



Beyond the motherhood penalty: Evidence of a (potentially race-based) parenthood boost in workplace evaluations[☆]

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ABSTRACT

According to previous research, people more readily question the competence of, and express greater discrimination against, women (vs. men) who are described as parents in the workplace. In the present manuscript, we sought to examine whether the magnitude of this bias, which is referred to as the motherhood penalty, would be moderated by whether the women and men in question are Black rather than White. To explore this possibility, we conducted four large-scale replication attempts of well-known studies on the motherhood penalty (three of Cuddy, Fiske, & Glick, 2004, reported in-text; one of Heilman & Okimoto, 2008, reported in the online supplement). Across replication attempts, we manipulated not just target employees' parenthood statuses and gender groups, but also their racial groups. To our surprise, results from these replication attempts—as well as those from an internal meta-analysis ($N = 4742$)—revealed no evidence of a motherhood penalty. Instead, we found evidence of a *parenthood boost*: a tendency for people to more positively evaluate employees who are described as parents than employees who are not. Moreover, while parenthood-boost magnitudes did not vary by employees' gender groups, there was some evidence that they varied by employees' racial groups, with White parents receiving larger boosts than Black parents. Overall, these findings suggest that the motherhood penalty may no longer emerge in single-shot evaluative contexts. Indeed, there may be contexts in which parenthood now conveys evaluative advantages to both women and men in the workplace, with potential racial differences in the magnitude of those advantages.

Becoming a parent is part and parcel of the American Dream. Parenthood is regarded as a prescribed attribute for both men and women in the U.S. (Prentice & Carranza, 2002), and even young children are socialized to dream of one day starting a family and having children of their own (Kane, 2006; Starrels & Holm, 2000). Given these cultural artifacts, it is perhaps unsurprising that people in the U.S. tend to have positive attitudes toward those who become parents and negative attitudes toward those who do not (e.g., Kopper & Smith, 2001; Mueller & Yoder, 1997, 1999). For example, people in the U.S. tend to think of parents as being more fulfilled, as being better psychologically adjusted, and as living more moral lives than individuals who choose not to become parents (Ashburn-Nardo, 2017; Jamison, Franzini, & Kaplan, 1979; Koropecj-Cox, Çopur, Romano, & Cody-Rydzewski, 2018).

Furthermore, these evaluative preferences for parents over non-parents can have consequences for how people are perceived at work. For example, employees rate their coworkers who have children as more trustworthy and as higher-status than their coworkers who do not have children (Zhang & Soderberg, 2023). In addition, people perceive more overlap between the attributes of parents and managers than they do between the attributes of non-parents and managers (Morgenroth, Ryan, & Sønderlund, 2020). Taken together, these findings paint a rosy portrait of parenthood. Perhaps becoming a parent can result in an evaluative boost—a tendency to be regarded more positively by one's peers when one is (vs. is not) described as a parent.

[☆] Pre-registration documentation, data files, codebooks, survey materials, and R scripts associated with this article are available on the OSF website: <https://osf.io/h35gm/>.

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1. The motherhood penalty

If being described as a parent comes with an evaluative boost, does this boost extend equally to men and women? In contrast to the perspective outlined above, the social scientific literature has suggested that whether parenthood conveys evaluative advantages versus disadvantages for targets depends fundamentally on their gender groups. Generally speaking, the findings in this literature suggest that although men can (and often do) reap benefits at work when they become parents, women very frequently do not (Cheung et al., 2022). For example, whereas men's wages tend to increase after having children, women's wages tend to decrease (Budig & England, 2001; Budig & Hodges, 2010; England, Bearak, Budig, & Hodges, 2016; Glauber, 2008; Kricheli-Katz, 2012; Lundberg & Rose, 2000). As another example, when women applicants in audit studies are identified as parents, they receive fewer callbacks from hiring managers; men applicants, in contrast, appear to face no such bias (Bernard & Correll, 2010; Correll, Bernard, & Paik, 2007; Firth, 1982). Findings such as these make it clear that although men may indeed receive an evaluative boost at work when described as parents, women tend to face backlash—backlash that, across the behavioral sciences, is typically referred to as the *motherhood penalty*.

Some of the best-known manifestations of the motherhood penalty come from experiments in which perceivers are asked to evaluate target employees whose workplace performances are held constant, but whose gender groups and parenthood statuses are manipulated. These experiments reveal that merely learning that women are parents (vs. not learning that they are parents) can cause perceivers to rate these women as less competent at their jobs (Cuddy et al., 2004; Heilman & Okimoto, 2008), and to advocate for greater discrimination against these women (e.g., not wanting these women to receive training, hiring, or promotion opportunities; Correll et al., 2007; Fuegen, Biernat, Haines, & Deaux, 2004). In contrast, these same experiments reveal that merely learning that men are parents does not cause perceivers to rate these men as any less competent at their jobs, or as any more deserving of discrimination.

However, it is worth noting that most experimental demonstrations of the motherhood penalty were conducted more than 15 years ago. Since then, not only have sample size standards for lab studies increased greatly,¹ but so too have stereotypic beliefs about women's competence (for a meta-analysis, see: Eagly, Nater, Miller, Kaufmann, & Sczesny, 2020). Thus, it remains an empirical question whether experimental demonstrations of the motherhood penalty hold up to scrutiny under contemporary sample-size conventions and among current cohorts of U.S. adults.

The present manuscript has two primary purposes. The first purpose is to test whether people continue to question the competence of, and to endorse greater discrimination against, women (vs. men) who are described as parents in the workplace (Correll et al., 2007; Cuddy et al., 2004; Fuegen et al., 2004; Heilman & Okimoto, 2008). To this end, we examine whether the findings of two well-cited demonstrations of the motherhood penalty (that of Cuddy et al., 2004, reported in-text; as well as that of Heilman & Okimoto, 2008, reported in the online supplement) hold up to scrutiny when tested in highly powered, contemporary samples of U.S. adults. The second purpose of the present manuscript is to examine this research question *intersectionally*—to explore whether the motherhood penalty, if it indeed emerges, is moderated by whether the mothers in question are Black versus White.

¹ For example, experimental tests of the motherhood penalty have relied on sample sizes of approximately $n = 50$ (Correll et al., 2007), $n = 40$ (Fuegen et al., 2004), $n = 30$ (Cuddy et al., 2004), and n s of 25–65 (Heilman & Okimoto, 2008) participants per between-subjects condition. Thus, most experimental tests of the motherhood penalty—which require estimating two-way interaction magnitudes between targets' gender groups (man, woman) and parenthood statuses (parent, non-parent)—are likely to have been underpowered by contemporary standards.

2. Taking an intersectional look at parenthood perceptions

The term intersectionality was borne out of Black-feminist scholarship as a tool for understanding the experiences of those who are multiply marginalized, on the one hand, and for analyzing the ways by which systems of oppression mutually reinforce one another, on the other hand (Bell, 1984; Crenshaw, 1991; King, 1988). In psychological science, intersectionality often refers more specifically to an epistemological approach in which one recognizes that various psychological phenomena—like how one is perceived—can depend on the multiple social identities one possesses (for recent reviews, see: Hudson, Myer, & Berney, 2024; Kang & Bodenhausen, 2015; Nicolas, de la Fuente, & Fiske, 2017; Rosette, Ponce de Leon, Koval, & Harrison, 2018, Rosette, Li, Samuel, & Petsko, 2025). In the context of the present paper, we invoke the concept of intersectionality to investigate whether parenthood penalties depend not just on the gender groups to which people belong, but likewise on their racial groups. In particular, we examine whether the tendency for women (more than men) to be subjected to a parenthood penalty at work is reduced when the women in question are Black rather than White.

Why might the tendency for women (more than men) to be subjected to a parenthood penalty at work be reduced when the women in question are Black rather than White? There are two main reasons. First, Black women are often rendered intersectionally invisible in the minds of perceivers—that is, as less prototypic of the category *women* (Ponce de Leon & Rosette, 2022; Purdie-Vaughns & Eibach, 2008; Sesko & Biernat, 2010)—which can, in some circumstances, mitigate the extent to which perceivers exhibit gender-based backlash against Black women (e.g., Biernat & Sesko, 2013; Livingston, Rosette, & Washington, 2012). Second, the limited data on how race may intersect with parenthood perceptions suggest that although it is true that men receive something of a wage boost when they become parents (e.g., Lundberg & Rose, 2000), the magnitude of these wage boosts may be smaller for Black men vs. White men (Glauber, 2008). Taking these observations together—that Black (vs. White) women might face less gender-based backlash, and that Black (vs. White) men might receive less of a parenthood boost—suggests that the tendency for women (more than men) to be subjected to a parenthood penalty at work may be attenuated when the women and men in question are Black rather than White.

We should note that the prediction described in the preceding paragraph—that Black (vs. White) women might face less of a motherhood penalty—is based on a theoretical tradition that emphasizes the importance of women's prototypicality when determining who is likely to face gender-based backlash (Purdie-Vaughns & Eibach, 2008). However, this prediction is just one of several predictions one might make on the basis of the intersectional stereotyping literature. Indeed, an alternative prediction—rooted in notions of double-jeopardy (King, 1988), gendered racism (Essed, 1991), and stereotypes about Black mothers in particular (Rosenthal & Lobel, 2016)—might instead be that motherhood penalties would be *exacerbated* rather than attenuated for Black (vs. White) women. A desirable feature of the experiments reported here is that they enable tests of both predictions.

3. The present experiments, transparency, and openness

This manuscript reports on the results of three experiments—plus an additional experiment reported in the online supplement (total $N = 4742$)—that weigh in on (1) whether women, more than men, continue to be subjected to a parenthood penalty at work, and if so, (2) whether this bias is moderated by whether the women and men in question are Black rather than White. The experiments reported in this manuscript (Experiments 1, 2, and 3) were adapted from the materials of Cuddy et al. (2004). The supplemental experiment—called Exp. S1—was adapted instead from the materials of Heilman and Okimoto (2008, Exp. 2). The decision to conduct a supplemental experiment that used the materials of Heilman and Okimoto (2008) rather than those of Cuddy

et al. (2004) was done largely to examine whether the conclusions reported in this manuscript would be robust across different methods of assessing the motherhood penalty. Generally speaking, the conclusions of Exp. S1 are indeed consistent with those of Experiments 1, 2, and 3.² After presenting the main findings of Experiments 1, 2, and 3, we will present the results of an internal meta-analysis of all the experiments we have conducted on this topic, inclusive of Exp. S1.

