The labour market impact of COVID-19 lockdowns: Evidence from Ghana

Abstract: In this paper, we provide causal evidence of the immediate and near-term impact of stringent COVID-19 lockdown policies on employment outcomes, using Ghana as a case study. We take advantage of a specific policy setting, in which strict stay-at-home orders were issued and enforced in two spatially delimited areas, bringing Ghana's major metropolitan centres to a standstill, while in the rest of the country less stringent regulations were in place. Using a difference-in-differences design, we find that the three-week lockdown had a large and significant immediate negative impact on employment in the treated districts, particularly among workers in informal self-employment. While the gap in employment between the treated and control districts had narrowed four months after the lockdown was lifted, we detect a persistent nationwide decline in both earnings and employment, jeopardizing particularly the livelihoods of small business owners mainly operating in the informal economy.

Key words: COVID-19, lockdown, employment, informal economy, Ghana

JEL classification: J6, I18, J46, O55

1 Introduction

To limit the spread of COVID-19, the infectious disease caused by the novel coronavirus, policymakers around the world have enacted stringent containment and closure policies. In April 2020, rules on hygiene and social distancing reshaped daily life, schools and businesses were closed, gatherings banned, and almost 2.7 billion workers, representing around 81 per cent of the world's workforce, were affected by partial or full lockdown regulations (ILO 2020a).

Stringent early confinement policies were implemented with the aim of reducing contagion and buy time for health systems to create additional testing and treatment capacity, but at a high cost. Beyond the drop in commodity prices and external demand, workplace closures and travel bans led to a reduction in economic activity. Simulating different scenarios for the impact of COVID-19 on global economic growth, the International Labour Organization (ILO) first warned in March 2020 against the risk of an economic and labour crisis that could increase global unemployment by between 5.3 million ('low' scenario) and 24.7 million ('high' scenario), from a base level of 188 million in 2019. Beyond job losses and business closures, underemployment was also expected to surge, as the economic consequences of the pandemic caused working hours and wages to decline, in combination leading to a sharp rise in working poverty (ILO 2020b). Informal self-employment, an activity of last resort that often serves to buffer the impact of economic shocks in developing countries, was generally not available due to the imposed restrictions. Workers in this sector, who need to earn a living on a day-to-day basis and have limited or no access to healthcare or social safety nets, were severely hit (Danquah et al. 2020; ILO 2020c).

In this paper, we investigate the immediate and near-term impact of stringent COVID-19 lockdown policies on employment and earnings, using Ghana as a case study.[1] In Ghana, a geographically contained three-week lockdown covering the Greater Accra and Greater Kumasi Metropolitan Areas and contiguous districts was implemented from 30 March to 20 April 2020, while in the rest of the country less stringent regulations were in place. We exploit this geographic variation in policy stringency levels using a difference-in-differences (DID) design, contrasting the employment outcomes of respondents in lockdown (treated) and no-lockdown (control) districts.[2]

For this study, we conducted a rapid phone survey with a subsample of 648 workers in urban areas drawn from the 2018/19 Ghana Socioeconomic Panel Survey (GSPS). The data were collected between 19 August and 17 September 2020 and comprised recall information for the months of February, before the coronavirus had reached Ghana, and April, when parts of Ghana were under lockdown, allowing us to construct a longitudinal dataset at the worker level.[3]

According to our preferred specification—which includes worker-fixed effects and limits control districts to those in a population density range that is comparable to the treated districts—legal shutdown orders induced a substantial decline in employment by 34.3 percentage points during the lockdown period. In line with the results obtained by other studies in the Sub-Saharan African context (Balde et al. 2020; Bassier et al. 2020; Lakuma and Sunday 2020), this effect was primarily driven by the break in economic activity experienced by workers in informal self-employment, who may have been most affected by lockdown policy regulations given the nature of their work.

[1] Lockdown here refers to a legally enforceable order for residents to remain in their homes except for essential trips.

[2] Other studies that have used the DID design to analyse the causal effect of the COVID-19 pandemic on employment outcomes and wellbeing are Bargain and Aminjonov (2020), Brodeur et al. (2021), and Fang et al. (2020).

[3] In this paper, the immediate impact refers to the period in April 2020 when strict lockdown policies were in place in Ghana, while the subsequent four-months period up to August/September 2020 is referred to as near-term.

At the same time, workers in informal self-employment were most likely to continue working throughout April 2020 in control districts in spite of the health risks posed by the pandemic, which may be explained by their need to earn a living on a day-to-day basis (Durizzo et al. 2021; Kazeem 2020). Importantly, our results reveal that the strong and significant immediate treatment effect of the lockdown had faded four months after restrictions had been lifted. However, nationwide, employment and labour earnings remained significantly below pre-COVID levels. Particularly the earnings of self-employed workers and of female workers remained more negatively affected in the near-term, pointing to a potential disequalising effect of the pandemic overall.

The paper is organized as follows. Section 2 provides relevant background information on the COVID-19-related policy environment in Ghana. Section 3 introduces the data, discusses our empirical approach and identification strategy, defines key variables of interest, and presents descriptive statistics. Section 4 presents our estimation results, while Section 5 provides the results of various robustness checks. Section 6 concludes.

2 Background and policy environment

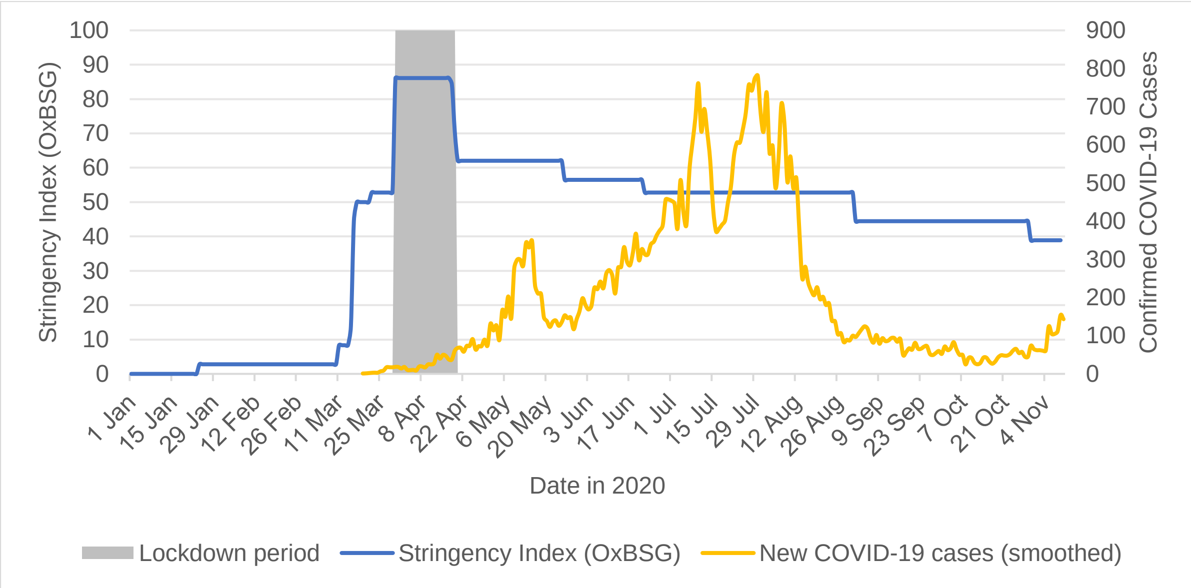
The first two cases of COVID-19 were reported in Ghana on 12 March 2020. As a first response, on 15 March, all public gatherings exceeding 25 people were banned, all schools and universities were closed, and on 23 March all borders were closed. Urban market centres, providing essential services, were exempted from the suspension (Asante and Mills 2020). Citizens were advised to strictly observe good personal hygiene and social distancing to prevent the spread of the disease.

Despite these preventive measures, cases continued to rise and the country's two largest cities, Accra and Kumasi, emerged as 'hotspots' of the disease. As a result, on 27 March, the President announced a partial lockdown of the Greater Accra and Greater Kumasi Metropolitan Areas and contiguous districts, which took effect from 30 March 2020, 48 hours after the announcement. Officers of the Ghana Police Service and Ghana Armed Forces ware tasked to strictly enforce the lockdown (Asante and Mills 2020). The lockdown required that residents of restricted districts stay at home, and all passenger travel between the restricted districts and other parts of the country was prohibited. Apart from essential workers, who continued their activities (including the production, distribution, and marketing of food, beverages, pharmaceuticals, medicine, paper, and plastic packages), people were allowed to leave home only to purchase essential goods, seek medical care, undertake banking transactions, or use public sanitation facilities. Businesses in contact intensive environments—often operated by workers in informal self-employment—such as bars and restaurants, tourism and transport businesses, hairdressers, small retail shops, and street vending, were particularly affected by direct business restrictions and social distancing measures, and the consequent reduction in customers. The partial lockdown was initially announced for a period of two weeks, but ultimately was extended to 20 April, lasting three weeks (21 days) in total. Other restrictions on public and social gatherings were gradually lifted in subsequent months.[4]

[4] On 5 June, public gatherings of up to 100 people were allowed. Junior and senior high schools and universities re-opened from 15 June. Large sporting events, political rallies, festivals, and religious events remained suspended until 31 July. From 1 August, restrictions on the number of people in public gatherings were further eased and tourist sites reopened (while beaches, pubs, cinemas, and nightclubs remained closed). International flights resumed from 1 September, while land and sea borders remained closed to human traffic.

Figure 1 illustrates the stringency of COVID-19 confinement policies implemented in Ghana between January and November 2020, as measured by the Blavatnik School of Government Coronavirus Government Response Tracker (Hale et al. 2020).

Figure 1: COVID-19 cases and government response stringency index in Ghana



Note: the stringency index shows the response level in the national subregion with the strictest policies (districts subject to lockdown regulations) and the grey shaded area indicates the lockdown period from 30 March to 19 April. The stringency index is a composite measure based on nine response indicators including school closures, workplace closures, and travel bans, rescaled to a value from 0 to 100 (strictest); it shows the pandemic response level in the districts subject to the strictest lockdown measures.

Source: authors' illustration based on Hale et al. (2020) and Roser et al. (2020).

