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Mitigating Gig and Remote Worker Misconduct: Evidence from a Real Effort Experiment

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Abstract. Employee misconduct is costly to organizations and has the potential to be even more common in gig and remote work contexts, in which workers are physically distant from their employers. There is, thus, a need for scholars to better understand what employers can do to mitigate misconduct in these nontraditional work environments, particularly as the prevalence of such work environments is increasing. We combine an agency perspective with a behavioral relationship-based perspective to consider two avenues through which gig employers can potentially mitigate misconduct: (1) through the communication of organizational values and (2) through the credible threat of monitoring. We implement a real effort experiment in a gig work context that enables us to clearly observe misconduct. Consistent with our theory, we present causal evidence that communication of organizational values, both externally facing in the form of social/environmental responsibility and internally facing in the form of an employee ethics code, decreases misconduct. This effect, however, is largely negated when workers are informed that they are being monitored. We provide suggestive evidence that this crowding out is due to a decrease in perceived trust that results from the threat of monitoring. Our results have important theoretical implications for research on employee misconduct and shed light on the trade-offs associated with various potential policy solutions.

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Keywords: [unethical behavior](#) • [employee misconduct](#) • [employee cheating](#) • [employee governance](#) • [organizational values](#) • [corporate social responsibility](#) • [ethics code](#) • [monitoring](#) • [human resource management](#) • [strategic human resources management](#) • [social responsibility](#) • [strategy and policy](#) • [governance and control](#) • [ethics](#)

Introduction

Jobs in which workers are physically distant from their employers are becoming increasingly prevalent. This is, in part, a result of a surge in the “gig” or “sharing” economy¹ (Kokkodis and Ipeirotis 2015, Sundararajan 2016, Stanford 2017, Hagiu and Wright 2019) and, in part, because of increases in remote work more broadly. According to the McKinsey Global Institute, roughly 11% of U.S. and European workers earn their full-time income through gig work, and another 14% participate in the gig economy through part-time or occasional freelance work.² Remote work more generally increased 159% in the United States between 2005 and 2017 and is expected to become a prominent feature of a “new normal” after COVID-19.³ Both gig and remote work contexts are marked by the fact that workers are physically distant from their employers, resulting in unique employee governance challenges. In particular, this physical separation often makes employee motivation and oversight more difficult (Shamir and Salomon 1985; Kurland and Egan 1999; Wiesenfeld et al. 1999, 2001; Mann and Holdsworth 2003).

Drawing from agency theory (e.g., Ross 1973, Jensen and Meckling 1976, Harris and Raviv 1979, Hölmstrom 1979), we posit that employee misconduct is likely to be more prevalent in gig and remote work settings. Specifically, we argue that the physical separation inherent in these contexts intensifies the principal-agent problem in two important ways. First, gig and remote workers are likely to feel less connected to their employing organizations, thus exacerbating the misalignment of interests between workers and the firm. Second, physical separation typically comes with a greater degree of information asymmetry. Under these conditions, employee misconduct—already a widespread problem costing U.S. firms as much as \$600 billion annually in traditional work settings (Murphy 1993, Shulman 2012, Pierce et al. 2015, List and Momeni 2021)—is likely to be a critical and growing challenge.

We combine an agency perspective with a relationship-based behavioral perspective to frame our consideration of potential policy solutions in gig and remote work contexts. We, thus, respond to a call

from management scholars to combine agency theory with complementary theories in order to incorporate a more complex and realistic view of human and organizational behavior than that afforded by agency theory alone (Eisenhardt 1989). Specifically, we draw on tenets of agency theory to describe the high-level levers that an employer can pull to mitigate gig and remote worker misconduct: (1) by increasing the interest alignment of goals between the worker and the organization and (2) by decreasing the degree of information asymmetry between the worker and the organization. Although the effects of pecuniary incentives on such levers have been explored extensively (e.g., Oyer 1998, Obloj and Sengul 2012, Frank and Obloj 2014, Larkin 2014, Larkin and Pierce 2015, Gubler et al. 2016), there has been relatively less consideration of how relationship-based motivators and incentives can influence these levers and interact with other policies designed to reduce worker misconduct (Flammer and Luo 2017).

We first propose an understudied avenue through which employers can potentially mitigate gig and remote worker misbehavior: though communication of organizational values, which we argue increases the alignment of interests between worker and employer by fostering a sense of shared values with the firm. Next, because monitoring gig and remote workers is often challenging in practice, we consider how the credibility of the threat of monitoring is likely to affect misconduct in these settings. Finally, we also predict that policies emphasizing organizational values are likely to be *less* effective when monitoring (or the threat thereof) is also utilized. Specifically, we argue that the threat of monitoring is likely to lower perceived trust between worker and employer. This reduction in trust inhibits workers from forming the sense of shared values with the employer that the communication of organizational values would otherwise elicit, thus reducing the effectiveness of this approach in aligning workers' interests with those of the organization. This crowding-out effect is likely to apply not only to gig or remote work settings, but to traditional work settings as well, and to our knowledge, it has not been theoretically examined or empirically tested in either.

Assessing the effectiveness of employer-level policies intended to reduce misconduct is empirically challenging for two critical reasons. First, misconduct is exceedingly difficult to measure with accuracy in real work settings because workers have the incentive to conceal such behavior (Burbano and Ostler 2020). Second, employer-level policies, such as those outlined here, are rarely exogenous in practice (Burbano 2016), making it difficult to separate their causal impact on misconduct from other potential confounding factors. To sidestep these challenges, we designed a novel real effort experiment in a gig work context that

enabled us to observe and accurately measure misconduct as well as to randomly assign communications regarding employer-level policies.

In our study, workers were hired on an online gig market platform to complete a short-term assignment that entailed entering information into the "contact" sections of various websites. They were also given the optional opportunity to earn bonus payments by contacting the website owners by phone with instructions to either leave a scripted voicemail or to obtain answers to a market research survey. Unbeknownst to the workers, we owned and operated both the websites to which they were directed and the corresponding phone numbers listed. As such, we were able to cleanly observe whether workers actually entered the requested information into the website as directed (or shirked on the job) as well as whether they fraudulently claimed any bonus payments from their employer.

Workers were randomly assigned to one of six conditions in a three-by-two design. First, workers randomly received a message about the employer's ethical values (an expression of *internally* oriented organizational values), a message about the employer's social/environmental values (an expression of *externally* oriented organizational values), or no additional messaging. Second, we manipulated whether workers were informed about the potential that their work would be monitored. Notably, from the workers' perspective, the credibility of the monitoring threat varied between the primary task (for which the threat was not particularly credible) and the bonus task (for which the threat was at least partially credible), allowing us to shed light on the extent to which credibility matters.

We find that, when implemented individually, both communication of organizational values and the threat of monitoring are effective at reducing employee misconduct by statistically and economically significant margins. (Indeed, even when *not* credible, the threat of monitoring reduces misconduct though the effect is less robust here versus when the threat is credible.) When implemented in combination, however, the effects of values-oriented and monitoring policies are not additive. Specifically, when the threat of monitoring is in effect, the communication of organizational values does very little to further reduce misconduct. We explore potential underlying mechanisms and find suggestive evidence consistent with the theory that monitoring erodes the trust needed to establish a sense of shared values between workers and the firm. Indeed, empirical results indicate that the threat of monitoring substantially reduces workers' perceptions of employer trust. Furthermore, the communication of ethical and social/environmental values increases workers' perceptions of shared values with the firm but only when monitoring is not also in effect.

We extend scholarship on misconduct by combining two perspectives—an agency and a relationship-based behavioral perspective—to develop theory on how employer-level policies are likely to influence gig and remote workers’ organization-harming behavior. In so doing, we apply a more complex view of agent (in our context, gig or remote worker) behavior than that afforded by agency theory alone (Eisenhardt 1989) and identify ways that policies designed to address worker misconduct could backfire when agents behave based on social and relationship-based considerations in addition to economic ones. Moreover, our paper contributes revealed rather than stated behavioral evidence on the way in which employer-level characteristics influence misconduct in a real work context—an important contribution to the broader literature on misconduct, which has been limited to date in that it has largely been based on laboratory experiments or self-reported survey data (Pierce and Snyder 2008, Edelman and Larkin 2014, Pierce and Balasubramanian 2015) rather than on behavioral evidence in which real work effort and outcomes are observed (Burbano and Ostler 2020, List and Momeni 2021). Although our paper is focused on gig and remote work contexts, we also discuss the extent to which our results may generalize to more traditional work contexts in our conclusions, arguing that the integration of a relationship-based perspective with agency theory leads to a deeper understanding of the drivers and mitigators of employee misconduct in organizations more broadly.

Mitigating Employee Misconduct

Employees ostensibly engage in misconduct because such behavior benefits them in some way. Workers who shirk, for example, gain time for leisure (or for other work for which they may receive additional compensation), and workers who engage in theft or fraud gain financially, at least if their actions go undetected. Yet, although workers may benefit from engaging in misconduct (Hirsh et al. 2018), the organizations for which they work are often harmed as a result. Although some prior literature has highlighted specific types of employee misconduct that might directly or indirectly benefit the organization (e.g., Vardi and Wiener 1996, Pinto et al. 2008, Umphress et al. 2010, Bennett et al. 2013, Pierce and Snyder 2015, Burbano and Ostler 2020), we focus on forms of misconduct that are detrimental to firm performance. From a strategic human capital management perspective, this organization-harming misconduct is arguably the type that firms most seek to curtail.

Organization-harming misconduct occurs in work contexts in which the interests of the employing organization and the worker are not naturally aligned.

When this misalignment of goals is combined with information asymmetry between the employer and employee, a canonical agency problem arises (e.g., Ross 1973, Jensen and Meckling 1976, Harris and Raviv 1979, Hölmstrom 1979) such that it is often optimal for agents (workers) to make choices that are not in the best interests of the principal (the employing organization).⁴

This general framing of employee misconduct is one of the oldest applications of agency theory, and it holds in both traditional workplaces and in gig work environments. The extent to which information asymmetry and incentive misalignment persists or is the more challenging problem likely differs across various organizational contexts, however. Our focus in this paper is on gig and remote work, and an important defining characteristic of these work environments is the presence of *physical distance* between employer and worker. This physical distance exacerbates the principal-agent problem in two important ways. First, physically distant workers are likely to feel less connected to and, thus, identify less strongly with their employers compared with traditional workers (Wiesenfeld et al. 1999). This lower level of identification with the organization implies that the interests of workers are inherently less aligned with those of the firm, increasing the incentives for misconduct. Indeed, in related literature, perception of greater physical distance has been associated with lower worker productivity (Cramton and Webber 2005), lower willingness to do extra work (Burbano 2021a), lower engagement (Kahn 1990), and lower overall performance (Cramton 2001, Kanawattanachai and Yoo 2007). In addition, an obvious consequence of physical distance in many instances is that information asymmetries are amplified. As such, misconduct is likely more rampant in many gig and remote work contexts.

Agency theory implies that organizations can work to mitigate employee misconduct in two key ways: (1) by increasing the extent to which employees’ interests are aligned with those of the firm and/or (2) by reducing information asymmetry. To understand how organizational policies might achieve each of these goals and, in turn, influence worker behavior, it is important to incorporate a relationship-based behavioral perspective to reflect complexities in human behavior that often depend not only on economic dynamics, but also social and relationship-based ones. Indeed, to allow for a more complex view of individual and organizational dynamics, scholars have applied tenets of agency theory in combination with other perspectives to derive implications for organization and strategy theory. Examples include Eisenhardt (1988), who combined agency and institutional theories; Anderson (1985), who coupled agency and transaction costs theories; Eccles (1985), who combined agency with equity

theory; and Flammer and Luo (2017), who, most closely related to our paper, draw on tenets of agency theory and introduce the importance of considering relationship-based incentives and motivators in influencing employee adverse behavior.

In what follows, we focus on two potential ways in which gig and remote employers might achieve these aims and thereby mitigate worker misconduct. Importantly, by conceptualizing the decisions of agents as influenced not only by economic considerations but also by social and relationship-based considerations (Dunning et al. 2012), we identify new ways that these policy approaches might negatively interact.

