Self-Selection as an Explanation for General Mental Ability Test Score Differences between Mobile and Non-Mobile Devices in Observational Studies

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Running Head: SELF-SELECTION AND MOBILE DEVICE EFFECTS

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Michael Grossenbacher and Zachary Warman both currently work at Wonderlic Inc., and Matt Brown was previously employed there. However, while the company publishes the assessments used in the study and aided in the data collection, they did not influence the design or outcome of the research.

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Abstract

Past studies have reported divergent results regarding the effect of mobile devices on general mental ability (GMA) test scores. We investigate selection bias as an explanation for this inconsistency in GMA score differences between applicants using mobile or non-mobile devices reported in observational and lab studies. We initially found that mobile test takers scored 0.58 standard deviations lower than non-mobile test takers in an operational sample of 76,948 applicants across over 400 occupations. However, we found that mobile device use was more prevalent among applicants with lower educational attainment and within jobs of lower complexity. These factors, among others, could potentially confound the observed GMA score differences between devices. The device effect shrank to $d = 0.25$ after controlling for selection bias in device choice using propensity score weighing. As an alternative, we also used post-stratification to control for selection bias and this yielded an even weaker device effect ($d = 0.10$). Our results indicate that the large device effects obtained in prior operational studies are possibly inflated by selection bias. Therefore, it is important to control for these demographic and occupational differences between self-selected device groups when analyzing operational data for research purposes. Propensity score weighting and post-stratification appear useful for reducing the impact of selection bias in real-world, observational data. We also strongly recommend the use of random assignment to prevent selection bias when evaluating device effects for new or adapted GMA or similar ability tests.

Keywords: cognitive ability; mobile assessment; unproctored internet testing; selection bias; propensity score weighting; personnel selection

Self-Selection as an Explanation for General Mental Ability Test Score Differences between Mobile and Non-Mobile Devices in Observational Studies

A common sampling method in personnel selection research is to analyze large, operational datasets of actual job applicants (e.g., Campion et al., 2019; Hardy et al., 2017). While such samples may be viewed favorably due to their ecological or external validity, however, their appeal may cause some researchers and practitioners to overlook substantial internal validity threats inherent to non-experimental studies (Highhouse, 2009). One common concern with non-experimental studies is the potential for selection bias when participant characteristics are not carefully controlled or balanced (Heckman, 1979). Selection bias is likely to occur when participants are not randomly assigned to levels of the independent variable. This can lead to endogeneity, where both the independent and dependent variables are related to one or more underlying variables. Endogeneity acts to bias the observed relationship between focal variables (Antonakis et al., 2010) which confounds any tests for causality and is observed by many as a threat to management and organizational research (e.g., Clougherty et al., 2016).

This concern is especially relevant for research on the effects of using a mobile device to complete unproctored, internet-based tests (UIT) for selection purposes. Past studies have generally found equivalence between mobile and non-mobile test-takers for self-report personality (Arthur et al., 2014) and biodata assessments (Morelli et al., 2014). However, several large-scale studies using operational, applicant data have found that applicants using non-mobile devices generally score higher on general mental ability (GMA) tests compared to applicants who used a mobile device (e.g., Arthur et al., 2014; Wood et al., 2015). According to a recent meta-analysis, non-mobile test takers score an average of 0.79 standard deviations higher on GMA tests than mobile test takers, summarized across 15 operational samples (Traylor et al.,

2021). These effects have been interpreted to suggest that GMA tests are more difficult to complete using mobile devices and some practitioners have recommended caution when implementing mobile-friendly GMA assessments for use in selection testing (e.g., Lawrence & Kinney, 2017). In contrast, studies using non-operational samples have found little to no effect of device type on GMA test scores (sample-weighted $d = 0.05$ from Traylor et al., 2021). These non-operational samples have typically consisted of Amazon's Mechanical Turk workers (e.g., Brown & Grossenbacher, 2017; Grelle & Gutierrez, 2019) or undergraduate students (Arthur et al., 2018; Traylor et al., 2021). Each of these populations are widely used in organizational research but are often thought to provide weaker ecological validity compared to applicant or employee samples (Landers & Behrend, 2015).

In the present study, we focus on selection bias as a potential explanation for the inconsistent findings for mobile device effects on GMA test scores in UIT research. One of the primary strengths of true experimental research is the random assignment of the independent variable (Stone-Romero, 2002). In the case of mobile UIT research, the independent variable is the form of device (mobile or non-mobile) used to complete the assessment. When device use is determined by researcher assignment, there appears to be little, if any, effect of mobile device use on GMA test scores. Studies using non-operational samples have determined device use by using quasi-random assignment (Brown & Grossenbacher, 2017) or required participants using both mobile and non-mobile devices in counterbalanced orders (Grelle & Gutierrez, 2019). Both designs prevent selection bias from confounding between-group differences in scores by ensuring equivalent device groups but these methods have not been used in an operational setting. On the other hand, the largest effects of mobile device use on GMA test scores have been reported in studies where participants were allowed to choose their preferred device when

completing pre-hire assessments. We investigate whether mobile device effects are present after attempting to control for selection bias using propensity score weighting and post-stratification.

Potential for Selection Bias in Operational Samples

Despite the importance of mobile UIT research for the practice of pre-employment testing and talent assessment, there have been few attempts to explain the inconsistent findings for mobile device use on GMA test scores. In a recent exception, Traylor et al. (2021) investigated the role of the testing environment and distractions while completing a mobile GMA assessment. The authors hypothesized that mobile test users perform worse on average due to their surrounding testing environment, which was expected to be more distracting for mobile test users. However, their experimental study did not result in any differences in test scores between device types, even when mobile devices were used outdoors and distractions were more commonly reported. Likewise, in a correlational study of 5,549 job applicants, Lawrence et al. (2017) reported that nearly all mobile job applicants reported completing their tests while at home (96%) and were more likely to test at home compared to non-mobile users. This contradicts the idea that mobile test-takers are more likely to test in public or more distracting environments just because mobile technology allows them to do so. Based on these findings, we suspect that differences in the testing environment are unlikely to explain the divergent findings for GMA score differences in mobile UIT studies.

Instead, we argue that endogeneity due to self-selection of the testing device is a more likely explanation for the differing results in operational and non-operational settings. This form of bias has long been a concern in fields which rely heavily on non-experimental or correlational data (e.g., economics; Heckman, 1990). Researchers have often observed systematic differences between those who use mobile versus non-mobile devices for UIT. The use of a mobile device to

complete selection assessments has been reported to be more prevalent amongst applicants who are younger (Illingworth et al., 2015) are female (Golubovich & Boyce, 2013), and identify as a member of a racial or ethnic minority group (Arthur et al., 2014; Illingworth et al., 2015). In addition, the proportion of mobile test takers may vary between industries (Jaber et al., 2020; PSI, 2018) with some studies indicating that mobile testing is more common among applicants for lower complexity jobs (e.g., lower O*NET job zones; Warman et al., 2020¹). These results align with broader demographic trends for smartphone use reported by Pew (2021), which indicates that smartphone reliance is highest among individuals who are younger, have lower educational attainment, and identify as Black or Hispanic.

