“Master” of None
Institutional Language Change Linked to Reduced Gender Bias

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Public Significance Statement

Do real-world changes to gendered language actually reduce gender biased thinking? In 2016, Yale University switched from using *master* (which emphasizes men, e.g., vs. *mistress*) to the more gender-inclusive term *head* for a leadership role, and we found that students exposed to *master* were more likely to think of and accurately remember men than women while students exposed to *head* showed no bias.

Abstract

Concern that masculine generic language (e.g., *man* to mean humanity) perpetuates gender inequity has led several institutions to formally discourage its use. While previous experimental research indicates that generic terms like *man* bring more exemplars of men than women to mind, only recently have researchers begun exploring additional consequences of gendered language. Understanding the range of processes affected is of particular importance when evaluating real-world policies. Yale University recently changed the title of a leadership role from *master* to *head*. The present study ($N=341$) investigated what exemplars come to mind (i.e., cognitive accessibility) while also probing memory for women and men in the leadership role both before and after Yale's language policy change. Students exposed to *master* generated a male exemplar more than would be expected by the incidence of men and recognized actual men than women in the role more accurately (d') in a face recognition task. Among students exposed to *head*, both biases were eliminated. The previous literature shows that masculine generic language brings men to mind. The present work demonstrates a similar effect but in an applied context while further documenting consequences for memory. Gender-inclusive language policies have potential to reduce gender biased thinking with applied significance.

Keywords: androcentrism, gender bias, language, memory, politically correct

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The use of terms such as *man* and *mankind* to refer to all genders (i.e., *masculine generic* terms) is common in many languages (Bodine, 1975; Hellinger & Bußmann, 2003). Concern that masculine generic language perpetuates gender inequity has led several institutions to formally discourage its use (e.g., APA, 2020; APA Publication Manual Task Force, 1978; GIGB, 2004). Although pedagogical policies and style guides targeting masculine generics indeed promote the use of alternative, more gender-inclusive language (Hegarty & Buechel, 2006; Prentice, 1994), few studies have empirically demonstrated whether such policies actually reduce gender biased thinking. Laboratory investigations show that masculine generics influence cognitive accessibility, bringing more exemplars of men than women to mind (for reviews see, Bigler & Leaper, 2015; Sczesny et al., 2016), but researchers have only recently begun to uncover more far-reaching consequences (e.g., Horvath & Sczesny, 2015; Stout & Dasgupta, 2011).

The present study followed the previous literature to investigate who comes to mind (i.e., cognitive accessibility), but in the applied context of an actual institutional policy change. This applied context further provided the conditions to test a novel cognitive consequence with theoretical and practical significance: accurate memory for women and men in a leadership role. Such cognitive gender biases shape how people perceive, make decisions about, and behave toward others, in ways that contribute to gender inequities (e.g., Banaji & Bhaskar, 2000; Moss-Racusin et al., 2012). The present study thus further investigated how language relates to bias in cognitive accessibility—but in an applied context—while also investigating memory in a face recognition task. Understanding the impact of actual language policy changes on these cognitive outcomes helps to establish the ecological validity of previous laboratory findings (i.e.,

concerning cognitive accessibility), tests the robustness of these effects in complex naturalistic circumstances, and offers a unique insight into the ways language, policy, and leadership perceptions relate to gender relations.

The specific term used for a concept can influence what information is most readily accessible (i.e., primed; Kahneman & Miller, 1986; Tulving & Pearlstone, 1966). A large body of research confirms that masculine generic language at least influences cognitive accessibility, temporarily bringing more men than women to mind (Bigler & Leaper, 2015; Sczesny et al., 2016; Silveira, 1980). For example, Schneider and Hacker (1973) found that U.S. students produced more images of men than women to represent textbook chapters with masculine generic titles (*Urban Man*) compared to gender-inclusive alternatives (*Urban Life*). German-speaking participants similarly list more exemplars of men for a variety of social groups when reading masculine labels (e.g., *heroes*) compared to more gender-inclusive labels (e.g., *heroine or hero*; Stahlberg et al., 2001). Related, reading about *heroes* caused German-speaking participants to estimate that men carry out a larger proportion of heroic acts compared with reading the same text with the gender-inclusive phrase *heroes and heroines* (Hansen et al., 2016). Indeed across several cultural and linguistic contexts, masculine generic language appears to prime men and causes children and adults to be more likely draw, pick, and imagine men than women compared to more gender-inclusive language (Bailey & LaFrance, 2016b; Bojarska, 2011; Gastil, 1990; Hyde, 1984; Lindqvist et al., 2019; Miller & James, 2009; Moulton et al., 1978; Switzer, 1990; Wilson & Ng, 1988; for an exception in Chinese, see, Su et al., 2016).

Further, there is also evidence that people think of masculine generic terms as male¹ words. Masculine generics like *mankind* are implicitly associated with male words more than female words (Bailey et al., 2020; Banaji & Hardin, 1996). Further, people encode masculine generics as belonging to the male category. This has been shown via proactive inhibition, which is the phenomenon whereby the final word in a list of words is recalled better if it belongs to a different category than those that preceded it (Mills & Tyrell, 1983; Wickens, 1970). Ng (1990) used this phenomenon to test the encoding of masculine generic terms *man* and *his* among adolescents. Across a series of trials, participants read words, read a string of letters (to prevent rehearsal of the words), and were then prompted to recall the initial words. In Ng's application of the proactive inhibition task to gendered language, participants did better at recalling *man* and *his* when they followed a series of female words (e.g., *queen* and *girl*) than male words (e.g., *king* and *boy*), which is consistent with masculine generics being encoded as male words. Thus, despite claims that masculine generic language is gender-inclusive (e.g., Strunk & White, 1979), masculine generics cause people to think of men more readily than women and are also encoded as male words.

Masculine generics may have more far-reaching consequences as well, namely for memory of men and women. Masculine generic terms for social roles bring men to mind (Hansen et al., 2016), activating stereotypic expectations that men fill those roles. Such expectations, or *schemas*, make it easier for people to process and later remember congruent information (Greve et al., 2019; Kan et al., 2001; Koriath et al., 2000; Payne et al., 2004; Tse et al., 2007; van Kesteren et al., 2012). Although information that is *incongruent* with expectations may sometimes be recalled and recognized better than congruent information (Stangor &

¹ Following recommendations by the American Psychological Association, we use the terms *male* and *female* only as adjectives to modify nouns and the terms *women* and *men* as nouns (2020). Both sets of terms refer to gender concepts (i.e., rather than distinguishing between biological sex and gender).

MacMillan, 1982; typically when the discrepancy is particularly salient, as in the proactive inhibition paradigm used by Ng [1990] described earlier), in face recognition tasks, such as the one we employed in the present research, people consistently have better recognition accuracy for information *congruent* with expectations, including those based on stereotypes (e.g., Cassidy & Gutchess, 2015; Cassidy et al., 2012; Klatzky et al., 1982; see also, Eberhardt et al., 2003; Nash et al., 2010).

One such prior study utilized procedures very similar to the present research. Hills and colleagues (2008) had participants view a series of faces that were pre-rated as being stereotypic of artists but that were either labeled congruently as artists or given some other incongruent category label (e.g., criminals) during the learning phase. Later, participants completed a recognition memory test that required them to correctly remember which faces belonged to which category. While accounting for tendencies to guess one way or the other, participants accurately recognized faces that were congruent with stereotypic expectations during learning (i.e., artists that looked like artists) better than incongruent faces (i.e., criminals that looked like artists). In the same way, masculine generic language activates male stereotypic expectations (e.g., Hansen et al., 2016) that likely also facilitate more accurate recognition of men than women.