Considering the evolution of newly confirmed COVID-19 cases (see Figure 1), the Ghanaian government was quick to implement stringent measures when case numbers were still relatively low. The number of confirmed COVID-19 infections continued to escalate during the lockdown and increased exponentially after restrictions were lifted, reaching peak levels only in late July or early August, after which the pandemic curve of the first infection wave flattened. The decision to lift the partial lockdown was largely influenced by mounting concerns regarding the severe economic burden that the restrictions posed, especially on the livelihoods of the urban poor, many of whom had by that time run out of money to buy food, due both to the hike in food prices and to the restricted possibilities to earn a living (Asante and Mills 2020).

The government of Ghana rolled out the Coronavirus Alleviation Programme (CAP) to address the disruption in economic activities. For instance, under CAP the government provided food (dry food packages and hot meals) for up to 470,000 individuals and homes in the affected areas of the restrictions. During April, May, and June, the government also fully absorbed the water bills for all Ghanaians, as well as 50 per cent of electricity bills. Electricity bills for lifeline consumers, who consume zero to 50 kilowatt hours a month, were fully absorbed for this period. Although there were no targeted government programs to provide direct earnings support, the National Board for Small Scale Industries disbursed soft loans to micro, small and medium scale businesses.

3 Data, empirical strategy, and descriptive statistics

3.1 Data sources

The sample for this study was drawn from the third round of the Ghana Socioeconomic Panel Survey (GSPS), which is a joint effort between Northwestern University and the Institute of Statistical, Social and Economic Research (ISSER) at the University of Ghana. The first round of the GSPS was collected in 2009/10, consisting of a nationally representative sample of 5,010 households in 334 enumeration areas containing 18,889 household members.[5] The two follow-up rounds were conducted in 2013/14 and 2018/19.

To construct the sampling frame for this study, we focused on the GSPS Wave 3 (W3) adult population in urban areas who were heads of household and had been working (outside of smallholder agriculture) in the last survey round. From these we drew a random sample of 918 respondents, stratified by geographic location, occupational position (wage employee vs. self-employed) and formality status (formal vs. informal employment). Among those who were contacted, 184 could not be reached, 52 refused to be interviewed, 16 were no longer members of the same household, 10 could not be unequivocally identified, and in 8 cases the interview was not completed, leaving us with a sample of 648 respondents, of whom 599 reported having been working in February 2020. We fit a probit model to test for non-random sample selection (see Table A1 Appendix for attrition rates by district treatment status), which shows that sample retention rates were lower in lockdown districts, among female respondents, and among respondents in early or late working life (see Table A2 Appendix). To correct for potential selection bias, we use this information to create inverse probability weights used in the descriptive analysis and add the inverse Mills ratio as a control to our main outcome model.

To respondents who were successfully contacted, a structured questionnaire was administered by trained local enumerators using phone interviews. The GSPS-COVID survey asked multiple questions about the respondents' perception of and compliance with the pandemic response measures implemented by the national government, and the economic and labour market impact that they had experienced (see Schotte et al., 2021 for a comprehensive overview). Concerning the latter, respondents were asked retrospectively about their household's economic wellbeing and their own employment situation in February, April, and the seven days prior to the interview, which took place between 19 August and 17 September 2020.

3.2 Empirical strategy and identification

We first investigate the policy impact at the extensive margin, focusing on the employment status of the worker. Here, the dependent variable is a binary indicator that takes on a value of one if the respondent is working (actively working or on paid leave) and zero otherwise (either temporarily or permanently out of work). Second, we investigate the impact at the intensive margin, focusing on labour earnings. Earnings are deflated to constant 2018 prices using the Ghana Statistical Service (GSS) monthly consumer price index as of August 2020 (GSS 2020).

Our DID design builds on a basic comparison between changes in employment and earnings among respondents in lockdown districts, considered 'treated', and respondents in no-lockdown districts, considered 'control'. Our analysis compares the changes in these outcomes between three points in time: (i) February 2020, the base period before the COVID-19 pandemic had reached

[5] The first and second waves of the GSPS was a collaboration between Economic Growth Center at Yale and ISSER

Ghana; (ii) April 2020, when parts of Ghana were under lockdown; and (iii) August/September 2020, when the most stringent policy measures had been relaxed. Changes that occurred between February and April 2020 (first post-treatment period) will give an indication of the immediate effects of the COVID-19 pandemic and related policy measures, while changes that occurred from February up to August/September 2020 (second post-treatment period) will give an indication of the near-term implications. In addition, a backward-looking comparison of changes in outcomes between 2018/19 and February 2020 (pre-treatment period) will serve to verify the common trends assumption underlying the DID identification strategy (provided as a robustness check).

We write the DID regression model as:

$$\begin{aligned} Y_{idt} = \beta_0 + \beta_1 LOCKDOWN_d + \beta_2(LOCKDOWN_d \times POST_{1t}) \\ + \beta_3(LOCKDOWN_d \times POST_{2t}) + \beta_4 X_i + \theta_t + \varepsilon_{idt} \end{aligned} \quad (1)$$

where the dependent variable Y_{idt} denotes the employment outcome of worker i in district d at time t. $LOCKDOWN_d$ is a dummy variable that defines the treatment status at the district level, taking on a value of one for districts that were subject to lockdown policies, and zero otherwise. $POST_{1t}$ and $POST_{2t}$ are dummy variables that take on a value of one for the first and second post-treatment period, respectively, and zero otherwise. The coefficients of the interaction terms, β_2 and β_3, yield the DID estimates that capture the effect of the lockdown policies on the outcome variables. We also control for time-fixed effects, θ_t, to identify period-specific effects across treated and control districts. X_{idt} is a vector of time-fixed worker-specific control variables (including the estimated inverse Mills ratio), and ε_{idt} is the error term. Standard errors are clustered at the district level. In consideration of the relatively small number of clusters, standard errors are bootstrapped with 100 replications.

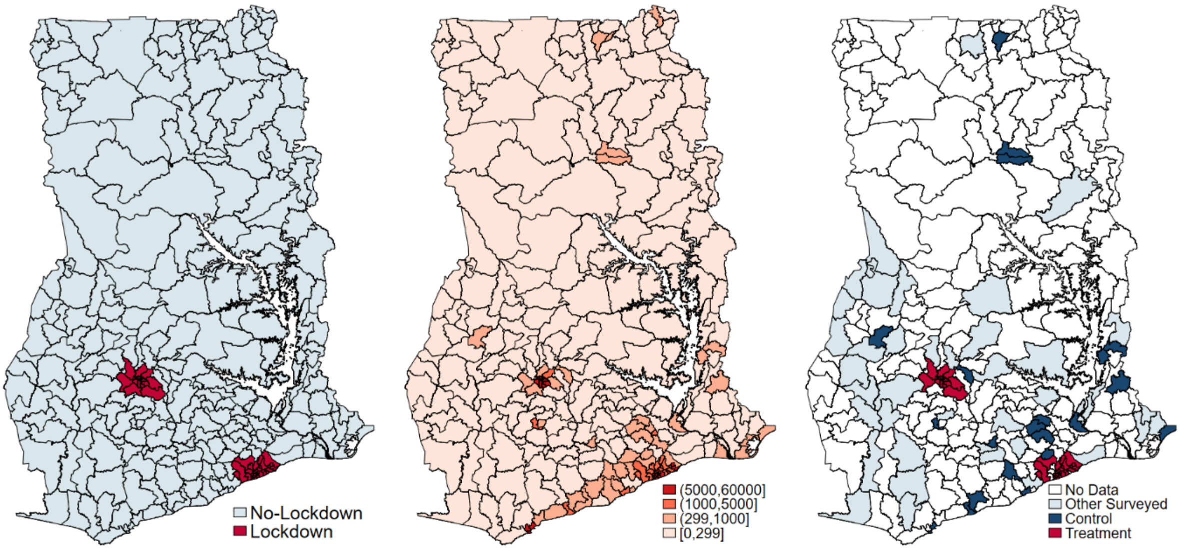
In the base specification, we estimate equation (1) using ordinary least squares (OLS) regression. Taking advantage of the panel structure of our data, we also estimate a second specification that controls for worker-fixed effects, μ_i:

$$Y_{idt} = \delta_0 + \delta_1(LOCKDOWN_d \times POST_{1t}) + \delta_2(LOCKDOWN_d \times POST_{2t}) + \mu_i + \theta_t + \varepsilon_{idt}. \quad (2)$$

This is our preferred specification, as the worker-fixed effects, μ_i, absorb any worker-specific heterogeneities that may contaminate our DID estimates (see Fang et al. 2020 for a similar specification used to quantify the causal impact of human mobility restrictions on the containment and delay of the spread of the novel coronavirus in China). Given that the location of workers is fixed in our data over the study period, the worker-fixed effects, μ_i, also absorb any time-constant differences between districts. To ensure the robustness of our results, we estimate several variants of this specification on different subsamples.

The survey data has been collected in 19 treated and 59 control districts, with the location of the respondent being pre-determined based on the 2018/19 dataset. As can be seen from Figure 2 panels (a) and (b), the lockdown treatment was not randomly assigned, but targeted the two most densely populated urban centres. To increase the comparability between the treatment and control groups, for most of the empirical estimation, control districts will be limited to those 20 that have a population density above 300/km². This cut-off value is fixed in reference to the population density in the least densely populated treated district.[6] As robustness check, we exclude the Accra Metropolitan and Kumasi Metropolitan city nucleus districts from the analysis.[7]

Figure 2: Lockdown versus no-lockdown study areas



a) Greater Accra and Greater Kumasi Metropolitan Areas and contiguous districts that were under lockdown

b) Population density across districts (population per km²)

c) Control districts limited to those with a population density above 300/km²

Note: population projections for 2020 by the Ghana Statistical Service (GSS) based on the 2010 Population and Housing Census.

Source: authors' illustration based own GSPS-COVID-19 survey.

3.3 Descriptive statistics

Worker characteristics

Table 1 reports the t-test of average worker characteristics by district treatment status (detailed summary statistics of all variables for the full and restricted samples are provided in Tables A3+A4 Appendix). Reflecting our sampling design, 91.7 per cent of respondents were working in February 2020, of whom 24.2 per cent were in formal employment, and 34.3 per cent were in wage employment.[8] Thus, despite the urban focus and exclusion of agriculture, the informality rate in our sample is above 70 per cent, and every second worker was in informal self-employment prior to the pandemic (both matching the shares observed in earlier GSPS rounds).

