0.344	0.220
Observations	3,583	1,629	1,586	2,015
District FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
PANEL B	*SUSENAS sample*			
PKH village share	0.028*	–0.028	0.041*	0.023
	(0.015)	(0.023)	(0.023)	(0.018)
Mean dependent	0.219	0.192	0.245	0.193
Observations	41,357	16,433	20,304	20,048
Sub-district FE, year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Note: The dependent variables refer to the last week before the survey and indicate whether an individual was reported having stayed idle (not engaged in work, household chores, or school). Panel A reports LATE estimates obtained from 2SLS based on the RCT evaluation sample. Panel B reports pooled cross-sections of SUSENAS national household survey data (2004 to 2011), restricted to sub-districts that received PKH by 2014, with the treatment variable capturing the share of PKH recipient communities within a sub-district and year. Controls include whether a household lives in an urban area, quintiles of household size, age, marital status, and indicators of the educational degree of the household head. Standard errors in parentheses are clustered at the sub-district level. */**/*** denote significance levels at 10/5/1% respectively.

Table A.21: *Mechanisms:* Impact of PKH on index of parental attention: OLS estimates on matched sample

Child's age in years:	Aged $<$ 18	Aged $<$ 7	Aged 7–15	Aged 16–17
	(1)	(2)	(3)	(4)
PKH	0.440***	0.029	0.754***	-0.222***
	(0.015)	(0.025)	(0.019)	(0.045)
Observations	92,504	28,342	49,205	9,585
Mean dependent	4.09	3.67	4.36	4.02
Province FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Notes: The sample is derived from the Parenting Module of SUSENAS 2018 and includes only individuals below the age of 18. The dependent variable is a composite index, summing-up ten binary parenting indicators. The models are run by OLS on a matched sample. The propensity score matching relies on the 5 nearest neighbours, based on child's age and gender, household size, household wealth quartile, parental education, and rural residence status as covariates. Robust standard errors are in parentheses. */**/*** denote significance levels at 10/5/1 percent respectively.

Table A.22: *Mechanisms:* Impact of PKH on index of parental attention: OLS estimates with interactions in matched sample

Child's age in years:	Aged 7–15	Aged 16–17	
	(1)	(2)	(3)
PKH	0.311**	-0.174	-0.280***
	(0.164)	(0.130)	(0.046)
Attending school	0.730***	1.034***	
	(0.050)	(0.076)	
PKH× Attending school	0.315***	-0.122	
	(0.100)	(0.136)	
Staying idle			-0.901***
			(0.106)
PKH × Staying idle			-0.135
			(0.204)
Mean dependent	2.58	2.50	2.50
Obs.	49,134	9,585	9,585
Province FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes

Notes: The dependent variable is a composite index, summing up ten binary parenting indicators. PKH is a binary indicator for PKH recipient households, 'Attending school' is a binary indicator capturing whether the child attends school, and 'Staying idle' is a binary indicator referring to when a child is neither attending school nor working. The sample is derived from the Parenting Module of SUSENAS 2018 and includes only individuals below the age of 18. Treated and control individuals are selected using a propensity score matching (PSM). The PSM relies on nearest neighbour matching (5 nearest neighbours), based on child's age, gender, and marital status, quintiles of household size, # children below the age of 12 in the household, # children between age 13 and 18 in the household, household wealth deciles, education of the household head, and rural residence status as covariates. Robust standard errors are in parentheses. */**/*** denote significance levels at 10/5/1 percent respectively.

Table A.23: *Mechanisms:* Country-wide effects of the CCT program on youth male idleness, depending on local labor markets

PKH roll-out:	Village share in sub-district (1)	At least 100 hhs in sub-district (2)
PKH roll-out	0.036**	0.028**
	(0.016)	(0.015)
PKH roll-out × Strong labor market	−0.041	−0.048
	(0.036)	(0.033)
Observations	41,357	41,357
Sub-district FE, year FE	Yes	Yes
Controls	Yes	Yes

Note: The dependent variables refer to the last week before the survey and indicate whether an individual reported having stayed idle (not engaged in work, household chores, or school). The treatment variable measures the share of PKH recipient communities within a sub-district and year, or indicates that at least 100 households received PKH in a year. Local (sub-district-level) labor markets are divided into strong and weaker based on the threshold of roughly at least two-thirds of adult (aged 26 to 55) males working at least 35 hours per week, which identifies the upper quintile of local labor markets. Results are based on pooled cross-sections of SUSENAS national household survey data (2004 to 2011), restricted to sub-districts that received the program by 2014. Only youth living in PKH eligible households are included. Controls include the household head's age, education, marital status, household size in quintiles, and urban status. Standard errors in parentheses are clustered at the sub-district level. */**/*** denote significance levels at 10/5/1% respectively.

Table A.24: *Mechanisms:* Effects of the CCT program on crime, depending on local labor markets

	RCT sample (1)	*Roll-out sample* (2)
PKH treatment	0.029***	0.006**
	(0.010)	(0.003)
PKH treatment × Strong labor market	−0.031*	0.003
	(0.018)	(0.004)
Mean dependent (control)	0.096	0.061
Share of villages with strong labor market	0.137	0.162
Observations	10,974	278,360
Community FE, district-year FE	Yes	Yes

Note: The dependent variable is a binary indicator that takes the value one if NVMS reported at least one violent crime incident in a community in a given year. Models are estimated by TWFE estimators. Local (sub-district-level) labor markets are divided into strong and weaker based on the threshold of at least two-thirds of adult (aged 26 to 55) males working at least 35 hours per week, which identifies the upper quintile of local labor markets. Robust standard errors are clustered at the sub-district level and reported in parentheses. */**/*** denote significance levels at 10/5/1% respectively.