Taken together, this body of work on schema and related theories about the influence of stereotypic expectations suggest that masculine generic language likely influences memory, as measured in the present research in terms of accuracy in a face recognition test. Thus, in addition to masculine generics temporarily making men more cognitively accessible, these terms might also bias more enduring memory processes relating to men and women. Masculine generics activate male stereotypic expectations, which likely makes it easier to process men than women

during learning, scaffolding more and better memories of men than women. Such a memory bias would result in men later being recognized more accurately than women for social roles designated by masculine generic language.

To the extent that memory processes shape people's broader social expectancies, associating men with particular—and traditionally more powerful—social roles, memories that favor men can form the foundations of disadvantages for women relative to men (Banaji & Bhaskar, 2000; Eagly & Karau, 2002). Further, remembering men better than women, including recognizing men more accurately than women in a leadership position, could create disadvantages for women (e.g., disproportionately crediting men's contributions; Lincoln et al., 2012; Sesko & Biernat, 2010), even in domains where women are well represented. Thus, investigating the influence of masculine generics on memory has applied implications and is theoretically important to understand the range of cognitive processes affected by gendered language.

Understanding the range of processes affected is particularly important when evaluating real-world policies. Several institutions discourage the use of masculine generics. Publishers and government entities alike recommend (or require) the use of gender-inclusive language forms (e.g., APA, 2020; APA Publication Manual Task Force, 1978; GIGB, 2004; Psychology of Women Quarterly, 2019). Such language reform policies can be controversial (e.g., Dalén, 2012; Strunk & White, 1979), and women tend to support them more than men (Parks & Robertson, 2004). These policies appear to be effective in reducing the use of masculine generics (Prentice, 1994). For instance, when the American Psychological Association recommended that authors avoid masculine generic language, the use of masculine generics indeed subsequently declined (Gannon et al., 1992). Hegarty and Buechel (2006) analyzed the content of published

psychological articles between 1965 and 2004 and found decreased use of the masculine generic pronoun *he* in later decades. Yet, few investigations have addressed the broader consequences of actual institutional changes in gendered language for gender-biased thinking, including for cognitive accessibility (which has mostly been studied in laboratory environments) and memory of women and men in important social roles.

As reviewed by Sczesny and colleagues (2016), much of the research on masculine generic language uses laboratory-based experimental paradigms. These paradigms provide compelling causal evidence that masculine generics make men cognitively accessible (Sczesny et al., 2016). Yet controlled experimental paradigms can be somewhat contrived and limited in their ability to generalize to real-world contexts. Sczesny and colleagues (2016) emphasized the importance of future work investigating gendered language, “in everyday life...in organizational communication...[and] consistently over a longer period” (p. 8). Using such a complementary methodological approach, the present study capitalized on a recent naturalistic change in institutional language policy. This applied context spotlights practical importance and allowed us to test implications for memories that develop from naturalistic experiences over time.

The Present Study

The present study examined potential cognitive consequences of a change from masculine generic language to more gender-inclusive language. At Yale University students are assigned to residential communities led by a prestigious live-in faculty member. These communities constitute a core aspect of undergraduate students’ experience because they are designed to simulate a smaller, more intimate college experience at a large research-intensive university. Tenured faculty members are appointed to lead these communities by living on site

and cultivating the social and intellectual culture. Until 2016, these faculty members were called *masters*. In 2016, the institution formally renamed this leadership position to become *head*.

The Oxford English Dictionary defines *master* as “a person (predominately, a man) having authority” (2001). The term *master* can sometimes refer specifically to a male person while the woman-specific counterpart is *mistress*. *Master* thus constitutes a masculine generic. In contrast, whereas *head* can also refer to leadership and may have masculine connotations, it does not have a woman-specific counterpart and is not defined as being man-specific in the same way as *master*; instead, *head* is defined as “a person to whom others are subordinate” (Oxford English Dictionary, 2013). Thus, the change from *master* to *head* provides the conditions to compare masculine generic language to more gender-inclusive language in a naturalistic quasi-experiment. As suggested by Dinardo (2008, para. 1), to establish the robustness and generalizability of effects it is particularly valuable to take advantage of naturalistic serendipitous conditions in situations that “‘approximate’ randomized design.” In the present study, students participated in either the *master* or *head* condition according to their year of matriculation. While not resulting from random assignment, the two samples were closely matched: the applicant pools to the institution were academically and demographically comparable, there were no systematic changes in admissions criteria used by the institution, and the two samples were tested relatively close in time. Studies of this type cannot definitively establish causal relationships and are not true experiments. Instead they can offer complementary evidence for the ecological validity of related laboratory-based findings in a naturalistic context that further obscures study aims and points to applied significance (e.g., Parker et al., 2006; Hershfield & Alter, 2019; see also, Campbell & Stanley, 1963; Rothstein & von Wachter, 2017).

We specifically assessed among cohorts of students before and after Yale initiated the gendered language change, which gender comes to mind (i.e., cognitive accessibility) as a typical exemplar of the leadership role and accurate memory for actual faculty members occupying the leadership role in a face recognition memory test. Cognitive accessibility and memory, while sometimes related, reflect distinct cognitive processes.

With respect to our measure of accessibility, we hypothesized that bias favoring men over women would be lower among students exposed to the title *head* than to *master*, particularly among female students who tend to be most supportive of gender language reforms (H1a; Parks & Robertson, 2004). We based this hypothesis on the laboratory-based experiments showing less bias in accessibility favoring men over women in response to more gender-inclusive language compared to masculine generic language (e.g., Sczesney et al., 2016)

In terms of our face recognition memory test, we based our hypothesis on research showing the effects of expectation-congruent information on memory (e.g., Payne et al., 2004), including specifically memory in face recognition tasks (Hills et al., 2008; Klatzky et al., 1982). Because *master* is a masculine generic (Oxford English Dictionary, 2001), we expected it to create more male-stereotypic expectations than *head* and thus facilitate relatively more accurate face recognition memory for men than women. Thus, we hypothesized that *master* would facilitate more accurate memory of men than women, which would be reduced under *head*, again particularly among female students who tend to be most supportive of gender language reforms (H1b; Parks & Robertson, 2004).

Finally, because all students in our research belonged to a residential community unit that was led by a specific master/head, we also anticipated that students' own master/head's gender may make a difference. We expected that most students knew their own master/head before

participating in our study (although there would likely be greater variation in their knowledge of *other* residential communities' master/heads). Students' own master/head's gender likely influences their conception of the role (Beaman et al., 2009), and thus students with a female master/head likely have less male-stereotypic expectations about the role than students with a male master/head. As such, we predicted there to be less bias favoring men among students whose own master/head was a woman compared to a man across time points in both (a) accessibility (H2a) and (b) memory (H2b).

Method

Study hypotheses, design, analyses and the plan for stopping data collection were preregistered before collecting data for the *head* condition (<http://aspredicted.org/blind.php?x=j64ui2>). All exclusions, manipulations, and measures are reported, and complete copies of the survey materials are available (https://osf.io/s5xw8/?view_only=6848be0f4c1b4fe095341d75f820c455). The institutional review board reviewed and approved this study.

Participants

Yale University undergraduate students completed the study described as involving “perception and decision-making” in exchange for course credit using an online platform; this is the population most directly served by masters/heads. A priori, we decided to collect data for one semester and from at least 100 participants at each time point to ensure that we could detect small to medium effects for the imagine exemplar accessibility measure ($\Phi = 0.20$, $\alpha = .05$, $1-\beta = 0.80$) and the memory measure ($\eta_p^2 = 0.01$, $\alpha = .05$, $1-\beta = 0.80$, $r = 0.50$; Cohen, 1992; Faul et al., 2007). After the sample size for the first time point was collected ($n = 121$), we updated our stopping rule as preregistered for the second time point to be the end of the semester with the

qualification that and we collected data from at least 121 participants (i.e., to at least match the sample size in the first time point).

