"Working" Remotely?
Selection, Treatment, and the Market Provision of Remote Work
Natalia Emanuel · Emma Harrington
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Abstract

Why was remote work so rare prior to Covid-19’s lockdown? One possibility is that working remotely reduces productivity. Another is that remote work attracts unobservably less productive workers. In our setting of call-center workers at a Fortune 500 retailer, two natural experiments reveal positive productivity effects of remote work. When Covid-19 closed down the retailer’s on-site call-centers, a difference-in-difference design suggests the transition from on-site to remote work increased the productivity of formerly on-site workers by 8% to 10% relative to their already remote peers. Similarly, when previously on-site workers took up opportunities to go remote in 2018-2019, their productivity rose by 7%. These two natural experiments also reveal negative selection into remote work. While all workers were remote due to Covid-19, those who were hired into remote jobs were 12% less productive than those hired into on-site jobs. Extending remote opportunities to on-site workers similarly attracted less productive workers to on-site jobs. Our model allows us to characterize the counterfactual in which remote workers were not adversely selected. Without adverse selection, the retailer would have hired 57% more remote workers and worker surplus from remote work would have been 32% greater. Given the central role of selection, Covid-19’s effect on remote work will persist if the lockdown disproportionately causes more productive workers to be willing to work remotely.

1Contact: Harvard University, 1805 Cambridge Street, Cambridge, MA 02138, eharrington@g.harvard.edu.
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Prior to Covid-19, 11% of Americans worked remotely. By now, many who could work remotely have done so (Brynjolfsson et al., 2020). Further, 56% of workers who are now remote report wanting to stay remote after offices reopen (PwC, 2020; Morning Consult, 2020). So, why was remote work so rare prior to Covid-19 and what can we expect once the pandemic subsides?

One possibility is that working remotely reduces productivity. Another possibility is that remote work attracts unobservably less productive workers.

We assess these possibilities in the context of call-center workers at a Fortune 500 online retailer. Prior to the pandemic, the retailer hired call-center workers into both remote and on-site jobs. Calls are routed randomly between remote and on-site workers, who are all paid by the hour rather than by the call. Workers operate independently and their productivity is tracked electronically. Thus, concerns about remote work’s impact on teamwork and monitoring are less relevant in our setting.

Two natural experiments reveal positive productivity effects of remote work in our context. In 2018 and 2019, the retailer posted opportunities for office workers to go remote to relieve office crowding and build out the retailer’s remote workforce. When pre-existing office workers went remote, their productivity rose by 7%. Similarly, after the retailer closed its offices on April 6, 2020 due to Covid-19, the hourly calls of formerly on-site workers rose by 8 to 10% relative to their already remote peers.

These same natural experiments suggest remote work attracts workers who are unobservably less productive. When all workers were working remotely due to the lock-down, those who were hired to be remote were 12% less productive than those who were initially on-site. This productivity difference grows even wider when we control for observable (although legally inadmissible) worker characteristics of age, gender, and child-care responsibilities. While one might think child-care responsibilities are the dimension along which adverse selection operates, we find that child-care responsibilities are positively correlated with worker pro-

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2In the American Time-Use Survey between 2013 and 2017, 20.5% of workers reported spending some time working from home and 11.4% reported spending the entire day working remotely that day (Brynjolfsson et al., 2020). By contrast, in the 2018 American Community Survey (ACS), only 5.3% of workers report working from home (based on the authors’ calculations). In Brynjolfsson et al. (2020)’s survey of workers after Covid-19, 15% report having worked remotely prior to the pandemic.

3In two recent surveys of 2,207 workers now working remotely (PwC, 2020; Morning Consult, 2020), 32% report wanting to remain fully remote after the pandemic and 56% report wanting to work more than half their week remotely. Revealed preference also suggests a latent demand for remote work among the Chinese travel agents studied by Bloom et al. (2015), about half of whom opt into a remote work experiment.

4This is analogous to the setting in Bloom et al. (2015)’s experiment in a Chinese travel agency.

5These findings complement and extend those of (Bloom et al., 2015), who find remote work increased call-center workers’ productivity by 13% among those who opted into remote work in a Chinese travel agency.

6Child-care responsibilities are reported in a June 2020 survey that the retailer conducted to understand the constraints facing their workers during the pandemic.
ductivity among both remote and on-site workers.

Similarly, using the introduction and phase-out of opportunities to go remote for on-site workers in 2018 and 2019, we find opportunities to go remote attract less productive applicants. This suggests firms cannot easily side-step adverse selection by reserving remote work for workers who start on-site.

To understand how these forces shape the market equilibrium before and after Covid-19, we adapt a classic selection model to a setting where workers either choose to be remote or choose to work in an office. In our model, a worker’s type determines both her preference for remote work and her productivity conditional on her observables. Adverse selection into remote work incentivizes marginal workers to go to the office to pool with more productive workers even if they would be more productive working remotely. This distortion leads to deadweight losses from the under-provision of remote work, the magnitude of which depends on the extent of adverse selection and workers’ demand for remote work.

As consistent with our model, the retailer pays remote workers 6% less than on-site workers to offset their lower productivity. Given this wage differential, less than a fifth of the retailer’s workforce was remote prior to the pandemic, which is representative of call-center work more broadly (Mas and Pallais, 2017).

The market equilibrium does not reflect the efficient provision of remote work because of the problem of adverse selection. To quantify the magnitude of the deadweight loss, we estimate the demand for remote work. To do so, we leverage the fact that in some places workers must accept larger pay cuts to accept the retailer’s uniform $14/hour remote wage instead of a local on-site option. The resulting variation in the implicit price of remote work allows us to trace out workers’ demand for remote work. In the market equilibrium, the average worker must accept a $1/hour lower wage to take the retailer’s remote job rather than a local on-site alternative. Workers still gain an average surplus of 11% of their wages from remote work, echoing the substantial surplus from flexibility in other alternative work arrangements (Chen et al., 2019).

Without adverse selection, the share of remote workers at the retailer would rise from 19% to 29% and total social surplus from remote work would rise by 32% since new recruits would either (a) prefer working remotely or (b) not dislike it strongly enough to offset its positive effects on their productivity.

These deadweight losses from the under-provision of remote work may not persist once the pandemic subsides. Our analysis suggests that a key determinant of the future of remote work...
work will be whether more productive workers learn that they like to work remotely during the lock-down. If more productive workers disproportionately change their minds about the desirability of remote work, this will attenuate adverse selection, thereby ameliorating the market failure and permanently increasing the provision of remote work in the post-pandemic world. These changes would be more marked than what would be expected in labor markets with no informational asymmetries.

Our analysis of the ways in which the pandemic can permanently affect the provision of remote work contributes to the growing literature on the pandemic’s immediate labor-market effects and their potential persistence beyond the lock-down (e.g., Bartik et al., 2020a,b; Cortes and Forsythe, 2020; Forsythe et al., 2020; Gallant et al., 2020; Stevenson, 2020).

Our paper also makes a number of contributions to the nascent literature on remote work. We corroborate existing estimates of a positive treatment effect of remote work for call-center workers (Bloom et al., 2015) and show that they extend to workers who do not voluntarily choose remote work. To our knowledge, we are also the first to compare the productivity of those hired directly into remote and on-site jobs, giving us a more direct test of selection into remote work (Linos, 2018). Finally, we develop a framework to integrate our estimates of the treatment and selection effect in an analysis of the provision of remote work and the distortions caused by adverse selection.

Our work may also speak to the broader question of why flexible work remains relatively rare among low-wage workers outside of the gig economy, despite some workers’ strong preferences for these arrangements (Mas and Pallais, 2017; Maestas et al., 2018; Adams-Prassl et al., 2020). Even in settings where flexibility does not compromise productivity on the margin, it may still reduce productivity on average because of selection into flexible jobs. If those who value flexibility are less productive conditional on their observables, the resulting adverse selection will cause work to be less flexible than optimal.

We also contribute to the literature on the potential for preferences for workplace amenities to be a signal of ability and thus distort the market provision of these amenities. Tô (2018) finds evidence that taking parental leave is a negative signal about a worker’s subsequent productivity (Goldin et al., 2020). Conversely, Jones et al. (2019) find that workplace wellness programs do not improve health but those who select into them tend to be much health-

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8While remote policies vary across firms within occupations, this variation does not normally exist within the bounds of a single firm (Bloom et al., 2015). Instead, Linos (2018)’s setting only allows for comparisons across those hired into office work with different information about opportunities to eventually go remote. At the US Patent Office, Linos (2018) finds that those who went remote and knew about these opportunities at the time of hire were significantly less productive than those who went remote but were willing to take the job before these opportunities were known. Our setting allows us to replicate her informational design but also more directly assess the productivity differences between those hired into remote and on-site jobs: this becomes an especially clean comparison when all workers are remote due to Covid-19.
ier. Lazear (2004) and Oyer and Schaefer (2005) find that equity attracts more productive employees, while Manchester (2012) finds a similar phenomenon for tuition reimbursement programs. 9

The possibility for self-selection into jobs with certain amenities has been stressed as a motivation for government mandated benefits broadly (Summers, 1989) and in the context of workers’ compensation insurance (Gruber and Krueger, 1991) and maternity leave specifically (Gruber, 1994; Ruhm, 1998).

Data linking care-giving responsibilities to both amenities and productivity allows us to contribute to the large literature on the career consequences of care-giving (e.g., Bertrand et al., 2010; Goldin and Katz, 2011), and the narrower strand that explores the intersection between remote work and care-giving (Adams-Prassl, 2020). 10

Section 1 presents a model of adverse selection in the market for remote work. The rest of the paper estimates the parameters of our model in the context of call-center work. Section 2 offers more detail about our data and setting. Section 3 presents the average productivity difference between on-site and remote workers. The subsequent two sections decompose this aggregate difference into treatment (Section 4) and selection (Section 5). Section 6 estimates the demand for remote work. Section 7 pulls these pieces together to quantify the status quo surplus from remote work and the welfare losses from adverse selection. Section 8 concludes.

1 MODEL

We model the market for remote work in order to derive the sufficient statistics that allow us to characterize the equilibrium and quantify the welfare losses from adverse selection in our empirical context of call-center jobs. The key input into our model is the assumption that a worker’s type can determine both her preferences and her productivity conditional on her observables. The key take-away from our model is that adverse selection into remote jobs drives a wedge between the efficient allocation of remote work and the equilibrium provision in the market. The workings of our model are illustrated graphically in Figure 1, which we refer to throughout this section; the empirical findings of our paper gives us a picture of this model in our setting of call-center work in Figure 11.

Model Set-Up. We consider a competitive market where identical firms each offer a menu of two call-center jobs — one remote and the other on-site. 11 Each firm chooses the price workers

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9 More generally, this relates to the literature on the sorting of workers with high valuation of particular amenities to the firms that provide them (Oyer et al., 2008; Eriksson and Kristensen, 2014).

10 Adams-Prassl (2020) finds that women working on MTurk who have an infant at home are more likely to have work interruptions. Concerns about such distractions are reinforced by popular articles that tout remote work as especially beneficial for parents (see, for example, Schulte, 2020).

11 Our model shares many features of standard models of health insurance where firms can choose the price workers must pay for more generous coverage as in Einav et al. (2010)’s formulation of Akerlof (1978). We also
must pay to work remotely, or equivalently the gap in wages between the on-site and remote job \( P_{\text{remote}} = w_{\text{on-site}} - w_{\text{remote}} \).\(^{12}\)

**Worker Preferences.** We index a worker’s type according to her position in the distribution of preferences for remote work, ranging uniformly from those who most prefer working at home \((s = 0)\) to those who feel most at home in the office \((s = 1)\). A worker’s type determines her willingness to pay for remote work, \( D(s) \), or the maximum price that she will pay to work at the remote job (the orange line of Figure 1), which may be negative. The utility of a worker of type \( s \) in a job with wage \( w \) is given by:

\[
U(s, w) = \begin{cases} 
  w + D(s) & \text{if the job is remote} \\
  w & \text{if the job is on-site} 
\end{cases}
\]

Under this quasilinear utility function, there are no income effects so the relative wage across the remote and on-site job fully determines each worker’s choice. We further assume workers are far from their reservation wages so will always work in either the on-site or remote job: it’s simply a question of which one they will choose.\(^{13}\)

**Worker Productivity.** A worker’s type not only determines her preferences but also her productivity. Those who value remote work more may be more or less productive than those who place less value on this work arrangement. This is illustrated graphically in Figure 1, where the solid blue line denotes the worker’s productivity on-site as a function of her type, \( MP_{\text{On-Site}}(s) \): here, we have illustrated the case where productivity is negatively related to a workers’ willingness to pay for remote work.

A worker’s productivity in the remote job will reflect her productivity in the on-site job plus the treatment effect of working remotely, which we denote by \( \tau(s) \). Thus, \( MP_{\text{Remote}}(s) = MP_{\text{On-Site}}(s) + \tau(s) \).

**Social Optimum.** On the margin, if a worker works chooses remote instead of on-site work, share many features of classical labor market models of adverse selection (Salop and Salop, 1976; Miyazaki, 1977; Weiss, 1995).

\(^{12}\)Since firms are all identical, we are shutting down the possibility that self-selection of workers into particular jobs could increase aggregate output by sorting more productive workers to firms with greater returns as in Stiglitz (1975).