A.2 Figures

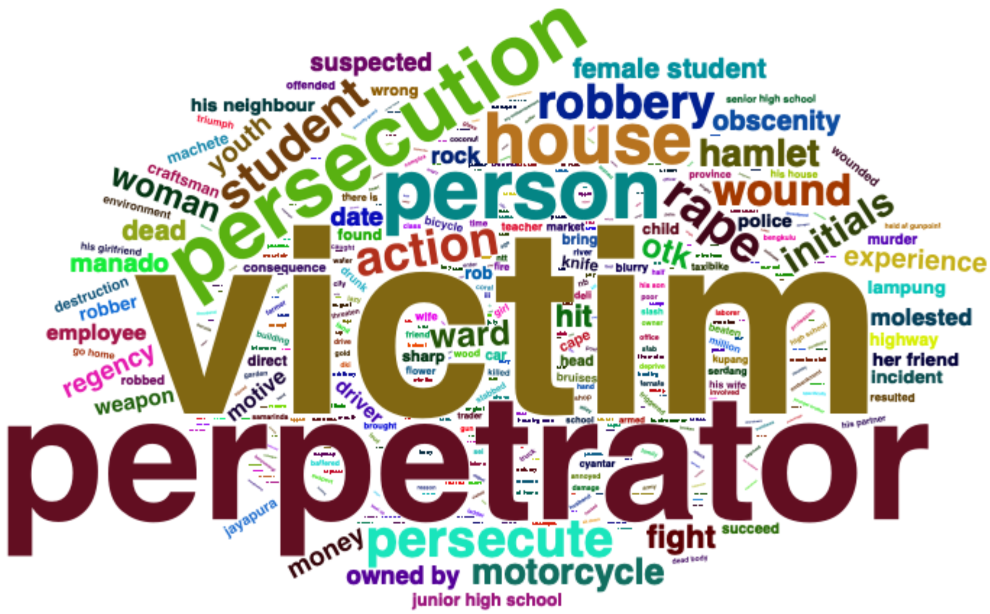
Figure A.1: Overview on the national rollout of PKH

Note: PKH access on the sub-district level. Rollout refers to the first year PKH is operating in a given community. Dashed areas indicate areas in which PKH had not been rolled out by 2014. Source: Own computation based on data from MoSA.

Figure A.2: NVMS coverage

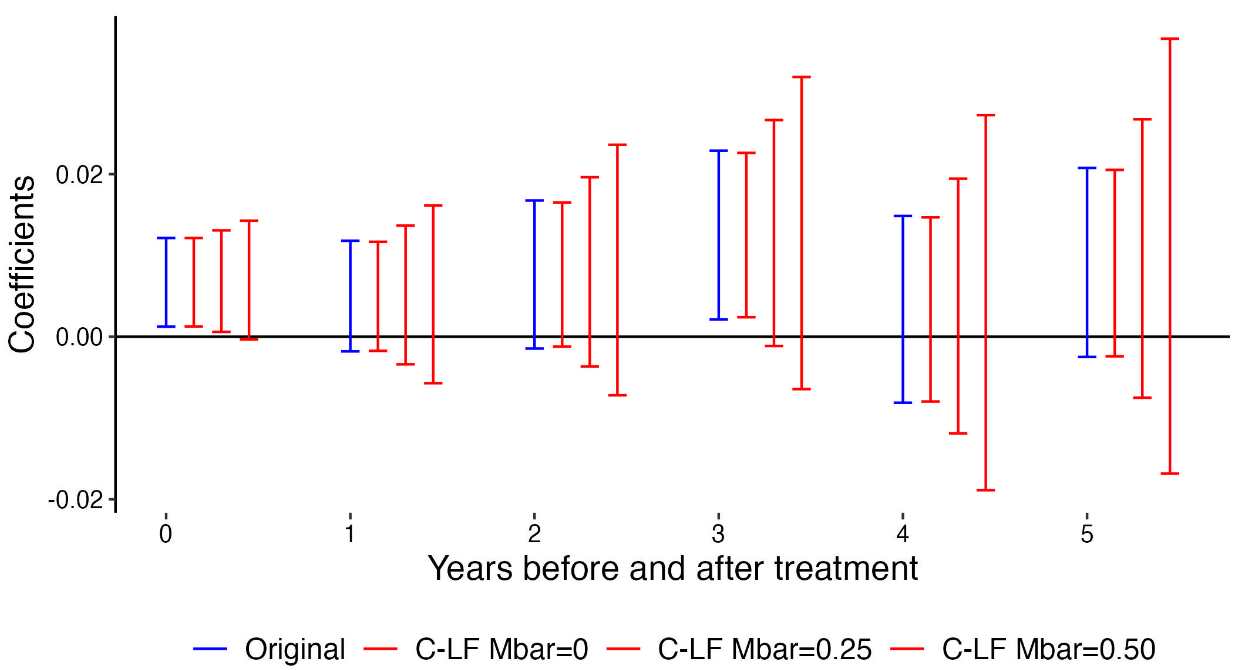
Note: Provinces shaded in grey are covered by the NVMS (16 in total), dashed provinces have only partial coverage, if at all. Source: Own computation based on NVMS (2014).

