External Stability Auditing to Test the Validity of Personality Prediction in AI Hiring

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Abstract Automated hiring systems are among the fastest-developing of all high-stakes AI systems. Among these are algorithmic personality tests that use insights from psychometric testing, and promise to surface personality traits indicative of future success based on job seekers’ resumes or social media profiles. We interrogate the validity of such systems using stability of the outputs they produce, noting that reliability is a necessary, but not a sufficient,
condition for validity. Our approach is to (a) develop a methodology for an external audit of stability of predictions made by algorithmic personality tests, and (b) instantiate this methodology in an audit of two systems, Humantic AI and Crystal. Crucially, rather than challenging or affirming the assumptions made in psychometric testing — that personality is a meaningful and measurable construct, and that personality traits are indicative of future success on the job — we frame our methodology around testing the underlying assumptions made by the vendors of the algorithmic personality tests themselves.

In our audit of Humantic AI and Crystal, we find that both systems show substantial instability with respect to key facets of measurement, and so cannot be considered valid testing instruments. For example, Crystal frequently computes different personality scores if the same resume is given in PDF vs. in raw text format, violating the assumption that the output of an algorithmic personality test is stable across job-irrelevant variations in the input. Among other notable findings is evidence of persistent — and often incorrect — data linkage by Humantic AI.

Keywords: Algorithm Audit · Validity · Stability · Reliability · Hiring · Personality

1 Introduction

AI-based automated hiring systems are seeing ever broader use and have become as varied as the traditional hiring practices they augment or replace. These systems include candidate sourcing and resume screening to help employers identify promising applicants, video and voice analysis to facilitate the interview process, and algorithmic personality assessments that purport to surface personality traits indicative of future success. HireVue, a company that sells one of these systems, estimates that the “pre-hire assessment” market is worth $3 billion annually [33]. Indeed, most Fortune 500 companies are using some form of algorithmic hiring [1]. Ian Siegel, the CEO of ZipRecruiter (a popular online employment marketplace), estimates that 75%-100% of all submitted resumes are now read by software, and that only a small fraction of those go on to be read by humans [1].

In this paper, we focus on automated pre-hire assessment systems, as some of the fastest-developing of all high-stakes uses of AI [33]. The popularity of automated hiring systems in general, and of pre-hire assessment in particular, is due in no small part to the hiring sector’s collective quest for efficiency. Employers choose to use them to source and screen candidates faster and with less paperwork and, in a world reshaped by the COVID-19 pandemic, with as little in-person contact as is practical. Job seekers are, in turn, promised a more streamlined job search experience, although they rarely have a choice in whether they are screened by an automated system, and they are typically not notified when algorithmic screening is used [65]. The flip side of efficiency potentially afforded by automation is that job seekers, the general public, and even employers themselves rarely understand how these systems work and, indeed, whether they work. Is a resume screener identifying promising candidates or is it picking up irrelevant — or even discriminatory — patterns from historical data, potentially exposing the employer to legal liability? Are job seekers participating in a fair competition if they are systematically unable to pass an online personality test, despite being well-qualified for the job [72]?

Personnel selection is an especially sensitive, high-stakes application of AI. Hiring decisions are often of great consequence to candidates’ financial and emotional well-being [6], and in aggregate contribute to widespread economic inequality [8,50]. Consequences for hiring organizations can be substantial as well: if their selection procedures are arbitrary or
unfair, they risk litigation and class action lawsuits. As such, any algorithms deployed in the field of hiring deserve rigorous scrutiny.

Reports of algorithmic hiring systems acting in ways that are discriminatory or unreliable abound [4,9,16,17,23,64]. In a recent example, when testing automated phone interview software, Hilke Schellmann found that the system produced “English competency” scores even when the candidate spoke exclusively in German or Chinese [55]. This finding undermines the validity of the tool, and crystallizes the fact that black-box algorithms may not act as we expect them to.

In our work we interrogate the validity of algorithmic pre-hiring assessment systems of a particular kind: those that purport to estimate a job seeker’s personality based on their resume or social media profile. Our focus on these systems is warranted both because the science behind personality testing (algorithmic or not) in hiring is controversial [19,38,62], and because algorithmic personality tests are rarely validated by third-parties [1]. Warning against this trend, Chamorro-Premuzic et al. [14] write in the Journal of Industrial and Organizational Psychology: “shiny new talent identification objects often bamboozle recruiters and talent acquisition professionals with no regard for predictive validity.” Despite this warning, unvalidated use of these “objects” continues. For example, as we will discuss in Section 4.1, DiSC, a psychometric instrument used by several algorithmic personality assessment systems, has not been validated in the hiring domain, and the company that produces DiSC specifically warns against using it for pre-employment screening.

Our approach is to (1) develop a methodology for an external audit of stability of predictions made by algorithmic personality tests, and (2) instantiate this methodology in an audit of two systems, Humantic AI and Crystal. These systems were selected as audit subjects because they each produce quantitative personality traits as output, accept easily-manipulated textual features as input, and allow multiple input types: both systems accept resumes and LinkedIn profiles, and Humantic AI additionally accepts Twitter profiles. These systems also have substantial presence in the algorithmic hiring market: Humantic AI reports that it is used by Apple, PayPal and McKinsey [1] and Crystal claims that 90% of Fortune 500 companies use their products, though neither company distinguishes between use for hiring and use for other purposes, such as sales [2].

Stability is closely related to the psychometric concept of reliability, which is a prerequisite of validity (see Section 2.1 for details). Crucially, based on the insights of Sloane et al. [63], we frame our methodology around testing the underlying assumptions made by the vendors of the algorithmic personality tests themselves.

In this paper, we make the following contributions:

1. We provide an overview of the key literature on psychometric testing applied to hiring (Section 2.1), and on algorithm auditing with a particular focus on hiring (Section 2.2). We find that reliability is seen as a crucial aspect of the validity of a psychometric instrument, yet it has not received substantial treatment in algorithm audits.

2. We develop a quantitative methodology, informed by psychometric theory and sociology, to audit the stability of black-box algorithms that predict personality for use in hiring (Section 3). Figure 1 gives an overview of our proposed methodology.

3. We instantiate this methodology by conducting an external stability audit of two broadly-used systems, Humantic AI and Crystal, over a dataset of job applicant profiles collected through an IRB-approved study (Section 4).

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[1] https://humantic.ai/
[2] https://www.crystalknows.com/
We find that both systems show substantial instability with respect to important facets of measurement. For example, personality profiles returned by both Humantic AI and Crystal are substantially different depending on whether they were computed based on a resume or a LinkedIn profile, violating the assumption that an algorithmic personality test is stable across input sources that are treated as interchangeable by the vendor. Further, Crystal frequently computes different personality scores if the same resume is given in PDF vs. in raw text format, violating the assumption that the output of an algorithmic personality test is stable across job-irrelevant variations in the input. We also found that Humantic AI creates a persistent (and sometimes incorrect!) linkage between an email address and a LinkedIn URL that appear in a resume, and then silently disregards resume information when computing the personality score.

We discuss the results and limitation of our work in Section 5 and conclude in Section 6.

2 Background and Related Work

2.1 Validity and Reliability in Psychometric Theory Applied to Hiring

Personality testing in hiring. Since the early 1900s, personnel selection practices have relied on the use of psychometric instruments such as personality tests to identify promising candidates [59], and the use of these tests continues to be wide-spread [39]. And although this practice is both longstanding and wide-spread, it has been met with skepticism from industrial-organizational (I-O) psychologists due to validity and reliability concerns, and even led to disagreements about whether personality itself is a meaningful and measurable
construct [59]. A comprehensive literature review of personality testing in personnel selection published in 1965 found little evidence of predictive validity, and concluded that “it is difficult to advocate, with a clear conscience, the use of personality measures in most situations as a basis for making employment decisions” [26]. Several other surveys would come to the same conclusion in the following decades [31, 57], yet, HR professionals continued to use personality testing for hiring [59]. The rise of the “Big Five” model of personality in the 1990s led to wider acceptance of personality testing in hiring amongst I-O psychologists, albeit not without controversy. (See Section 4.1 for more on the Big Five.)

The use of a traditional personality test in personnel selection relies on the following assumptions:

- the personality traits being measured are meaningful constructs;
- the test is a valid measurement instrument: it measures the traits it purports to measure;
- the test is a valid hiring instrument: its results are predictive of employee performance.

Validity and reliability of psychometric instruments. Within the field of psychometrics, instruments are considered useful only if they are both reliable and valid [12, 13]. Reliability refers to the consistency of an instrument’s measurements, and validity is the extent to which the instrument measures what it purports to measure [43]. Reliability is a necessary (although not a sufficient) condition for validity [45]. Thus, when considering psychometric instruments, the question of reliability is central to the question of validity.

Reliability can be measured across time (test-retest reliability), across equivalent forms of a test (parallel forms reliability), across testing environment (cross-situational consistency), etc. (Mueller and Knapp 2018). Each of the dimensions across which measurements are compared is referred to as a “facet,” such that we can talk about reliability with respect to some facet (e.g., time) that varies between measurements, while other facets (e.g., test location) are held constant [12]. Under Classical Test Theory (CTT), measurements can be decomposed into a true score and a measurement error [56]. The true score is the value of the underlying construct of interest (e.g., extraversion). Measurement error can be further broken down across various experiment facets [56].

Reliability is usually measured and evaluated with correlations. Although 0.80 is often cited as an acceptable threshold of reliability, Nunnally and Bernstein [45] differentiate between standards used to compare groups (for which 0.80 is an appropriate reliability), and those used to make decisions about individuals. For the latter type of test, they advise that 0.90 should be the “bare minimum,” and that 0.95 should be the “desirable standard.”

Algorithmic personality tests, on which we focus in this paper, constitute a category of psychometric instruments, and are thus relying on the same assumptions—about test validity as a measurement instrument and as a hiring instrument—as do their traditional counterparts. Guzzo et al. [27] caution that reliability and validity are “often overlooked yet critically important” in big-data applications of I-O psychology. In our work, we aim to fill this gap by interrogating the reliability of algorithmic personality predictors. Because the objects of our study are algorithmic systems that are used by employers in their talent acquisition pipelines, our work falls within the domain of hiring algorithm audits, discussed next.

