Common Concerns with MTurk as a Participant Pool: Evidence and Solutions

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Common Concerns with MTurk as a Participant Pool: Evidence and Solutions

Crowdsourced online samples, particularly those recruited from Amazon Mechanical Turk (MTurk), have become a popular source of research participants in the social sciences (Chandler & Shapiro 2016; Stewart, Chandler, & Paolacci, 2017). If anything, consumer research has been leading the move towards crowdsourced samples. Illustrating this tendency, 43% of articles published in volume 42 of the *Journal of Consumer Research* (between June 2015 and April 2016) reported at least one study that recruited participants from MTurk (Goodman & Paolacci, 2017).

The popularity of crowdsourcing platforms among consumer researchers is driven by the advantages they have over other means of recruiting participants. Crowdsourcing platforms are simple to use and can recruit large samples of diverse participants quickly and at low cost. Of particular importance to consumer researchers, these samples include adults, who are presumably more financially independent, used to more varied purchase decisions, and thus more similar to the typical consumer than college students (Peterson, 2001). These samples are also cheaper and more flexible than commercial market research panels (for a discussion see Craig et al., 2013). Crowdsourcing platforms also assign unique identifiers to individual participants, limiting multiple submissions to the same study (Casey, Chandler, Levine, Proctor, & Strolovitch, 2017; though see section on deception), and allowing researchers to specify which individuals should or should not be allowed to participate in any particular study.

As flexible and convenient as crowdsourcing platforms are, their ultimate value is contingent on whether crowdsourced samples produce high quality data, making it a concern for some researchers (e.g., Ford, 2017; Pham, 2013). Effects observed within undergraduate samples from fields such as cognitive psychology (Crump, McDonnell, & Gureckis, 2013), social psychology (Klein et al. 2014; 2017), judgment and decision-making (Paolacci, Chandler, & Ipeirotis, 2010), and economics (Amir, Rand, & Gal, 2012) have all successfully replicated with MTurk participants (MTurkers). Yet at the same time, editors and reviewers will sometimes reject conclusions based on MTurk studies. For instance, editorial instructions at a major marketing journal routinely urge authors "to move the non-student sample/studies beyond MTurk due to limitations of this sample," referring authors to a news article that highlighted the problems connected to MTurkers being experienced research participants. More strikingly, one collaborator of the authors received the following reviewer comment on a manuscript employing MTurk samples: "Experiments should be done in settings where people pay attention. There is nothing that can be learned from research on MTurk except that multitasking produces dumb answers." The editor agreed and the manuscript was rejected on this basis.

Concerns about the quality of data collected from MTurk and similar services (e.g., Prolific; see Palan & Schitter, 2017, and Peer, Brandimarte, Samat, & Acquisti, 2017) are best formulated as hypotheses and addressed with evidence. In this chapter, we discuss common concerns that researchers (and reviewers) have with MTurk, reviewing the evidence that bears upon each concern. In general, the available evidence shows both that high quality data can be collected from MTurk and that high quality data is by no means guaranteed. There are many pitfalls to collecting data from online respondents that researchers accustomed to running lab experiments may overlook. These issues are addressed in market research panels by the combined efforts of sampling statisticians, project managers, survey methodologists, and programmers. Whereas using MTurk allows researchers not to pay for such services, obtaining high-quality data requires researchers to successfully play all these roles. To help researchers think through the relevant design choices, we close our discussion with a table that should aid design considerations for researchers who are considering running an MTurk study.

Importantly, this chapter assumes that readers are knowledgeable about best practices in web study design (for an overview see Couper, 2008). Moreover, note that this chapter is not intended to provide a complete description of MTurk as a participant pool (e.g., Chandler & Shapiro, 2016; Paolacci & Chandler, 2014; for a review of MTurk as a participant pool for consumer psychology, please see Goodman & Paolacci 2017), a technical explanation about how to use MTurk (e.g., Mason & Suri, 2012), or a list of the possible research applications of MTurk (Stewart, Chandler & Paolacci, 2017). Similarly, we do not discuss the ethical aspects of using MTurk, but urge readers to consider both academic perspectives on these issues (Williamson, 2016; Gleibs, 2017) and the “Guidelines for Academic Requesters” provided by the MTurk community. Our focus is on addressing the practical concerns that the community has with the use of MTurk in consumer research.

Concern 1 – MTurkers do not invest sufficient effort in answering questions.

Consumer research relies on participants making a good faith effort to perform the tasks that are asked of them. Negligent participants who do not read or follow instructions, miss key details of experimental stimuli, or provide “random” or superficial responses to questions are a nuisance that obscure true relationships between variables of theoretical interest. Generally speaking, low-effort responding increases measurement error, especially when sample sizes are small (Lind & Zumbo, 1993). Though random error can lead to underestimating effect sizes, large sample sizes limit the potential for Type 2 errors (failure to correctly reject a null hypothesis when the alternative hypothesis is true). However, it is often overlooked that when inattentive response patterns are correlated with the substantive content of questions, inattentive responding can also increase *systematic* error, leading to false positives that cannot be overcome by recruiting larger samples. For instance, straight-lining (i.e., selecting the same option for all questions in a scale) creates the appearance of consistency in responses, leading to spurious correlations, inflated internal reliability of measures that do not include reverse-coded items (Barnette, 1990; Wood, Harms, Lowman, & de Simone, 2017), and reduced discriminant validity of different predictor variables (Hamby & Taylor, 2016). These issues are particularly relevant for consumer psychologists, because they complicate the use of mediation, factor analytic, and structural equation modeling techniques to disentangle the roles of different psychological processes in explaining an effect.

