

Peer Review Information

Journal: Nature Human Behaviour

Manuscript Title: The Effects of Remote Work on Collaboration Among Information Workers

Corresponding author name(s): Longqi Yang

Editorial Notes:

**Redactions –
unpublished data**

Parts of this Peer Review File have been redacted as indicated to maintain the confidentiality of unpublished data.

Reviewer Comments & Decisions:

Decision Letter, initial version:
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6th January 2021

Dear Longqi,

Thank you once again for your manuscript, entitled "The Effects of Remote Work on Collaboration Among Information Workers", and for your patience during the peer review process.

Your Article has now been evaluated by 3 referees. You will see from their comments copied below that, although they find your work of potential interest, they have raised quite substantial concerns. In light of these comments, we cannot accept the manuscript for publication, but would be interested in considering a revised version if you are willing and able to fully address reviewer and editorial concerns.

We hope you will find the referees' comments useful as you decide how to proceed. If you wish to submit a substantially revised manuscript, please bear in mind that we will be reluctant to approach the referees again in the absence of major revisions. We are committed to providing a fair and constructive peer-review process. Do not hesitate to contact us if there are specific requests from the reviewers that you believe are technically impossible or unlikely to yield a meaningful outcome.

To guide the scope of the revisions, the editors discuss the referee reports in detail within the team, including with the chief editor, with a view to (1) identifying key priorities that should be addressed in revision and (2) overruling referee requests that are deemed beyond the scope of the current study. We hope that you will find the prioritised set of referee points to be useful when revising your study. Please do not hesitate to get in touch if you would like to discuss these issues further.

You will see from their comments that the reviewers are somewhat divided in their views of the

contribution your manuscript makes. From an editorial standpoint, we feel that your study has the potential to offer an evidence-based advance by bringing a large-scale dataset to bear on an existing question, provided that it can be strengthened with additional analyses and evidence. Specifically, in order for us to consider your study further on this basis, your revision must address the following (as well as all other reviewer comments):

1. Carry out the additional analyses requested by Reviewer 3 to demonstrate impacts on work outcomes, including new data to the extent possible.
2. Address Reviewer 3's concerns about the clarity and validity of your causal identification strategy.
3. Ensure that claims are fully supported. Reviewer 3 highlights several ways in which more evidence is needed, including the need for further justification of variables, explanation of the data, and consideration of alternative operationalizations and measures.
4. Ensure that the context and limitations of your study are thoroughly discussed, including the limitations on generalizability highlighted by the reviewers.
5. More fully situate your study within existing literature, as requested by both Reviewers 2 and 3.

Finally, your revised manuscript must comply fully with our editorial policies and formatting requirements. Failure to do so will result in your manuscript being returned to you, which will delay its consideration. To assist you in this process, I have attached a checklist that lists all of our requirements. I have also attached a template manuscript file that exemplifies our policies and formatting requirements. If you have any questions about any of our policies or formatting, please don't hesitate to contact me.

If you wish to submit a suitably revised manuscript we would hope to receive it within 6 months. We understand that the COVID-19 pandemic is causing significant disruptions which may prevent you from carrying out the additional work required for resubmission of your manuscript within this timeframe. If you are unable to submit your revised manuscript within 6 months, please let us know. We will be happy to extend the submission date to enable you to complete your work on the revision.

With your revision, please:

- Include a "Response to the editors and reviewers" document detailing, point-by-point, how you addressed each editor and referee comment. If no action was taken to address a point, you must provide a compelling argument. This response will be used by the editors to evaluate your revision and sent back to the reviewers along with the revised manuscript.
- Highlight all changes made to your manuscript or provide us with a version that tracks changes.

Please use the link below to submit your revised manuscript and related files:

[REDACTED]

Note: This URL links to your confidential home page and associated information about manuscripts you may have submitted, or that you are reviewing for us. If you wish to forward

this email to co-authors, please delete the link to your homepage.

Thank you for the opportunity to review your work. Please do not hesitate to contact me if you have any questions or would like to discuss the required revisions further.

Sincerely,
Aisha

Aisha Bradshaw
Editor
Nature Human Behaviour

Reviewer expertise:

Reviewer #1: labour economics, workplace productivity

Reviewer #2: remote work

Reviewer #3: remote work, team effectiveness

REVIEWER COMMENTS:

Reviewer #1:

Remarks to the Author:

This is a truly great paper - really fantastic. Amazing data, completely believable results and well executed. Indeed, I would love to cite this when it's published and learned a lot. A few minor points:

- Entropy is (for economics) a strange concept. Is there some easier to explain measure - concentration for example?
- Figure 2 might look nicer (or better anyway) with CI bars on it (or maybe that is not Nature house style)? At least for economics journals it would be normal to have CI bars around each point.
- It was very odd for me reading Figure 3 before seeing the estimating equation and data. It felt like a "trust me" moment, but again I think that's maybe Nature style (put the details at the back)? Ideally I would want to know the data and approach underlying any results in advance as it helps to interpret them.
- Figure 4 would seem better in a Figure 3 set-up (coefficients with standard-errors). I don't think the time series for these is particularly important, but I do think CIs are so I can see what is (or is not) significant (versus noise)?
- Figures 7, 8 etc were hard to read (I physically printed the paper to read it first actually) as one of the colors was very light. I would personally make both darker so it's easy to see.
- On terminology what you call "Hybrid WFH" is normally called "Mixed-mode", which is some people in the office and some at home. What you call "some teams come in on some days..." is usually called "Hybrid", for example the Google 3-2-2 model. I am aware this terminology is not standardized, but from talking to a lot of CEOs, execs and journalists this seems to be the most common way to define

these terms.

- Finally, i fully agree with the sentiment of the conclusion. If people are in a hybrid situation with the entire team coming in 3 days and WFH for 2 days that is probably the best of both worlds. Enough time in the office to bridge and bond and enough time at home to work quietly and save on commute. And yes Mixed-mode is typically seen as an absolute disaster - i have yet to talk to a single manager who likes it (and I've probably spoken to maybe 200 to 300 managers since March 2020).

Reviewer #2:

Remarks to the Author:

The paper is timely because it addresses some critical issues relating to working from home versus working in the office. It is well written with some minor suggestions for improvement:

1. This is a US-centric paper with only data collected in the United States. There should be a justification for why you chose the US because working from home and during COVID-19 is an issue across the globe. Are the lessons or insights for other countries?
2. What was Microsoft's policy on working from home prior to the pandemic? I understand some employees already worked from home; however, what was the prevalence of this mode of working? Were there metrics in place to measure the effectiveness of working from home pre-COVID (including collaboration and communication)?
3. It would be useful to include that point in the background that some organisations in the technology sector (for example, Yahoo and IBM) required employees to be in the office for the very reasons the authors are suggesting are problematic (collaboration and communication). That is, not all organisations in the tech sector or elsewhere are necessarily going to find working from home or even a hybrid model suitable (or were unsuccessful in previous years). I suggest including this point because it shows that this mode of working has been tried before – what will be different this time?
4. Developing relationships with new contacts can only occur if there is a mechanism of some kind to bring new people together (for example, working on a new project with new colleagues). Otherwise, there are no opportunities for workers to meet new colleagues. That is why we have face to face meetings and social gatherings. This is very difficult to replicate online and can be very stressful for some employees. How can new and existing employees have opportunities to meet and develop relationships? The finding that people tended to rely on existing networks and relationships makes sense in this context.
5. The Microsoft experience is likely to be very different from other organisations such as those in the Finance sector, Education, Transport, Tourism and Government workers. This is one case study and the data is very specific to Microsoft. This point should be emphasised.
6. The method to collect data provides some good insights into how workers communicated and collaborated but only during the pandemic. It is important to stress that this way of working was mandated and employees and their managers had no choice but to adjust.
7. The statement 'The most effective way to determine the benefits and downsides of different hybrid work approaches will be through randomized field experiments' is not supported by any literature. Why have the authors suggested this is the most effective way? The statement needs to be justified. The emphasis appears to be on quantitative methods .
8. To be able to understand how the employees felt about this way of working and if they prefer working from home, how productive they felt they were and whether the technology they used to communicate and collaborate was effective (for the purposes of their ability to achieve the outcomes they need to achieve) aligns with qualitative methods. We would need to have a better understanding from the perspective of managers and their employees on the most effective mode of working will be

post-COVID-19 in addition to measuring outcomes.

Reviewer #3:

Remarks to the Author:

Key Results

Data on collaboration patterns and communication technology use were collected from nearly 62,000 US Microsoft employees (9,727 managers and 52,135 non-managerial positions) from February 2020 to June 2020. During this time, all employees had to shift to work-at-home (WAH – or WFH work from home) work (early March) due to COVID. This context provided a natural experimental setting.

Analyses was done to compare February collaboration patterns and communication media use (pre-treatment/shift to WAH/WFH). The results indicated no significant change in the number of connections made (i.e., people collaborated with); however, fewer “new” connections were made and less time was spent with the “new” connections (new was defined as someone who was not contacted the previous month). More time was spent with existing connections (presumably defined as people with whom one communicated with twice in the previous month). There was also a negative pattern with the number of groups interacted with. The analyses of the use of communication media found that the number of unscheduled calls (audio or video using Microsoft Teams) and the number of instant messages sent increased dramatically.

Validity

When I was invited to review this paper and provided with the abstract, the final sentence of the abstract interested me, as it suggested important findings (especially given the large data set). It says: “Our results suggest that firm-wide remote work may lead to communication patterns that hinder the collaborative production of high-quality knowledge work...”. After reviewing the paper carefully, I was disappointed in the support for this suggestion, and for many of the suggestions in the paper about the effect of remote work on collaboration. Given what was measured, there is no empirical evidence to allow the authors to determine if the communication patterns did affect the collaborative production of high-quality knowledge work.

The theory presented for the possible effect of collaboration on information work is the need for structurally diverse networks (both strong and weak ties) so one has access to novel ideas to stimulate innovation. As I read through the introduction and this logic, it led me to possibly expect network analyses and some relevant outcome such as novel ideas/information shared. However, the data and results presented were limited to communication and collaboration patterns, as outlined above. Also, beyond splitting the respondents by managerial versus non, there is no data on the type of work done, which could affect the need for information and innovation. Having access to diverse ideas is important for some types of work, but less in others. And the interdependence of activities can affect how much one person relies and needs to interact with others to get their job done well.

So overall, while it appears the analyses presented were validly done (see my comments below about my limited ability to assess this), the more general implications for the results lack much validity.

Originality and Significance

The volume of longitudinal data during the shift to work at home presents a significant opportunity to gain insights regarding the effect of this shift. My invitation to review listed several categories of acceptable manuscripts. As far as I can tell, there is no methodological or conceptual novelty (i.e., there is no theoretical contribution). The category that the paper might fit into is an evidence-based advance. This is defined as acceptable “if its scale and/or rigour supersede the existing literature and significantly strengthen confidence in a scientific finding or convincingly falsify it.

The scale of the data collected and analysed is impressive. While I cannot claim to know every study on remote work, I can't recall ever seeing a study have such a large number of respondents. So do the results also significantly strength confidence in previous findings (or falsify them?

Moving everyone to work at home stopped their ability to meet face-to-face (F2F) – for formal meetings, ad-hoc interactions, etc. The work continued so they adapted to use the communication channels they had available to interact (once they were forced to WAH). Therefore, it is not surprising that the number of communications via text and Teams (audio and/or video) increased. It would have been very surprising (and worrying) if it had not. The Information Systems field (and others) have studied the use and characteristics (e.g., richness) of various types of communication media for decades in various contexts. Having to rely more on electronic communication tools when separated virtually is well understood. Therefore, I would suggest that the originality and significance of the findings with respect to the communication media use is quite low.

The work isolation caused by forcing people to work from home did change some of the collaboration patterns. There are various terms used in research that are relevant to WAH including telecommuting, remote work, virtual work, virtual teams. These various work practices have been studied by different disciplines (e.g., Information Systems Organizational Behaviour, Psychology, etc.) for at least 3 decades, with more emphasis on virtual teams lately. Telecommuting and work at home has existed even longer, since this type of work was possible for some roles before communications technology evolved/improved (which made the practice possible for a larger number of types of jobs). Certainly an important thread of study in all these practices is how to create “effective” communication to enable productive work. There are various theoretical perspectives applied and studies that attempt to understand the nuances of what effective means and how to enable it (e.g., creating psychological safety, avoiding attribution errors, etc.). So, is a large-scale study that confirms that remote work changes collaboration patterns a significant contribution? I would lean towards thinking no. However, I found it interesting that the number of connections did not significantly decrease (Table S1 – Total effect). The main finding is that an individual forced to WAH narrows their focus to existing connections, such that “new” connections and the number of groups interacted with decreases. I am not surprised by this, at it would be easier to reach out to colleagues that you already have a relationship with, and that you have on-going interdependent activities with.

I will leave it to the editors to decide if the paper fits what they feel is an evidence-based advance. Certainly the topic is very timely and will be of interest to many people and organizations.

Data and Methodology and Analytical Approach

A key strength of this study is the longitudinal data, subject to the shortcomings and suggestions presented below. I would like to have seen more explanation about how the data were collected and how the various constructs were conceptually defined, and why they were operationalized the way they were. This is explained somewhat in the methods section, but more information would be helpful.

What data was measured in February? It is not clear to me if for the people working at a corporate office, did the measure of scheduled and unscheduled meetings include non-Teams interaction? At this point, likely much of the informal interaction was F2F. On page 13, you suggest that the total synchronous communication when down, both on and off Teams. So do you have a measure that captured synchronous scheduled and unscheduled meetings, regardless of the communication medium used (i.e., it includes F2F, as well as technology-mediated communication)? Also, could you provide descriptive information on the characteristics of the participants and their jobs/tasks? This could help the reader understand the generalizability of your findings.

Suggested Improvements

Experiments have some causal explanatory power because of the use of control groups and random assignment. I understand that since this was a natural experiment, there is no random assignment. I have limited understanding of the DiD statistical technique; however, the reading I did on it certainly emphasized the need for a control group. Do I understand correctly that the control group in your analyses is the employees that were working remotely in February? Clarifying this would be helpful for readers. Assuming that is the logic, then isn't there a confounding factor to the analyses that happens when a remote worker's co-workers all are forced to work from home in March? So in one sense, I am struggling to see how the February WAH employees can be a control group for the duration of the analyses since the "treatment" definitely affected them too.

I was particularly bothered by three things and hope you may be able to find ways to address these.

1. Your definition of a new connection. This was defined as someone who was not communicated with in the previous month (but was in the current month). To me, this does not necessarily equate to a weak tie. I could know someone quite well, but maybe our need to interact is sporadic or seasonal, based on the tasks that are on-going. Much of the contribution of your paper rests on the ability to argue that remote work hurts the ability to create new weak ties, which can hurt one's ability to innovate. Here are two thoughts I have that might help. One, could you provide more analyses to show different definitions of "new", such as the person has never interacted with the individual over your data collection period or maybe you have other data available to that you could use to provide levels of newness? Two, can you provide more conceptual explanation and background on the meaning of weak ties in structural networks and how this has been operationalized in past work?

2. Your lack of work outcome variables. Do you have access to any additional data for any of your subjects that might allow you to investigate their work outcomes, and explore for the impact of the changes in collaboration patterns and use of communication channels? This could be done on a small scale (i.e., a sub-set of your data). And/or could you do some network analyses for some of your subjects to more closely examine weak ties, strong ties and structural diversity changes? If any of this was possible, it would strengthen your potential contribution and lend support to your suggestions, in my opinion.