2.2 Auditing of Hiring Algorithms

Background on algorithm auditing. The algorithm audit is a crucial mechanism for ensuring that AI-supported decisions are fair, safe, ethical, and correct. Increasing demand for
such audits has led to the emergence of a new industry, termed Auditing and Assurance of
Algorithms by Koshiyama et al. [35].

Scholarly work on algorithm auditing acknowledges that auditing frameworks are in-
consistent in terms of scope, methodology, and evaluation metrics [4,10,35,49]. In this
landscape that offers many frameworks, yet minimal technical guidance, auditors are left
to define their own scope. As argued by several authors, stakeholder interests should be cen-
tral to the task of scoping [10,22,40,47,48,50,63]. Sloane et al. [63] argue that audits
ought to be specific to the domain and to the tool under study.

Much of the audit literature surrounding predictive hiring technology is significantly
concerned with legal liability as laid out in the Uniform Guidelines on Employee Selection
Procedures (UGESP) [34,48,73]. These guidelines, adopted by the Equal Employment Op-
portunity Commission in 1978 [20], revolve around a form of discrimination called disparate
impact, wherein a practice adversely affects a protected group of people at higher rates than
privileged groups. As a result, audits of AI hiring systems are often specifically concerned
with adverse impact [15,47,73]. It is often noted that avoiding liability is not actually suffi-
cient to ensure an ethical system; that is, a lack of adverse impact should be a baseline rather
than the goal [5,27,48,73].

One of the contributions of our work is an audit framework specific to personality pre-
prediction systems used in the hiring domain, and a technical instantiation of this framework
for two candidate screening systems. As we will discuss in Section 3, we build on Sloane
et al. [63] to interrogate the assumptions encoded by the systems.

Treatment of reliability in algorithm audits. The audit literature is inconsistent in whether
reliability is included as a concern and, if it is, how it is defined and treated. Specifically,
several impactful lines of work do not consider reliability [28,36,40,47,63]. Of the works that
do take reliability under consideration, some refer to this concept as “stabil-
ity” [10,35,51,63], some refer to it as “reliability” [22,48,50,66], and some refer
to it as “robustness” [15,22,44,46,47]. Bandy [4] forgoes specific terminology and simply
refers to changes to input and output. This difference in treatment is more than terminologi-
cal: stability relates to local numerical analyses, whereas robustness tends to refer to broad,
system-wide imperviousness to adversarial attack, and reliability connotes consistency and
trustworthiness.

This inconsistency is part of a larger problem within sensitivity analysis — the formal
study of how system inputs are related to system outputs. Razavi et al. [50] observe that sen-
sitivity analysis is not a unified discipline, but is instead spread across many fields, journals
and conferences, and notes that lack of common terminology remains a barrier to unifica-
tion. In our work, we use the term stability to refer to a property of an algorithm whereby
small changes in the input lead to small changes in the output. We adopt a psychometric
definition of reliability, which we use to guide the way in which we measure stability. By
considering algorithms within their sociotechnical context, we can also translate between
numerical stability and broader robustness.

Although reliability has not been centered in algorithm audits, the importance of model
stability has long been established [68]. The 2020 manifesto on responsible modeling by
Saltelli et al. [52] underscores the importance of sensitivity analysis, and both the European
Commission [21] and the European Science Academies [58] have called for sensitivity au-
diting in the policy domain. As detailed by Razavi et al. [50], sensitivity audits have also
been applied in the domains of education [3], food security [53], public health [37], and
sustainability [24]. We argue that algorithm auditors should consider stability among the
critical metrics they select from, as suggested by Brown et al. [10].
Our work is synergistic with two recent lines of work that contribute substantive quantitative methodologies for auditing algorithm stability. In the first, Xue et al. [74] introduce a suite of tools to study individual fairness in black-box models. In the second, Sharma et al. [60] offer a unified counterfactual framework to measure bias and robustness. Sharma et al.’s methodology relies on access to the features being used by the model, whereas the methods proposed by Xue et al. and by our work only require query access to black-box models. The key distinction between Xue et al. and our work is that Xue et al. build on notions of individual fairness that can be encoded by Wasserstein distance, while we approach stability through a sociotechnical lens, borrowing metrics that are familiar to I-O psychologists.

Audit scope. A number of recent algorithm audits focus on tools used at various stages in hiring pipelines. Wilson et al. [73] and O’Neil Risk Consulting and Algorithmic Auditing (ORCAA) [47] each focus on tools for pre-employment assessment (i.e., candidate screening). Raghavan et al. [48] evaluate the public claims about bias made by the vendors of 18 such tools. Chen et al. [15] audit three resume search engines, Hannák et al. [29] audit two online freelance marketplaces, and De-Arteaga et al. [18] builds and evaluates several classifiers that predict occupation from online bios. All of these studies focus primarily on bias and discrimination. It is also common to frame these audits around the promises that companies make in their public statements [47,48,73]. By contrast, in our work we focus on auditing stability, which is a necessary condition for the validity of an algorithmic hiring tool.

Access level is a critical factor in determining audit scope. Audits can be internal (where auditors are employed by the company being audited), cooperative (a collaboration between internal and external stakeholders), or external (where auditors are fully independent and do not work directly with vendors). Sloane et al. [63] explain that the credibility of internal audits must be questioned, because it is advantageous to the company if they perform well in the audit. Ajunwa [2] argues for both internal and external auditing imperatives, with the latter ideally performed by a new certifying authority. Brown et al. [10] offer a flexible framework for external audits that centers on stakeholder interests. Bogen and Rieke [9] stress the importance of independent algorithm evaluations and place the burden on vendors and employers to be “dramatically” more transparent to allow for rigorous external audits. Absent that transparency, however, external audits must be designed around what information is publicly available. In this work we develop an external auditing methodology.

3 Methodology

In accordance with Sloane et al. [63], we frame our methodology around testing the underlying assumptions made by algorithmic personality tests within the hiring domain. We note that, because algorithmic personality tests constitute a category of psychometric instrument, they are subject to the assumptions made by the traditional instruments, as laid out in Section 2.1. Validity of these tests is subject to the following additional assumptions:

– Assumption 1: The output of an algorithmic personality test is stable across input types (such as PDF or Docx) and other job-irrelevant variations in the input. This assumption corresponds to parallel forms reliability from psychometric testing (see Section 2.1).

Note that this list of assumptions is not exhaustive.
– **Assumption 2:** The output of an algorithmic personality test is stable across input sources (such as resume or LinkedIn) that are treated as interchangeable by the vendor. This assumption corresponds to cross-situational consistency (see Section 2.1).

– **Assumption 3:** The output of an algorithmic personality test on the same input is stable over time. This assumption corresponds to test-retest reliability (see Section 2.1).

Importantly, all these assumptions are testable via an external audit. Thus, these are the assumptions on which we focus our analysis, and with respect to which we quantify stability as a necessary condition for validity.

**Audit procedure.** We now present a procedure to assess the stability of algorithmic personality tests in hiring, inspired by the auditing framework of Brown et al. [10]. Our method requires numeric output, which can be a single personality measure or a vector of multiple measures.

1. **Collect preliminary information** to describe the sociotechnical context in which the system operates, and detail the system’s inputs and outputs.
2. **Identify key facets** of measurement across which the system assumes its outputs to be stable, based on validity assumptions.
3. **Collect or create an input corpus** that is representative of the tool’s intended context of use. Perturb the input across the features that correspond to each facet of measurement, while keeping all other features fixed to the extent possible, generating two treatments for assessing stability of each facet.
4. **Estimate stability with respect to each key facet** by querying the system with subsets of the input corpus that vary across that facet, while all other features remain fixed. Record the outputs produced by the system and compare them to assess facet-specific stability. The following metrics can be used, but other metrics may also be applicable:
   (a) **Rank-order stability.** As explained in Section 2.1, the reliability of psychometric instruments is measured with correlations. Thus, we recommend correlation to assess rank-order stability. Estimate the correlation between the outputs across each facet of interest. Morrow and Jackson [42] make a convincing argument against providing significance levels for reliability correlations. Instead, compare estimated correlations to the “bare minimum” of 0.90 and the “desirable standard” of 0.95, as proposed by Nunnally and Bernstein [45].
   (b) **Locational stability.** If a system allows users to compare output across a key facet, then we should also assess locational stability across that facet, i.e., whether one facet treatment generally yields higher overall scores. Choose an appropriate hypothesis test (e.g., paired t-test to compare treatment means, or the Wilcoxon signed-rank test, a non-parametric alternative which tests whether the median of the paired differences is significantly different than zero). Account for multiple hypothesis testing by adjusting the significance threshold for each system.

   – **Bonferroni correction** controls the family-wise error rate. It is guaranteed to falsely reject the null hypothesis no more often than the nominal significance level, however, it can be overly conservative, especially when sample sizes are low (i.e., it can falsely accept the null hypothesis more often than the nominal significance level implies) [69].

   \[
   \alpha_{\text{Bonferroni}} = \frac{\alpha_{\text{nominal}}}{\# \text{ tests performed}}
   \]
– **Benjamini-Hochberg correction** is a less conservative approach that controls the false discovery rate. The procedure ranks obtained p-values in ascending order and uses these ranks to derive corrected thresholds, which range between $\alpha_{\text{Bonferroni}}$ and $\alpha_{\text{nominal}}$ [7].

\[ \alpha_{\text{Benjamini-Hochberg}} = \frac{\text{p-value rank}}{\# \text{ tests performed}} \alpha_{\text{nominal}} \]

(c) **Total change.** To quantify the total change across a facet, select a distance measure and a normalization procedure that are appropriate for the type of system output.

(d) **Subgroup stability.** If subject-level demographic data is available, compute metrics (a)-(c) within each demographic group.

In the next section, we will use this procedure to conduct an external stability audit of two algorithmic personality tests, Humantic AI and Crystal.

### 4 Stability Audit of Humantic AI and Crystal

#### 4.1 Preliminary Information

**Systems of interest.** We assess stability of two automated text-based pre-selection employee screening systems provided by vendors Humantic AI and Crystal. Both Crystal and Humantic AI output candidate DiSC scores: vectors of 4 numeric values, each corresponding to a personality trait. Humantic AI produces a score for each trait on a scale from 0 to 10, while Crystal represents each trait as a percent of the whole, giving each a score from 0 to 100 such that all four traits sum to 100%. In addition to DiSC, Humantic AI also outputs scores for The Big Five model of personality. We will analyze DiSC and Big Five scores in our audit, and describe them in more detail below.