Insufficient effort is a major concern regardless of sample, but may feel particularly salient for MTurk—where participants are virtually unsupervised and are particularly motivated to complete studies as quickly as possible in order to maximize payout (for a review of this concern, see Ford, 2017). But at the same time, MTurkers have an incentive to maintain a high reputation that may discourage this behavior. Researchers can unilaterally reject MTurker submissions, which not only denies payment to the participant but also lowers their reputation and in turn reduces access to future studies. It is therefore an empirical question whether MTurk participants tend to behave more, less, or equally conscientiously compared to participants in physical labs.

Evidence

The evidence for whether MTurkers fail to invest enough effort in responding to surveys is mixed, but most evidence supporting this claim is circumstantial. MTurkers do self-report completing studies while also engaged in other tasks, like chatting online, listening to music, and watching TV (6%, 14%, and 18% of participants respectively; Chandler, Mueller, & Paolacci, 2014, see also Necka, Cacioppo, Norman, & Cacioppo, 2016). Also consistent with attentiveness concerns, MTurkers tend to complete studies faster than other populations (Smith, Roster, Golden, & Albaum, 2016; Kees, Berry, Burton, & Sheehan, 2017) and may show lower discriminant validity (Hamby & Taylor, 2016).

On the other hand, by many more direct indicators, the quality of data produced by MTurkers compares favorably to other samples typically used in consumer research. Measures of internal reliability, convergent reliability and (perhaps most compellingly) test-retest reliability are often quite good (Shapiro, Chandler, & Mueller, 2013). Much of the concern about inattentiveness is a result of normatively high failure rates on measures intended to assess participant attention ("attention checks", see Solutions below). These failure rates are less troubling for MTurk research in particular, and perhaps more troubling for researchers in general, when compared to other samples. On these measures, MTurkers perform at similar or higher rates than unsupervised and supervised college student samples (Hauser & Schwarz, 2016; Peer et al., 2017; Ramsey, Thompson, McKenzie, & Rosenbaum, 2016), samples recruited from online panels (Kees et al., 2017), and community samples (Peer, et al., 2017; for a review, see Thomas & Clifford, 2017). However, measures of attentiveness are sensitive to learning effects (reviewed below), and they should not be regarded as conclusive evidence.

Perhaps the best evidence that MTurkers can be attentive is that results of reaction time studies that measure response time differences in the tens of milliseconds (and are presumably very sensitive to inattentiveness) compare well between MTurkers and college students (Crump, et al., 2013; Enochson & Culbertson, 2015; Klein et al., 2013; Zwaan et al., 2017). In sum, there is mixed evidence that MTurkers are less attentive than other samples. However, since attentiveness is often critical to research results, researchers should consider how it will be ensured or controlled for regardless of the platform they use.

Solutions

Researchers who want to address inattention in their sample have three major decisions to make. First, they need to decide if they want to motivate MTurkers to be attentive or simply identify and exclude inattentive MTurkers (or both). Second, if they want to exclude inattentive MTurkers, they must decide how exactly to define them as such. Third, researchers have to decide whether to remove inattentive participants through ex post data cleaning or to prevent them from completing the study ex ante.

To motivate MTurkers to be attentive, studies should follow general principles of web survey design and be no longer or tedious than necessary (Galesic & Bosnjak, 2009). Beyond this uncontroversial advice, however, evidence on the success of other solutions is limited or mixed. One promising strategy is to ask (or plead) for participants from the outset to pay close attention lest they be detected by attention monitoring measures ("warnings", Huang, Curran, Keeney, Poposki, & DeShon, 2012). However, while this increases attention, it also seems to increase socially-desirable responding (Huang et al., 2012; Clifford & Jerit, 2015). Another strategy is to require participants to pass questions that provide non-obvious response instructions in larger blocks of text to highlight the necessity of reading all instructions ("trainers", Oppenheimer, Meyvis, & Davidenko, 2009), but some studies have found these to be ineffective (Berinsky, Margolis, & Sances, 2016). A third strategy is to remove the speed advantage of inattention by displaying text incrementally (Kapelner & Chandler, 2010) or imposing a time delay to prevent people from skipping through critical stimulus materials, though this may increase attrition and generally be irritating to participants.

Another way of ensuring an attentive sample is to select MTurkers on the basis of their past data quality [e.g., their Human Intelligence Task (HIT) Approval Ratio]. While this assumes that past data quality is a good proxy for future data quality (and, as a corollary, assumes that most requesters actively identify and reject low quality responses rather than freeride on others' quality control), MTurkers' history might be just as reliable an individual-level screening criterion as measures collected within the target study (e.g., attention as measured by attention checks, reviewed below). Indeed, MTurkers with a >= 95% HIT approval ratio (the vast majority) score better on measures of attentiveness compared to MTurkers with a < 95% HIT approval ratio (Peer, Vosgerau, & Acquisti, 2014).

Researchers frequently justify ex-post exclusions by assessing whether participants pass "attention checks" as defined by whether i.) they follow explicit experimenter directions (commonly called "instructional manipulation checks" Oppenheimer et al., 2009; Meade & Craig, 2012), ii.) they provide information about the world that is factually correct ("red herrings" or "catch trials"; Beach, 1989) or iii.) they are logically consistent with responses to other questions (Behrend, Sharek, Meade, & Wiebe, 2011).

