

Reducing discrimination against job seekers with and without employment gaps

Received: 28 October 2021

Accepted: 21 October 2022

Published online: 5 December 2022

 Check for updates

Ariella S. Kristal¹ , Leonie Nicks², Jamie L. Gloor³ & Oliver P. Hauser⁴ 

Past research shows that decision-makers discriminate against applicants with career breaks. Career breaks are common due to caring responsibilities, especially for working mothers, thereby leaving job seekers with employment gaps on their résumés. In a preregistered audit field experiment in the United Kingdom ($n = 9,022$), we show that rewriting a résumé so that previously held jobs are listed with the number of years worked (instead of employment dates) increases callbacks from real employers compared to résumés without employment gaps by approximately 8%, and with employment gaps by 15%. A series of lab studies (an online pilot and two preregistered experiments; $n = 2,650$) shows that this effect holds for both female and male applicants—even when compared to applicants without employment gaps—as well as and for applicants with less and more total job experience. The effect is driven by making the applicant's job experience salient, not as a result of novelty or ease of reading.

Many people experience voluntary or involuntary career breaks at some point during their working lives¹, leading to employment gaps on their résumés. Such employment gaps may be caused by external shocks (for example, sickness or downsizing due to the COVID-19 pandemic^{2,3}) or career and lifestyle choices. Women are particularly affected by employment gaps when they take family-related leaves; for example, in the United Kingdom, over 70% of previously full-time working women take between 6 and 18 months out of paid employment after the birth of a child⁴.

Even when employment gaps are transitory, workers may face discrimination upon work re-entry if these gaps are evident on their résumés^{5–10}. Whereas traditionally structural unemployment (for example, skill shortages) is a major concern to the economy and society at large¹¹, frictional unemployment (for example, short gaps between jobs or short-term leave) may pose challenges for individuals—in particular, the scarring effects of short-term unemployment gaps¹². Indeed, this underemployment in itself is a problem due to inefficiency, but could also lead to more structural problems if those job seekers decide to leave the labour market permanently. While penalties associated with employment gaps have been shown to affect male and female

workers^{6,7,13}, motherhood penalties may particularly penalize women for childcare-related leaves⁵. There is a long literature noting the scarring effects of gaps in employment and a closely aligned literature exploring the impact of maternity leave and adjacent career breaks on individuals' career trajectories¹². In fact, these additional barriers to re-entry for mothers may contribute to the well-known, persistent gender wage gap^{12,14–16} as well as to women's lower representation in the upper echelons of companies^{17–20}. These effects are likely compounded further by other factors that also contribute to gender inequalities in the labour market, including occupational segregation and differential job entry^{21,22}, hiring agencies' pre-emptive sorting by gender and industries²³, bias and discrimination within the workplace²⁴, negotiation decisions²⁵ and differential career advancement²⁶.

In this article, we focus on an early stage of the process: the initial screening of résumés—the first 'gateway'—when companies hire for a new position. To study discrimination during the hiring process against workers with employment gaps, researchers have in the past turned to audit studies. Audit studies²⁷ have been used extensively to examine the effects of gender (such as discrimination against women^{28,29}), race (such as discrimination against non-whites³⁰), unemployment (that

¹Management Division, Columbia Business School, New York, NY, USA. ²Behavioural Insights Team, London, UK. ³School of Management, University of St. Gallen, St. Gallen, Switzerland. ⁴Department of Economics, University of Exeter Business School, Exeter, UK. ✉e-mail: ask2304@columbia.edu; o.hauser@exeter.ac.uk

is, discrimination against those who are unemployed)^{6,7,31,32}, and more recently, parenthood and childcare-related leave (that is, discrimination against parents taking time out of the workforce to care for their children^{5,10,33}); for a comprehensive register of discrimination of various characteristics during hiring in audit experiments, see Baert²⁷. These studies measure the effects of applicant characteristics on ‘callbacks’ (that is, an employer invitation to the next stage in the recruitment process—often a job interview).

While reduced opportunities for workers with employment gaps have been widely documented, little research has explored ways to overcome these barriers and biases. Some research has focused on reducing bias towards female applicants and working mothers. Of these interventions, employer strategies require manager training^{34,35}, suppressing biases and taking more time to review applicants³⁶, or overhauling current assessment processes³⁷. Employee strategies encourage applicants to explain their employment gaps^{8,38,39}, highlight volunteer work⁴⁰ or deliberately manage others’ impressions^{28,41}. Although these interventions have shown promise, they also tend to require substantial extra effort from applicants and employers; some of these strategies may even create backlash or social penalties by creating incongruence with behavioural expectations for women⁴¹.

To reduce these burdens, we develop and test a costless intervention applicants can adopt to facilitate workforce re-entry without backlash. Our intervention is informed by research from psychology and the field of judgement and decision-making, which shows that people inherently categorize people into groups, particularly when the category is easily accessible and representative^{42,43}. Stereotype activation is an automatic process, but reliance on these stereotypes is also greater in contexts of high uncertainty and high subjectivity^{44–47}, which characterizes many personnel selection processes⁴⁸. In addition to reliance on stereotypes (for example, mothers are less committed to their jobs and less productive than their child-free and male counterparts, unemployed applicants are lower quality and less productive than employed persons, etc.), employers may also be comparing applicants to prototypical workers.

We therefore hypothesize that employees with employment gaps contrast with conceptions of the ‘ideal worker’ who begins employment in early adulthood, continuing full-time without interruption for several decades⁴⁹. Whether it is a mother who has taken a caregiving leave or a person who became unemployed due to job loss—the two most common reasons for disrupted employment—career breaks undermine decision-makers’ impressions of applicant job experience by breaking this pattern of continuous employment^{50,51}.

Employers may still attend to career breaks (and may even discount previous work experience) despite the break’s potential irrelevance for the quality of the worker; we therefore argue that it is desirable to obscure this information from decision-makers. Our intervention removes the career-break information from job-seekers’ résumés, while still conveying job-relevant information. Specifically, to decrease the salience of the employment gap and to increase the salience of applicant experience, our intervention displays work experience in a different format: the number of years of experience for each job held (Supplementary Fig. 1b) instead of the standard ‘date format’ (Supplementary Fig. 1a). That is, instead of an applicant’s résumé listing the two calendar dates between which the applicant started and finished a job (for example, ‘March 2011–March 2016’), the treatment résumé displays a single number indicating the number of years the applicant worked in each job (for example, ‘5 years’). As a result, the intervention draws attention to the applicants’ job experience while also obfuscating employment gaps by omission.

We hypothesize that our intervention will increase the likelihood of a qualified applicant advancing to the next stage of selection (such as receiving a callback in Study 1, or receiving increased ratings of perceived hireability in Studies 2–3). To test our theorized mechanism—perceived job experience—we also measure recalled years of

experience (Studies 2–3). A related but separate research question that we do not address here is whether applicants in the treatment group are treated differently from the control group once they progress past the first gateway (for example, at an in-person interview). While unequal treatment can still occur at the interview stage^{52–54}, other research aims to reduce bias during this stage of the application process^{55,56}. The powerful, lasting effects of first impressions and the necessity of passing the first gateway to get to the second gateway⁵⁷ further underlines the importance of the current research.

