Does Social Assistance Disincentivise Employment, Job Formality, and Mobility? Learning from Past Unconditional Cash Transfer Programmes in Indonesia

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Abstract

How do cash transfers affect employment, job formality, and mobility, especially in times of crises and economic recovery? I examine this question in the context of Indonesia's major unconditional cash transfer (UCT) programmes, rolled out in a targeted manner in response to adverse economic shocks. Identification is based on a generalised difference-in-differences with propensity score matching approach exploiting three waves of nationally-representative longitudinal data on household transfer receipts and labour market outcomes. Annual retrospective data in each survey wave allows me to look at immediate effects, potentially important due to the transient nature of the transfers. Consistent with income and substitution effects, the cash transfers reduce employment and job formality, especially among lower skill groups. Relatively larger effects on job formality highlight the importance of this margin of adjustment related to the targeting design.

Keywords: unconditional cash transfer, social assistance, employment, job formality, job mobility

JEL Classifications: I30, I38, J00, J22

1 Introduction

Many developing countries recently increased social protection spending, with targeted cash transfer programmes a key anti-poverty tool (Barrientos & Hulme, 2009; Devereux & Sabates-Wheeler, 2004; Estevez-Abe et al., 2001). While transfers can improve education, health, and other welfare outcomes among poor households (Gerard et al., 2020; Hanlon et al., 2012; Norton et al., 2002), there may be reduced work incentives and a greater inclination towards informal employment (Borjas, 2016). Protecting the poor and alleviating hardship during economic downturns through cash transfer may thus potentially undermine economic recovery if it has disincentive effects in labour markets. The disincentive effects of social assistance programs on employment are significant policy concerns influencing how these programs develop in the future. This paper measures the labour market impacts of Indonesia's

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large-scale unconditional cash transfer (UCT) programs intended explicitly as crisis stimulus, with a particular emphasis on employment, job formality, and mobility.

The Indonesian government launched its first large-scale cash transfer programme in 2005 in line with a large reform of its fuel subsidy programme, reallocating a part of the fuel subsidy savings to poor and vulnerable households. Despite its success in administering the assistance at the right time to alleviate potential negative impacts (Bazzi et al., 2015), it is not clear how such cash transfers affects labour market behaviour (see Al Izzati et al., 2020; World Bank, 2012 for several earlier studies of UCT in Indonesia). Most early work examining the labour market effects of social assistance focused on developed countries, and tended to find negative impacts on negative labour supply and work effort, largely due to generous welfare systems (see Burtless, 1986; Chan & Moffitt, 2018; Dague et al., 2017; Hoynes & Schanzenbach, 2012; Jacob & Ludwig, 2012; Moffitt, 2002). In the context of developing countries, empirical evidence is limited and effects tend to be around zero (e.g., Alzúa et al., 2013; Banerjee et al., 2017). Many programs studied to date only produce income effects because targeting relies on proxy-means tests that are rarely reassessed and exclude labour market outcomes (Gerard et al., 2021). Means-tested programs, in contrast, could potentially distort beneficiaries' incentives to work through substitution effects, especially in the formal economy where earnings are verifiable (Bergolo & Cruces, 2021; Levy, 2010).

By drawing on the case study of *Bantuan Langsung Tunai* (BLT) 2005, BLT 2008, and *Bantuan Langsung Sementara Masyarakat* (BLSM) 2013, administered by the Indonesian government in targeted manner, I examine the aggregate effects of social transfers on labour market outcomes by capturing both income and substitution effects. A standard labour supply model predicts that beneficiaries are less likely to seek out or maintain employment as a result of relatively higher unearned income (income effect). Beneficiaries may also experience a substitution effect, and be less likely to take on formal employment since higher formal earnings could render them ineligible for the transfers.. Since formal sector income is more easily verifiable, beneficiaries may avoid formal employment due to the consequently non-zero marginal tax rate. Thus, the targeting approach of cash transfer programmes determines the magnitude of any disincentive effects. However, since these UCTs essentially act as temporary fiscal stimulus, the negligible effects could be the case, especially in the long run.

Due to the targeted nature of the UCT programmes, it is often difficult to obtain statistically equivalent treatment and control groups to analyse social assistance's impacts. In practice, targeting was imperfect, with inclusion and exclusion errors abound. As a result, beneficiaries and non-beneficiaries should not be different based on observable features, and identification could exploit targeting errors. However, household and community characteristics here are statistically different, including in ways that may be correlated with treatment assignment and labour market outcomes. Therefore, I use generalised difference-in-differences (DD) with propensity score matching (PSM) to recover the effects of these UCT programmes. Since the UCTs are aimed at low-income households, specifically the poorest 30 percent, I complement the DD estimation with PSM to adjust my comparisons for any observable factors potentially determining treatment. The DD estimation is then carried with individual fixed effects and observations in common support. Individual-level annual panel data spanning over 14 years are constructed from the retrospective answers of employment from the Indonesia Family Life Survey (IFLS)—waves 3, 4, and 5—which also allow me to conduct an event studies to explore dynamic effects and in particular and whether any impacts are transitory or persistent.

I find statistically significant evidence of disincentives on employment and job formality consistent with both income and substitution effects, especially for BLT 2008/BLSM 2013. For the BLT 2005, however, there is an increased likelihood of formal employment, albeit with a two-year lag. The mixed results across settings may be explained in part by the differences in targeting approaches used in BLT 2005 and BLT 2008/BLSM 2013. In comparison to BLT 2005, which relied on a standard proxy-means test, BLT 2008/BLSM 2013 used a broader range of indicators, including information on employment and occupation combined with community-based targeting. The use of a more detailed proxy-means test in conjunction with more recent, dynamic welfare data may have stronger disincentive effects if these criteria are known to potential beneficiaries. Indeed, these disincentives are small and account for only a 2-percentage-point decline in employment and job formality (2 and 8 percent decrease from the mean outcome, respectively). The delayed positive effect on job formality from BLT 2005 may be due to higher labour demand in the formal sector, for example due to local multiplier effects and alleviating liquidity constraints in searching for better jobs (Baird et al., 2018; Egger et al., 2022; Neri et al., 2013).[2]

This paper relates most closely to World Bank (2012) and Al Izzati et al. (2020), which also evaluate the impacts of UCTs in Indonesia. World Bank (2012) examines the effect of UCTs (in either BLT 2005 or 2008) using the National Socio-economic Survey (SUSENAS),

[2] The positive effect on job formality is also observed in Brazil, the increased Bolsa Familia's transfers in 2009 led to an increase in formal employment, which occurred despite the fact that the program may in fact reduce formal labour supply among its beneficiaries (Gerard et al., 2021).

a repeat cross section, finding that UCT beneficiaries, on average, were not more likely to find or leave work. However, beneficiaries of BLT 2005 in the bottom three deciles were 10 percentage points more likely to report moving into employment. Al Izzati et al. (2020) use the main IFLS panel in the years 2000, 2007, and 2014 (i.e., not the retrospective annual data) to find that, among those already working, receiving the UCT has no statistically significant effects on working hours or activities in a farm or nonfarm business. Here, I additionally use IFLS' retrospective responses allowing evaluation with individual fixed effects and of short-term effects, a crucial but neglected dimension given the temporary nature of these crisis-response UCTs.

This paper thus makes three main contributions relative to prior work. First, I incorporate job formality and mobility, two important dimensions receiving relatively little attention to date. In the context of social assistance programmes using means-tested targeting, it is particularly important to account for the aggregate effects of cash transfer, especially in the formal sector. Recent studies (see Banerjee et al., 2017, Foguel & Barros, 2010; Galasso, 2006; Maluccio & Flores, 2005; Parker & Skoufias, 2000; Skoufias & Di Maro, 2008) tend to find little evidence of employment effect from cash transfers related to the main income effects, whereas the substitution effects—the margin of employment actually most likely affected from salient means testing—are often overlooked. Second, I add to emergent work focused on developing countries evidence based on rich, high-frequency longitudinal data with a good recall quality to examine whether any labour market responses to cash transfers are temporary or long-lasting. Third, I also provide a relatively novel exploration of treatment effect heterogeneity, not only based on different types of beneficiaries but also the different types of UCT designs, including how different approaches to means-testing may affect various margins of adjustment.

This paper proceeds as follows. Section 2 discusses previous work and provides a background on Indonesia's UCT programmes. Section 3 and Section 4 describe the data and empirical strategy. Section 5 presents the main results, followed by heterogeneity analysis and robustness checks in Section 6 and 7. Finally, Section 6 concludes with policy implication.

2 Literature Review

2.1 Earlier Studies

Economic theory has long predicted that welfare benefits present disincentives to individuals; it posits that welfare benefits reduce labour supply. According to the orthodox economic theory, welfare benefits have two potential consequences on labour supply (Brosnan et al., 1989). First, when the gains from returning to employment are insufficient to compensate for the loss of the welfare benefits, beneficiaries may be induced to leave and have no incentive to return to employment (extensive margin of unemployment). Second, in the absence of financial incentives to return to work, beneficiaries may prefer to accept more time spent for leisure or keep searching for the right job instead of accepting the first offer, extending the duration of unemployment (intensive margin of unemployment). In addition, particularly surrounding family income support programmes[3], there are concerns that the benefits provided will reduce any incentive for people to work and lift themselves out of poverty—often termed the "unemployment or poverty trap" (Brosnan et al., 1989). Theoretically, the decrease in the amount—or even absence—of income support as income rises will dissuade labour market participation and inevitably cause long-term dependence on social assistance. These outcomes are anticipated since finding and accepting a job offer often comes with high replacement costs and effective marginal tax rates that individuals must bear.

Empirical evidence investigating the effects of welfare benefits on labour supply has never been conclusive. Most of the early empirical literature centred around cases in developed countries supported by the relatively generous social welfare system and ample data availability. Burtless (1986), using an experiment framework, studies the implications of negative income tax in the United States on labour supply and finds that it reduces work effort among participants. The magnitude of reduction is moderate but larger than proponents of the policy had anticipated; beneficiaries reduced their reported work hours, for both males and females, by 7 and 17 percent, respectively. Hoynes and Schanzenbach (2012) examine the labour effects of the Food Stamp Programme (FSP) in the United States and find consistent results with the theory, where they observe substantial reductions in employment and hours worked when the programme is introduced. Similarly, Dague et al. (2017), by taking advantage of the sudden imposition of an enrolment cap of Medicaid among childless adults in Wisconsin, find that public insurance enrolment reduces the likelihood of an adult being employed (by just over five percentage points) and lower net earnings (by over US$315 per quarter for the next two years). Moreover, a study on the effects of housing assistance on labour supply by Jacob and Ludwig (2012) also shows evidence of the reduction of labour force participation by four percentage points among working-age adults in Chicago.

[3] Family income support programmes often provide beneficiaries with cash income supplements adjusted on the basis of family income status, family size, and other indicators of need. The level of income support benefit normally does not afford a living standard beyond the poverty line.

Despite vast results showing a negative impact on labour outcomes in developed countries, evidence for cases in developing countries remains limited. It was not until recently that researchers started to examine the implications of welfare benefits in developing economies since their governments started investing in major social protection programmes. Fernandez and Saldarriaga (2014), investigating labour supply responses to a Conditional Cash Transfer (CCT) in Peru, find evidence of a negative transitory effect that beneficiaries reduce their labour supply by 6 to 10 hours in the week following the payment date. However, the latest experimental evidence, overall, only shows minor effects of CCT on the labour supply of working adults from households that receive the benefits (see Foguel & Barros, 2010; Galasso, 2006; Maluccio & Flores, 2005; Parker & Skoufias, 2000; Skoufias & Di Maro, 2008). Moreover, Alzua et al. (2013) also find small and insignificant effects of various welfare programmes on the labour supply in Mexico, Nicaragua, and Honduras. Similarly, Abdulai et al. (2005) conclude that the food aid programme implemented in Ethiopia does not result in a disincentive for the work hours supplied. In sum, empirical findings in developing countries are relatively different from those in developed countries, partly because transfers in developing countries are often not that large relative to the average household consumption.[4]

Limited evidence exists about the impact of social assistance programmes on the labour market in Indonesia, despite persistent active policy to end extreme poverty through the provision of social assistance. More specifically, I am interested in examining the effects of UCT, typically provided as short-term compensation during crises. The UCT programme has never been more critical than it is now, particularly in light of crises like the COVID-19 pandemic, as it is relatively simple to administer and quick in mitigating the adverse effects of such crises (see Gentilini et al., 2022). This study aims to fill this gap in the literature by estimating the magnitude of effects on labour market behaviour and examining the transition across job types and sectors (i.e., labour mobility). Indonesia offers an excellent case study, particularly for investigating the effects of informality since the vast majority of workers are engaged in the informal economy. According to the National Labour Force Survey (*Sakernas*) 2020, about 55 percent of total workers and about 95 percent of self-employed workers work in the informal sector. Moreover, the informality of labour markets captures an important aspect of many developing countries—a condition that extends beyond unemployment to the point when individuals can no longer afford to remain jobless. The empirical literature investigating

[4] Contrary to welfare programmes in developing countries, beneficiaries in developed countries – for instance, the United States – may decide not to participate in the labour force altogether when benefits are sufficiently large (Meyer & Rosenbaum, 2001).

the effect of social benefits on the informality of labour markets is nearly absent, except for a few recent studies in Latin American countries. For instance, the study by Bosch and Campos-Vasquez (2014), using data at the municipality level in Mexico during the Seguro Popular (SP) programme, shows that the free health access programme generated a reallocation of workers away from formality.