\(^{13}\)If we allowed for movements in and out of the labor force, then we would need to consider the fiscal externalities of these extensive margin effects on the government. If those with a high willingness to pay for remote work were most likely to exit the labor force and there was an unlimited amount of work to do, the dynamics we find would be magnified. However, if work were scarce, then having more productive workers working would be more efficient. Thus, the potential sorting function of a higher price of remote work could be socially useful, muting the distortions we find.
this leads to a marginal cost (or benefit) of:

$$\text{MC}_{\text{Remote}}(s) = \Delta MP = \text{MP}_{\text{On-Site}}(s) - (\text{MP}_{\text{On-Site}}(s) + \tau(s)) = -\tau(s).$$

The efficient solution would be for the price of working remotely to reflect this treatment effect so that workers were incentivized to choose the work arrangement that maximized their productivity. In Figure 1, this would give rise the share $s_{\text{efficient}}$ working remotely because this is where the demand curve (in yellow) intersects the marginal cost curve (in dark green).\footnote{It is worth noting that this is also the allocation that would be achieved by a firm that had a fully fixed workforce. This seems to suggest that this is a setting where monopsony power could attenuate a market failure as documented by Veiga and Weyl (2016); Mahoney and Weyl (2017) in other selection markets. We plan to explore this possibility explicitly in future iterations of the model.}

**Market Equilibrium.** The problem of selection means firms face a different cost of hiring a remote worker instead of an on-site one. What matters to a firm hiring a new worker is the difference in the average product between those in remote and on-site jobs rather the difference in the output of a marginal worker in these two work arrangements.

To find the average product in remote jobs, we must integrate the marginal product in remote work from left to right since remote jobs will attract workers from the left of the distribution whose willingness to pay for remote work exceeds the given price:

$$\text{AP}_{\text{Remote}}(s) = \int_0^s \text{MP}_{\text{Remote}}(\tilde{s})d\tilde{s} = \int_0^s \text{MP}_{\text{On-Site}}(\tilde{s}) + \tau(\tilde{s})d\tilde{s}.$$  

This gives rise to the dashed orange line in Figure 1 that begins at the marginal product and then lies everywhere below the marginal product when remote workers are adversely selected. Since remote jobs always attract workers with the highest willingness to pay for remote work, the lower productivity of these workers will pull down the average productivity in remote jobs even when a low price of remote work attracts more productive workers to also work remotely.

To find the average product in on-site jobs, we must start at the opposite end of the distribution and integrate the marginal product from right to left since on-site jobs attract workers from the left of the distribution whose willingness to pay for remote work is lower than the given price:

$$\text{AP}_{\text{On-Site}}(s) = \int_1^s \text{MP}_{\text{On-Site}}(\tilde{s})d\tilde{s}.$$  

This gives rise to the dashed navy line in Figure 1 that starts at the marginal product at the right of the graph and then lies everywhere above the marginal product when on-site workers are advantageously selected (because remote workers are adversely selected). Since on-site jobs always attract those workers with the lowest willingness to pay for remote work, their
higher productivity will tend to pull up the average productivity in the on-site job even when a high price of remote work means that less productive workers also choose to work on-site.

The difference in these average products determines the average cost of hiring a remote worker instead of an on-site one.

\[
AC(s) = \Delta AP(s) = \frac{\int_{s}^{1} MP_{\text{On-Site}}(\hat{s}) d\hat{s}}{\int_{0}^{s} MP_{\text{On-Site}}(\hat{s}) d\hat{s}} - \int_{0}^{s} MP_{\text{On-Site}}(\hat{s}) d\hat{s} + \int_{0}^{s} -\tau(\hat{s}) d\hat{s},
\]

which is a reflection of both the selection and treatment effect. Since remote and on-site workers are drawn from different ends of the distribution of preferences and productivities, the average productivity in the on-site job can exceed the average productivity in the remote job, even when each individual workers would be more productive working remotely as pictured in Figure 1.

To give more intuition for the average cost curve, we can consider a particular parameterization of the marginal productivity curves. These can also be thought of lower-order approximations for more complex functions:

\[
MP_{\text{On-site}}(s) = \beta_0 + \frac{\beta_1}{2} s + \frac{\beta_2}{3} s^2
\]

\[
\tau(s) = \tau_0 + \frac{\tau_1}{2} s.
\]

This gives rise to:\textsuperscript{15}

\[
AC(s) = \Delta AP(s) = \beta_1 + \beta_2 - \tau_0 + (\beta_2 - \beta_1) s \\
AC'(s) = \beta_2 - \tau_1.
\]

In the case we have pictured, productivity is linearly increasing in \(s\) (\(\beta_2 = 0\)) and the treatment effect is homogeneous (\(\tau_1 = 0\)), yielding \(AC(s) = \beta_1 - \tau_0\), which is constant for all \(s\), leading to the horizontal red line in Figure 1.

In the linear case, the selection effect is constant because the changes in the average productivity of on-site and remote workers cancel out. If remote workers are adversely selected, the

\textsuperscript{15}To see this, note,

\[
AP_{\text{Remote}}(s) = \frac{1}{s} \int_{0}^{s} \beta_0 + \frac{\beta_1}{2} \hat{s} + \frac{\beta_2}{3} \hat{s}^2 + \tau_0 + \frac{\tau_1}{2} \hat{s} d\hat{s} = \beta_0 + \beta_1 s + \beta_2 s^2 + \tau_0 + \tau_1 s,
\]

while

\[
AP_{\text{On-Site}}(s) = \frac{1}{1-s} \int_{s}^{1} \beta_0 + \frac{\beta_1}{2} \hat{s} + \frac{\beta_2}{3} \hat{s}^2 d\hat{s} = \beta_0 + \beta_1 (1 + s) + \beta_2 (1 + s + s^2).
\]
selection in the remote job will improve as more workers work remotely because those working remotely will become less strongly selected to prefer remote work. At the same time, the productivity of workers in the on-site job will improve because those still working in the office when more workers are working remotely will be more strongly selected to prefer on-site work.

In the linear case with a homogeneous treatment effect, the average cost is constant. This is the price firms charge for remote work. At this price, a share \( s^* \) of workers work remotely because they have an even higher willingness to pay to work remotely. While the demand curve in Figure 1 features workers with higher willingness to pay than the market price, it is possible for the market to completely unravel if the demand curve lies fully below the average cost even if it lies at least partially above the marginal cost. In Figure 1, since some workers have even higher willingness to pay for remote work than the high price, there is surplus from the provision of remote work, highlighted in the dark green triangle. However, the distortion caused by selection leads to a deadweight loss from the under-provision of remote work, highlighted in the maroon triangle. If the price of working remotely were equated with its marginal rather than average cost, then more workers would work remotely and surplus would rise because these workers would either (a) prefer remote work or (b) not disprefer it enough to counteract their lower productivity in the office.

Under these simplifying assumptions, there is a unique equilibrium provision of remote work that is unaffected by the starting position. In this formulation, the forces that caused remote work to be rare prior to Covid-19 would lead remote work to be rare after the pandemic subsides unless it changes the underlying primitives. Some of this logic boils down to straightforward supply and demand. If the pandemic causes all workers to learn that remote work is more pleasant than they anticipated it would be, demand will shift to the right and the equilibrium provision of remote work will rise after the pandemic. If the pandemic increases productivity in remote work by forcing workers to gain experience in this work arrangement and firms to develop new strategies and technologies for teleworking, then this will shift the marginal cost of remote work down, pulling the average cost along with it and leading to a greater provision of remote work. Both of these changes would increase the provision of remote work but would not change the extent of the market failure. Adverse selection would continue to drive a wedge between the market provision and the efficient provision of remote work, leading to continued deadweight losses but now in a different part of the preference distribution.

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16 The assumption of a constant treatment effect is supported empirically by the similar treatment effects of remote work across those who voluntarily choose it and those who are forced to work remotely by Covid-19.

17 While this full unraveling has not occurred for remote work among call-center workers, it may have occurred in other sectors that could go remote. What’s more this type of unraveling may be the reason why we do not see other types of alternative work arrangements in the current labor market.
This model also suggests a novel consideration for predicting the impact of the pandemic on the future of work: the productivity of those who learn that they prefer remote work. If more productive workers disproportionately change their mind about the desirability of remote work, then this will attenuate the selection effect of remote work and increase the provision of remote work, by raising the average productivity of those who self-select into remote jobs. In contrast to straightforward shifts in demand and marginal costs, this possible effect of the pandemic would ameliorate the market failure caused by adverse selection and thereby increase the social surplus from the provision of remote work.

Another way for selection to allow for Covid-19 to permanently change the provision of remote work would be for the initial equilibrium to be just one of multiple possible market outcomes. This would mean that the starting point of the market could change the ultimate provision of remote work. Since historically remote work was prohibitively unproductive, segments of the pre-Covid labor market may have been stuck in an inefficient equilibrium where remote work was rare because the market was unable to fully escape the initial condition in which remote work was rare. This can occur when the average cost curve slopes downward and either the average costs or the demand curve are non-linear.

The average cost curve will slope down if $AC'(s) = \beta_2 - \tau_1 < 0$. This can occur if those who prefer to work in the office actually see larger productivity gains from working remotely ($\tau_1 > 0$). Section 4 finds evidence weakly consistent with this possibility by comparing the treatment effect of remote work for those who voluntarily chose to go remote versus those who were forced to go remote by Covid-19. The average cost curve can alternatively slope down if those who most value remote work are especially unproductive while those who actively prefer the office are not so exceptional (perhaps due to a little too much time at the proverbial water cooler): this would cause output to rise concavely in $s$ ($\beta_2 < 0$). We find empirical evidence of concavity in Section 5, using the roll-out of opportunities to go remote. Thus, our empirical setting does not rule out the possibility that Covid-19 could permanently change the equilibrium.

While our results are broadly consistent with a downward sloping average cost curve, for this to generate multiple equilibria still requires non-linearities in demand or average costs. While we do not think that our data will be able to speak to these non-linearities, in future drafts of the paper, we will try to put bounds on the set of parameters that would make it possible for Covid-19 to permanently change the provision of remote work by pushing us into a new equilibrium.

**Take-aways of the model.** In our model, the selection and treatment effects of remote work jointly determine the equilibrium price of working remotely. Together with workers’ demand, this market price determines the observed share of workers working remotely. However,
this need not reflect the efficient provision of remote work. The adverse selection of remote workers — conditional on their observable characteristics — causes $P_{\text{market}}$ to be higher from $P_{\text{efficient}}$, leading to deadweight losses relative to the social optimum. The sufficient statistics for quantifying these welfare losses and characterizing the market equilibrium and the efficient allocations are the treatment and selection effects of remote work and the slope and intercept of workers’ demand. The rest of the paper estimates each of these statistics.

2 DATA

Our data comes from a Fortune 500 online retailer, where call-center workers handle inbound calls about customers’ orders. Our data spans from January 1, 2018 to August 8, 2020. We have two sources of data: first, a personnel management tool that tracks the daily productivity and time-use of call-center workers; second, human-resources data that details the full history of each worker’s specific call-center, wage, and seniority level as well as recording their gender, age, and home-address. We supplement these two data sources with information about workers’ outside options in call-center work, using data from Emsi, an economic modeling company that the retailer uses to understand local labor markets. In June 2020, the retailer also conducted an online survey about workers’ care-giving responsibilities, which lets us characterize the child-care responsibilities across remote and on-site representatives.

Calls are randomly routed among the workers who are working at a particular time at a given seniority level. Entry- and mid-level call-center workers handle routine, low-stakes calls about returning a product, changing a shipping address, canceling an order, or expediting a delivery. Entry-level workers only handle small orders of households — e.g. a toaster oven or a deck chair. Mid-level representatives also handle households’ larger orders — e.g. a refrigerator or a patio set — as well as the orders of small businesses. The upper-level workers handle higher stakes’ decisions about refunds for missing or damaged products for both households and businesses. They also handle the orders of high-volume businesses. For these businesses, calls are routed to workers who are familiar with the account, violating the general random assignment of calls.

To ensure that we draw fair comparisons across remote and on-site workers, we focus on the first six months of workers’ time at the retailer. In Appendix Figure A.2, we plot the promotion rates into mid-level and upper-level positions against workers’ time at the retailer: as illustrated by the light blue and orange curves, entry-level hires are often promoted to mid-level positions within their first six months at the retailer, but these promotion rates are almost identical across on-site and remote workers. By contrast, in the next six months of their

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18 Emsi aggregates data from publicly available sources, particularly the Occupational Employment Statistics from the Bureau of Labor Statistics. We plan in future work to consider the robustness of our results to using this publicly available data source.

19 Promotions from entry-level to mid-level positions result in a $0.50/hr pay bump, while promotions from full-level to senior positions result in a $2/hr pay bump.
tenure, on-site representatives are significantly more likely to be promoted to senior positions that let them make higher stakes’ decisions and handle higher volume clients. By focusing on the first six months, we ensure that these differential promotion rates do not taint our analysis. We also effectively focus on the calls that are randomly assigned and for which call volumes, customer satisfaction reviews, and resolution rates are more meaningful measures of productivity.\textsuperscript{20}

Customers can call into the retailer between 8am and 10pm EST on weekdays, with slightly truncated hours on the weekend. The retailer covers this time by having workers from various different time-zones working in standard working hours of 8 to 4pm or 9 to 5pm in their home time-zone. Since there may be different times of the day and different days of the week, we include date by time-zone fixed effects in our preferred specifications.

Table 1 presents the summary statistics of workers who are in their first six months at the retailer and were hired into entry-level positions in 2019. In the first column, we present the mean across all workers; in the second column, we present the mean for remote workers; in the third column, we consider the mean for on-site workers; and in the fourth column, we report the difference.