While other worker characteristics are balanced between control and treated groups, we find that the average household size reported at the time of the interview (August/September 2020) among respondents in lockdown districts was significantly smaller than in districts not affected by the

[6] Note that most districts in this preferred control group do not directly border treated districts, which may reduce potential spill over effects (see Figure 2c).

[7] As an additional robustness check, we estimated a linear regression with endogenous treatment assignment (using the etregress in STATA, modelling treatment status as a function of the district population density, which yielded very similar estimates. Results are available from the authors upon request.

[8] Wage workers with written contracts and any social security withholdings from their salaries (for medical care or retirement provisions) are classified as formal. Self-employed workers are classified as formal if operating an enterprise that is officially registered with relevant national institutions.

lockdown, and in fact had declined by 0.64 members on average compared with the 2018/19 estimate. Previous research has shown that, in anticipation of the lockdown restrictions and the expected consequences for doing business in affected districts, a non-negligible number of migrant workers in Ghana relocated to their hometowns between 28 and 29 March 2020 (Asante and Mills 2020; see also Lee et al. 2020 for similar evidence from India). If respondents with a higher risk of losing work during the lockdown were more likely to move out of treated districts and continue work in districts with no lockdown policies in place, this self-selection could cause our estimates to be biased. We check the robustness of our findings to the exclusion of movers in Section 5.

Table 1: Average worker characteristics by district treatment status

Characteristics in Aug/Sep 2020 (unless otherwise specified)	**(1) Lockdown**	**(2) No-lockdown**	**(3) No-lockdown size cut-off**	**(1) - (3) Difference**	**P-value Ha: diff != 0**
Female	0.534	0.575	0.553	-0.019	0.705
	(0.031)	(0.026)	(0.039)	(0.049)	
Age in years	45.5	42.9	43.8	1.7	0.192
	(0.827)	(0.690)	(0.996)	(1.294)	
Head of household	0.805	0.780	0.821	-0.016	0.698
	(0.026)	(0.023)	(0.032)	(0.041)	
Household size	2.583	3.416	3.431	-0.848***	0.000
	(0.094)	(0.110)	(0.167)	(0.192)	
Moved since last interview	0.090	0.117	0.122	-0.033	0.292
	(0.018)	(0.017)	(0.025)	(0.031)	
Married (2018/19)[a]	0.445	0.466	0.442	0.003	0.947
	(0.031)	(0.026)	(0.039)	(0.050)	
Secondary education (2018/19)[a]	0.180	0.229	0.205	-0.025	0.544
	(0.024)	(0.023)	(0.033)	(0.041)	
Tertiary education (2018/19)[a]	0.124	0.109	0.157	-0.033	0.297
	(0.019)	(0.015)	(0.026)	(0.032)	
Working in Feb 2020	0.930	0.906	0.917	0.012	0.656
	(0.016)	(0.017)	(0.022)	(0.028)	
Formal employment in Feb 2020	0.221	0.260	0.248	-0.027	0.533
	(0.026)	(0.024)	(0.035)	(0.043)	
Wage employment in Feb 2020	0.390	0.304	0.337	0.053	0.278
	(0.031)	(0.025)	(0.038)	(0.049)	
Number of observations	272	376	169	441	441

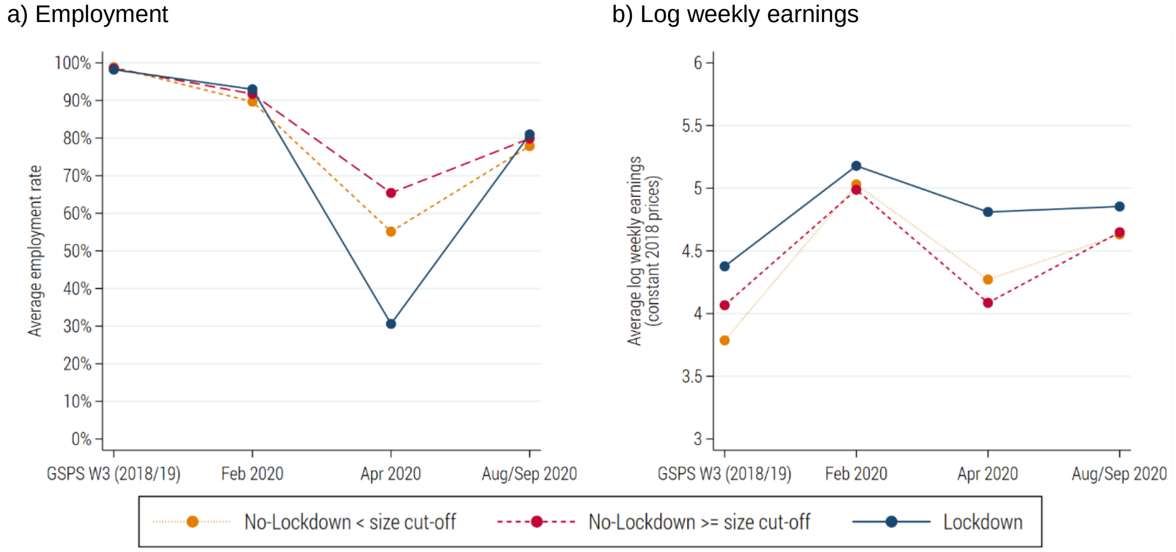
Notes: [a] Information not collected in the GSPS-COVID survey and therefore taken from GSPS Wave 3 (2018/19). Inverse probability of attrition weights used. Standard errors in parentheses, * p<0.10, ** p<0.05, *** p<0.01.

Source: authors' calculations based on GSPS-COVID-19 survey.

Trends in employment and earnings

The trends displayed in Figure 3a reveal a much sharper drop in employment rates in treated versus control districts during the lockdown period. Specifically, 65.9 per cent of respondents in no-lockdown districts continued working throughout April 2020, compared with 32.1 per cent of respondents in lockdown districts. Importantly, the majority (52.2 per cent) of respondents in lockdown districts said that they had stopped working temporarily, while 15.7 per cent considered this break to be permanent. In line with this perception, we observe a strong near-term recovery. At the time of the interview (August/September 2020), the gap in employment rates between lockdown and no-lockdown districts had closed. In districts that had been under lockdown, 83.8 per cent of respondents who had been working in February 2020 were observed to be working again, compared with 84.9 per cent of respondents in no-lockdown districts.

Figure 3: Time trends in employment and earnings, lockdown versus no-lockdown districts



Note: the GSPS-COVID-19 sample was drawn from the GSPS W3 adult population in urban areas, limited to those who were heads of household and had been working in 2018/19. We distinguish no-lockdown districts below and above the population density cut-off value set at 300/km².

Source: authors' illustration based on GSPS-COVID-19 survey.

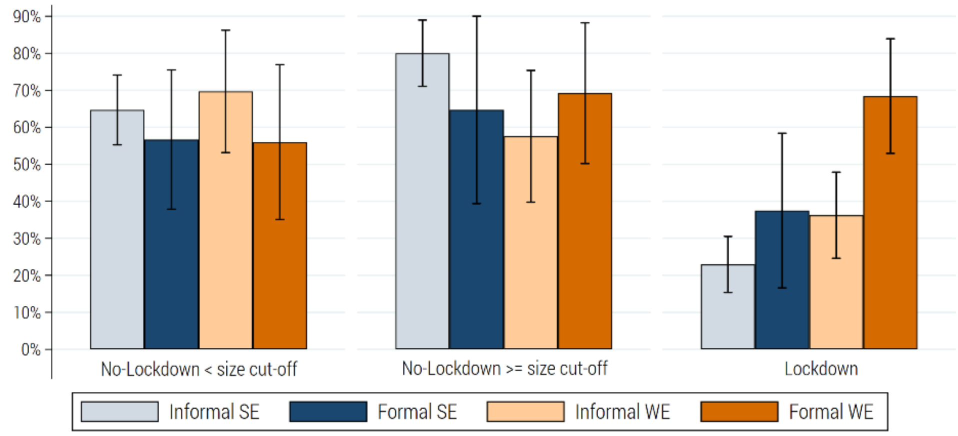
At the intensive margin, pre-COVID average earnings tended to be higher in treated than control districts, but followed relatively similar trends up to February 2020, when considering the preferred control group (Figure 3b).[9] The drop in average log weekly earnings during the lockdown period was more pronounced in districts not affected by the lockdown. This pattern is likely explained by a selection effect. Importantly, in districts under lockdown, a substantially larger share of workers had stopped working completely, and workers in informal self-employment were the most affected (Figure 4). This finding matches the evidence presented by other studies, which have shown that informal workers were at higher risk of dropping out of work, as they generally lack mechanisms for collective bargaining and tend to be in activities that are contact-intensive and thus particularly affected by the pandemic response measures—such as restaurants, tourism businesses, small retail shops, hairdressing, and taxi driving (Balde et al. 2020). As most workers in this group rely on daily sales for their earnings (Danquah et al. 2019), we also observe that workers in informal self-employment were the most likely to continue working in no-lockdown districts, in spite of the danger posed by the pandemic (similar patterns have been presented by Durizzo et al. 2021; Kazeem 2020). This suggests that a larger share of low-income workers continued working in no-lockdown districts, while formal wage workers, who tend to hold higher paying jobs, were the most likely to continue working in April in lockdown districts.[10] As for employment, we observe

[9] The visual analysis suggests that the common trends assumption for the pre-treatment period is more likely to hold when defining the control group, which comprises no-lockdown districts with a population density above 300/km². This will be formally tested in the next section.

[10] This can be attributed to the higher level of job security and employment protection characterizing these jobs (Danquah et al. 2019). It may also be partly explained by the type of tasks performed in these jobs, which tend to be higher skilled and may more often be performed from home.

a recovery in earnings up to August/September 2020 throughout the country, with earnings levels nevertheless remaining below the February average.

Figure 4: Employment rates in lockdown and no-lockdown districts in April, by work status in February 2020



Notes: sample limited to respondents who had been working February 2020. SE = self-employed; WE = wage employed. Average shares with 95% confidence intervals.

Source: authors' illustration based on GSPS-COVID-19 survey.

Potential confounders

To identify the treatment effect of the lockdown, we need to be confident that no confounding events occurred around the same time that had a differential impact on workers in treated versus control districts. While we may not be able to rule these out completely, we carefully considered two of the main confounding factors which may concern our analysis.