Across experiments, we report all conditions, measures, and exclusions. Predictions and analysis plans for Experiment 3 and Exp. S1 were pre-registered in advance of data collection. Throughout this manuscript, raw effect sizes are encompassed by 95 % confidence intervals (CIs), and standardized effect sizes are reported as standard betas (β s). Statistical power for detecting main effects and interactions will be presented in the results sections of each experiment. Pre-registration documentation, data files, codebooks, survey materials, and R scripts associated with this manuscript are available on the Open Science Framework (OSF) website: <http://osf.io/h35gm/>.

4. Experiment 1

Experiment 1 tested two main predictions: (1) that women, more than men, would be subjected to a penalty at their jobs when described as parents (vs. when not described as parents), and (2) that this bias would be attenuated (or perhaps alternatively, exacerbated) when the women and men in question are Black (vs. White). By “penalty,” we refer to a tendency for perceivers to question the competence of, and to endorse greater discrimination against, women (vs. men) who become parents in the workplace. As noted, the methods, manipulations, and measures for Experiment 1 were taken from Cuddy et al. (2004), which is just one of several notable experiments that first provided evidence of a motherhood penalty (see: Correll et al., 2007; Fuegen et al., 2004; Heilman & Okimoto, 2008).

4.1. Method

Participants read about a fictional employee—a consultant at McKinsey & Company—whose characteristics we manipulated in a 2 (parenthood status: parent, non-parent) \times 2 (gender: woman, man) \times 2 (race: Black, White) between-person experiment. Participants rated this fictional employee on their competence, their warmth, and on three items that serve as proxies for discrimination (taken from Cuddy et al., 2004). Notably, we did not anticipate that a parenthood penalty would extend to perceptions of employee warmth. If anything, previous research suggests that people who become parents (vs. not) tend to receive a parenthood boost when it comes to “how warm” they seem (e.g., Cuddy et al., 2004).

4.1.1. Participants

Experiment 1 had an a priori aim of obtaining $n = 100$ participants per condition. A total of $N = 800$ participants were recruited from [CloudResearch.com](https://www.cloudresearch.com), of whom we excluded $n = 8$ (1.00 %) for not responding “yes” to the question, “Did you take this survey seriously?” and an additional $n = 3$ (0.38 %) for failing to respond correctly to at least three out of four questions that were designed to screen out bots. Demographics on the final analytic sample can be found in Table 1.

4.1.2. Procedure

Participants in Experiment 1 were given instructions adapted from prior research on this topic (i.e., Cuddy et al., 2004). Specifically, participants were told:

We’re studying how people quickly form first impressions, making important decisions from little information. We’d like you to read the profile of a consultant at McKinsey & Company’s Manhattan office and give us your first impression of them. Imagine you’re a client, trying to choose a consultant from very little information. Please try to respond with your first, uncensored impression.

Participants were then shown a short description of a fictional employee. This description always had the following structure:

Katelyn is a 32-year-old associate consultant who graduated with an MBA. She’s been working in her current field for six years. When working with a client, her duties include identifying issues, planning and conducting interviews and analyses, synthesizing conclusions into recommendations, and helping to implement change in her client’s organizations. Her hobbies include swimming and tennis. Katelyn and her husband recently had their first baby. She lives in central New Jersey, typically commuting to work two days a week and telecommuting three days a week.³

Parenthood status of the employee was manipulated by either including or excluding the underlined sentence mentioning that the consultant had recently had a baby. Gender and race of the employee were manipulated by showing each participant one of 60 different first names validated in prior research (in a sample of more than $N = 8000$ U. S. adults; Gaddis, 2017) as implying that a target person is Black or White. The manipulation of employee gender was reinforced by changing the employee’s pronouns, and if applicable, using the term “husband” or “wife” to ensure that all employees were implicated in a heterosexual marriage. Of the 60 names, 15 were ostensible White men’s names (e.g., Hunter, Jake, Seth), 15 were ostensible White women’s names (e.g., Katelyn, Claire, Megan), 15 were ostensible Black men’s names (e.g., DaShawn, Tremayne, Jamal), and 15 were ostensible Black women’s names (e.g., Shanice, Tamika, Tyra). For a complete list of all 60 names, see Table 2.⁴

After reading the employee description, all participants completed three dependent variables: a measure of employee competence, a measure of employee warmth, and a discrimination proxy measure. Measures of employee competence and warmth, respectively, were taken from the most up-to-date version of the stereotype content model (Fiske & North, 2015; Fiske, Xu, Cuddy, & Glick, 1999; Fiske, Cuddy, & Glick, 2002). For these measures, participants were asked to rate the extent to which they would describe their randomly assigned target employee as competent (four items: “competent,” “capable,” “skillful,” and “intelligent”; McDonald’s $\omega = 0.94$) and as warm (four items: “warm,” “friendly,” “sincere,” and “trustworthy”; McDonald’s $\omega = 0.92$). Responses ranged from 0 = *Not at all* to 6 = *Extremely*.

Participants were then shown the same three discrimination proxy items that were used in the study we sought to replicate (i.e., Cuddy et al., 2004). These three items were: “As a client, how likely would you be to request [name] as one of your consultants?”; “As a client, how likely would you be to recommend [name] for a promotion?”; and “As a

³ A key difference between the experiment we sought to replicate (i.e., Cuddy et al., 2004) and the experiment described here is that the original experiment had participants read about three separate consultants: two bogus consultants, and a key consultant whose gender and parenthood status was manipulated. In our experiment, we had participants read only about the key consultant. Thus, our instructions only referenced one profile rather than several profiles. Instructions were otherwise parallel across the experiment of Cuddy et al. (2004) and present experiments.

⁴ The decision to use 60 names, rather than just two names (Dan and Kate), was meant to serve as a methodological improvement over the experiment we were attempting to replicate (that described in Cuddy et al., 2004). In general, sampling from a broad range of stimulus names reduces the risk of making a Type I error and increases the generality of one’s conclusions (e.g., Judd, Westfall, & Kenny, 2012).

² However, it is worth noting that parenthood boost magnitudes are smaller in Exp. S1 than they are in Experiments 1, 2, and 3 (see the online supplement for a full report).

Table 1
Final Participant Demographics (After Exclusions) Across Analytic Samples.

		Experiment 1 N = 789	Experiment 2 N = 1198	Experiment 3 N = 1569	Internal Meta N = 4742
Age	Mean	39.37	38.32	40.58	40.78
	SD	11.97	12.35	13.17	12.82
	Range	18 to 77	19 to 84	19 to 83	18 to 84
Gender	Male	49.9 %	47.0 %	47.0 %	48.7 %
	Female	48.4 %	51.0 %	51.9 %	50.1 %
	Trans	1.0 %	1.3 %	0.3 %	0.7 %
	Non-Binary	2.6 %	0.8 %	0.4 %	0.6 %
	Another Gender	0.0 %	0.5 %	0.1 %	0.2 %
Race	White	74.0 %	74.0 %	75.4 %	76.0 %
	Asian	11.3 %	6.8 %	09.8 %	9.0 %
	Black	8.4 %	11.3 %	8.3 %	9.0 %
	Latino/a	5.3 %	6.5 %	4.7 %	5.3 %
	Islander	0.3 %	0.5 %	0.4 %	0.4 %
	Native	0.3 %	0.7 %	0.6 %	0.7 %
	Alternative	0.6 %	1.5 %	1.3 %	1.1 %
Ideology	Mean	4.19	3.68	4.11	4.12
	SD	2.82	2.72	2.92	2.87
	Range	0 to 10	0 to 10	0 to 10	0 to 10
Edu.	Bachelor's or higher	63.4 %	56.0 %	57.0 %	58.9 %

Note. All participants were U.S. citizens. *Ideology* was measured on a scale from 0 = *Extremely Liberal* to 10 = *Extremely Conservative*. *Edu.* = participants' highest level of educational attainment. Instances in which percentages add up to more than 100 are a result of participants being allowed to select more than one gender and/or racial identity. The N for the Internal Meta is inclusive of participants from Exp. S1.

Table 2
Stimulus Names Implicating Each of Four Race-by-Gender Intersections.

Name Type	White Man	White Woman	Black Man	Black Woman
Hunter	Katelyn	DaShawn	Tanisha	
Jake	Claire	Tremayne	Lakisha	
Seth	Laurie	Jamal	Tamika	
Zachary	Megan	DaQuan	Latoya	
Todd	Kristen	DeAndre	Keyana	
Matthew	Emily	Tyrone	Latonya	
Ryan	Sarah	Keyshawn	Shanice	
Scott	Molly	Latrell	Tyra	
Dustin	Jill	Jayvon	Ebony	
Brett	Hilary	Darnell	Denisha	
Ethan	Meredith	Lamar	Keisha	
Connor	Amy	Terell	Aisha	
Steven	Susan	Darius	Ashanti	
Luke	Allison	Kareem	Precious	
Graham	Anne	Jermaine	Kenya	

Note. Each name has been normed as implying its designated racial category at least 80 % of the time in previous large-scale studies (see Gaddis, 2017). Across experiments, participants were assigned just one of these 60 names.

client, how likely would you be to recommend that McKinsey & Company invest in continuing training and education for [name]?”. Endorsement of these statements was measured from 0 = *Not at all* to 6 = *Extremely*. These three discrimination proxy items were internally reliable (McDonald's $\omega = 0.87$). As such, these items were averaged together and reverse-scored such that higher scores reflect greater discrimination against employees. Means, standard deviations, and standardized relationships between dependent measures can be found in Table 3.

4.2. Results

Each dependent variable—perceived competence, perceived warmth, and endorsed discrimination—was subjected to a 2 (parent-hood status: parent, non-parent) \times 2 (gender: woman, man) \times 2 (race:

Table 3
Standardized Relationships between Dependent Measures in Experiment 1.