The retailer assesses each worker’s performance primarily according to the number of calls they hand and the average customer satisfaction reviews across these calls. These productivity metrics form the backbone of the retailer’s quarterly bonus system, which rewards representatives based on their performance relative to their team.\textsuperscript{21}

In a typical day, a worker answers 26 calls or about one call every twenty minutes. On average, customers spend over a minute on-hold before a worker picks up. Workers average ten minutes talking to each customer, with remote representatives spending an extra 40 seconds on the line. An extra 40 seconds per call adds up to an extra 17 minutes over the course of an average day with 26 calls. This extra time does not translate into higher customer satisfaction ratings for remote workers, which are close to 5 out of 5 across the board. Workers report being able to handle nearly 80% of the calls themselves — without needing to transfer the call to a more experienced worker. The reported resolution rate is marginally lower for remote workers, as consistent with remote workers having fewer informal mechanisms by which to ask for assistance on a call.

\textsuperscript{20}It is worth noting that the differences in these career ladders — which may reflect either productivity differences or the perks of face-to-face contact Elsbach et al. (2010) — may affect who elects into remote jobs.\textsuperscript{21}The top 10\% of the team receive a bonus of $1500; the next 15\% receive $1000; the next 25\% receive $750; and the next 35\% receive $250. The bottom 15\% receive nothing. Part-time workers receive half of the bonus of full-time workers. Since these bonuses are all based on relative performance within a team, representatives who start in remote and on-site jobs are unlikely to compete with one another for the purposes of quarterly bonuses.
On average, workers clock in for 8 hours per day. Workers spend 47% of their scheduled work-time taking calls; 5.1% letting the phone ring; another 10% in the queue; and 48% of their time logged out of the system entirely. Remote and on-site workers have fairly similar time-use, except remote workers spend about 15 more minutes each day in the queue waiting for a call to be routed to them. Workers have some discretion over their time in the queue. workers can opt in and out of the queue. What’s more, the system tells them their place in the queue. Thus, workers can selectively opt out when they are up next. Such manipulation would allow them to appreciably increase their queue time per call.

For some of the time that workers are logged out of the system, they are absent from the job entirely. On average, workers miss 46 minutes per day. Most of these absences are unapproved and put other workers in the position of making up an unexpected gap in call-capacity. Absenteeism is a persistent problem for the retailer despite the rewards for perfect attendances and the threat of termination for too many unexcused absences. Remote workers rack up less unapproved absent time, which is consistent with remote work being more conducive to juggling work responsibilities with unexpected contingencies at home.

On average, the retailer pays its entry-level on-site customer service workers $15/hr (first row of Table 1) and its remote workers $14/hr (second row).

Customer service is a predominantly female occupation. Women account for 65% of customer service workers in the 2018 ACS, 74% at the retailer as a whole, and 88% among the retailer’s remote workers.

At the retailer, many customer service workers report child-care responsibilities in a company-wide survey in June 2020. Among remote workers, 57% report child-care responsibilities compared to 41% among on-site workers. These patterns are emblematic of the difference between remote and on-site customer service workers more broadly: in the 2018 ACS, 48% of remote customer service workers had children in the house compared to 34% of on-site workers.

22 Unlike other customer service jobs that feature significant part-time opportunities, only 10% of days have part-time schedules, while 15% of days feature over-time.
23 Prior to Covid-19, there was a quarterly $500 bonus for perfect attendance. During Covid, this bonus was upped to $250 per month. In addition to this carrot, the retailer uses a stick of potential termination for exceeding a six-month allotment of attendance credits.
24 When constructing the average in the ACS, we limit to the 28,151 individuals employed in occupation 5240, covering any customer service worker, including those who work in brick-and-mortar retailers rather than answering calls for online purchases. We use the person weights from the Census to account for differential sampling of certain populations. The greater representation of women among remote workers compared to on-site workers is consistent with patterns in the 2018 ACS and those in the US patent office, where Linos (2018) finds female workers are more likely to opt into remote work.
25 Another 15% report caring for an elder or someone with a disability.
26 To discern whether a worker was remote, we use the question about transportation to and from work to identify the 5.6% (1,552 of 26,056) of customer service workers who worked remotely. In addition to more children
Customer service is an occupation with a great deal of churn. In a typical month, about 6 out of every 100 workers leave their job — mostly of their own volition.\textsuperscript{27}

3 \textbf{Average Productivity Difference Between Remote and On-Site Workers}

The average productivity difference between those working remotely and those working on-site reflects both the treatment effect of remote work and the selection into this work arrangement. Our model predicts that, in the market equilibrium, the price of working remotely will reflect the difference in average productivity between remote and on-site workers. To assess these predictions in our empirical setting, we estimate the following specification:

\[ \log(Y_{it}) = \beta_{\text{Remote}} \mathbb{I}[\text{Hired Remote}_i] + \mu_{FE} + \epsilon_{it} \]  

(3)

where \( Y \) may denote either calls per hour, satisfaction reviews, or wages. To assure that remote and on-site workers are drawing calls from the same pool, Table 2 first includes date by time-zone fixed effects, then interacts these controls with the month of hire to account for differences in experience levels, and finally fully interacts with job title to account for any remaining discrepancies in positions within the company.

In the first panel of Table 2, we see that remote workers answered 9-11% fewer calls than on-site workers prior to Covid-19 (with 99% confidence intervals spanning from 4.5% to 15.8%). These differences in call volumes are robust to the inclusion of controls for experience and job title, which have minimal impact on the coefficients but nearly double the \( R^2 \) from 0.27 to 0.47.\textsuperscript{28} These differences in call volumes are much larger than we would expect from variation across MSAs alone: when we generate placebo comparisons between remote hires drawn from different MSAs, we never find as large a difference in call volumes as the observed difference between the retailer’s remote and on-site workers (Appendix Figure A.3).

Remote workers answered fewer calls primarily because they took longer to answer each one: as detailed in Appendix Table A.1, remote workers spent 45-61 additional seconds longer on each call, increasing the length of the average 9 minute call by 7-10%. Much longer calls did not make for much more satisfied customers. The differences in customer satisfaction in the second panel of Table 2 are neither statistically or economically significantly: with differences of more than 0.7% lie outside of the 99% confidence intervals.

in the house, remote workers in the ACS are more likely to have young children: 12.6% compared to 10% of on-site customer service workers.

\textsuperscript{27} As consistent with the many demands on call-center workers’ time, many of these separation are reported to be due to family constraints or geographic moves.

\textsuperscript{28} In Oster (2019)’s framework, the fact that the coefficient is stable despite improvements in the model’s fit suggests that unobservable differences between remote and on-site workers would not appreciably affect our results if they operated similarly as observable characteristics.
The productivity differences between remote and on-site workers emerge early in workers’ tenures and persist even as they gain experience as illustrated in Figure 2.\textsuperscript{29} The constancy of the gap in workers’ productivity suggests that these differences are due to fixed characteristics of workers or fixed impacts of working remotely, rather than the effect of remote work on on-the-job learning. If on-site workers learned more from their peers, we would expect the productivity gap to initially grow and then attenuating as diminishing returns set in.\textsuperscript{30} What’s more, the substantial on-the-job learning after an initial 3-week formal training suggests that it would be costly for the retailer to address the problem of adverse selection by firing underperforming workers.

In the context of our model, these productivity differences would suggest that, in equilibrium, remote workers would be paid 9-11\% less to offset their lower productivity. Indeed, among workers working in the same time-zone at the same time, we find remote workers earned less than their on-site peers. However, the 5-6\% pay gap in the third panel of Table 2 does not fully offset the productivity difference. However, a broader view aligns these two figures: once we incorporate the fact that remote workers do not require the office overhead of on-site workers, these wages differences are consistent with the market equilibrium setting wage differences to balance average productivity differences between remote and on-site workers net of overhead costs incurred from running an office.\textsuperscript{31}

**Tracing out the average productivity curve.** The observed productivity difference reflects the conditions in the current equilibrium. To see a more holistic view of the forces that guide the market to this equilibrium, we trace out the average productivity curve of remote workers, using variation in the price workers must pay to work remotely. The retailer’s uniform, national $14/hr remote wage creates variation in the implicit price that workers must pay to work remotely: in places where local on-site options offer higher wages, workers must implicitly pay more to take this remote job instead of an on-site alternative; by contrast, in places

\begin{equation}
\log(Y_{it}) = \sum_{m=2,8} \beta_{Remote,m} \mathbb{1}[\text{Remote}_{i}] \mathbb{1}[\text{Tenure}_{it} = m] + \sum_{m=3,8} \beta_{On-Site,m} \mathbb{1}[\text{On-Site}_{i}] \mathbb{1}[\text{Tenure}_{it}] + \mu_{t,z} + \epsilon_{it} \tag{4}
\end{equation}

where $\mu_{t,z}$ denotes date by time-zone fixed effects. The omitted category is on-site workers in their second month at the retailer. We omit the first month of formal training when workers’ calls are not handled independently.\textsuperscript{30} Such diminishing returns would mean that if on-site workers gained more skills in formal training, the resulting gap in productivity would likely attenuate with time as workers gained on-the-job experience.

\textsuperscript{31}Our back-of-the-envelope calculations suggest that reasonable office costs would offset the remaining discrepancy between average pay and average productivity. Prior to Covid-19, the retailer paid $5 for every call handled, \left( \frac{$15/hr}{3\text{ calls/hr}} = $5/call \right). Remote workers answered 0.28 fewer calls per hour, representing $1.40/hr lower output (0.28 calls/hr \cdot $5/call = $1.40/hr). At the same time, remote workers earned $0.80/hr less than on-site workers in the same time-zone, partially offsetting the productivity difference. Based on these factors alone, remote workers would be $0.60/hr less profitable than on-site workers. However, if the price per square foot of office overhead were $11.50 per year, then this would be sufficient to offset the remaining difference, assuming each full-time worker required 100 square feet of office space. In the non-urban areas where the retailer locates its call-centers, $11.50 per year is a plausible (although probably somewhat low-end) estimate of office rental costs.
where local on-site options offer relatively low wages, workers face a low opportunity cost of taking this remote job instead of a local on-site one.

This price variation allows us to correlate the implicit price workers must pay to work remotely with their on-the-job productivity. Specifically, we estimate:

$$\log(\text{Calls/Hr}_{it}) = \phi (14 \text{/hr} - \text{MSA On-Site Wage}_i) + \mu_{t,z,h} + \epsilon_{it},$$  \hspace{1cm} (5)$$

where $\mu_{t,z,h}$ denotes date by time-zone by hiring month fixed effects.\textsuperscript{32}

Figure 4 depicts the average productivity of remote workers relative to comparable on-site workers as a function of the implicit price they must pay to work remotely. In the first point, workers must be willing to sacrifice at least $1/hr to be remote. These workers were 12.5% less productive than comparable on-site workers prior to Covid-19. In the second point, workers may have similar high willingness to pay to work remotely, but may also only be willing to give up $0.50-$1/hr to be remote. In the third point, workers may even be roughly indifferent between remote and on-site work. Finally, in the fourth point, some workers may actively prefer the office but take this remote job because it offers a higher wage than they could find at a local on-site option. When the retailer’s pay exceeds the pay in local alternatives, the productivity of remote workers is still 7.5% lower than that of comparable on-site workers, but 5pp higher than the productivity of those who had to accept more than a $1/hr wage penalty in order to take this remote job.

The fact that productivity is higher when workers are less strongly selected according to their willingness to pay for remote work offers initial suggestive evidence of the importance of selection in our setting. The persistent difference between remote and on-site workers could be consistent with either treatment or selection driving the average productivity difference.

Selection can generate a persistent productivity difference because remote jobs drawn in workers from the left in the preference distribution. At each point in Figure 4, a remote job will attract all the workers who are willing to pay at least this price to work remotely: thus, the productivity of workers with the highest willingness to pay for remote work still influences the average productivity in places where the retailer’s remote wage is competitive with local on-site alternatives. This force means that firms may be unable to offset the adverse selection of remote workers by paying them wages that are competitive with those of on-site alternatives.\textsuperscript{33} While this dilutes the impact of the productivity of those with the highest willingness

\textsuperscript{32}We approximate the entry pay in the MSA according to the average of the 25th and 50th percentiles of the local customer-service wage distribution. We define customer service workers as all those with occupation SOC 5240, which includes those who work in brick-and-mortar retailers rather than answering calls for online purchases. In future work, we will consider the robustness of our inferences to other definitions of the relevant occupations.

\textsuperscript{33}We can also see this inference in a different way by noting these patterns are qualitatively similar when
to pay for remote work, it does not obviate their effect.

While the market equilibrium in our model depends only on the average productivity difference, the welfare implications hinge on the contributions of treatment and selection to this average difference. Thus, the next two sections decompose the average difference into treatment and selection.

4 Treatment Effect of Remote Work

We first estimate the treatment effects of working remotely on productivity. A priori, it is unclear how remote work will impact productivity. Remote work could reduce productivity because workers shirk when away from the watchful eyes of managers or lose focus amidst all of home’s distractions; alternatively, remote work could improve productivity because workers are less distracted by their coworkers or better equipped to respond to life’s contingencies. What’s more these effects could differ for those who voluntarily choose remote work versus those who are forced into this working from home. We first consider the sharp changes in productivity for workers who transitioned from on-site to remote work during the period when the retailer allowed such transitions. We then consider the differential effects of Covid-19 on formerly on-site workers who were forced to switch to remote work versus already remote workers to isolate the treatment effect of remote work net of the common shocks faced by the retailers’ workers.