This sampling approach resulted in a total of 341 participants. Specifically, in 2015 in the *master* condition 121 students participated who self-identified as women ($n = 66$), men ($n = 54$), and gender non-binary ($n = 1$), with a mean age of 19 ($SD_{age} = 0.96$, age range = 17-22) and the majority self-identified as White ($n = 65$; Asian: $n = 22$; African American or Black: $n = 19$; Hispanic or Latino: $n = 12$; multi-racial: $n = 7$; Middle Eastern: $n = 2$). In 2018 in the *head* condition, 220 students participated who self-identified as women ($n = 135$), men ($n = 81$), and gender non-binary ($n = 4$), with a mean age of 19 yr ($SD_{age} = 1.11$, age range = 18-24) and the majority self-identified as White ($n = 99$; Asian: $n = 67$; Hispanic or Latino: $n = 23$; African American or Black: $n = 19$; multi-racial: $n = 8$; Native American: $n = 2$; Middle Eastern: $n = 2$). To provide more detail about the key demographic variable, gender, students were asked, “What is your gender?” and responded by selecting either “man,” “woman,” “non-binary” (which included the option to write-in), or “prefer not to say.” Note that we waited three years between the two data collections to allow the terminology change to take hold.

Sensitivity analyses revealed that our observed sample could detect a small to medium effect size ($\Phi = 0.15$, $\alpha = .05$, $1-\beta = 0.80$) concerning the effect of title condition on accessibility (which gender comes to mind) and a small effect size ($\eta_p^2 = 0.01$, $\alpha = .05$, $1-\beta = 0.80$, $r = 0.50$) concerning memory (Faul et al., 2007). Note that student gender analyses only compared male and female students due to the small percentage of gender non-binary students (1%).

Materials and Procedure

Students were asked to imagine a typical master/head and to complete a memory test. These measures allowed us to test the hypothesis that the change from masculine generic

language to relatively more gender-inclusive language would correspond with reduced gender bias favoring men in cognitive accessibility and memory, particularly among female students (H1; Parks & Robertson, 2002). Students then completed three ideological measures designed to assess potential alternative explanations besides the language change. Finally, students provided demographic information, including their residential community affiliation, which allowed us to surreptitiously record the gender of students' actual master/head (related to H2).

Note that the survey included procedures that are not relevant to the present study. Specifically, at the very beginning of the survey, students first read a paragraph that either emphasized the Yale's stability or potential for change. However, because the manipulation check in 2015 indicated this article did not shift perceptions, it was included in 2018 only for consistency. In 2018 only, at the very end of the survey students also indicated their leadership aspirations and their perceived similarity to their real and imagined *head*. As is reflected in our preregistration, the paragraph manipulation and these latter measures did not bear on the current study and thus are not discussed further.

Accessibility: Imagined Exemplar

In prior experiments, masculine generics have been shown to bias who comes to mind (i.e., cognitive accessibility), such that participants were more likely to list a man when prompted to list an exemplar compared to participants exposed to more gender-inclusive language (Sczesny et al., 2016). Thus, we first asked students to think of a typical college *master* (in 2015) or *head* (in 2018) and to give that person a name. Students then indicated their imagined master/head's demographic characteristics, including gender (as in, Bailey & LaFrance, 2016a; Hamilton, 1991). In prior work, the gender indicated by participants was independently verified in nearly all cases based on the specified name (97%, Bailey & LaFrance, 2016a). Students could

also indicate that their imagined master/head was neither male nor female ($n = 0$ in 2015; $n = 4$ in 2018) or that the student did not consider gender ($n = 10$ in 2015; $n = 16$ in 2018). As preregistered, the students who gave either of these latter responses were excluded from this analysis. This measure thus provided the opportunity to test an approximate replication of prior laboratory-based experiments showing that masculine generic language biases cognitive accessibility, but in the context of an actual institutional language change.

Students were also asked to rate their imagined master/head on 30 traits on nine-point scales from 1 (*very uncharacteristic*) to 9 (*very characteristic*). As is reflected in our preregistration, these traits were included only for descriptive purposes, to provide more information about the person who came to mind (See *SI*, Section 1 for exploratory analyses) and to encourage students to imagine that person in detail. The traits were stereotypically feminine and positive (or prescribed, e.g., *friendly*), masculine and positive (e.g., *self-reliant*), feminine and negative (or proscribed, e.g., *emotional*), and masculine and negative (e.g., *arrogant*; Prentice & Carranza, 2002).

Memory: Accuracy of Face Recognition

Next, students completed a memory test that we designed to investigate the possibility that masculine generics would influence memory, resulting in more accurate face recognition memory of men than women in the leadership role. For each trial, students saw an image in the center of the screen and indicated whether or not the image was of a current *master* (in 2015) or a current *head* (in 2018) on a binary scale. Images were headshots of actual masters/heads and an equal number of non-master/head Yale faculty matched in terms of gender and race. This resulted in 24 trials in 2015 and 28 trials in 2018 (Yale added two positions in the interim). Headshots were taken from each faculty's online profile, gray-scaled, and cropped around the

face to the same size. Thus this task explicitly made both men and women accessible as potential master/heads while testing whether students could accurately remember which faculty filled the leadership role. The study itself did not expose students to information about who was or was not a master/head. Thus, the “learning phase” for this memory test is posited to have occurred in students’ experience on campus as part of their, “everyday life...in organizational communication...[and] consistently over a longer period” as recommended by Sczesny and colleagues (2016, p. 8).

When completing memory tests of this kind, people are guided both by their actual memory of who is and who is not in the role *and* by their desire to avoid making certain types of errors (Fraundorf et al., 2019; Lynn & Feldman Barrett, 2014; Wixted & Mickes, 2010). For instance, students might label a given woman as a master because they recognize her as such or, in contrast, because students are inclined to guess that any woman is a master. Signal Detection Theory (SDT) is a useful analytic tool that distinguishes these two possibilities by separately estimating students’ memory (d' , influenced by recognition and/or felt familiarity; Wixted & Mickes, 2010) and their general inclination to give any particular response (c , Green & Swets, 1966; Stanislaw & Todorov, 1999). Critically, because the study did not provide any information about who was and was not a master/head, students’ memory (d') reflected information learned during the “learning phase” as part of students’ prior experiences on campus.

To apply SDT, we first calculated the proportion of trials where master/heads were correctly labeled (i.e., hits), and the proportion of trials where other faculty were incorrectly labeled as master/heads (i.e., false alarms). A higher proportion of hits than false alarms is indicative of better memory, measured by d' (See *SI*, Section 2.1 for formulas). Higher d' values indicate better ability to accurately distinguish master/heads from non-master/head faculty (i.e.,

hits > false alarms). Lower d' values indicate that students either labeled most or none of the headshots as master/heads, in either case demonstrating poor memory (i.e., hits $\approx$ false alarms). By contrast, the c parameter represents students' general tendency to indicate master/head regardless of accuracy, and is the average proportion of hits and false alarms. Larger c values indicate a greater tendency to guess master/head, perhaps to avoid making certain type of errors (Fraundorf et al., 2019). As preregistered, we calculated d' and c for female and male trials separately. Note that the d' findings reported here replicate with the nonparametric alternative A' (See *SI*, Section 2.2 for formulas and analyses).