Attention checks have the potential to improve data quality but also have measurement problems that should not be overlooked. First, selecting participants conditional on passing attention checks is conceptually problematic because attention checks seem to screen individuals for *trait-attentiveness*, which itself is correlated with other participant characteristics (Berinsky et al., 2014; Thomas & Clifford, 2017). Second, attention check items vary widely in difficulty (Thomas & Clifford, 2017), and there is no accepted standard about what level of difficulty optimally classifies "attentive" and "inattentive" participants. Third, individual items meant to measure attention are only weakly correlated with each other (Berinsky et al., 2014; Meade & Craig, 2012; Curran, 2016; Huang, Bowling, Liu, & Li, 2015; Niessen, Meijer, & Tendeiro, 2016; Thomas & Clifford, 2017), suggesting that measuring attentiveness is inherently challenging. Fourth, like all measures that are repeatedly administered to a population, attention checks are likely sensitive to learning effects that negate their ability to assess attention (see Concern 3 – nonnaivete), so boilerplate attention checks may be ineffective. Finally, attention checks can contaminate participants' responses to later questions (Hauser & Schwarz, 2015, but also see Kung, Kwok, & Brown, 2017).

Instead of measuring attention in general, researchers can assess comprehension of critical experimental materials (a "factual manipulation check"). This approach measures directly what attention checks try to measure indirectly —whether the participant was aware of information that is necessary to produce the phenomena of interest. This approach can be challenging in some studies (e.g., ensuring that comprehension checks across conditions in a between-participants design are equally difficult to pass), but should be preferred to attention checks whenever possible. Note that comprehension checks that reflect on study-specific information are also less sensitive to learning effects.

An alternative to explicit attention or comprehension checks is to unobtrusively monitor responses for anomalous patterns that indicate potentially low-effort responding, such as selecting the same response for every question, selecting only extreme responses, or selecting responses in a stairwise or "Christmas-tree" manner. Many of these patterns are identified visually, which is qualitative, highly subjective, and difficult to defend. However, quantitative measures of these tendencies exist, such as the long strings index (Costa & McCrae, 2008; Johnson, 2005) and multivariate outlier detection tests, such as Mahalanobis distance (Huang et al., 2012; Ehlers, Greene-Shortridge, Weekley, & Zajack, 2009; Meade & Craig, 2012; Rasmussen, 1988). Similarly, unusually fast response times can also reveal inattentiveness (Kittur, Chi & Suh, 2008), and one second per item (on multi-item grids) has been suggested as optimal threshold for excluding inattentive (or at least inconsistent) participants (Wood et al., 2017).

Each method of detecting inattentiveness generally improves data quality by imperfectly eliminating a specific kind of problem participant. When these methods are used in conjunction, their results converge on a much more accurate and reliable assessment of inattentiveness (Huang et al., 2012; Meade & Craig, 2012; Curran, 2016).

Finally, if researchers decide to identify and remove inattentive participants, they must decide between making exclusions ex-ante or ex-post. On one hand, excluding participants ex post is wasteful, and if a data cleaning strategy is not preregistered, the sheer number of arbitrary data cleaning decisions inevitably raises concerns about researcher degrees of freedom (Simmons, Nelson, & Simonsohn, 2011). On the other hand, conditioning participant inclusion on attentiveness can impact theoretical inferences beyond simply ensuring that participants paid attention. When variables of interest in a study are correlated with attentiveness, selecting participants based on attentiveness can spuriously inflate or deflate relationships between them (for a detailed overview, see Elwert & Winship, 2014). Removing participants ex-post allows researchers to examine the impact of inattentive participants and to inspect inattentive and attentive participants for any important differences.

All in all, when researchers consider inattentiveness to be a potential issue (and when increasing statistical power is ineffective or inefficient), we recommend including comprehension checks and triangulating several attention measures that are collected after participants completed the dependent variable, with an analysis plan that is preregistered and reported.

Concern 2 – MTurkers can't speak English.

One of MTurk's benefits is its international reach. While a large amount of MTurkers are from the United States or other predominantly English-speaking nations (about half of the MTurk pool; Pavlick, Post, Irvine, Kachaev, & Callison-Burch, 2014), a substantial proportion of MTurkers, particularly those from India (the second most represented country), are non-native English speakers. Non-native language speakers may understand the gist of a question but may sometimes miss crucial details, particularly because in an online survey nobody is available to clarify any misunderstandings. Cultural and linguistic differences may cause participants to misunderstand complex or idiomatic language, cultural references, or mentions of recent public events (for a detailed overview, see Malhotra, Agarwal & Peterson, 1996). Extremely poor language comprehension is likely to create issues similar to inattention, and is of particular importance when comparing means or effect size coefficients across nationalities.

Evidence

Most English language comprehension issues are discussed in the context of comparing Indian and US MTurkers, because these are the two dominant national populations on MTurk. Indian MTurkers are often well-educated (Khanna, Ratan, Davis & Theis, 2010), honest (Suri et al., 2011), and highly motivated to provide quality work (Gupta, Martin, Hanrahan, & O'Neill, 2014). However, in English language tasks they nevertheless give poorer quality responses than American MTurkers (Kazai et al., 2012; Khanna et al., 2010; Shaw, Horton, & Chen, 2011; Feitosa, Joseph, & Newman, 2015).

Solutions

If a study requires gathering diverse populations, a researcher may want to offer it in different languages or require participants to complete a language assessment before entering the study (Rastchian, Young, Hodosh, & Hockenmaier, 2010; Samimi, Ravana, & Koh, 2016). For studies with no such requirement, researchers may want to avoid non-native language speakers entirely because the process of developing measures that work across cultures and levels or language comprehension are tedious. Many of the methods for filtering out inattentive participants probably also filter out participants with poor language comprehension (Goodman et al., 2013; Litman, Robinson & Rosenzweig, 2015). Examining responses to open-text questions also seems to be a promising way to detect non-native English speakers. A possibly more efficient strategy (for researchers and participants alike) is for researchers to restrict eligibility to MTurkers who are likely to be proficient in the language of the survey (e.g., American MTurkers for English language surveys). However, note that non-US MTurkers can masquerade as

Americans through the use of virtual private servers (VPS; for a recent example, see Dennis, Goodson, & Pearson, 2018) though tools to prevent VPS users from entering a survey exist (TurkPrime recently implemented one).