2.2 Unconditional Cash-Transfer Programmes in Indonesia

Table 1: UCT Programmes in Indonesia

Assistance	Crisis	Target	Sample of Beneficiaries
BLT 2005	Reform of fuel subsidy programme due to rise in global oil prices starting in 2004	15.5 million households	IFLS 4 (2007/08) 6,875 individuals in 2,840 households (23.9% of household respondents)
BLT 2008	Crises in the international financial markets and basic food commodity coupled with rapid ascent in international fuel prices resulted in a domestic cut in fuel subsidy in 2008	19.0 million households	IFLS 5 (2014/15) 5,966 individuals in 2,322 households (18.2% of household respondents)
BLSM 2013	Further domestic cut in fuel subsidy due to shortage in domestic oil production causing a severe deficit in the trade balance in 2012/13	15.5 million households	IFLS 5 (2014/15) 4,584 individuals in 1,774 households (13.1% of household respondents)

Source: World Bank (2012) and Indonesia Family Life Survey, waves 3 (2000), 4 (2007/08), and 5 (2014/15)

Following a persistent rise in global oil prices starting in 2004, the government of Indonesia significantly reformed the fuel subsidy programme[5] in 2005 by removing the subsidies for industrial users and raising the regulated price of household gasoline and kerosene purchases. This reform resulted in an increase in fuel prices by an average of over 125 percent between 2004 and 2005. Further reduction in fuel subsidies also occurred in the second quarter of 2008, leading to an additional increase in gasoline and kerosene prices by 33 and 50 percent, respectively (World Bank, 2012). The government's decision to reduce fuel subsidies was motivated not only by the increase in the price of oil on the global market but also by the programme's distorted outcome, in which the programme's benefits were disproportionately distributed among higher-income households, large agricultural holdings, and commercial transport operators, rather than the poor households who were intended to receive them. In

[5] The share of expenditures allocated to subsidies increased by 16 percentage points between 2003 and 2005, reached nearly 30 percent of the government expenditure in 2005. The fuel subsidies alone, on average, contributed to 75 percent of all subsidy and transfer spending administered by the government.

addition, due to the wide discrepancy between subsidised and non-subsidised fuel prices, there were also many cases where subsidised fuels were smuggled abroad, which heavily cost the government. Conditions surrounding these policy changes would have been challenging for poor households since the rise in fuel prices inevitably caused prices for food and other essential items to spike. The food expenditure share of the poor households in Indonesia reaches over two-thirds of their budget; therefore, a significant price increase in the poor household food basket has serious consequences.

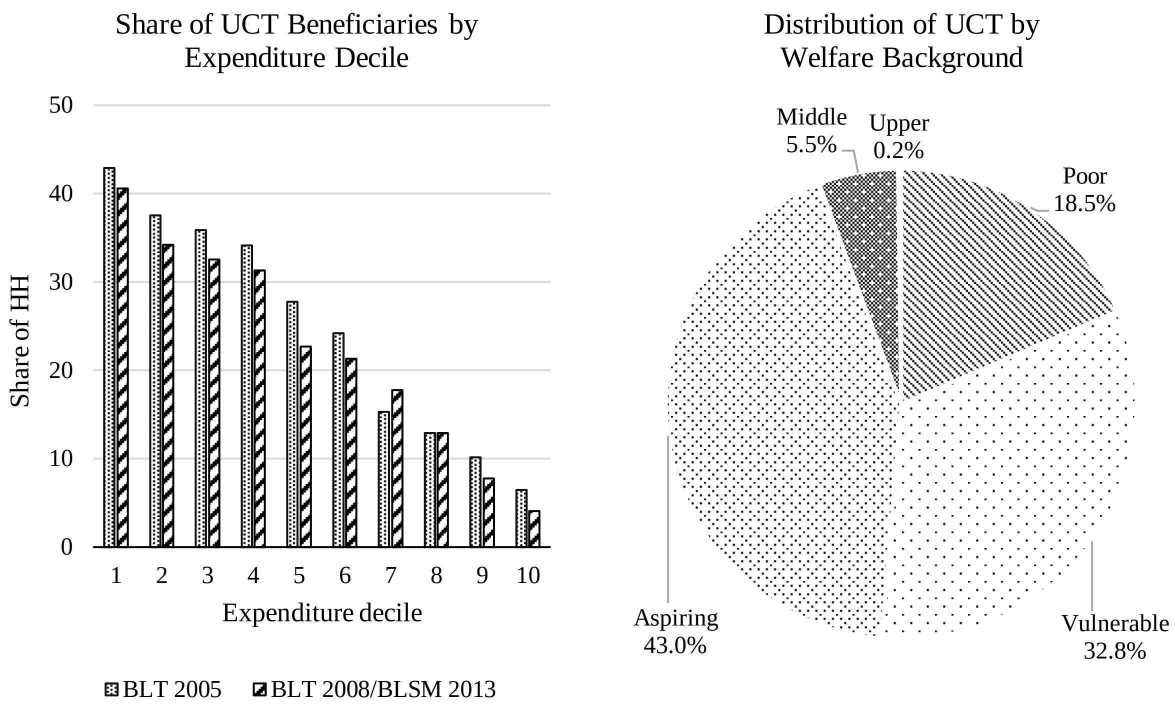

Figure 1: Coverage and Distribution of UCT

Source: Indonesia Family Life Survey, waves 3 (2000), 4 (2007/08), and 5 (2014/15)

In alleviating the adverse effects of domestic inflation, the government of Indonesia opted to reallocate a portion of fuel subsidy savings on an Unconditional Cash Transfer (UCT or *Bantuan Langsung Tunai*/BLT) to poor households[6]. The primary objective of the UCT programme is to supplement poor households in the bottom 30 percent of expenditure distribution with additional income to meet their needs due to the unprecedented price increases. UCT remains one of the largest cash transfer programmes in the world, providing emergency income support to nearly one-third of Indonesian households.[7] Government expenditure on

[6] BLT and BLSM was targeted to the poor households receiving the least benefit from previously implemented fuel subsidy programme and most vulnerable from the adverse effects on consumption from inflation. The basis for determining BLT and BLSM recipients is the result of the verification process of the targeted household (*Rumah Tangga Sasaran*/RTS) database determined by the Statistics Indonesia (BPS) through the Socio-economic Data Collection 2005.
[7] The households spread across all provinces in Indonesia, even in the most remote and geographically challenging regions.

UCT (BLT 2005 and BLT 2008) was substantial and accounted for more than Rp42 trillion, equivalent to over 50 percent of all household-targeted social assistance spending (World Bank, 2012). UCT was first distributed to the beneficiaries in October 2005 for the course of one year—long enough to enable households to adjust smoothly to the new price schedules. Around 15.5 million household beneficiaries received Rp1.2 million per household in total, delivered through post offices in four instalments of Rp300,000 payments. Furthermore, a closely comparable UCT was introduced again in 2008 due to crises in the international financial markets and food prices coupled with another domestic cut in fuel subsidies. However, the target base was expanded to 19 million household beneficiaries to also account for those who are just slightly above the poverty line. In 2008, there were three cash transfer instalments, in which each household in total received Rp900,000 over nine months. In 2013, the cash transfer was made available again, but it was provided for a few months under a different name (*Bantuan Langsung Sementara Masyarakat*/BLSM). On average, a household beneficiary received Rp100,000 per month, equivalent to around 15 percent of the average consumption expenditure of the target household (World Bank, 2012).

The basis for determining BLT recipients is the result of the verification process of the target household (*Rumah Tangga Sasaran*/RTS) database determined by the Statistics Indonesia (BPS) through the Socio-economic Data Collection 2005 (*Pendataan Sosial Ekonomi*/PSE 2005). PSE 2005 identified 14 non-monetary variables at the household level (see Appendix **Table A.1**) to use in measuring the well-being of households. The ranking of RTS was performed using the welfare index with local weights at municipality level (*kota/kabupaten*). Based on the welfare index, 19 million households were recorded in the PSE 2005 and ranked as (1) very poor, (2) poor, and (3) almost poor.[8] In 2008 and 2011, the government updated the database by conducting Data Collection for Social Protection Programs (*Pendataan Program Perlindungan Sosial*/PPLS). The ranking of RTS was carried out using the proxy means test (regression) model by involving not only household's but also individual's demographic and socio-economic characteristics, including information about individual employment, type of occupation, and work status (see Appendix **Table A.2**). Furthermore, as an effort to simultaneously update the database, the government also conducted public consultation at the community level (village/*kelurahan*), carried out prior to the data collection stage. This approach is often termed community-based targeting, which was achieved through

[8] The very poor and poor are the target base for BLT 2005, comprising of 15.5 million target household.

a Public Consultation Forum (*Forum Konsultasi Publik*/FKP) consisting of village chiefs, chairpersons of neighbourhoods and hamlets, and religious and community leaders.[9] Thus, there are two main phases to the data collection process. In the first stage, the lowest RTS (pre-printed) list is verified with a negative list to eliminate inclusion errors. Next, sweeps for new RTS are performed to include exclusion errors in the list of RTS, which will then be verified. In the second stage, the nominated RTS were enumerated by updating household variables and collecting basic demographic and individual socioeconomic information for all household members.

UCT programme is regarded to be able to appropriately mitigate severe consumption crises since it allows for rapid scale-up and distribution. Cash transfer is relatively easy to administer and can improve households' welfare by allowing them to have the freedom to spend the money received. As portrayed in **Figure 1**, the bottom 30 percent of households with the lowest expenditure (i.e., the poor and vulnerable) received approximately 50 percent of total UCT transfers. Only about 5 percent of UCT recipients were from the middle or upper socio-economic classes. This distribution contrasts with the old fuel subsidy regime, in which the wealthiest 40 percent of the population captures more than two-thirds of the total benefit (World Bank, 2012). However, regarding UCT coverage, only 30 to 40 percent of the poor and vulnerable received the UCT programme, indicating that a large share of eligible households did not receive it. Based on these stylised facts, it appears that the problem of exclusion error is more severe than the inclusion error. Variation in targeting performance is also observed, primarily due to complications of targeting in urban areas, discrepancy across regions in the capacity of programme socialisation and local government supervision of targeting, and varying local norms of dispute avoidance or sharing.

3 Data

The primary data for this study comes from the Indonesia Family Life Survey (IFLS) waves 3, 4, and 5, which were conducted in 2000, 2007, and 2014, respectively. IFLS is a nationally-representative survey collecting information for the same individuals over time, offering a rich panel that allows for dynamic analysis of behaviour at the individual, household,

[9] The argument for relying on community-provided data is that locals may have more up-to-date, dynamic welfare information about others than a centralized program implementer which will help in better targeting the beneficiaries. Alatas et al. (2012) find that the community method led to greater community satisfaction and better selected households that self-identify as poor. Although Trachtman et al. (2022) find community members' information sets are fairly concordant to the proxy-means test scores, community-based targeting methods are less useful in identifying acute short-term distress

and community levels. IFLS wave 1, conducted in 1993, covers13 of Indonesia's26 provinces and 83 percent of the population.[10] The following waves then track the same sample every seven years, with an average recontact rate of around 93.8 percent. Relevant for this study, IFLS provides a wealth of socioeconomic socio-economic information, including schooling, housing, social transfer receipts, and o labour market outcomes and history.

IFLS has data on past employment, status and conditions, as individual retrospective responses for each year at least until the previous survey (e.g., the 2014 survey collects historical employment data for each year from 2007 to 2014). The yearly retrospective employment summaries are based on primary job, recording the job status with the longest duration for that year if a responder was employed and unemployed in the same year. Although the information collected for the yearly retrospective employment summary is not as detailed as for the survey year[11], it is preferred here for a few reasons. First, it provides more frequent data points. Second, the questions for employment in the survey year mainly ask about primary activity during the *past week*[12], which does not adequately capture the breadth of an individual's employment condition. While long-span historical data collection has a certain degree of recall inaccuracy, IFLS allows us to partly check the accuracy of respondents' recall of past information by comparing their responses from a survey wave with the ones from the previous wave[13] and there are no statistically significant discrepancies (see Appendix **Table A.3**). Table

The dependent variables of the analysis are categorised into two groups: (1) employment and job formality, and (2) job mobility. Employment is a dummy variable that takes a value of 1 if the individual is self-reported being employed in a particular year, and 0 otherwise.[14] Job formality is a dummy variable with a value of 1 when someone is government, private sector worker, or self-employed with permanent worker in non-agricultural sector, and 0 otherwise. Although a common definition of the informal sector refers to economic activities that occur outside the legal rules and institutions of a country, this approach may not be suitable in

[10] Specifically, it includes the four provinces in Sumatra (North Sumatra, West Sumatra, South Sumatra, and Lampung), all five provinces in Java (DKI Jakarta, West Java, Central Java, DI Yogyakarta, and East Java), and four provinces covering the remaining major island groups (Bali, West Nusa Tenggara, South Kalimantan, and South Sulawesi).

[11] The yearly retrospective summary of employment only covers questions on the employment status (working or not working), name of the company or employer, what the company produces, what the respondent's main duty, the category that best describe the work that the respondent did, and whether or not the respondent has secondary jobs.

[12] Although this is the standard definition to measure employment or labour force participation, many have argued that such measurement is very restrictive as it is unable to capture the situation of labour underutilisation (U.S. Bureau of Labor Statistics, 2022).

[13] For example, the retrospective response for the year 2000 (2007) is collected in survey wave 3 (4), and is also recorded in survey wave 4 (5).

[14] The retrospective questionnaire asks the respondents "Do you work in [Year]?". If a respondent was both unemployed and employed in the same year, IFLS will record the status that lasted the longest.

developing countries where business activity records and legal compliance are lacking. Another alternative definition, and one used in Indonesia, is based on employment status—a proxy for income security and employment benefit (World Bank, 2010). The country's official definition of job formality combines work status and main occupation types (Appendix **Table A.4**). When data lacks information about main occupation types, the simplified definition is used (Appendix **Table A.5**), combining work status and agriculture/non-agriculture sector classification. I use the simplified definition here, as IFLS retrospective data omit the type of main occupations, employment benefits, or the number of workers.

I estimate job mobility effects based on employment and job formality values in the base year, defined as the year before the UCT programmes were introduced. Baseline years determine sub samples of impact analysis according to the employment and job formality status prior to the UCT programmes. I examine four types of mobility: (1) job finding, (2) job exit, (3) move into formality, and (4) move out of formality (see Appendix **Table A.6** for further variable details). The independent variables of interest are whether an individual in the household is receiving (1) BLT 2005 or (2) BLT 2008/BLSM 2013. Wave 4 asks if households received BLT 2005, while wave 5 asks about BLT 2008 and BLSM 2013. For BLT 2008/BLSM 2013, treatment assignment follows a staggered approach where about two-thirds of BLSM 2013 beneficiaries received BLT 2008 first. Hence, if an individual received BLT 2008 and BLSM 2013, year 2008 and all periods after that are considered treated. Meanwhile, if an individual only received BLSM 2013, only 2013 and 2014 are treated.

I restrict my estimation sample to individuals aged at least 15 years[15] interviewed in the employment module of IFLS which remain in the common support. Appendix **Table A.7** provides summary statistics for the variables included in the analysis using this sample. Using annual retrospective data from IFLS waves 3, 4 and 5, I construct annual individual-level panel dataset spanning over 14 years. IFLS wave 3 collects annual retrospective data from 1996 to 2000, wave 4 collects annual retrospective data from 1999 to 2007, and wave 5 collects annual retrospective data from 2007 to 2014. In the case of overlap, the data is taken from the survey wave closer to the retrospective years to minimise errors due to individual recall of employment details.[16] The effects of BLT 2005 are examined using 2000-2007 and BLT 2008/BLSM 2013 using 2007-2014 .

