# Working Remotely? Selection, Treatment, and the Market for Remote Work $^{\dagger}$

By NATALIA EMANUEL AND EMMA HARRINGTON\*

How does remote work affect productivity and how productive are workers who choose remote jobs? We decompose these effects in a Fortune 500 firm. Before COVID-19, remote workers answered 12 percent fewer calls per hour than on-site workers. After offices closed, the productivity gap narrowed by 4 percent, and formerly on-site workers' call quality and promotion rates declined. Even with everyone remote, an 8 percent productivity gap persisted, indicating negative selection into remote jobs. A cost-benefit analysis indicates savings in reduced turnover and office rents could outweigh remote work's negative productivity impact but not the costs of attracting less productive workers. (JEL D22, J22, J24, J63, L84, M12, M54)

**B**efore the COVID-19 pandemic, less than a fifth of Americans worked remotely.<sup>1</sup> Even in seemingly remotable tasks like call center work, remote work was uncommon.<sup>2</sup> This rarity was surprising since many workers were willing to take pay cuts to work from home (Mas and Pallais 2017; He, Neumark, and Weng 2021; Maestas et al. 2023; Lewandowski, Lipowska, and Smoter 2024), and working remotely seemed to boost productivity in call centers (Bloom et al. 2015).<sup>3</sup> It would seem that call center firms could pay remote workers less to do more. So, were call

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<sup>\*</sup>Emanuel: The Federal Reserve Bank of New York (email: natalia@nataliaemanuel.com). Harrington: University of Virginia (email: emma.k.harrington4@gmail.com). Xavier Jaravel was coeditor for this article. We thank two anonymous referees for their thoughtful feedback. We also would like to thank Lisa Abraham, Alex Albright, Jenna Anders, Alexander Bartik, Nick Bloom, Steven Davis, Andrew Garin, Ruobing Han, David Harrington, Nathan Hendren, Edward Glaeser, Claudia Goldin, Matthew Kahn, Louis Kaplow, Lawrence Katz, Jeff Liebman, Amanda Pallais, Dev Patel, Ashesh Rambachan, Ljubica Ristovska, Hannah Shaffer, Jan Sauermann, Elie Tamer, and participants at Stanford's Remote Work Conference, Princeton's JIM Seminar, Harvard's Public Finance and Labor Economics Workshop, ifo's Digital Transformation and Innovation Seminar, the UEA conference, the ASSA conference, and the AREUEA conference for helpful comments. This project would not have been possible without the curiosity and commitment to research of our colleagues at the firms who shared data: Lauren and Trevor. This project received IRB exempt status from Harvard (IRB20-2094), Princeton (14577), and University of Iowa (202205534). The findings and conclusions expressed are solely those of the authors and do not reflect the opinions or policy of the organizations that supported this work, the Federal Reserve Bank of New York or the Federal Reserve System.

<sup>&</sup>lt;sup>1</sup>In the 2019 American Community Survey (ACS), 5.6 percent of workers reported working from home (Ruggles et al. 2022). In the 2019 American Time-Use Survey, 7.4 percent reported spending the entire workday of the survey at home (Flood et al. 2023a).

<sup>&</sup>lt;sup>2</sup>In the 2019 ACS, 6.8 percent of phone workers worked at home, using Mas and Pallais' (2017) occupational definition, and 12.4 percent of computer programmers did so.

<sup>&</sup>lt;sup>3</sup>In an experiment in a Chinese call center, Bloom et al. (2015) find that remote work increased productivity by 13 percent.

center firms making mistakes? Or were there other pieces to the puzzle of remote work's rarity in remotable jobs?

We analyze remote work's impacts in the American call centers of a Fortune 500 firm that hired both remote workers (N = 344) and on-site workers (N = 1,592) before COVID-19. Pre-pandemic, managers expressed reservations about remote workers' productivity. This intuition was borne out in the data: remote workers answered 12 percent fewer calls per hour than on-site workers, despite handling calls randomly routed from the same queue.

The source of the lower productivity, however, remained unclear. It's possible that, in our setting, remote work reduces productivity, and any worker would be less productive at home. Workers may struggle with low motivation and self-control problems out of the office, particularly under relatively modest incentive pay.

Yet it's also possible that less productive workers choose remote jobs. There are widespread concerns that remote work impedes workers' promotions (Barrero, Bloom, and Davis 2022), which have been borne out even in settings where workers are more productive when remote (Bloom et al. 2015). Particularly ambitious or effective workers may therefore shy away from remote jobs. The resulting adverse selection into remote work could trap firms in a prisoner's dilemma: all firms might be better off offering remote work, but any individual firm might not do so out of fear of attracting less productive workers.

We use the office closures brought on by COVID-19 to help differentiate between remote work's impacts on worker productivity and worker selection in our American call center context. If remote work reduces productivity, then transitioning to remote work will cause formerly on-site workers to be less productive, thereby narrowing the initial gap in productivity.<sup>4</sup> If, however, less productive workers choose remote jobs, then the gap in productivity will persist (or potentially grow) once everyone is remote.<sup>5</sup>

Empirically, we find that the productivity gap narrowed but did not disappear in the months following the office closures. When the offices closed, the hourly calls of formerly on-site workers fell by 4 percent relative to that of already-remote workers (p-value = 0.017) off of a base of 3.8 calls per hour. This relative drop in productivity arose both because formerly on-site workers spent less of their time on the phone and took longer to answer each call.<sup>6</sup> Yet even when everyone was remote, workers who had originally chosen to be remote continued to be 8 percent less productive than those who had originally chosen to be on-site (p-value = 0.0002). Together,

<sup>&</sup>lt;sup>4</sup>For formerly on-site workers, unexpectedly switching to a new working arrangement could also reduce productivity. However, our effects persist a year after the closures, suggesting that they do not reflect transitory disruptions. Furthermore, the retailer was familiar with managing remote workers, which likely minimized organizational adjustments.

<sup>&</sup>lt;sup>5</sup>A persistent gap could be due to other persistent factors, like accumulated skills. Yet pre-COVID remote and on-site workers had similar productivity increases with experience, suggesting that differential skill accumulation is not the main driver here.

<sup>&</sup>lt;sup>6</sup>A previous draft of the paper found a modest positive effect of remote work because we did not have data on workers' schedules. This version uses detailed scheduling data to define calls per hour in terms of hours that workers were scheduled to answer calls rather than doing other productive tasks for the firm (e.g., answering customers' instant messages). When the offices closed, formerly on-site workers saw a relative increase in their hours scheduled to answer calls. Thus, without accounting for scheduling, we would uncover a modest positive impact of remote work.

these results indicate a third of the initial productivity gap was due to the negative treatment effect of remote work, with the remaining two thirds due to the negative selection into remote work.

Our identification strategy relies on the parallel-trends assumption that remote and on-site hires were similarly affected by the shocks of the pandemic. Our results are robust to allowing for differential effects of the pandemic based on workers' demographics, parental responsibilities, and local geographic characteristics. In a placebo check, we find no similar differential changes in productivity around other time periods that saw fluctuations in consumer demand similar to those at the onset of the pandemic. In a complementary design, we also find similar productivity declines around voluntary transitions from on-site to remote work before the pandemic.

Remote work not only reduces the quantity but also the quality of calls. In surveys we conducted, workers mentioned that working remotely made it harder to quickly consult with coworkers. Consistent with this, the shift to remote work increased customer hold times by 11 percent for formerly on-site workers relative to already-remote workers (p-value = 0.028). Remote work also increased customer callback rates by 3 percent, suggesting that workers were less likely to fully answer customers' initial questions when remote (p-value = 0.045). These negative effects are driven by less experienced workers, who might either wait longer for advice when remote or forgo this advice and answer queries less completely.

We find that remote work undermines workers' career advancement. Remote work reduces time spent with managers and in training sessions. Before the pandemic, remote workers were promoted at less than half the rate of their on-site peers. Once the offices closed, this gap disappeared. Our estimate of remote work's promotion penalty is similar to that in Bloom et al.'s (2015) experiment in a Chinese travel agency, where remote work halved workers' promotion chances despite improving productivity. After the experiment at the travel agency ended, remote work unraveled, as it came to be seen as something that only unproductive workers would choose.

To understand the implications of our analyses for remote work's potential to unravel, we consider the costs and benefits of hiring remote workers for the firm that we study. Our calculations suggest that remote work's negative productivity effects would be outweighed by the savings in reduced office space and lower worker turnover.<sup>7</sup> Yet the firm would still hesitate to hire remote workers at similar wages as on-site workers due to concerns about attracting less productive workers into remote jobs. Using Lewandowski, Lipowska, and Smoter's (2024) estimates of workers' demand for remote work, we find that firms like the one we study employed 36 percent fewer remote workers due to concerns over negative selection. In addition to these selection concerns, our model suggests that the firm's misperceptions about remote work's costs may have increased the wage penalty for remote work and deterred 11–16 percent of workers from working remotely.

The pandemic may have mitigated the underprovision of remote jobs. First, this mass experiment with remote work may have changed which workers choose

 $<sup>^{7}</sup>$ When the offices were open, remote workers were less likely to quit than on-site workers, suggesting that remote work reduces turnover as in Bloom et al. (2015).

remote jobs. By reducing stigma around remote work, this mass experiment may have widened the range of workers who choose remote jobs and thereby alleviated firms' concerns about negative selection. Second, the mass experiment with remote work may have corrected firms' misperceptions about remote work's costs.<sup>8</sup> Consistent with these possibilities, the firm we study permanently shifted most call center workers to remote work. Nationally, twice as many workers expect to work remotely post-pandemic as did pre-pandemic (Barrero, Bloom, and Davis 2022).

Our paper makes three contributions to the literature. First, we provide new evidence on the treatment effect of remote work in the US context. We find that remote work takes a small toll on both call quantity and quality in a context where the firm had experience managing remote workers before the pandemic. Our findings land between the positive effects found in Bloom et al.'s (2015) experiment in a Chinese travel agency and the large –18 percent effect in Atkin, Schoar, and Shinde's (2022) field experiment in India with workers in six-week data entry roles.<sup>9</sup> Our findings are consistent with the small negative effects of remote work found in Dutcher's (2012) lab experiment with US undergraduates doing data entry tasks. Our suggestive evidence that remote work impedes communication is consistent with Battiston, Kirchmaier, and Vidal's (2021) study of emergency phone operators and Yang et al.'s (2022) study of software engineers at Microsoft.<sup>10</sup> Our findings that remote work reduces training and promotion rates are consistent with Emanuel, Harrington, and Pallais's (2023) study of software engineers and Bloom et al.'s (2015) promotion effects.

Second, we contribute to research on how selection can limit the provision of desirable amenities by studying selection into remote work.<sup>11</sup> Our evidence bolsters the suggestive evidence in Linos's (2018) analysis of the rollout of the remote work program at the US Patent Office. Linos (2018) finds remote workers were only less productive than on-site workers if they had been hired after the introduction of the remote work program, and thus could have chosen the jobs because they wanted to work remotely. We offer a more direct test of adverse selection using the pandemic office closures.

Finally, our analysis helps explain the puzzling rarity of remote work before the pandemic in remotable tasks. While workers had a high willingness to pay for remote work (Mas and Pallais 2017; He, Neumark, and Weng 2021; Maestas et al.

<sup>8</sup>Even people in managerial roles often reported that they were more productive in remote work than they had expected to be, suggesting organizational updating about remote work's productivity costs (Barrero, Bloom, and Davis 2022).