Humantic AI also outputs seven traits, which they call the “Behavioral Work Factors.” We do not include these traits in our analysis. Finally, Crystal and Humantic AI both categorize candidates into one of several types and produce descriptive personality profiles. We believe that written profiles are likely influential in hiring decisions, however, in the interest of keeping the scope of our work feasible, we leave a treatment of stability in these textual profiles to future work.

**DiSC and Big Five personality tests.** DiSC is a behavioral psychology test that assesses the extent to which a person exhibits four personality traits: Dominance (D), Influence (I), Steadiness (S), and Conscientiousness (C). Although official DiSC documentation states that C represents “Conscientiousness,” Humantic AI states that C in DiSC stands for “Calculativeness.” Notably, although both Humantic AI and Crystal market DiSC as a rigorous psychology-based analysis methodology, scholarly work on DiSC in I-O psychology has been limited, especially with regard to its validity and reliability for hiring. In fact, the DiSC website explicitly states that DiSC scores are “not recommended for pre-employment screening.”

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4. https://www.discprofile.com/what-is-disc/how-disc-works
5. Humantic AI separately produces predictions on “Conscientiousness” within the Big Five model of personality. We posit that Humantic AI may have made the choice to rename the DiSC “Conscientiousness” trait to “Calculativeness” in order to avoid conflation with the Big Five trait by the same name.

6. https://www.discprofile.com/everything-disc/hiring
The Big Five model is far better studied than DiSC, and its use in personnel selection is considered acceptable by some I-O psychologists [25,32]). Still, the use of the Big Five in hiring is not without criticism. For example, Morgeson et al. [41] argue that “the validity of personality measures as predictors of job performance is often disappointingly low.” The Big Five model contains five traits: Openness (O), Conscientiousness (C), Extraversion (E), Agreeableness (A), and Neuroticism (N). Humantic AI replaces Neuroticism with the more palatable “Emotional Stability”, which, they explain, is “the same as Neuroticism rated on a reverse scale.”

**System design and validation.** Humantic AI and Crystal state that they use machine learning to extract personality profiles of job candidates based on the text of their resumes and LinkedIn profiles. However, public information about model design and validation is limited. Humantic AI states that “all profile attributes are determined deductively and predictively from a multitude of activity patterns, metadata or other linguistic data inputs.” Crystal explains that their personality profiles are “predicted through machine learning and use text sample analysis and attribute analysis.” Neither company makes its training data publicly available or discusses the data collection and selection methodology they used. For this reason, an external audit cannot assess whether the training data is representative of the populations on which the systems are deployed.

Information about validation is limited as well. Humantic AI reports that their outputs “have an accuracy between 80-100%” [10] Crystal advertises that “based on comparisons to verified profiles and our user’s direct accuracy validation through ratings and endorsements, Crystal has an 80% accuracy rating for Predicted [sic] profiles.” [11] No additional information is given about the validation methodology, the specific accuracy metrics, or results. Finally, update schedules for the models used by the systems are not publicly disclosed.

**Sociotechnical context of use.** Employers purchase candidate-screening tools from Crystal and Humantic AI and use them to build personality profiles of potential employees. Both systems offer functionality for ranking candidates based on their personality profiles. Crystal assigns a “job fit” score to candidates, which is measured based on a comparison to either a “benchmark candidate” with a user-specified ideal personality profile, or to a job description that is analyzed to “detect the most important personality traits.” Similarly, Humantic AI assigns a “match score” to candidates by comparing them to an “ideal candidate,” specified with a LinkedIn URL or an ideal personality score vector.

The hiring processes these systems support are not fully automated. Human decision-makers must choose whether and how to define an ideal candidate, at what stage of hiring to use the tool, and how to incorporate tool outputs into hiring decisions. For example, an HR professional may decide to use an existing employee to define an ideal candidate, then run all resumes they receive through the tool, and finally offer interviews to all candidates with match scores above 90%. A different HR department may use the system to filter resumes before human review, choosing to rank candidates based on predicted “Steadiness” scores, and then discard all but the top 25 candidates. As these examples illustrate, the human-in-the-loop implementation details are crucial to actual outcomes.

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7 https://app.humantic.ai/#/candidates
8 https://api.humantic.ai/
9 https://www.crystalknows.com/blog/crystal-accuracy
10 https://api.humantic.ai/
11 https://www.crystalknows.com/blog/crystal-accuracy
4.2 Key Facets of Measurement

We identify the following measurement facets across which Humantic AI and Crystal operationalize reliability, as discussed in Section 3.

- **Resume file format.** Absent specific formatting instructions, the file format of an applicant’s resume, such as PDF or Docx, should have no impact on their personality score. As stated in Assumption 1, stability estimates that compare personality scores across this facet quantify parallel forms reliability.

- **Source context.** Humantic AI and Crystal both measure implicit signals within certain contexts (i.e., resumes, LinkedIn profiles, and tweets), and use these signals to assign personality scores to job seekers. Further, both systems allow direct comparisons of personality scores derived from multiple source contexts, for example by ranking candidates on their “match score,” which is computed from resumes for some job seekers and from LinkedIn profiles for other job seekers. Per Assumption 2, stability estimates that compare personality scores across this facet quantify cross-situational consistency.

- **Inclusion of LinkedIn URL in a resume.** The decision to embed a LinkedIn URL into one’s resume should have no impact on the personality score computed from that resume. This is because we expect the output to be stable across input sources per Assumption 2, and across job-irrelevant variations in the input per Assumption 1.

- **Algorithm-time (i.e., the time at which an input is scored).** Humantic AI and Crystal can both generate personality scores for the same input at different points in time, and to compare and rank job seekers based on their scores made at different times. For example, consider an extended hiring process that takes place over the course of months, with new candidates being screened at different times. In this situation, Humantic AI and Crystal would both encourage users to compare output generated months apart. Based on Assumption 3, as a component of test-retest reliability, we expect the personality score computed on the same input to be the same, irrespective of when it is computed.

- **Participant-time (i.e., the time at which an input is produced).** Consider an HR department that keeps candidate resumes on file to consider them for future positions. A user in this department might be tempted to generate scores from old “on-file” resumes and compare them to scores of new candidates. Neither Humantic AI nor Crystal offers any guidance to users regarding the age of input data, or the forward-looking period to which results can be extended. Thus, they both encourage users to generalize across participant-time. Based on Assumption 3, and as a component of test-retest reliability, we expect the personality score computed based on time-varying input from the same individual to be the same, irrespective of when the input is generated.

4.3 Creation of the Input Corpus

**Primary data collection.** We conducted an IRB-approved human subjects research study at New York University to seed the input corpus for the audit. For this, we recruited current graduate students at New York University’s Center for Data Science (N = 33), Tandon School of Engineering (N = 51), and Courant Institute of Mathematical Sciences (N = 10). We further required that participants not be currently located in the European Union or the United Arab Emirates. Participants were asked to complete a survey to upload their resume, provide a link to their public LinkedIn URL, their public Twitter handle, and their demographic information. All survey questions were optional.
Table 1: Sample demographics.

| Group      | N  | %   |
|------------|----|-----|
| All        | 94 | 100%|
| Gender     |    |     |
| Male       | 56 | 60% |
| Female     | 36 | 38% |
| Other      | 2  | 2%  |
| Race       |    |     |
| Asian      | 57 | 61% |
| White      | 24 | 26% |
| Other      | 12 | 13% |
| No Answer  | 1  | 1%  |

In total, 94 participants qualified for the study, of whom 92 submitted LinkedIn URLs, 89 submitted resumes (in PDF, Microsoft Docx, or .txt format), and 32 submitted public Twitter handles. Participants were given access to their personality profiles computed by Crystal and Humantic AI in exchange for their participation in the study. 88% of participants were pursuing a Master's Degree and 12% were pursuing a PhD degree. 60% of participants identified as male, 38% as female, and 2% as non-binary. Students' ages ranged from 21-40 with a mean of 26.13. 60% of our sample identified as Asian, 25% as White, 5% as Hispanic or Latino, 3% as Black or African American, and 4% identified as two or more races. 1% declined to identify their race. 37% of participants were born in India, 30% were born in the US, 13% were born in China, and 20% were born elsewhere. 64% reported that English was their primary language. (See Table 1.)

Persistent linkage of email addresses to LinkedIn profiles, and the need for de-identification.
During the initial processing of participant-provided information in Humantic AI, we observed that the personality profile produced from LinkedIn is often identical to the one produced from a resume containing an embedded LinkedIn URL. We hypothesized that for such URL-embedded resumes, Humantic AI was disregarding any information on the resume itself and pulling information from LinkedIn to generate a personality score. We further hypothesized that the system may create persistent linkages between email addresses and LinkedIn profiles.

To investigate this trend, resumes containing a LinkedIn URL and an email address were passed to Humantic AI. Next, we created and submitted fake PDF “resumes,” which were blank except for the email addresses that had been passed along with LinkedIn URLs, and compared the Humantic AI output produced by these two treatments. (Note: Due to privacy concerns, all linkage experiments used researchers’ own accounts and either their own or synthetic email addresses.) It was revealed that, when Humantic AI encounters a document that contains both a LinkedIn URL and an email address, it persistently associates the two such that the system produces the same personality score whenever it encounters that email address in the future. Because Humantic AI uses the embedded URLs to import information directly from LinkedIn, the predicted profiles in our linkage experiments dis-
Table 2: Resume versions used as input.

| Resume Version | File Format | Pre-Processing |
|----------------|-------------|----------------|
| Original       | Not consistent | None |
| De-Identified  | PDF         | Remove identifiers (name, phone, email, social media links and usernames). Save as PDF. |
| Raw Text       | Raw Text    | Copy text. |
| PDF            | PDF         | Save as PDF (if original in other format). |
| DOCX           | DOCX        | Remove identifiers (name, phone, email, social media links and usernames). Save as DOCX. |
| URL-Embedded   | PDF         | Remove identifiers (name, phone, email, social media accounts, and LinkedIn URL). Insert hyperlinked LinkedIn URL into beginning of document. Save as PDF. |

played names, photos, and employment information present on LinkedIn, but not present on the submitted resumes.