First, differential effects of the pandemic shock across industries may confound the analysis, if the sectoral composition varied substantially between treated and control districts. Between February and May 2020, the global shock of the pandemic resulted in dampening global demand for cocoa, crude oil, and merchandised exports from Ghana (see Table A5 Appendix). Ghana's cocoa sector employs approximately 800,000 farm families spread over six of the ten regions. While smallholder farmers are excluded from the analysis, indirect effects likely percolated to the entire economy, including both treated and control areas. Similarly, the direct effect of the reduction in crude oil exports on employment in our sample is expected to be small, while indirect effects likely affected Ghana's economy across the board through a variety of channels, which cannot be spatially delimited. In addition, the hospitality service sector was adversely impacted by border closures and the general decline in tourism and international travel. Major tourist destinations in Ghana are spread across both treated and control areas, including Greater Accra and Kumasi, as well cultural heritage sites and national parks in the Volta, Central and Western Regions. Lastly, manufacturing was adversely impacted given its dependence on imports of raw materials and sharply disrupted supply chains. Major industrial centers in Ghana include the treated areas Accra and Kumasi, as well as Tamale and Takoradi in the control group. Importantly, in line with this discussion, we find no substantial differences in the pre-COVID sectoral composition of employment by district treatment status (see Table A6 Appendix).Second, workers' behavioral responses may confound the analysis; for example, if workers were inclined to stop work out of health concerns, independent of the lockdown treatment, or if government relief measures varied between treated and control districts and induced behavioural responses in terms of workers' labour supply decisions. When being asked about the aspects of the COVID-19 pandemic that had the largest impact on them personally, just under two-thirds of respondents selected unemployment or loss of income as the most important factor. Importantly, this applies equally to respondents located in lockdown versus no-lockdown districts (see Table A7 Appendix). Being sick or fear of getting sick was only mentioned by 13 per cent of respondents, without any statistically significant difference by district treatment status. Similarly, among those who had been working in February 2020 and had stopped work in April, 73.6 per cent named workplace and business closures due to government regulations or restrictions on mobility as the main reason for dropping out of work, again showing no statistically significant difference between treated and control districts (see Table A8 Appendix). This increases our confidence that observed labour market effects are mainly attributable to government policies, and to a lesser extent reflect general economic effects (17.6 per cent mentioned a lack of work/customers) or behavioural responses in relation to health concerns (across the sample, only 6 per cent mentioned being sick or fear of getting sick as the reason for stopping work).

The CAP government support measures were provided to ease the welfare effects of the pandemic. In line with government targeting, we observe that the receipt of free food parcels or hot meals was mainly confined to lockdown areas. These were provided by the government to ease the economic implications of the strict lockdown regulations on the poorest. At a smaller scale, religious and non-governmental organisation provided similar support to poor families in no-lockdown districts (see Table A9 Appendix). While we suspect that the provision of these increased compliance with lockdown measures, especially among low-income earners, we do not expect that these would have influenced employment decisions in absence of government restrictions on economic activity. This is, these in-kind provisions were intended to reduce hunger in a moment when many of the poor had (temporarily) lost their means of subsistence due to government restrictions on economic activity and mobility, and are considered insufficient to have induced people to stop working. The use of other support measures—such as the provision of free water supplies, subsidized electricity, and bank credit—which may have directly influence business performance, was largely balanced between treated and control districts.

As we do not find any indication of systematic bias by geographic location along any of the two considered dimensions, this increases our confidence that the pandemic as such would have similarly affected workers in treated and control districts in absence of the lockdown treatment.

4 Estimation results

4.1 Impact of the lockdown on employment

Table 2 shows the linear probability estimates of working in April 2020 and in the seven days prior to the interview in August/September 2020, relative to the base period in February 2020, depending on the treatment status of the districts where workers are located. Column (1) presents the estimates for the full sample, while columns (2)–(4) present estimates for our preferred sample specification, limiting no-lockdown control districts to those with a minimum population density of 300/km². Column (3) controls for a set of worker-level covariates—including gender, head of household, age categories (<25; 25–34; 35–44; 45–54; 55–64; 65+), marital status (married in 2018/19), education levels (pre-primary; primary; basic; secondary; tertiary; missing in 2018/19), and household size—while column (4) uses worker-level fixed effects in the regression.

The lockdown measures implemented in parts of the country had a large and significant negative immediate impact on employment in the affected districts. This effect is more pronounced when limiting control districts to those with a minimum population density. According to our preferred specification, which controls for individual-fixed effects to absorb worker-specific heterogeneities that may contaminate our DID estimates, workers located in districts under lockdown had a 34.3 per cent lower chance to continue working throughout April 2020 (column (4)), compared with workers located in districts with less stringent policies in place. On aggregate, workers in lockdown districts faced a 60.3 per cent risk of dropping out of work in April, compared with an average ceteris paribus risk of 26.0 per cent faced by workers in no-lockdown districts.

Confirming the descriptive patterns, we observe a strong recovery in employment about four months after the lockdown policies were lifted. As our estimates presented in Table 2 indicate, there was no statistically significant difference in chances of employment between lockdown and no-lockdown districts in August/September 2020. However, the average probability of being in work at the time of the interview was still 10.7 per cent below the February 2020 level.

Table 2: Impact of the coronavirus lockdown on employment

Dependent variable: Working in period *t* (=1 if YES)	**(1) Full sample**	**(2) District size cut-off**	**(3) District size cut-off with covariates**	**(4) District size cut-off with worker FE**
Post-period (base Feb 2020)				
April 2020	-0.282***	-0.262***	-0.262***	-0.260***
	(0.031)	(0.039)	(0.039)	(0.035)
Aug/Sep 2020	-0.125***	-0.117***	-0.117***	-0.107***
	(0.019)	(0.029)	(0.029)	(0.022)
Lockdown	0.016	0.011	0.022	
	(0.031)	(0.036)	(0.036)	
Lockdown × April 2020	-0.319***	-0.350***	-0.350***	-0.343***
	(0.047)	(0.058)	(0.058)	(0.046)
Lockdown × Aug/Sep 2020	-0.007	-0.008	-0.010	-0.007
	(0.025)	(0.033)	(0.033)	(0.029)
Inverse mills ratio (sample)	YES	YES	YES	NO
Covariates	NO	NO	YES	NO
Worker-fixed effects (FE)	NO	NO	NO	YES
Observations	1944	1323	1323	1323

Note: covariates include gender, head of household, age categories (<25; 25–34; 35–44; 45–54; 55–64; 65+), marital status (married in 2018/19), education levels (pre-primary; primary; basic; secondary; tertiary; missing in 2018/19), and household size. Linear probability model; FE = fixed effects (within) regression; Bootstrapped standard errors in parentheses, clustered at the district level; *p<0.10, **p<0.05, ***p<0.01.

Source: authors' estimates based on GSPS-COVID-19 survey.

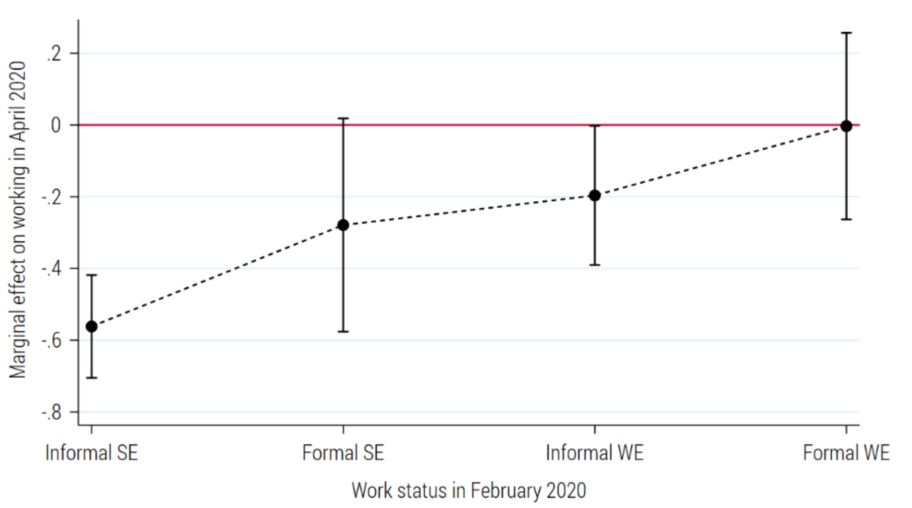
In the following, we provide indicative evidence concerning potential heterogeneities in the treatment effect.[11] Here, we focus on the first post-treatment period up to April 2020, for which we find a strong and significant impact of the lockdown, and limit the sample to respondents who were working in February 2020.

First, to check for potential heterogeneities across workers groups, we interact the treatment status, defined at the district level, with the workers' initial work status, defined by formality status (informal vs. formal) and occupational position (self- vs wage employment). Figure 5 reports the

[11] Note that some of the subgroups presented here are relatively small and we have not been able to verify the parallel trends assumption within all subgroups. Results should therefore be interpreted as indicative.

average marginal effects of the lockdown on the chances of employment in April 2020, by initial work status in February. Confirming our descriptive results, the negative impact of the lockdown on employment was most pronounced for workers in informal self-employment, while workers in formal wage work did not face a higher risk of being out of work in lockdown versus no-lockdown districts.[12] Interestingly, independent of the formality status, the lockdown seems to have affected self-employed workers more than wage employees (Figure 5). This observation could be explained by a larger decline in the activity of micro and small enterprises (often operated by own account workers or family enterprises without no external employees) compared with medium and large enterprises (Lakuma and Sunday 2020).

Figure 5: Impact of the coronavirus lockdown on employment in April, by work status in February 2020



Note: SE = self-employed; WE = wage employed. Control districts limited to those with a population density above 300/km². Sample limited to respondents who had been working in February 2020. Each point shows the estimated average marginal effect of the lockdown on employment in April 2020, by work status in February 2020. The dashed lines show the 95% confidence intervals. Bootstrapped standard errors clustered at the district level.