3. Can you clarify the nature of the tasks/work that people were doing?

Past research has shown that WAH/virtual work fits some types of tasks very well and is more challenging for others. Do you have any data that you can add to your analyses to explore if and how the patterns changes for different types of roles? Being able to get some understanding for the need for interaction would be helpful in understanding your results and possibly providing better guidance

for application of your findings. For example, if I am working on a task that I can do mostly by myself, being isolated for that work is not going to drive me to create ad-hoc interactions with others (as much as it would if I was doing something that relied on others /or they relied on me).

Clarity and Context

The paper is well written and generally easy to follow. I found the layout of the paper surprising, but presumably this fits with journal expectations. My “normal” expectation for most journals I review for would be some early discussion of what is known, then theoretical perspectives possibly leading to hypotheses to examine, an explanation of the method, then results and discussion of implications and limitations. The current paper starts with 4 pages of introduction, then results, then discussion, and the method description is at the end.

References

As mentioned above, there is considerable research on working at home/virtually in different disciplines. A small amount of this is referenced in the first few pages. The positioning of your work within the existing body of knowledge needs to be strengthened. For example, on page 4, you provide some references to previous work where you conclude previous research was restricted to two types of work, and/or did not examine causation. While I agree the effect of remote work will be moderated by the type of task, your summary of past work is very limited. Unfortunately (and somewhat ironic given the topic of your paper), I am prepared this review from home and can't access my office due to COVID restrictions. If I could, I could point you to some previous summaries of the literature (e.g., I know I have a fairly old book that is a review of past work in the area); however, there have been summaries of literature on various related topics published in journals over the years so you should be able to strengthen your grounding with some more comprehensive searching.

My expertise

My home discipline is Information Systems, with largely a focus on topics that bridge organizational behaviour (i.e., technology-enabled work practices). A major research area of mine for the first 15 years or so of my career was investigating remote work and remote teamwork (virtual teams). Therefore, while I am not totally up to date with the literature, I have a reasonable sense of the relevant knowledge base. My knowledge of the statistical techniques used in this paper is definitely limited and that is an important limitation the editors should be aware of. I am not very familiar with Differences-in-differences modelling. I am comfortable with statistical techniques such as regression, SEM, and multi-level modelling (to a limited degree). I interpreted the supplemental tables as I would regression results. It is important that someone on the review team can assess the validity of the analyses in detail, as I have assumed the various approaches used by the authors meet standard expectations (e.g., CEM weighting, etc.). My expertise in terms of research methods includes surveys, experiments, case studies (including qualitative data analyses), largely from a soft-positivist view.

Author Rebuttal to Initial comments

Dear Nature Human Behaviour Editors and Reviewers:

Thank you for the opportunity to revise and resubmit our work to Nature Human Behaviour. We are glad that you see its potential contributions to both policy and research, and appreciate the helpful feedback and comments you have offered. We have taken great care to respond to each of your requests and to revise our submission accordingly. The new revision represents a good faith effort on our part of addressing every reviewer concern. A summary of the substantial changes we have made follows herein.

In responding to all of the helpful reviewer comments, we significantly rewrote most, if not all, of the manuscript. We chose not to highlight all of the changes, as it would require us to highlight nearly the entire paper. This would have made the paper difficult to read. Instead, we chose to place a disclaimer at the beginning of the paper.

Editor

You will see from their comments that the reviewers are somewhat divided in their views of the contribution your manuscript makes. From an editorial standpoint, we feel that your study has the potential to offer an evidence-based advance by bringing a large-scale dataset to bear on an existing question, provided that it can be strengthened with additional analyses and evidence. Specifically, in order for us to consider your study further on this basis, your revision must address the following (as well as all other reviewer comments):

We have overhauled the entire paper in response to the excellent reviewer feedback we received. Detailed responses follow in our responses to specific reviewer comments, but below we will give you a high-level overview of how we have responded to each of your five points.

1. Carry out the additional analyses requested by Reviewer 3 to demonstrate impacts on work outcomes, including new data to the extent possible.

Reviewer 3 offered two avenues for improvement in this regard and we explored both thoroughly. First, they suggested analyzing work outcomes on a subset of our data. To do so we focused on the software developers in our data set and analyzed their work output in terms of lines of code written, number of code submissions, pull requests etc. We present all of the details of this analysis in response to Reviewer 3. As you will see, we conducted extensive analyses looking for signal in this data. Unfortunately, our analysis was too underpowered to yield any statistically significant results because the number of developers working remotely before the work from home (WFH) mandate was too small and the measures of developer productivity available to us did not carry very much signal.

Their second suggestion of conducting more network analyses, however, was much more fruitful. As a result, we have dramatically expanded the network analysis section of our paper (see pages 9-14). The story that emerges is that firm-wide remote work caused the collaboration network among employees to become more siloed, with fewer connections between siloes and more connections within the siloes, and more static, with fewer connections being added and fewer connections being deleted. We believe this reviewer's comments have substantially improved our paper and we are grateful for it.

2. Address Reviewer 3's concerns about the clarity and validity of your causal identification strategy.

We now devote over three pages (pages 27-29) in our Methods section to describing our causal identification strategy, stating the assumptions that we make for that strategy, and validating the plausibility of those assumptions with evidence and analysis.

3. Ensure that claims are fully supported. Reviewer 3 highlights several ways in which more evidence is needed, including the need for further justification of variables, explanation of the data, and consideration of alternative operationalizations and measures.

We have updated the framing of the paper, and in our reframing of the paper we focus on the effects of firm-wide remote work on collaboration and communication, rather than productivity and innovation where the data was less able to support the claims we were making.

We also added more details to the Methods section on the data collection process and how each of the measures in the paper is calculated. We also now consider alternative definitions of added ties, and calculate multiple different measures of concentration of the distribution of time spent with collaborators.

4. Ensure that the context and limitations of your study are thoroughly discussed, including the limitations on generalizability highlighted by the reviewers.

Thank you for your feedback. We have expanded the paragraph in the Discussion section to address all of the limitations brought up by the reviewers. For your convenience we have copied the text here:

Our work is not without its limitations. First, our study characterizes the impact of firm-wide remote work on the U.S. employees of one major technology firm. Although we expect our results to generalize to other technology firms, this may not be the case. Caution should also be exercised in generalizing our results to other sectors and other countries. Second, the period of time over which we measure the causal effects of remote work are quite short (three months), and it is possible that the long-term effects of firm-wide remote work are different. For example, at the beginning of the pandemic, workers were able to leverage existing

network connections, many of which were built in person. This may not be possible if firm-wide remote work were implemented long-term. Third, our analysis treats the effects of firm-wide remote work on peoples' collaboration networks and communication media usage as separate, whereas these two types of effects may interact and exacerbate one another. Fourth, while we believe that changes to workers' communication networks and media will affect productivity and innovation, we are unable to measure these outcomes directly. Finally, our ability to make causal claims is predicated on the validity of our modified DiD framework's identifying assumptions: parallel trends, conditional exogeneity after matching, and additively separable effects. Although we have taken steps to verify the plausibility of these assumptions and tested the robustness of our results to an alternative matching procedure [56] (see the Methods section for more details), they are assumptions nonetheless.

5. More fully situate your study within existing literature, as requested by both Reviewers 2 and 3.

Thank you for your feedback. As we note in our response to Reviewer 3, we have updated the introduction section of the paper, and now spend much more time situating our study within the existing research literatures that study networks in business organizations, communication media choice/usage, and virtual teams / remote work. In addition, we now devote the entirety of page 5 to describing previous work on remote work, virtual teams, and telecommuting, and articulate our specific contributions to this large literature. As Reviewer 3 suggests, one way in which we differentiate our work from prior work is the scale of data we analyze. Second, the fact that we analyze a natural experiment allows us to estimate causal effects of firm-wide remote work on collaboration networks and communication media. Research with causal identification under the Neyman-Rubin causal model is specifically called out as an important way to advance the field in the recent survey of the telecommuting literature by Allen et al (2015):

“Moreover, given the difficulty of conducting random-assignment experimental studies on a large scale, it would be useful to consider other analytic models (e.g., Rubin analytic model, regression-discontinuity approaches) to help support or refute causation in telecommuting studies (Hanges & Wang, 2012).”

Finally, our modified DiD analysis allows us to measure and isolate the effects of an individual switching to remote work from their collaborators switching to remote work. The two measurements will be of increasing importance going forward as firms consider hybrid, mixed-mode, and fully remote work policies.

Reviewer 1

This is a truly great paper - really fantastic. Amazing data, completely believable results and well executed. Indeed, I would love to cite this when it's published and learned a lot. A few minor points:

Thank you for your support of the manuscript. We are glad that you found the data interesting and the study to be well executed. We also appreciate your feedback and have worked to improve the paper as described below.

- Entropy is (for economics) a strange concept. Is there some easier to explain measure - concentration for example?

Thanks for this feedback. The main insight we were trying to communicate with entropy as an outcome measure was that the shift to firm-wide remote work was making workers spend more time with their strong ties and less time with their weak ties. In order to make this point more directly and also make our results more easily interpretable, we have replaced our entropy outcome with one that directly measures the share of time that workers spend with their "weak" ties (i.e., those below the worker-level median in terms of time spent in a given month). Our descriptive and causal results for this new "share of time spent with weak ties" outcome are shown in the updated Figures 2(h) and 3, respectively. The relevant portions of these figures are copied below for convenience:

Figure 2(h):

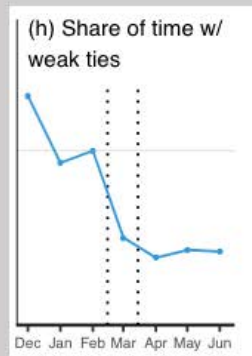
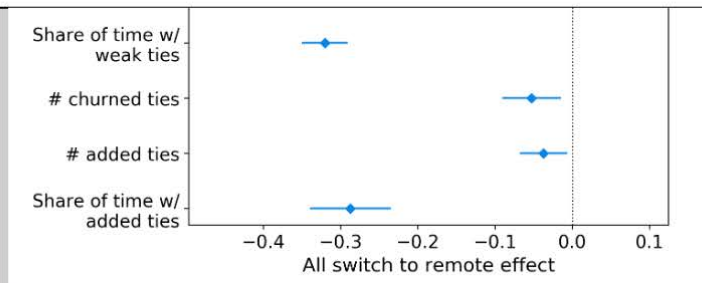


Figure 3:



And the portion of the results section where we discuss our results for this outcome are copied below:

The ability of a worker to effectively access knowledge from other parts of an organization is a function not only of the organizational and/or topological diversity of their connections, but also of the strength of those connections. For each month, we classify ties as “strong” when they are in the upper 50% of an employee’s ties in terms of hours spent communicating, and as “weak” otherwise. Although by definition, weak ties will always get less of an employee’s time than strong ties in a given month, we find that the shift to remote work reduced the share of time workers spent collaborating with weak ties by .32 FV, ($P < 0.001$).^{6,7} These results indicate that above and beyond the impact of firm-wide remote work on the organizational and structural diversity of workers’ ego networks, the shift to firm-wide remote work also made the allocation of workers’ time more heavily concentrated.

We have kept our previous entropy results as a robustness check on this new analysis, and have also added an additional robustness check where instead of measuring the effect of firm-wide remote work on the entropy of workers’ time spent across their different colleagues, we measure a normalized version of the Herfindahl-Hirschman Index (HHI) for the same distribution of time spent. These robustness checks are described in footnote 7, which is copied below:

Since the median is just one possible cutoff to distinguish between strong and weak ties, we also analyze the entire distribution of collaboration time for each worker and confirm that the average ego-level normalized Herfindahl-Hirschman Index (HHI) [43] of the collaboration time is increased by remote work, and that the average ego-level Shannon entropy [44] of collaboration time is decreased by remote work. The effects of firm-wide remote work on both of these outcomes are found in Table S2.

Table S2, which contains our entropy and normalized HHI results, is copied below:

	# Churned ties	# Added ties	% time with added ties	Network entropy	HHI
Ego switch to remote (δ)	0.014 (0.013)	-0.043*** (0.011)	-0.092*** (0.019)	-0.032*** (0.004)	0.127*** (0.030)
Collab. switch to remote (β)	-0.066*** (0.019)	0.005 (0.017)	-0.203*** (0.030)	-0.039*** (0.006)	0.240*** (0.054)
Total effect ($\delta + \beta$)	-0.053** (0.019)	-0.038* (0.016)	-0.295*** (0.027)	-0.070*** (0.005)	0.367*** (0.048)
Indiv. FE	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y
R^2 (Total)	0.793	0.789	0.535	0.875	0.689
R^2 (w/o FE)	0.000	0.000	0.005	0.009	0.004
N	61,861	61,861	61,861	61,861	61,861

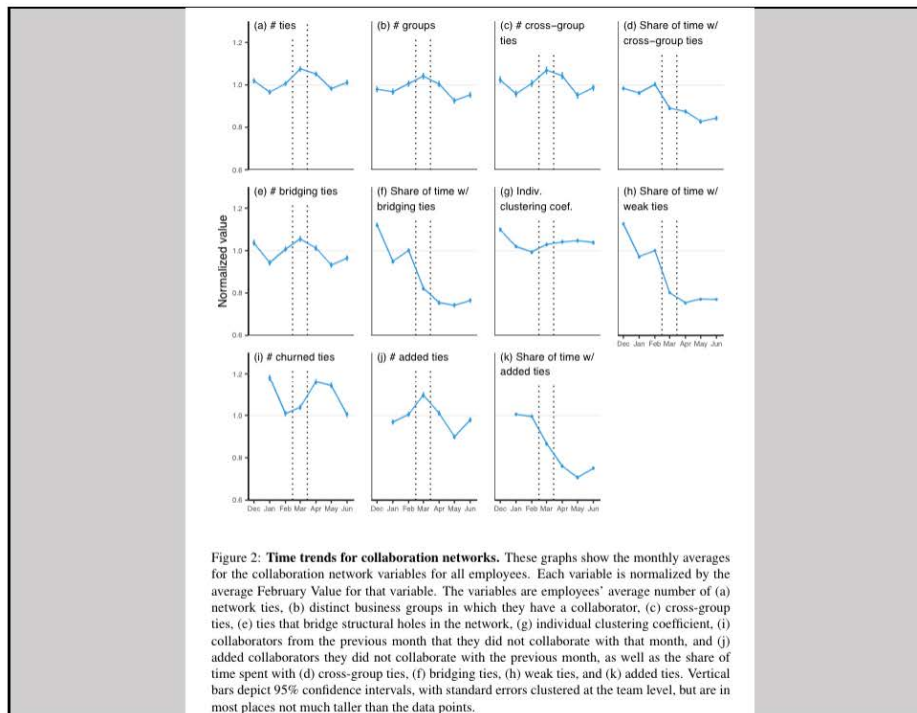
Table S2: **Effects of remote work on collaboration networks (part 2).** These are coefficients from regressions based on Equation (1). The first line is effect of an employee switching, the second line is the effect of all an employee's collaborators switching and the third line is the sum of the two, all normalized by the average level in February. All regressions include individual and month fixed effects. The dependent variables are the number of churned ties, the number of added ties, the share of time spent with added ties, the Shannon entropy of the distribution of time spent collaborating with each of their collaborators, and the normalized HHI index of the distribution of time spent collaborating with each of their collaborators. Standard errors are clustered at the level of the employee's manager. * indicates $p < 0.05$; ** indicates $p < 0.01$; *** indicates $p < 0.001$.