These findings further substantiate that Humantic AI operationalizes Assumption 2 of cross-situational consistency (see Section 3).

These findings necessitated the use of de-identified resumes in all future Humantic AI experiments. De-identification allows comparison of the algorithm’s predictions on resumes, without the obfuscating effect of information being pulled from LinkedIn. It also prevents participants’ emails from being linked to synthetically altered versions of their resumes. See Table 2 for de-identification details. Note that de-identification was not necessary in Crystal, as no such linkage was observed there. Further findings from our linkage explorations are detailed in Section 4.6.1.

Generating treatments for each facet. Recall from Section 4 that, in order to assess stability with respect to a facet of measurement, we need to perturb the input across the features that correspond to each facet, while keeping all other features fixed to the extent possible. As a result, we generate a pair of datasets, which we call treatments, for each facet. To isolate facet effects as cleanly as possible, we prepared several resume versions, described in Table 2. Details of each set of score-generating model calls that use these resume versions, or social media links, are presented in Table 3 for Humantic AI and in Table 4 for Crystal. In these tables, we list the type of input (e.g., Original Resume or LinkedIn profile), the identifier of the run that corresponds to this input, and the range of dates over which the system (Humantic AI or Crystal) was fed this type of input. We also list input size (“Inputs Submitted”) and output size (“Profiles Produced”). Note that output size may be smaller compared to input size, and sometimes substantially so. For example, for runs HT1 and HT2, we used 32 Twitter handles as input to Humantic AI, but we took only 21 personality profiles produced as output into consideration. This is because Humantic AI did not produce personality profiles from the remaining 11 accounts, but instead returned errors saying the Twitter profiles were “thin.”

We will explain how these versions are used as treatments in the stability experiments in Section 4.5.
4.4 Estimating Stability

To measure stability, we conduct a series of local sensitivity analyses [50] to probe the sensitivity of predicted personality traits to facets of interest. To conduct this analysis, we purchased nine months of Humantic AI basic organizational membership at a total cost of $2,250 and a combination of monthly, and annual Crystal memberships at a total cost of $753.82, and carried out the experiments over the period of November 23, 2020 through September 16, 2021.

One week into our evaluation, representatives from Humantic AI ascertained that we were using their tool to conduct an audit, and reached out to inform us that they would like to collaborate in the effort. In light of this development, we weighed the advantages and disadvantages of engaging with Humantic AI and decided to continue with a neutral external audit, to minimize the potential for conflicts of interest and maximize our ability to critically analyze the system for stability. The cost of that decision is that we had to forgo potential access to the underlying data, modeling decisions, features, and model parameters.

Table 3: Details of Humantic AI runs (i.e., sets of score-generating calls to Humantic AI models).

| Input                        | Run ID | Humantic AI Run Dates         | Inputs Submitted | Profiles Produced |
|------------------------------|--------|-------------------------------|------------------|-------------------|
| Original Resume              | HRo1   | 11/23/2020 - 01/14/2021       | 89               | 88                |
| De-Identified Resume         | HRi1   | 03/20/2021 - 03/28/2021       | 89               | 89                |
| De-Identified Resume         | HRi2   | 04/20/2021 - 04/28/2021       | 89               | 89                |
| De-Identified Resume         | HRi3   | 04/20/2021 - 04/28/2021       | 89               | 89                |
| DOCX Resume                  | HRd1   | 03/20/2021 - 03/28/2021       | 89               | 89                |
| URL-Embedded Resume          | HRu1   | 04/09/2021 - 04/11/2021       | 86               | 86                |
| LinkedIn                     | HL1    | 11/23/2020 - 01/14/2021       | 92               | 88                |
| LinkedIn                     | HL2    | 08/10/2021 - 08/11/2021       | 92               | 91                |
| Twitter                      | HT1    | 11/23/2020 - 01/14/2021       | 32               | 21                |
| Twitter                      | HT2    | 08/10/2021 - 08/11/2021       | 32               | 21                |

Table 4: Details of Crystal runs (i.e., sets of score-generating calls to Crystal models).

| Input                        | Run ID | Crystal Run Dates         | Inputs Submitted | Profiles Produced |
|------------------------------|--------|----------------------------|------------------|-------------------|
| Raw Text Resume              | CRr1   | 03/31/2021 - 04/02/2021     | 89               | 89                |
| Raw Text Resume              | CRr2   | 05/01/2021 - 05/03/2021     | 89               | 89                |
| Raw Text Resume              | CRr3   | 05/01/2021 - 05/03/2021     | 89               | 89                |
| PDF Resume                   | CRp1   | 11/23/2020 - 01/14/2021     | 89               | 89                |
| LinkedIn                     | CL1    | 11/23/2020 - 01/14/2021     | 92               | 91                |
| LinkedIn                     | CL2    | 09/13/2021 - 09/16/2021     | 89               | 89                |
that a collaboration with Humantic AI may have afforded. While we do not have any reason to believe that the discovery of our audit caused Humantic AI to change their models or operation, we cannot rule out this possibility.

4.5 Facet-Specific Stability Experiments

We performed the following experiments to test stability with respect to the key facets of measurement, described in Section 4.2:

- **Resume file format.** We tested sensitivity to file format by generating identical resumes in different formats. Humantic AI is able to accept PDF and Microsoft Word Docx documents, so we compared the output from de-identified resumes in PDF format (Table 3 run ID HRi1) to those same resumes in Docx format (Table 3 run ID HRd1). Crystal accepts PDFs or raw text, so we compared the output from raw text resumes (Table 4 run ID CRr1) to those same resumes in PDF format (Table 4 run ID CRp1).

- **Inclusion of LinkedIn URL in resume.** For each participant who submitted both a resume and a LinkedIn profile, we compared their Humantic AI personality profile results from de-identified resumes (Table 3 run ID HRi1) to the same resumes with the hyperlinked LinkedIn URL added before the first character of the resume, in the form https://www.linkedin.com/in/ParticipantUsername (i.e., URL-embedded resumes, Table 3 run ID HRu1).

- **Source context.** We tested Humantic AI’s sensitivity to input source context by comparing output from participants’ LinkedIn profiles (Table 3 run ID HL1), Twitter accounts (Table 3 run ID HT1), and resumes. Comparisons including resumes were repeated with original (Table 3 run ID HRo1), URL-embedded (Table 3 run ID HRou1), and de-identified resumes (Table 3 run ID HRi1). For Crystal, we compared output from PDF resumes (Table 4 run ID CRp1) to output from LinkedIn (Table 4 run ID CL1).

- **Algorithm-time / immediate.** We assessed the extent to which results from each system were immediately reproducible by inputting the same resume twice, consecutively. In Humantic AI, we compared de-identified resumes (Table 3 run IDs HRi2 and HRi3), and in Crystal, we compared raw text resumes (Table 4 run IDs CRr2 and CRr3).

- **Algorithm-time / 31 days.** We also tested the sensitivity of scores to longer differences in algorithm-time. We implemented this experiment by comparing the output of identical resumes scored 31 days apart from one another. The same resume versions were used in this comparison as in the algorithm-time / immediate experiment: we used de-identified resumes in Humantic AI (Table 3 run IDs HRi1 and HRi2) and raw text resumes in Crystal (Table 4 run IDs CRr1 and CRr2).

- **Participant-time.** To test the effect of differences in participant-time on score outcomes, we generated two time-separated scores from participants’ LinkedIn profiles (Table 3 run IDs HL1 and HL2; Table 4 run IDs CL1 and CL2). In Humantic AI we also generated two time-separated scores from participants’ Twitter accounts (Table 3 run IDs HT1 and HT2). The time elapsed between the sets of scores ranged from seven to nine months in Humantic AI, and eight to ten months in Crystal. This test was performed on social media profiles rather than on resumes because participants naturally update their social media profiles, whereas accessing updated resumes would require a second round of primary data collection.

We attempted to isolate the key facet of interest in each experiment by keeping all other measurement facets constant across the pair of treatments. In some cases, this was not pos-
sible (e.g., measuring across participant-time on social media necessitates also measuring across algorithm-time; see Section 4.6.6). Additionally, we discovered problematic mechanisms in Humantic AI (i.e., imperfect immediate reproducibility and linkage between email addresses and LinkedIn accounts) only after performing initial experiments, at which time it was no longer feasible to re-run all experiments. We chose to prioritize the use of de-identified resumes at the expense of allowing variations in algorithm-time. The implications of this choice are discussed in Sections 4.6.3 and 4.6.4.

4.6 Results

Summary of experimental results. Table 5 summarizes the results of our audit. We found that Humantic AI and Crystal predictions both exhibit rank-order instability with respect to source context and participant-time. In addition, Crystal is rank-order unstable with respect to file format, and Humantic AI is rank-order unstable with respect to URL-embedding in resumes. The systems were sufficiently rank-order stable with respect to all other facets. We did not find any significant locational instability in Crystal. Some traits in Humantic AI displayed significant locational instability with respect to URL-embedding, source context, and participant-time.

Further, the DiSC scores produced by Crystal exhibited clear discontinuities (see Section 4.6.1). Output scores in Humantic AI were approximately normally distributed, with the exception of DiSC Calculativeness, which was strongly left-skewed in all runs. We give additional information about each experiment, and present detailed experimental results in the remainder of this section.

Specific stability metrics

– Rank-order stability. Because DiSC scores were discontinuous in Crystal, we used Spearman rank correlation rather than Pearson’s correlation coefficient to quantify rank-order stability. Rank-order stability results are presented in Tables 6, 7, and 8 in the Appendix.