We believe that these factors likely contribute to selection bias by creating non-equivalent mobile and non-mobile groups in non-experimental studies where applicants select their own testing device. Under these conditions, we expect that endogeneity is highly likely given that several of the factors which are positively related to mobile usage also happen to be correlated with GMA or performance on ability tests. For example, test takers from minority racial or ethnic groups are more likely to test using mobile devices and also tend to score lower on GMA tests compared to White test takers (Bobko & Roth, 2013; Roth et al., 2017). Likewise, mobile use occurs less for highly complex or prestigious jobs, which tend to attract individuals with higher GMA (e.g., Brown et al., 2021; Sackett & Ostgaard, 1994; Warren et al., 2002; Wilk et al., 1995). As a result, operational differences between mobile and non-mobile applicants may be highly biased due to endogeneity. This makes it unclear whether score differences are due to individual differences between those who choose to use a mobile or non-mobile device or measurement invariance between device types. Although several scholars have suggested

¹ Note. A portion of the sample in the present study was also used in Warman et al., 2020

selection bias as a potential confound (e.g., Arthur et al., 2014; Brown & Grossenbacher, 2017; Gibson et al., 2017; Traylor et al., 2021), no study to our knowledge has attempted to control for demographic or occupational variables when estimating differences in GMA test scores between device groups in an operational sample.

Methods of Addressing Selection Bias

We focus on two commonly used approaches for combating selection bias in non-experimental data in the present study. We selected each of these methods due to their prevalence in other areas of social science research, including political science and economics. Although these methods are not widely used in applied psychology, they have been discussed in the organizational and human resource management literature (e.g., Connelly et al., 2013; Kulas et al., 2018). We did not have any specific hypotheses regarding the relative effectiveness of each method. Instead, we were interested in the similarity of results for each method and whether these corrected effect sizes were closer in magnitude to the effect sizes reported from experimental/ lab-based studies or observational studies. One method to address selection bias in research is post-stratification. Post-stratification, or sample weighting, is commonly used to transform data from a sample in order to better estimate population parameters (Gelman, 2007; Gelman et al., 2020). Often in field studies (e.g., when conducting an organizational survey) researchers are not able to control the makeup of their sample relative to the full participant pool. In these cases, a set of weights can be derived to reproduce a target distribution on some covariates exactly (Little, 1993). Thus, if two samples differ on potentially confounding variables of interest, they can be equated. This approach is commonly used in public opinion polling or surveys (Schonlau & Couper, 2017). In the context of mobile assessment, post-stratification on available demographic information may help minimize the effects of device self-

selection and determine whether observed differences in scores are due more to the device or the individual taking the test.

Another method for addressing selection bias is propensity score weighting (Hamilton & Nickerson, 2003). This method uses covariates to predict the likelihood of an individual receiving a treatment (Connelly et al., 2013). These predictive models are used to estimate propensity scores which are used to either match cases across treatment and control conditions or to differentially weigh participants to achieve the same ends. First suggested by Rosenbaum and Rubin (1984), propensity scoring is particularly useful in cases where covariates may predict or correlate with an individual's choice to self-select into a treatment or control condition. For example, the causal effect of standardized test preparation is potentially confounded by participant motivation or other individual differences in non-experimental studies where treatment conditions are not randomly assigned (Connelly et al., 2013). Propensity score weighting was originally popularized in economics (e.g., Dehejia & Wahba, 1999) and medical research (e.g., Rubin, 1997) as a means of making stronger causal claims from observational, non-experimental research. This method is especially useful for minimizing differences between treatment and control groups in situations without random assignment (e.g., Harter et al., 2010; Larzelere et al., 2004) which has made the approach particularly popular in medicine (e.g., Austin, 2008), despite having only been used sparingly in applied psychology or psychometric testing research (e.g., Lee & Geisinger, 2014; Schmidt & Pöhler, 2018).

Unlike post-stratification, which explicitly equates samples with regard to specified covariates, propensity scoring only considers their value in predicting the receipt of a treatment or membership in a group. In this way, variables that are not potentially biasing will not be controlled for. Additionally, being inherently based on logistic regression, propensity scoring can

make use of more complex and powerful predictive algorithms that are available to researchers, like boosted regression (e.g., Ridgeway et al., 2020). Once propensity scores are obtained, one of several approaches can be taken to remove the effect of the covariates. Common options include stratifying samples based on the propensity score, performing one-to-one matching based on propensity scores, and creating weighting schemes to equate (Li & Greene, 2013; Rosenbaum & Rubin, 1983). As a result of these procedures, the resulting samples will be relatively free of any confounding effect from the covariates.

Present Study

The primary goal of the present study is to test whether selection bias can explain the differences in mobile device effects on GMA test scores in operational and non-operational samples. We expect that endogeneity due to selection bias is a primary cause for this discrepancy. To this end, we examine whether post-stratification or propensity scores can help mitigate potential selection bias in an operational sample of U.S. job applicants. These methods can minimize selection bias in non-experimental studies but have been more widely used in survey research or fields outside of applied psychology (e.g., epidemiology or economics). We use these two methods in order to calculate two different estimates for the expected mobile device effect without selection bias. We seek determine the magnitude to which selection bias accounts for GMA score differences between mobile and non-mobile test takers and operationalize the presence of a selection bias as indicated by the observed change in device group effect after applying post-stratification or propensity score weights. Therefore, we hypothesize that using statistical methods for minimizing selection bias (propensity score weighting and post-stratification) will substantially reduce the observed differences in average GMA test scores between applicants who use mobile or non-mobile devices.

Hypothesis 1: Propensity score weighting or post-stratification will reduce the average GMA test score difference between mobile and non-mobile test takers in an operational sample.

In addition to testing our selection bias hypothesis, we also seek to explore several potential individual and occupational factors which may correlate with the likelihood of using a mobile device to complete selection tests and lead to selection bias. Given that some preliminary research has identified differences in mobile device usage for selection testing between O*NET job zones (Warman et al., 2020) or between entry-level and leadership-level roles (Wexler & Rossini, 2020), we suspect that test score differences in operational data are potentially confounded when the samples include applicants for different occupations. One variable which may act as a confound in this type of sample is educational attainment. The linkage between GMA and educational attainment has been demonstrated and replicated across several longitudinal, cohort-based studies spanning different countries and points in time (Brown et al., 2021; Deary et al., 2007). Not only do differences in GMA predict differences in educational attainment later in life, but researchers have also found that education has additive effects on subsequent ability scores (Ritchie & Tucker-Drob, 2018). Regardless of the true causal mechanism underlying this relationship, these findings indicate that groups which differ in educational attainment are also likely to differ in GMA. This is important because any differences in educational attainment between mobile and non-mobile test users would suggest that individuals may also differ in GMA which would lead to test score differences between device groups. Some scientist-practitioners have noted that they have observed differences in mobile testing by education level as indicated by biodata responses (Gibson et al., 2017). If this

is the case, an observed device effect may represent a true difference in ability between applicants who choose to test using a mobile or non-mobile device rather than evidence for a difference in test functioning.