Given the high prevalence of employment gaps among women due to family-related leave—which also remains a critical contributor to workplace gender inequalities—a key focus of our studies is on mothers returning to work. Studying discrimination against mothers (and fathers) has been a particular focus in the literature. Notably, Correll and colleagues found evidence of discrimination against mothers who received half as many callbacks as child-free women but no callback penalties for fathers (versus child-free men)⁵. Weisshaar¹⁰ found no statistically significant gender differences in callbacks. However, employed parents (versus unemployed parents who were laid off) received approximately 1.8 times more callbacks, and were approximately three times more likely to get a callback (versus parents who voluntarily left to take care of their children)¹⁰. Although our later studies also include men, this was primarily intended to test potential boundary conditions of our intervention. However, results from these additional studies show that the intervention appears to be useful for a range of job seekers: for men and women with various reasons for employment gaps and lengths of job experience.

Results

In a real-world setting with actual employers, Study 1 revealed that displaying the number of years of job experience (Years condition) on a résumé garnered more callbacks for job-seeking mothers than any other condition (Fig. 1). The other conditions are No Gap, where the résumé had the most recent employment date running from ‘July 2015 to Present’; an Unexplained Gap condition, where the last date in employment ended 2.5 years before the résumé was sent out, and an Explained Gap condition, where the last date in employment ended 2.5 years before the résumé was sent out, followed by the sentence, ‘Left to become a full-time mother and look after my children’.

Using linear probability models controlling for working pattern and region (as described in the preregistration), both the Unexplained Gap ($b = -0.049$, standard error (s.e.) = 0.014, $t(9,003) = -3.52$, $P < 0.001$; effect size (d) = -0.10 , 95% confidence interval (CI) -0.18 to -0.02) and Explained Gap ($b = -0.050$, s.e. = 0.014, $t(9,003) = -3.61$, $P < 0.001$; $d = -0.10$, 95% CI -0.18 to -0.02) conditions led to significantly lower callbacks than the Years condition. Furthermore, even the No Gap condition, which served as a conservative benchmark, received fewer callbacks ($b = -0.029$, s.e. = 0.014, $t(9,003) = -2.07$, $P = 0.038$; $d = -0.06$, 95% CI -0.14 to 0.02) than the Years condition. All results hold when including job types and county fixed effects, as well as when using a logistic regression model (Supplementary Table 2b). In sum, and as predicted, the redesigned résumé improved job prospects for mothers returning to paid employment in a large-scale field experiment, even when compared to similar mothers without employment gaps.

Our first study offers evidence that the Years intervention led to more callbacks for applicants in a real-world setting with real employers. To better understand the mechanism through which the Years résumé operates, we turned to controlled online vignette studies⁵⁸. We were particularly interested in capturing how the Years intervention is perceived along a number of dimensions (measured through Likert scales, Methods) in contrast to the standard résumé, although we also sought to capture a hypothetical proxy for our outcome variable (callback) in the field study. We used a ‘hireability’ outcome⁵⁹, measured on a scale from 0 to 100, which captured the likelihood that

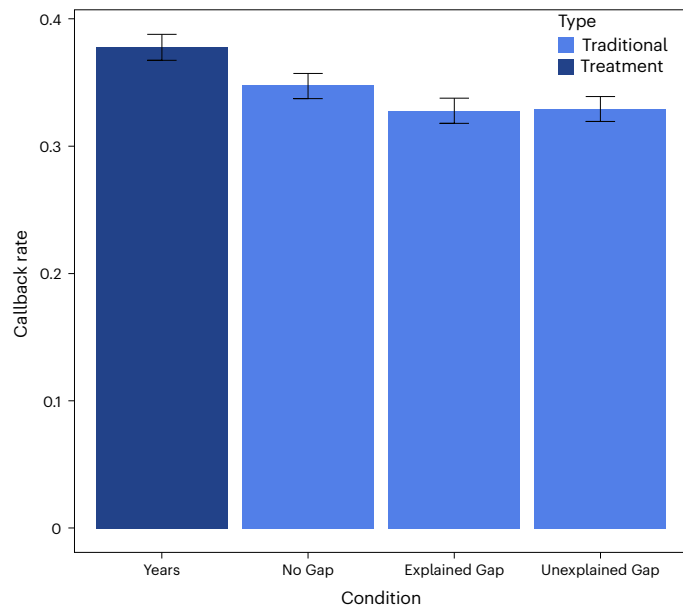


Fig. 1 | Callback rates by condition. The graph shows the percentage of callbacks per condition. Using a linear probability model, we find that the Years condition ($n = 2,255$) received significantly more callbacks than the No Gap condition ($n = 2,255$), the Explained Gap condition ($n = 2,256$) and the Unexplained Gap condition ($n = 2,256$). The error bars represent standard errors from the mean. The Unexplained Gap ($b = -0.049$, $s.e. = 0.014$, $t(9,003) = -3.52$, $P < 0.001$; $d = -0.10$, $95\% \text{ CI } -0.18 \text{ to } -0.02$) and Explained Gap ($b = -0.050$, $s.e. = 0.014$, $t(9,003) = -3.61$, $P < 0.001$; $d = -0.10$, $95\% \text{ CI } -0.18 \text{ to } -0.02$) conditions led to significantly lower callbacks than the Years condition. Furthermore, even the No Gap condition, which served as a conservative benchmark, received fewer callbacks ($b = -0.029$, $s.e. = 0.014$, $t(9,003) = -2.07$, $P = 0.038$, $d = -0.06$, $95\% \text{ CI } -0.14 \text{ to } 0.02$) than the Years condition. These results can also be found in Supplementary Table 2A.

the study participant would advance the applicant to the next stage of the application process.

We first explored the possible mechanisms with an online pilot study, in which we found no evidence in support of the most parsimonious explanations, namely, that the Years treatment is seen as easier to read ($b = 0.08$, $s.e. = 0.15$, $t(248) = 0.51$, $P = 0.61$; $d = 0.01$, $95\% \text{ CI } -0.34 \text{ to } 0.36$) or more novel ($b = 0.01$, $s.e. = 0.16$, $t(248) = 0.06$, $P = 0.95$; $d = 0.06$, $95\% \text{ CI } -0.30 \text{ to } 0.41$). Suggestive evidence for the mechanism emerged as increased perceptions of overall applicant experience in the treatment ($b = 0.37$, $s.e. = 0.12$, $t(248) = 3.19$, $P = 0.002$; $d = 0.40$, $95\% \text{ CI } 0.04 \text{ to } 0.76$) and years of applicant experience that participants recalled ($b = 0.59$, $s.e. = 0.29$, $t(248) = 2.00$, $P = 0.047$; $d = 0.25$, $95\% \text{ CI } -0.11 \text{ to } 0.61$). For the full regression results, see Supplementary Table 3.

Our preregistered Study 2 aimed to test this mechanism of increased perceptions of experience more explicitly and with a larger sample ($n = 800$). Study 2 was similar in many ways to Study 1 but differed from it in that we expanded it to also include résumés from male applicants. In particular, because the intervention in Study 1 was successful for applicants without an employment gap, we also sought to test whether the intervention would be moderated by, or would interact with, applicant gender.