[15] This sampling strategy adheres to the ILO's 1973 Minimum Age Convention, stating that the overall minimum age for job admittance is 15 years old.

[16] Annual retrospective data for year 1999 and 2000 is picked from IFLS 3 (rather than IFLS 4), and data for year 2007 is picked from IFLS 4 (rather than IFLS 5). However, as shown in Table 2, the share of matched response is relatively high indicating good quality of retrospective response; hence, concerns about recall error tend to be minor.

4 Empirical Strategy

Indonesia's UCTs target poor households, mainly the bottom 30 percent. In practice, however, targeting was highly imperfect (i.e., some non-eligible households received the assistance and those eligible did not). If targeting errors were random, beneficiary and non-beneficiary households should not be indistinguishable and identification using standard DD approaches exploiting this targeting error may be appropriate. Appendix **Table A.8** compares socio-economic characteristics of treatment and control groups in the baseline years, indicating discrepancies potentially affecting treatment assignment. To adjust for this imbalance, I estimate the effects of UCT using PSM DD, accounting for the main correlates of treatment assignment.

The PSM DD approach takes advantage of the matching method to create a comparison group based on the probability of treatment before estimating the DD impact. Thus, a combined PSM DD takes into account observable pre-programme characteristics and then estimates effects only on units that remain in common support. As social assistance programmes often target poor households with a low level of education, low quality housing, and other characteristics that are apt to be correlated with poverty, changes in the outcome over time may be a function of groups' initial differences, a concern the combined PSM DD approach helps alleviate.

To ensure effective propensity scoring, it is necessary to use baseline variables that influence targeting. The treatment variable in the post-treatment period is regressed on a set of explanatory variables in the pre-treatment period to generate propensity scores from the baseline variables at both the household and community levels. The propensity score matching method used is single nearest-neighbour matching with replacement. The standardised bias across covariates after matching is lower after matching, and the variance ratios of the individual variables are all close to one. Additionally, there are significant masses of both groups across the common support—where distributions of the propensity scores for treatment and comparison group overlap. See Appendix **Tables A.8, A.9,** and **A.10** and **Figures A.1** and **A.2** for matching diagnostics.

My primary specification is:

$$Y_{i,t} = \alpha + \beta T_{i,t} + \gamma t + m_i + \varepsilon_{it}$$

where $Y_{i,t}$ is the outcome variable for individual i in period t. The average treatment effect on the treated (ATT)[17] is given by the coefficient β of the treatment variable ($T_{i,t}$), a dummy equal to 1 if the individual i receives UCT and is in the t period. t and m_i are time and individual fixed effects. Once treatment for a unit begins, the unit remain in treatment for the rest of the period.[18] Thus, the control group is individuals in households that never and have not yet received UCT. ATT is estimated using a fixed-effect (within) estimator at the individual level. To examine how changes in model specification affect estimated values, regressions are also run without and with the interaction of year and region[19] fixed effects (i.e., year-region fixed effects) to account for region-specific shocks. Standard errors are clustered at the household level (Abadie et al., 2017), and at the community level[20] as a robustness check against potential spatial correlation.

I also use an event study to investigate dynamic year-by-year-specific effects, with the same sample and PSM DD approach, augmented with lags and leads. Lags can reveal whether the effect of a policy is permanent or transitory and how long it lasts, and leads allow for a transparent examination of pre-trends. The estimating equation for the event study approach is thus:

$$Y_{i,t} = \alpha + \sum_{m=2}^{M} \eta_m T_{i,t-m} + \sum_{s=0}^{S} \eta_s T_{i,t+s} + \gamma t + m_i + \varepsilon_{it}$$

η_m quantifies the impacts that occurred m periods before the programme was put into place (i.e., leads of the treatment variable), while η_s captures any effects following the programme that occur s periods after the implementation (i.e., lags of the treatment variable); and $t = -1$ is the base period against which all effects are compared. To ensure that the parallel trend assumption is satisfied, η_m must not be statistically different from zero, indicating no difference between the two groups prior to treatment.

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[17] Estimates based on DD are interpreted as average treatment effect on the treated (ATT), rather than average treatment effect (ATE). This is due to the fact that DD estimates are often considered as referring to a subset of the population that was actually treated (as opposed to the population as a whole).

[18] Using this approach means that I do not account for "switchers" (i.e., the previously treated units) in the specification; hence, potential problem regarding the "forbidden comparisons" should not be of concern.

[19] Region is a categorical variable with a value of 1 if observation's regional location is Java, and 0 otherwise.

[20] Community level is defined at the enumeration area, which is at village/*kelurahan* level. This is consistent with the targeting and distribution of UCT, which involves village/*kelurahan* officials.

5 Main Results

5.1 Effects on Employment and Job Formality

Table 2: Effects of UCT on Employment and Job Formality

	(1)	(2)
	Employment	Job formality
A. BLT 2005		
ATT	-0.002	0.008
	(0.007)	(0.006)
	[0.007]	[0.006]
Individual fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Mean outcome	0.90	0.26
No. of observations	67,532	67,532
No. of groups (individual)	8,455	8,455
B. BLT 2008/BLSM 2013		
ATT	-0.011*	-0.004
	(0.006)	(0.009)
	[0.007]	[0.010]
Individual fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Mean outcome	0.92	0.26
No. of observations	63,892	63,892
No. of groups (individual)	8,031	8,031

Note: Estimation of ATT is based on generalised PSM DD using a fixed-effect estimator at the individual level. The PSM method used is single nearest-neighbour matching with replacement. Standard errors clustered at the household level are in parentheses, and those clustered at the community level are in square brackets. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively, with standard errors clustered at the household level.

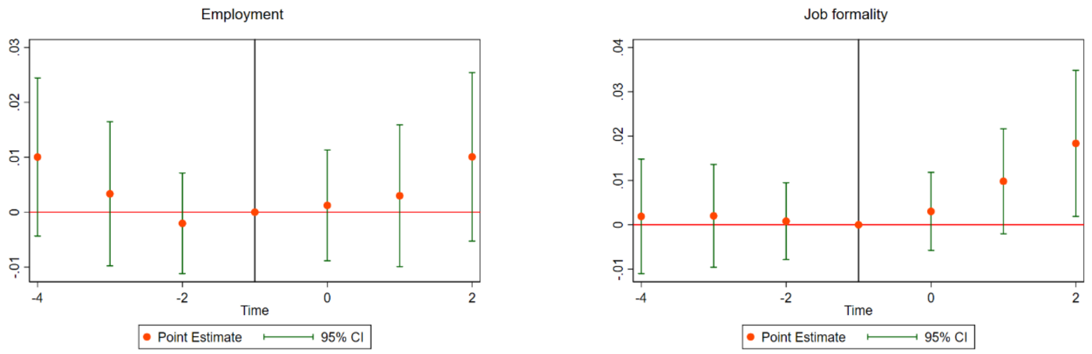
A reduction of labour supply may be the optimal response for an individual receiving UCT since income and substitution effects operate in the same negative direction (Borjas, 2016). An income effect induces the individual to consume more leisure, reducing work. The substitution effect is induced by the tax rate on labour earnings, which reduces the price of leisure; consequently, the UCT recipient will demand even more leisure and further lower the labour supply. Since the retrospective questionnaire did not collect data on work hours, I focus on the effects of the UCTs on labour supply in terms of extensive margin employment and job formality.[21] The effect on employment will mostly capture the income effect, while the effect on job formality will capture both income and substitution effects. In the case of Indonesia, since only those with formal employment are required to make income tax payments, people may be encouraged to continue working in the informal sector due to the substitution effect caused by the tax rate on labour earnings. Unemployment benefits are also minimal, and most

[21] Nevertheless, data on work hours is collected only in the main employment survey across three waves of the IFLS. As a robustness check, I also estimate the effect of UCT using work hours as the outcome variable (the result is presented in the Appendix **Table A.12**) and find no significant evidence of effect on work hours.

people cannot afford being out of work. Instead, they tend to maintain work in the informal sector. The effect on job formality is thus expected to be larger in magnitude than the effect on employment, as would capture both the income and substitution effects.

Table 2 presents the main results. Receiving UCTs generally has no statistically significant effect on employment and job formality, and the estimates coefficients on the treatment indicator across the different UCT types are statistically indistinguishable from each other. The BLT 2008/BLSM 2013 effect is, however, negative and significant only at the 10 percent level. These results, though different from the traditional theoretical prediction, are consistent with earlier research showing minimal employment effects in developing countries (see Foguel & Barros, 2010; Galasso, 2006; Maluccio & Flores, 2005; Parker & Skoufias, 2000; Skoufias & Di Maro, 2008). Since the UCT programme is only meant to be a short-term fiscal stimulus in times of crisis and is largely tied to recovery, the absence of any change in recipient's overall labour behaviour is unsurprising.

A. BLT 2005



B. BLT 2008/BLSM 2013

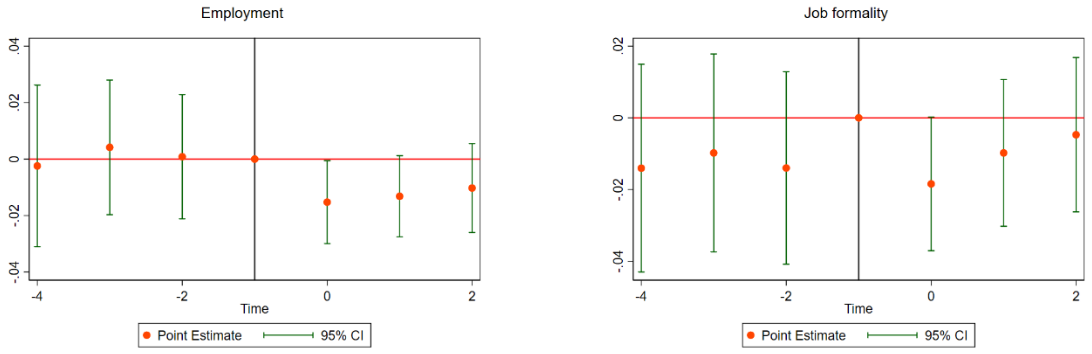


Figure 2: Event Study of Effects of UCT on Employment and Job Formality

Note: Event study approach is estimated using a fixed-effect estimator at the individual level with 4 leads and 2 lags in the specification, which also controls for year and individual fixed effects. Standard errors are cluster-robust at the household level.

The average effects in **Table 3** may mask different effects over time. **Figure *2*** presents the findings based on the event study approach. For BLT 2005 (Panel A), I find no evidence of an impact on employment, not even in the first year of roll out. Nevertheless, BLT 2005 recipients demonstrated a higher likelihood of holding a formal job two periods after the programme, with an increase of about 2 percentage points (8 percent increase from the mean). The estimated effect is statistically significant at conventional levels, despite larger confidence intervals. According to previous research, the increase in local resources resulting from UCT payments may potentially have multiplier effects on the local economy, hence increasing labour demand (Egger et al., 2022; Neri et al., 2013). Similarly, UCTs could alleviate beneficiaries' liquidity constraints in their job search, resulting in a positive income effect on their formal labour supply (Baird et al., 2018). The effect takes a little while to manifest, which is consistent with multiplier effects from the expansion of labour demand brought on by the increased spending of resources in the local economy (e.g., firms may wait to make staffing adjustments until a sustained uptick in demand becomes apparent) and with increases in formal labour supply (e.g., investments in job search may take time to yield returns) (Gerard et al., 2021).

On the other hand, for BLT 2008/BLSM2013 (Panel B), I observe a negative jump in the effects of UCT on employment and job formality in the first post-baseline year. The estimated effects are about 2 percentage point decreases in the likelihood of being employed and having a formal employment in the period when the UCT was initiated (2 and 8 percent decrease from the mean outcome, respectively). These effects then gradually disappear over the following periods, returning to pre-programme levels as early as one year after the UCT was introduced. This demonstrates that the immediate disincentive effects of UCT tends to be transitory, consistent with the modest transfers in crisis times and the existence of both income and substitution effects among beneficiaries. Recent evidence from Latin America shows that social assistance with means testing can indeed distort beneficiaries' incentives to work, especially in the formal economy where earnings are more easily verifiable (Bergolo & Cruces, 2021; Levy, 2010). Importantly, the event studies show no signs of pre-trends across the two UCT settings, for employment and job formality, thus providing no evidence of any major anticipatory effects.

5.2 Effects on Job Mobility

Table 3 presents the key findings on job mobility, a heterogeneity analysis based on initial job status before the UCT programme was launched. There are four types of job mobility:

job finding (Column 1), job exit (Column 2), move into formality (Column 3), and move out of formality (Column 4).

Table 3: Effects of UCT on Job Mobility

	(1)	(2)	(3)	(4)
	Job finding	Job exit	Move into formality	Move out of formality
A. BLT 2005				
ATT	-0.024	-0.002	0.012**	-0.019
	(0.047)	(0.006)	(0.006)	(0.023)
	[0.048]	[0.006]	[0.006]	[0.025]
Individual fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Mean outcome	0.34	0.05	0.04	0.15
No. of observations	7,323	62,627	50,378	19,309
No. of group (individual)	922	7,837	6,308	2,417
B. BLT 2008/BLSM 2013				
ATT	-0.042	0.009	-0.023**	0.033
	(0.029)	(0.006)	(0.009)	(0.025)
	[0.031]	[0.007]	[0.010]	[0.031]
Individual fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Mean outcome	0.55	0.08	0.15	0.33
No. of observations	8,738	63,431	53,360	22,943
No. of group (individual)	1,102	7,973	6,707	2,884

Note: Estimation of ATT is based on generalised PSM DD using a fixed-effect estimator at the individual level. The PSM method used is single nearest-neighbour matching with replacement. Standard errors clustered at the household level are in parentheses, and those clustered at the community level are in square brackets. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively, with standard errors clustered at the household level.

Comparing the effects of BLT 2005 and BLT 2008/BLSM2013, the results on job mobility are mixed.**Table *3*** On the one hand, there is no evidence of a significant difference between BLT 2005 and BLT 2008/BLSM 2013 regarding employment mobility (i.e., job finding and job exit). Both settings show no evidence of any effect on job finding and exit. On the other hand, the effect on transitioning into formality among those who were previously not in formal employment vary across the two settings. For BLT 2005, beneficiaries are more likely to move into formality, with an effect of around 1.2 percentage points—a 30 percent increase from the mean—statistically significant at conventional levels. Meanwhile, BLT 2008/BLSM 2013's beneficiaries exhibit statistically significant evidence of a reduced likelihood of moving into formality, with an effect of 2.3 percentage points, or a 15 percent decrease from the mean. Nevertheless, there is no evidence of any effect on transitioning out of formality, suggesting that the lower likelihood of formal employment in **Figure 2** (Panel B) is likely driven by the disincentive to move into formal economy, rather than individuals leaving formal positions.