Researchers have also found that greater GMA is linked to greater occupational attainment as indicated by employment in jobs with higher prestige (Cheng & Furnham, 2012), greater annual income (Judge et al., 2010), or of higher complexity (Sackett & Ostgaard, 1994). Greater GMA has also been found to correlate with the likelihood of occupying leadership roles later in life (Daly et al., 2015). Likewise, researchers have theorized that applicants are drawn to jobs which match their ability level (Wilk et al., 1995). This stratification of ability across different occupations presents a further confound for mobile device research when analyzing datasets which include applicants for a variety of jobs. If the proportion of mobile and non-mobile test takers is not equivalent across all jobs, the overall mean score differences between device groups may be partially explained by true differences in ability.

Given the degree of mean score differences in GMA tests between White and non-White minority groups typically reported in the research literature (Bobko & Roth, 2013; Roth et al., 2017), any imbalance in applicant demographics between device groups could further confound device effects. Little research has directly investigated this possibility but several operational studies have noted demographic differences among mobile and non-mobile test takers. For example, Arthur et al. (2014) observed that non-White applicants were overrepresented within their operational sample of mobile test takers. As there is little published research on demographic differences in mobile device use in selection testing, however, we do not make any formal hypotheses regarding the relative importance of each individual variable. Instead, we

intend to explore a variety of individual and occupational factors and report which are most strongly related to mobile device use for pre-employment testing.

Research Question 1: Which variables are most predictive of whether a job applicant chooses to test using a mobile or non-mobile device?

Method

Transparency and Openness

We describe our sampling plan, all data exclusions (if any), all manipulations, and all measures in the study, and we adhered to the Journal of Applied Psychology methodological checklist. Data were analyzed using R, version 3.6.1 (R Core Team, 2019) and the package *twang* (McCaffery et al., 2004). This study's design and its analysis were not preregistered. Data for this study are not available because they are proprietary.

Participants

Data associated with a standardized assessment battery developed by Wonderlic, Inc. were collected from 76,948 applicants for jobs in the United States between December 2019 to June 2020. Only applicants who selected the English form of the assessment battery were included in the study sample. All demographic information was self-reported by applicants as part of the assessment battery. Most applicants identified as female (52%) and Caucasian (56%). As typical in prior studies (e.g., Brown & Grossenbacher, 2017), we consider mobile devices to include smartphones and tablets, while desktop and laptop computers are considered non-mobile. Most applicants completed the battery using a non-mobile (desktop or laptop; 69%) rather than a mobile device. We provide a full summary of applicant demographics in Table 1.

Measures

All measures provided for this study were administered through an assessment platform developed by Wonderlic. This platform was designed to be mobile-responsive, allowing item presentation to dynamically update as the screen or browser size changes.

WPT-R. The Wonderlic Personnel Test-Revised (WPT-R; Wonderlic, 2002) was provided to applicants as the first component of the assessment battery. It is a speeded, 50-item measure of GMA and is one of the most widely used measures of GMA in personnel selection practice and research (Hunter & Schmidt, 2004). All tests were completed online without proctoring. Other mobile assessment studies have used similar, speeded ability tests (e.g., Arthur et al., 2014; Traylor et al., 2021). In both the present study and in Brown and Grossenbacher (2017), no meaningful difference in score variability has been observed between WPT forms completed on mobile versus non-mobile devices (present study mobile SD = 5.78, non-mobile SD = 5.85).

The development of the WPT-R and its validation across mobile and non-mobile devices was reported by Brown and Grossenbacher (2017). In this study, the authors randomly assigned participants recruited from Amazon's Mechanical Turk to complete the WPT-R using either a smartphone, tablet, or desktop/laptop computer depending on which devices were available to the participant. The authors did not stratify their sample based on occupational or demographic factors but the use of device assignment resulted in no statistically significant differences in educational attainment or ethnicity between device groups. Although the MTurk sample may not be fully representative of operational samples in actual preemployment testing, this study design provided strong internal validity by eliminating the potential influence of device selection on the observed test score differences between groups. To further ensure the equivalence between device groups, participants completed a parallel form of the WPT administered only using non-

mobile devices prior to the focal study. This allowed the authors to verify that there were no significant differences in pre-test scores between device groups. The authors used pre-test WPT scores to estimate the latent ability differences between device groups and test for differential functioning. Evidence for differential functioning was found for only two of the 50 total items which resulted in minimal differences in test functioning between mobile and non-mobile device users (e.g., signed difference of 0.05 in total test score points between non-mobile and smartphone users).

Although the research design in Brown and Grossenbacher (2017) was optimal for assessing differential functioning between device groups, little subsequent work has attempted to replicate their results in an operational, applicant sample. Therefore, we tested for differential functioning between testing devices for each of the WPT-R test forms used within our sample. Each applicant completed one of four parallel forms of the WPT-R and sample sizes for each form ranged between $n = 19,106$ to $19,347$ (see Table 2). Anchor selection was performed using the iterative forward all-other method described by Kopf, et al. (2013). DIF sweeps were conducted for each item using all others as anchors to identify an initial set of DIF-free items. Items displaying non-significant DIF via the Wald statistic were retained as anchors. Then, using the identified anchors, items were iteratively added to the anchor set if their Wald statistic indicated non-significant DIF until none remained. This approach resulted in between 7 and 11 anchor items per test form. To examine differential test functioning, we used the DTF function of the mirt R package (Chalmers, 2012), which utilizes the method described in Chalmers et al. (2016). Empirical 95% confidence intervals around the signed differential test functioning (sDTF) and unsigned differential test functioning (uDTF) statistics were constructed using 1000 plausible draws. As shown in Table 2, none of the test forms demonstrated significant uniform

DIF. The uDTF statistic did indicate statistically significant DIF, but its magnitude was small, with average effects of less than one point on the test. These results demonstrate that the practical impact of DTF (between -0.23 and 0.12 in test score points by form) was relatively small compared to the observed difference in scores between device types (3.20 in test score points). We also estimated the magnitude of DTF by form using the formula reported by Stark et al. (2004). These effect sizes ranged from $d_{DTF} = -0.02$ to 0.09 between forms. Therefore, we conclude that DTF is not a primary factor for the observed WPT-R score differences between device types in our sample.

PCI and RIASEC. In addition to the WPT-R, applicants also completed a 120-item self-report personality assessment (PCI; Barrick & Mount, 1993) based on the Five Factor Model of personality and a 58-item forced-choice measure of vocational interests (RIASEC; Wonderlic, 2019) based on the RIASEC model of occupational interests (Holland, 1996).

Researchers have consistently found that the effect of testing device on performance on non-cognitive assessments, like personality or interest inventories, is very small in magnitude across study settings (Traylor et al., 2021). Prior studies have also found evidence for measurement invariance for these non-cognitive assessments between device types (Illingworth et al., 2015; Morelli et al., 2014). Not only is there little evidence for device effects, personality or interest test scores are less strongly related to individual demographic or occupational differences relative to GMA test scores. Although some scholars have identified some degree of homogeneity of personality or vocational interest scores within occupations (King et al., 2017; Nye et al., 2018), these between-occupation effects are weaker relative to the stratification of GMA scores. Personality and interest tests also tend to yield only small to moderate demographic group mean score differences (Bobko & Roth, 2013; Roth et al., 2017).

