TRANSLATING REINFORCER DIMENSIONS AND BEHAVIORAL ECONOMIC DEMAND TO INFORM INCENTIVE DELIVERY IN ORGANIZATIONAL BEHAVIOR MANAGEMENT

BY

Amy Jessica Henley

Submitted to the graduate degree program in the Department of Applied Behavioral Science and the Graduate Faculty of the University of Kansas in partial fulfillment of the requirements for the degree of Doctor of Philosophy

Chairperson Florence D. DiGennaro Reed, Ph.D., BCBA-D

Derek D. Reed, Ph.D., BCBA-D

Peter G. Roma, Ph.D.

Jacob T. Fowles, Ph.D.

Edward K. Morris, Ph.D.

Date Defended: April 21, 2017

The Dissertation Committee for Amy Jessica Henley

certifies that this is the approved version of the following Dissertation:

TRANSLATING REINFORCER DIMENSIONS AND BEHAVIORAL ECONOMIC DEMAND TO INFORM INCENTIVE DELIVERY IN ORGANIZATIONAL BEHAVIOR MANAGEMENT

Florence D. DiGennaro Reed, Ph.D., BCBA-D

Date Approved: April 21, 2017

Abstract

Recent research has effectively translated behavioral economic demand curve analyses for use with work-related behavior and workplace incentives (e.g., Henley, DiGennaro Reed, Reed, & Kaplan, 2016). The present experiments integrated a hypothetical and experiential demand preparation into a computerized task for use with Amazon Mechanical Turk Workers to evaluate the effects of parametric manipulations of reinforcer dimensions on performance using a behavioral economic demand framework. The first experiment examined the effects of three incentive magnitudes ($0.05, $0.10, and $0.20) on performance assessed with a progressive ratio schedule. Results indicate responding on the hypothetical and experiential demand assessments was sensitive to incentive magnitude, with higher responding in the higher incentive magnitude conditions. Participant responses on the hypothetical assessment were in general agreement with observed responding in the experiential assessment. The second experiment extended the methods of Experiment 1 to evaluate the effects of three parametric values of reinforcer probability (10%, 50%, and 90% probability of earning incentives). Responding was generally comparable for all three probability conditions. Experiment 3 evaluated the effects of three delays to incentive receipt (1, 14, and 28 days). Responding was higher in the condition in which incentives were delayed by 1 as compared to 28 days. Results of the current studies may inform the development of novel methods for measuring reinforcer efficacy in organizations.

Acknowledgements

I would like to extend my sincere gratitude to all the wonderful members of the KU Performance Management and Applied Behavioral Economics laboratories, both past and present. I very much appreciate all your encouragement, feedback, and support over the last five years!

Special thanks to Sarah Jenkins for patiently listening and selflessly providing her time, guidance, and friendship.

A big thank you also goes out to Matt Novak for asking the tough questions and always being willing to hash out the answers over the office whiteboard in the middle of the night or at a good happy hour.

I would like to thank my committee—Drs. Peter Roma, Edward Morris, and Jacob Fowles—for their expert guidance and mentorship. Your thoughtful suggestions and insight were invaluable to this project. A special thank you to Dr. Derek Reed, this project would not have been possible if not for your willingness to share your time and expertise to help support my professional goals.

Thank you to my family and friends who provided encouragement to press on and reassurance that "I've got this."

Finally, I cannot express how thankful I am to have had such an incredible mentor, Dr. Florence DiGennaro Reed. There seems to be no bounds to your passion, generosity, and support, all of which helped me get here today. In addition to so many professional opportunities you made available to me over the years, I cannot thank you enough for your enormous support and flexibility in helping me pursue my research interests.

Table of Contents

List of Illustrative Materials

Appendix Index

Translating Reinforcer Dimensions and Behavioral Economic Demand to Inform Incentive Delivery in Organizational Behavior Management

Applied behavior analysis is a scientific discipline that uses the principles of behavior to identify practical strategies for improving socially significant behavior (Baer, Wolf, & Risley, 1968). Behavior analysts have made impressive advancements in numerous areas including autism treatment, addiction, educational practices, public health, parent training, and others. A sub-discipline of applied behavior analysis, known as organizational behavior management (OBM), has a rich history of changing behavior of individuals and groups in the workplace (Wilder, Austin, & Casella, 2009). The importance of targeting efforts in the workplace may not be immediately obvious until one considers that work is the single activity to which Americans allocate the most time and the workforce accounts for 60% of Americans over the age of 16 (Bureau of Labor Statistics, 2014, 2016a). The workplace is also affected by a host of pressing issues including job dissatisfaction among more than half of U.S. workers (Ray, Rizzacasa, & Levanon, 2013) and employee turnover rates that are often highest in human service and education settings where skilled employees delivering quality services are essential for consumer and learner outcomes (Dib & Sturmey, 2007; Gresham, Gansle, Noell, Cohen, & Rosenblum, 1993; Ingersoll, Merrill, & Stuckey, 2014; Strouse, Carroll-Hernandez, Sherman, & Sheldon, 2003). Moreover, nearly 5,000 work-related fatalities and 3 million injuries and illnesses occur annually (Bureau of Labor Statistics, 2016b) and over a quarter of Americans have been bullied at work (Workplace Bullying Institute, 2014). The workplace is paramount to our daily lives, increasing the need for strategies to alleviate the many issues faced by organizations, employees, and consumers of the goods and services they produce.

When one considers the central role work plays in our lives and the many workplace issues, it is not surprising that a specialized sub-discipline of applied behavior analysis was established to address behavior in the workplace. The burgeoning of OBM since its beginning in the 1960s may be attributable, at least in part, to the robust results the field has achieved in diverse industries, with organizations of all sizes, and in addressing issues of varying complexity (Dickinson, 2001). The expansion of OBM has resulted in the development of three sub-disciplines within the field: behavioral systems analysis, behavior-based safety, and performance management.

Behavioral Systems Analysis

Behavioral systems analysis is an approach to evaluating organizational performance that integrates traditional behavior analytic principles with general systems theory (Diener, McGee, & Miguel, 2009; Von Bertalanffy, 1950). Behavioral systems analysis views the organization as a whole and evaluates the interaction between the components (e.g., departments) and processes within the system (e.g., hiring practices, product development) and between the organization and external environment (e.g., consumers and competition; Johnson, Casella, McGee, & Lee, 2014). The overarching goal of behavioral systems analysis is to increase an organization's ability to adapt to and meet the needs and pressures of a dynamic and an ever-changing environmental context in which the organization exists (Diener et al., 2009; Johnson et al., 2014). Behavioral systems analysis focuses on changing organizational systems, processes, and individual employee performance to accomplish this goal. Typical procedures in behavioral systems analysis include, but are not limited to, process design, policy changes, organizational restructuring, and re-allocation of resources and individual employee performance. The substantial resources needed to carry out behavioral systems analysis is a noted disadvantage. In fact, it can take several weeks or even months to pinpoint organizational issues and identify a change strategy (Johnson et al., 2014). Although advocates of behavioral systems analysis report numerous successful applications in practice, to date there is limited published empirical or experimental evidence evaluating and consequently supporting its use (Johnson et al., 2014; Sigurdsson & McGee, 2015). Clearly more research is needed; however, the molar approach to improving organizations taken by behavioral systems analysis may ultimately prove propitious.

Behavior-Based Safety

As previously noted, work-related injuries and illnesses are a pervasive problem (e.g., Bureau of Labor Statistics, 2016b). In addition to personal and emotional harm, work-related injuries have tremendous financial ramifications. The National Safety Council (2015) estimates that medical costs, administrative expenses, and losses in wages and productivity totaled over $206 billion in 2013. In fact, Loafman (1996) argues the costs of work-related injuries and illnesses is one of the nation's largest *avoidable* expenditures. Although some instances may be unavoidable, many injuries result from unsafe behavior of staff. Traditional approaches to decreasing injuries involve reducing or eliminating physical hazards in the environment by developing equipment or mechanical safety devices that physically block contact with dangerous machines or to unsafe spaces (e.g., hard hat, safety goggles, railings). Unfortunately, preventable injuries continue to occur at unacceptable rates despite impressive advances in safety equipment. In many cases, safety equipment is not permanently affixed and requires behavior for safe use—this fact necessarily requires behavioral approaches to ameliorate workplace injuries.

Thus, behavior-based safety involves the application of behavioral principles and environmental manipulations to reduce the occurrence of preventable work-related injuries and illnesses (Sulzer-Azaroff & Austin, 2000). Grindle, Dickinson, and Boettcher (2000) suggest that close consideration of the consequences for safe and unsafe behavior may provide insight into the causes of work-related injuries. Specifically, they note:

> … natural consequences may support and encourage unsafe behaviors because: (a) performing in a safe manner results in immediate, probable, negative consequences such as discomfort and increased effort or time; while (b) performing in an unsafe manner rarely results in an injury but does result in immediate, probable positive consequences such as savings in time and effort and avoidance of discomfort. (p. 34)

The inadequacy of natural consequences operating in the workplace to evoke and maintain safe behavior is supported by the findings of Grindle and colleagues. In their review of behavioral safety interventions, Grindle et al. found that consequent interventions proved more effective than antecedent interventions for safety behavior. In this context, antecedent interventions include environmental modifications and other strategies (e.g., altering motivating operations) to change working conditions *before* workers emit behavior (Cooper, Heron, & Heward, 2006). Whereas consequent interventions involve contingent manipulations of stimuli that *follow* a behavior (i.e., modifying consequences). The findings of Grindle et al. suggest delay to reinforcement, probability of reinforcement, and effort required for safe behavior are important considerations to supplement inadequate natural contingencies when designing behavior change procedures for safety and potentially other behavior in the workplace.

Performance Management

The third OBM sub-discipline—performance management—involves the analysis and direct manipulation of antecedents and consequences to improve individual or group performance within an organization (Daniels & Daniels, 2006). In contrast with the relatively specific nature of behavior-based safety, performance management targets a wide variety of employee behavior including absenteeism (e.g., Camden & Ludwig, 2013), cleaning (e.g., Clayton & Blaskewicz, 2012; Doll, Livesey, McHaffie, & Ludwig, 2007), implementation of behavior support plans or teaching procedures (e.g., Miller, Carlson, & Sigurdsson, 2014), customer service (e.g., So, Lee, & Oah, 2013), identification-checking (e.g., Downing & Geller, 2012), and numerous others. Performance management procedures are a necessary component of behavioral systems analysis and behavior-based safety, rendering continued efforts to evaluate and maximize the effectiveness of performance management interventions of particular importance to OBM.

Within performance management, employee performance problems are frequently classified as either a skill or motivational deficit (Mager & Pipe, 1970). A skill deficit refers to performance problems that are a function of insufficient knowledge or practice necessary for correct performance to occur. In contrast, when an individual can correctly demonstrate a behavior but does not, the problem is likely attributable to a motivational deficit, wherein the consequences for correct performance are inadequate to maintain behavior. Accordingly, performance management interventions are often classified as antecedent- or consequent-based to address skill and motivational deficits, respectively (Mager & Pipe, 1970; Wilder et al., 2009). Common antecedent interventions in OBM include staff training, goal setting, instruction, and task clarification. Consequent-based interventions in OBM consist of performance feedback, incentives (monetary and non-monetary), and—although they are infrequently evaluated in the literature—other types of consequences including promotion and progressive discipline.

VanStelle et al. (2012) conducted a review of empirical research articles published in the *Journal of Organizational Behavior Management* (*JOBM*) between 1998 and 2009 to evaluate the types of performance issues researchers targeted for change as well as intervention strategies used. VanStelle et al. found that 78% of performance problems were due to a motivational deficit. The frequency of performance problems that are a function of motivational deficits underscores the importance of continued research examining consequent interventions. Performance feedback and incentives were the most frequently employed consequent interventions VanStelle and colleagues reported, used in 68% and 26% of studies, respectively. The importance of high-quality staff training and appropriate antecedents cannot be discounted; these components are necessary for correct performance. However, as evidenced by the greater relative frequency with which performance problems are a function of motivational deficits, antecedent-based interventions are not sufficient to maintain performance over time. Examination of consequences may shed light on specific consequence properties that support desirable and undesirable employee behavior, similar to the explanation proposed by Grindle and colleagues (2000) with respect to safety behavior. Thus, continued evaluation of consequent-based interventions that OBM researchers use most frequently (i.e., performance feedback and incentives) is a worthwhile endeavor.

Performance Feedback

Performance feedback—hereafter referred to in short as feedback—is the provision of information about performance that allows an individual to adjust his or her behavior (Daniels & Daniels, 2006; Prue & Fairbank, 1981). Feedback is the most frequently used of *any* performance management technique, including antecedent interventions (VanStelle et al., 2012). Although it is classified here and elsewhere as a consequent-based intervention (e.g., Wilder et al., 2009), feedback may also function as an antecedent for correct performance the next time an employee performs a task (Daniels, 2000). Numerous literature reviews and component analyses have evaluated various feedback characteristics in an effort to identify the method of delivery and content that maximizes feedback effectiveness (e.g., group vs. individual, weekly vs. daily, objective vs. evaluative; Alvero, Bucklin, & Austin, 2001; Balcazar, Hopkins, & Suarez, 1985;

Henley & DiGennaro Reed, 2015; Johnson, 2013, 2015; Pampino, MacDonald, Mullin, & Wilder, 2004; Prue & Fairbank, 1981; Reid & Parsons, 1996). The cumulative research examining feedback characteristics has provided valuable insight regarding effective delivery methods. In a review of the feedback literature published between 1985 and 1998, Alvero and colleagues found that only 47% of studies resulted in consistent performance improvements when feedback was the sole behavior change procedure. The inconsistency with which feedback interventions result in performance improvements is a noted disadvantage. However, feedback remains a valuable method because it is a flexible and cost-effective means for changing employee behavior that can be adapted for use in most organizational settings. Organizational behavior management would benefit from continued research on the characteristics and function of feedback to improve its effectiveness and consistency.

Incentives

Incentives are rewards delivered to an individual or group contingent on the occurrence of a desired behavior or reaching a performance criterion (e.g., fire drill completion, number of hours billed). Incentives may include money or other tangible or intangible items (i.e., non-monetary incentives) such as time off from work (Austin, Kessler, Riccobono, & Bailey, 1996), food (Kortick & O'Brien, 1996), gift certificates (Miller et al., 2014), and numerous others. The use of incentives to improve employee performance in OBM dates back to the early 1970s (for a review of the history of OBM see Dickinson, 2001). Incentives have documented advantages including reliable increases in net profits as well as improvements in performance, however measured (Bucklin & Dickinson, 2001). Furthermore, monetary and non-monetary incentives have been shown to be effective in both laboratory (e.g., Oah & Dickinson, 1992) and applied settings (e.g., Luiselli et al., 2009), when delivered alone (e.g., Lee & Oah, 2015) or in combination with other performance management procedures (e.g., Goomas & Ludwig, 2007), with lottery or probabilistic arrangements (e.g., Alavosius, Getting, Dagen, Newson, & Hopkins, 2009; Orpen, 1974), and with varied behaviors in diverse industries (for a review see Bucklin & Dickinson, 2001). The robust findings have resulted in sustained interest in incentives as an attractive and viable performance management procedure.

Like feedback, numerous methodological variations in the delivery of incentives exist. Empirical evaluations of incentive variations are important because effectiveness may depend, at least in part, on the method with which they are delivered. Unfortunately, experimental evaluations of methodological variations are relatively infrequent. Of the extant literature comparing individual incentive arrangements, several lines of inquiry have emerged (Bucklin & Dickinson, 2001).

Reinforcement schedules. One area of investigation popular in the 1970s and 1980s concerned experimental evaluations of reinforcement schedules for incentive delivery. Roughly half of these studies were conducted in the laboratory (Berger, Cummings, & Heneman, 1975; Lee & Oah, 2015; Pritchard, DeLeo, & Von Bergen, 1976; Pritchard, Hollenback, & DeLeo, 1980; Yukl, Wexley, & Seymore, 1972) and the remaining were conducted in the workplace (Latham & Dossett, 1978; Saari & Latham, 1982; Yukl & Latham, 1975; Yukl, Latham, & Pursell, 1976). Eight of the nine studies involved comparisons of continuous and variable ratio (VR) schedules of reinforcement on performance. For example, in an early applied investigation, Yukl and Latham evaluated the effects of three schedules of monetary incentives on bags of seedlings planted by 38 tree planters using a between groups design. In addition to a fixed $2 hourly wage, incentive amounts were $2, $4, and $8 for the groups that received incentives on a continuous reinforcement, VR2, and VR4 schedule, respectively. The authors also included a control condition in which planters earned fixed pay only in the amount of $3 per hour. Relative to baseline, performance in the continuous reinforcement condition increased by 33%, decreased by 8% in the VR2 condition, and increased by 18% in the VR4 condition. Performance in the control condition was unchanged. Although Yukl and Latham's results demonstrate differentiated performance in the incentive conditions with the highest performance in the condition with the highest rate of reinforcement (i.e., continuous reinforcement), their results should be interpreted with caution because of at least two limitations. Several participants in the VR2 condition had difficulty understanding the procedures and objected to the method for determining incentive receipt (i.e., correctly guessing a coin toss), potentially accounting for performance decreases. Relatedly, because incentive receipt was based on a coin toss, Dickinson and Poling (1996) estimate that the reinforcement schedule participants actually experienced was a VR1.75 rather than a VR2. The collective findings of research evaluating reinforcement schedules indicate performance was generally higher when participants earned incentives as compared to hourly pay. However, results comparing different schedules of incentive delivery were idiosyncratic.

Percentage of incentive pay. A second line of inquiry involves evaluations of varying percentages of incentive pay (Dickinson & Gillette, 1994; LaMere, Dickinson, Henry, Henry, & Poling, 1996; Matthews & Dickinson, 2000; Oah & Lee, 2011; Riedel, Nebeker, & Cooper, 1988). To examine percentage of incentive pay, researchers constrained total compensation and manipulated the proportion of pay available through incentives relative to base pay (i.e., guaranteed wages). Incentive pay ranged between 0% (i.e., no incentives) and 100% (i.e., piece-rate pay only) with higher percentages constituting a higher proportion of pay dependent on measureable performance. Although arrangements in which pay is primarily earned through incentives provide the strongest contingent relation, performance compensation is subject to vary across pay periods and may increase the unpredictability of pay. The potential outcome includes difficulty budgeting individual income and aggregate labor costs for employees and employers, respectively, which may be perceived as riskier and less preferred relative to fixed wages for both parties. In light of the large body of research revealing superior performance when individuals earn incentives, research examining the percentage of incentive pay sought to evaluate whether there was an optimal percentage that balances the advantages (performance improvements) with the disadvantages (risk) of incentive systems.

Despite what one might predict given contingent relations, all but one study evaluating the percentage of incentive pay found performance to be comparable across varying percentages of incentive pay. One criticism of this body of literature is the limited ecological validity of laboratory settings. Specifically, many of the contingencies for engaging in on- and off-task behavior, motivating operations, and competing response alternatives present in the workplace are often lacking from laboratory-based studies despite researchers' best efforts. To increase ecological validity, Oah and Lee (2011) used a modified experimental preparation by recruiting individuals with a three-year history of socializing with one another to jointly participate and increasing the number and length of sessions to better approximate the workplace (30 sessions each lasting 6 hr conducted 5 days a week). The authors found consistently higher productivity and duration of time working when participants earned 100% of their pay from incentives relative to conditions in which participants earned 10% and 0% of their pay from contingent incentives. Productivity and work duration did not differ between the 10% and 0% conditions. Although more research is needed, these initial findings suggest that higher percentages of incentive pay may result in higher levels of performance and that percentages at or below 10%

incentive pay may not be sufficient to generate meaningful performance increases. Additionally, the findings suggest OBM researchers can modify the laboratory setting to better approximate a workplace.

A possible explanation for the inconsistent findings across studies evaluating reinforcement schedules for incentive delivery and the percentage of incentive pay may involve the difficulty simulating complex and competing contingencies operating in the workplace. Results of studies evaluating reinforcement schedules appear initially to be idiosyncratic (Bucklin & Dickinson, 2001; Dickinson & Poling, 1996). Upon further inspection, an interesting pattern emerges. Performance was generally equivalent under fixed and variable schedules for studies conducted in a simulated work setting but differentiated in studies conducted in a real workplace. With respect to the line of research evaluating the percentage of incentive pay, LaMere and colleagues (1996) were the only researchers to evaluate this topic in an applied setting. Although LaMere and colleagues did not find differences in performance among percentages, the authors evaluated percentages of incentive pay below 10%, which may not be sufficiently high to result in performance differences revealed by Oah and Lee (2011). The inconsistent findings between laboratory and applied studies highlights a common and prominent limitation of laboratory-based OBM research; namely, the limited ecological validity associated with many simulated workplaces.

Based on findings from the basic literature, Bucklin and Dickinson (2001) argue, “the work environment can be viewed as analogous to a behavioral choice situation where multiple concurrent schedules of reinforcement exist” (p. 65) and individuals in this situation will maximize reinforcement. For that reason, even when the percentage of incentive pay is small or the reinforcement schedule is lean, performance may be undifferentiated across conditions when no alternative sources of reinforcement are available or when the reinforcing efficacy of concurrently available alternatives is low relative to the experimental task. For example, to simulate the presence of alternative activities present in real work settings, Matthews and Dickinson (2000) provided participants with the opportunity to engage in off-task behavior. During a single 70-min experimental session, a computer program provided either two or four prompted opportunities to play one of three computer games for a maximum of 5 min. The ecological validity of these procedures are questionable because the session duration, *prompts* to engage in off-task behavior for a *finite* duration, and limited available alternatives are not representative of the workplace. Oah and Lee (2011) evaluated the same percentages of incentive pay as Matthews and Dickinson (i.e., 0%, 10%, and 100% incentive pay), but Oah and Lee's experimental preparation produced performance differences. In addition to the features noted previously, participants in Oah and Lee were allowed to take breaks within or outside of the laboratory at any time and had unrestricted access to all computer features including high-speed internet. The presence of known peers with whom to socialize likely served as an additional alternative source of reinforcement (perhaps with high relative reinforcing efficacy) and the quantity and duration of sessions may have functioned as a motivating operation for engaging in off-task behavior over time. The features Oah and Lee included to increase simulation fidelity may better approximate the workplace and enhance the external validity of the findings. Despite the advantages, the financial resources needed to adopt Oah and Lee's preparation limit widespread use of this method. Researchers who wish to study variables relevant to the organization may need to identify an alternative analog setting that is sensitive to resources, but better approximates the workplace than the laboratory.

Preference assessments. Although not a variation of delivery, methods for selecting stimuli for use in an incentive arrangement is a related and imperative research area for incentive effectiveness. Preference assessments include a variety of methods for identifying stimuli likely to function as a reinforcer. Monetary incentives may obviate the need for organizational leaders to conduct preference assessments because money functions as a generalized conditioned reinforcer and is relatively independent of motivating operations. However, the financial and human resources needed to implement an incentive system may preclude the use of monetary incentives. Non-monetary incentives are a popular method to help mediate budgetary restrictions. The myriad types of non-monetary incentives are an advantage of their use, but can make it challenging for organizational leaders or managers to accurately select preferred incentives for employees; a supposition supported by recent empirical work (Wilder, Harris, Casella, Wine, & Postma, 2011; Wilder, Rost, McMahon, 2007). The inadequacy of manager selection underscores the need for the development and evaluation of methods to identify preferred incentives in organizations. Fortunately, interest in employee preference assessments is emerging.

Several studies have compared various preference assessment methods for use in the workplace (Waldvogel & Dixon, 2008; Wilder, Therrien, & Wine, 2006; Wine, Reis, & Hantula, 2014). In the most recent example, Wine and colleagues evaluated three preference assessment methods. For the *multiple stimulus without replacement*, Wine et al. asked participants to select the most preferred stimulus from an array after which the experimenter removed the selected item and presented the remaining stimuli. The experimenter repeated this process of elimination until all the stimuli were assessed. The *survey* method asked participants to rate each item on a scale of how much work they would be willing to complete for an item. Response options for the survey method included: (1) *none at all*, (2) *a little*, (3) *a fair amount,* (4) *much,* and (5) *very much*. Lastly, the *stimulus ranking* procedure asked participants to order the items (provided textually on index cards) from most to least preferred. After completing the preference assessments, Wine and colleagues conducted a reinforcer assessment to evaluate whether high-preferred stimuli functioned as reinforcers and low-preference stimuli did not function as reinforcers (i.e., accuracy). Reinforcer assessment results indicate the survey method consistently classified reinforcing stimuli as high-preferred whereas the multiple stimulus without replacement and ranking methods identified some but not all reinforcers as high-preferred. Wilder et al. obtained similar findings when comparing the survey method to a *paired stimulus* assessment in which each item assessed was paired with every other stimulus and participants selected the more preferred item of the two. Wilder and colleagues found the survey method to be more accurate than the paired stimulus procedure. Incorporating the amount of work an individual is willing to complete to earn an incentive is a distinguishing feature of the survey method from the other methods Wine et al. and Wilder et al. assessed. Although the survey method uses a subjective (e.g., "a fair amount" of work) rather than objective (e.g., reporting a quantity) measure of work output, the survey method is nonetheless an improvement over methods in which reinforcers are identified at low work requirements or without consideration of the work requirement, which is not representative of conditions employees will actually experience in the workplace to earn incentives. The results of Wine et al. and Wilder et al. suggest preference assessment methods for use in organizations may benefit from incorporating the work requirement needed to earn an incentive.

Prevalence of incentive use. DiGennaro Reed and Henley (2015) evaluated staff training and performance management practices offered to individuals certified by or seeking certification from the Behavior Analyst Certified Board® and working in applied settings. DiGennaro Reed and Henley found that just 8% of respondents reported receiving incentives contingent on performance more than twice per year. When considering the effectiveness of incentives and the frequency with which they are evaluated in the OBM literature, incentives appear to be used with relative paucity in applied settings employing board-certified staff.

The infrequent use of incentives observed by DiGennaro Reed and Henley (2015) suggests there may be a gap in the literature that serves as a barrier to incentive use in practice. Conceivably, the numerous possible incentive delivery methods may pose a challenge to their use. The literature comparing delivery methods only covers a modest portion of possibilities. It may be difficult for organizational leaders to draw clear conclusions from the extant literature comparing incentive delivery variations needed to guide the design of an *empirically supported* incentive system, particularly when one considers the conflicting findings noted previously. As a result, incentives may be delivered in ineffective and/or cost-prohibitive ways. For example, leaders must select an appropriate incentive amount for use in a system. Erring on the side of providing *larger* incentives may not be financially sustainable and organizational leaders could be forced to discontinue the program before obtaining the desired behavior change. Incentives that are *too small* may not be sufficient to motivate employees to change their behavior and leaders risk expending valuable resources on an ineffective system. Leaders must also select the work requirement needed to earn an incentive, the frequency of delivery, delay to delivery, and incentive type, as well as make decisions about many other variables. Decisions such as these can be detrimental if made arbitrarily. The numerous logistical questions that remain unanswered suggests that, although incentive effectiveness was established over 40 years ago, since then research has generated limited information to guide their use in the workplace. A general lack of helpful information to guide effective incentive delivery likely contributes to their lack of use or short half-life when used (DiGennaro Reed & Henley, 2015; Huselid, 1995).

In the present context incentives are, generally speaking, reinforcers delivered in the workplace. Comparisons or parametric analyses of basic reinforcer dimensions to effectively improve and maintain desired performance in a way that is appealing for employees and sustainable for the organization—factors that ultimately dictate whether an incentive system is used in the short- and long-term—are largely absent from the literature. Therefore, an understanding of how reinforcer dimensions influence incentive efficacy is important to guide their efficient and effective delivery in practice.

Reinforcer Dimensions

Reinforcers may vary along a number of dimensions that influence their efficacy including delay, response effort, magnitude, probability, quality, and rate of reinforcement. The experimental analysis of reinforcer dimensions has a long and rich history of study culminating in an extensive empirical literature. The findings of both laboratory (e.g., Chung & Herrnstein, 1967) and applied (e.g., Neef, Mace, & Shade, 1993) studies consistently demonstrate systematic changes in responding with changes in reinforcer dimensions. These findings have led to the successful application of numerous interventions that manipulate reinforcer dimensions of response alternatives to increase the frequency of desired behavior (e.g., Horner & Day, 1991). Reinforcer dimensions are therefore an important consideration that warrant attention in the OBM literature. Experimental investigations evaluating how reinforcer dimensions differentially affect performance may provide a better understanding of the variables that influence employee behavior in the workplace and help inform the development of behavior change procedures for promoting desired performance.

Reinforcer delay. Delay of reinforcement refers to the duration between behavior producing a reinforcer and subsequent reinforcer delivery (Mazur, 1993). A large literature base has shown that reinforcer efficacy systematically decreases as a function the delay to its receipt (Mazur, 1993). An understanding of how delayed reinforcement affects behavior may be of particular relevance to OBM because of practical restrictions that constrain the immediacy with which reinforcers may be delivered in the workplace (e.g., payroll processing). Such constraints render many delays in the workplace of greater duration than those observed in non-human animal studies of reinforcement. Malott (1993) suggests delay to reinforcement in the workplace is too great to directly control behavior. As a result, Malott makes an important distinction between direct- and indirect-acting contingencies. A direct-acting contingency is "a contingency for which the outcome of the response reinforces or punishes that response" (p. 46) and an indirect-acting contingency is "a contingency that controls behavior indirectly rather than through the direct action of reinforcement or punishment by the outcome of that contingency" (p. 46). Malott suggests most behavior in the workplace is controlled by indirect-acting contingencies and mediated by rules describing those contingencies, whereas non-human operant studies rely on direct-acting contingencies to control behavior. An understanding of how reinforcer dimensions, including delay, influence behavior governed by indirect-acting contingencies is less understood and represents a valuable area for future research. Although several OBM researchers have evaluated the effects of delayed feedback (Krumhus & Malott, 1980; Mason & Redmon, 1992; Reid & Parsons, 1996), this analysis has not been sufficiently extended to incentives.

