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The Labour Market Impact of COVID-19 Lockdowns: Evidence from Ghana

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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 3-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 4 months after the lockdown was lifted, we detect a persistent nationwide decline in both earnings and employment, jeopardising particularly the livelihoods of small business owners mainly operating in the informal economy.

Keywords: Ghana, informal economy, employment, lockdown, COVID-19

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% of the world's workforce, were affected by partial or full lockdown regulations (ILO, 2020a). The impact of government lockdowns may have been particularly severe in labour markets of developing countries, given the limited possibilities of teleworking for a large proportion of the workforce in these countries who work in the informal economy, as well as the lack of unemployment insurance provision in many low- and middle-income countries (Danquah *et al*., 2020; ILO, 2020b). In this paper, we investigate the immediate and near-term impact of stringent COVID-19 lockdown policies on employment and earnings for a developing country, using Ghana as a case study.[1]

A fast-moving literature has looked at the labour market impact of COVID-19, but these analyses have been mostly confined to developed countries (see, for example, Chetty *et al*., 2020; Crossley *et al*., 2020; Forsythe *et al*., 2020). An emerging set of studies examine the economic impact of the pandemic in developing countries (Egger *et al*., 2021; Gupta *et al*., 2021; Janssens *et al*., 2021; Kansiime *et al*., 2021; Khamis *et al*., 2021; Mahmud and Riley, 2021). These studies provide descriptive evidence on how household incomes may have changed over the course of the pandemic. However, systematic quantitative evidence on the effect of government lockdowns on employment and earnings of workers in developing countries is lacking.

In this paper, we present causal evidence on the impact of stringent COVID-19 lockdown policies on employment and earnings for Ghana, a low-middle income country. In Ghana, a geographically contained 3-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 data set 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. 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 (Kazeem, 2020; Durizzo *et al*., 2021). Importantly, our results reveal that the strong and significant immediate treatment effect of the lockdown had faded 4 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.

There is an important discussion in the recent literature concerning the identification of causal policy impacts in the context of COVID-19 (Goodman-Bacon and Marcus, 2020). Our main concern regarding the proposed identification is that labour markets in treated

[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), Fang *et al*. (2020) and Aum *et al*. (2021), for example.

[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 4 months period up to August/September 2020 is referred to as near-term.

and control districts may have reacted differently to the pandemic shock, even in absence of the lockdown. This could be explained by differences in information, enforcement and infection risks, for example. As we cannot rule this out completely, we offer a robustness check that seeks to quantify the bias based on a simulation exercise. By linking Google mobility data to changes in government restrictions and new COVID-19 cases—spanning a period of 2 years since the start of the pandemic—we find that workplace mobility in treated areas indeed tended to react more strongly to these factors. That is, even if the government had stepped up restrictions to the same extent across the country, we may still have seen a stronger reaction in employment in treated districts, especially in face of rising national case numbers falling over-proportionally on these two areas. Nonetheless, no more than one third of the estimated treatment effect can be explained by these factors. Reversely, this means that two-thirds of the estimated effect remain attributable to the lockdown treatment.

The paper is organised 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 2 weeks, but ultimately was extended to 20 April, lasting 3 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.

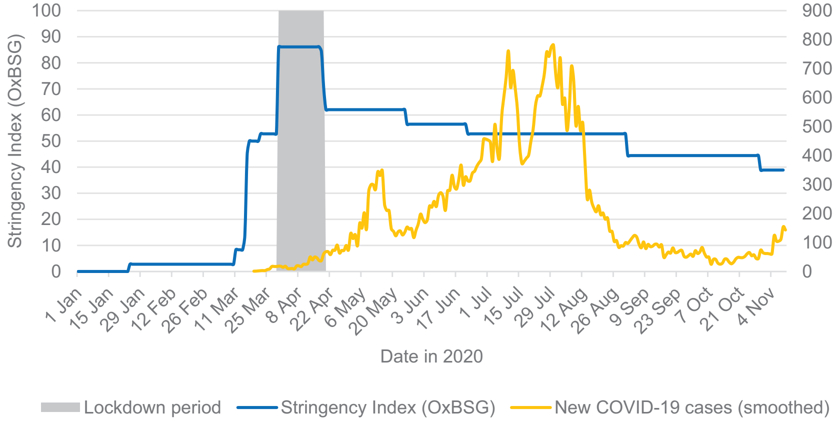


Figure 1. COVID-19 Cases and Government Response Stringency Index in Ghana. Note: the stringency index shows the response level in the national subregion with the strictest policies (districts subject to lockdown regulations) and the grey shaded area indicates the lockdown period from 30 March to 19 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).