We report the standardized mean differences in personality and vocational interest scores between mobile and non-mobile applicants in our operational sample in Table 3. Differences between device groups ranged from $d = -0.17$ to 0.20 among personality scales (average $d = -0.02$). Applicants who tested using a laptop or desktop computer scored higher on Openness ($d = 0.20$) and lower on Agreeableness ($d = -0.13$) and Stability ($d = -0.17$). We observed practically no differences in Conscientiousness ($d = -0.03$) or Extraversion scores ($d = 0.03$) between device groups. Among the interest scales (average $d = -0.12$ across all interest domains), non-mobile applicants scored higher on Artistic interests ($d = 0.11$) but lower on Realistic ($d = -0.23$), Social ($d = -0.24$), and Conventional interests ($d = -0.25$). We observed minimal differences in Investigative ($d = -0.02$) or Enterprising interests ($d = -0.08$). The aggregate effect size across both assessments (average $d = -0.07$) was similar in magnitude to the average effect sizes for noncognitive assessments reported by Traylor et al. (average $d = 0.02$).

Results

Post-Stratification Weights

Post-stratification weights were calculated based on the following variables: applicant gender, race/ethnicity, age, and educational attainment along with the specific O*NET job code for the posted position. Weights were derived such that the weighted proportions in the mobile group exactly matched those observed in the non-mobile group. Strata which were only present in one group but not the other were dropped to ensure maximal comparability of the groups. Due to differences in covariates between the mobile and non-mobile groups, 24,777 cases were dropped after applying post-stratification weights (32% of all observed cases). We report the demographic characteristics for the included participants in each group after post-stratification in

Table 4. These represent cases whose full demographic profile did not have any match in the opposite group. It is worth noting that even a single case with the same demographic profile in the opposite group was sufficient to retain all individuals with that profile. In cases where the proportional representations of those profiles differ, cases are weighted to equate those proportions.

As expected, the two groups did differ substantially on some demographic characteristics. For example, the cases removed from the mobile group had a much higher proportion of individuals identifying as Black/African American (22.1%) than those removed from the non-mobile group (12.9%). This is likely due to the high specificity of the demographic profiles used coupled with the relatively small sample sizes within the mobile group. In particular, the job code variable, which has 459 levels, is likely driving the majority of case removal. Regarding GMA between groups, those removed from the non-mobile sample were highly similar ($d = 0.01, p > .05, 95\% \text{ CI} = [-0.00, 0.03]$) while those removed from the mobile sample did score slightly lower on average than their retained counterparts, but this difference was still small in magnitude ($d = 0.24, p < .05, 95\% \text{ CI} = [0.21, 0.27]$).

Once the post-stratification weights are applied to these retained samples, the weighted demographic profiles of the two groups are exactly equal. Thus, our post-stratification estimates act to compare mobile test takers to a sample of non-mobile testers stratified to eliminate demographic and occupational differences between the two groups at the expense of losing representation of individuals who have no match between groups. The effect size after applying the post-stratification weights can be interpreted as the average device effect for the majority of applicants who tested using a non-mobile device. Statisticians define this estimate as the average treatment effect among the population or ATE (Austin, 2011). The ATE can be understood as the

expected effect size if device type was randomly assigned among a sample which is representative of the non-mobile applicants in our data.

Propensity Score Estimation

Propensity scores were estimated using generalized boosted regression as described by McCaffrey et al. (2004). We used the function “ps” as part of the twang R package to compute propensity scores, which allows for the exploration of both nonlinear and interactive effects amongst the chosen covariates in predicting the likelihood of receiving the treatment. Propensity scoring was performed using the variables outlined in Table 4 with the “ps” function set to use its default values. We found that these defaults, which prioritize the depth of the search and model effectiveness over calculation time, were sufficient to construct an optimal model. The overall performance of the model was examined by evaluating the balance plot provided in Figure 1. The balance of a solution is the mean standardized difference between groups in the prevalence of all covariates after propensity scores are applied. Completely balanced groups would have a balance score of zero, though this is rarely achievable in practice and instead must often simply be minimized. Adequate model performance is first examined by determining whether an optimal balance value is achieved within the models tested. This is suggested by a minima on the balance plot, which is achieved in our case at iteration 1681. Once a minima is found, the overall effectiveness of the model is evaluated by comparing how balanced the groups are on the covariates with and without propensity score weighting. While the unweighted data had an average balance of 0.03 across all variables, with a maximum single value of 0.53, the propensity-weighted data had an average balance of less than 0.01, with a maximum single value of only 0.08. This represents a substantial reduction in the difference between the mobile and nonmobile groups regarding the distribution of covariates. In examining the covariates

specifically, it was found that the two groups were better matched on 91% of the variables after propensity weighting, with no change observed on a further 2% of them. For those variables where more imbalance was observed after propensity weighting, the average absolute effect size change was only 0.005, suggesting a very minor impact.

Unlike the post-stratification approach, where we stratified mobile applicants to best match the characteristics of the non-mobile applicant sample to estimate the ATE, our propensity score weighting procedure was designed to estimate the average treatment effect on the evenly matchable units (ATM; Li & Greene, 2013) using formulas outlined in McGowan (2018). This procedure places greater weight on characteristics which are most closely represented in both groups rather than attempting to select mobile applicants who best match those in the non-mobile sample or vice versa. Therefore, this estimate is interpreted as the expected mobile effect among participants who were equally likely to use a mobile or non-mobile device for testing. This method resulted in retaining fewer overall participants (68%) as compared to those retained using post-stratification (79%).

Overall, the algorithm did a good job controlling for the covariates, as evidenced by the strong point-biserial correlation between propensity scores and use of a mobile device ($r_{pb} = .52$, $p < .05$) as well as a balance plot indicating a mean standardized effect size difference between the two groups on the covariates of less than 0.01 (see Figure 1). The utility of the ATM weights is demonstrated by Figure 2, which show the distribution of propensity scores before and after ATM weights were applied to the data. As shown, these distributions converge substantially after the weights are applied, indicating good balancing of the two groups on the covariates. While it may appear that these results were obtained without dropping any cases as occurred used post-stratification, this is not the case. The effective sample size of the two groups, which provides an

index of the power of any comparisons using propensity scores, indicates this. For the nonmobile group, the effective sample size was 46,710.5, a reduction of 6,716.5 cases. For the mobile group, the effective sample size was 14,065.26, a reduction of 9,455.74 cases, which exceeded the number of individuals removed from this group in post-stratification. Nonetheless, these values provide more than enough statistical power for estimating mean score differences between device types.