In our model, the treatment effect determines the socially efficient price of remote work that incentivizes workers to choose the work arrangement that maximizes their productivity.

4.A Event Study Around Individual Transitions to Remote Work

To assess the treatment effect of remote work on individual productivity, we first consider workers who switched from on-site to remote work when the retailer posted remote job openings for on-site workers in 2018 and 2019. In the first half of 2018, these job openings were primarily created to relieve crowding in some call-centers; in the second half of 2018 and 2019, they were primarily created to supplement the retailer’s remote workforce as the retailer built out its pipeline to hire workers directly into remote jobs. During both of these periods, the timing of on-site workers’ transitions to remote work was primarily determined by the retailer’s needs rather than the workers’ demands.34

Since the timing of a worker’s transition was not fully under her control, the change in her we limit the matches to workers with the same wages at the retailer or similar relative pay compared to their local alternatives in Appendix Table 2, suggesting that simply increasing pay would not fully offset differences in selection.

34According to our contact at the retailer, such transitions are “very business driven.” In her words, “a worker does not just go to her manager and say she wants to go remote. Instead, the retailer has to say, ‘we are opening remote positions, do you want to take them?’”
productivity around the switch to remote work is plausibly due to the treatment effect of working remotely rather than any shock that may have motivated her to seek out remote work. This motivates an event study design around individuals’ switches to remote work. Letting $e$ denote event time in number of weeks from the switch to remote work, we estimate:

$$\text{Calls/Hr}_{i,e} = \sum_{e \in [-4,4]} \beta_{ES,e} 1[e_i = e] + \epsilon_{ie}, \quad (6)$$

where we compare each week to the one before the worker’s switch to remote work. We also pool across months to estimate the productivity difference before and after workers’ transitions to remote work within one and six week bandwidths, according to:

$$\text{Calls/Hr}_{i,e} = \beta_{ES} 1[\text{Post Transition}] + \epsilon_{ie} \quad \text{if } |e| \leq \text{Bandwidth.} \quad (7)$$

Figure 5 considers the hourly call volumes of the 121 entry- and mid-level workers who transitioned from on-site to remote work while remaining in a constant role at the retailer for the full six weeks before and after this transition. The x-axis plots the event time in weeks from the switch from on-site to remote work. The y-axis plots the workers’ calls per hour. In the pre-period, hourly calls were steady: at the time of the transition to remote work, hourly calls rose by 6.9% (also reported in first column of Table 3) even though workers continued to handle similar calls of comparable complexity. These gains persisted throughout the next month and a half: aggregating across the six weeks before and after the transition to remote work, workers were 7.5% more productive after going remote than before this transition (also reported in the second column of Table 3).

The additional calls did not come at the expense of customer satisfaction reviews which remained stable around this transition (as detailed in columns two and three in Table 3). Transitioning to remote work also suggestively improved workers’ reliability, with unapproved absent minutes falling by 2 to 4 minutes off of a base of 10 minutes although this effect is measured imprecisely.

These findings suggest that the average gap in productivity does not stem from the treat-

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35 Particularly, of the 248 entry- and mid-level workers who transition to remote work in 2018 and 2019, we exclude 46 who were promoted in the six week window around their transition to remote work. Such a promotion would directly change their productivity by expanding set of calls they could handle. We also exclude the 81 workers who were not at the retailer for the full 6 weeks before and after their transition to remote work to generate a balanced panel.

36 Appendix Figure A.4 shows that these gains of the switchers were not replicated for their new remote teammates, suggesting the retailer’s demand for additional remote workers did not coincide with particular burdens on these workers’ time. Further, we do not see the same sharp increases in productivity for those switching between remote teams or between on-site teams suggesting that the increases of those switching from on-site to remote work were due to changing work environments not simply a desire to make a good impression on a new manager and teammates.
ment effect of remote work but rather reflects the selection into remote work, opening up the possibility for a market failure in the provision of remote work. Further, this positive treatment effect implies it would be socially efficient for workers to pay to be paid a premium to work remotely since each individual’s output is higher when working remotely. However, the treatment effect for those who opt into remote work may not generalize to those who prefer to work in the office. To assess this possibility, we next consider the productivity effects of remote work for those forced into remote work by Covid-19.


At the time when the retailer closed all of its on-site call-centers on April 6, 2020, 82% of its workforce was working on-site while the remaining 18% was already working remotely. While on-site workers were “treated” with a forced transition to remote work, already remote workers were unaffected by the office closures. Thus, already remote workers can serve as a control group for formerly on-site workers because they were exposed to similar shocks but were able to remain in a constant work environment.

To isolate the effects of going remote — which was unique to formerly on-site workers — net of the many shocks of Covid-19 that were commonly felt by all workers, we estimate the following difference-in-difference design:

\[
\text{Calls/Hr}_{it} = \beta_{\text{DiD}} \mathbb{1}[^{\text{Hired On-site}}]_{i} \mathbb{1}[^{\text{Post Closure}}]_{t} + \psi_{1} \mathbb{1}[^{\text{Hired On-site}}]_{i} + \mu_{t,z,h} + u_{it}, \quad (8)
\]

where \(\mu_{t,z,h}\) denotes date \((t)\) by time-zone \((z)\) by hiring month \((h)\) fixed effects to net out any differential effects of Covid-19 across space or across workers with different experience levels.

The timing of the office closures in early April helps us isolate the treatment effect of remote work net of the many shocks caused by Covid-19. Many schools closed in the middle of March, three weeks before the retailer closed its offices. Focusing on a three week bandwidth around the office closure consequently isolates a period when many of the shocks to workers’ personal lives were underway in the pre-period. Thus, even if these shocks were differentially born by remote and on-site workers, they would be netted out in the first difference that compares productivity before and after the office closure within workers who were either hired to be on-site or remote.

Figure 6 illustrates our difference-in-difference design visually. The x-axis plots the week, highlighting the office closures on April 6, 2020 with the red vertical line. The y-axis plots the average hourly calls handled by formerly on-site workers in the solid circles and by already remote workers in the dashed triangles. The analysis limits to workers who were working at the retailer and within their first six months for the entirety of the 6 weeks before and after the office closures. This restriction eliminates compositional changes that would otherwise affect the interpretation of these patterns.
Throughout this period, call-volumes of all workers rose as the pandemic increasingly drove consumers to purchase goods on the internet from firms like this online retailer. However, the productivity gains of those who used to work on-site outpaced the productivity gains of workers who were already working remotely. In the six weeks before the office closure, on-site workers answered 0.14 more calls per hour than remote workers; in the six weeks after the office closures, this gap rose to 0.39 calls per hour.

The difference in these differences suggests that going remote caused on-site workers to answer 0.25 more calls per hour, a 7.6% increase off a base of 3.22 calls per hour. This is nearly identical to the event study results, suggesting that the treatment effects of working remotely are similar for those who voluntarily opt into remote work as for those who are forced into working remotely. When we consider the period when schools were closed in the three weeks before the office closures, the estimated effect is marginally larger, suggesting a 10% productivity increase, as reported the second column of Panel A in Table 4. We do not find evidence that additional calls came at the expense of significantly reduced customer satisfaction, with changes of less than 1% of the average reviews in the last two columns of Panel A in Table 4.

Given the substantial increase in consumer demand during this period, one natural concern would be that remote and on-site workers would respond in systematically different ways to any rush in demand. To assess this hypothesis, we consider how the productivity differences between on-site and remote workers evolved around the peak in consumer demand during the 2019 holiday season. We replicate our difference-in-difference analysis of Covid-19 but instead consider the previous holiday season, focusing on the sharp uptick in demand in the week of December 1, 2019:

\[
\text{Calls/Hr}_{it} = \beta_{\text{Holiday}_t}\mathbb{1}[\text{Hired On-site}_i]\mathbb{1}[\text{Holiday}_t] + \psi_{2}\mathbb{1}[\text{Hired On-site}_i] + \mu_{t,z,h} + \zeta_{it}. \quad (9)
\]

We report the results of this analysis in the second panel of Table 4. During the previous holiday season, the differences in call volumes between remote and on-site workers compressed. Appendix Figure A.5 shows this compression graphically. The managers of remote workers may have been able to pressure remote workers to spend more of their working hours taking calls and to answer each call more quickly. Thus, if all that changed was rising consumer demand, we would expect remote workers would gain rather than lose ground relative to on-site workers.

These results provide evidence that remote work increases individual productivity even among those who do not voluntarily opt into this work arrangement: this in turn implies the marginal cost of having a worker work remotely is negative irregardless of their preference for this work arrangement. Thus, in this setting, it would always be efficient to pay a premium for remote work.
While we cannot say precisely whether productivity gains are larger among those who are forced into it, our estimates point in this direction. Since this would lead to a downward sloping average cost curve, this opens up the possibility that Covid-19 could permanently change the provision of remote work without altering the underlying primitives of the model, by pushing us into a new equilibrium.

5 Selection into Remote Work

Given the positive treatment effect of remote work that we estimated in the previous section, the lower productivity of remote workers in Section 3 implies these workers are negatively selected. If remote work makes workers 7.5% more productive (from Section 4), but those working remotely were 10.5% less productive prior to Covid-19 (from Section 3), this would suggest that remote workers would be 18% less productive than on-site workers if all workers were in the same work environment.

In addition to backing out this selection effect, we can also assess it more directly by evaluating (a) how productivity differs between those hired to be on-site and those hired to be remote after all the offices close, (b) how the roll-out of opportunities to go remote affects the selection of new on-site hires, and (c) how the phase out of these opportunities affects the gap in productivity between on-site and remote workers.

5.A Productivity Differences Between Remote and On-Site Hires During Covid-19

After Covid-19 forced all workers to be remote, those hired to be remote are 12% less productive than those hired to be on-site. We estimate the difference in productivity between those hired into remote and on-site jobs in the same month, who are taking calls at the same time and in the same time-zone:

\[
\text{Log}\left(\frac{\text{Calls/Hr}}{\text{Hr}_r}\right) = \beta_{\text{Remote Selection}} \mathbb{1}[\text{Hired Remote}] + X_i'\psi + \mu_{t,z,h} + \epsilon_{it} \tag{10}
\]

where \(\mu_{t,z,h}\) denotes date \(t\) by time-zone \(z\) by hiring month \(h\) fixed effects. We further include observable characteristics of workers to investigate whether the inclusion of this information affects the apparent selection effect of remote work.

We can see the distributions of productivity of on-site and remote hires graphically in Figure 7. On average, remote hires answer 12% fewer calls than on-site hires, as reported in column one of Table ??.

This productivity difference only grows wider when we account for differences in the age of remote and on-site hires in column two of Table ?? . Older workers tend to answer slightly more calls, possibly due to their greater prior experience, and are more likely to select into
remote work. Thus, conditioning on worker age magnifies the negative signal of selecting into remote work. This suggests that screening on worker age — or a correlate like prior experience — might magnify the problem of adverse selection since the older age of remote workers pushes in the direction of greater productivity. The fairly precisely estimated zero in the fourth row suggest that negative selection into remote work is not stronger in older or young workers.

The third column introduces childcare responsibilities as recorded in a company-wide survey in June 2020. Workers with child-care responsibilities were actually 6% more productive than those without such responsibilities during the pandemic. Since remote workers are more likely to have care-giving responsibilities, conditioning on these responsibilities further magnifies the negative selection effect of remote work. Thus, care-giving responsibilities do not appear to drive the adverse selection into remote work: instead some other mechanism generates a negative correlation between workers’ productivity and their preference for remote work.

While we lack statistical precision when evaluating the interaction between child-care responsibilities and remote work in predicting worker productivity, our point estimates in column four suggest that adverse selection into remote work operates within both care-givers and non-care-givers. Thus, firms should not be particularly eager to hire care-givers into remote work, but also should not be particularly averse from hiring care-givers into remote jobs. While firms cannot legally deny certain work arrangements to care-givers, such fears might lead remote work to be rarer than it might otherwise be in settings such as ours where many workers have care-giving responsibilities.

5.B Crowded Call-centers and the Opportunity to Go Remote
Unexpected opportunities to go remote allow us to evaluate how the intensity of a worker’s preference for remote work relates to her productivity.

In the beginning of 2018, the retailer introduced opportunities to go remote to relieve crowding in a few of its call-centers. For existing hires, these opportunities were unexpected at the time of hire; for new hires, these opportunities were known from the start. Among the workers who ultimately went remote, their initial decision to take the job revealed different preferences for remote work. Those who took the job when they thought it would stay on-site revealed a weak preference for remote work: their willingness to start an on-site job that they expected to stay on-site puts an upper bound on their preference for remote work. By contrast, those who took the job when they knew it might become remote revealed a potentially strong preference for remote work: these workers may have only started this on-site job because they knew it might become remote. Comparing the productivity of those who went remote but had different information at the time of hire consequently isolates the in-
tensive margin of selection by assessing the productivity difference between those who have revealed weaker and stronger preferences for remote work.

