

Methods for the Detection of Carelessly Invalid Responses in Survey Data

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Final Preprint Version

Journal of Experimental Social Psychology

Special Issue: “Rigorous and Replicable Methods in Social Psychology”

NOTE: This is a pre-print version of this article, and the final print version can be found here at <http://dx.doi.org/10.1016/j.jesp.2015.07.006> .

Author Note: The author would like to thank Katherine S. Corker, Brent M. Donnellan, Fred L. Oswald, and John F. Binning for valuable comments and suggestions on earlier drafts of this paper. In addition, the author would like to thank the editors of this special issue, Edward Lemay and Chuck Stangor, as well as two anonymous reviewers, for their valuable feedback and suggestions.

Abstract

Self-report data collections, particularly through online measures, are ubiquitous in both experimental and non-experimental psychology. Invalid data can be present in such data collections for a number of reasons. One reason is careless or insufficient effort (C/IE) responding. The past decade has seen a rise in research on techniques to detect and remove these data before normal analysis (Huang, Curran, Keeney, Poposki, & DeShon, 2012; Johnson, 2005; Meade & Craig, 2012). The rigorous use of these techniques is a valuable tool for the removal of error that can impact survey results (Huang, Liu, & Bowling, 2015). This research has encompassed a number of sub-fields of psychology, and this paper aims to integrate different perspectives into a review and assessment of current techniques, an introduction of new techniques, and a generation of recommendations for practical use. Concerns about C/IE responding are a factor any time self-report data are collected, and all such researchers should be well-versed on methods to detect this pattern of response.

Self-report psychological data are a ubiquitous source of information in many areas of psychological research. These self-reports are used in both experimental and non-experimental psychological research to measure a wide range of psychological constructs. Online data collection has made this process more accessible and widespread in recent years, but there are also many sources where error can enter this process. One type of this error comes from respondents who provide invalid data to survey questions. Fortunately, this is error that can be removed from the survey data collection process in order to produce more stable and consistent results (Huang, Liu, & Bowling, 2015; Maniaci & Rogge, 2014).

There are a number of reasons why research participants may provide responses that are in some way invalid; that is, data that do not represent actual ‘true’ values. Johnson (2005) identified three main classes of this invalid data: (1) linguistic incompetence or misunderstanding, (2) misrepresentation, and (3) careless or inattentive response. Linguistic incompetence deals with the construction of items and the process by which a survey is aimed (properly or improperly) at the population of interest. Misrepresentation deals with the issue of cheating, or faking (Griffith & Peterson, 2006), behaviors that are most likely on high-stakes surveys or tests.

Careless or inattentive responding deals with a problem that potentially impacts anyone who does research with participants in low- to medium-stakes settings (e.g. subject pool participants, MTurk workers, less than fully motivated workplace samples). Unfortunately, some participants will simply not put in the effort required to respond accurately or thoughtfully to all questions asked of them. The inclusion of these responses into a set of otherwise accurate data can have a host of unexpected and undesired effects on relationships being examined (Huang et al., 2015).

This problem is related but distinct from other problems found in data, such as missingness. In effect, C/IE responders are missing data that is not actually missing. C/IE responders have provided a response when they might have well left that response blank. While some methods of dealing with missingness may be useful after these individuals are removed, this is beyond the scope of this paper, as these individuals must first be detected.

Fortunately, many methods have been established to detect and remove these invalid responders from datasets. Although there have been a number of reviews of some of these methods (e.g., Desimone, Harms, & Desimone, 2015; Johnson, 2005), this paper aims to summarize, connect, and extend these modern methodological tools, as well as to explain actionable ways to implement them. This will be accomplished by first summarizing this type of data and general concepts of detection before outlining specific techniques in detail. Following this, recommendations for use and common issues will be highlighted with the aim to help increase understanding of these methods. It will be recommended that a number of these methods be used in series with the goal of each to reduce the invalid data that can be best detected by that particular technique, and that this process be transparent and replicable in any given case.

What Does Careless or Inattentive Data Look Like?

One of the most telling ways to examine the perceived impact of these responses is to look at the language used to describe them. For many years, individuals who exhibited this method of response were called *random responders* (e.g., Beach, 1989). This was influenced by the notion that responses from these individuals were produced by some completely random process, such as flipping a coin or rolling a die. From a scale validity standpoint this meant that random error was introduced into data. This is partly true, but not the complete story.

More recent research has shown that even those survey-takers instructed to complete a survey as quickly as possible without thinking will still produce some non-random patterns in their data. These patterns, on the surface, can mimic that of thoughtful response, without the commitment of time and thought (Huang, Curran, Keeney, Poposki, & DeShon, 2012). These data can take on many patterns, and each of these can have a drastically different impact on survey outcomes such as means, deviations, and reliability (Curran & Kotrba, 2012). Counter-intuitively, the characteristics of the valid data in these situations can be just as important as the characteristics of the invalid data. Huang et al. (2015), for instance, have shown that the locations of scale means in valid data will cause the introduction of careless data to produce differential effects in study outcomes.

Error is being produced by these responders, just not necessarily through a fully random process. Different motives may lead to different response strategies and patterns, or the same motives may produce different patterns from different individuals. These motives have the potential to shift this error away from truly random error to something more patterned. The language used to describe these individuals has similarly shifted over time from the concept of a ‘random’ responder to that of a *careless* (Meade & Craig, 2012) or *insufficient effort* (Huang et al., 2012) responder (C/IE responder). These classifications reflect the fact that some effort may still be exerted, simply not enough for thoughtful ‘true’ response. This reflects the underlying construct validity problem that these individuals represent: lack of attention and carelessness should moderate the linkage between an individual’s actual traits and states and the self-report manifestation of those same constructs as measured.

Practical estimates of C/IE responders in typical studies range from as low as about 1% (Gough & Bradley, 1996) to as high as about 30% (Burns, Christiansen, Morris, Periard, &

Coaster, 2014), with some consensus among research studies suggesting that the modal rate is somewhere near 8-12% (Curran, Kotrba, & Denison, 2010; DeRight & Jorgensen, 2015; Maniaci & Rogge, 2014; Meade & Craig, 2012). Inclusion of even a low proportion of these responses in datasets impact the usefulness of those data (Curran & Kotrba, 2012; Huang et al., 2015; Maniaci & Rogge, 2014; Woods, 2006), and it is clear that efforts should be made to either deter such data or remove it following collection.

C/IE Response and Replicability

If there is one consensus that all C/IE researchers can agree on it is that these types of responders exist. This may seem trivial, but the simple acceptance that these behaviors are not an imagined phenomenon raises more than the simple question above regarding the typical rate of these individuals. It raises the question of how much this behavior varies across studies.

There has been little work on concretely establishing the range of these values. Hauser and Schwarz (in press) examined the failure rates on instructional manipulation checks, or IMCs, across a number of different samples from Klein, et al (2014). This examination revealed a range of failure rates, by sample, ranging from less than 10% to almost 50%. Such a degree of variance is particularly disconcerting when coupled with the results of Huang et al. (2015): that the inclusion of C/IE responders can both inflate and attenuate statistical effects, and that this impact is related to the proportion of C/IE responders in the sample. In their own words: “the presence of 10% (and even 5%) ... can *cause* spurious relationships among otherwise uncorrelated measures” (Huang et al., 2015, p. 9).

If ignored, these C/IE responders pose a great threat to replicability in a number of ways. First and foremost, studies that inadvertently draw a larger proportion of these responders have a greater potential for false positives or false negatives. This may cause true findings to be

hidden by these C/IE responses, and cause otherwise successful studies to languish in the file drawer. This may also cause spurious results to be published that are only a product of the high rate of C/IE responders in that sample, and not true underlying effects.

In the best case following the first example, other researchers may later attempt the same study and have success, causing only a potential duplication of effort. In the second example, a best case is more difficult to come by. Such a spurious result may be questioned through replications, but without clear understanding of C/IE responders as the mechanism of that discrepancy these conflicting results may take volumes of time and effort to untangle. Beyond this, replications themselves may be plagued by the same problem, either confirming or denying an effect because of the proportion of C/IE responders in their sample.

This may seem like a grave proclamation, and it is. C/IE responders are a potential source of unbiased noise, error that can shift results in any direction. If rates of C/IE responders were fairly constant this would be bad enough news, but the prospect that rates can vary as much as they appear to is dire indeed. Huang et al. (2014) are well served to identify these responses as an ‘insidious confound in survey data.’

Detecting Careless/Insufficient Effort (C/IE) Responders

There are many potential causes of invalid data in general research. Invalid data on high-stakes tests and surveys, due to the valuable nature of these measures (e.g., clinical assessment and personnel selection), have been studied in depth for many decades (e.g., Berry, Baer, & Harris, 1991; Butcher, Dahlstrom, Graham, Tellegen, & Kaemmer, 1989; Birkeland, Manson, Kisamore, Brannick, & Smith, 2006; Dunnette, McCartney, Carlson, & Kirchner, 1962; Orpen, 1971). Researchers who study faking and cheating have the benefit that these behaviors are only manifested in very specific ways. Individuals do not cheat or fake to produce unwanted

outcomes; they cheat or fake to produce desired outcomes (a job, a particular clinical diagnosis).

While there may be different manifestations of this behavior that potentially reflect a spectrum of motivation (e.g., different individuals may fake illness or fake wellness on the same clinical inventory), faking still represents an effortful manipulation of responses toward some goal.

C/IE responding does not have such a clearly referenced goal that can be distinguished in the outcomes of survey response. The goal of C/IE responding is not one that is achieved by the application of effort, but rather through the minimization of effort. This separates it from other similar but distinct concepts such as social desirability or impression management, where effort is being applied to alter responses toward some goal. Because this focusing end goal is absent (or the goal is merely to save effort or time), C/IE response patterns can take a number of forms. Participants may manifest C/IE response by selecting some singular response option to every question. They may also choose one option a majority of the time, but occasionally vary that response to one choice higher or lower on the scale. It is also possible that a participant may simply provide something close to random data. All of these represent similar core motivations (or lack of motivations), but are manifested in drastically different ways. It is therefore hard to produce one singular method that captures all cases, and this makes C/IE detection inherently complex.

Research on the detection of invalid data from C/IE responders has instead produced a number of potential methods, each aimed at identifying and potentially eliminating a certain pattern of C/IE response that may appear valid to other methods. For instance, several methods assess consistency of response as a negative characteristic (e.g., long-string analysis), whereas other methods use it as a positive indicator (e.g., odd-even consistency). The disagreement

between these techniques is simple reflective of the fact that they are assessing different indicators of C/IE response. These methods will be discussed in detail in the following sections.