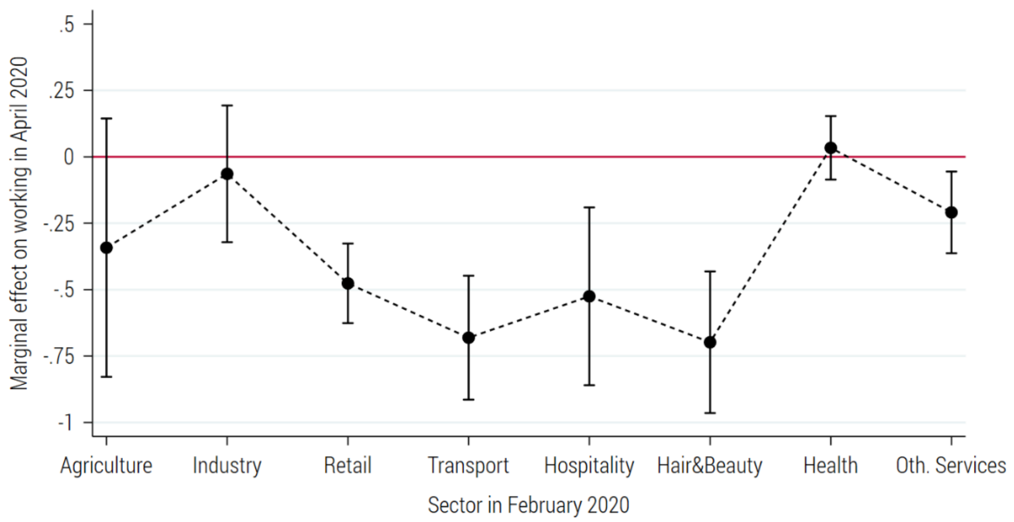
Source: authors' estimates based on GSPS-COVID-19 survey.

Second, considering that lockdown regulations are screwed toward limiting economic activity in certain areas, we check for potential heterogeneities in the treatment effect by initial sector of employment.[13] Figure 6 reports the results. As expected, the negative impact of the lockdown was concentrated among workers in more contact intensive sectors—like retail, including street vending, transport, hospitality, and personal services such as hairdressers, barbers, and beauticians—which tend to be characterised by a relatively high rate of informal business activities. By contrast, the lockdown regulations did not affect workers in industry and health services, and had a more moderate effect on other service workers, which includes those in the public sector.

[12] Note that 60 percent of formal wage workers in our sample work in the public sector.

[13] As discussed in section 3.3, there are no substantial differences in the sectoral composition of employment between treated and control districts. Altering the model specification to include sector-time fixed effects does not qualitatively change the results reported in Table2 (results are available from the authors upon request).

Figure 6: Impact of the coronavirus lockdown on employment in April, by sector of employment in February 2020



Note: Control districts limited to those with a population density above 300/km². Sample limited to respondents who had been working in February 2020. Each point shows the estimated average marginal effect of the lockdown on employment in April 2020, by sector of employment in February 2020. The dashed lines show the 95% confidence intervals. Bootstrapped standard errors clustered at the district level.

Source: authors' estimates based on GSPS-COVID-19 survey.

4.2 Impact of the lockdown on earnings

Table 3 shows the estimates on log weekly earnings by period and district treatment status. We observe that from February to April 2020, earnings fell more sharply in no-lockdown districts. As discussed in Section 3.3, this is likely explained by the selection of workers who were able to continue work in spite of the lockdown, as here we only consider non-zero earnings. When accounting for zero earnings of workers who dropped out of employment, we find a large and significant negative immediate treatment effect of the lockdown on earnings, which had faded four months after the restrictions were relaxed (see Table A10 Appendix).

Importantly, we find no statistically significant near-term impact of the coronavirus lockdown measures on earnings. However, across the sample, earnings in the seven days prior to the interview remained significantly below the pre-COVID level (Table 3). On average, depending on the specification, average weekly earnings in August/September were between 0.298 and 0.429 log points lower than in February 2020. These results imply a drop ranging from GH₵68.3 to GH₵92.4 relative to a base of GH₵265 in February 2020, equivalent to a decline of 25.8–34.9 per cent. It is important to note that in this estimation, all earnings have been deflated to constant 2018 prices, taking into account sharp price increases in consumer products and the falling purchasing power of earnings since the onset of the pandemic. Without accounting for inflation, a somewhat smaller decline in average weekly earnings of 20.2–29.9 per cent would have been estimated, depending on the specification.

Table 3: Impact of the coronavirus lockdown on log weekly earnings (non-zero)

Dependent variable: Log weekly earnings in period *t* (constant 2018 prices)	**(1) Full sample**	**(2) District size cut-off**	**(3) District size cut-off with covariates**	**(4) District size cut-off with worker FE**
Post-period (base Feb 2020)				
April 2020	-0.770***	-0.807***	-0.805***	-0.613***
	(0.099)	(0.191)	(0.170)	(0.124)
Aug/Sep 2020	-0.375***	-0.343***	-0.298***	-0.429***
	(0.051)	(0.085)	(0.086)	(0.096)
Lockdown	0.165*	0.170	0.179	
	(0.089)	(0.134)	(0.117)	
Lockdown × April 2020	0.433***	0.466*	0.398*	0.307**
	(0.148)	(0.242)	(0.211)	(0.139)
Lockdown × Aug/Sep 2020	0.077	0.044	-0.009	0.108
	(0.110)	(0.133)	(0.130)	(0.109)
Inverse mills ratio (sample)	YES	YES	YES	NO
Covariates	NO	NO	YES	NO
Worker-fixed effects (FE)	NO	NO	NO	YES
Observations	1044	700	700	700

Note: covariates include gender, head of household, age categories (<25; 25–34; 35–44; 45–54; 55–64; 65+), marital status (married in 2018/19), education levels (pre-primary; primary; basic; secondary; tertiary; missing in 2018/19), household size. FE = fixed effects (within) regression; bootstrapped standard errors in parentheses, clustered at the district level; *p<0.10, **p<0.05, ***p<0.01.

Source: authors' estimates based on GSPS-COVID-19 survey.

Next, we explore the characteristics of workers who experienced a sharper, lasting decline in earnings. The results are reported in Table 4. We find that the earnings of self-employed workers and the earnings of women remain more heavily affected in the near term. Given that women generally have lower earnings than men, and most self-employed workers are in the informal sector in Ghana, this finding gives rise to concerns that the pandemic may have aggravated existing labour market inequalities, leaving workers who had already been in a more vulnerable position prior to the pandemic in a yet more precarious position (see Crossley et al. 2020 for similar findings for the UK).

Table 4: Changes in log weekly earnings (non-zero) by employment status in February 2020

Dependent variable: **Log weekly earnings in period *t*** **(constant 2018 prices)**	**(1)** **Full sample**	**(2)** **Full sample with covariates**	**(3)** **Full sample with covariates**	**(4)** **Full sample with worker FE**
Post-Period (base Feb 2020)				
Aug/Sep 2020	-0.186*	-0.190**	-0.155*	-0.232***
	(0.098)	(0.086)	(0.087)	(0.063)
Self-employed in Feb 2020	0.075	0.293***	0.276***	
	(0.108)	(0.103)	(0.098)	
Formal work in Feb 2020	0.370***	0.229**	0.233**	
	(0.096)	(0.095)	(0.094)	
Female		-0.252**	-0.198*	
		(0.105)	(0.113)	
Aug/Sep 2020 × Self-employed in Feb 2020	-0.288***	-0.291***	-0.254**	-0.160**
	(0.099)	(0.097)	(0.102)	(0.075)
Aug/Sep 2020 × Formal in Feb 2020	0.159	0.176	0.165	0.076
	(0.112)	(0.110)	(0.109)	(0.094)
Aug/Sep 2020 × Female			-0.125	-0.186**
			(0.111)	(0.085)
Inverse mills ratio (sample)	YES	YES	YES	NO
Covariates	NO	YES	YES	NO
Worker-fixed effects (FE)	NO	NO	NO	YES
Observations	863	863	863	863

Note: Covariates include gender, head of household, age categories (<25; 25–34; 35–44; 45–54; 55–64; 65+), marital status (married in 2018/19), education levels (pre-primary; primary; basic; secondary; tertiary; missing in 2018/19), household size. FE = fixed effects (within) regression; bootstrapped standard errors in parentheses, clustered at the district level; *p<0.10, **p<0.05, ***p<0.01.

Source: authors' estimates based on GSPS-COVID-19 survey.

5 Robustness checks

To ensure the robustness of our findings, we estimate several variants of our preferred model specification (see column (4) in Tables 2+3) on different subsamples. First, we test whether our data support the assumption of common pre-treatment trends in employment outcomes of treated and control groups, underlying the DID identification. Second, to test for potential bias due to self-selection, we examine whether our results are robust to the exclusion of workers who have moved since the 2018/19 panel round. Third, to ensure that our results are not driven exclusively by the two major metropolitan districts, which together account for 65.6 per cent of the treated observations, we re-estimate the impact excluding the Accra Metropolitan and Kumasi Metropolitan city nucleus districts from the analysis, thus only keeping adjoining districts that were under lockdown. In addition, we define a randomly generated set of districts as the treatment districts as placebo test.

5.1 Testing the parallel trends assumption

To validate the parallel pre-trends assumption underlying our DID design, we use information on the employment outcomes of respondents reported in GSPS W3 (2018/19) and February 2020, before the coronavirus pandemic had reached Ghana. Table 5 reports the results. As explained before, our sample was drawn from the GSPS W3 adult population, limited to those who had been working in 2018/19. As indicated in column (1), across the sample, about 6 per cent of respondents had dropped out of employment by February 2020. Moreover, as reported in column (2), we find a positive trend in real earnings between 2018/19 and February 2020. Importantly, we find no evidence of a statistically significant difference in pre-treatment trends between workers in treated and control districts. That is, the coefficient estimates for the interaction terms $LOCKDOWN_d \times PRE_t$ are statistically insignificant, suggesting that the parallel pre-trend assumption is plausible.

Table 5: Pre-treatment trends in employment outcomes by treatment status

	(1) **Working in period *t* (=1 if YES)**	(2) **Log weekly earnings (constant 2018 prices) in period *t***
Pre-period (base Feb 2020)		
GSPS W3 (2018/19)	0.066***	-0.759***
	(0.022)	(0.143)
Lockdown × GSPS W3	-0.014	0.091
	(0.030)	(0.189)
Observations	880	728
Panel effects	FE	FE

Note: FE = fixed effects (within) regression; control districts limited to those with a population density above 300/km²; standard errors in parentheses, clustered at the district level; *p<0.10, **p<0.05, ***p<0.01.

Source: authors' estimates based on GSPS-COVID-19 survey.