To net out cross-cohort productivity differences, we consider a difference-in-difference design where we compare the productivity of remote and on-site workers across cohorts with and without information about opportunities to go remote at the time of hire. Specifically, we estimate:

\[
\log \left( \frac{\text{Calls}_{i,t}}{\text{Hr}_{i}} \right) = \beta_{\text{Strong Preference}} \mathbb{1}[\text{Remote}_i] \cdot \mathbb{1}[\text{Hire}_i \geq 2018] + \beta_{\text{Weak Preference}} \mathbb{1}[\text{Remote}_i] + \mu_{t,o,h} + \epsilon_{i,t} \tag{11}
\]

where \( \mu_{t,o,h} \) denotes date \((t)\) by hiring call-center \((o)\) by hiring month \((h)\) fixed effects to limit comparisons to those hired in the same call-center at the same time taking calls on the same day. To show the full dynamics of this comparison, we also estimate:

\[
\log \left( \frac{\text{Calls}_{Hr,i,t}}{Hr} \right) = \sum_{h \in \text{Cohorts}} \beta_{\text{Remote},h} \mathbb{1}[\text{Remote}_i] \cdot \mathbb{1}[\text{Cohort} = h] + \mu_{t,o,h} + \epsilon_{i,t} \tag{12}
\]

which allows selection into remote work to flexibly vary across cohorts.

Figure 8 illustrates the results of this design. The x-axis plots the hiring month. The y-axis plots the logged difference in calls per hour between those who were remote versus those who were on-site within each cohort. The dashed vertical line separates the cohorts who were unaware of the opportunity to go remote at the time of hire (to the left) from those who were aware of this opportunity at the time of hire (to the right). The productivity difference between those who went remote and those who stayed on-site changes sharply when their information at the time of hire changed. Pooling across pre- and post-cohorts reveals that those who went remote and were aware of this opportunity at the time of hire were 19-23% less productive than those who went remote but did not know about these opportunities ex-ante (the first row of columns one and two of Table 6). Lower quantity of calls did not translate into higher quality as reported in columns three and four of Table 6). This suggests those with a strong preference for remote work are substantially less productive than those with a weak preference for remote work in our setting.

Among these cohorts who did not know about the opportunity to go remote at the time of hire, remote workers were marginally more productive than on-site workers (the second row in columns one and two of Table 6). This 7% productivity difference is commensurate with the positive treatment effect of remote work estimated in Section 4, suggesting that those who went remote in these cohorts were no more nor less productive than those who stayed on-site. Since those who went remote when it was a surprise revealed a weak preference to work remotely, this comparison suggests there is not a large productivity difference between those who weakly prefer working remotely and those who actively prefer the office. This
contrasts with the 16% lower productivity of those who went remote in cohorts that knew about these opportunities at the time of hire, suggesting a strong preference for remote work is a strong negative signal about productivity.

These patterns suggest that the negative selection effect of remote work largely stems from attracting workers with a strong preference to work remotely rather than losing workers with an active preference to work on-site. In the context of the model, this points to falling rather than rising average cost curve of offering remote work, opening up the possibility of multiple equilibria in the provision of remote work and, thus, the possibility for Covid-19 to permanently change the prevalence of remote work without changing the underlying primitives of the model.

This analysis is inspired by Linos (2018) who leverages the roll out of the US Patent Office’s work-from-home program to assess whether the gap in productivity between remote and on-site workers differs for cohorts recruited before and after the program is launched. Like our results, she finds that on-site workers tend to be more productive than remote workers and that this gap is wider for cohorts recruited with the knowledge that they would have an opportunity to work from home. The concordance between our selection results for markedly different types of workers suggests that our results may generalize beyond the bounds of call-center workers, leading to larger welfare losses from the under-provision of remote work.

5.C Phase-Out of Internal Recruiting for Remote Jobs

In addition to the introduction of remote opportunities, we also assess the phase-out of these opportunities. Initially when the retailer was building out its remote workforce, it supplemented direct remote recruits with internal transfers from its on-site call-centers. As it developed a pipeline of direct remote recruits, the retailer phased out internal recruiting of on-site workers to take remote jobs. Without opportunities to go remote, on-site call-centers were less likely to attract workers who preferred remote work. If a taste for remote work negatively relates to productivity, we would expect the productivity gap between remote and on-site workers to widen as opportunities for remote work were phased out at on-site call-centers. To test this hypothesis, we estimate:

$$\log(\text{Calls/Hr})_{i,t} = \sum_{h} \beta_{\text{On-Site},c} [\text{Hired On-Site}_i] \text{[Hiring Cohort]}_i = c + \mu_{t,z,h} + \epsilon_{i,t} \quad (13)$$

where $c$ denotes a six-month hiring cohort and $\mu_{t,z,h}$ denotes date ($t$) by time-zone ($z$) by hiring month ($h$) fixed effects. We include these fixed effects to net out any differential consumer demand across space or time and any differential experience of remote and on-site hires, caused by different timing of these hires within six-month cohorts. For this analysis, we exclude observations after April 6, 2020 when on-site workers were working remotely.
During this period, the working conditions of on-site and remote workers remained stable: the retailer maintained the same workforce management system, the same routing system for calls, the same organization of workers into teams, and the same sorts of quarterly performance incentives. Thus, we do not have reason to believe that other changes would affect the productivity difference between remote and on-site hires.

In Figure 9, the rate at which on-site workers can go remote (in orange triangles) is mirrored in the productivity difference between on-site and remote workers (in blue circles). In the first six months of 2018, fully 13.5% of on-site hires transitioned to remote work in their first six months at the retailer. Workers recruited in the tail end of 2018 could have reasonably believed these opportunities would be available to them and indeed, 8% of these on-site hires eventually transitioned to remote work and 3% did so in first 6 months (the second orange triangle in Figure 9). For this cohort, there is no discernible productivity difference between those hired directly into remote jobs and those hired into on-site jobs which might turn into remote positions. By contrast, for those hired in the first six months of 2019, only 0.2% transitioned to remote work in their first 6 months. For this cohort, on-site hires were 5% more productive than their remote peers. In the subsequent two cohorts, no on-site workers transitioned to remote work within their first six months. For these cohorts, on-site hires were consistently 12% more productive than remote hires, as these jobs likely better sorted workers according to their preferences for remote work.

When the retailer extended opportunities to go remote to on-site workers, those hired on-site were more similarly selected as those hired directly into remote jobs. Since few on-site workers actually transitioned to remote work in the first 6 months of their tenure, this analysis offers direct evidence that offering remote worker degrades worker selection, which does not rely on our estimated treatment effect.37

These analyses suggest that remote jobs attract less productive workers. Further, firms cannot easily side-step negative selection by reserving remote work for those initially hired on-site. The problem of adverse selection means that remote work will be under-provided by the market relative to what would be socially efficient. To quantifying the deadweight losses from this under-provision and characterize the provision of remote work in the efficient equilibrium, we next estimate worker demand for remote work.

6 Estimating Demand for Remote Work

Through the country, the retailer advertises remote call-center jobs at a uniform $14/hr wage. The uniformity in this wage contrasts sharply with the heterogeneity in workers’ local outside

37We think the lower relative productivity of on-site hires is unlikely to be due to negative spillovers from workers transitioning to remote teams since remote and on-site workers are on independent teams unlike at the US Patent Office analyzed by Linos (2018). Further, transitions across teams are frequent since teams are of a uniform skill-level so promotions require workers to transition to new teams.
options. Together, this generates variation in the implicit price workers must pay to work at this remote job rather than a local on-site alternative.

In cities like Dallas TX, where the retailer’s $14/hr wage is far below the going rate for similar work, workers must pay a high implicit price to take this remote job. By contrast, in places like Lufkin, TX, a couple hours from Dallas, the retailer’s pay exceeds many of the less lucrative on-site alternatives: thus, even workers who prefer the office may take this remote job.

Since recruiters at the retailer are indifferent about the home locations of new hires and advertise these jobs on national job boards, the share of hires from places like Dallas TX versus places like Lufkin TX allows us to trace out workers’ demand for remote work. Specifically, we estimate:

\[
\text{Hires}_{2018-2019, MSA} = \beta_0 + \beta_1 P_{\text{Remote, MSA}} + f(N_{\text{Customer Service, MSA}}) + \epsilon_{\text{MSA}},
\]  

where we define the price of remote work as the gap between the entry-level wage rate for customer service in the MSA and the retailer’s uniform $14/hr remote wage. We also use Emsi data to flexibly control for the number of local customer service workers from which the retailer can draw. We weight this regression according to the number of customer service representatives in each MSA to put more weight on those places with larger available pools of customer service representatives.

Since there is considerable randomness in recruitment, it is important to put the number of hires on the left-hand side of this specification. In this formulation, the random idiosyncrasies in recruitment will not attenuate the coefficient on the price of remote work: other determinants of recruitment will only bias the estimated effect of pay if they systematically correlate with the implicit price of remote work, after conditioning on the size of the available pool of customer service workers.

On average, a customer service worker must pay $1/hr to accept the retailer’s $14/hr wage instead of a local on-site option. In MSAs where workers have to give up less to take this remote job instead of an on-site alternative, the retailer draws in more workers than one would expect given the size of the available pool of workers, as illustrated graphically in Figure 10. A $1/hr decrease in the implicit price of remote work translates into a 34% increase in recruit-

---

38 Our contact at the retailer described recruiters as “blind to the locations” when recruiting remote workers to the point that recruiters sometimes accidentally recruit workers from places where $14/hr falls below the local minimum wage.

39 As in Section 3, we approximate the entry pay in the MSA according to the average of the 25th and 50th percentiles of the local customer-service wage distribution. We define customer service workers as all those with occupation SOC 5240.

40 The unweighted analysis results in very similar qualitative and quantitative inferences about status quo social surplus and the gains from pricing remote work according to its marginal costs, as illustrated in Appendix Figure A.6.
ment on average (2.4 additional workers off of an average of 7 in our preferred specification in column three of Table 7). This implies a price elasticity of demand of 4.73, suggesting a fairly elastic demand for remote work, consistent with the heterogeneous preferences documented in Ma and Pallais (2017); Maestas et al. (2018). Our inferences are not sensitive to how we control for the size of the available pool of customer service workers, even though the $R^2$ improves from 13% with a linear control to 31% with a quartic control for the size of the available pool of customer service workers.

We can rearrange this to back out the implied demand for remote work. To find the slope of the demand curve, we take the reciprocal of the coefficient to express price as a function of quantity. To find the intercept, we use the retailer’s observed price and quantity of remote work to pin down the price at which the retailer would not recruit any remote workers. The intercept of this demand curve implies that the maximum price that workers are willing to pay to work remotely is 29% of their wages ($4/hr off a base of $14/hr). This is commensurate with the maximum willingness to pay for the option of working remotely elicited by Ma and Pallais (2017) among workers in a national call-center. Ma and Pallais (2017) find a long tail of workers with a high demand for remote work: thus, the maximum willingness to pay for remote work differs considerably from the 75th percentile of the distribution, where workers were only willing to give up 14% of their wages ($2.45/hr) for the option to work remotely.

7 Welfare Effects of the Under-Provision of Remote Work

We can leverage our estimated demand curve to characterize worker surplus from remote work in the market equilibrium and a counterfactual in which remote workers were not adversely selected. Figure 11 takes our theoretical model (illustrated in Figure 1) to our data on call-center workers.

In the market equilibrium, the average worker must give us $1/hr in wages to take a remote job at the retailer rather than a local on-site alternative, given by the orange line in Figure 11. Many workers’ willingness to pay for remote work exceeds even this relatively high price of working remotely, generating the worker surplus in the dark green triangle of Figure 11. Our estimates suggest total worker surplus of $644/hr for the retailer’s remote hires or $1.48/hr for each worker, representing 11% of the $14/hr wage. This significant surplus from the market for remote work parallels the surplus from other alternative work arrangements: while

---

41 Specifically, $\hat{D}_0 = P_{\text{remote at retailer}} - \delta\hat{\text{Realized Average Employment}}$ where we compute weighted averages of the price of remote work and the resulting employment, with weights given by the size of the pool of customer service workers in each MSA.

42 For this analysis, we re-scale our estimated demand curve so the x-axis reflects the total number of remote hires in the retailer rather than the number of hires in a specific MSA. For an unweighted analysis, we could simply multiply by the number of MSAs. For this weighted analysis, we instead scale by the effective number of MSAs such that multiplying the weighted average hires in an MSA by the number of effective MSAs results in the observed number of hires ($N_{\text{effective MSAs}} = \frac{\text{Total # Hires 2018-2020}}{\text{Weighted Avg # Hires 2018-2020}}$).
many workers may not value these alternatives, those who do can have high willingness to pay for these arrangements, generating substantial social surplus from their provision (Chen et al., 2019).

In the absence of adverse selection, the retailer would increase remote wages by about 12%, giving rise to the socially efficient price in the green line in Figure 11. At the resulting wage, this remote job would pay a premium over the on-site alternatives of the average customer service worker. Given our estimated demand curve, this would increase the number of remote hires in 2018 and 2019 from 435 to 682. This price change would imply 29% of the retailer’s hires would be remote instead of 19%. Social surplus would rise by $207/hr or 32.4% off the base of $644/hr.