Device Effects after Propensity Weighting or Post-Stratification

In Hypothesis 1 we predicted that propensity scoring weighting or post-stratification would yield a smaller difference in GMA test scores between device types compared to the observed effect without attempting to control for selection bias. To test this hypothesis, we first calculated the differences in WPT-R scores between the two groups prior to using propensity score weights or post-stratification (Table 6). For the unweighted data, the non-mobile group (mean WPT-R = 23.5) scored higher than those on mobile devices (mean WPT-R = 20.2). This difference was significant and had a sizeable effect size ($t = 76.20$, $df = 1$, 47042.6 , $p < .05$, $d = 0.58$, 95% CI [0.56, 0.59]). After the propensity score weights were applied, the non-mobile test-takers (mean WPT-R = 22.0) still scored lower than those on mobile devices (mean WPT-R = 20.6), and this difference was still statistically significant though substantially weaker in magnitude ($t = 34.96$, $df = 1$, 76809.1 , $p < .05$, $d = 0.25$, 95% CI [0.23, 0.26]). This indicates a much smaller advantage for non-mobile testers among participants who were equally likely to choose a mobile or non-mobile device for completing their GMA test. The overall impact of the propensity weighting can be seen by comparing the plots before and after applying weights in Figure 2. When propensity score weights are used, the overall score difference is substantially reduced, but not entirely eliminated. Using post-stratification yielded an even weaker estimate

for the score differences between mobile (mean WPT-R = 20.5) and non-mobile groups (mean WPT-R = 21.1). Relative to the unweighted data, the effect of device type was nearly six times weaker after post-stratification ($d = 0.11$, 95% CI [0.10, 0.13]). Based on these results, we expect a much weaker difference in GMA scores due to device type after balancing demographic differences between mobile and non-mobile groups. This suggests an even smaller device effect as estimated using propensity score weighting. Both sets of results provide support for Hypothesis 1.

Predictors of Mobile Device Use in Selection Testing

In order to investigate RQ 1, we examined the contribution of each covariate to the overall propensity score. While the `ps` function used in R does not return variable importance indices, importance can be approximated by regressing the propensity score upon each covariate and examining the model R s, which are provided in Table 5. The most important predictors of mobile usage were the job being applied for ($R = .84$, $p < .001$) and educational attainment ($R = .71$, $p < .001$). Along these lines, we observed that only 28% of job applicants with a Bachelors' degree or greater used a mobile device when testing. In contrast, mobile device use was more common among applicants reporting some high school to some college (46%). We observed relatively weaker effects for ethnicity ($R = .29$, $p < .001$), and the State one resides in ($R = .27$, $p < .001$). Individual characteristics like personality and occupational interests also had less predictive value than the job itself and educational attainment. Among these traits, we observed the strongest effects for Conventional ($R = .21$, $p < .001$), Realistic ($R = .21$, $p < .001$), and Social interests ($R = .19$, $p < .001$) along with Openness to Experience ($R = .19$, $p < .001$). It is also worth noting that GMA scores were also substantially correlated with propensity scores ($r = .37$, $p < .001$) despite not being used in post-stratification or propensity score estimation.

Mobile Use by Occupation

The likelihood of using a mobile device to complete selection tests varied greatly based on job (e.g., O*NET codes) and demographic factors. In the prior section, we observed that individual job codes and applicant educational attainment were most strongly related to the expected likelihood of testing using a mobile device and we conducted a more descriptive analysis of this effect by identifying the occupations at the extremes of mobile test usage (Table 7). Mobile testing was most common among more conventional and realistic occupations, including janitors (70.4% mobile), laborers (66.8%), and plumbers (61.9%). Of the ten occupations with the greatest expected proportion of mobile test takers, all were classified as either O*NET Job Zone 2 or 3. In contrast, relatively few mobile applicants were observed for jobs requiring high levels of formal education or technical expertise, including computer programmers (3.6%) and management analysts (7.2%). Besides one exception (web developers), these occupations are categorized as requiring considerable or extensive preparation (O*NET Job Zones 4 or 5). Based on these findings, job requirement data from O*NET could be helpful to practitioners for estimating the rate of mobile test use for a specific job posting and identifying when to prioritize mobile-friendly assessments for selection test batteries.

Given the variability in mobile use between occupations, we further explored whether observed device effects were any smaller when calculated within-occupations. We calculated the within-occupation, standardized mean GMA score differences between device groups for occupations where we had at least 100 applicants and at least 30 in each device group. This resulted in a sample of 97 occupations aggregated from a total of 63,134 individual applicants (82% of entire sample). Effect sizes still indicated higher scores for non-mobile test takers, but they varied greatly between occupations, ranging from $d = -0.15$ to 0.89. Nearly all effect sizes

were weaker relative to the sample-weighted average reported in previous observational studies ($d = 0.79$; Traylor et al., 2021). This indicates that within-occupation effects may be weaker than those observed in more heterogeneous samples, but selection bias may still be confounding these within-occupation comparisons. We also illustrate the correlation between the proportion of mobile test takers and within-occupation mean differences in Figure 4. Based on this plot, there does not appear to be a simple linear relationship between mobile use and the observed score differences between device types in our sample ($r = -.14, p > .05$). Focusing only on applicants within a given occupation helps reduce some confounding due to self-selection but our data suggest that these estimates are likely inflated due to the estimated device effects after balancing groups using either propensity score weighting or post-stratification.

Discussion

The practice of pre-employment testing has rapidly transformed in the 21st century. Many job applicants expect to access pre-employment test platforms using their own internet enabled device and the autonomy to determine when and where to complete their tests (Tippins, 2009). These changes have made it easier for hiring organizations to administer tests for selection use and has potentially increased the size and breadth of applicant pools (Woods et al., 2020). However, the adoption of unproctored, internet-based testing has also reduced the level of standardization in the testing process (Naglieri et al., 2004). This means that a greater number of contextual factors can potentially affect an applicant's test performance including the time of day, the surrounding test environment, the applicant's comfort with technology, and the characteristics of the testing device. In response, researchers have focused on studying whether the psychometric properties of cognitive and non-cognitive assessments are equivalent across

mobile and non-mobile devices. This literature has consistently found little to no mobile device effect for non-cognitive test scores but strong device effects on cognitive test scores in operational contexts (Traylor et al., 2021).

We investigated whether the divergent effects of mobile device use on GMA test scores in operational and non-operational samples can be explained by self-selection of testing device. Prior to using post-stratification or propensity score weighting, non-mobile applicants scored 0.58 standard deviations higher on the WPT-R compared to mobile applicants. This effect was slightly smaller than the average, sample-weighted effect size from prior operational studies ($d = 0.79$; Traylor et al., 2021) but indicated a sizeable disadvantage for mobile device users as also observed in other, operational samples (e.g., Wood et al., 2015). After controlling for selection bias using propensity score weights, however, our estimate for the device effect shrunk by over 50% ($d = 0.25$). This estimate represents the expected population effect of mobile device use for all applicants who could be matched across device conditions (ATM). The mean score difference between mobile and non-mobile users was even smaller after using post-stratification to estimate the average treatment effect among the population ($d = 0.11$). This estimate suggests that if we had randomly assigned devices for testing among a sample of applicants like those in the non-mobile condition, we would have found little to no device effect on WPT scores. These results suggest that most of the uncorrected, observed device effect may be attributed to individuals who could not be readily matched across device conditions. We also observed that only very small test score differences ($d_{DTF} < 0.10$ across forms) could be attributed to differential functioning between device types compared to the differences explained by applicant demographics or occupation. Therefore, we conclude that operational samples like ours likely overestimate the effect of mobile device use on GMA scores in selection settings.