As a more general reminder, many language problems on MTurk are not unique to the platform but are also observed in nationally-representative surveys that include non-native speakers as well as cross-cultural research projects (Sechrest, Fay, & Zaidi, 1972). Researchers are also likely to overestimate reading comprehension of a generalized non-scientific audience. When working with non-college samples, it is especially important to follow best practices in survey design, including using simple words and sentence structures and careful piloting or cognitive interviewing to ensure that participants understand what is asked of them (Gupta et al., 2014; Willis, 2004).

Concern 3 – MTurkers are experienced/nonnaive.

Another common concern with conducting studies on MTurk is that MTurkers are "professional participants" who have completed many social science studies and are likely to respond differently because of this experience. One version of this concern is that a general familiarity with how research experiments work will bias responses. For instance, experienced MTurkers may learn that deception is common in social science research and presume that researchers are likely to be deceptive, which may change the way they respond (e.g., Ortmann & Hertwig, 2002). A variation of this concern is that repeated participation in a specific paradigm can lead participants to acquire knowledge or experience that may change how they respond (for an empirical demonstration that these difference concerns are distinct, see Conte, Levati, & Montinari, 2014). Experience with specific paradigms gives participants the opportunity to practice a response or learn what the experimenter wants, the time to form strong opinions about issues that most people have not thought much about, and exposure to information from other experimental conditions that may contaminate their responses.

Concerns about nonnaiveté are warranted because the MTurk population is smaller than many assume (Difallah, Filatova, & Ipeirotis 2018; Fort, Adda, & Cohen 2014; Stewart et al. 2015) and is shared with many other researchers fielding unknown studies (a concern that also applies to market research panels; Hillygus, Jackson & Young, 2014). Moreover, whereas students eventually graduate and exit university participant pools, MTurkers (and other online panel members) can remain on the platform indefinitely.

Anecdotally, researchers have found instances where MTurkers share eligibility criteria and responses to common paradigms on MTurk forums. However, it is difficult to assess the impact of these posts without knowing how likely any particular MTurker is to see a post that is relevant to a study they have not yet completed or how discussion frequency in forums compares to the amount of crosstalk among physical lab participants. In one research study about 10% of participants reported finding a study on a site outside of MTurk (Casey et al., 2017), though most forums have strong norms against discussing study content (Chandler, Mueller, & Paolacci 2014). Note that, like most concerns in this chapter, worries about nonnaivete are not unique to only MTurk, as crosstalk has also been observed within college subject pools (Edlund, Sagarin, Skowronski, Johnson, & Kutter, 2009).

Evidence

The sheer number of studies that the typical MTurker participates in is likely orders of magnitude higher than the number completed by the typical college student. It is difficult to get exact numbers on how many HITs the average MTurker completes or has completed, but it may be dozens of studies per week (Kees et al., 2017; Smith et al., 2016). However, averages can be deceiving because experience follows a power law, with a small proportion of extremely active users and a long tail of less active users (Chandler et al., 2014; Rand et al., 2014). Active MTurkers tend to find and complete studies quickly (Casey et al., 2017), leading them to be both very experienced and overrepresented in studies of MTurker knowledge.

Evidence for the effects of MTurkers' general experience on responses is scant. Outside of MTurk, frequent participators in economic experiments trust less often and are less trustworthy than novice participators, even if their prior experience does not include trust games (Benndorf, Moellers & Normann, 2017). Similarly, though more research should be conducted, there are no indications so far that differently experienced participants may be differently likely to exhibit an effect on experiments involving political attitudes (Krupnikov & Levine, 2014).

On the other hand, many MTurkers report familiarity with paradigmatic experiments (Chandler et al., 2014), so several studies have investigated whether repeated exposure to the same research paradigm can undermine its validity. The answer is not clear-cut. Practice is known to improve scores on measures of ability. One such measure, the Cognitive Reflection Test (CRT; Frederick, 2005), is widely known among MTurkers (Thomson & Oppenheimer, 2016) and is frequently used in consumer research to assess individual differences in reflective thinking (e.g., Fernbach, Sloman, Louis, & Shube, 2012). Chandler and colleagues (2014) found that MTurker productivity, which likely correlates with exposure to the CRT, predicts scores on standard CRT items more than it predicts scores on a conceptually identical but cosmetically different test.

In other situations, knowledge about procedures and practice with experimental designs has a clear impact on responses. For example, MTurkers exposed to the same prescreening questions eventually learn what constitutes the "right" demographic features that enable them to gain entrance to a study and will claim to possess these attributes in subsequent studies (Chandler & Paolacci, 2017, Study 4). Knowing the incentive structure of an economic game might undermine the effectiveness of time pressure manipulations on decision making, as effortful decision making strategies become well learned (Rand et al., 2014). Similarly, Chandler et al. (2015) found a generalized reduction in effect sizes among repeated participants in a series of decision-making studies (see also DeVoe and House 2016), particularly when assigned to different conditions with short time delay. However, Zwaan and colleagues (2017) found no such effect using cognitive paradigms relying on automatic reactions, suggesting that nonnaiveté effects hinge on participants retrieving from memory information learned in previous participations. All in all, more research should be conducted to understand how, when, and why repeated exposure to a research paradigm affects future participation in MTurk as in other samples.