- Figure 2 might look nicer (or better anyway) with CI bars on it (or maybe that is not Nature house style)? At least for economics journals it would be normal to have CI bars around each point.

Thanks for this comment. Figure 2 in our submitted manuscript did have CI bars on each point, however, they were so small that we think it may have appeared that we were using short lines, as opposed to round circles, to mark points on the graphs. Also, for at least one subfigure (Figure 2c), the CI bars were too narrow to show up clearly in the figure.

To reduce confusion, we have changed figure legends elsewhere in the paper, and have tried to reformat Figure 2 to make the distinction between data points and error bars clearer, where possible. We have also made clear in the Figure 2 caption (and in captions elsewhere in the paper) which visual components correspond to data points, and which correspond to error bars.

The updated version of Figure 2 is copied below:



- It was very odd for me reading Figure 3 before seeing the estimating equation and data. It felt like a "trust me" moment, but again I think that's maybe Nature style (put the details at the back)? Ideally I would want to know the data and approach underlying any results in advance as it helps to interpret them.

The order of the sections is dictated by the Nature Human Behaviour style guidelines, which we follow. We agree that this leads to a "trust us" moment, but our hands are tied in this regard.

- Figure 4 would seem better in a Figure 3 set-up (coefficients with standard-errors). I don't think the time series for these is particularly important, but I do think CIs are so I can see what is (or is not) significant (versus noise)?

Figure 5 has the communication media results analogous to the network results in Figure 3. We've left Figure 4, which is analogous to Figure 2, for people who want to be able to compare the causal effects and the non-causal time trends.

As noted above for Figure 2, Figure 4 has CI, but some of them are too small to be seen clearly. As with Figure 2, we have tried to reformat Figure 4 to make the distinction between data points and error bars clearer, where possible. We have also made clear in the Figure 4 caption which visual components correspond to data points, and which correspond to error bars.

For convenience, the updated Figure 4 is copied below:

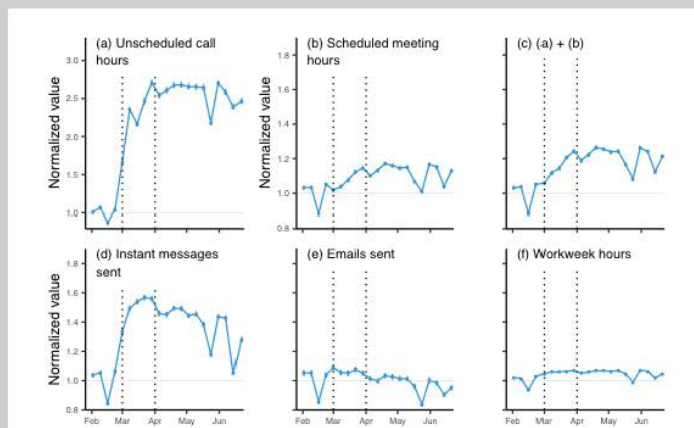
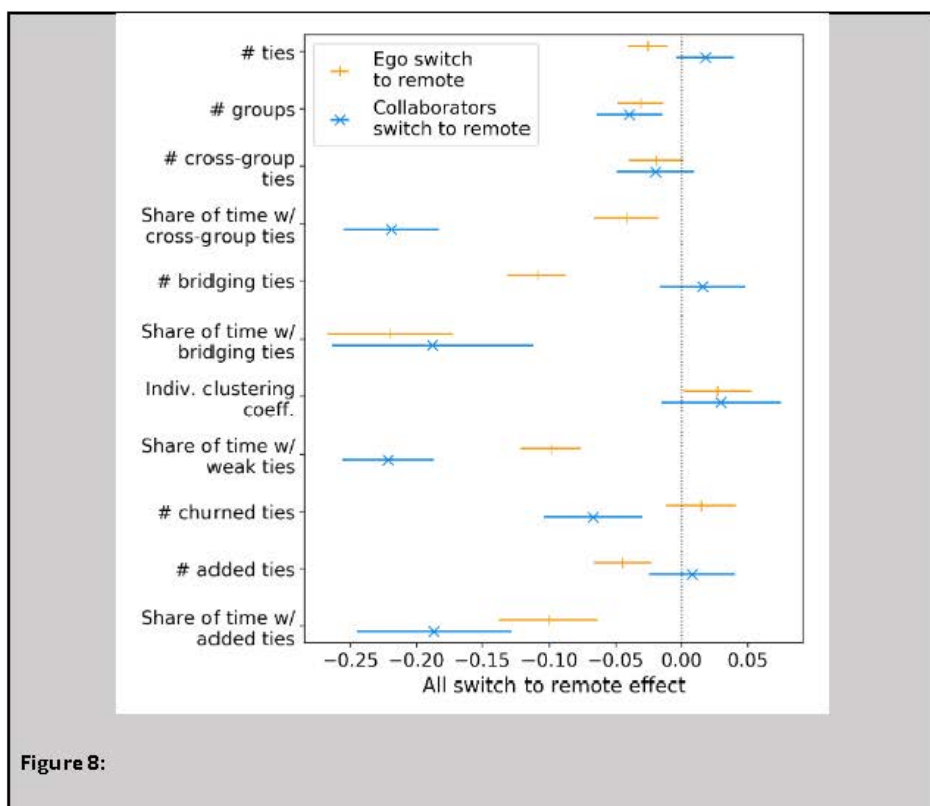


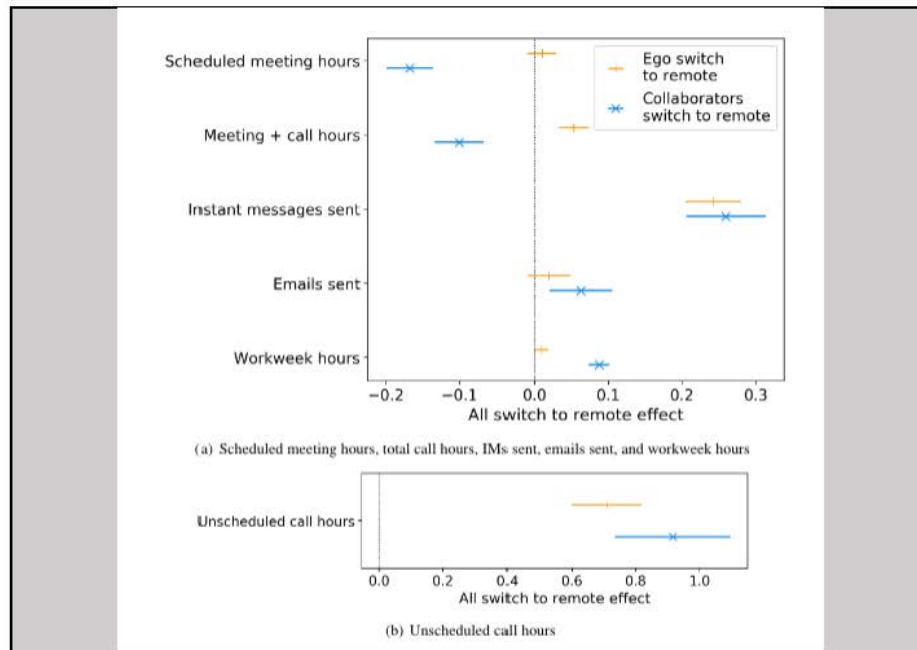
Figure 4: Time trends for communication media. These graphs show the weekly averages for each variable, normalized by the average February Value for that variable. The variables are the employees' average number of a) unscheduled audio/video call hours, b) scheduled meeting hours, c) total hours in scheduled meetings and unscheduled calls (sum of panels (a) and (b)), d) instant messages sent, e) emails sent, and f) hours between the first and last activity (sent email, scheduled meeting, or Microsoft Teams call or chat) in a day, summed across the workdays. Vertical bars depict 95% confidence intervals, with standard errors clustered at the team level, but are in many places not much taller than the data points in the graphs.

- Figures 7, 8 etc were hard to read (I physically printed the paper to read it first actually) as one of the colors was very light. I would personally make both darker so it's easy to see.

Thank you for this feedback. We have updated the colors in Figures 7 and 8 so that they are darker and easier to see. The updated versions of Figures 7 and 8 are copied below:

Figure 7:





- On terminology what you call "Hybrid WFH" is normally called "Mixed-mode", which is some people in the office and some at home. What you call "some teams come in on some days..." is usually called "Hybrid", for example the Google 3-2-2 model. I am aware this terminology is not standardized, but from talking to a lot of CEOs, execs and journalists this seems to be the most common way to define these terms.

Thank you for making us aware of this terminology. We've updated our language throughout the paper so that we now distinguish between these two different types of work arrangements, and reference hybrid and/or mixed-mode where appropriate. For instance, an updated excerpt of the introduction, in which we define both hybrid and mixed-mode work, is copied below:

More generally, COVID-19 has accelerated the shift away from traditional office work, such that even firms that do not keep full-time remote work policies in place after the pandemic has ended are unlikely to fully return to their pre-COVID-19 work arrangements [7]. Instead, they are likely to switch to some type of hybrid work model, in which employees split their time between remote and office work, or mixed-mode model, in which firms are comprised of a mixture of full-time remote employees and full-time office employees. For example, some scholars predict a long-run equilibrium in which knowledge workers will work from home approximately 20% of the time [1]. In order for long-term policy decisions regarding remote,

hybrid, and mixed-mode work to be well-informed, decision makers need to understand how remote work would impact information work in the absence of the effects of COVID-19. To answer this question, we treat Microsoft's company-wide WFH policy during the pandemic as a natural experiment that, subject to the validity of our identifying assumptions, allows us to causally identify the impact of firm-wide remote work on employees' collaboration networks and communication practices.

- Finally, i fully agree with the sentiment of the conclusion. If people are in a hybrid situation with the entire team coming in 3 days and WFH for 2 days that is probably the best of both worlds. Enough time in the office to bridge and bond and enough time at home to work quietly and save on commute. And yes Mixed-mode is typically seen as an absolute disaster - i have yet to talk to a single manager who likes it (and I've probably spoken to maybe 200 to 300 managers since March 2020).

Thanks for this insight! It's reassuring to know that our results are in line with feedback collected from managers during interviews.

Reviewer 2

The paper is timely because it addresses some critical issues relating to working from home versus working in the office. It is well written with some minor suggestions for improvement:

1. This is a US-centric paper with only data collected in the United States. There should be a justification for why you chose the US because working from home and during COVID-19 is an issue across the globe. Are the lessons or insights for other countries?

Thank you for pointing this out. We would have loved to analyze data from the entirety of Microsoft's global workforce, however, some countries have data-privacy / employee-protection laws that made it infeasible to use data for employees in those countries. Hence our focus on the US. We have now clarified this in the Data subsection of the Methods section. The relevant text is copied below:

Employee privacy restrictions in many countries prevent us from reporting on workers outside the U.S. However, an employee's communication and collaboration with international coworkers is still included in the data and those employees are still counted as part of each employee's network.

We also now briefly note in the Discussion section of the paper that our results, which describe the effects of firm-wide remote work on U.S. information workers, may not generalize to other countries. For your convenience, the relevant text is copied below:

[O]ur study characterizes the impact of firm-wide remote work on the U.S. employees of one major technology firm. Although we expect our results to generalize to other technology firms, this may not be the case. Caution should also be exercised in generalizing our results to other sectors and other countries.

2. What was Microsoft's policy on working from home prior to the pandemic? I understand some employees already worked from home; however, what was the prevalence of this mode of working? Were there metrics in place to measure the effectiveness of working from home pre-COVID (including collaboration and communication)?

Thank you for this great question. Prior to pandemic, Microsoft did not have a company-wide policy on flexible work arrangements, and left this up to the discretion of managers. That being said, flexible work arrangements prior to the pandemic were the exception, rather than the norm. We now note this in footnote 3, which is copied below for convenience:

Prior to the pandemic, managers at Microsoft used their own discretion in deciding whether an employee was allowed to work from home. During this period, flexible work arrangements (including working from home) were the exception, rather than the norm.

According to our definition of remote work, 18% of U.S. Microsoft workers were working remotely prior to the pandemic, whereas the remaining 82% were not. We now note this in the Results section of the paper. The relevant text is copied below:

Prior to the onset of the pandemic, 18% of U.S. Microsoft employees were working remote from their collaborators. For this subset of employees, the shift to firm-wide remote work did not cause a change in their own remote work status, but did induce variation in the share of their colleagues who were working remotely. For the remaining 82% of U.S. Microsoft employees, the shift to firm-wide remote work induced variation in both their own remote work status and in the remote work status of their coworkers.

With our dataset, we are unable to measure the prevalence of ad-hoc remote work prior to the pandemic (e.g., someone working from home because they are sick, have a maintenance worker coming to their home, etc.).

As far as we know, there were not any metrics in place to measure the effectiveness of WFH prior to the COVID-19 pandemic. However, the data we use is from Microsoft's Workplace Analytics tool, which passively collected telemetry data for Microsoft's US workers even prior to the onset of the COVID-19 pandemic. As a result, we are able to observe the time series for each of our outcomes for at least one month, if not longer, before the firmwide shift to WFH. For instance, Figure 1, which is copied below, shows the

monthly time series for number of bridging ties back to December 2019, and the weekly time series for unscheduled call hours back to the first week of February 2020:

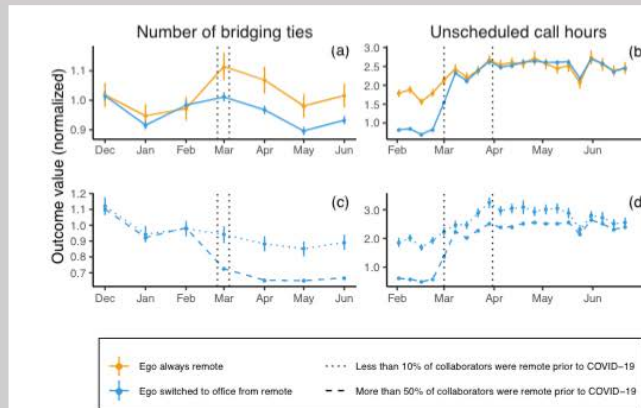


Figure 1: Time trend comparisons These graphs show the number of bridging ties per month (Graphs (a) and (c)) and the average unscheduled video/audio call hours per week (Graphs (b) and (d)) for different groups of employees, and establish the plausibility of the “parallel trends” assumption required by our modified DiD model. Error bars depict 95% confidence intervals, with standard errors clustered at the team level, and are in some places thinner than the symbols in the figure. Graphs (a) and (b) show employees who, prior to COVID-19, worked from the office (blue) and a matched sample of employees who worked remotely (orange). Graphs (c) and (d) show two sub-groups of the blue lines in (a) and (b): employees who, prior to COVID-19, had less than 10% of their collaborators working remotely (dotted) and those who had more than 50% of their coworkers working remotely (dashed). All the time series are normalized by the monthly average share of the relevant outcome across all employees during February 2020. Most employees transitioned to WFH during the week of March 1, 2020, though our analysis omits the month of March as a transition period.

3. It would be useful to include that point in the background that some organisations in the technology sector (for example, Yahoo and IBM) required employees to be in the office for the very reasons the authors are suggesting are problematic (collaboration and communication). That is, not all organisations in the tech sector or elsewhere are necessarily going to find working from home or even a hybrid model suitable (or were unsuccessful in previous years). I suggest including this point because it shows that this mode of working has been tried before – what will be different this time?