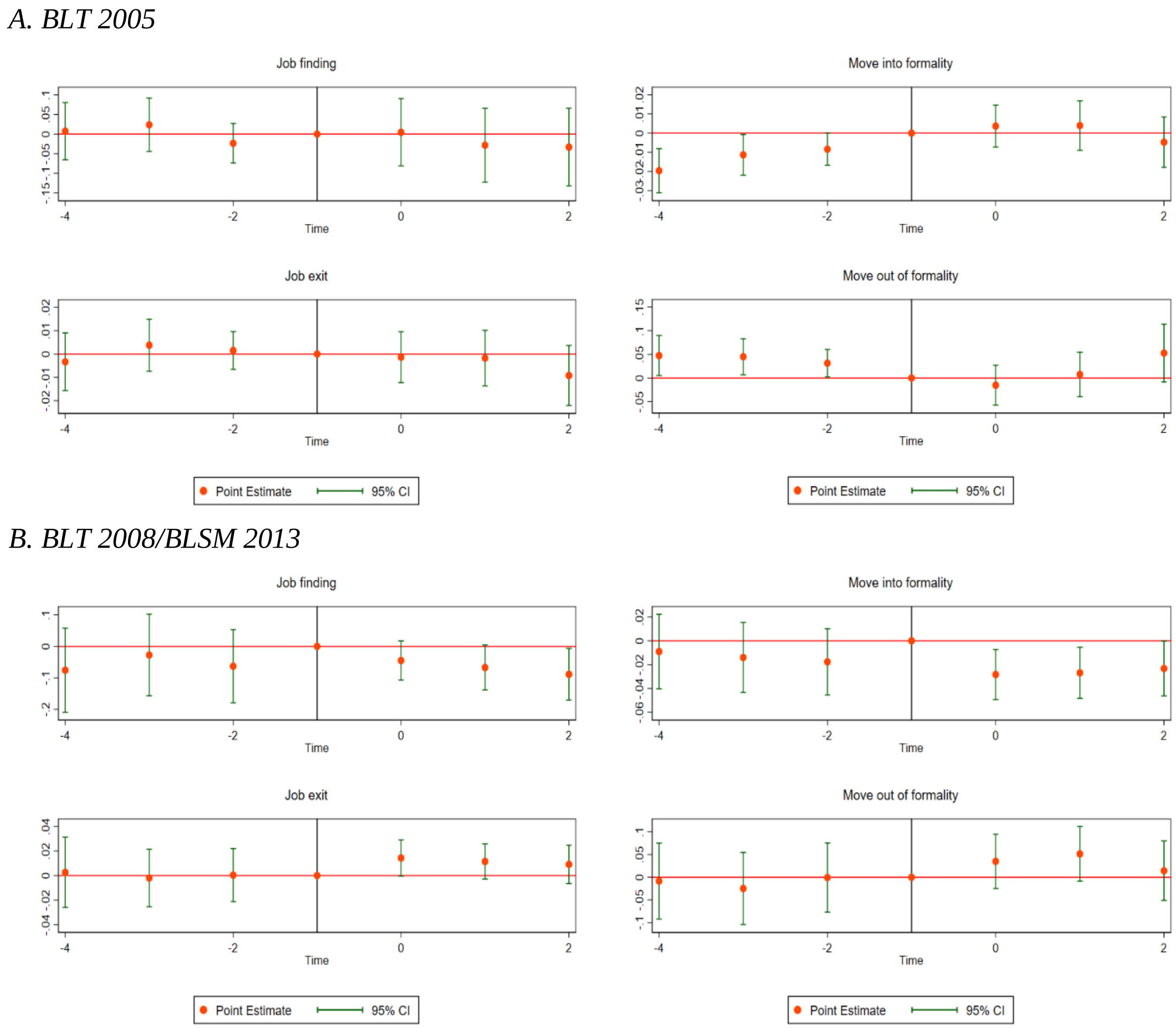


Figure 3: Event Study of Effects of UCT on Job Mobility

Note: Event study approach is estimated using a fixed-effect estimator at the individual level with 4 leads and 2 lags in the specification, which also controls for year and individual fixed effects. Standard errors are cluster-robust at the household level.

Figure 3 illustrates the job mobility findings using event studies, further revealing heterogenous effects across settings. For BLT 2005 (Panel A), I find no evidence of any effect on job mobility in terms of employment (i.e., job finding and job exit) and job formality (i.e., move into and move out of formality). For BLT 2008/BLSM2013 (Panel B), there is a modest increase in the probability of job exit among individuals who were previously employed, especially when the UCT programmes were introduced. The immediate effect is around 2 percentage points (25 percent increase from the mean), which reverts to pre-programme level at least a year later. Recipients of BLT 2008/BLSM 2013 also exhibit a decrease in the likelihood of job finding by 9 percentage points (16 percent decrease from the mean outcome) two years after receiving UCT payment. The estimated effect is statistically significant at conventional levels. ***Figure 2*** At the same time, recipients of the BLT 2008/BLSM 2013 are less likely to transition into formality, with an immediate effect of about 2.8 percentage points

(19 percent decrease from the mean). This effect, although diminishing, persists for at least two periods after receiving the UCT. In terms of transitioning out of formality, there is evidence of an increased likelihood by 5 percentage points (15 percent increase from the mean), with a year lag. The lagged effects for job finding and moving out of formality suggest the possibility of job status "stickiness", particularly for those who are previously unemployed or work in the formal sector (Pissarides, 2009, 2011).

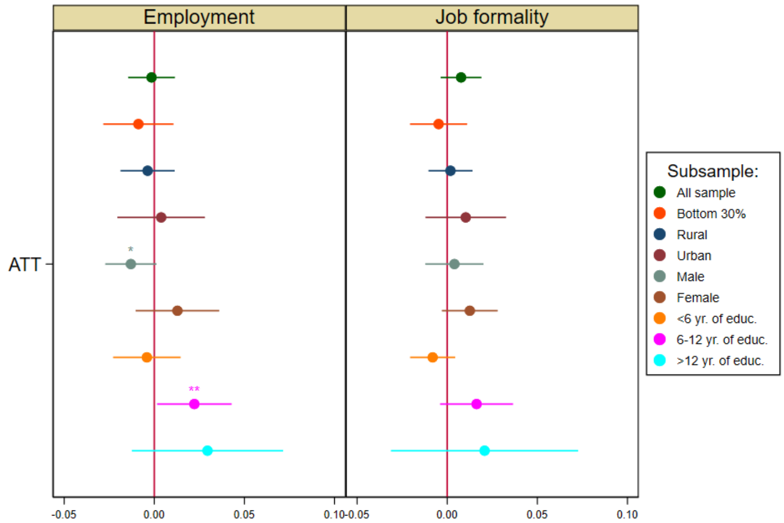
Although the results of the event study in **Figure 3** seem to deviate from the estimated ATT in **Table 3**, this variation is easily explained. The estimated ATT of BLT 2005 (**Table 3**, Panel A, Column 3) shows a higher likelihood of transitioning into formality unsupported by the event study (**Figure 3**, Panel A). Instead, the event study shows visual signs of a potential pre-trend. The presence of statistically significant effects prior to the UCT programme—though progressively disappear as it moves closer to the baseline period—may violate the identification assumption required for a causal interpretation. Thus, even though the result of the ATT effect is statistically significant at conventional levels, it seems likely to be biased due to confounders arguably better captured by the richer dynamic specification. Results are also mixed on job finding and exit. As observed in **Figure 3** (Panel B), the event study indicates a negative impact on job finding and a positive impact on job exit as a result of BLT 2008/BLSM 2013 while the estimated ATT (**Table 3**, Panel A, Column 1 and 2) reveals no such impact. Clearly, the estimated ATT masks period-by-period effects.

6 Heterogeneous treatment effects

I now ask which individuals were most affected by the UCT programmes, examining variability across different individual sub-samples: household expenditure distribution (individuals in the bottom 30 percent), location of residence (rural and urban), gender (male and female), and years of education (with less than 6, between 6 and 12, and above 12 years of education).

Figure 4 presents the heterogenous effects of UCT on employment and job formality by sub-sample (note these are of varying sizes, and thus varying power and precision). Across the two UCT settings, I find no evidence of a disincentive effect of UCT on employment and job formality when only using the bottom 30 percent sample. Furthermore, there are no discernible differences by location of or gender (there is, however, weak evidence at the 10 percent level that BLT 2005 and BLT 2008/BLSM 2013 reduces the likelihood of employment among males and urban individuals). Figure 4 reveals much stronger effects based on years of education for UCTs. Specifically, individuals with 6-12 years of education receiving BLT 2005 and BLT 2008/BLSM 2013 exhibit a much higher likelihood of being in employment or a formal job, significant at 5 and 10 percent levels. Thus, the disincentive effects on employment and job formality from BLT 2008/BLSM 2013 appear mostly driven by individuals with fewer than 6 years of education. The negative effects are about 2 and 3 percentage points, statistically significant at conventional levels, with a larger relative effect for job formality.

A. BLT 2005



B. BLT 2008/BLSM 2013

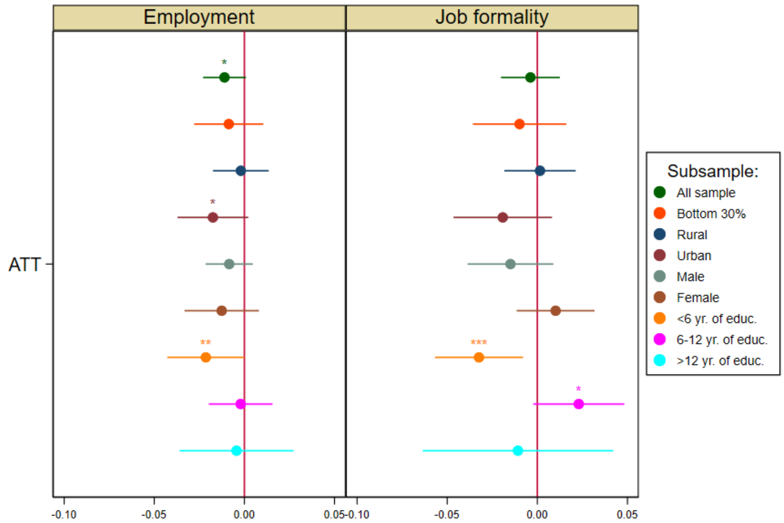
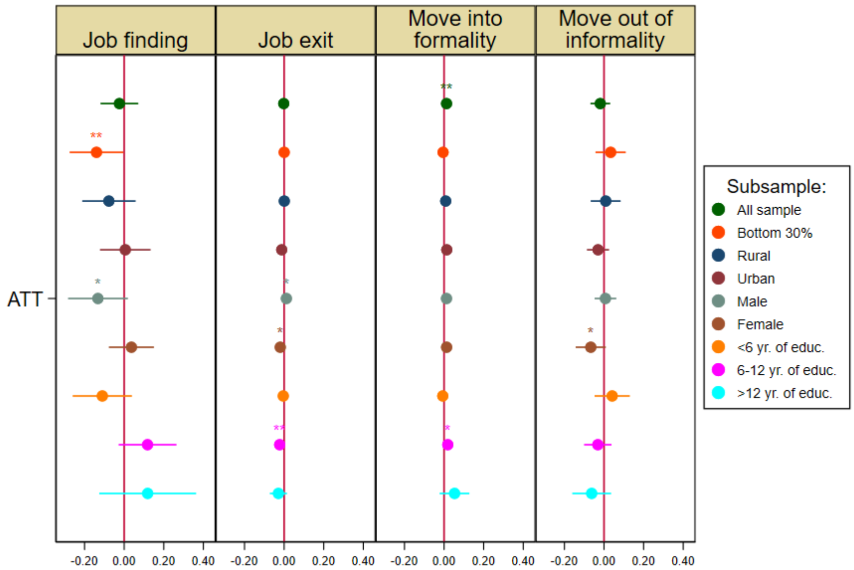


Figure 4: Heterogeneous Effects of UCT on Employment and Job Formality

Note: PSM DD approach is estimated using a fixed-effect estimator, which also controls for year and individual fixed effects. Standard errors are cluster-robust at the household level. The dot represents the point estimate of the coefficient, and the line represents the 95% confidence interval. Above the point estimates, ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively, with standard errors clustered at the household level.

A. BLT 2005



B. BLT 2008/BLSM 2013

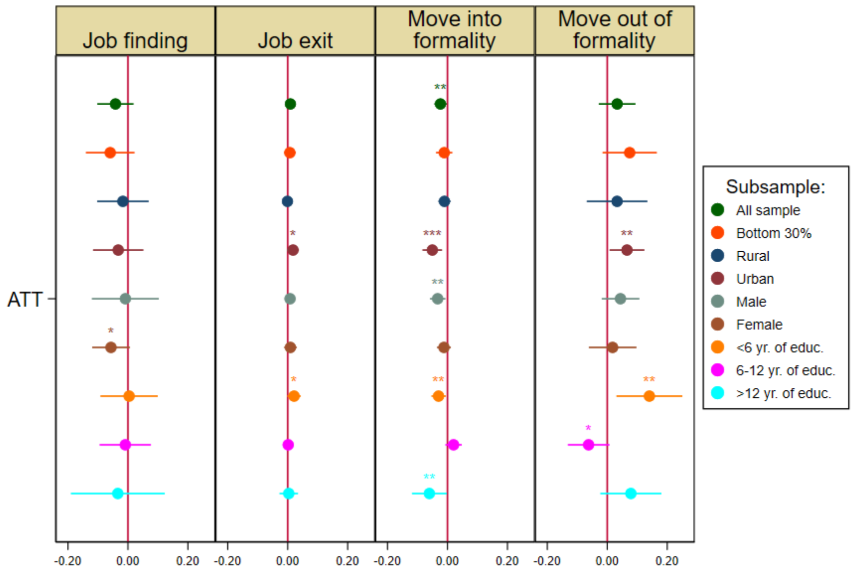


Figure 5: Heterogenous Effects of UCT on Job Mobility

Note: PSM DD approach is estimated using a fixed-effect estimator, which also controls for year and individual fixed effects. Standard errors are cluster-robust at the household level. The dot represents the point estimate of the coefficient, and the line represents the 95% confidence interval. Above the point estimates, ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively, with standard errors clustered at the household level.