Figure A.3: Word Cloud: Newspaper reports on crime



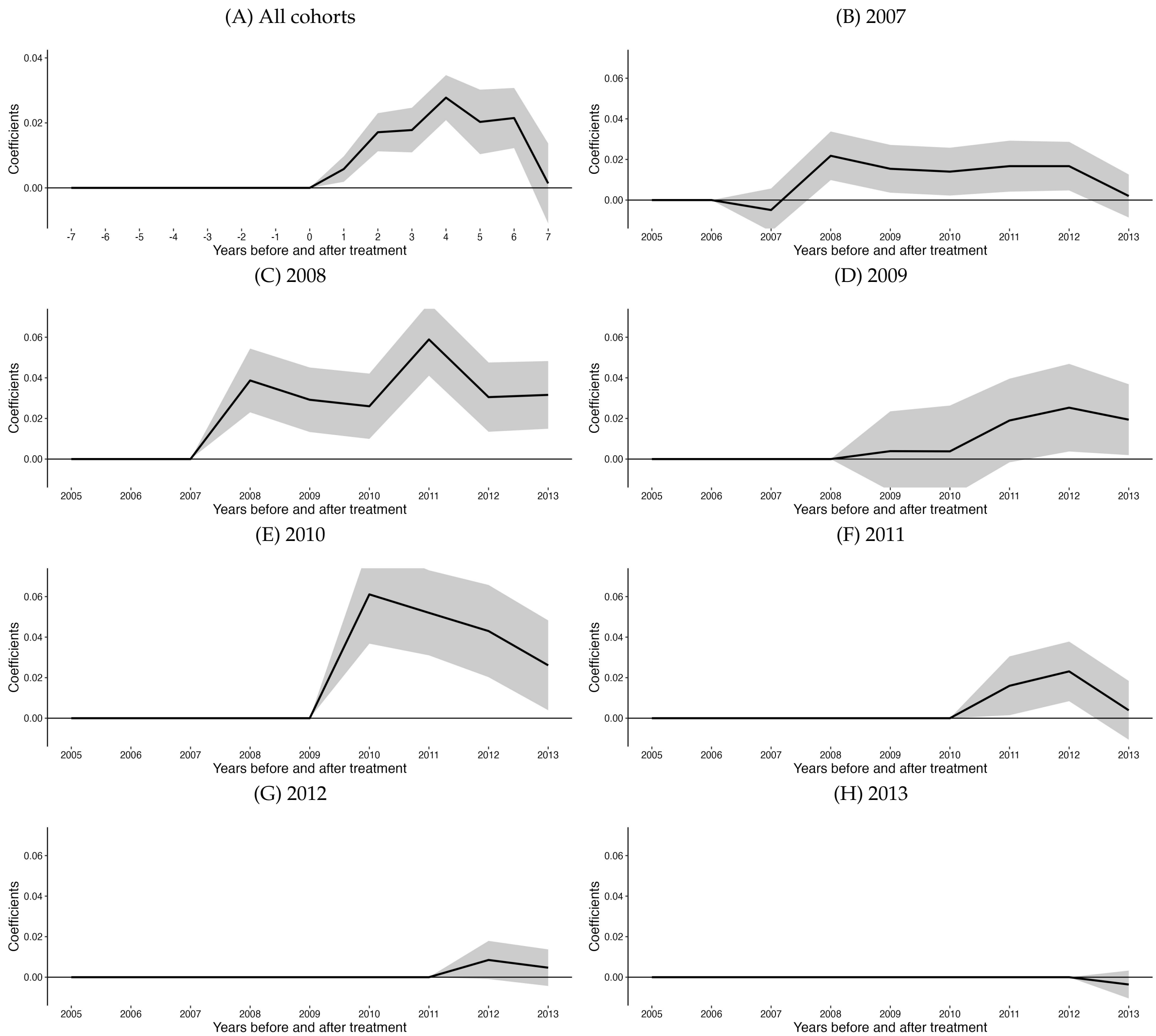
Note: The figure shows a Word Cloud for the crime incidents reported in the National Violence Monitoring System (NVMS). NVMS provides the text of the corresponding newspaper articles in *Bahasa* (limited to 244 characters). After excluding stopwords, the plot is based on term frequencies and then translated into English.

Figure A.4: Parallel trend checks: Roll-out sample



Note: Based on Rambachan and Roth (2023).

Figure A.5: PKH effects by cohort in the nation-wide roll out



Note: Results are based on the Roll-out sample restricted to the years 2005 to 2013, which includes 21,884 communities who received PKH by 2013 and 6,989 control communities who received PKH only in 2014. The dependent variable is a binary indicator that takes the value one if NVMS reported at least one incident of violent crime in a community in a given year. Estimates are based on a generalized synthetic control method with mean trajectory balancing in pre-treatment years (Hazlett and Xu, 2018). Uncertainty estimates are based on non-parametric bootstrapping with replacement. Shaded areas show 95% confidence intervals.

B Online Appendix: PKH and its Impact Evaluation

B.1 Data

The World Bank collected baseline and endline data as part of an impact evaluation that accompanied the initial rollout of PKH. The baseline survey was conducted between June and August 2007 (about a month before the first PKH transfers occurred), while the endline survey was fielded between October and December 2009.

B.2 PKH implementation

Table B.1: PKH conditionality criteria

Household category	Conditions
Household with pregnant or lactating women	Complete 4 pre-natal visits and take iron tablets during pregnancy; Give birth assisted by a trained professional; Complete two post-natal visits (lactating mothers).
Households with children aged 0-6 years	Ensure children receive all immunizations and take Vitamin A twice a year; Take children for growth monitoring check-ups (monthly for infants up to 11 months old, quarterly for children 1-6 years old.
Households with children aged 6-15 years	Enroll children in primary/secondary school and ensure minimum 85 percent attendance
Households with children aged 16-18 who have not yet completed 9 years of schooling	Enroll children in an education program to complete 9 years of schooling

Note: Table is adapted from Cahyadi et al. (2020).

Table B.2: PKH benefit payments (annual; Indonesian Rupiah)

Criteria	Year 2012	Years 2013/14
Fixed base transfer	200,000	300,000
Each child below age of 5	800,000	1,000,000
Each woman pregnant/lactating	800,000	1,000,000
Each child attending primary school	400,000	300,000
Each child attending junior secondary school	800,000	1,000,000
Maximum benefit amount	2,200,000	2,800,000

Note: Table is adapted from Nazara and Rahayu (2013).

B.3 A note on the construction of key variables

Variables are constructed based on PKH's impact evaluation surveys, which interview one main adult per household (typically the household head). The surveys start with enumerators collecting a household roster that among other information (age, gender, education) collects labor market information on each household member aged 5 and older. More specifically, the respondent is asked the following question for each household member separately: What was the main activity of ... in the last week? The question has six response options: (i) working, (ii) attending school, (iii) not working, (iv) being retired, (v) being unemployed, and (vi) doing household chores.

Based on this information, we construct a binary idleness indicator that takes the value of one if the person is neither working, nor doing household chores, nor attending school in the previous week. The other three labor market indicators that we use in the analysis are constructed following the same logic. "Doing market work" refers to having indicated to work in the last week, "household chores" refers to having indicated to have done household chores in the last week, and "being currently enrolled at school" refers to the response "attending school".