Thank you for pointing this out. In the final paragraph of the Discussion section, we now point out that organizations such as Yahoo! and IBM have experimented with (and ultimately retracted) flexible work policies, and briefly discuss why we think it is possible that flexible work arrangements may remain prevalent even after the pandemic has ended. The relevant text is copied below:

There are multiple high profile cases of firms such as IBM and Yahoo! enacting, but ultimately rescinding, flexible remote work policies prior to COVID-19, presumably due to the impacts of these policies on communication and collaboration [57, 58]. Based on these examples, one might conclude that the current enthusiasm for remote work may not ultimately translate into a long-lasting shift to remote work for the majority of firms. However, during the COVID-19 pandemic, workers and firms have invested in the physical and human capital required to support remote work [59] and innovation has shifted toward new technologies that support remote work [60]. Both of these factors make it more likely that for many firms, some version of remote work will persist beyond the pandemic. In light of this fact, the importance of deepening our understanding of remote work and its impacts has never been greater.

4. Developing relationships with new contacts can only occur if there is a mechanism of some kind to bring new people together (for example, working on a new project with new colleagues). Otherwise, there are no opportunities for workers to meet new colleagues. That is why we have face to face meetings and social gatherings. This is very difficult to replicate online and can be very stressful for some employees. How can new and existing employees have opportunities to meet and develop relationships? The finding that people tended to rely on existing networks and relationships makes sense in this context.

Thank you for this feedback. We agree that the slight decrease in the number of added connections caused by the shift to firm-wide remote work might have been expected. We also push this analysis one step further by noting that there was a substantial drop in the time spent with added connections. So, it's not just that employees were making adding fewer connections from month-to-month, they were also spending less time with the connections they did add. The portion of the updated manuscript where we present these results is copied below:

firm-wide remote work ... decreased the number of connections workers added month-to-month by .04 FV ($P = 0.015$). Furthermore, the shift to firm-wide remote work decreased the share of time workers spent collaborating with the connections they did add by .3 FV ($P < 0.001$).

We agree that figuring out how to provide opportunities for new and existing employees to meet others and develop firm-wide relationships under firm-wide remote work is an interesting and important question that we hope companies and academics will tackle in the future.

5. The Microsoft experience is likely to be very different from other organisations such as those in the Finance sector, Education, Transport, Tourism and Government workers. This is one case study and the data is very specific to Microsoft. This point should be emphasised.

Thank you for pointing this out. Although we study only one firm, we are trying to use our study to learn about information workers in general. In our view, some of the other sectors

you mentioned above, such as finance, are by and large information work, whereas others (such as tourism) generally are not. In order to address this point, we have included the following text in the paragraph on limitations in the Discussion section of the paper:

[O]ur study characterizes the impact of firm-wide remote work on the U.S. employees of one major technology firm. Although we expect our results to generalize to other technology firms, this may not be the case. Caution should also be exercised in generalizing our results to other sectors and other countries.

6. The method to collect data provides some good insights into how workers communicated and collaborated but only during the pandemic. It is important to stress that this way of working was mandated and employees and their managers had no choice but to adjust.

Thanks for this feedback. Looking forward, more firms are considering switching to an all-remote workforce. So, while the WFH mandate may have been a historically unique event, it may be extremely relevant to a variety of firms in the future precisely because it was mandated. As a result, we added this sentence to the introduction:

Because remote work was mandatory during the pandemic, we are able to quantify the effects of firm-wide remote work, which are most relevant for firms considering a transition to an all-remote workforce.

In order for our results to be as relevant as possible for firms considering an entirely remote work force, it was also important for us to disentangle the impacts of firm-wide remote work *specifically* from other effects of COVID-19 e.g., pandemic-related stress, increased caregiving responsibilities, etc.). We disentangle these two effects using our causal identification strategy, which is an extension of the “difference-in-differences” (DiD) reduced form econometric approach. Assuming that modified DiD’s identifying assumptions are satisfied, our approach allows us to draw inferences about the impacts of firm-wide remote work after the pandemic has ended, even though we only observe data collected during the pandemic. We now state this explicitly in the latter half of the paper’s first paragraph, which is copied below:

In order for long-term policy decisions regarding remote, hybrid, and mixed-mode work to be well-informed, decision makers need to understand how remote work would impact information work in the absence of the effects of COVID-19. To answer this question, we treat Microsoft’s company-wide WFH policy during the pandemic as a natural experiment that, subject to the validity of our identifying assumptions, allows us to causally identify the impact of firm-wide remote work on employees’ collaboration networks and communication practices.

The Results and Methods sections of the paper provide more details on the modified DiD method we use to analyze our data.

7. The statement ‘The most effective way to determine the benefits and downsides of different hybrid work approaches will be through randomized field experiments’ is not supported by any literature. Why have the authors suggested this is the most effective way? The statement needs to be justified. The emphasis appears to be on quantitative methods.

We appreciate you pointing this out. This claim was over-stated. What we were intending to say was that among the set of methods available to quantitative researchers, large-scale field experiments are best-suited to estimating the causal effects of firm-wide remote work as defined by the Neyman-Rubin causal model. Combining such field experiments with qualitative research would increase our understanding even more, and be even more impactful. Thus, we have modified that paragraph to read:

Although we believe these early insights to be helpful, firms and academics will need to undertake a combination of quantitative and qualitative research once the COVID-19 pandemic has ended in order to better measure both the benefits and the downsides of different remote work policies. Large firms, with the ability to collect rich telemetry data, will be particularly well-positioned to build on the quantitative insights presented in this work by conducting large-scale internal field experiments. If published externally, these experiments could have the capacity to greatly further our collective understanding of the causal effects of both firm-wide remote work and other work arrangements such as hybrid work and mixed-mode work. Our results, which report both direct effects and indirect effects of remote work, suggest that such experimentation needs to be conducted carefully. Simply comparing the work practices and/or productivity levels of remote workers and office workers will likely yield biased estimates of the global treatment effects of different remote work policies, due to the causal effects of one's colleagues working remotely. Ideally, such field experiments would be complemented with high-quality qualitative research that can describe emergent processes and workers' perceptions, and more generally, uncover insights beyond those that can be obtained through quantitative methods.

8. To be able to understand how the employees felt about this way of working and if they prefer working from home, how productive they felt they were and whether the technology they used to communicate and collaborate was effective (for the purposes of their ability to achieve the outcomes they need to achieve) aligns with qualitative methods. We would need to have a better understanding from the perspective of managers and their employees on the most effective mode of working will be post-COVID-19 in addition to measuring outcomes.

Good point. We strongly agree that qualitative research aligned with our research questions would shed even more light on the effects of firm-wide remote work than quantitative research can do alone. We've added a sentence to this effect to the Discussion section of the paper, which is copied below:

Ideally, such field experiments would be complemented with high-quality qualitative research that can describe emergent processes and workers' perceptions, and more generally, uncover insights beyond those that can be obtained through quantitative methods.

Reviewer 3

Key Results

Data on collaboration patterns and communication technology use were collected from nearly 62,000 US Microsoft employees (9,727 managers and 52,135 non-managerial positions) from February 2020 to June 2020. During this time, all employees had to shift to work-at-home (WAH – or WFH work from home) work (early March) due to COVID. This context provided a natural experimental setting.

Analyses was done to compare February collaboration patterns and communication media use (pre-treatment/shift to WAH/WFH). The results indicated no significant change in the number of connections made (i.e., people collaborated with); however, fewer “new” connections were made and less time was spent with the “new” connections (new was defined as someone who was not contacted the previous month). More time was spent with existing connections (presumably defined as people with whom one communicated with twice in the previous month). There was also a negative pattern with the number of groups interacted with. The analyses of the use of communication media found that the number of unscheduled calls (audio or video using Microsoft Teams) and the number of instant messages sent increased dramatically.

Validity

When I was invited to review this paper and provided with the abstract, the final sentence of the abstract interested me, as it suggested important findings (especially given the large data set). It says: “Our results suggest that firm-wide remote work may lead to communication patterns that hinder the collaborative production of high-quality knowledge work...”. After reviewing the paper carefully, I was disappointed in the support for this suggestion, and for many of the suggestions in the paper about the effect of remote work on collaboration. Given what was measured, there is no empirical evidence to allow the authors to determine if the communication patterns did affect the collaborative production of high-quality knowledge work.

Thank you for this comment. We agree with you that the previous draft of the paper was trying to make a connection between the effects we observe (which are on collaboration networks and communication media) and potential effects on productivity/innovation, and this connection was not well-supported by our data. We reframed the paper so that it now explicitly focuses on measuring the impacts of firm-wide remote work on collaboration and

communication, and have updated the abstract and introduction accordingly. The updated abstract can be found below:

COVID-19 caused a rapid shift to full-time remote work for many information workers. Viewing this shift as a natural experiment, where some workers were already working remotely prior to the pandemic, allows us to separate the effects of firm-wide remote work from other pandemic-related confounders. We use rich data on the emails, calendars, instant messages, video/audio calls, and workweek hours of nearly 62,000 U.S. Microsoft employees over the first six months of 2020 to estimate the causal effects of firm-wide remote work on collaboration and communication. Our results show that firm-wide remote work caused workers' collaboration network to become more static and siloed, with fewer bridges between disparate parts. Furthermore, there was a decrease in synchronous communication and an increase in asynchronous communication. Together, these effects may make it harder for employees to acquire and share new information across the network.

Now, the only place in the paper where we discuss the impacts of firm-wide remote work on productivity and innovation is in the Discussion section, where we speculate there could be an impact on productivity and, in the long-term, innovation. The relevant text is copied below:

We expect the effects we observe on workers' collaboration and communication patterns to impact productivity and, in the long-term, innovation.

Furthermore, we now explicitly note in our limitations paragraph that we are unable to observe the effects of firm-wide remote work on productivity/innovation with our data, as found below:

Fourth, while we believe that changes to workers' communication networks and media will affect productivity and innovation, we are unable to measure these outcomes directly.

The theory presented for the possible effect of collaboration on information work is the need for structurally diverse networks (both strong and weak ties) so one has access to novel ideas to stimulate innovation. As I read through the introduction and this logic, it led me to possibly expect network analyses and some relevant outcome such as novel ideas/information shared. However, the data and results presented were limited to communication and collaboration patterns, as outlined above. Also, beyond splitting the respondents by managerial versus non, there is no data on the type of work done, which could affect the need for information and innovation. Having access to diverse ideas is important for some types of work, but less in others. And the interdependence of activities can affect how much one person relies and needs to interact with others to get their job done well.

So overall, while it appears the analyses presented were validly done (see my comments below about my limited ability to assess this), the more general implications for the results lack much

validity.

Thank you for this feedback. We have reframed the paper to focus on the effects of firm-wide remote work on communication and collaboration. As a result, we have modified and expanded the manuscript in numerous ways in response to your comments, including:

- The network analysis section is substantially longer and more in depth than it was previously. For example, we now analyze the effects of firm-wide remote work on measures like clustering coefficient, network constraint, and a notion of tie strength that is more consistent with previous research.
- As a first step towards understanding how the effects of firm-wide remote work depend on the type of work done we now look for heterogeneous treatment effects for engineers vs. non-engineers, managers vs. non-managers, and employees who at have been at Microsoft a short time vs. a longer time.
- Explicitly stating that the potential impacts of the effects we observe on productivity and innovation will almost certainly be mediated by factors such as the types of tasks that workers are performing.

In our point-by-point responses that follow, we expand on these improvements, as well as others that have been made to the manuscript.

Originality and Significance

The volume of longitudinal data during the shift to work at home presents a significant opportunity to gain insights regarding the effect of this shift. My invitation to review listed several categories of acceptable manuscripts. As far as I can tell, there is no methodological or conceptual novelty (i.e., there is no theoretical contribution). The category that the paper might fit into is an evidence-based advance. This is defined as acceptable “if its scale and/or rigour supersede the existing literature and significantly strengthen confidence in a scientific finding or convincingly falsify it.

The scale of the data collected and analysed is impressive. While I cannot claim to know every study on remote work, I can’t recall ever seeing a study have such a large number of respondents. So do the results also significantly strengthen confidence in previous findings (or falsify them?

The work isolation caused by forcing people to work from home did change some of the collaboration patterns. There are various terms used in research that are relevant to WAH including telecommuting, remote work, virtual work, virtual teams. These various work practices have been studied by different disciplines (e.g., Information Systems Organizational Behaviour, Psychology, etc.) for at least 3 decades, with more emphasis on virtual teams lately. Telecommuting and work at home has existed even longer, since this type of work was possible

for some roles before communications technology evolved/improved (which made the practice possible for a larger number of types of jobs). Certainly an important thread of study in all these practices is how to create “effective” communication to enable productive work. There are various theoretical perspectives applied and studies that attempt to understand the nuances of what effective means and how to enable it (e.g., creating psychological safety, avoiding attribution errors, etc.). So, is a large-scale study that confirms that remote work changes collaboration patterns a significant contribution? I would lean towards thinking no. However, I found it interesting that the number of connections did not significantly decrease (Table S1 – Total effect). The main finding is that an individual forced to WAH narrows their focus to existing connections, such that “new” connections and the number of groups interacted with decreases. I am not surprised by this, as it would be easier to reach out to colleagues that you already have a relationship with, and that you have on-going interdependent activities with.

Thank you for your feedback. We have updated the introduction section of the paper, and now spend much more time situating our study within the existing research literatures that study networks in business organizations, communication media choice/usage, and virtual teams / remote work. In particular, we now devote the entirety of page 5 to describing previous work on remote work, virtual teams, and telecommuting, and articulate our specific contributions to this large literature. For convenience, the final two paragraphs of the introduction are copied below:

There is a large body of academic research across multiple disciplines that has studied remote work, virtual teams, and telecommuting (see [31] for a review of much of this work), including prior work that examines the network structure of virtual teams and how individual network position in virtual teams correlates with performance [32, 33, 34]. During the COVID-19 pandemic, there has been renewed public and academic interest in how virtual teams function. Recent analyses of telemetry and survey data show that the pandemic has affected both the who and the how of collaboration in information firms: while working remotely during the pandemic, workers are spending less time in meetings [35], communicating more via email [35], collaborating more with their strong ties, as opposed to their weak ties [36], and exhibiting patterns of communication that are more siloed and less stable [37]. However, these analyses, like much of the prior research on remote work, virtual teams, and telecommuting, are non-causal [31], and thus, are unable to separate the effects of remote work from the effects of pandemic-related confounders, such as reduced focus due to COVID-19-related stress or increased caregiving responsibilities while sheltering in place. Although prior research on the causal effects of remote work does exist, this work has mainly studied employees who volunteer to work remotely, and has focused on settings such as call centers and patent offices [38, 39], where relative to the majority of information work, tasks are more easily codifiable and less likely to depend on collaboration or the transfer of complex knowledge.

We contribute to the research literatures on remote work, virtual teams, and telecommuting by analyzing the large-scale natural experiment created by Microsoft's firm-wide WFH policy during the COVID-19 pandemic. Because remote work was mandatory during the pandemic,

we are able to quantify the effects of firm-wide remote work, which are most relevant for firms considering a transition to an all-remote workforce. Furthermore, because our model specification decomposes the overall effects of firm-wide remote work into ego remote work and collaborator remote work effects, our results also provide some insight into the possible impacts of remote work policies such as mixed-mode work and hybrid work.