The known negative relationship between different metrics (Meade & Craig, 2012) means that the differential application of techniques has the potential to remove certain sets of C/IE responders while retaining others who are equally invalid. For instance, simply using a method that examines consistency of response as a positive characteristic and neglecting a method that looks at excess consistency of response as a negative characteristic is likely to remove those individuals providing highly variable invalid responses while retaining those that are functionally invariant (e.g., all 'Strongly Agree' responses). The use of any of these techniques should not be applied in a vacuum void of other techniques. In fact, partial application or understanding of these methods has the possibility to weaken existing effects or even create spurious effects. While these are worst case scenarios, it is worth establishing robust data screening practices even for those situations where the removal of such cases does not change the outcomes of the study. If the data being removed are truly invalid, then this resulting error, a hindrance to future replicability, is still worth removing (Huang et al., 2015).

The following section will detail a number of existing and novel methods for the creation of within-person statistics that can be used in the removal of different types of invalid responding. These will follow a few classifications, from simple time of response and long patterns of the same response to calculations of internal consistency of individual responses and the introduction of specialized items and instructions in scales as potential traps for these C/IE responders.

Speed of response: Response time

Response time, the time it takes for an individual to respond to a set of items, is perhaps the most widely used tool for the elimination of C/IE responders. It is the most likely to be used on an intuitive basis even by those who have no knowledge of the C/IE literature. This intuitive use of response time can be independently derived by the practical extension of one simple assumption: there exists a minimum time needed to validly complete a survey.

Normal or average response time will obviously be different for different surveys. Response time is likely to correlate with number of items, but some items take longer to complete than others. Accordingly, it is difficult to create concrete rules for response time that differ from normal outlier analysis unless the status of the participant as a C/IE responder is already known (unlikely), or experimentally manipulated (e.g., Huang et al., 2012).

To its credit, response time does appear to be one of the hardest metrics of C/IE responding to fool in a way consistent with C/IE behaviors because of one simple fact: a presumed key underlying motive of C/IE responders is finishing the assessment as quickly as possible. When participants are instructed to respond as if they do not care about the survey but also do not want to be identified as C/IE responders (as in Huang et al., 2012), those participants are able to produce response patterns that look very much like normal responses, while still having response times much more consistent with participants instructed to simply respond as if they do not care at all. The best way to fool response time is to stall at certain points of the study; if this was occurring as a valid strategy it is hard to understand why participants wouldn't simply accomplish this stalling by validly responding to questions.

In many aspects, response time is fairly simple. At the same time, there is a reasonable degree of normal within-person variation regarding how quickly individuals can respond to a

survey. Not surprising to anyone who has done survey research is the idea that the time that it takes one individual to respond thoughtfully may be drastically different than the time it takes another. Not quite as obvious is the fact that variation also exists in the time that it takes individuals to respond carelessly or with insufficient effort.

Other methods of C/IE detection have benefited from simulation of response patterns, but response time is particularly difficult to simulate due to the host of underlying assumptions regarding the time it takes an individual to complete a survey. That said, general effects and difficulty of utilizing this method can be demonstrated with a simple sampling exercise. The reader can easily replicate this exercise with a variety of modified parameters, but for the purposes of this paper we will consider a survey that takes 10 minutes (600 seconds) to complete, *on average*, for normal, thoughtful respondents. We can, as a best-case scenario, imagine that the standard deviation of these respondents is relatively small. For these purposes, we can use a standard deviation of 100 seconds, or a little more than a minute and a half.

To simulate C/IE responders, we can also introduce 10% of individuals who have answered faster, on average, than our normal respondents. We can attempt this at a number of different effect sizes using Cohen's d , and in each case add 10 samples from these random normal distributions to the overall distribution. Figure 1 demonstrates this inclusion at three different levels: $d = 1$ (mean of 500 seconds for C/IE responders), $d = 2$ (mean of 400 seconds for C/IE responders), and $d = 3$ (mean of 300 seconds for C/IE responders). Because we are simulating this data we can examine both how this would appear to a researcher in practice (left column of figure 1), but also how this actual differential overlap is occurring by differentially coloring the normal data darker and leaving the C/IE data lighter (right column of figure 1).

The right column of figure 1 demonstrates how difficult the removal of all of these C/IE individuals is in even fairly extreme cases; $d = 3$ represents a case where C/IE responders are, on average, completing their responses in half the time of normal participants (300 seconds vs 600 seconds). Such difficulty comes from the fact that removal of C/IE responders in this case involves the separation of two overlapping normal (or, to further complicate things, two potentially non-normal or unknown) distributions from each other. This process demands thoughtful consideration of acceptable tradeoffs between Type I and Type II error (Curran, Kotrba, & Denison, 2010).

This should not, however, be a deterrent to collecting response time. If researchers can collect response time (e.g., through any online or computerized collection), it should be collected. Even if it is not used, it comes at minimal cost. Further, once a researcher has failed to collect response time it is no longer possible to go back and retroactively obtain it.

Huang et al. (2012) have suggested a cut score for response time at 2 seconds an item, which anecdotally appears to be gaining some informal traction in the field. While this may be a somewhat conservative cut score, and leave some careless responders in a sample, it will be the stance of this paper that researchers err in this way on all techniques. At the same time, researchers should consider the possibility that a thoughtful responder may be able to provide such a quick response on their specific survey instrument. Even established cut scores such as this should be applied with care and thoughtfulness.

Finally, while it is far too complex an issue to adequately cover here, it should be noted that analysis of response time in C/IE detection is currently a one-tail analysis. Respondents are only examined for speed of response that is too fast. The other tail of this distribution, response times that are too slow, is also potentially worthwhile to examine. However, many more factors

may contribute to a response time that is too slow, and no research to date has examined this situation. It may be that any added gain of examining these ‘too slow’ responders is not worth the added difficulty of doing so.

Invariability: Long-String Analysis

Perhaps the next most intuitive technique for use in the detection of C/IE responders is the analysis of strings of responses, known in the literature as ‘long-string analysis’ or ‘response pattern indices’ (Huang et al., 2012; Meade & Craig, 2012). This technique seems to have formally begun with Johnson’s (2005) use of a borrowed technique to later be described in the work of Costa and McCrae (2008). This technique involves examining the longest string of identical responses from each participant. This may be calculated singularly, on the response option that is selected the most frequently (Huang et al., 2012; Meade & Craig, 2012), or multiple times, once on each option for each participant (Johnson, 2005). Moreover, there are a number of ways to treat this calculated value, ranging from a pure count to an average of longest strings per page to a value weighted by the overall average length of strings of that response.

For example, take the following pattern of response on a set of 10 items, assuming a 1 to 5 Likert scale from strongly disagree to strongly agree.

[3, 3, 4, 4, 4, 3, 5, 3, 3, 4]

In this case, the longest string of the same response is on the third to fifth responses. It is a sequence of ‘4’ responses that is three in length. If only examining the length of the longest string of responses, the score for this individual would be a value of 3. If each response option was examined, this individual would have a set of scores for the responses from 1 to 5 as follows: 0, 0, 2, 3, 1. Take note that there are actually more ‘3’ responses in this example than

there are '4' responses, it just so happens that there are more '4' responses in an unbroken sequence. This unbroken sequence is what this method of long-string detection is identifying.

This technique is fairly straightforward; the assumption is that those individuals who are responding carelessly may do so by choosing the same response option to every question. The extension of this assumption is that individuals who are responding carefully, and with sufficient effort, will not use the same response option for long periods of time. This highlights the fact that this technique, like many of the techniques that will be discussed in this paper, is a form-dependent statistic. This simply means that raw scores will vary as a product of the form used to collect data. A 10-item scale has 10 as an upper bound on this statistic, while a 20-item scale has an upper bound of 20. Because values are simple counts of sequential matches, values will always take on integers bounded by 1 and the length of the assessment. In addition, a scale that has questions of varying intensity (e.g. 'I have a lot of friends' vs 'I make friends with every person I meet') should drive careful responders to change their response option more frequently than on a scale that has questions all geared at roughly the same level.

This technique also tends to be dependent on response option. That is, the typical frequency of a long-string on certain response options tends to be higher than on others; 'Agree' is a very popular choice on a typical scale (Curran, Kotrba, & Denison, 2010; Johnson, 2005). This property will vary from scale to scale and sample to sample (Johnson, 2005). Because of these factors, long string analysis can be difficult to compare across different data collections without engaging in some degree of scaling.

Additionally, consider the limitations of this technique on shorter scales (e.g., the 20-item Mini-IPIP by Donnellan, Oswald, Baird, & Lucas, 2006). Unless detailed measurement of some construct or group of related constructs is a central focus of a study, shorter scales of this type

are likely to be used. This has the effect of compressing, through ceiling and/or floor effects, the simple number of categories into which individuals can be grouped. As mentioned earlier, the longest string of responses on any given scale has a lower bound of 1, an upper bound of the number of items on that scale, and must take a value of an integer.

This technique captures what may be considered the low-hanging fruit of C/IE responders: those who take little or no effort to change their response throughout a survey. Similar to response time, however, the ease with which this technique can distinguish between valid and invalid responders will decrease as respondents start to drift from this ideal case of fully invariant response. Similar overlapping distributions, as in figure 1, need to be pulled apart to distinguish responders, and error in both directions (Type I & Type II) should always be a prime concern. Responders who take the time to occasionally vary their response in minimal ways may also easily fool this method. This is a limitation inherent in this method, and any attempts to fix such a flaw may be best compared to the idea of overfitting a model. Instead, the acknowledgement that each technique has blind spots is precisely the reason for using a number of techniques in concert, each balanced to provide full coverage of these potential pitfalls.

Long-string analysis has the potential to remove some of the worst of the worst responders, but may have difficulty doing much more. In conjunction with a screen for the most extreme response times, these two techniques are put forth as a recommended bare minimum for the removal of C/IE responders, and a good start before the implementation of more complex techniques. Despite some limitations, the removal of these worst responders is better than nothing at all.

Due to the scale-specific nature of this technique, there are no established global cut scores in place for it. In keeping with the approach of Huang et al. (2012) regarding a

conservative cut score for response time, this paper will suggest baseline rule of thumb that individuals with a string of consistent responses equal or greater than half the length of the total scale be considered as C/IE responders by this technique. Obviously this is not the best cut score for all scales (notably, this may be too strict on scales with very highly similar items, or too lenient on scales with easily recognized item variation), but at least provides a point for researchers to begin a deeper consideration.

Outlier Analysis: Mahalanobis Distance

Outlier analysis is a fairly simple concept that is often taught in the very early stages of statistical training. Broadly, outliers can be simply considered unusual data points relative to the remainder of a distribution (Peck & Devore, 2012). Outliers can exist for many reasons, and C/IE responding is certainly among them. Individuals who are responding without sufficient effort are likely to differ from their thoughtful counterparts in some way, and it is not unreasonable to believe that this difference may manifest in the presence of outliers.