<sup>10</sup>Relatedly, time series analyses around COVID-19 show declines in productivity of software engineers (Gibbs, Mengel, and Siemroth 2023) and chess players (Künn, Seel, and Zegners 2022).

<sup>11</sup>For example, selection concerns may limit the provision of maternity leave (Tô 2018), workers' compensation (Cabral, Cui, and Dworsky 2022), unemployment insurance (Hendren 2017), and short hours (Landers, Rebitzer, and Taylor 1996; Anger 2008).

<sup>&</sup>lt;sup>9</sup>Researchers have also found positive productivity effects of other facets of flexibility over where to work. In an experiment with technology workers, Bloom, Han, and Liang (2024) found hybrid work reduced attrition without significantly reducing lines of code written. Choudhury et al. (2022) also found promising impacts of hybrid work on the depth and uniqueness of email exchanges in a Bangladeshi NGO. Relatedly, in an experiment in an Italian firm, Angelici and Profeta (2023) found that giving workers locational and temporal flexibility one day per week reduced absences and improved self-perceived productivity and well-being. Choudhury, Foroughi, and Larson (2021) found that giving remote workers flexibility over where to live improved productivity at the US Patent Office.

2023), firms have been loath to offer remote work (Barrero, Bloom, and Davis 2022; Lewandowski, Lipowska, and Smoter 2024). The negative selection that our paper documents offers one explanation and points to a different set of reasons why remote work may or may not stick in a post-pandemic world (Bartik et al. 2020; Morales-Arilla and Daboín 2021; Barrero, Bloom, and Davis 2022).

The rest of the paper proceeds as follows. Section I describes our empirical setting. Section II details how we use the office closures due to COVID-19 to separately identify remote work's impacts on worker productivity and worker selection. Section III presents empirical findings on treatment effects, while Section IV focuses on selection effects. Section V analyzes our findings' implications for firm decision-making and discusses implications for the post-pandemic world. Section VI concludes.

## I. Data and Setting

We study a Fortune 500 firm's call center workers. Our data include the daily call logs and daily schedules of these workers between January 2019 and October 2021.<sup>12</sup> Personnel data identifies whether workers were hired into remote or on-site jobs, their pay rates, and their job titles. We supplement these data with two surveys: the firm conducted a caregiving survey in June 2020, and we supplemented this survey in April 2021.<sup>13</sup>

*Timeline of the Firm's Remote Work Policies.*—The firm hired both remote and on-site call center workers prior to COVID-19 and went entirely remote due to the pandemic.<sup>14</sup> On March 15, 2020, the firm allowed on-site hires to work from home, and on April 6, 2020, the firm closed down its on-site call centers. On-site workers took their headsets and computers home with them, so they did the same job with the same equipment—now from home.

When the offices closed, the firm employed 1,965 call center workers—344 of whom were hired to work remotely and 1,592 of whom were hired to work on-site but now had to work from home. At that time, the firm also employed 229 workers who had been hired to be on-site but had received permission to go remote prior to the office closures. We include these workers in supplementary analyses.<sup>15</sup>

*Routing of Calls.*—The firm's call center workers handle incoming calls from customers. Most calls fall into three queues that vary in their complexity. Workers in the simplest queue of calls handle questions such as "When will my couch arrive?" Workers in the most complex queue handle calls such as "Only half my couch arrived—what should we do?!" Within each queue, calls are randomly routed to workers in the same queue, regardless of whether they are remote or on-site. We

<sup>&</sup>lt;sup>12</sup> Previous drafts also included data from 2018, but information on workers' schedules only becomes available in 2019.

<sup>&</sup>lt;sup>13</sup>Together, these surveys give us caregiving information for 56 percent of our sample.

<sup>&</sup>lt;sup>14</sup>The firm started to hire remote workers in July 2018, and so we limit our sample to workers who were recruited after July 2018.

<sup>&</sup>lt;sup>15</sup> First, we study their transitions to remote work. Second, we use them as an alternative control group whose working arrangement was not impacted by the office closures.

exclude workers who handle calls outside these queues for specialized products or specific customers.

Workers are almost always scheduled for 8-hour shifts from 9AM to 5PM local time (online Appendix Figure A1, panel A).<sup>16</sup> The firm covers service hours from 8AM to midnight eastern time by having call center workers spread across every time zone. We account for workers' time zones in our analyses.

*Call Logs.*—The firm's routing system tracks the number of calls that each worker handled. We focus on the number of calls that the worker handled herself, excluding calls transferred to another worker.<sup>17</sup> The firm's software also records the amount of time that each worker spent talking to customers and the amount of time that customers were kept waiting on hold.

*Scheduling Data.*—The firm tracks workers' daily schedule in 15-minute increments. Our primary outcome measure is calls handled per hour that the worker was scheduled to be on the phone. Crucially, in the denominator, we exclude time that the worker was scheduled to answer customers' emails or chat messages, attend meetings, go to training sessions, and do other productive tasks for the firm.<sup>18</sup>

*Call-Quality Metrics.*—The firm tracks three proxies of call quality: how long customers waited on hold, whether or not customers call back within two days (often indicating that the initial question went unanswered), and customers' ratings of the satisfaction with their calls from one to five stars. Reassuringly, callback rates and hold times are predictive of customer satisfaction scores: customers are less satisfied when their questions are incompletely answered, or they must wait longer to speak to a customer service representative.<sup>19</sup>

These quality metrics are imperfect. Customers rarely review calls (the participation rate is 11 percent) and, when they do, they tend to be polite (the mean review is 4.8 out of 5).<sup>20</sup> The challenges of monitoring quality have two implications. First, the firm does not pay piece rates and instead primarily bases annual compensation on hourly wages (which average 97 percent of annual compensation). As a result, workers have limited incentive to trade quality for quantity, suggesting call

<sup>16</sup>When the offices were open, on-site workers had marginally more absent time than remote workers (45 min. versus 40 min., *p*-value of difference = 0.085, online Appendix Figure A1, panel B). Once the offices closed, this gap became smaller and insignificant (73 min. versus 71 min., *p*-value of difference = 0.69). These patterns suggest that remote work reduces absenteeism, but the effect is small and insignificant.

 $^{17}$ We use she/her/hers pronouns since 73 percent of the workers identify as female.

<sup>18</sup> Pre-pandemic, the schedules of remote and on-site workers were indistinguishable (online Appendix Figure A2). During the pandemic, there was an uptick in customer emails and chat messages, and workers who were initially remote were slightly more likely to be rescheduled to answer these messages instead of answering calls. The scheduling data is consequently key for analyzing calls per hour. We show robustness to controlling for hours spent on calls to account for fatigue.

<sup>19</sup>On average, a standard deviation increase in callback rates (of 11 percentage points) is associated with a 0.013 standard-deviation reduction in satisfaction scores (*p*-value < 0.0001). A standard deviation increase in hold time (of 1.8 minutes) is associated with a 0.024 standard-deviation reduction in satisfaction scores (*p*-value < 0.0001). Callback rates and hold times are not significantly correlated with one another so are independently predictive of satisfaction scores.

<sup>20</sup>The audio of each call is recorded for quality assurance checks. However, managers have limited time to review calls and, thus, may fail to catch calls that go awry.

quantity may be a useful barometer of productivity.<sup>21</sup> Second, being on-site can impact managers' information about workers and the likelihood of promotion to higher-stakes roles.

*Promotions.*—When workers are promoted to handling more complex or specialized calls, their pay increases by \$2 per hour or 13 percent. Managers have considerable input into promotion decisions, since they recommend which (if any) workers to promote during performance reviews. Managers can base these recommendations on both quantitative metrics and subjective assessments of workers on their small teams (median team size = 8).

Remote and on-site workers are on different teams with different managers. Thus, remote workers do not directly compete for promotions with on-site workers and are not simply being overlooked for promotions in favor of on-site teammates. Nonetheless, remote workers had half the promotion rates as on-site workers prior to the pandemic, as investigated in Section IIIA.

*Turnover.*—Call center jobs feature high churn both at this firm and nationally. In the six months before the offices closed, fully 20 percent of workers left the firm, which is in the typical range for the industry (Reynolds 2015). Turnover is costly for the firm. New recruits spend their first week in formal training and then learn on the job, averaging 20 percent fewer calls per hour in their first month before quickly converging to the firm's average productivity (online Appendix Figure A3).<sup>22</sup> Pre-pandemic, on-site workers were more likely to leave the firm (online Appendix Table A4). This differential was driven by quits and not involuntary terminations and persisted unchanged when the offices closed.

*The Sample.*—Table 1 provides summary statistics on our primary sample.<sup>23</sup> The first column describes our full sample. The subsequent columns split workers based on whether they chose remote or on-site jobs and whether we observe them before or after the offices closed in April 2020.

*Productivity Differences.*—Before the pandemic, the firm's remote workers answered fewer calls than the firm's on-site workers in each hour that they were scheduled to answer calls (row 1 in columns 2–4 of Table 1). The gap in calls per hour increases to 12 percent when controlling for the queue of calls and worker demographics (online Appendix Table A1). The productivity differences between

<sup>&</sup>lt;sup>21</sup> As Goodhart's Law warns, a useful number can cease to be useful once it is a measure of success: thus, call quantity can be a useful measure of productivity that is nonetheless problematic to use as the basis of pay.

<sup>&</sup>lt;sup>22</sup> After the first month, there is little relationship between tenure and productivity. These apparently limited returns to tenure may be due to limited learning. Alternatively, workers' learning may be obscured, as more experienced workers are promoted to higher-stakes, more time-consuming calls. Yet offering remote work would not be a fruitful way to increase how many workers handle high-stakes calls, since the effect of reduced attrition is more than offset by remote workers' slower promotion rates (see Figure 2, panel C).

<sup>&</sup>lt;sup>23</sup> Our primary sample limits to workers hired between July 1, 2018—when the firm started hiring remote workers directly—and March 15, 2020—when on-site workers were allowed to work from home. We further exclude workers who were hired to be on-site but were permitted to transition to remote work pre-pandemic. We separately consider these workers in supplementary analyses. Throughout, we exclude workers who handle calls for specialized products or specific customers because these calls are not randomly assigned.