Table 5: Summary of stability results for Crystal and Humantic AI, with respect to facets of measurement from Section 4.2. “✓” indicates sufficient rank-order stability ($r \geq 0.90$) and sufficient locational stability ($p \geq \alpha_{\text{Benjamini-Hochberg}}$) in all traits, “✗” indicates insufficient rank-order stability ($r < 0.90$) or significant locational instability ($p < \alpha_{\text{Benjamini-Hochberg}}$) in at least one trait, and “?” indicates the facet was not tested in our audit. For detailed results, see Tables 6, 7, 8, 9, 10, and 11 in the Appendix.

| Facet                        | Crystal | Humantic AI | Details |
|------------------------------|---------|-------------|---------|
| Resume file format           | ✗       | ✓           | Section 4.6.2 |
| LinkedIn URL in resume       | ?       | ✗           | Section 4.6.3 |
| Source context               | ✗       | ✗           | Section 4.6.4 |
| Algorithm-time / immediate reproducibility | ✓       | ✓           | Section 4.6.5 |
| Algorithm-time / 31 days apart | ✓     | ✗           | Section 4.6.5 |
| Participant-time / LinkedIn  | ✗       | ✗           | Section 4.6.6 |
| Participant-time / Twitter   | N/A     | ✓           | Section 4.6.6 |
Locational stability. Similarly, we use the Wilcoxon signed-rank test to assess the significance of paired differences. Unlike the Student’s t-test, the Wilcoxon signed-rank test does not assume the data is normally distributed. Locational stability results can be found in Tables 9, 10, and 11 in the Appendix. We start with a nominal \( \alpha \) of 0.05. In Crystal, we test the median change of the four DiSC traits across five facets, for a total of 20 tests and a Bonferroni-corrected \( \alpha \) of 0.0025. In Humantic AI, we test the Big Five traits and the four DiSC traits across eleven facets, for a total of 99 tests and a Bonferroni-corrected \( \alpha \) of 5.05 \( \times 10^{-4} \).

Total change. To compute total change, we calculate the L1 distance between the output vectors of the two runs for each subject. In order to compare results across different scales, this distance is normalized by the total range of output space. The normalization constant is the inverse of the sum of possible score ranges for each trait in the category. For example, Humantic AI produces four DiSC scores each measured on a scale from 0 to 10, so we divide the DiSC L1 distances by 40. Because Crystal constrains their DiSC scores to sum to 100, the maximum possible L1 change is 200, and we therefore use a normalization constant of 200. Normalized L1 change across key facets is plotted in Figures 10 (Crystal DiSC scores), 11 (Humantic AI DiSC scores), and 12 (Humantic AI Big Five scores) in the Appendix.

Subgroup stability. We use demographic information provided in our survey to estimate rank-order stability and normalized L1 distance within subgroups defined by race, gender, birth country, and primary language. With only 94 participants, we lacked the statistical power to perform hypothesis testing for each subgroup, and instead limit our locational stability analysis to the sample as a whole.

4.6.1 Qualitative Observations

Clusters in Crystal. We observed discontinuity in Crystal output, which was particularly marked in Steadiness and Conscientiousness. This can lead to increased instability when a small change in input may lead to a large change in output across the point of discontinuity. We observed evidence of this phenomenon in both Steadiness and Conscientiousness across all facets we tested in Crystal, see Figure 2 for an example.

Medians in Crystal. The median for each DiSC trait remained fairly constant across all Crystal runs. The median Dominance score was always 5, the median Influence score was always 10, the median Steadiness score wasalways 22 or 23, and the median Conscientiousness score ranged from 59 to 62. This result is especially notable, considering that we observed both rank-order and locational instability between treatments in Crystal (see Sections 4.6.2, 4.6.3, 4.6.4, 4.6.5, and 4.6.6).

Linkage and Privacy in Humantic AI. Investigative linkage experiments revealed that when Humantic AI encounters a document that contains a LinkedIn URL and an email address, the resulting profile will have a 100% confidence score, and it will contain information found only on LinkedIn (including name, profile picture, and job descriptions and dates). Furthermore, the Humantic AI model produces the same personality profile whenever it encounters that email address in the future. This linkage persists regardless of how different the new resume is from the one that initially formed the linkage. The email address in question need not be associated with the LinkedIn profile, or even with the candidate.
Fig. 2: Scatterplots comparing Crystal output across the resume file format facet show evidence of discontinuous measurement in Steadiness and Calculativeness, with some participants’ scores moving between clusters with different file formats.

We observed one case in which a participant listed contact information for references, and Humantic AI created a link between a reference’s email and the participant’s LinkedIn. (This profile was produced before we noticed the linkage issue.)

We also found that, once a linkage between an email address and a LinkedIn URL had been made, we were able to alter the personality score produced from a LinkedIn profile by submitting a resume with strong language, namely, containing keywords “sneaky” and “adversarial.” We therefore conclude that the linkage is used by Humantic AI in both directions: the content of a LinkedIn profile can affect the personality score computed from a linked resume, and the content of a linked resume can affect personality score computed based on a LinkedIn profile.

We did not observe any linkage with participants’ Twitter accounts. However, when we used high-profile celebrity Twitter accounts as input, Humantic AI produced profiles that contained links to several other profiles, including Google+, LinkedIn, Facebook, and Klout. These links do not seem to be validated. We observed one case in which a high-profile popstar was linked to a software engineer of the same name.
Although Humantic AI offers an option at the bottom of their website to “opt out of Humantic AI” by entering an email, social network username, LinkedIn URL, or phone number (see Figure 3), this feature seems to be inoperable. Various forms of participant information were entered into this field, yet, personality scores associated with this information in the past persisted on the Humantic AI dashboard, and new results were returned when the information was passed to Humantic AI in a new account. In cases where LinkedIn profiles were deactivated after profiles were created from them, it was observed that Humantic AI would still create new profiles from the deactivated LinkedIns, even on different Humantic AI accounts.

4.6.2 File Format Experiments

We determine that Humantic AI is in general sufficiently stable with respect to file format. Rank correlations range from 0.982 (Emotional Stability) to 0.998 (Steadiness). When we look at demographic subgroups, however, we do find several correlations that fall below the 0.95 threshold: Steadiness (0.947) and Openness (0.945) in participants born in China (N = 11), Big Five Conscientiousness in participants who are neither White nor Asian (r = 0.945, N = 11), and Emotional Stability in participants born in countries other than the US, China, or India (r = 0.946, N = 18). (The two sets of runs are constant with regard to participant-time, and are very close to each other in terms of algorithm-time; scores for the de-identified PDF and Docx resumes were generated on the same day, within minutes of each other.) Complete experimental results for Humantic AI are listed in Tables 7, 8, 10, and 11 in the Appendix.

Crystal’s overall stability across the file format facet fails to meet Nunnally and Bernstein’s preferred standard of 0.95 for Steadiness (0.918) and Conscientiousness (0.911), and falls below the minimum limit of 0.90 for Dominance (0.822) and Influence (0.826). In some subgroups, Steadiness and Conscientiousness do fall below 0.90: White (N = 23), female (N = 33), born in the US (N = 26), and those whose primary language is English (N = 56). See Figure 4 for comparison of normalized L1 distance between output of different groups. Although PDF resumes were scored by Crystal four months earlier than raw text resumes, given the perfect reproducibility of Crystal’s text predictions, albeit over a shorter time span, we can assume that algorithm-time is not a factor here. Complete experimental results for Crystal are listed in Tables 6 and 9 in the Appendix.

There were no significant locational stability differences across the file format facet in either Humantic AI or Crystal.
4.6.3 Inclusion of LinkedIn URL in Resume Experiments

We discovered substantial instability with regard to URL-embedding in Humantic AI. Correlations between de-identified resumes and the same resumes with LinkedIn URLs embedded into them range from 0.077 (Extraversion) to 0.688 (Calculativeness). The trait with the biggest difference in intragroup correlations was Emotional Stability, for which the correlations ranged from -0.285 for people born in China (N = 10) to 0.748 for people born in countries other than China, India, or the US (N = 18). We also discovered locational differences deemed significant by the Bonferroni threshold in Dominance (de-identified median 6.90, URL-embedded median 5.65; Wilcoxon p < 10^{-6}; see Figure 5), Steadiness (de-identified median 5.00, URL-embedded median 5.60; Wilcoxon p = 4.8 × 10^{-5}), Big Five Conscientiousness (de-identified median 5.60, URL-embedded median 6.17; Wilcoxon p = 2.1 × 10^{-5}), Extraversion (de-identified median 4.14, URL-embedded median 6.38; Wilcoxon p < 10^{-6}), and Agreeableness (de-identified median 5.56, URL-embedded median 6.07; Wilcoxon p = 1.6 × 10^{-4}). Under the more liberal Benjamini-Hochberg standard, there were also significant locational differences in DiSC Calculativeness (de-identified median 7.50, URL-embedded median 8.00; Wilcoxon p = 4.7 × 10^{-3}) and Openness (de-identified median 6.14, URL-embedded median 5.90; Wilcoxon p = 2.5 × 10^{-3}).

We note that algorithm-time is unfortunately an unavoidable factor here; the two resume versions were run about four months apart. Furthermore, if we accept that Humantic AI uses information from LinkedIn profiles when it encounters embedded LinkedIn URLs, then we are also faced with a mismatch in participant-time.

Correlations between LinkedIn scores and URL-embedded resumes ranged from 0.156 (Dominance) to 0.702 (Emotional Stability), and there was a significant difference in the medians of Big Five Conscientiousness (LinkedIn 5.72, resume 6.19; Wilcoxon p = 4.3 × 10^{-5}) per the Bonferroni-adjusted threshold. Under Benjamini-Hochberg correction, the differences in Dominance (LinkedIn median 4.90, resume median 5.60; Wilcoxon p = 6.6 × 10^{-3}) and Agreeableness (LinkedIn median 5.81, resume median 6.06; Wilcoxon p = 6.8 × 10^{-3}) were significant as well. We predicted higher correlations under the embedding hypothesis, but a four month gap in algorithm-time as well as participant-time is likely to degrade the correlations significantly. Still, LinkedIn scores are more highly correlated with URL-embedded resumes than they are with de-identified resumes. Although instability due to algorithm-time is not guaranteed to increase monotonically with chronological time, this finding holds slightly more weight given that there were two more weeks of time between the LinkedIn and URL-embedding resume scoring. We also find that scores from URL-embedded resumes correlate slightly better with those from LinkedIn (generated
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Fig. 5: Scatterplot of Humantic AI Dominance scores produced by de-identified resumes (median 6.90) and URL-embedded resumes (median 5.65). Locational instability determined to be significant by Wilcoxon signed-rank test ($p < 10^{-6}$), under Bonferroni correction.

four months earlier) than they do with those from de-identified resumes (generated just 2 weeks earlier).

Complete experimental results are listed in Tables 7, 8, 10, and 11 in the Appendix.