To expound on some of the points we make in the text copied above, and as the reviewer suggests, one way in which we differentiate our work from prior work is the scale of data we analyze. Second, the fact that we analyze a natural experiment allows us to estimate causal effects of firm-wide remote work on collaboration networks and communication media. Research with causal identification under the Neyman-Rubin causal model is specifically called out as an important way to advance the field in the recent survey of the telecommuting literature by Allen et al (2015):

“Moreover, given the difficulty of conducting random-assignment experimental studies on a large scale, it would be useful to consider other analytic models (e.g., Rubin analytic model, regression-discontinuity approaches) to help support or refute causation in telecommuting studies (Hangs & Wang, 2012).”

Finally, our modified DiD analysis allows us to measure and isolate the effects of an individual switching to remote work from their collaborators switching to remote work. The two measurements will be of increasing importance going forward as firms consider hybrid, mixed-mode, and fully remote work policies.

I will leave it to the editors to decide if the paper fits what they feel is an evidence-based advance. Certainly the topic is very timely and will be of interest to many people and organizations.

Data and Methodology and Analytical Approach

A key strength of this study is the longitudinal data, subject to the shortcomings and suggestions presented below. I would like to have seen more explanation about how the data were collected and how the various constructs were conceptually defined, and why they were operationalized the way they were. This is explained somewhat in the methods section, but more information would be helpful. What data was measured in February? It is not clear to me if for the people working at a corporate office, did the measure of scheduled and unscheduled meetings include non-Teams interaction? At this point, likely much of the informal interaction was F2F. On page 13, you suggest that the total synchronous communication when down, both on and off Teams. So do you have a measure that captured synchronous scheduled and unscheduled meetings, regardless of the communication medium used (i.e., it includes F2F, as well as technology-mediated communication)? Also, could you provide descriptive information on the characteristics of the participants and their jobs/tasks? This could help the reader understand the generalizability of your findings.

Thank you for this feedback. We have added some more detail to the data description in the methods.

With respect to the data collection, the data in our study is passively collected for all employees in our sample from two sources: Microsoft Teams, which is a workplace productivity tool that allows employees to text chat, video chat, schedule meetings, and also by Microsoft's corporate email *and calendar* accounts. The data collection procedure is the same in February as it is throughout the rest of our sample. So, any scheduled F2F meetings occurring in February will show up in our data as "scheduled meetings," any informal F2F interactions that the employee did not put on their calendar were not captured by our dataset. Because of this limitation, we are in some instances only able to *bound* the effects of the treatment on synchronous F2F interaction, as we do not know the magnitude of the unmeasured F2F interaction occurring in February. But, we do know that unmeasured F2F interaction after the WFH mandate essentially went to zero by definition, since workers were unable to meet F2F while WFH. Although not used to estimate our models, we were also able to extend the collection of our collaboration network data back to December, which allows us to test our modified DiD model's parallel trends assumptions for our network outcomes. Unfortunately, we were still not able to access communication media data from months prior to February.

In order to make it clearer what data we've collected, we have expanded and clarified the descriptions of our dataset that are found in the Results and Methods section of the paper. The relevant excerpts are copied below.

Results

We analyze anonymized individual-level data describing the communication practices of 61,967 U.S. Microsoft employees from December 2019 to June 2020, which gives us data from before and after Microsoft's shift to firm-wide remote work.² Our sample contains all U.S. Microsoft employees except for those who hold senior leadership positions and/or are members of teams that routinely handle particularly sensitive data. Given the scope of our dataset, the workers in our sample perform a wide variety of tasks, including software and hardware development, marketing, and business operations. For each employee, we observe 1) their remote work status prior to the COVID-19 pandemic, and what share of their colleagues were remote workers prior to the COVID-19 pandemic, 2) their managerial status, the business group they belong to, their role, and the length of their tenure at Microsoft as of February 2020, 3) a weekly summary of the amount of time spent in scheduled meetings, time spent in unscheduled video/audio calls, emails sent, and instant messages sent, and the length of their workweek, and 4) a monthly summary of their collaboration network.

²Our data on workers' use of communication media only goes back to February 2020.

Methods

Our data were passively collected by Microsoft's Workplace Analytics (WPA) product [61], which logs activity taking place in employees' work email accounts and in Microsoft Teams. Microsoft Teams is collaboration software that allows employees to video/audio call, video/audio teleconference, instant message, and share files. Employee privacy restrictions in many countries prevent us from reporting on workers outside the U.S. However, an employee's communication and collaboration with international coworkers is still included in the data and those employees are still counted as part of each employee's network.

In our collaboration network, each worker is a node. In order for a tie to exist between two workers in a given month, those two workers must have had at least one meaningful interaction through two of the following four communication media: email, instant message, scheduled meeting, and unscheduled video/audio call. A meaningful interaction is an email, instant message, scheduled meeting, or unscheduled video/audio call with a group of size no more than eight.

In our analysis, we classify a worker as working remotely if more than 80% of their collaboration hours in a given month are with colleagues remote to them. For employees working from home, all of their colleagues are considered remote from them, whereas for those in an office, colleagues are remote to them if those colleagues are working from home or are located on a Microsoft campus in a different city. After March 2020, all U.S. Microsoft employees are by definition working remotely, since they are WFH.

With respect to construct definition/operationalization, in many cases we, unfortunately, do not have access to the raw data, but have access to the aggregate metrics made available to clients of Microsoft through the Workplace Analytics tool (you can get a sense of the aggregate metrics available through Workplace Analytics by looking through the [product's documentation](https://docs.microsoft.com/en-us/workplace-analytics/use/metric-definitions): <https://docs.microsoft.com/en-us/workplace-analytics/use/metric-definitions>). Because of this, in some cases we've had to make do with the outcomes we have, as opposed to operationalizing constructs in the way that we find theoretically optimal. For instance, workweek span sums the time between first and last activity in each work day, but does not account for breaks taken during the day.

Due to privacy restrictions, we are not able to report any covariates describing the people in our sample beyond those used in our analysis (tenure at Microsoft, managerial status, role type, and business group), and even for those dimensions of analysis, we are not able to report on summary statistics that might help to characterize our sample (e.g., mean, median, etc.).

It is also difficult to characterize the tasks being done by the workers in our sample, since our sample contains the overwhelming majority of U.S. Microsoft employees. Given that Microsoft is a large corporation doing work across a number of different areas, including hardware development and software development, and the fact that the company employs people in a wide array of jobs ranging from marketing to software engineering to business development, our dataset contains employees doing a number of different jobs and many different tasks. We now acknowledge this fact in the portion of the Results section where we describe our dataset:

Given the scope of our dataset, the workers in our sample perform a wide variety of tasks, including software and hardware development, marketing, and business operations.

And also mention in a Discussion section footnote that our findings may have different implications for worker productivity/innovation depending on the type of tasks that workers/teams are performing:

Even if we were able to measure productivity and innovation, it is difficult to make general claims about how changes to network structure and communication media choice will impact performance, given that these impacts are likely contingent on a number of factors, including the type of task a given team/organization is trying to complete [52, 53, 54, 55].

Suggested Improvements

Experiments have some causal explanatory power because of the use of control groups and random assignment. I understand that since this was a natural experiment, there is no random assignment. I have limited understanding of the DiD statistical technique; however, the reading I did on it certainly emphasized the need for a control group. Do I understand correctly that the control group in your analyses is the employees that were working remotely in February? Clarifying this would be helpful for readers. Assuming that is the logic, then isn't there a confounding factor to the analyses that happens when a remote worker's co-workers all are forced to work from home in March? So in one sense, I am struggling to see how the February WAH employees can be a control group for the duration of the analyses since the "treatment" definitely affected them too.

We apologize that this wasn't clear. You're right that in the standard DiD model, there are distinct "control" and "treatment" groups. In this paper, we are modifying that standard DiD model, and as a result our analysis does not have cleanly delineated control and treatment groups. To make it clearer that our empirical strategy is not the standard DiD, we now refer to our empirical strategy as a "modified DiD" model throughout the paper.

Relative to standard DiD, our model is modified in two ways. First, we are using our model to measure the causal impact of changes to two “treatment” variables – one’s own remote work status, and the remote work status of one’s colleagues. This modification is needed because, as you note, had we estimated a normal DiD model, the fact that remote workers’ co-workers were all forced to WFH in March would confound the causal effect of WFH. The other way in which our modified DiD model is different from the base DiD model is that we need to make additional identifying assumptions, since the COVID-19 pandemic, which is the source of the exogenous variation in both of our treatment variables, affected all workers, even those whose remote work status and share of remote colleagues did not change during the shift to firm-wide WFH.

In the updated draft, we’ve made our modified DiD model (and its identifying assumptions) easier to follow for readers, especially those who may not be familiar with standard DiD. We have done so by adding the following text to the Results section:

We analyze this natural experiment using a modified difference-in-differences (DiD) model. Standard DiD is an econometric approach that enables researchers to infer the causal effect of a treatment by comparing longitudinal data from at least two groups, some of which are “treated” and some of which are not. So long as DiD’s identifying assumptions are satisfied, the causal effect of the treatment is obtained by comparing the magnitude of the gap between the treated and untreated groups after the treatment was delivered to the magnitude of the gap between the groups before the treatment was delivered. Our modified DiD model extends standard DiD by estimating the causal effects of changes in two different treatment variables (one’s own remote work status, and the remote work status of one’s colleagues) and by introducing additional identifying assumptions such that it is possible to draw causal inferences in the presence of an additional shock (in our case, the non-WFH-related aspects of COVID-19) that affects both treated and untreated units, and is concurrent with the the exogenous shock(s) to our treatment variables. The time series trends depicted in Figure 1 suggest that the identifying assumptions of our modified DiD model are plausible; more detail on the model can be found in the Methods section.

And adding the text below to the Methods section:

Our modified DiD model extends standard DiD in two ways. First, rather than measuring the effect of changes in one treatment variable, our model measures the effects of changes in two different treatment variables: 1) whether an employee is working remotely 2) whether that employee’s colleagues are working remotely, and assumes that these two effects are additively separable. Second, our model allows the variation in our treatment variables to be induced by one exogenous shock that affects all workers in our sample, but affects some workers differently than others. More specifically, although all Microsoft employees were

affected by COVID-19, only some employees experienced changes in their remote work status and/or the share of their collaborators that were working remotely due to Microsoft's company-wide WFH mandate during the pandemic.

We estimate the average treatment effect for the treated (ATT) of ego remote work and collaborator remote work on all outcome measures with the following specification:

$$Y_{it} = \alpha_i + \tau_t + \delta D_{it} + \beta s_{it} + \epsilon_{it},$$

where Y_{it} denotes the work outcome, α_i is an employee fixed-effect, τ_t is a month fixed effect, D_{it} indicates whether employee i was a treated employee forced to work remotely in month t , s_{it} is the share of employee i 's coworkers who were working remotely in month t and ϵ_{it} denotes the error term. Observations are weighted using coarsened exact matching (CEM) weights, and standard errors are clustered at the level of an employee's manager. We estimate this model using data from February, April, May and June. We omit March because workers were transitioning from office work to WFH beginning in the first week of the month.

Our ability to causally identify both ATTs is predicated on a number of identifying assumptions, some of which are standard in difference-in-differences analyses and some of which are specific to our research setting. First, we assume that for both of our "treatment" variables, the time series for "treated" and "untreated" workers would have evolved in parallel absent the treatment. Time series trends for different subsets of the matched sample are compared in Figure \ref{fig:did_bridge}. These comparisons suggest that for both of our treatment variables, DiD's parallel trends assumption is plausible, both when measuring the effect of the treatment on network measures (Figures 1(a) and 1(c)) and when measuring the effect of the treatment on communication media measures (Figures 1(b) and 1(d)). Analogous figures for our full set of outcome variables can be found in the Supplementary Materials. In all cases, the time series appear to move in parallel both prior to the transition to remote work, and once the transition to remote work concluded, suggesting that this identifying assumption is reasonable.

Second, we assume "strict exogeneity," i.e., that the timing of the switch to remote work must be independent of employees' outcomes. Because the "treatment group" was all switched to WFH due to COVID-19, we are less concerned about endogeneity of treatment than we might be in other settings. However, we do need to assume that workers' remote work status prior to the pandemic and the percentage of workers' colleagues that work remotely prior to the pandemic are independent of how they are affected by the pandemic. This assumption would be violated if, for instance, those who worked remotely prior to the pandemic were less likely to have unforeseen childcare responsibilities from school closures

caused by the pandemic. In order to make this identifying assumption more plausible, we use the CEM procedure described below.

Finally, we assume that ego remote work effects, collaborator remote work effects, and non-remote-work-related COVID-19 effects are additively separable. More precisely, we assume that Y_{it} can be written as

$$Y_{it}(RW_{it}, s_{it}, C_{it}) = Y_{it}(0, 0, 0) + \delta RW_{it} + \beta s_{it} + \gamma C_{it},$$

where RW_{it} is a binary variable indicating whether employee i is working remotely at time t , s_{it} is the share of employee i 's collaborators working remotely in month t , C_{it} is a binary variable indicating whether employee i was subject to the COVID-19 pandemic at time t , and $Y_{it}(0, 0, 0)$ is worker i 's outcome at time t if all three variables were equal to 0. This assumption is an extension of the standard difference-in-differences assumption that treatment effects, cross-group differences and time-effects are additively separable, and would be violated if, for instance, the effects of ego remote work and/or collaborator remote work were amplified in a multiplicative fashion due to other aspects of the COVID-19 pandemic (e.g., childcare responsibilities or pandemic-induced changes to Microsoft's product roadmaps). With our data, we are unable to validate the plausibility of this important identifying assumption, however, it is worth noting that causal estimates produced by standard difference-in-differences models also rely on the validity of parametric assumptions [62].

We have also updated Figure 1 so that it establishes the plausibility of the modified DiD model's parallel trends assumption not just for our communication media outcomes, but also for our collaboration network outcomes:

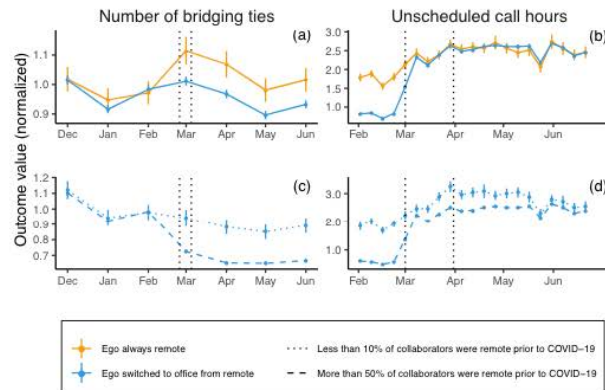


Figure 1: Time trend comparisons These graphs show the number of bridging ties per month (Graphs (a) and (c)) and the average unscheduled video/audio call hours per week (Graphs (b) and (d)) for different groups of employees, and establish the plausibility of the “parallel trends” assumption required by our modified DiD model. Error bars depict 95% confidence intervals, with standard errors clustered at the team level, and are in some places thinner than the symbols in the figure. Graphs (a) and (b) show employees who, prior to COVID-19, worked from the office (blue) and a matched sample of employees who worked remotely (orange). Graphs (c) and (d) show two sub-groups of the blue lines in (a) and (b): employees who, prior to COVID-19, had less than 10% of their collaborators working remotely (dotted) and those who had more than 50% of their coworkers working remotely (dashed). All the time series are normalized by the monthly average share of the relevant outcome across all employees during February 2020. Most employees transitioned to WFH during the week of March 1, 2020, though our analysis omits the month of March as a transition period.