The results reported in Table 5 use our preferred model specification. For completeness, we also estimate a set of alternative specifications that combine the pre-treatment period and the two post-treatment periods in the same estimation. The results, reported in Tables A11 and A12 Appendix, reconfirm the above result. As can be seen from Table A10, we fail to reject that labour earnings in years prior to treatment exhibit parallel trends when we estimate the regression for the full sample, as shown in column (1). However, as shown in columns (2)–(4), we find no statistically significant difference in pre-treatment trends between workers in treated and control districts once limiting no-lockdown control districts to those with a minimum population density of 300/km².

5.2 Exclusion of movers

As discussed in Section 3.3, one possible concern with our DID approach is self-selection out of treatment as workers move between treated and control districts. To test for potential bias due to self-selection, we re-estimate the impact of the lockdown on employment outcomes excluding respondents who reported having moved since the 2018/19 panel round. Importantly, not all of these respondents moved between treated and control districts. The coefficient estimates on the reduced sample of stayers are reported in Table 6. When reducing the sample to respondents who remained in the same geographic location between 2018/19 and August/ September 2020, we find no significant difference in the impact of the lockdown on employment (column (1)). However, we find weak evidence for a somewhat larger gap in average post-treatment earnings between workers in treated versus control districts (column (2)). This is mainly explained by the lower earnings reported by workers in lockdown districts who had moved since the last round of interviews in 2018/19, who were excluded in this estimation.

Table 6: Impact of the coronavirus lockdown on employment outcomes, exclusion of movers

	(1) **Working in period *t* (=1 if YES)**	**(2)** **Log weekly earnings (constant 2018 prices) in period *t***
Post-period (base Feb 2020)		
April 2020	-0.282***	-0.710***
	(0.038)	(0.121)
Aug/Sep 2020	-0.114***	-0.507***
	(0.026)	(0.085)
Lockdown × April 2020	-0.349***	0.386***
	(0.050)	(0.126)
Lockdown × Aug/Sep 2020	-0.0208	0.216**
	(0.035)	(0.100)
Observations	1,178	636
Panel effects	FE	FE

Note: FE = fixed effects (within) regression; control districts limited to those with a population density above 300/km²; standard errors in parentheses, clustered at the district level; *p<0.10, **p<0.05, ***p<0.01.

Source: authors' estimates based on GSPS-COVID-19 survey.

5.3 Exclusion of major metropolitan districts and random treatment assignment

The lockdown in Ghana was implemented in the two largest cities, Accra and Kumasi, which had emerged as 'hotspots' of the pandemic. It affected the immediate Accra Metropolitan and Kumasi Metropolitan districts as well as the Greater Metropolitan Areas and contiguous districts. To ensure that our results are not driven exclusively by the two major city centres, which together account for 65.6 per cent of the treated observations, we re-estimate the impact of the lockdown on employment outcomes, excluding the Accra Metropolitan and Kumasi Metropolitan districts.

The results are reported in columns (1) and (2) of Table 7. We find a somewhat smaller immediate treatment effect on employment in April 2020. At the intensive margin, for the first post-treatment period, we observe a slightly smaller differential trend in earnings between treated and control groups. However, The differences in coefficient estimates are not statistically significant and the overall patterns remain robust across specifications.

In addition, as a final robustness check, we estimate a specification with random treatment assignment at the district level.[14] Out of 78 districts covered in our data, 39 exceed the defined population density cut-off, of which 19 districts had lockdown policies in place, while 20 are in the control group. In this final specification, we randomly assign treatment status to 19 out of the 39 districts. As the results reported in columns (3) and (4) of Table 7 indicate, we find no statistically significant effect for this placebo treatment.

[14] See Bertrand et al. (2004) for a discussion of this robustness test, where we are in effect enacting 'placebo' lockdowns on 'treatment' districts that are chosen at random.

Table 7: Impact of the coronavirus lockdown on employment outcomes, variation in treatment assignment

	Exclude metropolitan districts		**Random treatment assignment**	
	(1)	**(2)**	**(3)**	**(4)**
	Working in period *t* (=1 if YES)	**Log weekly earnings in period *t* (constant 2018 prices)**	**Working in period *t* (=1 if YES)**	**Log weekly earnings in period *t* (constant 2018 prices)**
Post-period (base Feb 2020)				
April 2020	-0.260***	-0.613***	-0.448***	-0.498**
	(0.032)	(0.103)	(0.039)	(0.162)
Aug/Sep 2020	-0.107***	-0.429***	-0.099***	-0.384***
	(0.023)	(0.082)	(0.027)	(0.078)
Lockdown × April 2020	-0.299***	0.258**	-0.045	0.038
	(0.060)	(0.124)	(0.054)	(0.171)
Lockdown × Aug/Sep 2020	-0.001	0.101	-0.023	0.039
	(0.039)	(0.110)	(0.034)	(0.102)
Observations	786	437	1,323	700
Panel effects	FE	FE	FE	FE

Note: FE = fixed effects (within) regression; control districts limited to those with a population density above 300/km²; standard errors in parentheses, clustered at the district level; *p<0.10, **p<0.05, ***p<0.01.

Source: authors' estimates based on GSPS-COVID-19 survey.

6 Conclusions

In this paper, we provide valuable causal evidence that stringent lockdown policies impact on employment outcomes, using Ghana as a case study. We find that the three-week lockdown of the Greater Accra and Greater Kumasi Metropolitan Areas and contiguous districts, which was in effect from 30 March to 21 April 2020, had a large and significant immediate negative impact on employment in the affected districts. However, the lockdown also provided the opportunity for the Ghanaian government to build her capacity to trace, test, isolate and quarantine, and treat victims of the disease. This to a large extent led to the suppression of the transmission of the virus and therefore limited the impact of the virus on social and economic life. Many Ghanaians in the lockdown districts have been able to resume work after the lockdown was relaxed.

While the gap in employment between workers located in treated versus control districts had narrowed four months after legal shutdown orders had been lifted, we find a persistent nationwide effect of the pandemic on employment outcomes in Ghana, at both the extensive and the intensive margins. This effect, however, does not seem to depend on the stringency level of confinement policies, but may rather be attributable to an overall economic decline, which in the case of Ghana has been driven by the global drop in commodity prices and external demand from the main trading partners—including China, India, the United States, and several European countries—amongst other factors.

Importantly, we find that the immediate employment effect of the lockdown was felt most by workers in informal self-employment and, across the country, the earnings of self-employed workers and women remained more negatively affected in the near term. To this extent, our results also echo concerns regarding the poverty and livelihoods implications of the COVID-19 pandemic. As Bassier et al. (2020) point out in their analysis on South Africa, not only were informal workers and their households particularly vulnerable to the negative economic consequences of the pandemic and associated lockdown measures, considering their need to earn a living on a daily basis, but the very fact of their informality also presented a challenge for governments to provide targeted economic relief. To prevent a persistent deepening of existing vulnerabilities and labour market inequalities, our results point to a continued need for effective strategies to address the business and livelihood needs of small business owners, especially women and those operating in the informal sector.

References

Asante, L.A., and R.O. Mills (2020). 'Exploring the Socio-Economic Impact of COVID-19 Pandemic in Marketplaces in Urban Ghana'. *Africa Spectrum*, 55(2): 170–81. https://doi.org/10.1177/0002039720943612

Balde, R., M. Boly, and E. Avenyo (2020). 'Labour Market Effects of COVID-19 in Sub-Saharan Africa: an Informality Lens from Burkina Faso, Mali and Senegal'. Working Paper 022. Maastricht: UNU-MERIT.

Bargain, O., and U. Aminjonov (2020). 'Trust and Compliance to Public Health Policies in Times of COVID-19'. *Journal of Public Economics*, 19: 104316. https://doi.org/10.1016/j.jpubeco.2020.104316

Bassier, I., J. Budlender, R. Zizzamia, M. Leibbrandt, and V. Ranchhod (2020). 'Locked down and Locked out: Repurposing Social Assistance as Emergency Relief to Informal Workers'. *World Development*, 139: 105271. https://doi.org/10.1016/j.jpubeco.2020.104316

Bertrand, M., E. Duflo, and S. Mullainathan (2004). 'How Much Should We Trust Differences-in-Differences Estimates?'. *The Quarterly Journal of Economics*, 119(1): 249–75. https://doi.org/10.1162/003355304772839588

Brodeur, A., A.E. Clark, S. Fleche, and N. Powdthavee (2021). 'COVID-19, Lockdowns and Well-being: Evidence from Google Trends'. *Journal of Public Economics*, 193: 104346. https://doi.org/10.1016/j.jpubeco.2020.104346

Crossley, T.F., P. Fisher, and H. Low (2020). 'The Heterogenous and Regressive Consequences of COVID-19: Evidence from High Quality Panel Data'. *Journal of Public Economics*, 19: 104334. https://doi.org/10.1016/j.jpubeco.2020.104334

Danquah, M., S. Schotte, and K. Sen (2019). 'Informal Work in Sub-Saharan Africa: Dead End or Steppingstone?'. UNU-WIDER Working Paper 107/2019. Helsinki: UNU-WIDER. https://doi.org/10.35188/UNU-WIDER/2019/743-9

Danquah, M., S. Schotte, and K. Sen (2020). 'COVID-19 and Employment: Insights from the Sub-Saharan African Experience'. *The Indian Journal of Labour Economics*, 63: 23–30. https://doi.org/10.1007/s41027-020-00251-4

Durizzo, K., E. Asiedu, A.V.D. Merwe, A. Niekerk, and I. Gunther (2021). 'Managing the COVID-19 Pandemic in Poor Urban Neighbourhoods: the Case of Accra and Johannesburg'. *World Development*, 137: 105175. https://doi.org/10.1016/j.worlddev.2020.105175

Fang, H., L. Wang, and Y. Yang (2020). 'Human Mobility Restrictions and the Spread of the Novel Coronavirus (2019-nCoV) in China'. *Journal of Public Economics*, 19: 104272. https://doi.org/10.1016/j.jpubeco.2020.104272

GSS (2020). Newsletter: Consumer Price Index (CPI) October 2020. Accra: Ghana Statistical Service. Available at: https://www.statsghana.gov.gh/gssmain/storage/img/marqueeupdater/Newsletter_CPI_Oct_2020.pdf (accessed 15 November 2020).

Hale, T., A. Petherick, T. Phillips, and S. Webster (2020). 'Oxford COVID-19 Government Response Tracker (OxCGRT)', last updated 10 November 2020, 08:30 (London time). Available at: https://ourworldindata.org/coronavirus (accessed 10 November 2020).