Ideological Measures

Students then completed three ideological measures to allow us to test potential alternative explanations for any accessibility and memory effects. For each measure, students indicated their agreement with statements using nine-point scales from 1 (*strongly disagree*) to 9 (*strongly agree*). Any observed changes from 2015 to 2018 might be due to incidental shifts in these related social ideologies between cohorts rather than specifically due to the language change. First, because the title change from *master* to *head* constituted a change to the status quo, we measured students' commitment to the status quo using the eight-item General System Justification Scale (GSJ; Kay & Jost, 2003; e.g., "Society is set up so that people usually get what they deserve"; $\alpha = 0.75$). If students in 2015, for instance, were more committed to the status quo than students in 2018, then a reduction in gender bias favoring men from 2015 to 2018 could be due to this lessening commitment to status quo gender hierarchies that connect men with leadership roles more than women (Koenig et al., 2011) rather than due to the language change. Second, because the label change also had the potential to highlight gender-based social inequalities, we collected students' support for social inequality with the sixteen-item Social

Dominance Orientation Scale (SDO; Sidanius & Pratto, 2001; e.g., “Inferior groups should stay in their place”; $\alpha = 0.89$), and an eight-item version of the Authoritarianism Scale (RWA; Altemeyer, 2008; e.g., “The only way our country can get through the crisis ahead is to get back to our traditional values, put some tough leaders in power, and silence the troublemakers spreading bad ideas”; $\alpha = 0.74$). Both measures relate to support for traditional gender hierarchies (Sibley et al., 2007), and thus a change in these ideologies across cohorts rather than the language change might explain any reduction in gender bias favoring men.

Results

Over the course of the study, the master/head position in the Yale residential colleges was roughly gender-balanced. In 2015, there was an equal percentage of women and men (50%), and in 2018 there were slightly more men (57%; Figure 1a). Further, students rated their imagined master/head relatively high in both positive feminine ($M = 6.80$, $SD = 1.86$) and positive masculine traits ($M = 6.68$, $SD = 1.77$) and low in negative feminine ($M = 4.39$, $SD = 2.05$) and negative masculine traits ($M = 3.16$, $SD = 1.86$; See *SI*, Section 1 for exploratory analyses²), which suggested that students might plausibly display no gender bias and imagine male and female master/heads at comparable rates and accurately remember male and female masters/heads comparably in the face recognition test.

Accessibility: Imagined Exemplar

To assess potential gender bias in which gender came to mind (i.e., cognitive accessibility), chi-square goodness of fit tests were employed with the actual gender proportions as the comparison point. Students were more likely to imagine a man than expected by the actual proportion of men (0.63 vs. 0.54), $\chi^2(1, n = 311) = 9.80, p = .002, \Phi = 0.18$. We further analyzed

² There was no evidence that students’ conceptions of their imagined master/head’s femininity and masculinity varied by title condition, but overall, students rated their imagined master/head higher in feminine than masculine traits. See *SI*, Section 1 for details for these exploratory analyses.

differences in this gender bias due to the title change from *master* to *head*, student gender, and students' actual master/head's gender using preregistered logistic regressions with odds ratio serving as an estimate of effect size.

We expected students exposed to the *head* title (vs. *master*) to show less bias favoring men, particularly female students (H1a). To test this possibility we first ran a preregistered logistic regression with title condition, student gender, and their interaction. However, there was little evidence that *head* uniquely affected female students. The interaction term between title condition and student gender did not reach statistical significance, $B = 0.44$, $SE = 0.56$, $p = .433$, $OR = 1.55$, and simple slopes analyses indicated that although the effect of title condition was only significant for female students, $B = -1.13$, $SE = 0.34$, $p < .001$, $OR = 0.32$, it was in the same direction for male students, $B = -0.68$, $SE = 0.45$, $p = .127$, $OR = 0.51$ (see *SI*, Section 3.1 for additional student gender analyses).

The data thus lent themselves to the interpretation that *master* versus *head* had an overall effect. In a trimmed model removing student gender, more students imagined a male exemplar in the *master* condition (0.77) compared with the *head* condition (0.56), $B = -0.98$, $SE = 0.27$, $p < .001$, $OR = 0.37$. Further, in follow-up chi-square tests, more students imagined a male exemplar in the *master* condition compared with the actual proportion of men in 2015 (0.77 vs. 0.50) $\chi^2(1, n = 111) = 31.36$, $p < .001$, $\Phi = 0.53$, but in the *head* condition, there was no evidence that the proportion of imagined men differed from the actual proportion of men in 2018 (0.56 vs. 0.57) at conventional significance levels, $\chi^2(1, n = 200) = 0.37$, $p = .541$, $\Phi = 0.04$ (Figure 1b). Note that student gender had an independent effect; in a model with student gender as the sole predictor female students (0.57) were less likely than male students (0.73) to imagine a man across title conditions, $B = 0.70$, $SE = 0.25$, $p = .006$, $OR = 2.01$. But the effect of title condition did not

appear to be unique to female students and was robust to controlling for a variety of factors including students' gender and the gender of their actual master/head (see *SI*, Section 4.1 for analyses).

Figure 1

The Actual Proportion of Masters/Heads (a) Alongside Gender Bias when Prompted to Generate a Typical Master/Head (b) and Remember Actual Master/Heads (c)

Note. Error bars represent 95% CIs with the shared variance removed (Cousineau, 2005).

Significant differences are indicated in (b) from the actual proportion of men and in (c) between headshot gender conditions, *** $p < .001$, * $p < .05$, ns $p > .05$.

We also expected that the gender of students' actual master/head would matter, specifically that having a female (vs. male) master/head would correspond with less gender bias favoring men (H2a). To test this hypothesis, we ran a preregistered logistic regression with actual

master/head as the sole predictor. Students led by a female master/head were less likely to imagine a man (0.48) than students led by a male master/head (0.72), $B = 1.02$, $SE = 0.24$, $p < .001$, $OR = 2.76$, $\Phi = 0.23$. Further in follow-up chi-square tests, students led by a male master/head imagined a man more than expected by the actual proportion of men (i.e., 0.54), $\chi^2(1, n = 191) = 24.56$, $p < .001$, $\Phi = 0.36$, but there was no evidence that students led by a female master/head differed from the actual proportion of men (i.e., 0.54), $\chi^2(1, n = 119) = 1.70$, $p = .193$, $\Phi = 0.12$.

Memory: Accuracy of Face Recognition

In addition to assessing cognitive accessibility, the present study tested students' memory for actual male and female faculty in the leadership role in a face recognition task. Memory was represented by d' , which captures students' ability to accurately recognize masters/heads and non-master/head faculty (influenced by recognition memory and/or felt familiarity; Wixted & Mickes, 2010). Responses on the memory test reflect both memory, d' , and students' overall willingness to label headshots as master/heads, c , regardless of accuracy. This latter guessing bias parameter can reflect a desire to avoid making certain types of errors (Fraundorf et al., 2019). As preregistered, d' and c were both submitted to separate 2 (title condition: *master*, *head*) $\times$ 2 (actual master/head gender: men, women) $\times$ 2 (student gender: women, men) $\times$ 2 (headshot gender: men, women) analysis of variances (ANOVA) with headshot gender as a repeated measures factor.

For the measure of memory, there was a main effect of headshot gender: overall students displayed better memory, represented by greater accuracy in the face recognition task (d'), for men ($M = 0.68$, $SD = 0.68$) than women ($M = 0.56$, $SD = 0.75$), $F(1,327) = 4.01$, $p = .046$, $\eta_p^2 = 0.01$. This main effect was qualified by an interaction between headshot gender and title

condition, $F(1,327) = 4.50, p = .035, \eta_p^2 = 0.01$. Men were remembered better than women under the *master* title ($M_{man} = 0.83, SD_{man} = 0.74$, vs. $M_{woman} = 0.55, SD_{female} = 0.84$), $F(1,327) = 6.71, p = .020, \eta_p^2 = 0.02$, but there was no evidence for memory gender bias under the *head* title at conventional significance levels ($M_{man} = 0.60, SD_{man} = 0.64$, vs. $M_{woman} = 0.56, SD_{woman} = 0.71$), $F(1,327) = 0.01, p = .920, \eta_p^2 < 0.01$ (Figure 1c). We hypothesized that female students would be particularly influenced by the change from *master* to *head* (H1b). However, there was again no evidence for a qualifying effect of student gender: the interaction among headshot gender, title condition, and student gender was non-significant at conventional levels, $F(1,327) = 1.10, p = .296, \eta_p^2 < 0.01$ (see *SI*, Section 3.2 for additional student gender analyses). The data thus again indicated that *master* versus *head* had an overarching effect on gender bias.