Finally, we have expanded the paragraph in the Discussion where we discuss the limitations of our work, and made it clearer that our ability to causally estimate the ATTs of one’s own remote work status and the share of one’s colleagues working remotely depends on our modified DID’s identifying assumptions, which, as is the case for all identifying assumptions, are ultimately unprovable:

Finally, our ability to make causal claims is predicated on the validity of our modified DiD framework’s identifying assumptions: parallel trends, conditional exogeneity after matching, and additively separable effects. Although we have taken steps to verify the plausibility of these assumptions and tested the robustness of our results to an alternative matching procedure [56] (see the Methods section for more details), they are assumptions nonetheless.

I was particularly bothered by three things and hope you may be able to find ways to address these.

1. Your definition of a new connection. This was defined as someone who was not communicated with in the previous month (but was in the current month). To me, this does not necessarily equate to a weak tie. I could know someone quite well, but maybe our need to interact is sporadic or seasonal, based on the tasks that are on-going. Much of the contribution of your paper rests on the ability to argue that remote work hurts the ability to create new weak ties, which can hurt one's ability to innovate. Here are two thoughts I have that might help. One, could you provide more analyses to show different definitions of "new", such as the person has never interacted with the individual over your data collection period or maybe you have other data available to that you could use to provide levels of newness? Two, can you provide more conceptual explanation and background on the meaning of weak ties in structural networks and how this has been operationalized in past work?

Thank you for the suggestions. We agree with both of your points: that equating new ties with weak ties was a stretch, and that ties that are "new" from month-to-month do not necessarily correspond to two people who have never interacted before.

In order to address the first point, we have introduced a new weak tie/strong tie definition that we believe is more in line with previous operationalizations of tie strength. Identifying strong and weak ties in our data is difficult for two reasons: 1) we are analyzing a longitudinal dataset, so we need a definition that allows for a tie to be strong in one month, but weak in the next (or vice-versa) 2) there is an across-the-board increase in observed communication intensity after the shift to firm-wide remote work, which prevents us from using a definition of tie strength based on a static threshold. In light of these challenges, we define a connection as "strong" if it is in the top 50% of a worker's ties in terms of time spent in a given month, and as "weak" otherwise. Although we have not seen strong ties and weak ties defined in this exact way elsewhere in the research on social networks, it is relatively close to a common operationalization that simply counts the *amount* of contact between ties (used in, e.g., Onella et al. 2007, Aral and Van Alstyne 2011, and Brashears and Quintane 2018) and it allows for tie strength to vary over time based on the relative amount of contact between two people (Burke and Kraut 2014). The updated section of the introduction in which we discuss tie strength is copied below:

Conditional on a given network position or configuration, the efficacy with which a given tie can transfer or provide access to novel information depends on its strength.¹ Two people connected by a strong tie can often transfer information more easily, since they are more likely to share a common perspective, to trust one another, to cooperate with one another, and to expend effort to ensure that recently transferred knowledge is well understood and can be utilized [10, 13, 14, 15]. In contrast, weak ties require less time and energy to maintain [8, 16] and are more likely to provide access to novel, nonredundant information [8, 17, 18].

And the updated section of the Results section where we present our causal tie strength results is copied below:

The ability of a worker to effectively access knowledge from other parts of an organization is a function not only of the organizational and/or topological diversity of their connections, but also of the strength of those connections. For each month, we classify ties as "strong" when they are in the upper 50% of an employee's ties in terms of hours spent communicating, and as "weak" otherwise. Although by definition, weak ties will always get less of an employee's time than strong ties in a given month, we find that the shift to remote work reduced the share of time workers spent collaborating with weak ties by .32 FV ($P < 0.001$).^{6,7} These results indicate that above and beyond the impact of firm-wide remote work on the organizational and structural diversity of workers' ego networks, the shift to firm-wide remote work also made the allocation of workers' time more heavily concentrated.

We have also added the following footnote to the introduction, which provides more background on how tie strength has been measured elsewhere in the research literature:

¹In "The Strength of Weak Ties," Granovetter posits that tie strength is determined by a combination of "the amount of time, the emotional intensity, the intimacy (mutual confiding) and the reciprocal services which characterize the tie" [8]. To our knowledge the research community has not converged on a standard way to measure the strength of a tie.

And the following footnotes to the results section, which provide more insight into our operationalization of tie strength and the logic behind it, as well as the robustness of our tie strength results:

⁶We choose an ego-level cutoff, as opposed to a global cutoff, for each month with the reasoning that every worker should have some ties that are weak and some ties that are strong.

⁷Since the median is just one possible cutoff to distinguish between strong and weak ties, we also analyze the entire distribution of collaboration time for each worker and confirm that the average ego-level normalized Herfindahl-Hirschman Index (HHI) [43] of the collaboration time is increased by remote work, and that the average ego-level Shannon entropy [44] of collaboration time is decreased by remote work. The effects of firm-wide remote work on both of these outcomes are found in Table S2.

Although we no longer equate weak ties with new ties, we found value in keeping our "new ties" analysis in the paper, relabeling what we had previously called "new ties" as "added ties," and connecting these results to recent research that has examined the relationships between temporal network dynamics and business outcomes (Baum et al. 2012, Levin et al. 2012, Burt and Merluzzi 2016, Kneeland 2019, Kumar and Zaheer 2019, Soda et al. 2021, Zeng et al. 2021). The portion of the introduction where we discuss this literature is copied below:

Prior work has also shown that workers' performance is affected not only by the structure of the network and the strength of their ties, but also by the temporal dynamics of the network. Not only do the benefits of different types of ties vary with their age [19], but people benefit from changing their network position [20, 21, 22], adding new ties [23, 24], and reconnecting with dormant ties [25]. We find that the shift to firm-wide remote work may have reduced these benefits by making workers' collaboration network more static: individuals added and deleted fewer ties from month-to-month, and spent less time with newly added ties.

And the portion of the Results section where we present our results related to network temporal dynamics is copied below:

We also find that the shift to firm-wide remote work caused workers' ego networks to become more static; firm-wide remote work reduced the number of existing connections that churned from month-to-month by .05 FV ($P < 0.01$), and decreased the number of connections workers added month-to-month by .04 FV ($P = 0.015$).^{8,9} Furthermore, the shift to firm-wide remote work decreased the share of time workers spent collaborating with the connections they did add by .3 FV ($P < 0.001$). These changes to the temporal dynamics of ego networks may have made it more difficult for workers to capture the benefits associated with forming new connections [23, 24], reconnecting with dormant connections [25], and modulating their network position [20, 21, 22].

Given that we kept these results in the paper, we thought it important to follow your suggestions and a) test the robustness of our results to different definitions of "added" and b) understand whether the added ties we observe are truly "new," or if they are just ties that a given worker has not engaged with in some time (i.e., ties that Levin et al. (2012) might call "dormant ties.")).

To address suggestion a), we have replicated our network temporal dynamics analyses with alternate definitions of "added" and "churned" ties that look back two months, rather than one month. This analysis is described in footnote 9, which is copied below:

In our main analysis, we define a tie as "added" if it did not exist in month $t-1$, but existed in month t , and a tie as "deleted" if it existed in month $t-1$, but not month t . As a robustness check, we repeat our network dynamics-related analyses with alternative definitions in which a tie is "added" if it did not exist in month $t-2$ and month $t-1$, but existed in month t , and a tie is "deleted" if it existed in month $t-2$ and month $t-1$, but not in month t . The results of these analyses are qualitatively similar to our main results, and are found in the Supplementary Materials.

And present these results in full in the Supplementary Materials:

As a robustness check, we repeat our network dynamics-related collaboration network analyses using an alternative definition for a new tie, in which a tie is “added” if it did not exist in month $t-2$ and month $t-1$, but exists in month t , and an alternative definition for a deleted tie, in which a tie is “deleted” if it existed in both month $t-2$ and month $t-1$, but does not exist in month t . The results of these analyses are found Table S25, and are qualitatively similar to our main network dynamics results.

For convenience, Table S25 is copied below:

	# Churned ties	# Added ties	% time with added ties
Ego switch to remote (δ)	0.017 (0.011)	-0.047*** (0.013)	-0.107*** (0.023)
Collab. switch to remote (β)	-0.083*** (0.016)	0.034 (0.019)	-0.180*** (0.035)
Total effect ($\delta + \beta$)	-0.065*** (0.015)	-0.013 (0.018)	-0.287*** (0.030)
Indiv. FE	Y	Y	Y
Month FE	Y	Y	Y
R^2 (Total)	0.874	0.750	0.520
R^2 (w/o FE)	0.001	0.000	0.004
N	61,861	61,861	61,861

Table S25: **Effects of remote work on collaboration network dynamics (alternate definitions).** These are coefficients from regressions based on Equation (1). The first line is effect of an employee switching, the second line is the effect of all an employee’s collaborators switching and the third line is the sum of the two, all normalized by the average level in February. All regressions include individual and month fixed effects. The dependent variables are the number of added ties, the number of churned ties, and the share of time spent with added ties. Estimation is done using alternate definitions for these outcomes. Standard errors are clustered at the level of the employee’s manager. * indicates $p < 0.05$; ** indicates $p < 0.01$; *** indicates $p < 0.001$.

To address suggestion b), we look at all ties that were classified as “added” ties in June 2020, and measure what percentage of them existed in any month prior to May. This gives us a rough sense of what percentage of added ties are new ties, and what percentage are in fact dormant ties. We find **that 40% of the added ties existed in a month prior to May 2020**, whereas the remaining 60% did not. This suggests that the “added ties” we observe are a mixture of new ties and dormant ties. Footnote 8, where we describe this analysis, is copied below:

Of the added ties we observe in June 2020, 40% existed in at least one month between January 2020 and May 2020, whereas the remaining 60% did not. This suggests that the added ties we observe are a mixture of “dormant” ties [25] and ties that are truly new.

This result gives us a lower bound on the fraction of added ties that are dormant; because our collaboration network data only goes back to December 2019, there are likely some ties that were active in earlier months for which we do not have observations.

2. Your lack of work outcome variables. Do you have access to any additional data for any of your subjects that might allow you to investigate their work outcomes, and explore for the impact of the changes in collaboration patterns and use of communication channels? This could be done on a small scale (i.e., a sub-set of your data). And/or could you do some network analyses for some of your subjects to more closely examine weak ties, strong ties and structural diversity changes? If any of this was possible, it would strengthen your potential contribution and lend support to your suggestions, in my opinion.

Thanks for these great ideas.

Work Outcome Variables

We tried to take your first suggestion and measure the causal effect of firm-wide remote work on work outcomes. Given that the overwhelming majority of Microsoft's U.S. workforce appears in our sample, it was not possible to come up with a one-size-fits-all work outcome variable that made sense across the entire population we study. Instead, we decided to focus on the software engineers in our sample, since relative to the other information workers in our sample, there are concrete measures of output (e.g., pull requests, code commits, code reviews) for which we have high quality data. That being said, even for software engineers, measuring output is difficult, since the code software engineers write can vary in its quantity, quality, and importance. The existing research literature studying the work outcomes of software engineers has not converged on one agreed upon way to measure output or work quality, and different studies have used a range of different measures, including subjective performance evaluations (Rasch and Tosi, 1992), units of source code per unit time (Curtis 1981), time to complete a development tasks (Espinosa et al. 2007), number of tasks and commits completed (Cataldo and Herbsleb, 2008), and number of defects fixed (Ehrlich and Cataldo 2012).

Another difficulty we encountered when trying to measure the causal effect of firm-wide remote work on work outcomes for software engineers was that, due to Microsoft's privacy restrictions, we were unable to merge our data on pull requests, code commits, and code reviews per month to the dataset that we used to define workers' monthly collaboration networks. As a result, it was not immediately straightforward how to estimate a version of our modified DiD model that would yield comparable results to those that already exist in the paper for our network outcomes and communication media outcomes.

Finally, while our analyses in the paper have enough statistical power to detect reasonably sized effects, we found that our analysis of the causal effect of firm-wide remote work on software engineers' work outcomes was underpowered. This was due to a combination of factors: first, when we restricted our analysis to the subset of employees who were software engineers, the size of our sample shrinks from 61,967 to 16,128. Second, relative to the entire sample, remote work was relatively uncommon among software engineers prior to the COVID-19 pandemic; only 6.6% of the software engineers in our sample were remote before March. Finally, the work outcomes we were able to use in our analysis were generally sparse and/or had little variation. For instance, the average number of code reviews per month in our sample is [REDACTED], with a standard deviation of [REDACTED].

Ultimately, and in spite of all these sources of complexity, we attempted to measure the causal effects of firm-wide remote work on three different work outcomes for software engineers: the number of code commits per month, the number of pull requests per month, and the number of code reviews performed per month. A code commit is a set of changes that developers designate to track and save their progress, a pull request is a collection of code commits that developers submit for review before being integrated into the production codebase, and a code review occurs when a developer checks that another developer's pull request is acceptable to be integrated into the production code base. Given the fact that we were unable to join our code-related data to our collaboration network data, we conducted our analyses for all three of these outcomes (code commit, pull requests, code reviews) in two different ways:

1) An analysis that estimated only ego effects of shifting to firm-wide remote work, and did not account for the potential impact of a software engineer's collaborators shifting to remote work. This approach does not allow us to decompose the effect of firm-wide remote work into ego effects and collaborator effects, and is also subject to stable unit treatment value assumption violations, since the remote work status of a worker's collaborators can affect their outcomes, but is not accounted for in the model specification.

2) An analysis that uses our modified DiD model specification, but builds a collaboration network between *only software engineers* based on whether or not two software engineers have both committed code to the same repository. This approach does allow us to decompose the effects of remote work into ego effects and collaborator effects. However, it only partially accounts for SUTVA violations, since it does not take into account the remote work status of non-engineers. It also measures a different type of adjacency than our other analyses – some engineers may both commit code to the same repository, but not communicate with one another, while some engineers may communicate, but may commit code to different repositories. Because of these limitations, it is difficult to compare our results using this approach to those found in the paper.

The tables found below show our estimates of the causal effect of firm-wide remote work using each combination of software engineer work outcome and model specification. All of the results are null results, likely because they are underpowered to detect effects that are not extremely large.

Ego-only model:

	Pull requests	Commits	Code reviews
Ego switch to remote (δ)	0.079 (0.088)	0.308 (0.245)	-0.130 (0.123)
Indiv. FE	Y	Y	Y
Month FE	Y	Y	Y
R^2 (Total)	0.719	0.208	0.772
R^2 (w/o FE)	0.000	0	0.000
N	16,128	16,128	16,128

Decomposition-friendly model:

	Pull requests	Commits	Code Reviews
Ego switch to remote (δ)	-0.005 (0.034)	0.040 (0.055)	-0.042 (0.117)
Collab. switch to remote (β)	-0.196 (0.155)	0.807 (1.165)	0.224 (0.185)
Total effect ($\delta + \beta$)	-0.201 (0.155)	0.847 (1.204)	0.183 (0.160)
Indiv. FE	Y	Y	Y
Month FE	Y	Y	Y
R^2 (Total)	0.719	0.208	0.765
R^2 (w/o FE)	0	0	0
N	16,128	16,128	16,128

Network Analyses

Given the fact that we are not well-positioned to estimate the effects of firm-wide remote work on work outcomes, even for software engineers, we also followed your second suggestion and added additional network analyses that help strengthen our contribution and lend support to the suggestions we make throughout the paper.