Dependent Variable	M (SD)	Competence	Warmth	Discrimination
Competence	4.91 (0.90)	–		
Warmth	4.30 (1.02)	–0.63***	–	
Discrimination	1.65 (1.09)	–0.68***	–0.63***	–

Note. *** = $p < .001$. *Competence* = average competence rating of consultant; *warmth* = average warmth rating of consultant; *discrimination* = average consultant ratings on discrimination proxy items, reverse-scored such that higher scores are indicative of greater discrimination against consultants (i.e., a greater desire *not* to request, train, or promote consultants). Standardized relationships represent Pearson's r values.

Black, White) analysis of variance (ANOVA). According to Monte Carlo simulations, our sample size and analytic approach gave us at least 80 % power to detect main effects as small as $\beta = 0.20$, two-way interactions as small as $\beta = 0.40$, and three-way interactions as small as $\beta = 0.80$ (see Table 4 for all model estimates).⁵

4.2.1. Do Parenthood Perceptions Depend on Employee Gender?

On the basis of prior research (Cuddy et al., 2004), we anticipated that women, more than men, would be subjected to a penalty at their jobs when described as parents. That is, we expected a tendency for participants to question the competence of, and to endorse greater discrimination against, women (vs. men) who were described as parents in the workplace. Was there any support for this prediction? To our surprise, there was not. Subjecting each of the three dependent

⁵ All power analyses were conducted using the “simr” package in R (Green & MacLeod, 2016), which runs Monte Carlo simulations on one's models to arrive at power estimates (for more on this technique, see Bolger, Stadler, & Laurenceau, 2012). Throughout this manuscript, standardized effect sizes (β s) were computed by regressing z-standardized outcomes onto orthogonal condition contrasts (which summed to zero and always had a range of one; Judd, McClelland, & Ryan, 2017). Under this approach, standard betas for main effect tests can be interpreted similarly to Cohen's d s.

Table 4
Model Estimates from Experiment 1.

Effect Estimates	Model 1: Employee Competence			Model 2: Employee Warmth			Model 3: Discrimination		
	β	SE	<i>p</i>	β	SE	<i>p</i>	β	SE	<i>p</i>
Intercept	0.00	0.04	.988	0.00	0.03	.971	0.00	0.04	.982
Parenthood	0.17	0.07	.016	0.35	0.07	< .001	-0.17	0.07	.018
Gender	0.07	0.07	.327	0.15	0.07	.038	-0.11	0.07	.119
Race	0.12	0.07	.086	0.05	0.07	.450	-0.13	0.07	.072
P × G	0.07	0.14	.624	0.06	0.14	.686	0.05	0.14	.715
P × R	0.05	0.14	.728	-0.09	0.14	.525	-0.01	0.14	.959
G × R	-0.38	0.14	.008	-0.21	0.14	.134	0.24	0.14	.089
P × G × R	0.16	0.28	.574	0.44	0.28	.114	-0.10	0.28	.723

Note. For each model, dependent variables were z-standardized and regressed onto contrast codes representing the effects of parenthood status (coded as: *parents* = ½ and *non-parents* = -½), employee gender (coded as *women* = ½ and *men* = -½), employee race (coded as *Black* = ½ and *White* = -½), as well as their interactions with each other. P = parenthood status; G = employee gender; R = employee race.

variables—perceived competence, perceived warmth, and endorsed discrimination—to a 2 (parenthood status) × 2 (gender) × 2 (race) ANOVA revealed no evidence of a two-way interaction between parenthood status and gender for any of our three dependent variables. There was no evidence of a parenthood status × gender interaction for perceived competence [$\beta = 0.07$, $F(1, 781) = 0.24$, $p = .624$], no evidence of a parenthood status × gender interaction for perceived warmth [$\beta = 0.06$, $F(1, 781) = 0.16$, $p = .686$], and there was no evidence of a parenthood status × gender interaction for endorsed discrimination [$\beta = 0.05$, $F(1, 781) = 0.13$, $p = .715$]. Instead, these ANOVAs revealed only main effects of parenthood status, which suggested that participants regarded parents, in comparison with non-parents, as more competent [$M_{diff} = 0.15$, 95 % CI[0.03, 0.28], $\beta = 0.17$, $F(1, 781) = 5.86$, $p = .016$], as more warm [$M_{diff} = 0.36$, 95 % CI[0.22, 0.50], $\beta = 0.35$, $F(1, 781) = 25.60$, $p < .001$], and as less deserving of discrimination [$M_{diff} = -0.18$, 95 % CI[-0.33, -0.03], $\beta = -0.17$, $F(1, 781) = 5.62$, $p = .018$]. Thus, we did not find support for the hypothesis that women, more than men, would face a penalty at work when described as parents. Instead, we found that all employees, regardless of gender, were regarded more positively when they were described (vs. not described) as parents (see Fig. 1).

4.2.2. Do Parenthood Perceptions Depend on Employee Race?

What about race? Was there evidence that women (vs. men) who are described as parents are held to different standards when they are Black (vs. White)? In contrast to our predictions, there was not. The same 2 × 2 × 2 ANOVAs described in the prior paragraph revealed no evidence of three-way interactions [perceived competence: $\beta = 0.16$, $F(1, 781) = 0.32$, $p = .574$; perceived warmth: $\beta = 0.44$, $F(1, 781) = 2.50$, $p = .114$; endorsed discrimination: $\beta = -0.10$, $F(1, 781) = 0.13$, $p = .723$], and they likewise revealed no evidence of two-way interactions between parenthood status and race [perceived competence: $\beta = 0.05$, $F(1, 781) = 0.12$, $p = .728$; perceived warmth: $\beta = -0.09$, $F(1, 781) = 0.41$, $p = .525$; endorsed discrimination: $\beta = -0.01$, $F(1, 781) < 0.01$, $p = .959$]. Thus, it was not the case, at least in this first experiment, that employees' racial groups influenced judgments of how competent, how warm, or how deserving of discrimination they seemed when described (vs. not described) as parents. Race did not moderate perceptions of parenthood on its own, and race and gender did not moderate perceptions of parenthood in tandem (for all model estimates, see Table 4).⁶

⁶ Across experiments, we occasionally find evidence for (a) main effects of employee race, and (b) two-way interactions between employee race and employee gender. Because these findings are ancillary to the question of how parents (vs. non-parents) are judged, we do not discuss them in this manuscript. However, we provide a full report on these findings in the online supplement.

4.3. Discussion

Experiment 1 examined whether the findings of one well-cited demonstration of the motherhood penalty (that of Cuddy et al., 2004) would hold up to scrutiny when tested in a highly powered, contemporary sample of U.S. adults. To our surprise, Experiment 1 revealed no evidence that women, more than men, continue to be subjected to a parenthood penalty at work (c.f. Correll et al., 2007; Fuegan et al., 2004; and Heilman & Okimoto, 2008). Instead, Experiment 1 revealed that those who were described (vs. not described) as parents received a generalized *parenthood boost*: a tendency to be evaluated more positively by perceivers when described as parents. Moreover, the magnitude of this parenthood boost appeared to extend equally to men and women, as well as to Black employees and White employees—at least in this first experiment.

5. Experiment 2

A limitation of Experiment 1 is that its manipulation of parenthood status contains a confound (one that tends to be common in paradigms that examine the motherhood penalty: e.g., Cuddy et al., 2004; Heilman & Okimoto, 2008). Target employees who were described as parents were likewise described as having spouses. As such, in Experiment 2 we decided to replicate Experiment 1, but to change the manipulation of parenthood status so that it eliminated this confound. In particular, participants in Experiment 2 completed a nearly identical procedure to that of Experiment 1, with one exception—parents were described as *single* parents rather than as parents who were raising a child with their spouse.

An additional benefit of Experiment 2, beyond eliminating the confound described above, is that it provided an alternative context for examining the possibility that motherhood might be evaluated differently among Black women versus White women in the workplace. Given that Black women, more than White women, tend to be stereotyped as raising children as single mothers (Mehra et al., 2020) and given also that Black women tend to be stereotyped negatively in this context (Rosenthal & Lobel, 2016), it seemed plausible that a vignette that referred to parents as *single* parents might facilitate an intersectional penalty specifically against Black women. Of note, the possibility that priming participants with the concept of single parenthood might change responses to Black women is consistent with recent accounts of intersectional stereotyping (e.g., Petsko, Rosette, & Bodenhausen, 2022), which suggest that intersectional prejudices may not be ever-present, but may instead require situational triggers that bring intersections of targets' identities into focus.

5.1. Method

As in Experiment 1, participants in Experiment 2 read about a target

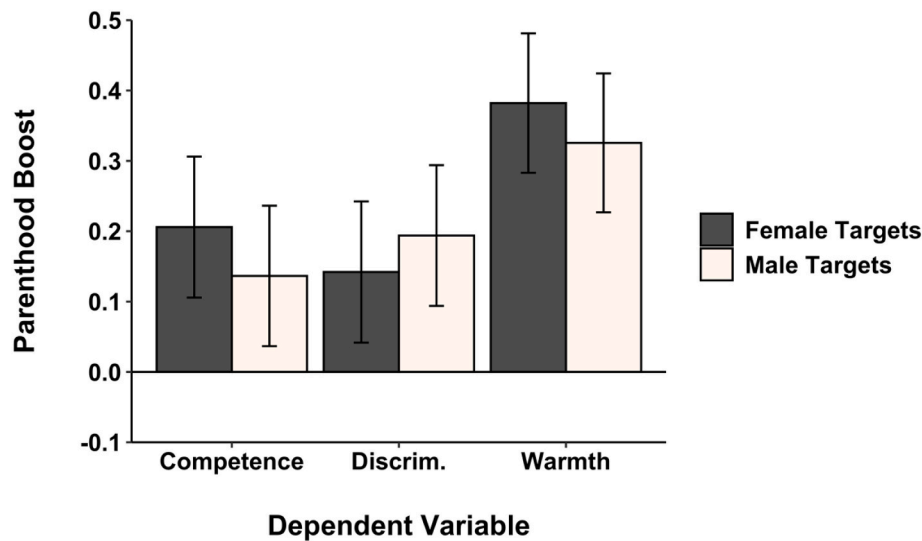


Fig. 1. Parenthood Boost Magnitudes as a Function of Targets' Gender Groups (Exp. 1).