We expected that having an actual master/head who was a woman (vs. a man) would also lower any male memory advantage (H2b). Consistent with this hypothesis, there was an interaction between actual master/head gender and headshot gender, $F(1,327) = 12.74, p < .001, \eta_p^2 = 0.04$. Men were remembered better than women among students led by an actual male master/head ($M_{man} = 0.75, SD_{man} = 0.70$, vs. $M_{woman} = 0.49, SD_{woman} = 0.77$), $F(1,327) = 19.90, p < .001, \eta_p^2 = 0.06$, but there was no evidence for a memory gender bias among students led by a female master/head ($M_{man} = 0.57, SD_{man} = 0.64$, vs. $M_{woman} = 0.67, SD_{woman} = 0.72$), $F(1,327) = 1.01, p = .316, \eta_p^2 < 0.01$. Note that we also found an interaction between title condition and student gender, $F(1,327) = 4.78, p = .030, \eta_p^2 = 0.02$. However, because this interaction did not involve the headshot gender condition it cannot inform the key issue in the present research: potential gender bias in memory based on men compared to women. All other main and interactive effects were non-significant at conventional levels and all η_p^2 's < 0.01 (title condition: $F(1,327) = 2.65, p = .105$, actual master/head gender: $F(1,327) = 0.03, p = .866$, student gender:

$F(1,327) = 1.51, p = .219$, title condition x actual master/head gender: $F(1,327) = 0.56, p = .454$, actual master/head gender x student gender: $F(1,327) = 0.10, p = .750$, headshot gender x student gender: $F(1,327) = 0.02, p = .876$, title condition x actual master/head gender x student gender, $F(1,327) = 1.53, p = .217$, headshot gender x title condition x actual master/head gender: $F(1,327) = 0.19, p = .659$, headshot gender x actual master/head gender x student gender: $F(1,327) = 1.26, p = .262$, headshot gender x title condition x actual master/head gender x student gender: $F(1,327) = 0.01, p = .933$).

For students' guessing bias (c), there was a main effect of headshot gender, indicating that regardless of accuracy students were more willing to guess that women were master/heads ($M = -0.07, SD = 0.43$) than men ($M = -0.41, SD = 0.40$), $F(1,327) = 120.84, p < .001, \eta_p^2 = 0.27$. Students showed this tendency in both the *master* title condition ($M_{woman} = -0.12, SD_{woman} = 0.41$ vs. $M_{man} = -0.28, SD_{man} = 0.37$), $F(1,327) = 16.09, p < .001, \eta_p^2 = 0.05$, and the *head* title condition ($M_{woman} = -0.04, SD_{woman} = 0.44$ vs. $M_{man} = -0.48, SD_{man} = 0.41$), $F(1,327) = 165.97, p < .001, \eta_p^2 = 0.34$, although an interaction indicated the effect was larger in the *head* condition, $F(1,327) = 21.22, p < .001, \eta_p^2 = 0.06$. All other main and interaction effects did not reach statistical significance at conventional levels and all η_p^2 's < 0.01 (title condition: $F(1,327) = 2.34, p = .127$, actual master/head gender: $F(1,327) = 3.05, p = .082$, student gender: $F(1,327) = 0.66, p = .419$, title condition x actual master/head gender: $F(1,327) = 0.06, p = .810$, title condition x student gender: $F(1,327) = 0.01, p = .907$, actual master/head gender x student gender: $F(1,327) = 0.12, p = .731$, headshot gender x student gender: $F(1,327) = 0.01, p = .907$, headshot gender x actual master/head gender: $F(1,327) = 3.16, p = .076$, title condition x actual master/head gender x student gender, $F(1,327) < 0.01, p = .964$, headshot gender x title condition x actual master/head gender: $F(1,327) = 1.21, p = .273$, headshot gender x title condition x

student gender: $F(1,327) = 0.18, p = .675$, headshot gender x student gender x actual master/head gender: $F(1,327) = 0.73, p = .394$, headshot gender x student gender x actual master/head gender x title condition: $F(1,327) = 0.05, p = .819$).

Together the results from the face recognition memory test indicate that students' were more willing to guess that women than men are master/heads, perhaps to avoid making certain kinds of seemingly "sexist" errors. However, students actually remembered the men better than the women in the *master* condition, evidenced by a greater ability to accurately distinguish male than female masters from non-masters (d'). In the *head* condition, this male memory advantage was eliminated.

Exploratory Analyses

The reduction in gender bias favoring men over women in who comes to mind (i.e., cognitive accessibility) and who is remembered among students exposed to *head* (vs. *master*) could be due to a variety of factors besides the title change. Accordingly, we explored whether changes between time points in students' demographics, students' ideologies, or the proportion of students' with a male (vs. female) master/head might account for these results.

Concerning cognitive accessibility, the likelihood of imagining a man increased among students who scored higher in SDO, $B = 0.28, SE = 0.11, p = .011, OR = 1.10$, and separately, who scored higher in RWA, $B = 0.23, SE = 0.12, p = .048, OR = 1.05$. Crucially, the effect of title was robust to controlling for SDO and RWA in separate logistic regressions, and further, robust to each of the other ideological and demographic factors measured—GSJ, political stance on social and economic issues, gender, race, age, class-year, English as a second language status, and first-generation college student status. The proportion of students with a male master/head happened to be higher than those with a female master/head at both time points (60% under

master and 62% under *head*). But notably, this proportion of students was similar under *master* (60%) and *head* (62%), and crucially the effect of title condition on accessibility was robust to controlling for students' actual master/head's gender (see *SI*, Section 4.1 for analyses). The effect of the title change on students' gender biased memory of actual masters/heads (d') was similarly robust to students' demographics, students' ideologies, and students' own master/head's gender. Note that adding first-generation college student status to the model, caused the interaction between title condition and headshot gender to no longer reach statistical significance, although the effect size remained unchanged (see *SI*, Section 4.2 for analyses). Thus, neither students' demographics, students' ideologies, or the gender of students' own master/head could account for the reduction of gender bias favoring men that corresponded with the title change from *master* to *head*.

Finally, the present study investigated memory based on the premise that masculine generics bring men to mind and activate male-stereotypic expectations during learning might, which, with repetition, scaffold more and better memories for men than women. Thus, there may be some correspondence between bias in accessibility and bias in memory in the face recognition task (d'). In an exploratory analysis, individual differences in students' likelihood of imagining a man (vs. a woman) was correlated with better memory for men than women ($d'_{\text{men}} - d'_{\text{women}}$), $r(309) = 0.11$, $p = .047$, but only weakly. The binary accessibility measure best captures bias in aggregate, rather than at the level of individual differences, which could account for the weak relationship, and the two measures plausibly also capture some non-overlapping processes (i.e., accessibility vs. memory formation, consolidation, and maintenance).

Discussion

Gender bias in cognitive accessibility and memory can create disadvantages for women and contribute to gender inequity (Banaji & Baskhar, 2000; Heilman, 2001). In the present research, students displayed less gender bias favoring men in cognitive accessibility and accurate memory concerning a campus leadership role when it was called *head* compared to the masculine generic term *master*. While gender-related expectations and stereotypes can make gender *incongruent* information more accessible and produce relatively better memory for it under particular circumstances—when people notice a salient discrepancy, are motivated to resolve it, and have adequate processing resources to do so (Fiske & Neuberg, 1990; Hess, 2005)—our findings are consistent with a large body of evidence indicating that congruent information is typically more accessible and can be better remembered (Bayen et al., 2000; Frankenstein et al., 2020). This advantage for congruent information has been shown for memory of faces during impression formation (Cassidy et al., 2012; Eberhardt et al., 2003; Nash et al., 2010) as well as specifically for gender stereotypical information (Ellmers, 2018).