Communication of Values as a Tool for Mitigating Employee Misconduct

One way in which organizations can mitigate the agency problem is through the implementation of policies that increase alignment between the goals of workers and those of the firm. In practice, this might be achieved in a number of ways. Various forms of financial incentive structures have been explored extensively (e.g., Oyer 1998, Obloj and Sengul 2012, Frank and Obloj 2014, Larkin 2014, Larkin and Pierce 2015, Gubler et al. 2016). Although these solutions may be effective by some metrics, such as increased productivity and less shirking, they often come with their own set of challenges, which include the crowding out of intrinsic motivation (Deci et al. 1999, Benabou and Tirole 2006), the proliferation of gaming behavior (Larkin 2014), increased fraudulent reporting and dishonesty (Balasubramanian et al. 2017), increased employee theft (Chen and Sandino 2012), and coworker sabotage (Charness et al. 2013, Flory et al. 2016). Apart from pecuniary incentives, employers may be able to better align employees' interests with those of the firm using relationship-based motivators and incentives (Flammer and Luo 2017). Exploration of the nonpecuniary drivers of misconduct suggests that relationship-based motivators and incentives are indeed important. For example, perceptions of unfairness and inequity (Greenberg 1990, 1993; Gino and Pierce 2009, 2010a, b; Larkin et al. 2012) have been shown to influence individuals' propensity to shirk, cheat, and misreport, for example, because of a tendency to justify dishonest acts based on benefits to others (Wiltermuth 2011, Erat and Gneezy 2012, Wiltermuth et al. 2013). Likewise, relationship-oriented factors, such as a sense of rivalry (Kilduff et al. 2015, Kilduff and Galinsky 2017), competition (Pierce et al. 2013), and feelings of power relative to others (Dubois et al. 2015) are shown to increase unethical behavior by individuals.

Employers might attempt to better align employees' interests with those of the firm through the adoption of policies that foster a sense of shared

purpose between workers and the organization. One important way in which employers might elicit a sense of shared values is by emphasizing organizational values that workers are likely to share. Examples of this in practice might include communications about a commitment to particular values *within* the organization (e.g., expectations that employees adhere to ethical values, such as honesty, fairness, etc.) or, alternatively, communications about the firm's commitment to certain values *outside* the organization (e.g., through various social and/or environmental initiatives).⁵

To the extent that workers share the expressed values, emphasizing them in communications should increase workers' perceived value congruence and person–organization fit with their employer. Value congruence is shown to be correlated with organization-benefiting perceptions (see Arthur et al. 2006 for a meta-analysis), identification with the firm (Tajfel and Turner 1979), and organizational citizenship behavior (Cable and DeRue 2002), while value incongruence is shown to result in organization-harming behavior, such as reduced motivation and work quality in gig and short-term work contexts (Burbano 2021b). Importantly for its potential to mitigate misconduct, value congruence and person–organization fit has also been linked with employees' sense of organizational commitment and support for their employing organization's objective (Valentine et al. 2002, Kristoff-Brown et al. 2005, Amos and Weathington 2008). By increasing support for the employing organization's goals and objectives, increased perceived value congruence on the part of the employee with the employer, thus, increases the alignment of interests between principal and agent. Put plainly, the more that a worker feels a sense of shared values with the organization, the more the employee will support the organization's interests and goals and the less likely the employee will be to engage in behavior that harms that organization through misconduct. We, thus, predict that communications emphasizing organizational values mitigate adverse behavior among workers.

Hypothesis 1. *Communications from the employer that emphasize organizational values reduce gig/remote worker misconduct.*

This hypothesis is contrary to a related prediction by List and Momeni (2021), who posit that communication about corporate social responsibility (CSR), which, by our argument, can be used to elicit a sense of shared values between the employer and employee and, thus, decrease misconduct, should, in fact, *increase* worker misconduct by eliciting moral licensing. As we discuss in more detail in the conclusion, our theorizing highlights the importance that an organizational policy, such as

CSR, be communicated in a way that elicits a sense of shared values between the worker and employer. Without achieving this sense of shared values, communication of organizational values will not reduce the misalignment of interests between the worker and firm, which is the key mechanism through which communication of organizational values is likely to *decrease* misconduct.

In traditional workplaces, espousing organizational values might be one part of a larger set of policies intended to build and reinforce a sense of shared values (Stoner 1989), and a broader and deeper approach to fostering shared values is likely to be more effective at mitigating misconduct than values-oriented messaging alone. Indeed, it is unclear whether claims, without the backing of actions, should be sufficient in influencing stakeholder behavior (Bromley and Powell 2012). In the case of gig and remote work, in which building a strong sense of organizational identification is challenging because of the physical separation of workers from the employer and from one another (Wiesenfeld et al. 1999, Bartel et al. 2012, Petriglieri et al. 2019), understanding the potential effectiveness of values-oriented messaging as a stand-alone policy tool is particularly important. Moreover, to the extent that there are measurable effects from messaging alone, such effects can be viewed as a lower bound for the potential benefit when a more comprehensive set of policies is available to be deployed in concert.

Threat of Monitoring as a Tool for Mitigating Employee Misconduct

In addition to policies that increase the degree of interest alignment between workers (agents) and the firm (the principal), another mechanism through which firms can mitigate misconduct is by decreasing the degree of information asymmetry between workers and the organization. Monitoring has long been a commonly proposed solution for reducing adverse behavior by this means (e.g., Becker 1968). When the principal can observe what the agent is actually doing, this curbs agent opportunism because the agent realizes that he or she cannot deceive the principal (Eisenhardt 1989). Essentially, increased monitoring of employee behavior by organizations directly reduces information asymmetry between principal and agent. Under these circumstances, workers should, thus, engage in less misconduct because the extent to which they are able to misbehave without detection and subsequent consequences is reduced. Indeed, monitoring is shown to be effective at reducing employee misconduct in a range of empirical settings (Hubbard 2000, Nagin et al. 2002, Detert et al. 2007, Olken 2007, DeHoratius and Raman 2008, Duflo et al. 2012, Pierce et al. 2015).

Actual monitoring, however, requires the ability to directly observe individuals' actions—an undertaking

that is simply not feasible in some work contexts. In gig and remote work settings in which the principal and agent do not share a physical space or interact in person, for example, there is notably less opportunity for an employer to observe the agents' behavior than in settings in which workers and employers share a physical space and interact frequently in person. Even within gig and remote work contexts, there is variance in the feasibility of monitoring worker behavior across settings. In some physically distant worker arrangements, technology can enable observation akin to that which would be achieved by sharing a common physical space. For example, Uber can monitor drivers' driving patterns, time spent on the road, etc. In such worker arrangements, monitoring is more easily implemented. In many others, though, it is difficult for (current) technology to facilitate the same degree of observation that can be conducted in a shared physical space and in contexts of constant interaction between the employee and employer. It is challenging for Upwork and Freelancer.com to accurately ascertain the amount of productive time spent working on jobs contracted through their online platforms, for example. Moreover, even in cases when direct monitoring is feasible, it is often costly for firms to implement (e.g., Dickens et al. 1989 note that many firms expend substantial resources on employee monitoring).

In gig or remote work contexts in which actual monitoring is often infeasible or may be too costly to implement, it is possible that the mere *threat* of monitoring could achieve similar goals. The threat of monitoring could heighten workers' sense of organizational oversight and, thus, decrease workers' *perceptions* of informational asymmetry between themselves and their employer (even if the actual amount of informational asymmetry remains the same). This would increase the workers' perceptions of the expected probability of being caught and punished for misconduct, which is likely to deter them from engaging in misconduct. The threat of monitoring is only likely to achieve these goals, however, if it is perceived to be credible. If not perceived to be credible, the threat of monitoring would arguably affect neither workers' perception of the amount of information asymmetry that exists between them and their employer nor their expectations about the likelihood that they would be caught. Thus, if gig or remote workers are discerning and perceive that the threat of monitoring is not credible given the physically distant nature of the work, we might expect no effect at all on misconduct. The expected effect of the threat of monitoring on gig or remote worker misconduct, then, depends on the credibility of the threat:

Hypothesis 2. *The threat of monitoring reduces gig/remote worker misconduct when the threat of monitoring is*

credible, but not when the threat of monitoring is not credible.

Finally, even when direct monitoring is feasible and not prohibitively costly to implement or the threat of monitoring is feasible and likely to be perceived to be credible, it is important to note that (the threat of) monitoring may be accompanied by unwanted negative consequences. Bernstein (2012), for example, finds monitoring to be associated with increased levels of gaming behavior among workers as well as reduced problem-solving capabilities. Frey (1993) and Litzky et al. (2006) argue that monitoring destroys trust between workers and the firm, decreasing intrinsic motivation and organizational commitment. Others have found that monitoring results in increased levels of employee stress and lower job satisfaction (e.g., Aiello and Svec 1993, Tabak and Smith 2005). The potential for these negative side effects must be taken into account as employers consider whether a monitoring policy is preferable to alternative approaches and/or compatible with other policies that the firm may be simultaneously pursuing. In particular, when conceptualizing the behavior of agents as influenced by social and relationship-based dynamics in addition to economic dynamics (Dunning et al. 2012), (the threat of) monitoring may carry unintended consequences that weaken the mechanisms through which other simultaneous policy efforts operate, and we address one particular instance of this possibility next.

The Interaction Between (the Threat of) Monitoring and the Communication of Values

We argue that both the communication of organizational values and the threat of monitoring should each independently reduce worker misconduct. Given that each is directed at a distinct driver of misconduct identified by agency theory (the threat of monitoring directed at reducing perceived information asymmetry and the communication of organizational values directed at reducing the misalignment of interests between the principal and agent), it seems reasonable that firms seeking to reduce misconduct might be most effective by implementing policies that address each of the drivers. We, thus, now focus on how effective these policies might be at reducing misconduct when used in combination with one another.

Of particular concern is the possibility that the threat of monitoring signals to workers that the organization does not trust them (Frey 1993, Cialdini 1996, Litzky et al. 2006, Ferrin et al. 2007). Indeed, control systems more generally have been shown to result in lower levels of trust and, subsequently, less *voluntary* cooperative behavior (e.g., Malhotra and Murnighan 2002, Mulder et al. 2006). Communication of organizational values, when fostering a sense of shared values,

improves interest alignment between the worker and employer and intrinsically motivates workers to *voluntarily want* to behave better toward their employer. (The threat of) monitoring, on the other hand, reduces misconduct because it decreases the (perception of) informational asymmetry between the worker and employer, which reduces misconduct because it makes workers feel as though they *have* to behave better toward their employer or else risk being caught and punished. Although this may induce “forced” good behavior on the one hand, it is likely that it simultaneously erodes the likelihood of additional “voluntary” good behavior. That is, when a control system that destroys trust, such as (the threat of) monitoring, is utilized, the sense of shared values and subsequent increase in alignment of goals between the principal and agent is eroded such that workers have no intrinsic desire to behave better. This argument is consistent with work that shows trust to help explain the effect of value congruence between the employee and organization (Edwards and Cable 2009) as well as to facilitate or hinder effects of other determinants on worker outcomes (Brockner and Siegel 1996, Dirks and Ferrin 2001).

In summary, (the threat of) monitoring destroys trust between workers and the firm. We posit that this lack of trust, in turn, inhibits workers from forming the sense of shared values and interest alignment with the firm that the communication of organizational values would otherwise elicit. Thus, we expect that monitoring negatively moderates the benefits that organizations might gain from the communication of values with regard to worker misconduct.

Hypothesis 3. *The communication of organizational values is less effective at reducing gig/remote worker misconduct when the threat of monitoring is also in effect.*

Although our focus in this paper is primarily on physically distant gig or remote work, this reasoning should apply to traditional work contexts as well. Furthermore, given that destruction of trust as a result of monitoring is exacerbated in contexts in which the relationship between the principal and agent is more interpersonal (Frey 1993), it is likely that this undesirable moderating effect would be even greater in traditional, physically close, work contexts. To our knowledge, this hypothesis has not been theorized nor empirically tested in either traditional or physically distant work contexts.

Experimental Setting and Design

Our study was conducted in May 2019 on Amazon Mechanical Turk (MTurk), a large, prototypical, crowd-based gig work platform (Scholz 2017).⁶ As with most major players in the gig economy, MTurk is

an intermediary platform that connects requesters (gig employers) with on-demand gig workers who complete jobs that are task based and short term while physically distant from their employer (Kaine and Josserand 2019, Meijerink and Keegan 2019). It facilitates the use of human intelligence to perform tasks for requesters (Kaine and Josserand 2019); as such, MTurk jobs are commonly referred to as human intelligence tasks (HITs). Typical jobs include simple data entry and survey completion tasks.

The experiment that we implement follows a three-by-two design as illustrated in Figure 1. We operationalize communication of organizational values in two ways: (1) communication of ethical values, an expression of mainly internally facing organizational values, and (2) communication of social/environmental values, an expression of mainly externally facing organizational values. This design allows us to observe the main effects of communication of organizational values (Hypothesis 1) and the threat of monitoring (Hypothesis 2) as well as any potential interaction effects between the two (Hypothesis 3).

Figure 2 details the basic design and logistics of our study. Acting as a hiring employer (specifically, a start-up firm offering a software product for interior designers), we advertised a data entry job estimated to take 5–10 minutes to complete.⁷ To help ensure that workers were unaware that they were participating in a research study, we used the identity of a real start-up organization when hiring workers (with the organization's approval). Importantly, during the time frame that the study was run, this company's website indicated that it was "under construction," preventing workers from discovering any potential confounding information about the hiring employer online.⁸ Payment for the job was \$1.50, with additional opportunities to earn bonus payments. The pay and nature of the job were designed to be typical of jobs in the MTurk context.