Worker traits 10. Firm tenure

11. % Female

13. % Parent

14. % Mother

12. Age

248.1

72.8

34.6

42.3

35.4

194.1

70.3

33.5

39.9

32.6

190.3

88.2

37.9

54.7

52.5

|                               |                | IADLE I              | SOMMA             | CI DIAIISI                                    | 105                  |                  |   |   |
|-------------------------------|----------------|----------------------|-------------------|---|----------------------|------------------|---|---|
|                               |                | Bet                  | fore the closures |   | Af                   | After the close  |   |   |
|                               | All<br>workers | Initially<br>on-site | Initially remote  | $\Delta_0$                                    | Initially<br>on-Site | Initially remote | $\Delta_1$                                    | $\Delta_1 - \Delta_0$                           |
| 1. Calls/scheduled hour       | 4.0            | 3.8                  | 3.4               | 0.39<br>(0.06)                                | 4.2                  | 4.0              | 0.22<br>(0.07)                                | -0.18<br>(0.06)                                 |
| Call quantity components      |                |                      |                   |   |                      |                  |   |   |
| 2. % On phone when schedule   | d 76.8         | 74.3                 | 71.8              | 2.53<br>(0.61)                                | 79.7                 | 79.4             | 0.27<br>(0.47)                                | -2.26<br>(0.57)                                 |
| 3. Min. per call              | 13.0           | 13.2                 | 14.3              | -1.08<br>(0.26)                               | 12.5                 | 13.3             | -0.78<br>(0.21)                               | $ \begin{array}{c} 0.30 \\ (0.22) \end{array} $ |
| Call quality metrics          |                |                      |                   |   |                      |                  |   |   |
| 4. Hold min. per call         | 1.2            | 1.1                  | 1.1               | $\begin{array}{c} 0.02 \\ (0.04) \end{array}$ | 1.3                  | 1.1              | $\begin{array}{c} 0.14 \\ (0.05) \end{array}$ | $0.12 \\ (0.05)$                                |
| 5. % Call back within two day | vs 14.1        | 15.9                 | 15.8              | 0.01<br>(0.19)                                | 12.5                 | 12.1             | 0.41<br>(0.17)                                | 0.40<br>(0.19)                                  |
| 6. Satisfaction rating        | 4.8            | 4.9                  | 4.9               | -0.00<br>(0.01)                               | 4.8                  | 4.8              | -0.00<br>(0.01)                               | -0.00<br>(0.01)                                 |
| Local traits                  |                |                      |                   |   |                      |                  |   |   |
| 7. Entry wage                 | 15.0           | 15.1                 | 14.0              | $1.14 \\ (0.03)$                              | 15.3                 | 14.0             | 1.26<br>(0.03)                                | $\begin{array}{c} 0.12 \\ (0.02) \end{array}$   |
| 8. MSA CSR wage               | 17.2           | 16.9                 | 17.3              | -0.35<br>(0.12)                               | 17.4                 | 17.5             | -0.18<br>(0.13)                               | 0.17<br>(0.09)                                  |
| 9. Covid cases per 10K        | 0.3            | 0.0                  | 0.0               | 0.00  | 0.5                  | 1.0              | -0.48   | -0.48   |

(0.00)

3.82

(9.61)

(2.42)

-4.48

(0.71)

(4.95)

(4.92)

-14.75

-19.90

-17.87

297.5

68.4

34.1

40.2

31.8

303.1

88.8

38.3

50.4

48.2

(0.04)

-5.62

(12.29)

-20.37

(2.56)

-4.17

(0.81)

(4.78)

(4.73)

-10.15

-16.36

(0.04)

-9.44

(7.52)

-2.50

(1.70)

0.31

(0.46)

4.60

(2.82)

3.55

(2.80)

| TABLE | 1— | SUMMARY  | <b>STATISTICS</b> |
|-------|----|----------|-------------------|
| TUDLL |    | 00mminut | 011101100         |

Notes: This table characterizes the firm's on-site and remote call center workers. The sample is limited to workers hired between July 1, 2018-when the firm started hiring remote workers-and March 15, 2020-when the firm let on-site workers start to work at home. The sample excludes workers who were hired to be on-site and then were permitted to transition to remote work before the pandemic, whom we analyze separately. The sample excludes workers who handle specialized calls for specific products or specific customers (like firms or non-English speakers), since these calls are not randomly assigned. Data on the mean wage in customer service (CSR) in the worker's metropolitan statistical area (MSA) comes from the Occupational Employment and Wage Statistics (OES) (Bureau of Labor Statistics, 2021b). Data on COVID-19 cases and deaths come from data compiled in New York Times (2021). Parenting information comes from a June 2020 survey conducted by the firm that we supplemented with our own survey in April 2021. Standard errors are clustered by worker.

remote and on-site workers are present when workers first start at the firm, suggesting that these gaps are not due to differential learning (online Appendix Figure A3).

Remote workers answered fewer calls because they spent less of their time on the phone (row 2) and answered each call more slowly (row 3). The differences in call quantity were not offset by differences in call quality, which were similar for remote and on-site, pre-pandemic (rows 4–6).

Once everyone worked remotely due to COVID-19, the gap in calls per hour narrowed but much of the gap persisted (row 1, columns 5-7). Sections II–IV make sense of these patterns and probe their robustness.

*Pay and Outside Options.*—On average, remote workers were paid \$1 less than on-site workers at the firm (row 7): all the firm's remote workers had entry pay of \$14 per hour, while some on-site locations had entry pay of \$16 per hour. Remote workers also had marginally better outside options. We use data on each worker's home address to characterize each worker's local labor market. Remote workers tend to live in metropolitan statistical areas (MSA) where the average customer-service worker earns 35 cents more per hour (row 8).<sup>24</sup>

While remote workers at the firm are paid less than on-site workers both in absolute and relative terms, these differences are comparable to the value that many workers place on working from home (Lewandowski, Lipowska, and Smoter 2024; Maestas et al. 2023; Mas and Pallais 2017). Thus, after adjusting for amenities, the remote and on-site jobs offered by the firm are similarly attractive. Further, our results are similar when limiting the sample to workers with the same wages and when controlling for geographic differences in where remote and on-site workers are drawn (online Appendix Tables 5, panels A1, A2, and A17).

*Worker Traits.*—Before the COVID-19 office closures, workers had been at the firm about six months on average (row 10). Nearly three-quarters of the firm's call center workers identify as female (row 11). The average age of workers is 35 (row 12). About 40 percent of workers report being parents in the caregiving surveys (row 13). Remote workers tend to be a few years older and are more likely to report being female and parents.

## **II. Empirical Framework**

This section uses the potential outcomes framework to illustrate how the office closures due to COVID-19 can separately identify remote work's impacts on worker productivity and worker selection.

Let  $Y_{i,j}$  denote the potential outcome of worker *i* in job *j*, which can be remote (j = r) or on-site (j = o). Let *R* denote the set of workers who choose remote jobs and *O*, the set of workers who choose on-site jobs.

A worker's potential outcome might differ in a remote and on-site job,  $Y_{i,r} \neq Y_{i,o}$  if, for example, worker *i* is more distracted by family at home (so  $Y_{i,r} < Y_{i,o}$ ) or coworkers in the office (so  $Y_{i,r} > Y_{i,o}$ ). The sets of workers who choose remote and on-site jobs might also differ in their potential outcomes if, for example, more

<sup>&</sup>lt;sup>24</sup> This gap in workers' alternatives is similar for adjacent occupations to customer service—such as bookkeeping and clerical tasks (online Appendix Table A3 for common occupational transitions). We characterize adjacent occupations using data on past occupations in the Current Population Survey (Flood et al. 2023b), as in Schubert, Stansbury, and Taska (2021). We then construct a more general measure of workers' outside options that weights each occupation by the likelihood of a transition between that occupation and customer service. We find a similar gap in outside options in this broader measure (\$17.29 per hour for remote workers versus \$16.93 per hour for on-site workers pre-pandemic). Given the similarity of these measures, we focus on the customer-service wage, but results are similar when we control for the broader measure of outside options.

productive workers are more deterred by remote work's promotion penalties or less diligent workers prefer to work in their pajamas at home (so  $E[Y_{i,j}|R] < E[Y_{i,j}|O]$ ). The productivity difference before the offices closed is given by

$$E[Y_{i,r}|i \in R] - E[Y_{i,o}|i \in O].$$

The challenge is that we observe different potential outcomes for different sets of workers.<sup>25</sup> Thus, the productivity difference combines differences in worker selection (*R* versus *O*) with differences in treatment ( $Y_{i,r}$  versus  $Y_{i,o}$  for each worker):

$$E[Y_{i,r}|i \in R] - E[Y_{i,o}|i \in O] = \underbrace{\left(E[Y_{i,r}|i \in R] - E[Y_{i,r}|i \in O]\right)}_{\text{Selection}} + \underbrace{\left(E[Y_{i,r}|i \in O] - E[Y_{i,o}|i \in O]\right)}_{\text{Treatment}}.$$

Remote workers might be less productive than on-site workers because the treatment effect caused them to be less productive. If so, on-site hires would be as unproductive at home as remote hires  $(E[Y_{i,r}|i \in O] - E[Y_{i,o}|i \in O] < 0)$ . Alternatively, remote work could select for less productive workers. If so, workers who chose to be remote would be less productive than workers who chose to be on-site even if all workers were working at home  $(E[Y_{i,r}|i \in R] - E[Y_{i,r}|i \in O] < 0)$ .

Without a shock to work arrangements, we could not disentangle treatment from selection because we would never observe the potential outcome of workers who chose to be on-site instead working remotely  $(E[Y_{i,r}|i \in O])$ . The office closures of COVID-19 reveal this missing potential outcome.

# A. The Treatment Effect of Remote Work

When the offices closed due to COVID-19, on-site workers transitioned to remote work but were also impacted by the pandemic. Indexing potential outcomes by time t and letting  $t_0$  denote the pre-pandemic period and  $t_{+1}$  denote the lockdown:

$$E[Y_{i,r,t_{+1}} - Y_{i,o,t_0}|i \in O] = \underbrace{E[Y_{i,r,t_0} - Y_{i,o,t_0}|i \in O]}_{\text{Treatment Effect}} + \underbrace{E[Y_{i,r,t_{+1}} - Y_{i,r,t_0}|i \in O]}_{\text{Pandemic Effect}},$$

where the pandemic effect captures shocks to both workers and to consumers, who may call in at different rates (and with different courtesy) during the pandemic.

<sup>&</sup>lt;sup>25</sup> This is a canonical challenge in markets for credit (Karlan and Zinman 2009), health insurance (Einav, Finkelstein, and Cullen 2010), and labor (Lazear 2000), where contracts can have causal effects on behavior and contracts can differ in who selects into them.

Workers who were already working remotely were affected only by the pandemic, not by the office closure. We use these workers as a control group to net out the pandemic's effect in a difference-in-differences design:

(1) 
$$E[Y_{i,r,t_{+1}} - Y_{i,o,t_{0}}|i \in O] - E[Y_{i,r,t_{+1}} - Y_{i,r,t_{0}}|i \in R]$$
$$= \underbrace{E[Y_{i,r,t_{0}} - Y_{o,r,t_{0}}|i \in O]}_{\text{Treatment Effect}} + \underbrace{E[Y_{i,r,t_{+1}} - Y_{i,r,t_{0}}|i \in O]}_{\text{Pandemic Effect + }i \in O}$$
$$- \underbrace{E[Y_{i,r,t_{+1}} - Y_{i,r,t_{0}}|i \in R]}_{\text{Pandemic Effect + }i \in R} \right).$$

This design identifies the treatment effect of remote work under the parallel-trends assumption that workers who chose to be on-site face similar pandemic shocks as those who chose to be remote. We probe this assumption in a few ways. First, we show robustness to controls described in Section IIC. Second, in a placebo check, we do not find similar differential changes in productivity in other periods. Third, we do not find any differential trends in productivity between remote and on-site hires leading up to the closures, nor any differential changes in the likelihood of departing the firm, particularly due to personal reasons like family sickness (online Appendix Table A4). Finally, we find similar results using an event study around voluntary transitions to remote work that occurred before the pandemic.<sup>26</sup>

# B. Selection Effect of Remote Work

During the COVID-19 office closures, all workers were remote, allowing us to observe the same potential outcome for workers, regardless of their initially chosen job. Thus, to assess the selection effect of remote work, we can simply compare the productivity of workers who originally chose remote jobs and workers who originally chose on-site jobs:

(2) 
$$E[Y_{i,r,t_{+1}}|i \in R] - E[Y_{i,r,t_{+1}}|i \in O].$$

For this comparison to isolate remote work's impact on worker selection, workers who initially chose remote and on-site jobs must face similar pandemic shocks. Further, other potential determinants of worker selection—such as the attractiveness of the posted job or the conditions in their labor market—must be as good as constant. To probe these assumptions, we consider robustness to controls described in Section IIC. Further, we consider a placebo check that tests whether differences in worker selection persist among workers hired when the offices were closed due to COVID-19. During the pandemic, the firm continued to advertise on-site jobs that would require a return to in-person work once it was safe to do so. This promise lost

 $<sup>^{26}</sup>$  We also find similar estimates when we use these pre-COVID switchers as an alternative control group for our difference-in-differences design.

teeth as the pandemic dragged out. Consistent with the differences in selection being due to on-site versus remote work, we find that differences in selection dissipate over the course of the pandemic (Section IV).