The present research extends understanding of gender bias and language in two important ways. First, prior experiments have revealed that, compared to more gender-inclusive language, masculine generic terms influence cognitive accessibility, causing men to come to mind more than women (for reviews see, Bigler & Leaper, 2015; Sczesny et al., 2016). The present study demonstrated this effect, but notably, in the context of a real-world language change. Students exposed to the title *master* were more likely to list a man as an example of the particular leadership role compared both with the actual incidence of men in the role and compared with students exposed to the title *head*. Although the naturalistic design prevents definitive causal interpretations of the present study, the striking correspondence between this finding and prior experiments (for a review see, Sczesny et al., 2016) suggests that in this applied context

masculine generic language (i.e., *master*) made it easier for students to think of men than women. The present study thus complements prior experimental research but with enhanced ecological validity.

Second, the present research suggests that masculine generic terminology biases accurate memory—creating more and better memories of men, consistent with previous findings of expectancy-congruent memory effects for social categories (e.g., Hills et al., 2008). The title *master* was associated with better memory (d') for actual men than women in the relevant leadership role compared with the title *head*. This memory task explicitly made both men and women cognitively accessible, but students more accurately recognized the male *masters* than the female *masters*. A key feature of the design is that the survey itself did not include any information about which faculty were masters/heads. Thus, the “learning phase” for the memory test occurred during students’ everyday lives before they began the survey. This feature of the design spotlights practical significance by demonstrating more far-reaching consequences of gendered language. That is, because learning occurred before the survey began (and because d' accounts for guessing) mere exposure to the terms *master* (in 2015) and *head* (in 2018) in the survey itself cannot fully explain the observed effects on accurate memory. The terms in the survey itself might have contributed to students being less likely to guess (c) that women were *masters* than *heads*. But the term *master* (vs. *head*) in the survey itself cannot fully explain differences in students’ accurate memories (d') learned before the survey began.

By revealing gender bias in accurate memory, the present study joins with recent work in demonstrating additional and more far-reaching consequences of masculine generic language beyond accessibility. For instance, women exposed to a masculine generic term in a mock, practice interview expressed less interest in pursuing the job and a lower sense of belonging

(Stout & Dasgupta, 2011). Further, masculine generic terms in high status job advertisements cause men to be rated as more suitable for the job than women (Horvath & Sczesny, 2015). The memory finding from the present study thus advances understanding of the range of processes affected by masculine generics with theoretical and practical significance.

Other factors besides language that associate men with the master/head role might also then be expected to correspond with gender bias favoring men in these cognitive outcomes. Indeed, students' whose own master/head was a man demonstrated male bias in which gender came to mind and was better remembered. By contrast, students led by a female master/head showed no evidence for gender bias in either direction on both measures (similar to Beaman et al., 2009). People tend to associate leadership with men (Koenig et al., 2011) and indeed humanity in general with men (i.e., *androcentrism*; Bailey et al., 2019), which could explain why having a female master/head reduced bias favoring men but did not reverse it.

Although the present research has several strengths for ecological validity and practical importance, limitations and avenues for future work must be highlighted. The present design did not involve random assignment and thus was not a true experiment. Students were from the same institution and were similar between title conditions on relevant demographic dimensions and ideologies, but by necessity they were admitted to the university 3 years apart—that is, before and after the language policy change. In exploratory analyses, we ruled out the possibility that the observed bias reduction was due to changes between cohorts in students' demographic characteristics and ideological beliefs that we measured.

Nevertheless, future experimental work is necessary to definitely establish a causal link. Such experimental work would be particularly valuable if it were designed in a way that investigated relatively enduring memory effects that are built up and strengthened over time

through repeated experiences rather than due to discrete priming influences, which tend to be limited in duration (Maddox et al., 2019). To create enduring changes in memory, future experimental research might consider language-related interventions that, as in our current research, are sustained, immersive, and directly relevant to the experiences of participants (as called for by Sczesny et al., 2016). For instance, the connection between *master* or *head* and specific individuals in the role could be established through a relatively intensive learning phase in the study. However, even then, experimental research might also vary the social importance of the terms *master* and *head* for participants. In our research, the distinction between *master* and *head* was integral to a language-policy change that was viewed as significant by students within the university. If appropriate conditions can be established experimentally, then additional experimental research might also test the effects of the term *mistress* compared to *master* in a context in which the terms are comparably applicable and credible. Because the present study leveraged a real-world language change, we were confined to only examining the terms used by the institution (i.e., *master* and *head*). In an appropriate experimental context, it is likely that *mistress* would bring women to mind more than men. In fact, while *master* might be a masculine generic term that is used both specifically for male persons and, in the present case, generically for people of all genders, *mistress* is likely only used specifically for female persons.

Note that the greater complexity of influences in naturalistic research permits also other alternative explanations beyond participant cohorts (Campbell & Stanley, 1963; Rothstein & von Wachter, 2017). For example, the change in gendered language policy may have signaled to students an organizational shift to greater gender egalitarian norms. Thus, this signal about gender egalitarianism, rather than the terminology change per se, might be what caused the observed reduction in gender bias. However, Yale University's president publicly indicated that

the language change was not motivated by concerns about gender egalitarianism but instead by concerns about racial bias (i.e., “[*master*] is also associated with the ownership of slaves;” Salovey, 2016). The lack of an explicitly gendered motivation aligns with the possibility that the terms themselves, and not signals about increasing gender egalitarian norms, caused the corresponding reduction in gender bias favoring men.

Future experimental research might also examine how the social-motivational context of language reforms affects their influence. For example, on the one hand it is possible that a change to more gender-inclusive language would have the largest influence when it is part of a comprehensive organizational initiative to improve gender equity (e.g., Smith et al., 2015). Multi-faceted approaches can be mutually reinforcing. On the other hand, a policy change to use gender-inclusive language may be less effective when it is embedded in a comprehensive initiative, particularly if that initiative is perceived to be driven by feminist motivations. Feminism is contentious. Even people who endorse feminist values can have negative attitudes toward feminism (Glick & Fiske, 1996; Jenen et al., 2009; Zucker, 2004). The change from *master* to *head* might have aligned with a reduction in gender bias in the present research precisely because feminist motivations were not obvious to students. Future research could investigate whether (de)emphasizing feminist motivations causes gender language reforms to be more or less successful in reducing gender biases.

As noted earlier, because the present research leverages an actual policy change, it only compared *master* to *head*. Theoretical work on masculine generic language, however, documents two prominent alternatives: “neutralization,” which does not explicitly include any gendered information (e.g., *they*) and “feminization,” which explicitly includes both common genders (e.g., *he or she*; Sczesny et al., 2016). Because *head* does not explicitly make mention of gender

(Oxford English Dictionary, 2013) it is an example of neutralization. In contrast, feminized language such as *master or mistress* might be even more effective than *head* in reducing gender bias favoring men because it explicitly names both women and men. Bailey and LaFrance (2016b) and Khosroshai (1989) found that compared to masculine generic language, feminized terms (e.g., *he or she*) reduced gender bias favoring men in cognitive accessibility more than neutralized terms (e.g., *they*). Future work could investigate this possibly in a real-world context by examining language policy changes involving feminized terms that explicitly reference both women and men. But such feminized terms also have key drawbacks (e.g., Liben & Bigler, 2017). Developmentally, binary terms such as *boys and girls* and *ladies and gentlemen* can cause children to attach undue meaning to gender and reinforce male advantaging gender hierarchies (Bigler & Liben, 2007). Such phrases also notably exclude gender non-binary individuals. In contrast, by not explicitly mentioning gender, neutralized terms (e.g., *they*) allows for a wider range of gender identities to be cognitively accessible beyond women and men.