Further the problem of adverse selection depresses the surplus of workers who choose to take the remote job at the equilibrium price (the rectangle below the dark green triangle and the dark green horizontal line in Figure 11). Even though this is solely a transfer in the context of the model, it may reflect a real social cost if those with have high willingness to pay for remote work have higher welfare weights than on-site workers or are difficult to transfer resources to through other means. Given our finding that remote work is a tag for lower productivity, standard models of optimal taxation would suggest that subsidizing remote work would be a more efficient means of transferring resources to remote workers than other perturbations of the tax system (Mirrlees, 1976; Atkinson and Stiglitz, 1976; Kaplow, 2006). Thus, there would be a public policy motive to subsidize remote work even if adverse selection did not also cause a market failure in its provision.44

In summary, our estimates suggest that the social surplus from remote work is substantial in the market equilibrium, at $1.48/hr per worker or 11% of the $14/hr wage. However, this surplus would rise by 32% in the absence of adverse selection. Further, the welfare of those

8 Conclusion

We presented a simple model of the market for remote work, where adverse selection drives a wedge between the efficient price of remote work — which solely reflects the treatment effect — and the market price of remote work — which reflects both the treatment and selection effects. We estimated the sufficient statistics of this model in the context of call-center workers at a Fortune 500 online retailer. We found that while remote work increased individual productivity, it worsened worker selection. As a result, workers had to pay to work remotely even though work arrangement improved individual productivity. Even at an inefficiently high price, remote workers benefited by $1.48/hr (or 11% of their wages) on average from

43This analysis limits to the time when the retailer hired remote workers, starting in July of 2018.
44These concerns might be especially acute given the prevalence of child-care responsibilities among remote workers, if policy makers worried that these losses would not be offset by other compensatory changes in the tax system.
working remotely. However, without the market failure of adverse selection, social surplus from the provision of remote work would rise by 32%. Our model implies that the productivity of workers who learned they preferred working remotely during the lockdown will be a crucial determinant of the future of remote work after the pandemic.

This suggests an exciting area of future work to investigate which workers changed their mind about remote work during the pandemic. Further, the dependence of some of our theoretical predictions on high order moments of selection and treatment effects suggests it would be informative to trace out the full relationship between preferences for remote work and productivity in both on-site and remote work — an endeavor we hope to pursue with this retailer.

We were limited in the observable characteristics that we could see about workers. While age, gender, and care-giving status were not predictive of on-the-job performance, other facets of workers might be predictive of their on-the-job performance and observable to hiring managers. Since many hires have limited prior experience and thus few signals of ability, we believe that much of the negative selection we document is in fact adverse selection on unobservable dimensions of productivity but we cannot verify this conjecture. In addition to assessing the extent to which selection is truly unobservable, additional information about the credentials of workers might give firms mechanisms for mitigating selection effects, by, for example, requiring remote hires to have long histories of on-site experience.

We have estimated the sufficient statistics of our model in the specific context of call-center work but have proposed a framework that could be applied in other settings. Painting a similar picture in other sectors of the economy would be a fruitful direction for future work. Understanding the interplay between remote work and teamwork would be a particularly important step forward.

It would also be informative to investigate how the opportunities of remote workers affects the selection into remote jobs. In keeping with others’ findings (Bloom et al., 2015), remote workers at the retailer tend to have flatter career ladders. If new remote hires anticipate short career ladders, then workers who don’t expect to advance may be the ones who accept remote offers: thus, negative selection may be endogenous to the opportunities afforded to remote workers. Future work that experimented with offering a steeper career ladder to remote workers would be useful to unpack how much differential promotion opportunities drives differential selection.

We found that firms like this retailer cannot easily side-step the adverse selection of remote workers by only offering remote work to those who were initially hired on-site. On its face, this suggests that other intermediate options that allow workers to work from home for part of their week might also run into selection problems. However, our finding that the negative selection effect appears to be driven by those with the strongest preferences for working re-
motely suggest that such a split-time approach might not lead to the same selection patterns that we document. If workers with the strongest preferences for remote work are not attracted to jobs that require them to spend part of each week in the office, then this middle ground might be a promising one for firms to consider and for researchers to analyze. If face-time is the key determinant of promotions, such a policy might also steepen the career ladders of those in partially remote positions, potentially further inducing more advantageous selection of workers.
REFERENCES


### Table 1: Summary Statistics from Representatives’ First 6 months at the Retailer in 2019

<table>
<thead>
<tr>
<th></th>
<th>All</th>
<th>Remote</th>
<th>On-Site</th>
<th>Δ</th>
<th>Δ_{err}</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Compensation and Outside Options</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Entry $/hr</td>
<td>15.02</td>
<td>14.05</td>
<td>15.26</td>
<td>-1.21***</td>
<td>(0.36)</td>
</tr>
<tr>
<td>Entry Customer Service $/hr in MSA</td>
<td>14.32</td>
<td>14.56</td>
<td>14.27</td>
<td>0.29</td>
<td>(0.47)</td>
</tr>
<tr>
<td>Location Q of CSR in MSA</td>
<td>1.01</td>
<td>1.21</td>
<td>0.96</td>
<td>0.25</td>
<td>(0.17)</td>
</tr>
<tr>
<td><strong>Demographics</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>% Female</td>
<td>74.08</td>
<td>87.82</td>
<td>70.87</td>
<td>16.95***</td>
<td>(2.77)</td>
</tr>
<tr>
<td>% Child care-giver</td>
<td>44.41</td>
<td>56.51</td>
<td>41.16</td>
<td>15.36***</td>
<td>(4.27)</td>
</tr>
<tr>
<td>Age</td>
<td>33.01</td>
<td>36.89</td>
<td>32.10</td>
<td>4.79***</td>
<td>(0.88)</td>
</tr>
<tr>
<td><strong>Monthly Turnover</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>% Turnover in Retailer</td>
<td>6.96</td>
<td>7.51</td>
<td>6.83</td>
<td>0.67</td>
<td>(0.98)</td>
</tr>
<tr>
<td>Turnover in CSR in MSA</td>
<td>6.35</td>
<td>7.03</td>
<td>6.19</td>
<td>0.84*</td>
<td>(0.46)</td>
</tr>
<tr>
<td><strong>Productivity</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Calls/Day</td>
<td>25.61</td>
<td>24.31</td>
<td>25.92</td>
<td>-1.62**</td>
<td>(0.80)</td>
</tr>
<tr>
<td>Queued Min</td>
<td>52.47</td>
<td>65.38</td>
<td>49.41</td>
<td>15.97***</td>
<td>(4.44)</td>
</tr>
<tr>
<td>Phone Min/Call</td>
<td>9.61</td>
<td>10.16</td>
<td>9.49</td>
<td>0.67***</td>
<td>(0.16)</td>
</tr>
<tr>
<td>On-Hold Min/Call</td>
<td>1.15</td>
<td>1.19</td>
<td>1.14</td>
<td>0.05</td>
<td>(0.05)</td>
</tr>
<tr>
<td>Customer Satisfaction Rating</td>
<td>4.89</td>
<td>4.89</td>
<td>4.89</td>
<td>0.00</td>
<td>(0.01)</td>
</tr>
<tr>
<td>% Resolve</td>
<td>75.62</td>
<td>74.52</td>
<td>75.89</td>
<td>-1.37*</td>
<td>(0.81)</td>
</tr>
<tr>
<td><strong>Hours and Absenteeism</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Scheduled Hours</td>
<td>8.03</td>
<td>7.98</td>
<td>8.04</td>
<td>-0.07**</td>
<td>(0.03)</td>
</tr>
<tr>
<td>Absent Minutes</td>
<td>45.81</td>
<td>43.41</td>
<td>46.39</td>
<td>-2.98</td>
<td>(3.76)</td>
</tr>
<tr>
<td>Absent Unapproved Minutes</td>
<td>28.35</td>
<td>25.07</td>
<td>29.14</td>
<td>-4.07**</td>
<td>(1.89)</td>
</tr>
<tr>
<td># Days</td>
<td>280458</td>
<td>53959</td>
<td>226499</td>
<td>280458</td>
<td></td>
</tr>
<tr>
<td># Employees</td>
<td>2224</td>
<td>467</td>
<td>1757</td>
<td>2224</td>
<td></td>
</tr>
</tbody>
</table>

*Note: Standard errors clustered at the MSA-level.  
*p<0.1; **p<0.05; ***p<0.01

We focus on hires in 2019 and 2020 when on-site representatives had no expectation of going remote. We focus on entry-level hires’ first 6 months at the retailer to assure uniformity in the nature of incoming calls.

Customer service workers often report child-care responsibilities in a company-wide survey in June 2020. Representatives answer about 26 calls per day or about one call every 20 minutes. Remote representatives answer nearly two fewer calls per day. This discrepancy reflects both the fact that spend longer in the queue but not actively on the phone and the longer duration of remote representatives’ calls. This extra time does not boost customer satisfaction, which is nearly 5 out of 5 across the board.

Representatives resolve most calls themselves without escalating the call to a more senior representative, with slightly higher resolution rates among on-site representatives.

Representatives average more than 45 minutes of absent time, most of which is unapproved. Working-from-home appears to ease juggling unexpected shocks at home.
Table 2: Average Differences Between Workers Hired to be Remote vs On-Site Before Covid-19

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Panel A: Log(Calls/Hr)</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Hired Remote</td>
<td>-0.089***</td>
<td>-0.102***</td>
<td>-0.108***</td>
</tr>
<tr>
<td></td>
<td>(0.0186)</td>
<td>(0.0216)</td>
<td>(0.0212)</td>
</tr>
<tr>
<td></td>
<td>$[R^2 = 0.271]$</td>
<td>$[R^2 = 0.413]$</td>
<td>$[R^2 = 0.468]$</td>
</tr>
<tr>
<td><strong>Panel B: Log(Satisfaction Reviews)</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Hired Remote</td>
<td>0.0015</td>
<td>0.0020</td>
<td>0.0024</td>
</tr>
<tr>
<td></td>
<td>(0.0014)</td>
<td>(0.0017)</td>
<td>(0.0017)</td>
</tr>
<tr>
<td></td>
<td>$[R^2 = 0.023]$</td>
<td>$[R^2 = 0.090]$</td>
<td>$[R^2 = 0.129]$</td>
</tr>
<tr>
<td><strong>Panel C: Log(Wages)</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Hired Remote</td>
<td>-0.053***</td>
<td>-0.052***</td>
<td>-0.049***</td>
</tr>
<tr>
<td></td>
<td>(0.0036)</td>
<td>(0.0042)</td>
<td>(0.0040)</td>
</tr>
<tr>
<td></td>
<td>$[R^2 = 0.574]$</td>
<td>$[R^2 = 0.644]$</td>
<td>$[R^2 = 0.708]$</td>
</tr>
</tbody>
</table>

# On-Site Workers  | 2621         | 2621         | 2621         |
# Remote Workers   | 442          | 442          | 442          |
# Workers          | 3063         | 3063         | 3063         |
# Days             | 195389       | 195389       | 195389       |
# Days with Reviews| 160140       | 160140       | 160140       |

*Note:* This table reports the logged difference in productivity and compensation between workers hired to be remote versus those hired to be on-site. This analysis limits to workers in their first six months of their tenure when workers all handle routine calls. Column one estimates specification 3 with date by time-zone fixed effects to assure workers were answering calls at comparable times. Column two interacts date and time-zone by hiring month to compare workers with comparable experience at the retailer. Column three further interacts this with job title time to net out any remaining differences in workers’ positions within the company. Standard errors are clustered at the worker-level. In brackets, the $R^2$ from each specification is reported to ease comparisons of coefficient stability with the change in the model’s explanatory power. Since satisfaction reviews were only left on 10% of calls, the number of days with reviews is lower than the total number of days on which workers took calls. Table A.1 decomposes the differences in calls per hour into minutes per call and the share of working minutes that workers spent on the phone.
Table 3: Event Study Around Individual Switches to Remote Work

<table>
<thead>
<tr>
<th></th>
<th>Calls/Hr 1 week</th>
<th>Calls/Hr 6 weeks</th>
<th>Satisfaction Reviews 1 week</th>
<th>Satisfaction Reviews 6 weeks</th>
<th>Unexcused Absent Min. 1 week</th>
<th>Unexcused Absent Min. 6 weeks</th>
</tr>
</thead>
<tbody>
<tr>
<td>Post</td>
<td>0.202**</td>
<td>0.223***</td>
<td>0.003</td>
<td>0.011</td>
<td>-4.312</td>
<td>-1.897</td>
</tr>
<tr>
<td></td>
<td>(0.084)</td>
<td>(0.068)</td>
<td>(0.027)</td>
<td>(0.011)</td>
<td>(2.638)</td>
<td>(1.493)</td>
</tr>
<tr>
<td>% Effect</td>
<td>6.92</td>
<td>7.45</td>
<td>0.06</td>
<td>0.22</td>
<td>-41.93</td>
<td>-18.95</td>
</tr>
<tr>
<td>Mean</td>
<td>2.93</td>
<td>3</td>
<td>4.9</td>
<td>4.89</td>
<td>10.28</td>
<td>10.01</td>
</tr>
<tr>
<td># Workers</td>
<td>121</td>
<td>121</td>
<td>121</td>
<td>121</td>
<td>121</td>
<td>121</td>
</tr>
<tr>
<td># Days</td>
<td>988</td>
<td>5813</td>
<td>765</td>
<td>4562</td>
<td>988</td>
<td>5813</td>
</tr>
</tbody>
</table>

Note: This table reports the change in productivity around workers’ switches from on-site to remote work during periods when the retailer posted remote job openings for on-site workers. The odd columns limit to one week on either side of the transition to remote work. The even columns consider the six weeks before and after the transition to remote work, as depicted graphically in the case of calls per hour in Figure 5. Standard errors are clustered at the individual level. The sample is limited to workers who switched to remote work while in entry- or mid-level roles and whose transition to remote work did not coincide with a promotion or departure from the retailer within the 6 week window.
Table 4: Difference-in-Difference of Forced Transition of On-site Workers to Remote Work

