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Digital labour platforms as shock absorbers

Evidence from COVID-19

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Abstract: Digital labour platforms have grown five-fold over the last decade, enabling significant expansion in gig work worldwide. We interrogate the criticism that these platforms tend to amplify aggregate economic shocks for registered users (workers). Based on the universe of records from a matching platform for informal sector manual freelancers in Mozambique, we analyse how task supply and demand altered with the onset of COVID-19. Treating the pandemic as a structural break, which extends to an event study analysis, we find it was associated with a net increase in tasks demanded per worker, but no clear change in supply growth (new registrations). These trends are evident across multiple market segments, including female-dominated professions, suggesting digital labour markets can help workers adjust to economic shocks in low-income contexts.

Key words: COVID-19, digital labour markets, economic shocks, freelancers, Mozambique

JEL classification: J23, J46, O17

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1 Introduction

Digital platforms are expanding in many economic spheres, particularly labour markets. The International Labour Organization estimates that over the ten years to 2019 the number of such platforms increased five-fold worldwide (ILO 2021); and Kässi et al. (2021) note that in 2020 up to 200 million workers were registered for online gig work, of which at least 14 million were active. While the bulk of these activities are concentrated in the United States and Asia, these platforms are now growing rapidly in low-income contexts. In sub-Saharan Africa, for instance, Johnson et al. (2020) count 365 unique platforms operating in a sample of eight countries as of 2019, representing a 37 per cent increase from the year before and covering over 92,000 users.

Despite their prominence, the extent to which digital labour platforms tend to enhance workers' welfare remains controversial (Anwar and Graham 2021; Tan et al. 2021). At the heart of this debate is a tension between risks and rewards. Although platform or gig work appears to offer few protections to workers, often being sporadic and uncertain (Han and Hart 2021; Urzì Brancati et al. 2020), these platforms can also reduce search frictions and expand the effective choice set for *both* workers and clients, potentially raising welfare (Berger et al. 2019). Exploratory analysis by the ILO (2021) suggests app-based workers often earn more per hour than their offline counterparts.

This debate raises questions regarding the vulnerability of gig economy workers to external shocks, of which the COVID-19 pandemic has been a prime example. In keeping with an emerging literature (e.g. Stephany et al. 2020), we argue the effect of the pandemic on freelance work is ambiguous. Economic losses associated with restrictions on business activity would be expected to weaken demand for goods and services throughout the economy. However, formal sector job losses or reduced working hours may push individuals to seek additional informal opportunities, particularly via online platforms (Cao et al. 2020). The combination of changes in the composition of demand may divert demand to specific kinds of (more flexible) labour services, such as manual work related to home improvements.

In this paper we leverage the onset of COVID-19 to explore how the market for manual gig work across a set of fixed occupations responds to external shocks. We do so using complete micro-data from *Biscate*, a digital platform in Mozambique for contracting manual freelancers such as plumbers, carpenters, and hairdressers. To capture the dynamics on both the supply and demand sides of this market we analyse trends in multiple outcomes at the market level, combining effects at the extensive and intensive margins, observed on a weekly basis before and after the pandemic. To identify the specific causal effect of the shock, we begin by treating it as a non-linear (evolving) structural break, which extends to an event study analysis. Our results, which are robust to pre-filtering the outcome series (as per Fezzi and Fanghella 2021), indicate a clear positive shift in demand for informal manual workers coinciding with the start of COVID-19. Our most simple specification shows the task agreement rate increased by between approximately 20 and 40 per cent in the first year of the pandemic (e.g., from around 1.6 tasks per week per 100 workers to around 2.3 tasks). However, we find growth in supply, as measured via new registrations, was generally below trend during the first six months of the pandemic but—perhaps adjusting to the higher level of demand—moved above trend in the final months of the analysis, yielding a null effect on average. We also explore the extent to which these responses may be attributable to more specific impact channels associated with the pandemic (e.g., public health restrictions, job losses). Here we find a negative relationship between employment conditions and demand on the platform; but that higher official COVID-19 cases tended to dampen activity on both sides of the market.

Due to resilient and even surging demand on *Biscate*, we conclude that use of digital labour platforms by manual freelancers helped workers weather the COVID-19 shock. Furthermore, we show that gains did not only accrue to established (experienced) participants on the platform. We show that more specific market segments within the platform—namely those with a greater share of women or less-experienced

workers—recorded relatively higher demand growth. This case suggests that the shock-absorbing role of the informal sector in developing countries (see Burger and Fourie 2019; Kapelyushnikov et al. 2012; Restrepo-Echavarria 2014) may be supported by digital matching tools, allowing entrepreneurs to take advantage of disruption to 'business as usual'.

More generally, these findings contribute evidence on the extent to which digital labour platforms can help workers respond to economic shocks, as well as the role of digital technologies in enhancing resilience. A number of scholars have argued that COVID-19 has underlined the precarious nature of gig work, including in lower-income countries (e.g. Rani and Dhir 2020). In contrast, following a brief initial dip, other studies show that work intermediated by digital platforms grew comparatively faster after the onset of the pandemic compared to its prior trend (e.g. Mueller-Langer and Gomez-Herrera 2022). The revealed preference for digital channels during COVID-19 speaks to work on the disruptive nature of digital platforms, the economic value of flexibility (Chen et al. 2019), and the way in which (new) digital opportunities can stabilize income streams (Hall and Krueger 2018; Raj et al. 2020). This study adds to the empirical evidence in these fields, extending coverage to a low-income setting where informal work is already predominant, and highlights the ability of digital platforms to facilitate adjustments in labour demand.

2 Shocks and digital labour platforms

Debates around the extent to which gig workers are excessively vulnerable to economic shocks are highly pertinent to the case of COVID-19. While the overall picture associated with COVID-19 is undoubtedly negative, there are—as with other economic shocks, such as natural disasters or commodity price swings—important nuances. Within countries, large sectoral differences in the magnitude of economic impacts associated with the pandemic have emerged. Generally, tourism, restaurants, entertainment, transport, and (non-essential) in-store retail commerce have been most affected by restrictions on mobility. At least for high(er)-income countries, a key distinction has been between who can and cannot effectively work from home (Garrote Sanchez et al. 2021).

The disruption of the pandemic has also created opportunities, shifting purchases online as well as altering the composition of demand. For instance, Chang and Meyerhoefer (2021) find that an additional confirmed case of COVID-19 in Taiwan increased the value of online food sales by 5.7 per cent and the number of customers by 4.9 per cent.[1] In many different contexts, home improvement spending registered very large increases as restrictions on movement were enacted. According to data from the United States, around three out of four homeowners carried out at least one home improvement project since the pandemic began (Porch Research 2020).[2] In connection, demand for freelance contractors (handypersons), already increasingly sourced via online marketplaces, surged. For example, one digital platform matching freelance workers to demand for service tasks in South Africa recorded a 750 per cent increase in the number of posted requests when comparing March 2021 to April 2020 (Engineering News 2021).[3]

Focusing specifically on informal manual freelancers operating in low-income contexts (pertinent to our case; see more below), Table 1 proposes four distinct channels through which supply or demand may be altered due to the pandemic shock. The first is the health response to the virus itself, whereby individuals

[1] Also, the stock price of Africa's largest online shopping platform, *Jumia*, increased over six-fold during 2020 on the back of increased traffic and sales volumes (Ehl 2020).

[2] Tracking of social media and internet usage also reveals a booming interest in DIY projects (Burke 2020).

[3] The US online re-modelling platform, Houzz, recorded a 58 per cent increase in demand for professional contractors to work on home improvements for the year to June 2020 (Olick 2020).

choose not to engage in specific activities for fear of exposing themselves or their families to the virus. For informal workers, this could entail limiting both job search and work availability, in extreme cases moving residence to areas of perceived lower risk. For example, Valsecchi and Durante (2020) show that during localized negative health shocks in Italy, such as experienced during the COVID-19 pandemic, people migrated from higher-risk (outbreak) to lower-risk (non-outbreak) regions. Along the same lines in India, COVID-19 triggered massive migration out of cities, with millions returning to their home states, mostly due to unemployment and uncertainty (Mukhra et al. 2020).

A caveat here, however, is that while fear of the virus may lead individuals to avoid undertaking activities in customary physical locations, such as shops and marketplaces, they may redirect this demand elsewhere—for example, from the formal to the informal sector or to locations over which they exert control over social mixing. Hairdressing is one example—rather than attending a salon, individuals may prefer to invite a stylist to their own homes, where they can better control infection risks. For these reasons, we classify the direction of this effect on the demand side as ambiguous (denoted '?' in Table 1).

Table 1: Summary of potential impact channels of COVID-19 on manual freelancers

	Effect direction	
Channel	Supply side	Demand side
Health concerns	−	?
Business restrictions	?	?
Increased leisure/home time	+	+
Income losses	+	?

Note: +, −, and ? respectively indicate positive, negative, and ambiguous effect directions of COVID-19 on different sides of the market for freelancers (see columns) associated with alternative channels (in rows).

Source: authors' compilation.

A second channel refers to formal government restrictions placed on business activities, ranging from temporarily shutting down certain establishments (e.g., bars, gyms) to reducing worker numbers or opening times. This channel is likely to pertain primarily to formal establishments and/or workers operating from fixed locations. To the extent that individuals continue to be permitted to leave their place of residence during working hours (as in Mozambique), we expect such restrictions to have less 'bite' for mobile freelancers. Indeed, increased restrictions on fixed-location businesses may push affected (formal sector) workers into the mobile (informal) segment, as well as directly increase demand for such services. So, as before, disruption caused by COVID-19 may displace activity rather than merely dampen it, generating new opportunities for platform workers. Thus, again, we classify the direction of this effect as ambiguous.

The third channel captures the implications of additional leisure time, a plausible indirect consequence of increased social distancing (virus fear) and formal business restrictions. With less time spent at fixed locations outside the home, we hypothesize that the availability of workers to undertake (local) informal tasks could increase. On the demand side, additional free time as well as suppressed demand for certain goods and services (e.g. entertainment) could stimulate individuals to invest in new projects, such as home improvements. For this reason, we expect this channel may well operate in a positive direction on both sides of the market.

Finally, there is the knock-on effects of lost income, such as due to foregone employment or wage reductions. On the supply side of markets for manual freelancers, our expectation is this would tend to have a positive effect, reflecting low entry costs and that at least some of this work is undertaken when formal opportunities are exhausted (Jones and Tarp 2015). On the demand side, the effect is ambiguous. To the extent that informal services may represent an inferior and cheaper substitute to formal alternatives, it is possible that demand increases as (aggregate) incomes decline. Also, where digital platforms offer a broader range of suppliers, this can allow potential buyers to find a competitively

priced provider relative to pre-existing contacts. The fact this can be done using digital tools, rather than in-person, may be particularly appealing in the context of a pandemic.

In sum, we hypothesize that disruptions to traditional forms of informal work associated with COVID-19 may entail a complex range of effects. Within the specific domain of manual freelancers, which is usually task-specific and mobile in nature, COVID-19 would seem to present both challenges *and* opportunities. Perceived health risks and formal public health restrictions would be expected to dampen activity in this market in general. However, potential displacement of demand from formal to informal services, redirection of search from in-person to digital tools, substitution towards inferior services, and low entry costs into this market segment all suggest that both demand and supply may also have been affected positively during the pandemic.

The ambiguous direction of the effect of COVID-19 on digital labour markets is supported elsewhere. Although a number of studies highlight enhanced risks and reduced flexibility for platform workers due to the pandemic (e.g., Han and Hart 2021; Rani and Dhir 2020), Stephany et al. (2020) pose a contrast between a so-called distancing bonus and losses from downscaling. They argue the net balance between these effects is unclear and likely to be highly heterogeneous (e.g., varying across locations and occupations). In addition, as the response to the pandemic has evolved over time and firms and workers have adapted, we should not necessarily expect a one-off constant effect on either demand for or supply of manual freelancers. Indeed, data suggests that demand on major online labour platforms suffered a sharp drop but was followed by a significant above-trend increase in the early months of the pandemic (Banga and te Velde 2020; Mueller-Langer and Gomez-Herrera 2022; Stephany et al. 2020). So potential for variation over time in market dynamics during the pandemic should be kept in mind.

3 Study context

3.1 The *Biscate* platform

Our analysis of how digital labour platforms mediate economic shocks for workers focuses on the *Biscate* platform in Mozambique. Our interest here is two-fold. First, we contend the platform can provide granular insights into the dynamics of supply and demand for specific types of labour both before and during the COVID-19 crisis. Second, following Section 2, it provides an opportunity to investigate the extent to which digital platforms of this kind tend to amplify broader economic shocks or might help absorb them, at least in the specific environment of Mozambique where labour informality (mostly, precarious work) is already predominant.

Biscate is a free-to-use digital platform, launched in October 2016, that connects manual freelancers, such as plumbers or hairdressers, to clients in order to undertake specific tasks.[4] In Portuguese 'biscate' means an odd-job, essentially some type of temporary cash-in-hand work to complete a specific task, usually based on a verbal agreement. As such, this platform consists of a quick and convenient way for informal workers to find clients, as well as for potential clients to find workers in their location with particular technical skills. Thus, the *Biscate* platform provides a means to match supply and demand for informal labour for technical-professional service tasks sold to third parties and generally provided on-site (e.g. at the home of the client), for payment in cash.

[4] The platform is owned and operated by the digital services company UX (www.ux.co.mz). It was established under a partnership between Vodacom (a leading mobile phone company in Mozambique), the Let's Work group of the World Bank, AIESEC (an international student association), Oxford Policy Management (OPML), IdeaLab, and UPA (a local NGO). See also: www.biscate.co.mz/sobre.

To access the service, prospective workers must register on the platform. Registering can be undertaken either directly via the internet or via any type of mobile phone (not only smart phones). For the latter, the platform makes use of Unstructured Supplementary Service Data (USSD) codes, which is a protocol used to communicate via text message with the telephone service provider's computer servers. To register, workers must give their name, gender, the service (profession) they provide, and the province and district in which they reside. After that a profile is created for the worker on the platform, where clients can request their contact number to negotiate a service request.

Currently, *Biscate* covers 18 professional categories, including carpentry, manicure, hairdressing, and gardening (see Appendix Table A1 for a full list). Although *Biscate* is free to use for both workers and clients, it is only available to users of the Vodacom mobile network. Therefore, any person with access to a mobile phone and a Vodacom number, which in 2019 accounted for around 50 per cent of all active numbers in the country,[5] can access the platform. Rates of mobile phone penetration in Mozambique are comparatively high: by January 2020, around half of Mozambican adults had access to a mobile connection (Kemp 2020). Thus, it is reasonable to conclude that most (urban) informal workers theoretically have access to the technological tools to use *Biscate*. To date, more than 50,000 unique workers have registered on the platform.

On the demand side, prospective clients must create an account (also using a Vodacom phone number), after which they can search for relevant workers using the profiles on the platform, filtering by professions and locations. At the end of 2020, around 30,000 unique clients had registered on the platform. In turn, there are three specific moments in which we observe worker–client connections. First, if a prospective client identifies a suitable candidate, they can make a request for the worker's phone number so as to negotiate a possible task. We refer to this as a contact request, which triggers an automatic SMS message from the platform to the selected worker. Second, after receiving this contact request, the worker can signal back to the platform (by SMS) whether they are free to work, in which case their number is shared. We refer to this as availability. And, third, if a work agreement (a *biscate*) is agreed, this is also recorded on the system based on follow-up SMS messages to the client. Note that no financial transactions are mediated by the platform, which means we do not observe worker income or other proxies for welfare effects.

Through a collaboration with UX, the operator of the platform, we were given access to fully anonymized individual-level data from *Biscate* observed on a weekly basis. In addition to basic profile information entered by each worker (e.g., account creation date, location, profession, gender, experience, and education level), their administrative data includes the number of contact requests (per person per week), the number of positive availability responses, the number of tasks concluded, and client-provided ratings. The latter information is also collected automatically by the platform by sending follow-up text messages to clients after the contact has been requested. Since this more detailed information is only available from mid-2018 onwards, we start our analysis at this point in time, effectively excluding the initial start-up period.

3.2 Evolution of COVID-19 in Mozambique

We now describe how the COVID-19 pandemic evolved in Mozambique during the period from its inception through to the end of Q1 2021 (our focus herein). Figure 1(a) illustrates that in the early months of the crisis there was little evidence of widespread community transmission in Mozambique.[6] In part, this is explained by the country's distance from the initial epicentre as well as a combination of international travel restrictions and quick imposition of a range of domestic measures to contain the

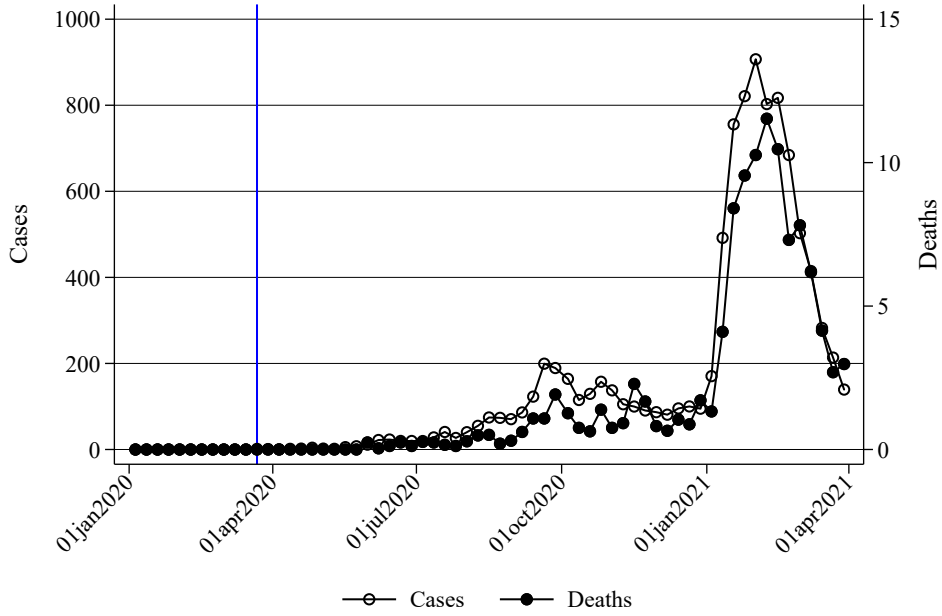
[5] Personal communication from the National Institute of Communications of Mozambique, 2 January 2019.