Response effort. Response effort (referred to hereafter as effort) involves the type or amount of behavior required to access reinforcement. Effort encompasses various facets of a response including difficulty, physical exertion (e.g., weight, force required, distance traveled), or time necessary to successfully perform a behavior. Research manipulating response effort reliably demonstrates an inverse relation between effort and response rate, with higher rates associated with lower effort responses (Friman & Poling, 1995). Therefore, careful consideration and evaluation of response effort is warranted (DiGennaro Reed, Henley, Hirst, Doucette, & Jenkins, 2015). With respect to the workplace, employees have multiple job responsibilities that differ in the amount of effort required by an employee. The effort needed may influence the tasks to which employees allocate their time, and could result in allocation to irrelevant, inappropriate, or unsafe tasks or behaviors. A strategically delivered incentive contingent on completion of high-effort responses could help ameliorate this issue. Effort may also influence whether employees engage in a desired behavior. In one of the only published experimental accounts of an effort manipulation in *JOBM*, Shook, Johnson, and Uhlman (1978) slightly improved the frequency direct care staff graph client behavior by moving the necessary materials closer to the location in which graphing was to take place, thereby reducing the effort required to perform this job responsibility. Unfortunately, further evaluations of response effort in OBM are well overdue.

Reinforcer magnitude. Also commonly referred to as amount, reinforcer magnitude is the quantity of a reinforcer and can be measured in terms of number or duration of access. Research has consistently documented that magnitude influences reinforcer efficacy with reinforcers of greater magnitude being relatively more efficacious (Fantino, 1977). Although money is generally assumed to be an effective reinforcer for humans, Daniels (2000) suggests the reinforcing efficacy of money may still depend on amount. The reinforcer magnitude necessary to evoke behavior change is important to organizations because it must be financially feasible to implement any incentive system involving reinforcers that cost money, directly or indirectly (i.e., personnel time to implement the system). Identifying the lowest-magnitude reinforcer that can effectively maintain behavior may be especially important for human service, educational, or nonprofit organizations where finances are often limited.

Reinforcer probability. Probability of reinforcement is the likelihood, on a given schedule, a reinforcer will be delivered (Ferster & Skinner, 1957). Numerous studies have demonstrated incentive effectiveness when delivered in a probabilistic or lottery arrangement (e.g., Alavosius et al., 2009; Orpen, 1974; Reed, DiGennaro Reed, Campisano, LaCourse, & Azulay, 2012). However, many of these studies do not specify the programmed and/or experienced probability. For example, Cook and Dixon (2006) evaluated the effects of a $50 probabilistic incentive on the percentage of completed forms for three supervisors working with individuals with intellectual and developmental disabilities. All participants had the opportunity to earn the incentive each week regardless of performance; however, the experienced probability varied weekly because it was determined based on performance of all three participants. The highest performer had a 50% chance of earning the incentive, the second and third highest performers had a 33% and 17% chance, respectively. If two participants completed an equal percentage of forms for a given week, both participants had a 40% probability, whereas the lowest performer had a 20% probability of earning the incentive. Although performance was highest for all participants during the lottery incentive system, the probability associated with performance gains remains unclear. Research evaluating incentive probability may help mediate budgetary restrictions associated with delivering higher magnitude or more frequent incentives.

Reinforcer quality. Reinforcer effectiveness depends in part on the relative preference for a reinforcing stimulus, referred to as quality (DiGennaro Reed et al., 2015). As previously discussed, OBM researchers are beginning to develop preference assessment methods that will provide useful information about reinforcer quality (e.g., Wine et al., 2014). Research has also demonstrated that quality is influenced by other reinforcer dimensions such as magnitude (Trosclaire-Lasserre, Lerman, Call, Addison, & Kodak, 2008). Because reinforcer dimensions may interact and influence quality in unpredictable ways, leaders would benefit from having reliable and efficient methods to assess reinforcer preference in OBM. Despite a robust literature examining methods for identifying preferred reinforcers with individuals with intellectual and developmental disabilities (see Cannella, O'Reilly, & Lancioni, 2005 for a review), research in OBM has only just started to evaluate methods for identifying reinforcers in the workplace.

Rate of reinforcement. Lastly, rate of reinforcement refers to the number of reinforcers per unit of time (DiGennaro Reed et al., 2015). Because *rate* of reinforcement is influenced by the *schedule* of reinforcement, investigations of reinforcement schedules involving incentives may provide insight to the effects of reinforcement rate on work-related behavior. Features of the organizational setting make it challenging to experimentally investigate this dimension despite its relevance to leaders and employees. For example, restrictions associated with how often reinforcement can be delivered in an organization—payroll processing, duration of job responsibilities for which reinforcement is contingent—constrain the strategic use of reinforcement schedules. Unfortunately, response alternatives unrelated to job responsibilities may provide higher rates of reinforcement for off-task behavior (e.g., engaging social media several times during a work day). These and other variables emphasize the importance of identifying ways to maximize the effects of *other* reinforcer dimensions (e.g., probability, delay) to combat challenges associated with addressing concurrently available reinforcers in the workplace.

The wealth of basic behavioral research demonstrates that variations in reinforcer dimensions modulate reinforcer efficacy. Much of the research, however, has been conducted with non-human animals in highly controlled settings using choice procedures in which an animal responds to two concurrent alternatives. The workplace is replete with innumerable contingencies associated with consequences that vary asymmetrically in terms of the dimensions reviewed in the preceding discussion. Moreover, reinforcer efficacy is modulated by the context and concurrent response options. Therefore, it is important for incentive efficacy that reinforcers are delivered in a way that effectively competes with alternative sources of reinforcement available in the workplace. The translation of basic literature to inform an understanding of dimensions influencing reinforcer efficacy and performance in organizations is vital for the effective use of incentives.

Reinforcer Dimensions and OBM

As part of a comprehensive review of the literature (Appendix A), I identified 75 experimental studies that evaluated the effects of incentives on work behavior.[1] Seven studies, discussed previously, compared the effects of different schedules of reinforcement (i.e., rate; Berger et al., 1975; Latham & Dossett, 1978; Pritchard et al., 1976; Saari & Latham, 1982; Yukl & Latham, 1975; Yukl et al., 1976; Yukl et al., 1972). One study evaluated the effects of two probabilistic arrangements (Evans, Kienast, & Mitchell, 1988). No studies examined reinforcer magnitude, reinforcer quality, or response effort. Therefore, outside of evaluations of reinforcement schedules, only one published study has *systematically* manipulated and *directly* evaluated the effects of varied features of reinforcer dimensions on work performance (i.e.,

[1] Although researchers in the field of industrial-organizational psychology frequently study incentives, much of this research is correlational in nature and—despite being identified in the early stages of the literature review—did not meet the inclusionary criterion outlined in Appendix A requiring that researchers *manipulate* a reinforcer dimension.

Evans et al., 1988). Evans and colleagues evaluated the effects of two probabilistic incentive arrangements on performance of 18 automobile service mechanics across two dealerships. For each service repair, the experimenters measured the difference between standard repair duration indicated in the automobiles' service manual and observed participant repair time, referred to as *time saved*. Following a baseline period in which service mechanics earned hourly pay only, the experimenters implemented one of two incentive arrangements for eight weeks. In both incentive arrangements, participants could draw a token for every hour of time saved during the previous workday. Drawings occurred every morning. Tokens were worth zero, one, two, or five Washington State Lottery Tickets. In the first arrangement, 100% of tokens were worth at least one lottery ticket delivered immediately. In the second arrangement, only 10% of tokens resulted in the receipt of at least one lottery ticket. In the second arrangement only, all tokens were entered into a weekly drawing for the chance to win $150 regardless of whether the participant received any lottery tickets. Winners of the weekly drawing were then entered into a second drawing for $350, conducted every four weeks in lieu of the weekly drawing. Participants could collect monetary earnings in the form of a check immediately after the weekly and four-week drawings. The authors found that both incentive systems increased performance above baseline (hourly pay), however, there were no differences between the two incentive arrangements.

Despite empirically evaluating two incentive probabilities, several features of Evans et al.'s (1988) experiment preclude any conclusions about the influence of probability level on performance. First, participants in the second arrangement were entered into two additional drawings for which the probability of reinforcement was unknown. The additional drawings also differed in magnitude and delay making it difficult to isolate the effects of probability on performance. Moreover, the authors found that both incentive arrangements were less effective as compared to seven comparison dealerships with "traditional" incentive programs and there were no significant differences in cost-effectiveness from baseline to intervention for either group or between groups. The authors failed to specify what "traditional" incentive programs were; thus, this comparison provides little if any new information about the effects of incentives on work behavior. Because Evans et al. did not provide a rationale for the reinforcer magnitude selected, perhaps the amount was selected arbitrarily and may have been too expensive to offset the savings resulting from performance improvements. If this was the case, the program may have been cost-effective if the authors had made decisions about the incentive system arrangement based on empirical evidence. Unfortunately, as previously noted, such empirical evidence is lacking.

The paucity of OBM research evaluating reinforcer dimensions is concerning. Additionally, of the research that has manipulated and compared reinforcer dimensions, the most recent was conducted nearly 30 years ago (Evans et al., 1988). Although reinforcer dimensions received some limited interest early in OBM's history, manipulations of reinforcer dimensions based on extrapolations from laboratory findings with non-humans have dwindled. Consequently, incentive applications arguably stand decades behind basic behavior analytic developments (Poling & Braatz, 2001).

A possible explanation for the disconnect between OBM and basic behavior analytic research may be, broadly speaking, a function of the reinforcement history of OBM applications. After all, scientific behavior is subject to operant contingencies (Skinner, 1956). Poling and Braatz (2001) argue OBM interventions are often "without fine-grained analysis of the variables controlling behavior… or of the behavioral mechanism through which an intervention works" (pp. 44-45). Despite this observation, OBM interventions have produced desired changes in performance on an assortment of socially important behaviors, irrespective of the procedure used. Poling and Braatz further submit that the effectiveness of such “crude” applications was likely possible because many procedures and contingencies operating in organizations were arranged by organizational leaders who have limited knowledge of controlling variables. As a result, it is “not terribly difficult for a behavioral technician to make reasonable suggestions for improvement” (p. 45) based on a knowledge of operant behavior and contingencies—even without an understanding of the function of target behavior or a proposed intervention. It is therefore conceivable that the disconnect between basic research findings and OBM is partly attributable to early researchers having contacted punishment when extrapolating from basic non-human animal findings (as evidenced by the incongruous findings of evaluations of reinforcement schedules) but contacted reinforcement for implementing interventions not directly derived from the basic behavioral literature. Poling and Braatz caution that the challenges associated with keeping up with competition and managing expenses in a tough economic climate will in turn demand a higher level of analysis for understanding, predicting, and managing employee behavior.

Considering the incentive literature is insufficient to guide data-based decisions when organizational leaders are tasked with designing a system, perhaps the contingencies operating in many workplaces no longer support haphazardly selecting the reinforcer type, magnitude, rate, or probability, providing a possible explanation for the lack of incentive use in organizations. Incentives may be more popular—and possibly more effective—if the extant literature better guided leaders on the successful application of incentives. To remain relevant and advance the field, OBM researchers must identify and apply techniques that allow for greater precision in the prediction and control of employee behavior. Fortunately, such a method may already exist in the basic behavioral literature.

Quantitative Analyses

Over the last several decades, basic behavioral research has developed and popularized techniques that use mathematical and statistical modeling (i.e., quantitative analyses; Nevin, 2008). Quantitative analyses in the basic literature frequently include matching (e.g., Baum, 1979), discounting (e.g., Bickel, Jarmolowicz, Mueller, Koffarnus, & Gatchalian, 2012), behavioral momentum (e.g., Nevin, 1988), and demand (e.g., Hursh & Silberberg, 2008). One important advantage of quantitative analyses is that they measure higher-order dependent variables that capture changes in behavior among varied situations; a feature that may be advantageous in applied settings in which numerous variables may differ between any two instances of a given behavior (Critchfield & Reed, 2009). Quantitative analyses also allow for more nuanced measurement and prediction of the *direction* and *degree* of behavior change. This advantage is particularly relevant because organizational viability depends on employee performance and the careful and appropriate allocation of resources. Therefore, *a priori* understanding of the costs and benefits of an intervention is critical to organizational success. Researchers have successfully applied quantitative analyses in organizations (e.g., Manevska-Tasevsk, Hansson, & Labajova, 2016; Moncrief, Hoverstad, & Lucas, 1989), however, they are relatively underexplored in OBM. Behavior analytic researchers have investigated the utility of quantitative models in other applied areas such as with individuals with intellectual and developmental disabilities (e.g., Neef, Shade, & Miller, 1994; Waltz & Follette, 2009) and athletic performance (e.g., Reed, Critchfield, & Martens, 2006). The translation of novel quantitative analyses for evaluating staff performance and reinforcer efficacy in organizational settings may serve to facilitate scientific progress and stimulate research to improve application technology ultimately advancing the field of OBM.

Behavioral economics. Behavioral economics is a specialized sub-field within behavior analysis that seeks to understand and improve the human condition by blending and applying principles from microeconomics with behavioral science (Hursh, 1980). Behavioral economics consists largely of two major quantitative models, discounting and demand, both of which have been suggested for use in OBM.

Discounting. Discounting describes a pattern of responding in which contextual factors associated with a reinforcer reduce its value (Reed, Niileksela, & Kaplan, 2013). Research has most commonly assessed discounting within the context of reinforcer delay (see Madden & Bickel, 2010 for a comprehensive review). Although less common, researchers have also focused on discounting of other contextual factors and behaviors such as probabilistic outcomes and sexual behavior (Johnson & Bruner, 2012; Rachlin, Raineri, & Cross, 1991). Higher rates of delay discounting have been associated with numerous maladaptive behaviors of social significance including obesity, drug abuse, gambling, and risky sexual behavior (Bickel et al., 2012). Although researchers have evaluated discounting with non-human animals using actual reinforcers, most preparations for evaluating discounting with humans rely on hypothetical procedures (Odum, 2011). In a typical delay discounting experimental preparation with humans, participants are presented with a series of choices between two alternatives that vary along the contextual feature of interest (e.g., delay). For example, participants may be asked to choose between the receipt of $100 now or $200 in one year. Over successive presentations the amount and/or delay are manipulated until researchers identify the point at which the two alternatives are subjectively equivalent, referred to as an indifference point. Indifference points can then be used to obtain the rate at which an individual subjectively discounts the value of a delayed outcome. Despite a reliance on hypothetical questionnaires, research evaluating the correspondence between responding to procedures using hypothetical and real reinforcers suggests the degree of discounting is similar across methods (Odum, 2011).

Discounting and OBM. Many of the contextual variables assessed in discounting, such as delay and probability, may influence behavior in the workplace. Although discounting has implications for OBM, research in this area is sparse. In one interesting application to work behavior, Sigurdsson, Taylor, and Wirth (2013) evaluated the relation between discounting of risk and effort in occupational settings to understand safety-related decision-making using a hypothetical task with undergraduate participants. The hypothetical scenario described a job in which participants were working on the roof of a building. Across a series of questions, participants selected in which of two situations they would be more likely to wear safety equipment. Based on participant selections, the distance from the roof to the ground and the time needed to put on a safety harness was adjusted. The authors found that increased effort associated with engaging in safety behavior contributes to riskier decision-making. Specifically, participants discounted the risk associated with working at a given height as the effort to engage in safe behavior increased. Research has also begun to evaluate discounting of the availability of alternative off-task activities in the workplace such as cell phone use (e.g., Hirst & DiGennaro Reed, 2016). Findings of initial discounting investigations make a compelling case for continued scholarly attention in this area. Choice, as assessed via discounting, undoubtedly plays an important role in work behavior and it is possible that measures of delay discounting correlate with other behaviors of interest to the organization.

Demand. Although others had suggested the utility of economic demand theory for behavior analysis (e.g., Lea, 1978), the use of demand curve analyses in behavioral economics was more formally introduced by Hursh in the early 1980s (1980, 1984). Demand curve analyses are predicated on one of the most fundamental economic concepts, the *law of demand*, which states that as the price of a commodity increases consumption of that commodity decreases (Samuelson & Nordhaus, 1985). A demand curve graphically depicts the relation between consumption and price by plotting changes in consumption across a range of increasing prices—on the y- and x-axes, respectively (Figure 1). Demand curve analyses then, quantify the sensitivity of consumption to increases in price for a given commodity. Hursh noted overlap between economic and behavior analytic concepts and subsequently operationalized economic terminology for use within a behavior analytic framework. In the case of behavior analysis, a commodity refers to a reinforcer. Because reinforcers encompass edible (e.g., food), inedible (e.g., stickers, fuel), and even intangible goods (e.g., attention), consumption can refer to the amount (in terms of quantity or duration of access) of the commodity *obtained* or *consumed* for a given unit of time (i.e., rate of reinforcement). Price is the *combined* cost and benefit of a commodity wherein cost is an environmental constraint imposed on obtaining a commodity and benefit is the amount of commodity available at a given cost. Price can refer to a variety of environmental constraints such as effort, time, or money required to produce a reinforcer, delay to reinforcement, and others. Price is often used interchangeably with unit price which is a cost-benefit *ratio*. Demand curve analyses have received considerable attention in the literature and behavior analysts have successfully used them to evaluate a wide range of commodities with diverse populations (for a review see Reed, Kaplan, & Becirevic, 2015).

Several features of demand curves—illustrated in Figure 1—are worthy of note. First, demand intensity is equal to the level of consumption when the commodity is available for free or at a near-free price. Intensity is the maximum level of demand, or the level of consumption the organism behaves to defend as price increases. Breakpoint is the highest price the organism will tolerate to access the commodity. In accordance with the law of demand, consumption decreases as price increases along the x-axis such that demand curves necessarily slope downward; the slope is therefore always negative. Elasticity is an index describing the degree to which consumption is sensitive to increases in price (Hursh, 1984). When the curve is *inelastic*, consumption declines slowly with increases in price, meaning the organism increases expenditure to access an equal or similar rate of consumption. Specifically, when demand is inelastic, a 1% increase in price results in a less than 1% decrease in consumption, resulting in a slope greater than -1. However, because (according to the law of demand) consumption does not increase with price increases, the slope of the inelastic portion of the curve, although greater than -1, should theoretically also be less than zero. *Unit elasticity* is the point at which the curve shifts from inelastic to elastic. At this point, a 1% increase in price is met with exactly a 1% decrease in consumption, resulting in a slope equal to -1. At prices higher than the point of unit elasticity, the curve becomes *elastic* wherein price increases result in a greater than 1% decrease in consumption with a 1% increase in price and the slope is less than -1. Lastly, demand curves are plotted in log-log coordinates to facilitate visual inspection of the proportional changes in consumption and price (Hursh, Madden, Spiga, DeLeon, & Francisco, 2013).

Factors influencing elasticity. Several variables have been found to modulate demand elasticity, including economy type, the availability of substitutes and complements, the species of the consumer, and the type of commodity (Hursh, 1984). Economy type ranges along a continuum from open to closed (Hursh, 1984). In an open economy, an organism has some degree of access to the target commodity outside of the experimental or target environment. Access may be concurrent or delayed, such as post-session feeding to maintain the organism at 80% of free-feeding weight. In a closed economy, access to the commodity is restricted to the environment being studied and all reinforcers in the experimental setting are earned (i.e., contingent on behavior). All else being equal, research generally suggests demand is more elastic in an open economy than in a closed economy. Manipulations of economy type may have important applications outside of the laboratory (e.g., animal training, treatment of problem behavior). Johnson, Mawhinney, and Redmon (2001) suggest the effects of economy type may be an important variable of interest for future OBM research. For example, piece-rate pay systems in which employees earn a fixed monetary incentive for each pre-specified unit of work completed may closely approximate a closed economy. However, the economy type may not be exclusively closed as employees could still have access to outside sources of income such as that of a spouse or from a second job. A pay system in which compensation is relatively less dependent on actual work output (e.g., hourly or salaried pay) may be more analogous to an open economy. A reasonable postulation is that pay arrangements in which employees earn a proportion of pay through contingent incentives are effective because they shift the economy type towards the closed end of the continuum. Presently, however, the influence of economy type on behavior in organizations remains to be explored.

Another variable influencing demand elasticity that has implications for the workplace is the availability of other reinforcers. The relation between two reinforcers varies along a continuum between perfect substitutes and complementary reinforcers, with partial substitutes and independent reinforcers falling intermediary. Substitutable reinforcers are functionally similar stimuli that individuals will readily consume interchangeably. For example, a gift card may function as a partial substitute for a cash incentive of equal value. When substitutable reinforcers are available concurrently or delayed, consumption decreases and elasticity increases regardless of whether the alternative reinforcer is a perfect or partial substitute; although perfect substitutes influence consumption and elasticity of the target commodity to a greater extent than partial substitutes (Hursh, 1980). Independent reinforcers are functionally unrelated and do not influence elasticity. Complementary reinforcers are those that are typically consumed together. In contrast with substitutes, concurrently available complementary reinforcers increase demand. That is, the efficacy of a reinforcer decreases when the complement is unavailable or price is too high (e.g., music and headphones in a shared office space).

Linear elasticity model. Since the introduction of demand curve analyses to operant behavior, several equations have been proposed to quantify elasticity. In 1989, Hursh, Raslear, Bauman, and Black introduced the first widely used equation, the linear elasticity model of demand:

$$\ln Q = \ln L + b\ln P - aP \qquad \text{Equation 1}$$

where Q is the quantity consumed, L is the level of consumption when the commodity is free or a near-zero price (i.e., intensity), b is the slope of the demand curve after an imperceptibly small increase in price from a zero level price, P is price, and a is a coefficient. Though providing an indispensable first step towards a quantitative analysis of reinforcer efficacy, the linear equation relies on two parameters to model demand (i.e., a and b). If, as Hursh (1980) proposes, reinforcer efficacy is captured in the rate of change in elasticity as price increases, a model that incorporates multiple parameters obviates a single molar metric of reinforcer effectiveness.

Instead, Hursh et al. (1989) provided an equation to solve for elasticity at specific price points, a relatively more molecular measure of efficacy:

$$Elasticity = b - aP \qquad \text{Equation 2}$$

Equation 2 could also be used to derive the price at which consumption shifts from inelastic to elastic, by setting elasticity to the point of unit elasticity (i.e., -1), which Hursh et al. refer to as P_{max}. Although P_{max} is not a measure of overall elasticity, its identification allows researchers to classify demand at prices lower and higher than P_{max} as inelastic and elastic, respectively, which has value.

Exponential demand equation. In 2008, Hursh and Silberberg modified the demand equation to include a single quantitative measure of an organism's defense of consumption. The exponential model is as follows:

$$\log Q = \log Q_0 + k(\mathrm{e}^{-\alpha(Q_0 \cdot C)} - 1) \qquad \text{Equation 3}$$

where Q is consumption and C is cost. Similar to the L parameter in the linear elasticity model, Q_0 is equal to consumption when the price is free or near-free (i.e., intensity), and k is the scaling constant equal to the range of consumption in log units. Hursh and Silberberg recommend setting k to a common constant value to facilitate comparisons between or among commodities. Lastly, alpha (α) is a rate constant equal to the rate in change in elasticity across the entire range in prices. Alpha is inversely related to reinforcer efficacy; larger α values reflect steeper (i.e., more elastic) demand curves. Therefore, the equation allows for an evaluation of reinforcer efficacy using a single parameter. Additionally, by placing Q_0 and C in the exponent, the exponential equation controls for scalar differences of a reinforcer by standardizing the cost of obtaining baseline levels of reinforcer consumption (Q_0). Although other models have been suggested (e.g., Koffarnus, Franck, Stein, & Bickel, 2015), the exponential equation is generally accepted as the contemporary standard model of demand.

Several other indices of demand may be derived from α including essential value (*EV*), P_{max}, and O_{max}. Essential value is a quantitative index of the relative reinforcing efficacy of the commodity calculated using the following equation (Hursh, 2014):

$$EV = \frac{1}{100 \cdot \alpha \cdot k^{1.5}} \qquad \text{Equation 4}$$

Essential value may be a more intuitive metric because, unlike α, *EV* is directly related to reinforcer efficacy. That is, larger *EV*s indicate relatively greater reinforcing efficacy. P_{max} is also directly proportional to reinforcer efficacy and represents the point of unit elasticity, or the highest price participants will tolerate before the curve becomes elastic and consumption declines markedly. When derived using α, P_{max} is calculated using the following equation introduced by Hursh in 2014:

$$P_{max} = \frac{m}{(Q_0 \cdot \alpha \cdot k^{1.5})}\text{, where } m = 0.083k + 0.65. \qquad \text{Equation 5}$$

P_{max} is also the point at which expenditure or responding is greatest, referred to as O_{max}, which is equal to the product of P_{max} and predicted consumption at P_{max}. O_{max} is plotted on the work function (also referred to as an expenditure curve); a graphic depiction of how expenditure, rather than consumption, changes as a function of price. Expenditure is calculated by deriving the product of consumption and price. The general shape of the work function is that of an inverted U with expenditure increasing along the left limb until peak expenditure at O_{max}, followed by a decline along the right limb of the curve. Regardless of the equation used, research has consistently demonstrated a robust relation between consumption and price with a wide range of commodities and populations (Reed et al., 2015).

It is important to note that, in addition to the derived measures just described, P_{max} and O_{max} can be determined empirically (i.e., observed). This can be accomplished by creating an expenditure curve and finding the maximum output value (O_{max}) and its associated price (P_{max}). Obtaining these parameters empirically may have particular value because they can be determined without the need for nonlinear regression. Individuals wishing to use demand analyses for applied purposes in real world settings may not have the resources to purchase software or employ an individual capable of performing complex nonlinear regression techniques. Murphy and MacKillop (2006) have found a high correlation between empirical and derived measures for O_{max} and P_{max}. However, they suggest the empirical parameters are more reliable than their derived counterparts (MacKillop & Murphy, 2007). The latter statement was unfortunately based on unpublished data and it is not clear what factors may be compromising the reliability of derived metrics. Given the applied value of obtaining these measures without the need for nonlinear regression, continued evaluation of the relation between observed and derived demand parameters is a worthwhile endeavor.

Application of demand curve analyses. Although much of the behavioral economic demand research has been conducted in highly controlled laboratory settings (Hursh & Silberberg, 2008), demand analyses may have important practical utility in applied settings. For example, determining the degree to which responding is sensitive to price increases for a range of commodities may help practitioners select the reinforcer that is likely to produce persistent responding. In fact, researchers have effectively used demand curve analyses to inform complex issues of societal concern including assessments of the abuse liability of drugs (e.g., Hursh, Galuska, Winger, & Woods, 2005), responsiveness to treatment for addiction (Bickel, Johnson, Koffarnus, MacKillop, & Murphy, 2014), and the relative efficacy of therapeutic reinforcers

(Roane, Lerman, & Vorndran, 2001). The benefit of demand curve analyses to *inform* applied issues of social significance is rarely disputed. However, there have been noted concerns regarding the viability of *assessing* demand in applied settings (e.g., Hursh et al., 2013). Generating the data needed for a full demand curve requires exposing an organism to long sessions at several prices in which an organism may consume large quantities of a commodity. This experimental arrangement may have negative outcomes depending on the applied context, target problem, and commodity. For example, when working with individuals with intellectual or developmental disabilities, time spent assessing consumption may delay treatment or reduce time spent engaging in educational activities. Additionally, the extensive financial (e.g., cost of commodity or equipment) and time (e.g., staff to conduct sessions) resources necessary to carry out such a study further limits the feasibility. Researchers examining the effects of addictive and illicit substances have raised similar concerns (Jacobs & Bickel, 1999). Providing long-duration access to an illegal drug at a nominal price to individuals who abuse substances raises obvious ethical concerns.

Though the possible outcomes may not be as grave, similar logistical and ethical restrictions in the workplace may preclude a researcher's ability to generate demand curves in a real organization. First, as it is, OBM researchers often battle financial pressures from organizations. Researchers may experience considerable difficulty convincing organizational leaders that the information gained from a demand analysis justifies the needed resources to conduct the analysis. In addition, common workplace reinforcers may complicate demand curve analyses. For example, employees are unlikely to satiate even over long duration sessions (an important feature for analysis; Hursh et al., 2013) when the commodity of interest is a monetary incentive. It may also be difficult to measure repeated instances of consumption for a number of non-monetary incentives (e.g., access to a preferred parking spot). Importantly, to measure changes in consumption, researchers would need to systematically increase the work requirement necessary to access an incentive. Even if a researcher identified an organization in which labor laws, unions, or employee contracts permitted such manipulations of compensation, it is easy to imagine the employee reprisal this analysis might cause. Clearly, it is ethically questionable to impose such conditions on employees. The difficulty assessing demand in applied contexts—despite undeniable utility—does not obviate the need for demand analyses but instead raises methodological questions about effective, practical, and resource-efficient methods for making use of a model with wide-reaching benefits (Jacobs & Bickel, 1999).

Hypothetical purchase tasks. In light of the aforementioned ethical and practical constraints associated with experiential preparations, hypothetical purchase tasks (HPT) were proposed to efficiently assess reinforcer demand without the need for individuals to experience the outcomes (Jacobs & Bickel, 1999). Hypothetical purchase tasks are a simulation procedure for assessing demand in which researchers provide individuals with a vignette describing the commodity of interest and the context under which purchases and consumption are to take place. Participants then report an estimation of the amount of the commodity they would consume across increasing prices. Prices commonly range from free or near-free prices (demand intensity; Q_0) to prices that are sufficiently high to suppress an individual's demand for the commodity to zero, thus allowing for evaluation of changes in consumption across a broad array of prices. Additionally, the information described in the vignette can be used to control for extraneous factors (e.g., deprivation, income, economy type) providing researchers access to variables that are often difficult to manipulate or control in applied contexts.

In a seminal HPT study, Jacobs and Bickel (1999) evaluated self-report consumption patterns of heroin and cigarettes with 17 opioid-dependent participants. The contextual features in the vignette indicated that (a) there were no legal consequences for using heroin, (b) participants were not currently in treatment, (c) participants had no access to other drugs, and (d) any purchases had to be used by participants alone within a 24-hour period. The purchase tasks asked participants to report the number of bags of heroin or cigarettes they would hypothetically purchase across 15 prices ranging from \$0.01 to \$1,120. The authors found demand for heroin was more inelastic than demand for cigarettes. Consistent with previous human operant studies using real consequences, the demand curves were a positively decelerating function of price and were well accounted for by the linear elasticity model and later the exponential model of demand when Hursh and Silberberg (2008) reanalyzed the data. The method Jacobs and Bickel used provided valuable insight to purchasing and consumption patterns in a time- and cost-efficient manner. The finding that elasticity varied in expected ways—with higher demand for heroin given its greater abuse liability as compared to cigarettes—provides some initial, albeit limited, evidence of the validity of the HPT despite the hypothetical nature of the data. Similar results with HPTs have been obtained with other drug reinforcers including alcohol (e.g., Murphy & MacKillop, 2006), cocaine (e.g., Bruner & Johnson, 2014), and marijuana (e.g., Collins, Vincent, Yu, Liu, & Epstein, 2014). Hypothetical purchase tasks therefore provide insight to questions that may be difficult or even unanswerable using traditional operant techniques.