**Panel A: Covid-19 Difference-In-Difference**

<table>
<thead>
<tr>
<th></th>
<th>Calls/Hr</th>
<th>Satisfaction Reviews</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>3 weeks</td>
<td>6 weeks</td>
</tr>
<tr>
<td></td>
<td>3 weeks</td>
<td>6 weeks</td>
</tr>
<tr>
<td>Hired On-Site : Post Office Closure</td>
<td>0.318***</td>
<td>0.246**</td>
</tr>
<tr>
<td></td>
<td>(0.121)</td>
<td>(0.111)</td>
</tr>
<tr>
<td>Hired On-Site</td>
<td>0.201**</td>
<td>0.140</td>
</tr>
<tr>
<td></td>
<td>(0.100)</td>
<td>(0.104)</td>
</tr>
</tbody>
</table>

% Effect 10.05 7.64 -0.09 -0.53
Mean 3.16 3.22 4.89 4.89
# On-Site, Treated Workers 386 386 386 386
# Remote, Control Workers 95 95 95 95
# Workers 481 481 481 481
# Days 11620 21569 10225 16419

**Panel B: Previous Holiday Rush**

<table>
<thead>
<tr>
<th></th>
<th>Calls/Hr</th>
<th>Satisfaction Reviews</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>3 weeks</td>
<td>6 weeks</td>
</tr>
<tr>
<td></td>
<td>3 weeks</td>
<td>6 weeks</td>
</tr>
<tr>
<td>Hired On-Site : Holiday Rush</td>
<td>−0.189*</td>
<td>−0.142*</td>
</tr>
<tr>
<td></td>
<td>(0.104)</td>
<td>(0.083)</td>
</tr>
<tr>
<td>Hired On-Site</td>
<td>0.311**</td>
<td>0.387***</td>
</tr>
<tr>
<td></td>
<td>(0.137)</td>
<td>(0.122)</td>
</tr>
</tbody>
</table>

% Effect -5.4 -4.23 -0.11 -0.34
Mean 3.5 3.35 4.85 4.87
# On-Site Workers 287 287 287 287
# Remote Workers 62 62 62 62
# Workers 349 349 349 349
# Days 9338 15690 7771 13046

**Note:** Panel A presents the difference-in-difference of how the productivity of on-site workers changed relative to remote workers after the retailer closed its offices on April 6, 2020 due to Covid-19 (equation 8). Panel B instead considers the relative change in productivity of on-site and remote workers around the previous holiday rush that saw similar spikes in demand but did not feature a forced transition of on-site workers to remote work (equation 9). Both analyses limit to a balanced panel of workers who were employed at the retailer and still in their first six months in the six weeks before and after the focal event. The odd columns limit the comparison to the 3 weeks before and after the focal event while the even columns limit the comparison to the 6 weeks before and after the focal event. In the case of Covid-19, the three week bandwidth isolates the period after which most schools were closed. All specifications include date by time-zone by hiring month fixed effects as in equation 8 and cluster standard errors at the individual level.
Table 5: Productivity Differences Between Workers Hired to be Remote vs On-Site After Covid-19

<table>
<thead>
<tr>
<th></th>
<th>Log(Calls/Hr)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Hired Remote</td>
</tr>
<tr>
<td></td>
<td>−0.121***</td>
</tr>
<tr>
<td></td>
<td>(0.039)</td>
</tr>
<tr>
<td></td>
<td>−0.145***</td>
</tr>
<tr>
<td></td>
<td>(0.042)</td>
</tr>
<tr>
<td></td>
<td>−0.185***</td>
</tr>
<tr>
<td></td>
<td>(0.053)</td>
</tr>
<tr>
<td></td>
<td>−0.196***</td>
</tr>
<tr>
<td></td>
<td>(0.070)</td>
</tr>
<tr>
<td>Age in Decades vs Avg</td>
<td>0.040***</td>
</tr>
<tr>
<td></td>
<td>(0.011)</td>
</tr>
<tr>
<td></td>
<td>0.042***</td>
</tr>
<tr>
<td></td>
<td>(0.013)</td>
</tr>
<tr>
<td></td>
<td>0.040***</td>
</tr>
<tr>
<td></td>
<td>(0.014)</td>
</tr>
<tr>
<td>Child Care-giver</td>
<td>0.060*</td>
</tr>
<tr>
<td></td>
<td>(0.031)</td>
</tr>
<tr>
<td></td>
<td>0.059*</td>
</tr>
<tr>
<td></td>
<td>(0.034)</td>
</tr>
<tr>
<td>Hired Remote x Age</td>
<td>0.008</td>
</tr>
<tr>
<td></td>
<td>(0.029)</td>
</tr>
<tr>
<td></td>
<td>0.014</td>
</tr>
<tr>
<td></td>
<td>(0.040)</td>
</tr>
<tr>
<td>Hired Remote x Child Care-giver</td>
<td>0.009</td>
</tr>
<tr>
<td></td>
<td>(0.082)</td>
</tr>
</tbody>
</table>

# Remote Workers | 128 | 128 | 68  | 68  |
# On-Site Workers| 580 | 580 | 263 | 263 |
# Workers        | 708 | 708 | 331 | 331 |
# Days           | 26885 | 26885 | 14520 | 14520 |

Note: This table reports the logged difference in productivity between workers hired to be remote versus on-site after all workers are remote due to Covid-19. This analysis limits to workers hired before the office closures on April 6, 2020, who are in their first six months of their tenure. Each model estimates specification 10 with date by time-zone by hiring month fixed effects and clusters standard errors at the worker-level. The first column considers the unconditional signal of selecting into remote work; the second column conditions on worker age measured in decades versus the average so the coefficient on remote is interpretable as the signal for a worker of average age; the third column introduces care-giving status recorded in a June 2020 company wide survey; the fourth column interacts both age and care-giving status with being a remote hire.
Table 6: Remote vs On-Site Productivity Gap by Knowledge of Opportunities to go Remote at the Time of Hire

<table>
<thead>
<tr>
<th></th>
<th>Log(Calls/Hr)</th>
<th>Log(Satisfaction Reviews)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Remote x Hired ≥ 2018</td>
<td>-0.226***</td>
<td>-0.192***</td>
</tr>
<tr>
<td></td>
<td>(0.064)</td>
<td>(0.053)</td>
</tr>
<tr>
<td></td>
<td>0.003</td>
<td>0.007</td>
</tr>
<tr>
<td></td>
<td>(0.008)</td>
<td>(0.008)</td>
</tr>
<tr>
<td>Remote</td>
<td>0.070</td>
<td>0.076*</td>
</tr>
<tr>
<td></td>
<td>(0.054)</td>
<td>(0.043)</td>
</tr>
<tr>
<td></td>
<td>-0.001</td>
<td>-0.006</td>
</tr>
<tr>
<td></td>
<td>(0.007)</td>
<td>(0.007)</td>
</tr>
</tbody>
</table>

Note: This table reports the differences in logged calls per hour and logged satisfaction reviews as a function of whether or not an individual worker went remote and whether or not she knew about this opportunity at the time of hire. The retailer introduced opportunities to go remote at the beginning of 2018 to relieve crowding in some of its call-centers. In these call-centers, the opportunity to go remote was unexpected for existing hires but known at the time of hire for new cohorts. Each specification controls for fixed effects for the date of the call, the hiring call center, and the hiring cohort as in specification 11. The even columns also interact these controls with the worker’s current job-title to take out any differences in the pool of calls between those who go remote and those who stay on-site. The second row reports the average productivity difference between those who were remote and those who were on-site from the cohorts who did not know about opportunities to go remote at the time of hire. The first row reports the difference between this average productivity gap and the corresponding gap for cohorts who did know about opportunities to go remote at the time of hire. All standard errors are clustered at the individual level.
We consider how the number of retailer hires between 2018-2020 in a particular MSA relates to the implicit price that workers pay to work in this remote job instead of a local on-site alternative. In MSAs with more lucrative outside options, workers must implicitly pay more to take the fixed national wage of $14/hr for remote work at the retailer. We weight the regressions according to the size of the available pool of customer-service workers in the MSA and cluster our standard errors at the MSA-level. We find that in MSAs where workers must implicitly pay $1/hr more to work at this remote job, the retailer hires approximately 3 fewer workers offer a base of 9 workers. This implies a price elasticity of demand between 4.2 and 5.5 suggesting workers’ demand for remote work is quite elastic. Our inferences are stable across a variety of controls for the size of the pool of available workers even though more flexible controls more than double the explanatory power of the model.
Figure 1: Model of the Market for Remote Work

Notes: This figure presents a model of remote work for a set of workers with similar observable characteristics and a set of firms that offer comparable remote and on-site jobs. The y-axis gives the implicit price of remote work — the difference in the wage between the on-site and the remote job. The x-axis is the share working remotely. Individuals are organized according to their willingness to pay for remote work, leading to the downward sloping demand curve in yellow. In this model, a worker’s preference for remote work can also relate to her productivity, with the solid blue line depicting productivity in the on-site job and the solid orange line depicting productivity in the remote job. We have pictured the case where productivity is (a) inversely related to a worker’s preference for remote work and (b) remote work has a homogeneous, positive treatment effect on workers’ productivity, leading the marginal worker to always be more productive working remotely. The efficient solution would be to price remote work according to its treatment effect for the marginal worker. At that price a share $s_{efficient}$ would work remotely because this reflects the intersection of $\Delta MP$ in dark green and demand in yellow. The problem of adverse selection means that the firm must set wages according to differences in average productivity rather than differences in marginal productivity. At any given price, to find the average productivity of those in the remote job, we must integrate from left to right since remote work will draw workers from the left of the distribution who have higher willingness to pay for remote work than the given price. This integration results in the orange dotted line that lies everywhere below the marginal product when remote workers are adversely selected. By contrast, to find the average productivity of those in the on-site job, we must integrate from right to left because the on-site job will attract workers from the right of the distribution who have lower willingness to pay than the given price. This integration results in the dotted blue line, which lies everywhere above the marginal product of on-site workers when remote workers are adversely selected (and thus on-site workers are advantageously selected). The intersection of the difference in average productivity in red with demand in yellow will determine the market provision of remote work, denoted by $s^*$. In the status quo, worker surplus from remote work is given by the dark green triangle. However, selection means this surplus is smaller than it would optimally be: by distorting the price of remote work, adverse selection generates the red deadweight loss. In this depiction, we have assumed the marginal products are linear and the treatment effects of remote work are homogeneous, leading to a constant difference in average products and at most one intersection with the downward sloping demand curve. This is what we will assume when taking this model to the data but we also investigate under what conditions equilibrium uniqueness would not be assured.
Figure 2: Trajectory of calls per hour for remote and on-site workers

Figure 3: Note: The first panel plots the hourly call volumes of remote and on-site workers as a function of their time at the retailer. The second panel plots the minutes spent on each call. The x-axis plots the months in the retailer. The y-axis plots the hourly calls of on-site workers in blue circles and remote workers in orange triangles. This analysis omits the first month of formal training when workers do not handle calls independently and considers the period from July 2018 when the retailer began to hire remote workers to April 2020 when the retailer closed its offices due to Covid-19. These estimates are adjusted for date by time-zone fixed effects in order to absorb any shocks in consumer demand as in specification 4. The error ribbons reflect the 95% confidence interval with standard errors clustered at the worker level.
Figure 4: Productivity of Remote Workers as a Function of the Implicit Price of Remote Work

Note: This figures plots the productivity of remote workers versus on-site workers as a function of the implicit price that remote workers pay to work remotely. The x-axis plots this implicit price from highest to lowest, where the price is approximated according to the difference between the retailer’s uniform $14/hr wage and entry-level customer service pay in the worker’s local metropolitan statistical area. The y-axis is the logged difference in hourly calls between remote workers who have paid the given price to work remotely and on-site workers who were taking calls on the same day and in the same time-zone and who were hired in the same month. The coefficient is from a regression of logged calls per hour against relative pay of the retailer for this remote job relative to the local on-site alternatives as in specification 5. The error ribbons reflect 95% confidence intervals of the comparison between remote workers and their on-site counterparts. All standard errors are clustered at the individual level.
Figure 5: Event Study Around Individual Switches to Remote Work

Note: This figure depicts the change in calls per hour of workers who voluntarily switch from on-site to remote work when the retailer opened up remote slots for on-site workers. The x-axis plots event time in weeks from the switch to remote work. The y-axis plots the worker’s calls taken per hour. The error ribbon reflects a 95% confidence interval for the comparison of each interval to the week before the switch to remote work according to equation 6, with standard errors clustered at the individual level. The coefficient compares the full six weeks before and after individuals switch to remote work with the percentage effect in square brackets, as in equation 7. This analysis limits to the 121 entry- and mid-level workers who made a switch from on-site to remote work that was not proximate to a promotion or departure from the retailer. This analysis is consequently a balanced panel, in which workers handle calls from the same pool on either side of their transition to remote work.
Figure 6: Difference-in-Difference of Calls/Hr Around Forced Transition to Remote Work for On-Site Workers

Note: This figure plots the hourly calls of formerly on-site and already remote workers around the retailer’s office closures due to Covid-19 on April 6, 2020, highlighted here in the red vertical line. The x-axis plots the week. The y-axis plots the average hourly calls for those hired to be on-site in the solid circles and for those hired to be remote in the dashed triangles. This analysis limits to a balanced panel of those who were in their first six months at the retailer for the six weeks before and after the office closures. The coefficient in the top left is the difference in hourly calls between those hired to be on-site and those hired to be remote in the 6 weeks before the office closures, with fixed effects for date by time-zone by hiring month. The coefficient in the top right is this same difference in the 6 weeks after the office closures. The coefficient in the bottom right is the difference in these differences estimated according to equation 8. All standard errors are clustered at the individual level.
Figure 7: Distributions of Calls/Hr for Already Remote and Formerly On-Site Workers After Covid-19