Note. The y-axis depicts the (z-standardized) magnitudes by which parents are evaluated more favorably than non-parents (i.e., parenthood boosts) across three dependent variables. Scores above zero indicate that parents are evaluated more positively than non-parents; scores below zero indicate the reverse. Competence = ratings of employee competence; Discrim. = ratings of discrimination in favor of employees (i.e., greater intentions to request, promote, and train employees); Warmth = ratings of employee warmth. Parenthood boosts for women employees are depicted in dark gray; parenthood boosts for men employees are depicted in light pink. Effect sizes are encompassed by standard errors.

employee whose characteristics we manipulated in a 2 (parenthood status: parent, non-parent) \times 2 (gender: woman, man) \times 2 (race: Black, White) between-person experimental design. Unlike Experiment 1, parents in Experiment 2 were described as *single* parents—as raising children by themselves.

5.1.1. Participants

Experiment 2 aimed to recruit $n = 150$ participants per condition. This recruitment goal thus represented a 50 % increase over the recruitment goal of Experiment 1. A total of $N = 1202$ participants were recruited from Prolific.com, of whom $n = 4$ (0.33 %) were excluded because they did not respond affirmatively to the question, “Did you take this survey seriously?” See Table 1 for demographics on the final sample.

5.1.2. Procedure

Participants in Experiment 2 saw the same instructions as participants in Experiments 1. The only thing that differentiated Experiment 2 from Experiment 1 was that parents were described as *single* parents (rather than as parents who were raising a child with their spouse). In particular, participants in Experiment 2 were all shown a vignette that had the following structure:

Katelyn is a 32-year-old associate consultant who graduated with an MBA. She's been working in her current field for six years. When working with a client, her duties include identifying issues, planning and conducting interviews and analyses, synthesizing conclusions into recommendations, and helping to implement change in her client's organizations. Her hobbies include swimming and tennis. Katelyn has one child, whom she is raising as a single mother. She lives in central New Jersey, typically commuting to work two days a week and telecommuting three days a week.

Parenthood status of the employee was again manipulated by either including or excluding the underlined sentence mentioning that the consultant had recently had a baby. Gender and race of the employee were manipulated by showing each participant one of the same 60 different first names that were used in Experiment 1 (see Table 2). Target gender manipulations were reinforced by changing the phrase “single mother” to “single father” in the male-target conditions, as well as by changing pronouns from *she/her* to *he/him*.

After reading the description of their randomly assigned target employee, all participants completed the same measures of competence (McDonald's $\omega = 0.94$), warmth (McDonald's $\omega = 0.92$), and discrimination (McDonald's $\omega = 0.88$) that were described in Experiment 1. Again, discrimination proxy items were reverse-scored such that higher scores indicate higher levels of discrimination against the employee. Scores on competence, warmth, and discrimination ranged from 0 = *Not at all* to 6 = *Extremely*. Finally, Experiment 2 contained two exploratory measures (taken from Morgenroth & Heilman, 2017): a three-item measure of job commitment (e.g., “This person is committed to his or her job”; McDonald's $\omega = 0.67$), and a three-item measure of family commitment (e.g., “This person is committed to his or her family”; McDonald's $\omega = 0.85$). Exploratory measures were completed on scales from 1 = *Strongly Disagree* to 7 = *Strongly Agree*. Means, standard deviations, and standardized relationships between dependent measures can be found in Table 5.

5.2. Results

Each dependent variable—perceived competence, perceived warmth, and endorsed discrimination—was subjected to a 2 (parenthood status: parent, non-parent) \times 2 (gender: woman, man) \times 2 (race: Black, White) analysis of variance (ANOVA).⁷ According to Monte Carlo simulations, our sample size and analytic approach gave us at least 80 % power to detect main effects as small as $\beta = 0.17$, to detect two-way interactions as small as $\beta = 0.33$, and to detect three-way interactions as small as $\beta = 0.66$. Thus, Experiment 2 indeed had higher statistical power than Experiment 1 (see Table 6 for all model estimates).

5.2.1. Do Parenthood Perceptions Depend on Employee Gender?

In Experiment 1, we found that there was no detectable evidence of a motherhood penalty. What did we find in Experiment 2, which featured

⁷ In the service of brevity, we report on only the main dependent variables of competence, warmth, and endorsed discrimination. Analyses and findings related to the exploratory measures (job commitment and family commitment), for Experiment 2 as well as for Experiment 3, are reported in full in the online supplement.

Table 5
Standardized Relationships between Dependent Measures in Experiment 2.

Dependent Variable	<i>M</i> (<i>SD</i>)	Competence	Warmth	Discrimination	Job Comm.
Competence	4.92 (0.82)	–			
Warmth	4.36 (0.94)	–0.66***	–		
Discrimination	1.65 (1.03)	–0.65***	–0.66***	–	
Job Comm.	5.45 (0.99)	–0.57***	–0.40***	–0.52***	–
Family Comm.	5.15 (1.22)	–0.36***	–0.47***	–0.36***	–0.27***

Note. *** = $p < .001$. *Competence* = average competence rating of consultant; *warmth* = average warmth rating of consultant; *discrimination* = average consultant ratings on discrimination proxy items, reverse-scored such that higher scores are indicative of greater discrimination against consultants (i.e., a greater desire to not request, train, or promote consultants); *job comm.* = perceived job commitment of consultant; *family comm.* = perceived family commitment of consultant. Standardized relationships represent Pearson's r values.

Table 6
Model Estimates from Experiment 2.

Effect Estimates	Model 1: Employee Competence			Model 2: Employee Warmth			Model 3: Discrimination		
	β	<i>SE</i>	<i>p</i>	β	<i>SE</i>	<i>p</i>	β	<i>SE</i>	<i>p</i>
Intercept	0.00	0.03	.997	0.00	0.03	.989	0.00	0.03	.994
Parenthood	0.13	0.06	.027	0.36	0.06	< .001	–0.16	0.06	.006
Gender	0.08	0.06	.165	0.08	0.06	.185	–0.07	0.06	.251
Race	–0.09	0.06	.133	0.07	0.06	.236	–0.01	0.06	.855
P × G	–0.12	0.12	.305	–0.20	0.11	.086	0.18	0.12	.111
P × R	0.00	0.12	.985	–0.05	0.11	.673	0.03	0.12	.826
G × R	–0.15	0.12	.194	–0.06	0.11	.625	0.31	0.12	.008
P × G × R	0.01	0.23	.949	0.04	0.23	.852	0.08	0.23	.734

Note. For each model, dependent variables were z-standardized and regressed onto contrast codes representing the effects of parenthood status (coded as: *single parents* = ½ and *non-parents* = –½), employee gender (coded as *women* = ½ and *men* = –½), employee race (coded as *Black* = ½ and *White* = –½), as well as their interactions with each other. P = parenthood status; G = employee gender; R = employee race.

single parents rather than partnered parents? Consistent with what we found previously, subjecting each of the three dependent variables—perceived competence, perceived warmth, and endorsed discrimination—to 2 (parenthood status) × 2 (gender) × 2 (race) ANOVAs revealed no evidence of two-way interactions between parenthood status and gender for any of our three dependent variables. There was no evidence of a parenthood status × gender interaction for perceived competence [$\beta = -0.12$, $F(1, 1190) = 1.05$, $p = .305$], no evidence of a parenthood status × gender interaction for perceived warmth [$\beta = -0.20$, $F(1, 1190) = 2.95$, $p = .086$], and there was no evidence of a parenthood status × gender interaction for endorsed discrimination [$\beta = 0.18$, $F(1, 1190) = 2.54$, $p = .111$]. Instead, and as reported in Experiment 1, these ANOVAs revealed only main effects of parenthood status, which suggested that participants regarded parents, in comparison with non-parents, as more competent [$M_{diff} = 0.10$, 95 % CI [0.01, 0.20], $\beta = 0.13$, $F(1, 1190) = 4.92$, $p = .027$], as more warm [$M_{diff} = 0.34$, 95 % CI [0.24, 0.45], $\beta = 0.36$, $F(1, 1190) = 50.86$, $p < .001$], and as less deserving of discrimination [$M_{diff} = -0.16$, 95 % CI [–0.28, –0.05], $\beta = -0.16$, $F(1, 1190) = 7.56$, $p = .006$]. Thus, consistent with Experiment 1, we do not find evidence that women, more than men, face a penalty at work when described as parents. Instead, we again find that all employees, regardless of their gender groups, receive a parenthood boost.

5.2.2. Do Parenthood Perceptions Depend on Employee Race?

What about race? Was there evidence that women (vs. men) who are described as single parents are held to different standards when they are Black (vs. White)? Consistent with Experiment 1, there was no evidence that parenthood perceptions were moderated by targets' racial groups—either alone or in tandem with targets' gender groups. This is to say that the same 2 × 2 × 2 ANOVAs described in the prior paragraph revealed no evidence of three-way interactions [competence: $\beta = 0.01$, $F(1, 1190) < 0.01$, $p = .949$; warmth: $\beta = 0.04$, $F(1, 1190) = 0.03$, $p = .852$; discrimination: $\beta = 0.08$, $F(1, 1190) = 0.12$, $p = .734$], and they likewise revealed no evidence of two-way interactions between parenthood status and race [competence: $\beta < 0.01$, $F(1, 1190) < 0.01$, $p = .985$; warmth: $\beta = -0.05$, $F(1, 1190) = 0.18$, $p = .673$; discrimination: $\beta = 0.03$, $F(1, 1190) = 0.05$, $p = .826$]. Thus, it was not the case that

employees' racial groups influenced judgments of how competent, warm, or deserving of discrimination they seemed when described (vs. not described) as parents. As in Experiment 1, race did not moderate perceptions of parenthood on its own, nor did race and gender moderate perceptions of parenthood in tandem—even when parents were described as *single parents* (for model estimates, see Table 6).