Although much of the research has focused on illicit and addictive substances, researchers are beginning to examine the utility of HPTs with non-drug reinforcers including snack foods (Epstein, Dearing, & Roba, 2010), chocolate (Chase, MacKillop, & Hogarth, 2013), fuel (Reed, Partington, Kaplan, Roma, & Hursh, 2014), and luxury goods/activities (Roma, Hursh, & Hudja, 2016). The numerous commodities and experimental questions that can been evaluated make HPTs a versatile methodology worthy of continued research interest. In addition to mediating the above-referenced restrictions, purchase task methodology has allowed for extensions and adaptations of demand preparations, thereby allowing researchers to assess a wider range of reinforcers and contextual factors. For example, Roma et al. evaluated commodities, such as refrigerators, that are typically only consumed in small and/or infrequent quantities. Rather than asking participants to report the number of refrigerators they would purchase across a range of prices, as had been done in previous purchase task preparations, Roma and colleagues asked participants to report *probability* of purchase, which extends the literature by providing a measure that may be more representative of a typical purchasing situation for certain commodities.

Critics have argued that HPT responses are self-reports of consumption and may not reflect behavior that would be observed in an actual situation. A growing body of literature has sought to evaluate the reliability and validity of purchase task methodology as a result of this criticism. In one example, Madden and Kalman (2010) asked 60 smokers to complete two purchase tasks, one during intake and one completed after one week of treatment. Treatment consisted of one counseling session and one week of taking either Bupropion or a placebo. The HPT asked participants to report the number of cigarettes they would purchase for themselves alone and smoke each day at 26 prices that ranged from $0 to $1,120 per cigarette. Participants returned after 10 weeks of treatment to evaluate their smoking status. Despite no significant differences in elasticity at intake between those who did and did not quit smoking, Madden and Kalman found that changes in elasticity between intake and one week of treatment predicted treatment success, regardless of whether participants received Bupropion or the placebo. That is, individuals with a greater increase in elasticity (i.e., less demand) from intake to one week of treatment were significantly more likely to successfully quit smoking at a 10-week follow-up. The results of Madden and Kalman demonstrate the predictive validity of purchase tasks. Researchers have demonstrated other psychometric properties of HPTs including construct validity (e.g., MacKillop et al., 2010), concurrent validity (e.g., Murphy & MacKillop, 2006), convergent validity (e.g., MacKillop et al. 2008), divergent validity (e.g., Murphy, MacKillop, Tidey, Brazil, & Colby, 2011), test-retest reliability (e.g., Murphy, MacKillop, Skidmore, & Pederson, 2009), and inter-method reliability (Reed, Kaplan, Roma, & Hursh, 2014).

Other procedures for evaluating HPTs have examined the correspondence between responding on a purchase task and observed behavior (e.g., Amlung, Acker, Stojek, Murphy, & MacKillop, 2012). In one of the most comprehensive measures of HPT validity to date, Amlung and MacKillop (2015) evaluated the correspondence between self-report responses on a purchase task with actual consumption of alcohol during a laboratory self-administration period with 19 heavy drinkers. The alcohol purchase task indicated participants (1) had \$15 to purchase "mini" alcoholic beverages that were approximately half the volume of a standard drink, (2) could purchase a maximum of eight mini-drinks of their typical alcoholic beverage, and (3) could keep any money not spent on alcohol. Participants then reported their estimated consumption across 22 prices ranging from \$0.01 to \$15 per drink presented in a random order. Participants completed two versions of the alcohol purchase task, hypothetical and incentivized. In the hypothetical condition, the instructions indicated participants would not receive any alcohol or money from their choices. The incentivized purchase task informed participants that the experimenter would randomly select one of their choices and provide participants with the quantity indicated on the purchase task during a self-administration period. Amlung and

MacKillop found a significant positive correlation between participant responses on the hypothetical and incentivized purchase tasks. Additionally, participants who received alcohol during the self-administration period consumed an average of 89% of provided alcohol—a significant positive correlation—suggesting self-report responses on the purchase task have strong correspondence with observed consumption.

However, the $15 budget and maximum of eight mini-drinks is a noted limitation of Amlung and MacKillop (2015). For the $2, $3, $4 to $5, and $6 to $7 price points, the maximum possible drinks participants could purchase were seven, five, three, and two, respectively. At the eight prices lower than $2 and higher than $7, participants could purchase a maximum of eight and one drink(s), respectively. Providing a budget and capping consumption effectively restricted the range of drinks participants could purchase and consume at each price, thereby artificially forcing decreases in consumption with increases in price. Forced decreases in consumption resulted in elastic demand that may not represent participants' actual demand for alcohol if purchases were not restricted. This limitation underscores the practical and ethical constraints researchers face when examining demand for addictive substances, which highlights the need for purchase tasks to facilitate continued experimental investigation of this important area of study. Regardless, the collective literature suggests HPT methodology is a promising method for evaluating demand. The increased feasibility of assessing demand afforded by purchase task methodology has opened the possibility for evaluating the utility of demand curve analyses in other settings, including organizations.

Demand and OBM. Demand curve analyses applied to the organization may help identify the extent to which employees will work to obtain a given incentive as well as the highest price (or work requirement) employees are willing to complete to access that incentive. This application of demand would help inform durable resource-efficient interventions capable of maintaining high levels of responding. Purchase task methodology may be especially well suited for examining reinforcer demand in the workplace to circumvent issues that may arise when assessing demand in applied contexts or undifferentiated responding in contrived laboratory workplaces. Despite recent calls from researchers in *both* OBM and behavioral economics (Hursh & Roma, 2016; Roma, Reed, DiGennaro Reed, & Hursh, 2017; Jarmolowicz, Reed, DiGennaro Reed, & Bickel, 2015; Johnson et al., 2001; Roma et al., 2016; Sigurdsson et al., 2013; Wine, Gilroy, & Hantula, 2012), the generality of behavioral economic methodology and demand curve analyses to the organizational setting remains largely unexamined. This translation can lead to a greater understanding of behavior and motivation in the workplace and offer a unique approach for evaluating the dimensions of reinforcement to inform incentive delivery and ultimately address workplace challenges experienced by many organizations. Although a relatively new line of inquiry, several studies have begun to examine evaluations of reinforcer dimensions by investigating the applicability of demand curve analyses to work-related behavior.

One of the first experiments to apply behavioral economic demand curve analyses to OBM evaluated the applicability of purchase task methodology to work-related behavior and workplace reinforcers. In a two-part study, Henley, DiGennaro Reed, Kaplan, and Reed (2016) used a hypothetical work task (HWT) with undergraduate participants to assess the effects of experience on responding and the effects of delay of monetary compensation on demand. The HWT, a variation of the hypothetical purchase task adapted for work behavior, described a job in which students were hired to pass out flyers on campus for 1 hr in exchange for $10. Similar to Roma and colleagues (2016), participants reported the likelihood they would distribute a given number of flyers across an increasing work requirement (i.e., number of flyers passed out to earn pay). The first study compared responding of individuals with and without actual work experience distributing flyers to evaluate the influence of a pre-experimental history of engaging in the behavior-for-hire on responding. Henley, DiGennaro Reed, Kaplan et al. found no difference in demand at the group or the individual level suggesting individuals without formal experience completing the work task responded comparably despite not having directly contacted the contingencies under investigation.

In the second study, participants completed two variations of the HWT that differed only in the duration to payment. The short delay condition indicated participants would receive payment immediately after working and the long delay condition indicated payment would be delayed by four weeks. Henley, DiGennaro Reed, Kaplan et al. (2016) found a statistically significant difference in demand at the group and individual levels, with higher demand for the short delay condition. At the group level, the difference in the amount of work completed at P_{max} was equal to a 45% increase in the number of flyers participants would be willing to distribute when compensation was delayed by 1 hr as compared to 4 weeks. In both studies, distribution likelihood was a positively decelerating function of the work requirement and the exponential demand equation provided an excellent fit to the data (minimum R^2 values of .94). Adequacy of the model fit in combination with the finding that contextual variables in the HWT vignette (i.e., delay) modulated participant responding in ways that are consistent with previous research on delay of reinforcement (e.g., Grace, Schwendiman, & Nevin, 1998; Hursh & Fantino, 1973; Woolverton & Anderson, 2006) suggests that demand analyses using hypothetical procedures have merit in evaluations of reinforcer efficacy in the workplace.

Henley, DiGennaro Reed, Kaplan et al. (2016) discuss several areas in which demand analyses may have utility in OBM. For example, they note that the HWT could serve as a novel method for assessing employee preference for incentives. Moreover, P_{max} may help organizational leaders identify appropriate employee goals and work requirements because it specifies the highest work requirement that employees will tolerate before performance rapidly declines. The external validity of these findings is limited, however, because participants were not employees and did not experience the outcomes of the study. Therefore, questions regarding the correspondence of responding on the HWT to actual behavior remain unanswered. Because of the difficulties assessing demand for workplace reinforcers in applied contexts and arranging laboratory settings that simulate complexities of the workplace (Oah & Lee, 2011), it may be necessary to identify an alternative setting that allows for the manipulation of reinforcers and better simulates the organizational environment to examine the predictive validity of the HWT.

In an attempt to identify a novel solution for conducting OBM research, Henley, DiGennaro Reed, Reed, and Kaplan (2016) used Amazon Mechanical Turk (www.mTurk.com) to extend their evaluation of demand curve analyses to work-related behavior. Amazon Mechanical Turk (mTurk) is an online crowdsourcing site in which requesters post brief computer-mediated tasks (referred to as Human Intelligence Tasks or HITs) to the online platform for individuals (referred to as Workers) to complete in exchange for monetary compensation. Several features render mTurk propitious for OBM research. First, because "employers" are requesters posting HITs, the requester (i.e., researcher) functionally serves as the employer. Therefore, researchers do not need to compromise experimental control as a result of financial pressures from an organization to improve performance or concerns about how manipulations may influence productivity and profit. Amazon Mechanical Turk has fewer restrictions with respect to manipulating pay that may allow researchers to answer questions that may be unethical in the workplace when adopting a demand assay. This flexibility is possible in part because participant's livelihood is not dependent on a single work task, Workers choose which HITs to complete and can discontinue at any time to begin another paid HIT in a matter of seconds; in the workplace employees cannot easily change jobs without considerable burden (e.g., searching and interviewing). Next, Workers complete HITs in the "natural" environment. Regardless of location—mTurk Workers can complete HITs while sitting in a coffee shop, in a home office, or in an airport terminal—there are undoubtedly alternative sources of reinforcement that compete with performance on the work task similar to traditional work environments. Finally, the several hundred thousand mTurk Workers boast unprecedented geographic and demographic diversity when compared to typical university-based populations used in laboratory OBM research (Paolacci & Chandler, 2014). Presumably, mTurk Workers have longer and more varied work histories and may be more representative of the American workforce. These collective attributes may provide a beneficial balance between the pros and cons of applied and laboratory OBM research settings and render mTurk well-suited for OBM research, a hypothesis Henley, DiGennaro Reed, Reed et al. (2016) explored.

To this end, Henley, DiGennaro Reed, Reed, et al. (2016) used a novel experiential procedure to evaluate the effects of two incentive magnitudes on performance using a between groups design. To accomplish this aim, the authors integrated a human operant preparation, in which participants expended effort in exchange for monetary incentives, into a computerized format for use with mTurk Workers. The simulated work consisted of a match to sample task. Participants were asked if they would like to complete a given work requirement in exchange for a monetary incentive across a progressively increasing response requirement ranging from one to 256 work tasks. Participants then completed the ratio if they selected yes, or exited the survey if they selected no. Completion of each ratio resulted in a fixed monetary incentive in which half of the participants had the opportunity to earn \$0.05 per ratio requirement completed and the other half could earn \$0.10 for each ratio completed. This method differed from traditional demand analyses in several ways. First, responding at each price was dichotomous. Participants either did or did not complete the ratio, as compared to measuring the quantity consumed or probability of consumption at each price. Additionally, participants experienced each ratio requirement just once. Third, because these experimental features precluded demand curve analyses of individual performance, the authors translated a market percent consumption approach (Greenwald & Hursh, 2006) and the Kaplan-Meier estimate of survival (Kaplan & Meier, 1958) to convert individual dichotomous choices at each price into a group level demand curve. Henley, DiGennaro Reed, Reed et al. found a significant difference in elasticity with greater inelasticity in the smaller magnitude. Although participants completed slightly more work in the \$0.10 condition, participants in the \$0.05 condition completed more work per penny, suggesting the smaller magnitude was a more efficient use of resources.

The results of Henley, DiGennaro Reed, Reed et al. (2016) demonstrate responding is sensitive to incentive amount, revealing that magnitude is an important reinforcer dimension deserving of consideration for effective incentive delivery for which no guidelines currently exist. Additionally, mTurk proved to be a promising platform for OBM research. Henley, DiGennaro Reed, Reed et al.'s experimental preparation provides a possible method for evaluating the correspondence between self-report responses on a HWT and actual behavior, an important research area noted in previous work by these authors (Henley, DiGennaro Reed, Kaplan et al., 2016). Two limitations are worthy of note. First, the authors examined a limited range of incentive magnitudes. Next, Henley, DiGennaro Reed, Reed et al. only evaluated the quantity of work participants completed. Many work settings rely on several important performance indices, such as accuracy. Because the simulated work task required participants to respond correctly to progress through the ratio requirements the extent to which reinforcer magnitude influences other performance indices is unknown.

Purpose

A greater understanding of the relative efficacy of reinforcer dimensions and how parametric manipulations of their values differentially influence performance could lead to effective strategies for predicting and managing employee behavior. Moreover, a method for quantifying the relation between reinforcer dimensions and performance may provide greater precision to guide behavior change strategies. Therefore, the present studies served to extend the prevailing literature applying behavioral economic demand curve analyses in two ways. First, these studies evaluated the effects of parametric manipulations of reinforcer dimensions on performance. Second, they evaluated the predictive validity of the HWT. These aims were investigated using a crowdsourced human operant procedure adapted from Henley, DiGennaro Reed, Reed et al. (2016). Experiment 1 replicated and extended Henley, DiGennaro Reed, Reed et al.'s findings by examining the effects of three incentive magnitudes ($0.05, $0.10, and $0.20) and incorporating a HWT to evaluate the correspondence between self-report responses on the HWT and observed responding on the experiential procedure. Using a similar experimental preparation, Experiments 2 and 3 examined the effects of three parametric values of reinforcer probability (90%, 50%, and 10% probability of earning incentives) and delay (incentive receipt following 1, 14, and 28 days). Additionally, all three experiments employed an adapted version of the simulated work task to allow for evaluations of the accuracy with which participants completed the task.

Experiment 1: Reinforcer Magnitude

Method

Participants. To recruit participants, I posted three HITs to the mTurk platform containing inclusion criteria, a brief description of the study, compensation amount, instructions for receiving compensation, and a link to the study. The HITs, featured in Appendix B, contained identical study descriptions but each HIT presented a unique survey URL corresponding to one of three conditions assessed. I required Workers to meet four criteria for inclusion: (1) reside in the United States, (2) have greater than 1,000 previously approved HITs, (3) have a 95% or above approval rate on completed HITs, and (4) submit a unique access code within 3 hr of selecting the survey link. The HIT informed Workers they would receive \$0.25 for participation and involved an academic survey about monetary rewards, which was created using Qualtrics Research Suite® (http://www.qualtrics.com). Note "survey" in this context refers to the various activities comprising the experimental preparation hosted by an online survey software program. Lastly, the HIT instructed Workers to enter a unique access code into the textbox below the survey link following survey completion to receive compensation. The survey generated and displayed the unique 17-character alpha-numeric access code. After selecting the link, the survey presented Workers with an information statement. Workers who agreed to participate and submitted an access code to the HIT within 3 hr served as participants.

A total of 324 unique access codes were submitted. For Workers who completed the study more than once, I retained the first attempt completed by a participant. As a result, I excluded 35 responses (\$0.05 condition = 6; \$0.10 condition = 8; \$0.20 condition = 21); 289 participants remained and served as participants. Participant ages ranged between 20 and 72 (M = 36, SD = 11.6) and were primarily Caucasian (n = 243, 84.1%). Participants indicated living in

43 of the 52 states and territories with the highest concentration of participants residing in California (*n* = 26, 9.0%), Pennsylvania (*n* = 25, 8.7%), and Texas (*n* = 23, 8.0%). Half of the participant sample was female (*n* = 146, 50.5%). Participants reported a wide range of education and income levels; a four-year college degree (*n* = 115, 38.9%) and an annual household income of less than $30,000 (*n* = 90, 31.1%) had the highest frequency. For participants who indicated having a disability, a physical disability was reported with the highest frequency (*n* = 18, 6.2%). When asked to indicate if they are currently or have ever been a smoker, more than half of participants reported "No – Never" (*n* = 158, 54.7%) and 25% of participants indicated "Yes – Previously." Table 1 summarizes participant demographic variables, separated by condition, in more detail.

Simulated work task. The present study adapted the simulated work task used by Henley, DiGennaro Reed, Reed et al. (2016), illustrated in Appendix C. Specifically, the work task contained a visual analog scale that displayed a number line ranging from -100 to 100. The scale contained 11 evenly spaced labels designating the numerical values along the scale. The manipulandum default start position was always zero (i.e., centered on the scale). For each work task, the survey displayed a *target value* on the far-left side of the scale. As participants moved the manipulandum, the survey displayed a second value located to the far right of the scale, opposite the target value. This second value represented the integer at which the manipulandum was located on the scale. Participants were instructed to move a manipulandum along the scale to match the value on the right side of the visual analog scale with the target value on the left. The work task was referred to as a "slider question" in the instructions to participants. A single slider question is hereafter referred to as a work unit.

The work task also included a progressive ratio (PR) schedule in which the ratio requirement increased after the delivery of each reinforcer (Hodos, 1961). The PR schedule contained the following 15 ratio requirements: 1, 2, 4, 8, 10, 12, 15, 20, 25, 32, 48, 64, 96, 128, and 256 work units presented in an ascending order. To select target values, I first generated a list of random values for each ratio requirement using Microsoft Excel's (2013) random between function, which populates selected cells with random integers between a specified upper and lower boundary, in this case between -100 and 100. Next, I removed target values equal to the manipulandum start point (i.e., zero). The remaining target values were adjusted such that for each ratio requirement, the target values equaled zero when summed to equate effort across ratios. This summation was not possible for the first ratio requirement or the practice trial, each of which contained only one work unit. As a result, the first ratio requirement and practice trial values equaled zero when summed. Therefore, target values were selected in a pseudorandom fashion.

The survey required participants to provide a response for each work unit in the ratio. If participants selected the "next" button without moving the manipulandum from the start point, the survey displayed the error message, "Please answer this question" in red text above the unanswered work unit. However, the survey did not require responses to be accurate. That is, participants could continue to the next work unit as long as they provided a response, regardless of the correspondence between the response and target value.

Experimental conditions. Using a between-groups design, I evaluated the effects of three incentive magnitudes on performance. The incentive magnitude conditions included \$0.05 (M_{05}, n = 99), \$0.10 (M_{10}, n = 95), and \$0.20 (M_{20}, n = 95).

Procedure. The survey contained six sections presented in the following order: (1) 27-item monetary choice questionnaire (MCQ; Kirby, Petry, & Bickel, 1999), (2) behavioral inhibition system/behavioral activation system scales (BIS/BAS; Carver & White, 1994), (3) demographic questionnaire, (4) practice trial, (5) hypothetical work task (HWT), and (6) experiential work task (EWT).

Monetary choice questionnaire. After reviewing an information statement, participants completed the 27-item MCQ (Kirby et al., 1999; Appendix D). The MCQ is a method for assessing rates of delay discounting with demonstrated test-retest reliability (Intraclass correlation coefficient [ICC] = .56) and internal consistency scores (Kuder Richardson 20 [KR-20] = .94 to .96) in previous research (Black & Rosen, 2011). The purpose of the MCQ was to provide preliminary information evaluating the relation between discounting and more traditional performance measures (work output). For each question, participants were asked which of two outcomes they would rather have: a smaller immediate reward or a larger delayed reward. For example, "Would you rather have *$11 tonight* or *$30 in 7 days*?" All 27 questions were displayed on a single screen.

Behavioral inhibition system/behavioral activation system. The BIS/BAS scale evaluates the extent to which behavior is motivated by achieving positive consequences or by avoidance of loss (Carver & White, 1994). Carver and White demonstrate acceptable test-retest reliability and validity of the BIS/BAS. Alpha reliabilities range from .73 to .76 for the BIS, BAS-RR, and BAS-D. Reliability for the BAS-FS is lower (α = .66). The BIS/BAS was included to evaluate its relation to objective performance measures. The BIS/BAS contains 24 items and is comprised of four subscales listed in Appendix E. The BIS scale asks participants to rate the degree to which they perceive their behavior as being governed by or sensitive to punishment (seven items). The BAS comprises the remaining three subscales. The drive (BAS-D; four items), fun seeking (BAS-FS; four items), and reward responsiveness (BAS-RR; five items) subscales assess the extent to which participants report (1) they engage in behavior to access a reinforcer, (2) their behavior is influenced by novel reinforcing stimuli and events, and (3) their behavior is sensitive to reinforcement, respectively. The survey also includes four filler questions. For each item, participants read a statement describing behavior and indicated the degree to which the statement is representative of their behavior on a four-point Likert scale (*strongly disagree*, *disagree*, *agree*, *strongly agree*).

Demographic questionnaire. Next, participants completed an eight-item demographic questionnaire (Appendix F). The demographic information included: (1) age, (2) gender, (3) ethnicity, (4), state or territory of residence, (5) highest level of education completed, (6) combined annual household income, (7) disability status, and (8) smoking status.

Practice trial. During the practice trial, participants read instructions describing how to perform the simulated work task (Appendix G). The survey then provided participants with the opportunity to complete one work unit. The practice trial was included so all participants had an understanding of and experience with the simulated work before completing the HWT and EWT.

Hypothetical work task. After completing the practice trial, the survey presented participants with instructions for the HWT (Appendix G). The purpose of the HWT was to assess the amount of work participants reported they would hypothetically complete in exchange for monetary incentives. The HWT was also used to evaluate the correspondence between self-reported and observed performance on the EWT. The instructions informed participants that on the following page they would be asked to indicate their willingness to complete a given number of work units to receive an incentive if provided the opportunity. The term “bonus” rather than incentive was used throughout the survey to remain consistent with mTurk terminology when communicating with participants. However, I will use the term incentive here to remain consistent with the OBM literature. The instructions also asked participants to answer honestly and thoughtfully as if they were in the situation. After selecting the next button, the survey presented the first ratio requirement (i.e., one work unit). For each ratio in the progression, participants were asked, “Would you complete XX slider question(s) in exchange for a YY cent bonus?” with XX corresponding to each of the ratio requirements in the PR schedule. The value of YY varied and was equal to the incentive magnitude condition to which the participant was assigned ($0.05, $0.10, or $0.20). Each question contained a dichotomous *yes*/*no* response option. Participants could select *yes,* indicating they would be willing to complete the ratio, or *no,* they would not complete the ratio if given the opportunity. Participants were unable to continue until they selected a response option. The survey displayed the following error message, “please answer this question” above any question for which a participant failed to provide a response. If the participant selected *yes*, the survey presented the same question for the subsequent ratio. The survey continued to present increasing ratio requirements in accordance with the PR schedule until the participant selected *no*, at which point the HWT ended. Each ratio requirement was presented on a separate page. Upon completion of the HWT, the survey informed participants they would have the opportunity to earn real incentives in the following section.

Experiential work task. Next, participants completed the EWT. The purpose of the EWT was to assess the highest ratio requirement participants complete when given the opportunity to earn real incentives. After reviewing the instructions (Appendix G), the survey presented the first ratio requirement in the PR progression. Each ratio began with a question reading, “You can earn a bonus of XX cents by completing YY question(s) in this section. Would you like to continue?" with the value of XX corresponding to the incentive magnitude condition and YY the ratio requirement. If the participant selected *yes*, the survey displayed the corresponding number of work units, with each work unit presented on a separate page. Each work unit was labeled with the number in the progression. For example, in a ratio containing 20 work units the fifth unit was labeled, "Question 5 of 20." After completing a ratio, the survey presented the same question for the subsequent ratio in the progression. This sequence continued until the participant selected *no*, at which point the survey ended.

Payment. Participants earned a base pay of \$0.25 for completing the MCQ, BIS/BAS, practice trial, demographic survey, and HWT. On average, participants required 5 min to complete these tasks. Average completion time for the entire battery, including the EWT, was 14 min and 32 s. Participants in the M_{05}, M_{10}, and M_{20} conditions earned an average of \$0.47 (*SD* = 0.13), \$0.74 (*SD* = 0.28), \$1.38 (*SD* = 0.65), respectively. Incentives were delivered 1 to 3 days following participation.

Data analyses. Before performing subsequent analyses, I first examined whether participant demographic variables (with the exception of state of residence), BIS/BAS subscale scores, and delay discounting rates were evenly distributed among the three incentive magnitude conditions using a series of Chi-Square and one-way Analysis of Variance (ANOVA) tests. To evaluate the influence of incentive magnitude on participant responding, I examined several performance parameters including: highest ratio requirement completed (i.e., breakpoint), accuracy of work unit completion, and group-level elasticity. Additionally, I evaluated the predictive validity of the HWT. Unless otherwise indicated, all statistical analyses were two-tailed.

Breakpoint. I first obtained a breakpoint for all participants in the HWT and EWT for use in subsequent analyses. Breakpoint was defined as the highest ratio requirement participants indicated willingness to complete in the HWT or completed in the EWT. Any participant whose responding did not break during the HWT or EWT was assigned a value of 256, the highest ratio requirement assessed. Responding for one participant in the HWT for both the M_{05} and M_{10} conditions and for one participant in the EWT for the M_{20} condition did not reach a breakpoint; all three were assigned a breakpoint value of 256.

Based on results of a D'Agostino and Pearson omnibus normality test, the distribution of breakpoint deviated from normality for all conditions in the HWT and EWT. Therefore, I used a Kruskal-Wallis one-way ANOVA to examine the effects of incentive magnitude on breakpoint for the HWT and EWT. To determine for which conditions breakpoint differed significantly, I used a Dunn's multiple comparison test. Additionally, to evaluate within-subject differences in breakpoint between the HWT and EWT, I conducted a Wilcoxon matched pairs signed-rank test for each incentive magnitude.

Unit price breakpoint. I also performed similar analyses as those listed above using a transformation of breakpoint—referred to as unit price breakpoint—to equate the costs and benefits across conditions. Specifically, for analyses involving comparisons of HWT or EWT breakpoint *among* incentive magnitudes, I divided participants' breakpoint by the incentive magnitude to facilitate evaluations of the amount of work performed per unit of compensation earned. Unit price breakpoint therefore represents the number of work units completed per penny. For example, a participant with a breakpoint of 25 in the M_{10} condition would have a unit price breakpoint of 2.5 work units to earn \$0.01. Based on results of a D'Agostino and Pearson omnibus normality test, none of the conditions were normally distributed for the HWT or EWT.

Therefore, I used a Kruskal-Wallis one-way ANOVA to examine the effects of incentive magnitude on unit price breakpoint for the HWT and EWT with a Dunn's multiple comparison test was used to evaluate which conditions differed significantly.

Monetary choice questionnaire. For all conditions, participants' rates of delay discounting (i.e., *k*) were derived from their responses on the MCQ by entering their selections into a freely available Microsoft Excel-based tool (Kaplan, Lemley, Reed, & Jarmolowicz, 2014). This tool follows the standard calculation procedures outlined by Kirby et al. (1999; presented in Appendix H). Specifically, the 27 MCQ items are assigned one of nine possible *k* values and re-arranged in an ascending order based on *k*, thereby creating nine groups of three questions. The three questions in each group include a small, medium, and large magnitude reward. The *k* value assigned to each group is equal to the discounting rate that would be derived given indifference between the smaller-sooner and larger-later options presented in the question. That is, an individual with a *k* of .25 would select to receive $20 tonight and $55 in 7 days with equal frequency. Therefore, *k* can be estimated based on an individual's response pattern such that for a given question, selecting the smaller-sooner reward suggests a *k* greater than the value associated with that question, and selecting the larger-later reward suggests a smaller *k* value. Although participants' exact indifference points are not obtained with the MCQ, we can presume the value of *k* is somewhere between the values at which participants switch from selecting the larger-later response to the smaller-sooner response. This value is estimated by calculating the geometric mean of the *k* values associated with the two responses at which the switch occurred. To identify unsystematic responding, I evaluated the consistency score provided by Kaplan et al.'s tool for all participants. The consistency score is a relative measure of the overall percentage of an individual's selections that are consistent with his or her *k* value. Consistency is calculated by summing the number of larger-later responses before and smaller-sooner responses after the switch, and dividing by 27. I selected a minimum consistency score of 75%. Consistency scores for two participants in the M_{05} condition and two participants in the M_{20} condition fell below this criterion and were excluded from MCQ analyses. To evaluate the relation between rates of delay discounting and quantity of work completion, I conducted a Spearman rank-order correlation between *k* values and EWT breakpoint. A non-parametric test was selected because a D'Agostino and Pearson omnibus normality test indicated EWT breakpoint was non-normally distributed.

Behavioral inhibition system/behavioral activation system. For each item on the BIS/BAS, apart from two items that were reverse-scored, responses were assigned a value of one, two, three, or four, for *strongly disagree*, *disagree*, *agree*, and *strongly agree*, respectively. I then summed participant responses for each item related to the four subscales. I conducted separate Spearman rank-order correlations to evaluate the relation between participant scores on the four BIS/BAS subscales (BIS, BAS-D, BAS-FS, and BAS-RR) and EWT breakpoint.

Accuracy. Next, I evaluated differences in accuracy of work completion among conditions in the EWT. An inaccurate response was defined as any response value that did not match the target value. Because ratio requirements differed in the number of opportunities for inaccurate responses, I calculated percentage accuracy to compare relative differences in accuracy across ratio requirements. To calculate percentage accuracy, I multiplied the ratio value by the number of participants who completed the ratio to obtain the total number of opportunities. I then summed the number of inaccurate responses for a given ratio and subtracted this value by total opportunities to derive the number of correctly completed work units. Lastly, I divided correctly completed work units by total opportunities and multiplied by 100 to obtain a percentage [(Ratio value × Number of participants) – Inaccurate responses ÷ (Ratio Value × Number of participants) × 100]. Differences in accuracy among incentive magnitude conditions were determined using visual inspection of percentage accuracy across ratio requirements.