First, we now more explicitly measure the changes in the structural diversity of workers' ego networks, both with respect to the formal org chart (number of distinct groups to which the worker is connected, number of cross-group ties, and share of time with cross-group ties) and with respect to the informal network structure of the organization (number of bridging ties, share of time with bridging ties, individual clustering coefficient). We chose to measure structural diversity with respect to both the formal org chart and with respect to the informal organizational structure because the two notions of structural diversity are distinct, although they are related and one might expect them to be correlated with one another (McEvily et al. 2014). We define a bridging tie as one with a local network constraint (Burt 2004, Everett and Borgatti 2020) that is below the median local network constraint observed in February 2020. Quantifying structural diversity in the informal collaboration network using local network constraint is consistent with previous research in management, organization science, and information systems, such as Kleinbaum 2012, Suh et al. 2011, and Goldberg et al. 2016. In addition to measuring structural diversity with respect to both the formal org chart and the informal collaboration network, we now also measure the amount of clustering in each worker's ego network using the individual clustering coefficient.

Beyond explicitly measuring changes to the structural diversity of workers' ego networks, we also now measure changes to share of time that they spend with weak ties (as opposed to strong ties), using the operationalization of tie strength that is described above.

Finally, as mentioned above, we now measure changes to the dynamics of workers' ego networks (number of churned ties, number of new ties, share of time with new ties). Existing research has found that network churn is associated with individual-level performance (Burt and Merluzzi 2016, Kneeland 2019, Kumar and Zaheer 2019), that individuals benefit from adding new ties (Soda et al. 2021, Zeng et al. 2021), that the performance benefits of both strong and weak ties vary with age (Baum et al. 2012) and that there are benefits to reconnecting with "dormant" ties (Levin et al. 2012). We define a "churned" tie as one that existed in month $t-1$, but not in month t , and an "added" tie as one that did not exist in month $t-1$, but did exist in month t .

For your convenience, we have copied the relevant text for each of these changes below, as well as Figures 2 and 3, which show our results.

Formal/informal diversity (Introduction)

Previous research has shown that network topology, including the strength of ties, plays an important role in the success of both individuals and organizations. For individuals, it is beneficial to have access to novel, non-redundant information through connections to

different parts of an organization's formal org chart and through connections to different parts of an organization's informal communication network [8]. Furthermore, being a conduit through which such information flows by bridging "structural holes" [9] in the organization can have additional benefits for individuals [10]. For firms, certain network configurations are associated with the production of high quality creative output [11], and there is a competitive advantage to successfully engaging in the practice of "knowledge transfer," in which experiences from one set of people within an organization are transferred to and used by another set of people within that same organization [12]. Conditional on a given network position or configuration, the efficacy with which a given tie can transfer or provide access to novel information depends on its strength. Two people connected by a strong tie can often transfer information more easily, since they are more likely to share a common perspective, to trust one another, to cooperate with one another, and to expend effort to ensure that recently transferred knowledge is well understood and can be utilized [10, 13, 14, 15]. In contrast, weak ties require less time and energy to maintain [8, 16] and are more likely to provide access to novel, nonredundant information [8, 17, 18].

Formal/informal diversity and clustering (Results)

Firm-wide remote work also decreased workers' cross-group connections by .04 FV ($P < 0.01$) and the share of collaboration time workers spent with cross-group connections by .26 FV, ($P < 0.001$). In other words, firm-wide remote work caused an overall decrease in the number of cross-group interactions and the fraction of attention paid to groups other than one's own.

Although formal organizational boundaries shape informal interactions [40], the formal organization of firms and their informal social structure are two distinct, interrelated concepts [41]. Connections that provide access to diverse teams may not bridge structural holes in the network sense [9], and connections that bridge structural holes in the network sense may not provide access to different parts of the formal org chart. Therefore, we also analyze how the shift to firm-wide remote work affected the structural diversity of employees' ego networks with respect to the firm's observed communication network, as opposed to the formal org chart. We label each tie as "bridging" or "non-bridging" based on its local network constraint, which is a measure of the extent to which a given tie bridges structural holes in a network [9, 42]. We then measure the effect of firm-wide remote work on the number of bridging ties each worker had and the amount of time each worker spent with their bridging ties. We find that on average, firm-wide remote work decreased the number of bridging ties by .09 FV ($P < 0.001$) and the share of time with bridging ties by .41 FV ($P < 0.001$). The fact that firm-wide remote work caused workers to have fewer bridging ties, and to spend less time with their remaining bridging ties, suggests that firm-wide remote work may have reduced workers' ability to access novel information in other parts of the network. These results, in conjunction

with our finding that firm-wide remote work reduced workers' cross-group interactions, also suggest that firm-wide remote work caused the collaboration network to become more siloed, both in a formal sense and in an informal sense.

We also find that firm-wide remote work caused a .06 FV ($P < 0.01$) increase in the individual clustering coefficient, which provides a measure of what proportion of an individual's network connections are also connected to each other (the higher a person's individual clustering coefficient, the more dense their ego network). Given the fact that we do not observe a statistically significant effect of remote work on the number of colleagues with whom workers collaborate, this result suggests that on average, firm-wide remote work caused workers to substitute ties that were not connected to one another for those that were. Put differently, different portions of the network, which became less interconnected, also became more intra-connected.

Tie strength (Introduction):

Conditional on a given network position or configuration, the efficacy with which a given tie can transfer or provide access to novel information depends on its strength. Two people connected by a strong tie can often transfer information more easily, since they are more likely to share a common perspective, to trust one another, to cooperate with one another, and to expend effort to ensure that recently transferred knowledge is well understood and can be utilized [10, 13, 14, 15]. In contrast, weak ties require less time and energy to maintain [8, 16] and are more likely to provide access to novel, nonredundant information [8, 17, 18].

Tie strength (Results)

The ability of a worker to effectively access knowledge from other parts of an organization is a function not only of the organizational and/or topological diversity of their connections, but also of the strength of those connections. For each month, we classify ties as "strong" when they are in the upper 50% of an employee's ties in terms of hours spent communicating, and as "weak" otherwise. Although by definition, weak ties will always get less of an employee's time than strong ties in a given month, we find that the shift to remote work reduced the share of time workers spent collaborating with weak ties by .32 FV ($P < 0.001$). These results indicate that above and beyond the impact of firm-wide remote work on the organizational and structural diversity of workers' ego networks, the shift to firm-wide remote work also made the allocation of workers' time more heavily concentrated.

Network dynamics (Introduction)

Prior work has also shown that workers' performance is affected not only by the structure of the network and the strength of their ties, but also by the temporal dynamics of the network. Not only do the benefits of different types of ties vary with their age [19], but people benefit from changing their network position [20, 21, 22], adding new ties [23, 24], and reconnecting with dormant ties [25]. We find that the shift to firm-wide remote work may have reduced these benefits by making workers' collaboration network more static: individuals added and deleted fewer ties from month-to-month, and spent less time with newly added ties.

Network dynamics (Results)

We also find that the shift to firm-wide remote work caused workers' ego networks to become more static; firm-wide remote work reduced the number of existing connections that churned from month-to-month by .05 FV ($P < 0.01$), and decreased the number of connections workers added month-to-month by .04 FV ($P = 0.015$).^{8,9} Furthermore, the shift to firm-wide remote work decreased the share of time workers spent collaborating with the connections they did add by .3 FV ($P < 0.001$). These changes to the temporal dynamics of ego networks may have made it more difficult for workers to capture the benefits associated with forming new connections [23, 24], reconnecting with dormant connections [25], and modulating their network position [20, 21, 22].

Figure 2:

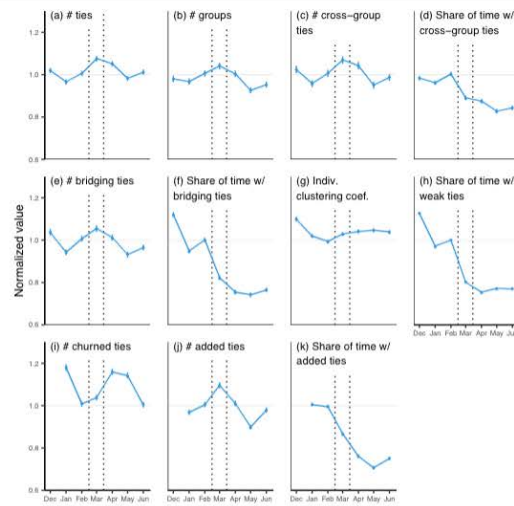


Figure 2: Time trends for collaboration networks. These graphs show the monthly averages for the collaboration network variables for all employees. Each variable is normalized by the average February Value for that variable. The variables are employees' average number of (a) network ties, (b) distinct business groups in which they have a collaborator, (c) cross-group ties, (e) ties that bridge structural holes in the network, (g) individual clustering coefficient, (i) collaborators from the previous month that they did not collaborate with that month, and (j) added collaborators they did not collaborate with the previous month, as well as the share of time spent with (d) cross-group ties, (f) bridging ties, (h) weak ties, and (k) added ties. Vertical bars depict 95% confidence intervals, with standard errors clustered at the team level, but are in most places not much taller than the data points.

Figure 3

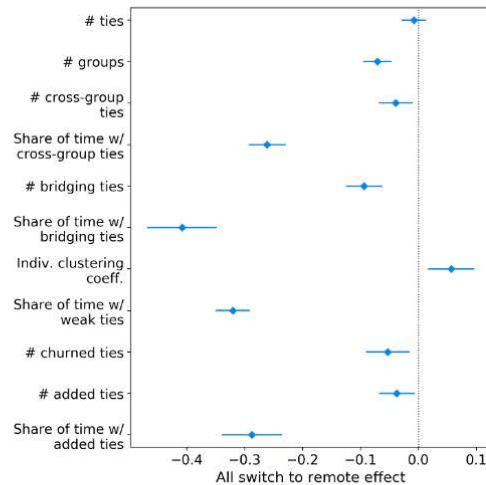


Figure 3: Effects of firm-wide remote work on collaboration networks These are the estimated causal effects of both an employee and that employee's colleagues switching to remote work on the number of collaborators an employee has, the number of distinct groups the employee collaborates with, the number of cross-group ties an employee has, the share of time an employee spends collaborating with cross-group ties, the number of bridging ties an employee has, the share of time an employee spends collaborating with bridging ties, the individual clustering coefficient of an individual's ego network, the share of time an employee spent collaborating with weak ties, the number of churned collaborators, the number of added collaborators, and the share of time spent with added collaborators. They are $(\beta + \delta)$ from Equation (1), normalized by the average level in February. Symbols depict point estimates and lines depict 95% confidence intervals.

As described above, as a robustness check, we also replicated our network dynamics-related analyses using alternate definitions of “churned ties” and “added ties” that look back two months, as opposed to one month. The results obtained when using these alternative definitions are qualitatively similar to our main results, and are found in Supplementary Materials. The relevant section of the Supplementary Materials is copied below:

As a robustness check, we repeat our network dynamics-related collaboration network analyses using an alternative definition for a new tie, in which a tie is “added” if it did not exist in month $t-2$ and month $t-1$, but exists in month t , and an alternative definition for a deleted tie, in which a tie is “deleted” if it existed in both month $t-2$ and month $t-1$, but does not exist in month t . The results of these analyses are found Table S25, and are qualitatively similar to our main network dynamics results.

	# Churned ties	# Added ties	% time with added ties
Ego switch to remote (δ)	0.017 (0.011)	-0.047*** (0.013)	-0.107*** (0.023)
Collab. switch to remote (β)	-0.083*** (0.016)	0.034 (0.019)	-0.180*** (0.035)
Total effect ($\delta + \beta$)	-0.065*** (0.015)	-0.013 (0.018)	-0.287*** (0.030)
Indiv. FE	Y	Y	Y
Month FE	Y	Y	Y
R^2 (Total)	0.874	0.750	0.520
R^2 (w/o FE)	0.001	0.000	0.004
N	61,861	61,861	61,861

Table S25: **Effects of remote work on collaboration network dynamics (alternate definitions)**. These are coefficients from regressions based on Equation (1). The first line is effect of an employee switching, the second line is the effect of all an employee's collaborators switching and the third line is the sum of the two, all normalized by the average level in February. All regressions include individual and month fixed effects. The dependent variables are the number of added ties, the number of churned ties, and the share of time spent with added ties. Estimation is done using alternate definitions for these outcomes. Standard errors are clustered at the level of the employee's manager. * indicates $p < 0.05$; ** indicates $p < 0.01$; *** indicates $p < 0.001$.

3. Can you clarify the nature of the tasks/work that people were doing? Past research has shown that WAH/virtual work fits some types of tasks very well and is more challenging for others. Do you have any data that you can add to your analyses to explore if and how the patterns changes for different types of roles? Being able to get some understanding for the need for interaction would be helpful in understanding your results and possibly providing better guidance for application of your findings. For example, if I am working on a task that I can do mostly by myself, being isolated for that work is not going to drive me to create ad-hoc interactions with others (as much as it would if I was doing something that relied on others /or they relied on me).

Thanks for this question – you make some great points here.

We've now made clearer the full extent of the dataset that we analyze. Given that our sample is comprised of nearly every single U.S. Microsoft employee, there are workers in many different roles in our dataset that are working on a wide variety of tasks. The portion of the Results section where we point this out is copied below:

Our sample contains all U.S. Microsoft employees except for those who hold senior leadership positions and/or are members of teams that routinely handle particularly sensitive data. Given the scope of our dataset, the workers in our sample perform a wide variety of tasks, including software and hardware development, marketing, and business operations.

Unfortunately, our data does not describe the particular tasks that any given worker is doing on a given day, and a worker in any given role has a wide variety of tasks (and task types) that they may be working on.

In order to make it clearer that the guidance for application of our findings is contingent on the types of tasks being performed by workers, we have now added the following footnote text to the Discussion section of the paper:

Even if we were able to measure productivity and innovation, it is difficult to make general claims about how changes to network structure and communication media choice will impact performance, given that these impacts are likely contingent on a number of factors, including the type of task a given team/organization is trying to complete [52, 53, 54, 55].

Finally, although we do not observe the tasks that workers are performing, we do measure treatment effect heterogeneity with respect to three dimensions: managerial status (manager/IC), tenure at Microsoft (new hire / tenured employee), and role type (engineer vs. non-engineer). Our role type HTE analysis in particular is new to the most recent draft of the paper.

As noted in the text below, which is copied from the paper, we do not observe notable heterogeneity in the effect of firm-wide remote work on collaboration networks across any of these three dimensions:

We test for heterogeneity in the effects of the shift to firm-wide remote work on collaboration ego networks with respect to a worker's managerial status (manager vs. individual contributor), tenure at Microsoft (shorter tenure vs. longer tenure), and role type (engineering vs. non-engineering), and do not find meaningful heterogeneity across any of these dimensions (see Supplementary Figures S1, S2 and S4).