Figure 5 presents the heterogenous effects for job mobility. Among the previously unemployed (in the base year), BLT 2005 reduces the likelihood of job finding by 14 percentage points for individuals in the bottom 30 percent. A lower likelihood of job finding is also found among males, with a magnitude of 13 percentage points (though the effects are only significant at the 10 percent level). Beneficiaries of BLT 2005 with 6-12 years of education have a slightly lower likelihood of leaving their jobs, with the effect magnitude of 2 percentage points and statistically significant at 5 percent level. For BLT 2008/BLSM 2013, I find that the decrease in the likelihood of moving into formality is mainly driven by urban residents, males, individuals with less than 6 years of education, and those with more than 12 years of education. The effects for these subgroups are about 3 to 6 percentage points, statistically significant at conventional levels. When looking at job mobility among individuals with formal occupations in the base year, BLT 2008/BLSM 2013 is found to increase the likelihood of transitioning out of formality, particularly among urban dwellers and those with less than 6 years of education.

7 Further robustness

The event studies in Section 5.2 show no major pre-event trends, indicating that policy change generally appears unrelated with the outcome before it occurs (except for mobility into and out of formality). This section provides an additional empirical investigation of parallel trends not only on the basis of treatment assignment (**Figure 6**) but on the specific timing of the UCT's roll out (**Figure 7**). The latter may be particularly important here as for BLT 2008/BLSM 2013 estimation follows a staggered approach.

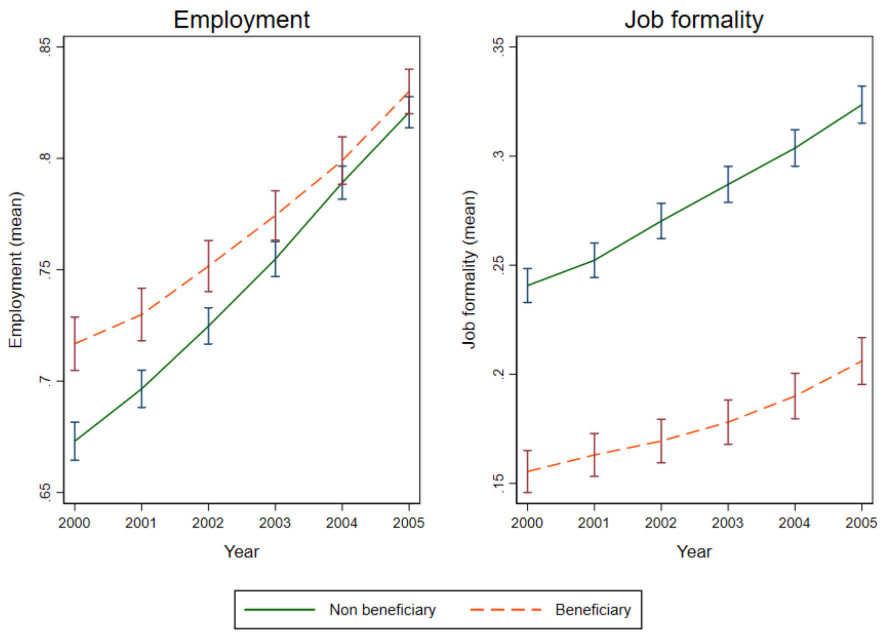


Figure 6: Trends of Outcomes Pre-UCT Programme, by Beneficiary Status

Source: Author's calculation based on Indonesia Family Life Survey (IFLS).

Similar to the standard DD estimation, the critical identifying assumption of a PSM DD estimation is the assumption of parallel trend, in which the unobserved characteristics affecting programme participation do not vary over time with treatment status. As a partial test of the parallel trends underlying the identification strategy, **Figure 6** shows the pre-programme trends for each average value of the outcome variables over the periods before the UCT programme was first introduced in 2005. Although visually, there is a slight difference in the slopes when examining only the trend of the mean values, these differences are not statistically significant. Overall, labour outcome trends before the programme implementation do not statistically differ by treatment assignment. Hence, any difference post-programme can likely be attributed to the programme after accounting for the pre-existing differences between the treatment and control groups.

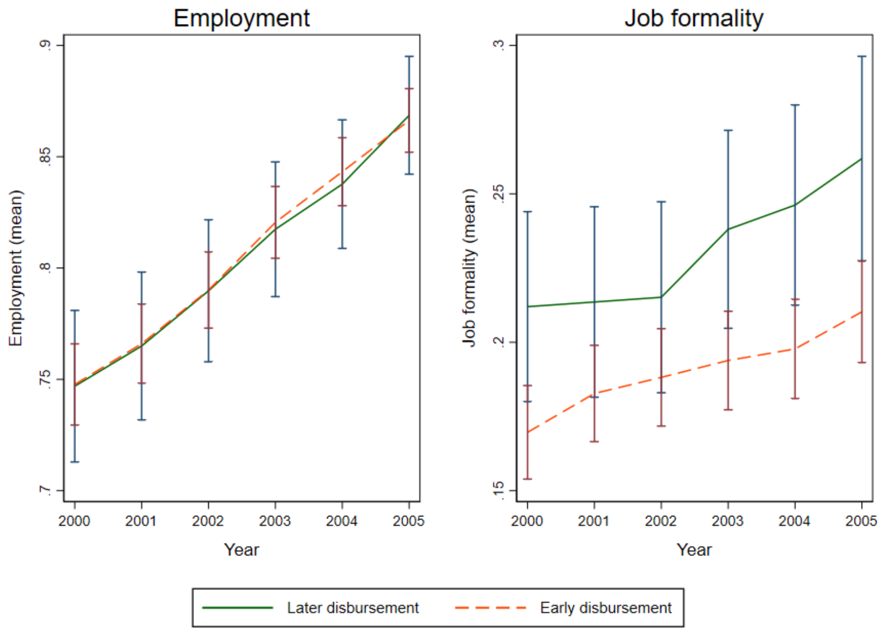


Figure 7: Trends of Outcomes Pre-UCT Programme, by Roll-out Timing

Source: Author's calculation based on Indonesia Family Life Survey (IFLS).

Figure *6* Instead of comparing pre-trends based on treatment assignments, Figure 6 compares trends for different roll-out timings among UCT beneficiaries. For BLT 2008/BLSM 2013, early disbursement recipients are people who get BLT 2008 or BLT 2008/BLSM 2013. However, if they only receive BLSM 2013, they will be categorized as beneficiaries with later disbursement. Differences in the slope of trend plots across the two groups may indicate time-varying unobserved heterogeneity amongst beneficiaries with differing initial times of transfer receipt, which might confound estimates. **Figure 7** shows no statistically significant differences in pre-programme trends (and mean values) of labour outcomes between individuals who received the disbursement earlier and those who received it later. Thus, the introduction of the UCT programme, as well as the staggered timing of its implementation, could be considered plausibly exogenous, allowing a causal interpretation.

8 Discussion and conclusion

8.1 Summary

Using panel datasets from IFLS waves 3, 4, and 5, I examine the effects of UCTs on employment, job formality, and mobility estimated using a blended PSM DD approach. I look at two distinct UCT contexts, BLT 2005, provided following the fuel subsidy reform in 2004 where targeting was based on a simplified proxy-means test, and BLT 2008/BLSM 2013, aimed at alleviating the negative effects of crises in the international financial markets in 2008 and severe deficit in the trade balance in 2012/13 where targeting used an extensive set of indicators involving community recommendation. In addition to estimating the average effects following receipt, I estimate period-by-period effects to comprehensively evaluate dynamics.

First, in terms of the impact on employment and job formality, the primary findings show that receiving UCT has no statistically significant impact on these outcomes for either BLT 2005 or BLT 2008/BLSM 2013 (although the 2008/2013 BLT effect is negative and significant at the 10 percent level, the finding is no longer significant when the standard error is clustered at the community level). However, when the findings are examined further using the event study, the results are mixed. Among those who received the BLT 2005, I find evidence of a higher likelihood of holding a formal job, by 2 percentage points, with a two-year lag. On the other hand, for beneficiaries of BLT 2008/BLSM2013, I find evidence of a negative effect on the likelihood of employment and job formality, especially during the period when the programme was introduced. The estimated effects are, on average, about 2 percentage points and mainly driven by individuals with less than 6 years of education.

Second, the results on job mobility (i.e., effect heterogeneity on employment and job formality based on status in the base year) are also mixed. Beneficiaries of BLT 2005 are more likely to move into formality, with an effect of around 1.2 percentage points, which is statistically significant at the conventional levels. However, the event study results suggest that there may be pre-trends that violate the identification assumption, so it cannot be used to confidently draw conclusions about the increased likelihood of entering formality. Beneficiaries of BLT 2008/BLSM 2013, on the other hand, show statistically significant evidence of a lower likelihood of moving into formality, with a 2.3 percentage point effect. These findings are consistent with the event study and appear to be persistent and lasting for at least two periods following UCT administration. The effects on these job formality transitions are mainly driven by urban dwellers and low skilled individuals. Aside from that, there is a slight increase in the likelihood of job exit among previously employed individuals. The immediate effect is around 2 percentage points, which returns to pre-programme levels after at least a year. Recipients of BLT 2008/BLSM 2013 also experience a 9-percentage-point decrease in the likelihood of finding work, especially two years after receiving the UCT payment.

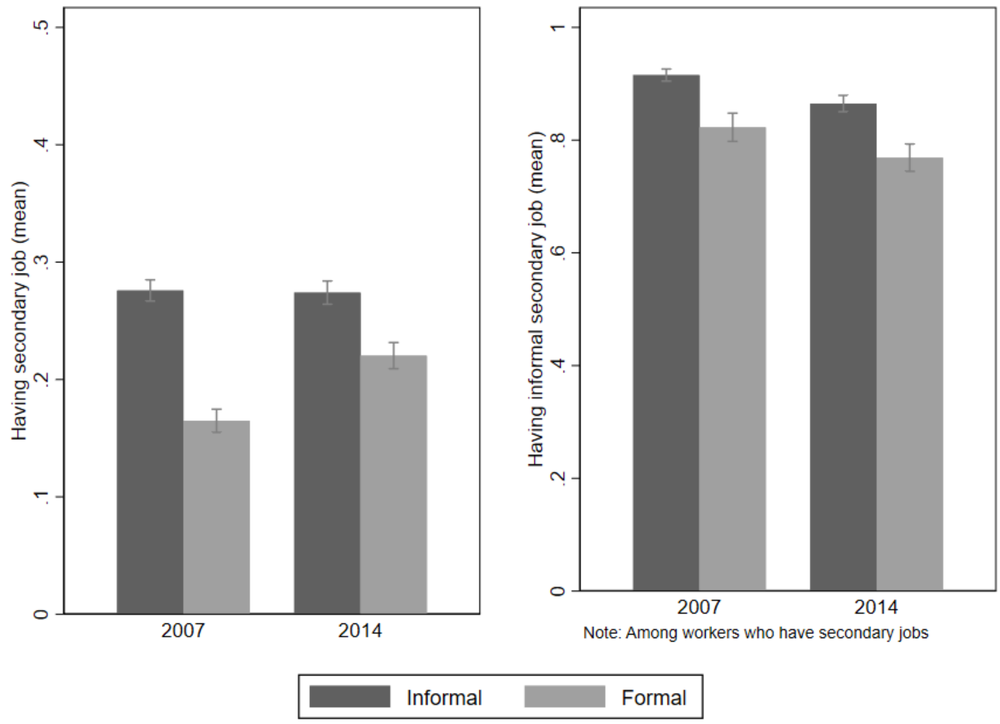
8.2 Discussion: Explanation and policy implications

The primary form of safety net in middle-income countries is cash transfer programs, either unconditional or conditional, and the COVID-19 crisis sped up the development of new programs and the expansion of already existing ones worldwide (Gentilini et al., 2022). I presented evidence on the impact of such cash transfer programs, particularly the unconditional ones, which are often provided in times of crisis. In policy debates, the notion that these programs could hurt labour markets, especially in the formal sector, has a lot of weight and can affect the future of these programs. Despite relatively mixed results across the two UCT settings, I find statistically significant evidence of disincentives to employment and job formality consistent with the income and substation effect, particularly for BLT 2008/BLSM 2013. However, for the BLT 2005, there is evidence of an increase in the likelihood of formal employment. The latter result is in fact in line with the recent finding in Brazil, which also find an increase in formal employment in localities where the transfers increased (Gerard et al., 2021).

The mixed results between the two UCT settings can be partially attributed to the different targeting approaches. While BLT 2005 used a standard proxy-means test, BLT 2008/BLSM 2013 utilised a more comprehensive set of indicators, including data on individual employment, occupation type, and work status. In addition, the process of data collection was preceded by a community-based targeting, identifying which households had moved domicile or were considered either in need or not in need of social assistance. The use of a more detailed proxy-means test coupled with more current, dynamic welfare information are likely to have stronger disincentive effects. Since earnings in the formal sector can be verified more easily (e.g., through third-party reports to the government), such means testing may have significant effects on labour supply through substitution effect, especially in the formal sector. In the case of means-tested programmes, the programme design (including marginal effective tax rates on earned income) is likely to shape incentives in addition to the income effect.

While disincentive effects were discovered, the magnitude was relatively minimal, accounting for only a 2-percentage-point drop in employment and job formality (2 and 8 percent decrease from the mean outcome, respectively). As was predicted, the small effect size is consistent with the relatively modest cash transfers, amounting to around Rp100,000 rupiah per month per household (or 15 percent of the average consumption expenditure of the target household). However, the effect on job formality is greater than on employment per se, which is in accordance with the mechanism predicted by the theory where substitution effect of cash transfers is present. It is a margin of employment adjustment affected by the disincentive effect from means testing. The majority of the evidence that is currently available focuses on programs that use proxy-means testing to define eligibility based on geographic location and household assets (e.g., Banerjee et al., 2017), such that these programs only generate income effects. In contrast, means-tested programs have the potential to generate both income and substitution effects due to the fact that workers may alter their labour supply in order to obtain or retain eligibility for the program (Bergolo & Cruces, 2021; Gerard et al., 2021). This is especially true for formal employment, where earnings are easier to verify.

Table 4: Trend and Interaction between Job Formality and Secondary Job



Source: Author's calculation based on Indonesia Family Life Survey (IFLS).

Another possible explanation for the greater effect on job formality is that some workers in the formal sector have informal jobs in addition to their primary employment. As a result, if they are affected by the crisis and are unable to keep their jobs, they will work as informal workers. Furthermore, in Indonesia, where unemployment benefits are still limited, most people cannot afford to be unemployed and will instead work in the informal sector. **Table 4** shows how job formality and secondary job interact and change over time. It can be observed that some portion of workers, either working in the formal or informal sectors, have a secondary job—on average about a quarter of workers have a secondary job (with a relatively lower rate of among those who work in the formal sector). Looking further, among those who have a formal job (as their primary occupation) and have a secondary job, more than 75 percent work in the informal sector for their second job in 2007 and 2014. This stylised fact demonstrates that formal-sector workers also engage in informal work; thus, when they lose formal employment, they are likely to be recorded as being in informal work.