Basic methods of outlier analysis examine one value from a distribution relative to other values in that distribution. For example, a value that exists 1.5 standard deviations from the mean (or 1.5 interquartile ranges from the median) is commonly treated as an outlier when cleaning data, or producing visualizations such as boxplots (Peck & Devore, 2012). This technique could potentially be used as a C/IE method to detect unusual responses to each item of a scale, or even to the overall scale score. This is not common practice in C/IE response detection for several reasons. Using this technique at the item level produces a situation where only some subset of item responses are considered valid. Using it at the scale level neglects the fact that there are many reasons why individuals may exist as basic statistical outliers. Among these reasons is individuals' actual standing on the latent trait being measured. Simple outlier analysis at the

scale level also assumes that C/IE responders should exist in the tails of such a score distribution. This would only be the case if their method of response was to select many of the same high or low response options, a tactic which is already targeted in a more appropriate way using techniques such as long string analysis.

There are techniques of outlier analysis that exist beyond these simple methods.

Mahalanobis distance, or Mahalanobis D, is a multivariate outlier technique which is a simple extension of normal outlier analysis into multivariate space (Mahalanobis, 1936). This technique has shown some promise as a method of C/IE detection (Ehlers, Greene-Shortridge, Weekley, & Zajack, 2009; Maniaci & Rogge, 2014; Meade & Craig, 2012). The easiest method of explaining Mahalanobis distance is through the two variable case, which will be outlined here. For a much deeper explanation of Mahalanobis distance, see Maesschalck, Jouan-Rimbaud, and Massart (2000).

In a one-dimensional case, outliers are examined by their distance to some center of a distribution. This is most often through the use of means and standard deviations, as above. Points in this distribution exist in a one-dimensional space (a line), and so the distance from the center can be expressed as a simple difference, such as the difference between an observation and the mean:

$$distance = x - \bar{x}$$

This is a distance which is still in the units of the original scale, but can be transformed into a unitless distance by simple scaling the distance by the standard deviation, producing a z-score:

$$z - score = \frac{x - \bar{x}}{S}$$

These same concepts can be applied when examining a multivariate system. Extensions of these concepts reflect the shift from distances on a one-dimensional line to distances on a two-dimensional plane, and beyond. Instead of points on a line making up a distribution, a two-dimensional multivariate (bivariate) distribution is defined as points on a plane: a scatterplot. The simple distance of any point from the multivariate center of this distribution (\bar{x}_1, \bar{x}_2) can be found by:

$$distance = \sqrt{(x_1 - \bar{x}_1)^2 + (x_2 - \bar{x}_2)^2}$$

This distance, just like the raw distance in the one-dimensional case, does not take into account the standard deviations of each of these variables. Movement along x_1 is treated the same as movement along x_2 , even if the distribution of x_1 is much more tightly clustered. In contrast, the Mahalanobis distance takes into account the underlying variance structure of this by incorporating the covariance matrix of these two variables (C). For a value x_i in this person by item matrix, X, Mahalanobis D can thus be found by the following equation (Maesschalck, Jouan-Rimbaud, & Massart, 2000):

$$Mahalanobis\ D = \sqrt{(x_i - \bar{x})C_x^{-1}(x_i - \bar{x})^T}$$

Again, the reader is directed to Maesschalck, Jouan-Rimbaud, and Massart (2000) for a much deeper mathematical discussion. What is important for this current discussion is the conceptual understanding that Mahalanobis D can be used as a method of multivariate outlier analysis in self-report survey data (Ehlers, et al., 2009; Maniaci & Rogge, 2014; Meade & Craig, 2012). Instead of knowing that an individual is in the tails of a distribution on one item, Mahalanobis D can inform a researcher that an individual is on the outskirts of the multivariate distribution formed by responses to all items.

This metric does seem to correlate well with some other metrics of C/IE response detection (Maniaci & Rogge, 2014; Meade & Craig, 2012), but has some limitations. First, as Meade & Craig (2012) note, Mahalanobis D is a computationally intensive procedure, and they were forced to run a series of reduced calculations on their data, which were then averaged. This is a limitation which will be reduced by the march of time and technology, but worth noting. Second, this technique is built from the foundation of outlier analysis using spread from center metrics, such as z-score. These metrics rely on a certain degree of normality in the data, and can be influenced by deviations from normality in items, as well as from too much normality in C/IE responders (Meade & Craig, 2012). Again, C/IE responders may be well versed at responding near the midpoint of other responses.

Conceptually, these violations of normality could manifest in a number of different ways. Meade and Craig (2012) highlight the case where item distributions are highly skewed to either end of the scale by floor and ceiling effects. In these cases, Mahalanobis D performed exceptionally well, as C/IE responses on the other end of the scale stood out very clearly. Unlike the earlier examples with univariate outliers and the number line, the practical application of Mahalanobis D in C/IE responding has to deal with the fact that responses are on a scale with constrained limits. While not encountered by Meade and Craig (2012), another possible situation is items in which all response options are used, creating more of a uniform distribution at the item level. In this case, multivariate outliers may only exist outside the bounds of the scale (i.e., above and below the highest and lowest response options, respectively).

Mahalanobis distance has only been examined in a small handful of studies involving C/IE responding to this point, and full understanding of its properties are still unclear. As a fairly

untested metric for C/IE response, it should be used as a way to flag individuals for deeper examination, but may not yet be a clear means of eliminating C/IE responders on its own.

Individual Consistency

Individual consistency, in the case of C/IE response detection, is a reference to the consistency of a response string within an individual. There are a number of techniques which all measure this consistency, and which will each be examined in each of the following sections. The underlying assumption of these individual consistency methods is, simply put, that an attentive respondent provides a pattern of responses that is internally consistent.

Consider early research, when responses from individuals were still assumed to be almost entirely random. It is a fair assumption that an individual's complete response pattern, full of such random data, should be easily identifiable as noise against the background of valid response. This is the genesis of such techniques, though underlying assumptions of randomness are no longer so simplistic. All of these techniques to be discussed in the next few sections are founded on the assumption that an individual's response string has stable internal relationships among responses to individual items. This is by virtue of the assumed unidimensional properties of scales, and the positive inter-item correlations this unidimensionality implies. Many of these techniques stem from consistency measures from the sample level, such as reliability, simply applied to the individual person as a unit of analysis.

It is worth noting, even at the point of overstating it, that all of these techniques are designed to create within-person statistics that can then be compared at the sample level. This is a relatively uncommon mindset, and an easy place to get lost in this discussion. In the description of the following techniques, the use of the term individual consistency is indicative of some within-person consistency. While this is conceptually a type of internal consistency it should not

be confused with broader aggregate techniques that address scale-level or between-subjects consistency of survey items.

Individual Consistency: Odd-Even Consistency and Resampled Individual Reliability

One of the simplest individual consistency techniques is known as odd-even consistency (Meade & Craig, 2012) or individual reliability (Huang et al., 2012, Huang et al., 2014, Jackson, 1977; Johnson, 2005). The use of these two terms to describe the same technique is indicative of concepts potentially lost in the distinction between the two. This distinction is difficult to explain without describing the current technique which shares these names, so such description is a natural next step.

In current use, odd-even consistency (the name that will be suggested for use with this version of the technique) is computed by breaking each individual's responses on each unidimensional subscale or facet of a measure into two sets of items: one set of the responses to even items, and one set of the responses to odd items. If reverse worded items are present, they are recoded to reflect the direction of the other items before this scoring. These halved subscales are then averaged to provide an approximation of the individual's score on that subset of items. These values are paired by scale; the number of pairs produced is dependent on the number of unidimensional groups of items that can be created in the larger scale. This results in two vectors for further computation, for each person: [scale 1A, scale 2A, scale 3A...] and [scale 1B, scale 2B, scale 3B...]. These two vectors are then correlated, producing a correlation for that individual that acts as his or her score on this metric. This correlation coefficient can be corrected for decreased length of the scale using the Spearman-Brown prophecy formula (Johnson, 2005). This odd-even consistency coefficient can be computed on a single scale with correlated facets, or across a series of similar scales using the same response format (e.g. Likert).

Although Jackson (1976; 1977) did make suggestions about the potential scores on this metric to be used to eliminate response strings, this cut score seems to be fairly scale dependent, working in some studies but being all but unusable in others (Curran et al., 2010). This scale dependency should be related to the degree of correlation between items, as well as the number of subscales that can be created. Even if subscales existed as two item pairs (one item subscales are useless here), the correlation generated has a maximum N of half the number of items in the total survey. In such an extreme setting (not recommended), item scores are serving as the averages of scale scores, and this technique is effectively no more than an application of semantic synonyms, which will be described later in this paper. The fact that the sample size for this correlation is constrained not by the size of the sample, but instead the number of subscales and/or items in the scale, is an important limitation of this technique.

Despite the monumental shift in computing power since the original establishment of odd-even correlations, little has changed in its calculation. The selection of one singular pair of items to split scales (odd items vs even items) was most likely a simple product of the time, accepted as best practice over the resulting decades. There is much room for technological and computational improvement in this concept without changing the core of the idea: items measuring the same latent construct should positively correlate within persons. This idea is what will be considered here as the broader concept of *individual reliability*, of which odd-even consistency is a specific case.

The overarching concept of individual reliability takes advantage of the fact that items from the same scale should be measuring the same concepts, and the scale scores generated by these items should be positively correlated within an individual. Odd-even consistency, again, is

simply a specific case of this more general concept that utilizes only one (constant) pair of item sets.

In the simplest case, let us start with a unidimensional scale of 18 items. If we also imagine that this scale contains three sequential subscales, A, B, and C, we can find the components of the odd-even subscales for the j^{th} person as follows:

$$\begin{aligned} A_{\text{odd}} &= (1_j + 3_j + 5_j)/3 & A_{\text{even}} &= (2_j + 4_j + 6_j)/3 \\ B_{\text{odd}} &= (7_j + 9_j + 11_j)/3 & B_{\text{even}} &= (8_j + 10_j + 12_j)/3 \\ C_{\text{odd}} &= (13_j + 15_j + 17_j)/3 & C_{\text{even}} &= (14_j + 16_j + 18_j)/3 \end{aligned}$$

The score for this person in terms of odd-even consistency is then simply the correlation between the vectors $(A_{\text{odd}}, B_{\text{odd}}, C_{\text{odd}})$ and $(A_{\text{even}}, B_{\text{even}}, C_{\text{even}})$, corrected with the Spearman-Brown prophecy formula, if desired. Take note that it is a mathematical limitation of this technique that variance must be present at this halved-scale level. Fortunately, an individual who provides no variance (and is thus unscorable on this technique) would be easily caught by other techniques, such as long-string analysis.