Workers interested in the job could click on the link included in our MTurk posting to receive further instructions on an external site before deciding whether to proceed. Neither the initial MTurk posting nor the

initial instructions on our external site contained any language associated with the treatment conditions; rather, treatment occurred only after workers began the job so as to mitigate potential selection effects. Only after choosing to continue the task were workers randomly assigned to one of our six conditions.

After random assignment, workers received subsequent information about the employer and the job corresponding to their assigned condition. Those in the ethical values conditions were informed that the hiring company believes in a culture of accountability and transparency in the workplace and strives to be honest, ethical, and fair. Those in the social/environmental (SE) values condition were informed that the hiring company believes in giving back to the community and in improving the environment, donating 5% of its profits to that end. Wording in both conditions was constructed to be representative of how companies typically describe such initiatives. For the SE values condition in particular, we used language corresponding to a common type of externally facing CSR initiative, namely charitable giving to benefit the community and environment. Finally, those in the threat of monitoring conditions were informed that 5% of HITs would be randomly selected to be reviewed for data quality. See Figure 2 for the exact wording used in each condition.⁹ Importantly, though we could, indeed, monitor actual effort/outcomes given the nature of the study's design (as described in more detail as follows), workers were not aware of this fact.

The job itself consisted of three key sets of activities: a (required) main task with multiple parts, optional bonus tasks, and a worker survey administered at the end of the engagement to collect demographic information and explore potential mechanisms/moderators. In both the main and bonus tasks, workers had both the opportunity and the incentive to engage in misconduct that, from their perspective, would potentially go undetected by the employer. In particular, the extent to which the threat of monitoring could be perceived as credible varied between the main task and the optional bonus task as the nature of the outputs requested varied between these two activities. We address this important distinction in more detail in the following section.

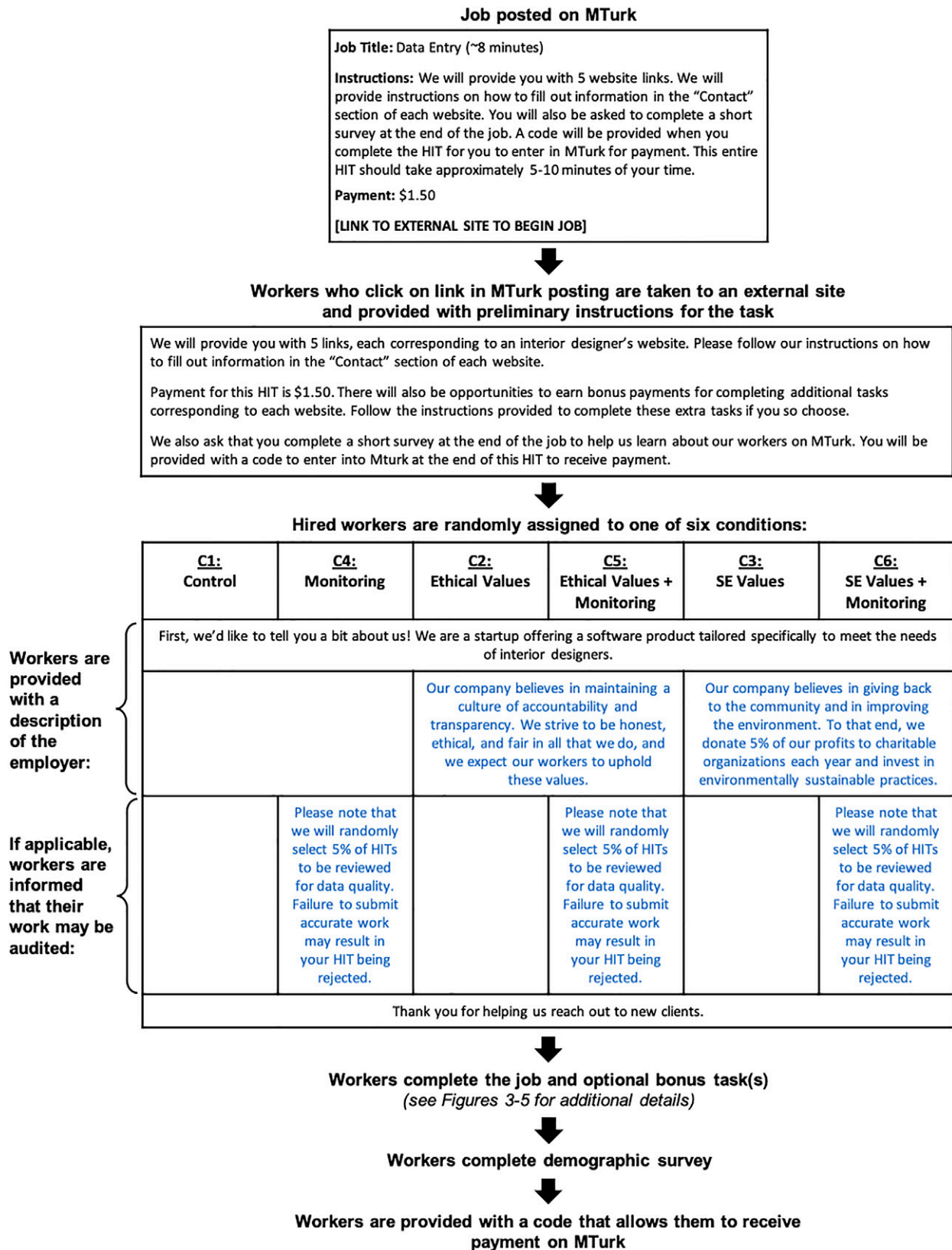
Figure 1. Three-by-Two Experimental Design

| | | No Values-Oriented Messaging | Ethical Values | SE Values |
|------------|------------|------------------------------|------------------------------------|-------------------------------|
| Monitoring | No | C1: Control | C2: Only Ethical Values | C3: Only SE Values |
| | Monitoring | C4: Only Monitoring | C5: Ethical Values + Monitoring | C6: SE Values + Monitoring |

Notes. For brevity, we use the term "monitoring," but in all cases, it is only the *threat* of monitoring that we actually implement. SE Values, social/environmental values.

Detailed Job Description

Figure 3 presents a diagram illustrating the main task, which required workers to visit five interior designers' websites and copy/paste a specified set of information into the contact form on each site. (Figure 4 presents screenshots from one of these interior design websites.) Workers' instructions were identical with the exception of language corresponding to the various treatment conditions (see Figure 3 for exact wording). Under typical conditions, it would be impossible

Figure 2. (Color online) Summary of Experiment Flow

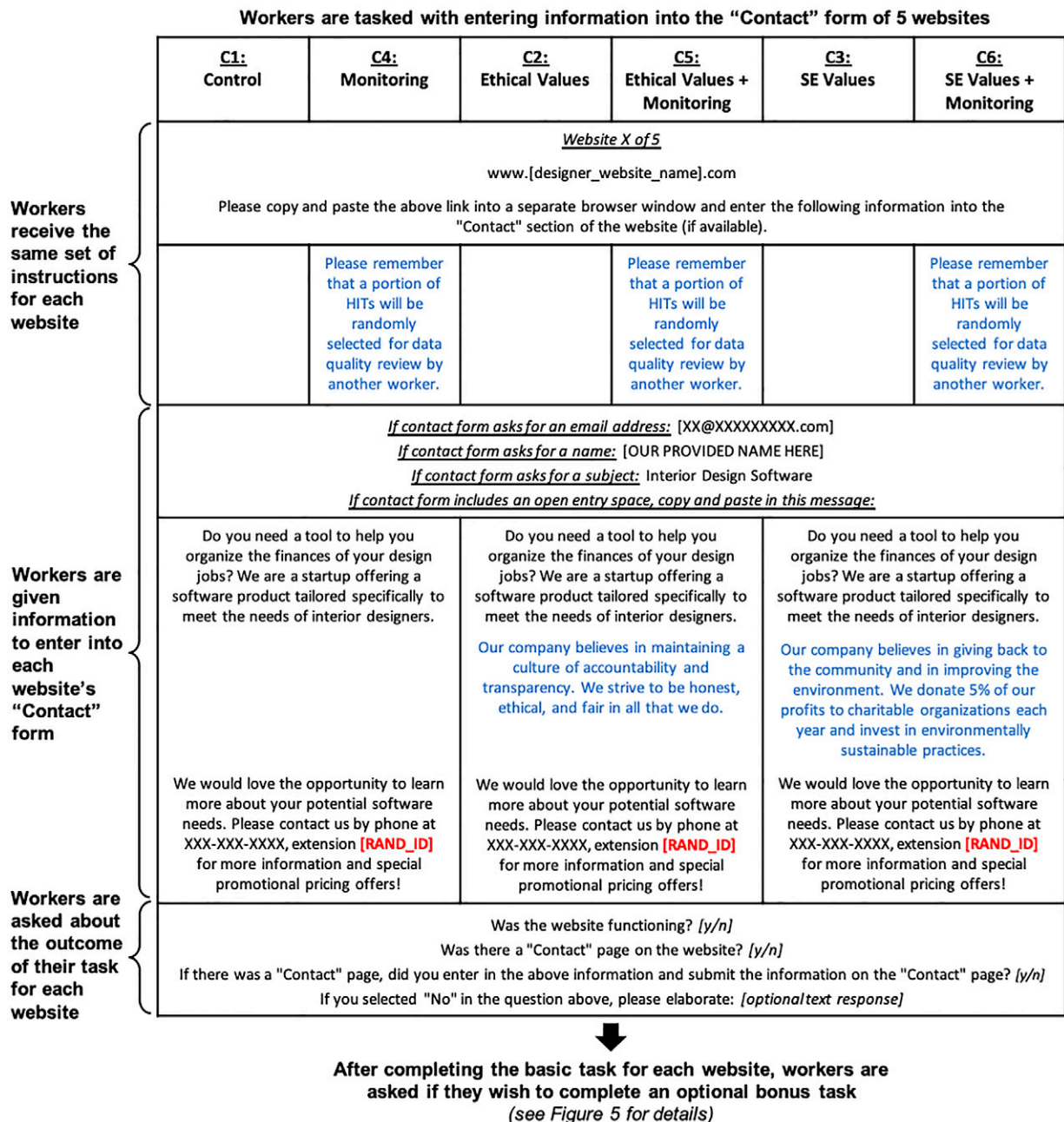
Notes. For brevity, we use the term “monitoring,” but in all cases, it is only the *threat* of monitoring that we actually implement. SE values, social/environmental values. Color highlights manipulated text for the benefit of the reader, and does not represent the text color seen by the workers.

for an employer to ascertain whether workers actually completed this task as instructed; thus, our threat of monitoring treatment was arguably not at all credible from the worker's perspective in this context. In actuality, though, we were the owners and operators of all five interior design websites and, thus, had access to all the data that workers were entering (or not entering). Moreover, upon hiring, in addition to being assigned a treatment condition, each worker was also assigned a unique random number. This number was

inserted into the text that workers were to submit to each of the five websites (in the form of a telephone extension number; see Figure 3). This identifying number, thus, allowed us to ascertain which workers had completed the data entry task as instructed and which had shirked (i.e., not entered the required data into the contact form for a given website).

After completing the main task for each of the five websites, workers were presented with an opportunity to complete an optional bonus task, detailed in

Figure 3. (Color online) Main Task Flow



Notes. For brevity, we use the term "monitoring," but in all cases, it is only the *threat* of monitoring that we actually implement. SE values, social/environmental values. Color highlights manipulated text for the benefit of the reader, and does not represent the text color seen by the workers.

Figure 4. (Color online) Screenshots from One of Our Interior Design Websites

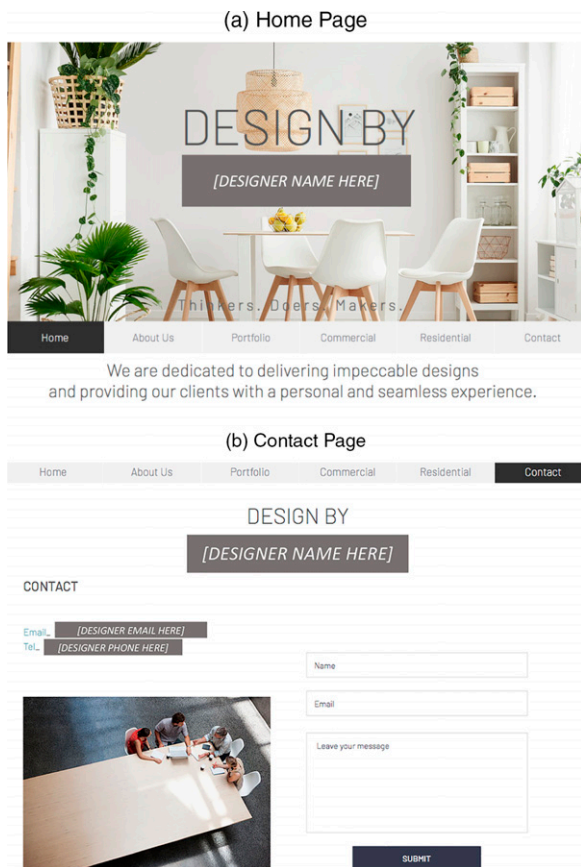


Figure 5. These bonus tasks allowed for a different form of misconduct as well as more opportunities to engage in misconduct in general (allowing for additional variation in misconduct at the intensive margin). This bonus task required workers to attempt to contact the designer(s) by phone using contact information listed on the corresponding website(s). We provided workers with two scripts: one containing a market research survey to administer if a call recipient answered, and one to read if a call rang to voicemail (see Figure 5 for details; language corresponding to the social/environmental and ethical values treatments was again incorporated here). Bonus payment differed depending on the call outcome. We offered \$0.25 for a completed market research survey (i.e., a maximum of \$1.25 in bonus payment if responses were obtained from all five interior designers) and \$0.10 for each voicemail left per our instructions. In practice, obtaining responses to the market research survey was impossible. As previously discussed, we were the owners of all of the interior designers' websites. Accordingly, we also controlled all of the phone numbers listed on these websites, and during the

course of this experiment, we ensured that no calls were answered. We could, thus, easily infer that any worker claiming to have obtained a response to the market research questions was misreporting this information.