## C. Estimating Equations

Our estimating equation for remote work's treatment effect is the empirical analogue of equation (1):

(3) 
$$\frac{Calls}{Hour_{i,t}} = \beta \text{ Initially } On-Site_i \times Post_t + \psi \text{ Initially } On-Site_i + \varrho Post_t + X'_{i,t}\kappa + \epsilon_{i,t},$$

and our estimating equation for the selection effect of remote work is the empirical analogue of equation (2) estimated during the closure-period:

(4) 
$$\frac{Calls}{Hour_{i,t}} = \theta Initially Remote_i + X'_{i,t}\alpha + u_{i,t}$$

Observations are at the worker-day level, with standard errors clustered by worker. Our primary sample limits to a six-month bandwidth around the office closures, excluding the three weeks from March 15 to April 6, 2020, when on-site hires could work from home but did not yet have to do so.

The controls in  $X_{i,t}$  relax the identifying assumption that remote and on-site hires faced similar pandemic shocks. Our preferred set of controls include call queue fixed effects and demographic controls. Call queue fixed effects control for the day of the call interacted with the worker's time zone and call type (routine, standard, or complex). Demographic controls allow workers of different ages and genders to face different pandemic shocks, by interacting a worker's gender and age with the post-period indicator (*Post*<sub>t</sub>). When estimating the treatment effect in equation (3), our preferred specification also includes worker fixed effects.

We consider robustness to including additional demographic and geographic controls. We control for local COVID-19 case counts, unemployment rates, and wages in other local call center jobs. We further allow for differential pandemic shocks for mothers and fathers in the subsample of workers who responded to one of the caregiver surveys. We test whether we arrive at similar conclusions in the subsample of workers with \$14/hour entry wages. We finally consider the inclusion of fixed effects for hours scheduled for various tasks to account for fatigue.

#### III. Results: The Treatment Effect of Remote Work

Our difference-in-differences design leveraging the COVID-19 office closures compares the change in productivity of formerly on-site workers who went remote to the change in productivity of already-remote workers.

Once on-site hires started to work remotely due to COVID-19, their productivity declined relative to that of already-remote workers. Figure 1, panel A plots the



Panel A. Raw averages of calls per hour





FIGURE 1. DIFFERENCE-IN-DIFFERENCES AROUND COVID-19 OFFICE CLOSURES

*Note:* This figure illustrates the difference-in-differences in calls taken per hour between on-site workers who went remote during the COVID-19 office closures (Observations: 1,592) and remote workers who were already working from home (Observations: 344). Panel A plots three-week averages for remote and on-site workers matched on age, gender, and call queue. Panel B plots conditional gaps relative to February 16 to March 7, 2020, using our preferred set of controls for worker fixed effects, call queue fixed effects, and time-varying effects of worker demographics (see Section IIC). The annotated coefficient indicates the difference-in-differences estimate of the effect of going remote from equation (3), with a six-month bandwidth excluding the gray shaded region, which spans from March 15, 2020—when on-site workers could start working remotely—to April 6, 2020—when the offices fully closed. Calls per hour is computed as the ratio of the number of calls answered over the number of hours scheduled for answering calls. The sample is our primary sample summarized in footnote 23. Ribbons reflect 95 percent confidence intervals. Standard errors are clustered by worker.

unconditional average volume of calls per hour. Initially, there is a sizable gap in productivity between remote and on-site hires, which narrows once on-site hires also work at home. Figure 1, panel B illustrates the conditional differences, using our preferred controls (see Section IIC).

Our difference-in-differences estimate indicates that working remotely decreased productivity by 0.15 calls per hour or 3.9 percent (p-value = 0.017, column 4 of

|   |                                  | Calls per hour                   |                                  |   |   |   |  |  |
|---|----------------------------------|----------------------------------|----------------------------------|---|---|---|--|--|
|   | (1)                              | (2)                              | (3)                              | (4)   | (5)   | (6)   |  |  |
| Initially on-site $\times$ post                                       | -0.19<br>(0.07)                  | -0.14<br>(0.07)                  | -0.16<br>(0.08)                  | -0.15<br>(0.06)   | -0.15<br>(0.06)                               | -0.21<br>(0.08)                               |  |  |
| Initially on-site   | 0.39<br>(0.06)                   | 0.45<br>(0.06)                   | $0.45 \\ (0.08)$                 |   |   |   |  |  |
| Post  | 0.79<br>(0.06)                   |                                  |                                  |   |   |   |  |  |
| County covid cases/10K  |                                  |                                  |                                  |   | $\begin{array}{c} 0.02 \\ (0.01) \end{array}$ | $\begin{array}{c} 0.01 \\ (0.02) \end{array}$ |  |  |
| Mother $\times$ post  |                                  |                                  |                                  |   |   | -0.04<br>(0.06)                               |  |  |
| Father $\times$ post  |                                  |                                  |                                  |   |   | -0.14<br>(0.13)                               |  |  |
| Pre-dependent mean on-site Initially on-site $\times$ post in %       | 3.8<br>-5.1%<br>(1.80)           | 3.8<br>-3.6%<br>(1.80)           | 3.8<br>-4.1%<br>(2.20)           | 3.8<br>-3.9%<br>(1.60)  | 3.8 - 3.9%<br>(1.60)                          | 3.8<br>-5.5%<br>(2.00)                        |  |  |
| Age × gender × post FE<br>Call queue FE<br>Worker FE                  |                                  | $\checkmark$                     | $\checkmark$                     | $\begin{array}{c} \checkmark \\ \checkmark \\ \checkmark \end{array}$ | $\checkmark \\ \checkmark \\ \checkmark$      | $\checkmark \\ \checkmark \\ \checkmark$      |  |  |
| # Workers<br># Initially on-site<br># Already remote<br># Worker days | 1,965<br>1,621<br>344<br>224,447 | 1,965<br>1,621<br>344<br>224,447 | 1,965<br>1,621<br>344<br>224,447 | 1,965<br>1,621<br>344<br>224,447                                      | 1,965<br>1,621<br>344<br>224,447              | 840<br>678<br>162<br>126,603                  |  |  |
| Rž  | 0.05                             | 0.08                             | 0.17                             | 0.44  | 0.44  | 0.45  |  |  |

| TABLE 2—TREATMENT EFFECT OF REMOTE WORK ON PRODUCTIVITY: DIFFERENCE-IN-DIFFERENCES AROUND |
|---|
| COVID-19 OFFICE CLOSURES  |

*Notes:* This table presents a difference-in-differences design that compares the change in productivity of on-site workers who went remote during the COVID-19 office closures to that of already-remote workers. The dependent variable is calls answered per hour that the worker is scheduled to answer customers' calls. Each specification estimates equation (3) in a six-month bandwidth excluding the period from March 15, 2020—when on-site workers could work from home—to April 6, 2020—when remote work was required. Online Appendix Table A.5 includes the full period and defines the post date as March 15, 2020. The call queue fixed effects specify the date, time zone, and call type. Covid-19 cases come from *New York Times* (2021). Parenting characteristics in the fifth column come from a caregiving survey that the firm fielded in June of 2020 and that we supplemented in April of 2021. Standard errors are clustered by worker.

Table 2). The effects of remote work are persistent: our estimates are similar with a post-period of 1 to 12 months (online Appendix Figure A4).

The control group of already-remote workers is pivotal for making accurate inferences about remote work's causal effect. During the pandemic, many consumers switched from brick-and-mortar shopping to online retail, increasing the volume of calls to the firm's service lines. This uptick caused all workers to handle more calls per hour. Only by comparing the productivity of on-site hires to that of already-remote workers can we see the relative decline in on-site hires' productivity when they started to work from home. We do not think the uptick in customer calls itself led to differential changes in productivity: in a placebo check, we find no significant effect for any month other than the treated ones (online Appendix Figure A5), despite similar upticks in customer call volumes during the previous holiday season.

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*Robustness.*—We show robustness of these results to alternative specifications. We find consistent results when including a variety of controls (Table 2), a stability that is notable given the increase in the variation explained  $(R^2)$  from 5 to 45 percent. The results are also robust to including the omitted period around the office closures when on-site workers could choose whether to work from home (online Appendix Table A5). We find no significant differences in pre-trends prior to the office closures (*p*-value = 0.38 in a Wald test of the joint significance of the pre-period gaps). Our estimates are similar if we consider only call centers with entry pay of \$14 per hour (online Appendix Table A6, online Appendix Figure A6), if we control for the hours scheduled for calls or other tasks (online Appendix Table A7), or if we control for additional geographic traits such as the unemployment rate (online Appendix Table A8). Finally, we consider an alternative control group composed of the workers who were permitted to go remote pre-COVID and find a similar decline in calls per hour of 2.8 to 5.3 percent (online Appendix Table A9).

The Sources of Productivity Changes.—On-site hires answered fewer calls after going remote both because they spent relatively less time on the phone and because they answered each call more slowly. Prior to the pandemic, on-site hires spent three-quarters of their scheduled calling time actually on the phone. Once the offices closed and on-site hires started to work remotely, they spent 2 percentage points (or 2.7 percent) less time on the phone (*p*-value = 0.0002, column 1 of Table 3 and online Appendix Figure A7, panel A). In addition to spending less time on the phone, on-site hires took 0.37 minutes longer to answer customers' questions once they were remote, an increase of 2.8 percent relative to their pre-period mean (*p*-value = 0.093, column 2 of Table 3, panel A and online Appendix Figure A7, panel B).<sup>27</sup> These effects are similar for more and less experienced workers at the firm (Table 3, panel B).

*Call Quality.*—In addition to reducing the quantity of calls, remote work reduced their quality. Once on-site hires started to work from home, they kept customers waiting on hold for longer, increasing customers' hold time by 0.12 minutes per call or 10.6 percent (*p*-value = 0.028, column 3 of Table 3, panel A and online Appendix Figure A8, panel A). The increase in hold times is driven by workers who were in their first six months at the firm when the offices closed, who increase hold times by 24.2 percent when they go remote (*p*-value = 0.0003, column 3 of Table 3, panel B and online Appendix Figure A8, panel B).<sup>28</sup>

<sup>&</sup>lt;sup>27</sup> In Bloom et al.'s (2015) experiment, remote work's productivity advantages primarily came from workers spending more time on the phone although call speeds also became marginally faster.