Finally, the present study sampled Yale undergraduate students, which is the population most directly affected by the institutional policy change, but this sample limits generalizability. Student samples from elite U.S. universities are more educated, wealthy, and politically liberal than the average U.S. citizen (Buhrmester et al., 2011; Henrich et al., 2010). Students were also English-speaking and the survey was conducted in English. Masculine generics occur in many other languages besides English (Hellinger & Bußmann, 2003). Future work could further advance the present conclusions by assessing whether real-world language policy shifts in other cultural and linguistic contexts also affect cognitive accessibility and accurate memory.

Conclusions

While it appears that social science researchers are using fewer masculine generic terms in their writing than they used to (Hegarty & Buechel, 2006), masculine generics persist in formal academic writing (e.g., *man* to mean humanity, Kteily et al., 2015) and in everyday causal speech (e.g., *guys* to mean women and men, MacArthur et al., 2019). Even though efforts to reduce masculine generic language can elicit controversy and claims that masculine generics are interpreted as including men and women (e.g., Dalén, 2012), evidence from experimental studies demonstrates otherwise. Masculine generic language biases cognitive accessibility and causes men to come to mind more than women (Bigler & Leaper, 2015; Sczesny et al., 2016). The present study demonstrated this effect, but in the context of a real-world language change. Further, students displayed better memory for men than women in a campus leadership role when it was called a *master*, but this male memory advantage was eliminated among students exposed to *head*. This reduction in gender bias favoring men from 2015 to 2018 cannot be explained by changes in the incidence of women and men in the role, in students' demographic attributes, in students' ideologies, or in students' actual master/heads' gender.

The present study points to real-world implications. The accessibility and memory biases demonstrated here in a naturalistic context could conceivably limit women's opportunities, likely in unintentional but consequential ways. If women are less likely than men to come to mind, women might be overlooked for leadership appointments, awards, or accolades. Failing to remember women relative to men likely undermines women's leadership effectiveness even when women are well represented in leadership roles (cf., UN Women, 2017). For instance, if women's contributions are not remembered or if their ideas are misremembered as coming from someone else (Sesko & Biernat, 2010), they are effectively made "invisible". Being overlooked also reduces individuals' sense of importance and impairs interpersonal relationships (Ray et al.,

2019). The present study suggests that a simple, albeit sometimes politically charged, switch to gender-inclusive language might help to alleviate these negative outcomes. All the more so because gender-inclusive language is contagious—mere exposure to others’ gender-inclusive language reduces people’s own likelihood of using masculine generic terms (Hansen et al., 2016; Koeser et al., 2015). Thus, institutionalized use of gender-inclusive language can positively effect women and create a feedback loop promoting greater gender equity.

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Supplemental Information

Supplemental information for the article entitled, “‘Master’ of none: Institutional language change linked to reduced gender bias.”

Section 1 Masculine and feminine trait analyses

In line with our preregistration, the trait ratings are only reported descriptively in the main text. The means for each trait and for each average of positive feminine, positive masculine, negative feminine, and negative masculine are reported in Figure S1.

Figure S1

Mean Trait Ratings of Imagined Master/Head

Note. Error bars represent 95% confidence intervals.

In supplementary analyses we explored the possibility that students' conceptions of the master/head role's femininity and masculinity differed as a function of their exposure to *master* or *head* while also considering students' own gender and the gender of their actual master/head. We conducted an exploratory 2 (traits: feminine, masculine) x 2 (title condition: *master*, *head*) x 2 (student gender: women, men) x 2 (actual master/head gender: man, woman) ANOVA with traits as a repeated measures factor. There was only one main effect: students rated their imagined master/head higher in feminine traits ($M = 6.17$, $SD = 0.78$) than masculine traits ($M = 5.77$, $SD = 0.70$), $F(1,327) = 59.30$, $p < .001$, $\eta_p^2 = 0.15$. Thus, there was no evidence that students imagined a more masculine or more feminine individual depending on their exposure to *master* vs. *head*, $F(1,327) = 1.20$, $p = .267$, $\eta_p^2 < 0.01$. All other main and interaction effects did not reach statistical significance at conventional levels and all η_p^2 's < 0.01 (title condition: $F(1,327) = 2.03$, $p = .155$, actual master/head gender: $F(1,327) = 0.67$, $p = .413$, student gender: $F(1,327) = 0.46$, $p = .499$, title condition x actual master/head gender: $F(1,327) = 0.01$, $p = .937$, title condition x student gender: $F(1,327) = 1.45$, $p = .229$, actual master/head gender x student gender: $F(1,327) = 0.93$, $p = .336$, traits x actual master/head gender: $F(1,327) = 0.07$, $p = .792$, traits x student gender: $F(1,327) = 0.07$, $p = .789$, title condition x actual master/head gender x student gender, $F(1,327) = 3.57$, $p = .060$, traits x title condition x actual master/head gender: $F(1,327) = 1.41$, $p = .236$, traits x title condition x student gender: $F(1,327) = 3.59$, $p = .059$, traits x student gender x actual master/head gender: $F(1,327) = 0.01$, $p = .914$, traits x student gender x actual master/head gender x title condition: $F(1,327) = 0.03$, $p = .855$).

Section 2.1. Memory test formulas

To calculate the signal detection parameters we first applied the loglinear correction as pre-registered, which is appropriate when some participants answer all (or none) of the trials correctly (Stanislaw & Todorov, 1999). The hit rate (H) was the number of hits divided by the number of master/head trials and calculated the false alarm rate (F) in kind. The formulas for d' (memory, or *sensitivity*) and c (guessing bias) provided below use the H and F parameters normalized on the z distribution.

$$d' = \Phi^{-1}(H) - \Phi^{-1}(F)$$

$$c = \frac{\Phi^{-1}(H) + \Phi^{-1}(F)}{2}$$

Section 2.2. Alternative memory test formulas and analyses

The d' and c parameters are presented in the main text as pre-registered. These parameters rely on the assumptions that (a) the signal and noise distributions are normal and (b) the signal and noise distributions have the same standard deviation. These assumptions cannot be tested concerning binary decisions tasks as employed here (e.g., master vs. not a master). A' is a nonparametric alternative to d' that does not rely on these assumptions (Stanislaw & Todorov, 1999). Thus, in exploratory analyses reported here we calculated A' and submitted it to the same analyses for d' reported in the main text. The formula for A' (nonparamenteric memory) is provide below using the H and F parameters.

$$A' = .5 + \frac{(H - F)(1 + H - F)}{4H(1 - F)} \text{ when } H \geq F$$

$$A' = .5 + \frac{(F - H)(1 + F - H)}{4F(1 - H)} \text{ when } H < F$$

As with d' , larger A' values indicate better memory—i.e., better ability to distinguish master/heads from non-master/head faculty. A' values were submitted to the same factorial model used to evaluate d' : 2 (title condition: *master*, *head*) x 2 (actual master/head gender: man, woman) x 2 (student gender: woman, man) x 2 (headshot gender: man, woman) ANOVA with headshot gender as a repeated measures factor.

Replicating the d' results, there was a main effect of headshot gender, indicating that overall students had better memory for men ($M = 0.67$, $SD = 0.15$) than women ($M = 0.63$, $SD = 0.22$), $F(1,327) = 7.12$, $p = .008$, $\eta_p^2 = 0.02$. This main effect was qualified by an interaction between headshot gender and title condition, $F(1,327) = 6.54$, $p = .011$, $\eta_p^2 = 0.02$. Men were remembered better than women under the *master* ($M_{man} = 0.70$, $SD_{man} = 0.16$, vs. $M_{woman} = 0.60$, $SD_{woman} = 0.28$), $F(1,327) = 10.78$, $p = .002$, $\eta_p^2 = 0.03$, but there was no evidence for memory gender bias under the *head* title at conventional statistical levels ($M_{man} = 0.66$, $SD_{man} = 0.15$, vs. $M_{woman} = 0.64$, $SD_{woman} = 0.18$), $F(1,327) = 0.01$, $p = .927$, $\eta_p^2 < 0.01$.