Analog to Demand. The present experimental preparation differs from those typically used in behavioral economic demand analyses. Specifically, responding at each price was binary (i.e., participants indicated either *yes* or *no*) as compared to measuring the quantity consumed, or more recently the likelihood of consumption, an element that may be a fundamental feature of demand (Henley, DiGennaro Reed, Reed et al., 2016). Additionally, participants in the present study only experienced the price sequence once. Although not frequently used, researchers have measured the percentage of participants that consume a commodity when presented with a dichotomous choice, referred to as market-percent consumption (e.g., Greenwald & Hursh, 2006). Experiencing a dichotomous choice once at each PR value renders individual data across ratio requirements more closely analogous to data used in survival curve analyses than demand, and thereby preclude an analysis of elasticity at the individual level. Survival curve analyses have been used successfully in the workplace (e.g., Lane & Andrew, 1955; Moncrief et al., 1989); however, they lack the benefits and application utility afforded by the quantitative metrics derived from measures of elasticity (e.g., *EV*).

Henley, DiGennaro Reed, Reed et al. (2016) used a novel adaptation of the Kaplan and Meier (1958) estimate of survival and the exponential equation to derive a group-level demand curve. Given the noted deviations from traditional demand curve analyses, Henley, DiGennaro Reed, Reed et al. refer to this measure as an *analog to* demand (*AtD*), which is similarly appropriate in the present study. Therefore, to compare differences in elasticity among incentive magnitude conditions and between the hypothetical and experiential work tasks, I adopted the methods used by Henley, DiGennaro Reed, Reed et al. to convert participant's binary choices at each price into a group-level demand curve, thus yielding an analysis of the elasticity of *workforce* responding across successive ratio requirements.

To obtain values for use in fitting the exponential demand equation, I first calculated the percentage of participants indicating willingness to complete (HWT) or for whom I observed completion (EWT) at each ratio requirement (i.e., market-percent consumption). I used two methods in fitting the percentage of participants to the data. First, I fit Hursh and Silberberg's (2008) exponential model of demand (Equation 3) to the percentage of participants using the ratio requirement as price. Additionally, given the differences in incentive size, I divided the ratio requirement by the incentive amount associated with each condition (i.e., \$0.05, \$0.10, or \$0.20) to establish a common price across conditions (i.e., unit price). I then used the unit price values in fitting the percentage of participants to the exponential demand model. For all demand analyses, k was set to a shared constant value of 2, Q_0 was set to 100 (i.e., maximum possible consumption, 100% of participants), and α was left unconstrained when fitting Equation 3 to the data for all conditions. A freely available tool (Exponential Model of Demand template; www.ibrinc.org) was used with GraphPad Prism version 7.01 for Windows (GraphPad Software, La Jolla, California, USA; www.graphpad.com) to fit the demand equation to the data. Three separate Extra sum-of-squares F-tests were used to evaluate whether differences in α differed significantly between each condition when compared with every other condition. Additionally, an Extra sum-of-squares F-test was used to compare elasticity between the HWT and EWT for all three conditions.

I used Kaplan and Reed's (2014) Microsoft Excel-based tool to obtain derived calculations of EV (Equation 4) and P_{max} (Equation 5). However, because consumption was measured in units that differed from traditional demand analyses in the present experimental preparation—percentage of participants responding rather than quantity—O_{max} must be interpreted with caution. When consumption is measured as the percentage of individuals, the traditional equation for O_{max} (consumption at $P_{max} \times P_{max}$) represents peak response output per 100 individuals because percentage is a relative measure that standardizes quantity out of 100 and is therefore unaffected by sample size. In other words, O_{max} is equal to the derived value for a sample of 100 individuals even if, for example, the actual sample size is 10 or even 200 individuals. There are undoubtedly situations in which standardized O_{max} is a desired and useful metric, for instance, when comparing reinforcer efficacy in terms of peak response output with groups of unequal size. Nevertheless, because the traditional calculation for O_{max} reflects response output when the sample size is 100, with less than 100 participants per condition in the present study, O_{max} is not an accurate reflection of peak responding.

Therefore, I used a modified equation to calculate derived O_{max}. Like the traditional method, I first obtained consumption at P_{max} by inputting the derived P_{max} value into the exponent (C) of Equation 3 and solving for Q. After transforming consumption at P_{max} into a proportion by dividing by 100, I multiplied this value by the sample size to obtain the number of participants responding at P_{max}. To avoid irrational numbers, I then rounded the number of participants responding at P_{max} down to the nearest integer. Lastly, I multiplied the number of individuals responding by P_{max}. Although deviating from the traditional calculation, this modified O_{max} equation quantifies the number of work units completed at P_{max}.

In addition to the derived measures, I generated empirical values for P_{max} and O_{max} using observed responding on the HWT and EWT as opposed to estimates of consumption generated via nonlinear regression. Therefore, to obtain empirical O_{max}, I plotted a work function and identified the point at which response output was the greatest. I then obtained empirical P_{max} by identifying the ratio requirement (price) at which peak responding occurred.

Researchers often report R^2 when conducting demand curve analyses as a measure of how well the best-fit curve generated by the exponential demand equation account for the data (i.e., goodness of fit; Motulsky, 2013). Although researchers have speculated about the adequacy of R^2 for a given model (e.g., Shull, 1991)—including its validity with nonlinear models (Johnson & Bickel, 2008)—this metric provides some initial indication of how well the model describes the data. However, R^2 does not provide information on whether the model is *appropriate*. Motulsky and Cristopoulos (2006) suggest a better indication of a model's appropriateness is whether the data are randomly distributed around the best-fit curve. It is possible for a curve to deviate in small but systematic ways from the data that are not captured by R^2. For example, data points could be consistently further from the curve at low prices than at high prices or clustered above or below the best-fit line. Careful inspection of residual plots of the *AtD* curves may be especially important for the current experiment given the novelty of this research and the ways in which the preparation deviate from more traditional demand curve analyses mentioned previously. Therefore, in addition to reporting R^2, I also used visual inspection of residual plots for *AtD* curves in the HWT and EWT to examine the distribution and trends present in the residuals. This analysis will provide information about whether best-fit curves systematically deviate from the data.

Predictive validity. Two measures were used to evaluate the predictive validity of the HWT. First, I conducted a series of Spearman rank-order correlations to assess the relation between responding on the HWT and EWT for each incentive magnitude condition. Second, I calculated the percentage change in predicted work output (and corresponding cost) based on

HWT responses to observed output and cost in the EWT. This analysis was done by subtracting the number of participants who indicated willingness to complete a given ratio requirement from the number of participants who completed the requirement in the EWT, dividing by the number of participants indicating willingness in the HWT, and multiplying by 100 to obtain a percentage for each ratio requirement. For example, suppose seven participants completed a ratio of 20 in the EWT but 10 participants indicated willingness to complete the same ratio in the HWT [(7-10) ÷ 10 × 100]. The calculation reveals a 30% decrease in the amount of work (and cost) completed from what was predicted based on the HWT.

Cost-benefit analysis. To compare expected aggregate costs and benefits for the three incentive magnitudes, I overlaid the amount paid at each ratio requirement onto the work function for the EWT. This depiction allows for visual inspection of the relative changes in cost and output between successive ratio requirements and how these changes differ among conditions.

Results and Discussion

A series of Chi-Square analyses revealed that magnitude conditions did not significantly differ for gender, $\chi^2(2) = 1.33$, $p = .52$, ethnicity, $\chi^2(10) = 8.63$, $p = .57$, highest level of education, $\chi^2(14) = 19.44$, $p = .15$, smoking status, $\chi^2(4) = 3.75$, $p = .44$, and disability status, $\chi^2(2) = 1.08$, $p = .58$. One-way ANOVA results indicate the conditions did not differ in age, $F(2, 286) = 0.78$, $p = .46$, or for scores on the BAS-D, $F(2, 286) = 0.70$, $p = .50$, BAS-FS, $F(2, 286) = 1.38$, $p = .25$, BAS-RR, $F(2, 286) = 1.19$, $p = .31$, and BIS, $F(2, 286) = 0.11$, $p = .90$. Because income was broken into nine ranges, when separated by condition several of the categories contained too few participants to accurately perform a Chi-Square test. Therefore, to evaluate whether conditions differed on income bracket, I combined several income ranges to create five categories (< \$30,000; \$30,000 - \$49,999; \$50,000 - \$69,999; \$70,000 - \$89,999; > \$90,000). Based on this analysis, income did not differ significantly among conditions, $\chi^2(8) = 6.54$, $p = .59$. Additionally, results of two Kruskal-Wallis one-way ANOVAs also indicated the conditions did not differ in *k*, $H(2) = .25$, $p = .88$.

Breakpoint. Figure 2 presents breakpoint for all conditions in the HWT and EWT. I observed a positive relation between incentive amount and median breakpoint in the HWT with systematic increases in breakpoint with increases in incentive magnitude. Specifically, median breakpoint in the M_{05}, M_{10}, and M_{20} conditions was 4, 8, and 10 work units, respectively. A Kruskal-Wallis one-way ANOVA indicated a significant difference in breakpoint, $H(2) = 23.09$, $p < .0001$. Results of a Dunn's multiple comparison test revealed breakpoint was significantly lower in M_{05} condition than either the M_{10} ($p = .0031$) or M_{20} conditions ($p < .0001$). Differences between the M_{10} and M_{20} conditions were not significant ($p = .51$). In the EWT, differences in median breakpoint were less orderly than the HWT, with values of 8, 8, and 10 for the M_{05}, M_{10}, and M_{20} conditions. Nonetheless, I observed a significant main effect in EWT breakpoint, $H(2) = 7.01$, $p = .03$. However, the only conditions in which breakpoint differed significantly was between M_{05} and M_{20} ($p = .025$) with higher breakpoints in the M_{20} condition.

Results of the Wilcoxon matched pairs signed-rank tests indicated differences in breakpoint between the HWT and EWT were significant for the M_{05} condition, $W = 1045$, $p = .0011$. Participants in the M_{05} condition completed higher ratio requirements in the EWT than they indicated in the HWT. I did not observe significant differences in breakpoint between the HWT and EWT for the M_{10}, $W = 207$, $p = .49$, or M_{20} conditions, $W = 366$, $p = .31$.

Unit price breakpoint. Figure 3 portrays HWT and EWT unit price breakpoints for the three incentive magnitudes to compare the number of work units completed per penny earned.

For the HWT, median unit price breakpoints were 0.80, 0.80, and 0.50, for the M_{05}, M_{10}, and M_{20} conditions, respectively. Results of the HWT Kruskal-Wallis one-way ANOVA showed a significant main effect of incentive magnitude on unit price breakpoint, $H(2) = 9.8$, $p = .0074$. Unit price breakpoint was significantly lower in the M_{20} condition than the M_{05} ($p = .012$) and M_{10} conditions ($p = .037$). There was no significant difference between the M_{05} and M_{10} conditions.

Median unit price breakpoints for the EWT were 1.60 (M_{05}), 0.80 (M_{10}), and 0.50 (M_{20}). Results of a Kruskal-Wallis one-way ANOVA revealed differences in unit price breakpoint were also significant for the EWT, $H(2) = 33.59$, $p < .0001$. Based on a Dunn's multiple comparison test, differences in unit price breakpoint were significant among all conditions in the EWT. Specifically, unit price breakpoint for the M_{05} condition was significantly higher than the M_{10} ($p = .0075$) and M_{20} conditions ($p < .0001$). Results demonstrate the M_{10} condition was also significantly higher than the M_{20} condition ($p = .019$). These results stand in contrast with the results of the traditional breakpoint analyses in that, when controlling for incentive amount, I observed a negative relation between incentive magnitude and unit price breakpoint with individuals in the lower incentive magnitude conditions completing more work per penny earned.

Monetary choice questionnaire. Results of a Spearman rank-order correlation revealed a nonsignificant negative correlation of -.04 ($p = .51$) between overall k and EWT breakpoint. These results suggest the rate at which participants discount delayed monetary rewards is not predictive of sensitivity to price in the current experimental preparation.

Behavioral inhibition system/behavioral activation system. Of the four BIS/BAS subscales assessed, the BAS-RR (reward responsiveness) was the only measure found to be significantly correlated with EWT breakpoint, $r_s(289) = -.13$, $p = .034$. Correlations between EWT and BAS-D, $r_s(289) = .01$, $p = .87$, BAS-FS, $r_s(289) = .07$, $p = .21$, and BIS, $r_s(289) = -.06$, $p = .34$, subscales were not significant.

Accuracy. Figure 4 illustrates the percentage of accurately completed work units across the 15 ratio requirements for the M_{05}, M_{10}, and M_{20} conditions. Overall, accuracy was high for the three magnitude conditions across all ratio requirements assessed. Relative to other conditions, accuracy in the M_{05} condition was slightly lower for the first ratio with an increasing trend for the next three ratio requirements (two, four, and eight). Accuracy stabilized around 99% for the remaining requirements in the M_{05} condition. Median accuracy for the M_{05} condition was 99.4% (range: 90.4 to 100.0%). Accuracy in the M_{10} condition was variable and ranged from 90.6 to 100.0% with a median of 96.7%. More than half of the errors in the M_{10} condition were committed by one participant, whose highest ratio requirement was 25. When this participant stopped responding and exited the study, M_{10} accuracy improved and stabilized near 99%. I observed an increase in accuracy in the M_{20} condition for the first three ratio requirements, which stabilized between ratios four and 15. Variability in accuracy for the M_{20} condition increased at ratio values greater than 15. Median accuracy in the M_{20} condition was 98.3% (range: 91.4 to 99.2%).

Analog to demand. When plotted using the untransformed ratio requirement displayed in Figure 5, elasticity increased systematically with decreases in incentive magnitude in the HWT (top panel) and EWT (bottom panel). That is, responding was more inelastic in the larger incentive magnitude conditions. Similar to the results of the traditional breakpoint analyses for the HWT, Extra sum-of-squares F-tests revealed that the M_{05} ($\alpha = 2.9 \times 10^4$) condition was significantly more elastic than the M_{10}, $\alpha = 1.9 \times 10^4$, $F(1, 28) = 7.6$, $p = .01$, or M_{20}, $\alpha = 1.8 \times$

10^4, $F(1, 27) = 16.0$, $p = .0005$. However, there was no significant difference in elasticity between the M_{10} and M_{20} conditions in the HWT, $F(1, 27) = 1.4$, $p = .25$. In the bottom panel of Figure 5, the percentage of participants who completed the ratio requirement in the EWT is shown for all three incentive magnitudes. I observed a similar pattern in the EWT as the HWT, with systematic increases in elasticity with decreases in incentive magnitude. Extra sum-of-squares *F*-test results indicated that elasticity differed among all incentive magnitude conditions, $F(2, 38) = 22$, $p = < .0001$.

Work functions for the HWT and EWT used to determine empirical demand indices are shown in Figures 6 and 7, respectively. Additionally, all derived and empirical metrics, including model fits, are listed in Table 2. Except for empirical P_{max} in the HWT which was highest in the M_{05} condition, empirical and derived demand indices—*EV*, P_{max}, and O_{max}—for both the HWT and EWT were lowest in the M_{05} condition and highest in the M_{20} condition. It is likely that empirical P_{max} was inflated in the M_{05} condition as a result of participants who did not reach a breakpoint in the HWT. Given the additive nature of the ratio progression, continued responding of just a few participants at the highest ratio requirement may cause the tail end of the work function to increase—and in some cases, to increase above the output observed at lower prices. To illustrate, three individuals indicating willingness to complete the highest ratio requirement (256) would result in a total output of 768, which is higher than if an entire sample of 95 participants completed ratio 8, totaling 760 work units. This increase along the right tail end of the expenditure curve may prove especially problematic in practice because discrepant empirical measures could alter important intervention decisions and compromise outcomes. Quite possibly, in these instances, inflated empirical measures may be a function of the method with which they were obtained (i.e., determining P_{max} by identifying peak output) rather than an accurate reflection of the facet of demand they are meant to capture (e.g., unit elasticity). As for the M_{05} condition in the HWT, the increase in output at higher ratio requirements resulted in an empirical P_{max} of 128, which is markedly higher than the derived value of 9.95. Because relatively few participants did not reach a breakpoint in the study, the M_{05} condition was the only condition in which the data appear to be affected by this pattern of persistent responding.

Figure 8 displays residual plots for all conditions in the HWT and EWT when data were plotted as a function of the ratio requirement. For the HWT, data points were consistently above but close to the best fit line at low ratio requirements. At higher ratio requirements, I observed clusters of data points above the best fit line in the HWT for the M_{05} and M_{10} conditions. For the EWT, data points at low ratio requirements were clustered above the best-fit curve and generally demonstrate an increasing trend. The trend of increasing distance from the best-fit line at lower prices for the M_{05} and M_{20} conditions peaked closely to derived P_{max}. At prices above P_{max}, data points in the EWT were more evenly distributed above and below the curve fit.

Figure 9 depicts a comparison of the HWT and EWT *AtD* curves for each incentive magnitude separately. Responding in the hypothetical and experiential work tasks decreased at almost imperceptibly similar rates for all magnitude conditions. This is supported by the results of the Extra sum-of-squares *F*-tests which were not significant for any of the conditions, M_{05}, $F(1, 26) = 0.43$, $p = .52$, M_{10}, $F(1, 26) = 2.4$, $p = .13$, M_{20}, $F(1, 27) = 0.3$, $p = .59$. The overlapping *AtD* curves suggest a high degree of correspondence in responding between the hypothetical and experiential *AtD* preparations. For the remaining *AtD* indices, I observed a systematic pattern in which derived indices for each condition were similar but slightly lower in the EWT than the HWT (Table 2). For example, in the M_{05} condition derived P_{max} was 9.95 and 9.02 in the HWT and EWT, respectively. However, I observed some inconsistencies when comparing empirical measures between the HWT and EWT. To illustrate, despite empirical P_{max} being markedly higher in the HWT for the M_{05} condition, O_{max} was lower. Empirical P_{max}, and O_{max} values were nonetheless similar in the HWT and EWT for the M_{10} and M_{20} conditions.

Figure 10 displays *AtD* curves for the three magnitude conditions in the HWT (top panel) and EWT (bottom panel) with the percentage of participants completing the requirement plotted as a function of unit price. Responding on the HWT was relatively more inelastic in the M_{05} condition ($\alpha = 1.5 \times 10^3$) than the M_{10} ($\alpha = 1.9 \times 10^3$) and M_{20} conditions ($\alpha = 3.4 \times 10^3$). Three separate Extra sum-of-squares *F*-tests were conducted to evaluate whether the differences in elasticity were statistically significant. Results indicate responding was significantly more inelastic in the M_{05} condition than the M_{20} condition, $F(1, 27) = 37$, $p < .0001$. I also found statistically significant differences in elasticity between the M_{10} and M_{20} conditions with greater elasticity in the M_{20} condition, $F(1, 27) = 35$, $p < .0001$. However, differences in α between the M_{05} and M_{10} conditions did not reach significance, $F(1, 28) = 2.9$, $p = .099$. Accordingly, the associated empirical and derived demand indices were highest in the M_{05} condition and lowest in the M_{20} condition. Essential value was 2.36 in the M_{05} condition, 1.86 in the M_{10} condition, and 1.04 in the M_{20} condition. Derived P_{max}, expressed as the number of work units per \$0.01, was 1.92, 1.52, and 0.85 for the M_{05}, M_{10}, and M_{20} conditions, respectively. Derived O_{max} was 55.78 in the M_{05} condition, 44.03 in the M_{10} condition, and 24.61 in the M_{20} condition. Similar to when the data were fitted using the ratio requirement, empirical values for P_{max} were somewhat lower and O_{max} values were higher than the derived values for all conditions.

I observed a similar pattern for the EWT. Responding in the M_{05} condition was the most inelastic relative to the other conditions ($\alpha = 1.6 \times 10^3$) followed by the M_{10} condition ($\alpha = 2.3 \times 10^3$). The M_{20} condition was the most elastic ($\alpha = 3.6 \times 10^3$). Extra sum-of-squares *F*-tests revealed statistically significant differences in elasticity among all conditions; M_{05} and M_{10}, $F(1, 24) = 14$, $p = .0011$; M_{05} and M_{20}, $F(1, 26) = 69$, $p < .0001$; M_{10} and M_{20}, $F(1, 26) = 26$, $p < .0001$. Like the HWT, derived demand indices were highest in the M_{05} condition, ($EV = 2.21$; $P_{max} = 1.80$; $O_{max} = 55.90$) followed by the M_{10} condition ($EV = 1.54$; $P_{max} = 1.25$; $O_{max} = 36.38$), and lowest in the M_{20} condition ($EV = 0.98$; $P_{max} = 0.80$; $O_{max} = 23.24$). Similarly, empirical O_{max} values were highest in the M_{05} condition and lowest in the M_{20} condition, however, empirical P_{max} was lowest in the M_{10} condition.

Predictive validity. The experiential work task breakpoint plotted as a function of HWT breakpoint is shown in Figure 11 for each incentive magnitude. For the predictive validity graphs, data points falling within the gray shaded area indicate the participants had a higher breakpoint on the HWT than the EWT; that is, they completed less work when given the opportunity than they originally indicated. Data points falling in the white portion indicate a higher breakpoint on the EWT task; these participants completed more work than they indicated they would on the HWT. Data points located along the intersection of the white and gray regions indicate perfect correspondence between breakpoint on the HWT and EWT. The size of the data point symbols reflects the number of participants whose data are represented by that value (smallest points represent a single participant, largest points equal to 10 participants). Results of Spearman rank-order correlations indicated a significant positive correlation between responding on the HWT and EWT for all three conditions; M_{05}, $r_s(99) = .47$, $p < .0001$; M_{10}, $r_s(95) = .59$, $p < .0001$; M_{20}, $r_s(95) = .57$, $p < .0001$.

I observed a slightly lower correlation in the M_{05} condition that may be a function of the relatively greater proportion of participants completing more work in the EWT than the HWT. Fifty-one percent of participants in the M_{05} condition ($n = 51$) completed more work in the EWT than they indicated on the HWT as compared with 38% (n = 36) and 40% (n = 38) of participants for the M_{10} and M_{20} conditions, respectively. The M_{10} condition had the highest percentage of participants with perfect correspondence on the HWT and EWT with 33.7% (n = 32). This value was followed by the M_{05} condition (n = 29, 29.3%). The M_{20} condition had the fewest participants with perfect correspondence (n = 23, 24.2%).

To evaluate predictive validity at the aggregate (or organizational) level, I calculated the percentage change in work completed, and corresponding cost, from the HWT to the EWT (Figure 12). I observed more variability at ratio requirements above 25, possibly as a result of the lower number of participants continuing to respond at these higher ratios. At requirements below 25, I observed a higher percentage change in the M_{05} condition relative to the other conditions with the M_{10} and M_{20} conditions generally remaining stable near zero. These data reveal that the predictive validity of the HWT at the aggregate level was sufficient for the M_{10} and M_{20} conditions up to and including a work requirement of 25 units.

Cost-benefit analysis. Lastly, Figure 7 displays the amount of money paid and work units completed to assess the relative costs and benefits at each ratio requirement among conditions in the EWT. Work completion was nearly identical through ratio 4, but costs differed substantially. Thus, in instances where P_{max} is equal to or less than 4, it would be wise to select the M_{05} condition for use (similar productivity at a relatively lower cost). Beginning at ratio 8, the expenditure curves diverge with participants in the M_{20} condition completing more work at each ratio than the M_{05} or M_{10} conditions. Participants in the M_{05} and M_{10} conditions completed roughly an equal number of work units at all prices; however, cost in the M_{05} condition was always lower except for two ratio requirements (32 and 48) wherein cost was equivalent. Mirroring decisions made in an organization from a cost-benefit perspective, it would not be ideal to select the M_{10} condition for use in an incentive program. Although participants in the M_{20} condition completed the most work units for 11 of the 15 ratio requirements, the cost was also substantially higher for all ratios. Opting to use the largest incentive magnitude may therefore depend on whether the organizational goals are more in line with maximizing output or minimizing cost, in which case the largest and smallest incentive magnitudes would be preferred, respectively.

Overall, the results of Experiment 1 indicate responding on the HWT and EWT was sensitive to incentive magnitude and participant responses on the HWT were in general agreement with observed responding on the EWT. Rates of delay discounting derived from the MCQ were not significantly correlated with responding on the EWT. However, the absence of a significant relation between delay discounting and breakpoint does not indicate that discounting does not influence work-related behavior. As evidenced by the findings of Sigurdsson et al. (2013), discounting measures may yield valuable insight to decision-making in the workplace. Instead, findings of Experiment 1 provide support for the notion that elasticity derived from the exponential demand equation may provide a more useful quantitative model than discounting for researchers interested in the amount of behavior maintained by a reinforcer. Although BAS-RR scores were significantly correlated with EWT breakpoint, the effect was minimal at best and it is unlikely BAS-RR alone would provide sufficient information of value to organizational leaders designing incentive systems to justify its use.

The traditional breakpoint analyses generally revealed a linear relation in the direction expected based on the reinforcer magnitude literature, with higher breakpoints in the larger incentive magnitude conditions. However, differences in breakpoint among magnitude conditions were often small (particularly when compared to the increase in incentive amount) and failed to reach significance in several instances (i.e., between the M_{10} and M_{20} conditions in the HWT and EWT and between the M_{05} and M_{10} conditions in the EWT). When using unit price to equate the benefit across conditions, the relation between breakpoint and magnitude was reversed. Consistent with the results of Henley, DiGennaro Reed, Reed et al. (2016), I observed systematic decreases in unit price breakpoint with increasing incentive magnitudes. The finding that unit price breakpoint was lower in the higher incentive magnitude conditions does not suggest higher magnitude incentives produce less work output. As observed in the cost-benefit analysis (Figure 7), larger incentives resulted in the completion of more work units. However, by equating costs and benefits, unit price reveals important information; namely, increases in responding were not proportional to increases in incentive magnitude. Therefore, the M_{05} condition resulted in a more efficient use of resources.

Although there were slight differences among conditions, accuracy was high for all incentive magnitudes. It is possible, however, that high levels of accuracy observed in all conditions were a result of the contingencies for completing the practice trial. That is, unlike the work units in the EWT, the practice trial required participants to provide an accurate response to continue to the next section. Therefore, participants who initially provided an inaccurate practice trial response experienced an error message presented in red text that delayed survey progression and required additional effort to correct the response before proceeding to the HWT. If the consequences for inaccurate responding on the practice trial functioned as punishment and decreased the frequency of inaccurate responses in the EWT, a punishment contingency could account for high levels of accuracy observed. It is unclear if removing the contingency for accurate responding on the practice trial would influence accuracy in the EWT.

For the *AtD* analyses, I observed a similar pattern of responding for the HWT and EWT. Overall, the percentage of participants completing the ratio requirement decreased systematically as a function of the number of work units required to earn the incentive for both the HWT and EWT. The data were well accounted for by the exponential model of demand with R^2 values of .87 or above for all conditions (range: .87 to .98). Although this finding is promising, as mentioned previously, careful inspection of residuals may provide insight into *AtD* analyses not captured by R^2. Residual plots for the present experiment suggest the empirical data differ in small but relatively systematic ways from derived estimates of consumption (percentage of participants working) represented by the best-fit curve.

Depending on whether price was expressed as the ratio requirement needed to earn the incentive or the ratio requirement divided by incentive size (i.e., unit price) the order of conditions differed. When using the ratio requirement, elasticity was greatest in the M_{05} condition and lowest in the M_{20} condition for both the hypothetical and experiential tasks, with the opposite relation observed when using unit price to fit the exponential demand equation. Though I observed general correspondence between empirical and derived demand measures supporting previous research reporting strong correlations between the two metrics (e.g., Murphy & MacKillop, 2006), the inconsistency obtained with P_{max} in the M_{05} HWT condition raises some concern regarding the utility of empirical measures for informing real-world interventions. Therefore, the future of translational applications of demand curve analyses would benefit from the identification and evaluation of a simple and consistent method for improving the accuracy of empirical measures.

Regardless, *AtD* findings replicate similar analyses completed by Henley, DiGennaro Reed, Reed et al. (2016) and provide further support for the importance of evaluating reinforcer dimensions to improve incentive delivery and continued investigation of the HWT. Additionally, the similarity in measures of elasticity and *AtD* indices (derived and observed; e.g., *EV*, O_{max}) between the HWT and EWT for all incentive magnitudes supports previous research evaluating the correspondence between hypothetical and experiential demand preparations (e.g., Amlung & MacKillop, 2015) and extends this literature to work-related behavior and workplace reinforcers.

Responding on the HWT was generally a good predictor of actual work performance, particularly at the aggregate level. However, responding on the EWT may have been influenced by responses on the HWT completed minutes prior and separated only by a single screen containing instructions for the EWT. As such, sequence effects may have influenced responding and would benefit from explicit empirical investigation in future research. The influence of sequence may be mitigated to a degree by separating the HWT and EWT by a distractor task.

Given the demographic diversity of mTurk Workers, information regarding participants' employment history and experience receiving incentives in paid positions outside of mTurk may provide insight to observed response patterns. Previous employment information and history with incentive receipt information may also serve to provide a more complete understanding of the frequency with which incentives are delivered outside of behavior analytic service delivery organizations documented by DiGennaro Reed and Henley (2015). Relatedly, HIT completion serves various functions for mTurk Workers. Ipeirotis (2010) reported nearly 70% of mTurk Workers in the United States complete HITs as a "fruitful way to spend free time" whereas approximately 15% use mTurk as a source of primary income and just over 60% use mTurk as a secondary source of income. Quite possibly, the degree to which participant responding persists despite increasing ratio requirements may vary systematically with the function of HIT completion.

Experiment 2: Incentive Probability

The purpose of Experiment 2 was to examine parametric effects of varying levels of probabilistic incentives on performance of mTurk Workers engaged in a simulated work task within a behavioral economic demand framework. Experiment 2 extends the findings of Experiment 1 by examining a second reinforcer dimension and is one of the first OBM studies to conduct a parametric evaluation of incentive probability. Conditions included a 90%, 50%, and 10% probability of earning incentives in the EWT.