[6] COVID-19 was officially declared a pandemic by the World Health Organization in early March 2020.

virus. Even before the country's first official positive case, recorded on 22 March 2020, the government had already announced the cancellation of travel entry visas, closure of schools, and suspension of social events with more than 50 participants. And, despite having accumulated only eight positive cases, a formal State of Emergency was announced on 30 March 2020, implying a set of additional restrictive measures, including the prohibition of any non-essential movement across national borders.

Figure 1: Timeline of the evolution of the COVID-19 pandemic in Mozambique

(a) Weekly rolling seven-day averages of reported cases and deaths



(b) Relative intensity of restrictions and effects associated with COVID-19

Note: all observations are weekly; cases and deaths are rolling seven-day averages; indexes in panel (b) proxy for different potential COVID-19 effect channels relating to the stringency of public health restrictions, workplace mobility, and employment conditions (see Section 4); all these indexes are centred on zero in the pre-pandemic period for 2020; the vertical blue line indicates the start of the pandemic in Mozambique.

Source: authors' compilation based on data from https://coronavirus.jhu.edu/data/mortality (mortality), https://www.bsg.ox.ac.uk/research/research-projects/covid-19-governmentresponse-tracker (stringency), Google's Mobility Index, and the National Institute of Statistic's current employment conditions index.

Figure 1(b) illustrates the relative severity of official public restrictions imposed in Mozambique in connection with COVID-19, based on the Oxford stringency index (see further below) and where the maximum possible score is 1, obtained under a full lockdown. As shown, relatively severe restrictions were implemented very early on (under the State of Emergency) and were maintained for approximately five months.[7] By this time, however, it was clear the virus would not be eradicated and there was a need to adapt to a so-called *nova realidade* (new reality). Thus, from the end of August 2020 some restrictions were gradually relaxed and new legislation was passed to allow for a (somewhat weaker) State of Public Calamity, including a more open border policy. Even so, relaxation of restrictions coincided with a first wave of infections, which increased from an average of 70 daily cases in the month of August 2020 to 160 cases in September and 132 in October 2020—that is, average daily cases doubled.[8]

A more severe second wave of infections emerged in the first months of 2021, closely following the discovery of the Beta variant in neighbouring South Africa. In January and February 2021, the country registered over 600 daily positive cases on average, prompting the government to reimpose and tighten restrictions in some areas. For instance, from early February, a curfew was instigated in the Greater Maputo region (in effect until early 2022). By mid-April 2021, while the number of new cases had fallen to fewer than 100 per day, many restrictions remained in force, although the government had decided to reopen schools and allow some national sports activities to restart.

In comparative international perspective, the public health effects of COVID-19 in Mozambique appear to have been *relatively* moderate. Up until the end of 2021, only around 2,000 deaths from COVID-19 had been officially recorded (on a cumulative basis), equivalent to just 6.5 deaths per 100,000 population. So, even if the true figures were ten times larger (due to under-counting), mortality associated with COVID-19 in Mozambique would still be significantly below the rates observed in some high-income countries (e.g. 225 per 100,000 population in the UK).[9] Yet this does not mean the economic implications also have been mild. Comparing 2020 quarterly real GDP with the same period of 2019, the country only registered positive growth in the first quarter (see Figure A2). Overall, real GDP recorded a fall of around 2.3 per cent in 2020 versus 2019. From a more disaggregated perspective, the majority of sectors registered negative real growth in 2020. As elsewhere (e.g. ILO 2020), some sectors suffered much more than others. For instance, the hospitality sector was worst hit by a large margin, recording an overall real contraction of 30 per cent from the second to the fourth quarter compared to the same period in 2019. Double-digit contractions were only also recorded in the mining industry, largely reflecting global demand challenges; but some sectors, such as agriculture and health, even sustained positive growth through the year.

Granular information regarding the magnitude and nature of the economic effects of COVID-19 has been scarce. Even so, early survey data collected by the National Institute of Statistics (INE 2020) confirm significant impacts across most sectors. Their report estimates more than 70,000 small enterprises were negatively impacted by the pandemic, leading to extensive firm closures and a loss of at least 40,000 jobs. A case study of the beach tourism sector also found sales volumes fell by around 90 per cent in 2020 versus 2019 across a wide range of firms within the sector, as well as employment losses affecting around 60 per cent of workers (Aly et al. 2021).

Macroeconomic simulations have sought to isolate the (counterfactual) contribution of the pandemic to aggregate economic outcomes. As set out by Betho et al. (2022), the COVID-19 shock caused the economy to contract by 3.26 per cent on aggregate, with the largest contractions (in terms of value added)

[7] See Figure A1 for a narrative plot of the various restrictions over time.

[8] Testing capacity has been limited in Mozambique, so it is fair to assume that official case numbers are biased downward by some order of magnitude.

[9] For details, see: https://coronavirus.jhu.edu/data/mortality.

being in the hospitality, trade, and transport sectors, all of which recorded declines of over 10 per cent. A complementary study at the microeconomic level similarly finds material increases in household poverty (Barletta et al. 2021), primarily driven by losses to income through employment. The authors find consumption poverty may have increased by almost 10 percentage points in 2020, pushing roughly two million people or upwards of 5 per cent of the population below the official poverty line. For informal sector workers in particular, the UNDP (2020) estimate weekly profits among petty traders in Maputo fell by around 60 per cent during the early phase of the pandemic, with women much more affected than men. The depth of the shock is supported by a World Bank phone survey of urban households (based on a nationally representative household survey), which in June 2020 found that around one-third of all workers aged over 18 had stopped working since the start of the pandemic.[10]

These negative economic impacts associated with the COVID-19 pandemic are captured by two aggregate time series, plotted in Figure 1(b) (see also Table 2). The first is Google's Mobility Index for travel to/from a workplace, taken from their 'Community Mobility Reports'. Defining the reference period as the average of the index for all weeks in 2020 before the first case of COVID-19 in the country (hereafter, the start of the pandemic), the figure shows relative deviations to this baseline. In line with the introduction of the State of Emergency, workplace mobility dropped by around 25 per cent and remained depressed throughout. The second series is the National Institute of Statistic's current employment conditions index, based on a survey of firms across multiple sectors. Using the same reference period, this shows a decline of around 15 per cent in the early phase of the pandemic. And, despite some recovery in the last two quarters of 2020, coinciding with the shift to a State of Public Calamity, in early 2021 the index was almost 30 per cent below its level a year earlier.

Overall, a diverse range of evidence indicates that the economic fallout from the COVID-19 pandemic in Mozambique has been significant, being particularly severe for export-oriented sectors as well as those in the non-agricultural informal sector. This is in line with evidence elsewhere. For example, as early as April 2020, the ILO estimated 1.6 billion informal sector workers were being severely affected, facing up to a 60 per cent drop in earnings (ILO 2020). Survey data summarized by Egger et al. (2021) similarly points to large negative impacts on households in developing countries, but with more acute effects on the poorest.[11] It further merits note that informal work is the dominant source of employment in urban areas throughout the country, especially for women and younger people (see Jones and Tarp 2013, 2015, 2016). Moreover, contributory and non-contributory social safety nets are limited in scope, and recorded very limited effective expansion in response to the pandemic. Put differently, public sector mechanisms to help absorb the economic shock(s) associated with COVID-19 have been absent for most households.

3.3 Descriptive statistics

Table 2 reports summary statistics from our dataset for the period 2019–21. Panel (a) gives the headline outcomes of interest, covering variation in market supply, proxied by the number of workers registered on the platform and the associated rate of supply growth, as well as the client–worker connection indicators (contacts, availability, and task agreements). While the raw absolute counts of these latter metrics capture the scale of market demand, meaningful interpretation also depends on the supply of workers on the platform at a given time.[12] For this reason, we report demand per registered worker (similar to vacancies per worker considered elsewhere; e.g. Gautier and Moraga-González 2018). Thus, henceforth, client–worker connections are reported as counts per 100 workers, formally calculated at the market-

[10] See www.worldbank.org/en/data/interactive/2020/11/11/covid-19-high-frequency-monitoring-dashboard.

[11] For further discussion and evidence, see also Alfaro et al. (2020), Bussolo et al. (2021), and Sumner et al. (2020).

[12] For example, if there are no workers in region A for profession B, then there is no possibility of any connections being registered for this demand combination.

wide level as:

$$y_{jkt} = \frac{100 \sum_{i|jkt} Y_{ijkt}}{\sum_{i|jkt} 1_{ijkt}} \tag{1}$$

where Y is an absolute outcome count (e.g., number of contact requests); i indexes individuals, j professions, k provinces, and t time (in weeks). So, the denominator gives the number of registered workers in a given province and profession in week t. Figure A3 illustrates national averages for the three primary outcomes by period (week).

Table 2: Aggregate descriptive statistics by period, from weekly observations (2019–21)

		2019		2020				2021
		S1	S2	Q1	Q2	Q3	Q4	Q1
(a)	Registered workers (1000s)	27.60	33.07	34.98	36.38	37.63	40.94	46.24
	Registered workers (Δ)	0.69	0.70	0.43	0.30	0.26	0.65	0.94
	Contact rate	4.35	4.05	2.51	3.36	2.60	3.19	3.87
	Availability rate	1.69	1.61	0.97	1.05	0.77	1.23	1.40
	Agreement rate	1.69	1.60	0.96	1.03	0.77	1.22	1.39
(b)	Active workers (1000s)	11.80	14.91	14.24	14.48	14.41	17.27	22.46
	Active workers (Δ)	0.85	0.91	–0.35	0.13	–0.04	1.41	2.05
	Demand index	10.47	9.13	5.84	8.37	6.60	8.01	8.62
	Supply response index	38.64	39.61	39.27	30.57	29.71	37.52	36.27
	Demand/supply balance	51.79	47.80	38.41	52.24	47.35	44.89	48.78
(c)	Female (%)	14.15	14.62	14.78	15.14	15.37	15.45	15.42
	Education (years)	10.47	10.40	10.36	10.37	10.35	10.29	10.16
	Experience (years)	5.80	5.79	5.79	5.77	5.76	5.74	5.67
	No personal info. (%)	9.46	8.44	8.03	7.88	7.81	6.75	6.12
(d)	COVID-19 cases	0.00	0.00	0.18	9.30	79.00	112.10	538.72
	Stringency index	0.00	0.00	0.09	0.59	0.67	0.57	0.58
	Mobility index	0.00	0.00	–0.01	–0.16	–0.17	–0.22	–0.34
	Employment index	0.06	0.01	–0.02	–0.11	–0.07	–0.06	–0.22

Note: cells report unweighted aggregate (national-level) weekly means by semester (S) or quarter (Q), with the exception of the raw number of workers, which reports the value in the final week of each sub-period; Δ is the growth rate in percent; variables in panels (a)–(c) are calculated from the *Biscate* database; connection rates in panel (a) are counts per 100 registered workers; variables in panel (d) are normalized to zero (within each sector/province) for the period January–March 2020.

Source: authors' estimates.

Panel (b) uses the same base data to develop further summary metrics. While the platform has now established an algorithm to de-list dormant workers (e.g. where their contact telephone number is no longer active), implying the rate of growth in registered users *may* be negative, in reality we only observe one week (out of 150 observations) where this is the case on aggregate. Consequently, we apply a stricter procedure to identify which registered workers are plausibly active—namely, we define individuals as active if they either registered on the platform within the past six months or responded as being available for work in the same period. So, accounts registered more than six months ago but with no work connections in the same period are considered inactive, which represents around 50 per cent of all registered workers.

Combining our estimate of active workers with the number of contact requests (per worker) yields a simple measure of market demand conditions, which is equivalent to the contact rate but with a modified denominator:

$$d_{jkt} = \frac{100 \sum_{i|jkt} \text{Contacts}_{ijkt}}{\sum_{i|jkt} \text{Active}_{ijkt}} \geq 0 \tag{2}$$

where 'Active' is a dummy variable taking a value of 1 if the individual is classified as active, and 'Contacts' is the simple count of requests. At the minimum, $d_{jkt} = 0$, demand is zero, implying all capacity on the platform is surplus to needs. Similarly, we can create a complementary measure of

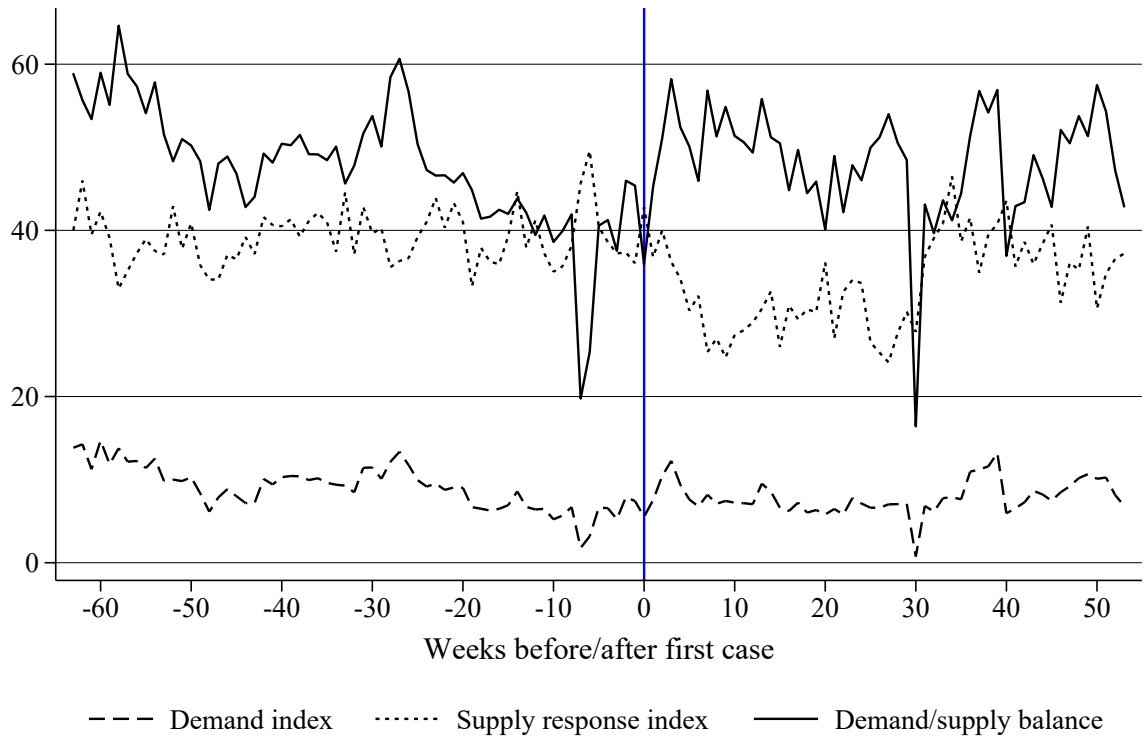
effective supply response conditions, given as:

$$s_{jkt} = \frac{100 \sum_{i|jkt} \text{Available}_{ijkt}}{\sum_{i|jkt} \text{Contacts}_{ijkt}} \geq 0 \tag{3}$$

which takes a minimum of zero if none of the contacted workers is available, meaning all potential demand (captured by the denominator) is unmet. Finally, the square root of the ratio of these two measures: $b_{jkt} = 100(d_{jkt}/s_{jkt})^{1/2}$, yields an overall summary indicator of market conditions, namely the relative balance between demand and supply response conditions.[13] This is informative when viewed over time—for example, if demand conditions strengthen while supply response conditions remain stable or decline, the index will increase in value, indicating a shift towards relatively more favourable demand conditions.

Looking across these outcomes, we observe consistent growth in the absolute number of workers registered on the platform. This roughly doubles from around 27,000 at the end of the first half of 2019 to 46,000 by the end of the first quarter of 2021. At the same time, such growth was accompanied by a trend decline in demand-side outcomes (e.g. contact and agreement rates per 100 workers) prior to the COVID-19 period. This is underlined by our demand conditions index, illustrated in Figure 2, which fell by around 40 per cent from early 2019 to end Q1 2020. However, over the same period, supply response conditions remained stable—two of every five contact requests received a positive response, translating almost one-to-one into agreed tasks.

Figure 2: Time series of market conditions indices



Note: the plots show aggregate (national-level) averages per period, indexed by periods before and after the first case in the country.

Source: authors' estimates.

[13] We use the square root because the resulting measure then is the geometric mean of d and s^{-1}. Also, note the two indexes theoretically are not bound from above and can exceed 1.

Following the emergence of COVID-19 (Q2 2020 onwards), we perceive a moderate up-tick in demand conditions. For instance, comparing the first two quarters of 2021, the contact rate increased from 2.5 to 3.7 per 100 registered workers. However, supply response conditions simultaneously weakened. Thereby, our index of demand/supply balance records a substantial jump at the onset of COVID-19 (increasing from 38 to 52) and remains broadly stable thereafter, averaging a score of 48 for the COVID-19 period to end Q1 2021. This constitutes indicative evidence of material changes in dynamics on the *Biscate* platform associated with COVID-19, motivating further econometric analysis, which is pursued below.