5.3. Discussion

In Experiment 2, participants evaluated target employees who either were or were not described as *single parents*, and whose race and gender we manipulated. Once again, we found no evidence of a motherhood penalty (c.f. Correll et al., 2007; Fuegan et al., 2004; and Heilman & Okimoto, 2008). Instead, and replicating Experiment 1, the most consistent effect we observed was that those who were described as parents (vs. not described as parents) received a *parenthood boost*: more positive evaluations in the eyes of perceivers when described as having children. The findings of Experiment 2 are significant for two main reasons. First, these findings suggest that the parenthood boosts identified in Experiment 1 are not attributable to the fact that parents (vs. non-parents) in Experiment 1 had been described as having spouses. Parenthood boosts are observable whether parents are raising children with their spouses or are instead raising children as single mothers and fathers. Second, these findings suggest that negative stereotypes about Black mothers, which no doubt emerge in many contexts (Mehra et al., 2020; Rosenthal & Lobel, 2016), may occasionally be supplanted in perceivers' minds by positive stereotypes related to parenthood. Indeed, in Experiment 2, parenthood boosts were directed toward targets irrespective of their gender groups and racial groups.

6. Experiment 3

Because the prior findings surprised us, we decided to conduct one final experiment: a large, pre-registered replication of Experiment 1. For this final experiment (Experiment 3), we pre-registered the prediction that we would replicate all previously observed findings: that we would

see confirmatory evidence of (1) a general parenthood boost that held (2) regardless of target employees' gender groups, racial groups, and intersecting race-by-gender groups. Experiment 3 contained double the sample-size of Experiment 1 ($N = 1604$ people in total).

6.1. Method

As in the prior experiments, participants in Experiment 3 read about a target employee whose characteristics we manipulated in a 2 (parenthood status: parent, non-parent) \times 2 (gender: woman, man) \times 2 (race: Black, White) between-person experimental design.

6.1.1. Participants

Experiment 3 had an a priori aim of obtaining $n = 200$ participants per condition. A total of $N = 1604$ participants were recruited from [CloudResearch.com](https://www.cloudresearch.com), of whom we excluded $n = 35$ (2.18 %) for not responding "yes" to the question, "Did you take this survey seriously?" when asked explicitly at the end of the experiment. See [Table 1](#) for demographics on the final sample.

6.1.2. Procedure

Experiment 3 was a direct replication of Experiment 1. As such, its survey flow, instructions, and experimental materials were the same as those described in Experiment 1 (which is to say, nearly identical to those of [Cuddy et al., 2004](#)). Again, participants were shown a short description of a fictional employee—a 32-year-old associate at McKinsey & Company—whose parenthood status, gender group, and racial group was experimentally manipulated.

After reading the description of their randomly assigned target employee, all participants completed the same measures of competence (McDonald's $\omega = 0.94$) warmth (McDonald's $\omega = 0.93$), and discrimination (McDonald's $\omega = 0.88$) that were described in the prior experiments. Again, discrimination proxy items were reverse-scored such that higher scores indicate higher levels of discrimination against the employee. Scores on competence, warmth, and discrimination ranged from 0 = *Not at all* to 6 = *Extremely*. Finally, Experiment 3 contained the same two exploratory measures that were described in Experiment 2: a three-item measure of job commitment (McDonald's $\omega = 0.74$), and a three-item measure of family commitment (McDonald's $\omega = 0.82$). Exploratory measures were completed on scales from 1 = *Strongly Disagree* to 7 = *Strongly Agree*. Means, standard deviations, and standardized relationships between dependent measures can be found in [Table 7](#).

6.2. Results

Analyses in Experiment 3 relied on 2 (parenthood status: parent, non-parent) \times 2 (gender: woman, man) \times 2 (race: Black, White) ANOVAs. According to Monte Carlo simulations, our sample size and analytic approach gave us at least 80 % power to detect main effects as small as $\beta = 0.15$, to detect two-way interactions as small as $\beta = 0.29$, and to detect three-way interactions as small as $\beta = 0.57$. Thus, Experiment 3 was the most highly powered standalone experiment reported in this manuscript (see [Table 8](#) for all model estimates).

6.2.1. Do Parenthood Perceptions Depend on Employee Gender?

Our pre-registered prediction was that we would replicate the (surprising) findings from the prior experiments. In particular, we anticipated that parenthood perceptions would *not* depend on employee gender. To investigate whether this prediction was supported, each of the three main dependent variables—perceived competence, perceived warmth, and endorsed discrimination—was subjected to a 2 (parenthood status) \times 2 (gender) \times 2 (race) ANOVA. Consistent with the prior experiments as well as with our pre-registered predictions, each of these analyses revealed no evidence of a two-way interaction between parenthood status and gender. There was no evidence of a parenthood status \times gender interaction for perceived competence [$\beta = 0.04$, $F(1,$

1561) = 0.18, $p = .667$], for perceived warmth [$\beta = 0.05$, $F(1, 1561) = 0.21$, $p = .647$], or for endorsed discrimination [$\beta = -0.06$, $F(1, 1561) = 0.33$, $p = .567$]. Thus, in Experiment 3, as in Experiments 1 and 2, women and men were *not* held to different standards when they were described (vs. not described) as parents.

In the prior experiments, we observed clear main effects of parenthood status, with employees who were described as parents (vs. employees who were not described as parents) being evaluated more positively across each of our three dependent variables. As such, we pre-registered an expectation that we would observe the same pattern of main effects in Experiment 3. However, main effect tests from the ANOVAs described in the preceding paragraph were more variable in Experiment 3 than they had been in the prior experiments. Although parents were indeed regarded as warmer than non-parents [$M_{\text{diff}} = 0.26$, 95 % CI[0.16, 0.36], $\beta = 0.26$, $F(1, 1561) = 27.53$, $p < .001$], parents were not rated as significantly more competent than non-parents [$M_{\text{diff}} = 0.02$, 95 % CI[-0.07, 0.11], $\beta = 0.02$, $F(1, 1561) = 0.19$, $p = .666$], and parents were only directionally less likely than non-parents to be targeted by discriminatory beliefs [$M_{\text{diff}} = -0.11$, 95 % CI[-0.22, 0.00], $\beta = -0.10$, $F(1, 1561) = 3.74$, $p = .053$]. Thus, although there was some evidence of a "parenthood boost" in Experiment 3, this pattern was less stark in Experiment 3 than it was in the prior experiments.

6.2.2. Do Parenthood Perceptions Depend on Employee Race?

Based on the findings from our prior experiments, we pre-registered an expectation that perceptions of parenthood would not be moderated by whether employees were Black versus White. However, in contrast to this expectation, we actually found evidence across each of our dependent variables that employee race interacted with parenthood status—at least in this final experiment. That is, a parenthood status \times race interaction emerged for perceived competence [$\beta = -0.20$, $F(1, 1561) = 4.03$, $p = .045$], for perceived warmth [$\beta = -0.29$, $F(1, 1561) = 8.64$, $p = .003$], and for endorsed discrimination [$\beta = 0.27$, $F(1, 1561) = 7.21$, $p = .007$]. In all three cases, the direction of these two-way interactions was the same. White employees received more of an evaluative boost than Black employees did when they were described (vs. not described) as parents. Whereas participants rated White parents (vs. non-parents) as warmer [$M_{\text{diff}} = 0.41$, 95 % CI[0.27, 0.55], $\beta = 0.41$, $F(1, 1561) = 33.63$, $p < .001$], as less deserving of discrimination [$M_{\text{diff}} = -0.26$, 95 % CI[-0.42, -0.10], $\beta = -0.23$, $F(1, 1561) = 10.70$, $p = .001$], and as marginally more competent [$M_{\text{diff}} = 0.11$, 95 % CI[-0.01, 0.23], $\beta = 0.12$, $F(1, 1561) = 2.99$, $p = .084$], this evaluative boost did not extend to Black employees. Black employees were viewed as no warmer [$M_{\text{diff}} = 0.12$, 95 % CI[-0.02, 0.25], $\beta = 0.11$, $F(1, 1561) = 2.65$, $p = .104$], as no less deserving of discrimination [$M_{\text{diff}} = 0.04$, 95 % CI[-0.11, 0.20], $\beta = 0.04$, $F(1, 1561) = 0.28$, $p = .596$], and as no more competent [$M_{\text{diff}} = -0.07$, 95 % CI[-0.20, 0.05], $\beta = -0.23$, $F(1, 1561) = 1.24$, $p = .266$] when they were described (vs. not described) as parents. Thus, although it was not the case that women vs. men were held to different standards when they were described as parents, it was the case—at least in the context of this final, highly-powered experiment—that Black employees vs. White employees were held to different standards. Whereas White employees received an evaluative "boost" when described as parents, Black employees did not.⁸

6.3. Discussion

Unlike the prior experiments, the predictions and analytic strategy for Experiment 3 were pre-registered in advance of data collection. In

⁸ As can be seen in [Table 8](#), there was no evidence that these parenthood status \times employee race interactions were further moderated by employee gender. This suggests that although there was clear evidence of a racial bias—a stronger "parenthood boost" for White employees than Black employees—there was no evidence of a gender bias, or of a bias that depended on employees' intersecting race-by-gender identities.

Table 7
Standardized Relationships between Dependent Measures in Experiment 3.

Dependent Variable	<i>M</i> (<i>SD</i>)	Competence	Warmth	Discrimination	Job Comm.
Competence	4.93 (0.89)	–			
Warmth	4.34 (1.01)	–0.68***	–		
Discrimination	1.74 (1.12)	–0.71***	–0.70***	–	
Job Comm.	5.49 (1.04)	–0.62***	–0.44***	–0.56***	–
Family Comm.	5.11 (1.21)	–0.34***	–0.42***	–0.34***	–0.25***

Note. *** = $p < .001$. *Competence* = average competence rating of consultant; *warmth* = average warmth rating of consultant; *discrimination* = average consultant ratings on discrimination proxy items, reverse-scored such that higher scores are indicative of greater discrimination against consultants (i.e., a greater desire to not request, train, or promote consultants); *job comm.* = perceived job commitment of consultant; *family comm.* = perceived family commitment of consultant. Standardized relationships represent Pearson's r values.

Table 8
Model Estimates from Experiment 3.