4.6.4 Source Context Experiments

Humantic AI and Crystal both displayed alarmingly low stability across input sources.

Crystal’s rank-order correlations between PDF Resumes and LinkedIn profiles were all below the 0.90 threshold; they ranged from 0.233 (Dominance) to 0.526 (Influence). The trait with the biggest difference in correlations across subgroups was Dominance, which ranged from 0.054 (India, $N = 32$) to 0.720 (China, $N = 10$) when broken down by birth country. The median paired differences in Influence scores were significantly different than zero according to the Bonferroni standard ($Wilcoxon p = 1.2 \times 10^{-3}$), although the median for both treatments was 10.00. PDF resumes and LinkedIn URLs were scored the same day, and, as we discuss in Section 4.6.5, Crystal is immediately reproducible, and so we can exclude algorithm-time as a factor in this finding. Furthermore, for each candidate, this scoring took place within two weeks of resumes being submitted; thus, the participant-time of the resume matches very nearly to the participant-time of the LinkedIn. With all other facets being identical or near-identical, we can safely attribute the observed score differences to differences in source context. Complete experimental results for Crystal are listed in Tables 6 and 9 in the Appendix.

De-identified resumes were not submitted to Humantic AI until 4 months after LinkedIn profiles had been run. This difference in algorithm-time hampers our interpretation of cross-profile correlations. Nonetheless, it is undeniably troublesome that the observed correlations are as low as 0.090 (Dominance), and that there were significant locational differences under Bonferroni in Dominance (LinkedIn median 4.85, resume median 6.85; Wilcoxon $p < 10^{-6}$) and Extraversion (LinkedIn median 6.44, resume median 4.06; Wilcoxon $p < 10^{-6}$, and under Benjamini–Hochberg in Steadiness (LinkedIn median 5.30, resume median
Fig. 6: Scatterplot of Humantic AI Extraversion scores produced by de-identified resumes (median 4.06) and LinkedIn profiles (median 6.44). Locational instability determined to be significant by Wilcoxon signed-rank test ($p < 10^{-6}$), under the Bonferroni correction.

Fig. 7: Normalized L1 distances between Humantic AI DiSC and Big Five scores produced from pairs of treatments which vary with respect to their input source.

5.00; Wilcoxon $p = 1.3 \times 10^{-3}$) and Openness (LinkedIn median 6.01, resume median 6.14; Wilcoxon $p = 7.7 \times 10^{-3}$); see Figure 6. We can avoid the issue of algorithm-time (though not the lesser issue of immediate reproducibility) by using Humantic AI scores derived from original resumes, which were run at the same time as LinkedIn profiles. These results derived from original resumes are somewhat misleading, as 57 of the 84 resumes in this experiment contained some form of LinkedIn URL. Given the evidence that Humantic AI uses information directly from LinkedIn in such cases, correlations derived from original resumes are likely to overestimate cross-contextual stability. Nevertheless, the correlations we observe across all 84 participants range from 0.177 (Dominance) to 0.712 (Big Five Conscientiousness), with significant locational differences under Bonferroni in Dominance (LinkedIn median 4.85, resume median 5.95; Wilcoxon $p = 7 \times 10^{-6}$), Big Five Conscientiousness (LinkedIn median 5.73, resume median 5.98; Wilcoxon $p = 2.8 \times 10^{-4}$), and Extraversion (LinkedIn median 6.44, resume median 5.75; Wilcoxon $p = 6.9 \times 10^{-5}$) and significant locational differences under Benjamini-Hochberg in Influence (LinkedIn median 4.60, resume median 4.85; Wilcoxon $p = 5.0 \times 10^{-3}$). Limiting analysis to the 27 participants whose original resumes contained no reference to LinkedIn, we find that the correlations straddle zero, ranging from -0.310 (Influence) to 0.297 (DiSC Calculativeness).
Comparing Humantic AI scores from Twitter to those from original resumes, we find correlations ranging from -0.521 (Dominance) to 0.232 (Big Five Conscientiousness). We easily avoid the issue of algorithm-time by using original resumes, which were run the same day as Twitter. None of the original resumes contain references to participants’ Twitter accounts, and furthermore we did not find evidence of linkage with Twitter profiles, so we need not worry about data leakage in this case. A major caveat to this result is the small sample size ($N = 20$). Although the locational differences were insignificant when compared to the Bonferroni-corrected threshold, the Benjamini-Hochberg correction found significant locational differences in Openness (resume median 6.03, Twitter median 8.16; Wilcoxon $p = 1.2 \times 10^{-2}$), Agreeableness (resume median 6.37, Twitter median 3.32; Wilcoxon $p = 2.0 \times 10^{-3}$), and Emotional Stability (resume median 5.42, Twitter median 7.97; Wilcoxon $p = 1.0 \times 10^{-3}$).

Finally, we compare the Humantic AI scores from LinkedIn and Twitter. Again we have a small sample size ($N = 18$), however the results are striking. Only one of the correlations is positive (Influence, $r = 0.020$), and the others are as low as -0.433 (DiSC Calculativeness). Again there are no significant locational differences under Bonferroni, but using the Benjamini-Hochberg correction we find significant differences in Openness (LinkedIn median 5.82, Twitter median 8.16; Wilcoxon $p = 2.3 \times 10^{-3}$), Big Five Conscientiousness (LinkedIn median 5.77, Twitter median 7.16; Wilcoxon $p = 4.7 \times 10^{-3}$), Extraversion (LinkedIn median 6.80, Twitter median 4.72; Wilcoxon $p = 6.7 \times 10^{-4}$), Agreeableness (LinkedIn median 6.32, Twitter median 3.32; Wilcoxon $p = 4.7 \times 10^{-3}$), and Emotional Stability (LinkedIn median 4.86, Twitter median 7.97; Wilcoxon $p = 6.7 \times 10^{-4}$). Participant-time and algorithm-time are both guaranteed to be constant in this experiment, as profiles were generated on the same day.

See Figure 7 for comparison of L1 distances between each treatment of the input source facet in Humantic AI. Complete experimental results for Humantic AI are listed in Tables 7, 8, 10, and 11 in the Appendix.

4.6.5 Algorithm-time Experiments

Crystal results on resumes were reproducible immediately, as well as one month later. We can conclude that Crystal’s text prediction tool is deterministic and was not updated over the course of April 2021, when the experiment was performed.

Humantic AI results were not perfectly reproducible, even immediately. This result could be explained by a non-deterministic prediction function, or by an online model that is updated with each prediction it makes. The latter explanation is in line with our findings in the linkage investigations, where we observed that one call to the model can influence the outcome of other calls. Only Steadiness and DiSC Calculativeness remained constant for all participants when identical resumes were run back-to-back. One participant had changes in their Dominance and Influence scores (DiSC total normalized L1 difference was 0.005), and two participants had changes in their Big Five scores (maximum Big Five total normalized L1 difference was 0.003). (The correlations for immediate reproducibility were all above the 0.95 threshold, and there were no significant locational differences.)

After 31 days, rank-order correlations in Humantic AI ranged from 0.962 (Extraversion) to 0.998 (DiSC Calculativeness). Although the overall Humantic AI correlations across algorithm-time were all above the 0.95 threshold, we find that Dominance fell below 0.95 for participants born in India ($r = 0.940, N = 33$) and for non-native English speakers ($r = 0.946, N = 33$), and that Extraversion fell below 0.95 for Asian participants ($r = 0.940,$
Fig. 8: Normalized L1 distances between Humantic AI DiSC and Big Five scores produced from identical resumes scored at different points in time.

$N = 54$), for participants born in India ($r = 0.916, N = 33$), and for non-native English speakers ($r = 0.934, N = 33$). The trait with the biggest difference in correlations across subgroups was Extraversion, which ranged from 0.916 (India, $N = 33$) to 0.997 (USA, $N = 26$) when broken down by birth country. Figure 8 shows that these substandard sub-group correlations result from two participants whose resumes were scored very differently a month apart; we also note that the lack of immediate reproducibility we observed in Humantic AI did not affect these two particular individuals. We did not find any significant locational differences across algorithm-time using the Bonferroni correction, but under Benjamini-Hochberg we found significant differences in Openness, where the median decreased from 6.15 to 6.13 over the course of a month (Wilcoxon $p = 7.1 \times 10^{-3}$).

Humantic AI’s lack of immediate reproducibility, discussed earlier in this section, must be kept in mind when evaluating comparisons across all other facets.

Complete experimental results for Humantic AI are listed in Tables 7, 8, 10, and 11 in the Appendix.

4.6.6 Participant-time Experiments

Humantic AI scores on Twitter accounts showed no change over 7-9 months.

Overall, Humantic AI LinkedIn correlations across 7-9 months of participant-time were all below the 0.90 threshold: they ranged from 0.225 (Dominance) to 0.768 (Emotional Stability). The trait with the biggest difference in correlations across subgroups was DiSC Calculativeness, which ranged from 0.092 (race other than White or Asian, $N = 9$) to 0.879 (White, $N = 22$) when broken down by race. Built into these correlations is the corrosive effect of 7-9 months of algorithm time; this helps to explain, but does not justify, the unacceptably low test-retest reliability. Under Bonferroni correction, we found a significant difference in Big Five Conscientiousness scores, with the median increasing from 5.72 to 6.17 (Wilcoxon $p = 4 \times 10^{-6}$), and under Benjamini-Hochberg we also found a significant difference in Agreeableness, where the median increased from 5.81 to 5.99 (Wilcoxon...
Fig. 9: Normalized L1 distances between Crystal DiSC scores produced from LinkedIn profiles scored 8-10 months apart.

$p = 7.2 \times 10^{-3}$). Complete experimental results for Humantic AI are listed in Tables 7, 8, 10, and 11 in the Appendix.

Overall, Crystal LinkedIn correlations across 8-10 months of participant-time were all below the 0.90 threshold as well, ranging from 0.531 (Dominance) to 0.868 (Steadiness). The trait with the biggest difference in correlations across subgroups was Dominance, which ranged from 0.232 (male, $N = 53$) to 0.933 (female, $N = 34$) when broken down by gender. See Figure 9 for cross-gender comparison of L1 distances between participant-time treatments. There was no significant locational instability across participant-time in Crystal. Complete experimental results for Crystal are listed in Tables 6 and 9 in the Appendix.