However, we do observe heterogeneity in the effects of firm-wide remote work on communication media usage across managerial status and role type. The portion of the text where we describe these results is copied below, as is Figure 6:

While the effects of firm-wide remote work on collaboration networks did not exhibit heterogeneity across the worker attributes we observe, the effects of firm-wide remote work on communication media were in some cases larger for managers and engineers. We find that the switch to firm-wide remote work caused larger increases for managers than individual contributors in IMs sent, email sent, and unscheduled video/audio call hours (left panels of Figure 6). This is likely because relative to individual contributors, a larger share of managers' time is dedicated to communicating with others, i.e. their direct reports (e.g., to address issues blocking progress or conduct performance reviews) and representatives of other groups within the organization (e.g., to coordinate activity and goals across different groups). We also find that the shift to firm-wide remote work caused larger increases for engineers than non-engineers in the number of IMs sent and the number of unscheduled call hours (right panels of Figure 6). This may be reflective of the fact that software development teams are particularly reliant on informal communication [45, 46, 47], much of which may have taken place in-person prior to the shift to firm-wide remote work. We do not find meaningful heterogeneity with respect to employee tenure at Microsoft.

Figure 6

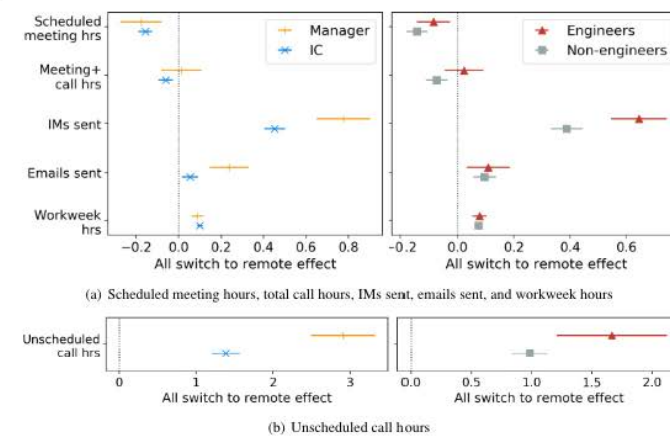


Figure 6: Effects of remote work on the use of communication media by Manager/IC status and role type. These are the causal effects, estimated separately for managers and individual contributors (ICs) (left two panels), and engineers and non-engineers (right two panels), of both an employee and their colleagues switching to remote work on hours spent in scheduled meetings, the sum of scheduled meetings and unscheduled call hours, instant message sent, emails sent, and estimated workweek hours (all shown in Panel (a)), and hours spent in unscheduled calls (shown in Panel (b)). They are $(\beta + \delta)$ from Equation (1), normalized by the average level for all employees in February. Symbols depict point estimates and lines depict 95% confidence intervals.

Our full results for treatment effect heterogeneity across managerial status, tenure at Microsoft, and role type are found in Tables S4 through S24, Figure 6, and Figures S1 through S4.

Clarity and Context

The paper is well written and generally easy to follow. I found the layout of the paper surprising, but presumably this fits with journal expectations. My “normal” expectation for most journals I review for would be some early discussion of what is known, then theoretical perspectives possibly leading to hypotheses to examine, an explanation of the method, then results and discussion of implications and limitations. The current paper starts with 4 pages of introduction, then results, then discussion, and the method description is at the end.

Thank you for your feedback - we're glad you found the paper well written and easy to follow.

With regard to the layout of the paper, we are following Nature Human Behaviour's requirements for article structure, which can be found here:
<https://www.nature.com/nathumbehav/about/content>

References

As mentioned above, there is considerable research on working at home/virtually in different disciplines. A small amount of this is referenced in the first few pages. The positioning of your work within the existing body of knowledge needs to be strengthened. For example, on page 4, you provide some references to previous work where you conclude previous research was restricted to two types of work, and/or did not examine causation. While I agree the effect of remote work will be moderated by the type of task, your summary of past work is very limited. Unfortunately (and somewhat ironic given the topic of your paper), I am prepared this review from home and can't access my office due to COVID restrictions. If I could, I could point you to some previous summaries of the literature (e.g., I know I have a fairly old book that is a review of past work in the area); however, there have been summaries of literature on various related topics published in journals over the years so you should be able to strengthen your grounding with some more comprehensive searching.

Thank you for this feedback. As noted above, we now spend much more time in the paper's introduction situating our study within the existing research literatures that study networks in business organizations, communication media choice/usage, and virtual teams / remote work, including an explicit reference to a recent review of the research on telecommuting (Allen et al. 2015) and references to numerous recent papers that study the network structure of virtual teams (Ahuja and Carley 1999, Ahuja et al. 2003, Suh et al. 2011). We also now clearly articulate the ways in which we feel our work contributes to the existing research literature. For convenience, the final two paragraphs of the introduction are copied below:

There is a large body of academic research across multiple disciplines that has studied remote work, virtual teams, and telecommuting (see [31] for a review of much of this work), including prior work that examines the network structure of virtual teams and how individual network position in virtual teams correlates with performance [32, 33, 34]. During the COVID-19 pandemic, there has been renewed public and academic interest in how virtual teams function. Recent analyses of telemetry and survey data show that the pandemic has affected both the who and the how of collaboration in information firms: while working remotely during the pandemic, workers are spending less time in meetings [35], communicating more via email [35], collaborating more with their strong ties, as opposed to their weak ties [36], and exhibiting patterns of communication that are more siloed and less stable [37]. However, these analyses, like much of the prior research on remote work, virtual teams, and telecommuting, are non-causal [31], and thus, are unable to separate the effects of remote work from the effects of pandemic-related confounders, such as reduced focus due to COVID-19-related stress or increased caregiving responsibilities while sheltering in place. Although prior research on the causal effects of remote work does exist, this work has mainly studied employees who volunteer to work remotely, and has focused on settings such as call centers and patent offices [38, 39], where relative to the majority of information work, tasks are more

easily codifiable and less likely to depend on collaboration or the transfer of complex knowledge.

We contribute to the research literatures on remote work, virtual teams, and telecommuting by analyzing the large-scale natural experiment created by Microsoft's firm-wide WFH policy during the COVID-19 pandemic. Because remote work was mandatory during the pandemic, we are able to quantify the effects of firm-wide remote work, which are most relevant for firms considering a transition to an all-remote workforce. Furthermore, because our model specification decomposes the overall effects of firm-wide remote work into ego remote work and collaborator remote work effects, our results also provide some insight into the possible impacts of remote work policies such as mixed-mode work and hybrid work.

My expertise

My home discipline is Information Systems, with largely a focus on topics that bridge organizational behaviour (i.e., technology-enabled work practices). A major research area of mine for the first 15 years or so of my career was investigating remote work and remote teamwork (virtual teams). Therefore, while I am not totally up to date with the literature, I have a reasonable sense of the relevant knowledge base. My knowledge of the statistical techniques used in this paper is definitely limited and that is an important limitation the editors should be aware of. I am not very familiar with Differences-in-differences modelling. I am comfortable with statistical techniques such as regression, SEM, and multi-level modelling (to a limited degree). I interpreted the supplemental tables as I would regression results. It is important that someone on the review team can assess the validity of the analyses in detail, as I have assumed the various approaches used by the authors meet standard expectations (e.g., CEM weighting, etc.). My expertise in terms of research methods includes surveys, experiments, case studies (including qualitative data analyses), largely from a soft-positivist view.

Thank you again for your helpful feedback. We have tried our best to address all of your questions and concerns, both in this letter and in our revised manuscript, and feel the paper is much improved. We hope you agree.

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Decision Letter, first revision:

7th May 2021

Dear Longqi,

RE: "The Effects of Remote Work on Collaboration Among Information Workers"

Thank you for submitting your revised manuscript and for all your work on the revision. I am very sorry for the delay in getting in touch with a decision.

Although your manuscript has been revised in response to reviewer comments, it does not fully comply with our editorial policies and formatting requirements. In particular, we require at this stage that all inferential statistical results be fully reported, including coefficients and/or effect sizes, confidence intervals, and p-values. In cases where it would be cumbersome to provide all of this information in-text, authors can refer to tables (either in the main text or Supplementary Information) that contain full results. However, any tables supporting the main results must include full statistical results, including both confidence intervals and exact p-values (i.e. asterisks should not be used to denote statistical significance).

Before we can send the manuscript back to our reviewers, we ask that you revise it to ensure that it complies fully with our formatting policies regarding statistical reporting. We also ask that as you revise, you ensure that your manuscript does not contain footnotes. To assist with this process, I have attached another copy of our checklist, which contains details on our formatting requirements. If you are uncertain as to how to address any of the points in the checklist, please don't hesitate to contact me.

Please use the link below to submit your revised manuscript and related files:

[REDACTED]

Note: This URL links to your confidential home page and associated information about manuscripts you may have submitted, or that you are reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage.

Thank you in advance for attending to these requests and I look forward to receiving your revised manuscript.

Sincerely,
Aisha

Aisha Bradshaw
Editor
Nature Human Behaviour

Decision Letter, second revision:

Our ref: NATHUMBEHAV-200812368D

13th July 2021

Dear Longqi,

Thank you for submitting your revised manuscript "The Effects of Remote Work on Collaboration Among Information Workers" (NATHUMBEHAV-200812368D). It has now been seen by two of the the original referees (Reviewers 2 and 3), and their comments are below. Reviewer 1 was unable to return to provide a full evaluation of your revision, but in brief remarks to the editors, they indicated their support for publication of your study. As you can see, the reviewers find that the paper has improved in revision. We will therefore be happy in principle to publish it in Nature Human Behaviour, pending minor revisions to comply with our editorial and formatting guidelines.

We are now performing detailed checks on your paper and will send you a checklist detailing our editorial and formatting requirements in about a week. Please do not upload the final materials and make any revisions until you receive this additional information from us.

Please do not hesitate to contact me if you have any questions.

Sincerely,
Aisha

Aisha Bradshaw, PhD
Senior Editor
Nature Human Behaviour

Reviewer #2 (Remarks to the Author):

I enjoyed reading this paper and I found that the revisions strengthened the paper considerably. The fact that it is still unclear about the effects of remote work on collaboration and communication, as well as the impact on relationships (existing and new), requires scholars to examine this issue in new ways to deepen our understanding.

Reviewer #3 (Remarks to the Author):

I have carefully reviewed the new manuscript and the authors' response document. Thank you for writing that document that explains very well how you addressed our concerns and suggestions.

It is clear to me that the authors worked very hard to consider the reviewing team's suggestions and did everything reasonable to address them. It was not possible to address everything (due to data limitations, etc.) but the authors have reflected the concerns and ideas well in their discussion, limitations, and elsewhere.

As before, the authors' have the potential to make an empirical contribution to the existing knowledge

of remote work given the large dataset. Improvements I see from the initial version include:

- The introduction does a stronger job positioning their work in the existing literature and theories.
- the nature of the data and its limitations are clearer. The analyses is explained more fully.
- shifting the analysis and reporting to the remote worker's network (using tie strength) makes the overall story more compelling.
- the discussion around the effects of remote work on network structure (and communications) is well done including clarity around the ability to identify possible effects on outcomes such as productivity and innovation.
- the discussion section on future research ideas and limitations will be helpful to readers and future researchers.

Overall, I have no further specific revisions to suggest and hope to see the article in print soon.

Decision letter, final requests:

** Please ensure you delete the link to your author homepage in this e-mail if you wish to forward it to your co-authors. **

Our ref: NATHUMBEHAV-200812368D

23rd July 2021

Dear Dr. Yang,

Thank you for your patience as we've prepared the guidelines for final submission of your Nature Human Behaviour manuscript, "The Effects of Remote Work on Collaboration Among Information Workers" (NATHUMBEHAV-200812368D). Please carefully follow the step-by-step instructions provided in the attached file, and add a response in each row of the table to indicate the changes that you have made. Please also check and comment on any additional marked-up edits we have proposed within the text. Ensuring that each point is addressed will help to ensure that your revised manuscript can be swiftly handed over to our production team.

We would like to start working on your revised paper, with all of the requested files and forms, as soon as possible (preferably within two weeks). Please get in contact with us if you anticipate delays.

When you upload your final materials, please include a point-by-point response to any remaining reviewer comments.

If you have not done so already, please alert us to any related manuscripts from your group that are under consideration or in press at other journals, or are being written up for submission to other journals (see: <https://www.nature.com/nature-research/editorial-policies/plagiarism#policy-on-duplicate-publication> for details).

Nature Human Behaviour offers a Transparent Peer Review option for new original research manuscripts submitted after December 1st, 2019. As part of this initiative, we encourage our authors to support increased transparency into the peer review process by agreeing to have the reviewer comments, author rebuttal letters, and editorial decision letters published as a Supplementary item.

When you submit your final files please clearly state in your cover letter whether or not you would like to participate in this initiative. Please note that failure to state your preference will result in delays in accepting your manuscript for publication.

In recognition of the time and expertise our reviewers provide to Nature Human Behaviour's editorial process, we would like to formally acknowledge their contribution to the external peer review of your manuscript entitled "The Effects of Remote Work on Collaboration Among Information Workers". For those reviewers who give their assent, we will be publishing their names alongside the published article.

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If you have any further questions, please feel free to contact me.

Best regards,
Chloe Knight
Editorial Assistant
Nature Human Behaviour

On behalf of

Aisha Bradshaw, PhD
Senior Editor
Nature Human Behaviour

Reviewer #1:
None

Reviewer #2:
Remarks to the Author:

I enjoyed reading this paper and I found that the revisions strengthened the paper considerably. The fact that it is still unclear about the effects of remote work on collaboration and communication, as well as the impact on relationships (existing and new), requires scholars to examine this issue in new ways to deepen our understanding.

Reviewer #3:

Remarks to the Author:

I have carefully reviewed the new manuscript and the authors' response document. Thank you for writing that document that explains very well how you addressed our concerns and suggestions.

It is clear to me that the authors worked very hard to consider the reviewing team's suggestions and did everything reasonable to address them. It was not possible to address everything (due to data limitations, etc.) but the authors have reflected the concerns and ideas well in their discussion, limitations, and elsewhere.

As before, the authors' have the potential to make an empirical contribution to the existing knowledge of remote work given the large dataset. Improvements I see from the initial version include:

- The introduction does a stronger job positioning their work in the existing literature and theories.
- the nature of the data and its limitations are clearer. The analyses is explained more fully.
- shifting the analysis and reporting to the remote worker's network (using tie strength) makes the overall story more compelling.
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- the discussion section on future research ideas and limitations will be helpful to readers and future researchers.

Overall, I have no further specific revisions to suggest and hope to see the article in print soon.

Final Decision Letter:

Dear Longqi,

We are pleased to inform you that your Article "The Effects of Remote Work on Collaboration Among Information Workers", has now been accepted for publication in Nature Human Behaviour.

Before your manuscript is typeset, we will edit the text to ensure it is intelligible to our wide readership and conforms to house style. We look particularly carefully at the titles of all papers to ensure that they are relatively brief and understandable.

Once your manuscript is typeset and you have completed the appropriate grant of rights, you will receive a link to your electronic proof via email with a request to make any corrections within 48 hours. If, when you receive your proof, you cannot meet this deadline, please inform us at rjsproduction@springernature.com immediately. Once your paper has been scheduled for online publication, the Nature press office will be in touch to confirm the details.

Acceptance of your manuscript is conditional on all authors' agreement with our publication policies (see <http://www.nature.com/nathumbehav/info/gta>). In particular your manuscript must not be published elsewhere and there must be no announcement of the work to any media outlet until the

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We look forward to publishing your paper.

With best regards,
Aisha

Aisha Bradshaw, PhD
Senior Editor
Nature Human Behaviour

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