Both estimates after post-stratification and propensity score weighting were closer to the effects reported in non-operational samples where mobile device use was manipulated by researcher assignment (e.g., Brown & Grossenbacher, 2017; Grelle & Gutierrez, 2019; King et al., 2015). However, we also removed between 21-32% of cases when attempting to control for selection bias. Much of this is likely due to the pervasive differences among applicants choosing to test using a mobile or non-mobile device. For example, applicants with a Bachelors' degree or greater were greatly undersampled within the mobile device group (20% of mobile applicants compared to 53% of non-mobile applicants). Likewise, higher rates of mobile use among applicants were found among less complex jobs (as indicated by O*NET job zones). This indicates that much of the uncorrected effect size may be explained by ability differences between applicants resulting from confounding variables related to both device choice and GMA test performance. In contrast, Brown and Grossenbacher (2017) observed no mobile device effect within a sample characterized by an equal rate of high educational attainment (55% with a Bachelors' degree or higher) as found in the non-mobile applicant sample in the present study (53%). Our findings support our self-selection bias hypothesis as an explanatory mechanism for the discrepant findings observed in prior operational and non-operational studies. Differences in GMA test scores between mobile and non-mobile users in prior, operational samples appear likely to be inflated because of non-equivalent device groups (e.g., individual differences in device choice) rather than evidence for a systematic disadvantage of using a mobile device to complete an ability test in operational settings. Our findings converge with other comparisons between treatment effects estimated from experimental studies and observational studies after applying statistical controls like propensity score weighting. For example, Gordon et al. (2019) recently found that effect sizes from observational studies still greatly overestimated the

treatment effects of marketing campaigns as compared to effects found in randomized control trials. These and our results suggest that these statistical procedures can help address confounding factors within observational data, but experimental methods remain more effective for causal inference.

These findings are important given the growing proportion of mobile device use for testing among job applicants. The overall rate of mobile device usage in our operational sample (30.6%) was 15 times greater than estimates reported by Arthur et al. (approximately 2% mobile users; 2014) only six years ago and nearly double the rate observed by Lawrence et al. three years ago (approximately 16% mobile users; 2017). Not only are mobile-friendly assessments attractive to hiring organizations, but they are also reported to yield more favorable applicant reactions and more diverse applicant pools (Woods et al., 2020). More generally, mobile-friendly UIT programs may increase applicant pools by providing access to assessments for a wider range of devices (Auer et al., 2022). Mobile cognitive assessments are also becoming more widely used in education and other areas of psychological research (e.g., Nikou & Economides, 2018; Pahor et al., 2019; Sliwinski et al., 2018). Researchers and test developers will need to ensure that the measurement properties of new assessments are equivalent across testing device types to meet the growing demand for mobile-friendly assessments (Morelli et al., 2017). To this end, the use of random assignment is essential for determining whether a new or revised assessment is suitable for UIT in selection contexts where applicants may test using mobile or non-mobile devices. Operational testing data, while often providing great statistical power and ecological validity, appears likely to confound device effects with individual or occupational differences which may be endogenous to differences in GMA test scores.

Implications for Research and Practice

The findings from our study have important implications for future use of internet-based GMA tests in personnel selection and beyond. As mobile device ownership becomes more prevalent relative to desktop or laptop computers, mobile-friendly assessments are vital for providing equal access for all potential job applicants. Our results suggest that applicants do not necessarily perform worse on GMA tests when administered on mobile devices at the degree to which the uncorrected, observed scores suggest. Not only does this indicate that ability tests can be administered fairly across device types for preemployment testing, but this also suggests that mobile ability testing is potentially feasible in other contexts beyond personnel selection. In response to the COVID-19 pandemic, for example, some scholars have begun discussing whether admissions testing should shift from centralized, on-site testing to remote or at-home testing (Camara, 2020). The pandemic has also accelerated the demand for online, remote cognitive testing in the research and practice of clinical psychology (Türközer & Öngür, 2020). Therefore, the development of ability tests which provide comparable scores across device types is highly relevant for testing in educational and clinical settings as well as in selection testing. Our research indicates that a careful triangulation of experimental studies and observational field studies is necessary to establish whether ability tests provide equivalent measurement across device types.

The need for this research will only grow in importance given that individuals are increasingly more likely to access the internet via smartphone or other mobile devices rather than traditional desktop or laptop computers (Pew, 2021). Ideally, candidates would be able to test using the technology which is most accessible based on their needs while also producing comparable test scores. Mobile-friendly assessments may enable organizations to reach a more diverse population of applicants and make the application process more accessible to candidates

as a result. This may be especially important for entry-level positions given that we observed the highest rates of mobile test use among applicants whose highest educational attainment at the time was some college experience or a high school degree. However, further research is needed to observe how the implementation of mobile testing affects accessibility in organizational or educational practice. This research could also help practitioners introduce mobile assessment into fields which have been reluctant to adopt non-proctored, internet-based testing like in admissions testing or for hiring in the public sector (Nesnidol & Highhouse, 2018).

Our findings highlight the importance of random assignment and we echo prior calls to make greater use of experimental methods in applied psychological research (e.g., Aguinis & Edwards, 2014). Although random assignment is necessary for determining causal relationships, experimental research is sometimes thought to be unimportant for informing organizational practice due to the use of artificial environment conditions (e.g., Stone-Romero, 2002). Not only are operational samples assumed to yield greater ecological validity (Highhouse, 2009), these samples can often be much larger than those obtained in laboratory or experimental research (Arthur et al., 2018; Shen et al., 2011). This provides researchers with ample statistical power to test a wide variety of research questions and conduct highly complex statistical analyses. However, sheer sample size does not make a study immune to threats to internal validity nor does it guarantee that results will generalize to other samples. We observed that self-selection created biased estimates of testing device effects in a sample of over 76,000 job applicants. After using statistical methods to minimize selection bias, our results converged with findings from non-operational, experimental or quasi-experimental studies which tend to detect practically no practical difference in test scores between devices (e.g., Brown & Grossenbacher, 2017; Grelle & Gutierrez, 2019; Traylor et al., 2021). The magnitude of our effects after adjustments were

also similar to the uncorrected device effect sizes which we observed for personality and vocational interest scores in our sample.

We strongly recommend that future research should triangulate the advantages of controlled, experimental studies with the ecological advantages of large-scale observational field studies as articulated by Lin and colleagues (2021). Ideally, researchers should conduct experimental studies to ensure that a new or adapted test functions similarly across mobile and non-mobile devices. This is akin to the use of random equivalent groups in educational testing in order to ensure test score comparability between devices (Camara & Harris, 2020). Well-powered, experimental research can provide initial evidence for a true device effect independent of self-selection or other potential confounds. To this end, Highhouse and Zhang (2015) recommend the use of MTurk (or other crowdsourcing services) as a convenient means for conducting more experimental research in applied psychology. However, we acknowledge that MTurk samples are not inherently free of selection bias. Chueng et al. (2017) note that MTurk workers self-select into working on the site and actively choose among a wide variety of tasks. Each of these steps may influence the characteristics of who participates in a given study. For test development and validation research, this may result in samples where individuals who enjoy taking tests are over-represented, compared to the population of job applicants who are likely to take the test in a selection context. Brown and Grossenbacher (2017) also noted that MTurk participants scored higher relative to the population norms for the WPT. The effect of these selection biases may limit the generalizability of findings based solely on crowdsourced samples, however, the internal validity provided by random assignment may still outweigh these external validity concerns.