Additional descriptive statistics are given in Table A2. Here we report the number of workers by profession and region (north, centre, and south) at the end of 2019, as well as the average weekly agreement rate (per 100 workers) for the full year of 2019. These show that the bulk of registered workers are located in the south and north respectively; these regions also show comparatively higher task agreement rates–for example, in the south the unconditional probability of agreeing a task was around 4 per cent versus 1 per cent in the centre. However, there are large differences across professions, both in terms of the average number of registered workers and agreement rates. Indeed, even in the south the latter range from over 16 per cent (cooking) to under half of 1 per cent (manicure).

Before proceeding, we recognize that demand-side outcomes are moderate when viewed in absolute terms—for example, for every 100 workers, on average fewer than five contacts are requested per week and fewer than two work agreements concluded. This partly reflects the fact that approximately two-thirds of all registered workers do not record any agreed tasks, supporting our attempt to identify the subset of active workers via the demand-side index. Of course, it is feasible that some contact requests result in subsequent work agreements that are not registered on the platform or are used (shared) on a private basis. In this sense, the final demand-side outcomes may represent lower bounds. Even so, it is unlikely that those individuals who never received or responded to any contact requests in the *Biscate* data ever found work via the platform.

4 Empirical approach

Our analytical aim is to estimate changes in the dynamics of both demand and supply on the *Biscate* platform specifically associated with the COVID-19 pandemic. At the outset this raises a question of the appropriate level of analysis. Following debates regarding the magnitude of labour supply elasticities (e.g., Chetty et al. 2011; Fiorito and Zanella 2012; Horton 2021), some form of macro-level (aggregated) analysis can be valuable to capture movements at both the intensive and extensive response margins, where the latter refers to activities undertaken by new market participants. Also, as already highlighted, since we observe both registration (potential supply) and jobs undertaken (effectively, instances of supply matched to a demand), metrics derived from aggregated statistics can help investigate the relative balance between demand and supply conditions.

As a consequence, our primary focus is a panel constructed at the profession $\times$ province level, yielding 198 distinct units observed over a maximum of 150 weeks.[14] This not only provides for substantial degrees of freedom, but also allows us to control for unobserved differences at the specified level of aggregation, and it forms a basis to investigate response heterogeneity (e.g. between male- vs female-dominated professions).

[14] Later, we report results at alternative levels, including for individuals—see Section 5.2.

Our generic aggregate empirical model thus takes the following form:

$$y_{jkt} = \alpha_{jk} + f(\cdot) + x'_{jkt}\beta + \sum_{m=1}^{12} \lambda_{jkm}[\text{Month}_t \equiv m] + \sum_{n=2018}^{2021} \varphi_{jkn}[\text{Year}_t \equiv n] + \varepsilon_{jkt} \tag{4}$$

where (as before) j indexes professions and k provinces; square brackets denote indicator functions (taking a value of 1 if the enclosed expression is true, and 0 otherwise). The term $f(\cdot)$ stands in for a variety of different functions (regression terms) that might capture variation in the outcome (y) over time due to the pandemic, conditional on included covariates (vector x) and an extensive range of fixed effects. The former are chosen to represent potential time-varying influences on the outcomes, but which are not themselves plausibly influenced by the pandemic. They include the average daily temperature and accumulated precipitation for the capital city of each province, represented as standardized deviations from monthly medians, as well as dummy variables for the first and last weeks in each month.[15] The fixed effects cover each individual market (combinations of professions $\times$ provinces), as well as their interactions with calendar months and years.[16]

Four alternatives for f are considered. The first three treat the pandemic effect as an unobserved latent trend. Concretely, defining $t = t^*$ as the start of the COVID-19 pandemic, we have:

1. *Pandemic dummy variable*: the simplest formulation is just a dummy variable for the entire pandemic period: $f_1(t - t^*) = \delta[t - t^* \geq 0]$, in which case δ estimates the conditional average difference in the outcome during the pandemic relative to the before period, effectively a constant (level shift) structural break.
2. *Pandemic polynomial in time*: to capture possible variation in responses *during* the COVID-19 period (as opposed to a constant effect), we construct a polynomial in time, given by: $f_2(t - t^*) = \sum_{p=0}^{3} \delta_p (t - t^*)^p [t - t^* \geq 0]$, which represents a kind of dynamic structural break, which nests the dummy variable formulation as a special case.
3. *Event study*: to test/adjust for pre-existing trends, as well as to allow for a fully flexible dynamic pattern of responses to the pandemic, we adopt an event study specification with separate coefficients both after *and* before the start of the pandemic: $f_3(t - t^*) = \sum_{p=-a}^{b} \delta_p [t - t^* \equiv p]$.[17]

Estimates of the effects of the pandemic derived on the basis of the above options should capture the systematic differences between the pandemic and pre-pandemic periods after removing various confounding factors. As such, the resulting vector of treatment effects is expected to indicate the *net* contribution associated with all underlying COVID-19 impact channels.

Our fourth choice for f attempts to parse these channels directly. In similar fashion to Mueller-Langer and Gomez-Herrera (2022), we directly include empirical proxies for different COVID-19 impact channels. We now define: $f_4(z) = z'_{jkt}\theta$, where the vector z contains variables introduced previously:

- '*Cases*' captures (the perception of) health risks associated with the virus, represented by the seven-day rolling average of diagnosed COVID-19 cases, available at the province level.[18]

[15] Climate data is taken from: https://climexp.knmi.nl, using the CMORPH and ERA5 series for temperature and precipitation, respectively.

[16] Month fixed effects adjust for potential seasonality, while aggregate year effects capture additional unobserved variation in each calendar year. Since all observations in 2021 fall within the pandemic period, we code both 2021 and 2020 as a single fixed effect.

[17] To ensure consistent interpretation, we follow the guidance of Borusyak and Jaravel (2018) and normalize two temporally distant coefficients in the pre-period to zero (months –5 and –1).

[18] This data was manually collated from daily bulletins provided by the National Health Institute, available at: http://covid19.ins.gov.mz.

- '*Restrictions*' is the original Oxford (Blavatnik School of Government) stringency index that measures the overall strictness of policies enacted by governments to restrict people's behaviour, with values ranging from 1 to 100. The data is available only at the national level but on a daily basis, from which we calculate weekly averages.[19]
- '*Mobility*' is the original Google Mobility Index for travel to/from a workplace taken from their Community Mobility Reports, available separately for each province, which indicate the negative or positive deviation of mobility compared to baseline days. A baseline day represents the *normal* level of mobility for a given day of the week, calculated as the median value from the five-week period running from 3 January to 6 February 2020.[20]
- '*Income*' is the monthly current employment conditions index, taken from the National Institute of Statistics' series of Indicators of Economic Confidence and Conditions (IECC), based on a survey of firms across multiple sectors. Since this index is available at the level of each broad economic production sector, we match professions in the *Biscate* data to a relevant economic sector and use this particular measure of employment conditions.[21]

Two additional points merit emphasis. First, as public health restrictions were imposed on a nationwide basis at the outset of the pandemic (only much later did some regional variation emerge), we assume the timing of the pandemic 'event' is identical for all units throughout. Thus, there is no contemporaneous control group or staggered treatment design. Second, the first two formulations for f impose a constant conditional effect of zero in the entire pre-pandemic period. In contrast, the event study design (f_3) and proxy formulation (f_4) do allow for variation in the before period. Even so, none of these options control for how any pre-existing trends may have evolved during the pandemic period in the absence of COVID-19—that is, in the case of the latent trend estimates, the estimated effects for the COVID-19 period are expected to combine both the specific effects of the pandemic *and* any continuation of prior trends in the outcomes.

To address this potential source of confounding, other studies have opted to pre-filter outcomes so as to remove the contribution of trends occurring prior to a specified event (see Bhuller et al. 2013; Kleven et al. 2014; Oster 2018). Pertinent to our own context, Fezzi and Fanghella (2021) seek to identify the effect of COVID-19 on GDP in Europe from energy consumption data. To remove effects associated with both short- and long-term pre-existing drivers of energy consumption, they project the outcome of interest on a set of controls based on observations from the pre-pandemic period only. The authors then use these estimates to predict the outcome for the full period, yielding a complete vector of residuals. This constitutes the pre-filtered outcome, which represents the difference in the path of the outcome variable to its expected path had the contributions of short- and long-term drivers continued unabated.

We follow this generic approach. Our first-stage model, estimated on observations before the onset of COVID-19, includes unit fixed effects, time-varying exogenous controls, plus calendar month dummies. In addition, to capture the remaining unobserved trends in the outcomes prior to the onset of COVID-19, we add a quadratic trend at the unit level (professions $\times$ provinces):

$$\forall t \text{ s.t. } t - t^* < 0 : y_{jkt} = \alpha_{jk} + \varphi_{jk} t + \psi_{jk} t^2 + x'_{jkt}\beta + \sum_{m=1}^{12} \lambda_{jkm} [\text{Month}_t \equiv m] + \varepsilon_{jkt} \tag{5}$$

[19] For further details and source data, see: https://www.bsg.ox.ac.uk/research/research-projects/covid-19-government-response-tracker.

[20] Since mobility data before 2020 is not available, we set all values in this period as equal to the baseline period. For further details and source data, see: www.google.com/covid19/mobility/.

[21] See Table A1 for the match of professions to aggregate sectors; from the IECC data we use the specific index '*emprego actual*'. Since data from March 2021 is not available, we presume this observation is unchanged from the previous month. For further information, see: www.ine.gov.mz/estatisticas/estatisticas-economicas/icce.

Next, we construct the pre-filtered outcome for all observations:

$$\tilde{y}_{jkt} = y_{jkt} - (\hat{\alpha}_{jk} + \hat{\varphi}_{jk}t + \hat{\psi}_{jk}t^2 + x'_{jkt}\hat{\beta} + \sum_{m=1}^{12} \hat{\lambda}_{jkm}[\text{Month}_t \equiv m]) \tag{6}$$

which is then used as the adjusted dependent variable in the core specification (equation 4). Throughout, we compare estimates using both the raw and pre-filtered outcome series.

5 Results

5.1 Market-level responses

Table 3 reports summary results of the application of our econometric approach, focusing on the primary outcomes taken from panel (a) of Table 2.[22] For the three dependent variables, given in panels (i)—(iii), each column refers to a separate OLS regression on the form of equation (4), applying a specific outcome transform (raw or pre-filtered) and a formulation for f (dummy variable, cubic in time, event study, proxies). To account for differences in the underlying size of the market segment represented by each unit (provinces and professions), we weight each unit by its respective total market share in the given period—namely, the number of workers in that unit divided by the count of all workers. However, to reduce noise from cells derived from a small number of workers, we drop markets that contain fewer than 20 registered individuals over one-third or more of all observations.

To facilitate comparison across the different models containing latent trends (f_1, f_2, f_3), the coefficient denoted 'COVID-19 (mean)' reports the period average of the estimated latent trend for the pandemic period only (i.e. for $t - t^* \geq 0$). Thus, for the dummy variable specification this is just the coefficient on the COVID-19 period dummy. In the two other cases, we construct the sum of the unique contribution of the f terms for each period (week) after the onset of COVID-19, from which we then derive the mean effect (and its standard error). For the event study formulation, this is then just the average of the event study coefficients in the COVID-19 period. Also, in implementing the event study specification, we reduce the number of coefficients to be estimated by: pooling distinct observations into consecutive blocks of 4 weeks; combining all observations 24 weeks prior to the onset of COVID-19 into the first event study coefficient; and combining all observations 48 weeks after the onset of COVID-19 into the last coefficient. This yields a total of six coefficients in the pre-COVID-19 period and 13 coefficients after the pandemic onset.

For the last model formulation, f_4, we report the coefficients corresponding to the four proposed pandemic channels. As previously noted (see Table 2(d)), each of these proxies is transformed to reflect relative intensities and is normalized to take a mean of zero in the immediate pre-COVID period.[23] For this specification we add two further tests. First, we report the pairwise correlation between the estimate of the cubic latent trend (f_3) and the trend derived from the set of proxies (f_4), considering observations in the pandemic period only. This indicates the degree of similarity between the two estimated series. The second test, denoted 'COVID-19 residual', takes the residuals from the proxy-based estimates and proceeds to regress them on a single pandemic period dummy variable. We report the estimated t-statistic on this variable, which indicates the presence of a remaining structural break in the residual series coinciding with the pandemic.

[22] We ignore the availability rate since it is almost identical to the agreement rate.

[23] Due to the presence of zero values, rolling COVID-19 case numbers are transformed using the inverse hyperbolic sine transform.

Table 3: Net effect of COVID-19 on core outcomes, data aggregated by combinations of provinces and professions

Transform →	Raw				Pre-filtered			
f →	Dummy	Cubic	Event	Proxy	Dummy	Cubic	Event	Proxy
(i) *Change in registrations*:								
COVID-19 (mean)	0.14	0.22	0.10		–0.20*	–0.26	–0.35	
	(0.10)	(0.20)	(0.31)		(0.10)	(0.18)	(0.30)	
New cases (roll av.)				0.01				–0.04
				(0.05)				(0.04)
Stringency index				–0.62*				–1.08***
				(0.35)				(0.32)
Mobility index				–1.70**				–1.52**
				(0.73)				(0.69)
Employment index				–0.39				–0.94***
				(0.35)				(0.33)
Constant	0.65***	0.67***	0.67***	0.73***	0.69***	0.72***	0.64***	0.79***
	(0.06)	(0.06)	(0.11)	(0.07)	(0.06)	(0.06)	(0.11)	(0.07)
R^2 (adj.)	0.11	0.12	0.12	0.11	0.30	0.30	0.31	0.30
$\rho(f_2, f_4)$				0.64				0.46
COVID-19 resid.				0.59				0.21
(ii) *Contact rate*:								
COVID-19 (mean)	1.20***	1.32	0.42		1.97***	2.64***	1.77***	
	(0.40)	(0.83)	(0.61)		(0.42)	(0.76)	(0.63)	
New cases (roll av.)				–0.30**				–0.18
				(0.13)				(0.12)
Stringency index				1.21				1.96**
				(0.97)				(0.93)
Mobility index				–3.44				–4.20**
				(2.20)				(1.98)
Employment index				–1.52*				–1.68**
				(0.80)				(0.70)
Constant	3.52***	3.58***	3.56***	3.65***	4.33***	4.39***	4.68***	4.47***
	(0.18)	(0.18)	(0.33)	(0.17)	(0.17)	(0.18)	(0.34)	(0.17)
R^2 (adj.)	0.50	0.50	0.52	0.50	0.31	0.31	0.33	0.31
$\rho(f_2, f_4)$				0.07				0.36
COVID-19 resid.				0.47				0.14
(iii) *Agreement rate*:								
COVID-19 (mean)	0.30*	0.34	0.00		0.68***	0.97***	0.64	
	(0.16)	(0.33)	(0.40)		(0.17)	(0.30)	(0.42)	
New cases (roll av.)				–0.09*				–0.02
				(0.05)				(0.05)
Stringency index				0.06				0.36
				(0.41)				(0.40)
Mobility index				–1.36				–1.93**
				(0.98)				(0.92)
Employment index				–0.35				–0.28
				(0.33)				(0.28)
Constant	1.34***	1.37***	1.25***	1.40***	1.65***	1.69***	1.71***	1.72***
	(0.07)	(0.07)	(0.15)	(0.07)	(0.07)	(0.07)	(0.16)	(0.07)
R^2 (adj.)	0.46	0.47	0.49	0.46	0.34	0.35	0.36	0.34
$\rho(f_2, f_4)$				0.24				0.57
COVID-19 resid.				0.30				–0.20

Note: significance: *** 1, ** 5, * 10 per cent. Columns and panels summarize separate regressions on the form of equation (4), with N = 22,200; panels (i)–(iii) refer to distinct outcomes; different columns refer to combinations of alternative outcome transforms and choices for f; all models include a full set of unit fixed effects, unit by month and unit by year effects, and time-varying controls; 'COVID-19' is the simple average of estimated time effects in the COVID-19 period of the estimated latent trend; market share weights (based on worker counts) applied throughout; standard errors in parentheses, clustered by time (unique weeks).

Source: authors' estimates.

Four main points emerge from these results. First, in line with the descriptive statistics, the majority of point estimates based on the latent trend specifications indicate the COVID-19 period was associated with a positive and significant net increase in demand-side outcomes (rates of contact and agreement), but no clear effect on the supply side (growth in registrations). For instance, taking the simple dummy variable formulation with the raw outcome data (column 1), the contact rate was 1.2 points higher in the COVID-19 period than before, representing a proportional increase of around one-third (using the constant term as the pre-COVID-19 reference). Similarly, the agreement rate increased by 0.3 points on average, equivalent to a 20 per cent increment.

Second, use of the pre-filtering procedure does not substantively alter any of the headline findings, relative to those from the raw outcomes. Nonetheless, the magnitudes of the coefficients of interest generally are somewhat larger under pre-filtering, especially for the demand-side outcomes, consistent with a trend decline in these outcomes before the onset of the pandemic. That is, if pre-pandemic trends had been sustained, we would have expected the demand-side outcomes to trend lower through 2020 onwards.