Method

Participants. To recruit participants, I posted a HIT to the mTurk platform containing inclusionary criteria, a brief study description, compensation amount, and a link to the survey. Sample size was determined by conducting a power analysis using an effect size of .25, an alpha error probability of .05, and power of .95. The analysis indicated I would need a total sample size of 252 participants (84 per group) to detect differences in performance if they exist (G*Power version 3.1). Because the data were likely to deviate from normality based on Experiment 1, I increased the sample size by approximately 15% informed by recommendations for conducting power analyses with nonparametric methods (Motulsky, 2017). Workers were required to meet four criteria for inclusion: (1) reside in the United States, (2) have greater than 1,000 previously approved HITs, (3) have a 95% or above approval rate on previously completed HITs, and (4) submit a unique access code within 4 hr of selecting the survey link. Rather than post three identical HITs with unique survey links (as was done in Experiment 1), I posted one HIT and programmed the Qualtrics® hosted survey (www.qualtrics.com) to randomly assign participants to a condition. The information contained in the HIT (Appendix I) differed slightly from Experiment 1 to: (1) adjust the HIT description from evaluating "attitudes about life events and monetary rewards” to “monetary reinforcement in the workplace,” (2) the statement indicating participants will have the opportunity to earn bonuses was removed to decrease the likelihood of selection bias, (3) include a statement indicating the project is personally funded based on correspondence with and online information from mTurk Workers regarding best practices for posting a HIT, and (4) extend HIT completion time from 3 hr to 4 hr to ensure participants with a high breakpoint had sufficient time to complete the task. To make certain I would not need to exclude data for participants who completed the study multiple times, the HIT was also programmed to ensure Workers could not participate more than once. Workers who agreed to participate and submitted a unique access code—provided following study completion—served as participants.

Simulated work task. The work task (Appendix C) and PR schedule (1, 2, 4, 8, 10, 12, 15, 20, 25, 32, 48, 64, 96, 128, 256) were identical to Experiment 1. However, I adjusted the target values from Experiment 1 to exclude values equal to -100 and 100, the highest and lowest values on the visual analog scale range. Excluding target values of -100 and 100 prevented participants from providing an accurate response when quickly sliding the manipulandum to the end of the scale, a relatively low effort response compared to target values for which this was not possible. Therefore, *target values* ranged from -99 to 99 with the exception of zero but the *visual analog scale* ranged from -100 to 100.

Experimental conditions. I used a between-groups design to evaluate the effects of incentive probability. Participants were randomly assigned to one of three conditions that differed in the probability of receiving incentives in the EWT. Conditions included a 90% probability (P_{90}), 50% probability (P_{50}), and 10% probability (P_{10}) of earning incentives. Incentive magnitude for each condition was yoked to equal $0.20 if the probability of incentive receipt was 100%, thereby equating amount of compensation across conditions and isolating the effects of probability. Thus, the incentive equaled \$0.22, \$0.40, and \$2.00 for the P_{90}, P_{50}, and P_{10} conditions, respectively. I yoked the incentive amount to a \$0.20 incentive to increase total compensation based on Worker feedback in Experiment 1.

Procedures. The study contained four sections presented in the following order: (1) practice trial, (2) HWT, (3) demographic, employment, and incentive survey, and (4) EWT.

Practice trial. After selecting the study link from the mTurk HIT and reviewing an information statement, participants completed the practice trial. The practice trial included instructions for completing the simulated work task (Appendix J) and provided participants with three opportunities to perform the simulated work task. The practice trial was increased from one work task in Experiment 1 to three work tasks in the present experiment to give participants experience with the repetitive nature of the work—similar to what participants experience at ratio requirements above one—prior to completing the HWT and EWT. Unlike Experiment 1, participants were not required to provide accurate responses to the practice opportunities. Although a lack of feedback regarding inaccurate responding may also influence the likelihood of inaccurate responding, the contingency for correct responding in Experiment 1 was removed to avoid inflating response accuracy on the EWT.

Hypothetical work task. Instructions for the HWT were adjusted to include a statement indicating that there are no right or wrong responses. This information is frequently included in hypothetical demand assays—including the HWT evaluated by Henley, DiGennaro Reed, Kaplan et al. (2016). The question phrasing at each ratio requirement for the HWT was adjusted slightly from Experiment 1 to reflect changes in the independent variable. For example, the P_{50} condition at a ratio value of 20 read, “Would you complete 20 slider questions in exchange for a

5 in 10 (50%) chance of receiving a 40 cent bonus paid in 1 day?" Information about the duration to incentive receipt (i.e., one day) was included to control for delay to reinforcement, assessed in Experiment 3. Additionally, I phrased the delay as the number of days to minimize changes in wording between Experiment 2 and 3. For each of the 15 ratios in the PR schedule, participants either selected *yes* or *no* indicating whether they would or would not complete the ratio requirement if provided the opportunity. The survey presented the HWT on a single page with the instructions positioned at the top of the page and questions for each ratio requirement presented in an ascending order. An important difference from Experiment 1 is that the survey required participants to provide a response to all 15 ratio requirements rather than ending after the first ratio at which a participant select *no*. This change was implemented to evaluate the consistency of responding as price increases. Observed inconsistencies could suggest a problem with the HWT. Appendix J provides instructions and question phrasing for the HWT.

Demographic, employment, and incentive survey. To better understand participants' experience in the workplace, self-reported motivation for completing mTurk tasks, and history receiving incentives, participants completed a demographic, employment, and incentive survey following the HWT (Appendix K). The demographic, employment, and incentive survey also functioned as a distractor task before participants proceeded to the EWT. The survey items pertaining to participant demographic information were: (1) age, (2) gender, (3) ethnicity, (4), state of residence, (5) highest level of education completed, and (6) combined annual household income. Several demographic items unrelated to the independent variable included in Experiment 1 were removed from the current study to minimize HIT completion time (i.e., disability and smoking status). One participant in the P_{90} condition reported an age of three. Because this value is likely not possible, this participant was excluded from analyses containing age. I also included three questions specific to mTurk employment, including: (1) hours per week spent on mTurk, (2) reason for completing mTurk tasks, and (3) amount of money earned per week on mTurk. When asked about the average amount of income earned per week on mTurk, three participants ($P_{90} = 1$; $P_{10} = 2$) provided a range rather than a single value. In these instances, I recoded their reported weekly mTurk income to equal the average of the range (e.g., 150-200 was recoded as 175). Questions regarding employment at their most recent paid position outside of mTurk included: (1) tenure, (2) number of promotions received, (3) occupation category, and (4) reason for leaving last paid position. Participants who reported that they have not held a paid position in the last 10 years when asked about tenure, were redirected to the next section (i.e., EWT directions). Finally, participants indicated whether monetary and/or non-monetary incentives (referred to as "other gifts") were available at their most recent paid position and, if so, how often these incentives were made available.

Experiential work task. After the demographic, employment, and incentive survey, participants reviewed the EWT instructions and began the task. Similar to the HWT, I adjusted the phrasing for each ratio requirement in the EWT to reflect changes in the independent variable. For example, the P_{50} condition read, "Would you like to complete 20 slider questions in exchange for a 5 in 10 (50%) chance of earning a 40 cent bonus paid in 1 day?" Unlike Experiment 1, the study did not end when participants select *no* for a ratio requirement. This change was made for the same reason as stated in the HWT. As a result, if participants selected *yes* for a given ratio, the survey displayed the corresponding number of work units, with each work unit presented on a separate page and numbered in the same manner as Experiment 1 (i.e., Question X of Y). After completing the ratio, or if the participant selected *no* for the previous ratio requirement, the survey presented the same question for the subsequent ratio in the sequence. This format continued until participants responded to all 15 ratio requirements at which point the survey displayed a unique access code and a free response text box for participants to provide feedback, as desired.

Each ratio requirement completed in the EWT had an equal probability of being selected to receive the incentive and varied according to the probability condition to which the participant was assigned (90%, 50%, or 10%). For example, a participant assigned to the P_{50} condition who completed the first four ratios had four separate opportunities each with a 50% probability of receiving a $0.40 incentive.

Payment. Participants received a base pay of $0.25 for completing the practice trial, HWT, demographic, employment, and incentive survey, and providing *yes*/*no* responses to the 15 questions that preceded each ratio requirement in the EWT (i.e., "Would you like to complete…?"). All participants received base pay and total incentives accrued 1 day following study completion. Average completion time for the entire study, including the EWT, was 11 min and 41 s (range: 2 min 45 s to 2 hr 18 min 39 s). Participants in the P_{90}, P_{50}, and P_{10} conditions earned an average of $1.08 (*SD* = 0.42), $1.21 (*SD* = 0.66), $1.26 (*SD* = 1.48), respectively.

To randomly determine whether participants received the incentive for each ratio completed, I used Microsoft Excel's random between function (described in Experiment 1). For each ratio requirement a participant completed in the EWT, I generated a random number between 1 and 100. Participants received an incentive if the random value was less than or equal to the value of the probability for the condition to which the participant was assigned. Said another way, to receive an incentive for a completed ratio the random value was required to be less than or equal to 90, 50, and 10 for the P_{90}, P_{50}, and P_{10} conditions, respectively. For example, a P_{10} participant who completed the first four ratio requirements in the EWT had four opportunities to receive \$2.00 (each with a 10% probability). If the random numbers were 10, 9, 88, and 53, the participant received \$4.00 in incentives because the first two values are less than or equal to 10.

Data analyses. Like Experiment 1, I examined whether participant demographic variables were evenly distributed among the conditions using a series of Chi-Square and one-way ANOVAs. For all data analyses, nonparametric tests were used for data that deviated significantly from normality assessed using a D'Agostino and Pearson omnibus normality test. Unless otherwise indicated, all statistical analyses were two-tailed.

Experienced probabilities. Because probability was determined by random values, it was possible for the experienced probability to deviate from the programmed value across ratios. Therefore, I also calculated the observed probabilities for the three conditions by dividing the number of participants in each condition who received the incentive by the total number of participants who completed the response requirement and multiplying by 100 to obtain a percentage for each ratio value. I then used a one sample t-test for each condition to compare whether experienced probabilities differed significantly from the programmed value.

Breakpoint. I identified the highest ratio requirement participants indicated willingness to complete in the HWT or completed in the EWT (i.e., breakpoint). Any participant whose responding did not break during the HWT or EWT was assigned a value of 256, the highest ratio requirement assessed. Responding for three participants in the HWT for both the P_{90} and P_{50} conditions and five participants in the P_{10} condition did not reach a breakpoint and were assigned a breakpoint value of 256. I did not use unit price breakpoint for any analyses. For the HWT and EWT, a Kruskal-Wallis one-way ANOVA was used to examine breakpoint among the P_{90}, P_{50}, and P_{10} conditions with post-hoc comparisons when warranted. To evaluate within-subject differences in breakpoint between the HWT and EWT, I used a Wilcoxon matched pairs signed-rank test for each probability condition.

Spearman rank order correlation analyses were conducted to evaluate the relation between EWT breakpoint and education, income, employment tenure, and number of promotions received. Additionally, point biserial correlations were used to evaluate the relation between EWT breakpoint and reasons for completing mTurk tasks.

Accuracy. Accuracy was defined and calculated in the same manner as Experiment 1. I also evaluated the percentage of accurately completed work units during the practice trial.

Analog to demand. To calculate elasticity, I used the same procedures and parameter values for Q_0 and k as Experiment 1. I also used the same methods for calculating empirical and derived *AtD* measures. However, I only used ratio requirement values as opposed to unit price in fitting the percentage of participants to the exponential demand equation. Extra sum-of-squares F-tests were used to compare elasticity among probability conditions and between the HWT and EWT within each probability condition.

Predictive validity. Correlation analyses were identical to Experiment 1. However, because the number of participants who received the incentive at each ratio was probabilistic, the number of incentives delivered in the EWT (i.e., cost) was no longer a *direct* function of the number of participants who completed the ratio requirement. Thus, although percentage change for *work output* was calculated identically to Experiment 1, I also calculated percentage change from projected *cost* in the HWT to observed cost in the EWT. Percentage change in cost was obtained by multiplying the incentive amount (P_{90}, \$0.22; P_{50}, \$0.40; P_{10}, \$2.00) by the product of the number of participants who indicated willingness to complete the ratio in the HWT and the probability associated with that condition (P_{90}, .90; P_{50}, .20; P_{10}, .10). After obtaining the difference between amount paid at each ratio in the HWT and EWT, I divided the resulting value by the cost in the HWT and multiplied by 100.

Cost-benefit analysis. To compare expected aggregate costs and benefits for the three incentive probabilities, I plotted the total number of work units completed (i.e., a work function) against the amount paid at each ratio requirement for the EWT as well as the percentage of maximum possible output.

Results and Discussion

A total of 304 Workers completed all study procedures and submitted access codes to the mTurk HIT. Participants were excluded from analyses if responding was inconsistent in the HWT or EWT. Inconsistent responding was defined as having at least one instance in which a participant reached a breakpoint and subsequently indicated willingness to complete (HWT) or completed (EWT) a higher ratio requirement. Because it is unclear which price should signify the breakpoint, participants were excluded from analyses rather than assigned a breakpoint value. This criterion for identifying and excluding inconsistent responding differs from the method employed in more recent behavioral economic demand research as outlined by Stein, Koffarnus, Snider, Quisenberry, and Bickel (2015). At the individual level, the present experimental preparation generated binary data that effectively restricted any change in consumption to 100%. Binary measures of consumption are not sensitive enough to be amenable to much of Stein and colleagues' recommendations that are primarily based on the degree to which consumption changes.

Fourteen participants were excluded based on inconsistent responding on the HWT (P_{90} = 4; P_{50} = 3; P_{10} = 7), two were excluded due to inconsistent responding on the EWT (P_{50} = 1; P_{10} = 1), and three participants displayed inconsistent responding on both the HWT and EWT and were subsequently excluded (P_{90} = 2; P_{50} = 1). A total of 19 participants were excluded (6.25%) and data for 285 participants were retained for analysis (P_{90} = 95; P_{50} = 95; P_{10} = 95).

Demographic, employment, and incentive survey. Participant information separated by condition are reported in Tables 3, 4, and 5, for demographic information, mTurk employment characteristics, and non-mTurk employment and incentive history, respectively. Participants ranged from 19 to 72 years with a mean age of 36 years (SD = 11.05) with males accounting for a slightly greater percentage of participants than females (n_{male} = 154, 54.0%; n_{female} = 131, 46.0%). Similar to Experiment 1, participants were primarily Caucasian (n = 236, 82.8%) and reported living in 43 of the 52 states and territories in the United States with the highest concentration of participants living in California (n = 34, 11.9%), Pennsylvania (n = 18, 6.3%), and Texas (n = 18, 6.3%). Education ranged from less than a high school diploma (n = 2, 0.7%) to doctoral (n = 3, 1.1%) and professional degrees (n = 3, 1.1%) with a four-year college degree reported with the highest frequency (n = 100, 35.1%). A combined annual household income of less than $30,000 per year was reported with the highest frequency (n = 62, 21.8%) followed by an income in the $30,000 to $39,999 range ($n$ = 50, 17.5%). A series of Chi-Square analyses revealed that conditions did not differ significantly for gender, $\chi^2(2) = 0.54$, $p = .77$, ethnicity, $\chi^2(12) = 7.28$, $p = .84$, education, $\chi^2(14) = 18.72$, $p = .18$, or income, $\chi^2(8) = 9.06$, $p = .34$ (using the same aggregating procedure as Experiment 1). Additionally, a one-way ANOVA indicated the conditions did not differ significantly in participant age, $F(2, 282) = 0.20$, $p = .82$.

On average, participants reported spending 16.69 hr per week completing HITs on Amazon Mechanical Turk (range: 1 to 100 hr, SD = 13.5). Average weekly income earned on mTurk ranged from $1.50 to $1000 with a mean of $91.19 per week ($SD$ = 13.49). To standardize mTurk income, I divided the reported average income by the average number of hours spent on mTurk per week to obtain participants average hourly mTurk wage, which equaled \$6.39 (range: \$0.15 to \$50.00, *SD* = 5.61). The survey also asked participants to indicate their reason(s) for completing mTurk tasks. Because participants were asked to indicate all the applicable reasons, the sum of percentages is greater than 100. The most frequently endorsed reason for completing mTurk HITs was as a secondary source of income (*n* = 208, 73.0%) followed by a fruitful way to spend free time (*n* = 157, 55.1%). Besides one participant who indicated "other" (i.e., "to pay for insurance required by the ACA [obamacare]"), participants reported completing HITs because they are currently unemployed or only employed part-time with the lowest frequency (*n* = 35, 12.3%).

With respect to current or previous employment outside of mTurk, nearly one quarter (*n* = 67, 23.5%) indicated being employed between 1 and 3 years at their most recent paid position. Participants also frequently reported tenure durations of 3 to 5 years (*n* = 54, 18.9%) and 5 to 10 years (*n* = 56, 19.6%). Participants who reported not having held a paid position outside of mTurk in the last 10 years (*n* = 11, 3.9%) were redirected to the end of the demographic, employment, and incentive survey and not asked any further questions related to employment outside of mTurk. Thus, their responses are not included in the data presented below or in the data for the corresponding questions presented in Table 5. Even though three quarters of participants have held their position for 1 year or longer, nearly half of the sample reported never having received a promotion at their current place of employment (*n* = 123, 44.9%) and another 28.5% have only received one promotion (*n* = 78). Participants reported a wide range of occupations with office/administrative support (*n* = 42, 15.3%), sales (*n* = 34, 12.4%), and computer/mathematical (*n* = 32, 11.7%) positions reported with the highest frequency. Accepting a new job at a different company was the most endorsed reason for leaving their last paid position (n = 95, 34.7%). Participants selected the remaining reasons for leaving their last paid position with a similar frequency (range: 10.9 to 14.6%).

Approximately half of the participants indicated they did not receive monetary and/or non-monetary incentives at their most recent paid position (n = 152, 53.3%). Of the participants who received incentives, monetary and non-monetary goods were used with a similar frequency (monetary, n = 78, 27.4%; non-monetary, n = 74, 26.0%). With respect to how often incentives were available to employees—regardless of incentive type—twice yearly was reported with the highest frequency (n = 40, 32.8%).

Experienced probabilities. Although the experienced probabilities were generally similar to the programmed values at the aggregate level, I observed some deviations in experienced probabilities from the programmed levels (Figure 13). However, one sample t-tests indicated the experienced probabilities did not differ significantly from 90% for the P_{90} condition, $t(11) = 0.44$, $p = .67$, from 50% in the P_{50} condition, $t(10) = 1.58$, $p = .15$, or from 10% for the P_{10} condition, $t(12) = 0.22$, $p = .83$. Probabilities in the P_{90} condition ranged from 66.7% at a ratio requirement of 48 to 100% at ratio values 25, 32, and 64 with a mean probability of 88.53% likelihood of receiving the incentive. Experienced probabilities for the P_{50} condition were generally higher than 50%, ranging from 41.7% at ratio 20 to 100% at ratio 48 (M = 57.54%). Experienced probabilities for the P_{10} condition were, on average, slightly less than 10 (M = 9.35%, range: 0 to 33.3%). Differences from the programmed probabilities were slightly more pronounced at higher ratio requirements during which fewer participants continued to respond.

Breakpoint. Figure 14 displays breakpoints in the HWT and EWT for all three probability conditions. For the HWT, median breakpoint was 12 for the P_{90} and P_{50} conditions and 15 for the P_{10} condition. Breakpoint for all conditions in the HWT ranged from 0 to 256 work units. Results of a Kruskal-Wallis one-way ANOVA indicated breakpoint did not differ significantly among probability conditions, $H(2) = 1.14$, $p = .57$. Median breakpoint in the EWT was 8 for the P_{90} (range: 0 to 64) and P_{10} (range: 1 to 96) conditions and 4 for the P_{50} condition (range: 0 to 48). There was no significant difference in breakpoint among any of the conditions in the EWT, $H(2) = 0.21$, $p = .90$. A Wilcoxon matched pairs signed-rank test indicated significant differences in breakpoint between the HWT and EWT for all three conditions (P_{90}: $W = -2382$, $p < .0001$; P_{50}: $W = -2077$, $p < .0001$; P_{10}: $W = -2758$, $p < .0001$), with higher breakpoints observed in the HWT than the EWT in all cases. Of the variables assessed in the demographic, employment, and incentive survey, only one question was significantly correlated with EWT breakpoint, $r_{pb} = .19$, $p < .001$ (i.e., completing mTurk HITs because participants find the tasks to be fun).

Accuracy. Accuracy was high and stable for all three conditions through a ratio requirement of 15 after which I observed a decrease for the P_{90} and P_{10} conditions (Figure 15). Decreases in accuracy at ratios greater than 15 also coincided with increases in variability in participant accuracy. Increased variability is likely a function of the relatively fewer number of participants continuing to respond at higher requirements; with fewer opportunities, inaccurate responses have a disproportionately large effect on percentage accuracy as compared to lower response requirements with greater proportions of the participant sample responding. Median accuracy for the P_{90} condition was 90.22% (range: 66.67 to 100%) and 91.84% for the P_{50} condition (range: 85.27 to 100%). Accuracy was lowest in the P_{10} condition with a median of 84.88% (range: 0.00 to 95.45%), which was likely due to the pronounced decreases at higher ratio requirements.

Of the 95 participants in each condition, 27, 29, and 27 participants inaccurately completed at least one work unit in the P_{90}, P_{50}, and P_{10} conditions, respectively. Of those, just 7 (P_{90}), 7 (P_{50}), and 6 (P_{10}) participants accurately completed less than 50% of work units as compared to the 82, 83, and 82 participants who accurately completed at least 90% of work units in the P_{90}, P_{50}, and P_{10} conditions, respectively. Closer inspection of inaccurate responses revealed they were typically due to participants matching the inverse of the target number (e.g., 65 for the target -65) or missing the target value by a few integers rather than sliding the manipulandum without attempting to match the target number.

Analog to demand. Figure 16 shows *AtD* curves for the three probability conditions in the HWT and EWT with the percentage of participants plotted as a function of the ratio requirement (i.e., price). Overall, the percentage of participants completing the ratio requirement decreased systematically as a function of the number of work units required to earn the incentive for both the HWT and EWT.

Hypothetical work task. The top panel of Figure 16 portrays the percentage of participants indicating willingness to complete the ratio requirement on the HWT for all three probability conditions. Aggregate responding was idiosyncratic in terms of incentive probability. Specifically, the P_{10} condition was the most inelastic, followed by the P_{90} condition, with the P_{50} condition being the most elastic. The observed difference in elasticity between the P_{90} and P_{50} conditions was less pronounced, $F(1, 28) = 2.0$, $p = .17$, than the difference between the P_{10} condition and either the P_{90}, $F(1, 28) = 13$ $p = .0011$, or P_{50} conditions, $F(1, 22) = 28$, $p < .0001$.

Elasticity, R^2, and all values for P_{max} and O_{max} are listed in Table 6. Except for empirical P_{max}, the remaining derived and empirical demand indices for the HWT followed the same pattern as elasticity noted above. That is, all metrics were highest in the P_{10} condition (derived

P_{max}, 40.21; derived O_{max}, 1195.21; empirical O_{max}, 1280.00), followed by the P_{90} condition (derived P_{max}, 24.04; derived O_{max}, 697.21; empirical O_{max}, 800.00), with the lowest values in the P_{50} condition (derived P_{max}, 20.61; derived O_{max}, 597.61; empirical O_{max}, 768.00).

Empirical P_{max}, however, was equal in the P_{50} and P_{10} conditions with a value of 256 and lowest in the P_{90} condition (empirical P_{max}, 20.00). Examination of the work functions for the HWT plotted in Figure 17 suggests empirical metrics may be influenced by the number of participants who did not reach a breakpoint. I observed a pronounced increase in the number of participants who exhibited this pattern of persistent responding in the HWT in the present experiment, particularly in the P_{10} condition, which may explain the discrepancies between derived and empirical P_{max}. One solution to addressing instances in which empirical metrics are inflated because of persistent responding may be to disregard the tail end of the expenditure curve (e.g., the last three prices). In doing so, empirical P_{max} would equal 20, 20, and 48, in the P_{90}, P_{50}, and P_{10} conditions, respectively, which more closely approximate the derived P_{max} values for each condition. Although certainly not without its limitations, this adjustment may be one approach worth considering for addressing such discrepancies when attempting to make decisions in applied settings.

Experiential work task. Analog to demand curves for the EWT are shown in the bottom panel of Figure 16. Responding in the EWT was largely undifferentiated among probability conditions. An Extra sum-of-squares F-test revealed elasticity did not significantly differ among any of the conditions, $F(2, 33) = 1.2$, $p = .31$, and thus is plotted with a simpler model in which a single global curve is fit to all the data. Although I observed differences in derived demand indices among conditions with the largest values in the P_{50} condition (derived P_{max}, 9.62; derived O_{max}, 278.88) followed by the P_{10} condition (derived P_{max}, 8.24; derived O_{max}, 239.04), and lowest in the P_{90} condition (derived P_{max}, 7.80; derived O_{max}, 226.12), these differences were small (Table 6).

Figure 18 illustrates work functions for the probability conditions in the EWT. The ordering of the values of the empirical indices differed from that observed with the derived indices. Relative to the other conditions, empirical P_{max} was still highest in the P_{50} condition but I observed equal values for the P_{90} and P_{10} conditions. Although empirical P_{max} was highest in the P_{50} condition, empirical O_{max} was lowest in this condition.

Residual plots for all conditions in the HWT and EWT are shown in Figure 19. Residuals for the P_{90} and P_{50} conditions in the HWT followed a similar trend. The data were slightly above the best-fit curve with a modest increase up to a ratio requirement of 10, which was followed by a subtle decrease and subsequent increase at derived P_{max}. Residuals at prices near P_{max} were generally close to the best-fit line for these conditions. At higher ratio requirements, data for the P_{90} and P_{50} conditions fell below the best fit line and rapidly increased along the right tail of the figures. Data for the P_{10} condition generally followed a similar trend. However, derived P_{max} was more closely aligned with the point at which the residuals were negative and furthest from the best-fit curve.

For all conditions in the EWT, residuals at low prices demonstrate a positive increase in distance from the best-fit curve as price increases. This trend was observed until the price denoted by derived P_{max}. After P_{max}, the pattern of residuals for each probability condition in the EWT differs. Residuals at prices above P_{max} were somewhat clustered below the best-fit curve in the P_{90} condition, but generally well scattered, particularly when compared to the data for the lower half of the prices. Apart from the highest price, data in the P_{50} condition largely remained above the best-fit curve. After P_{max}, data for the P_{10} condition decreased steeply and subsequently increased to levels observed at P_{max}.

Figure 20 separates *AtD* curves by probability condition to facilitate comparisons of elasticity between the HWT and EWT. Inspection of the curves reveals that differences between the HWT and EWT were pronounced, with higher demand in the HWT than the EWT in all cases. Results of three separate Extra sum-of-squares *F*-tests revealed differences in elasticity were significant for all conditions, P_{90}, $F(1, 25) = 95$, $p < .0001$, P_{50}, $F(1, 24) = 32$, $p < .0001$, P_{10}, $F(1, 26) = 118$, $p < .0001$.

Predictive validity. Figure 21 displays the results of correlation analyses between the HWT and EWT breakpoint using the same graphing conventions as Experiment 1. Results of Spearman rank-order correlations showed a significant positive correlation between responding on the HWT and EWT for all three probability conditions; P_{90}, $r_s(95) = .48$, $p < .0001$; P_{50}, $r_s(95) = .49$, $p < .0001$; P_{10}, $r_s(95) = .51$, $p < .0001$.

Figures 22 and 23 depict the percentage change in work output and cost from the HWT to the EWT, respectively. Overall, predictive validity in terms of percentage change for work output and cost was low and followed a similar pattern across probability conditions. For all three groups, percentage change in work output was close to 0% during the first two ratio requirements after which I observed marked decreases in the predictive validity of the HWT as the ratio requirement increased. That is, participants largely overestimated the amount of work they would be willing to complete in the HWT, but it does not appear the probability level systematically influenced predictive validity. Similarly, percentage change in cost was at or slightly above 0% for the first two ratio requirements. After the second ratio, percentage change for all conditions markedly decreased to -100% at ratio requirements above 64 for all conditions.

Variability in the data appear to be a direct function of the probability level, with the highest variability observed in the lowest probability condition.

Cost-benefit analysis. A graphic depiction of the relation between cost and work output (i.e., organizational benefit) is presented in Figure 18. Although more variable in the P_{10} condition than the P_{90} and P_{50} conditions, cost showed a decreasing trend as the ratio requirement increased for all three conditions. Work output generally followed an inverted U shape. Work output peaked at ratio 8 for the P_{90} and P_{10} conditions with the highest output occurring at ratio 10 for the P_{50} condition. Although the price at which responding in the P_{50} condition peaked was higher than the other conditions, peak output at ratio 10 was 380 work units, which is lower than 440 work units completed in the other conditions at ratio 8. Organizational leaders wishing to inform an incentive arrangement based on the cost-benefit analysis results might wish to select a 50% probability of receiving an incentive. Although peak work output is not as high compared to the other conditions, responding persists at higher ratio requirements in which the costs are substantially lower than that observed at the ratio requirement associated with peak output for the P_{90} and P_{10} conditions. Relative to the P_{10} condition, the P_{50} condition was less influenced by momentary fluctuations in the amount paid, making it easier for organizational leaders to accurately budget incentives.

In sum, unlike the effects of incentive magnitude observed in Experiment 1, responding in the present experiment did not appear to differ systematically as a function of the probability of incentive receipt for most response measures. Specifically, the highest median breakpoint in the HWT was obtained in the lowest probability condition, and in the EWT the highest median breakpoint was identical in both the highest and lowest probability conditions. Comparisons of breakpoint among probability conditions were not statistically significant in the hypothetical or experiential work tasks. Although not in the direction one might expect given programmed probabilities, the only conditions to yield significant differences when individual breakpoints were used to generate a group-level measure of elasticity (i.e., *AtD*) were between the P_{10} and both the P_{90} and P_{50} conditions in the HWT, with greater inelasticity in the P_{10} condition. This apparent effect was not replicated in the EWT in which α did not differ significantly among any of the conditions.

Previous research in OBM has demonstrated that incentives are consistently effective when the probability of incentive receipt is less than 100% (e.g., Alavosius et al., 2009; Orpen, 1974; Reed et al., 2012). Researchers have shown that incentive probabilities as low as 17% may still be effective at maintaining work-related behavior (e.g., Cook & Dixon, 2006). However, research has often failed to hold the probability constant or even report the probability participants experienced.