Study 2 replicated and extended the effect of the Years condition, demonstrating that there was no statistically significant moderation by applicant gender: the redesigned résumé led applicants to be evaluated as more likely to be hired than applicants using a standard résumé, both when controlling for applicant gender (treatment: $b = 2.13$, $s.e. = 0.94$, $t(758) = 2.25$, $P = 0.025$; $d = 0.16$, $95\% \text{ CI } -0.04 \text{ to } 0.36$; applicant gender: $b = -1.31$, $s.e. = 0.94$, $t(758) = -1.39$, $P = 0.17$; $d = -0.10$, $95\% \text{ CI } -0.30 \text{ to } 0.10$; see Supplementary Table 4a, columns 1 and 2) and when including an interaction term between applicant gender and the intervention

(treatment: $b = 3.29$, $s.e. = 1.33$, $t(757) = 2.47$, $P = 0.01$; gender: $b = -0.12$, $s.e. = 1.35$, $t(757) = -0.09$, $P = 0.93$; and treatment \times gender interaction: $b = -2.34$, $s.e. = 1.89$, $t(757) = -1.24$, $P = 0.22$; see Supplementary Table 4a, column 3). Furthermore, Supplementary Table 4a, column 4 shows the robustness of the results by including both the interaction term and job fixed effects, while column 5 shows robustness by additionally excluding participants whose responses were outliers in the top 1% for the variable of years recalled.

We also confirmed the role of years of experience as a key mechanism: while the actual amount of job experience was 10 cumulative years for applicants in both conditions, participants who evaluated a résumé in the Years treatment more accurately recalled the number of years of experience that the applicant had (mean (M) = 9.41, $s.e. = 0.34$) than those in the standard résumé condition ($M = 8.35$, $s.e. = 0.24$; $b = 1.06$, $t(759) = 3.16$, $P = 0.002$; $d = 0.23$, $95\% \text{ CI } 0.03 \text{ to } 0.43$). This finding held after controlling for applicant gender and job type (Supplementary Table 4b) and was not significantly moderated by either or both factors.

In our preregistration, we said that we would exclude those who failed the gender manipulation check because we figured that those individuals would not be paying sufficient attention to the task at hand. For robustness, we provide the intention-to-treat (ITT) analysis without those exclusions; however, we expect adding in these additional inattentive participants would introduce noise to our analysis. In the ITT analysis, the treatment effect on applicant advancement becomes slightly marginal in two specifications (Supplementary Table 4c: $P = 0.054$ in our baseline specification with the treatment dummy in column 1; and $P = 0.056$ with job fixed effects in column 2) and remains significant in the two remaining specifications (Supplementary Table 4c: $P = 0.021$ when we include the interaction term in column 3; and $P = 0.021$ when we include both the interaction term and job fixed effects in column 4). Furthermore, in the ITT analysis, the treatment effect on recalled years of applicant experience is significant across all specifications (Supplementary Table 4d). In sum, the findings from the robustness analyses are broadly consistent with our preregistered analyses, although the estimates in some specifications are noisier, which we discuss in more detail below.

Finally, we sought to explore a policy-relevant boundary condition of the intervention. As the Years intervention focuses hiring managers' attention on applicants' amount of accumulated experience, it is plausible that the effect becomes less pronounced for more experienced workers (whose prior experience may be sufficiently long to be imprinted on hiring managers even with the standard résumé) or for less experienced workers (whose prior experience is too short to be highlighted effectively with the Years intervention).

Our preregistered Study 3 ($n = 1,600$) demonstrated that neither of these potential boundary conditions is of particular concern: the Years intervention worked successfully for applicants with 5 years or 15 years of experience, increasing hireability for applicants with fewer years of experience (5 years) ($b = 2.36$, $s.e. = 1.03$, $t(762) = 2.29$, $P = 0.023$; $d = 0.17$, $95\% \text{ CI } -0.02 \text{ to } 0.32$, Supplementary Table 5a, column 1) and with a greater number of years of experience (15 years) ($b = 2.21$, $s.e. = 0.99$, $t(755) = 2.23$, $P = 0.026$; $d = 0.16$, $95\% \text{ CI } 0.02 \text{ to } 0.29$, Supplementary Table 5a, column 2). In our preregistration, we said that we would exclude those who failed the gender manipulation check; however, for robustness, we include the ITT analysis without those exclusions (Supplementary Table 5b). All results remain significant except the probability of applicant advancement for 15 years of experience, which is marginal ($P = 0.052$). In sum, the findings from these robustness analyses are broadly consistent with our preregistered analyses.

Discussion

While the onus should not be on unemployed applicants to prevent others' bias against them, ample evidence has demonstrated that applicants with employment gaps face lower employment prospects,

and therefore would benefit from seeking ways to remain competitive when re-entering the workforce. For working mothers in particular, a frequently recommended strategy is to ‘explain the gap’³⁹. Despite this proactive attempt to reframe the conversation—highlighting the skills, dedication and hard work needed to be a caregiver—we found no empirical support that this strategy works any better than an unexplained gap in our large-scale audit experiment in Study 1 (Supplementary Table 2c). However, our results from the field experiment offer applicants a promising and effective strategy to overcome barriers to work re-entry. Low-effort and costless, our intervention replaces the standard employment dates on the résumé with the length of time of employment and thereby highlights applicants’ experience to prospective employers, eliminating employment gap penalties that hinder these applicants’ advancement beyond the first gateway of the selection process. Furthermore, by conducting this study in a field setting, we prioritize high external validity. However, in a field setting it can be more difficult (and more expensive) to test mechanism and boundary conditions. Therefore, we combined these findings with additional studies in a more controlled ‘online lab’ setting for Studies 2 and 3 (ref. ⁶⁰).

Given the positive callback outcomes of the redesigned résumé for women in Study 1 compared to both No Gap and Gap résumés, we expanded this research to also include male applicants. In an online study, we tested and found that the intervention works well; also, its success is not moderated by the gender of the applicant, even when compared to résumés without an employment gap. These results suggest that résumés could be improved for a variety of applicants. And while there was no evidence that the treatment had an effect on perceptions of novelty or ease of reading, Study 2 demonstrated that the redesigned résumés facilitated reviewers’ recall of applicants’ years of job experience. Our final Study 3 provided additional evidence that this treatment can work for applicants with shorter and longer job experience, further suggesting that this intervention is fairly generalizable for various types of applicants. Because findings from our field studies and online vignette studies converge, we believe this is promising for the validity of our results⁶⁰.

Our research makes several contributions. First, this intervention provides a blueprint for how the judgement and decision-making literature can theoretically and practically contribute to practical interventions in the real world: by taking into account the mental machinery of hiring managers, we show how the kinds of mental shortcuts that can lead to bias (for example, seeing only gaps in employment) can instead be redirected to focus on positive associations (for example, helping hiring managers appreciate applicants’ accumulated experience). Our research further contributes to the literature on gender discrimination, demonstrating a costless way for returning working mothers to show their potential to hiring managers and have a chance to proceed past the first gateway. Finally, our research contributes to understanding the wider experiences of discrimination for men and women who were temporarily unemployed. Helping people return to work after a prolonged unemployment spell is critical for public policy and social welfare support processes.

While this intervention predicted more callbacks and greater hireability, it is possible that this progress could be undone later in the interview process. For example, hiring managers might enquire about the exact dates of employment during an interview and, if learning about an employment gap, treat these applicants more negatively. However, it is also possible that interviewers rely less on stereotypes at this later stage, thus granting applicants a fairer, more merit-based opportunity. We encourage future research to explore this possibility. Furthermore, as hiring managers seem to assume that applicants with the standard ‘dates’ résumé have less experience than those with the ‘years’ résumé, future research should also attempt to quantify exactly how many years of experience the intervention can compensate for.