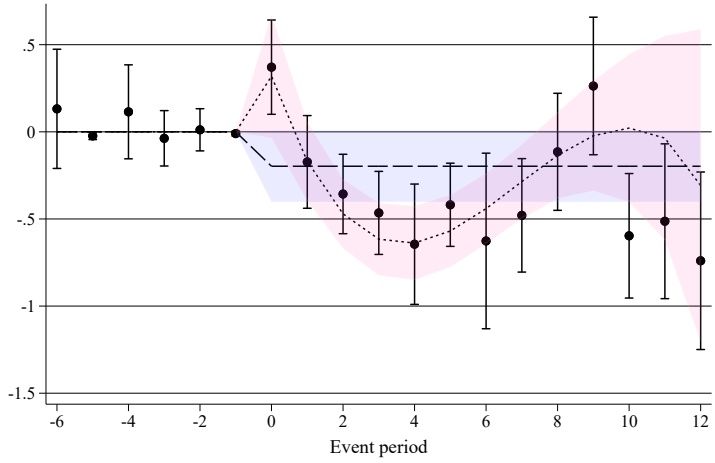
Third, the pandemic period-averages for the dynamic latent trend specifications (namely, the cubic and event study models) closely align in sign and magnitude with results from the basic dummy variable specifications. Even so, the results are not identical, suggesting that the effect of COVID-19 is not necessarily well described as a one-off level shift. To further explore temporal variation in the net effect of the COVID-19 pandemic, Figure 3 thus plots the underlying results from the regressions summarized in Table 3, focusing on the three latent trend specifications with pre-filtered outcomes (Figure A4 gives the equivalent plots for the raw outcomes). For simplicity, we collapse the results to the event periods, such that event zero represents the first four weeks of the pandemic. The points (and confidence intervals) illustrated in the plots for each distinct event period refer to the set of event study coefficients; the dashed and dotted lines give the trends derived from the dummy and cubic specifications, which impose a zero effect in all event periods before zero; and the shaded areas give the corresponding confidence intervals for these same two specifications.

The plots nuance the summary regression findings. On the one hand, we note that all outcomes—including registration growth—show a positive and significant increment on the immediate onset of COVID-19 (event zero). While this supports the hypothesis of a material COVID-19 effect, the positive response on the supply side is only temporary, being quickly offset by a negative tendency, such that registration growth was significantly below the counterfactual (no COVID-19) rate within four months of the start of the pandemic. Demand-side outcomes remain in the positive domain but also weaken as the pandemic enters its second quarter (Q3 2020). But, coinciding with the relaxation of the harshest public health restrictions from Q4 2020 onwards, we see that all outcomes strengthen. This reveals that the estimates associated with COVID-19 for the supply side (registration growth) include both positive and negative responses at different times, giving an average effect that is not different from zero.

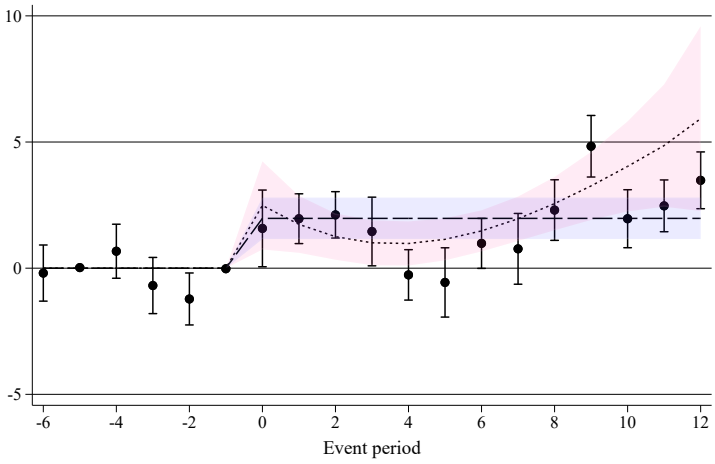
Fourth, estimates from the proxy variable specifications (f_4) suggest the included channels are indeed relevant. Although not all coefficient estimates are statistically significant, their overall pattern is consistent within each outcome. In particular, we observe a negative relationship between all three outcomes and both workplace mobility and employment conditions. That is, deterioration in these conditions—plausibly corresponding to increases in leisure time and loss of income (see Table 1)—are associated with increases in supply and demand on the platform. At the same time, the direct response to the official virus case-count is negatively related to *Biscate* demand, implying that contact and agreement rates tended to fall as cases increased. For instance, a doubling of cases is approximately associated with a 10 per cent fall in the contact rate. And for the stringency index, we find a negative association with supply growth, but a positive association on the demand side. These results support the hypothesis that different impact channels likely operated in opposite directions.

Figure 3: Dynamics in pre-filtered outcomes by event study periods

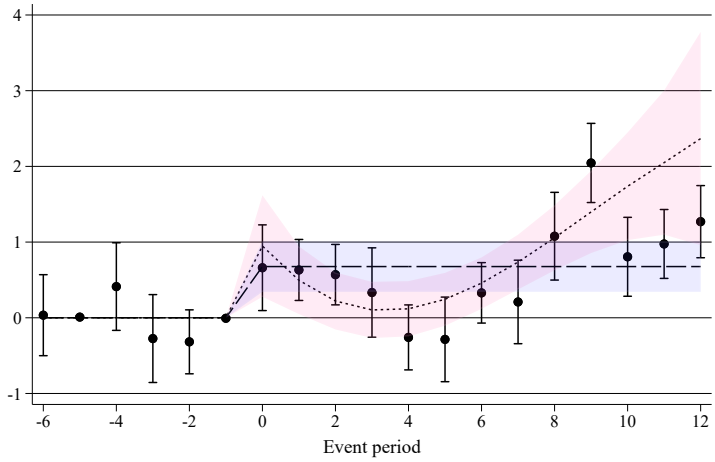
(i) *Change in registrations*



(ii) *Contact rate*



(iii) *Agreement rate*



Note: the plots illustrate results from regressions underlying those summarized in Table 3, applying alternative formulations of f to different pre-filtered outcomes (in the panels); event periods are four-week blocks, where zero is the first block of the COVID-19 period; period-specific dots (and 95 per cent confidence intervals) are event study estimates (f_3); dashed line (with shaded blue 95 per cent confidence area) shows the dummy variable specification (f_1); and dotted line (with shaded pink 95 per cent confidence area) shows estimates from the cubic specification (f_2).

Source: authors' estimates.

In addition, the correlation and residual tests applied to the proxy specification results, reported in the footer of each panel of Table 3, confirm the synthetic trends estimated from the four channels correspond closely to the (cubic) dynamic latent trends. For instance, the pairwise correlation coefficient between our estimates for f_2 and f_4 averages around 0.40 across the various models; and we no longer observe any residual one-time structural break associated with COVID-19 once these channels have been accounted for. To give one example, the t-statistic associated with the period average of the cubic latent trend estimated for the contact rate is over 3, while the corresponding t-statistic estimated from the residuals of the proxy specification is just 0.14.

Finally, we consider the additional (derived) summary metrics from Table 2(b). Results for these outcomes, analysed in the same fashion as above, are reported in Table 4, and Figure A5 illustrates the dynamic effects from these pre-filtered outcomes. These results confirm a material positive improvement in demand conditions on the *Biscate* platform during the pandemic, now adjusting for worker inactivity. There is no clear evidence of a material change in the supply response index during the pandemic, suggesting workers' availability in response to a contact request generally remained unchanged. Even so, for the proxy variable specification, we see a negative and significant association with the stringency index. Taken together, the measure of demand–supply balance shows a strong positive response to the pandemic, with both a positive overall association with the stringency index and a negative association with employment conditions.

5.2 Robustness

We investigate the robustness of our results along three main lines. First, we re-run the previous analysis but now aggregate the data to consecutively higher levels—namely, professions (18 units), provinces (11 units), and at the national level. Tables A5–A7 report the corresponding results as per the previous analyses, while Figures A6–A8 illustrate the dynamics of the estimated latent trends derived from the same results. All these are extremely consistent with the earlier analysis undertaken at a more granular level.

Second, we consider the extent to which our results may be driven by changes in market composition. As indicated in Table 2, the share of female workers has been steadily increasing over time, while average education levels have been falling. Plausibly, changes in the profile of existing workers on the platform may influence both demand and supply dynamics. Our previous analysis did not control for market composition, thereby allowing for a pathway running from COVID-19 to altered composition to outcomes—that is, previous estimates would capture both direct and compositional effects associated with the pandemic. To block this latter (indirect) channel, we now include a set of contemporaneous compositional variables, observed at the market level. These include the share female, mean education, prior work experience, share with a rating on the platform, and average time registered on the platform (among others).[24] These variables are included in both the pre-filtering stage and the main regression.

The results from this exercise are reported in Tables A3 and A4, pertaining to the core and additional outcomes respectively. Overall, these reveal only minor differences compared to the previous analysis, implying the contribution of the pandemic to variation in outcomes on the platform was not driven primarily by compositional effects. The main exception is for supply growth (see Table A3(i)), where we find a small positive (average) direct effect associated with the pandemic. Our interpretation is that changes in market composition during the pandemic may have dampened subsequent registration growth, but in the absence of such changes the direct effect of the pandemic on registration would have been positive.

[24] Full details available on request.

Table 4: Net effect of COVID-19 on additional outcomes, data aggregated by combinations of provinces and professions

Transform →	Raw				Pre-filtered			
f →	Dummy	Cubic	Event	Proxy	Dummy	Cubic	Event	Proxy
(i) *Demand index*:								
COVID-19 (mean)	4.23***	4.64**	2.53**		3.07***	3.23*	1.39	
	(1.08)	(2.01)	(0.99)		(0.93)	(1.82)	(0.97)	
New cases (roll av.)				–0.27				0.14
				(0.38)				(0.37)
Stringency index				2.94				0.22
				(2.51)				(2.29)
Mobility index				–11.78*				–8.33
				(6.29)				(5.50)
Employment index				–3.37				–5.81***
				(2.34)				(2.13)
Constant	8.20***	8.29***	9.31***	8.57***	10.09***	10.22***	10.97***	10.55***
	(0.45)	(0.44)	(0.77)	(0.42)	(0.41)	(0.42)	(0.75)	(0.40)
R^2 (adj.)	0.24	0.24	0.26	0.24	0.20	0.20	0.22	0.20
$\rho(f_2, f_4)$				0.37				0.54
COVID-19 resid.				0.23				–1.38
(ii) *Supply response index*:								
COVID-19 (mean)	–2.41	–2.74	–1.78		–0.95	–0.56	0.33	
	(1.60)	(2.40)	(1.31)		(1.58)	(2.43)	(1.31)	
New cases (roll av.)				–0.42				0.01
				(0.46)				(0.46)
Stringency index				–6.67**				–5.68*
				(3.26)				(3.30)
Mobility index				–7.01				–10.84
				(8.07)				(8.44)
Employment index				1.39				5.86
				(3.58)				(3.68)
Constant	39.42***	39.66***	37.86***	39.85***	39.11***	39.33***	37.93***	39.46***
	(0.67)	(0.67)	(1.63)	(0.72)	(0.66)	(0.67)	(1.63)	(0.70)
R^2 (adj.)	0.01	0.01	0.02	0.01	0.14	0.14	0.14	0.14
$\rho(f_2, f_4)$				0.58				0.54
COVID-19 resid.				0.57				0.64
(iii) *Demand/supply balance*:								
COVID-19 (mean)	17.45***	17.12***	10.33***		16.75***	16.90***	10.07***	
	(3.86)	(6.01)	(1.82)		(3.83)	(5.80)	(1.83)	
New cases (roll av.)				–1.95				–0.89
				(1.42)				(1.38)
Stringency index				29.44***				25.82***
				(8.61)				(8.22)
Mobility index				–10.43				–5.41
				(19.25)				(16.50)
Employment index				–18.15**				–23.69***
				(7.05)				(6.61)
Constant	47.63***	47.47***	51.87***	47.88***	51.79***	51.69***	56.42***	52.19***
	(1.37)	(1.31)	(2.26)	(1.24)	(1.40)	(1.39)	(2.26)	(1.26)
R^2 (adj.)	0.09	0.09	0.09	0.09	0.11	0.11	0.12	0.11
$\rho(f_2, f_4)$				0.24				0.50
COVID-19 resid.				0.09				–1.08

Note: significance: *** 1, ** 5, * 10 per cent. Columns and panels summarize separate regressions on the form of equation (4), with N = 22,200; panels (i)–(iii) refer to distinct outcomes; different columns refer to combinations of alternative outcome transforms and choices for f; all models include a full set of unit fixed effects, unit by month and unit by year effects, and time-varying controls for provincial temperature and precipitation deviations; 'COVID-19' is the simple average of estimated time effects in the COVID-19 period only; market share weights (based on worker counts) applied throughout; standard errors in parentheses, clustered by time (unique weeks).

Source: authors' estimates.

Third, we shift to an analysis at the individual level. One advantage here is that this allows us to control for fixed unobserved individual characteristics. However, a disadvantage is that analysis of demand-side outcomes (e.g. task agreements) naturally can only be undertaken meaningfully for those

individuals registered on the platform. As such, variation in these conditional outcomes will only capture effects at the intensive margin of behaviour. Nonetheless, similar to extant analyses of individual-level employment events (e.g. Cederlöf 2020), we start by creating a dummy variable that takes a value of 1 if the individual is registered on the platform at a given time and 0 if not. For client contacts and agreements, we simply use the raw count of these outcomes for each individual in each week, treating these as missing for individuals not registered on the platform.

Since the unconditional likelihood of registering is strictly increasing over the period observed, pre-filtering the data to take into account existing trends is necessary. Thus, in all estimates we apply a first-stage pre-filter (as per equations 5 and 6), here including individual-level fixed effects and allowing a quadratic in time at both the province and profession levels for the pre-COVID-19 period. The second stage (outcome models) also incorporates unit fixed effects, as well as separate month and year effects for each combination of province and profession.

The results are reported in Table 5. To assist comparison with the analysis based on aggregated data, all outcomes are pre-multiplied by 100. So, for instance, the estimate for the mean COVID-19 effect reported in the first column gives the estimated change in the likelihood of registration (in percentage points) associated with the pandemic period. For each outcome, we report results using both the simple dummy variable and cubic specifications for trends in the COVID-19 period (f). In general, the main qualitative insights are not changed when we move to this level. Nonetheless, we now see a consistent negative average overall effect of the pandemic on the (pre-filtered) registration likelihood, which contrasts with the null effect found in Table 3, also based on pre-filtered data. However, we continue to find a consistent and significant positive effect on the number of contacts and agreements, which also are of a similar magnitude to those found at the aggregate levels. For example, the cubic latent trend results (f_3) indicate the agreement count—conditional on being registered—increased by around 25 per cent during the pandemic compared to the pre-pandemic period. Combining the estimated effects on supply and demand, the derived total effect of the pandemic on task agreement rates remains positive, being equal to around 18 per cent.

Figure 4 illustrates the estimated latent trends from the individual-level regressions. These are also consistent with the aggregate-level analysis, confirming that dynamic responses during the pandemic followed a form of (shallow) U-shape for all outcomes, as well as stronger positive trends in the first quarter of 2021. Estimates from the proxy specification are also consistent with earlier analysis. For instance, we find a consistent negative direct effect of the virus on demand and supply. In contrast, more stringent public health restrictions were associated with a higher contact rate; and worsening employment conditions were associated with relatively higher registrations and contact rates (*ceteris paribus*). This underscores the point that responses to the pandemic varied over time as conditions changed, and that different effect channels operated in opposing directions.

Table 5: Effect of COVID-19 on pre-filtered individual-level outcomes

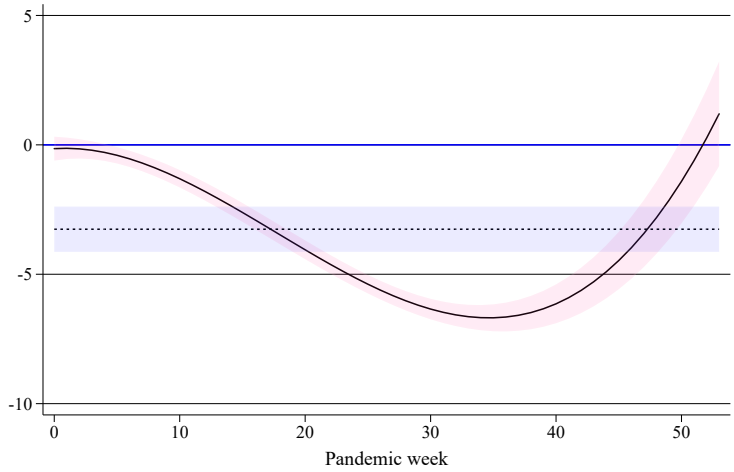
Outcome →	Registration dummy			Contact count			Agreement count		
f →	Dummy	Cubic	Proxy	Dummy	Cubic	Proxy	Dummy	Cubic	Proxy
COVID-19 (mean)	–3.26***	–3.56***		1.92***	3.09***		0.61***	1.09***	
	(0.45)	(0.32)		(0.41)	(0.77)		(0.15)	(0.29)	
New cases (roll av.)			–1.65***			–0.28***			–0.06*
			(0.11)			(0.09)			(0.04)
Stringency index			–0.46			2.49***			0.58
			(0.63)			(0.90)			(0.37)
Mobility index			1.93			–3.53*			–1.38*
			(1.50)			(1.91)			(0.81)
Employment index			–1.23*			–1.37**			–0.23
			(0.72)			(0.66)			(0.25)
Constant	55.16***	55.24***	55.10***	7.18***	10.57***	6.84**	2.77***	4.18***	2.64**
	(0.17)	(0.07)	(0.11)	(2.37)	(2.48)	(2.69)	(0.91)	(0.95)	(1.04)
Obs	7,072,200	7,072,200	7,070,746	4,321,942	4,321,942	4,321,942	4,321,942	4,321,942	4,321,942
R^2 adj.	0.16	0.16	0.16	0.03	0.03	0.03	0.02	0.02	0.02

Note: significance: *** 1, ** 5, * 10 per cent. Each column in each panel summarizes results from a separate regression on individual-level data; outcomes, in the columns, are a dummy variable for being registered on the platform, the number of contacts (per week), and the number of agreements (per week); all outcomes are multiplied by 100 and pre-filtered; contact and agreement outcomes are only observed for registered workers; all models include a full set of month, year, and individual fixed effects; sub-columns refer to alternative specifications for f; 'COVID-19 (mean)' is the simple average of estimated time effects in the COVID-19 period only; standard errors in parentheses, clustered by time (unique weeks).