B.4 RCT-related Tables

Table B.3: *RCT sample:* Covariate balance at baseline

	Control	Treatment	Difference	
	(1)	(2)	(3)	(4)
Panel A: Community level				
Population in 2007 [in 1,000]	3.448	3.472	−0.024	(0.068)
Violent crime in 2005	0.100	0.101	0.001	(0.014)
Violent crime in 2006	0.141	0.119	−0.022	(0.015)
Panel B: Household level				
Rural location	0.714	0.721	0.008	(0.033)
Age (resp.)	24.973	25.020	0.047	(0.379)
Female (resp.)	0.502	0.497	−0.005	(0.008)
Married (resp.)	0.492	0.499	0.007	(0.011)
HH size	5.155	5.115	−0.040	(0.079)
HH wealth	−0.011	−0.030	−0.019	(0.051)
HH has a radio	0.386	0.376	−0.010	(0.017)
HH has a TV	0.527	0.514	−0.013	(0.028)
HH has an antenna	0.010	0.009	−0.001	(0.002)
HH has a fridge	0.026	0.026	0.000	(0.004)
HH has a bike	0.480	0.470	−0.010	(0.036)
HH has a motor bike	0.160	0.159	−0.001	(0.014)
HH has a car	0.004	0.003	−0.001	(0.001)
HH has a cellphone	0.094	0.096	0.002	(0.009)
Total expenditures (monthly)	928.613	905.294	−23.319	(21.728)
Total food expenditures (monthly)	649.693	635.273	−14.420	(15.330)
Total non-food expenditures (monthly)	278.920	270.021	−8.898	(8.961)
PCA index on community engagement	−0.003	−0.059	−0.056	(0.081)
HH engages in community organization	0.780	0.773	−0.007	(0.018)
# community organization HH is involved	0.780	0.774	−0.006	(0.018)
# hh members involved with comm. organizations	2.193	2.090	−0.103	(0.109)
# meetings attended with comm. organ. (last 3 months)	1.659	1.573	−0.086	(0.099)
Works in agriculture (head)	0.647	0.663	0.016	(0.027)
Works in services (head)	0.135	0.124	−0.011	(0.014)
Highest degree is primary (head)	0.101	0.106	0.005	(0.006)
Highest degree is junior secondary (head)	0.029	0.025	−0.004	(0.003)
Highest degree is senior secondary or more (head)	0.014	0.013	−0.001	(0.002)

Note: Panel A shows statistics on 932 treatment and 897 control group communities. Panel B shows statistics on 7,184 treatment and 7,118 control group households. Mean values are reported in columns 1 and 2, respectively. In column 3 the simple differences between control and treatment group communities are displayed. Standard errors are clustered at the sub-district level and reported in parentheses in column 4. */**/*** denote significance levels at 10/5/1% respectively.

Table B.4: RCT sample: Covariate balance at the individual level at baseline

	Group	N	Control	Treatment	Difference	
	(1)	(2)	(3)	(4)	(5)	(6)
Working	Male (age 13–17)	3,552	0.096	0.083	-0.013	(0.013)
Working	Male (age 18–25)	3,821	0.303	0.293	-0.010	(0.021)
Working	Male (age 26–49)	12,540	0.346	0.345	-0.001	(0.020)
Working	Female (age 13–17)	4,068	0.140	0.137	-0.003	(0.013)
Working	Female (age 18–25)	3,760	0.628	0.646	0.018	(0.021)
Working	Female (age 26–49)	12,200	0.947	0.939	-0.008	(0.006)
Household chores	Male (age 13–17)	3,552	0.034	0.036	0.002	(0.007)
Household chores	Male (age 18–25)	3,821	0.465	0.467	0.002	(0.025)
Household chores	Male (age 26–49)	12,540	0.614	0.614	0.001	(0.021)
Household chores	Female (age 13–17)	4,068	0.003	0.004	0.001	(0.002)
Household chores	Female (age 18–25)	3,760	0.007	0.004	-0.002	(0.003)
Household chores	Female (age 26–49)	12,200	0.003	0.003	0.001	(0.001)
Attending school	Male (age 13–17)	3,552	0.636	0.633	-0.003	(0.023)
Attending school	Male (age 18–25)	3,821	0.053	0.045	-0.009	(0.008)
Attending school	Male (age 26–49)	12,540	0.003	0.002	-0.000	(0.001)
Attending school	Female (age 13–17)	4,068	0.613	0.598	-0.016	(0.024)
Attending school	Female (age 18–25)	3,760	0.068	0.056	-0.012	(0.009)
Attending school	Female (age 26–49)	12,200	0.003	0.002	-0.001	(0.001)
Staying idle	Male (age 13–17)	3,552	0.240	0.255	0.016	(0.021)
Staying idle	Male (age 18–25)	3,821	0.180	0.197	0.017	(0.016)
Staying idle	Male (age 26–49)	12,540	0.038	0.039	0.001	(0.004)
Staying idle	Female (age 13–17)	4,068	0.251	0.272	0.020	(0.020)
Staying idle	Female (age 18–25)	3,760	0.299	0.296	-0.003	(0.021)
Staying idle	Female (age 26–49)	12,200	0.048	0.056	0.008	(0.006)

Note: Standard errors are clustered at the sub-district level and reported in column 6. */**/*** denote significance levels at 10/5/1% respectively.

Table B.5: *RCT sample:* Compliance at the community level

	Control	Treatment
	(1)	(2)
Community-level compliance in..		
2007	0.995	0.998
2008	0.890	0.998
2009	0.736	0.998
2010	0.735	0.999
2011	0.572	1
Sub-district-level compliance in..		
2007	0.955	0.985
2008	0.829	0.985
2009	0.676	0.985
2010	0.667	0.993
2011	0.559	1

Note: Table shows yearly compliance shares of the 897 (111) control and 933 (127) treated communities (sub-districts).

Table B.6: *RCT household survey:* Receiving PKH at endline

	Control	Treatment
	(1)	(2)
% Receiving PKH	0.095	0.484

Note: Information on PKH recipients is derived from households' self-reports at the endline survey in 2009. The baseline survey did not include information on who was considered eligible, as this was only determined later by the MoSA. Therefore, the compliance rates represent a lower bound of actual compliance.

Table B.7: *RCT household survey:* Attrition

	Control group		Treatment group		All	
	# obs.	%	# obs.	%	# obs.	%
	(1)	(2)	(3)	(4)	(5)	(6)
Baseline (2006)	7,131	100	7,196	100	14,327	100
Endline (2009)	6,946	0.974	7,024	0.976	13,970	0.975

Note: Table shows attrition rates between PKH's RCT baseline and endline survey. In both, control and treatment group, about 97.5% of all baseline households could be re-surveyed.

Table B.8: *RCT household survey:* Determinants of attrition

	Specification 1	Specification 2
	(1)	(2)
Treatment assignment	-0.002	-0.002
	(0.003)	(0.003)
Observations	14,327	14,327

Note: The table examines the extent to which PKH is related to attrition. It shows coefficients after regressing (OLS) a dummy of 'being re-surveyed at the endline' on the original treatment assignment indicator. Specification 1 includes district fixed effects and basic controls. Specification 2, in addition, includes a set of extended controls. Standard errors are depicted in parentheses and are clustered at the sub-district level. */**/*** denote significance levels at 10/5/1% respectively.