Our studies necessarily involved several design choices that other researchers may choose to explore differently. First, we focused on

between-subject designs for our studies. While both between-subject and within-subject designs have their respective strengths and weaknesses, by not exposing participants to both treatment and control sequentially, the between-subject design is often a ‘cleaner’ if statistically less efficient test of causality^{61,62}. On the other hand, we cannot speak to whether the same decision-maker would make different choices between the two résumés, which we encourage future research to explore. An additional consideration for choosing the between-subject design in the field context was that it reduces the burden on each individual employer (that is, the same employer is not sent multiple fictitious résumés). Second, we chose to replicate our field findings using online subject samples. While moving from the field into the ‘online lab’ reduces external validity, it also offers more experimental control and the potential to explore underlying mechanisms (for example, via survey scales)⁶³. We chose to run our studies on Prolific Academic because it enabled us to reach a sample of working adults in the United Kingdom, which was similar to our field experiment sample⁶⁴. Additionally, recent research on data quality across multiple platforms has shown Prolific to be of substantially higher quality than alternative platforms⁶⁵. Because our results converge in both the field and online settings, it heightens our confidence in these findings.

However, there are also several limitations of this work. First, we only tested this intervention in the United Kingdom; however, we believe these findings should generalize because of the mechanism we identified. The ‘years’ résumé seems to operate on a cognitive level, not a cultural level. Therefore, we would expect this intervention to be effective in countries with less generous parental leave policies (for example, the United States) or more generous policies (for example, Scandinavian countries). That said, we encourage researchers to experimentally test the effectiveness of the intervention in other countries. Furthermore, as we only tested four specific levels of job experience (that is, 5, 9, 10 and 15 years), it is possible that there may be a lower bound of experience (for example, ≤ 1 year) below which the ‘years’ résumé might actually make a résumé appear less impressive than the standard ‘dates’ résumé. We also believe that the positive effects of this intervention may be limited to fields where more years is a proxy for more experience, and thus viewed favourably. If, however, a job applicant had a career break in certain fields (for example, while finishing a PhD in an academic context), the ‘years’ résumé might call attention to the extended timeframe, potentially triggering a negative effect (for example, signalling low motivation)¹³. Another potential limitation is in Studies 2 and 3 where we preregistered our analysis to exclude participants who did not pay sufficient attention and failed the attention check in the study. Doing so reduces the extent to which our results allow for a causal interpretation for all participants; rather they represent the causal treatment effect for participants who paid attention (that is, treatment-on-treated). However, our results are largely robust—with two out of eight regression specifications becoming marginally significant and the other six specifications remaining significant—to including even participants who did not pay sufficient attention in the study. Finally, a potential limitation of our design in Study 1 is that both the CV and the cover letter changed, introducing a potential confound. While this means that we cannot precisely identify which element in Study 1 caused our main effect, there is additional evidence that is consistent with our conclusion about the ‘years’ résumé: we replicated the main effect in online studies, where we only manipulated the résumés and did not provide a cover letter.

Our audit study was primarily conducted before the onset of COVID-19, yet it might offer insights into how employees can navigate a pandemic-induced employment gap. Due to the COVID-19 pandemic, millions of women and men now have employment gaps on their résumés⁶⁶, especially previously working mothers³. While hiring penalties may be lower for applicants whose employment gaps are due to external forces¹⁰, the intervention tested here could theoretically

help all applicants to receive appropriate recognition for their years of job experience.

While our results primarily speak to applicants, we believe this research also contributes to understanding ways stereotyping can be overcome and helping organizations with the design of their hiring processes. Hiring managers can add this intervention to their toolbox of ‘debiasing’ strategies (that is, by explicitly requesting that all résumés be submitted with years instead of dates), just as ‘blinding’ résumés has become commonplace in many settings²⁴. While the general equilibrium effect of this intervention is an important question for future research if this intervention becomes more widely adopted, we predict that it would generally contribute to levelling the playing field if adopted more widely across applicants with and without employment gaps. In this way, applicants with equal experience receive equal employment opportunities, without the biasing stereotypes that more salient gaps may evoke.

Methods

Materials, data and code for all studies are available at <https://osf.io/3gahc>. Ethics oversight for the field experiment was provided by the Behavioural Insights Team’s internal ethics process, and ethics oversight for the online lab experiments was provided by the University of Exeter ethics committee (eUEBS003871) and the Harvard University IRB (IRB20-1467). It is worth noting that the initial field study (Study 1) did not obtain explicit informed consent due to the impossibility of mitigating deception in this design; there was also no debriefing, which the research team deemed would create more harm than benefit. Moreover, the email inboxes and phones were monitored daily, and the research team politely declined any positive callbacks within one working day to reduce the potential burden on employers. Participants in the online studies did provide informed consent.

Study 1

We aimed to send 9,000 applications to detect an effect size of $d = 0.08$ with 80% power. We manipulated the presentation of the applicant’s prior experience in a job in the form of dates (as is the case on traditional résumés) or summarizing the number of years the applicant held the job (on the redesigned résumé). We sent one of four different résumés and cover letters (conditions described below) to 9,022 employers across eight different sectors representing high- and low-skill jobs, in both male- and female-dominated fields (that is, software engineering, human resources, call centre operations, warehouse operations, finance, manufacturing production management, administrative work and social care work) who were advertising vacancies on a job-search platform from October 2019 to March 2020 in the United Kingdom. We aimed to assess a broad range of jobs that vary in the representation of men and women as well as the extent to which the job requirements might be linked to the male or female gender⁵⁸.

All résumés belonged to a fictitious applicant who had 9 years of work experience, was employed in two previous roles and, most importantly, was a mother. We selected 9 years because the average age of women in the United Kingdom having their first child is 28.8 (ref. ⁶⁷) and 50% of the population start full-time employment by 19 years old⁶⁸, which implies approximately 9–10 years of work experience before the birth of a first child. The fictitious applicant was named ‘Sarah Smith’. Sarah was selected because it is one of the most common first names for women born in the United Kingdom between 1984 and 1994 (ref. ⁶⁹) without strong associations with a particular social class⁷⁰ and ‘Smith’ is the most common last name in the United Kingdom⁷¹. Where there was a gap, we selected a 2.5-year gap, because it is the average amount of time out of the workforce taken by women who choose to leave paid employment (beyond maternity or shared parental leave) for childcare-related reasons in the United Kingdom and then seek to return to paid employment⁷². We tailored the highest level of education and specifics of work experience to slightly exceed the typical

requirements of each role. We conveyed parental status in all conditions with parent–teacher association involvement on résumés and stating that applicants were relocating to the hiring city with their family in cover letters^{5,10}.

We randomly assigned employers to receive one of four résumés (and corresponding cover letters). Three conditions used the ‘traditional’ résumé format, listing previously held jobs with their corresponding dates of employment. We varied whether an employment gap was present and, if so, whether this gap was explained (by stating that the applicant took time out of the workforce to look after her children) or unexplained. In the No Gap condition, the résumé had the most recent employment date running from ‘July 2015 to Present’, along with a line in the cover letter that said, ‘I am currently employed at [Organization]’. In the Unexplained Gap condition, the last date in employment ended 2.5 years before the résumé was sent out and there was no explanation in the cover letter. In the Explained Gap condition, the last date in employment ended 2.5 years before the résumé was sent out, followed by the sentence, ‘Left to become a full-time mother and look after my children’. The Explained Gap condition also included the following sentence in the cover letter, ‘I was most recently employed at [Organization] and left in [Date] to become a full-time mother and care for my children, and am now eager to return to work’. We included the Explained Gap condition because it is a frequently recommended ‘solution’ on job-seekers’ websites and thus offers a useful comparison against a common real-world benchmark.