Again, this would appear to be a clever simplification of the era, one that is no longer necessary. Keep in mind, this is not a sample correlation, but a correlation computed for each individual in the sample. While it might seem fairly straightforward to carry out these correlations for every individual in a sample today, imagine doing so nearly 40 years ago (as in Jackson, 1977). The selection of one stable specific case (odd-even consistency) would have been an impressive undertaking of its own accord, and it is not surprising that researchers of the era stopped here.

That said, modern researchers are awash with computational power, and no longer need to be constrained to such 20th century thinking. When considering the broader concept of

individual reliability, a subscale score produced from a random half of any subscale paired with the subscale of the remaining half should still assess the same underlying consistency as is assessed with odd-even consistency. For instance, items in the above example were associated with scales in the following manner:

$$\begin{aligned} A_{\text{odd}} &= (1_j \ 3_j \ 5_j) & A_{\text{even}} &= (2_j \ 4_j \ 6_j) \\ B_{\text{odd}} &= (7_j \ 9_j \ 11_j) & B_{\text{even}} &= (8_j \ 10_j \ 12_j) \\ C_{\text{odd}} &= (13_j \ 15_j \ 17_j) & C_{\text{even}} &= (14_j \ 16_j \ 18_j) \end{aligned}$$

There is nothing inherently special about these groupings, and the same calculations should hold for randomly drawn halves of subscales paired with their remaining halves, as in the following:

$$\begin{aligned} A_{\text{odd}} &= (1_j \ 2_j \ 4_j) & A_{\text{even}} &= (3_j \ 5_j \ 6_j) \\ B_{\text{odd}} &= (10_j \ 11_j \ 12_j) & B_{\text{even}} &= (7_j \ 8_j \ 9_j) \\ C_{\text{odd}} &= (13_j \ 16_j \ 17_j) & C_{\text{even}} &= (14_j \ 15_j \ 18_j) \end{aligned}$$

The core idea of *individual reliability* is the notion that responses should be similar on items measuring the same construct, and these two sets of pairings still honor this notion. It is not simply *these* pairs of items, however, but rather the mechanism by which they were generated. By simply randomly sampling, without replacement, the integer distribution between 1 and the total number of items in each subscale, the researcher can create a vast number of these pairings.

No one random pairing of these items should be any better than what is already in practice: the standard use of odd and even paired sets within each subscale. The benefit of modern computational power is that we do not need to use just one of these randomly generated pairings. We can instead use as many as we want, then average the result. This is simply the

application of resampling and bootstrapping to the core of Jackson's (1976; 1977) original methodology meant to assess the deeper concept of individual reliability.

This new method of *Resampled Individual Reliability* (RIR), proposed here, provides a more robust picture of the consistency and reliability of an individual's responses to items measuring any given unidimensional construct. The use of any one paired set of these items may be influenced by random odd pairings; this resampling technique should allow this random error to cancel out over the averaging of multiple draws.

These random items are not important so much as is the system by which they were generated. Through this, RIR is presented as a more rigorous implementation of the original spirit of individual reliability than can be obtained with the simple specific case implementation of odd-even consistency. While values from RIR should correlate highly with values computed from odd-even consistency, validating RIR as a means of C/IE detection is beyond the scope of this paper. Initial cut score benchmarks should be identical to those used in odd-even consistency, but informed by future experience and practice. Researchers interested in use of this method may explore this question, and for now, use this new technique with due caution.

Individual Consistency: Semantic and Psychometric Antonyms/Synonyms

The next family of techniques involves four techniques that are the result of crossing two dichotomous ideas. The first part of this is the simple distinction between antonyms and synonyms. Antonyms utilize pairs of opposite items, whereas synonyms utilize pairs of similar items. The second distinction is between semantic pairs and psychometric pairs. Semantic pairs are those that are paired from a purely linguistic approach (e.g., happy/sad), whereas psychometric pairs are those that are paired from a data-driven approach to be explained below.

The crossing of these concepts produces four different C/IE metrics: (1) psychometric antonyms, (2) psychometric synonyms, (3) semantic antonyms, and (4) semantic synonyms.

The emergence of this family of techniques for C/IE detection appears to have been with semantic antonyms, that is, pairs of items that are semantically opposite (Goldberg & Kilkowski, 1985). These pairs are determined to be opposite simply on their content, and such pairs can be (and should be) created in the absence of data. Semantic antonyms, and the related semantic synonyms, are a priori pairings. The natural extension from a priori pairings to data-driven pairings led to the examination of psychometric antonyms, pairs of opposite items that are found by searching for large negative correlations between individual items in the full sample or in a secondary dataset (Johnson, 2005). There is no concrete value of correlation magnitude that is considered large enough, leaving the pairing of these items up to the largest correlations found in the data. It is the opinion of this author that these correlations should have a magnitude of at least .60, but there is no firm basis for this rule of thumb.

Given this opposite linguistic meaning between items in a pair, and/or negative correlation in non-C/IE samples, it can be assumed that more careful responders should answer oppositely to these pairs of antonyms, whereas more C/IE responders should not. That is, the within-person correlation between all of the first items in these antonym pairs against all of the second items in these antonym pairs should be negative. Conversely, this correlation should be positive for synonyms. This computation will first be described for psychometric antonyms and then generalized to the remaining cases (psychometric synonyms, semantic antonyms and synonyms).

The value of the psychometric antonym coefficient is generated by creating two vectors of data for each respondent. The pairs across these sets of data are each pair of psychometrically

opposite items, and a correlation coefficient is generated for each individual. For example, consider the case where three pairs of psychometric antonyms are identified in a sequence of ten items: (x_1, x_2) , (y_1, y_2) , and (z_1, z_2) . The item x_1 might be the phrase ‘I am happy right now,’ while the item x_2 might be opposite, such as ‘I am sad right now.’ These pairs of items might be embedded in these ten items as so:

[Item 1, Item 2, y_1 , Item 4, y_2 , x_1 , z_1 , z_2 , Item 9, x_2]

On a five-point Likert scale, this might produce a pattern of response such as:

[4, 5, 4, 3, 2, 3, 5, 1, 3, 2]

On the surface, this string of responses doesn’t tell us much. But, because we know something about the negative correlations between responses on these pairs of items, we can use this information to compute psychometric antonyms. The resulting correlation is then carried out between the two vectors of:

$[x_1, y_1, z_1]$

$[x_2, y_2, z_2]$

Or, in the specific example, between the vectors of:

[3, 4, 5]

[2, 2, 1]

The psychometric antonym coefficient for the individual who provided responses to these negatively correlated pairs of items is the value of the correlation between these vectors. In this case, this value is -.866. The value for all of these coefficients is constrained by the same values as correlations, and, like a correlation, a value of -.866 is fairly high. This demonstrates an individual who is responding oppositely to these negatively correlated items.

Take note that the N for these correlations is equal to the number of pairs of items that are used in this process. The maximum N for any given set of items is half the total number of items, assuming all items are used to create pairs, without replacement, in this process. That said, it is not recommended that all items be used, or for items to be used more than once. Individuals who have stronger negative correlations are presumed to be responding more carefully than those who have weaker or positive correlations.

The extension to the remaining metrics is fairly straightforward. Psychometric synonyms use the same technique, but search for high positive between-person correlations between items to generate pairs. Semantic methods simply start with established pairs of items and skip this between-person correlation-search step. Instead, these methods move straight to calculating the within-person correlations between these established pairs of items.

Psychometric antonyms appear to be the most popular of these techniques, though some studies have begun to also test the concept of psychometric synonyms (Meade & Craig, 2012). Interestingly, sample level values for these two techniques (psychometric antonyms and synonyms) only seem to correlate (between individuals) at $r = .44$ ($n = 385$) in the one study where they were both examined (Meade & Craig, 2012).

In addition, and similar to odd-even consistency, this technique seems to produce somewhat scale dependent distributions, with the mean of these coefficients being driven by the magnitude of these correlations in the original data (Curran et al., 2010). If the pairs of items used have very high correlations, then the overall scores on this metric should be higher than if the pairs had lower correlations. The examination of this relationship is beyond the scope of this paper, but does not stop these techniques from being used in a careful and thoughtful way.

Similar to response time, there are scores that are almost certain to indicate C/IE responding. For instance, negative within-person correlations on psychometric synonyms or positive within-person correlations on psychometric antonyms may serve similarly to the two second per item response time cutoff put forward by Huang et al. (2012). This overly conservative stance ensures that the worst responders are removed, and also allows room for thoughtful researchers to raise (or even lower) this cut score when informed by practice.

Individual Consistency: Inter-item Standard Deviation

On the far opposite end from long-string analysis lies the relatively new method of inter-item standard deviations. This technique, proposed and tested by Marjanovic, Holden, Struthers, Cribbie, and Greenglass (2015), measures how much an individual strays from their own personal midpoint across a set of scale items. This measure is accomplished through the following formula, from Marjanovic, et al. (2015):

$$ISD_j = \sqrt{\frac{\sum_{j=1}^k (X_j - \bar{X}_i)^2}{(k - 1)}}$$

In this case, X_j represents the response to any given item, and \bar{X}_i represents the respondent's mean score across all items. k is simply the number of items in the scale. As the name implies, this is nothing more than a within-individual standard deviation, similar to how odd-even consistency or psychometric antonyms is nothing more than a within-individual correlation.

The suggested use of this metric can be outlined with a simple example. Consider the following three response strings:

Person 1: 3, 3, 3, 3, 3, 3, 3, 3, 3, 3

Person 2: 1, 2, 3, 4, 5, 1, 2, 3, 4, 5

Person 3: 1, 1, 1, 1, 1, 5, 5, 5, 5, 5

The mean response for each of these individuals is 3, though their ISD will vary greatly. In fact, for a ten-item scale ($k = 10$) with 5 response options, person 1 and person 3 represent the lower and upper bounds of this metric, respectively. There is no difference between person 1's responses and person 1's mean, and such their ISD will be zero. There is maximal distance (for this scenario) between person 3's responses and person 3's mean, and their ISD will be large (for those interested, it is 2.11). Person 2 will be somewhere in between these two extremes (again, for those interested, person 2's ISD = 1.49).

It is proposed by Marjanovic, et al. (2015) that higher values on ISD are more indicative of random responding. It is important here to continue to distinguish this older specific concept of random responding from the more modern and general concept of C/IE responding.

Marjanovic et al. (2015) tested the effectiveness of ISD on detecting pure random responses (responses drawn from a uniform random distribution, independently on each item), something that it is fairly easy to do with almost any of these metrics. However, scores on this technique do not appear to be linearly related to randomness, per the example above.