Once a test is in use and operational data are available, we strongly suggest using propensity scores or post-stratification when conducting research post hoc. Presently, pre-employment testing is commonly conducted in non-proctored settings where individuals are free to choose when, where, and how (i.e., the device) they complete selection tests (Tippins et al., 2006; Woods et al., 2020). As a result, attempts to study whether these environmental differences affect the measurement properties of tests using operational data are likely confounded by self-selection. Weighting using propensity scores or post-stratification are both promising methods for minimizing the impact of selection bias for conducting research using operational data. This approach has also been recommended for assessing the comparability of educational test scores between device types (Dadey et al., 2018). To be clear, we are not suggesting that any form of weighting or stratification be used for operational use or scoring of applicants. We suggest that our recommended approaches can be used to conduct a sensitivity analysis to quantify the potential impact of selection bias as indicated by a change in effect size after including propensity score or post-stratification weights. We have demonstrated the usefulness of these methods when studying selection testing, but these methods can also be applied to a variety of organizational and management research topics. For example, Schmidt and Pohler (2018) used propensity scores to control for selection bias when studying the effects of high-performance work systems on employee satisfaction. Kulas and colleagues (2018) provide a tutorial for how to use post-stratification for human resource management research and practice. We recommend that industrial-organizational psychology researchers and practitioners learn more about these methods to help balance threats to internal and external validity when using real-world, operational or organizational data. These complimentary approaches would not only help

researchers make stronger causal claims but also contribute to the broader effort to improve the reproducibility of research findings (e.g., Munafo et al., 2017).

Limitations

There are several limitations which should be considered when drawing conclusions from the results of our study. For one, it is not certain how applicant use of mobile devices for completing pre-hire assessments will continue to evolve over time. Current trends suggest that mobile use for pre-employment testing will continue to grow which could minimize the impact of selection bias. Yet, we expect that mobile test taking will not be equally adopted across all employment sectors or applicant demographic groups as indicated by the wide range of mobile usage across occupations with at least 200 applicants (between 2.8% and 79.6%; Table 6). Not only is more research needed to determine the predictors of device choice but it is also possible that the predictive nature of these factors may change over time as mobile testing becomes more commonplace. Researchers and practitioners should continue to monitor mobile test usage in practice to observe whether these relationships change as technologies and their use evolve over time. In addition, our study focused on a GMA test which was optimized for use on mobile devices and has shown to not disadvantage mobile test takers in a prior experimental study (Brown & Grossenbacher, 2017). Our findings support those prior results but may not generalize to different forms of ability tests which have not been designed for mobile administration. Research suggests that “mobile-first” approaches to test development may help produce ability tests which are invariant between testing device types (e.g., Grelle & Gutierrez, 2019).

Conclusions

We conducted the present study to examine whether controlling for selection bias reduces GMA test score differences between applicants using mobile and non-mobile devices. Before

applying any statistical controls, mobile applicants scored over 0.58 standard deviations lower on GMA tests compared to applicants who used a non-mobile device. These results mirror those observed in other observational studies (mean $d = 0.79$ reported by Traylor et al., 2021). However, both propensity score weighting and post-stratification dramatically reduced the differences in test scores between device types. These results suggest that operational estimates of device effects on GMA test scores can be inflated by a self-selection effect in device choice. Consequently, it is vital that researchers use random assignment when testing for the equivalence of mobile and non-mobile forms of new or revised selection tests. This methodology will help ensure that there is little to no true device effect on test scores or the psychometric properties of the test. Otherwise, our results indicate that observational data from job applicants are susceptible to selection bias. We recommend using propensity score weighting or post-stratification to help balance device groups when investigating for test score differences in non-experimental, operational studies.

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Table 1.
Sample Demographics by Device Choice

	Non-Mobile (n = 53,427)		Mobile (n = 23,521)	
	n	%	n	%
<i>Gender</i>				
Male	26,244	49.1%	9,498	40.4%
Female	26,055	48.8%	13,752	58.5%
Non-Binary/Third Gender	158	0.3%	53	0.2%
I Prefer Not to Answer	970	1.8%	218	0.9%
<i>Race/Ethnicity</i>				
Asian/Pacific Islander	5,585	10.5%	991	4.2%
Black/African American	6,654	12.5%	4,438	18.9%
Hispanic	5,762	10.8%	3,515	14.9%
Native American	240	0.4%	173	0.7%
White/Caucasian	30,491	57.1%	12,325	52.4%
Two or More Races	2,409	4.5%	1,379	5.9%
I Prefer Not to Answer	2,286	4.3%	700	3.0%
Asian/Pacific Islander	5,585	10.5%	991	4.2%
<i>Age</i>				
Less Than 18	114	0.2%	154	0.7%
18 - 29	20,990	39.3%	10,048	42.7%
30 - 39	12,878	24.1%	6,283	26.7%
40 - 49	8,651	16.2%	3,927	16.7%
50 - 59	6,230	11.7%	2,107	9.0%
60+	1,727	3.2%	452	1.9%
I Prefer Not to Answer	2,837	5.3%	550	2.3%
<i>Educational Attainment</i>				
Some High School	358	0.7%	560	2.4%
High School Diploma	4,938	9.2%	5,847	24.9%
GED	811	1.5%	1,256	5.3%
Some College	12,431	23.3%	8,125	34.5%
Associate Degree	5,525	10.3%	2,829	12.0%
Bachelor's Degree	21,971	41.1%	3,796	16.1%
Graduate Degree	6,385	12.0%	826	3.5%
I Prefer Not to Answer	1,008	1.9%	282	1.2%

Table 2.

Results of WPT-R Differential Test Functioning Analyses

Form	<i>n</i>	α	sDTF		uDTF		d_{DTF}
			Score	% Score	Score	% Score	
E8	19,106	.91	0.17	0.35 [−0.15, 0.63]	0.94	1.88 [1.52, 2.78]	0.09
E9	19,347	.92	0.23	−0.45 [−0.10, 0.54]	0.71	1.41 [0.47, 0.99]	0.01
E10	19,250	.91	−0.12	0.24 [−0.49, 0.28]	0.83	1.67 [0.57, 1.26]	−0.02
E11	19,245	.91	0.23	0.46 [−0.08, 0.61]	0.72	1.44 [0.48, 1.05]	0.04

Note. 95% confidence intervals for each percent score difference are reported in brackets; sDTF is the signed differential test functioning statistic; uDTF is the unsigned differential test functioning statistic; d_{DTF} is the standardized expected total test score difference; % Score is the DTF statistic as a percentage of the overall WPT-R score range.

Table 3.