<sup>&</sup>lt;sup>28</sup> Once on-site hires were remote, they were also more likely to keep customers waiting on hold for more than two minutes. Hold times in excess of two minutes increased by 4.28 percentage points (*p*-value = 0.004, online Appendix Figure A9). This increase is also driven by less experienced workers: those in their first six months when the offices closed became 9.1 percentage points more likely to keep customers on hold for more than two minutes (*p*-value = 0.000087).

|   | Decom                 | position                |                                 | Call quality                  |   |  |  |
|---|-----------------------|-------------------------|---------------------------------|-------------------------------|---|--|--|
|   | % On<br>phone<br>(1)  | Min.<br>per call<br>(2) | Hold<br>min. per<br>call<br>(3) | % Call back<br>(2 day)<br>(4) | Satisfaction<br>rating<br>(5)                   | Call without<br>call back per<br>hour<br>(6) |  |
| Panel A. Difference-in-differences arou   | und Covid             | 19 office c             | losures                         |                               |   |  |  |
| Initially on-site $\times$ post   | -1.99<br>(0.54)       | 0.37<br>(0.22)          | $0.12 \\ (0.05)$                | 0.40<br>(0.20)                | -0.002<br>(0.01)                                | -0.13<br>(0.05)                              |  |
| $R^2$<br>Pre mean on-site<br>Initially on-site × post in %                          | 0.63<br>74.3<br>-2.7% | 0.38<br>13.2<br>2.8%    | 0.18<br>1.1<br>10.6%            | 0.13<br>15.8<br>2.5%          | $0.09 \\ 4.9 \\ -0.03\%$                        | 0.42<br>3.2<br>-4%                           |  |
|   | (0.7)                 | (1.7)                   | (4.8)                           | (1.3)                         | (0.20)  | (1.7)  |  |
| Panel B. Heterogeneity by tenure  |                       |                         |                                 |                               |   |  |  |
| Low tenure $\times$ initially on-site $\times$ post                                 | -2.68<br>(0.64)       | 0.45<br>(0.32)          | 0.29<br>(0.08)                  | 0.86<br>(0.31)                | -0.01<br>(0.01)                                 | -0.13<br>(0.07)                              |  |
| High tenure $\times$ initially on-site $\times$ post                                | $-1.36 \\ (0.78)$     | 0.25<br>(0.30)          | $-0.04 \\ (0.06)$               | $-0.03 \\ (0.25)$             | $0.01 \\ (0.01)$                                | -0.11<br>(0.07)                              |  |
| $R^2$   | 0.63                  | 0.38                    | 0.18                            | 0.13                          | 0.09  | 0.42   |  |
| Pre mean on-site, low tenure<br>Pre mean on-site, high tenure<br>Percentage effects | 71.7<br>76.1          | 12.9<br>13.4            | 1.2<br>1.1                      | 16.1<br>15.7                  | 4.9<br>4.9                                      | 3.2<br>3.2                                   |  |
| Low tenure $\times$ initially on-site $\times$ post                                 | -3.7%<br>(0.9)        | 3.5%<br>(2.5)           | 24.2%<br>(6.7)                  | 5.3%<br>(1.9)                 | $\begin{array}{c} -0.17\% \ (0.30) \end{array}$ | -4.12%<br>(2.30)                             |  |
| High tenure $\times$ initially on-site $\times$ post                                | -1.8% (1.0)           | 1.8%<br>(2.2)           | -3.8%<br>(5.9)                  | -0.2%<br>(1.6)                | 0.12%<br>(0.30)                                 | -3.45%<br>(2.20)                             |  |
| Preferred controls<br># Workers<br># Initially on-site                              | √<br>1,965<br>1,621   | √<br>1,965<br>1,621     | √<br>1,965<br>1,621             | √<br>1,965<br>1,621           | √<br>1,954<br>1,610                             | √<br>1,965<br>1,621                          |  |
| # Already remote<br># Worker days   | 344<br>216,671        | 344<br>216,671          | 344<br>216,671                  | 344<br>224,447                | 344<br>189,285                                  | 344<br>224,447                               |  |

TABLE 3—TREATMENT EFFECT OF REMOTE WORK ON CALL QUALITY

*Notes:* This table presents difference-in-differences designs that compare the change in productivity metrics of on-site workers who went remote during the COVID-19 office closures to that of already-remote workers. Panel A shows this for all workers. Panel B shows this separately for workers with low and high tenure, where we split by the median tenure of six months before the offices closed. Using a continuous measure of tenure yields similar hetero-geneity (online Appendix Table A.11). Each column estimates the preferred specification in column 4 of Table 2. Columns 1–2 decompose the change in call volumes into (1) the percent of workers' scheduled call time that they spend on the phone and (2) the average duration of each call in minutes. Columns 3–5 consider three metrics of call quality: (3) minutes that customers are kept waiting on hold; (4) the rate at which customers call back to the service line within two days, likely with unanswered questions; and (5) average customer satisfaction scores on a five-point scale. Column 6 considers a composite measure that captures the number of customer calls that do not lead to a call back that the worker answers each hour. Standard errors are clustered by worker. Data on call time and hold time is missing for 3.5 percent of observations. Satisfaction ratings are missing for 15.7 percent of worker-days because none of the worker's customers filled out the rating form. Results for the other outcomes are similar when limiting to these subsamples (online Appendix Table A.10).

When asked about remote work's impact in our survey, many workers noted challenges communicating with colleagues.<sup>29</sup> One respondent said her biggest challenge was "people not answering you in chat and managers not being readily available." Another said she missed "having neighbors to turn to for assistance."

<sup>29</sup>Specifically, we asked: "If you would like to share any challenges that you have faced when working from home during the pandemic, we would love to hear them."

Our empirical results suggest that some inexperienced workers wait longer for their colleagues' digital input once they are remote and consequently keep customers on hold for longer.<sup>30</sup>

In addition to waiting longer for advice from their colleagues, inexperienced workers may simply forgo such advice once they are remote and consequently answer customer calls less completely. Indeed, when on-site hires transitioned to remote work, they were 0.40 percentage points or 2.5 percent more likely to have customers call back within two days, suggesting that their initial question went unanswered (*p*-value = 0.045, column 4 of Table 3, panel A and online Appendix Figure A11, panel A). The increase in callback rates is concentrated among less experienced workers who see a 5.3 percent increase in callback rates when they go remote (*p*-value = 0.007, column 4 of Table 3, panel B and online Appendix Figure A11, panel B).

We do not see significant effects on customer satisfaction scores (column 5 of Table 3, panel A): while the onset of the pandemic led to poorer reviews (online Appendix Figure A12, panel A), the difference-in-differences design suggests that this was due to the strains of the pandemic (on workers and customers) rather than the effects of remote work.

*Heterogeneous Treatment Effects by Distractions at Home.*—Our results offer suggestive evidence that remote work's negative treatment effect is not primarily driven by workers facing more distractions while at home. We find no significant heterogeneity in remote work's effects by parental status (online Appendix Table A12).<sup>31</sup> We also find that most workers have a private workspace to take calls,<sup>32</sup> and the negative treatment effect is indistinguishable for workers who do and do not have a private workspace (online Appendix Table A13).

*Pre-COVID Switches to Remote Work.*—In a complementary design, we estimate changes in workers' productivity around voluntary transitions from on-site to remote work that occurred before the pandemic.<sup>33</sup> Even among those who chose

<sup>&</sup>lt;sup>30</sup>Experienced workers—who may give more advice than they receive—keep customers on hold for marginally less time once remote (online Appendix Figure A8, panel B). These heterogeneous effects of remote work mimic the baseline differences between remote and on-site workers: when the offices were open, the least experienced remote workers kept customers on hold longer than their on-site peers, while the most experienced remote workers kept customers on hold for less time (online Appendix Figure A10).

<sup>&</sup>lt;sup>31</sup>We also find no gender difference in the treatment effect of remote work on either call quantity or quality (online Appendix Table A14). This result differs from Dutcher's (2012) finding of a particularly negative treatment effect of remote work for male undergraduates in a lab-based experiment and from Adams-Prassl et al.'s (2023) finding that female MTurkers with children at home have more interruptions.

<sup>&</sup>lt;sup>32</sup>We asked respondents, "During the past week, what room have you typically worked in?" We limited this question to nonparents to minimize the burden of the childcare-oriented survey on parents. Fully 56 percent of respondents had a home office, another 23 percent worked in a private bedroom, and 21 percent worked in a shared space (typically a living room or kitchen). We categorize those taking calls in a home office or bedroom as having a private workspace.

<sup>&</sup>lt;sup>33</sup>We use the estimation approach proposed by Dube et al. (2023) to address concerns over staggered difference-in-differences designs (De Chaisemartin and d'Haultfoeuille 2020; Borusyak, Jaravel, and Spiess 2021; Callaway and Sant'Anna 2021; Goodman-Bacon 2021). We create a stacked dataset that includes a separate dataset *s* for each individual who switches to remote work. Each dataset includes the individual who switches to remote work and a set of control individuals who handle calls of the same type (*c*), in the same time zone ( $\ell$ ), and at the same time (*t*) but who stay on-site until COVID-19. We fully interact our controls with the dataset *s* so that we

|  |                       | Decom                   | position              |                         |                           |                        |                          |
|--|-----------------------|-------------------------|-----------------------|-------------------------|---------------------------|------------------------|--------------------------|
|  | Calls                 | % On                    | Min.                  | Hold min.               | % Call back               | Satisfaction           | Call without             |
|  | per hour              | phone                   | per call              | per call                | (2 day)                   | rating                 | call back hour           |
|  | (1)                   | (2)                     | (3)                   | (4)                     | (5)                       | (6)                    | (7)                      |
| Remote   | -0.14                 | -0.78                   | 0.99                  | 0.01                    | -0.06                     | 0.001                  | -0.12                    |
|  | (0.03)                | (0.42)                  | (0.25)                | (0.03)                  | (0.13)                    | (0.01)                 | (0.03)                   |
| Pre-mean for switchers<br>Remote in %            | 4.0<br>-3.5%<br>(0.8) | $74.3 \\ -1\% \\ (0.6)$ | 12.6<br>7.8%<br>(2.0) | $1.0 \\ 0.6\% \\ (2.7)$ | $15.5 \\ -0.4\% \\ (0.9)$ | 4.9<br>0.01%<br>(0.10) | $3.3 \\ -3.6\% \\ (0.8)$ |
| Worker fixed effects<br>Call queue fixed effects | $\checkmark$          | $\checkmark$            | $\checkmark$          | $\checkmark$            | $\checkmark$              | $\checkmark$           | $\checkmark$             |
| # Workers  | 2,570                 | 2,570                   | 2,570                 | 2,570                   | 2,570                     | 2,555                  | 2,570                    |
| # Switch to remote                               | 163                   | 163                     | 163                   | 163                     | 163                       | 162                    | 163                      |
| # Stay on-site                                   | 2,407                 | 2,407                   | 2,407                 | 2,407                   | 2,407                     | 2,393                  | 2,407                    |
| # Worker days                                    | 130,649               | 130,645                 | 130,645               | 130,645                 | 130,649                   | 112,292                | 130,649                  |
| R <sup>2</sup>                                   | 0.67                  | 0.76                    | 0.55                  | 0.33                    | 0.34                      | 0.30                   | 0.63                     |

TABLE 4—TREATMENT EFFECT FROM SWITCHES TO REMOTE WORK BEFORE COVID-19

*Notes:* This table presents difference-in-differences designs that compare the change in productivity of on-site workers who were permitted to go remote to that of workers who stayed on-site until the offices closed for COVID-19. Column 1 shows calls answered per hour that the worker is scheduled to answer customers' calls. Columns 2-3 decompose the change in call volumes into (2) the percent of workers' scheduled call time that she spends on the phone, and (3) the average duration of each call in minutes. Columns 4-6 consider three metrics of call quality: (4) minutes that customers are kept waiting on hold; (5) the rate at which customers call back to the service line within two days, likely with unanswered questions; (6) average customer satisfaction scores on a five-point scale. The final column considers an alternative measure of productivity that considers the number of calls handled per hour that do not lead to a call back. Each specification estimates equation (5) in a six-month bandwidth. As summarized in footnote 33, we follow the approach of Dube et al. (2023) to limit the control group to workers who took calls from the same queue but stayed on-site until the pandemic. The call queue fixed effects specify the date, time zone, and call type. The sample excludes workers who handle specialized calls. Standard errors are clustered by worker.

to go remote and were granted permission to do so by the firm, we find a negative treatment effect akin to those in our main estimation: workers answered 3.5 percent fewer calls after they went remote (Table 4; online Appendix Figure A13). The decrease in calls handled is driven by a decrease in the time spent on the phone and an increase in the duration of any given call. We find no significant changes in call quality with remote work, which is consistent with our findings that the adverse effects on call quality are driven by less experienced workers who were not allowed to transition to remote work before the pandemic.