Given recent crises such as the COVID-19 pandemic, many governments have adopted cash transfer programmes as part of the mitigation plan to alleviate the adverse effect on welfare. The disincentive effects of social assistance programs on labour outcomes, however, are a top policy concern, both in developed and developing countries. UCT is predicted not to have distorting effects as it is provided to compensate for the negative impact of the crisis and the amount granted is often not large relative to the average expenditure of target households. Nonetheless, this paper demonstrates that, despite the small magnitude of effects, UCT has significant negative impacts on employment and job formality, as well as their mobility, which potentially undermine job recovery, especially in informality margin. Using a large developing country context, I find that job formality offers a more useful lens through which to examine the impact of cash transfer programmes than employment status per se. The negative effect of UCT would be underestimated when employment status alone is considered. Moreover, economic policies in many developing countries have also put a lot of emphasis on creating formal jobs (Levy, 2010).[22] As such, it is important to account for the aggregate effects to capture the complete picture of cash transfers across all economic sectors, including the impact on job formality. This is important from a policy perspective because, as economies develop and incomes become more verifiable across the income distribution (Jensen, 2022), means testing will be used more widely as targeting for social transfers.

[22] According to Perry et al. (2007), formal jobs are associated with higher output and total factor productivity and are more likely to offer employees social security coverage and better working conditions (e.g., Ulyssea, 2020).

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Appendix

Table A.1: Variables in the Socio-economic Data Collection 2005 (PSE 2005)

No.	Variables in household prosperity	Criteria for being classified as poor
1	Area of floor space per HH member	< 8 m2
2	Type of floor in the house	Earth/plywood/low quality
3	Type of walls in the house	Not present
4	Toilet facilities	Not available
5	Availability of drinking water	Clean water unavailable
6	Type of lighting used	Not electric
7	Fuel used	Wood/charcoal
8	Number of meals per day	Less than 2
9	Ability to buy chicken, meat or milk every week	No
10	Ability to buy new clothes for each household member	No
11	Ability to get treatment at a local community clinic	No
12	Household head's type of work	Small scale farming, fishing, gardening
13	Household head's level of education	Never attended school/ Did not complete year 6
14	Ownership of asset/valuable worth at least Rp500,000	None

Source: National Team for the Acceleration of Poverty Reduction, 2015. *Indonesia's Unified Database for Social Protection Programmes*.

Table A.2: Variables in the Data Collection for Social Protection Programmes

No.	Individual variable	Household variable
1	Age	Owns a home
2	Gender	Measurement of floor area
3	Marital status	Primary type of flooring in the house
4	Relationship with the household's head and family	Primary type of walls in the house
5	In possession of an identity card	Primary type of roof on the house
6	Disabilities	Availability of clean drinking water
7	Chronic illness	Procedure for obtaining clean drinking water
8	Pregnancy status	Main type of fuel used for cooking
9	School attendance	Owns a toilet
10	Level of education	Assets owned
11	Duration of schooling	Participates in the family planning and Cluster 1 programmes[23]
12	Currently employed	
13	Type of occupation	
14	Work status	

Source: National Team for the Acceleration of Poverty Reduction, 2015. *Indonesia's Unified Database for Social Protection Programmes***.**

[23] Cluster 1 includes rice subsidies for low-income households, the Rice for the Poor programme (referred to as the *Raskin* programme), the Cash Transfers for Poor Students programme (*Bantuan Siswa Miskin*/BSM), the public health insurance programme (referred to as *Jamkesmas*) and the Conditional Cash Transfer Programme for Poor Families (*Program Keluarga Harapan*/PKH).

Table A.3: Quality Check of Retrospective Responses

	(1)		(2)	
	Share of matched response (%) between IFLS 3 (2000) & IFLS 4 (2007)	Mean difference	Share of matched response (%) between IFLS 4 (2007) & IFLS 5 (2014)	Mean difference
Employment				
2000	81.75	.006		
2007			82.22	.029
Job Formality				
2000	80.34	.090		
2007			83.35	.004

Note: Accuracy check is carried out by comparing responses from two consecutive survey waves. Accuracy check for the year 2000 is carried out by comparing responses in IFLS 3 and 4, whereas for the year 2007 is carried out by comparing responses in IFLS 4 and 5.

Table A.4: Statistics Indonesia (BPS)'s official definition of job formality

Work status	**Occupation types**				
	Professional, Director, Manager	Sales, Labour	Agricultural Workers	Production, Transport, Unskilled	Other
Self-employed	Formal	Informal	Informal	Informal	Informal
Self-employed with family workers	Formal	Formal	Informal	Formal	Informal
Self-employed with permanent workers	Formal	Formal	Formal	Formal	Formal
Employees (government or private worker)	Formal	Formal	Formal	Formal	Formal
Casual employees, agriculture	Formal	Informal	Informal	Informal	Informal
Casual employees, non-agricultural	Formal	Informal	Informal	Informal	Informal
Family workers	Informal	Informal	Informal	Informal	Informal

Source:World Bank, 2010. *Indonesia Job Report: Towards Better Jobs and Security for All.*

Table A.5: Statistics Indonesia (BPS)'s simplified definition of job formality

Work Status	**Sector**	
	Non-Agriculture	Agriculture
Self-employed	Informal	Informal
Self-employed with family workers	Informal	Informal
Self-employed with permanent workers	Formal	Informal
Employees (government or private worker)	Formal	Formal
Casual employees	Informal	Informal
Family workers	Informal	Informal

Source: World Bank, 2010. *Indonesia Job Report: Towards Better Jobs and Security for All.*

Table A.6: Outcome Indicators

Variable	Description	Unit
Employment and job formality		
Employment	Individual is self-reported being employed in a particular year. The retrospective questionnaire asks the respondents "Do you work in [Year]?". If a respondent was both unemployed and employed in the same year, IFLS will record the status that lasted the longest.	dummy [Yes=1; 0=otherwise]
Job formality	Individual works in government, private sector employed, and self-employed with permanent worker in non-agricultural sector.	dummy [Yes=1; 0=otherwise]
Job mobility (transition)		
Job finding	Probability of being employed among those who were unemployed in the base year.	dummy [Yes=1; 0=otherwise]
Job exit	Probability of being unemployed among those who were employed in the base year.	dummy [Yes=1; 0=otherwise]
Move into formality	Probability of having formal job among those who had informal job in the base year.	dummy [Yes=1; 0=otherwise]
Move out of formality	Probability of having informal job among those who had formal job in the base year.	dummy [Yes=1; 0=otherwise]

Note: Effects of UCT on job mobility are examined by estimating the employment and job formality effects based on their employment and job formality in the base year, defined as one year status he UCT programmes were introduced. These baseline years will determine the sub sample of impact analysis according to the employment and job formality status prior to the UCT programmes.

Table A.7: Summary Statistics

Variable	Annual panel dataset (2000-2014)								Unit
	2000-2007				2007-2014				
	Mean	S.D.	No. of Observations	No. of groups (individual)	Mean	S.D.	No. of Observations	No. of groups (individual)	
Treatment									
BLT 2005	0.26	0.44	67,532	8,455					dummy [0,1]
BLT 2008/BLSM 2013					0.26	0.44	63,982	8,031	dummy [0,1]
Outcome									
Employment & job formality									
Employment	0.90	0.30	67,532	8,455	0.92	0.28	63,982	8,031	dummy [0,1]
Job formality	0.26	0.44	67,532	8,455	0.26	0.44	63,982	8,031	dummy [0,1]
Job mobility (transition)									
Job finding	0.34	0.48	7,323	922	0.55	0.50	8,738	1,102	dummy [0,1]
Job exit	0.05	0.22	62,627	7,837	0.08	0.27	63,431	7,973	dummy [0,1]
Move into formality	0.04	0.21	50,378	6,308	0.15	0.35	53,360	6,707	dummy [0,1]
Move out of formality	0.15	0.36	19,309	2,417	0.33	0.47	22,943	2,884	dummy [0,1]

Note: A multi-year individual-level panel dataset is constructed using annual retrospective data collected from IFLS waves 3, 4, and 5, spanning from 200 to 2014.Tthe effects of BLT 2005 are examined using 2000-2007 dataset, and those of BLT 2008/BLSM 2013 are examined using 2007-2014 dataset.

Table A.8: Characteristics of Treatment and Control Groups in the Baseline Years, Pre-Matching Process

Variables	2000				2007			
	Mean		Bias t-test p>|t|	Ratio	Mean		Bias t-test p>|t|	Ratio
	Treatment	Control			Treatment	Control		
HH characteristics								
Live in Java	0.59	0.60	0.43	0.99	0.61	0.55	0.00	1.12
Live in urban area	0.32	0.49	0.00	0.64	0.43	0.51	0.00	0.84
HH head is working	0.98	0.98	0.87	1.00	0.99	0.99	0.86	1.00
HH head has formal job	0.42	0.42	0.59	0.99	0.24	0.35	0.00	0.69
HH head works in agriculture	0.58	0.41	0.00	1.42	0.54	0.40	0.00	1.37
Total household member	5.70	5.78	0.10	0.99	5.79	5.71	0.10	1.01
No. of child < 4 y.o.	0.41	0.39	0.04	1.06	0.39	0.36	0.00	1.09
No. of child in elementary school	0.68	0.58	0.00	1.18	0.63	0.50	0.00	1.25
No. of children in junior high school	0.22	0.26	0.00	0.83	0.21	0.22	0.16	0.95
No. of children in senior high school	0.11	0.21	0.00	0.50	0.11	0.18	0.00	0.64
Highest years of education in HH	8.15	10.72	0.00	0.76	9.21	11.47	0.00	0.80
Ever used poor letter	0.09	0.05	0.00	2.00	0.20	0.09	0.00	2.26
Own house	0.85	0.81	0.00	1.05	0.82	0.76	0.00	1.07
Own land	0.39	0.39	0.86	1.00	0.31	0.35	0.00	0.88
Own HH enterprise	0.43	0.53	0.00	0.82	0.39	0.46	0.00	0.83
Own television	0.37	0.69	0.00	0.53	0.63	0.82	0.00	0.77
Own refrigerator	0.02	0.16	0.00	0.12	0.08	0.33	0.00	0.24
HH total expenditure	14.66	15.03	0.00	0.98	15.47	15.82	0.00	0.98
Expenditure per capita	13.03	13.39	0.00	0.97	13.83	14.20	0.00	0.97
Access to improved toilet	0.25	0.54	0.00	0.45	0.44	0.70	0.00	0.64
Access to improved water quality	0.44	0.68	0.00	0.65	0.58	0.76	0.00	0.77
Access to electricity	0.81	0.93	0.00	0.87	0.93	0.97	0.00	0.96
Use improved cooking fuel	0.45	0.67	0.00	0.68	0.42	0.64	0.00	0.65
Community characteristics								
No. of elementary school	4.60	5.41	0.00	0.85	4.55	4.88	0.00	0.93
No. of junior high school	3.66	3.69	0.48	0.99	3.43	3.44	0.77	1.00
No. of senior high school	3.46	3.51	0.29	0.99	3.13	3.14	0.79	1.00
No. of Puskesmas	3.37	3.44	0.03	0.98	2.18	2.15	0.17	1.01
No. of Posyandu	4.71	5.41	0.00	0.87	6.92	7.38	0.00	0.94
No. of Village Midwives	0.72	0.68	0.01	1.06	0.95	0.99	0.02	0.96
No. of health practitioner	10.98	11.91	0.00	0.92	4.51	4.71	0.00	0.96
Availability of asphalt road	0.72	0.79	0.00	0.91	0.86	0.88	0.00	0.98
Availability of PAM water source	0.47	0.58	0.00	0.80	0.49	0.53	0.00	0.92
Main economic activity	2.61	3.35	0.00	0.78	2.40	2.84	0.00	0.84
Main economic activity is farming	0.72	0.59	0.00	1.21	0.73	0.68	0.00	1.06
MeanBias		24.7				20.9		
MedBias		18.9				14.4		
Rubin's B		104.2				104.1		
Rubin's R		0.74				0.67		
%Var		94				88		

Note: Rubin's B is the absolute standardized difference of the means of the linear index of the propensity score in the treated and (matched) non-treated group, and Rubin's R is the ratio of treated to (matched) non-treated variances of the propensity score index. If B>25%, R outside [0.5; 2], sample is considered unbalanced.

Table A.9: Propensity Score Matching of Treatment Assignment

Variables	BLT 2005			BLT 2008/BLSM 2013		
	Coef.	**Std. Err.**	**P>|z|**	**Coef.**	**Std. Err.**	**P>|z|**
HH characteristics						
Live in Java	-0.02	0.04	0.65	0.25	0.04	0.00
Live in urban area	-0.02	0.05	0.63	0.20	0.05	0.00
HH head is working	-0.27	0.11	0.02	-0.08	0.17	0.63
HH head has formal job	0.09	0.03	0.01	-0.07	0.04	0.07
HH head works in agriculture	0.01	0.04	0.78	0.02	0.04	0.57
Total household member	0.04	0.02	0.06	0.02	0.02	0.19
No. of child < 4 y.o.	-0.02	0.03	0.41	0.17	0.03	0.00
No. of child in elementary school	-0.02	0.02	0.29	0.09	0.02	0.00
No. of children in junior high school	-0.03	0.03	0.41	-0.05	0.03	0.12
No. of children in senior high school	-0.10	0.04	0.01	-0.08	0.04	0.04
Highest years of education in HH	-0.07	0.01	0.00	-0.07	0.01	0.00
Ever used poor letter	0.33	0.06	0.00	0.35	0.04	0.00
Own house	0.12	0.05	0.03	0.12	0.05	0.01
Own land	-0.22	0.04	0.00	-0.30	0.04	0.00
Own HH enterprise	-0.03	0.04	0.42	-0.02	0.03	0.62
Own television	-0.37	0.04	0.00	-0.27	0.04	0.00
Own refrigerator	-0.46	0.08	0.00	-0.55	0.05	0.00
HH total expenditure	-0.12	0.12	0.30	-0.16	0.11	0.15
Expenditure per capita	-0.14	0.12	0.22	-0.04	0.11	0.72
Access to improved toilet	-0.25	0.04	0.00	-0.22	0.03	0.00
Access to improved water quality	-0.12	0.04	0.00	-0.18	0.04	0.00
Access to electricity	-0.27	0.05	0.00	0.03	0.07	0.70
Use improved cooking fuel	-0.06	0.04	0.12	-0.16	0.04	0.00
Community characteristics						
No. of elementary school	-0.02	0.01	0.01	-0.02	0.01	0.01
No. of junior high school	0.04	0.01	0.00	-0.01	0.01	0.69
No. of senior high school	-0.01	0.01	0.10	0.02	0.01	0.13
No. of Puskesmas	-0.01	0.01	0.30	0.02	0.02	0.20
No. of Posyandu	0.01	0.01	0.46	0.00	0.00	0.53
No. of Village Midwives	-0.04	0.03	0.15	-0.09	0.02	0.00
No. of health practitioner	0.01	0.00	0.00	0.00	0.01	0.94
Availability of asphalt road	0.11	0.04	0.01	0.08	0.05	0.08
Availability of PAM water source	-0.09	0.04	0.02	0.03	0.04	0.37
Main economic activity	0.00	0.01	0.80	-0.03	0.01	0.02
Main economic activity is farming	-0.08	0.08	0.35	-0.26	0.08	0.00
Matching category	**Treatment**	**Control**	**Total**	**Treatment**	**Control**	**Total**
Matched	17,807	49,725	67,532	16,548	47,344	63,892
Unmatched	9,805	31,429	41,234	14,130	49,071	63,201
Total	27,612	81,154	108,766	30,678	96,415	127,093

Note: The propensity score matching method used is single nearest-neighbour matching with replacement. The matching method matches nearest neighbour and other controls with identical (tied) propensity scores and imposes a common support by dropping treatment observations whose propensity scores are higher than the maximum or less than the minimum propensity scores of the controls.