5 Discussion

5.1 Stability Audit Conclusions

Humantic AI and Crystal both exhibit problematically low reliabilities across time and input source context. Humantic AI also exhibited low reliability with respect to the presence of LinkedIn URLs in resumes. Crystal’s reliability with respect to resume format is unacceptably low as well. The correlations we observed allow us to conclude that the tools cannot be considered valid instruments in high-stakes decisions.

Overall, each of these observed unreliabilities undermines the cost and effort reduction that employers seek from candidate screening tools. Employers’ desire for valid decisions reflective of job performance is severely compromised by sensitivity to job-irrelevant factors. Thus, we find that Humantic AI’s sensitivities to participant-time, URL-embedding, and source context, and Crystal’s sensitivities to file format and source context, could be quite problematic for employers. The sensitivity of these algorithms to job-irrelevant factors is also a threat to individual fairness; a job seeker could reasonably conclude from the present audit that Humantic AI and Crystal are both likely to judge their job-worthiness unfairly, letting meaningless criteria dictate their outcomes.

These unreliabilities are also at odds with the trustworthiness that society seeks in its AI products. Humantic AI’s lack of reproducibility is a particularly insidious violation of trustworthiness, because it undermines the power of audits on its system. Although Humantic AI’s stability over algorithm-time exceeds Nunnally and Bernstein’s classical 0.95 reliability threshold for tests used to make decisions about individuals (see Section 2.1), the Supreme Audit Institutions of Finland, Germany, the Netherlands, Norway and the UK...
have asserted that reproducibility is “a mandatory condition for reliability.” Irreproducibility resulting from frequently or continuously updated models poses a threat to the ongoing monitoring and auditing necessary to ensure a system is working as expected [9,35].

Finally, Humantic AI’s and Crystal’s lack of transparency regarding training data and model architecture are at odds with privacy concerns. Humantic AI’s deceptive and ineffective opt-out option is an example of what Ajunwa [2] calls “algorithmic blackballing,” whereby an applicant’s profile is allowed to live on past its shelf-life. This is especially dangerous in combination with the potential to leverage Humantic AI’s email linkage mechanism in an adversarial attack. Humantic AI’s failed opt-out option may also violate the California Consumer Privacy Act’s right to delete [11].

5.2 Study Limitations

In our audit we do not conduct stakeholder evaluations. Several audits and framework documents emphasize the importance of algorithmic impact assessment and stakeholder evaluations [10,22,47,49,50,63]. Metcalf et al. [40] explain that an external audit must not stand in as an impact assessment. Without collaboration of internal agents, third parties do not have access to design decisions or stakeholder interviews, and cannot directly influence change in the design or operation of the algorithm should it be needed. Per Ajunwa [2], algorithms need to be audited internally as well as externally.

Although this audit considers various dimensions of reliability and stability, the analysis is not comprehensive. We have constrained our focus in this study to analyze DiSC and Big Five scores, which claim to offer a quantitative measure of “personality.” However, much of the advertising of both tools focuses on the profiles holistically, not just on the scores.

Humantic AI often fails to produce profiles from inputs (see the discrepancies between number of inputs submitted and number of profiles produced in Table 3). This is especially common in Twitter profiles. By simply disregarding the failed inputs, we may be introducing some sampling bias into our results. Furthermore, such non-results may exhibit problematic biases [47].

Our study population was constrained to technical graduate students at NYU, studying in the realms of computer and data science. This was done in an attempt to control for differences in algorithm response due to characteristics such as job field, experience level, and writing style. We also felt that this restriction more closely replicated a pool of candidates who might realistically be compared to one another in a job search. However, this narrowness, and our modest cohort size (N = 94), restrict the generalizability of our results.

Additionally, this audit evaluates only the intermediate personality profile results, and does not relate them to hiring outcomes. Our audit did not use the “job fit” or “match score” features because, as external auditors, we did not have access to information on how ideal candidates are defined or how thresholds are set. Without this information, we cannot assess outcomes-based fairness metrics. This means that critical questions of discrimination remain out of scope for this study. We caution that the adverse impact of human-in-the-loop hiring systems must be assessed on an employer-by-employer basis in order to account for crucial implementation details and differences in the context of use.
6 Conclusions

In this paper, we investigated the reliability of algorithmic personality tests used in hiring. We gave an overview of the key literature on psychometric testing applied to hiring and in algorithm auditing, and found that, although reliability is seen as a necessary condition for the validity of a psychometric instrument, it has not received substantial treatment in algorithm audits. Based on this observation, we developed a quantitative methodology, informed by psychometric theory and sociology, to audit the stability of black-box algorithms that predict personality for use in hiring. We then instantiated this methodology in an external audit of two systems, Humantic AI and Crystal, using a dataset of job applicant profiles collected through an IRB-approved study. We found that both systems lack reliability across key facets of measurement, and concluded that they cannot be considered valid personality assessment instruments.

The present study demonstrates that stability, though often overlooked in algorithm audits, is an accessible metric for external auditors. We found that stability is highly relevant to the application of personality prediction. Furthermore, because reliability is a prerequisite of validity, stability is in fact relevant whenever validity is. Importantly, we note that, while reliability is a necessary condition for validity, it is not a sufficient condition. Further evidence of domain-specific validity is essential to support the use of algorithmic personality tests in hiring.

Our methodology can be used by employers to make informed purchasing and usage decisions, and to better interpret algorithm outputs, by legislators to guide regulation, and by consumers to make informed decisions about how and when to disclose their information to potential employers. Furthermore, vendors and employers ought to be aware of the risks associated with using social media data in the hiring context.

Algorithmic audits must not be one-size-fits-all. The tendency of auditors, especially within the hiring domain, to rely on legal frameworks as a scoping mechanism is likely to leave important risks undetected. Current legal frameworks are insufficient; furthermore, legality does not equate to ethics. Instead, we recommend that auditors interrogate the assumptions operationalized by systems, and design audits accordingly.

Finally, we note that this work was conducted by an interdisciplinary team that included computer and data scientists, a sociologist, an industrial psychologist, and an investigative journalist. This collaboration was both necessary and challenging, requiring us to reconcile our approaches and methodological toolkits, forging new methods for interdisciplinary collaboration.

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7 Declarations

7.1 Funding

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7.2 Conflict of interest

The authors declare that they have no conflict of interest.

7.3 Availability of data and material

Anonymized datasets generated for and analysed during the current study are available at [https://github.com/DataResponsibly/hiring-stability-audit/tree/main/data](https://github.com/DataResponsibly/hiring-stability-audit/tree/main/data).

7.4 Code availability

Code for data cleaning and analysis is provided for replication. It is available at [https://github.com/DataResponsibly/hiring-stability-audit](https://github.com/DataResponsibly/hiring-stability-audit).

7.5 Ethics approval

All procedures performed involving human participants were in accordance with the ethical standards of the NYU institutional review board and with the 1964 Helsinki declaration and its later amendments or comparable ethical standards.

7.6 Consent to participate

Informed consent was obtained from all individual participants included in the study.

7.7 Consent for publication

Participants granted informed consent to publish information not containing identifiers, including personality profile results and demographic data.

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Table 6: Rank-order stability of Crystal DiSC scores, as measured by Spearman’s rank correlations. Reliabilities below 0.90 highlighted in yellow; those between 0.90 and 0.95 highlighted in lighter yellow. Results are discussed in Sections 4.6.2, 4.6.3, 4.6.4, 4.6.5, and 4.6.6.

| Facet                  | Input Versions                                    | N  | Dominance | Influence | Steadiness | Conscientiousness |
|------------------------|----------------------------------------------------|----|-----------|-----------|------------|-------------------|
| File Format            | Raw Text Resume (CRr1) vs. PDF Resume (CRp1)       | 89 | 0.8225    | 0.8260    | 0.9184     | 0.9114            |
|                        | Source Context                                     |    |           |           |            |                   |
|                        | PDF Resume (CRp1) vs. LinkedIn (CL1)               | 86 | 0.2335    | 0.5258    | 0.5103     | 0.3585            |
| Immediate Reproducibility | Raw Text Resume back-to-back (CRr2 vs. CRr3)     | 89 | 1.0000    | 1.0000    | 1.0000     | 1.0000            |
| Algorithm-Time         | Raw Text Resume 31 days apart (CRr1 vs. CRr2)     | 89 | 1.0000    | 1.0000    | 1.0000     | 1.0000            |
| Participant-Time       | LinkedIn 8-10 months apart (CL1 vs. CL2)          | 89 | 0.5314    | 0.7062    | 0.8676     | 0.7811            |
Table 7: Rank-order stability of humantic AI DiSC scores, as measured by Spearman's rank correlations. Reliabilities below 0.90 highlighted in yellow. Results are discussed in Sections 4.6.2, 4.6.3, 4.6.4, 4.6.5, and 4.6.6.

| Facet                  | Input Versions                                                                 | N  | Dominance | Influence | Steadiness | Calculativeness |
|------------------------|-------------------------------------------------------------------------------|----|-----------|-----------|------------|----------------|
| File Format            | De-Identified Resume (HRi1) vs. DOCX Resume (HRd1)                             | 89 | 0.9956    | 0.9924    | 0.9978     | 0.9959         |
| URL Embedding          | URL-Embedded Resume (HRu1) vs. De-Identified Resume (HRi1)                    | 86 | 0.3570    | 0.6253    | 0.5480     | 0.6878         |
| URL Embedding          | URL-Embedded Resume (HRu1) vs. LinkedIn (HL1)                                 | 83 | 0.1555    | 0.3382    | 0.6074     | 0.4701         |
| Source Context         | De-Identified Resume (HRi1) vs. LinkedIn (HL1)                                | 84 | 0.0903    | 0.2553    | 0.3941     | 0.3331         |
| Source Context         | Original Resume (HRo1) vs. LinkedIn (HL1)                                    | 84 | 0.1775    | 0.4016    | 0.6939     | 0.6249         |
| Source Context         | Original Resume (HRo1) vs. Twitter (HT1)                                     | 20 | -0.5211   | 0.1026    | 0.0382     | -0.1475        |
| Source Context         | LinkedIn (HL1) vs. Twitter (HT1)                                             | 18 | -0.1317   | 0.0203    | -0.1120    | -0.4329        |
| Immediate Reproducibility | De-Identified Resume back-to-back (HRi2 vs. HRi3)                      | 89 | 0.9999    | 1.0000    | 1.0000     | 1.0000         |
| Algorithm-Time         | De-Identified Resume 31 days apart (HRi1 vs. HRi2)                           | 89 | 0.9726    | 0.9948    | 0.9925     | 0.9980         |
| Participant-Time       | LinkedIn 7-9 months apart (HL1 vs. HL2)                                     | 88 | 0.2248    | 0.4186    | 0.6597     | 0.5827         |
| Participant-Time       | Twitter 7-9 months apart (HT1 vs. HT2)                                      | 21 | 1.0000    | 1.0000    | 1.0000     | 1.0000         |
Table 8: Rank-order stability of Humantic AI Big Five scores, as measured by Spearman’s rank correlations. Reliabilities below 0.90 highlighted in yellow. Results are discussed in Sections 4.6.2, 4.6.3, 4.6.4, 4.6.5, and 4.6.6.