Even for those who asked to go remote in normal times, the transition to remote work is associated with a decline in productivity. Thus, these results suggest that the negative effects in our main difference-in-differences design were not solely driven by the sudden and unexpected nature of the transition to remote work caused by COVID-19.

(5) 
$$Calls/Hour_{i,t,s} = \phi 1 \{Remote_{i,t,s}\} + \mu_{i,s} + \mu_{t,\ell(i),c(i,t),s} + v_{i,t,s}.$$

We give a weight of 1 to observations of workers who switch to remote work and a weight of  $\frac{1}{N_{L,\ell(i),c(i,I),s}}$  for control observations where  $N_{L,\ell(i),c(i,I),s}$  denotes the number of control observations for each treated observation.

effectively estimate the effect of each switch to remote work separately and then aggregate these effects in a single estimate. We use the following specification:



Panel A. Difference-in-differences in investments and promotions

Panel B. Pre-pandemic promotion differences



FIGURE 2. EFFECT OF REMOTE WORK ON WORKERS' CAREERS

*Notes:* This figure investigates remote work's impact on workers' careers. Panel A considers difference-in-differences in career investments and promotion outcomes. The left plot captures time spent on training for new skills each month; the middle plot captures time spent attending one-on-one meetings with managers; the right plot presents the percent of workers who are promoted to higher-stakes roles that feature 13-percent pay raises. In each plot, the first coefficient reflects the pre-period differences; and the final arrow and coefficient reflect the difference-in-differences estimate. Each estimate includes call queue fixed effects and date-by-hire-month fixed effects to compare workers with similar tenure. Online Appendix Tables A15–A16 show robustness to alternative controls. Online Appendix Figure A14 shows the time-series averages. Panel B presents the share of workers who have been promoted as a function of the months since their hire date in the pre-pandemic period; online Appendix Figure A15 shows promotions conditional on persisting in the firm. Ribbons and error bars reflect 95 percent confidence intervals. Standard errors are clustered by worker.

## A. Treatment Effects on Workers' Career Trajectories

In addition to analyzing remote work's immediate effects on productivity, we can investigate its effects on workers' career trajectories.

We find that remote work reduces training time and manager one-on-one meetings. When the offices were open, on-site hires spent more time in training sessions devoted to developing new skills and in one-on-one meetings with their managers planning their short-term path to promotion over the next 30, 60, and 90 days. Once the offices closed, both advantages disappeared. The difference-in-differences estimates indicate that remote work reduced training time by 19.1 minutes per month or 26.3 percent (*p*-value = 0.022, leftmost plot in Figure 2, panel A) and manager one-on-one time by 10.2 minutes or 34.1 percent (middle plot).<sup>34</sup>

Consistent with remote work reducing workers' opportunities to pick up skills and bond with managers, we see stark differences in promotions prior to the pandemic: a year after hire, 44.0 percent of on-site hires had been promoted compared to just 20.9 percent of remote hires (Figure 2, panel B).<sup>35</sup> The gap in monthly promotion rates disappears once the offices close (the rightmost plot in Figure 2, panel A): thus, the difference-in-differences estimate indicates that remote work decreases promotion rates by 4.1 percentage points or 67.5 percent, similar to the effect in Bloom et al.'s (2015) experiment.<sup>36</sup> If workers anticipate this promotion penalty, more ambitious workers may gravitate away from remote jobs. The next section investigates the consequences for worker selection.

## **IV. Results: The Selection Effect of Remote Work**

During COVID-19's office closures, all workers worked remotely. Thus, any remaining productivity differences between workers who initially chose to be remote and workers who initially chose to be on-site primarily reflects differential selection into remote work.<sup>37</sup> Those who initially opted for remote roles continue to be less productive, averaging 0.30 (or 7.8 percent) fewer calls per hour with our preferred controls (*p*-value = 0.00004 in column 3 of Table 5). Indeed, even when all workers are at home, the entire productivity distribution of originally remote workers is shifted to the left compared to originally on-site workers (Figure 3). These results are robust to the inclusion of our standard controls (Table 5), fixed effects for the number of hours that workers are scheduled to answer calls (online Appendix Table A18), and additional geographic controls (online Appendix Table A17).

Originally remote workers answered fewer calls per hour primarily because they took longer to answer each call (column 2 of Table 6). They kept customers on hold for similar durations and had similar customer ratings as workers who were initially on-site (column 3 and 5 of Table 6).

<sup>&</sup>lt;sup>34</sup>The time-series patterns reveal consistent levels of training but a precipitous decline in manager one-on-one meetings for all workers once the offices close (online Appendix Figure A14).

<sup>&</sup>lt;sup>35</sup> Figure 2, panel B plots unconditional promotion rates. If we instead condition on persisting in the firm, the share of workers who have been promoted starts to approach one, so remote workers catch up to their on-site counterparts by about 15 months at the firm (online Appendix Figure A15).

<sup>&</sup>lt;sup>36</sup>Our results are particularly striking because remote and on-site workers are on different teams so do not directly compete for promotions.

<sup>&</sup>lt;sup>37</sup>In addition to worker selection, differences in productivity could reflect differences in learning and differences in preparedness for remote work during COVID-19. However, we find limited evidence of differences in learning on-site and remote (online Appendix Figure A3). We also find limited evidence that the unexpected transition to remote work substantially depressed formerly on-site workers' productivity (see Section III).

| Calls per hour                   |   |   |   |   |  |  |
|----------------------------------|---|---|---|---|--|--|
| (1)                              | (2)   | (3)   | (4)   | (5)   | (6)  | (7)  |
| -0.20<br>(0.07)                  | -0.31<br>(0.07)   | -0.30<br>(0.08)                                       | -0.30<br>(0.08)                                       | -0.24<br>(0.09)                                       | -0.27<br>(0.11)  | -0.21<br>(0.13)  |
|                                  |   |   | 0.01<br>(0.02)  | $0.02 \\ (0.02)$                                      | $0.02 \\ (0.02)$                                       | $0.02 \\ (0.02)$                                       |
|                                  |   |   |   | $0.06 \\ (0.04)$                                      | $0.04 \\ (0.04)$                                       | $0.07 \\ (0.05)$                                       |
|                                  |   |   |   |   | 0.03<br>(0.03)   | $0.04 \\ (0.03)$                                       |
|                                  |   |   |   |   | -0.01<br>(0.02)  | -0.004<br>(0.02)                                       |
|                                  |   |   |   |   |  | $0.07 \\ (0.08)$                                       |
|                                  |   |   |   |   |  | $\begin{array}{c} -0.04 \\ (0.15) \end{array}$         |
| 3.8<br>-5.3%<br>(1.9)            | 3.8<br>-8.2%<br>(1.9)   | 3.8<br>-7.8%<br>(2.1)                                 | 3.8<br>-7.9%<br>(2.1)                                 | 3.8<br>-6.4%<br>(2.4)                                 | 3.8<br>-7.2%<br>(2.9)                                  | 3.8<br>-5.6%<br>(3.5)                                  |
|                                  | $\checkmark$  | $\checkmark$  | $\checkmark$  | $\checkmark$  | $\checkmark$   | $\checkmark$   |
| 1,436<br>1,174<br>262<br>108,174 | 1,436<br>1,174<br>262<br>108,174  | 1,436<br>1,174<br>262<br>108,174                      | 1,436<br>1,174<br>262<br>108,174                      | 1,436<br>1,174<br>262<br>108,174                      | 1,436<br>1,174<br>262<br>108,174<br>0,13               | 785<br>634<br>151<br>70,453<br>0.16                    |
|                                  | $(1) \\ -0.20 \\ (0.07) \\ (0.07) \\ (0.07) \\ (1.9) \\ 1.436 \\ 1.174 \\ 262 \\ 108,174 \\ 0.002 \\ (0.02) \\ (1.9) \\ (1$ | $\begin{array}{c ccccccccccccccccccccccccccccccccccc$ | $\begin{array}{c ccccccccccccccccccccccccccccccccccc$ | $\begin{array}{c ccccccccccccccccccccccccccccccccccc$ | $\begin{array}{c c c c c c c c c c c c c c c c c c c $ | $\begin{array}{c c c c c c c c c c c c c c c c c c c $ |

| TABLE 5—SELECTION EFFECT OF REMOTE WORK: PRODUCTIVITY DIFFERENCES ' | WHEN ALL | WORKERS AR | E |
|---|----------|------------|---|
| Remote Due to Covid-19  |          |            |   |

*Notes:* This table presents the differences in calls taken per hour between workers who initially chose on-site jobs and those who initially chose remote jobs in the six months after the offices closed. Each specification estimates equation (4). Calls per hour is computed as the ratio of the number of completed calls over the number of hours that the worker was scheduled to answer calls that day (as opposed to, e.g., answering customer emails). Call queue fixed effects specify the date of the call, the worker's time zone, and the call level (routine, intermediate, or complex) to limit comparisons to workers handling calls randomly routed from the same queue. Covid-19 cases come from *New York Times* (2021). Pay for customer service representatives in the worker's metropolitan statistical area comes from occupational employment statistics (Bureau of Labor Statistics 2021b). Parenting characteristics come from a caregiving survey that the firm fielded in June of 2020 and that we supplemented with a survey run in April of 2021. The sample is our primary sample summarized in footnote 23. Standard errors are clustered by worker.

Originally remote workers are more likely to forward challenging calls, transferring fully 4.0 percentage points (or 19.1 percent) more calls to other workers (*p*-value < 0.00001 in column 3 of online Appendix Table A23). Consistent with this, they are less likely to have customers callback to the service line (column 4 of Table 6). If we consider a composite measure of productivity—the number of calls that the worker answers each hour that do *not* yield a callback within two days—our results continue to indicate negative selection into remote work in column 5 of Table 6.<sup>38</sup>

We do not find any meaningful differences in worker selection based on gender, parental status, or tenure (online Appendix Table A25). We do not see negative

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<sup>&</sup>lt;sup>38</sup>We show robustness tables for these outcomes in online Appendix Table A20–A23.