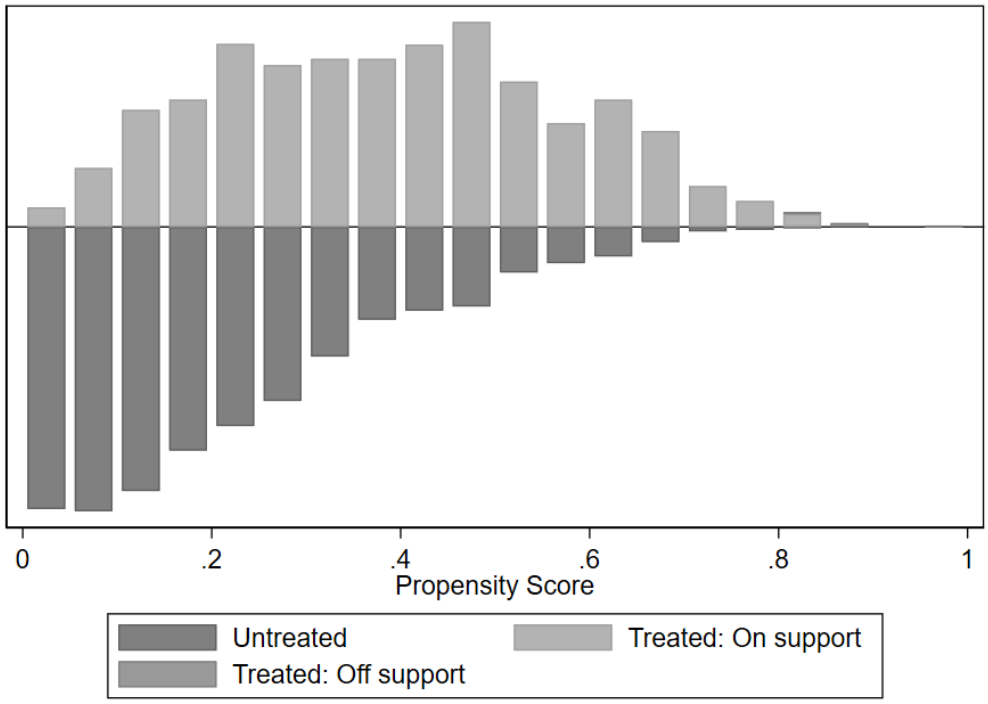
Table A.10: Characteristics of Treatment and Control Groups in the Baseline Years, Post-Matching Process

Variables	2000 Mean Treatment	2000 Mean Control	2000 Bias t-test p>|t|	2000 Ratio	2007 Mean Treatment	2007 Mean Control	2007 Bias t-test p>|t|	2007 Ratio
HH characteristics								
Live in Java	0.58	0.58	0.93	1.00	0.61	0.61	0.95	1.00
Live in urban area	0.30	0.31	0.48	0.97	0.39	0.35	0.00	1.12
HH head is working	0.98	0.98	0.69	1.00	0.99	0.98	0.01	1.01
HH head has formal job	0.42	0.43	0.49	0.98	0.21	0.19	0.11	1.09
HH head works in agriculture	0.62	0.62	0.88	1.00	0.60	0.64	0.01	0.94
Total household member	5.98	6.15	0.02	0.97	6.17	6.37	0.01	0.97
No. of child < 4 y.o.	0.37	0.39	0.22	0.95	0.38	0.38	0.84	1.01
No. of child in elementary school	0.70	0.73	0.15	0.95	0.65	0.60	0.03	1.08
No. of children in junior high school	0.25	0.25	0.53	0.97	0.22	0.25	0.03	0.88
No. of children in senior high school	0.12	0.13	0.54	0.95	0.12	0.14	0.24	0.91
Highest years of education in HH	8.21	8.26	0.59	0.99	9.21	9.17	0.65	1.00
Ever used poor letter	0.09	0.09	0.88	0.99	0.20	0.21	0.65	0.98
Own house	0.90	0.90	0.39	1.01	0.89	0.90	0.14	0.99
Own land	0.46	0.46	0.61	0.98	0.38	0.37	0.62	1.02
Own HH enterprise	0.44	0.41	0.03	1.07	0.42	0.41	0.42	1.03
Own television	0.37	0.39	0.14	0.95	0.63	0.62	0.47	1.02
Own refrigerator	0.02	0.02	0.62	0.91	0.07	0.06	0.17	1.16
HH total expenditure	14.68	14.66	0.23	1.00	15.48	15.49	0.55	1.00
Expenditure per capita	12.99	12.95	0.02	1.00	13.76	13.74	0.24	1.00
Access to improved toilet	0.23	0.23	0.97	1.00	0.41	0.42	0.65	0.98
Access to improved water quality	0.43	0.43	0.91	1.00	0.54	0.56	0.27	0.97
Access to electricity	0.79	0.80	0.92	1.00	0.93	0.92	0.13	1.01
Use improved cooking fuel	0.41	0.40	0.64	1.02	0.37	0.36	0.52	1.02
Community characteristics								
No. of elementary school	4.55	4.60	0.53	0.99	4.49	4.46	0.55	1.01
No. of junior high school	3.74	3.76	0.72	1.00	3.54	3.48	0.22	1.02
No. of senior high school	3.49	3.55	0.34	0.98	3.20	3.14	0.27	1.02
No. of Puskesmas	3.57	3.63	0.15	0.98	2.28	2.23	0.08	1.03
No. of Posyandu	4.41	4.39	0.73	1.01	6.52	6.37	0.21	1.02
No. of Village Midwives	0.70	0.70	0.78	0.99	0.92	0.91	0.58	1.01
No. of health practitioner	11.40	11.52	0.36	0.99	4.48	4.28	0.00	1.05
Availability of asphalt road	0.72	0.71	0.53	1.01	0.85	0.85	0.85	1.00
Availability of PAM water source	0.49	0.50	0.69	0.99	0.51	0.47	0.01	1.08
Main economic activity	2.50	2.52	0.83	0.99	2.26	2.21	0.49	1.02
Main economic activity is farming	0.73	0.71	0.24	1.02	0.75	0.76	0.25	0.98
MeanBias	2.1				3.6			
MedBias	1.5				3.1			
Rubin's B	17.0				24.5			
Rubin's R	0.93				0.95			
%Var	44				31			

Note: Rubin's B is the absolute standardized difference of the means of the linear index of the propensity score in the treated and (matched) non-treated group, and Rubin's R is the ratio of treated to (matched) non-treated variances of the propensity score index. If B>25%, R outside [0.5; 2], sample is considered unbalanced.

Figure A.1: Propensity Scores Distribution of Treatment Assignment

A. BLT 2005 Treatment (Baseline Year 2000)



B. BLT 2008/BLSM 2013 Treatment (Baseline Year 2007)

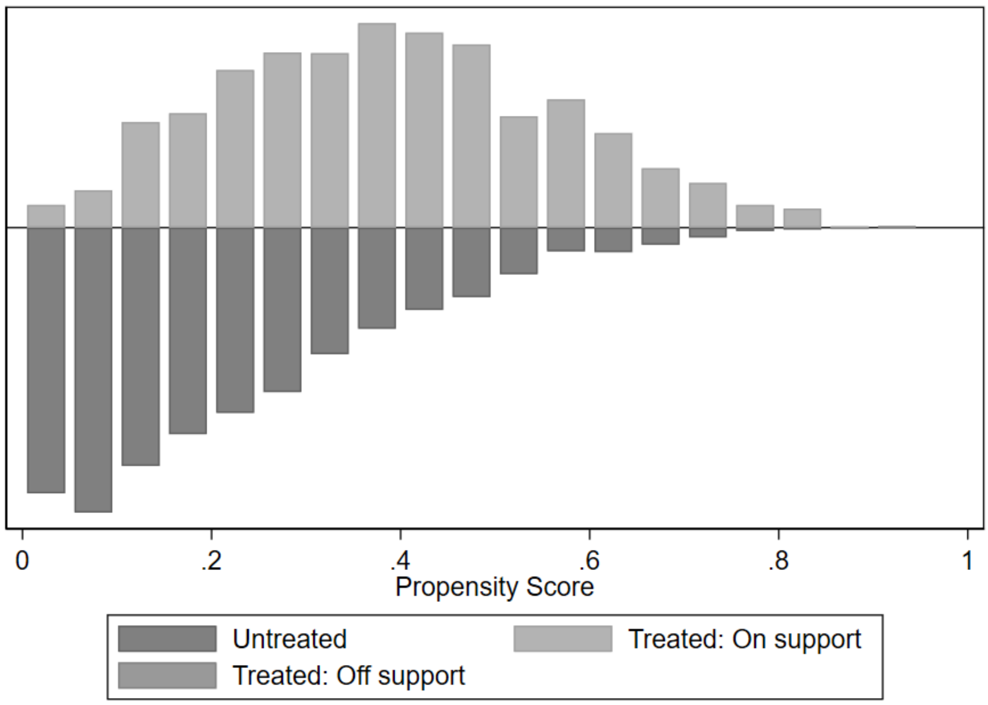
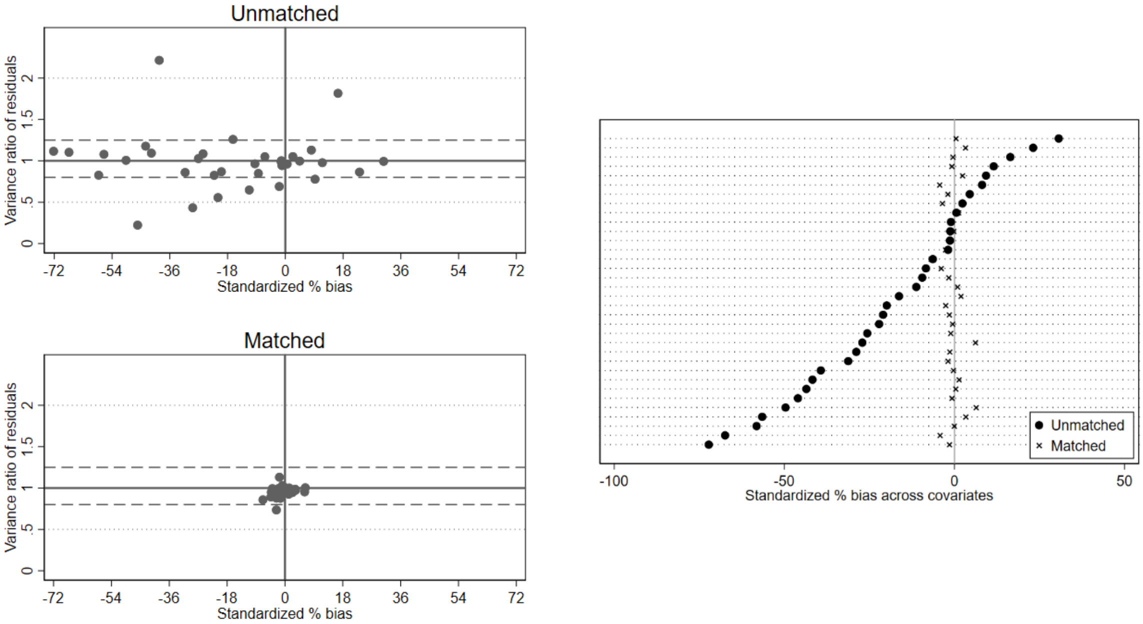


Figure A.2: Covariate Balance Post-Matching

A. BLT 2005 Treatment (Baseline Year 2000)



B. BLT 2008/BLSM 2013 Treatment (Baseline Year 2007)

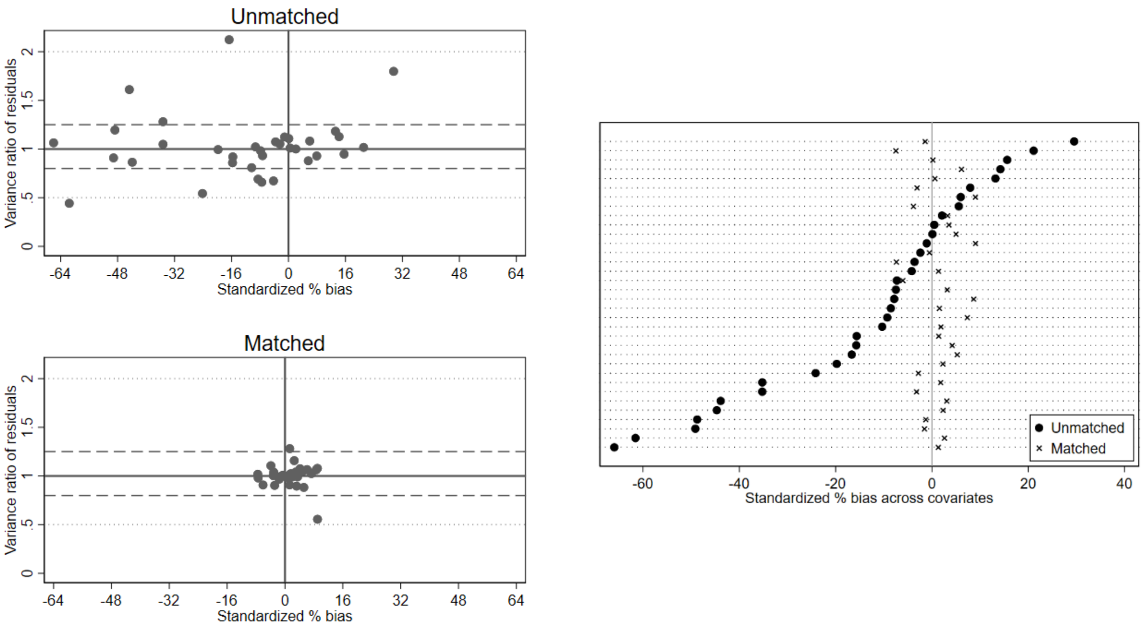
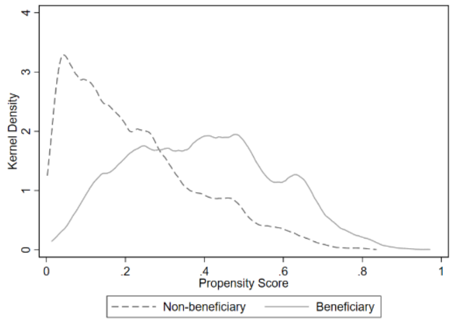