Figure 1 illustrates the stringency of COVID-19 confinement policies implemented in Ghana between January and November 2020, as measured by the Oxford Blavatnik School of Government (OxBSG) Coronavirus Government Response Tracker (Hale *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% 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 microscale, 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 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 in working age (15 to 64 years old), 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 eight 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 7 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).[6]

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 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

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

[6] In the phone survey, earnings of both wage and self-employed workers are measured using a one-shot question.

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:

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

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 periods, 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 regression. Taking advantage of the panel structure of our data, we also estimate a second specification that controls for worker-fixed effects, μ_i:

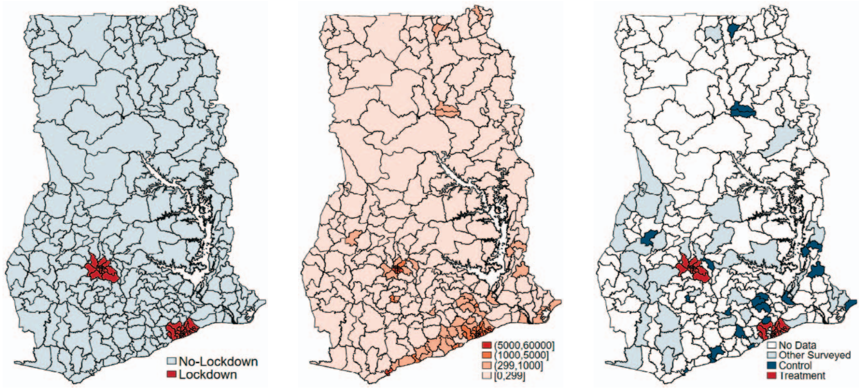
$$Y_{idt} = \delta_0 + \delta_1 \left(LOCKDOWN_d \times POST_{1t}\right) + \delta_2 \left(LOCKDOWN_d \times POST_{2t}\right) + \mu_i + \theta_t + \varepsilon_{idt}. \quad (2)$$

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 data set. 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/km^2. This cut-off value is fixed in reference to the population density in the least densely populated treated district.[7] As robustness check, we exclude the Accra Metropolitan and Kumasi Metropolitan city nucleus districts from the analysis.[8]

[7] 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).

[8] 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.



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

b) Population density across districts (population per km^2)

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

Figure 2. Lockdown versus No-lockdown Study Areas. Note: population projections for 2020 by the Ghana Statistical Service (GSS) based on the 2010 Population and Housing Census. Source: authors' illustration based on GSPS-COVID-19 survey.

3.3. Descriptive statistics

3.3.1. 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% of respondents were working in February 2020, of whom 24.2% were in formal employment, and 34.3% were in wage employment.[9] Thus, despite the urban focus and exclusion of agriculture, the informality rate in our sample is above 70%, 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 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.

3.3.2. 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% of respondents in no-lockdown districts continued working throughout April 2020, compared with 32.1%

[9] 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.

Table 1. Average worker characteristics by district treatment status

Characteristics in August/ September 2020 (unless otherwise specified)	(1) Lockdown	(2) No-lockdown	(3) No-lockdown size cutoff	(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 February 2020	0.930	0.906	0.917	0.012	0.656
	(0.016)	(0.017)	(0.022)	(0.028)	
Formal employment in February 2020	0.221	0.260	0.248	−0.027	0.533
	(0.026)	(0.024)	(0.035)	(0.043)	
Wage employment in February 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.

of respondents in lockdown districts. Importantly, the majority (52.2%) of respondents in lockdown districts said that they had stopped working temporarily, while 15.7% 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% of respondents who had been working in February 2020 were observed to be working again, compared with 84.9% of respondents in no-lockdown districts.

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).[10] 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,

[10] 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/km^2. This will be formally tested in the next section.

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/km^2. Source: authors' illustration based on GSPS-COVID-19 survey.