The fourth condition—the ‘Years’ condition—is our main treatment of interest, in which we replaced the dates of employment with the number of years in each role with no explicit mention of current employment in the cover letter. In this condition, employment gaps were, by design, not visible to the employer as this format conveys applicant job experience without revealing when the jobs were held.

We were interested in studying whether an application received a ‘callback’ from an employer. To capture callbacks, we assigned each condition a unique corresponding email address and phone number and monitored both. Following the literature^{5,10,29}, we defined a callback as the employer progressing the applicant to the next stage in the process (for example, invitation to an online test, an interview or an in-person assessment), demonstrating strong positive interest, inquiring about start date availability, requesting that the applicant get in touch again once she moved, or if there was more than one missed call from the same employer. Our preregistration can be accessed at <https://aspredicted.org/z2s6w.pdf>.

The vast majority of applications for Study 1 (92.9%) were submitted before March 2020 when the United Kingdom enacted social distancing measures related to COVID-19. However, our results are also robust if we exclude data from March 2020 from the analysis.

Online pilot

For this exploratory study, we recruited 250 employees with hiring experience (33.6% male, $M_{\text{age}} = 35.62$, $SD_{\text{age}} = 12.38$; see Supplementary information for details) from the United Kingdom through Prolific Academic and collected data using a Qualtrics survey. After being randomized to either the Traditional (no gap) or Years résumé of a female applicant, participants rated whether they found the résumé easy to read or novel and how much professional experience they thought the applicant had; participants also recalled the applicant’s years of job experience and demographics (for example, gender).

Study 2

We aimed to recruit 800 full-time employees from the United Kingdom through Prolific Academic and collected data using a Qualtrics survey to be able to detect an effect size of $d = 0.20$ with 80% power. After excluding participants who failed manipulation checks, we were left with 761 participants (54.7% male, $M_{\text{age}} = 36.15$, $SD_{\text{age}} = 10.68$). We said we would exclude participants who failed the manipulation checks in our

preregistration, so our main analysis here excludes them; however, we provide the full ITT analysis in the Supplementary information; these results are consistent with our main findings.

Participants saw one of two different job types (that is, software engineer, which is a traditionally male job, or human resources manager, which is a traditionally female job). Participants were then randomly assigned to view a male or a female applicant and a control (Traditional without a gap) or treatment (Years) résumé. After seeing the résumé, participants were asked, 'How likely are you to advance this candidate to the next stage in the process?' on a scale from 1 (Definitely not) to 100 (Definitely yes).

After seeing the résumé and rating the applicant, participants proceeded to the next page of the survey where they no longer saw the résumé and were asked to recall the number of years of experience the applicant had and the number of previous jobs the applicant held, as well as identify the gender of the applicant (a manipulation check). Our preregistration can be accessed at <https://aspredicted.org/is2b7.pdf>.

Study 3

We recruited participants residing in the United Kingdom through Prolific Academic and collected data using a Qualtrics survey. We aimed to recruit 1,600 participants to be able to detect an effect size of $d = 0.25$ with 80% power. We excluded participants who failed an attention check before randomization and those who failed the gender manipulation check. We were left with a sample of 1,521 participants (38.7% men, $M_{age} = 34.8$, $SD_{age} = 9.7$). Because in our preregistration, we said that we would exclude these participants, our main analysis here excludes them; however, we provide the full ITT analysis in the Supplementary information; these results are consistent with our main findings.

Participants were randomly assigned to view the control (Traditional without a gap) or Years résumé. Within each condition, participants were then randomly assigned to see a résumé with fewer years (5 years) or more years (15 years) of job experience. Participants were then asked to rate on a 1–100 scale how likely they would be to advance the applicant to the next stage in the application process. After seeing the résumé and rating the applicant, participants proceeded to the next page of the survey where they no longer saw the résumé and were asked to recall the applicant's number of years of job experience and their demographics (as in Study 2). Our preregistration can be accessed at <https://aspredicted.org/id5m4.pdf>.

Reporting summary

Further information on research design is available in the Nature Portfolio Reporting Summary linked to this article.

Data availability

Data for all studies are available at https://osf.io/3gahc/?view_only=a8188dc8f9e8473e8722fd57b92484ba.

Code availability

Code for all studies is available at https://osf.io/3gahc/?view_only=a8188dc8f9e8473e8722fd57b92484ba.

References

1. Terrelonge, Z. Most common reasons for a career break and staff expectations when returning to work. *RealBusiness* <https://realbusiness.co.uk/most-common-reason-for-a-career-break-and-staff-expectations-afterwards> (2017).
2. Montenovo, L. et al. *Determinants of Disparities in Covid-19 Job Losses*. NBER Working Paper No. 27132 (National Bureau of Economic Research, 2020).
3. Fuller, S. & Qian, Y. Covid-19 and the gender gap in employment among parents of young children in Canada. *Gend. Soc.* **35**, 206–217 (2021).
4. Thévenon, O. & Solaz, A. Labour market effects of parental leave policies in OECD countries. OECDiLibrary https://www.oecd-ilibrary.org/social-issues-migration-health/labour-market-effects-of-parental-leave-policies-in-oecd-countries_5k8xb6hw1wjf-en (2013).
5. Correll, S. J., Benard, S. & Paik, I. Getting a job: is there a motherhood penalty? *Am. J. Sociol.* **112**, 1297–1338 (2007).
6. Eriksson, S. & Rooth, D. O. Do employers use unemployment as a sorting criterion when hiring? Evidence from a field experiment. *Am. Econ. Rev.* **104**, 1014–1039 (2014).
7. Kroft, K., Lange, F. & Notowidigdo, M. J. Duration dependence and labor market conditions: evidence from a field experiment. *Q. J. Econ.* **128**, 1123–1167 (2013).
8. Namingit, S., Blankenau, W. & Schwab, B. Sick and tell: a field experiment analyzing the effects of an illness-related employment gap on the callback rate. *J. Econ. Behav. Org.* **185**, 865–882 (2021).
9. Oberholzer-Gee, F. Nonemployment stigma as rational herding: a field experiment. *J. Econ. Behav. Org.* **65**, 30–40 (2008).
10. Weisshaar, K. From opt out to blocked out: the challenges for labor market re-entry after family-related employment lapses. *Am. Sociol. Rev.* **83**, 34–60 (2018).
11. Borjas, G. J. *Labor Economics* 8th edn (McGraw-Hill, 2020).
12. Bertrand, M., Goldin, C. & Katz, L. F. Dynamics of the gender gap for young professionals in the financial and corporate sectors. *Am. Economic J. Appl. Econ.* **2**, 228–255 (2010).
13. Van Belle, E., Di Stasio, V., Caers, R., De Couck, M. & Baert, S. Why are employers put off by long spells of unemployment? *Eur. Sociol. Rev.* **34**, 694–710 (2018).
14. Budig, M. J. & England, P. The wage penalty for motherhood. *Am. Sociol. Rev.* **66**, 204–225 (2001).
15. Gough, M. & Noonan, M. A review of the motherhood wage penalty in the United States. *Sociol. Compass* **7**, 328–342 (2013).
16. Sigle-Rushton, W. & Waldfogel, J. Motherhood and women's earnings in Anglo-American, Continental European, and Nordic countries. *Fem. Econ.* **13**, 55–91 (2007).
17. Brown, L. M. The relationship between motherhood and professional advancement. *Empl. Relat.* **32**, 470–494 (2010).
18. Hoyt, C. L. Women, men, and leadership: exploring the gender gap at the top. *Soc. Pers. Psychol. Compass* **4**, 484–498 (2010).
19. Schein, V. E. The work/family interface: challenging "corporate convenient". *Women Manag. Rev.* **8**, 22–27 (1993).
20. Williams, J. C. Beyond the glass ceiling: the maternal wall as a barrier to gender equality. *T. Jefferson L. Rev.* **26**, 1–14 (2003).
21. Blau, F. D., Brummund, P. & Liu, A. Y. H. Trends in occupational segregation by gender 1970–2009: adjusting for the impact of changes in the occupational coding system. *Demography* **50**, 471–492 (2013).
22. Samek, A. Gender differences in job entry decisions: a university-wide field experiment. *Manag. Sci.* **65**, 3272–3281 (2019).
23. Fernandez-Mateo, I. & King, Z. Anticipatory sorting and gender segregation in temporary employment. *Manag. Sci.* **57**, 989–1008 (2011).
24. Bohnet, I. *What Works: Gender Equality by Design* (Harvard Univ. Press, 2016).
25. Exley, C. L., Niederle, M. & Vesterlund, L. Knowing when to ask: the cost of leaning in. *J. Polit. Econ.* **128**, 816–854 (2020).
26. Brands, R. A. & Fernandez-Mateo, I. Leaning out: how negative recruitment experiences shape women's decisions to compete for executive roles. *Adm. Sci. Q.* **62**, 405–442 (2017).
27. Baert, S. in *Audit Studies: Behind the Scenes with Theory, Method, and Nuance* (ed. Gaddis, S. M.) 63–77 (Springer, 2018).
28. Booth, A. & Leigh, A. Do employers discriminate by gender? A field experiment in female-dominated occupations. *Econ. Lett.* **107**, 236–238 (2010).