Solutions

Not all studies are vulnerable to participant nonnaiveté: for example, the conjunction fallacy is notoriously robust to even direct instruction on why it is an error (Stolarz-Fantino, Fantino, Zizzo, & Wen, 2003). Likewise, the predictive ability of the CRT (e.g., for susceptibility to heuristics and biases) appears not to be undermined by multiple exposures, because gains associated with practice reduce the extreme right skew (and associated range restriction) of the measure in naïve samples and tend to be disproportionately realized by people who score high on this construct (Bialek & Pennycook 2017; Meyer, Zhou, & Frederick 2018).However, many paradigms are more fragile, and for them nonnaiveté can be mitigated in several ways.

First, researchers may be tempted to place an upper limit on the total number of HITs completed to mitigate concerns about general nonnaiveté (TurkPrime has recently implemented a similar feature.). This practice has unknown effects on data quality and, depending on the threshold set, may slow data collection dramatically. It will certainly remove MTurkers with general experience, but there is currently little evidence that general experience is a problem.

Some sources of paradigm-specific nonnaiveté are more controllable. When researchers run sequences of studies that are conceptually related or cosmetically similar (e.g., replications or follow-up studies), they may want to exclude MTurkers who completed previous studies by manually assigning qualifications or using TurkPrime (Litman, Robinson, & Abberbock, 2017). Unfortunately, studies with similar procedures and measures are often conducted by other researchers, which is much more of a problem on MTurk compared to physical laboratories where researchers can coordinate to avoid it. Preventing previous participants from completing follow-up studies is thus a useful but often incomplete approach to eliminating non naive participants.

There are no perfect solutions to avoiding recruiting MTurkers exposed to similar studies by other researchers. It is, again, tempting to consider limiting the general experience of MTurkers, but the sensitivity and specificity of different experience thresholds for identifying those with study-specific experience is unknown and surely varies across different study paradigms. Asking about previous participation does not help, as MTurkers are not particularly accurate in their reports of which studies they have or have not completed, and the decline in effect sizes associated with prior exposure does not seem to be associated with self-reported memory of prior exposure (Chandler et al., 2015).

Fortunately, in many cases nonnaiveté may not be as problematic as existence-proof demonstrations of its impact may suggest. The decline in effect sizes associated with previous exposure is less pronounced when the time delay is longer (Chandler et al., 2015), which, combined with the slow but steady replenishment of the pool, attenuates nonnaiveté concerns for only occasionally-used experimental paradigms. However, some popular paradigms that may not be robust to participant nonnaiveté and that have been used extensively (anecdotally or based on recent publications, such as power manipulations) should be avoided on MTurk or modified in ways that prevent participants from drawing from memories of previous participation (Chandler et al. 2014).

Researchers should also monitor MTurk forums in order to verify that MTurkers are not sharing compromising information about the study (e.g., turkernation.com; mturkforum.com; reddit.com/r/hitsworthturkingfor; Hauser & Schwarz, 2016; Casey et al., 2017). Google seems to have good indices of these forums, so researchers can monitor by googling their requester name and the name of their HIT in order to find crosstalk.

Concern 4 – MTurkers are deceptive.

Social science research often relies on participants providing honest responses. However, there are many situations in which MTurkers may deceive researchers. We assume that most people are honest under most circumstances because intentionally deceptive responses are more difficult than intentionally honest responses (Fine & Pirak, 2016). In this review we ignore issues of social desirability, which are comprehensively discussed elsewhere (Krumpal, 2013), and instead focus on issues specific to MTurk.

The most obvious incentive to deceive is the subject fee payment itself. Web surveys are known to be vulnerable to people who try to complete them multiple times ("survey farming"; Chesney & Penny, 2013; Konstan, Rosser, Ross, Stanton, & Edwards, 2005) even though it is a clear violation of the Terms of Service of MTurk. For the researcher, duplicate participants violate the assumption of independence of observations and, for studies that contain multiple between-participants groups, could exacerbate issues of nonnaivete if the duplicate participant is exposed to different conditions. Moreover, as it has been show recently on MTurk (Dennis, Goodson, and Pearson 2018), willingness and efforts to engage in survey farming likely reveal lack of concern with providing valid and conscientious responses.

Research participants may also falsely represent themselves in order to gain access to a paid study that is intended for a certain population ("misrepresentation") regardless of whether the study is fielded on the web (Downes-Le Guin, Mechling, & Baker, 2006) or in the lab (Devine et al., 2013). For researchers, this increases sampling error and may also increase systematic error because fraudulent participants may use incorrect lay theories about how actual participants would respond in order to formulate their responses.

Because MTurkers have Internet access, MTurkers may also deceive researchers by looking up the answers to factual questions when researchers want them to respond with what they know (i.e., "cheating"; Clifford & Jerit, 2016). For the researcher, cheating participants add systematic error to measurements and muddies their interpretability. However, what looks like cheating may sometimes have been a genuine misunderstanding about how answers should be retrieved. For instance, participants may confuse knowing where to find the information with actually knowing it (Sparrow, Liu, & Wegner, 2011), which would presumably justify looking up the answers. Mere access to the Internet may explain this effect entirely, as cheating has also been observed in student and community samples asked to complete similar tasks online (Clifford & Jerit, 2014; Prior & Lupia, 2008).