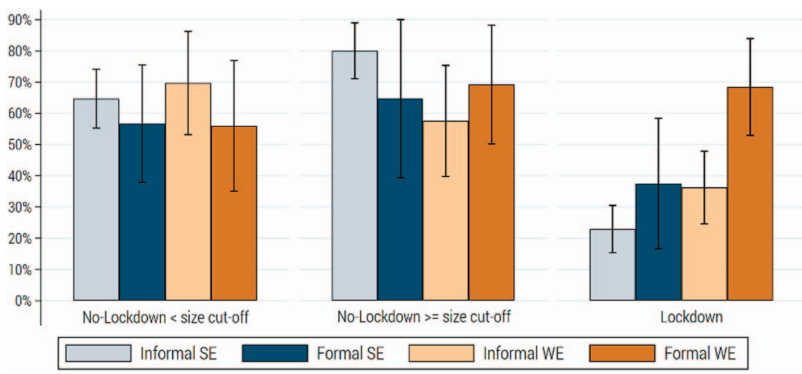


Figure 4. Employment Rates in Lockdown and No-lockdown Districts in April, by Work Status in February 2020. Notes: sample limited to respondents who had been working February 2020. SE = self-employed; WE = wage employed. Average shares with 95% confidence intervals. Source: authors' illustration based on GSPS-COVID-19 survey.

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.[11] As for employment, we observe a recovery in earnings up to August/September 2020 throughout the country, with earnings levels nevertheless remaining below the February average.

3.3.3. Potential confounders

There is an important discussion in the recent literature concerning the identification of causal policy impacts in the context of COVID-19 (Goodman-Bacon and Marcus, 2020). Our DID design relies on the comparability of treated and control groups. Specifically, we need to assume that labour markets in treated and control districts would have reacted in the same way to the pandemic shock in absence of the lockdown. To assess the validity of this assumption, 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 (see Khamis *et al*., 2021), 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% 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% 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% mentioned a lack of work/customers) or

[11] 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.

behavioural responses in relation to health concerns (across the sample, only 6% 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, subsidised electricity and bank credit—which may have directly influence business performance, was largely balanced between treated and control districts.

While we do not find any indication of systematic bias by geographic location along the two considered dimensions in our data, we offer a robustness check using Google mobility data in section 5.2.

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/km^2. 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% 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% risk of dropping out of work in April, compared with an average ceteris paribus risk of 26.0% faced by workers in no-lockdown districts.[12]

Confirming the descriptive patterns, we observe a strong recovery in employment about 4 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

[12] The estimated effects are in a similar range as detected by other, descriptive studies. For example, using phone survey data collected between April and July 2020, Khamis *et al.* (2020) find that on average 26 per cent of respondents stopped work during the early phase of the pandemic in Ghana.

Table 2. Impact of the coronavirus lockdown on employment

Dependent variable: Working in period t (=1 if YES)	(1) Full sample	(2) District size cutoff	(3) District size cutoff with covariates	(4) District size cutoff with worker FE
Post-period (base February 2020)				
April 2020	−0.282***	−0.262***	−0.262***	−0.260***
	(0.031)	(0.039)	(0.039)	(0.035)
August/September 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× August/ September 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; fixed effects (FE) (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.

probability of being in work at the time of the interview was still 10.7% below the February 2020 level.

In the following, we provide indicative evidence concerning potential heterogeneities in the treatment effect.[13] 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-employment vs wage employment). Figure 5 reports the 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.[14] Interestingly, independent of the formality status, the lockdown seems to have affected self-employed workers more than wage employees (Figure 5). This observation—which matches the results obtained by other studies (for example, Khamis *et al.*, 2021)—could be explained by a larger decline in the activity of micro and small enterprises (often operated by own account

13 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.
14 Note that 60% of formal wage workers in our sample work in the public sector.