29. He, J. & Kang, S. Covering in cover letters: gender and self-presentation in job applications. *Acad. Manag. J.* **64**, 1097–1126 (2021).
30. Bertrand, M. & Mullainathan, S. Are Emily and Greg more employable than Lakisha and Jamal? A field experiment on labor market discrimination. *Am. Econ. Rev.* **94**, 991–1013 (2004).
31. Ghayad, R. *The Jobless Trap* (Northeastern Univ. Press, 2013).
32. Pedulla, D. S. Penalized or protected? Gender and the consequences of nonstandard and mismatched employment histories. *Am. Sociol. Rev.* **81**, 262–289 (2016).
33. Firth, M. Sex discrimination in job opportunities for women. *Sex Roles* **8**, 891–901 (1982).
34. Dobbin, F. & Kalev, A. The promise and peril of sexual harassment programs. *Proc. Natl Acad. Sci. USA* **116**, 12255–12260 (2019).
35. Galinsky, A. D. & Moskowitz, G. Perspective-taking: decreasing stereotype expression, stereotype accessibility, and in-group favoritism. *J. Pers. Soc. Psychol.* **78**, 708–724 (2000).
36. Axt, J. R. & Lai, C. K. Reducing discrimination: a bias versus noise perspective. *J. Pers. Soc. Psychol.* **117**, 26 (2019).
37. Bohnet, I., Van Geen, A. & Bazerman, M. When performance trumps gender bias: joint vs. separate evaluation. *Manag. Sci.* **62**, 1225–1234 (2016).
38. Landivar, L. C., Ruppner, L., Scarborough, W. J. & Collins, C. Early signs indicate that COVID-19 is exacerbating gender inequality in the labor force. *Socius* **6**, 1–3 (2020).
39. Nicks, L., Burd, H. & Barnes, J. Returners qualitative analysis: organisations' experiences with returners. *Government Equalities Office* https://assets.publishing.service.gov.uk/government/uploads/system/uploads/attachment_data/file/790394/Returners-qualitative-analysis3.pdf (2019).
40. Baert, S. & Vujić, S. Does it pay to care? Volunteering and employment opportunities. *J. Popul. Econ.* **31**, 819–836 (2018).
41. Morgan, W. B., Walker, S. S., Hebl, M. M. & King, E. B. A field experiment: reducing interpersonal discrimination toward pregnant job applicants. *J. Appl. Psychol.* **98**, 799–809 (2013).
42. Hogg, M. A. in *Self and Social Identity* (eds Brewer, M. B. & Hewstone, M.) 203–231 (Blackwell, 2004).
43. Hogg, M. A. & Turner, J. C. Interpersonal attraction, social identification and psychological group formation. *Eur. J. Soc. Psychol.* **15**, 51–66 (1985).
44. Kahneman, D. & Tversky, A. Subjective probability: a judgment of representativeness. *Cogn. Psychol.* **3**, 430–454 (1972).
45. Kobryniewicz, D. & Biernat, M. Decoding subjective evaluations: how stereotypes provide shifting standards. *J. Exp. Soc. Psychol.* **33**, 579–601 (1997).
46. Milkman, K. L., Akinola, M. & Chugh, D. Temporal distance and discrimination: an audit study in academia. *Psychol. Sci.* **23**, 710–717 (2012).
47. Tversky, A. & Kahneman, D. Judgment under uncertainty: heuristics and biases. *Science* **85**, 1124–1131 (1974).
48. Uhlmann, E. L. & Cohen, G. L. Constructed criteria: redefining merit to justify discrimination. *Psychol. Sci.* **16**, 474–480 (2005).
49. Williams, J. C. The pandemic has exposed the fallacy of the “ideal worker”. *Harvard Business Review* (11 May 2020).
50. Karren, R. & Sherman, K. Layoffs and unemployment discrimination: a new stigma. *J. Manage. Psychol.* **27**, 848–863 (2012).
51. Weisshaar, K. Employment lapses and subsequent hiring disadvantages: an experimental approach examining types of discrimination and mechanisms. *Socius* **7** (2021).
52. Cunningham, J. & Macan, T. Effects of applicant pregnancy on hiring decisions and interview ratings. *Sex Roles* **57**, 497–508 (2007).
53. Dana, J., Dawes, R. & Peterson, N. Belief in the unstructured interview: the persistence of an illusion. *Judgm. Decis. Mak.* **8**, 512 (2013).
54. Kausel, E. E., Culbertson, S. S. & Madrid, H. P. Overconfidence in personnel selection: when and why unstructured interview information can hurt hiring decisions. *Organ. Behav. Hum. Decis. Process.* **137**, 27–44 (2016).
55. Bragger, J. D., Kutcher, E., Morgan, J. & Firth, P. The effects of the structured interview on reducing biases against pregnant job applicants. *Sex Roles* **46**, 215–226 (2002).
56. Brecher, E., Bragger, J. & Kutcher, E. The structured interview: reducing biases toward job applicants with physical disabilities. *Emp. Resp. Rights J.* **18**, 155–170 (2006).
57. Black, D. E. & Vance, M. D. Do first impressions last? The impact of initial assessments and subsequent performance on promotion decisions. *Manag. Sci.* **67**, 3985–4642 (2020).
58. Van Borm, H. & Baert, S. *Diving in the Minds of Recruiters: What Triggers Gender Stereotypes in Hiring?* Working Paper No. 22/1043 (Ghent University, Faculty of Economics and Business Administration, 2022).
59. Madera, J. M., Hebl, M. R. & Martin, R. C. Gender and letters of recommendation for academia: agentic and communal differences. *J. Appl. Psychol.* **94**, 1591 (2009).
60. Hainmueller, J., Hangartner, D. & Yamamoto, T. Validating vignette and conjoint survey experiments against real-world behavior. *Proc. Natl Acad. Sci. USA* **112**, 2395–2400 (2015).
61. Rubin, D. B. Estimating causal effects of treatments in randomized and nonrandomized studies. *J. Educ. Psychol.* **66**, 688 (1974).
62. Angrist, J. D. & Pischke, J. S. *Mostly Harmless Econometrics* (Princeton Univ. Press, 2008).
63. Hauser, O. P., Linos, E. & Rogers, T. Innovation with field experiments: studying organizational behaviors in actual organizations. *Res. Organ. Behav.* **37**, 185–198 (2017).
64. Palan, S. & Schitter, C. Prolific.ac—A subject pool for online experiments. *J. Behav. Exp. Finance* **17**, 22–27 (2018).
65. Peer, E., David, R., Andrew, G., Zak, E. & Ekaterina, D. Data quality of platforms and panels for online behavioral research. *Behav. Res. Methods* **54**, 1643–1662 (2022).
66. *Unemployment Rates During the COVID-19 Pandemic: In Brief* (Congressional Research Service, 2021); <https://fas.org/sgp/crs/misc/R46554.pdf>
67. *Births by Parents' Characteristics in England and Wales: 2016* (Office for National Statistics, 2017); <https://www.ons.gov.uk/peoplepopulationandcommunity/birthsdeathsandmarriages/livebirths/bulletins/birthsbyparentscharacteristicsinenglandandwales/2016>
68. *Milestones: Journeying into Adulthood* (Office for National Statistics, 2019); <https://www.ons.gov.uk/peoplepopulationandcommunity/populationandmigration/populationestimates/articles/milestonesjourneyinginto adulthood/2019-02-18>
69. *Top 100 Baby Names in England and Wales: Historical Data* (Office for National Statistics, 2014); <https://www.ons.gov.uk/peoplepopulationandcommunity/birthsdeathsandmarriages/livebirths/datasets/babynamesenglandandwalestop100babynameshistoricaldata>
70. “Sarah”. *Behind the Name* <https://www.behindthename.com/name/sarah/rating> (2020).
71. Hanks, P., Coates, R. & McClure, P. *The Oxford Dictionary of Family Names in Britain and Ireland* (Oxford Univ. Press, 2016).
72. Paull, G. Quantitative analysis of those returning to the labour market following a break to care for others. *Government Equalities Office* https://assets.publishing.service.gov.uk/government/uploads/system/uploads/attachment_data/file/717833/Characteristics_of_Returners_and_Potential_Returners.pdf (2018).