We were also able to observe whether workers who claimed to have left a voicemail had actually done so. The same random number that allowed us to identify workers in the main task was also inserted into the voicemail script for the bonus task (once again, in the form of a telephone extension number; see Figure 5 for details). We were, thus, able to use this identifying number to discern whether workers had actually left voicemails in cases in which claims were made.

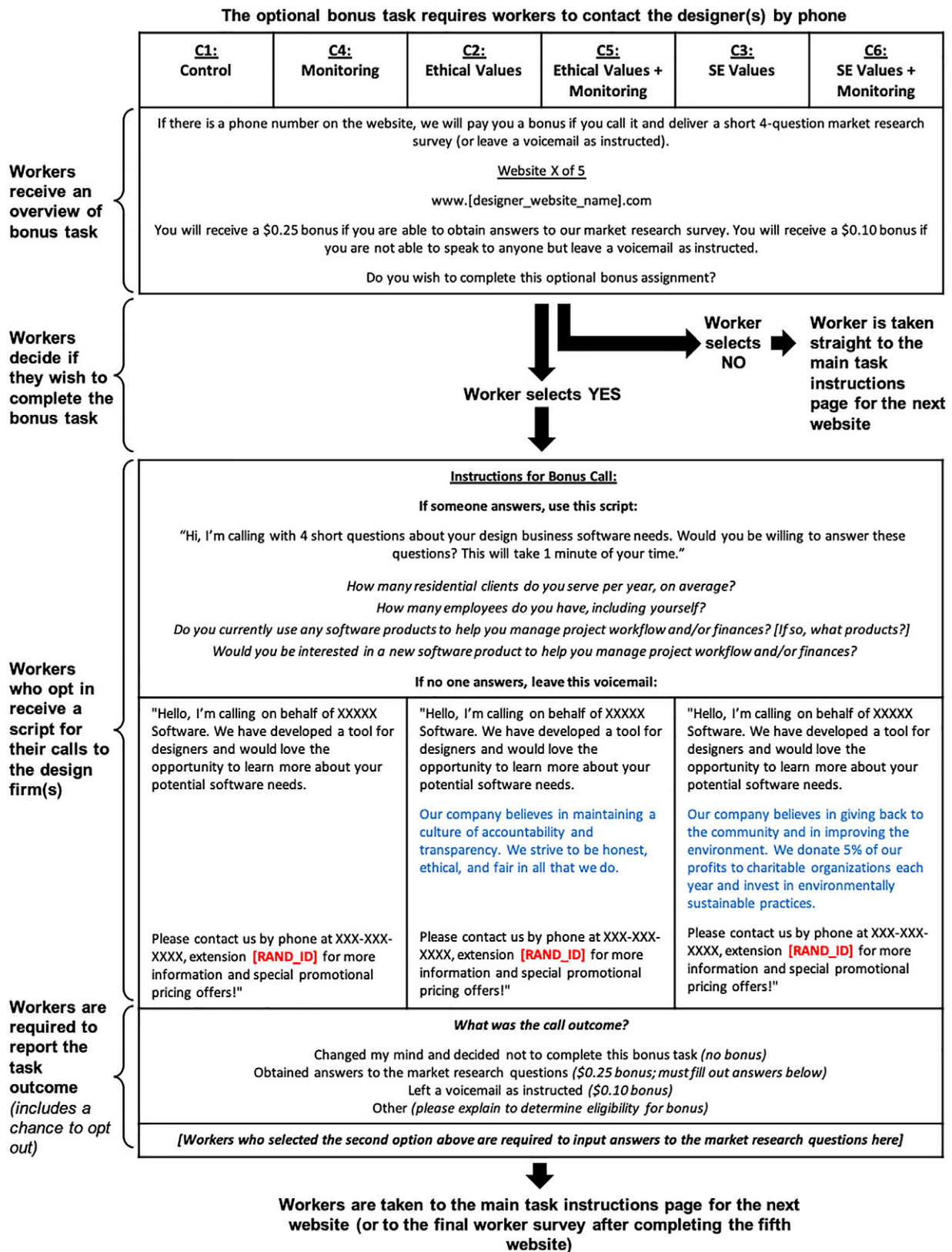
What about the credibility of our monitoring threat in the context of the bonus task? Recall that workers in this condition were informed that 5% of HITs would be randomly selected to be reviewed for data quality. The voicemail outcome, as with the central outcome in the main task, essentially involves workers leaving information on an external platform that a real employer would have no way of verifying. The higher paying form of misconduct in the context of the bonus task, however, required the fabrication of market research survey responses. This is an outcome that (from the worker's perspective) might plausibly be checked for accuracy, for example, if multiple MTurk workers were hired to replicate the survey/validate responses for some subset of HITs. The threat of monitoring, then, was at least partially credible in this context.

Following completion of the main task (and bonus task(s) if applicable), workers were also asked to fill out a short survey. In this survey, we collected basic demographic information (gender, age, etc.) and asked exploratory questions aimed at assessing how workers' agreement (on a five-point Likert scale) with statements about the employer and about misconduct in general might mediate or moderate any observed treatment effects.¹⁰ We presented this survey to workers as a way for us (as an employer) to learn more about our employees on MTurk, though it is possible that savvy workers might suspect at this point that they were actually participating in an academic study. However, because the survey was administered *after* workers had already completed the actual portions of the job in which misconduct was observed, we have no reason to believe that the nature of these questions influenced our key dependent variables in any way.

Key Outcome Variables

As discussed, a unique feature of our experimental setting was that we were able to cleanly observe various forms of employee misconduct unbeknownst to the workers. In particular, we focus on two opportunities for misconduct in our empirical analysis: *shirking*

Figure 5. (Color online) Bonus Task Flow



Notes. For brevity, we use the term “monitoring,” but in all cases, it is only the *threat* of monitoring that we actually implement. SE values, social/environmental values. Color highlights manipulated text for the benefit of the reader, and does not represent the text color seen by the workers.

in the main task and fraudulent bonus claims. Within fraudulent bonus claims, we are also able to distinguish between *fraudulent voicemail claims* (worth \$0.10 each) and *fraudulent market research survey response claims* (which were worth more at \$0.25 each but which also required more effort to perpetrate because workers had to actually concoct answers to the market research questions in order to claim them).¹¹

The incentives that motivate workers to engage in these forms of misconduct arguably come down to money and time. Workers who make fraudulent bonus claims do so for a fairly obvious reason: direct financial gain. In contrast, the motivation to shirk is slightly more nuanced though still likely related to financial gain at least indirectly. Workers who shirk (i.e., those who report having completed a required portion of the main task without having actually done so) ostensibly engage in this form of misconduct in order to finish the job (and receive their flat-rate payment) more quickly. In other words, workers who shirk do so to conserve *time*, which can then be spent earning money via other forms of work (on MTurk or elsewhere) or in leisure. The trade-off for workers in both cases is the risk that the employer might detect their behavior and subsequently reject their submission, resulting in both a lost payment and reputational damage to their approval rating on MTurk.

Key Explanatory Variables

Our primary independent variables of focus are the randomized employer-level treatments previously discussed (*ethical values*, *social/environmental values*, and *threat of monitoring* as well as relevant interactions). In addition to these, we capture and include as controls in various specifications individual-level characteristics, including worker *gender*, *age*, *education*, *income*, and *volunteer/donation activity* as well as data on workers' (self-reported) relationship to the MTurk platform, specifically, the *importance of the income earned on MTurk*, *their tenure on MTurk*, and their *MTurk rating*.

Results

Our raw sample consisted of 4,000 observations.¹² After eliminating 93 observations in which workers appear to have taken the survey multiple times (based on IP address and latitude/longitude data), our final sample consists of 3,907 observations.¹³ Note that, for conciseness in presentation, we refer to the social/environmental values treatments as “SE values” and to our threat of monitoring treatment as “monitoring” in what follows. Table 1 reports sample characteristics by condition and indicates that randomization across observable characteristics was successful with no statistical differences between the treatment groups and

the control group. Overall, workers in the sample are 54% female with an average age of 35. Just over half the sample has a college degree or more advanced education. Roughly a quarter of workers have been engaged in gig work on MTurk for more than two years, and (though not reported in Table 1), nearly 90% report having been on the platform for at least one month. Most workers (76%) report their MTurk approval rating as 99% or higher. Table A.1 in the appendix provides a correlation matrix for worker characteristics. All correlations are directionally sensible (e.g., workers with lower incomes indicate that the money they make on MTurk is more important to them, workers with college educations have higher incomes, etc.). This bolsters our confidence in the general integrity of the self-reported survey data and provides context for interpreting subsequent results.

Figure 6 summarizes the overall incidence of worker misconduct (both shirking in the main task and fraudulent bonus claims) in our study. Roughly 80% of workers completed the main task in full, and 20% shirked at least one website entry. Among those who shirked, the distribution of outcomes is bimodal: most shirked either only once or in all five instances. Overall rates of fraudulent bonus claims are substantially lower with fewer than 8% of workers engaging in this form of misconduct. However, bonus claims are markedly more common among workers who engage in shirking. Among workers who did *not* shirk any entries in the main task, only 2% claimed any fraudulent bonuses. This figure rises sharply to nearly 7% among workers who shirked once and to more than 52% among workers who shirked all five entry tasks.

Effects of Employer-Level Treatments

Table 2 presents baseline differences in mean levels of misconduct by condition. In all instances, levels of misconduct are, as expected, lower in the treatment groups versus the control group though not all differences between groups are statistically significant. Tables 3 and 4 utilize regression analysis to examine the effects of our employer-level treatments more rigorously, and here, statistical differences become more apparent, especially after worker controls are introduced to increase model precision. Our analysis focuses primarily on misconduct at the extensive margin (i.e., binary indicators for whether a worker engaged in *any* misconduct) though results are similar when outcomes are instead specified as counts of the number of instances of shirking (from zero to five) or, in the case of bonuses, as a dollar value indicating the total amount of fraudulent bonuses claimed (see Models 4 and 8 in Tables 3 and 4).¹⁴ For ease of interpretation, we utilize ordinary least squares (OLS) in the specifications presented, but all results for binary outcomes are substantively robust to logit specifications.

Table 1. Sample Characteristics by Condition (Randomization Balance)

| | Full sample | Control | Only ethical values | Only SE values ^a | Only monitoring ^b | Ethical values + monitoring ^b | SE values ^a + monitoring ^b |
|--|----------------|----------------|---------------------|-----------------------------|------------------------------|--|--|
| <i>N</i> | 3,907 | 635 | 632 | 692 | 667 | 633 | 648 |
| <i>Female</i> | 0.54 (0.50) | 0.53 (0.50) | 0.54 (0.50) | 0.52 (0.50) | 0.56 (0.50) | 0.54 (0.50) | 0.56 (0.50) |
| <i>Age</i> | 35.3 (10.7) | 35.2 (10.5) | 35.4 (10.9) | 35.7 (10.3) | 35.3 (11.1) | 34.7 (10.7) | 35.5 (10.9) |
| <i>Four-year college degree or higher</i> | 0.54 (0.50) | 0.56 (0.50) | 0.52 (0.50) | 0.54 (0.50) | 0.54 (0.50) | 0.53 (0.50) | 0.54 (0.50) |
| <i>Income ≥ \$50K</i> | 0.51 (0.50) | 0.53 (0.50) | 0.49 (0.50) | 0.52 (0.50) | 0.52 (0.50) | 0.49 (0.50) | 0.52 (0.50) |
| <i>Volunteer/donate ≥ sample median</i> | 0.54 (0.50) | 0.53 (0.50) | 0.51 (0.50) | 0.53 (0.50) | 0.55 (0.50) | 0.56 (0.50) | 0.55 (0.50) |
| <i>MTurk income important</i> | 3.57 (1.13) | 3.57 (1.16) | 3.51 (1.16) | 3.60 (1.12) | 3.58 (1.10) | 3.57 (1.11) | 3.58 (1.13) |
| <i>Active on MTurk more than two years</i> | 0.28 (0.45) | 0.27 (0.44) | 0.30 (0.46) | 0.27 (0.44) | 0.25 (0.44) | 0.27 (0.45) | 0.29 (0.45) |
| <i>MTurk approval rate ≥ 99%</i> | 0.76 (0.42) | 0.75 (0.43) | 0.75 (0.43) | 0.76 (0.43) | 0.77 (0.42) | 0.78 (0.42) | 0.78 (0.42) |

Notes. All variables are binary except *Age* (which takes integer values) and *MTurk income important*, which is measured on a five-point Likert Scale (five = strongly agree that money earned on MTurk is an important source of income). Standard deviations reported in parentheses. There are no statistically significant differences ($p < 0.10$) in sample means between the treatment and control groups.