Notes: This figure plots the distribution of calls per hour for workers who are hired into remote and on-site jobs after Covid-19 causes all workers to work remotely. The coefficient reflects the estimate from specification 10 that includes date by time-zone by hiring month fixed effects.
Figure 8: Remote vs On-Site Productivity Gap by Knowledge of Opportunities to go Remote at the Time of Hire

Note: This figure plots the productivity difference between remote and on-site workers as a function of the knowledge of opportunities to go remote at the time of hire in those call-centers where the retailer introduced opportunities to go remote in the beginning of 2018 to relieve over-crowding. The x-axis plots the month of hire. The y-axis plots the logged difference in calls handled per hour for remote and on-site workers among those hired in the corresponding month with controls for the date of call by the hiring call-center by the hiring month as detailed in equation 12. The vertical dashed line highlights the divide between those cohorts who did not know about opportunities to go remote to the left and those who did know about the opportunity to go remote on the right. The difference reported in the top left reports the logged difference in calls handled pooling across the three hiring cohorts in the pre-period when opportunities to go remote were unknown. The difference reported in the top right reports the same difference pooling across the three hiring cohorts in the post-period when opportunities to go remote were known at the time of hire. The double difference reports the difference in these differences. All standard errors are clustered at the individual-level. The error ribbon reflects the 95% confidence interval.
Figure 9: Phase-Out of Internal Recruiting for Remote Jobs and the Productivity Gap between Remote and On-Site Workers

Notes: This figure considers the phase-out of remote opportunities at the retailer’s on-site locations. The x-axis plots the season of hire of each cohort, defined in 6 months segments. The y-axis plots the percentage of on-site workers who switch to remote work in their first six months at the retailer in orange and the logged difference in calls/hr in blue. The logged difference in calls/hr is estimated according to equation 13, which includes date by time-zone by hired month fixed effects to absorb any shocks across space or time or gaps in experience due to differences in the timing of on-site and remote hires. The errorbars reflect 95% confidence intervals with standard errors clustered at the individual level. Comparisons between remote and on-site hires are only possible after the retailer started hiring workers directly into remote jobs in July of 2018. This analysis excludes the period after April 6, 2020 when on-site workers were working remotely.
Figure 10: Estimating the Demand for Remote Work

Notes: This figure depicts the relationship between the implicit price of remote work and the number of remote hires made by the retailer. The x-axis plots the implicit price of remote work given by the comparison between the retailer’s uniform $14/hr wage and the local on-site alternatives in the MSA. The y-axis plots the number of hires made by the retailer. Both sides of this relationship are residualized by a quartic in the number of customer service workers in the local MSA. Each point reflects a decile of the distribution of the price of remote work residualized by the number of local customer service workers: its height is then determined by the number of recruits versus what would be expected given the number of local customer service workers weighted by the size of that available pool of workers. The fit line and coefficient come from estimating equation 14.
Figure 11: Status Quo and Efficient Welfare from Remote Work

Notes: This figure evaluates the theoretical model in Figure 1 in our empirical context of call-center workers. The x-axis plots the number of remote hires between 2018 and 2019. The y-axis plots the implicit price of remote work, determined by the wage that workers must sacrifice to take this remote job instead of a local on-site option. The horizontal orange line reflects the current average price workers must pay to take this remote job, averaging across all MSAs weighted by the size of the available pool of customer service workers. This intersects the yellow demand curve at the realized 435 remote hires between July of 2018 and the end of 2019, who constitute 19% of the retailer’s hires during this period. The demand curve is found by pivoting the mapping between the implicit price of remote work and the number of hires at the retailer pictured in Figure 10. This demand curve implies that many workers’ willingness to pay for remote work exceeds the retailer’s equilibrium wage, generating $644/hr in worker surplus or $1.48/hr per worker (11% of the $14/hr wage). The green horizontal line reflects the price the retailer would charge for remote work if it ignored the selection into remote work, using a lower bound estimate of a 12% selection effect. This price would intersect the demand curve at 682 remote workers, which would constitute 29% of the retailer’s hires during this period. Since these new recruits would either prefer remote work or not disprefer it strongly enough to offset its positive effects on their productivity, social surplus would rise by $207/hr or 32.4%, reflected in the deadweight loss triangle in red.
This figure uses data from the ACS to plot the share of workers who reported working at home among those who were currently employed. The analysis uses the sample weights from the Census to compute the means in each year.
This figure plots the share of workers who have been promoted to mid- and upper-level roles on the y-axis as a function of their tenure at the retailer on the x-axis. The lighter curves that start rising early in workers’ tenures depict the share of workers promoted to mid-level roles, with on-site workers in blue circles and remote workers in orange triangles. The darker curves that start rising later in workers’ tenure show the percent of workers who have been promoted to upper-level roles, with on-site workers again in blue and remote workers in orange. The vertical dashed line highlights six months, which is our primary sample of interest because during this time remote and on-site workers are promoted at parallel workers and few workers have been promoted to senior roles where call volume becomes a less informative metric of productivity.
Table A.1: Decomposition of Productivity Differences

<table>
<thead>
<tr>
<th></th>
<th>Column (1)</th>
<th>Column (2)</th>
<th>Column (3)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Panel A: Log(Calls/Hr)</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Hired Remote</td>
<td>-0.077***</td>
<td>-0.103***</td>
<td>-0.109***</td>
</tr>
<tr>
<td></td>
<td>(0.0204)</td>
<td>(0.0230)</td>
<td>(0.0229)</td>
</tr>
<tr>
<td></td>
<td>([R^2 = 0.232])</td>
<td>([R^2 = 0.362])</td>
<td>([R^2 = 0.417])</td>
</tr>
<tr>
<td><strong>Panel B: Log(Min/Call)</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Hired Remote</td>
<td>0.0734***</td>
<td>0.0870***</td>
<td>0.0999***</td>
</tr>
<tr>
<td></td>
<td>(0.0205)</td>
<td>(0.0232)</td>
<td>(0.0225)</td>
</tr>
<tr>
<td></td>
<td>([R^2 = 0.051])</td>
<td>([R^2 = 0.121])</td>
<td>([R^2 = 0.172])</td>
</tr>
<tr>
<td><strong>Panel C: Log(Calling Min/Working Min)</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Hired Remote</td>
<td>-0.003</td>
<td>-0.016</td>
<td>-0.009</td>
</tr>
<tr>
<td></td>
<td>(0.0141)</td>
<td>(0.0167)</td>
<td>(0.0166)</td>
</tr>
<tr>
<td></td>
<td>([R^2 = 0.263])</td>
<td>([R^2 = 0.398])</td>
<td>([R^2 = 0.452])</td>
</tr>
</tbody>
</table>

# On-Site Workers | 1875 | 1875 | 1875 |
# Remote Workers   | 428  | 428  | 428  |
# Workers          | 2303 | 2303 | 2303 |
# Days             | 122767 | 122767 | 122767 |

Notes: This table decomposed the difference in call volumes between remote and on-site workers into the component coming from time spent on each call and total time spent on the phone. This table limits to the observations where detailed time-use data is available since the beginning of 2019. As in Table ??, the first column includes date by time-zone fixed effects to assure workers were answering calls at comparable times; the second column interacts with the month of hire; and the third column further interacts with job title. Standard errors are clustered at the worker-level. In brackets, the \(R^2\) from each specification is reported to ease comparisons of coefficient stability with the change in the model’s explanatory power.
Notes: This figure considers the distribution of productivity differences that would be expected solely from variation across MSAs in terms of daily calls in panel (a) and customer satisfaction reviews in panel (b). These distributions were constructed by drawing a random set of remote workers with replacement and comparing their productivity to those of other remote workers answering calls on the same day in the same time-zone but living in different MSAs. The solid black lines report the realized differences between remote and on-site workers.
Table A.2: Remote versus On-site Productivity Differences

<table>
<thead>
<tr>
<th></th>
<th>All Matches</th>
<th>Only $14/hr Call-Centers</th>
<th>≤ $1.33/hr Relative $</th>
</tr>
</thead>
<tbody>
<tr>
<td>Calls/Day</td>
<td>-1.883***</td>
<td>-2.032***</td>
<td>-2.968***</td>
</tr>
<tr>
<td></td>
<td>(0.461)</td>
<td>(0.610)</td>
<td>(0.713)</td>
</tr>
<tr>
<td>µ</td>
<td>25.12</td>
<td>25.45</td>
<td>25.72</td>
</tr>
<tr>
<td>Customer Satisfaction</td>
<td>0.006</td>
<td>0.006</td>
<td>0.016***</td>
</tr>
<tr>
<td></td>
<td>(0.004)</td>
<td>(0.005)</td>
<td>(0.005)</td>
</tr>
<tr>
<td>µ</td>
<td>4.88</td>
<td>4.88</td>
<td>4.88</td>
</tr>
<tr>
<td>% Resolve</td>
<td>-2.101***</td>
<td>-2.768***</td>
<td>-4.692***</td>
</tr>
<tr>
<td></td>
<td>(0.720)</td>
<td>(0.624)</td>
<td>(0.951)</td>
</tr>
<tr>
<td>µ</td>
<td>76.81</td>
<td>76.78</td>
<td>77.24</td>
</tr>
<tr>
<td>Absent Unapproved Min</td>
<td>-0.367</td>
<td>-0.161</td>
<td>0.909</td>
</tr>
<tr>
<td></td>
<td>(0.530)</td>
<td>(0.623)</td>
<td>(1.219)</td>
</tr>
<tr>
<td>µ</td>
<td>9.95</td>
<td>10.34</td>
<td>10.14</td>
</tr>
<tr>
<td>Relative $/hr</td>
<td>-2.113***</td>
<td>-1.685***</td>
<td>-0.201</td>
</tr>
<tr>
<td></td>
<td>(0.346)</td>
<td>(0.436)</td>
<td>(0.149)</td>
</tr>
<tr>
<td>µ</td>
<td>1.20</td>
<td>1.05</td>
<td>0.56</td>
</tr>
<tr>
<td># Remote Reps</td>
<td>351</td>
<td>265</td>
<td>111</td>
</tr>
<tr>
<td># On-Site Reps</td>
<td>1797</td>
<td>771</td>
<td>1420</td>
</tr>
<tr>
<td># Remote Days</td>
<td>22884</td>
<td>17623</td>
<td>6468</td>
</tr>
<tr>
<td># On-Site Days</td>
<td>107023</td>
<td>45911</td>
<td>70181</td>
</tr>
</tbody>
</table>

Notes: For each day a remote representative works, we match her to all the on-site representatives answering calls that day in the same time-zone, who were hired in the same season. When there are multiple matches to a remote representative, we down-weight the on-site representatives so there is effectively one on-site representative for each remote representative. We focus on representatives’ first six months at the retailer during which time representatives are in comparable roles answering routine calls of similar complexity. The first column considers the differences between remote and on-site representatives for all matches. The next column limits comparisons to the retailer’s on-site call-centers that pay $14/hr at entry — the same wage paid to all entry-level remote representatives. The final column limits our comparisons to representatives whose pay differs by no more than $1.33/hr. As documented in the last row, these restrictions limit the differences in relative pay across remote and on-site workers. Across all of these specifications, we find that remote workers answer significantly fewer calls with gaps ranging from 7.5% to 12% of the dependent mean. There is some limited evidence that remote workers are trading off lower quantity for higher quality. However, remote workers are much less likely to resolve calls themselves. If they are especially likely to transfer calls of difficult customers, satisfaction reviews may be misleading. Reliability is similar across remote and on-site workers, possibly because differences in selection offset the treatment effect of remote work in reducing absenteeism (as analyzed in Section 4).
Figure A.4: Event Studies with Placebo Comparisons

Notes: This figure plots the event studies of individuals switching from on-site to remote work together with two placebo comparisons. The x-axis plots event time in weeks from the time of the switch from on-site to remote work. The y-axis plots the average of the outcome of interest. The event studies of those who switch from on-site to remote work are in black. The light blue lines depict the changes for the new teammates of the switcher to see whether shocks to the demand faced by the switchers’ new teams could explain the changes in their productivity. The orange lines depict the changes for switchers between on-site teams or remote teams to assess the extent to which productivity changes for any transition across teams. These analyses limit to workers who are at the retailer for the full month before and after the focal switch and are not promoted in this time to achieve a balanced panel of workers taking comparable calls throughout.
Figure A.5: Placebo Difference-in-Difference of Calls/Hr Around Previous Holiday Rush

Note: This figure plots the hourly calls of those hired into on-site and remote jobs around the 2019 holiday rush, which peaked on December 1, 2019, highlighted here in the red vertical line. The x-axis plots the week. The y-axis plots the average hourly calls for those hired to be on-site in the solid circles and for those hired to be remote in the dashed triangles. This analysis limits to a balanced panel of those who were in their first six months at the retailer for the six weeks before and after December 1, 2019. The coefficient in the top left is the difference in hourly calls between those hired to be on-site and those hired to be remote in the 6 weeks before the holiday rush, with fixed effects for date by time-zone by hiring month. The coefficient in the top right is this same difference in the 6 weeks after the holiday rush. The coefficient in the bottom right is the difference in these differences estimated according to equation 9. All standard errors are clustered at the individual level.
Figure A.6: Status Quo and Efficient Welfare from Remote Work with Unweighted Demand

Notes: This figure replicates the analysis in 11 but does not weight by the number of customer service workers in an MSA when estimating the demand curve.