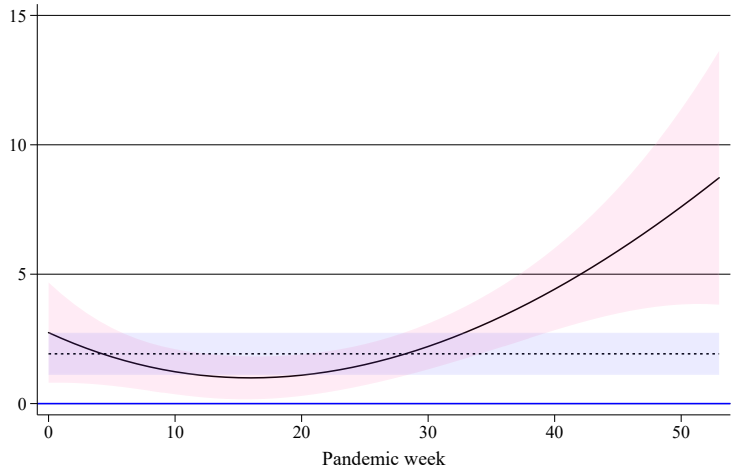
Source: authors' estimates.

Figure 4: Dynamics in pre-filtered individual-level outcomes during the pandemic

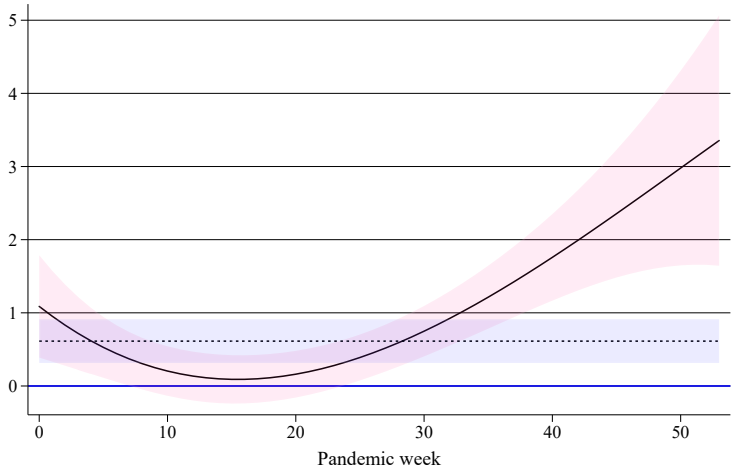
(i) *Registration dummy*:



(ii) *Contact count*:



(iii) *Agreement count*:



Note: the plots illustrate results from regressions underlying those summarized in Table 5; dashed line (with shaded blue 95 per cent confidence area) shows results from the dummy variable specification; and dotted line (with shaded pink 95 per cent confidence area) shows estimates from the cubic specification.

Source: authors' estimates.

5.3 Heterogeneity

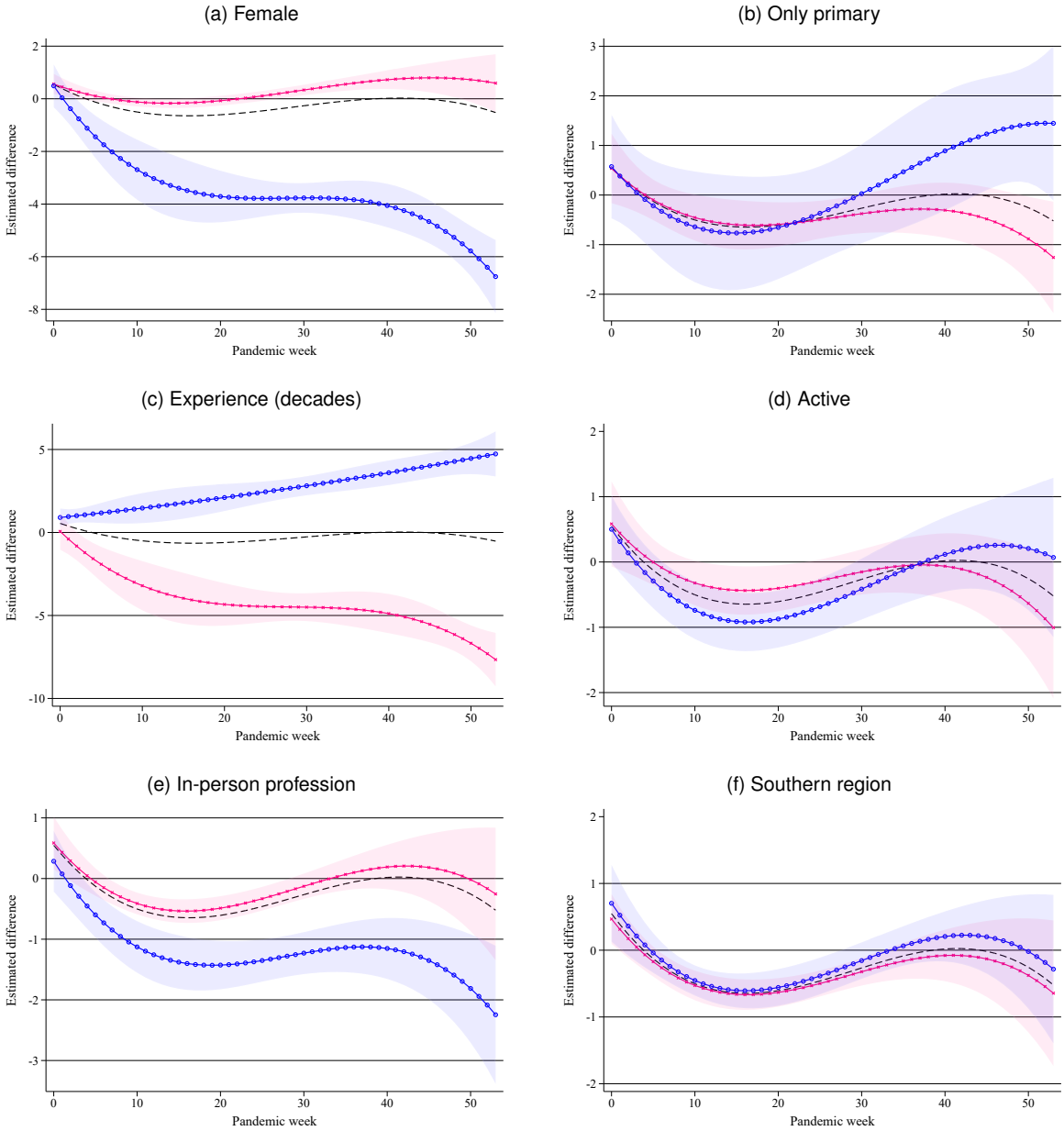
As a last exercise we explore the extent of heterogeneity in responses to the pandemic, the objective being to identify dimensions where this may be material. To do so, we return to the initial aggregate analysis (provinces $\times$ professions) and add a range of interactions between the pandemic latent cubic trend (f_2) and indicators describing how sub-markets (units) were composed *before* the start of the pandemic.[25] These include: (a) the share of registered workers that are female; (b) the share of workers with a primary education or less; (c) average previous work experience (in decades), as per the workers' profiles; (d) the share of workers classified as active (as described above); whether the profession is primarily realized on an in-person basis based on our own classification—see Table A1; and (e) whether the worker is registered as working in the southern region of the country, which has been most affected by the pandemic.

The results are plotted in Figures 5–7, which refer to the three main outcomes. In each figure, the panels refer to separate regressions equivalent to those reported in Table 3, but with added individual interaction terms. In all cases, the data is pre-filtered, also adding the interaction term to the first-stage regression to account for differences in trends along the chosen dimension in the pre-pandemic period. The figures show the estimated dynamic responses to the pandemic (by week) based on a cubic trend specification only, the maintained assumption being that zero represents the expected path had trends prior to the pandemic continued unchanged. The blue line (hollow circle marker) gives the predicted response to the COVID-19 shock for units where the given interaction term takes a value of 1 (e.g. if all workers were female or for in-person professions only) and the red line (cross marker symbol) gives the predicted response for units where the given interaction term takes a value of 0 (e.g. workers with no experience or regions outside the south).

Four findings merit comment. First, gender differences are material. Growth in registrations were significantly lower during the pandemic in sub-markets with larger female shares, but the positive overall effect of the pandemic on the demand side (contact and agreement rates) was generally more pronounced in less male-dominated sub-markets, perhaps partly reflecting weaker growth in absolute worker numbers. Second, sub-markets with a larger share of more experienced workers (the average being around 0.5 of a decade) experienced comparatively higher supply growth rates, but somewhat weaker demand. In contrast, third, sub-markets with a larger share of active workers showed more positive demand-side trends during the pandemic, relative to initially more 'dormant' sub-markets. Finally, in-person professions showed considerably slower growth during the pandemic, but no material differences to other professions in terms of demand dynamics.

[25] We use averages for the six months before the start of COVID-19. Doing so rules-out possible endogenous responses in the interaction terms to the pandemic, and assists with interpretation of the results.

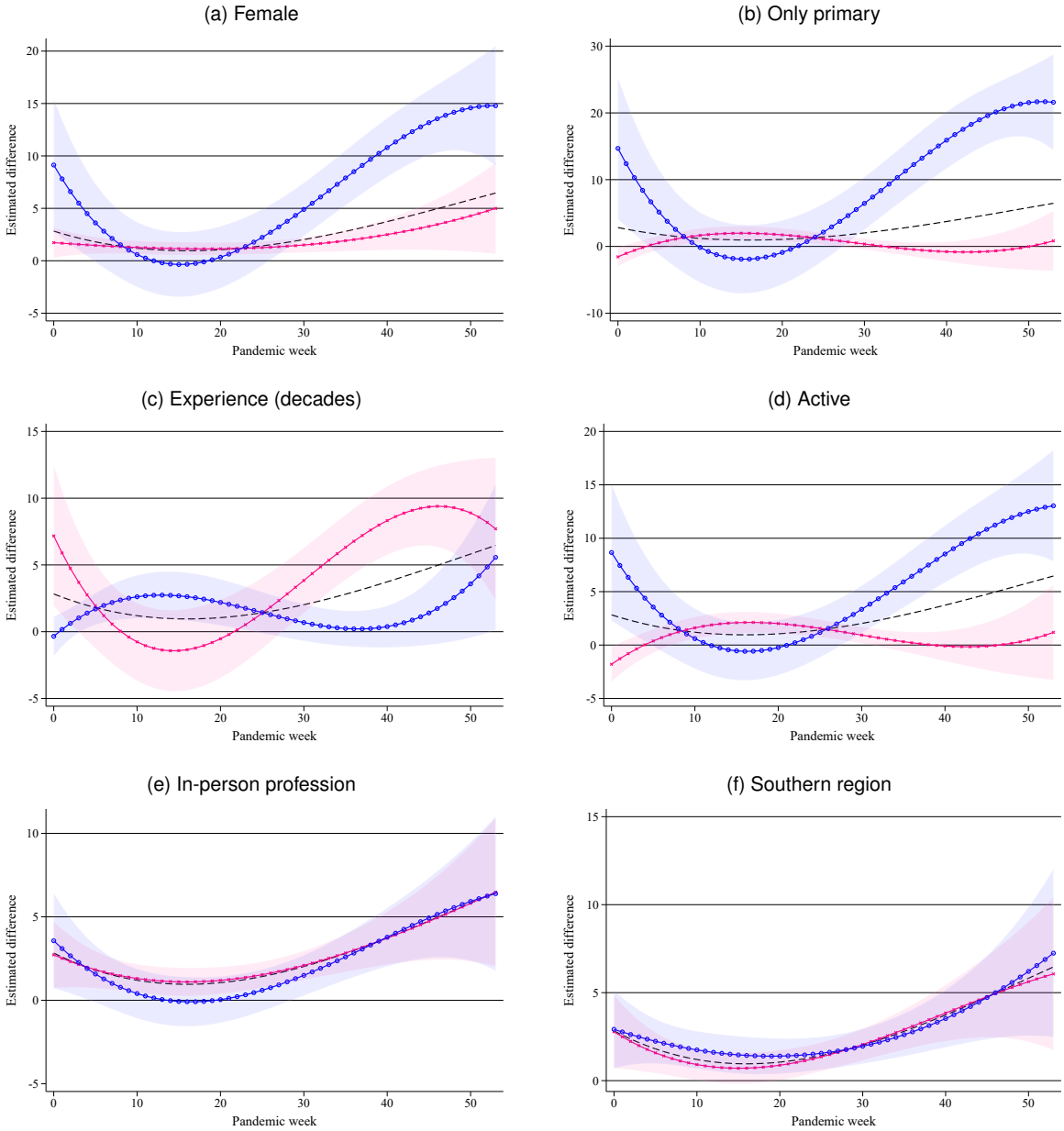
Figure 5: Heterogeneity in the effect of COVID-19 on registration growth



Note: the plots illustrate results from regressions equivalent to those in Table 3(i) with the pre-filtered outcome and cubic trend specification, extended to allow for heterogeneous trends along the dimension indicated in each sub-plot; pandemic weeks shown only (week zero = start of COVID-19); the blue line (and 95 per cent confidence area) gives predicted responses to the COVID-19 shock for units where the given interaction term takes a value of 1; the red line (and 95 per cent confidence area) gives the predicted response for units where the given interaction term takes a value of 0.

Source: authors' estimates.

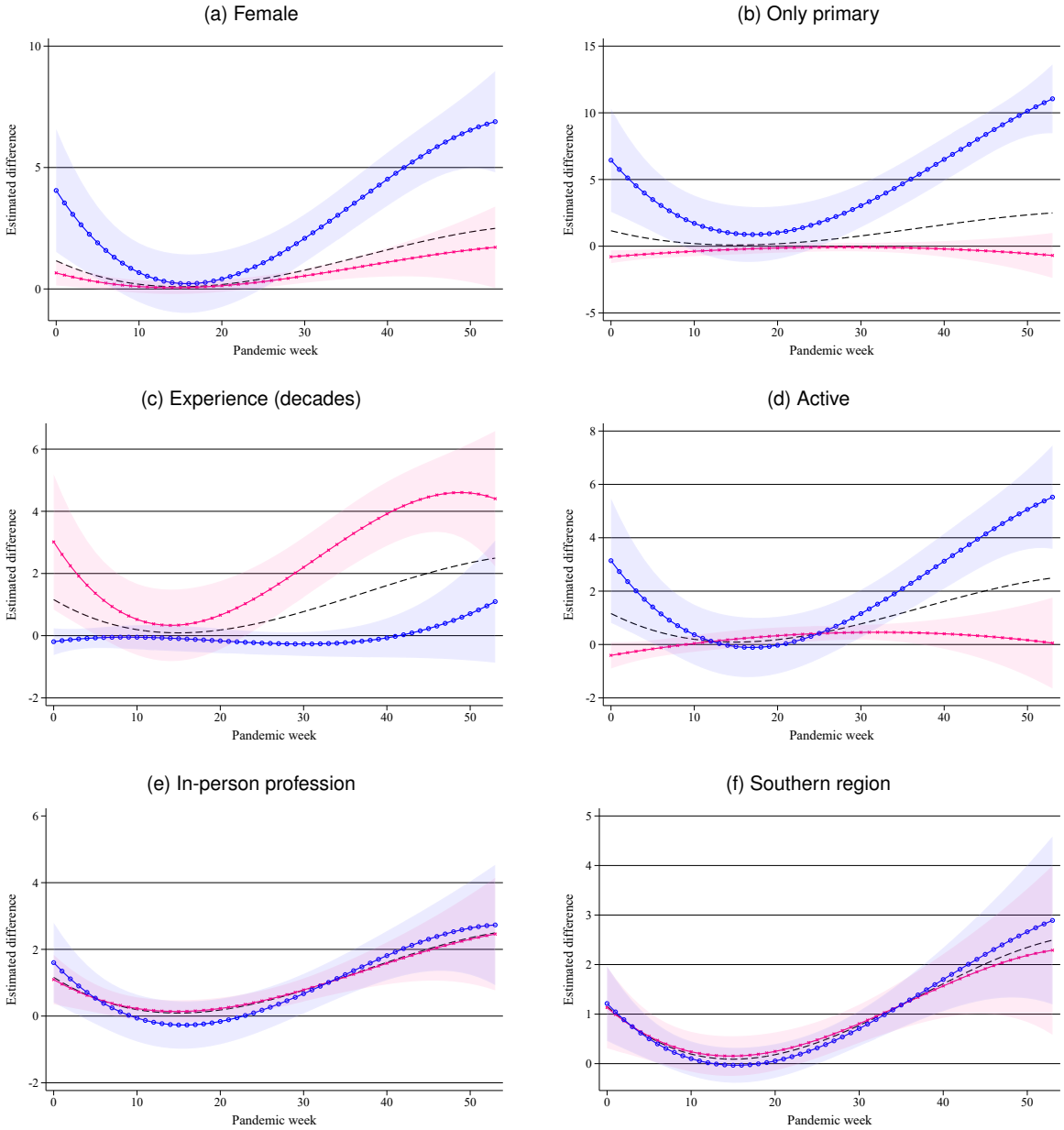
Figure 6: Heterogeneity in the effect of COVID-19 on contact rates



Note: the plots illustrate results from regressions equivalent to those in Table 3(ii) with the pre-filtered outcome and cubic trend specification, extended to allow for heterogeneous trends along the dimension indicated in each sub-plot; pandemic weeks shown only (week zero = start of COVID-19); the blue line (and 95 per cent confidence area) gives predicted responses to the COVID-19 shock for units where the given interaction term takes a value of 1; the red line (and 95 per cent confidence area) gives the predicted response for units where the given interaction term takes a value of zero.

Source: authors' estimates.