Evidence

In practice, the proportion of MTurkers with multiple accounts, required for repeated participation, is likely to be low. There is little empirical evidence that MTurkers engage in survey farming unless one assumes that they are careful to cover their tracks. Typically less than 5% of participants submit responses from IP addresses that are shared with other participants. Some of these appear to be proxy servers used by ISPs, and of the remainder, about half of the responses seem to be generated by different people in the same household completing the survey (Chandler et al., 2014; Casey et al., 2017). Recently, Dennis and colleagues (2018) found evidence that some workers use virtual private servers to participate in multiple studies. Though these skilled and malicious workers may be very few, they can contribute a non-negligible fraction of responses in a given study, and they should therefore be identified and screened out.

Some recent research has examined the prevalence of misrepresentation in order to gain access to studies. Again, only a small percentage of MTurkers appear to engage in this behavior (Chandler and Paolacci, 2017). However, this kind of deception can have an outsized impact when the base rate of participants who truly possess a characteristic is low. For example, if 2% of the MTurkers who can participate in a study are deceptive and a study recruits a sample with characteristics that have a 6% incidence rate, a quarter of the sample (2/[2+6]) will consist of fraudulent participants. Consequently, studies that recruit low incidence populations report that a large portion of their final sample misrepresented themselves (Chandler & Paolacci, 2017; Sharpe-Wessling, Huber, & Netzer, 2017; Kan & Drummey, 2018).

Misrepresentation can impact data quality in different ways. When possible, participants who misrepresent themselves seem to respond to downstream questions in a manner consistent with their fraudulent identity. These responses will be undetectable (though still wrong) when participants' belief about how someone with their assumed identity is likely to respond is consistent with researcher expectations, but these responses will become apparent when their beliefs are wrong and their true identity is known. For example, people who claim to own pets (but do not) over-report purchasing name brand pet foods, people who claim to be old (but are not) over-report consuming fiber, and people who claim to be women (but are not) report a greater desire to own products that are pink rather than some other color (Sharpe-Wessling et al., 2017).

In other cases, participants may misrepresent their identity but answer other questions truthfully, particularly when they lack insight into how someone who truly possessed these characteristics would feel (Siegel, Navarro, & Thomson, 2015). This will tend to attenuate any differences between participants who claim this identity and those who claim other identities. It will also obscure true relationships between variables. For example, people who are truly older tend to feel that their future potential is more constrained than people who are younger, but this difference is not observed among deceptively old and young participants (Kan & Drummey, 2018).

In regards to looking up factual information, MTurkers score higher than other populations on many different measures of knowledge, including civics (Berinsky, Huber, &

Lenz, 2012), personal finance (Krische, 2014), science (Cooper & Farid, 2016), and computers (Behrend et al., 2011), raising the possibility of cheating. Some of these increased scores are at least partially attributable to higher levels of education and increased knowledge (Berinsky et al., 2012; Casey et al., 2017), but MTurkers have been found to look up the answers to questions in knowledge tasks (Peer et al., 2017), particularly when doing so allows earning a bonus (Goodman et al., 2013). For example, Goodman and colleagues (2013) found that 10% of MTurkers reported the Wikipedia answer to the question of how many African countries exist, as compared to none of the participants in a US community sample. Importantly, MTurk forums can be sources of information too: Sharpe-Wessling and colleagues (2017) asked MTurkers to guess the correct number of gumballs in a jar for $1, and the correct answer was posted on one such forum (though it was quickly removed and the amount of resulting deception was minimal).

Solutions

Preventing participant misrepresentation requires more than simply asking participants to refrain from enrolling in a study if they are ineligible. Unlike trait-inattentive participants who may be willing to self-identify as inattentive (Necka et al., 2016), people who lie will not admit it (Chandler & Paolacci, 2017). It is also difficult to conclusively prove that anonymous participants are lying, so researchers worried about deception must take care to design their study to prevent deception.

Deception occurs when it is incentivized, both on MTurk and in physical labs (e.g., Fischbacher and Heusi, 2013). Similarly, misrepresentation requires that people are aware that one response is incentivized and the other is not. This is especially true when the incentive for claiming a particular identity is high, so increasing the payment offered for a study targeting a specific subpopulation can increase misrepresentation (Chandler and Paolacci, 2017). In general, eligibility criteria should never be made explicit, not even when screening out participants, because excluded participants may re-attempt the survey by changing their responses. Technical methods to circumvent multiple responses help, but these can be defeated by participants and may not be sufficient to address this issue (Chandler & Paolacci, 2017).

If a study design requires prescreening, responses should be examined for potentially deceptive participants. Survey software should be configured to collect MTurk WorkerIDs, and multiple responses from the same MTurker should be compared to identify those who report different demographic characteristics after being screened out of the study. Another solution is to divorce study eligibility from payment, either by collecting eligibility criteria in a separate survey and re-recruiting participants into a new study (Chandler, et al., 2014) or, more elegantly, routing eligible participants immediately to a second study that they can complete in exchange for a bonus payment (Springer, Martini, Lindsey, & Vezich, 2016; for a validation, see Hydock, 2017).

There are few clear-cut solutions to avoid participants cheating when asked to provide answers to factual questions. Explicit warnings to not cheat may attenuate cheating (Corrigan-Gibbs, Gupta, Northcutt, Cutrell, & Thies, 2015; Goodman et al., 2013) but rarely eliminate it. Therefore, knowledge-based questions for which the answer is searchable online should be avoided if possible. At the least, researchers should avoid incentivizing correct answers, and monitor MTurk forums to verify that MTurkers are not sharing answers.

Concern 5 – MTurkers drop out of surveys.

Another recent concern around MTurk centers on attrition. Attrition occurs when participants who begin a study fail to complete it. While the impact of attrition on statistical inference has been studied for some time (Campbell & Stanley, 1963), it has recently been revisited in the context of online research, and MTurk in particular.