Effect Estimates	Model 1: Employee Competence			Model 2: Employee Warmth			Model 3: Discrimination		
	β	<i>SE</i>	<i>p</i>	β	<i>SE</i>	<i>p</i>	β	<i>SE</i>	<i>p</i>
Intercept	0.00	0.03	.980	0.00	0.02	.975	0.00	0.03	.974
Parenthood	0.02	0.05	.666	0.26	0.05	< .001	–0.10	0.05	.053
Gender	–0.00	0.05	.934	0.08	0.05	.120	–0.05	0.05	.291
Race	0.16	0.05	.001	0.21	0.05	< .001	–0.22	0.05	< .001
P × G	0.04	0.10	.667	0.05	0.10	.647	–0.06	0.10	.567
P × R	–0.20	0.10	.045	–0.29	0.10	.003	0.27	0.10	.007
G × R	–0.13	0.10	.193	–0.08	0.10	.402	0.10	0.10	.306
P × G × R	–0.14	0.20	.491	–0.04	0.20	.839	0.04	0.20	.836

Note. For each model, dependent variables were z-standardized and regressed onto contrast codes representing the effects of parenthood status (coded as: *parents* = $\frac{1}{2}$ and *non-parents* = $-\frac{1}{2}$), employee gender (coded as *women* = $\frac{1}{2}$ and *men* = $-\frac{1}{2}$), employee race (coded as *Black* = $\frac{1}{2}$ and *White* = $-\frac{1}{2}$), as well as their interactions with each other. P = parenthood status; G = employee gender; R = employee race.

addition, Experiment 3 had double the sample size of the experiment on which it was based (Experiment 1), allowing for a replication of Experiment 1 in a design with markedly higher statistical power. Much like the prior experiments, Experiment 3 showed no evidence of a motherhood penalty—no evidence that evaluations of parents (vs. non-parents) differed as a function of whether they were women vs. men. Instead, Experiment 3 revealed evidence of what appears to be a race-based parenthood boost—specifically, a tendency among participants to evaluate parents more favorably than non-parents when the parents in question are White, but not when the parents in question are Black. To be clear, this final finding was not something we expected, and it should therefore be interpreted as exploratory. Nevertheless, it is worth noting that in Exp. S1, which was a pre-registered experiment that relied on Heilman and Okimoto's (2008) paradigm rather than Cuddy et al.'s (2004) paradigm, we again found evidence to suggest that parenthood boosts may be dependent on targets' racial groups. See the online supplement for a full report.

7. Internal meta-analysis

Although findings from Experiments 1 and 2 were highly consistent, the findings from Experiment 3 were somewhat more variable. Given this variability, we sought to conduct one final analysis: an internal meta-analysis, following the approach pre-registered here (for more on this topic, see: Goh, Hall, & Rosenthal, 2016; McShane & Böckenholt, 2017), to assess whether parenthood boosts—and perhaps even race-by-parenthood interactions, as detected in Experiment 3—would hold up to scrutiny when examined in the most highly-powered set of tests that we could conduct. A notable feature of this analysis is that, in addition to combining data from Experiments 1, 2, and 3, this analysis was likewise able to incorporate the data from Exp. S1 (which is reported in full in the online supplement).

Unlike Experiments 1, 2, and 3—which each relied on the vignette-based paradigm of Cuddy et al. (2004)—Exp. S1 relied on the motherhood-penalty paradigm reported in Heilman and Okimoto (2008). In this latter paradigm, participants were told that they would be asked to read two pieces of information: a) a job description about a particular job

(for all participants, the job of *Assistant VP of Financial Affairs* at a fictional company) as well as b) an “information sheet” about a particular employee who was being considered for promotion to that job. As in Experiments 1, 2, and 3, participants always read about a candidate whose parental status (children, no children), gender identity (man, woman), and racial identity (White, Black) were manipulated. In addition, Exp. S1 included dependent measures that were conceptually parallel with those of Experiments 1, 2, and 3.⁹ Given these structural similarities, the internal meta-analysis reported below was able to incorporate participants from Exp. S1 alongside those from Experiments 1, 2, and 3. Aggregating the data in this way afforded an analysis on a total meta-analytic sample of $N = 4742$ participants (approximately $n = 593$ people per condition; see Table 1 for demographics on the final sample).

7.1. Method and results

In order to conduct an internal (i.e., single-paper) meta-analysis, each dependent variable was regressed, in a multilevel model, onto orthogonal contrast codes that represented the 2 (parenthood status: parent, non-parent) × 2 (gender: woman, man) × 2 (race: Black, White) factorial structure of our experiments. Contrast codes themselves represented the fixed effects of parenthood status (coded as: *parents* = $\frac{1}{2}$ and *non-parents* = $-\frac{1}{2}$), employee gender (coded as *women* = $\frac{1}{2}$ and *men* = $-\frac{1}{2}$), and employee race (coded as *Black* = $\frac{1}{2}$ and *White* = $-\frac{1}{2}$), as well as their interactions with each other. This model included one random effect: a random effect of experiment intercept (Exp. 1 vs. Exp. 2 vs. Exp. 3 vs. Exp. S1), which statistically adjusted for the fact that each participant was nested within one of four different experimental samples that was collected at one of four different points in time. According to Monte Carlo simulations, our total sample size and analytic approach gave us at

⁹ Generally speaking, the conclusions of Exp. S1 were consistent with those of Experiments 1, 2, and 3. However, as mentioned previously, parenthood boost magnitudes were smaller in Exp. S1 than they were in Experiments 1, 2, and 3 (see the online supplement for a full report).

Table 9
Model Estimates from Internal Meta-Analysis of Experiments 1, 2, 3 and Study S1 (N = 4742).

Random Effects	Model 1: Employee Competence		Model 2: Employee Warmth			Model 3: Discrimination			
	Var.	SD	Var.	SD	Var.	SD			
Intercept	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01	0.03			
Residual	0.995	0.997	0.972	0.986	0.991	0.995			
Fixed Effects	β	SE	p	β	SE	p	β	SE	p
Intercept	0.00	0.01	.968	0.00	0.01	.974	0.00	0.01	.969
Parenthood	0.07	0.03	.016	0.28	0.03	< .001	-0.09	0.03	.002
Gender	0.05	0.03	.098	0.11	0.03	< .001	-0.06	0.03	.036
Race	0.06	0.03	.056	0.12	0.03	< .001	-0.11	0.03	< .001
P × G	-0.01	0.06	.904	-0.00	0.06	.995	0.03	0.06	.665
P × R	-0.13	0.06	.029	-0.17	0.06	.003	0.16	0.06	.006
G × R	-0.14	0.06	.017	-0.12	0.06	.043	0.14	0.06	.012
P × G × R	0.01	0.12	.935	0.06	0.11	.594	0.03	0.12	.778

Note. For each model, dependent variables were z-standardized and regressed onto contrast codes representing the fixed effects of parenthood status (coded as *parents* = ½ and *non-parents* = -½), employee gender (coded as *women* = ½ and *men* = -½), employee race (coded as *Black* = ½ and *White* = -½), as well as their interactions with each other. P = parenthood status; G = employee gender; R = employee race. All models included just one random effect: a random effect of intercept for the experiment within which data were nested.

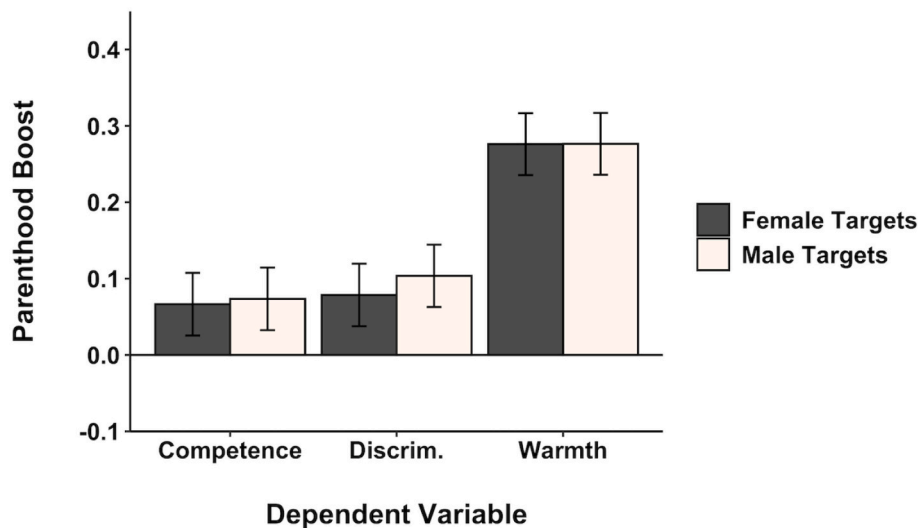


Fig. 2. (Meta-Analytic) Parenthood Boost Magnitudes as a Function of Targets' Gender Groups.

Note. The y-axis depicts the (z-standardized) magnitudes by which parents are evaluated more favorably than non-parents (i.e., parenthood boosts) across three dependent variables. Scores above zero indicate that parents are evaluated more positively than non-parents; scores below zero indicate the reverse. Competence = ratings of employee competence; Discrim. = ratings of discrimination in favor of employees (e.g., greater intentions to request, promote, and train employees); Warmth = ratings of employee warmth. Parenthood boosts for women employees are depicted in dark gray; parenthood boosts for men employees are depicted in light pink. Effect sizes are encompassed by standard errors.

least 80 % power to detect main effects as small as $\beta = 0.09$, two-way interactions as small as $\beta = 0.17$, and three-way interactions as small as $\beta = 0.33$. Thus, this internal meta-analysis indeed had the highest statistical power of any analysis that we have conducted so far (see Table 9 for all model estimates).