^aSocial/environmental values.

^bFor brevity, we use the term “monitoring,” but in all cases it is only the *threat* of monitoring that we actually implement.

Figure 6. (Color online) Overall Incidence of Worker Misconduct

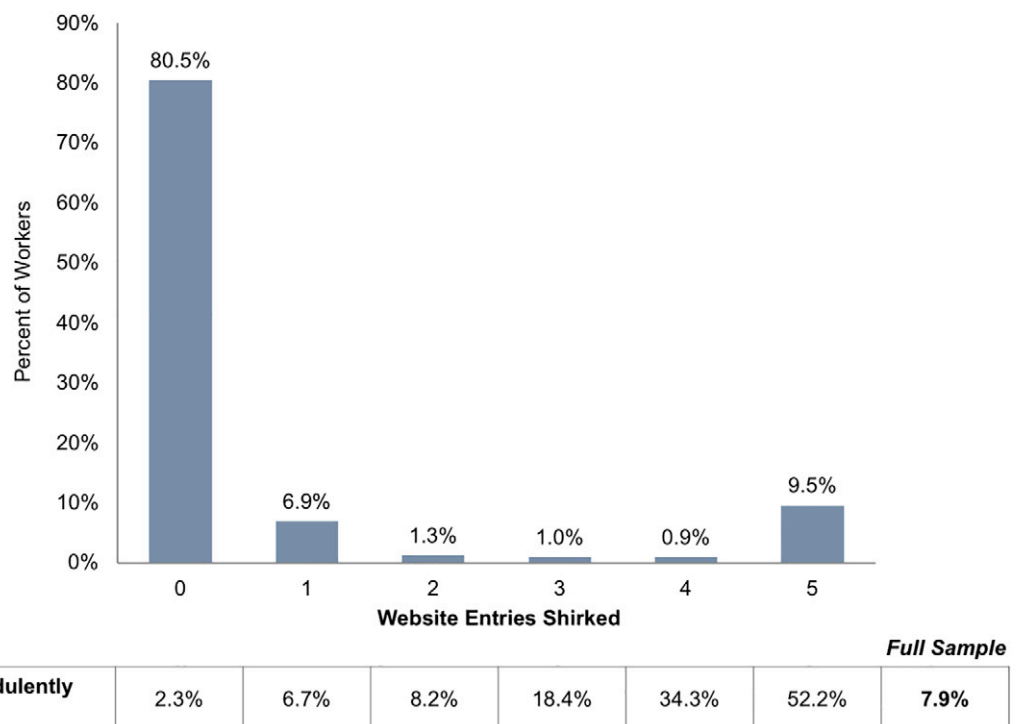


Table 2. Baseline Levels of Misconduct by Condition

| | Full sample | Control | Only ethical values | Only SE values ^a | Only monitoring ^b | Ethical values + monitoring ^b | SE values ^a + monitoring ^b |
|--|------------------|------------------|---------------------|-----------------------------|------------------------------|--|--|
| <i>N</i> | 3,907 | 635 | 632 | 692 | 667 | 633 | 648 |
| <i>Shirked any websites?</i> | 0.195 (0.396) | 0.220 (0.415) | 0.204 (0.403) | 0.188 (0.399) | 0.198 (0.389) | 0.177* (0.382) | 0.185 (0.391) |
| <i>Fraudulently claimed any bonuses?</i> | 0.079 (0.269) | 0.107 (0.309) | 0.076* (0.265) | 0.087 (0.282) | 0.069** (0.254) | 0.073** (0.260) | 0.060*** (0.238) |
| <i>Average number of websites shirked</i> | 0.635 (1.527) | 0.669 (1.545) | 0.668 (1.561) | 0.647 (1.554) | 0.652 (1.542) | 0.553 (1.431) | 0.619 (1.517) |
| <i>Average number of fraudulent bonus claims</i> | 0.248 (0.947) | 0.321 (1.059) | 0.264 (1.003) | 0.257 (0.941) | 0.213** (0.870) | 0.215* (0.869) | 0.218* (0.929) |
| <i>Average total \$ value of fraudulent bonus claims</i> | 0.039 (0.162) | 0.054 (0.195) | 0.039 (0.157) | 0.040 (0.166) | 0.032** (0.142) | 0.033** (0.141) | 0.035* (0.165) |

Notes. The first two variables are binary indicators for whether a worker engaged in any misconduct as specified. Workers could shirk between zero and five websites and fraudulently claim between zero and five bonuses (worth up to \$1.25 in fraudulent claims). Asterisks indicate sample means that are statistically different from the control group.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

^aSocial/environmental values.

^bFor brevity, we use the term “monitoring,” but in all cases it is only the *threat* of monitoring that we actually implement.

Table 3 examines shirking in the main task (i.e., not entering specified information into the websites as instructed). Here, in the full sample (first four columns), all three employer-level treatments appear to have a negative effect on shirking though only the organizational values-oriented treatments (ethical values and SE values) are statistically significant in any specification. The last four columns of Table 3 replicate these results, but here, we truncate our sample based on the time that workers spent viewing the page on which the employer-level treatments were initially presented. In particular, this sample omits workers in the bottom 10% of the distribution for time spent viewing the treatment language page, that is, those who were less likely to have read the treatment language carefully or at all.¹⁵ Given that our treatments were administered solely via information presented in writing, we view this truncated sample of workers as a rough proxy for those workers who actually *received* our treatments. And, indeed, in this truncated sample, we find that the effects of all three individual treatments are larger and more statistically robust. Moreover, in both the full and truncated samples, the reduction in misconduct attributable to the treatments is meaningful in terms of economic magnitude. A coefficient of -0.03 in Model 2, for example, indicates that the proportion of workers who shirk falls by three percentage points (off a base of roughly 20%) when the values-oriented treatments are utilized. This reduction in misconduct is more substantial (five to seven percentage points) in the truncated sample.

Table 4 mirrors Table 3 but focuses instead on misconduct in the bonus task. These results are, on the whole, more statistically robust than those for shirking in the main task. Here, we see negative and statistically significant coefficients on the main effects for all three treatments in both the full and truncated samples (though, again, both effect size and statistical significance are greater in the truncated sample). The magnitude of these treatments is again economically significant. Roughly 8% of workers make fraudulent bonus claims in aggregate. Results in Table 4 indicate that the employer-level treatments we utilize each reduce the proportion of workers who make bonus claims by two to five percentage points (depending on the specification), lowering the base rate substantially.

Taken together, then, Tables 3 and 4 provide support for Hypothesis 1 (relating to communication of organizational values). With regard to Hypothesis 2 (relating to the threat of monitoring), results are slightly more complicated. Specifically, Hypothesis 2 posits that the threat of monitoring would reduce misconduct only in instances in which this threat is credible. In actuality, results in Tables 3 and 4 suggest that the threat of monitoring may reduce misconduct even when this threat is entirely implausible. Indeed, results for the monitoring treatment are overall most robust (highly statistically significant across *all* specifications) in Table 4, in which the monitoring threat was at least partially credible. But coefficient estimates on the monitoring treatment are also statistically significant in at least some specifications even in Table 3, in which our threat of monitoring was not at all

Table 3. Treatment Effects on Shirking

| | Full sample | | | | Workers more likely to have read treatment language ^a | | | |
|---|--|-------------------|-------------------|--|--|--------------------|--------------------|--|
| | Any websites entries shirked? (binary) | | | Number of websites shirked (count 0–5) | Any websites entries shirked? (binary) | | | Number of websites shirked (count 0–5) |
| | (1) | (2) | (3) | | (5) | (6) | (7) | |
| <i>Ethical values</i> | –0.02 (0.02) | –0.03* (0.01) | –0.03 (0.02) | –0.05 (0.07) | –0.03** (0.02) | –0.03** (0.02) | –0.05** (0.03) | –0.16* (0.09) |
| <i>SE values</i> ^b | –0.02 (0.02) | –0.03** (0.01) | –0.05** (0.02) | –0.10 (0.07) | –0.04** (0.02) | –0.04*** (0.02) | –0.07*** (0.02) | –0.21** (0.08) |
| <i>Monitoring</i> ^c | –0.02 (0.01) | –0.02 (0.01) | –0.02 (0.02) | –0.03 (0.07) | –0.03** (0.01) | –0.03** (0.01) | –0.06** (0.02) | –0.16* (0.09) |
| <i>Monitoring</i> ^c × <i>ethical values</i> | | | –0.00 (0.03) | –0.10 (0.10) | | | 0.03 (0.03) | 0.02 (0.11) |
| <i>Monitoring</i> ^c × <i>SE values</i> ^b | | | 0.02 (0.03) | 0.00 (0.09) | | | 0.05* (0.03) | 0.12 (0.11) |
| Controls | No | Yes | Yes | Yes | No | Yes | Yes | Yes |
| Constant | 0.22*** (0.01) | 0.66*** (0.10) | 0.66*** (0.10) | 4.04*** (0.41) | 0.24*** (0.02) | 0.69*** (0.11) | 0.71*** (0.11) | 4.40*** (0.44) |
| Observations | 3,907 | 3,907 | 3,907 | 3,907 | 3,517 | 3,517 | 3,517 | 3,517 |
| R ² | 0.00 | 0.20 | 0.20 | 0.37 | 0.00 | 0.21 | 0.21 | 0.38 |

Notes. All regressions are OLS (though all results for binary outcomes are substantively robust to logit specifications). Models 2–4 and 6–8 include the worker-level control variables from Table 1 (*Female*, *Age*, *≥ four-year college*, *Income ≥ \$50K*, *Volunteer/donate ≥ median*, *MTurk \$ important*, *On MTurk more than two years*, *MTurk approval ≥ 99%*) as well as the following variables: the natural log of the total seconds the worker took to complete the job, the natural log of the total seconds the worker spent viewing the treatment language page, an indicator for whether the worker passed an attention check question, indicators for political affiliation and military service, and indicators for inconsistencies in the data (ZIP code and geo-coordinate mismatches and gender mismatches on primary and follow-up surveys). Robust standard errors in parentheses.

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

^aThis sample omits workers in the bottom 10% of the distribution for time spent viewing the page containing condition-specific language.

^bSocial/environmental values.

^cFor brevity, we use the term “monitoring,” but in all cases it is only the *threat* of monitoring that we actually implement.

credible. There are many potential explanations for why this might be. For one, it is possible that a large portion of workers do not fully think through whether the threat of monitoring is credible; rather, they simply take the threat at face value and modify their behavior accordingly. It could also be the case that the monitoring messaging has a subconscious effect (e.g., increasing the salience of rules, of being caught and punished, or reducing workers’ feeling of autonomy) that results in behavioral changes even if workers conclude, rationally, that there is no way for the employer to act on this threat. It is shown, for example, that feeling unconstrained by rules (Gino and Wiltermuth 2014) and job autonomy is positively associated with individuals’ tendency to behave unethically (Lu et al. 2017). Future research could further explore this somewhat counterintuitive finding in further depth.

Finally, results here provide some evidence in support of Hypothesis 3 (an interaction effect), particularly in the bonus task. In Table 4, in particular, positive and significant estimates on the interaction terms in Models 3, 4, 7, and 8 indicate that reductions in misconduct from organizational values-oriented treatments and monitoring are not additive. For example,

when monitoring is in effect, estimates in Models 7 and 8 suggest that there is little incremental benefit to implementing values-oriented policies as coefficient estimates on main effects and interaction effects sum to roughly zero.