Figure 7: Heterogeneity in the effect of COVID-19 on agreement rates



Note: the plots illustrate results from regressions equivalent to those in Table 3(iii) with the pre-filtered outcome and cubic trend specification, extended to allow for heterogeneous trends along the dimension indicated in each sub-plot; pandemic weeks shown only (week zero = start of COVID-19); the blue line (and 95 per cent confidence area) gives predicted responses to the COVID-19 shock for units where the given interaction term takes a value of 1; the red line (and 95 per cent confidence area) gives the predicted response for units where the given interaction term takes a value of 0.

Source: authors' estimates.

6 Conclusion

This paper sought to contribute to the empirical literature on the economic effects of digital labour platforms, particularly their role in helping absorb (mitigate) economic shocks. Leveraging the disruption caused by the COVID-19 pandemic, we noted that despite evidence of a severe negative economic shock in general, the combination of a shift towards online platforms and changes in the composition of demand also created opportunities. We hypothesized that restrictions on formal activities or services offered at fixed locations may divert demand elsewhere, encouraging individuals to use new channels to search for workers. More time spent at home may have induced individuals to complete home improvement projects, raising demand for specific types of goods and services. Informal services may be inferior in nature and price-competitive, so shocks to income may raise demand in this market segment.

To test this we used data from the *Biscate* platform in Mozambique, a digital tool to facilitate the matching of manual freelancers to tasks, and analysed how components of supply and demand changed over the pandemic period. Running a wide range of different models at alternative levels of aggregation and filtering out pre-trends, we found highly consistent results. In line with our hypotheses, the pandemic shock played out in complex ways, with different mechanisms operating in different directions (at different times). On balance, the evidence is that platform work did help absorb the COVID-19 shock. As aggregate employment conditions declined and restrictions on businesses were imposed, demand for informal freelancers increased relative to the pre-pandemic trend, but, at the same time, the supply of new workers did not grow at a relatively faster rate during the pandemic. Consequently, the average worker was able to find more work via the platform than in the pre-pandemic period.

These findings connect to an emerging literature on the impacts of shocks on platform work. While some of the evidence here has highlighted the vulnerability of gig workers (Han and Hart 2021), others point to the ways in which digital platforms can enhance resilience (Raj et al. 2020). This not only includes expanding opportunities for clients and workers, but also allowing for greater flexibility in employment arrangements, such as when and where work will take place. Existing evidence has underlined the value of such flexibility to employees and firms (Chen et al. 2019; Hall and Krueger 2018); our own evidence suggests this is also the case on the labour demand side. Furthermore, our findings suggest that even in Mozambique, where most labour can already be described as 'precarious' by any reasonable definition, a net positive contribution of digital platforms in absorbing shocks is encountered.

None of this is to argue that digital labour platforms represent an off-the-shelf solution to deeper employment challenges, especially in countries such as Mozambique. The share of all workers using these platforms remains low, and demand in absolute terms is limited—for example, many registered workers never obtain any work via the *Biscate* platform. Also, even when they do, these jobs may be largely used as a secondary income source. We also make no claim that our analysis is representative of the informal sector in Mozambique. Workers registered on *Biscate* are predominantly urban, literate, have some professional experience, and have regular access to digital tools (at least a mobile phone). As such, they may have already been in a relatively privileged position compared to other workers in the informal sector.

Even so, some broader lessons can be drawn. Perhaps most obviously, disruption to usual market conditions can bring new opportunities in specific segments. Digital marketplaces such as *Biscate*, which facilitate labour matching and support price competition, can be useful vehicles through which more entrepreneurial or quick-to-adapt individuals can thrive. Clearly, preferences to use these tools have not been diminished by the COVID-19 shock. The absence of negative overall impacts in our data—both on aggregate and in specific segments (e.g. female-dominated professions)—also supports the view that informal work can be an important shock-absorber, especially where government assistance either to formal sector workers or affected communities has been minimal, as in Mozambique. In turn, we rec-

ommend that support to expand the scope and quality of digital marketplaces for informal (freelance) labour may be a relevant tool in the poverty-reduction toolbox, especially where they can be combined with products to actively enhance income smoothing.

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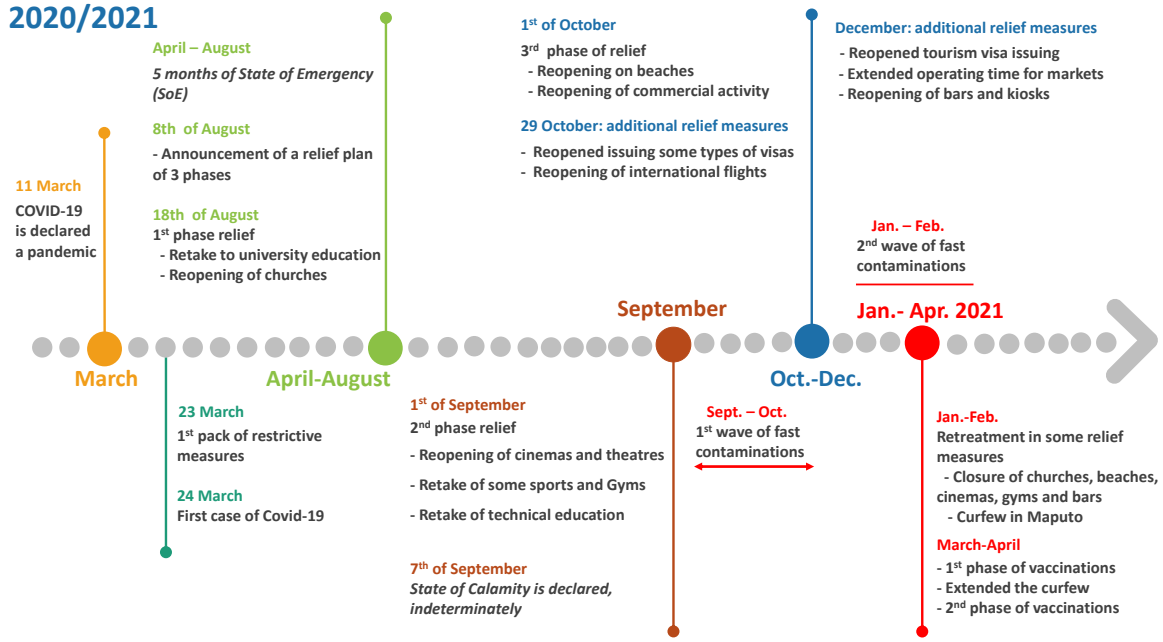
Appendix A

A1 Additional figures

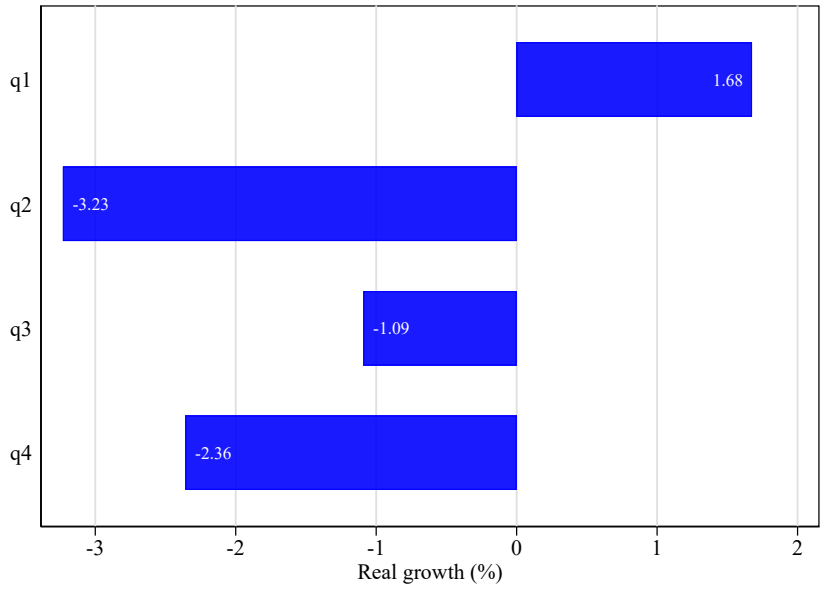
Figure A1: Narrative timeline of the evolution of the COVID-19 pandemic in Mozambique



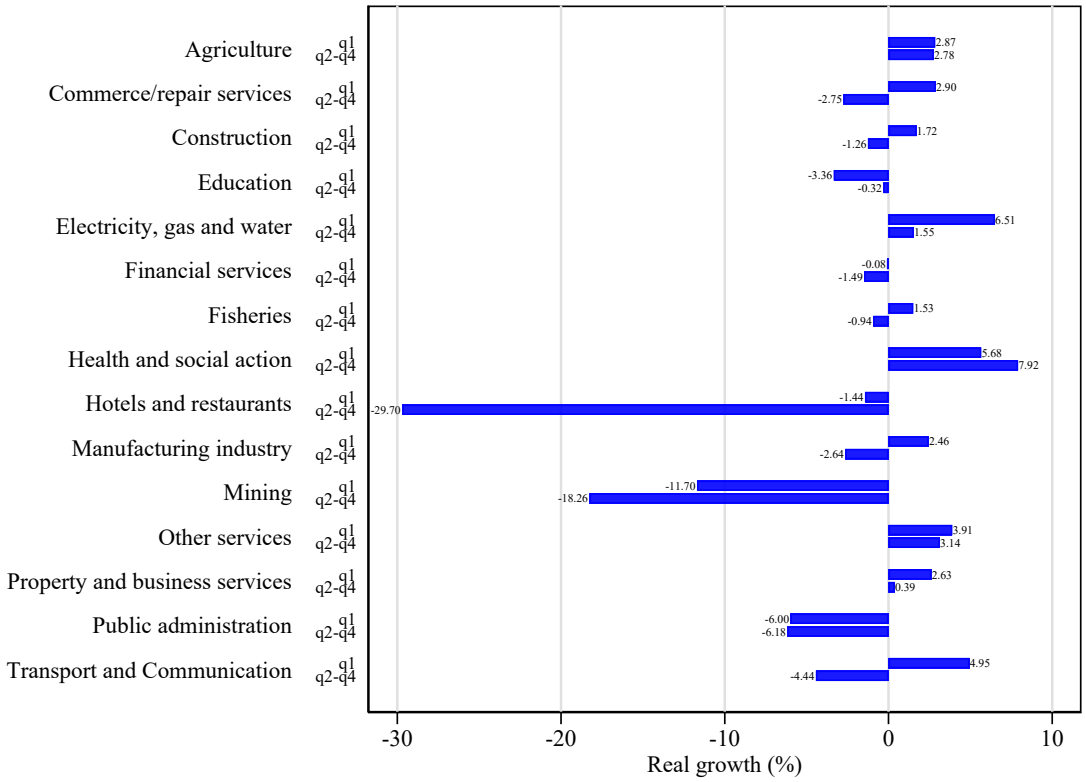
Source: authors' elaboration.

Figure A2: Quarterly real GDP growth in Mozambique, 2020

(a) Economy-wide growth:



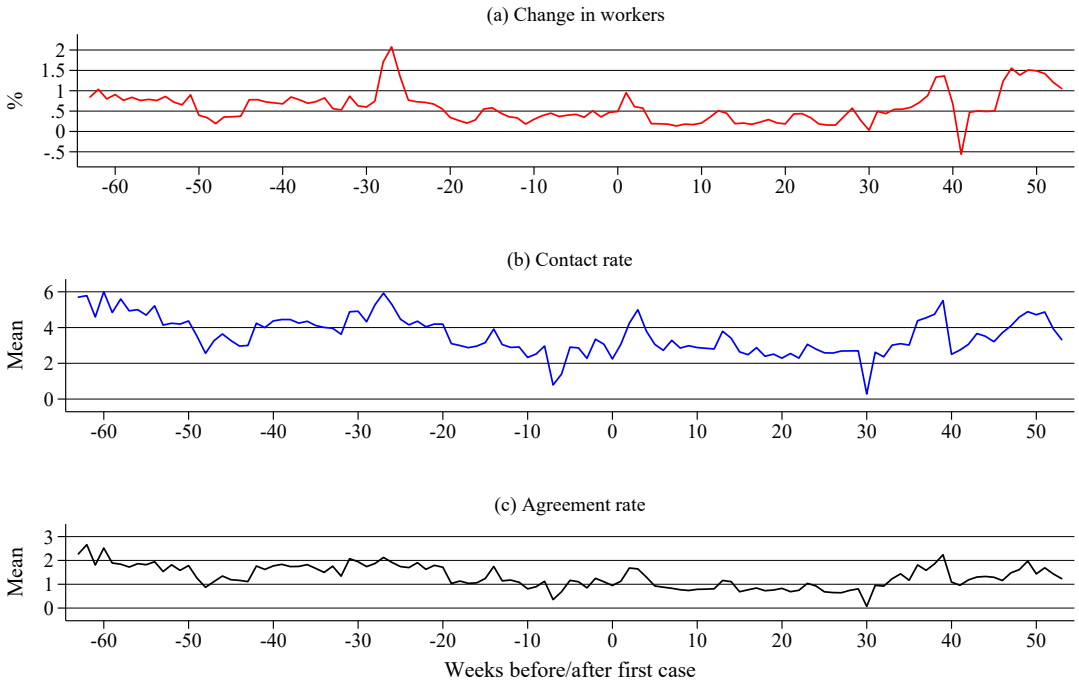
(b) Sectoral growth:



Note: all growth rates compare equivalent periods of 2020 versus 2019.

Source: authors' compilation using data provided by the National Statistics Institute (www.ine.gov.mz).

Figure A3: Time series of primary outcomes

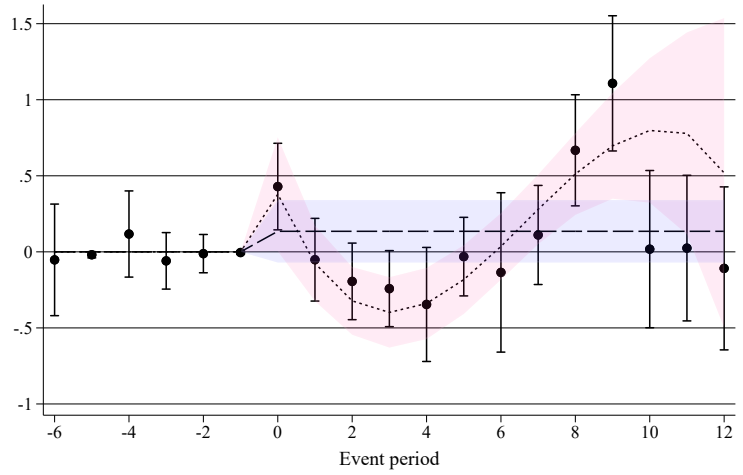


Note: the plots show aggregate (national-level) averages per period, indexed by periods before and after the first case in the country.

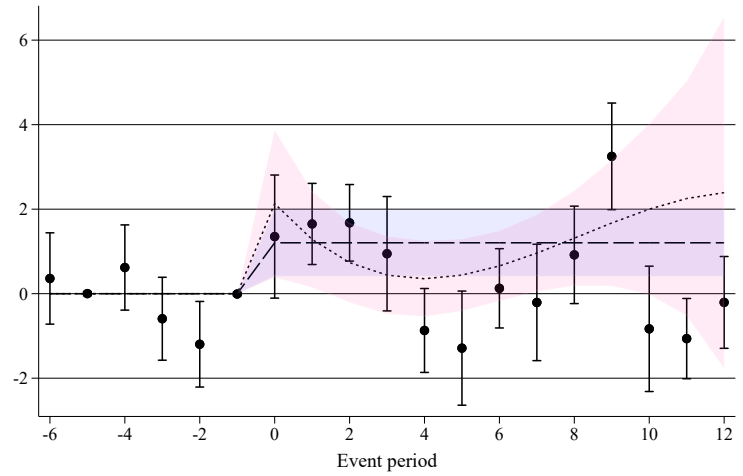
Source: authors' estimates.

Figure A4: Dynamics in raw (unfiltered) outcomes by event study periods

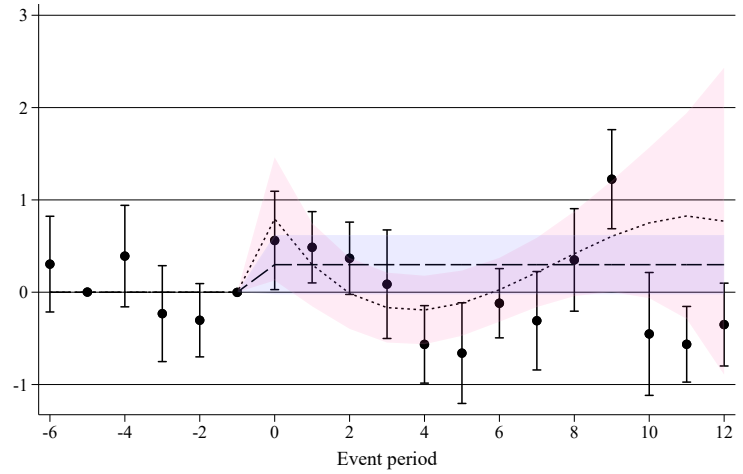
(i) *Change in registrations*:



(ii) *Contact rate*:



(iii) *Agreement rate*:

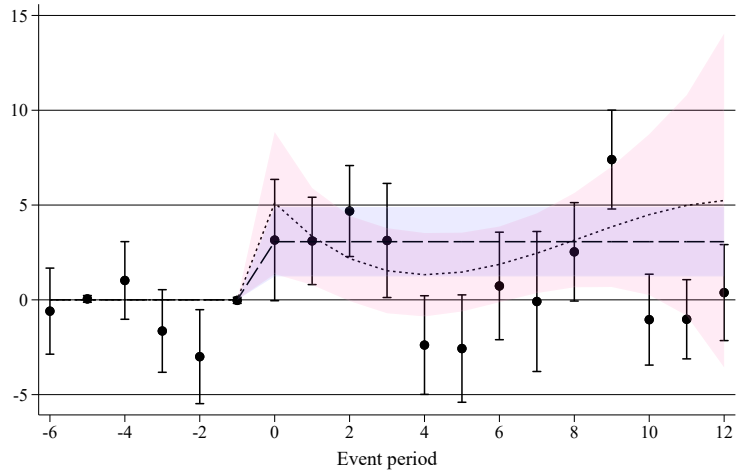


Note: the plots illustrate results from regressions underlying those summarized in Table 3, applying alternative formulations of f to different raw (not pre-filtered) outcomes; event periods are four-week blocks, where zero is the first block of the COVID-19 period; period-specific dots (and 95 per cent confidence intervals) are event study estimates; dashed line (with shaded blue 95 per cent confidence area) shows the dummy variable specification; and dotted line (with shaded pink 95 per cent confidence area) shows estimates from the cubic specification.