ILO (2020a). *ILO Monitor, second edition: COVID-19 and the World of Work, Updated Estimates and Analysis* (18 March 2020). Geneva: ILO.

ILO (2020b). *ILO Monitor, first edition: COVID-19 and the World of Work, Impact and Policy Responses* (7 April 2020). Geneva: ILO.

ILO (2020c). *ILO Monitor, third edition: COVID-19 and the World of Work, Updated Estimates and Analysis* (29 April 2020). Geneva: ILO.

Kazeem, Y. (2020). 'Staying Alive: Across Africa, a Reliance on the Informal Sector Threatens Effective Coronavirus Lockdowns'. *Quartz Africa*. Available at: https://qz.com/africa/1831785/coronavirus-citizens-in-africas-informal-economy-try-to-survive/ (accessed 29 November 2020).

Lakuma, C.P., and N. Sunday (2020). 'Africa in Focus: Impact of COVID-19 on Micro, Small, and Medium Businesses in Uganda'. Available at: https://www.brookings.edu/blog/africa-in-focus/2020/05/19/impact-of-covid-19-on-micro-small-and-medium-businesses-in-uganda/ (accessed 15 June 2020).

Lee, J.N., M. Mahmud, J. Morduch, S. Ravindran, and A.S. Shonchoy (2020). 'Migration, Externalities, and the Diffusion of COVID-19 in South Asia'. *Journal of Public Economics*, 193: 104312. https://doi.org/10.1016/j.jpubeco.2020.104312

Roser, M., H. Ritchie, E. Ortiz-Ospina, and J. Hasell (2020). 'Coronavirus Pandemic (COVID-19)'. Our World Data file. Available at: https://ourworldindata.org/coronavirus (accessed 10 November 2020).

Schotte, S., M. Danquah, R.D. Osei, and K. Sen (2021). 'How Covid-19 is Affecting Workers and their Livelihoods in Urban Ghana: Results from the GSPS-COVID Panel Survey'. Helsinki: UNU-WIDER. https://doi.org/10.35188/UNU-WIDER/2021-rep-1

Appendix

Table A1: Attrition rates by lockdown status

	Total		Lockdown (19 districts)		No-Lockdown (59 districts)		No-Lockdown > size cut-off (20 districts)	
	Obs	Share	Obs	Share	Obs	Share	Obs	Share
Attrition	270	29.4 %	125	31.5 %	145	27.8 %	60	26.2 %
Could note be reached	*184*	20.0 %	83	20.9 %	101	19.4 %	42	18.3 %
Refused to be interviewed	*52*	5.7 %	27	6.8 %	25	4.8 %	7	3.1 %
No longer a member of the same household	*16*	1.7 %	4	1.0 %	12	2.3 %	7	3.1 %
Not unequivocally identified	*10*	1.1 %	6	1.5 %	4	0.8 %	4	1.7 %
Interview ended midstream	*8*	0.9 %	5	1.3 %	3	0.6 %	0	0.0 %
Sample	648	70.6 %	272	68.5 %	376	72.2 %	169	73.8 %
Total contacted	918	100 %	397	100 %	521	100 %	229	100 %

Source: authors' estimates based on GSPS-COVID-19 survey.

Table A2: Probability estimates on sample retention

Average marginal effects from probit estimation Dependent variable: Sample retention (=1 if YES) Baseline characteristics as of 2018/19	Sample retention
Location in lockdown district	-0.049*
	(0.028)
Female	-0.077***
	(0.027)
Household size	-0.000
	(0.009)
Married	0.001
	(0.035)
Age	0.024***
	(0.009)
Age squared (x0.01)	-0.028***
	(0.009)
Education (base: Pre-primary education)	
Primary education	-0.034
	(0.063)
Basic education	-0.002
	(0.045)
Secondary education	-0.010
	(0.063)
Tertiary education	0.026
	(0.060)
Missing education information	-0.038
	(0.051)
Formal employment	0.024
	(0.035)
Wage employment	0.012
	(0.031)
Observations	918
Pseudo R2	0.019

Note: Standard errors in parentheses, clustered at the district level; *p<0.10,**p<0.05,***p<0.01.

Source: authors' estimates based on GSPS-COVID-19 survey.

Table A3: Descriptive statistics, full sample

	Obs	Mean	Std. Dev.	Min	Max
Lockdown	648	0.4480	0.4986	0	1
Female	648	0.5564	0.4982	0	1
Head of household	648	0.7912	0.4075	0	1
Household size	648	3.0426	1.8976	1	13
Moved since last interview	648	0.1048	0.3071	0	1
Married (2018/19)[a]	648	0.4565	0.4994	0	1
Age group					
Ages <25 years	648	0.0630	0.2436	0	1
Ages 25-34 years	648	0.1825	0.3873	0	1
Ages 35-44 years	648	0.2520	0.4354	0	1
Ages 45-54 years	648	0.2716	0.4460	0	1
Ages 55-64 years	648	0.1875	0.3914	0	1
Ages 65+ years	648	0.0434	0.2044	0	1
Education (2018/19)[a]					
Pre-primary education	648	0.0593	0.2368	0	1
Primary education	648	0.1118	0.3160	0	1
Basic education	648	0.3936	0.4899	0	1
Secondary education	648	0.2073	0.4065	0	1
Tertiary education	648	0.1158	0.3208	0	1
Missing education information	648	0.1122	0.3164	0	1
Employment indicators in Feb 2020					
Work status (=1 if working)	648	0.9166	0.2773	0	1
Formal employment (working only)	599	0.2424	0.4298	0	1
Wage employment (working only)	599	0.3429	0.4761	0	1
Real earnings (non-zero)	497	262.30	293.78	6.18	1789.06
Employment indicators in Apr 2020					
Work status (=1 if working)	648	0.4667	0.5002	0	1
Formal employment (working only)	322	0.2558	0.4388	0	1
Wage employment (working only)	322	0.3586	0.4824	0	1
Real earnings (non-zero)	181	137.39	155.79	0.40	950.40
Employment indicators in Aug/Sep 2020					
Work status (=1 if working)	648	0.7796	0.4156	0	1
Formal employment (working only)	516	0.2444	0.4312	0	1
Wage employment (working only)	516	0.3385	0.4748	0	1
Real earnings (non-zero)	366	197.85	235.65	1.66	1493.78

Notes: [a] Information not collected in the GSPS-COVID survey and therefore taken from GSPS Wave 3 (2018/19). Inverse probability of attrition weights used. Population projections for 2020 by the Ghana Statistical Service (GSS) based on the 2010 Population and Housing Census.

Source: authors' estimates based on GSPS-COVID-19 survey.

Table A4: Descriptive statistics, restricted sample (above district size cut-off)

	Obs	Mean	Std. Dev.	Min	Max
Lockdown	441	0.6459	0.4801	0	1
Female	441	0.5406	0.5003	0	1
Head of household	441	0.8106	0.3933	0	1
Household size	441	2.8830	1.8055	1	13
Moved since last interview	441	0.1011	0.3027	0	1
Married (2018/19)[a]	441	0.4437	0.4988	0	1
Age group					
Ages <25 years	441	0.0506	0.2200	0	1
Ages 25-34 years	441	0.1724	0.3792	0	1
Ages 35-44 years	441	0.2388	0.4280	0	1
Ages 45-54 years	441	0.2946	0.4576	0	1
Ages 55-64 years	441	0.1938	0.3968	0	1
Ages 65+ years	441	0.0498	0.2183	0	1
Education (2018/19)[a]					
Pre-primary education	441	0.0564	0.2315	0	1
Primary education	441	0.1152	0.3205	0	1
Basic education	441	0.3832	0.4881	0	1
Secondary education	441	0.1891	0.3931	0	1
Tertiary education	441	0.1357	0.3438	0	1
Missing education information	441	0.1204	0.3267	0	1
Employment indicators in Feb 2020					
Work status (=1 if working)	441	0.9253	0.2639	0	1
Formal employment (working only)	410	0.2303	0.4228	0	1
Wage employment (working only)	410	0.3714	0.4852	0	1
Real earnings (non-zero)	340	265.10	299.58	6.18	1789.06
Employment indicators in Apr 2020					
Work status (=1 if working)	441	0.4293	0.4969	0	1
Formal employment (working only)	202	0.2671	0.4465	0	1
Wage employment (working only)	202	0.3952	0.4934	0	1
Real earnings (non-zero)	111	149.92	168.44	0.40	950.40
Employment indicators in Aug/Sep 2020					
Work status (=1 if working)	441	0.7911	0.4081	0	1
Formal employment (working only)	355	0.2300	0.4229	0	1
Wage employment (working only)	355	0.3690	0.4849	0	1
Real earnings (non-zero)	249	197.90	213.96	1.66	1437.43

Notes: [a] Information not collected in the GSPS-COVID survey and therefore taken from GSPS Wave 3 (2018/19). Inverse probability of attrition weights used. Population projections for 2020 by the Ghana Statistical Service (GSS) based on the 2010 Population and Housing Census.

Source: authors' estimates based on GSPS-COVID-19 survey.

Table A5: Monthly value of Exports (In Million US$)

	Jan	Feb	Mar	April	May	June
Merchandise Exports (f.o.b)	1 437,07	1 364,50	1 088,46	1 249,01	1 249,01	1 437,07
Cocoa Beans	322,38	236,96	180,42	119,78	119,78	322,38
Crude oil	315,11	366,70	108,15	226,56	226,56	315,11

Source: Bank of Ghana

Table A6: Average employment characteristics (working in February 2020 only), by district treatment status

	(1)	(2)	(3)	(1) - (3)	
Type of employment in Feb 2020	**Lockdown**	**No-lockdown**	**No-lockdown size cut-off**	**Difference**	**P-value Ha: diff != 0**
Occupational position					
Formal wage employment	0.125	0.123	0.133	-0.008	0.795
	(0.020)	(0.017)	(0.026)	(0.032)	
Informal wage employment	0.265	0.181	0.203	0.062	0.155
	(0.028)	(0.021)	(0.033)	(0.043)	
Formal self-employment	0.096	0.137	0.114	-0.019	0.573
	(0.019)	(0.019)	(0.027)	(0.033)	
Informal self-employment	0.514	0.559	0.549	-0.035	0.504
	(0.032)	(0.027)	(0.041)	(0.052)	
Sector of employment					
Agriculture	0.037	0.095	0.085	-0.047*	0.075
	(0.012)	(0.016)	(0.024)	(0.027)	
Industry	0.119	0.084	0.071	0.048*	0.092
	(0.020)	(0.015)	(0.020)	(0.028)	
Retail	0.345	0.368	0.402	-0.057	0.275
	(0.032)	(0.027)	(0.041)	(0.052)	
Transport	0.064	0.045	0.034	0.029	0.145
	(0.014)	(0.011)	(0.014)	(0.020)	
Hospitality	0.087	0.075	0.070	0.017	0.549
	(0.019)	(0.015)	(0.021)	(0.028)	
Hair and Beauty	0.040	0.033	0.020	0.020	0.266
	(0.013)	(0.011)	(0.012)	(0.018)	
Health	0.014	0.026	0.017	-0.003	0.804
	(0.007)	(0.009)	(0.010)	(0.012)	
Other Services	0.295	0.275	0.301	-0.007	0.884
	(0.029)	(0.024)	(0.036)	(0.046)	
Number of observations	254	345	156	410	410

Notes: Inverse probability of attrition weights used. Standard errors in parentheses, *p<0.10,**p<0.05,***p<0.01.
Source: authors' estimates based on GSPS-COVID-19 survey.