Also replicating the d' results, there was an interaction between actual master/head gender and headshot gender, $F(1,327) = 9.64$, $p = .002$, $\eta_p^2 = 0.03$. Men were remembered better than women among students led by a male master/head ($M_{man} = 0.69$, $SD_{man} = 0.16$, vs. $M_{woman} = 0.61$, $SD_{woman} = 0.22$), $F(1,327) = 21.38$, $p < .001$, $\eta_p^2 = 0.06$, but there was no evidence for a memory gender bias among students led by a female master/head ($M_{man} = 0.65$, $SD_{man} = 0.15$, vs. $M_{woman} = 0.66$, $SD_{woman} = 0.22$), $F(1,327) = 0.08$, $p = .780$, $\eta_p^2 < 0.01$.

Unlike d' , there was also an interaction among title condition, participant gender, and actual master/head gender, $F(1,327) = 4.92$, $p = .027$, $\eta_p^2 = 0.01$. However, this interaction did not involve the headshot gender condition, and thus cannot inform the key issue in the present research: potential gender bias in memory based on men compared to women.

Similar to d' all other main and interaction effects did not reach statistical significance at conventional levels and all η_p^2 's < 0.01 (title condition: $F(1,327) = 0.02, p = .898$, actual master/head gender: $F(1,327) = 0.01, p = .942$, student gender: $F(1,327) = 0.87, p = .352$, title condition x actual master/head gender: $F(1,327) = 0.14, p = .713$, title condition x student gender: $F(1,327) = 2.22, p = .137$, actual master/head gender x student gender: $F(1,327) = 1.87, p = .666$, headshot gender x student gender: $F(1,327) = 0.16, p = .691$, headshot gender x actual master/head gender x student gender, $F(1,327) = 2.89, p = .090$, headshot gender x title condition x actual master/head gender: $F(1,327) = 0.13, p = .719$, headshot gender x title condition x student gender: $F(1,327) = 0.39, p = .531$, headshot gender x student gender x actual master/head gender x title condition: $F(1,327) = 1.16, p = .283$).

Section 3.1. Additional student gender analyses for the imagined exemplar

We pre-registered separate analyses for female and male students in each title condition concerning which gender comes to mind. Given that the student gender x title condition interaction was non-significant and that these analyses do not change the conclusions of the main text, they were omitted from the main text to streamline the results section. Female students exposed to the *master* wording were more likely to imagine a man than expected by the actual proportion of men (0.75 vs. 0.50), $\chi^2(1, n=63) = 15.25, p < .001, \Phi = 0.49$, and female students exposed to the *head* wording also showed no bias compared to the actual proportion of men (0.49 vs. 0.57), $\chi^2(1, n=125) = 3.55, p = .060, \Phi = 0.17$. Male students exposed to *master* were more likely to imagine a man than expected by the actual proportion of men (0.81 vs. 0.50), $\chi^2(1, n=47) = 17.89, p < .001, \Phi = 0.62$, and male students exposed to *head* showed no bias compared to the actual proportion of men (0.68 vs. 0.57), $\chi^2(1, n=72) = 3.50, p = .061, \Phi = 0.22$.

Section 3.2. Additional student gender analyses for the memory test

We pre-registered separate ANOVA analyses for female and male students in each title condition concerning memory (d'). Given that the student gender x title condition x headshot gender interaction was non-significant and that these analyses do not change the conclusions of the main text, they were omitted from the main text to streamline the results section. Among female students, the interaction between title condition and headshot gender was non-significant at conventional levels, $F(1,198) = 0.53, p = .468, \eta_p^2 < 0.01$; however, the male memory bias was directionally larger under the *master* title ($M_{man} = 0.89, SD_{man} = 0.71$, vs. $M_{woman} = 0.70, SD_{woman} = 0.82$), $F(1,198) = 2.45, p = .239, \eta_p^2 = 0.01$, compared to the *head* title ($M_{man} = 0.59, SD_{man} = 0.58$, vs. $M_{woman} = 0.51, SD_{woman} = 0.70$), $F(1,198) = 0.96, p = .329, \eta_p^2 < 0.01$. Among male students, there was an interaction between title condition and headshot gender, $F(1,133) = 6.04, p = .015, \eta_p^2 = 0.04$. Male students displayed better memory for men than women under the *master* title ($M_{man} = 0.76, SD_{man} = 0.77$, vs. $M_{woman} = 0.37, SD_{woman} = 0.83$), $F(1,133) = 8.88, p = .006, \eta_p^2 = 0.06$, but displayed no evidence for bias under the *head* title at conventional statistical levels ($M_{man} = 0.62, SD_{man} = 0.72$, vs. $M_{woman} = 0.64, SD_{woman} = 0.71$), $F(1,133) = 0.06, p = .812, \eta_p^2 < 0.01$.

Section 4.1. Exploratory analyses for the imagined exemplar

In exploratory analyses we tested the impact of title condition controlling for each ideological and demographic factor in separate logistic regressions and controlling for the effect of students' actual master/head's gender. The effect of title condition persisted in direction and significance across these models (Table S1).

Table S1*Effect of Title Condition on Gender of Imagined Master/Head with Each Control Factor*

Control Factor	<i>B</i>	<i>SE</i>	OR
Ideological Factors			
SDO ^a	- 0.99***	0.27	0.37
RWA ^b	- 0.99***	0.27	0.37
GSJ ^c	- 0.96***	0.27	0.39
Political Stance, Social Issues	- 0.99***	0.27	0.37
Political Stance, Economic Issues	- 0.96***	0.27	0.38
Student Demographic Factors			
Gender ^d	- 0.97***	0.27	0.38
Race ^e	- 0.98***	0.27	0.37
Age	- 1.04***	0.27	0.35
Class-year	- 1.01***	0.27	0.37
English Speaking Status ^f	- 0.98***	0.27	0.38
First-Generation ^g	- 1.03***	0.27	0.36
Actual Master/Head Gender ^d	- 1.07***	0.28	0.34

^aSocial Dominance Orientation. ^bAuthoritarianism. ^cGeneral System-Justification. ^d0 = women, 1 = men. ^e0 = non-White, 1 = White. ^f0 = English as first language, 1 = English as second language. ^g0 = not a first-generation college student, 1 = first-generation college student.

*** $p < .001$.

Section 4.2. Exploratory analyses for the memory test

We also tested the interaction between title condition and headshot gender on memory (d') while including each ideological and demographic factor (including students' own master/head's gender) and its interaction term with headshot gender as covariates in the model. The effect of title condition persisted in direction and size across these 12 models and in significance across 11 of the 12 (Table S2).

Table S2*Interaction between title condition and headshot gender for memory with each control factor*

Control Factor	<i>F</i> (df1, df2)	<i>p</i>	η_p^2
Ideological Factors			
SDO ^a	5.20 (1, 332)	.023*	0.02
RWA ^b	5.03 (1, 338)	.026*	0.01
GSJ ^c	5.61 (1, 338)	.018*	0.02
Political Stance, Social Issues	5.12 (1, 338)	.024*	0.01
Political Stance, Economic Issues	4.79 (1, 338)	.029*	0.01
Student Demographic Factors			0.01
Gender ^d	4.55 (1, 333)	.033*	0.01
Race ^e	5.31 (1, 338)	.022*	0.02
Age	5.38 (1, 335)	.021*	0.02
Class-year	5.13 (1, 338)	.024*	0.01
English Speaking Status ^f	5.60 (1, 338)	.019*	0.02
First-Generation ^g	3.75 (1, 336)	.054	0.01
Actual Master/Head Gender ^d	5.95 (1, 337)	.015*	0.02

^aSocial Dominance Orientation. ^aAuthoritarianism. ^cGeneral System-Justification. ^d0 = women,

1 = men. ^e0 = non-White, 1 = White. ^f0 = English as first language, 1 = English as second

language. ^g0 = not a first-generation college student, 1 = first-generation college student.

* $p < .05$.