C Online Appendix: SUSENAS Description

C.1 Sample construction and variables

SUSENAS is a cross-sectional household survey collected annually by Statistics Indonesia (BPS). The data constitutes the empirical foundation for a number of the country's official statistics—in particular, poverty, expenditure, demographic, and education statistics. Its sampling frame is determined by BPS and is aimed to be representative at the district level.

Crime data Since 2007, SUSENAS gathers disaggregated information on crime. Crime information relates to self-reported information on whether a household experienced certain types of crime. Over time, the SUSENAS crime module became more comprehensive. While in earlier years crime information was collected at the household level, later rounds collected individual-level crime information. Crime types collected in SUSENAS relate to theft, robbery, homicide, rape, and other types of crime, whereby reported crimes are dominated by far by thefts and robberies. For our baseline results, we use a binary indicator on crime that includes theft- and robbery-coded incidents as they are separately asked for in the survey instrument every year. Alternatively, in panel B of table A.13 we also show results relying on all reported crimes that include other minor types of crime as well, which were not always separately asked for. Given that SUSENAS's survey instruments changed over time, we construct two separate samples:

1. **Sample 2007–2011:**
   Our main sample reported in the paper is compiled by pooling SUSENAS data for the years 2007 to 2011. The sample matches approximately the time period of our main analysis that uses the NVMS data. While crime data disaggregated by type is not available from SUSENAS for the pre-2007 period, BPS decided to strip the data from sub-district identifiers from 2011 onwards. Hence, we cannot link SUSENAS with the administrative PKH rollout data at the sub-district level in later SUSENAS rounds. Data is available both in provinces with and without NVMS coverage.

2. **Sample 2013–2019:**
   The second sample is compiled by pooling SUSENAS data for the years 2013, 2014, 2017, 2018, and 2019. This sample is used in Online Appendix C.2. The rounds were selected since they include both, information on crime (self-reported) and a household's PKH membership (self-reported). Survey items related to PKH were introduced in 2013, though were excluded in the 2015 and 2016 rounds, until rein-

troduced again in 2017. In addition, SUSENAS also asks if households reported crime incidents to the police (by type). The sample is available for both provinces with and without NVMS coverage. Table C.1 presents descriptive crime statistics based on SUSENAS for the years 2007 to 2019.

Time use data For analyzing mechanisms, we pool the yearly SUSENAS modules on labor market participation and time use. We rely on the years 2004 to 2011 which contain sub-district identifiers and hence can be connected over time.

SUSENAS conducts interviews with one main person per household, typically the household head or her/his spouse. During the interviews (in the labor market section of the questionnaire) the respondent is asked to answer the following four questions regarding each single household member aged 10 or older:

1. Did he/she work last week? (yes/no)
2. Did he/she attend school last week? (yes/no)
3. Did he/she do household chores last week? (yes/no)
4. Was he/she engaged in other activities (personal, sports, mosque, etc.) last week? (yes/no)

Based on this information, we construct four binary indicator variables. *Market work* takes the value of one if a person was engaged in any type of work except for household chores (answered yes to question 1). *Household chores* takes the value of one if a person has reported doing household chores (question 3). *In school* takes the value of one if the person has reported having attended school during the last week (question 2). Finally, the indicator *Idle* takes the value of one if a person did not pursue any of the first three listed activities in the previous week. Thus while market work, household chores, and school attendance are not mutually exclusive categories but may overlap, idleness is defined by the absence of these first three reported activities.

Regressions based on SUSENAS utilize a range of further variables:

- Poverty status of households (defining potential eligibility as CCT recipient households) is measured by comparing monthly household expenditures per capita to the value of the provincial poverty line (in per capita terms) in a given year.
- Household head characteristics record the years of completed education, age and marital status.

- Further household characteristics include household size (in quintiles), and urban status (defined by BPS).
- Individual characteristics capture completed degree of education (none, primary, lower secondary, upper secondary, or tertiary), age, and marital status.

Parental Attention Every three years, the autumn round of SUSENAS includes a special module that asks parents to provide child-specific information on the type of activities that parents engage in with their children over the last week. The information is gathered for each child aged 0 to 17. To each respective question, parents provide simple binary answers (yes vs. no).

Until mid-2024, Statistics Indonesia has collected only two rounds of SUSENAS that included both PKH information and parental attention information (year 2018, year 2021). Since we do not have access to the specific module for the year 2021, we focus our analysis exclusively on the round for the year 2018.

Our main specification rests on an additive index that sums all ten specified activities. The related questions to the ten activities are as follows:

- Eating together with parents
- Watching TV together with parents
- Learning/reading books under parental supervision
- Read story books with/by parents
- Pray together
- Chat/Talk together
- Play together (simple)
- Play together (complex)
- Being on the internet together
- Having food/drinks in a food place together

For robustness checks we calculate additional indices that (i) using principal component analysis (instead of the construction of a simple additive index) and (ii) that exclude possible school-related activities such as learning together. Our main results do not change once using these alternative indices.

C.2 PKH and Crime/Parental Attention: PSM

To derive the ATE from propensity score matching (PSM) we rely on STATA's `teffects` package. Given that in observational studies, propensity scores are not known, we do not match on the true propensity scores, $p(X)$, but on an estimate of it. More specifically, and as is common in economics, we consider a generalized linear specification for the propensity score, $p(X) = F(X'\theta)$, and use a Logit as a link function.

The link function is specified as follows:

$$logit\ \theta_{icpt} = \ln \frac{\theta_{icpt}}{1-\theta_{icpt}} \quad = \quad Z'_{icpt}\beta + \lambda_p + \theta_t + \epsilon_{icpt}, \tag{6}$$

where θ_{icpt} is the probability to participate in PKH for household i in community c in province p in year t. Z'_{icpt} represents an array of household characteristics to predict PKH participation. Controls at the household level include household size in quintiles, the number of children below the age of 12 and between 13 and 18, a rural binary variable, and a set of indicator variables that classify households into expenditure per capita deciles. Further controls relate to the age, gender, marital status, and education of the household head. λ and θ represent district and time fixed effects. Standard errors are clustered at the community level taking into account that propensity scores are estimated and treatment is set at the community level (Abadie and Imbens, 2016).