Acknowledgements

We are grateful to the Behavioural Insights Team for their research design and implementation of the field experiment, including substantial

contributions from V. Roy-Chowdhury and T. Hardy. We are also grateful to A. Sutherland from the Behavioural Insights Team for his help as well. This study was supported by the UK Government Equalities Office (to BIT's Gender and Behavioural Insights (GABI) programme), the Swiss National Science Foundation (grant No. PROOP1_193128 to J.L.G.) and the UKRI Future Leaders Fellowship (grant No. MR/T020253/1 to O.P.H.). The funders had no role in the design of the study, data collection and analysis, decision to publish or preparation of the manuscript.

Author contributions

L.N. designed the intervention and field experiment; A.S.K. and O.P.H. input into the field experiment and designed the online experiments; A.S.K. performed the online experiment research; A.S.K. analysed the data; and all authors wrote the paper.

Competing interests

L.N. is employed by the Behavioural Insights Team. The rest of the authors declare no competing interests.

Additional information

Supplementary information The online version contains supplementary material available at <https://doi.org/10.1038/s41562-022-01485-6>.

Correspondence and requests for materials should be addressed to Ariella S. Kristal or Oliver P. Hauser.

Peer review information *Nature Human Behaviour* thanks Christopher Bryan, Eva Van Belle and Sunčica Vujić for their contribution to the peer review of this work.

Reprints and permissions information is available at www.nature.com/reprints.

Publisher's note Springer Nature remains neutral with regard to jurisdictional claims in published maps and institutional affiliations.

Springer Nature or its licensor (e.g. a society or other partner) holds exclusive rights to this article under a publishing agreement with the author(s) or other rightsholder(s); author self-archiving of the accepted manuscript version of this article is solely governed by the terms of such publishing agreement and applicable law.

© The Author(s), under exclusive licence to Springer Nature Limited 2022

Reporting Summary

Nature Portfolio wishes to improve the reproducibility of the work that we publish. This form provides structure for consistency and transparency in reporting. For further information on Nature Portfolio policies, see our [Editorial Policies](#) and the [Editorial Policy Checklist](#).

Statistics

For all statistical analyses, confirm that the following items are present in the figure legend, table legend, main text, or Methods section.

n/a Confirmed

- | | | |
|-------------------------------------|-------------------------------------|--|
| <input type="checkbox"/> | <input checked="" type="checkbox"/> | The exact sample size (n) for each experimental group/condition, given as a discrete number and unit of measurement |
| <input type="checkbox"/> | <input checked="" type="checkbox"/> | A statement on whether measurements were taken from distinct samples or whether the same sample was measured repeatedly |
| <input type="checkbox"/> | <input checked="" type="checkbox"/> | The statistical test(s) used AND whether they are one- or two-sided
<i>Only common tests should be described solely by name; describe more complex techniques in the Methods section.</i> |
| <input type="checkbox"/> | <input checked="" type="checkbox"/> | A description of all covariates tested |
| <input type="checkbox"/> | <input checked="" type="checkbox"/> | A description of any assumptions or corrections, such as tests of normality and adjustment for multiple comparisons |
| <input type="checkbox"/> | <input checked="" type="checkbox"/> | A full description of the statistical parameters including central tendency (e.g. means) or other basic estimates (e.g. regression coefficient) AND variation (e.g. standard deviation) or associated estimates of uncertainty (e.g. confidence intervals) |
| <input type="checkbox"/> | <input checked="" type="checkbox"/> | For null hypothesis testing, the test statistic (e.g. F , t , r) with confidence intervals, effect sizes, degrees of freedom and P value noted
<i>Give P values as exact values whenever suitable.</i> |
| <input checked="" type="checkbox"/> | <input type="checkbox"/> | For Bayesian analysis, information on the choice of priors and Markov chain Monte Carlo settings |
| <input checked="" type="checkbox"/> | <input type="checkbox"/> | For hierarchical and complex designs, identification of the appropriate level for tests and full reporting of outcomes |
| <input type="checkbox"/> | <input checked="" type="checkbox"/> | Estimates of effect sizes (e.g. Cohen's d , Pearson's r), indicating how they were calculated |

Our web collection on [statistics for biologists](#) contains articles on many of the points above.