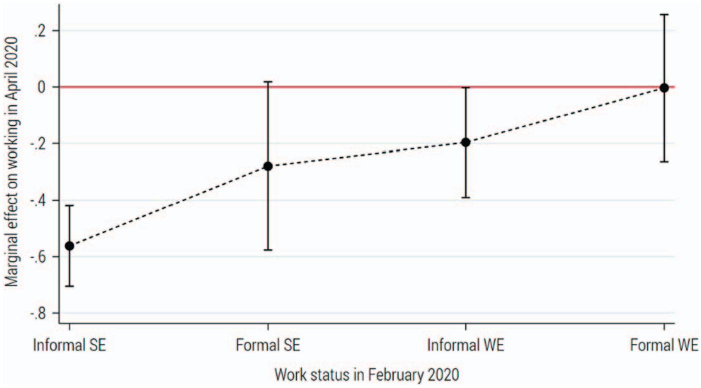


Figure 5. Impact of the Coronavirus Lockdown on Employment in April, by Work Status in February 2020. Note: SE = self-employed; WE = wage employed. Control districts limited to those with a population density above 300/km^2. Sample limited to respondents who had been working in February 2020. Each point 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.

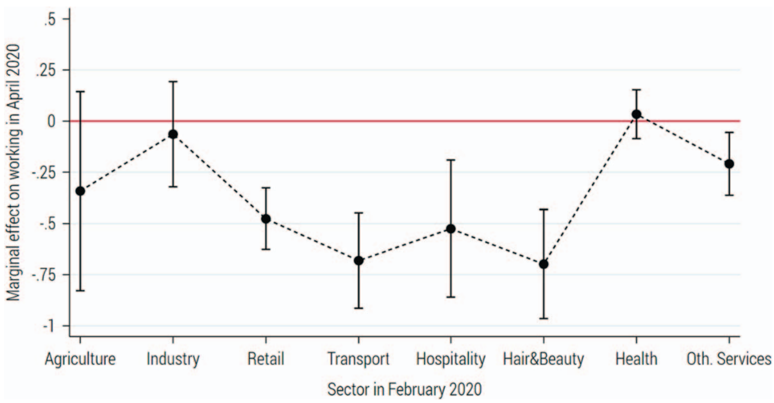


Figure 6. Impact of the Coronavirus Lockdown on Employment in April, by Sector of Employment in February 2020. Note: Control districts limited to those with a population density above 300/km^2. Sample limited to respondents who had been working in February 2020. Each point shows the estimated average 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.

workers or family enterprises without no external employees) compared with medium and large enterprises (Lakuma and Sunday, 2020).

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.[15] 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

[15] 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 Table 2 (results are available from the authors upon request).

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

Dependent variable: Log weekly earnings in *t* (constant 2018 prices)	**(1) Full sample**	**(2) District size cutoff**	**(3) District size cutoff with covariates**	**(4) District size cutoff 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. Fixed effects (FE) (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.

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.

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 4 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%to 34.9%. 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

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 February 2020)				
August/September 2020	−0.186*	−0.190**	−0.155*	−0.232***
	(0.098)	(0.086)	(0.087)	(0.063)
Self-employed in February 2020	0.075	0.293***	0.276***	
	(0.108)	(0.103)	(0.098)	
Formal work in February 2020	0.370***	0.229**	0.233**	
	(0.096)	(0.095)	(0.094)	
Female		−0.252**	−0.198*	
		(0.105)	(0.113)	
August/September 2020× Self-employed in Feb 2020	−0.288***	−0.291***	−0.254**	−0.160**
	(0.099)	(0.097)	(0.102)	(0.075)
August/September 2020× Formal in February 2020	0.159	0.176	0.165	0.076
	(0.112)	(0.110)	(0.109)	(0.094)
August/September 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. Fixed effects (FE) (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.

onset of the pandemic. Without accounting for inflation, a somewhat smaller decline in average weekly earnings of 20.2% to 29.9% would have been estimated, depending on the specification.

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).

5. Robustness checks

We conduct four sets of robustness checks. First, we test for parallel pre-treatment trends. Second, we use secondary data on workplace mobility to simulate a potential violation of the parallel trends assumption due to confounding factors. Third, to test for potential bias due

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: Fixed effects (FE) (within) regression; control districts limited to those with a population density above 300/km^2; 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.

to self-selection, we examine whether our results are robust to the exclusion of workers who have moved since the 2018/19 panel round. Fourth, to ensure that our results are not driven exclusively by the two major metropolitan districts, which together account for 65.6% 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. Pre-trend testing

To test for differential pre-trends, 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% 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.

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 to 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/km^2.