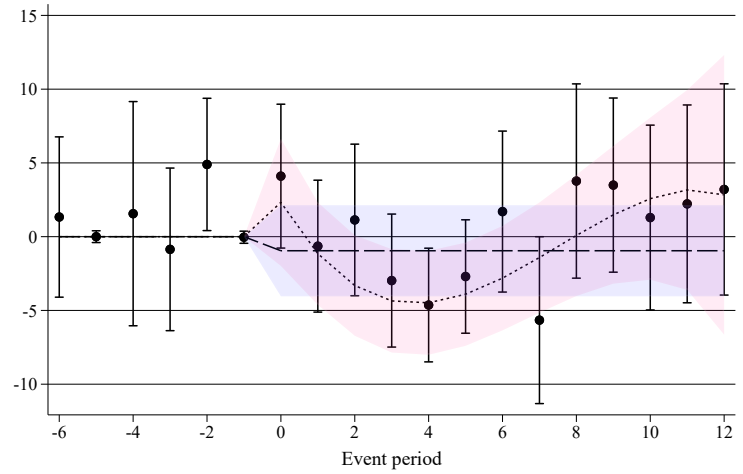
Source: authors' estimates.

Figure A5: Dynamics in additional pre-filtered outcomes by event study periods

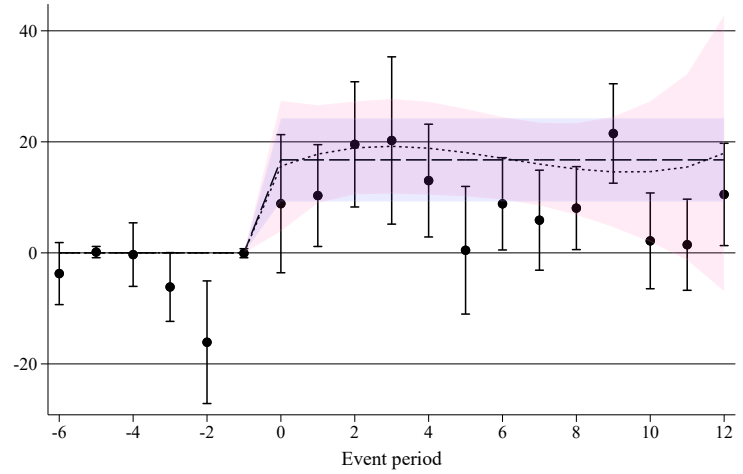
(i) *Demand index*:



(ii) *Supply response index*:



(iii) *Demand/supply balance*:

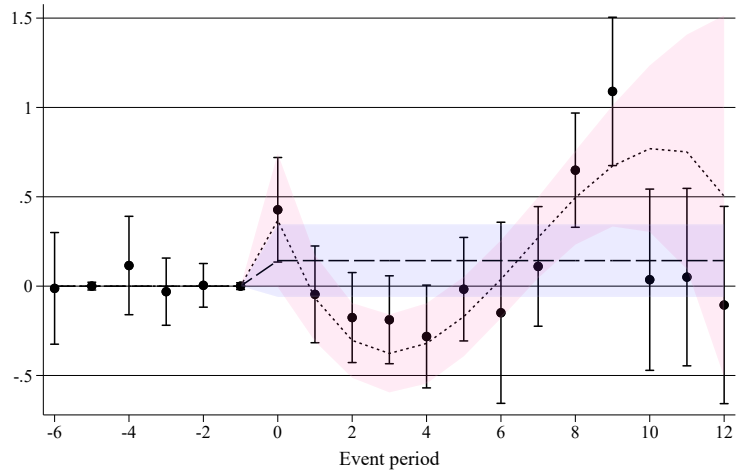


Note: the plots illustrate results from regressions underlying those summarized in Table 4, applying alternative formulations of f to different pre-filtered outcomes (in the panels); event periods are four-week blocks, where zero is the first block of the COVID-19 period; period-specific dots (and 95 per cent confidence intervals) are event study estimates; dashed line (with shaded blue 95 per cent confidence area) shows the dummy variable specification; and dotted line (with shaded pink 95 per cent confidence area) shows estimates from the cubic specification.

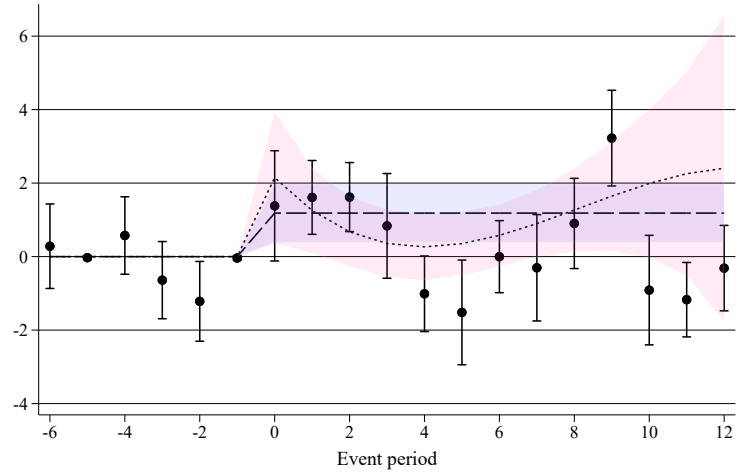
Source: authors' estimates.

Figure A6: Dynamics in pre-filtered outcomes by event study periods, data aggregated by professions

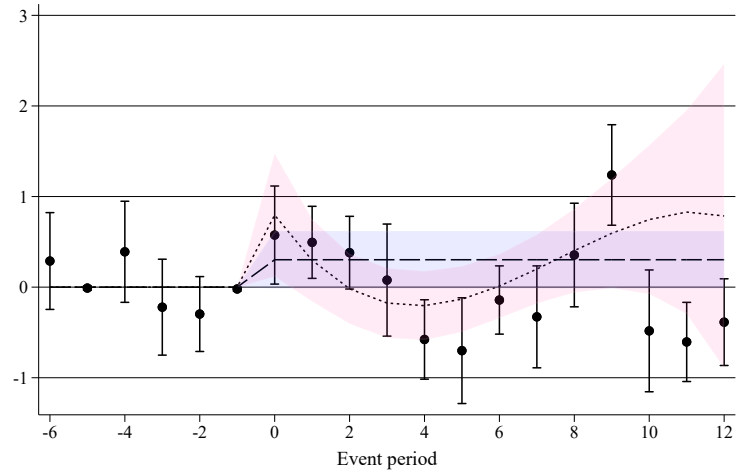
(i) *Change in registrations*:



(ii) *Contact rate*:



(iii) *Agreement rate*:

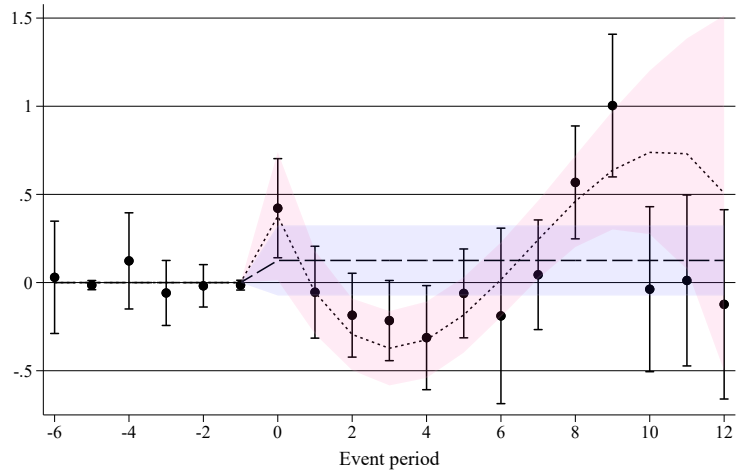


Note: the plots replicate those of Figure 3, but with data aggregated to professions.

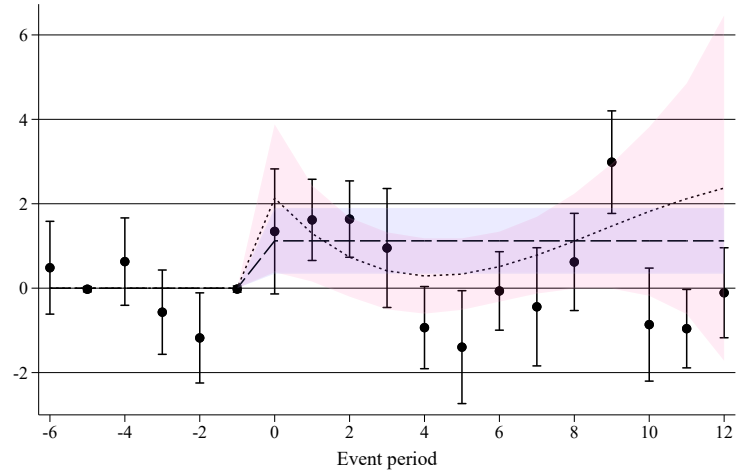
Source: authors' estimates.

Figure A7: Dynamics in pre-filtered outcomes by event study periods, data aggregated by provinces

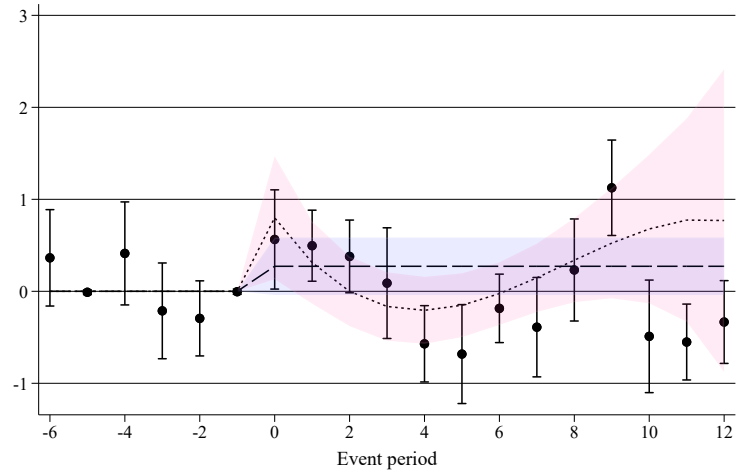
(i) *Change in registrations*:



(ii) *Contact rate*:



(iii) *Agreement rate*:

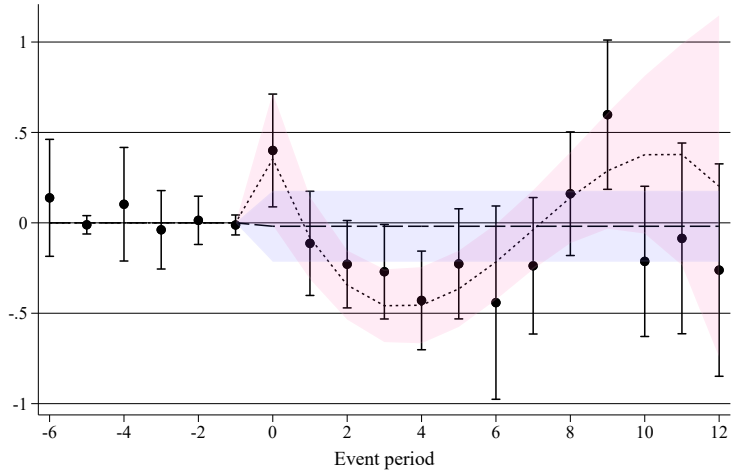


Note: the plots replicate those of Figure 3, but with data aggregated to provinces.

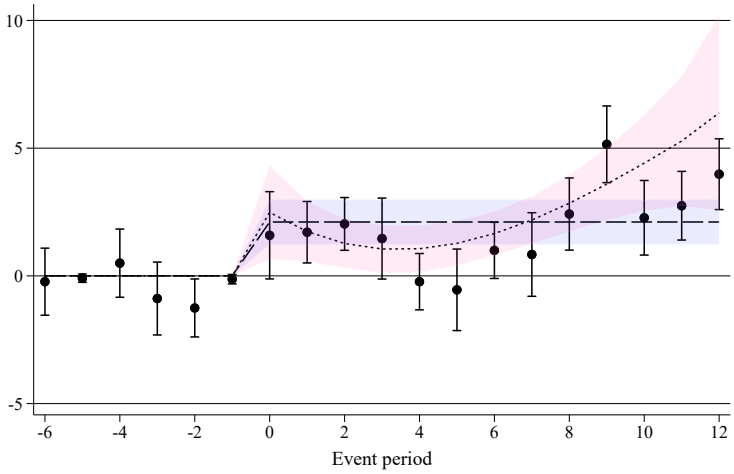
Source: authors' estimates.

Figure A8: Dynamics in pre-filtered outcomes by event study periods, data aggregated to the national level

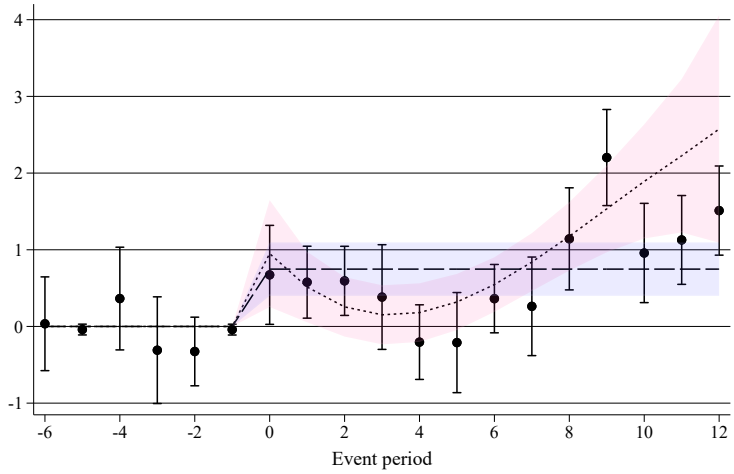
(i) *Change in registrations*:



(ii) *Contact rate*:



(iii) *Agreement rate*:



Note: the plots replicate those of Figure 3, but with data aggregated to the national level.

Source: authors' estimates.

A2 Additional tables

Table A1: Classification of professions on the *Biscate* platform

Portuguese	English	Industry	Services	In-person
Cabeleireiro	Hairdressing	0	1	1
Canalização	Plumbing	1	0	0
Carpintaria	Carpentry	1	0	0
Construção e Reparação	Construction/repair	1	0	0
Costura	Sewing	0	1	1
Cozinha	Cooking	0	1	0
Electricidade	Electrician	1	0	0
Entregas	Delivery	0	1	0
Estética	Beauty	0	1	1
Instalação de TV	TV installation	1	0	0
Jardinagem	Gardening	1	0	0
Mecânica	Mechanic	1	0	0
Pintura	Painting	1	0	0
Reboque	Towing	1	0	0
Reparação de AC	AC repair	1	0	0
Serralharia	Metalwork	1	0	0

Note: classifications, as indicated in the last three columns, are our own.
Source: authors' compilation.

Table A2: Average province-level outcomes, by region and profession in 2019

	Registered workers ('000s)				Agreement rate (per 100)			
	N	C	S	All	N	C	S	All
AC repair	0.41	0.28	0.46	1.14	24.94	16.09	48.94	28.27
Beauty	0.04	0.04	0.08	0.16	20.22	13.78	27.83	19.95
Carpentry	0.63	0.38	0.80	1.80	137.97	74.95	269.12	150.82
Construction/repair	1.48	0.95	1.59	4.02	144.72	92.94	301.30	168.60
Cooking	1.36	0.86	1.82	4.05	477.48	292.04	908.60	527.62
Delivery	0.72	0.39	0.69	1.81	149.31	100.25	221.71	151.21
Electrician	2.25	1.85	3.25	7.35	271.57	206.52	758.90	380.82
Gardening	0.18	0.07	0.14	0.40	31.10	18.13	39.96	28.80
Hairdressing	1.02	0.53	1.55	3.10	312.93	156.79	545.59	319.60
Manicure	0.04	0.02	0.03	0.09	17.17	8.98	24.82	15.70
Mechanic	0.56	0.49	0.74	1.78	49.15	51.55	132.08	72.64
Metalwork	0.55	0.33	0.37	1.26	24.45	13.44	85.24	37.02
Painting	0.70	0.34	0.48	1.53	38.50	24.86	46.03	35.59
Plumbing	0.78	0.43	0.92	2.13	215.17	79.49	350.75	202.81
Sewing	0.31	0.23	0.40	0.93	115.30	54.17	178.63	110.35
TV installation	0.35	0.34	0.56	1.26	46.17	45.53	127.03	67.99
Towing	0.03	0.04	0.11	0.18	16.77	16.57	38.95	22.85
Upholstery	0.04	0.02	0.03	0.09	10.46	12.37	16.53	12.64
All	11.45	7.60	14.02	33.07	118.31	72.21	237.01	133.38

Note: cells report the total number of registered workers (in thousands) in a given profession and region at the end of 2019, and the mean weekly task agreement rate (per 100 workers) for the full year 2019; N, C, and S refer to northern, central, and southern regions, respectively.