Software and code

Policy information about [availability of computer code](#)

Data collection

Data analysis

For manuscripts utilizing custom algorithms or software that are central to the research but not yet described in published literature, software must be made available to editors and reviewers. We strongly encourage code deposition in a community repository (e.g. GitHub). See the Nature Portfolio [guidelines for submitting code & software](#) for further information.

Data

Policy information about [availability of data](#)

All manuscripts must include a [data availability statement](#). This statement should provide the following information, where applicable:

- Accession codes, unique identifiers, or web links for publicly available datasets
- A description of any restrictions on data availability
- For clinical datasets or third party data, please ensure that the statement adheres to our [policy](#)

Data for all studies are available at https://osf.io/3gahc/?view_only=a8188dc8f9e8473e8722fd57b92484ba.

Field-specific reporting

Please select the one below that is the best fit for your research. If you are not sure, read the appropriate sections before making your selection.

Life sciences Behavioural & social sciences Ecological, evolutionary & environmental sciences

For a reference copy of the document with all sections, see [nature.com/documents/nr-reporting-summary-flat.pdf](https://www.nature.com/documents/nr-reporting-summary-flat.pdf)

Behavioural & social sciences study design

All studies must disclose on these points even when the disclosure is negative.

Study description	<p>These are quantitative data collected in a randomized controlled trial as well as several online lab experiments. In the first field experiment, we sent one of four different résumés and cover letters (conditions described below) to 9,022 employers across eight different sectors representing high and low-skill jobs, in both male- and female-dominated fields (i.e., software engineering, human resources, call center operations, warehouse operations, finance, manufacturing production management, administrative work, and social care work) who were advertising vacancies on a job-search platform from October 2019 to March 2020 in the U.K. We aimed to assess a broad range of jobs that vary in the representation of men and women as well as the extent to which the job requirements might be linked to the male or female gender. We randomly assigned employers to receive one of four résumés (and corresponding cover letters). Three conditions used the “traditional” résumé format, listing previously held jobs with their corresponding dates of employment. We varied whether an employment gap was present and, if so, whether this gap was explained (by stating that the applicant took time out of the workforce to look after her children) or unexplained. In the No Gap condition, the résumé had the most recent employment date running from “July 2015 to Present,” along with a line in the cover letter that said, “I am currently employed at [Organization].” In the Unexplained Gap condition with the last date in employment ending two and a half years before the résumé was sent out and there was no explanation in the cover letter. In the Explained Gap condition, the last date in employment ended two and a half years before the résumé was sent out, followed by the sentence, “Left to become a full-time mother and look after my children.” The Explained Gap condition also included the following sentence in the cover letter, “I was most recently employed at [Organization] and left in [Date] to become a full-time mother and care for my children, and am now eager to return to work.” We included the Explained Gap condition because it is a frequently recommended “solution” on job seekers websites and thus offers a useful comparison against a common real-world benchmark.</p> <p>The fourth condition—the “Years” condition—is our main treatment of interest, in which we replaced the dates of employment with the number of years in each role with no explicit mention of current employment in the cover letter. In this condition, employment gaps were, by design, not visible to the employer since this format conveys applicant job experience without revealing when the jobs were held.</p> <p>We were interested in studying whether an application received a “callback” from an employer. To capture callbacks, we assigned each condition a unique corresponding email address and phone number and monitored both. In the two online lab experiments, we showed participants control and years resumes and asked about perceptions of experience and likelihood of hiring.</p>
Research sample	<p>The research sample includes hiring managers/employers in the UK for which we did not collect demographic information. Given this was a natural field experiment, this sample consists of actual decision-makers responsible for hiring in organizations. For the online samples, we recruited (a non-representative sample) of full-time employed adults who are members of Prolific Academic and based in the UK, to try to approximate the types of people from Study 1 and who could realistically be involved in hiring decisions. For Study 2 we had 761 participants (54.7% male, Mage = 36.15, SDage = 10.68) and for Study 3 we had 1,521 participants (38.7% men, Mage = 34.8, SDage = 9.7).</p>
Sampling strategy	<p>Power calculations were conducted to determine the sample size needed for these randomized experiments.</p>
Data collection	<p>For the field experiment in Study 1, participants were actual hiring managers or HR officials working in their regular workplace and were unaware of the fact that they were participating in an experiment. These workers either chose to advance our fictitious candidate to the next round by calling or emailing, or not. RAs blind to condition checked the given phone numbers for voicemails and the emails for hiring responses and collated that data in a spreadsheet. The online experimental data was collected using a Qualtrics survey and participants recruited from Prolific Academic completed the survey online without an experimenter present at the time and location of their choosing.</p>
Timing	<p>The field experiment took place between October 2019 - March 2020. The online data collection took places between September 2020-June 2021.</p>
Data exclusions	<p>No data was excluded from the field experiment, and only those who failed the pre-registered attention/manipulation checks were excluded from the online studies.</p>
Non-participation	<p>No participants dropped out.</p>
Randomization	<p>All randomization occurred at the individual level.</p>

Reporting for specific materials, systems and methods

We require information from authors about some types of materials, experimental systems and methods used in many studies. Here, indicate whether each material, system or method listed is relevant to your study. If you are not sure if a list item applies to your research, read the appropriate section before selecting a response.

Materials & experimental systems

n/a	Involvement	Included
<input checked="" type="checkbox"/>	<input type="checkbox"/>	Antibodies
<input checked="" type="checkbox"/>	<input type="checkbox"/>	Eukaryotic cell lines
<input checked="" type="checkbox"/>	<input type="checkbox"/>	Palaeontology and archaeology
<input checked="" type="checkbox"/>	<input type="checkbox"/>	Animals and other organisms
<input type="checkbox"/>	<input checked="" type="checkbox"/>	Human research participants
<input checked="" type="checkbox"/>	<input type="checkbox"/>	Clinical data
<input checked="" type="checkbox"/>	<input type="checkbox"/>	Dual use research of concern

Methods

n/a	Involvement	Included
<input checked="" type="checkbox"/>	<input type="checkbox"/>	ChIP-seq
<input checked="" type="checkbox"/>	<input type="checkbox"/>	Flow cytometry
<input checked="" type="checkbox"/>	<input type="checkbox"/>	MRI-based neuroimaging

Human research participants

Policy information about [studies involving human research participants](#)

Population characteristics

We do not have the population characteristics of the field experiment participants - as resumes were sent to UK companies posting open positions. The online experiments consisted of full-time employees in the UK who use Prolific Academic. The demographics of the pilot are: 33.6% male, Mage = 35.62, SDage = 12.38; Study 2 demographics: 54.7% male, Mage = 36.15, SDage = 10.68; Study 3 demographics: 38.7% men, Mage = 34.8, SDage = 9.7

Recruitment

Because we did not actively recruit for the field experiment, but rather included all eligible employers in our sample, our study has a lower risk of selection-bias. Employers in the field study were selected by posting certain job openings for certain positions during the trial window. Online study participants were recruited through Prolific Academic.

Ethics oversight

Ethics oversight for the field experiment was provided by the Behavioural Insights Team's internal ethics process, and ethics oversight for the lab experiments was provided by the University of Exeter ethics committee (eUEBS003871) and the Harvard University IRB (IRB20-1467). It is worth noting that the initial field study (Study 1) did not obtain explicit informed consent due to the impossibility of mitigating deception in this design, nor was there was a debrief, which the research team deemed would create more harm than benefit.

Note that full information on the approval of the study protocol must also be provided in the manuscript.