This interaction effect suggests that policies intended to better align workers’ interests with those of the firm by appealing to shared values may not be as successful when monitoring is also utilized. Figure 7 provides some suggestive evidence on the potential underlying mechanism for this interaction. On the left side of this figure, monitoring is not in effect. Here, we note that the ethical and SE values treatments increase agreement with the statement, “I share this employer’s values” (versus the control group) by a statistically significant margin. On the right side of the figure, when monitoring is in effect, the addition of values-oriented treatments does not change workers’ sense of shared values by a statistically significant amount. We posited previously that this might be because monitoring reduces trust between workers and the firm, causing it to be difficult for workers to form a sense of shared values. And, indeed, the bottom of Figure 7 indicates that workers in our

Table 4. Treatment Effects on Fraudulent Bonus Claims

| | Full sample | | | | Workers more likely to have read treatment language ^a | | | |
|---|--|--------------------|--------------------|--------------------------------------|--|--------------------|--------------------|--------------------------------------|
| | Any fraudulent bonuses claimed? (binary) | | | Value of fraudulent claims (dollars) | Any fraudulent bonuses claimed? (binary) | | | Value of fraudulent claims (dollars) |
| | (1) | (2) | (3) | | (5) | (6) | (7) | |
| <i>Ethical values</i> | −0.01 (0.01) | −0.02 (0.01) | −0.03** (0.01) | −0.02* (0.01) | −0.02** (0.01) | −0.02* (0.01) | −0.05*** (0.02) | −0.03*** (0.01) |
| <i>SE values</i> ^b | −0.01 (0.01) | −0.02* (0.01) | −0.03* (0.01) | −0.02** (0.01) | −0.02** (0.01) | −0.02** (0.01) | −0.04** (0.02) | −0.03*** (0.01) |
| <i>Monitoring</i> ^c | −0.02*** (0.01) | −0.02*** (0.01) | −0.04*** (0.01) | −0.02*** (0.01) | −0.03*** (0.01) | −0.02*** (0.01) | −0.05*** (0.02) | −0.04*** (0.01) |
| <i>Monitoring</i> ^c × <i>ethical values</i> | | | 0.04* (0.02) | 0.02 (0.01) | | | 0.05** (0.02) | 0.03** (0.01) |
| <i>Monitoring</i> ^c × <i>SE values</i> ^b | | | 0.02 (0.02) | 0.02* (0.01) | | | 0.03 (0.02) | 0.04** (0.01) |
| Controls | No | Yes | Yes | Yes | No | Yes | Yes | Yes |
| Constant | 0.10*** (0.01) | 0.23*** (0.08) | 0.24*** (0.08) | 0.06 (0.05) | 0.11*** (0.01) | 0.24*** (0.08) | 0.26*** (0.09) | 0.08 (0.05) |
| Observations | 3,907 | 3,907 | 3,907 | 3,907 | 3,517 | 3,517 | 3,517 | 3,517 |
| R ² | 0.00 | 0.23 | 0.23 | 0.22 | 0.00 | 0.24 | 0.25 | 0.22 |

Notes. All regressions are OLS (though all results for binary outcomes are substantively robust to logit specifications). Models 2–4 and 6–8 include the worker-level control variables from Table 1 (*Female*, *Age*, *≥ four-year college*, *Income ≥ \$50K*, *Volunteer/donate ≥ median*, *MTurk \$ important*, *On MTurk more than two years*, *MTurk approval ≥ 99%*) as well as the following variables: the natural log of the total seconds the worker took to complete the job, the natural log of the total seconds the worker spent viewing the treatment language page, an indicator for whether the worker passed an attention check question, indicators for political affiliation and military service, and indicators for inconsistencies in the data (ZIP code and geo-coordinate mismatches and gender mismatches on primary and follow-up surveys). Robust standard errors in parentheses.

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

^aThis sample omits workers in the bottom 10% of the distribution for time spent viewing the page containing condition-specific language.

^bSocial/environmental values.

^cFor brevity, we use the term “monitoring,” but in all cases it is only the *threat* of monitoring that we actually implement. Here, in the bonus task, this threat was (at least partially) credible from the perspective of the worker.

experiment who were subject to monitoring were substantially less likely to agree with the statement “This employer trusts its workers” than those who were not (72.3% versus 78.1%, $p < 0.001$).¹⁶ This association is consistent with the theoretical underpinnings of Hypothesis 3 though we forthrightly caveat that our analysis is only exploratory in nature. We cannot conclusively rule out alternate explanations that could also contribute to the interaction, such as the possibility that any two treatments might be substitutes for one another. Future work could explore these mechanisms in more detail.

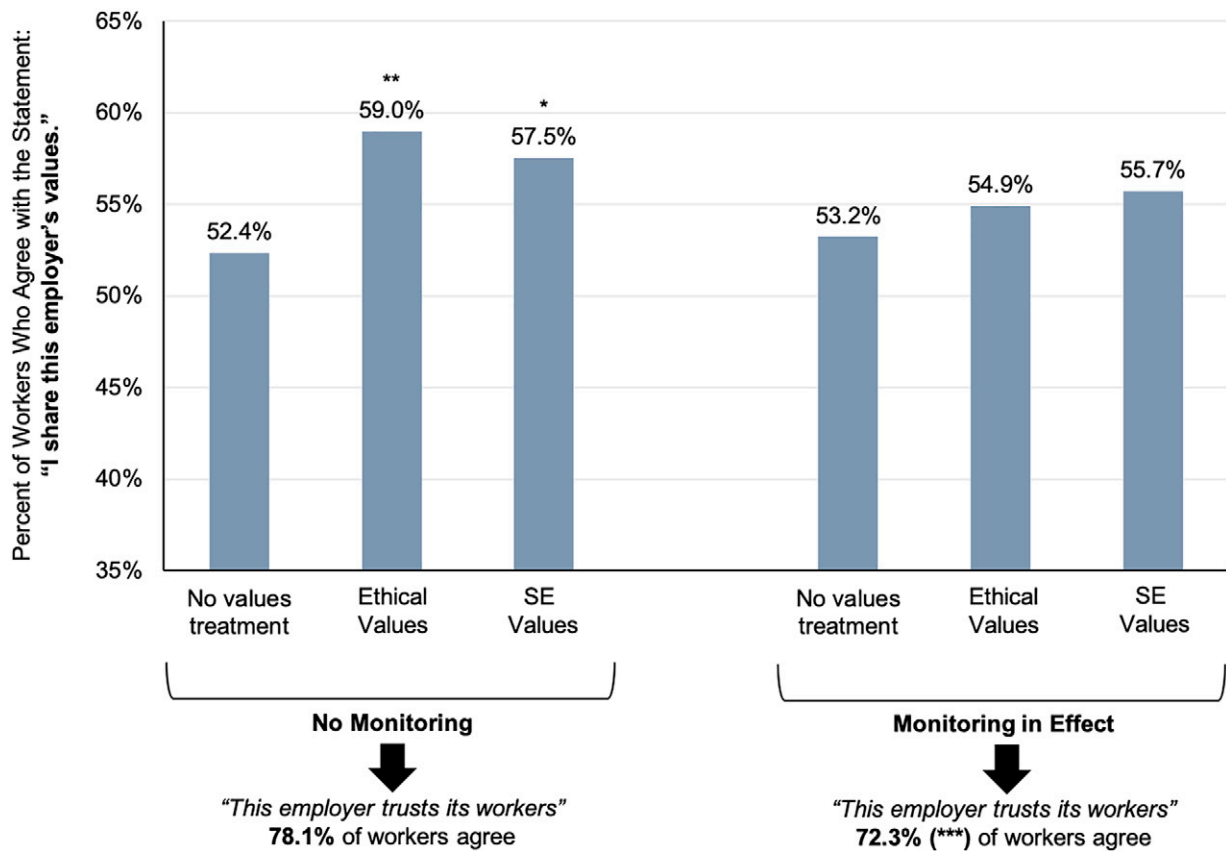
Individual Worker Characteristics and Misconduct

In addition to our employer-level treatments, Tables 3 and 4 both include a set of worker-level control variables in some models as indicated. Although understanding the correlation between various worker characteristics and misconduct is not our primary focus, we do note that several characteristics are strongly correlated with misconduct and that observable individual characteristics explain a substantial

portion of the variation in worker misconduct: between 20% and 37% depending on the specific outcome variable in question. We present key results in Table 5 with the caveat that these estimates should not be interpreted as causal (because individual characteristics are, of course, not randomly assigned).

For the most part, correlational results here are unsurprising. High-performing workers (i.e., those with approval ratings of 99% or more) are substantially less likely to engage in misconduct than low-performing workers (those with approval ratings of less than 99%). Indeed, of the variables that we collected, workers’ MTurk rating is, perhaps unsurprisingly, the strongest individual-level predictor of misconduct. Women are also less likely than men to engage in misconduct, consistent with previous findings that show women to be less likely to cheat in the classroom, for example (Whitley et al. 1999). Interestingly, more highly educated individuals (those with at least a four-year college degree) are most likely to engage in misconduct as are those for whom the money earned on MTurk is more important.

Figure 7. (Color online) Effect of Treatments on Shared Values and Trust



Notes. For both shared values and trust, workers were asked about the extent to which they agreed with the corresponding statements on a five-point Likert scale (one = strongly disagree, five = strongly agree). In the data, workers who responded with a four or five on this scale are included as "agreeing." Asterisks indicate sample means that are statistically different from the relevant control group. For brevity, we use the term "monitoring," but in all cases, it is only the *threat* of monitoring that we actually implement. SE values, social/environmental values. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Other statistically significant individual-level predictors may also be worth exploring more in future work, but we opt here to focus primarily on one particularly notable result: workers who report that they volunteer and donate more frequently¹⁷ are *more* likely to engage in misconduct (three percentage points more likely to shirk and four percentage points more likely to fraudulently claim bonuses). This result initially seems counterintuitive, but it is actually in accordance with other findings—notably List and Momeni (2021), who suggest that moral licensing may result in increased levels of worker misconduct. In other words, workers who feel that they are "doing good" in some ways may feel licensed to behave badly on the job. Specifically, List and Momeni (2021) posit that this mechanism may explain their finding that worker misconduct increases when firms engage in certain types of CSR.

Our finding that workers who volunteer and donate are more likely to engage in misconduct is in line with such a moral licensing mechanism. We, thus, explore this result more rigorously by conducting a follow-up

survey with the same set of workers to determine its robustness. In particular, because responses to our initial survey were self-reported after workers had completed the main part of the task, it is reasonable to be concerned that perhaps causation might run in reverse, that is, that engaging in misconduct *caused* workers to report higher levels of volunteer/donation activity perhaps to compensate and make themselves feel better about having cheated.

We conducted our follow-up study in September 2019 (four months after our initial study) under the name of a different requester on MTurk so that workers would have no way of connecting this follow-up survey with our initial job.¹⁸ We invited all 3,907 workers from our initial study to complete the follow-up survey, for which we offered a payment of \$0.50.¹⁹ We ultimately received 1,956 responses—just slightly more than a 50% response rate. Our follow-up survey asked workers to answer the same demographic questions that were included in our initial study as well as some exploratory questions pertaining to their views

Table 5. Individual Worker Characteristics and Misconduct

| | Shirking | | Fraudulent bonus claims | |
|-------------------------------------|--------------------|--------------------|-------------------------|--------------------|
| | Binary (1) | Count (0–5) (2) | Binary (3) | Value (\$) (4) |
| <i>Female</i> | −0.04*** (0.01) | −0.18*** (0.04) | −0.01* (0.01) | −0.01** (0.00) |
| <i>Age^a</i> | −0.00 (0.00) | 0.00 (0.00) | −0.01*** (0.00) | −0.01*** (0.00) |
| <i>≥ four years college</i> | 0.04*** (0.01) | 0.20*** (0.04) | 0.04*** (0.01) | 0.02*** (0.00) |
| <i>Income ≥ \$50K</i> | −0.00 (0.01) | −0.03 (0.04) | −0.02** (0.01) | −0.01 (0.01) |
| <i>Volunteer/Donate ≥ median</i> | 0.03*** (0.01) | 0.16*** (0.04) | 0.04*** (0.01) | 0.02*** (0.00) |
| <i>MTurk \$ important</i> | 0.02*** (0.01) | 0.08*** (0.02) | 0.02*** (0.00) | 0.01*** (0.00) |
| <i>On MTurk more than two years</i> | 0.03** (0.01) | −0.03 (0.04) | −0.00 (0.01) | −0.00 (0.00) |
| <i>MTurk approval ≥ 99%</i> | −0.12*** (0.02) | −0.58*** (0.06) | −0.08*** (0.01) | −0.04*** (0.01) |
| <i>Constant</i> | 0.63*** (0.10) | 3.94*** (0.41) | 0.21*** (0.08) | 0.04 (0.05) |
| <i>Observations</i> | 3,907 | 3,907 | 3,907 | 3,907 |
| <i>R²</i> | 0.20 | 0.37 | 0.23 | 0.22 |

Notes. All regressions are OLS (though all results for binary outcomes are substantively robust to logit specifications). All models also include the following worker-level control variables: the natural log of the total seconds the worker took to complete the job, the natural log of the total seconds the worker spent viewing the treatment language page, an indicator for whether the worker passed an attention check question, indicators for political affiliation and military service, and indicators for inconsistencies in the data (ZIP code and geo-coordinate mismatches and gender mismatches on primary and follow-up surveys). Robust standard errors in parentheses.