FIGURE 3. PRODUCTIVITY DIFFERENCES WHEN ALL WORKERS ARE REMOTE DUE TO COVID-19

*Notes:* This figure illustrates the differences in calls taken per hour between workers who initially chose on-site jobs (Observations: 1,391) and those who initially chose remote jobs (Observations: 242) in the six months after the offices closed (April 2020 to October 2020). The histograms show the distribution of calls taken per hour on each worker–day. There are 50 evenly sized bins. Values are winsorized at the top 0.1 percent for ease of viewing. The annotated coefficient estimates equation (4) using our preferred set of controls of call queue fixed effects and worker age and gender. Calls per hour is computed as the ratio of the number of completed calls over the number of hours that the worker was scheduled to answer calls that day (as opposed to, e.g., answering customer emails). The sample is our primary sample summarized in footnote 23. Standard errors are clustered by worker.

|   | Decom                  | position              |                          | Call Quality           |                        |                     |  |
|---|------------------------|-----------------------|--------------------------|------------------------|------------------------|---------------------|--|
|   | % On                   | Min.                  | Hold min.                | % Call back            | Satisfaction           | Call without call   |  |
|   | phone                  | per call              | per call                 | (2 day)                | rating                 | back per hour       |  |
|   | (1)                    | (2)                   | (3)                      | (4)                    | (5)                    | (6)                 |  |
| Initially remote                          | -0.54                  | 0.95                  | -0.02                    | -0.62                  | 0.01                   | -0.24               |  |
|   | (0.50)                 | (0.25)                | (0.06)                   | (0.20)                 | (0.01)                 | (0.07)              |  |
| Pre-mean on-site<br>Initially remote in % | 74.3<br>-0.7%<br>(0.7) | 13.2<br>7.2%<br>(1.9) | $1.1 \\ -2.2\% \\ (5.2)$ | 15.9<br>-3.9%<br>(1.3) | 4.9<br>0.25%<br>(0.23) | 3.2 -7.4%<br>(2.20) |  |
| Preferred controls                        | $\checkmark$           | $\checkmark$          | $\checkmark$             | $\checkmark$           | $\checkmark$           | $\checkmark$        |  |
| # Workers                                 | 1,436                  | 1,436                 | 1,436                    | 1,436                  | 1,429                  | 1,436               |  |
| # Initially on-site                       | 1,174                  | 1,174                 | 1,174                    | 1,174                  | 1,168                  | 1,174               |  |
| # Initially remote                        | 262                    | 262                   | 262                      | 262                    | 261                    | 262                 |  |
| # Worker days                             | 100,414                | 99,504                | 100,414                  | 108,174                | 89,143                 | 108,174             |  |
| R <sup>2</sup>                            | 0.46                   | 0.08                  | 0.12                     | 0.08                   | 0.08                   | 0.13                |  |

TABLE 6-SELECTION EFFECT OF REMOTE WORK: AUXILIARY MEASURES

*Notes:* This table presents the differences between workers who initially chose on-site jobs and those who initially chose remote jobs in the six months after the offices closed. Columns 1-2 decompose the difference in call volumes into (1) the percent of workers' scheduled call time that she spends on the phone and (2) the average duration of each call in minutes. Columns 3-5 consider three metrics of call quality: (3) minutes that customers are kept waiting on hold; (4) the rate at which customers call back to the service line within two days, likely with unanswered questions; and (5) average customer satisfaction scores on a five-point scale. Column 6 considers an alternative measure of productivity that considers the number of customer calls that do not lead to a call back that the worker answers each hour. Each specification estimates equation (4), including our preferred set of controls for demographics and call queue fixed effects. The sample is our primary sample summarized in footnote 23. Standard errors are clustered by worker.

selection among workers who were permitted to go remote before COVID-19 (Table 4, online Appendix Figure A13), consistent with the firm selectively granting approval to only some workers to go remote.

Location Expectations and Selection.—We find that the difference in selection between remote and on-site hires moves in lockstep with expectations about returning to the office. For all the cohorts hired before the offices closed, remote hires were less productive than on-site hires—even once everyone was remote (Figure 4). This pattern persists largely unchanged soon after the offices close, when workers may have still expected on-site jobs to quickly return to the office. However, as the return to the office came to seem like a distant possibility, the differences in productivity narrowed. Indeed, during the winter of 2021—when 61 percent of Americans believed that a return to normal pre-COVID life was at least 6 months away (Ipsos 2021)—we see no appreciable productivity difference between new remote and on-site hires (Figure 4). Once remote and on-site work became a distinction without a difference, there ceased to be a difference in worker selection. The fact that selection changes with expectations about the pandemic's duration suggests that the initial gap was due to the jobs being remote versus on-site and not differences in geography or compensation that did not change over the course of the pandemic.

#### V. Remote Work's Prevalence

Remote work's prevalence hinges on both the costs to firms of supplying remote jobs—which is the focus of our paper—and the demand of workers to work remotely. We calibrate a simple demand and supply model, illustrated in Figure 5, to analyze remote work's prevalence in jobs similar to those in our study. To estimate the supply of remote jobs, we use the case study of the retailer, leveraging our estimates of remote work's productivity costs and our approximations of its savings in office rents and turnover costs. To estimate the demand for remote work, we draw on existing studies that evaluate workers' willingness to forgo higher wages to work remotely.

# A. Supply of Remote Work

Firms weigh a few factors when choosing whether to supply remote jobs at a given wage: the treatment effect of remote work, differential selection into remote jobs, and the savings in turnover and office space.

*Treatment Effect of Remote Work.*—Our estimates indicate that working remotely reduces productivity by 4 percent (column 4 of Table 2). The negative impact of remote work thus requires 4 percent more remote workers to handle a given volume of calls and raises total labor costs of remote work by 4 percent. We assume that this treatment effect is constant and does not depend on the share of employees working remotely.<sup>39</sup>

<sup>&</sup>lt;sup>39</sup>Our empirical results support this assumption. First, we find similar treatment effects for workers who voluntarily chose to work from home before COVID-19 and for workers who were forced to work from home due to COVID-19. Second, we find that the treatment effect of remote work is not hugely different by gender and parental



FIGURE 4. PRODUCTIVITY DIFFERENCES BETWEEN REMOTE AND ON-SITE HIRES BASED ON EVOLVING EXPECTATIONS OF ON-SITE WORK

*Notes:* This figure illustrates the productivity gap between workers hired into on-site and remote jobs when everyone was working remotely due to COVID-19 between April 2020 and April 2021. Differences are shown separately for workers hired in different seasons. The sample is limited to seasons with at least 25 remote and 25 on-site hires and excludes workers who handle specialized calls. The vertical line highlights the office closures of COVID-19. On-site workers hired before the office closures (N = 741) expected to work on-site. Workers hired into on-site jobs after the office closures (Observations: 336) were told that they would eventually need to return to the office but would initially work remotely. Workers hired into remote jobs before the office closures (N = 741) expected to work on-site. Calls per hour is computed as the ratio of the number of completed calls over the number of hours that the worker was scheduled to answer calls that day (as opposed to, e.g., answering customer emails). We include our preferred set of controls of call queue fixed effects and worker age and gender (see Section IIC). Error bars reflect 95 percent confidence intervals, with standard errors clustered by worker.

Selection into Remote Jobs.—Our estimate indicates workers who choose remote jobs answer 8 percent fewer calls per hour than workers who choose on-site jobs (column 3 of Table 5). This difference in selection would increase the firm's labor costs of hiring remote workers by 8 percent if it cannot screen for productive hires. In contexts like ours, where new hires often enter with little prior experience and often exit quickly, it is plausible that firms struggle to effectively screen workers. We assume that the *difference* in selection between workers who choose remote and on-site jobs is constant—even if remote work becomes more common. This stability can arise when workers' remote work choices reflect their productivity. In this case, if the remote workforce expands, it becomes less adversely selected, but the on-site workforce simultaneously contracts and becomes more positively selected. Thus, remote work's prevalence affects the *levels* of productivity in remote

status, even though these traits are quite predictive of demand for remote work (Mas and Pallais 2017; Barrero, Bloom, and Davis 2022; Maestas et al. 2023; Lewandowski, Lipowska, and Smoter 2024).



FIGURE 5. PRISONER'S DILEMMA IN THE MARKET FOR REMOTE WORK

*Note:* This figure illustrates how call center firms like the one that we study could have been trapped in a prisoner's dilemma before the pandemic because they all may have been better off offering remote work with no wage penalty, but any individual firm that did so would have disproportionately attracted less productive workers. In this demand and supply framework, the *x*-axis represents the percent of workers who are remote. The *y*-axis represents the price of remote work to workers or the wage gap between on-site and remote jobs. The estimated demand curve for fully remote jobs comes from Lewandowski, Lipowska, and Smoter's (2024) choice experiment (see online Appendix Figure A26), with alternatives shown in online Appendix Figure A16. The estimated cost of remote work to the firm (in orange) comes from our estimates of remote work's treatment and selection effects, net of the savings in office rents (explained in footnote 41) and turnover. The estimated cost of remote work to society (in dark blue) excludes the selection effect of remote work. For reference, the green line uses Bloom et al.'s (2015) estimate of the positive treatment effect of remote work. The deadweight loss integrates over the losses of all the workers who work on-site in the market but would work remotely in the efficient solution.

and on-site jobs but need not affect the *difference* between them.<sup>40</sup> Indeed, online Appendix A shows that the selection effect of remote work is constant under a range of microfoundations.

*Savings in Office Rents.*—Our back-of-the-envelope calculation suggests that the retailer spent \$1 per worker-hour on office space for on-site workers.<sup>41</sup> Compared to

<sup>&</sup>lt;sup>40</sup>To see this concretely, imagine there are three workers who average one, two, and three calls per hour and whose demand for remote work negatively correlates with their productivity. As such, the remote worker handles one call per hour. If the worker handling two calls per hour switches from on-site to remote work, average productivity in remote work rises from 1 to 1.5 calls per hour, while on-site productivity also rises from 2.5 to 3 calls per hour. This switch thus maintains a constant difference of 1.5 calls per hour. This logic generalizes: moving a marginal worker from on-site to remote work tends to increase the average productivity in both groups, as this worker is typically less productive than those remaining on-site but more productive than those already working remotely.

<sup>&</sup>lt;sup>41</sup>Each worker typically needs about 100 square feet of space (Colacino 2017). The retailer pays about \$20/square foot annually in rent and utilities in its call centers. For a full-time worker, this amounts to \$0.96/hour (at \$20/square foot per year  $\times$  100 square feet per worker  $\div$  2,080 hours per worker-year = \$0.96/worker-hour in rent).

the average wage, this spending represents 6 percent of labor costs. For simplicity, we assume constant savings from remote work in office expenses, so abstract from general-equilibrium effects in the real-estate market.

Savings in Turnover Costs.—Remote workers have lower turnover, mitigating multiple costs to the firm. First, workers go through a formal training period, which costs \$689 per hire according to the firm's finance department. Second, new recruits answer about 20 percent fewer calls in their first month than more experienced workers, costing about \$480 per hire.<sup>42</sup> After this time, new workers are as productive as more experienced workers. Finally, the finance department estimates that other costs of new hires—such as advertising new jobs, interviewing candidates, and conducting background checks—costs about \$300 per hire. Together, a new hire costs \$1,469, and so remote work's reduction in turnover of 0.34 percentage points (online Appendix Table A4) saves the firm 0.8 percent of the wage bill.

The firm weighs these savings against remote work's productivity costs when choosing what price to offer remote work. How many workers choose remote jobs at this price depends on workers' demand for remote work.

# B. Demand for Remote Work

We first describe how we identify the demand curve and then describe the existing studies whose data we leverage. Online Appendix B provides more detail.