7.1.1. Do Parenthood Perceptions Depend on Employee Gender?

On the basis of our previous findings, we anticipated that meta-analytically, parenthood perceptions would *not* depend on employee gender. To test this prediction, each of our three dependent variables—perceived competence, perceived warmth, and endorsed discrimination—was subjected to the 2 (parenthood status) × 2 (gender) × 2 (race) factorial analysis described above. Consistent with the prior experiments, each of these analyses revealed no evidence of a two-way interaction between parenthood status and gender. There was no evidence of a parenthood status × gender interaction for perceived competence [$\beta < |0.01|$, $F(1, 4742) = 0.01$, $p = .904$], no evidence of a parenthood status × gender interaction for perceived warmth [$\beta < |$

$0.01|$, $F(1, 4742) < 0.01$, $p = .995$], and there was evidence of a parenthood status × gender interaction for endorsed discrimination [$\beta = 0.03$, $F(1, 4742) = 0.19$, $p = .665$]. This is to say that in this internal meta-analysis, as well as in Experiments 1, 2, and 3, women and men were not held to different standards when they were described as parents in the workplace (see Fig. 2).¹⁰

In Experiments 1 and 2, we found that employees who were described as parents (vs. employees who were not) tended to be evaluated more positively across all three dependent variables. In Experiment

¹⁰ Meta-analytically, we found not just that there was no evidence of a motherhood penalty, but likewise that estimates of the motherhood penalty (operationalized as parenthood status × gender interactions) were significantly closer to zero than they were to the least non-zero effects that we had 80 % power to observe. Thus, evidence in favor of the motherhood penalty was so null that it was practically equivalent to zero (see the online supplement for equivalence tests from every individual experiment as well as from the Internal Meta-Analysis).

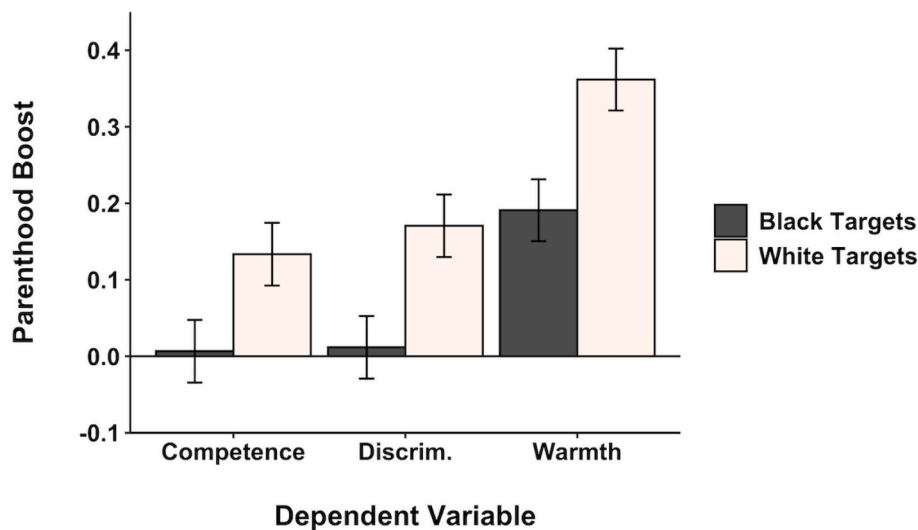


Fig. 3. (Meta-Analytic) Parenthood Boost Magnitudes as a Function of Targets' Racial Groups.

Note. The y-axis depicts the (z-standardized) meta-analytic magnitudes by which parents are evaluated more favorably than non-parents (i.e., parenthood boosts) across three dependent variables. Scores above zero indicate that parents are evaluated more positively than non-parents; scores below zero indicate the reverse. Competence = ratings of employee competence; Discrim. = ratings of discrimination in favor of employees (e.g., greater intentions to request, promote, and train employees); Warmth = ratings of employee warmth. Parenthood boosts for Black employees are depicted in dark gray; parenthood boosts for White employees are depicted in light pink. Effect sizes are encompassed by standard errors.

3 (as well as in Exp. S1; see the online supplement), we found more limited evidence of a “parenthood boost.” What did we find when all the available data from these experiments were meta-analyzed? Overall, we found continued evidence for the possibility that being described as a parent (vs. not being described as a parent) positively impacted evaluations of employees. Once again, parents were rated as warmer than non-parents [$\beta = 0.28$, 95 % CI[0.22, 0.33], $F(1, 4742) = 93.10$, $p < .001$]. In addition, parents were significantly less likely than non-parents to be targeted by discriminatory beliefs [$\beta = -0.09$, 95 % CI[-0.15, -0.03], $F(1, 4742) = 9.94$, $p = .002$]. Finally, parents were rated as more competent than non-parents [$\beta = 0.07$, 95 % CI[0.01, 0.13], $F(1, 4742) = 5.84$, $p = .016$]. Thus, meta-analytically, there indeed seems to be robust evidence of a parenthood boost in the eyes of perceivers.

7.1.2. Do Parenthood Perceptions Depend on Employee Race?

Although we never once saw evidence that parenthood differentially impacted evaluations of men vs. women, we occasionally did see evidence (in Experiment 3 and also in Exp. S1 of the online supplement), that parenthood differentially impacted evaluations of White employees vs. Black employees. That is, we found occasional evidence that White employees received stronger parenthood boosts than did Black employees. Did this phenomenon hold up to scrutiny in this internal meta-analysis? It appears so, yes. The $2 \times 2 \times 2$ factorial analysis described above revealed evidence of a parenthood status \times race interaction for perceived warmth [$\beta = -0.17$, $F(1, 4742) = 8.87$, $p = .003$], for endorsed discrimination [$\beta = 0.16$, $F(1, 4742) = 7.55$, $p = .006$], and for perceived competence [$\beta = -0.13$, $F(1, 4742) = 4.79$, $p = .029$]. In all three cases, parenthood boosts seemed to emerge more strongly for White employees than for Black employees. For example, whereas both White and Black employees who were described as parents were rated as warmer than those who were not, this effect was 89 % larger in magnitude for White employees [$\beta = 0.36$, 95 % CI[0.28, 0.44], $F(1, 4742) = 79.69$, $p < .001$] than it was for Black employees [$\beta = 0.19$, 95 % CI[0.11, 0.27], $F(1, 4742) = 22.25$, $p < .001$]. In addition, whereas learning that an employee was a parent *decreased* discrimination against White employees [$\beta = -0.17$, 95 % CI[-0.25, -0.09], $F(1, 4742) = 17.39$, $p < .001$], learning that an employee was a parent (vs. not) led to no detectable decrease in discrimination against Black employees [$\beta = -0.01$, 95 % CI[-0.09, 0.07], $F(1, 4742) = 0.08$, $p = .774$]. Finally,

whereas White employees gained a small (but significant) boost to their perceived competence when described as parents [$\beta = 0.13$, 95 % CI [0.05, 0.21], $F(1, 4742) = 10.60$, $p = .001$], Black employees did not [$\beta = 0.01$, 95 % CI[-0.07, 0.09], $F(1, 4742) = 0.03$, $p = .872$]. In other words, this internal meta-analysis provided evidence that across all the collected data, perceptions of parenthood indeed appeared to depend, at least to some degree, on target employees' racial groups. Being described as a parent in the workplace can cause an employee to seem more competent, more warm, and less deserving of discrimination than they would otherwise—but the magnitude of these effects seems to be larger and more consistent for White employees than for Black employees (see Fig. 3 for meta-analytic parenthood boosts broken down by employee race).

7.2. Discussion

None of our experiments revealed evidence of a motherhood penalty. However, all experiments revealed (at least some) evidence of a parenthood boost: a tendency to evaluate target employees more positively when they are described as parents relative to when they are not described as parents. Because the magnitude of parenthood boosts was somewhat variable from experiment to experiment—and also because some experiments (Exp. 3, Exp. S1) revealed evidence race-by-parenthood interactions whereas others (Exp. 1, Exp. 2) did not—we decided to conduct an internal meta-analysis to examine the robustness of all findings. The results of this internal meta-analysis (total $N = 4742$) suggested that overall, parenthood boost magnitudes are indeed robust and are not moderated by target employees' gender groups. However, parenthood boosts are occasionally moderated by target employees' racial groups. If anything, these boosts are bigger and more reliable for White parents than they are for Black parents—at least in the context of the paradigms studied here.

8. General discussion

This manuscript was designed to examine (1) whether women, more than men, continue to be subjected to an evaluative penalty at work when described (vs. not described) as parents, and if so, (2) whether this bias is moderated by whether the women in question are Black vs.

White. In the aggregate, the data reported in this manuscript revealed no evidence of a motherhood penalty—at least not in the context of the paradigms investigated here (i.e., those of Cuddy et al., 2004, and Heilman & Okimoto, 2008).¹¹ That is, there was no evidence that perceivers were more likely to question the competence of, or endorse greater discrimination against, women (vs. men) who are described as parents in the workplace. Instead, we found evidence of what appears to be a generalized *parenthood boost*: a tendency for perceivers to more positively evaluate employees who are described as parents than employees who are not described as parents. Moreover, although the magnitude of this boost was not moderated by targets employees' gender groups, it was moderated—at least occasionally—by target employees' racial groups. Meta-analytic estimates revealed that overall, White employees tended to receive a larger boost from being described as parents than did Black employees.

8.1. Theoretical implications of the present findings

The findings reported in this manuscript raise important questions: Why did we see no evidence of a motherhood penalty? And why did we instead see evidence of a parenthood boost that was, in at least two of our experiments, moderated by race? To be clear, such a pattern of results was not what we expected at the outset. Instead, we had anticipated that target employees' racial and gender identities would interactively shape the magnitude of the parenthood penalty they faced: that mothers would be penalized more than fathers, and that the magnitude by which this occurred would be moderated by these mothers' racial groups. Such a pattern would have been consistent with the larger research literature on intersectional stereotyping, which suggests that gender bias often manifests differently toward members of different racial groups (e.g., Ponce de Leon & Rosette, 2022; Rosette et al., 2018). So why, in the context of the present experiments, did target employees' parental statuses (or in some cases, target employees' parental statuses in tandem with their racial groups) take the perceptual foreground? And why did target employees' gender groups—at least with respect to how they influenced parenthood perceptions—seem to fade into the perceptual background?