Surprisingly, the current experiment is the first in nearly three decades to directly manipulate and compare the effects of varying levels of incentive probability (Evans et al., 1988). Evans and colleagues' experimental preparation included multiple interlocking probabilistic incentive arrangements in which *probability* and *magnitude* varied. Such features of the experimental preparation make it difficult to draw conclusions about the effects of probability alone on performance. Although Evans and colleagues failed to find differences in performance between the two probability conditions—which is consistent with the results of the present experiment—the disparate methodologies used to evaluate probability make it challenging to compare across studies.

One consistent finding in the present study was that individual breakpoints in the hypothetical task were significantly higher than their experiential counterparts for all conditions.

These findings, though observed for all probability conditions, also stand in contrast to much of the research evaluating hypothetical and experiential demand assessments (e.g., Amlung & MacKillop, 2015; Amlung et al., 2012). Although significant, correlation coefficients between breakpoint in the HWT and EWT were generally lower than Experiment 1. This decline is likely attributable to the relatively greater proportion of participants who overestimated the amount of work they would be willing to complete in the HWT. The percentage of participants in Experiment 1 who completed more work in the EWT than they indicated on the HWT ranged from 38% to 51%. This is contrasted with the current experiment in which the percentage of participants completing more work was 9%, 12%, and 7% in the P_{90}, P_{50}, and P_{10} conditions, respectively. This effect was consistently observed in comparisons between responding on the HWT and EWT regardless of the analysis method. That is, not only was breakpoint significantly lower in the EWT than the HWT in all conditions, responding was also strikingly more elastic and therefore more sensitive to increases in price in the EWT and percentage change from predicted output and cost rapidly declined as the ratio requirement increased. Similar to the lack of orderly changes in responding with changes in probability level, the difference in responding between hypothetical and experiential assessment methods stands in contrast with the findings from similar analyses in Experiment 1.

One possible explanation for the lack of differentiation among incentive probabilities involves the procedure for equating the conditions. It is imperative to manipulate only the dimension of reinforcement under investigation to isolate the effect of that dimension. Because lower probabilities would also decrease compensation, the magnitude of reinforcement was increased with decreases in probability. That is, incentive amount was yoked across conditions to equate compensation when only 90%, 50%, or 10% of completed ratio requirements resulted in incentive receipt. Controlling for magnitude can be seen by inspecting the cost-benefit analysis and noting the similarity in cost curves among conditions despite large differences in programmed probabilities. It is possible, however, that this control measure may have rendered the conditions functionally equivalent in the present experimental preparation and contributed to the lack of differentiation among conditions.

The yoking procedure may also be considered a limitation with respect to external validity. That is, an important reason for using probabilistic incentive systems is to mediate budgetary restrictions. Organizations may not always use a higher magnitude incentive when implementing a probabilistic incentive system. Consequently, an important area of investigation may be to evaluate the extent to which responding persists at a similar level despite decreases in probability while holding magnitude constant.

An interesting finding pertinent to organizational incentive systems is that the standard deviation in participant compensation increased with decreases in probability. Greater variability in compensation amounts may produce wide fluctuations in expenses and compromise the ability of organizational leaders to accurately predict and budget incentives. Because responding was similar across conditions, organizational leaders may wish to select an incentive arrangement with a higher probability of incentive receipt.

It is also possible that withholding information from participants about their receipt of an incentive after completing each EWT ratio may have inadvertently increased the uncertainty of compensation (beyond the manipulated probabilities) and influenced responding in unknown ways. This uncertainty may have also been influenced by participants' individual histories contacting reinforcement under probabilistic schedules. For example, an individual with a dense or recent history receiving probabilistic outcomes may have created a self-generated rule about the likelihood of his or her behavior contacting reinforcement that was higher than the programmed probability. Perhaps providing participants with information about whether they received the incentive following each ratio requirement may mitigate the effects of individual histories over successive requirements. Though generally versatile, the constraints associated with using a survey software for the present study prevented this feature from being incorporated into the current experimental arrangement. Future research could address this issue.

Experiment 3: Incentive Delay

An extensive literature base has documented the impact of delay on reinforcer value. However, delay remains largely unexplored in the OBM incentive literature. The purpose of Experiment 3 is to examine the effects of delayed incentives on performance of mTurk Workers. I evaluated three incentive delays using a behavioral economic demand framework. To facilitate comparisons of the influence of reinforcer dimensions across experiments, the experimental preparation and data analyses for Experiment 3 were identical to Experiment 2 with several noted exceptions.

Method

Participants. Two hundred and ninety-six participants were recruited from mTurk using the same procedures as Experiment 2.

Simulated work task. The work task, PR schedule, and target values were identical to Experiment 2.

Experimental conditions. To evaluate the effects of incentive delay, participants were randomly assigned to one of three conditions. Incentives were delivered after 1, 14, or 28 days following participation for the D_{01}, D_{14}, and D_{28} conditions, respectively

Procedures. The survey contained four sections presented in the following order: (1) practice trial, (2) HWT, (3) demographic, employment, and incentive survey, and (4) EWT.

Practice trial. The practice trial was identical to Experiment 2.

Hypothetical work task. The question phrasing at each ratio requirement for the HWT was adjusted slightly from Experiment 2 to reflect changes in the independent variable (Appendix J). For example, the D_{14} condition at a ratio value of 20 read, "Would you complete 20 slider questions in exchange for a 20 cent bonus paid for certain in 14 days?" Information about the probability of receiving the incentive (i.e., *for certain*) was included to control for probability of reinforcement, assessed in Experiment 2. The remaining features of the HWT were identical.

Demographic, employment, and incentive survey. After the HWT, participants completed the demographic, employment, and incentive survey, identical to Experiment 2. When asked about the average amount of income earned per week on mTurk two participants in the D_{14} condition provided a range rather than a single value, which were recoded to equal the average of the range. One participant in the D_{01} condition failed to provide an average income and instead wrote "Not enough." Data for this participant were excluded from analyses containing weekly mTurk income because I was unable to generate a usable value based on this individual's response.

Experiential work task. After the demographic, employment, and incentive survey, participants completed the EWT. The only change to the EWT from the previous experiment was that the phrasing for each ratio differed slightly to reflect changes in the independent variable. For example, the D_{28} condition read, "Would you like to complete 20 slider questions in exchange for a 20 cent bonus paid for certain in 28 days?"

Payment. Participants received a base pay of \$0.25 for completing the practice trial, HWT, demographic, employment, and incentive survey, and providing *yes*/*no* responses to the 15 questions that preceded each ratio requirement in the EWT (i.e., “Would you like to complete…?”). Participants received the \$0.25 base pay 24 hr following participation regardless of the delay condition to which they were assigned. Therefore, participants in the D_{01} condition received base pay and incentive pay the same day. However, receipt of the incentive was delayed by 14 days for the D_{14} and 28 days for the D_{28} condition. Average completion time for the entire study, including the EWT, was 11 min and 39 s. Participants in the D_{01}, D_{14}, and D_{28} conditions earned an average of \$1.28 (SD = 0.57), \$1.15 (SD = 0.52), \$1.06 (SD = .49), respectively.

Data analyses. I completed identical analyses for breakpoint, accuracy, *AtD*, predictive validity, and a cost-benefit analysis as Experiment 2.

Results and Discussion

Of the 296 Workers who submitted access codes, eight were excluded for inconsistent responding on the HWT (D_{01} = 4; D_{14} = 3; D_{28} = 1), one participant in the D_{28} condition for inconsistent responding on the EWT, and two participants were excluded for inconsistent responding in both the HWT and EWT (D_{01} = 1; D_{28} = 1). After excluding the 11 participants with inconsistent responding from analyses (3.27%), 95 participants remained in each of the three delay conditions.

Demographic, employment, and incentive survey. Demographic information, mTurk employment characteristics, and non-mTurk employment and incentive history for the remaining 285 participants are listed by condition in Tables 7, 8, and 9, respectively. Participants ranged from 18 to 75 years of age (M = 32.56). Males accounted for 58.6% of the sample (n = 167) and females accounted for 41.4% of the sample (n = 118). The sample was predominantly Caucasian

(n = 238, 82.8%) and reported living in 43 of the 52 states and territories in the United States with the highest concentration of participants living in California (n = 33, 11.6%), Florida (n = 22, 7.7%), and Texas (n = 19, 6.7%). All participants held a least a high school diploma or equivalent certification with the largest percentage of participants having received a four-year college degree (n = 102, 35.8%). With respect to income, more than one quarter of participants reported earning an average annual household income of less than $30,000 per year ($n$ = 78, 27.4%). A series of Chi-Square analyses revealed that conditions did not differ significantly for gender, $\chi^2(2) = 1.24$, $p = .54$, ethnicity, $\chi^2(12) = 13.18$, $p = .36$, education, $\chi^2(14) = 8.67$, $p = .56$, or income, $\chi^2(8) = 6.01$, $p = .65$. A one-way ANOVA also indicated the conditions did not differ significantly in age, $F(2, 282) = 1.37$, $p = .26$.

Participants spent an average of 1 to 100 hours engaged in mTurk HITs per week (M = 19.6, SD = 14.13) with earnings averaging $98.25 per week (range: $0.00 to $680, SD = 92.34). Thus, hourly wages calculated based on average weekly time and compensation ranged between $0.00 and $25.00 an hour with an average $5.96 per hour. The most commonly reported reasons for completing mTurk HITs was as a source of secondary income (n = 175, 61.4%) and as a fruitful way to spend free time (n = 146, 51.2%).

Similar to Experiment 2, participants most frequently reported having held their most recent paid position for 1 to 3 years (n = 80, 28.1%) followed by tenure durations of 3 to 5 years (n = 62, 21.8%) and 5 to 10 years (n = 46, 16.1%). Five participants reported not having held a paid position in the previous 10 years and were excluded from subsequent data summaries involving employment outside of mTurk. Half of the participants reported never having received a promotion at their current or most recent place of employment (n = 142, 50.7%). The most frequently reported occupations included sales (n = 42, 15.0%), office/administrative support (n

= 31, 11.1%), and food preparation/serving (*n* = 30, 10.7%). Approximately one quarter of the participants reported leaving their last paid position because they accepted a new job at a different company (*n* = 74, 26.4%). None of the demographic variables assessed were significantly correlated with EWT breakpoint.

Sixty-three percent of participants reported monetary and/or non-monetary incentives were not available at their most recent paid position (*n* = 180). A quarter of the participants indicated monetary incentives were available (*n* = 78) and 16% reported the availability of non-monetary incentives (*n* = 46). Incentives delivered weekly (*n* = 26, 26.0%), monthly (*n* = 26, 26.0%), and twice yearly (*n* = 28, 28.0%), were the most common and reported with a similar frequency.

Breakpoint. Any participant whose responding did not break during the HWT or EWT was assigned a value of 256, the highest ratio requirement assessed. Responding for 12 participants did not reach a breakpoint during the HWT (D_{01} = 4; D_{14} = 2; D_{28} = 6) and one participant in both the D_{01} and D_{14} conditions did not reach a breakpoint in the EWT. Figure 24 displays breakpoint for the HWT and EWT for each delay condition separately. With a median of 15, breakpoint in the HWT was highest in the D_{01} condition (range: 1 to 256). Median HWT breakpoint was 10 and ranged from 0 to 256 work units for the D_{14} and D_{28} conditions. Despite a higher median breakpoint in the D_{01} condition, results of a Kruskal-Wallis one-way ANOVA suggest this difference was not statistically significant, $H(2) = 5.92$, $p = .052$. In contrast, median EWT breakpoint was 8 for all of the delay conditions, but results of a Kruskal-Wallis one-way ANOVA revealed a significant main effect of condition, $H(2) = 7.42$, $p = .024$. Based on a Dunn's multiple comparison test, differences in breakpoint were significant between the D_{01} and D_{28} conditions, $p = .02$, with higher breakpoints in the D_{01} condition. Results of the Wilcoxon matched pairs signed-rank tests indicated breakpoint was significantly higher in the HWT than the EWT for all three conditions, D_{01}, $W = -1360$, $p < .0001$; D_{14}, $W = -1583$, $p < .0001$; D_{28}, $W = -1487$, $p < .0001$.

Accuracy. Figure 25 displays the percentage of accurately completed work units across ratio requirements for all conditions. The D_{01} and D_{28} conditions followed a similar pattern in which accuracy showed an increasing trend from the practice trial up to a ratio requirement of 15 after which both conditions remained stable near 100% accurately completed work units. However, accuracy was slightly higher in the D_{01} condition with a median of 99.17% (range: 90.53 to 100%) than the D_{28} condition with a median of 95.86% (range: 93.41 to 100%). Accuracy was lowest in the D_{14} condition with a median of 85.53% (range: 35.42 to 100%). Percentage accuracy in the D_{14} condition was stable near 85% during the first half of the progression. Unlike the other delay conditions, responding became variable and decreased following ratio 12 and did not stabilize until ratio 64. Once stable, however, accuracy for the D_{14} condition was 100% for the remaining ratio requirements.

Of the 95 participants in each condition, 30, 38, and 19 participants inaccurately completed at least one work unit in the D_{01}, D_{14}, and D_{28} conditions, respectively. Few participants in the D_{01} ($n = 5$) and D_{28} ($n = 3$) conditions accurately completed more than half of work units. However, 14 participants in the D_{14} condition inaccurately completed more than 95% of work units. Said another way, most participants performed the work task with high accuracy and decrements to aggregate percentage accuracy was largely a function of a handful of participants who inaccurately completed nearly all of the work units.

Analog to demand. Figure 26 depicts *AtD* curves for delay conditions in the HWT and EWT pictured in the top and bottom panels, respectively. In the HWT, responding was most elastic in the D_{14} condition ($\alpha = 1.4 \times 10^{-4}$). Elasticity was equal in the D_{01} and D_{28} conditions ($\alpha = 1.1 \times 10^{-4}$) and did not differ significantly, $F(1, 28) = 0.02$, $p = .88$. Despite identical α values, I observed a significant difference in elasticity between the D_{14} and D_{01} conditions, $F(1, 28) = 4.4$, $p = .046$, but not between the D_{14} and D_{28} conditions, $F(1, 28) = 1.8$, $p = .19$.

Consistent with the α values from which they were derived, *EV*, P_{max}, and O_{max} were identical in the D_{01} and D_{28} conditions and lowest in the D_{14} condition. Based on the work function for the HWT pictured in Figure 27, I observed an equal response output at more than one ratio requirement in the D_{14} condition (ratio 10 and 20). In this case, the higher price (i.e., higher ratio requirement) was selected for empirical P_{max}. Although there may be reasons that favor using the lower ratio for empirical P_{max} (e.g., retaining a greater portion of the workforce), I used the higher ratio because the purpose of this line of research is to inform organizational incentive systems. From an organizational perspective, both ratio requirements will produce equal output. However, expenses would be lower in the higher ratio requirement. The higher ratio would better maximize the relative costs and benefits and would likely be viewed more favorably by an organization. As such, the selected method serves to frame my results in a manner consistent with which they are likely to be used in applied settings. Therefore, empirical P_{max} was equal to 20 in the D_{14} condition, this value was lower than the D_{01} and D_{28} conditions that were both equal to 256. Similarly, empirical O_{max} was lowest in the D_{14} condition and highest in the D_{28} condition. Although both measures generally followed a similar pattern, like the previous experiments, increases in the right tail of the expenditure curve led to empirical values that were often noticeably higher than the derived values. These increases were more pronounced in the conditions with a higher number of participants who did not reach a breakpoint (i.e., D_{28} and D_{01}). Elasticity and related indices are summarized in Table 10.

Within the EWT, I observed a significant main effect of delay on elasticity, $F(2, 37) = 11$, $p = .0002$. The main effect was attributable to significant pairwise comparisons between the D_{01} and D_{14} conditions, $F(1, 28) = 17$, $p = .0003$, and between the D_{01} and D_{28} conditions, $F(1, 23) = 17$, $p = .0004$, with responding being the most inelastic in the D_{01} condition in both cases. However elasticity was equivalent in the D_{14} and D_{28} conditions, $F(1, 23) = 0.009$, $p = .93$. Figure 28 displays the work functions for the three delay conditions in the EWT. In all three conditions, empirical P_{max} was lower than the derived values summarized in Table 10 whereas empirical O_{max} was higher than the derived value.

As highlighted in Figure 29, residual plots follow a systematic pattern in which data points in the HWT and EWT were consistently clustered above the best-fit curve and slightly increasing at low ratio requirements. At higher ratio requirements (above derived P_{max}), residuals in the EWT varied among conditions. Data in the D_{01} condition were generally well scattered above and below the best-fit curve line with clusters of no more than three data points above or below zero. Following derived P_{max}, residuals decreased in the D_{28} condition. The D_{14} condition followed the same pattern as the D_{28} condition, but I observed a subsequent increase back to zero at the higher end of the ratio requirements. For all conditions in the HWT, residuals for ratio requirements higher than derived P_{max} demonstrate a decreasing trend that was followed by a rapid increase during the highest ratios.

Similar to Experiment 2, I observed appreciable differences in elasticity between the HWT and EWT with responding being significantly more elastic in the EWT for all delay conditions, D_{01}, $F(1, 28) = 42$, $p < .0001$, D_{14}, $F(1, 28) = 74$, $p < .0001$, D_{28}, $F(1, 23) = 32$, $p < .0001$. These differences are shown in Figure 30. Empirical and derived demand indices were markedly lower in the EWT than the HWT within each delay condition.

Predictive validity. To examine the relation between breakpoint on the hypothetical and experiential work tasks, Figure 31 plots EWT breakpoint as a function of HWT breakpoint, separated by delay condition. Results of three separate Spearman rank-order correlations revealed a significant positive relation between responding on the HWT and the EWT for all three conditions, D_{01}, $r_s(95) = .45$, $p < .0001$; D_{14}, $r_s(95) = .62$, $p < .0001$; D_{28}, $r_s(95) = .42$, $p < .0001$. Despite significant correlations in all three delay conditions, participants frequently overestimated the amount of work they would complete (i.e., higher breakpoint in the HWT than EWT). At the aggregate level, frequent overestimations of work output resulted in marked decreases in percentage change in all conditions (Figure 32). Similar to Experiment 2, more than half of the participants in each condition overestimated how much they would complete in the EWT (range: D_{28}, 51.58% to D_{01}, 55.79%). I observed perfect correspondence in breakpoint between the HWT and EWT in approximately one quarter of participants in each condition (D_{01}, 25.26%; D_{14}, 24.21%; D_{28}, 24.21%).

Cost-benefit analysis. Figure 28 displays the relation between the work function on the left y-axis and cost on the right y-axis at each ratio requirement for all three conditions. Because work output was generally higher in the D_{01} condition, cost was also higher. However, work output and cost in the D_{14} and D_{28} conditions were generally equivalent through ratio 32. Between ratio requirements of 8 and 25, work output in D_{01} condition decreased slightly but remained relatively stable while cost decreased drastically. Thus, if it is feasible to deliver the incentive following a 1 day delay, selecting the D_{01} condition at a ratio requirement of 25 would appear to maximize benefits while minimizing cost. Several constraints in the workplace (e.g., payroll processing) may limit the feasibility of delivering monetary reinforcement after only one day. Perhaps—because of the wide range of possibilities—non-monetary incentives such as access to a preferred parking spot or allowing an employee to bring his or her dog to work may be amenable to relatively immediate delivery. It remains to be seen, however, if delayed non-monetary reinforcers result in response patterns similar to that found with monetary incentives in the present experiment. I observed numerous points of overlap between the D_{14} and D_{28} conditions for curves depicting both work output and cost despite the delay between the conditions. Two points of Figure 28 suggest the D_{14} condition may yield favorable results for an organization. First, at ratio values of 10 and 12, work functions for the D_{14} and D_{28} conditions diverge with greater output in the D_{14} condition. Second, by ratio 48, all participants in the D_{28} condition reached a breakpoint, whereas several participants in the D_{14} condition continued to respond at ratio 256.

In contrast with Experiment 2, I observed some differences in responding among incentive delay conditions. Responding was highest and more inelastic in the condition associated with the most immediate incentive delivery (i.e., D_{01}). Despite being consistent with previous research on the effects of reinforcer delay previously discussed, this effect was often small and not always significantly different than responding in the other conditions. For example, median HWT breakpoint was highest in the D_{01} condition, but the differences between the D_{01} condition and the other conditions was not significant. Median EWT breakpoint was equal among all conditions, but significantly higher in the D_{01} condition than the D_{28} condition. The significant differences, despite equal median breakpoint values, may be a result of the spread of the data, which are difficult to ascertain based on Figure 24. In fact, the highest breakpoint values were 96, 48, and 32, in the D_{01}, D_{14}, and D_{28} conditions, respectively. The effects of delays greater than one day were more variable, but never differed significantly from one another for the breakpoint or *AtD* analyses. The present data are tentative and more research is needed to support the findings. Nonetheless, two findings warrant additional discussion.

First, accuracy in the D_{14} condition was consistently lower than the D_{01} and D_{28} conditions, though typically remaining above 80%. This finding differs from Experiments 1 and 2 in which percentage accuracy was largely undifferentiated among conditions. Upon closer inspection of the data, the lower levels of accuracy observed in the D_{14} condition were likely due to a greater percentage of participants in that group who inaccurately completed nearly all work units. Unfortunately, based on the present data, it is not clear if the decreased accuracy in the D_{14} condition is related to the incentive delay.

Importantly, the determination that delays greater than one day did not drastically change participant responding might indicate a relative insensitivity to delay beyond a certain threshold. This result, if it holds in future research, may be viewed as a success from a business standpoint. Delays of 14 and 28 days are especially relevant to organizational incentive arrangements because organizational payroll systems commonly occur on a bi-weekly or monthly cycle. When distributed with fixed earnings, monetary incentive payouts follow similar delays. At least two interpretations may provide insight regarding the variables controlling such an insensitivity to increases in delay and suggest areas for future research.

First, Malott (2003) argues that employee responding is not controlled by direct-acting contingencies because the delay to reinforcement is simply too long. Instead, indirect-acting contingencies control behavior. Indirect-acting contingencies mediated by rules describing those contingencies may moderate the effects of longer delays to reinforcement in the workplace and account for the data observed in the present study. It might be that participants generated rules that functionally equated the value of the incentive despite the delays. A function-altering effect of rules in this way would likely lead to the present findings. Further research is needed to provide support for this interpretation.

Second, research in delay discounting commonly finds that money is discounted less steeply than other commodities (e.g., Charlton & Fantino, 2008). Results of a recent study by Holt, Glodowski, Smits-Seemann, and Tiry (2016) suggest this may be partially due to the fact that money is low in terms of perishability and high in terms of fungibility. Said another way, money can be used over long durations (i.e., in the future) and exchanged for a wide variety of goods or services (i.e., generalized conditioned reinforcer). It is possible that the same relative insensitivity to delay may not be observed when using other incentive types. For instance, a visa gift-card that expires in six months may not maintain the same levels of responding as cash. Although a gift-card can be used to purchase many other goods and services (highly fungible), it is also more perishable because it expires. This can further be contrasted with other non-monetary incentives. Providing an employee with access to a preferred parking spot for six months is perishable and not fungible and may result in marked decreases in responding at longer delays. Researchers in OBM may benefit from considering the delay discounting literature when making decisions about items used in an incentive arrangement.

The findings are in general agreement with previous research evaluating the effects of delay on work-related behavior within a behavioral economic demand framework. Although I did not observe significant differences in elasticity between the D_{01} and D_{28} conditions in the HWT, this effect was obtained in the EWT. Henley, DiGennaro Reed, Kaplan, et al. (2016) used a HWT to assess delays of 1 hr and 4 weeks with undergraduate participants. They found significant differences in elasticity with greater inelasticity in the 1-hr delay condition. An interesting feature of the present experiment that differs from Henley et al. was the use of mTurk workers, which may contribute to the differences in findings of the HWT.

As previously discussed, OBM research evaluating the effects of delayed incentives is largely lacking. Several researchers have evaluated the effects of delay on feedback (Krumhus & Malott, 1980; Mason & Redmon, 1992; Reid & Parsons, 1996), the findings of which may have implications for the use of delayed incentives. Research on delayed feedback suggests, when provided the option, 100% of participants chose to receive feedback delivered immediately rather than delayed (Reid & Parsons, 1996). Although the present study showed similarities in responding among delay conditions for a brief research session, different findings may emerge if participant preference impacted responding over longer periods (i.e., stronger preference for shorter delays may be correlated with or functionally related to responding). Quite possibly, employee acceptability with intervention procedures may effect intervention effectiveness and should be considered when evaluating the relative pros and cons of an incentive arrangement. If the difficulties of arranging incentive delivery every other week versus every four weeks are roughly equivalent in a given situation, Reid and Parsons' findings suggest that a relatively shorter delay should be selected. Evaluations of interactions between incentive delay and preference remain to be explored in future research.

One limitation of the present experiment involves the influence of the analog preparation on the accuracy of the cost-benefit analyses. Specifically, procedures for processing and delivering incentives to participants differed from real work settings. Paying participants in the present study required a few minutes per participant, and in fact, required slightly less work on the part of the experimenter in the D_{01} condition because there was no need to track the passage of time. Delivering monetary incentives in real world settings likely requires additional resources to expedite their delivery. The analog nature of the present experiment prevented me from quantifying and including such costs in the cost-benefit analysis. The significant increase in responding in the D_{01} condition may not be sufficient to offset the costs an organization would need to allocate to carry out such an arrangement.

General Discussion

The current experiments examined the relative efficacy of reinforcer dimensions within a behavioral economic demand framework and how parametric manipulations of their values differentially influence performance. The goal was to provide insight into potentially effective strategies for predicting and managing behavior in the workplace. The present studies also sought to evaluate the predictive validity of a hypothetical assessment. Experiment 1 tested the effects of three incentive magnitudes and found an orderly relation between magnitude and responding as well as high correspondence between responding using hypothetical and experiential demand assessment procedures. Experiments 2 and 3 evaluated the effects of parametric values of incentive probability and delay, respectively. When the magnitude of reinforcement was yoked, responding was comparable across multiple response measures (e.g., breakpoint, accuracy) regardless of probability. Results of Experiment 3 revealed small effects of incentive delay in favor of the more immediate incentive delivery. Observed responding for delays greater than one day were roughly equivalent.

Dimensions of Reinforcement

Although there are certainly exceptions, the results of these collective studies suggest reinforcer dimensions influence responding and their values should not be haphazardly selected. The most consistent effects of these parametric manipulations *within* each reinforcer dimension were observed between the highest and lowest values. In Experiment 1, breakpoint was consistently higher and elasticity consistently lower in the M_{20} condition relative to responding in the M_{05} condition in both the HWT and EWT. Although few comparisons between any of the conditions reached significance in Experiment 2, a significant difference in elasticity between the P_{90} and P_{10} conditions was observed in the HWT. Lastly, EWT breakpoint in Experiment 3 was significantly higher in the D_{01} condition compared to the D_{28} condition. The same effect of delay was observed in comparisons of elasticity in the EWT.

When comparing *among* dimensions assessed, the effects of incentive magnitude had the most consistent and discernable effect on responding with higher magnitude incentives being relatively more efficacious. Results of the incentive magnitude manipulations are in general agreement with the larger literature evaluating reinforcer amount (Fantino, 1977). Probability, however, yielded no significant differences between any conditions despite comparisons of widely disparate probability values (i.e., 10% and 90%). As briefly mentioned, yoking the incentive magnitude to equate the amount earned after the probabilistic contingency was applied may have rendered the conditions functionally equivalent. Magnitude then, may have exerted a relatively greater influence on responding compared to probability when associated with the same consequence. Although I observed some mixed effects of incentive delay, the present findings are in general agreement with the literature on delayed reinforcement (e.g., Chung & Herrnstein, 1967).

One goal of this line of research is to identify the reinforcer dimension that competes most effectively with concurrent sources of reinforcement in the workplace. A greater understanding of the relative efficacy of reinforcer dimensions and how parametric manipulations of their values differentially influence performance could lead to effective strategies for predicting and managing employee behavior. This information can then be used to maximize the effectiveness of incentive interventions. Neef and Noone Lutz (2001) provide a possible definition of what constitutes the most effective dimension which states, “[the] dimension that consistently produced the highest proportion of time allocation in relation to any of the other three dimensions with which it competed” (p. 58). Considering the differences in relative consistency and differentiation noted above, an extension of Neef and Noone Lutz’s definition to the current research supports an interpretation that work-related behavior may be more sensitive to manipulations of magnitude and delay than probability. Other researchers have made similar arguments (Catania, 1979; Kimble, 1961; Lattal, 2010). For example, Lattal suggests, “along with rate, quality, and magnitude, delay has been considered a primary determinant of the effectiveness of a reinforcer” (p. 129). Further research is needed to investigate relative contributions of these reinforcer dimensions on responding in the work environment.

Accuracy

The accuracy with which employees perform their job is of great importance to organizational leaders in a diverse range of fields, in part because of the tremendous impact inaccurate performance can have on the consumers of the goods or services organizations produce. For example, Makary and Daniel (2016) recently estimated more than 250,000 Americans die each year from preventable medical errors (e.g., inaccuracies during medical procedures or drug administration). This finding ranks medical mistakes as the third leading cause of death in the U.S. and underscores the importance of identifying interventions that support high levels of this socially significant organizational metric.

One important finding across all three experiments is the consistently high levels of accurate work completion. As evidenced by the data presented in Table 11, overall median accuracy never dropped below 85% for any condition. High levels of accuracy were maintained despite removing the contingency for correct responding in the practice trial in Experiments 2 and 3. This finding suggests that the contingency for accurate responding in the practice trial was not responsible for the high levels of accuracy in Experiment 1. The finding that accuracy remained high irrespective of magnitude, probability, or delay is potentially promising for organizations (recognizing the translational nature of the present study, of course). Additionally, findings of all three studies suggest that decrements in accuracy could primarily be attributed to a relatively small percentage of participants. From an organizational perspective, this is good news because it may allow for organizations to focus behavior change efforts (e.g., feedback) on a low number of workers. This approach would likely require fewer resources than if the entire workforce consistently made errors that required intervention.