C.3 Appendix C: References

Abadie, A. and G. Imbens (2016). Matching on the estimated propensity score. *Econometrica* 84(2), 781–807.

C.4 SUSENAS Tables

Table C.1: SUSENAS 2007-2019: Share (%) of households experiencing crime

Year	Theft	Robbery	Crime	Police	Obs.
	(1)	(2)	(3)	(4)	(5)
2007	0.0275	0.0245	0.0520	.	285,186
2008	0.0252	0.0222	0.0474	.	282,387
2009	0.0265	0.0049	0.0314	.	291,753
2010	0.0273	0.0031	0.0304	.	293,716
2011	0.0237	0.0032	0.0269	.	285,186
2012	0.0185	0.0023	0.0208	.	286,113
2013	0.0202	0.0021	0.0223	0.1909	284,063
2014	0.0231	0.0019	0.0250	0.1919	285,400
2015	0.0284	0.0007	0.0291	.	285,908
2016	0.0275	0.0008	0.0283	.	291,414
2017	0.0350	0.0008	0.0358	0.1623	297,276
2018	0.0341	0.0011	0.0352	0.3634	295,155
2019	0.0315	0.0010	0.0325	0.3684	315,672

Note: Statistics are derived by the authors from SUSENAS data and refer to the 16 NVMS provinces. We define violent crime as the sum of theft and robbery incidences. 'Police' captures the share of violent crimes that, according to survey respondents, were reported to the police.

Table C.2: *Alternative measure:* PKH effects on the probability of being a victim of violent crime (PSM)

Region:	NVMS	Non-NVMS
Household sample:	Poor	Poor
	PSM (1)	PSM (2)
PKH beneficiary	0.004* (0.002)	0.004*** (0.001)
Province FE	Yes	Yes
Year FE	Yes	Yes
Controls	Yes	Yes
Observations	722,084	722,084

Note: The estimation sample is based on pooled SUSENAS rounds from the years 2013, 2014, 2017, 2018, and 2019, which received the PKH program until 2014. Columns 1 and 2 distinguish between households residing in provinces with and without NVMS coverage. The dependent variable is a binary indicator that takes the value of one, if households reported being a victim of violent crime (robbery and/or theft) in a given year. Results rely on PSM as described above in section C.2. Standard errors in parentheses are clustered at the level of sub-districts. */**/*** denote significance levels at 10/5/1% respectively.

Table C.3: Impact of PKH on the reporting of violent crime to the police (PSM)

Parameter	Share (%) reported	
	(1)	(2)
PKH	-0.008	0.004
	(0.018)	(0.023)
Obs.	722,084	722,084
Province FE	Yes	Yes
Year FE	Yes	Yes
Controls	Yes	Yes
Expenditure controls	No	Yes

Note: The sample is based on pooled SUSENAS rounds from the years 2013, 2014, 2017, 2018, and 2019 for NVMS provinces. The dependent variable is derived as the ratio of violent crimes reported to the police divided by the total number of violent crimes experienced by a household. Robust standard errors are reported in parentheses. */**/*** denote significance levels at 10/5/1% respectively.

D Online Appendix: Back-of-the envelope calculations

We estimate the change in violent crime events at the extensive margin, focusing on the likelihood of a violent crime being reported in each community in a given year. We use the increase in the incidence of violent crime from Table 1, which is 1.4 percentage points in column (4) of panel B. According to our administrative data, the number of PKH recipient villages by 2014 amounted to 55,631. On the extensive margin, our results predict an increase in violent crimes by 779 additional events per year:

$$\underbrace{\frac{\partial Crime}{\partial CCT}}_{1.4pp} \times \underbrace{\text{\# of villages}}_{55{,}631} = 779$$

According to administrative data, 2,696,890 households were receiving PKH by 2014. Based on the closest wave of nationally representative SUSENAS household survey data, in 2011, the average number of male youth (aged 18–25) living in eligible poor households was 0.269. Assuming constant family composition over time, this results in 725,463 youth male household members living in PKH-receiving households by 2014. The PKH-induced increase in idleness among male youth is estimated at 2.8 percentage points (based on column (2) of Table 5). Consequently, according to our estimates, about 20,313 male youth became newly idle due to within-household spillover effects.

$$\underbrace{\frac{\partial Idleness}{\partial CCT}}_{2.8\,pp} \times \underbrace{\text{\# CCT households}}_{2{,}696{,}890} \times \underbrace{\frac{\text{\# CCT male youth}}{\text{\# CCT households}}}_{0.269} = 20,313$$

These estimates result in one additional violent crime event per 26 newly idle young males, or a crime rate of 3,8%:

$$\text{Crime rate} = \frac{\text{\# New violent crime events}}{\text{\# Newly idle male youth}} = \frac{779}{20{,}313} = 3,8\%$$

According to aggregate criminality statistics, in 2011 there were 33,552 youth crime suspects (aged 19–24) in Indonesia (BPS, 2014). Based on this, we derive upper and lower bound estimates for youth crime rates. Under the extreme assumption that all youth crime suspects are idle, compared to the estimated total idle male youth population (irrespective of household income status, about 1,6 million) in the same age bracket, this yields a criminalization rate of 2.1% (33,552/1,610,461). When compared to the estimated size of idle male youth living below the poverty line (230,948)—assuming most youth crime is perpetrated by idle poor individuals—this results in a criminalization rate of 14.5% (33,552/230,948). To estimate lower bounds, we assume the share of unemployed individuals among youth criminals matches the proportion in the overall criminal population (28% according to POLRI, 2019). This yields 9,395 idle youth crime suspects ($0.28\times$ 33,552). Comparing this to the total idle youth population produces a criminalization rate of 0.6% (9,359/1,610,461). Comparing it to idle poor youth results in a criminalization rate of 4.1% (9,359/230,948).

As we lack information on the socio-economic and occupational status of youth crime suspects, none of these rates allow for accurate comparisons and may understate or overstate group-specific crime rates. However, they establish a range of crime rates (0.6% to 14.5%) that align with the order of magnitude of the CCT-induced crime rate of 3.8% derived from our back-of-the-envelope calculations.