Consider again the three cases above. Person 1 would receive the best possible score on ISD, and be classified by Marjanovic et al. (2015) as conscientious, as opposed to random. This is a fair assessment as long as the dichotomy is between conscientious and random, but hardly useful if the dichotomy is between conscientious and C/IE. If any individual in the sample of interest was classified as random, then person 3 would also be (they have the maximum value). While it may be argued that person 3 should be removed, their data is hardly more random than person 1. Person 2, who represents an ideal case of much more random data, in fact falls somewhere in the middle of this continuum.

This technique (ISD) should be used with care. While it shows some potential promise as a new method of C/IE detection, it was specifically designed and tested only for the random responder case, and has fundamental flaws even in that situation. Actual use in practice requires a much deeper understanding of the underlying distribution of scores and reconceptualization of the dichotomization of outcomes. There may in fact be use for this technique in the future of C/IE detection, but the work required to demonstrate this is beyond the scope of this paper.

Individual Consistency: Polytomous Guttman Errors

Guttman errors (Guttman, 1944; Guttman, 1950) are a concept originally designed for application on dichotomous test items. The premise of perfect Guttman scaling is that when items are ordered on difficulty, individuals should get easy items correct up to a point, then get all remaining, and more difficult, items wrong. Breaks in this expected sequence are called Guttman errors.

The calculation of this statistic on dichotomous (correct/incorrect) test items is based on the adjacent pairwise comparison between items *ordered on difficulty*. These pairs are created from 1) the response of each item and 2) the response on the immediately more difficult item. Whenever the more difficult item in this pair is correct and the easier item in this pair is incorrect a Guttman error is registered. This can be expressed (as in Karabatsos, 2003, p 294-295) as:

$$G = \sum_{h,e} X_{nh}(1 - X_{ne})$$

In this situation, X_{nh} represents the test-taker's score (correct = 1, incorrect = 0) on the harder of the two items, while X_{ne} represents the test-taker's score on the easier of the two items. If both values are the same (0,0 or 1,1), this equation goes to 0. If the easier item is correct and the more difficult item is incorrect, this equation also goes to 0. It is only when the more difficult item is correct and the easier item is incorrect that a value of 1 is produced, signaling the

Guttman error. The sum over all of these item pairs gives the total number of Guttman errors for an individual.

Although this metric was originally designed for dichotomous test items, it can be extended into polytomous items (e.g., Likert-type items) as well (Emons, 2008). However, Emons (2008) current calculation of this value requires an understanding of Mokken scaling and Nonparametric Polytomous Item Response Theory (Emons, 2008, Sijtsma & Molenaar, 2002). Because of the small scope of many studies utilizing these C/IE metrics, the implementation of Emons (2008) methods to generate such a metric in common practice is unlikely, at best. Instead, it is proposed that a much simpler metric can be used to quantify Polytomous Guttman Errors: item difficulty as operationalized with sample (or population) item means.

In this way, an item with a known or calculated mean of '4' on a five-point scale (of 'strongly disagree' to 'strongly agree') is considered easier than an item with a known or calculated mean of '3' on the same five-point scale. This relative difficulty ranking is simply a product of fact that the item with mean '3' is less likely to have individuals scoring higher on the item. This is very much in keeping with the spirit of Guttman errors on dichotomous items, but it is also recognized to be somewhat of a simplification.

It follows from this that participants who are actively reading and thinking about each item will be more likely to follow the general pattern of item 'difficulty' as they move through the scale. This is still a metric of within-person internal consistency, but simply utilizing additional sample (or in ideal case, population) level aggregate information about the items that is disregarded by many other techniques. The process by which this score is calculated is a simple extension of Guttman's original work, with the addition of more than two levels for item

scores and the resultant shift from dichotomous item scores to dichotomous values obtained through simple item comparisons, as follows:

$$G = \sum_{h,e} \text{if } (X_{nh} > X_{ne}) \rightarrow 1; \text{ else } \rightarrow 0$$

Note that some Guttman errors are to be expected! As with other techniques, this method is more about building a distribution across individuals on which decisions can be made.

Guttman errors are only registered when an individual scores lower on the item in each pair that has a higher mean, and not registered when the score on these items is identical or the correct order is present.

There are limits to the maximum number of Guttman errors that can be made on any given scale. Because of the paired nature of the comparison, the absolute maximum is one less than the number of items. This, however, assumes that the number of response options is equal or greater to the number of items. If the number of responses options are less than the number of items, then maximal errors need room to loop back to the other end of the scale (which is then itself not a Guttman error). If k is the number of items and x is the number of response options, then the maximum Polytomous Guttman Errors is found by subtracting the integer quotient of k/x from one less than the total number of items.

As a new metric for this C/IE detection, just like RIR, work on understanding its practical use is needed, but some conservative benchmarks can still be suggested. It is the suggestion of this paper that individuals who have a count of Polytomous Guttman Errors equal or greater than half of these possible Polytomous Guttman Errors be considered as C/IE responders by this technique. Again, this is obviously this is not the best cut score for all scales, but also again at least provides a point for researchers to begin a deeper consideration.

Individual Consistency: Person Total Correlation

As with the extension of Guttman errors to polytomous Guttman errors, there is much that can be learned from long-established research on the detection of aberrant response patterns in testing data. One such technique from the testing literature is the “personal biserial” correlation (Donlon & Fischer, 1968). This concept is itself an extension of the idea of item-total correlations, and as such a brief discussion of item-total correlations is required.

An item-total correlation on some item in a scale is calculated as the between-person correlation of scores on that item with scale scores on the overall set of items. A corrected or adjusted item-total correlation removes the value of the item in question from the calculation of the scale score. This corrected form is preferable, though for simplicity in this paper all reference to the general concept will remove references to ‘corrected’ item-total correlations on the assumption that the uncorrected form is simply never used and common usage of ‘item-total correlation’ implies the corrected form.

Just as this examines an item among a set of items, the same can be constructed for individuals in a larger sample of persons. Conceptually, this *person-total correlation* is a measure of how consistently each person is acting like all other persons. This is then a measure of how consistent any given person is, relative to the expected patterns generated by all other persons. The practical implementation of person-total correlations, as distinct from item-total correlations, is highlighted by figure 2.

The core of this idea of person-total correlations, as stated above, is an extension of work on tests done by Donlon and Fischer (1968). Donlon and Fischer describe this exact process on dichotomous test data, and suggest that it could be used to provide extra information about examinees and their responses. Later work in the testing literature, particularly work dealing with

the detection of aberrant response on tests, has shown Donlon and Fischer's (1968) technique to work as well as many other detection methods (Karabatsos, 2003).

Person-total correlations, as proposed for use in C/IE response detection, are a simple application of this idea to polytomous data. Unlike Guttman errors, which require a revision of calculation, this technique works identically in both situations (dichotomous and polytomous). While the calculations might be the same, this new term is meant to distinguish its use in this distinct area.

In practice, the calculation of person-total correlations first involves a simple transpose of the person by item matrix into an item by person matrix. Item-total correlations (as part of reliability analysis in typical statistical software) performed on this transposed matrix (in which items are rows and persons are columns) will now produce person-total correlations. These values can then simply be stored, transposed, and appended to the original data for each individual. Again, this is identical to the procedure outlined in Donlon and Fischer (1968).

Obviously, this hinges on one large assumption: the majority of respondents in any given sample are responding thoughtfully. Given that some of the highest estimates of these behaviors are around 30%, this seems a safe assumption. However, conditions that might break this assumption may invalidate this metric. In fact, if the majority of responders in a sample were C/IE responders, this method would potentially recommend removing the thoughtful responders! It may also be the case that the effectiveness of this technique is related to the proportion of C/IE responders in the sample, even if they do not constitute a majority. In either case, this technique should be used with care in situations where large proportions of the sample are suspected to be C/IE responders.

Given the somewhat ubiquitous understanding of item-total correlations, person-total correlation is potentially one of the easiest C/IE metrics to understand, to calculate, and to norm across samples and forms. For instance, negative item-total correlations are always symptomatic of a problem, as would be the case for negative person-total correlations. An individual with a negative person-total correlation is acting counter to the expected pattern set out by other individuals.

Like RIR and this paper's calculation of Polytomous Guttman Errors, this is a new metric in the framework of C/IE detection, and future work should examine its characteristics in practical use outside of its established use on test data, especially in comparison with other methods. However, some conservative benchmarks can be applied from what is well known about item-total correlations. It is the recommendation of this paper that individuals with negative person-total correlations be considered C/IE responders by this technique. While this will not eliminate all C/IE responders, this again serves as a starting point for individual researchers to have thoughtful discussions about the use of different cut scores as informed by future work and practice.

Bogus/Infrequency/Attention Check Items/IMCs

The techniques discussed to this point have relied on post-hoc within-person statistics calculated from the sample of interest. These techniques can be applied to the data from already completed survey data collections, as long as the right types of items are present in the right quantities. Simply put, there isn't that much that can be calculated in terms of consistency on a one-item measure.

Conversely, a different family of techniques use the inclusion of specific items in scales to check respondents' attention. By adding these items before data collection, responses on these

specific items can be observed and used to make data quality decisions. The content of these items varies, but the underlying theme is that there is one ‘correct’ response or range of responses, and one ‘incorrect’ response or range of responses. Similar to response time, this family of techniques are among the most transparent, direct, and intuitive.

One common and fairly transparent technique instructs respondents to answer with a particular response option, such as “Please select Moderately Inaccurate for this item” (Huang et al., 2012). Individuals who respond with the prompted response are considered correct, and individuals who respond with any other are considered incorrect. The simple assumption here is that any individual who has read such an item should respond correctly. Though these items are relatively transparent, it is unclear how participants view them or what actually drives responses on these items. Curran and Hauser (2015) have found, through verbal protocol analysis, that there are a proportion of responders who do not answer these questions correctly, even after reading them aloud. While this proportion of individuals was small ($< 10\%$), it suggests that the linkage between simply reading these items and responding correctly may not be flawless, particularly in low-stakes situations.

Items which prompt for a clear correct answer represent almost full transparency to the survey taker. There is another school of thought, however: these items should be less transparent in order to work without standing out from other items (Huang et al., 2014; Meade & Craig, 2012). In this framework, items are constructed to appear face-valid on a quick visual inspection, but obvious or absurd on deeper inspection. These items include: “I am interested in pursuing a degree in parabanjology” (Huang et al., 2014) and “I am paid biweekly by leprechauns” (Meade & Craig, 2012). Known as bogus or infrequency items, the assumption is that there are a range of correct and incorrect responses to these items. Current use makes cuts at the extremes of the

correct end of the scale, such that the correct answers to “I am paid biweekly by leprechauns” may be ‘strongly disagree’ and ‘disagree.’ Incorrect answers may include ‘slightly disagree,’ ‘neither agree nor disagree,’ and all of the identically mirrored ‘agree’ options. Items written in the opposite direction (where ‘agree’ would indicate attentiveness) simply use the options on that end of the scale as correct.