Compared to supervised surveys in physical labs, attrition is more likely to occur because participants lack costly commitments to participate, such as traveling to a physical lab. But attrition is also less obvious to detect because it is only observable as a partial completion record by survey software platforms and may not be included in data exports by default (Zhou & Fishbach, 2016). Moreover, survey platforms may sometimes not measure attrition unless participants answer at least one question, leading attrition rates to be underestimated.

Attrition can be fatal for inferences made from experiments because it can compromise random assignment (for an overview, see Greenberg & Barrow, 2014). Disproportionate attrition between groups may mean that the groups do not resemble each other on characteristics that may create confounds. For example, if one group is asked to complete very difficult math problems and another is asked to complete easy ones, participants with low math ability may drop out of the difficult condition but not the easy condition. If all participants are later asked to complete a measure of math ability, the researcher may find that the difficult math problem group performs better on the test, leading to the (false) conclusion that giving people difficult math problems improves math ability. The potential for attrition to bias responses depends upon both the overall attrition rate and the difference in attrition rates across groups following random assignment (for a detailed discussion, see Deke & Chiang, 2017). It is thus important to assess both overall and differential attrition.

Evidence

The level of MTurker attrition varies according to the study's characteristics, with wide variations seen between different studies ranging from 0% all the way up to 77.6% (Zhou & Fishbach, 2017). We're only aware of one paper demonstrating the consequences of attrition on MTurk (Zhou & Fishbach, 2017). In one study, MTurkers who were asked to recall twelve happy events from the past year were more likely to quit the task than those asked to recall only four happy events. As a result of this selective attrition, the recall-twelve group contained more successful people (or people with lower thresholds for happiness), leading them to report less difficulty in recalling twelve happy events than the group asked to recall only four happy events (Zhou & Fishbach, Study 2a, 2017). Another study demonstrates that even when attrition is comparable across groups, high levels can introduce bias. MTurkers were asked to either imagine applying eyeliner or aftershave cream. Even though both groups had similar levels of attrition, males (females) tended to drop out of the former (latter) group. Because men tend to weigh more than women, this lead to the observation that imagining applying eyeliner decreases self-reported weight (Zhou & Fishbach, Study 2b, 2017).

Solutions

Overall attrition can be minimized by avoiding fielding long studies or studies that include burdensome tasks such as mandatory wait times (Kapelner & Chandler, 2010) and attention trainers (Berinsky et al., 2016) unless they are necessary (see Concern 1). Participants' expectations about what would be required of them should also be aligned with what they are actually recruited to do, which requires providing them with clear expectations at recruitment.

Researchers can minimize concerns about differential attrition among groups in two ways. They could encourage people who are likely to drop out to do so before random assignment to conditions (Horton, Rand, & Zeckhauser, 2011). For instance, if the groups differ in difficulty of the task, researchers can give all participants a sample of the difficult task before assignment to groups so that participants who are sensitive to difficulty drop out during the sample task. Alternatively, they could ensure that all participants are exposed to materials from all experimental conditions and that participants who drop out at any point in time are excluded from analysis (Rand, 2012). For instance, if the groups are asked to rate joy- or disgust-eliciting images, the stimuli from other condition could be presented after the dependent measure so that disgust-sensitive participants are removed from both experimental conditions.

After data collection is completed, researchers should examine and report overall and differential attrition. Researchers could also consider collecting individual difference measures early in the experiment so they can examine whether these characteristics predict attrition (Jurs & Glass, 1971; Kazai, Kamps, & Millic-Frayling, 2012; Schleider & Weisz, 2015).

Concern 6 – MTurkers self-select into surveys.

One concern that is rarely addressed in psychology or consumer behavior research, but occasionally raised by critics from disciplines that often rely on representative samples (e.g., political science), is that MTurk samples are self-selected. Self-selection occurs when people decide whether to participate in a survey. On MTurk there are two layers of self-selection that could potentially cause issues: people self-select into joining the MTurk workforce and then, as MTurkers, they decide whether or not to complete a specific study. Like attrition, self-selection can bias estimates of associations between other variables that it is correlated with.

In this chapter, we do not discuss how self-selection can impact generalizability, but we want to note that self-selection at the task level can also impact the *validity* of findings, particularly when researchers recruit participants into different conditions using different tasks. This can be a tempting design choice when conditions vary substantially in length, compensation, or in what the participant is expected to do, but can ultimately result in different conditions recruiting different types of participants.

Self-selection can also cause the composition of samples to differ across studies, or over time within the same study (for a detailed discussion, see Casey et al., 2017). This can cause results observed across different studies, or at the beginning or end of the study to differ, potentially causing difficulties in exactly replicating the sampling procedures used by other MTurk studies.

Self-selection can be caused by many different design features of MTurk studies. Some aspects are extrinsic to the study, such as the expected pay/effort ratio or the overall *N* of the study. MTurkers that care disproportionately about making money, such as those who rely on MTurk as a major source of income, may be oversampled in high-paying HITs. Active MTurkers who aggressively monitor the platform for new HITs may be oversampled in HITs that have smaller *N*s. Other aspects might concern how the task is advertised, e.g., when topics are polarizing or more generally when they are non-uniformly interesting to the participant population. If a study advertises to be about a topic that is uninteresting or taboo to a group of people, then it may be less likely that these people will participate.