5.2. Simulated violation of parallel trends

Our identification of the treatment effect relies on the comparability of labour market conditions in treated and control districts. While we cannot reject the hypothesis that outcomes evolved in parallel during the pre-treatment period, labour markets in treated and control districts may still have reacted differently to the pandemic shock, even in absence of the lockdown.

As we cannot rule this out completely, we attempt to quantity the bias using a simulation approach. Our idea is inspired by Rambachan and Roth's (2021) 'honest approach to parallel trends'. Instead of requiring that the parallel trends assumption holds exactly, they suggest a robustness check that extrapolates pre-treatment violations of parallel trends to the post-treatment period. In our application, pre-pandemic trends may carry little information to assess the bias stemming from the pandemic shock. Therefore, we attempt to map out the counterfactual time trend using time-series data that has been collected since the start of the pandemic.

Our approach uses additional secondary data on workplaces mobility from the Google COVID-19 Community Mobility Reports.[16] The daily data allow us to track changes in workplaces mobility over a two-year period, from 15 February 2020 to 22 January 2022, for three subregions: the two main areas affected by the lockdown—the Accra Metropolitan Area (AMA) and Kumasi Metropolitan Area (KMA)—and the rest of Ghana.[17]

We relate the changes in mobility to two factors that likely play a role in explaining mobility trends: (i) confirmed daily COVID-19 infections and (ii) the strictness of government policies (see Figure A1 Appendix). To assess the extent to which mobility in lockdown areas reacted differently to these two factors compared to the rest of the country, we fit the following regression model:

$$\begin{aligned}\Delta M_{rt} = \rho_0 + \rho_1 S_{rt} + \rho_2 C_t + \rho_3 \left(LOCKDOWN_r \times S_{rt}\right) + \rho_4 \left(LOCKDOWN_d \times C_t\right) \\ + \mu_r + \theta_t + \mu_r \times \theta_t + \varepsilon_{rt} \qquad (3)\end{aligned}$$

where ΔM_{rt} is the percentage change in workplaces mobility in subregion r at time t (relative to the baseline median value calculated between 3 January and 6 February 2020), S_{rt} is the standardised OxBSG Government Stringency Index[18] (demeaned and divided by the standard deviation), C_t is the natural logarithm of smoothed new case numbers, $LOCKDOWN_r$ is a dummy variable equal to one for AMA and KMA and zero for the rest of Ghana, r is a vector of subregion fixed effects and t is a linear time trend, which we allow to vary by subregion.

The results (see Table A13 Appendix) suggest that mobility in the two treated subregions (AMA and KMA) reacted more strongly to changes in both government restrictions and national case numbers. The former may be explained by better knowledge or enforcement of government regulations in the two metropolitan subregions. The latter may be associated with a larger fear of the virus in AMA and KMA as 'hotspots' of transmissions, which may have caused more workers to stay home voluntarily and/or affected business operations as customers stay away.[19]

[16] We consider the workplaces mobility measure most informative with regard to our analysis of employment, as it tracks workplace visits directly. In addition, we also conducted the same robustness check using the inverse residential mobility measure, which tracks the time spent outside the home for any sort of activity. Findings were consistent and indicated a yet smaller deviation from the parallel trends assumptions, which is why this additional check has not been included here. Results are available from the authors upon request.

[17] We calculate the 7-day moving average to account for weekday fluctuations. Data are available at the national level and for the AMA and KMA subregions. To back out mobility in the rest of Ghana, we make the simplifying assumption that each subregion's contribution to total national mobility can be approximated by its population share (16% AMA and 11 per cent KMA).

[18] The OxBSG Government Stringency Index is defined at the national level. If policies vary at the subnational level, the index shows the response level of the strictest subregion. For the lockdown period, it thus reflects the stringency level in AMA and KMA. For the rest of Ghana, we set the stringency index for this period to the first post-lockdown observation (lockdown was lifted while other measures remained in place).

[19] High-frequency information on infection rates in Ghana is only available at the national level, which is why we are only able to capture this channel indirectly.