*Estimation.*—To estimate workers' demand for remote work, researchers often ask workers to choose between hypothetical jobs that differ in their wages and remote work arrangements. For a given wage difference between a hypothetical on-site and remote job, the share of workers who choose the remote job pins down the quantity of remote work at that wage penalty—or price of remote work. Thus, each wage difference in the hypothetical-choice data identifies a different point on the demand curve. With infinite data, simply connecting these points would reveal the demand curve. In real-world data, it's useful to put more structure on the estimation. We adapt Mas and Pallais' (2017) approach, which assumes workers' willingness to pay for remote work follows a logistic distribution (WTP<sub>i</sub> ~ Logistic( $\mu$ , s)) with CDF,  $F(.;\mu,s)$ . They also assume a share  $\alpha$  of respondents are inattentive and so choose randomly between offered jobs.<sup>43</sup> The observed choices are then generated by

$$\Pr(Choose \ Remote_i = 1 | \% \Delta w) = \underbrace{F(\% \Delta w; \mu, s)(1 - \alpha)}_{\text{Attentively Choose Remote}}$$

 $+ \underbrace{\frac{1}{2}\alpha}_{\text{Randomly Choose Remote}}$ ,

<sup>42</sup>This estimate scales the productivity losses by the average wage that these workers are paid to learn on-the-job for their first four 40-hour weeks  $(0.2 \times \$15 \times 4 \times 40 = \$480)$ .

<sup>&</sup>lt;sup>43</sup> The trick questions in Mas and Pallais (2017) and Maestas et al. (2023) suggest 26 and 35 percent of respondents are inattentive, respectively. We assume similar inattention in Lewandowski, Lipowska, and Smoter (2024) who do not ask trick questions and, instead, ask math questions that few get wrong (0.6 percent).

where  $\%\Delta w = 100 \cdot (w_{on-site} - w_{remote})/w_{on-site}$  is the effective price of remote work in percentage terms. We estimate this by maximum likelihood.<sup>44</sup> We then back out what the demand curve for remote work would be if all workers were attentive:

(6) 
$$\tilde{Q}(\Delta w) = F(\Delta w; \hat{\mu}, \hat{s}) \rightarrow \hat{D}(Q) = \tilde{F}^{-1}(Q; \hat{\mu}, \hat{s}),$$

which is pictured in Figures 5 and online Appendix Figure A16.

*Estimated Demand Curve.*—Our preferred approach uses choice data from Lewandowski, Lipowska, and Smoter (2024) because this is the only study, to our knowledge, that elicits demand for fully remote work—rather than hybrid options. The authors ask a representative sample of Polish workers to choose between on-site jobs and jobs where they "work from home 5 days a week [with] no on-site work." The average worker is willing to sacrifice less than 1 percent of the wage to be fully remote instead of fully on-site. Reassuringly, when the authors ask workers to choose between on-site and *hybrid* jobs, the average worker is willing to give up 5.1 percent of their wage for hybrid work, which lies between the estimates in the US-based studies of Maestas et al. (2023) and Mas and Pallais (2017). This similarity suggests that the estimates from this Polish context may generalize well to our American setting. In addition, we discuss how using demand curves from Mas and Pallais (2017) and Maestas et al. (2023) would impact our calibrated model (online Appendix B for details).

# C. Equilibrium

We find that the savings in office real estate (at 6 percent of labor costs) and in reduced turnover (at nearly 1 percent of labor costs) could more than offset the productivity penalty of remote work (at 4 percent of hourly output). Thus, if these were the only factors that the firm considered, it would be willing to pay a 3 percent wage *premium* for workers to work remotely (as pictured in the navy line in Figure 5). Based on the estimated demand curve from Lewandowski, Lipowska, and Smoter (2024), 70 percent of workers would then work remotely. Using alternative demand estimates generates a similar picture of a plurality of workers working remotely in this counterfactual (online Appendix Figure A16).

Yet a profit-maximizing firm also considers the impacts of offering remote work on the selection of workers it attracts. Our estimates suggest that firms like this one would only hire remote workers at a 5 percent wage *penalty* in order to offset its expected costs.<sup>45</sup> Given Lewandowski, Lipowska, and Smoter's (2024) estimates, only 34 percent of workers would be willing to make this sacrifice.

The retailer's average wage penalty for remote work of 8 percent (in Table 1) exceeds our estimate of remote work's average cost of 5 percent. Given the estimated demand

<sup>&</sup>lt;sup>44</sup>We follow Mas and Pallais (2017) and use a preset value of  $\hat{\alpha}$  (described in footnote 43) rather than relying on functional-form assumptions for identification.

 $<sup>^{45}</sup>$ The firm considers 8 percent from a negative selection effect + 4 percent from a negative treatment effect - 6 percent real estate costs - 1 percent turnover costs = 5 percent.

curve, we would expect only 23 percent of workers to be willing to sacrifice 8 percent of pay to work remotely. This prediction closely matches the retailer's pre-pandemic rate of remote work of 18 percent. Thus, if our model is well-calibrated, correcting this mispricing could increase remote work's prevalence by 11-16 percentage points.<sup>46</sup>

## D. Welfare Consequences

Our results suggest that each individual firm acting rationally and in its own self-interest would set a sizeable wage penalty for remote work, even though all firms would collectively be better off if they did not penalize remote work. This prisoner's dilemma for firms leads to a deadweight loss for society. From society's perspective, attracting latently less productive workers into remote jobs at firm x does not impact overall output, since these workers would also be less productive in on-site jobs at firm y. Thus, selection concerns cause the private costs of offering remote work to exceed the social costs. Our estimates suggest that the selection effect of remote work deters 36 percent of workers from working remotely using the estimates of Lewandowski, Lipowska, and Smoter (2024) and 14–36 percent of workers using the alternative demand curves (Online Appendix Figure A16).

The distortion leads to a deadweight loss valued at 1.4 percent of workers' compensation averaged over all workers (the red area in Figure 5) or \$459 per year for a full-time, full-year worker. For the 36 percent of workers who are directly affected by the distortion, this implies lost surplus worth 3.94 percent of their compensation or 1,254 annually.<sup>47</sup>

# E. Implications for a Post-pandemic World

Our findings suggest several reasons why the pandemic's mass experiment with remote work will permanently affect its market provision.

First, the mass experiment with remote work could have corrected firms' misperceptions about the productivity costs of remote work and resolved uncertainty about remote work's downside risks. Further, the pandemic could have overcome fixed costs of adopting remote work. These factors may have previously depressed the supply of remote work below our model's predicted levels. If our model is well-calibrated, overcoming these hurdles alone would increase remote work by 11-16 percentage points at the retailer and potentially increase remote work by even more at other firms that had not yet adopted remote work.

Second, this mass experiment may have changed *who* chooses remote jobs. Workers may increasingly sort into remote and on-site jobs on the basis of their preferences for working from home rather than their concerns about promotion. Workers may have learned more about their preferences for remote work, as seen in the increasing variance in workers' stated willingness to pay for remote work (Barrero, Bloom, and

<sup>&</sup>lt;sup>46</sup>Our model may also omit real costs of remote work. For example, it's possible that lower promotion rates in remote work not only hurt workers but also hurt firms.

 $<sup>^{47}</sup>$  Adverse selection into remote work may also be socially costly if society is particularly concerned about inframarginal remote workers who choose remote work because of (1) latently low-ability or (2) strong tastes, such as those arising from caregiving responsibilities.

Davis 2022; online Appendix Figure A19). At the same time, stigma associated with remote work has fallen (Barrero, Bloom, and Davis 2022), which may reduce workers' incentives to choose on-site jobs to improve their career opportunities.

Third, the pandemic may have improved the treatment effect of remote work, as firms invested in complementary management practices and information technologies (Kwan 2022).

Consistent with these factors, the firm we study chose to close some but not all of its on-site call centers over the first year of the pandemic. In this firm, remote hires composed only 17.5 percent of the sample before the offices closed but 64.5 percent by April 2021. Similarly, in the American Community Survey, just 6.8 percent of phone workers were fully remote in 2019, but 32.7 percent were remote in 2021 and 29.8 percent in 2022 (online Appendix Figure A18). In 2022, the firm that we study adopted a plan to close all of its on-site locations, and ultimately decided to only retain on-site call centers in low-cost locations. These patterns suggest that the mass experiment with remote work may have, at least partially, freed firms from a prisoner's dilemma that led to an underprovision of remote work.

# **VI.** Conclusion

We consider why so few Americans worked remotely before COVID-19—even in remotable jobs. In our call center context, the rarity of remote work was particularly puzzling since (1) workers expressed strong tastes for remote work (Mas and Pallais, 2017) and (2) existing evidence indicated that working remotely increased productivity in call center jobs (Bloom et al. 2015).

We ask two questions: how does remote work affect productivity, and how productive are the workers who choose remote jobs? We quantify each factor using data from an American Fortune 500 firm that hired both remote and on-site workers prior to COVID-19. Before the offices closed, remote workers were 12 percent less productive than on-site workers. Around the office closures, the hourly calls of on-site workers going remote fell by 4 percent relative to that of already-remote workers, indicating that a negative treatment effect accounted for a third of the productivity gap. After the offices were closed, workers who initially chose remote jobs were 8 percent less productive than those who initially chose on-site jobs, even though all workers were working at home. Thus, two thirds of the initial productivity gap was due to worker selection.

Adverse selection consequently offers an important missing piece to the puzzle of remote work's rarity prior to COVID-19. Our estimates suggest that adverse selection distorts the decisions of 36 percent of call center workers who do not choose to be remote because they do not want to pool with less productive types. There is promise that the pandemic could nudge the market into a more efficient equilibrium. Yet distortions will likely persist unless career opportunities can be equalized.

Our paper has a few important limitations. We identify a negative but small treatment effect of remote work for relatively autonomous tasks but cannot speak to intensely collaborative tasks, where more negative effects have been found (Battiston, Kirchmaier, and Vidal's 2021; Gibbs, Mengel, and Siemroth 2023). We also cannot disentangle the potential reasons why our treatment effect may differ

from those in other studies; understanding the role of performance pay, management practices, site selection (Allcott 2015), or other contextual forces on productivity effects of remote work is an important area of future work.

Further, while we hypothesize that the estimated selection effect stems from remote work's promotion penalty, which likely generalizes to other settings, we cannot test this conjecture.<sup>48</sup> Differences in worker selection could also arise from, for example, a correlation between workers' preferences for remote work and their ability, work ethic, or education. Understanding the underlying sources of selection would suggest different levers that firms might use to sidestep selection challenges.

Finally, we study the binary case of fully remote and fully in-person work, where teams are either entirely remote or entirely on-site. We cannot speak to the effects of being in the minority of remote workers on a primarily in-person team, which may feature even more acute promotion penalties. In addition, we cannot directly speak to hybrid work arrangements, which may achieve the flexibility of work-from-home without some of the drawbacks of never going into the office (Bloom, Han, and Liang 2024; Choudhury et al. 2022). Yet if face time in the office colors promotions, these hybrid arrangements may also be hard to sustain. Unpacking the effects of these alternative arrangements will help us understand what types of locational flexibility will be most likely to persist.

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<sup>48</sup>We also do not pinpoint the sources of lower promotion rates in remote jobs, which could reflect biased beliefs about remote workers' productivity (Dutcher and Saral 2022), lesser opportunities to learn from coworkers (Emanuel, Harrington, and Pallais 2023), or fewer chances to schmooze with bosses (Cullen and Perez-Truglia 2023). Relatedly, we cannot assess whether firms can credibly signal their openness to promoting remote workers and thereby attract workers who both want to advance and do so from home.

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