Source: authors' estimates.

Table A3: Direct effect of COVID-19 on core outcomes, data aggregated by combinations of provinces and professions, including contemporaneous compositional controls

Transform →	Raw				Pre-filtered			
f →	Dummy	Cubic	Event	Proxy	Dummy	Cubic	Event	Proxy
(i) *Change in registrations*:								
COVID-19 (mean)	0.26***	0.51***	0.32		0.52***	0.88***	0.49	
	(0.08)	(0.18)	(0.36)		(0.11)	(0.18)	(0.40)	
New cases (roll av.)				0.05*				0.31***
				(0.03)				(0.04)
Stringency index				–0.34				–0.51
				(0.26)				(0.33)
Mobility index				–1.34**				–1.71**
				(0.60)				(0.80)
Employment index				–0.72**				–0.41
				(0.29)				(0.33)
Constant	1.09***	1.18***	1.02***	1.21***	0.40**	0.52***	0.17	0.71***
	(0.10)	(0.10)	(0.12)	(0.13)	(0.16)	(0.12)	(0.14)	(0.17)
R^2 (adj.)	0.19	0.20	0.20	0.19	0.40	0.42	0.42	0.41
$\rho(f_2, f_4)$				0.72				0.51
COVID-19 resid.				0.51				0.82
(ii) *Contact rate*:								
COVID-19 (mean)	1.89***	2.59***	1.29**		1.91***	2.38***	0.98	
	(0.41)	(0.73)	(0.64)		(0.43)	(0.72)	(0.68)	
New cases (roll av.)				–0.08				0.21***
				(0.07)				(0.07)
Stringency index				1.75*				1.13
				(0.89)				(0.93)
Mobility index				–3.61*				–3.33*
				(1.87)				(2.00)
Employment index				–1.94***				–2.19***
				(0.66)				(0.70)
Constant	4.48***	4.72***	4.38***	4.65***	3.98***	4.15***	3.72***	4.39***
	(0.36)	(0.36)	(0.38)	(0.41)	(0.42)	(0.39)	(0.41)	(0.44)
R^2 (adj.)	0.51	0.51	0.52	0.51	0.30	0.31	0.32	0.30
$\rho(f_2, f_4)$				0.47				0.54
COVID-19 resid.				0.41				0.55
(iii) *Agreement rate*:								
COVID-19 (mean)	0.58***	0.90***	0.38		0.67***	1.01***	0.44	
	(0.16)	(0.28)	(0.42)		(0.17)	(0.28)	(0.45)	
New cases (roll av.)				0.01				0.15***
				(0.03)				(0.03)
Stringency index				0.29				0.04
				(0.38)				(0.41)
Mobility index				–1.42*				–1.71*
				(0.83)				(0.92)
Employment index				–0.51**				–0.44
				(0.26)				(0.28)
Constant	1.72***	1.83***	1.58***	1.80***	1.48***	1.61***	1.32***	1.68***
	(0.15)	(0.14)	(0.17)	(0.16)	(0.17)	(0.16)	(0.18)	(0.19)
R^2 (adj.)	0.47	0.48	0.49	0.47	0.33	0.34	0.35	0.33
$\rho(f_2, f_4)$				0.61				0.66
COVID-19 resid.				0.20				0.14

Note: significance: *** 1, ** 5, * 10 percent. This table replicates Table 3, adding a vector of contemporaneous controls (e.g. share female) at the market level in both the pre-filter and main regressions.

Source: authors' estimates.

Table A4: Direct effect of COVID-19 on additional outcomes, data aggregated by combinations of provinces and professions, including contemporaneous compositional controls

Transform →	Raw				Pre-filtered			
f →	Dummy	Cubic	Event	Proxy	Dummy	Cubic	Event	Proxy
(i) *Demand index*:								
COVID-19 (mean)	4.98***	6.40***	3.24***		4.80***	5.90***	2.36**	
	(1.04)	(1.76)	(1.03)		(1.06)	(1.83)	(1.07)	
New cases (roll av.)				0.11				0.83***
				(0.27)				(0.31)
Stringency index				4.13*				2.46
				(2.16)				(2.28)
Mobility index				–9.77*				–8.05
				(4.99)				(5.18)
Employment index				–5.15***				–5.45***
				(1.72)				(1.80)
Constant	10.88***	11.35***	10.87***	11.56***	10.06***	10.45***	9.38***	11.30***
	(0.96)	(0.96)	(0.92)	(1.10)	(1.01)	(0.99)	(0.96)	(1.12)
R^2 (adj.)	0.25	0.25	0.27	0.25	0.26	0.26	0.28	0.26
$\rho(f_2, f_4)$				0.60				0.66
COVID-19 resid.				0.24				–1.23
(ii) *Supply response index*:								
COVID-19 (mean)	–0.97	–0.18	0.64		0.28	2.60	3.28**	
	(1.74)	(2.63)	(1.45)		(1.72)	(2.62)	(1.44)	
New cases (roll av.)				0.04				0.64
				(0.45)				(0.45)
Stringency index				–6.32*				–5.95*
				(3.41)				(3.44)
Mobility index				–8.72				–13.70
				(8.19)				(8.45)
Employment index				1.22				4.91
				(3.56)				(3.64)
Constant	40.26***	40.75***	39.38***	40.47***	39.66***	40.53***	39.28***	40.18***
	(1.06)	(1.10)	(2.06)	(1.14)	(1.03)	(1.08)	(2.06)	(1.10)
R^2 (adj.)	0.01	0.01	0.02	0.01	0.14	0.14	0.14	0.14
$\rho(f_2, f_4)$				0.71				0.71
COVID-19 resid.				0.48				0.28
(iii) *Demand/supply balance*:								
COVID-19 (mean)	20.12***	22.43***	12.94***		21.34***	24.03***	12.84***	
	(4.33)	(6.05)	(1.97)		(4.68)	(6.35)	(2.04)	
New cases (roll av.)				–0.24				1.25
				(1.22)				(1.28)
Stringency index				30.69***				31.12***
				(7.74)				(8.18)
Mobility index				–8.84				–3.59
				(15.86)				(16.17)
Employment index				–22.88***				–21.82***
				(5.79)				(6.16)
Constant	54.13***	54.27***	56.74***	55.59***	52.34***	52.40***	54.44***	54.58***
	(2.57)	(2.57)	(2.96)	(2.74)	(2.72)	(2.59)	(3.01)	(2.78)
R^2 (adj.)	0.09	0.09	0.09	0.09	0.15	0.15	0.16	0.15
$\rho(f_2, f_4)$				0.54				0.59
COVID-19 resid.				0.09				–0.73

Note: significance: *** 1, ** 5, * 10 per cent. This table replicates Table 4, adding a vector of contemporaneous controls (e.g. share female) at the market level in both the pre-filter and main regressions.

Source: authors' estimates.

Table A5: Net effect of COVID-19 on core outcomes, data aggregated by professions

Transform →	Raw				Pre-filtered			
f →	Dummy	Cubic	Event	Proxy	Dummy	Cubic	Event	Proxy
(i) *Change in registrations*:								
COVID-19 (mean)	0.14	0.21	0.11		–0.11	–0.14	–0.22	
	(0.10)	(0.20)	(0.32)		(0.10)	(0.18)	(0.31)	
New cases (roll av.)				0.05				–0.02
				(0.05)				(0.05)
Stringency index				–1.00**				–0.86**
				(0.49)				(0.43)
Mobility index				–1.88*				–1.61*
				(0.96)				(0.82)
Employment index				–0.51				–0.77**
				(0.37)				(0.33)
Constant	0.65***	0.67***	0.65***	0.73***	0.69***	0.71***	0.62***	0.76***
	(0.06)	(0.06)	(0.10)	(0.07)	(0.06)	(0.05)	(0.09)	(0.06)
R^2 (adj.)	0.43	0.49	0.51	0.44	0.31	0.36	0.38	0.33
$\rho(f_2, f_4)$				0.75				0.55
COVID-19 resid.				0.52				1.24
(ii) *Contact rate*:								
COVID-19 (mean)	1.18***	1.28	0.33		1.95***	2.59***	1.69**	
	(0.40)	(0.83)	(0.64)		(0.42)	(0.77)	(0.65)	
New cases (roll av.)				–0.26*				–0.01
				(0.15)				(0.16)
Stringency index				2.23				1.56
				(1.60)				(1.54)
Mobility index				–2.83				–3.35
				(3.39)				(3.21)
Employment index				–1.16				–1.82**
				(0.93)				(0.81)
Constant	3.59***	3.64***	3.65***	3.71***	4.34***	4.39***	4.69***	4.50***
	(0.19)	(0.19)	(0.36)	(0.19)	(0.19)	(0.20)	(0.37)	(0.19)
R^2 (adj.)	0.72	0.72	0.76	0.72	0.50	0.52	0.57	0.50
$\rho(f_2, f_4)$				–0.08				0.60
COVID-19 resid.				0.37				–0.47
(iii) *Agreement rate*:								
COVID-19 (mean)	0.30*	0.33	–0.01		0.69***	0.98***	0.65	
	(0.16)	(0.34)	(0.42)		(0.17)	(0.31)	(0.43)	
New cases (roll av.)				–0.11*				0.01
				(0.06)				(0.07)
Stringency index				0.54				0.24
				(0.67)				(0.66)
Mobility index				–1.49				–1.78
				(1.38)				(1.33)
Employment index				–0.19				–0.34
				(0.38)				(0.33)
Constant	1.38***	1.40***	1.28***	1.43***	1.67***	1.70***	1.71***	1.74***
	(0.08)	(0.08)	(0.16)	(0.08)	(0.07)	(0.08)	(0.16)	(0.08)
R^2 (adj.)	0.69	0.71	0.74	0.69	0.51	0.55	0.59	0.51
$\rho(f_2, f_4)$				0.03				0.72
COVID-19 resid.				0.49				–0.27

Note: significance: *** 1, ** 5, * 10 per cent. Columns and panels summarize separate regressions on the form of equation (4), with N = 2,700; panels (i)–(iii) refer to distinct outcomes; different columns refer to combinations of alternative outcome transforms and choices for f; all models include a full set of unit fixed effects, unit by month and unit by year effects, and time-varying controls for provincial temperature and precipitation deviations; 'COVID-19' is the simple average of estimated time effects in the COVID-19 period only; market share weights (based on worker counts) applied throughout; standard errors in parentheses, clustered by time (unique weeks).

Source: authors' estimates.

Table A6: Net effect of COVID-19 on core outcomes, data aggregated by provinces

Transform →	Raw				Pre-filtered			
f →	Dummy	Cubic	Event	Proxy	Dummy	Cubic	Event	Proxy
(i) *Change in registrations*:								
COVID-19 (mean)	0.13	0.20	0.07		–0.05	–0.04	–0.16	
	(0.10)	(0.19)	(0.31)		(0.10)	(0.18)	(0.30)	
New cases (roll av.)				0.01				–0.01
				(0.05)				(0.04)
Stringency index				–0.72*				–1.05***
				(0.41)				(0.38)
Mobility index				–1.60**				–1.51**
				(0.73)				(0.68)
Employment index				–0.96				–1.60**
				(0.83)				(0.77)
Constant	0.64***	0.66***	0.63***	0.73***	0.70***	0.73***	0.64***	0.82***
	(0.05)	(0.05)	(0.10)	(0.07)	(0.05)	(0.05)	(0.09)	(0.07)
R^2 (adj.)	0.26	0.31	0.33	0.27	0.25	0.29	0.30	0.27
$\rho(f_2, f_4)$				0.64				0.61
COVID-19 resid.				0.45				0.22
(ii) *Contact rate*:								
COVID-19 (mean)	1.12***	1.23	0.34		2.11***	2.86***	1.98***	
	(0.40)	(0.82)	(0.61)		(0.44)	(0.77)	(0.63)	
New cases (roll av.)				–0.29**				–0.16
				(0.13)				(0.12)
Stringency index				0.93				2.01*
				(1.12)				(1.02)
Mobility index				–3.21				–4.58**
				(2.26)				(2.05)
Employment index				–2.46				–1.83
				(1.88)				(1.67)
Constant	3.59***	3.65***	3.57***	3.75***	4.38***	4.45***	4.75***	4.53***
	(0.18)	(0.18)	(0.34)	(0.17)	(0.18)	(0.19)	(0.35)	(0.18)
R^2 (adj.)	0.58	0.59	0.66	0.59	0.59	0.61	0.66	0.59
$\rho(f_2, f_4)$				0.20				0.42
COVID-19 resid.				0.32				0.31
(iii) *Agreement rate*:								
COVID-19 (mean)	0.27*	0.31	–0.02		0.73***	1.05***	0.71*	
	(0.16)	(0.33)	(0.40)		(0.17)	(0.31)	(0.42)	
New cases (roll av.)				–0.09				–0.02
				(0.06)				(0.05)
Stringency index				–0.04				0.41
				(0.47)				(0.44)
Mobility index				–1.27				–1.99**
				(1.00)				(0.94)
Employment index				–0.76				–0.39
				(0.77)				(0.69)
Constant	1.36***	1.40***	1.25***	1.44***	1.68***	1.71***	1.73***	1.75***
	(0.07)	(0.07)	(0.15)	(0.07)	(0.07)	(0.07)	(0.16)	(0.07)
R^2 (adj.)	0.60	0.62	0.68	0.60	0.60	0.64	0.68	0.60
$\rho(f_2, f_4)$				0.33				0.56
COVID-19 resid.				0.19				–0.12

Note: significance: *** 1, ** 5, * 10 per cent. Columns and panels summarize separate regressions on the form of equation (4), with N = 1,650; panels (i)–(iii) refer to distinct outcomes; different columns refer to combinations of alternative outcome transforms and choices for f; all models include a full set of unit fixed effects, unit by month and unit by year effects, and time-varying controls for provincial temperature and precipitation deviations; 'COVID-19' is the simple average of estimated time effects in the COVID-19 period only; market share weights (based on worker counts) applied throughout; standard errors in parentheses, clustered by time (unique weeks).

Source: authors' estimates.

Table A7: Net effect of COVID-19 on core outcomes, data aggregated to the national level

Transform →	Raw				Pre-filtered			
f →	Dummy	Cubic	Event	Proxy	Dummy	Cubic	Event	Proxy
(i) *Change in registrations*:								
COVID-19 (mean)	0.13	0.20	0.09		–0.02	–0.01	–0.10	
	(0.10)	(0.20)	(0.33)		(0.10)	(0.18)	(0.32)	
New cases (roll av.)				0.07				0.03
				(0.06)				(0.06)
Stringency index				–1.38*				–1.38*
				(0.76)				(0.70)
Mobility index				–2.05*				–1.90*
				(1.15)				(1.02)
Employment index				–1.42				–1.78*
				(1.07)				(0.99)
Constant	0.65***	0.67***	0.63***	0.77***	0.69***	0.71***	0.63***	0.81***
	(0.06)	(0.06)	(0.10)	(0.08)	(0.06)	(0.05)	(0.10)	(0.07)
R^2 (adj.)	0.42	0.52	0.53	0.44	0.29	0.40	0.40	0.34
$\rho(f_2, f_4)$				0.76				0.69
COVID-19 resid.				0.29				0.28
(ii) *Contact rate*:								
COVID-19 (mean)	1.10***	1.19	0.20		2.11***	2.85***	1.88***	
	(0.40)	(0.84)	(0.68)		(0.45)	(0.80)	(0.71)	
New cases (roll av.)				–0.28				0.07
				(0.17)				(0.18)
Stringency index				2.24				1.01
				(2.09)				(1.97)
Mobility index				–2.69				–3.76
				(3.64)				(3.53)
Employment index				–1.47				–2.48
				(2.46)				(2.23)
Constant	3.61***	3.66***	3.59***	3.73***	4.33***	4.39***	4.70***	4.53***
	(0.19)	(0.20)	(0.40)	(0.19)	(0.19)	(0.20)	(0.41)	(0.20)
R^2 (adj.)	0.46	0.48	0.62	0.46	0.71	0.74	0.80	0.70
$\rho(f_2, f_4)$				0.01				0.78
COVID-19 resid.				0.31				0.31
(iii) *Agreement rate*:								
COVID-19 (mean)	0.28*	0.30	–0.05		0.75***	1.07***	0.72	
	(0.16)	(0.34)	(0.45)		(0.18)	(0.32)	(0.47)	
New cases (roll av.)				–0.12				0.04
				(0.07)				(0.08)
Stringency index				0.57				0.04
				(0.88)				(0.85)
Mobility index				–1.43				–1.93
				(1.49)				(1.48)
Employment index				–0.28				–0.61
				(1.00)				(0.93)
Constant	1.38***	1.41***	1.25***	1.44***	1.66***	1.69***	1.71***	1.75***
	(0.08)	(0.08)	(0.18)	(0.08)	(0.08)	(0.08)	(0.18)	(0.08)
R^2 (adj.)	0.48	0.52	0.63	0.48	0.69	0.74	0.79	0.68
$\rho(f_2, f_4)$				0.05				0.80
COVID-19 resid.				0.34				0.34

Note: significance: *** 1, ** 5, * 10 per cent. Columns and panels summarize separate regressions on the form of equation (4), with N = 150; panels (i)–(iii) refer to distinct outcomes; different columns refer to combinations of alternative outcome transforms and choices for f; all models include a full set of unit fixed effects, unit by month and unit by year effects, and time-varying controls for provincial temperature and precipitation deviations; 'COVID-19' is the simple average of estimated time effects in the COVID-19 period only; market share weights (based on worker counts) applied throughout; standard errors in parentheses, clustered by time (unique weeks).

Source: authors' estimates.