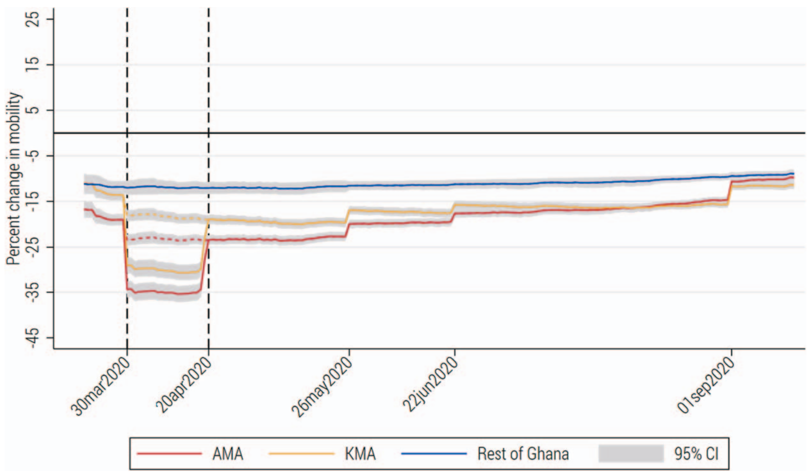


Figure 7. Simulated Percentage Change in Workplaces Mobility in Presence and Absence of the Lockdown. Note: we simulate the response in mobility under two alternative scenarios: (i) Presence of the lockdown (solid lines) and (ii)) absence of the lockdown (short-dashed lines). Outside of the lockdown period (demarcated by dashed vertical lines), de-facto values are used and the two scenarios coincide. Source: authors' estimates based on Google COVID-19 Community Mobility Reports, OxBSG Government Stringency Index, and John Hopkins University new case numbers (smoothed as of 26 January 2022.

The significantly different elasticity in workplaces mobility in treated areas compared to the rest of Ghana points to a likely violation of the common trends assumption, which is expected to cause our treatment effect to be upward biased. To quantify the deviation, we use the coefficient estimates from equation (3) to simulate the reaction in mobility in treated and control areas under two alternative scenarios (illustrated in Figure 7):

i) Simulation with lockdown: To simulate outcomes in presence of the lockdown (solid lines in Figure 7), we set the stringency level in AMA and KMA to the de-facto values of the OxBSG stringency index reported for this period, and set the stringency level in the rest of Ghana to the first value observed once the lockdown had been lifted, while other restrictions still remained in place.
ii) Simulation without lockdown: To simulate outcomes in absence of the lockdown (short-dashed lines in Figure 7), we set the stringency level in all subregions to the first value observed once the lockdown had been lifted. Outside of the lockdown period, de-facto values are used and the two scenarios coincide.

The simulated time trends reported in Figure 7 illustrate an important pattern: While the simulated decline in workplaces mobility in treated areas is significantly more pronounced in presence of the lockdown, we still observe a dip in mobility among the treated group when setting government restrictions in treated areas to the control group level.

Table 6 gives an indication of the treatment effect estimates under the two alternative scenarios. We use the days before the lockdown as the base period and define two post-treatment periods that resemble our survey data: $POST_1$ is the immediate lockdown period from 30 March to 20 April 2020, and $POST_2$ is the near-term period between 19 August and 17 September 2020.

Table 6. DID estimates for simulated percentage change in mobility in presence and absence of the lockdown

Dependent variable: Percentage change in workplaces mobility	(1) Observed	(2) Simulated with lockdown	(3) Simulated without lockdown	(3)/(2) Ratio
LOCKDOWN × $POST_1$	−15.422***	−16.321***	−5.023***	0.3077
	(2.246)	(1.006)	(0.945)	
LOCKDOWN × $POST_2$	0.979	0.382	0.382	1
	(2.147)	(0.962)	(0.904)	
Lockdown and period fixed effects	YES	YES	YES	
Observations	180	180	180	

Note: Standard errors in parentheses; $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.
Source: authors' estimates based on Google COVID-19 Community Mobility Reports, OxBSG Government Stringency Index, and John Hopkins University new case numbers (smoothed) as of 22 January 2022.