Evidence

Self-selection is difficult to detect because MTurkers are not a well-defined population and workers may view HITs or even study materials without this activity being detected. However, mundane details of a study, such as when in the day and in the week the study is launched, can influence who participates in a study. Relevant for certain studies, Casey et al. (2017) found that night participants were less prolific and conscientious, and more likely than morning participants to complete the survey using a smartphone (see also Arechar, Kraft-Todd, & Rand, 2017). Time of posting has a particularly large effect on the region that MTurk workers come from, with HITs posted in the morning (EST) particularly unlikely to attract respondents in more Western time zones. Currently, there is no evidence that these differences impact findings from experimental studies (Arechar et al., 2017).

Solutions

To make self-selection easier to detect, researchers should provide only basic descriptions of the nature of the study in the task description, such as payment, time estimates, and the general methodology of the study (e.g., survey). This keeps aspects of the survey hidden until workers open the survey link that might be disproportionately attractive to selected members of the population. Additionally, researchers can monitor their survey for potential self-selection by examining how many people dropped out of the survey after viewing the study description. Note that some survey platforms (like Qualtrics) will not record a participant until they provide at least one response. Bounce rates (i.e., how many people view the HIT but do not accept it) can be viewed on TurkPrime (Litman et al., 2017). Finally, the inherent difficulty of explaining the causes of self-selection makes it important to document and report the characteristics of the study (e.g., payment; Stewart et al., 2017) and on how the study was advertised (Paolacci & Chandler, 2018).

Considerations for running an MTurk study

The concerns outlined above impact a wide array of measures and manipulations, and there are many different ways to address them. However, these concerns are not applicable to all studies, and the solutions are often suited to particular circumstances. To that end, we have developed a table of considerations for researchers to review when running a study on MTurk (Table 1). Starting with the presumption that the researcher already has a study design in mind, the table guides the researcher through determining whether the major concerns are applicable to their study, suggests ways to address if it they are, and provides guidelines for what should be reported in manuscripts. It is not comprehensive by any stretch, but it is a good place to start for researchers who are new to MTurk.

Table 1. Considerations to review for researchers who want to field a study on MTurk

Ensuring Attentiveness

If your study contains response matrices, mark all questions, vignettes with subtle differences in wording/framing, scales with no reverse-coded items, or other questions that could be negatively affected by inattentiveness…

If you generally need your participants to be paying full attention to and giving full consideration to the questions and tasks in your study…

- Restrict your HIT to only MTurkers who have a ≤ 95% HIT approval ratio.
- Warnings, trainers, and incremental text display may motivate participants to pay attention, but be aware of their impact on later questions.
- Measure response speed for grid questions, and/or include comprehension checks after the dependent measure. Exclude workers using pre-determined and ideally pre-registered criteria.

Language comprehension

If your study does not examine linguistic differences or require gathering diverse populations…

- Restrict eligibility to only those in the geographic regions that are likely to speak the language of the survey fluently.
- Include questions that require free response text to detect language comprehension issues.

Minimizing Nonnaivete

If your study contains tasks that are sensitive to learning effects (i.e., prior exposure to the task allows the participant to complete/experience it in a way that may restrict the range of responses or undermine the association between the measure and the intended construct)...

If your study contains between-subjects manipulations that would be influenced by substantive knowledge of other experimental conditions...

If your study contains manipulations or measures you personally have used in the last month…

- Exclude MTurkers who completed your previous HITs.

If your study contains frequently used manipulations or measures (e.g., used in several *published MTurk studies)...*

- Use less popular manipulations/measures.
- Consider limiting study eligibility to novice MTurk workers or use alternate study populations.

Minimizing Deception

If your study relies on sampling a subpopulation of MTurk (i.e., females, homosexuals, Republicans, gamers, etc), and you do not know who belongs to the subpopulation...

- Pre-screen unobtrusively for the target population, and route eligible respondents to the full study to prevent misrepresentation.
- Record workerIDs for each survey attempt so that multiple responses from the same account can be linked.

If your study contains questions with factually correct answers that you do not want participants to look up…

- Instruct participants to not look up the answers and avoid rewarding them for getting questions correct.
- Look for evidence of crosstalk by searching MTurk forums for mentions of your HIT that may contain information about the study (asking participants where they found out about the study might help).

Attrition

If your study is experimental and contains difficult tasks, mandatory wait times, IMC trainers, or other questions or designs that are known to increase attrition...

- Collect individual difference measures early in the experiment to examine whether these characteristics predict differential attrition across groups.
- Consider exposing participants to stimuli from all conditions, even if using a between groups design focused on the first treatment condition.
- Encourage people to drop out before random assignment to avoid differential attrition across groups.

When your study is completed…

- Close open study cases before exporting data. Check attrition rates, and report both

overall attrition rate and group attrition rates.
Self-selection *When designing your HIT...* ● Consider only advertising the basic details of the experiment so as to prevent self-selection, and report how the study was advertised. ● Require participants to make some sort of response before reading the consent form. This will cause a record of the attempt to be generated and (if desired) linked to a workerID.

Conclusion

Consumer research using MTurk samples is common, but many still have concerns about using them. The evidence bearing upon these concerns does not demonstrate that MTurk samples are fatally flawed, but does provide many illustrations of problems that researchers should be aware of and take care to avoid. These problems apply to different studies to different degrees, and those who conduct or evaluate research should consider whether these issues can plausibly impact or explain the substantive conclusions of a research study. Importantly, there are tools to help address these issues, and this chapter reviewed how each concern can (or cannot) be attenuated. Our discussion of these issues should also help reviewers articulate their concerns and link them directly to how they expect these concerns to impact results. As long as researchers consider how these issues may affect their MTurk study, take the appropriate measures to assuage them, and report what they did in their manuscripts, then crowdsourcing platforms like MTurk can serve as a rich source data for consumer research.

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