| Facet                  | Input Versions                                      | N  | Openness | Conscientiousness | Extraversion | Agreeableness | Emotional Stability |
|------------------------|------------------------------------------------------|----|----------|-------------------|--------------|----------------|---------------------|
| File Format            | De-Identified Resume (HRi1) vs. DOCX Resume (HRd1)    | 89 | 0.9891   | 0.9936            | 0.9939       | 0.9927         | 0.9816              |
| URL Embedding          | URL-Embedded Resume (HRu1) vs. De-Identified Resume (HRi1) | 86 | 0.3988   | 0.3845            | 0.0772       | 0.4190         | 0.4040              |
| URL Embedding          | URL-Embedded Resume (HRu1) vs. LinkedIn (HL1)         | 83 | 0.6381   | 0.5470            | 0.5786       | 0.6839         | 0.7018              |
| Source Context         | De-Identified Resume (HRi1) vs. LinkedIn (HL1)        | 84 | 0.2180   | 0.1558            | 0.1198       | 0.2020         | 0.2186              |
| Source Context         | Original Resume (HRo1) vs. LinkedIn (HL1)            | 84 | 0.5985   | 0.7124            | 0.5827       | 0.6136         | 0.5990              |
| Source Context         | Original Resume (HRo1) vs. Twitter (HT1)             | 20 | -0.1768  | 0.2324            | -0.1128      | -0.2316        | 0.0692              |
| Source Context         | LinkedIn (HL1) vs. Twitter (HT1)                    | 18 | -0.2158  | 0.0000            | -0.1559      | -0.1517        | -0.1125             |
| Immediate Reproducibility | De-Identified Resume back-to-back (HRi2 vs. HRi3)    | 89 | 1.0000   | 1.0000            | 1.0000       | 0.9999         | 1.0000              |
| Algorithm-Time         | De-Identified Resume 31 days apart (HRi1 vs. HRi2)   | 89 | 0.9954   | 0.9969            | 0.9618       | 0.9921         | 0.9854              |
| Participant-Time       | LinkedIn 7-9 months apart (HL1 vs. HL2)             | 88 | 0.6879   | 0.6928            | 0.7301       | 0.7518         | 0.7678              |
| Participant-Time       | Twitter 7-9 months apart (HT1 vs. HT2)              | 21 | 1.0000   | 1.0000            | 1.0000       | 1.0000         | 1.0000              |
Table 9: Significance in locational instability of Crystal DiSC scores, as measured by two-tailed Wilcoxon signed-rank test p-values. Yellow highlighting indicates value below Bonferroni-corrected threshold based on $\alpha$ of 0.05. Results are discussed in Sections 4.6.2, 4.6.4, 4.6.5 and 4.6.6.

| Facet                      | Input Versions                                      | N  | Dominance | Influence | Steadiness | Conscientiousness |
|----------------------------|------------------------------------------------------|----|-----------|-----------|------------|-------------------|
| File Format                | Raw Text Resume (CRr1) vs. PDF Resume (CRp1)         | 89 | 0.5026    | 0.4208    | 0.0173     | 0.0370            |
| Source Context             | PDF Resume (CRp1) vs. LinkedIn (CL1)                 | 86 | 0.4190    | **0.0012**| 0.7010     | 0.8421            |
| Immediate Reproducibility  | Raw Text Resume back-to-back (CRr2 vs. CRr3)         | 89 | N/A       | N/A       | N/A        | N/A               |
| Algorithm-Time             | Raw Text Resume 31 days apart (CRr1 vs. CRr2)        | 89 | N/A       | N/A       | N/A        | N/A               |
| Participant-Time           | LinkedIn 8-10 months apart (CL1 vs. CL2)             | 89 | 0.7299    | 0.6518    | 0.3305     | 0.2870            |
Table 10: Significance in locational instability of Humantic AI DiSC scores, as measured by two-tailed Wilcoxon signed-rank test p-values. Yellow highlighting indicates value below Bonferroni-corrected threshold based on α of 0.05. Lighter yellow indicates p-value below Benjamini-Hochberg corrected threshold and above Bonferroni-corrected threshold. Results are discussed in Sections 4.6.2, 4.6.3, 4.6.4, 4.6.5, and 4.6.6.

| Facet                        | Input Versions                                      | N  | Dominance | Influence | Steadiness | Calculativeness |
|------------------------------|------------------------------------------------------|----|-----------|-----------|------------|----------------|
| **File Format**              | De-Identified Resume (HRi1) vs. DOCX Resume (HRd1)   | 89 | 0.2510    | 0.2940    | 0.4574     | 0.2539         |
| **URL Embedding**            | URL-Embedded Resume (HRu1) vs. De-Identified Resume (HRi1) | 86 | 0.0000    | 0.3194    | 0.0005     | 0.0047         |
| **URL Embedding**            | URL-Embedded Resume (HRu1) vs. LinkedIn (HL1)        | 83 | 0.0066    | 0.1825    | 0.5324     | 0.1213         |
| **Source Context**           | De-Identified Resume (HRi1) vs. LinkedIn (HL1)       | 84 | 0.0000    | 0.0580    | 0.0013     | 0.3259         |
| **Source Context**           | Original Resume (HRo1) vs. LinkedIn (HL1)            | 84 | 0.0000    | 0.0050    | 0.2299     | 0.5911         |
| **Source Context**           | Original Resume (HRo1) vs. Twitter (HT1)             | 20 | 0.5706    | 0.3118    | 0.1975     | 0.6874         |
| **Source Context**           | LinkedIn (HL1) vs. Twitter (HT1)                    | 18 | 0.0342    | 0.3247    | 0.6095     | 0.5539         |
| **Immediate Reproducibility**| De-Identified Resume back-to-back (HRi2 vs. HRi3)     | 89 | 0.3173    | 0.3173    | N/A        | N/A            |
| **Algorithm-Time**           | De-Identified Resume 31 days apart (HRi1 vs. HRi2)   | 89 | 0.1416    | 0.5971    | 0.5690     | 0.0307         |
| **Participant-Time**         | LinkedIn 7-9 months apart (HL1 vs. HL2)             | 88 | 0.0709    | 0.0800    | 0.3457     | 0.2969         |
| **Participant-Time**         | Twitter 7-9 months apart (HT1 vs. HT2)              | 21 | N/A       | N/A       | N/A        | N/A            |
Table 11: Significance in locational instability of Humantic AI Big Five scores, as measured by two-tailed Wilcoxon signed-rank test p-values. Yellow highlighting indicates value below Bonferroni-corrected threshold based on $\alpha$ of 0.05. Lighter yellow indicates p-value below Benjamini-Hochberg corrected threshold and above Bonferroni-corrected threshold. Results are discussed in Sections 4.6.2, 4.6.3, 4.6.4, 4.6.5, and 4.6.6.

| Facet                    | Input Versions                                      | N  | Openness | Conscientiousness | Extraversion | Agreeableness | Emotional Stability |
|--------------------------|-----------------------------------------------------|----|----------|-------------------|--------------|---------------|-------------------|
| File Format              | De-Identified Resume (HRi1) vs. DOCX Resume (HRd1)   | 89 | 0.7193   | 0.9248            | 0.5306       | 0.3003        | 0.9771            |
| URL Embedding            | URL-Embedded Resume (HRu1) vs. De-Identified Resume (HRi1) | 86 | 0.0025   | 0.0000            | 0.0000       | 0.0002        | 0.2214            |
| Source Context           | Original Resume (HRo1) vs. LinkedIn (HL1)            | 84 | 0.0777   | 0.3997            | 0.0000       | 0.1730        | 0.6718            |
| Source Context           | Original Resume (HRo1) vs. LinkedIn (HL1)            | 84 | 0.5300   | 0.0003            | 0.0001       | 0.0221        | 0.4553            |
| Source Context           | Original Resume (HRo1) vs. Twitter (HT1)             | 20 | 0.0121   | 0.0826            | 0.8983       | 0.0020        | 0.0010            |
| Source Context           | LinkedIn (HL1) vs. Twitter (HT1)                    | 18 | 0.0023   | 0.0047            | 0.0007       | 0.0047        | 0.0007            |
| Immediate Reproducibility| De-Identified Resume back-to-back (HRi2 vs. HRi3)    | 89 | 0.1797   | 0.3173            | 0.3173       | 0.6547        | 0.6547            |
| Algorithm-Time           | De-Identified Resume 31 days apart (HRi1 vs. HRi2)   | 89 | 0.0071   | 0.5314            | 0.2540       | 0.0516        | 0.2424            |
| Participant-Time         | LinkedIn 7-9 months apart (HL1 vs. HL2)             | 88 | 0.6487   | 0.0000            | 0.9615       | 0.0072        | 0.6011            |
| Participant-Time         | Twitter 7-9 months apart (HT1 vs. HT2)              | 21 | N/A      | N/A               | N/A          | N/A           | N/A               |
Fig. 10: Normalized L1 distance in Crystal DiSC scores across key facets. Results are discussed in Section 4.6.
Fig. 11: Normalized L1 distance in Human AI DiSC and Big Five scores across resume file formatting, URL-embedding, and source context. Results are discussed in Section 4.6.
Fig. 12: Normalized L1 distance in Humantic AI DiSC and Big Five scores across algorithm-time and participant-time. Results are discussed in Section 4.6.