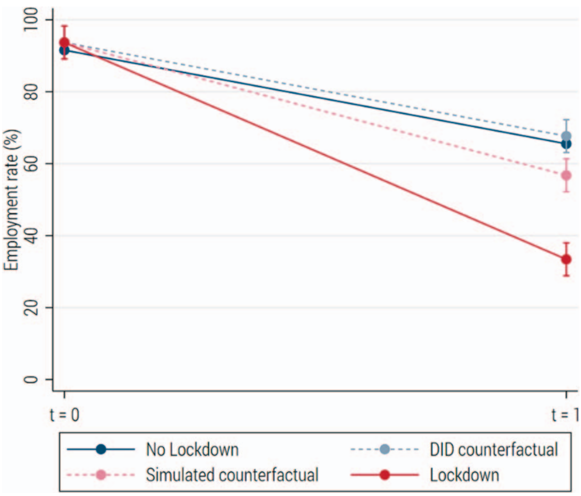


Figure 8. Deviation from Common Trends. Note: Pre-treatment (t = 0) and lockdown (t = 1) period. Source: authors' estimates on GSPS-COVID-19 survey.

In line with our survey estimates, we only find a significant difference in mobility trends between treated and control areas during the immediate lockdown period, while no near-term effect is found. Specifically, in presence of the lockdown, treated areas would have seen an additional decline in workplaces mobility by 16.3%, compared to the control group (column 2). This is in close range to the treatment effect estimated using observational data (column 1). The smaller effect size compared to our survey estimate is likely explained by the limitations of the Google mobility data in capturing mobility of low-income earners without smartphones access.

Simulating the change in mobility during April 2020 in absence of strict lockdown restrictions, we find that workplaces mobility would on average still have shown a 5% stronger decline in AMA and KMA compared with the rest of the country (column 3). Calculating the ratio between the two simulated effects, we conclude that 30.8% of the total treatment effect may be explained by confounding factors; this is, it may have occurred even in absence of the treatment.

Table 7. 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 February 2020)		
April 2020	−0.282***	−0.710***
	(0.038)	(0.121)
August/September 2020	−0.114***	−0.507***
	(0.026)	(0.085)
Lockdown× April 2020	−0.349***	0.386***
	(0.050)	(0.126)
Lockdown× August/September 2020	−0.0208	0.216**
	(0.035)	(0.100)
Observations	1,178	636
Panel effects	FE	FE

Note: Fixed effects (FE) (within) regression; control districts limited to those with a population density above 300/km^2; 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 a last step, we illustrate the implications of this simulation exercise for our DID survey estimates (see Figure 8). Instead of assuming that the evolution of employment during the early phase of the pandemic would have followed the same time trend in treated and control districts in absence of the lockdown (DID counterfactual), we can allow for a deviation from parallel trends. If 30.8% of the total treatment effect were attributable to other factors, workers located in the Greater Accra and Greater Kumasi Metropolitan Areas and contiguous districts would have had a 10.6% lower chance to continue working throughout April 2020, even in absence of the lockdown (counterfactual based on simulation exercise). Nonetheless, the lockdown measures would have further decreased their chances to continue work by an additional 23.7%—pointing to a smaller but still sizable and significant treatment effect.

5.3. 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 7. 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.

5.4. 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

Table 8. 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 February 2020)				
April 2020	−0.260***	−0.613***	−0.448***	−0.498**
	(0.032)	(0.103)	(0.039)	(0.162)
August/September 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× August/ September 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: Fixed effects (FE) (within) regression; control districts limited to those with a population density above 300/km^2; 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.

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% 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 8. 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.[20] Of 78 districts covered in our data, 39 exceed the defined population density cutoff, 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 of the 39 districts. As the results reported in columns 3 and 4 of Table 8 indicate, we find no statistically significant effect for this placebo treatment.

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 3-week lockdown

20 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.

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 4 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. As of June 2022, the government policies on continued revitalisation and transformation to 'build forward better' through the Ghana CARES programme and agenda 111 had impacted positively on growth and employment in the service sector but agriculture and particularly industrial sector are still lagging behind. The industrial sector is still experiencing a negative growth. The availability of loans under the Coronavirus Alleviation Programme Business Support Scheme to both formal and informal enterprises have contributed significantly to recovery in employment in the service sector.

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.

As Ghana moves toward post-COVID recovery, measures to prevent a persistent deepening of labour market vulnerabilities and inequalities will remain paramount. 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.

Supplementary material

Supplementary material is available at *Journal of African Economies* online.

Data availability

The dataset used for the study will be made available on UNU WIDER's data portal at a later date.

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