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

^aThe age variable is scaled down by a factor of 10 to make coefficients more interpretable.

on work, businesses/corporations, and lying. Our primary variables of interest, however, were those related to volunteer and donation history because workers' responses to these questions in the follow-up survey should not have been influenced in any direct way by their assigned condition or (mis)behavior in the initial job (i.e., previously discussed concerns of reverse causation are alleviated).²⁰

Overall, responses were quite consistent across surveys. The correlation between workers' composite volunteer/donate scores in the initial study (calculated from their originally reported answers) and their composite scores in the follow-up survey was 0.64: 84% of workers' scores changed by no more than one point between surveys, and 96% of workers' scores changed by no more than two points. Table A.2 compares coefficient estimates for regressions that utilize the original volunteer/donate variable to those that utilize an equivalent volunteer/donate variable calculated from follow-up survey data. Estimates are consistent across the original and follow-up data, bolstering confidence in our initially observed relationship between misconduct and volunteer/donation history. Although not conclusive, these results strongly suggest that *individual-level* good behavior may, indeed, cause employees to feel licensed to behave badly. In contrast, our core

finding that the communication of organizational values reduces misconduct suggests that *employer-level* "good behavior" does not elicit this same sort of moral licensing among workers. We discuss this nuance more in our concluding section.

Treatment Effect Heterogeneity Across Worker Types

On the whole, the Table 5 results discussed in the preceding section suggest that some types of workers are much more (or less) prone to misconduct than others. In this section, we explore the extent to which our various treatments are effective on workers with a high propensity for misconduct versus those with a low propensity for misconduct. We approach this analysis for both shirking and fraudulent bonuses as follows: (1) we fit logit models with (the relevant form of) misconduct as the dependent variable and all of the worker-level characteristics listed in Table 5 as predictors; (2) we use estimates from these models to generate predicted "propensity for misconduct" scores for all 3,907 workers; and (3) we use these predicted misconduct scores to split our sample of workers into two groups: workers with a high propensity for misconduct (those with propensity scores for misconduct in the top one third of the distribution) and workers

with medium/low propensity for misconduct (all remaining workers).²¹

It is not obvious what to expect *ex ante* with regard to where our treatments have a stronger effect. On the one hand, “bad apple” workers with a high propensity for misconduct might be less receptive to values-oriented messaging and more skeptical of the threat of monitoring (or, alternatively, less likely to read the task instructions in full, thus missing condition-specific language altogether). On the other hand, when the base rate of misconduct among workers is lower, there is simply less room for employer-level policies to further improve worker behavior (and vice versa). Table 6 presents results broken out across worker subsamples. Here, it is clear that, for both shirking and fraudulent bonus claims, our employer-level treatments have a larger and more statistically significant effect on workers with a *high* propensity for misconduct than on workers with a medium/low propensity for misconduct. Indeed, these results suggest that there may be very little benefit to implementing employer-level policies such as the ones we explore here when rates of worker misconduct are already low.

Breakdown of Treatment Effects on Fraudulent Bonus Claims

Because the bonus task was optional, it is useful to understand how our various treatments may have

affected two key outcomes separately: (1) selection into the bonus task itself and (2) misconduct rates conditional on selection into the bonus task.²² Table 7 presents these more granular results for both the full sample and the subsample of workers more likely to have read the treatment language (also utilized in parts of Tables 3 and 4). Results in both samples indicate that the threat of monitoring has a large and highly statistically significant effect on initial selection into the bonus condition (whereas the values-oriented treatments have none). Perhaps the most likely explanation for this result is that many workers who select into the bonus condition do so because they believe they can get away with cheating. The threat of monitoring (at least partially credible in this context) changes this dynamic substantially, causing fewer workers who *would* have cheated to select into the bonus task in the first place. Other potential explanations may also be relevant (e.g., monitoring may demotivate workers, causing them to be less interested in completing “bonus” work), and future research could shed additional light on the specific mechanisms underlying selection patterns. Conditional on selection into the bonus task, Models 2, 3, 5, and 6 suggest that the effects of our employer-level treatments are most concentrated in reductions in fraudulent survey bonus claims (the higher valued type of claim). In

Table 6. Treatment Effects on Rates of Shirking and Fraudulent Bonus Claims: Comparison of Workers with High vs. Medium/Low Propensity to Engage in Misconduct

| | Any websites entries shirked? | | | Any fraudulent bonuses claimed? | | |
|--|-------------------------------|--|---|---------------------------------|--|---|
| | Full sample (1) | Workers with high propensity for misconduct (2) | Workers with medium/low propensity for misconduct (3) | Full sample (4) | Workers with high propensity for misconduct (5) | Workers with medium/low propensity for misconduct (6) |
| <i>Ethical values</i> | −0.03 (0.02) | −0.07* (0.04) | 0.01 (0.02) | −0.03** (0.01) | −0.08** (0.04) | −0.00 (0.01) |
| <i>SE values</i> ^a | −0.05** (0.02) | −0.09** (0.04) | −0.02 (0.02) | −0.03* (0.01) | −0.08** (0.04) | 0.00 (0.01) |
| <i>Monitoring</i> ^b | −0.02 (0.02) | −0.02 (0.04) | −0.02 (0.02) | −0.04*** (0.01) | −0.10*** (0.04) | −0.01 (0.01) |
| <i>Monitoring</i> ^b × <i>ethical values</i> | −0.00 (0.03) | −0.00 (0.06) | −0.01 (0.03) | 0.04* (0.02) | 0.08* (0.05) | 0.01 (0.01) |
| <i>Monitoring</i> ^b × <i>SE</i> <i>values</i> ^a | 0.02 (0.03) | 0.06 (0.06) | 0.01 (0.03) | 0.02 (0.02) | 0.08* (0.05) | −0.02 (0.01) |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Constant | 0.66*** (0.10) | 1.27*** (0.16) | −0.25* (0.15) | 0.24*** (0.08) | 0.22 (0.14) | −0.03 (0.07) |
| Observations | 3,907 | 1,301 | 2,606 | 3,907 | 1,301 | 2,606 |
| R ² | 0.20 | 0.30 | 0.02 | 0.23 | 0.21 | 0.02 |

Notes. All regressions are OLS (though results are substantively robust to logit specifications). All models include the same control variables listed in Tables 3–5. Robust standard errors in parentheses.

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

^aSocial/environmental values.

^bFor brevity, we use the term “monitoring,” but in all cases it is only the *threat* of monitoring that we actually implement.

Table 7. Breakdown of Treatment Effects on Fraudulent Bonus Claims

| | Full sample | | | | Workers more likely to have read treatment language ^a | | |
|--|---|-------------------------------|---|-----------------------------------|---|----------------------------|-----------------------------------|
| | Any fraudulent bonus claims? (conditional on selecting into any bonus opportunities) | | Worker selected into any bonus opportunities? | Survey answers (\$0.25) (3) | Any fraudulent bonus claims? (conditional on selecting into any bonus opportunities) | | Survey answers (\$0.25) (6) |
| | Worker selected into any bonus opportunities? (1) | Voicemails (\$0.10) (2) | | | Worker selected into any bonus opportunities? (4) | Voicemails (\$0.10) (5) | |
| <i>Ethical values</i> | −0.00 (0.02) | −0.08 (0.07) | | −0.11** (0.05) | −0.01 (0.02) | −0.08 (0.08) | −0.14** (0.06) |
| <i>SE values</i> ^b | −0.01 (0.02) | −0.09 (0.07) | | −0.09* (0.05) | −0.01 (0.02) | −0.07 (0.08) | −0.15** (0.06) |
| <i>Monitoring</i> ^c | −0.05*** (0.02) | −0.06 (0.07) | | −0.10 (0.06) | −0.05** (0.02) | −0.03 (0.08) | −0.15** (0.07) |
| <i>Monitoring</i> ^c × <i>ethical values</i> | 0.03 (0.02) | 0.05 (0.10) | | 0.14* (0.08) | 0.04 (0.03) | 0.04 (0.11) | 0.18** (0.09) |
| <i>Monitoring</i> ^c × <i>SE</i> <i>values</i> ^b | 0.03 (0.02) | −0.01 (0.10) | | 0.10 (0.08) | 0.03 (0.03) | −0.03 (0.11) | 0.15* (0.09) |
| Controls | Yes | Yes | | Yes | Yes | Yes | Yes |
| Constant | −0.14 (0.10) | 1.17*** (0.26) | | 0.10 (0.19) | −0.12 (0.10) | 1.05*** (0.28) | 0.26 (0.21) |
| Observations | 3,907 | 576 | | 576 | 3,517 | 521 | 521 |
| R ² | 0.21 | 0.09 | | 0.30 | 0.23 | 0.09 | 0.30 |

Notes. All regressions are OLS (though results are substantively robust to logit specifications). All models include the same control variables listed in Tables 3–5. Robust standard errors in parentheses.

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

^aThis sample omits workers in the bottom 10% of the distribution for time spent viewing the page containing condition-specific language.

^bSocial/environmental values.

^cFor brevity, we use the term “monitoring,” but in all cases it is only the *threat* of monitoring that we actually implement.

this outcome, we again find empirical support for Hypotheses 1–3.

Concluding Discussion

Employee misconduct is often challenging to observe empirically. Here, we present a novel real effort experiment revealing just how common misconduct can be in physically distant work settings in which effort and outcomes are not (from the workers' perspective) easily observable to the employer. In aggregate, more than 20% of workers in our study engaged in some form of misconduct on the job. The majority of this misconduct occurred in the form of shirking in the main task, in which 6.9% of workers shirked data entry for one assigned website (out of five), and another 9.5% shirked *all five* data entry assignments. Many workers also fraudulently claimed bonuses for optional tasks that they had not actually completed. The cost of these fraudulent bonus claims was nontrivial, totaling 4% of our total wage bill paid to workers.

In traditional work contexts, there are many strategies that employers might utilize to curb such misconduct: the cultivation of a strong ethical company culture, for example (Pierce and Snyder 2008). In the gig economy, however, many of these avenues tend to be unavailable because of the remote nature of the work. We combine an agency perspective with a relationship-based behavioral perspective to develop theory around an alternative means by which gig and remote employers (and potentially traditional employers as well though we recommend a healthy level of caution in the extrapolation of our results to traditional work contexts) can mitigate worker misbehavior: by communicating organizational values to the worker. In particular, we study communication of organizational values as a potential governance tool for guarding against shirking and misreporting. Theoretically, by increasing the degree of interest alignment between the worker and employer, communication of organizational values should decrease the incidence of worker misconduct. And, indeed, in our empirical results, the inclusion of messaging that emphasized either ethical values or social/environmental values reduced the proportion of workers who engaged in misconduct by a statistically and economically significant margin. This finding suggests that, beyond pecuniary incentives, which have been the focus of most extant research (e.g., Oyer 1998, Obloj and Sengul 2012; Frank and Obloj 2014, Larkin 2014, Larkin and Pierce 2015, Gubler et al. 2016) relationship-based motivators can serve as effective ways to improve interest alignment between principal and agent and, thus, mitigate adverse agent (employee) behavior (Flammer and Luo 2017).

We also study the effect of a monitoring treatment. Though extant work has repeatedly shown direct monitoring to be effective in reducing misconduct (Becker 1968, Hubbard 2000, Nagin et al. 2002, Detert et al. 2007, Olken 2007, DeHoratius and Raman 2008, Pierce et al. 2015), our specific implementation differs slightly in that we focus instead on the mere threat of monitoring. Theoretically, we argue that this threat should only induce a behavioral reduction in misconduct when it is sufficiently credible to the worker. Our findings, however, suggest that the threat of monitoring can reduce misconduct even when the threat lacks credibility (though it is important to note that findings are more robust when the threat is credible versus when it is not). Future work could explore whether even a noncredible threat of monitoring might have a subconscious effect on workers that increases the salience of rules or of being caught and punished and/or reduces workers' feeling of autonomy for example. Indeed, it is shown that feeling unconstrained by rules (Gino and Wiltermuth 2014) and a sense of autonomy are positively associated with individuals' tendency to behave unethically (Lu et al. 2017).

Although the threat of monitoring alone reduces misconduct (ostensibly by decreasing the perception of information asymmetry between principal and agent), we also find that it diminishes the effectiveness of values-oriented messaging as a complementary tool. We provide suggestive evidence consistent with our theoretical argument that the threat of monitoring is likely to reduce trust between the employer and employee. We argue that this reduction in trust makes it more difficult for employees to develop the sense of shared values (and corresponding improvements in interest alignment) that would otherwise occur as a result of these values-oriented policies. This finding is consistent with extant work that has shown that, even in cases where monitoring is possible, it may have negative spillover effects (Aiello 1993, Holland et al. 2015). More generally, it also points to the relevance of considering social and emotional influences (in addition to economic ones) when predicting the behavior of agents (Dunning et al. 2012). Indeed, our paper contributes to a stream of work seeking to combine agency theory with complementary perspectives to generate a more complex, realistic view of individual and organizational behavior (Anderson 1985, Eccles 1985, Eisenhardt 1988, Flammer and Luo 2017).