However promising, it is possible that the contingencies in place for mTurk Workers contributed to the high levels of accuracy observed. Requesters may reject a HIT if Workers' performance is not considered acceptable. In addition to not receiving compensation for their work in these cases, Amazon tracks Workers' approval rate (i.e., the percentage of completed HITs accepted by requesters) and allows requesters to set a minimum acceptable level when posting a HIT; in the present experiments, Workers were required to have at least a 95% approval rate. Thus, having a low approval rate results in decreased opportunities to complete HITs. Workers may have responded accurately to avoid rejection of their work. Future research may wish to evaluate whether the percentage of accurately completed work varies as a function of Worker approval rates. It is also possible that decreases in accuracy would emerge if measured over longer periods of time. Thus, the effects of reinforcer dimensions on accuracy must be interpreted with some degree of caution.

HWT Predictive Validity

I also observed significant positive correlations between breakpoint on the HWT and EWT for every condition in all three experiments. The consistent correlations suggest the HWT is a promising method for future research evaluating the effects of reinforcer dimension and the application of demand curve analyses to work-related behavior. However, comparisons of changes in the predictive validity of the HWT across studies suggested several features of the experimental arrangement and methodology may have influenced the correspondence between the HWT and EWT. Each of these experimental features are discussed in detail below (see the section with a heading labeled "Experimental Preparation Influences on Predictive Validity").

An important point regarding the HWT is the conceptualization of this measure as having *predictive* validity. Predictive validity assesses the degree to which a measure predicts performance assessed at some point in the future (Crocker & Algina, 1986). Arguably, in the current preparation, the HWT may not be assessing predictive validity because the EWT was completed only a few minutes following the HWT. Unfortunately, it is unclear how long the duration between an initial assessment and performance measurement must be for the original assessment to be accurately conceptualized as having predictive validity. Because the purpose of the HWT is to draw inferences to behavior in the workplace, the HWT likely falls under the general umbrella of criterion-related validity (Crocker & Algina, 1986), but may also be conceptualized as concurrent validity. According to Crocker and Algina, concurrent validity is another type of criterion-related validity that:

> "refers to the relationship between test scores and criterion measurements made at the same time the test was given…a sufficient correlation between the two would justify the use of the test in place of the less efficient and more costly observation system" (p. 224).

The HWT, if proven useful, would likely function in place of less efficient methods (i.e., experiential demand assessments) but will be used to predict a certain level of performance in the future when the contingencies described in the HWT are implemented in the organization. Thus, both predictive and concurrent validity may be appropriate. However, only predictive validity is used throughout the present document for consistency.

Alternative Predictive Measures of Performance

One goal of the present experiments was to identify behavioral scales, demographic characteristics, or employment-related variables that have predictive validity for how individuals will respond on the EWT. In the first experiment, rates of delay discounting assessed via the monetary choice questionnaire were not significantly correlated with work output, and only one measure from the behavioral inhibition/behavioral activation scale (BIS/BAS) was significantly related to breakpoint. Given the limited findings from Experiment 1, the monetary choice questionnaire and the BIS/BAS were replaced with the demographic, employment, and incentive survey in Experiments 2 and 3. Unfortunately, the only variable significantly related to EWT breakpoint was completing mTurk HITs because participants find the tasks to be fun (Experiment 2 only). This finding could be interpreted as an indication that a possible relation exists between enjoyment of work responsibilities and output, which certainly has some face validity. Nonetheless, the question was specific to the completion of mTurk HITs and in its current form would likely not prove probative in traditional work settings. Moreover, this relation was not observed in Experiment 3. Identifying a measure predictive of work output through additional research could have implications for a number of organizational practices, such as personnel selection and hiring practices.

The paucity of research on personnel selection and hiring practices in OBM stands in contrast to the substantial extant literature on the topic in industrial and organizational psychology (DiGennaro Reed, Hirst, & Howard, 2013). Personnel selection represents a major responsibility that organizational leaders must effectively navigate, for which the OBM literature offers little guidance. The relative skill of employees can substantially impact the effectiveness of an organization. In fact, highly successful organizations like Google frequently allocate six-figure salaries to employ individuals whose primary role is to identify and recruit skilled staff. Quite possibly, increased scholarly attention to personnel selection in OBM could generate methods that, when implemented in practice, reduce the frequency or intensity of initial and ongoing training or interventions needed to address skill and performance deficits. Identification of such personnel selection methods would allow organizations to re-allocate valuable resources from recruitment efforts to empirically supported staff training and support programs. If this supposition holds, higher quality staff supports could lead to improvements in a host of socially significant issues that plague organizations, their employees, and consumers (e.g., turnover, workplace injuries, compromised treatment integrity). Such endeavors, if successful, may also serve to increase the exposure of OBM outside of behavior analysis given the premium businesses place on staff selection previously noted.

Contributions to the Literature

In spite of several noted inconsistencies between the current findings and previous research, these studies contribute to the literature in a number of ways. First, these studies are some of the only studies in OBM to systematically manipulate reinforcer dimensions in a way that allows for direct comparisons among parametric values. Second, the inclusion of the HWT to evaluate the correspondence between hypothetical and experiential demand preparations supports and extends previous research by allowing for a direct comparison of responding at every price. Research using experiential methods to validate hypothetical purchase tasks have typically only provided participants with one real outcome based on their responding on the hypothetical task—including Amlung and MacKillop's (2015) study on alcohol purchases described previously.

A third contribution of the present experiments was the inclusion of cost-benefit analyses. Cost is a major factor for organizations that influences intervention acceptability and implementation. Cost-benefit analyses provide organizational leaders with an efficient means of evaluating the economic feasibility of an intervention (Wells, Reimer, & Houmanfar, 2013). Despite numerous calls urging the use of such analyses (e.g., Andrasik, 1979; Balcazar, Shupert, Daniels, Mawhinney, & Hopkins, 1989; Poling, Smith, & Braatz, 1993), Wells and colleagues recently reported that just 10% of experimental OBM research published between 1977 and 2011 include cost-benefit analyses. Higher rates of reporting cost-benefit analyses may also hold OBM researchers accountable for developing interventions in a cost-sensitive manner, selecting only those interventions indicated by the function of the problem behavior.

A fourth contribution relates to the use of demand curve analyses. Consistent with the law of demand, the percentage of participants who completed the work requirement was inelastic and highest at a price of 1 and steadily decreased and became elastic with increases in the number of work units required to earn the incentive. I observed similar positively accelerating response patterns in the HWT and EWT across all conditions and experiments. Thus, despite parametric manipulations of reinforcer dimensions, responding in all cases was consistent with behavioral economic demand theory (Hursh, 1980). This finding extends previous research applying behavioral economic demand curve analyses to work-related behavior and monetary incentives in at least three ways. First, it evaluated a wider range of reinforcer magnitudes than Henley, DiGennaro Reed, Reed, et al. (2016) with the inclusion of a $0.20 incentive in Experiment 1. Second, Experiment 2 evaluated parametric values of incentive probability previously unexplored in this novel line of research. Third, these studies also included evaluations of incentive delay using a different work task and participant pool using an experiential arrangement in addition to examinations of delay using a HWT performed by Henley, DiGennaro Reed, Kaplan, et al. (2016).

Experimental Preparation Influences on Predictive Validity

As noted previously, an interesting finding across studies was the marked decrease in several measures of predictive validity from Experiment 1 to Experiments 2 and 3. For all conditions in Experiments 2 and 3, I observed significant differences in breakpoint between the HWT and EWT; striking differences in the *AtD* curves between the HWT and EWT; and pronounced decreases in percentage change. Results for all three analyses stand in contrast to the findings of Experiment 1. Despite high correlations between breakpoints, the consistent and pronounced differences in responding between the HWT and EWT call into question the utility of the HWT for use in an organization. Interestingly though, decreases in predictive validity measures across studies were even observed between the M_{20} and D_{01} conditions. Both conditions had a 100% likelihood of earning a $0.20 incentive at a relatively short delay; one day in the D_{01} condition and between one and three days in the M_{20} condition. The finding that predictive validity was high for all measures, but the same measures resulted in highly discrepant response patterns on the HWT despite experiencing very similar contingencies suggests this difference may not be a function of the reinforcer dimensions assessed in Experiments 2 and 3. Rather, several features of the experimental arrangement and methodology were changed after

Experiment 1 and may have influenced the correspondence between the HWT and EWT in Experiments 2 and 3. If the source of this discrepancy cannot be identified through additional research, and the same general level of overestimation is consistently observed, it may be possible to quantify the extent to which individuals overestimate work completion using hypothetical tasks. Organizational leaders may then account for this difference when using the HWT to predict work output and cost or researchers may be able to identify a method handling data for participants without a breakpoint to ameliorate some of the effects on predictive validity. Even so, the implications of this finding for research and application warrants additional attention.

Decreases in predictive validity in Experiments 2 and 3 co-occurred with increases in the numbers of participants who did not reach a breakpoint in the HWT. The percentage of participants who did not reach a breakpoint in the HWT was 0.69%, 4.9%, and 4.2% in Experiments 1, 2, and 3, respectively. Because several of the dependent measures were based on aggregate group performance, retaining data for these participants resulted in pronounced increases in projected performance and contributed to the disparity between the HWT and EWT. Two features of the HWT presentation might have influenced responding on the HWT. First, the HWT ratio requirements in Experiments 2 and 3 were presented simultaneously on a single page rather than presenting a single ratio requirement per page in a sequential manner. Skidmore and Murphy (2011), although evaluating alcohol consumption with a purchase task, used a similar arrangement in that the prices were presented in an ascending order and shown all at once. When Skidmore and Murphy presented the prices in this fashion, nearly a quarter of their sample (n = 47; 22.7%) were excluded from analyses because their R^2 values when fit to the demand equation fell below 0.3. In 2012, Gentile, Librizzi, and Martinetti sought to address the limitations of

Skidmore and Murphy, including how prices were presented to participants. In their extension, only one price was shown at a time. No participants were excluded due to unsystematic responding on the purchase task or low R^2 values. Although a seemingly minor variation, HPT research has consistently demonstrated changes in responding with minor procedural manipulations (e.g., Skidmore & Murphy, 2011). Thus, it is possible that presenting the HWT on a single page resulted in a higher number of participants who did not reach a breakpoint. It would be informative for future research to examine whether this difference systematically affects HWT responding. The fact that responding is highly sensitive to minor manipulations could be used as evidence against the use of hypothetical tasks as it may result in inconsistent or variable results. However, basic research on instructional control has also consistently demonstrated robust effects of minor variations on response patterns (e.g., Henley, Hirst, DiGennaro Reed, Becirevic, & Reed, 2016).

It may also be the case that requiring participants to respond to every ratio requirement in the HWT may have influenced predictive validity. This contingency increased the number of overall required responses and several participants noted frustration with this experimental feature when given the opportunity to provide feedback following study completion. For example, one participant commented, “I'm not sure why you asked me at the end the same questions you asked at the beginning, but if you had some good reason to ask them all again, and all on different pages, at least you could have stopped asking me once I said no - why would anyone possibly be uninterested in answering 10, but be *interested* in doing *more* than 10? That part was just totally a waste of time.” Responses such as these—though unclear if related to changes in predictive validity among experiments—may have implications for the social validity of HWT methodology and could inform any procedures recommended for use in practice.

Third, decreases in predictive validity may be due to the increased time between completion of the HWT and EWT as a result of requiring participants to complete the demographic, employment, and incentive survey between the tasks. Separating the work tasks was intended to mitigate sequence effects of completing the HWT prior to the EWT, but may have inadvertently decreased predictive validity. In real work environments, however, assessments intended to predict subsequent job performance would likely be separated in time by days, weeks, months, or even years. It is important to note that psychometric studies of predictive validity involve varying time differences suggesting this issue is likely not responsible for the low predictive validity I observed. Systematic manipulation and evaluation of how delay between assessment and performance influences predictive validity represents a logical and important area for future research on this topic. In addition, the aforementioned features may also have contributed to the increase in the number of participants who overestimated their work completion and contributed to the striking differences in breakpoint and elasticity between the HWT and EWT for all probability and delay conditions.

One possible feature contributing to the number of individuals who overestimated their responding involves the use of a PR schedule. A notable limitation of the use of the PR schedule in the present study involves the cumulative number of responses emitted as the ratio requirement increased within the session. Responding at each ratio requirement was not necessarily reflective of that reinforcement schedule as it may also have been influenced by each prior ratio presented in the session; a limitation tacted by several participants in the present studies. For example, when given the opportunity to provide feedback one participant noted, “Doing 25 sliders is not the same as doing the amount of each number up to 25.” Such responses raise the possibility that some participants may have been responding as if the requirement would be presented in isolation when prompted to indicate their willingness to complete increasing requirements in the HWT. When presented consecutively in the EWT, however, the experimental preparation may have led to lower breakpoints than might otherwise be observed if the ratio requirements were not presented in a progressive fashion within session. Future research could assess the predictive validity of the HWT using a derivation of the PR arrangement wherein participants complete progressively increasing ratio requirements during different sessions separated in time (Jarmolowicz & Lattal, 2010). Researchers have successfully used modified PR schedules wherein the response requirement is increased *between* rather than *within* session in human operant drug self-administration studies (e.g., Bickel & Madden, 1999). The effects of using a PR schedule, however relevant, does not explain why overestimations increased in Experiments 2 and 3 when the schedule remained unchanged from Experiment 1.

Limitations and Future Research

In addition to those already identified, several other limitations inform areas for future research. As mentioned previously, nonmonetary reinforcers are a flexible means for mediating budgetary restrictions associated with implementing an incentive system. Recall also that response effort may affect the allocation of employee responding among concurrently available work tasks that require varying levels of effort. A logical extension, therefore, would be to examine parametric effects of effort and quality.

Second, the current experimental preparation was designed to address the limitations of typical laboratory-based analog OBM research employing undergraduate participants in which limited concurrent response options capable of effectively competing with the simulated work task exist. Despite improvements, the preparation nonetheless differed form a real work environment in several potentially important ways. Additionally, because a similar preparation was used, all three experiments are subject to the same limitations regarding external validity some of which include the duration and complexity of the work task, relative immediacy of base pay (24 hr), and incentive amounts. It is also likely that progressive reinforcement contingencies are uncommon in the workplace. As a result, the analog arrangement limits conclusions about how the effects may generalize to real workplaces. However, this line of research is still in the early stages and transitioning to an applied setting at this point may result in unintended consequences. A related limitation of this arrangement is that I did not have control over the environment in which participants completed the experimental procedures. The variability in environmental conditions may have obscured differences that would otherwise emerged if participants completed study procedures under similar settings.

Third, despite observing consistent differences among magnitude conditions, increases in responding in the larger magnitude conditions were relatively modest in comparison to the relatively large differences in incentive amount. As illustrated by the reversed relation between response output and magnitude observed when using unit price, a one-unit increase in magnitude was met with a less than one-unit increase in response output. Small changes in behavior despite relatively large differences in reinforcer magnitude have been consistently documented in the literature since the early 1960s (Catania, 1963; Fantino & Logan, 1979), but this relation is typically observed in single operant arrangements. When researchers manipulate magnitude in a concurrent operant arrangement, however, large differences in responding emerge. Researchers have long commented on the advantages of concurrent operant arrangements as compared to single operant arrangements for examining the effects of manipulating reinforcer dimensions on behavior (e.g., Fisher & Mazur, 1997). Therefore, future investigations may wish to extend the current experimental paradigm to a concurrent operant arrangement. Extending choice procedures used to evaluate reinforcer dimensions and preference in other applied settings (e.g., Neef et al., 1994) to evaluations of incentive arrangements in the workplace may also provide valuable information on the social validity of such arrangements (Schwartz & Baer, 1991).

Results of several studies evaluating reinforcer dimensions in applied settings suggest the most efficacious dimension often differs for each participant (Neef et al., 1994; Neef & Noone Lutz, 2001). Based on these results, comparing the current findings to those obtained using within-subject methods may be an important contribution to the literature evaluating the effects of reinforcer dimensions on work-related responding. A within-subject approach may provide insight into how varying parametric levels within a reinforcer dimension as well as between dimensions may influence responding within-subject as compared to group responding. Because performance management interventions frequently target the behavior of individuals and groups any differences may help improve application technology.

AtD Model Fits

One final observation that warrants some discussion is the *AtD* model fits. Across the three experiments, R^2 values for *AtD* curves were consistently high, ranging from .74 to .98 (M = .93). As previously mentioned, R^2 may not be the most informative measure for evaluating the applicability and appropriateness of the exponential model of demand. Consistent with Motulsky and Cristopoulos' (2006) suggestion, I carefully inspected residual plots for all *AtD* model fits to evaluate whether residuals are randomly distributed around the best-fit curve.

Visual inspection of the residual plots for systematic deviations from the best-fit curves revealed an interesting pattern. In the EWT, residuals at lower prices demonstrate an increasing trend away from the curve fit, the peak of which closely aligns with derived P_{max}. Thus, the point at which consumption values derived from the exponential model deviate the most in the EWT occurs at the point of unit elasticity. This is also the point at which one would expect peak response output and has been suggested in research as the point with the greatest potential for applied utility.

This discrepancy may explain some of the inconsistencies with empirical and derived O_{max} in the EWT. Because consumption at P_{max} is used to quantify derived O_{max}, if the curve is consistently under-fitting the data at this point, derived O_{max} is calculated based on deflated consumption values that are not reflective of observed responding. For example, derived O_{max} for the P_{90} condition in the EWT was 226 work units at a price of 7.8. Despite similar derived and empirical P_{max} values, the number of work units completed at empirical O_{max} is approximately double.

The same extended sequence of increasing data points above the best fit line at low prices can also be seen in the HWT, however, the residuals at these values are more closely aligned with the best fit curve. This increase is followed by a stretch of negative residuals and subsequently a considerable positive deviation from the best fit at the highest prices. The pattern observed in the HWT was partially influenced by the number of individuals who did not reach a breakpoint. The presence of extreme values could also influence empirical and derived parameter estimates.

Cursory examination of previous research using the exponential model suggests this trend may not be unique to the current data (e.g., Hursh, 2014; Reed et al., 2014). If MacKillop and Murphy (2007) experienced a similar issue, it may explain their statement "data from our laboratory suggest that the observed parameters are more reliable than the derived values" (p. 229). Similar fitting issues have been noted in the delay discounting literature. For example, Odum (2011) suggests Mazur's (1987) discounting equation tends to overpredict indifference points at shorter delays and underpredict indifference points at longer delays. Ultimately, questions remain about how systematic deviations from the best fit line—particularly at P_{max}—may influence the applied utility of the exponential model and how it may be addressed in future research.

References

Alavosius, M., Getting, J., Dagen, J., Newson, W., & Hopkins, B. (2009). Use of a cooperative to interlock contingencies and balance the commonwealth. *Journal of Organizational Behavior Management, 29,* 193-211. doi: 10.1080/01608060902874575

Alvero, A. M., Bucklin, B. R., & Austin, J. (2001). An objective review of the effectiveness and essential characteristics of performance feedback in organizational settings. *Journal of Organizational Behavior Management, 21*(1), 3-29. doi: 10.1300/J075v21n01_02

Amlung, M. T., Acker, J., Stojek, M. K., Murphy, J. G., & MacKillop, J., (2012). Is talk 'cheap'?: An initial investigation of the equivalence of alcohol purchase task performance for hypothetical and actual rewards. *Alcoholism: Clinical and Experimental Research, 36,* 716–724. doi: 10.1111/j.1530-0277.2011.01656.x

Amlung, M. T., & MacKillop, J. (2015). Further evidence of close correspondence for alcohol demand decision making for hypothetical and incentivized rewards. *Behavioral Processes, 113,* 187-191. doi: 10.1016/j.beproc.2015.02.012

Andrasik, F. (1979). Organizational behavior modification in business settings: A methodological and content review. *Journal of Organizational Behavior Management*, *2*(2), 85–102.

Austin, J., Kessler, M. L., Riccobono, J. E., & Bailey, J. S. (1996). Using feedback and reinforcement to improve the performance and safety of a roofing crew. *Journal of Organizational Behavior Management, 16,* 49-75. doi: 10.1300/J075v16n02_04

Baer, D. M., Wolf, M, M., & Risley, T. R. (1968). Some current dimensions of applied behavior analysis. *Journal of Applied Behavior Analysis, 1,* 91–97. doi: 10.1901/jaba.1968.1-91

Balcazar, F., Hopkins, B. L., & Suarez, Y. (1985). A critical, objective review of performance feedback. *Journal of Organizational Behavior Management, 7*, 65-89. doi: 10.1300/J075v07n03_05

Balcazar, F., Shupert, M., Daniels, A., Mawhinney, T., & Hopkins, B. (1989). An objective review and analysis of ten years of publication in the *Journal of Organizational Behavior Management. Journal of Organizational Behavior Management*, *10*(1), 7–37.

Baum, W. M. (1979). Matching, undermatching, and overmatching in studies of choice. *Journal of the Experimental Analysis of Behavior, 32,* 269-281. doi: 10.1901/jeab.1979.32-269

Berger, C. J., Cummings, L. L., & Heneman, H. G. (1975). Expectancy theory and operant conditioning predictions of performance under variable ratio and continuous schedules of reinforcement. *Organizational Behavior and Human Performance, 14,* 227-243. doi: 10.1016/0030-5073(75)90026-4

Bickel, W. K., Jarmolowicz, D. P., Mueller, E. T., Koffarnus, M. N., & Gatchalian, K. M. (2012). Excessive discounting of delayed reinforcers as a trans-disease process contributing to addiction and other disease-related vulnerabilities: Emerging evidence. *Pharmacology & Therapeutics, 134,* 287-297. doi: 10.1016/j.pharmthera.2012.02.004

Bickel, W. K., Johnson, M. W., Koffarnus, M. N., MacKillop, J., & Murphy, J. G. (2014). The behavioral economics of substance use disorders: Reinforcement pathologies and their repair. *Annual Review of Clinical Psychology, 10(1)*, 641–677. doi: 10.1146/annurev-clinpsy-032813-153724

Bickel, W. K., & Madden, G. J. (1999). A comparison of measures of relative reinforcing efficacy and behavioral economics: cigarettes and money in smokers. *Behavioural Pharmacology, 10(6),* 627–638. doi:10.1097/00008877-199911000-00009

Black, A. C., & Rosen, M. I. (2011). A money management-based substance use treatment increases valuation of future rewards. *Addictive Behaviors*, 36, *125-128.* doi: 10.1016/j.addbeh.2010.08.014

Bruner, N. R., & Johnson, M. W. (2014). Demand curves for hypothetical cocaine in cocaine-dependent individuals. *Psychopharmacology, 231,* 889-897. doi: 10.1007/s00213-013-3312-5

Bucklin, B. R., & Dickinson, A. M. (2001). Individual monetary incentives. *Journal of Organizational Behavior Management, 21 (3),* 45-137. doi: 10.1300/J075v21n03_03

Bureau of Labor Statistics (2014). *American time use survey*. Retrieved from http://www.bls.gov/tus/charts/#work

Bureau of Labor Statistics (2016a). *Labor force statistics from the current population survey.* Retrieved from http://data.bls.gov/timeseries/LNS11300000

Bureau of Labor Statistics (2016b). *Occupational injuries and illnesses industry data*. Retrieved from http://data.bls.gov/timeseries/ISU00000000061100

Camden, M. C., & Ludwig, T. D. (2013). Absenteeism in health care: Using interlocking behavioral contingency feedback to increase attendance with certified nursing assistants. *Journal of Organizational Behavior Management, 33,* 165-184. doi: 10.1080/01608061.2013.814521

Cannella, H. I., O'Reilly, M. F., & Lancioni, G. E. (2005). Choice and preference assessment research with people with severe to profound disabilities: A review of the literature. *Research in Developmental Disabilities, 26,* 1-5. doi: 10.1016/j.ridd.2004.01.006

Carver, C. S., & White, T. L. (1994). Behavioral inhibition, behavioral activation, and affective responses to impending reward and punishment: The BIS/BAS scales. *Journal of Personality and Social Psychology, 67,* 319-333. doi: 10.1037/0022-3514.67.2.319

Catania, A. C. (1963). Concurrent performances: A baseline for the study of reinforcement magnitude. *Journal of the Experimental Analysis of Behavior, 6,* 299-300.

Catania, A. C. (1979). *Learning*. Englewood Cliffs, NJ: Prentice Hall.

Charlton, S. R., & Fantino, E. (2008). Commodity specific rates of temporal discounting: does metabolic function underlie differences in rates of discounting? *Behavioural Processes, 77,* 334–342.

Chase, H. W., MacKillop, J., & Hogarth, L. (2013). Isolating behavioral economic indices of demand in relation to nicotine dependence. *Psychopharmacology, 226,* 371-380. doi: 10.1007/s00213-012-2911-x

Chung, S., & Herrnstein, R. J. (1967). Choice and delayed reinforcement. *Journal of the Experimental Analysis of Behavior, 10,* 67-74.

Clayton, M. C., & Blaskewicz, J. (2012). The use of visual prompts to increase the cleanliness of restrooms on a college campus. *Journal of Organizational Behavior Management, 32,* 329-337. doi. 10.1080/01608061.2012.729393

Collins, R. L., Vincent, P. C., Yu, J., Liu, L., & Epstein, L. H. (2014). A behavioral economic approach to assessing demand for marijuana. *Experimental and Clinical Psychopharmacology, 22,* 211-221. doi: 10.1037/a0035318

Cook, T., & Dixon, M. R. (2006). Performance feedback and probabilistic bonus contingencies among employees in a human service organization. *Journal of Organizational Behavior Management, 25,* 45-63. doi: 10.1300/J075v25n03_04

Cooper, J. O., Heron, T. E., & Heward, W. L. (2006). *Applied behavior analysis.* Upper Saddle River, NJ: Merrill Prentice Hall.

Critchfield, T. S., & Reed, D. D. (2009). What are we doing when we translate from quantitative models? *The Behavior Analyst, 32,* 339-362.

Crocker, L., & Algina, J. (1986). *Introduction to classical and modern test theory.* Toronto: Holt, RineHart, and Winston, Inc.

Daniels, A. C. (2000). *Bringing out the best in people: How to apply the astonishing power of positive reinforcement.* New York, NY: McGraw-Hill.

Daniels, A. C., & Daniels, J. E. (2006) *Performance management*. Atlanta: Performance Management Publications.

Dib, N., & Sturmey, P. (2007). Reducing student stereotypy by improving teachers' implementation of discrete trial teaching. *Journal of Applied Behavior Analysis, 40,* 339–343. doi:10.1901/jaba.2007.52-06

Dickinson, A. M. (2001). The historical roots of organizational behavior management in the private sector. *Journal of Organizational Behavior Management, 20,* 9-58. doi: 10.1300/J075v20n03_02

Dickinson, A. M., & Gillette, K. L. (1994). A comparison of the effects of two individual monetary incentive systems on productivity: Piece rate pay versus base pay plus incentives. *Journal of Organizational Behavior Management*, *14* (1), 3-82. doi: 10.1300/J075v14n01_02

Dickinson, A. M., & Poling, A. D. (1996). Schedules of monetary reinforcement in organizational behavior management: Latham and Huber (1992) revisited. *Journal of Organizational Behavior Management, 16,* 17-91. doi: 10.1300/J075v16n01_05

Diener, L. H., McGee, H. M., & Miguel, C. F. (2009). An integrated approach for conducting a behavioral systems analysis. *Journal of Organizational Behavior Management*, *29*, 108–135. doi:10.1080/01608060902874534

DiGennaro Reed, F. D., & Henley, A. J. (2015). A survey of staff training and performance management practices: The good, the bad, and the ugly. *Behavior Analysis in Practice, 8,* 16-26. doi: 10.1007/s40617-015-0044-5

DiGennaro Reed, F. D., Henley, A. J., Hirst, J. M., Doucette, J. L., & Jenkins, S. R. (2015). Basic research considerations for performance management of staff. In F.D. DiGennaro Reed & D.D. Reed (Eds.), *Bridging the gap between science and practice in autism service delivery.* New York: Springer.

DiGennaro Reed, F. D., Hirst, J. M., & Howard, V. J. (2013). Empirically supported staff selection, training, and management strategies. In D. D. Reed, F. D. DiGennaro Reed, & J. K. Luiselli (Eds.), *Handbook of crisis intervention for individuals with developmental disabilities.* New York: Springer.

Doll, J., Livesey, J., McHaffie, E., & Ludwig, T. D. (2007). Keeping an uphill edge. *Journal of Organizational Behavior Management, 27,* 41-60. doi: 10.1300/J075v27n03_04

Downing, C. O., & Geller, E. S. (2012). A goal-setting and feedback intervention to increase ID-checking behavior: An assessment of social validity and behavioral impact. *Journal of Applied Behavior Analysis, 32,* 297-306. doi: 10.1080/01608061.2012.729383

Epstein, L. H., Dearing, K. K., & Roba, L. G. (2010). A questionnaire approach to measuring the relative reinforcing efficacy of snack foods. *Eating Behaviors, 11,* 67-73. doi: 10.1016/j.eatbeh.2009.09.006

Evans, K. M., Kienast, P., & Mitchell, T. R., (1988). The effects of lottery incentive programs on performance. *Journal of Organizational Behavior Management, 9,* 113-135. doi: 10.1300/J075v09n02_08

Fantino, E. (1977). Conditioned reinforcement: Choice and information. In W. Honig & J. Staddon (Eds.), *Handbook of Operant Behavior* (pp. 313-339). Englewood Cliffs, NJ: Prentice-Hall.

Fantino, E., & Logan, C. L. (1979). Choice behavior. In E. Fantino, & C. Logan (Eds.), *The experimental analysis of behavior: A biological perspective* (208-241). San Francisco: Freeman.

Ferster, C. B., & Skinner, B. F. (1957). *Schedules of reinforcement.* New York: Appleton-Century-Crofts.

Fisher, W. W., & Mazur, J. E. (1997). Basic and applied research on choice responding. *Journal of Applied Behavior Analysis, 30,* 387-410.