E Online Appendix: Alternative mechanisms

E.1 Changing costs of crime

Investment into crime Beyond changing victimization potential and the documented idleness effects, CCT could have also changed crime rates by reducing the costs of engaging in criminal activities. Income effects from CCT may (i) enable the purchase of weapons (Duggan, 2001; Koenig and Schindler, 2021), (ii) increase mobility and social interactions (Glaeser et al., 1996), and (iii) improve information acquisition about potential crime victims (Glaeser et al., 1996). Based on the RCT data, we see no changes in transport-related assets and expenditures (panel B in Table 3), nor in information and communication-related assets (panel C in Table 3).

Community-level monitoring Although the empirical evidence on the impact of CCTs on social ties and community participation is rather mixed (Attanasio et al., 2009; Cameron and Shah, 2014; Attanasio et al., 2015), we assess whether PKH has led to a decrease in community involvement, which in turn might have led to a decrease in community-based monitoring of social piece. We contrast four different measures of household-level engagement in community organizations and observe no decreases in any measure of community involvement by beneficiary households (panel D in Table 3).

Likelihood of punishment Crime rates have also been shown to be sensitive to actual punishment costs (Levitt, 1998; Drago and Galbiati, 2012). While the CCT program is unlikely to have altered the stringency of actual punishment or formal and informal sanctions and litigation costs, the program might have changed subjective perceptions of expected punishment costs (Sah, 1991).[19] Bearing in mind that Indonesia is a predominantly Muslim country and that we do not have data on individual-level alcohol and drug consumption, we examine whether the program altered household expenditures on alcohol and legal drugs (betel nuts, which are stimulant drugs that many Indonesians consume). As shown in panel D of Table 3 we find no changes in household spending on these consumption items.

E.2 Changing marriage and fertility

Several studies in economics have pointed out that changes to the structure of the marriage market can induce crime or violence (Edlund et al., 2013; Rexer, 2022). In our con-

[19] Evidence from other cash transfer programs across the globe tend to show rather negative effects on consumption of alcohol and drugs (cf. Evans and Popova, 2017).

text, the CCT could possibly alter the marriage market for young people directly (by e.g., lifting cash constraints or increasing education for younger children) or indirectly (if youth idleness reduces the attractiveness in matching markets or increases the available time to search for partners). For instance, if the CCT leads to reductions (or delays) in early marriages then the CCT-induced potentially breakdown traditional norms and might cause deviant behaviours, including criminal activities. Furthermore, a disintegration of crime-preventing social norms might be particularly relevant for young and idle persons. Nonetheless, we believe that changes in the marriage market are unlikely to play an important role in our setting. First, as shown in Priebe and Sumarto (2023), PKH did not change early marriage patterns for boys and men whom we suspect to be driving the observed increase in crime due to the CCT. Second, we do not observe changes in social norms in both rural and urban locations as a result of PKH (cf. Table 3).

F Online Appendix: Perceptions on crime, idleness and poverty

F.1 Introduction

In January 2023, we fielded an online vignette experiment in Indonesia to elicit whether a young person's idleness and poverty status affect individuals' perceptions of being a perpetrator of crime. Bearing in mind that our vignettes might trigger representative bias and stereotyping, we assume, similar to Bordalo et al. (2016), that people's mental models of crime are based on a 'kernel of truth'. Although individuals might overestimate the magnitude of the relationship between idleness and crime but the direction of this relationship is indicative of real-world crime patterns.[20] Therefore, the vignette results help us establish whether idleness is publicly believed to be a crime-enabling factor in Indonesia beyond of what is described in the official police reports (BPS, 2013; POLRI, 2019). Furthermore, the vignette allows us to gain insights into the interaction of poverty and crime, something not directly available from the Indonesian police reports.

F.2 The vignette experiment

The vignette experiment was conducted as an online survey between January 15th and February 1st, 2023, in Indonesia. The survey for the vignette experiment was programmed using the platform UNIPARK. Respondents were recruited via one of Asia's largest online panel providers, called 'dataSpring'. To become eligible for the survey, respondents had to be between 18 and 50 years of age. Survey implementation involved quotas regarding age, education, and region (defined as major islands) in order to obtain a sample that is likely to be more representative of Indonesia's general population (in that age group). In total, 1,763 persons completed the survey successfully. As shown in Table F.1, the average person who completed the survey was about 32 years old. About 48 percent of respondents were female.

We implemented a between-subjects design with each vignette describing a specific young man, Budi (aged 20), who lives with his parents and younger siblings in Central Java and was recently arrested by the police as a suspect for having committed a theft. The vignette experiment randomized three factors, namely the suspect's idleness status (working full-

[20] A different line of argument for interpreting our results this way comes from the empirical, sociological crime literature that often finds that individuals are rather good in identifying actual perpetrators from standardized photos and socio-demographic descriptions (Dressel and Farid, 2018; Lin et al., 2020)

time in a small kiosk vs. not working or attending school), the value of the loot (about 100 USD vs. 2,000 USD), and the suspect's wealth (rich vs. poor vs. very poor). Therefore, the experiment consisted of 12 unique texts (2x2x3 factorial design). The three factors were implemented as follows:

- Factor 1: Idleness vs. Work (2 attributes)

  Idle: 'is not working or attending school these days'

  Work: 'working 40h per week in a small kiosk'

- Factor 2: Value of the loot (2 attributes)

  Low: '1 Million Rupiah' (approx. 100 USD)

  High: '20 Million Rupiah' (approx. 2,000 USD)

- Factor 3: Socio-economic background (3 attributes)

  Poor: 'is very poor' or 'is very poor and receives PKH'

  Rich: 'is rather rich'

The implemented vignettes read as follows:

> *"Budi, 20 years old, lives in the city of Purwokerto in Central Java. He still lives with his parents and has two younger siblings (ages 5 and 12). His father works as a cab driver while his mother handles the household. Budi is* ***working 40 hours per week in a small kiosk. [is not working or attending school these days.]*** *This morning, Budi got arrested by the police and is suspected to have stolen items from a store in a local shopping mall worth IDR* ***1 Million [20 Million]****. If found guilty, a fitting punishment must be imposed. Budi comes from a family that is* ***very poor [is very poor and receives PKH] [is rather rich.]****."*

Directly after the respondent had read the text, he/she was asked to rate on a 10-item Likert scale whether he/she believed that Budi has committed the crime. As shown in Table F.1, respondents' replies range from 1 (extremely unlikely that Budi committed the crime) to 10 (extremely likely that Budi committed the crime), with people on average rating the suspect with 6.6 (median of 7). Table F.2 shows balanced statistics between the subsamples.