Promising answers to these questions come from an emerging theoretical perspective called the *lens model* of intersectional stereotyping (Petsko et al., 2022). According to the lens model, perceivers have a repertoire of different lenses that they can use as bases for stereotyping social targets. Lenses, in this perspective, are construed as identity-specific schemas that can become situationally activated. A fundamental tenet of the lens model—which sets it apart from alternative models of intersectional stereotyping (e.g., Freeman, Stoller, & Brooks, 2020; Hall, Hall, Galinsky, & Phillips, 2019)—is that perceivers are expected to only use one lens at a time in a given social environment. The idea, for example, is that if perceivers are in a context that makes the lens of gender salient, perceivers will exhibit gender bias and gender stereotyping, but not any bias or stereotyping on the basis of targets' racial groups (Petsko & Vogler, 2023). Likewise, if perceivers are in a context that makes the lens of race salient, perceivers are expected to do the reverse: that is, exhibit racial bias and racial stereotyping, but not any bias or stereotyping on the basis of targets' gender groups (Petsko et al., 2022, Exp. 3a). Finally, if perceivers are in a context that makes an intersectional lens salient, perceivers are expected to exhibit intersectional biases and intersectional stereotypes, but not biases or stereotypes that are relevant to targets' broader, single-axis identities.

¹¹ In the online supplement, we investigated whether demographic differences between our participants and those of earlier experiments on the motherhood penalty might explain why we failed to find evidence of a motherhood penalty. In general, these analyses yielded no credible evidence that demographic differences across samples could explain why our findings diverge from those reported two decades ago (see the online supplement for more detail).

If target employees' parental statuses, or even target employees' parental statuses and racial groups in tandem, were the identities that were most situationally salient to perceivers in the present experiments, then the patterns of findings we observed here can be readily explained. That is, under these circumstances, it would make sense that perceivers would sharpen their focus on parenthood by itself (Exp. 1, Exp. 2)—or perhaps instead on the intersection of parenthood and race in tandem (Exp. 3, Exp. S1)—at the expense of focusing on target employees' gender groups. In other words, it would make sense for prejudices related to gender, like a motherhood penalty, to be “switched off” in these contexts, and for alternative prejudices, like pro-parent prejudices—or even pro-White-parent prejudices—to be contextually “switched on.” Thus, while these findings may have been counter to our expectations, they are nevertheless theoretically useful. Indeed, these findings add to a growing theoretical perspective suggesting that sometimes, perceivers may sharpen their focus certain dimensions of a target person's identity so strongly that they temporarily cease to focus on others (see Petsko & Bodenhausen, 2020, for a review).

Of course, the paragraph above raises a corollary question: Why might the lens of *parental status*, or in some cases the intersectional lens of *parental-status-by-race*, have been the lenses that were “switched on” in the present experiments? Although this question cannot be definitively answered on the basis of the present data, one theoretical possibility that warrants empirical attention has to do with the principle of distinctiveness (Petsko et al., 2022; Petsko & Bodenhausen, 2020). According to this principle, the more distinctive a lens-associated identity is in a given social environment, the more likely a perceiver is to use the lens associated with that identity over alternatives as a basis for stereotyping targets. The idea, for example, is that if one's parental status is more distinctive than, say, one's gender group in a given social environment, perceivers may be inclined to use the lens of parental status over the lens of gender for stereotyping targets.¹²

An identity can be perceived as distinctive in a particular context either because (1) an identity is stereotypically incongruent with that context, or because (2) an identity is statistically rare in that context. Relevant to the first of these issues is the fact that parenthood is typically perceived as being stereotypically incongruent with one's professional identity. For example, having aspirations of raising children is often regarded as being in conflict with one's professional ambitions (Mumford, Wilkinson, & Carroll, 2023), which can motivate white-collar employees to refrain from discussing issues of parenthood at work entirely (Gulbrandsen, 2022). Relevant to the second of these issues is the fact that in industries like consulting and finance—along with industries that require MBA degrees more generally—the underrepresentation of Black Americans tends to be starker than the underrepresentation of women (Hancock, Williams, Manyika, Yee, & Wong, 2021; Field, Krivkovich, Kügele, Robinson, & Yee, 2023; U.S. Government Accountability Office, 2023). Taking these observations together suggests that parenthood and race, respectively, may have been distinctive in the paradigms we employed for different reasons. That is, parenthood may have been distinctive because it is stereotypically incongruent with one's professional identity, whereas race (rather than gender) may have been distinctive because of the markedly low base rates of being Black American in the domains under study. In turn, the current findings set the stage for continued research that may explore how these different types of distinctiveness may interactively influence intersectional

¹² To be clear, the notion that perceivers' attention tends to be disproportionately grabbed by social information that is distinctive in a given social environment has been studied for decades (Hamilton & Gifford, 1976; McGuire, McGuire, Child, & Fujioka, 1978). However, this notion has been largely overlooked as a theoretical factor that may be relevant to the topic of intersectional stereotyping. As such, further examining the role of distinctiveness in shaping intersectional stereotyping provides a promising pathway to theoretical development.

stereotyping.

8.2. Limitations and future directions

A limitation of the present analysis is that although the lens-based account of intersectional stereotyping is useful for making sense of these findings, the present experiments do not test this account directly. A fruitful set of future experiments, therefore, might manipulate not just the race and gender of parents (vs. non-parents), but likewise whether the context itself activates the lens of gender versus the lens of race. Building on the theoretical principle of distinctiveness discussed above, one might activate the lens of gender by telling participants that the base rate of being a woman in an organization is exceedingly low relative to the base rate of being a man. By similar logic, one might activate the lens of race by telling participants that the base rate of being Black in an organization is exceedingly low relative to the base rate of being White. It might be the case that when a vignette activates the lens of gender (vs. race), we see a reversal of the demographic biases identified in our meta-analysis: evidence of a gender bias that interacts with parenthood (e.g., a motherhood penalty), and perhaps no evidence of a racial bias that interacts with parenthood (i.e., no evidence of a race-based parenthood boost). A set of experiments along these lines would be helpful for identifying to what extent and under what circumstances a motherhood penalty might emerge. Moreover, a set of experiments along these lines might help to underscore why in the present experiments, the lens of race was only occasionally used by participants. For example, if the present experiments were somewhat ambiguous in the extent to which they implied that it might be rare (and thus attention-grabbing) to be a Black employee at a given company, this might explain why race-by-gender intersections only occasionally influenced participants' evaluations.

A second limitation of the present analysis is that we cannot say for sure why the data reported here differ from those collected 20 years ago (Cuddy et al., 2004). Perhaps the relatively small sample sizes that were conventional 20 years ago inflated Type I error rates in the original empirical demonstrations of the motherhood penalty, which in turn allowed for inflated evidence in favor of the motherhood penalty. Alternatively, the data reported here may differ from those reported 20 years ago not because the data from 20 years ago were error-prone, but because men and women's social roles—and in turn, social attitudes toward men and women (Eagly & Wood, 2012)—have themselves changed (see Eagly et al., 2020, for a meta-analysis). Future research would benefit from more directly exploring what might explain the divergence between the data reported here and those reported in early research on the motherhood penalty. At present, it seems plausible that motherhood penalties may no longer hold in contexts in which women are now well-represented, but that they may nevertheless persist in contexts where women continue to be underrepresented.

A third limitation of the present analysis is that the data speak only to whether the motherhood penalty is observable in a very specific kind of paradigm—single-shot evaluative experiments (Correll et al., 2007; Cuddy et al., 2004; Fuegan et al., 2004; Heilman & Okimoto, 2008). This analysis does not speak to whether the motherhood penalty more broadly (e.g., Budig & England, 2001; Budig & Hodges, 2010; England et al., 2016; Glauber, 2008; Kricheli-Katz, 2012; Lundberg & Rose, 2000) has faded with time. For example, it may well be the case that single-shot decision-making scenarios that feature women who are (or are not) mothers fail to evince a motherhood penalty. But, at the same time, motherhood penalties may abound in other contexts. This is all to say that it would be a fallacy to presuppose, on the basis of the reported data, that motherhood penalties no longer exist. A much more fruitful way to think about the present data is that they raise questions about what the boundary conditions of motherhood penalties might be—under what circumstances they do emerge, and under what circumstances they do not.

A final limitation worth raising is that it is not clear what participants

assumed about target employees in the control conditions of the present experiments. Although we referred to these target employees as non-parents throughout this analysis, participants were technically given no information about these employees' parental statuses. Thus, it may be more accurate to say that these target employees had *unknown* parental statuses than it is to say that these employees were non-parents. In light of this issue, future research might consider investigating evaluations of target employees who choose to remain childfree (e.g., Ashburn-Nardo, 2017; Jamison et al., 1979; Koropecj-Cox et al., 2018) in addition to those of target employees who either are or are not described as parents. Such an investigation would help to identify what specifically may be driving the parenthood boost effects identified here: whether these boosts are driven by positive evaluations of parents, negative evaluations of those (presumed to be) childfree, or perhaps by both of these prejudices simultaneously.

9. Concluding remarks

Can merely learning that someone is a parent cause us to perceive them as more competent at their jobs, as more likable, and as more deserving of promotion? Consistent with recent reports on parenthood perceptions (Morgenroth et al., 2020; Zhang & Soderberg, 2023), the present data suggest that the answer may be “yes.” In the experiments reported here, parents were more likely than non-parents to receive a generalized boost, defined as more positive workplace evaluations in the eyes of those around them. Moreover, in contrast to prior experiments (Correll et al., 2007; Cuddy et al., 2004; Fuegan et al., 2004; Heilman & Okimoto, 2008), the experiments reported here revealed that evaluations of parents (vs. non-parents) did *not* depend on target employees' gender groups—but that they *did*, at least occasionally, depend on target employees' racial groups. Although these findings are perhaps surprising on their surface, they are nevertheless theoretically sensible. They add to a growing body of evidence suggesting that sometimes, perceivers' may sharpen their focus so strongly on certain dimensions of identity that they temporarily cease to focus on others. Impressions of parents may at times depend on gender (and not so much at race; Correll et al., 2007), but may as well at other times depend on race (and not so much on gender).

Open practices

This manuscript includes Open Data and Open Materials. Pre-registration documentation, data files, codebooks, survey materials, and R scripts associated with this article are available on OSF: <http://osf.io/h35gm/>.

CRedit authorship contribution statement

Christopher D. Petsko: Writing – original draft, Formal analysis, Conceptualization. **Rebecca Ponce de Leon:** Writing – review & editing, Conceptualization. **Ashleigh Shelby Rosette:** Writing – review & editing, Conceptualization.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix A. Supplementary data

Supplementary data to this article can be found online at <https://doi.org/10.1016/j.jesp.2025.104753>.

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