Descriptive Statistics of RIASEC and Personality Measures by Device Type

	Non-Mobile (<i>n</i> = 53,427)		Mobile (<i>n</i> = 23,521)			
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>d</i>	<i>α</i>
<i>RIASEC</i>						
Realistic	−0.16	0.69	0.00	0.74	−0.23 [−0.24, −0.21]	.92
Investigative	0.58	0.66	0.59	0.61	−0.02 [−0.03, 0.00]	.93
Artistic	0.08	0.61	0.01	0.60	0.12 [0.10, 0.13]	.94
Social	0.29	0.58	0.43	0.57	−0.24 [−0.26, −0.23]	.95
Enterprising	0.50	0.49	0.54	0.47	−0.08 [−0.10, −0.07]	.94
Conventional	0.27	0.64	0.43	0.60	−0.25 [−0.27, −0.24]	.96
<i>Personality</i>						
Agreeableness	55.62	4.46	56.16	3.94	−0.13 [−0.14, −0.11]	.82
Conscientiousness	84.75	4.57	84.87	4.17	−0.03 [−0.04, −0.01]	.87
Extraversion	70.85	8.45	70.61	7.63	0.03 [0.01, 0.04]	.86
Openness	50.43	4.98	49.45	4.74	0.20 [0.18, 0.22]	.83
Stability	52.94	6.43	53.98	5.71	−0.17 [−0.18, −0.15]	.86

Note. 95% confidence intervals for each standardized mean difference (Cohen's *d*) are reported in brackets; RIASEC interest scores are reported as theta values (population mean = 0.00); Reliability estimates for each RIASEC scale are reported from the test manual (Wonderlic, Inc., 2019); Personality scale scores ranged from 20 to 60 for Agreeableness, Openness, and Stability; Extraversion and Conscientiousness scores ranged from 30 to 90.

Table 4.
Sample Demographics after Post-Stratification

	Non-Mobile (<i>n</i> = 33,703)		Mobile (<i>n</i> = 18,466)	
	<i>n</i>	%	<i>n</i>	%
<i>Gender</i>				
Male	15,277	45.3%	7,088	38.4%
Female	18,050	53.6%	11,263	61.0%
Non-Binary/Third Gender	5	>0.1%	2	>0.1%
I Prefer Not to Answer	353	1.0%	116	0.6%
<i>Race/Ethnicity</i>				
Asian/Pacific Islander	2,799	8.3%	704	3.8%
Black/African American	4,106	12.2%	3,321	18.0%
Hispanic	3,180	9.4%	2,631	14.2%
Native American	24	0.1%	17	0.1%
White/Caucasian	21,911	65.0%	10,686	57.9%
Two or More Races	1010	3.0%	797	4.3%
I Prefer Not to Answer	353	1.0%	116	0.6%
<i>Age</i>				
Less Than 18	54	0.2%	83	0.4%
18 - 29	14,931	44.3%	8,457	45.8%
30 - 39	8,462	25.1%	4,858	26.3%
40 - 49	5,185	15.4%	2,927	15.9%
50 - 59	3,615	10.7%	1,559	8.4%
60+	602	1.8%	278	1.5%
I Prefer Not to Answer	353	1.0%	116	0.6%
<i>Educational Attainment</i>				
Some High School	156	0.5%	245	1.3%
High School Diploma	3,594	10.7%	4,649	25.2%
GED	410	1.2%	613	3.3%
Some College	9,209	27.3%	6,878	37.2%
Associate Degree	3,259	9.7%	2,119	11.5%
Bachelor's Degree	14,537	43.1%	3,293	17.8%
Graduate Degree	2,185	6.5%	553	3.0%
I Prefer Not to Answer	353	1.0%	116	0.6%

Note. Demographic frequencies correspond to the cases retained after post-stratification; Any observed differences in proportions between mobile and non-mobile groups were minimized by post-stratification weights.

Table 5.

Covariates Used for Propensity Score Weighting and their Importance

Covariate	Levels	Multiple R
<i>Applicant Demographics</i>		
Age	7	.18
Gender	4	.18
Race/Ethnicity	7	.29
Educational Attainment	8	.71
Location	54	.27
<i>Personality and Interests</i>		
Agreeableness	1	.12
Conscientiousness	1	.05
Extraversion	1	.04
Openness	1	.19
Stability	1	.13
Realistic	1	.21
Investigative	1	.02
Artistic	1	.10
Social	1	.19
Enterprising	1	.07
Conventional	1	.21
<i>Job Characteristics</i>		
O*NET Code	459	.84

Note. All five personality traits were measured using the PCI (Personality Characteristics Inventory); Job complexity was measured using the O*NET job zone categories; Multiple R = based on regressing propensity scores on the specified covariate; All p values < .001

Table 6.

General Mental Ability Score Differences between Mobile and Non-Mobile Test Takers

	Non-Mobile			Mobile			<i>d</i>
	<i>M</i>	<i>SD</i>	<i>n</i>	<i>M</i>	<i>SD</i>	<i>n</i>	
All observed cases	23.50	5.85	53,427	20.20	5.57	23,521	0.58 [0.56, 0.59]
After propensity score weighting ^a	22.00	5.73	46,711	20.60	5.57	14,065	0.25 [0.23, 0.26]
After post-stratification	21.11	5.73	33,703	20.47	5.54	18,466	0.11 [0.10, 0.13]

Note. 95% confidence intervals for each standardized mean difference (Cohen's *d*) are reported in brackets. ^a Sample sizes after applying propensity score weights are estimated based on weights and rounded to whole numbers.

Table 7.
Jobs Most and Least Likely to Have Mobile Applicants

O*NET Code	Title	% Mobile	Mean Propensity Score	Job Zone
<i>Highest Mean Propensity Scores</i>				
29-2081.00	Opticians, Dispensing	79.6%	0.77	3
37-2011.00	Janitors and Cleaners	70.4%	0.70	2
29-2057.00	Ophthalmic Medical Technicians	69.9%	0.69	3
53-7062.00	Laborers and Freight, Stock, and Material Movers, Hand	66.8%	0.66	2
31-9092.00	Medical Assistants	63.3%	0.63	3
47-2152.02	Plumbers	61.9%	0.62	3
51-9198.00	Helpers--Production Workers	61.5%	0.61	2
51-4011.00	Computer-Controlled Machine Tool Operators	61.4%	0.61	3
43-4171.00	Receptionists and Information Clerks	61.0%	0.61	2
43-6013.00	Medical Secretaries	60.2%	0.60	3
<i>Lowest Mean Propensity Scores</i>				
15-1132.00	Software Developers, Applications	7.4%	0.08	4
27-1024.00	Graphic Designers	6.7%	0.07	4
13-1111.00	Management Analysts	7.2%	0.07	5
15-1199.10	Search Marketing Strategists	6.1%	0.07	4
15-1199.09	Information Technology Project Managers	5.1%	0.06	4
15-1199.08	Business Intelligence Analysts	4.4%	0.05	4
15-1134.00	Web Developers	3.3%	0.05	3
15-1131.00	Computer Programmers	3.6%	0.05	4
13-2051.00	Financial Analysts	2.8%	0.05	4
17-2199.05	Mechatronics Engineers	3.3%	0.05	4

Note. Only jobs with more than 200 applicants were included in this table; % Mobile = observed proportion of applicants who tested using a mobile device

Figure 1. Balance statistics of boosted regression analysis.

Figure 2. Distribution of propensity scores by mobile usage before and after weighting.

Figure 3. Unweighted and weighted WPT-R score distribution between device groups.

Figure 4. Within-occupation standardized mean differences in WPT-R scores between device groups by the proportion of mobile test takers.