For the most part, these items seem to work as well as any of the other established methods in this paper (Huang et al., 2014; Meade & Craig, 2012). By using a number of items throughout the scale, and computing a total correct score for each participant, small oddities and error in response are generally averaged out. While this paper will generally recommend the use of these items for C/IE detection, it will do so with a number of small caveats and suggestions for future research.

As mentioned, some preliminary work has begun to examine valid reasons why individuals choose what would be considered invalid responses (Curran & Hauser, 2015). Though the bogus/infrequency item approach may work best on an item as extreme as “I am paid biweekly by leprechauns,” a thoughtful participant might in fact agree to the item “All my friends say I would make a great poodle,” (both items from Meade & Craig, 2012) giving the justification that their friends did consider them to have dog-like traits such as loyalty (Curran & Hauser, 2015). In the best case, the identification of valid responders being flagged as C/IE simply highlights some Type I error that is already assumed to be present in all techniques. In fact, it should be implicit in the use of these techniques that there is a potential for the good to be thrown out with the bad. In the worst case, results such as these (Curran & Hauser, 2015) mean that some particular segment of responders are being removed for invalid reasons. Researchers using these items should make sure that they are used consistently to the ways in which they

have been validated. Cherry-picking one or two items from these scales for use as a shortened method opens the researcher up to heightened risk of these types of error, as there are fewer items on which the error can be averaged away.

It may in fact be the case that the items which work best in these situations are those that are the most extreme (e.g. “I have never used a computer”; Huang et al., 2014), and in essence, the most similar to simple items instructing a specific response described above (e.g. “Please select Moderately Inaccurate for this item”). Those items with a looser interpretation, which might appear the most face valid in a normal personality scale (e.g. “I can eat as much as a horse”; Curran & Hauser, 2015), may simply be more open to interpretation that is outside of what the researcher intended. If these items are used without question, or produced ac-hoc by researchers without proper consideration, this error may go unassessed. Future work in this area should be directed toward finding properties of these items that best strike this balance between face validity in the scale and construct validity in terms of C/IE detection.

Another method, known as instructional manipulation checks or IMCs (Oppenheimer, Meyvis, & Davidenko, 2009), adds a key phrase to a longer set of instructions, usually on some long item with a single response. This phrase can vary in length, but should functionally look just like the rest of the instructional text for an item. This phrase generally instructs the respondent to disregard the rest of the question at hand and respond in some other manner. Instead of responding normally, then, an attentive respondent should respond in the unlikely way outlined in this phrase. This phrase might instruct respondents to ignore the item responses and the ‘continue’ option, instructing them instead to click on title text or other unlikely parts of the page (Oppenheimer et al., 2009). Respondents might also be instructed to pick two or more disparate or unlikely choices from a list of ‘choose all that apply’ responses, or to choose the ‘other’ option

and then insert into that choice a string of experimenter-determined text (Hauser & Schwarz, in press). An example of how this may be presented to participants can be found in figure 3.

On the surface, this technique can be invaluable if the manipulation of a study is hidden in similar text somewhere else in that study (e.g. goal-framing language). If the participants fail to read the entirety of instructions of such an item, it is likely that they also failed to read the instructional text containing the study manipulation. In this way, this technique is, as the name would indicate, a manipulation check. On a survey without such instructions, however, these IMCs are likely to stand out as different from other items.

In fact, even used in a study with similar items, some work has begun to find sample effects in terms of what appears to be learned skill on these items. Specifically, Hauser and Schwarz (in press) have found that participants from Amazon's Mechanical Turk (MTurk) appear to be caught by these IMCs at a significantly lower rate than participants from other samples, despite lower pass rates of these individuals only some years ago (Oppenheimer et al., 2009). There are a number of conflicting studies and explanations on this topic, but one of the general outcomes seems to be that frequent research participants (e.g., MTurkers) have, over time, potentially learned the structure of these items meant to capture their inattentiveness, and adapted accordingly. The core of this problem appears to be that there exists a fundamental knowledge about these items that can be learned. At least part of this knowledge is the simple fact that these items exist in the first place. Researchers should remember that MTurk participants are by and large participating to make money, and being a more knowledgeable participant can have direct impact on the amount of money that is made. Entire websites (e.g. www.turkernation.com) exist to help these participants collect and share knowledge about studies and researchers.

The removal of individuals who have failed these IMCs does not seem to have any substantive effect on study outcomes, even in large multi-site samples (Klein, Vianello, & Ratliff, 2015). This topic, like many others in this paper, needs more study, but it appears that respondent knowledge of this technique, in particular on MTurk samples, may have already invalidated any meaningful use that existed initially.

Across all of these general attention check techniques, researchers should think carefully about how inserted items or instructions will work in the study at hand. There are many cases where such items might be perceived as an insult to an already highly motivated sample (e.g., a client satisfaction survey). At the very least, such items and the time they require may simply be unnecessary. In addition, an unfortunate paradox emerges in practical recommendations regarding such items. On one hand, researchers should not simply create these types of items for single use without any validation, but should look to those that have already been psychometrically validated or use a pilot study to ensure new items are working as desired. At the same time, re-use of the same items and checks are likely to cause respondent acclimation to those stock items over time. These items may best be conceptualized through the lens of item security and item exposure used in testing (Davey & Nering, 2002). For example, any respondent who has knowledge of Huang et al. (2014) or Meade and Craig (2012) effectively knows what to look for and how to respond to the items in those scales. This may be a small subset of any sample, but the trend of exposure to such items is presumed to be monotonically increasing.

Because of the transparent nature of these techniques, producing cut scores on them is much more frequently left to (or allowed to rest with) the judgment of the researcher. Regarding attention check and bogus/infrequency items, this paper will again err on a conservative suggestion of 50% inaccuracy as a signal that participants should be considered as C/IE

responders by these techniques, assuming that some reasonable number of these items are being used. IMCs are more distinct, both because they are less susceptible to type II error (it is more difficult to get them right on accident) and are likely to be used in smaller quantity than these other, shorter items. Manipulation checks of any type, because of their clear linkage to the manipulation of the study, are simply easier to frame as all-or-none decisions. Because of the much more transparent and intuitive nature of these items, this paper will suggest the cut score decisions be fully left to the researcher, but with the caveat that such decisions should be considered and decided before data is collected.

Self-Report Data

Another technique for identifying C/IE responders comes with a blend of transparency, simplicity, and forethought. Simply put, it is possible to ask respondents if they have responded in a way that they believe is valid (though perhaps not quite in those words). Meade and Craig (2012) created and tested a scale of participant engagement which produced two factors: one of diligence and one of interest. This diligence scale correlated reasonably well with some of the other techniques of C/IE detection in their study (magnitude of r ranging from roughly .16-.51), and may prove to be as useful as some of these techniques on samples that have no reason for deception. Meade and Craig (2012) also tested a number of single item measures of attention and effort, as well as a one item measure simply asking “In your honest opinion, should we use your data?” Each of these items had similar but weaker correlations compared to the longer diligence scale.

These techniques do appear to have promise, at least in low-stakes situations where respondents have nothing to lose by being honest. Even in situations where rewards are being offered for survey completion (e.g., psychology subject pools, MTurk), an honest and open

questioning of individuals at the end of a study may still produce some useful and actionable data, especially if it is noted that rewards will be fulfilled regardless of response. Anecdotally, this author can confirm that through initial and limited use of this scale some small segment of participants (~3-5%) do appear willing to acknowledge that their data may be worth discarding.

If done accurately, it is far better for the researcher that a respondent simply admits that their data is of poor quality, rather than having to determine that same fact through analysis of their data. The same Type I error problem (throwing out good data as bad) applies, however, in the possibility that a *mostly* careful respondent might identify their data as of poor quality even if it is data that, by reasonable standards, should have been kept. Participants may also not have any idea of what qualifies as ‘useable data,’ and responses that one participant consider of good quality may be considered poor quality by any other participant. As well, more self-aware participants might take note of their attention waning while less self-aware (or more inattentive!) participants do not. Regardless of this risk, the use of such scales is a promising area that also stands to benefit from continued research.

Additional Factors: Reverse Worded Items

There are many aspects of C/IE responding that have not been examined in robust detail in prior research. Similar to adding another set of conditions to an experiment, many of these aspects effectively double the complexity of any given examination or understanding. Chief among these is the concept of reverse worded items. Reverse worded items are simply those which are directionally disparate from the ‘normal’ items in a scale. That is, individuals with higher levels of the underlying latent construct should produce scores that are *lower* on these items. As such, these items need to be reverse scored before many types of analysis.

These items should also be reverse coded before the application of some of these C/IE detection techniques, though not necessarily all. Some of these techniques can be used on both raw and recoded data, to similar or distinct effect (Curran & Kotrba, 2012). These distinctions are summarized in table 1, which highlights the recommended use of these techniques in three different settings: 1) data without any reverse worded items, 2) data with reverse worded items that have not been recoded, and 3) data with reverse worded items that have been recoded.

In the case of scales without any reverse worded items, these techniques work in a fairly straightforward manner as outlined above. The notable distinctions here come in the case of psychometric and semantic antonyms. Semantic antonyms, by their very definition, imply that reverse worded items are being included in the scale (e.g., ‘I am happy’ vs ‘I am sad’). A scale without these reverse worded items is likely to lack any truly semantically opposite items. While psychometric antonyms aren’t constrained to only function with reverse worded items, they do take advantage of such items in their search for those items which possess large negative correlations. In a situation without reverse coded items, such negative correlations are likely to be weaker, if negative correlations exist at all.

If reverse worded items are used, there is information that can be gained from both the raw data before such items are reverse coded, and the same data after recoding. First and foremost, semantic and psychometric synonyms and antonyms will both work normally in each of these situations. Synonym pairs are not likely to be influenced by any recoding. Antonyms will work to produce within-person correlations in both cases, though it should be noted that higher negative correlations should only indicate thoughtful response before items have been recoded. After recoding, higher positive correlation between these items will indicate thoughtful response. Regardless of this calculation, psychometric antonym pairs should be identified on data

before recoding in order to isolate these negative correlations distinctly from normal positive correlations.

Long-string analysis is unique in that it works in both situations as well, but to different effects. Before recoding of reverse worded items, long-string analysis will detect individuals who completely missed the presence of these reverse worded items. This represents extreme carelessness, and would in fact not be detected if these responses were recoded. In fact, such recoding would make these responses look more legitimate to long-string analysis! It is not to say there is no place for long-string analysis on recoded data; long-string analysis on recoded data helps to identify those participants who had the minimal presence of mind to notice reverse worded items and change their responses accordingly (e.g., among a string of ‘agree’ responses the participant shifts to ‘disagree’ for each reverse worded item). This may be a fairly specific application, but it is not difficult to imagine a participant who simply looks for words such as ‘never’ and ‘not’ and ‘don’t,’ the usual hallmarks of reverse worded items. This minimal attention might be enough for the participant to believe their data will not be caught by a screen, but might not be enough to be considered thoughtful, as in Huang et al. (2012).