F.3 Methods and results

We evaluate the experiment by estimating the following regression model by OLS:

$$Y_i = \alpha + \beta T_i + \mathbf{X}_i' \gamma + \mathbf{Z}_i' \theta + \epsilon_i, \tag{7}$$

Table F.1: Summary statistics (vignette experiment sample)

Variable	Mean	Median	SD	Min.	Max.	Obs.
	(1)	(2)	(3)	(4)	(5)	(6)
Age (years)	32.02	32.00	8.74	18.00	55.00	1,763
Female	0.48	0.00	0.50	0.00	1.00	1,763
Dep. variable						
Likelihood perpetrator	6.62	7.00	2.15	1.00	10.00	1,763

Note: The dependent variable reflects the likelihood that respondents believe the described person committed the crime (theft in our case) under investigation. It is coded on a 10-item Likert scale with "1" meaning "extremely unlikely to have committed the crime" and "10" meaning "extremely likely to have committed the crime".

Table F.2: Balance table (vignette experiment sample)

	Mean values for each sub-sample					Test for differences between sub-samples			
	Poor	PKH	Rich	Works	Idle	Poor vs. PKH	Poor vs. Rich	PKH vs. Rich	Works vs. Idle
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Loot	0.495	0.482	0.502	0.480	0.506	0.013	-0.007	-0.020	0.027
						(0.029)	(0.029)	(0.029)	(0.024)
Idle	0.488	0.489	0.522	0.000	1.000	-0.001	-0.034	-0.033	1.000
						(0.029)	(0.029)	(0.029)	(0.000)
Age	32.218	31.889	31.950	31.793	32.246	0.328	0.268	-0.060	0.454
						(0.513)	(0.507)	(0.512)	(0.416)
Female	0.468	0.489	0.492	0.486	0.479	-0.021	-0.024	-0.003	-0.007
						(0.029)	(0.029)	(0.029)	(0.024)
Poor	1.000	0.000	0.000	0.341	0.326	1.000	1.000	0.000	-0.016
						(0.000)	(0.000)	(0.000)	(0.022)
PKH	0.000	1.000	0.000	0.336	0.321	-1.000	0.000	1.000	-0.014
						(0.000)	(0.000)	(0.000)	(0.022)
Rich	0.000	0.000	1.000	0.323	0.353	0.000	-1.000	-1.000	0.030
						(0.000)	(0.000)	(0.000)	(0.023)
Guilt verdict	6.721	6.556	6.579	6.475	6.763	0.165	0.142	-0.023	0.288
						(0.128)	(0.123)	(0.125)	(0.102)
Obs.	588	579	596	882	881	1,167	1,184	1,175	1,763

Note: The balance table divides the sample into three groups according to the description of the perpetrator's socio-economic background. The background categories are 'Poor' (poor background without explicit mentioning of PKH), 'PKH' (poor background in which parents receive PKH, and 'Rich' (wealthy background). Other variables are defined as follows: 'Guilt verdict' refers to our outcome variable to what extent the suspect committed the crime. 'Loot' refers to the share of the sample that received vignettes in which the crime suspect is stealing something of high value. 'Idle' refers to the share of the sample in which the crime suspect is described as being idle (neither work nor schooling). Standard errors are shown in parentheses and were estimated via OLS using heteroskedasticity robust standard error adjustments.

where Y_i refers to the outcome variable for individual i, α indicates the intercept, $\mathbf{X}$ refers to individual-level control variables, Z includes the other two vignette factors (value of stolen items, socio-economic background of the suspect). Lastly, T_i is a binary indicator variable that takes the value 1 if the suspect was described as idle. Our main coefficient of interest is β.

Furthermore, we interact the idleness factor with the characteristics describing the subject as poor using the following model:

$$Y_i = \alpha + \beta T_i + \delta T_i \times P_i + \mathbf{X}_i^{'}\gamma + \mathbf{Z}_i^{'}\theta + \epsilon_i, \tag{8}$$

where P_i denotes an individual who was described as poor.

Table F.3 depicts our main findings. Columns 1–4 show estimates for the full sample, and columns 5–6 contain split-sample estimates.[21] Overall, we find that respondents strongly link a young man's idleness status to committing a crime (column 2). This relationship is almost entirely driven by crime suspects who are described as coming from a poorer socio-economic background (columns 4 & 5). We conclude that individuals' mental models predict idleness, when combined with poverty, to be a crime-enabling factor in Indonesia, which seems to independently corroborate information from the country's official police reports. In line with other studies in economics that hint at the positive correlation between youth idleness and crime in other world regions such as Latin America (Chioda et al., 2016; Machado et al., 2018), the U.S. (Jacob and Lefgren, 2003; Foley, 2011), and Europe (Bratsberg et al., 2019), the idleness channel among young men appears to be a likely explanatory factor for crime in Indonesia too.

[21] The experiment was pre-registered under the Open Science Framework (https://osf.io/y9exw).

Table F.3: Vignette Experiment: Likelihood of suspect committing the crime

Coefficient	Full sample				Sub-samples	
					Poor=1	Rich=1
	(1)	(2)	(3)	(4)	(5)	(6)
Poor	0.062			-0.041		
	(0.106)			(0.168)		
Idle		0.294***		0.041	0.431***	0.056
		(0.102)		(0.170)	(0.127)	(0.170)
Loot			-0.039	-0.047	-0.139	0.176
			(0.102)	(0.102)	(0.128)	(0.170)
Idle x Poor				0.387*		
				(0.212)		
Observations	1,763	1,763	1,763	1,763	1,167	596

Note: The dependent variable measures the likelihood that respondent's believe the described person committed the crime (theft) under investigation. It is coded on a 10-item Likert scale with "1" meaning "extremely unlikely to have committed the crime" and "10" meaning "extremely likely to have committed the crime". 'Idle' is a binary indicator indicating whether the suspect was described as idle (neither work nor school) or as working (working in a kiosk). 'Poor' is a binary indicator indicating whether the suspect is poor. 'Loot' is a binary indicator indicating whether the stolen things were of high value. All specifications include a respondent's age and gender as controls. All specifications were estimated by OLS, using robust standard errors. */**/*** denote significance levels at 10/5/1% respectively.

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