In addition, although not the primary focus of this paper, it is worth noting that our results indicate substantial variation in workers' baseline propensity to engage in misconduct depending on individual-level characteristics. For researchers relying on MTurk samples, these differences shed light on the characteristics

of participants who are most likely to cheat: those with lower MTurk approval ratings, who are more highly educated, who rely more heavily on MTurk as a source of income, and who are male (as opposed to female). Additionally, one finding in particular stands out: workers who report that they volunteer and donate more frequently actually engage in misconduct at *higher rates*.²³ Taken together with our finding that communication of organizational values *decreases* worker misconduct, this result has potentially intriguing implications for the theoretical construct of moral licensing, particularly in the context of List and Momeni (2021), who find a (weakly) positive relationship between CSR and rates of shirking among workers.

The theoretical argument put forth by List and Momeni (2021) is that CSR should elicit moral licensing and, thus, increase worker misconduct. Notably, though, the authors' main results are statistically significant only when the prosocial act is framed in direct relation to the *individual employee*.²⁴ Consistent with this notion, our results around (self-reported) worker volunteer/donation habits and misconduct (bolstered by evidence from a follow-up survey) suggest that moral licensing may indeed be relevant when employees who behave well on an individual basis in one context (volunteering/donating) feel licensed to behave badly in another (misconduct in the workplace). A key distinction here is that the act that elicits moral licensing occurs at the same level (empirically, at the same unit of observation) as the adverse behavior of focus. Indeed, this is also in line with extant work that suggests good behavior at the *organization* level may result in adverse behavior at the *organization* level with Luo et al. (2018) showing evidence that firms that donate more have more subsequent oil spills, for example.

Our findings suggest that *individual-level* good behavior does seem to appear to make workers feel licensed to behave badly as *individuals*. In contrast, if it is the organization that is framed as “doing good” (as is the case in our values-oriented treatments), individual-level adverse behavior does not appear to ensue. Put simply, our results suggest that, as long as social responsibility is framed at the firm-level (as opposed to the individual level), it should decrease (rather than increase) misconduct. This distinction is nuanced but also appears to be quite important. Indeed, our results here, taken in concert with those of List and Momeni (2021), suggest that there may, more generally, be substantial variation with respect to the way in which different types of CSR affect worker misconduct (and likely worker behavior more broadly). The specific way in which CSR is implemented and communicated in practice can, seemingly, have a

large effect on the magnitude and even the direction of its impact. A more thorough understanding of this nuance is critical for scholars, who are increasingly recognizing that the broader category of “CSR” needs to be deconstructed into its various parts to understand the mechanisms through which various socially responsible policies and activities can benefit the firm (Burbano et al. 2018).

In conclusion, the theory that we develop around communication of values and misconduct is highly relevant for employers and platforms in the growing gig economy as well as in increasingly prevalent remote work contexts. Although we forthrightly acknowledge that MTurk has important limitations as a setting for studying issues pertaining to traditional work contexts, we posit that this setting is actually ideal for studying misconduct in the gig economy as MTurk jobs fit all of the criteria for prototypical gig work: jobs are completed while physically distant from the employer and are furthermore task based (Kaine and Josserand 2019), short term, and facilitated by an intermediary platform that connects the requesters with the workers (Meijerink and Keegan 2019). Furthermore, as MTurk workers completing short-term jobs are arguably less likely to develop a sense of obligation or connection to their employer (compared with workers in relatively longer term gig or remote work contexts), we would expect this to bias our results downward. The effects of communicating organizational values and the interaction effect of such communication with the threat of monitoring are, thus, likely to be even greater in gig or remote contexts that are longer-term. Future work might examine how task/engagement duration and perceived distance from the employer (Burbano 2021a) shape the efficacy of various policies aimed at reducing misconduct.

Although our empirical results are drawn from MTurk specifically, the mechanisms underlying the policies that we study should theoretically extend to other types of gig work (e.g., point-to-point transport and food delivery services) and to remote work. These mechanisms should also likely apply in many traditional work contexts, though here it is not clear how important these mechanisms might be in relation to other factors, such as organizational culture. We thus recommend a healthy level of caution in the extrapolation of our results to traditional work settings. Future work that examines employee misconduct in other contexts will serve as useful complements to this research.

Finally, given the challenge of observing misconduct in practice (as well as that of establishing a causal relationship between organization-level characteristics and individual-level misconduct), our innovative

experimental design is also an important contribution, allowing us to accurately observe and measure employee misconduct in a natural work context and to examine the causal effects of various organization-level policies on this outcome. There is substantial value in a study such as ours that focuses on the estimation of treatment effects and is intentionally designed to eliminate the possibility that omitted variables or differences in worker selection patterns confound results. That said, it is also important to consider the fact that individual-level characteristics appear to explain a substantial portion of the observed variation in misconduct across workers in our data—indeed, substantially more of the variation than our treatment effects explain. This implies that attracting and selecting the right workers is also an important component of any broader organizational strategy for mitigating

misconduct. Future work might, thus, examine the way in which various organization-level policies affect the *selection* of different types of workers into different opportunities and, subsequently, how these differences in selection patterns influence corresponding misconduct outcomes.

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Appendix. Additional Tables

Table A.1. Correlation Matrix for Individual-Level Worker Characteristics

| | Female | Age | Equal to or more than four years college | Income ≥ \$50K | Volunteer/donate ≥ median | MTurk \$ important | On MTurk > two years |
|---|----------|---------|--|----------------|---------------------------|--------------------|----------------------|
| <i>Age</i> | 0.09*** | | | | | | |
| <i>Equal to or more than four years college</i> | −0.09*** | −0.02 | | | | | |
| <i>Income ≥ \$50K</i> | −0.01 | 0.05*** | 0.28*** | | | | |
| <i>Volunteer/donate ≥ median</i> | 0.11*** | 0.11*** | 0.14*** | 0.14*** | | | |
| <i>MTurk \$ important</i> | 0.09*** | −0.02 | −0.17*** | −0.25*** | 0.01 | | |
| <i>On MTurk > two years</i> | 0.00 | 0.17*** | −0.00 | 0.01 | −0.06*** | −0.01 | |
| <i>MTurk approval ≥ 99%</i> | −0.04*** | 0.08*** | 0.01 | 0.08*** | −0.07*** | −0.02 | 0.14*** |

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table A.2. OLS Coefficient Estimates on the Binary Volunteer/Donate Variable *Original vs. Follow-Up Survey Data*

| | Any website entries shirked? | | Any bonuses claimed? | |
|----------------------------------|------------------------------|--------------------|----------------------|---------------------|
| | Original data (1) | Follow-up data (2) | Original data (3) | Follow-up data (4) |
| <i>Volunteer/donate ≥ median</i> | 0.028* (0.016) | 0.028* (0.016) | 0.026*** (0.010) | 0.032*** (0.008) |
| Observations | 1,956 | 1,956 | 1,956 | 1,956 |
| R^2 | 0.15 | 0.15 | 0.20 | 0.21 |

Notes. All models also contain all of the variables included in Table 5. Models 1 and 3 here mirror the original specifications 1 and 3 in Table 5 exactly except for the fact that results here are estimated using only the 1,956 workers who also completed the follow-up survey rather than the full sample of 3,907 workers. Models 2 and 4 replace the original volunteer/donate data with corresponding data collected in the follow-up survey. There is very little difference in results obtained using the original data versus those obtained using the follow-up data (if anything, the initially observed positive correlation between volunteer/donation activity and misconduct is actually stronger when follow-up data are utilized). Robust standard errors in parentheses.

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Endnotes

¹ The system in which intermediary platform firms connect requesters (e.g., employers or consumers) with on-demand gig workers.

² See McKinsey Global Institute (2016) Independent work: Choice, necessity, and the gig economy. Retrieved from <https://www.mckinsey.com/featured-insights/employment-and-growth/independent-work-choice-necessity-and-the-gig-economy>.

³ See J. Wu, A deep dive into remote work for our future of work. *Forbes*. March 9, 2020, and Harper, Flexible working will be a new normal after virus, *BBC News*. May 22, 2020.

⁴ In this framing, an employing organization, the principal, engages agents (workers) to act on its behalf. The agents' precise actions, however, are not observable to the principal in full, at least not without effort or cost on the principal's part.

⁵ Although Flammer and Luo (2017) make a compelling case that firms appear to be attempting to use CSR as a governance mechanism to reduce employee misconduct, their data does not allow them to test whether CSR is actually effective in serving this purpose.

⁶ Institutional review board approval was obtained. The experiment was also preregistered on Open Science Framework under the project "The Effect of CSR, Ethics Codes, and Monitoring on Employee Misconduct," available at osf.io/2q5cw.

⁷ The actual median time that workers took to complete the job was roughly 10 minutes.

⁸ Given that all workers were presented with the same employing organization and website, any effects of the website or name of the organization on workers would be constant across conditions.

⁹ Although it would have been ideal to present the exact same amount of words/information in each of the treatment groups, it is, in practice, challenging to construct additional generic control group wording/information that does not bias our results in other ways. Though workers in the treatment groups were presented with more words/information to read than workers in the control group, we expect that any difference in time spent reading is unlikely to have biased our results upward. First, workers in all of the treatment groups engaged in less misconduct than workers in the control group. If workers in the treatment groups were shirking more at the margin to gain time back (after being asked to read more), for example, this would bias our estimates of treatment effects toward zero, suggesting that our results are actually conservative rather than potentially overstated. Second, the amount of time required to read the incremental treatment language is fairly small (a few seconds) versus the amount of time it takes to actually complete any of the five individual required tasks (a couple of minutes on average).

¹⁰ We do not find robust evidence of mediation and, thus, do not present any mediation results in this paper. We do, however, find suggestive evidence in support of our proposed theoretical arguments, which we present as exploratory analyses at the end of our Results section.

¹¹ To claim this bonus, workers had to actually fabricate responses to market research questions, such as "Do you currently use any software products to help you manage project workflow and/or finances, and if so, what products?" (Worker responses to the aforementioned question included answers, such as "workflow software," "Microsoft," and "Acumatica." The last of these is an actual small-business software solution.)

¹² In our preregistration, we specified that we would continue to collect data either until we reached 4,000 observations or until four weeks had passed (whichever occurred first).

¹³ All results presented are substantively robust to the inclusion of these 93 omitted observations.

¹⁴ Recall that workers had five opportunities to claim bonuses and, in each instance, could claim either a voicemail bonus (worth \$0.10) or a survey bonus (worth \$0.25). In analyses not presented, we also examine the effect of treatments on misconduct at the intensive margin; in other words, conditional on engaging in *any* misconduct, do treatments have any effect on the *amount* of misconduct a worker engages in? Here, we find no statistically significant effects.

¹⁵ The "treatment language page" corresponds to the section depicted in Figure 2 in which workers are initially presented with a condition-specific description of the hiring employer.

¹⁶ We emphasize that these findings are exploratory in nature and should be taken as suggestive rather than conclusive. A formal mediation analysis focused on a more precise understanding of this mechanism is beyond the scope of this paper.

¹⁷ The volunteer/donate score was constructed from responses to two individual questions in our survey: "Do you participate in volunteer work?" and "Do you donate to charity?" Workers could choose from four responses in each case, ranging from "Not at all," assigned a value of one in our variable construction, to "Often" (for volunteering) or "Yes, a large amount" (for donations), assigned a value of four in our variable construction. The responses for these two questions were then summed to create an aggregate volunteer/donate score. The median of this composite variable was a score of five (on a scale ranging from two to eight). Our binary variable of focus assigns a value of zero if a worker scores below five and a value of one if a worker scores a five or above.

¹⁸ This follow-up study was preregistered as an amendment to the initial preregistration for the main study.

¹⁹ Note that the main (and bonus) tasks assigned to workers in our original job were *not* part of our follow-up study (which contained only a survey). Because this follow-up survey required substantially less effort and time than our initial job, we paid workers less here than in our original study. In the follow-up survey, estimated completion time was three minutes, and workers' actual median completion time was 1.7 minutes.

²⁰ Although we are able to effectively rule out reverse causation with this follow-up study, one possibility that we are *not* able to rule out is that of an omitted variable. Because data are self-reported, it is possible that workers who tend to engage in misconduct also tend to generally inflate their answers to questions about volunteer/donation history because of some other unobserved, underlying factor. Put differently, perhaps workers who are more likely to engage in misconduct are also more likely to lie in self-reported survey answers.

²¹ Results are substantively robust to alternative splits in the distribution (e.g., top versus bottom 50%, top 25% versus bottom 75%, etc.) as well as to splits of the distribution into three distinct segments rather than two.

²² Note that, conditional on selection into the bonus task, fraudulent claims occurred at a relatively high rate of roughly 50%.

²³ It is possible that individuals who are more prone to engaging in misconduct might also be more likely to lie in general (and, thus, misrepresent or have an exaggerated view of their charitable behavior).

²⁴ Specifically, only when workers are told, "We donate the equivalent of x% of our wage bill in cash (*on behalf of all workers who help us with this project*) to UNICEF Education Programs" do List and Momeni (2021) find a statistically significant effect on unethical behavior. When the same statement without the parenthetical is administered, key results are insignificant. One important open question, then, is whether moral licensing is elicited when CSR is framed in terms of the organization rather than the individual.

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