Some techniques do not work in both of these situations, or are untested. Odd-even consistency and resampled internal reliability simply do not have their foundational assumptions met when reverse coded items are used and not recoded. Their use in this situation is not only not recommended, but will produce completely meaningless results. These techniques require unidimensionality (even if that unidimensionality is somewhat forced, as in the case of reverse coding reverse worded items). Polytomous Guttman errors and person total correlations, as described here, do not have such an absolute reliance on unidimensionality. However, the reliance on item means in this process suggests that differences will exist between these two

computations. Further study may examine this difference and how it might be conceptualized, but these techniques are only suggested for current use in the case where data are either unidimensional by nature or recoding. This may change in the future as they become better understood.

Some initial work has shown that reverse worded items do not reduce the presence of invalid response (Sonderen, Sanderman, & Coyne, 2013), and they should not be used or considered a panacea for these problems. In addition, there has been a fairly long history of examining the impacts of C/IE response (and other invalid patterns) on reverse worded items (Schmitt & Stults, 1985; Spector, Van Katwyk, Brannick, & Chen, 1997). More recently, Woods (2006) examined the inclusion of C/IE data in the scale construction process and found that roughly 10% C/IE responders were enough to begin to produce spurious methods factors outside of a unidimensional framework. This oddly overlaps with best estimates for C/IE data in normal samples, and suggests another motive for the removal of these participants if reverse worded items are to be used.

There are complex issues to be solved regarding the use of reverse worded items in the survey process, and there are even more complex issues regarding their relationship with C/IE detection techniques. However, as a field of research on C/IE response, the unidimensional case without reverse worded items is still not fully understood. It is a strong recommendation of this paper that future research on these techniques first focuses on the case without reverse worded items. Only after sufficient understanding of this simpler case is it truly justified to move to the more complex reverse worded case.

In practice, scales with reverse worded items will continue to be used. This prior paragraph is not meant to be a call for the complete eradication of these items. Further, this prior

paragraph should not be grounds to consider the use of these techniques irrelevant on scales with reverse worded items. There are many reasons to include reverse-worded items other than the detection of C/IE responders, and these reasons are left to the consideration of the individual researcher. It may be that reverse worded items play an integral part in the future of this research, or that the removal of C/IE responders fixes some of the current problems of reverse-worded items, but neither of these has yet to be demonstrated.

Recommendation: Multiple Hurdles Approach

A number of methods have been illustrated here, and a summary of these techniques can be found in table 2. The reader may recall or recognize that not all of these techniques measure the same construct, and may correlate positively, negatively, or not at all with each other (Meade & Craig, 2012). As an extension of this, individuals who are identified by one technique are not necessarily identified by the others. In fact, identification by one technique sometimes means a reduced likelihood of detection by another (e.g., long-string analysis and odd-even consistency). Because of this, using a raw count of the number of metrics that identify an individual is somewhat self-defeating, and not advised.

Each class of these techniques has pros and cons, and is designed to identify a different *type* of invalid data. The strongest use of these methods is to use them in concert; to balance the weaknesses of each technique and not simply eliminate individuals based on benchmarks that the researcher does not fully understand or cannot adequately defend. To attempt to find this balance, the sequential use of these methods can identify individuals for deeper examination, thoughtful consideration, and transparent exclusion or inclusion.

It has been mentioned several times during this paper, but it is worth restating: there is a fundamental decision about Type I and Type II error in the elimination of individuals from a

dataset that all researchers considering these techniques should think about deeply. It should also be recognized that not using these techniques at all defaults to maximum Type II error. The overall recommendation of this paper is to err on the side of Type II error in each of these techniques, unless individual assessment reveals drastically greater consequences of Type II error relative to Type I error. This conservative approach is in part due to the simple fact that these techniques, even those that have been in use for decades, are not completely understood. It is also the case that, unless data are of particularly low quality, valid respondents will always outnumber invalid respondents. The removal of the worst of these responders of any given technique will eliminate the easiest to identify, and, potentially the most impactful. This also minimizes risk of also eliminating normal valid responders by making too deep a cut with any given technique. The more that these techniques are pushed to their limits (i.e. the more Type II error is reduced), the greater will grow the ratio of valid data discarded with this invalid data.

As a hedge against this restricted use (and resulting restricted effectiveness) of each individual technique, it is then also suggested that these techniques be used in sequence. These multiple methods can be viewed each as a hurdle which respondents must pass, and by which the most extreme to each technique are identified. By using multiple hurdles, and setting the bar for each of these techniques lower than if that technique was being used in isolation, the most-likely-invalid individuals will be identified and eliminated by each technique without making too deep of a cut into those who have taken the time to provide valid data.

For example, an initial cut on response time may identify those individuals who are simply responding below some threshold under which thoughtful responses could *never* be produced. Consider figure 1 once again. This was used to demonstrate the difficulty of eliminating all invalid responders without removing any valid responders, a task which is

exceptionally difficult or sometimes impossible with the response time information of the left column of figure 1 alone. However, the identification and elimination of a smaller proportion of invalid responders, even only one or two, is almost certain to remove some invalid responders with a reduced risk of removing any valid data. This smaller proportion that are eliminated also have the virtue of being those that are likely to have exerted the least attention or care of any in this group.

If methods are used as sequential hurdles, the remainder of individuals who pass this screen of response time are still not free and clear. Leaving some borderline individuals on response time simply means these individuals will be tested by each remaining method in turn. Consider an individual who responded with the same response to every item, but who did it slowly enough to pass the response time screen (an oddity, but certainly not impossible). This individual would not be identified by response time as a C/IE responder, but would be easily identified as such by long-string analysis. Individuals who passed both of these screens (hopefully the large majority of respondents) would then be examined by the next, continuing through all that the researcher has decided to utilize.

Some techniques may disagree in the classification of C/IE responders, and this is to be expected. For instance, individuals who provide long strings of the same response may very well produce a response string that appears highly consistent to other methods, such as odd-even consistency or psychometric synonyms.¹ While this may represent blind spots present in each of these techniques it does not represent a flaw in the use of them as a whole. In fact, it provides the strongest case for using more than one of these techniques, as the more that are used the fewer blind spots will remain unaddressed. Researchers should consider this fact when choosing

¹ Take note that in some situations some of these techniques cannot even be computed, such as an individual who provides no variance in response.

techniques, and ensuring that these weaknesses of any given technique are balanced against the strengths of others. Using techniques that are positively correlated should not do much more than provide some redundancy in those areas.

This conservative and serial use of each technique retains some invalid responders, but also provides other techniques with the follow-up opportunity to identify those invalid responders that remain. This continuing process then has the power to potentially remove more and more individuals through this process. This both relieves pressure on each of these techniques through reduction in sample size (each technique in this sequence will potentially have fewer individuals to examine than the preceding technique) and distribution of work (each technique is carrying a share of the total responsibility to remove individuals). In addition, this distribution of work also results in specialization of function: each technique is only tasked to remove those invalid responders that it can conclusively eliminate. Instead of removing individuals because they were identified by some count of total metrics without fully understanding why those individuals were removed, each individual is eliminated because they violated some specific or conceptual benchmark established by the researcher (e.g., finished a 50-item survey in 20 seconds).

A number of cut scores have been highlighted from prior research or recommended in this paper. When present, these have been put forth as conservative starting points for each technique, and malleable as experience and practice dictate. Above all else, the researcher using these techniques should have some understanding of when and how they are being applied, and how and then those techniques are eliminating individuals from the larger dataset. Whenever possible, this should be reported as such. Reporting this sequential process allows for a deeper

understanding of why each individual was removed, particularly when contrasted to the much simpler statement that X number of individuals were removed for C/IE responding.

Broadly, these techniques, and this somewhat *carte blanche* recommendation for their implementation, should not be seen as an invitation or opportunity for *p*-hacking (Head, Holman, Lanfear, Kahn, & Jennions, 2015) or other data tweaking. Researchers should consider use of these methods in advance, and be willing to acknowledge what the removal of these individuals may do to their data. These invalid responders represent a form of error, and the manifestation of that error can take many forms. Removal of these invalid responders may strengthen effects by reducing error, but may just as easily weaken or eliminate effects that were only a product of that same error (Huang et al., 2015). Researchers in such situations need resist the temptation to replace these individuals, as such replacement may simply be in an effort to save potentially spurious results.

As an extension of this, it is recommended that researchers run analyses on both original data and data cleaned through these techniques. Situations where these analyses disagree (if at all) may be highlighted and made transparent for others working on the same ideas. For small effects the inclusion of these C/IE responders has the potential to change results, but for large effects the shift may be trivial. Regardless of the triviality of this change, whether actual or perceived, these techniques are still meant to reduce noise and error in data. The resulting cleaned data should be stronger for this cleaning.

Conclusions

Overall, there are a number of ways to screen survey data for C/IE responders. This paper has presented a number of new additional techniques, and this entire group of techniques should be regularly used by survey researchers. The removal of these invalid responders has been shown

to reduce error and provide more valid results. Best guesses put the average inclusion of C/IE responders at around 10% (Curran et al., 2010; DeRight & Jorgensen, 2015; Maniaci & Rogge, 2014; Meade & Craig, 2012), but with potentially high variance on that estimate in normal research studies (e.g. Burns, Christiansen, Morris, Periard, & Coaster, 2014; Gough & Bradley, 1996). Simply put, our best estimates are potentially imprecise, but these individuals and their responses do exist.

As long as it can be accepted that finite numbers of these individuals exist, even small quantities of these individuals in a sample have been shown to have meaningful consequences on properties of scales (e.g. means, variance, reliability), and the relationships between scales (Curran & Kotrba, 2012; Huang et al., 2015; Maniaci & Rogge, 2014; Woods, 2006). The removal of these invalid data should thus be a priority for all research where such responses might be reasonably be expected. As an extension of this, the knowledge that these individuals exist and can influence study outcomes has deep importance for the replicability of any given effect and/or study. If left to freely vary, different samples may have different levels of C/IE responders. A study with 20% C/IE responders may well produce a spurious effect that cannot be replicated by later studies that either randomly or systematically have a lower proportion of these individuals. The same can work in reverse, with original effects clouded by C/IE responders in later replications. Without seeing this as a potential problem, and seeing no other differences between samples, confusion abounds.