Figure A.3: Propensity Scores Distribution of Treatment Assignment by Household Expenditure Decile

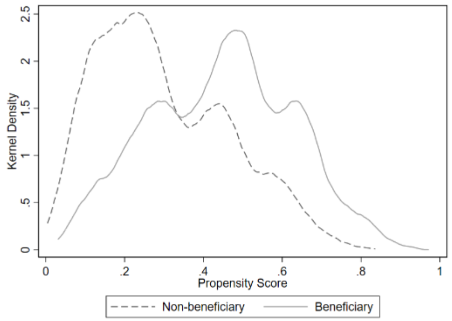
A. BLT 2005 Treatment (Propensity Scores Based on Baseline Year 2000)

All sample



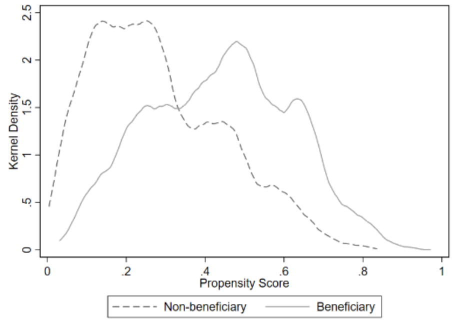
P-value of Kolmogorov-Smirnov test: 0.0000

Bottom 20%



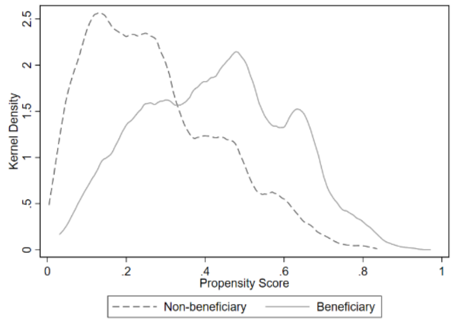
P-value of Kolmogorov-Smirnov test: 0.0000

Bottom 30%



P-value of Kolmogorov-Smirnov test: 0.0000

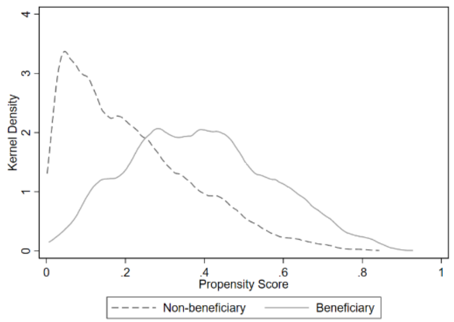
Bottom 40%



P-value of Kolmogorov-Smirnov test: 0.0000

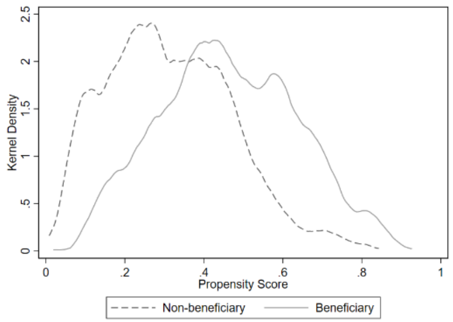
B. BLT 2008/BLSM 2013 Treatment (Propensity Scores Based on Baseline Year 2007)

All sample



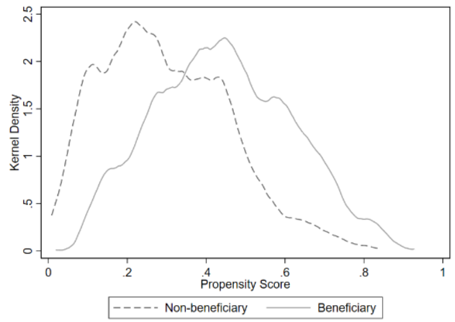
P-value of Kolmogorov-Smirnov test: 0.0000

Bottom 20%



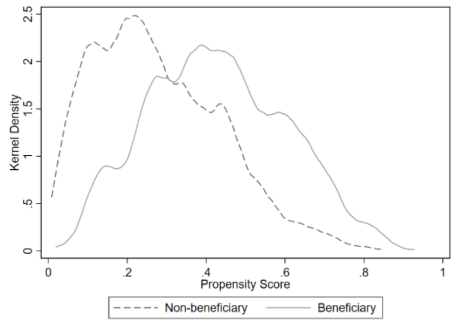
P-value of Kolmogorov-Smirnov test: 0.0000

Bottom 30%



P-value of Kolmogorov-Smirnov test: 0.0000

Bottom 40%



P-value of Kolmogorov-Smirnov test: 0.0000

Table A.11: Effects of UCT on Employment, Job Formality, and Mobility – All Sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Employment		**Job formality**		**Job finding**		**Job exit**		**Move into formality**		**Move out of formality**	
A. BLT 2005												
ATT	-0.002	-0.001	0.008	0.008	0.013	-0.024	0.005	-0.002	0.008	0.012**	0.017	-0.019
	(0.007)	(0.007)	(0.006)	(0.006)	(0.047)	(0.047)	(0.005)	(0.006)	(0.005)	(0.006)	(0.021)	(0.023)
	[0.007]	[0.007]	[0.006]	[0.006]	[0.047]	[0.048]	[0.005]	[0.006]	[0.005]	[0.006]	[0.023]	[0.025]
Individual fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region × Year fixed effects	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Mean outcome	0.90	0.90	0.26	0.26	0.34	0.34	0.05	0.05	0.04	0.04	0.15	0.15
No. of observations	67,532	67,532	67,532	67,532	7,323	7,323	62,627	62,627	50,378	50,378	19,309	19,309
No. of group (individual)	8,455	8,455	8,455	8,455	922	922	7,837	7,837	6,308	6,308	2,417	2,417
B. BLT 2008/BLSM 2013												
ATT	-0.011*	-0.010*	-0.004	-0.003	-0.092	-0.041	0.008	0.008	-0.028***	-0.025***	0.081***	0.031
	(0.006)	(0.006)	(0.009)	(0.009)	(0.056)	(0.029)	(0.006)	(0.006)	(0.008)	(0.009)	(0.021)	(0.025)
	[0.007]	[0.007]	[0.010]	[0.010]	[0.058]	[0.031]	[0.006]	[0.007]	[0.009]	[0.011]	[0.027]	[0.031]
Individual fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region × Year fixed effects	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Mean outcome	0.92	0.92	0.26	0.26	0.55	0.55	0.08	0.08	0.15	0.15	0.33	0.33
No. of observations	63,892	63,892	63,892	63,892	8,738	8,738	63,431	63,431	53,360	53,360	22,943	22,943
No. of group (individual)	8,031	8,031	8,031	8,031	1,102	1,102	7,973	7,973	6,707	6,707	2,884	2,884

Note: Estimation of ATT is based on generalised PSM DD using a fixed-effect estimator at the individual level. The PSM method used is single nearest-neighbour matching with replacement. Region is a categorical variable with a value of 1 if observation's regional location is Java, and 0 otherwise. Standard errors clustered at the household level are in parentheses, and those clustered at the community level are in square brackets. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table A.12: Event Study of UCT on Employment, Job Formality, and Mobility – All Sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Employment		Job formality		Job finding		Job exit		Move into formality		Move out of formality	
A. BLT 2005												
Lead 4	0.010	0.010	0.002	0.002	0.007	0.006	-0.003	-0.003	-0.020***	-0.020***	0.048**	0.048**
	(0.007)	(0.007)	(0.007)	(0.007)	(0.037)	(0.037)	(0.006)	(0.006)	(0.006)	(0.006)	(0.022)	(0.022)
Lead 3	0.003	0.003	0.002	0.002	0.024	0.023	0.004	0.004	-0.011**	-0.011**	0.045**	0.045**
	(0.007)	(0.007)	(0.006)	(0.006)	(0.035)	(0.034)	(0.006)	(0.006)	(0.005)	(0.005)	(0.019)	(0.019)
Lead 2	-0.002	-0.002	0.001	0.001	-0.023	-0.023	0.002	0.002	-0.008**	-0.008**	0.031**	0.031**
	(0.005)	(0.005)	(0.004)	(0.004)	(0.026)	(0.026)	(0.004)	(0.004)	(0.004)	(0.004)	(0.015)	(0.015)
Lag 0	0.001	0.001	0.003	0.003	0.005	0.004	-0.001	-0.001	0.004	0.004	-0.015	-0.015
	(0.005)	(0.005)	(0.004)	(0.004)	(0.044)	(0.044)	(0.006)	(0.006)	(0.006)	(0.006)	(0.022)	(0.022)
Lag 1	0.003	0.003	0.010	0.010	-0.028	-0.029	-0.002	-0.002	0.004	0.004	0.008	0.008
	(0.007)	(0.007)	(0.006)	(0.006)	(0.048)	(0.048)	(0.006)	(0.006)	(0.007)	(0.007)	(0.024)	(0.024)
Lag 2	0.010	0.010	0.018**	0.018**	-0.033	-0.033	-0.009	-0.009	-0.005	-0.005	0.053*	0.053*
	(0.008)	(0.008)	(0.008)	(0.008)	(0.050)	(0.050)	(0.007)	(0.007)	(0.007)	(0.007)	(0.031)	(0.031)
Individual fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region × Year fixed effects	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Mean outcome	0.90	0.90	0.26	0.26	0.34	0.34	0.05	0.05	0.04	0.04	0.15	0.15
No. of observations	67,532	67,532	67,532	67,532	7,323	7,323	62,627	62,627	50,378	50,378	19,309	19,309
No. of group (individual)	8,455	8,455	8,455	8,455	922	922	7,837	7,837	6,308	6,308	2,417	2,417
B. BLT 2008/BLSM 2013												
Lead 4	-0.002	-0.002	-0.014	-0.015	-0.078	-0.078	0.002	0.002	-0.010	-0.010	-0.009	-0.009
	(0.015)	(0.015)	(0.015)	(0.015)	(0.068)	(0.068)	(0.015)	(0.015)	(0.016)	(0.016)	(0.043)	(0.043)
Lead 3	0.004	0.005	-0.010	-0.010	-0.027	-0.027	-0.002	-0.002	-0.015	-0.015	-0.026	-0.026
	(0.012)	(0.012)	(0.014)	(0.014)	(0.066)	(0.066)	(0.012)	(0.012)	(0.015)	(0.015)	(0.040)	(0.040)
Lead 2	0.001	0.001	-0.014	-0.014	-0.064	-0.064	-0.000	-0.000	-0.019	-0.019	-0.001	-0.001
	(0.011)	(0.011)	(0.014)	(0.014)	(0.059)	(0.059)	(0.011)	(0.011)	(0.014)	(0.014)	(0.039)	(0.039)
Lag 0	-0.015**	-0.014*	-0.018*	-0.019**	-0.041	-0.041	0.013*	0.013*	-0.029***	-0.029***	0.034	0.034
	(0.007)	(0.008)	(0.010)	(0.010)	(0.032)	(0.032)	(0.008)	(0.008)	(0.011)	(0.011)	(0.031)	(0.031)
Lag 1	-0.013*	-0.013*	-0.010	-0.010	-0.070*	-0.070*	0.011	0.011	-0.029***	-0.029***	0.049	0.049
	(0.007)	(0.007)	(0.010)	(0.010)	(0.036)	(0.036)	(0.007)	(0.007)	(0.011)	(0.011)	(0.031)	(0.031)
Lag 2	-0.010	-0.009	-0.005	-0.004	-0.088**	-0.088**	0.008	0.008	-0.025**	-0.025**	0.012	0.012
	(0.008)	(0.008)	(0.011)	(0.011)	(0.042)	(0.042)	(0.008)	(0.008)	(0.012)	(0.012)	(0.033)	(0.033)
Individual fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region × Year fixed effects	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Mean outcome	0.92	0.92	0.26	0.26	0.55	0.55	0.08	0.08	0.15	0.15	0.33	0.33
No. of observations	63,892	63,892	63,892	63,892	8,738	8,738	63,431	63,431	53,360	53,360	22,943	22,943
No. of group (individual)	8,031	8,031	8,031	8,031	1,102	1,102	7,973	7,973	6,707	6,707	2,884	2,884

Note: Event study approach is estimated using a fixed-effect estimator at the individual level with 4 leads and 2 lags in the specification. Region is a categorical variable with a value of 1 if observation's regional location is Java, and 0 otherwise. Standard errors are cluster-robust at the household level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table A.13: Event Study of Effects of UCT on Work Hours – All Sample

	(1)	(2)
	Work hours	
A. BLT 2005		
ATT	-0.048	-0.047
	(0.035)	(0.035)
	[0.052]	[0.052]
Individual fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Region × Year fixed effects	No	Yes
Mean outcome	3.51	3.51
No. of observations	16,782	16,782
No. of group (individual)	8,391	8,391
B. BLT 2008/BLSM 2013		
ATT	-0.022	-0.020
	(0.033)	(0.034)
	[0.046]	[0.046]
Individual fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Region × Year fixed effects	No	Yes
Mean outcome	3.53	3.53
No. of observations	15,936	15,936
No. of group (individual)	7,968	7,968

Note: Estimation of ATT is based on generalised PSM DD using a fixed-effect estimator at the individual level. The PSM method used is single nearest-neighbour matching with replacement. Outcome variable work hours is total working hour per week in log term. Region is a categorical variable with a value of 1 if observation's regional location is Java, and 0 otherwise. Standard errors clustered at the household level are in parentheses, and those clustered at the community level are in square brackets. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.