Table A7: Aspect of the COVID-19 pandemic that had the largest impact on respondent, by district treatment status

Aspect of COVID-19 pandemic that had the largest impact	(1) Lockdown	(2) No-lockdown	(3) No-lockdown size cut-off	(1) - (3) Difference	P-value Ha: diff != 0
Unemployment / loss of income	0.631	0.632	0.650	-0.022	0.652
	(0.030)	(0.026)	(0.038)	(0.671)	
Restrictions on movements	0.148	0.114	0.100	0.042	0.190
	(0.022)	(0.016)	(0.023)	(0.032)	
Being sick or fear of getting sick	0.147	0.116	0.106	0.036	0.313
	(0.022)	(0.017)	(0.025)	(0.035)	
Shortages in food supply	0.042	0.029	0.040	-0.002	0.936
	(0.012)	(0.009)	(0.017)	(0.019)	
Childcare / home-schooling	0.013	0.065	0.055	-0.035**	0.043
	(0.007)	(0.013)	(0.018)	(0.017)	
Other	0.002	0.018	0.024	-0.015	0.147
	(0.002)	(0.007)	(0.012)	(0.011)	
None	0.017	0.027	0.024	-0.003	0.811
	(0.008)	(0.008)	(0.011)	(0.014)	
Number of observations	272	376	169	441	441

Notes: Inverse probability of attrition weights used. Standard errors in parentheses, *p<0.10,**p<0.05,***p<0.01.

Source: authors' estimates based on GSPS-COVID-19 survey.

Table A8: Main reason for stopping work (only if working in February and not working in April 2020), by district treatment status

Main reason for stopping work in April 2020	(1) Lockdown	(2) No-lockdown	(3) No-lockdown size cut-off	(1) - (3) Difference	P-value Ha: diff != 0
Workplace / business had to close due to government regulations	0.673	0.642	0.699	-0.026	0.721
	(0.036)	(0.042)	(0.063)	(0.072)	
Lack of work / no customers	0.189	0.157	0.149	0.040	0.480
	(0.029)	(0.032)	(0.049)	(0.057)	
Could not reach workplace due to mobility restrictions / lack of transport	0.078	0.071	0.070	0.008	0.837
	(0.021)	(0.023)	(0.035)	(0.040)	
Sickness / health reasons	0.043	0.085	0.066	-0.023	0.569
	(0.016)	(0.025)	(0.037)	(0.040)	
Stop work to look after children	0.017	0.029	..	0.017*	0.090
	(0.010)	(0.014)	..	(0.010)	
Other	..	0.016	0.017	-0.017	0.316
	..	(0.011)	(0.017)	(0.016)	
Number of observations	182	145	61	243	243

Notes: Inverse probability of attrition weights used. Standard errors in parentheses, *p<0.10,**p<0.05,***p<0.01.

Source: authors' estimates based on GSPS-COVID-19 survey.

Table A9: Average use of government support by district treatment status

Made use of measures provided by the government under CAP	(1) Lockdown	(2) No-lockdown	(3) No-lockdown size cut-off	(1) - (3) Difference	P-value Ha: diff != 0
Free food parcels or hot meals	0.215	0.041	0.031	0.184***	0.000
	(0.026)	(0.011)	(0.014)	(0.029)	
Free water supplies	0.683	0.644	0.649	0.034	0.480
	(0.029)	(0.025)	(0.038)	(0.048)	
Subsidized electricity	0.827	0.711	0.744	0.083*	0.051
	(0.024)	(0.024)	(0.035)	(0.043)	
Bank credit (at reduced interest rate)	0.047	0.071	0.058	-0.011	0.624
	(0.013)	(0.014)	(0.018)	(0.023)	
Sent mobile money (at reduced transaction cost / free of charge)	0.321	0.144	0.134	0.187***	0.000
	(0.029)	(0.018)	(0.026)	(0.039)	
Number of observations	272	376	169	441	441

Notes: Inverse probability of attrition weights used. Standard errors in parentheses, *p<0.10,**p<0.05,***p<0.01.

Source: authors’ estimates based on GSPS-COVID-19 survey.

Table A10: Impact of the coronavirus lockdown on employment outcomes, same samples

	(1) **Working in *t* (=1 if YES)**	(2) **Working in *t* (=1 if YES), excl. working but missing income information**	(3) **Log weekly earnings in *t* (2018 prices)**	(4) **Log weekly earnings in *t* (2018 prices), incl. zero earnings for out of work[a]**
Post-period (base Feb 2020)				
April 2020	-0.260***	-0.310***	-0.613***	-2.656***
	(0.035)	(0.042)	(0.124)	(0.299)
Aug/Sep 2020	-0.107***	-0.119***	-0.429***	-1.181***
	(0.022)	(0.028)	(0.096)	(0.193)
Lockdown × April 2020	-0.343***	-0.365***	0.307**	-2.471***
	(0.046)	(0.047)	(0.139)	(0.352)
Lockdown × Aug/Sep 2020	-0.007	-0.022	0.108	-0.080
	(0.029)	(0.036)	(0.109)	(0.251)
Panel effects	FE	FE	FE	FE
Observations	1323	1050	700	1050

Notes: [a] Zero earnings of non-working individuals are set to 0.1 before log transformation. FE = fixed effects (within) regression; control districts limited to those with a population density above 300/km²; standard errors in parentheses, clustered at the district level; *p<0.10,**p<0.05,***p<0.01.

Source: authors’ estimates based on GSPS-COVID-19 survey.

Table A11: Impact of the coronavirus lockdown on employment, pre- and post-treatment periods

Dependent variable: Working in period *t* (=1 if YES)	**(1) Full sample**	**(2) District size cut-off**	**(3) District size cut-off with covariates**	**(4) District size cut-off with worker FE**
Pre-period (base Feb 2020)	0.072***	0.065**	0.064**	0.063***
GSPS W3 (2018/19)	(0.015)	(0.027)	(0.027)	(0.022)
Post-period (base Feb 2020)				
April 2020	-0.301***	-0.262***	-0.262***	-0.260***
	(0.030)	(0.039)	(0.039)	(0.035)
Aug/Sep 2020	-0.109***	-0.117***	-0.116***	-0.107***
	(0.019)	(0.029)	(0.029)	(0.022)
Lockdown	0.016	0.009	0.016	
	(0.030)	(0.039)	(0.041)	
Lockdown × GSPS W3	-0.020	-0.012	-0.012	-0.012
	(0.027)	(0.039)	(0.039)	(0.030)
Lockdown × April 2020	-0.302***	-0.350***	-0.350***	-0.343***
	(0.046)	(0.058)	(0.058)	(0.046)
Lockdown × Aug/Sep 2020	-0.005	-0.008	-0.009	-0.007
	(0.021)	(0.033)	(0.033)	(0.029)
Observations	2,588	1,762	1,762	1,762
Covariates	NO	NO	YES	NO
Panel effects	RE	RE	RE	FE

Note: covariates include gender, head of household, age categories (<25; 25–34; 35–44; 45–54; 55–64; 65+), marital status (married in 2018/19), education levels (pre-primary; primary; basic; secondary; tertiary; missing in 2018/19), household size. FE = fixed effects (within) regression; standard errors in parentheses, clustered at the district level; *p<0.10,**p<0.05,***p<0.01.

Source: authors' estimates based on GSPS-COVID-19 survey.

Table A12: Impact of the coronavirus lockdown measures on log weekly earnings, pre- and post-treatment periods

Dependent variable: Log weekly earnings in period *t* (constant 2018 prices)	**(1) Full sample**	**(2) District size cut-off**	**(3) District size cut-off with covariates**	**(4) District size cut-off with worker FE**
Pre-period (base Feb 2020)				
GSPS W3 (2018/19)	-0.961***	-0.777***	-0.770***	-0.754***
	(0.120)	(0.143)	(0.139)	(0.140)
Post-period (base Feb 2020)				
April 2020	-0.770***	-0.807***	-0.799***	-0.756***
	(0.107)	(0.191)	(0.167)	(0.131)
Aug/Sep 2020	-0.375***	-0.341***	-0.289***	-0.412***
	(0.057)	(0.087)	(0.087)	(0.095)
Lockdown	0.165	0.170	0.181	
	(0.102)	(0.133)	(0.116)	
Lockdown × GSPS W3	0.290*	0.105	0.095	0.074
	(0.156)	(0.176)	(0.170)	(0.154)
Lockdown × April 2020	0.433***	0.467*	0.330	0.360**
	(0.155)	(0.241)	(0.213)	(0.147)
Lockdown × Aug/Sep 2020	0.077	0.043	-0.034	0.111
	(0.117)	(0.133)	(0.133)	(0.111)
Observations	1,608	1,088	1,088	1,088
Covariates	NO	NO	YES	NO
Panel effects	RE	RE	RE	FE

Note: covariates include gender, head of household, age categories (<25; 25–34; 35–44; 45–54; 55–64; 65+), marital status (married in 2018/19), education levels (pre-primary; primary; basic; secondary; tertiary; missing in 2018/19), household size. FE = fixed effects (within) regression; standard errors in parentheses, clustered at the district level; *p<0.10,**p<0.05,***p<0.01.

Source: authors' estimates based on GSPS-COVID-19 survey.