The best way to standardize and hold constant the level of C/IE responders across samples is through best efforts to reduce their proportion in any given sample to as close to zero as is reasonably possible. To this end, researchers and practitioners should keep in mind a

number of points from earlier in this paper, as well a number of extensions of these earlier arguments:

(1) Researchers should not rely too heavily on any one technique, and instead use a host of techniques to cover the range of possible C/IE response.

(2) Methods should not be applied blindly, as planning out this process for each given study is likely to result in a process which is more accurately aligned to that situation.

(3) Response time should always be collected, even if it is not used.

(4) Consideration should be given, during study planning, to the inclusion of custom items or instruction sets for later use as detection methods.

(5) Each method chosen should be used conservatively, in sequence, as a family of techniques, rather than viewing each technique in isolation or relying on straight agreement or count of identifications between methods.

(6) If these techniques are used to reduce the size of a sample, analysis should be run on both original and reduced samples, and differences between these samples should be noted.

(7) Even if the application of these techniques appear to do nothing to effects under study in any given dataset, researchers should take note that important cleaning of error may still have occurred. This invalid data may sometimes strengthen effects and sometimes weaken them, and occasionally these opposite effects may simply cancel out.

Researchers should be aware that even with largely conservative approaches as outlined here, Type I error in these techniques will almost always be present. Some good data are likely to be removed while cleaning the bad. If removing some good data in the course of removing invalid data is unacceptable in a given data collection, these techniques are simply not recommended. Rather, the likely inclusion of these individuals should instead be considered as a

potential limitation (particularly in keeping with Huang et al., 2015), and noted frankly and honestly in the evaluation of the results taken from those data.

In fact, whether individuals are to be eliminated or not, these techniques should be widely used as part of normal data screening and reporting. Through the use of these techniques much more can be learned about the psychological process of responding to self-report measures. Common understanding of these techniques can help to ensure a trend toward future standardization that works well for the widest range of applications.

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Table 1			
<i>Recommendations for the use of techniques on data based on inclusion and recoding of reverse worded items.</i>			
Method	Raw data, no reverse worded items	Reverse worded items, before reverse coding	Reverse worded items, after reverse coding
Long-string analysis	recommended	detects those who missed RW items	detects those who noticed RW items
Mahalanobis Distance	recommended	use with caution	recommended
Odd-even consistency	recommended	not recommended	recommended
Resampled Individual Reliability	recommended	not recommended	recommended
Semantic antonyms/synonyms	recommended	note expected sign of coefficient	note expected sign of coefficient
Psychometric antonyms/synonyms	use with caution	note expected sign of coefficient	note expected sign of coefficient
Inter-item Standard Deviation	use with caution	not recommended	use with caution
Polytomous Guttman Errors	recommended	use with caution	recommended
Person total correlation	recommended	use with caution	recommended

Table 2		
<i>Summary of methods of C/IE detection</i>		
Method	Type	Description
Response time	observational	Time of survey completion.
Long-string analysis	invariance	Length of longest sequential string of the same response.
Mahalanobis Distance	outlier	Distance of response pattern from multidimensional center of all responses.
Odd-even consistency	consistency	Within-person correlation of odd numbered scale scores with even numbered scale scores.
Resampled Individual Reliability	consistency	Resampled correlation on partial scale scores from two random halves of their responses on each scale.
Semantic antonyms/synonyms	consistency	Within-person correlations on sets of semantically matched pairs of items with opposite or similar meaning.
Psychometric antonyms/synonyms	consistency	Within-person correlations on sets of correlation-based matched pairs of items with opposite or similar meaning.
Inter-item Standard Deviation	consistency	Degree of drift in responses from mean of individual response pattern.
Polytomous Guttman Errors	consistency	Count of the number of instances where a respondent broke the pattern of monotonically increasing response on the set of survey items ordered by difficulty.
Person total correlation	consistency	Correlation of an individual's response string with the average responses of all others.
Bogus/infrequency items	check items	Odd items placed in scale to solicit particular responses.
Attention check items	check items	Items placed in scale with explicit correct response (e.g., Answer with 'agree')
Instructional manipulation checks	check items	Items with extended instructions which include instructing participant to answer in unique manner.
Self-report scales	self-report	Items which ask the participant how much effort they applied or how they judge the quality of their data.

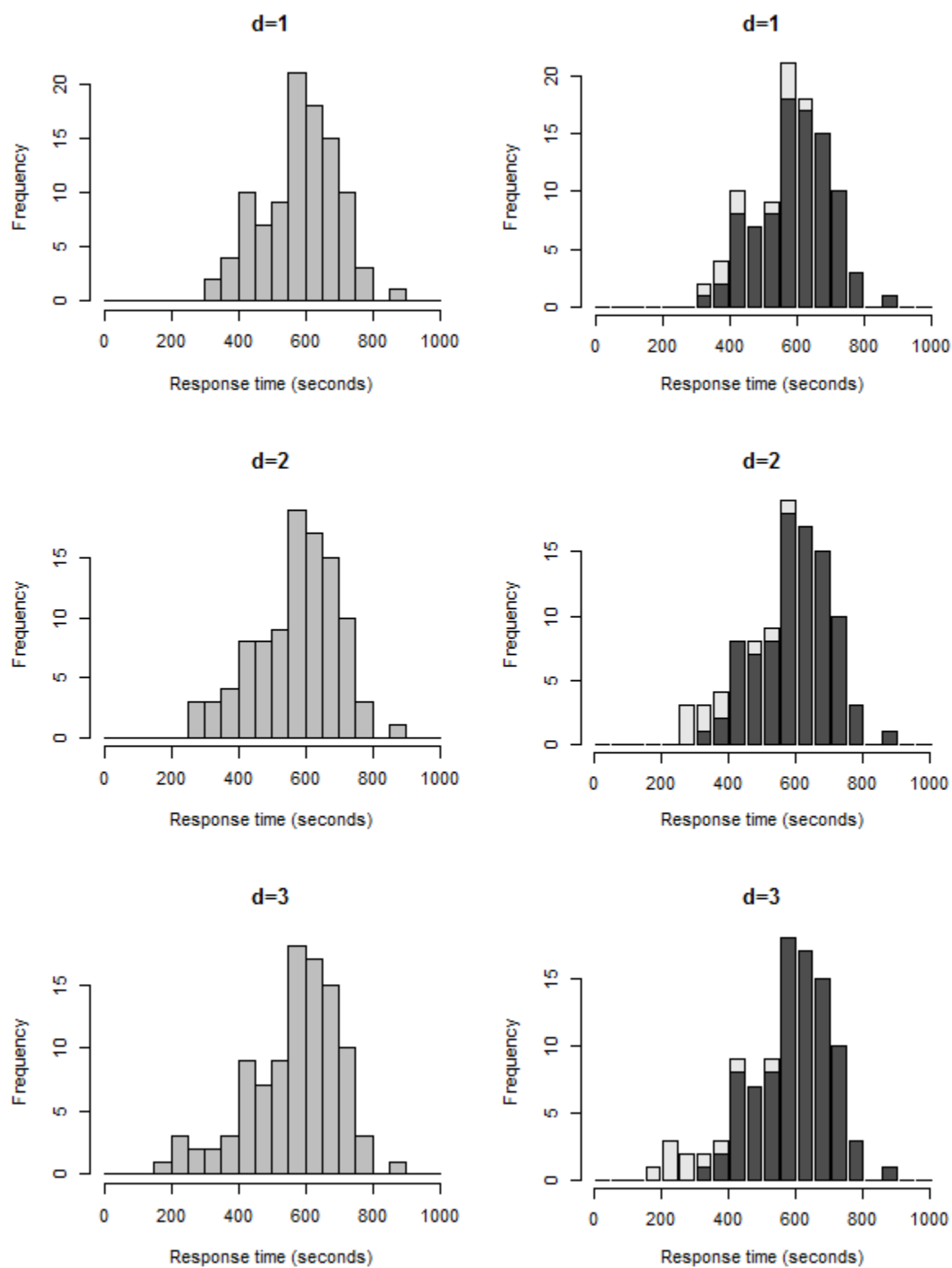


Figure 1. Inclusion of 10% simulated careless response of different magnitudes in both known (right) and unknown (left) cases. Note: in right column normal data are dark; invalid data are light.

	Item 1	Item 2	Item 3	Item 4	Item 5	Item 6	Item 7	Item 8	Item 9	Item 10	Mean(Item 2: Item 10)
Person 1	1	2	3	2	3	2	3	3	1	2	2.333333333
Person 2	2	2	3	2	3	2	3	3	1	2	2.333333333
Person 3	1	1	2	1	1	1	3	2	1	2	1.555555556
Person 4	1	2	3	2	3	2	4	4	1	2	2.555555556
Person 5	1	2	3	1	2	2	3	3	1	2	2.111111111
Person 6	1	2	3	2	2	2	3	3	1	2	2.222222222
Person 7	1	2	3	1	2	2	3	1	1	2	1.888888889
Person 8	1	2	3	2	3	2	4	3	1	2	2.444444444
Person 9	1	2	3	1	2	2	3	3	1	2	2.111111111
Person 10	1	2	3	2	3	2	3	3	1	2	2.333333333
Mean(Person 2: Person 10)	1.11	1.889	2.889	1.556	2.333	1.889	3.222	2.778	1	2	

Correlation between Item 1 (column) and the mean of all other items (column) is the (corrected) item-total correlation for Item 1.

Correlation between Person 1 (row) and the mean of all other persons (row) is the person-total correlation for Person 1.

Figure 2. Demonstration of difference between item-total correlation for Item 1 and person-total correlation for Person 1.

SPORTS PARTICIPATION Most modern theories of decision making recognize the fact that decisions do not take place in a vacuum. Individual preferences and knowledge, along with situational variables can greatly impact the decision process. In order to facilitate our research on decision making we are interested in knowing certain factors about you, the decision maker. Specifically, we are interested in whether you actually take the time to read the directions; if not, then some of our manipulations that rely on changes in the instructions will be ineffective. So, in order to demonstrate that you have read the instructions, please ignore the sports items below. Instead, select the box marked 'other' and type "I read the instructions" (no quotes) in the text box, then click continue. Thank you very much.

Which of these activities do you engage in regularly? (click on all that apply)

- | | |
|---------------------------------------|---|
| <input type="checkbox"/> Skiing | <input type="checkbox"/> Swimming |
| <input type="checkbox"/> Soccer | <input type="checkbox"/> Tennis |
| <input type="checkbox"/> Snowboarding | <input type="checkbox"/> Basketball |
| <input type="checkbox"/> Running | <input type="checkbox"/> Cycling |
| <input type="checkbox"/> Hockey | <input type="checkbox"/> None |
| <input type="checkbox"/> Football | <input type="checkbox"/> Other <input type="text"/> |

Figure 3. Example of an IMC based on Oppenheimer et al. (2009).