Friman, P. C., & Poling, A. (1995). Making life easier with effort: Basic findings and applied research on response effort. *Journal of Applied Behavior Analysis, 28,* 583-590. doi: 10.1901/jaba.1995.28-583

Gentile, N. D., Librizzi, E. H., & Martinetti, M. P. (2012). Academic constraints on alcohol consumption in college students: A behavioral economic analysis. *Experimental and Clinical Psychopharmacology, 20(5),* 390–399. doi:10.1037/a0029665

Goomas, D. T., & Ludwig, T. D. (2007). Enhancing incentive programs with proximal goals and immediate feedback. *Journal of Organizational Behavior Management, 27,* 33-68. doi: 10.1300/J075v27n01_02

Grace, R. C., Schwendiman, J. W., & Nevin, J. A. (1998). Effects of unsignaled delay of reinforcement on preference and resistance to change. *Journal of the Experimental Analysis of Behavior*, *69* (3), 247-261. doi: 10.1901/jeab.1998.69-247

Greenwald, M. K., & Hursh, S. R. (2006). Behavioral economic analysis of opioid consumption in heroin-dependent individuals: Effects of unit price and pre-session drug supply. *Drug and Alcohol Dependence*, *85* (1), 35-48. doi: 10.1016/j.drugalcdep.2006.03.007

Gresham, F. M., Gansle, K. A., Noell, G. H., Cohen, S., & Rosenblum, S. (1993). Treatment integrity of school-based behavioral intervention studies: 1980–1990. *School Psychology Review, 22,* 254–272.

Grindle, A. C., Dickinson, A. M., & Boettcher, W. (2000). Behavioral safety research in manufacturing settings. *Journal of Organizational Behavior Management, 20,* 29-68. doi: 10.1300/J075v20n01_03

Henley, A. J., & DiGennaro Reed, F. D. (2015). Should you order the feedback sandwich? Efficacy of feedback sequence and timing. *Journal of Organizational Behavior Management, 35,* 321-335. doi: 10.1080/01608061.2015.1093057

Henley, A. J., DiGennaro Reed, F. D., Kaplan, B. A., & Reed, D. D. (2016). Quantifying efficacy of workplace reinforcers: An application of behavioral economic demand to evaluate hypothetical work performance. *Translational Issues in Psychological Science, 2,* 174-183. doi: 10.1037/tps0000068

Henley, A. J., DiGennaro Reed, F. D., Reed, D. D., & Kaplan, B. A. (2016). A crowdsourced nickel-and-dime approach to Analog OBM research: A behavioral economic framework for understanding workforce attrition. *Journal of the Experimental Analysis of Behavior. 106,* 134-144. doi: 10.1002/jeab.220

Henley, A. J., Hirst, J. M., DiGennaro Reed, F. D., Becirevic, A., & Reed, D. D. (2016). Function-altering effects of rule phrasing in the modulation of instructional control. *The Analysis of Verbal Behavior.* Advance online publication. doi: 10.1007/s40616-016-0063-5

Hirst, J. M., & DiGennaro Reed, F. D. (2016). Cross-commodity discounting of monetary outcomes and access to leisure activities. *The Psychological Record, 66,* 515-526. doi: 10.1007/s40732-016-0201-4

Hodos, W. (1961, September 29). Progressive ratio as a measure of reward strength. *Science, 134,* 943-944. doi: 10.1126/science.134.3483.943

Holt, D. D., Glodowski, K., Smits-Seemann, R. R., & Tiry, A. M. (2016). The domain effect in delay discounting: The roles of fungibility and perishability. *Behavioural Processes, 131,* 47-52.

Horner, R. H., & Day, H. M. (1991). The effects of response efficiency on functionally equivalent competing behaviors. *Journal of Applied Behavior Analysis, 24,* 719-732. doi: 10.1901/jaba.1991.24-719

Hursh, S. R. (1980). Economic concepts for the analysis of behavior. *Journal of the Experimental Analysis of Behavior, 34*, 219-238. doi: 10.1901/jeab.1980.34-219

Hursh, S. R. (1984). Behavioral economics. *Journal of the Experimental Analysis of Behavior, 42,* 435-452. doi: 10.1901/jeab.1984.42-435

Hursh, S. R. (2014). Behavioral economics and the analysis of consumption and choice. In F. K. McSweeney & E. S. Murphy (Eds.), *The Wiley Blackwell handbook of operant and classical conditioning* (pp. 275–305). Hoboken, NJ: Wiley.

Hursh, S. R., & Fantino, E. (1973). Relative delay of reinforcement and choice. *Journal of the Experimental Analysis of Behavior, 19,* 437-450. doi: 10.1901/jeab.1973.19-437

Hursh, S. R., Galuska, C. M., Winger, G., & Woods, J. H. (2005). The economics of drug abuse: A quantitative assessment of drug demand. *Molecular Interventions, 5,* 20-28. doi: 10.1124/mi.5.1.6

Hursh, S. R., Madden, G. J., Spiga, R., DeLeon, I. G., & Francisco, M. T. (2013). The translational utility of behavioral economics: Experimental analysis of consumption and choice. In G. Madden, W. Dube, T. Hackenberg, G. Hanley, & K. Lattal (Eds). *APA handbook of behavior analysis, Vol. 2: Translating principles into practice* (pp. 191-224). Washington, DC: American Psychological Association.

Hursh, S. R., Raslear, T. G., Bauman, R., & Black, H. (1989). The quantitative analysis of economic behavior with laboratory animals. In K. Grunert & F. Olander (Eds.), *Understanding economic behaviour* (pp. 117-1657). New York, NY, US: Kluwer Academic Publishers, Theory and Decision Library. doi: 10.1007/978-94-009-2470-3_22

Hursh, S. R., & Roma, P. G. (2016). Behavioral economics and the analysis of consumption and choice. *Managerial and Decision Economics*, *37*(4-5), 224-238. doi: 10.1002/mde.2724

Hursh, S. R., & Silberberg, A. (2008). Economic demand and essential value. P*sychological Review, 115 (1),* 186-198. doi: 10.1037/0033-295X.115.1.186

Huselid, M. A. (1995). The impact of human resource management practices on turnover, productivity, and corporate financial performance. *Academy of Management Journal¸ 38 (3),* 635-672. doi: 10.2307/256741

Ingersoll, R., Merrill, L., & Stuckey, D. (2014). *Seven trends: The transformation of the teaching force* (April 2014 ed). Retrieved from http://www.cpre.org/sites/default/files/workingpapers/1506_7trendsapril2014.pdf

Ipeirotis, P. (2010). Demographics of Mechanical Turk. (CeDER Working Paper-10-01). New York University. Retrieved from: http://hdl.handle.net/2451/29585.

Jacobs, E. A., & Bickel, W. K. (1999). Modeling drug consumption in the clinic using simulation procedures: Demand for heroin and cigarettes in opioid-dependent outpatients. *Experimental and Clinical Psychopharmacology, 7,* 412-426. doi: 10.1037//1064-1297.7.4.412

Jarmolowicz, D. P., & Lattal, K. A. (2010). On distinguishing progressively increasing response requirements for reinforcement. *The Behavior Analyst, 33(1),* 119–125.

Jarmolowicz, D. P., Reed, D. D., DiGennaro Reed, F. D., & Bickel, W. K. (2015). The behavioral and neuroeconomics of reinforcer pathologies: Implications for managerial and health decision making. *Managerial and Decision Economics, 37,* 274-293. doi: 10.1002/mde/2716

Johnson, C. M., Mawhinney, T. C., & Redmon, W. K. (2001). Introduction to organizational performance: Behavior analysis and management. In C. Johnson, W. Redmon, & T. Mawhinney (Eds.), *Handbook of organizational performance: Behavior analysis and management* (pp. 3-22). New York, NY: The Hawthorn Press, Inc.

Johnson, D. A. (2013). A component analysis of the impact of evaluative and objective feedback on performance. *Journal of Organizational Behavior Management, 33,* 89-103. doi: 10.1080/01608061.2013.785879

Johnson, D. A. (2015). Considerations in feedback delivery: The role of accuracy and type of evaluation. *Journal of Organizational Behavior Management, 35,* 240-258. doi: 10.1080/01608061.2015.1093055

Johnson, D. A., Casella, S. E., McGee, H., & Lee, S. C. (2014). The use and validation of preintervention diagnostic tools in organizational behavior management. *Journal of Organizational Behavior Management*, *34* (2), 104-121. doi: 10.1080/01608061.2014.914009

Johnson, M. W., & Bickel, W. K. (2008). An algorithm for identifying nonsystematic delay-discounting data. *Experimental Clinical Psychopharmacology, 16,* 264-274. doi: 10.1037/1064-1297.16.3.264

Johnson, M. W., & Bruner, N. R. (2012). The sexual discounting task: HIV risk behavior and the discounting of delayed sexual rewards in cocaine dependence. *Drug Alcohol Depend, 123,* 15-21. doi: 10.1016/j.drugalcdep.2011.09.032

Kaplan, B. A., Lemley, S. M., Reed, D. D., & Jarmolowicz, D. P. (2014). 2-1 and 27-item monetary choice questionnaire automated scorers [spreadsheet application]. Retrieved from: http://hdl.handle.net/1808/15424.

Kaplan, B. A., & Reed, D. D. (2014). Essential value, P_{max}, and O_{max} automated calculator [spreadsheet application]. Retrieved from: http://hdl.handle.net/1808/14934.

Kaplan, E. L., & Meier, P. (1958). Nonparametric estimation for incomplete observations. *Journal of the American Statistical Association, 53,* 457–481. doi: 10.1080/01621459.1958.10501452

Kimble, G. A. (1961). *Hilgard and Marquis'* Conditioning and Learning. New York: Appleton Century Crofts.

Kirby, K. N., Petry, N. M., & Bickel, W. K. (1999). Heroin addicts have higher discount rates for delayed rewards than non-drug-using controls. *Journal of Experimental Psychology: General, 128,* 78-87. doi: 10.1037/0096-3445.128.1.78

Koffarnus, M. N., Franck, C. T., Stein, J. S., & Bickel, W. K. (2015). A modified exponential behavioral economic demand model to better describe consumption data. *Experimental and Clinical Psychopharmacology, 23,* 504-512. doi: 10.1037/pha0000045

Kortick, S. A., & O'Brien, R. M. (1996). The world series of quality control: A case study in the package delivery industry. *Journal of Organizational Behavior Management, 16,* 77-93. doi: 10.1300/J075v16n02_05

Krumhus, K. M., & Malott, R. W. (1980). The effects of modeling and immediate and delayed feedback in staff training. *Journal of Organizational Behavior Management, 2,* 279-293. doi: 10.1300/J075v02n04_05

LaMere, J. M., Dickinson, A. M., Henry, M., Henry, G., & Poling, A. (1996). Effects of multicomponent monetary incentive program on the performance of truck drivers. *Behavior Modification, 4,* 385-405.

Lane, K. F., & Andrew, J. E. (1955). A method of labour turnover analysis. *Journal of the Royal Statistical Society. Series A (General), 118* (3), 296-323. doi: 10.2307/2342495

Latham, G. P., & Dossett, D. L. (1978). Designing incentive plans for unionized employees: A comparison of continuous and variable ratio reinforcement schedules. *Personnel Psychology, 31,* 47-61. doi: 10.1111/j.1744-6570.1978.tb02108.x

Lattal, K. A. (2010). Delayed reinforcement of operant behavior. *Journal of the Experimental Analysis of Behavior, 93,* 129-139.

Lea, S. E. G. (1978). The psychology and economics of demand. *Psychological Bulletin, 85,* 441-466. doi: 10.1037/0033-2909.85.3.441

Lee, J., & Oah. S. (2015). A comparison of the effects of incentive and penalty procedures on work performance: A simulation. *Journal of Organizational Behavior Management, 35,* 336-345. doi: 10.1080/01608061.2015.1093056

Loafman, B. (1996). Rescue from the safety plateau. *Performance Management Magazine, 14*(3), 3-10.

Luiselli, J. K., DiGennaro Reed, F. D., Christian, W. P., Markowski, A., Rue, H. C., St. Amand, C., & Ryan, C. J. (2009). Effects of an informational brochure, lottery-based financial incentive, and public posting on absenteeism of direct-care human services employees. *Behavior Modification, 33*, 175–181. doi: 10.1177/0145445508320624

MacKillop J., & Murphy, J. G. (2007). A behavioral economic measure of demand for alcohol predicts brief intervention outcomes. *Drug and Alcohol Dependence, 89,* 227-233. doi: 10.1016/j.drugalcdep.2007.01.002

MacKillop, J., Murphy, J. G., Ray, L. A., Eisenberg, D. T., Lisman, S. A., Lum, J. K., & Wilson, D. S. (2008). Further validation of a cigarette purchase task for assessing the relative reinforcing efficacy of nicotine in college smokers. *Experimental and Clinical Psychopharmacology, 16,* 57-65. doi: 10.1037/1064-1297.16.1.57

MacKillop, J., O'Hagen, S., Lisman, S. A., Murphy, J. G., Ray, L. A., Tidey, J. W., & Monti, P. M. (2010). Behavioral economic analysis of cue-elicited craving for alcohol. *Addiction, 105,* 1599–1607. doi:10.1111/j.1360-0443.2010.03004.x

Madden, G. J., & Bickel, W. K. (2010). *Impulsivity: The behavioral and neurological science of discounting.* Washington, DC: American Psychological Association.

Madden, G. J., & Kalman, D. (2010). Effects of bupropion on simulated demand for cigarettes and the subjective effects of smoking. *Nicotine & Tobacco Research, 12(4),* 416-422. doi: 10.1093/ntr/ntq018

Makary, M. A., & Daniel, M. (2016). Medical error—the third leading cause of death in the US. *BMJ*, *353*, 1-5. doi: 10.1136/bmj.i2139

Mager, R. F., & Pipe, P. (1970). *Analyzing performance problems or 'You really oughta wanna'.* Belmont, CA: Fearon.

Malott, M. (2003). *The paradox of organizational change: Engineering organizations with behavioral systems analysis.* Context Press: Reno, NV. Malott, R. W. (1993). A theory of rule-governed behavior and organizational behavior management. *Journal of Organizational Behavior Management, 12,* 45-65. doi: 10.1300/J075v12n02_03

Malott, R. W. (1993). A theory of rule-governed behavior and organizational behavior management. *Journal of Organizational Behavior Management, 12,* 45-65.

Manevska-Tasevska, G., Hansson, H., & Labajova, K. (2016). Impact of management practices on persistent and residual technical efficiency-a study of Swedish pig farming. *Managerial and Decision Economics.* Advanced online publication. doi: 10.1002/mde.2823

Mason, M. A., & Redmon, W. K. (1992). Effects of immediate versus delayed feedback on error detection accuracy in a quality control simulation. *Journal of Organizational Behavior Management, 13,* 49-83. doi: 10.1300/J075v13n01_05

Matthews, G. A., & Dickinson, A. M. (2000). Effects of alternative activities on time allocated to task performance under different percentages of incentive pay. *Journal of Organizational Behavior Management, 20 (1),* 3-27. doi: 10.1300/J075v20n01_02

Mazur, J. E. (1987). An adjusting procedure for studying delayed reinforcement. In M.L. Commons, J.E. Mazur, J.A. Nevin, & H. Rachlin (Eds.), *Quantitative analysis of behavior: Vol. 5. The effect of delay and of intervening events of reinforcement value* (pp. 55–73). Hillsdale, NJ: Erlbaum.

Mazur, J. E. (1993). Predicting the strength of a conditioned reinforcer: Effects of delay and uncertainty. *Current Directions in Psychological Science, 2,* 70-74.

Miller, M. V., Carlson, J., & Sigurdsson, S. O. (2014). Improving treatment integrity in a human service setting using lottery-based incentives. *Journal of Organizational Behavior Management*, *34* (1), 29-38. doi: 10.1080/01608061.2013.873381

Moncrief III, W. C., Hoverstad, R., & Lucas Jr, G. H. (1989). Survival analysis: A new approach to analyzing sales force retention. *Journal of Personal Selling & Sales Management*, *9* (2), 19-30. doi: 10.1080/08853134.1989.10754521

Motulksy, H. J. (2013). *Intuitive biostatistics: A nonmathematical guide to statistical thinking (3rd ed.).* Oxford University Press.

Motulsky, H. J. (2017). Sample size for nonparametric tests, *GraphPad Statistics Guide*. Retrieved from: http://www.graphpad.com/guides/prism/7/statistics/index.htm?stat_sample_size_for_non parametric_.htm

Motulsky, H. J., & Christopoulis, A. (2006). *Fitting models to biological data using linear and nonlinear regression: A practical guide to curve fitting.* San Diego, CA: GraphPad Software.

Murphy, J. G., & MacKillop, J. (2006). Relative reinforcing efficacy of alcohol among college student drinkers. *Experimental and Clinical Psychopharmacology, 14,* 219-227. doi: 10.1037/1064-1297.14.2.219

Murphy, J. G., MacKillop, J., Skidmore, J. R., & Pederson, A. A. (2009). Reliability and validity of demand curve measure of alcohol reinforcement. *Experimental and Clinical Psychopharmacology, 14,* 394-404. doi: 10.1037/a0017684

Murphy, J. G., MacKillop, J., Tidey, J. W., Brazil, L. A., & Colby, S. M. (2011). Validity of a demand curve measure of nicotine reinforcement with adolescent smokers. *Drug and Alcohol Dependence, 113,* 207-214. doi: 10.1016/j.drugalcdep.2010.08.004

National Safety Council (2015). *Injury facts, 2015 edition.* Itasca, IL: Author.

Neef, N. A., Mace, F. C., & Shade, D. (1993). Impulsivity in students with serious emotional disturbance: The interactive effects of reinforcer rate, delay, and quality. *Journal of Applied Behavior Analysis, 26,* 37-52. doi: 10.1901/jaba.1993.26-37

Neef, N. A., & Noone Lutz, M. (2001). A brief computer-based assessment of reinforcer dimensions affecting choice. *Journal of Applied Behavior Analysis, 34,* 57-60.

Neef, N. A., Shade, D., & Miller, M. S. (1994). Assessing influential dimensions of reinforcers on choice in students with serious emotional disturbance. *Journal of Applied Behavior Analysis, 27,* 575-583. doi: 10.1901/jaba.1994.27-575

Nevin, J. A. (1988). Behavioral momentum and the partial reinforcement effect. *Psychological Bulletin*, *103,* 44–56. doi: 10.1037/0033-2909.103.1.44

Nevin, J. A. (2008). Control, prediction, order, and the joys of research. *Journal of the Experimental Analysis of Behavior, 89,* 119-123. doi: 10.1901/jeab.2008.89-119

Oah, S., & Dickinson, A. M. (1992). A comparison of the effects of a linear and an exponential performance pay function on work productivity. *Journal of Organizational Behavior Management, 12,* 85-123. doi: 10.1300/J075v12n01_05

Oah, S., & Lee, J. H. (2011). Effects of hourly, low-incentive, and high-incentive pay on simulated work productivity: Initial findings with a new laboratory method. *Journal of Organizational Behavior Management*, *31* (1), 21-42. doi: 10.1080/01608061.2011.541820

Odum, A. L. (2011). Delay discounting: I'm a k, you're a k. *Journal of the Experimental Analysis of Behavior, 96,* 427-439. doi: 10.1901/jeab.2011.96-423

Orpen, C. (1974). The effect of reward contingencies on the job satisfaction-task performance relationship: An industrial Experiment. *Psychology: A Journal of Human Behavior.*

Pampino, R. N. Jr., MacDonald, J. E., Mullin, J. E., & Wilder, D. A. (2004). Weekly feedback vs. daily feedback. *Journal of Organizational Behavior Management, 23,* 21-43. doi: 10.1300/J075v23n02_03

Paolacci, G., & Chandler, J. (2014). Inside the Turk: Understanding Mechanical Turk as a participant pool. *Current Directions in Psychological Science*, *23* (3), 184-188. doi: 10.1177/0963721414531598

Poling, A., Smith, J., & Braatz, D. (1993). Data sets in organizational behavior management: Do we measure enough? *Journal of Organizational Behavior Management, 14(1),* 99–116

Poling, A., & Braatz, D. (2001). Principles of learning: Respondent and operant conditioning and human behavior. In C. Johnson, W. Redmon, & T. Mawhinney (Eds.), *Handbook of organizational performance: Behavior analysis and management* (pp. 23-49). New York, NY: The Hawthorn Press, Inc.

Pritchard, R. D., DeLeo, P. J., & Von Bergen, C. W. (1976). A field experimental text of expectancy-valence incentive motivation techniques. *Organizational Behavior and Human Performance, 15,* 355-406.

Pritchard, R. D., Hollenback, J., & DeLeo, P. J. (1980). The effects of continuous and partial schedules of reinforcement on effort, performance, and satisfaction. *Organizational Behavior and Human Performance, 25,* 336-353. doi: 10.1016/0030-5073(80)90032-X

Prue, D. M., & Fairbank, J. A. (1981). Performance feedback in organizational behavior management: A review. *Journal of Organizational Behavior Management, 3,* 1-16. doi: 10.1300/J075v03n01_01

Rachlin, H., Raineri, A., & Cross, D. (1991). Subjective probability and delay. *Journal of the Experimental Analysis of Behavior*, *55*, 233–244. doi:10.1901/jeab.1991.55-233

Ray, R. L., Rizzacasa, T., & Levanon, G. (2013). *Job satisfaction: 2013 edition* (Report No. TCB_R-1524-33-RR). Retrieved from http://www.conferenceboard.org/publications/publicationdetail.cfm?publicationid=2522

Reed, D. D., Critchfield, T. S., & Martens, B. K. (2006). The generalized matching law in elite sport competition: Football play calling as operant choice. *Journal of Applied Behavior Analysis, 39,* 281-297. doi: 10.1901/jaba.2006.146-05

Reed, D. D., DiGennaro Reed, F. D., Campisano, N., LaCourse, K., & Azulay, R. L. (2012). Assessing and increasing staff preference for job tasks using concurrent-chains schedules and probabilistic outcomes. *Journal of Organizational Behavior Management, 32,* 253-262. doi: 10.1080/01608061.2012.698121

Reed, D. D., Kaplan, B. A., & Becirevic, A. (2015). Basic research on preference and reinforcer demand functions. In F. D. DiGennaro Reed & D. D. Reed (Eds.), *Bridging the gap between science and practice in autism service delivery*. New York, NY: Springer.

Reed, D. D., Kaplan, B. A., Roma, P. G., & Hursh, S. R. (2014). Inter-method reliability of progression sizes in a hypothetical purchase task: Implications for empirical public policy. *The Psychological Record, 65*(4), 671-679. doi: 10.1007/s40732-014-0076-1

Reed, D. D., Niileksela, C., & Kaplan, B. A. (2013). Behavioral economics: A tutorial for behavior analysts in practice. *Behavior Analysis in Practice*, *6,* 34-54.

Reed, D. D., Partington, S. W., Kaplan, B. A., Roma, P. G., & Hursh, S. R. (2013). Behavioral economic analysis of demand for fuel in North America. *Journal of Applied Behavior Analysis, 46,* 1-5. doi: 10.1002/jaba.64

Reid, D. H., & Parsons, M. B. (1996). A comparison of staff acceptability of immediate versus delayed verbal feedback in staff training. *Journal of Organizational Behavior Management, 16,* 35-47. doi: 10.1300/J075v16n02_03

Riedel, J. A., Nebecker, D. M., & Cooper, B. L. (1988). The influence of monetary incentives on goal choice, goal commitment, and task performance. *Organizational Behavior and Human Decision Processes, 42,* 155-180. doi: 10.1016/0749-5978(88)90010-6

Roane, H. S., Lerman, D. C., & Vorndran, C. M. (2001). Assessing reinforcers under progressive schedule requirements. *Journal of Applied Behavior Analysis, 18,* 249-255. doi: 10.1901/jaba.2001.34-145

Roma, P. G., Hursh, S. R., & Hudja, S. (2016). Hypothetical purchase task questionnaires for behavioral economic assessments of value and motivation. *Managerial and Decision Economics*, *37,* 306-323. doi: 10.1002/mde.2718

Roma, P. G., Reed, D. D., DiGennaro Reed, F. D., & Hursh, S. R. (2017). Progress of and prospects for hypothetical purchase task questionnaires in consumer behavior analysis and public policy. *The Behavior Analyst*. Advanced online publication. doi: 10.1007/s40614-017-0100-2.

Saari, L. M., & Latham, G. P. (1982). Employee reactions to continuous and variable ratio reinforcement schedules involving a monetary incentive. *Journal of Applied Psychology, 67,* 506-508. doi: 10.1037/0021-9010.67.4.506

Samuelson, P. A., & Nordhaus, W. D. (1985). *Economics*. New York, NY: McGraw-Hill.

Schwartz, I. S., & Baer, D. M. (1991). Social validity assessments: Is current practice state of the art? *Journal of Applied Behavior Analysis, 24,* 189–204.

Shook, G. L., Johnson, M., & Uhlman, W. F. (1978). The effect of response effort reduction, instructions, group and individual feedback, and reinforcement on staff performance. *Journal of Organizational Behavior Management, 1,* 206-215. doi: 10.1300/J075v01n03_05

Shull, R. L. (1991). Mathematical description of operant behavior: An introduction. In I. H. Iversen & K. A. Lattal (Eds.), *Experimental analysis of behavior* (Part 2, pp. 243–282). Amsterdam: Elsevier.

Sigurdsson, S. O., & McGee, H. M. (2015). Organizational behavior management: Systems analysis. In H. Roane, J. Ringdahl, & T. Falcomata (Eds.), *Clinical and organizational applications of applied behavior analysis* (627-647). San Diego, CA: Academic Press.

Sigurdsson, S. O., Taylor, M. A., & Wirth, O. (2013). Discounting the value of safety: Effects of perceived risk and effort. *Journal of Safety Research, 46,* 127-134. doi: 10.1016/j.jsr.2013.04.006

Skidmore, J. R., & Murphy, J. G. (2011). The effects of drink price and next day responsibilities on college drinking: A behavioral economic analysis. *Psychology of Addictive Behaviors, 25*(1), 57-68. doi: 10.1037/a0021118

Skidmore, J. R., & Murphy, J. G. (2011). The effect of drink price and next-day responsibilities on college student drinking: A behavioral economic analysis. Psychology of Addictive Behaviors, 25(1), 57–68. doi:10.1037/a0021118

Skinner, B. F. (1956). A case history in scientific method. *American Psychologist, 11,* 221–233. doi: 10.1037/h0047662

So, Y., Lee, K., & Oah, S. (2013). Relative effects of daily feedback and weekly feedback on customer service behavior at a gas station. *Journal of Organizational Behavior Management, 33,* 137-151. doi: 10.1080/01608061.2013.785898

Stein, J. S., Koffarnus, M. N., Snider, S. E., Quisenberry, A. J., & Bickel, W. K. (2015). Identification and management of nonsystematic purchase task data: Toward best practice. *Experimental and Clinical Psychopharmacology, 23(5),* 377–386. doi:10.1037/pha0000020

Strouse, M. C., Carroll-Hernandez, T. A., Sherman, J. A., & Sheldon, J. B. (2003). Turning over turnover. *Journal of Organizational Behavior Management, 23,* 45–63. doi:10.1300/ J075v23n02_04.

Sulzer-Azaroff, B., & Austin, J. (2000). Does BBS work? Behavior-based safety & injury reduction: A survey of the evidence. *Professional Safety*, *45*, 19–24.

Trosclaire-Lasserre, N. M., Lerman, D. C., Call, N. A., Addison, L. R., & Kodak, T. (2008). Reinforcement magnitude: An evaluation of preference and reinforcer efficacy. *Journal of Applied Behavior Analysis, 41,* 203-220. doi: 10.1901/jaba.2008.41-203

VanStelle, S. E., Vicars, S. M., Harr, V., Miguel, C. F., Koerber, J. L., Kazbour, R., & Austin, J. (2012). The publication history of *Journal of Organizational Behavior Management:* An objective review and analysis: 1998-2009. *Journal of Organizational Behavior Management, 32*(2), 93-123. doi: 10.1080/01608061.2012.675864

Von Bertalanffy, L. (1950). An outline of general systems theory. *The British Journal for the Philosophy of Science, 1,* 134-165. doi: 10.1093/bjps/I.2.134

Waldvogel, J. M., & Dixon, M. R. (2008). Exploring the utility of preference assessments in organizational behavior management. *Journal of Organizational Behavior Management, 28*, 76-87. doi: 10.1080/01608060802006831

Waltz, T. J., & Follette, W. C. (2009). Molar functional relations and clinical behavior analysis: Implications for assessment and treatment. *The Behavior Analyst, 32,* 51-68.

Wells, J., Reimer, D., & Houmanfar, R. (2013). Money and the Journal of Organizational Behavior Management interventions: A review. *Journal of Organizational Behavior Management, 33,* 276-289.

Wilder, D. A., Austin, J., Casella, S. (2009). Applying behavior analysis in organizations: Organizational behavior management. *Psychological Services, 6(3),* 202-211. doi: 10.1037/a0015393

Wilder, D. A., Harris, C., Casella, S., Wine, B., & Postma, N. (2011). Further evaluation of the accuracy of managerial prediction of employee preference. *Journal of Organizational Behavior Management, 31*, 130–139. doi: 10.1080/01608061.2011.569202

Wilder, D. A., Rost, K., & McMahon, M. (2007). The accuracy of managerial prediction of employee preference: A brief report. *Journal of Organizational Behavior Management, 27,* 1-14. doi: 10.1300/J075v27n02_01

Wilder, D. A., Therrien, K., & Wine, B. (2006). A comparison between survey and verbal choice methods of identifying potential reinforcers among employees. *Journal of Organizational Behavior Management, 25,* 1-13. doi: 10.1300/J075v25n04_01

Wine, B., Gilroy, S., & Hantula, D. A. (2012). Temporal (in)stability of employee preference for rewards. *Journal of Organizational Behavior Management, 32,* 58-64. doi: 10.1080/01608061.2012.646854

Wine, B., Reis, M., & Hantula, D. A. (2014). An evaluation of stimulus preference assessment methodology in organizational behavior management. *Journal of Organizational Behavior Management, 34,* 7-15. doi: 10.1080/01608061.2013.873379

Woolverton, W. L., & Anderson, K. G. (2006). Effects of delay to reinforcement on the choice between cocaine and food in rhesus monkeys. *Psychopharmacology*, *186* (1), 99-106. doi: 10.1007/s00213-006-0355-x

Workplace Bullying Institute (2014). *2014 WBI U.S. workplace bullying survey*. Retrieved from http://workplacebullying.org/multi/pdf/WBI-2014-US-Survey.pdf

Yukl, G. A., & Latham, G. P. (1975). Consequences of reinforcement schedules and incentive magnitudes for employee performance: Problems encountered in an industrial setting. *Journal of Applied Psychology, 60,* 294-298. doi: 10.1037/h0076755

Yukl, G. A., Latham, G. P., & Pursell, E. D. (1976). The effectiveness of performance incentives under continuous and variable ratio schedules of reinforcement. *Personnel Psychology, 29,* 221-231. doi: 10.1111/j.1744-6570.1976.tb00410.x

Yukl, G. A., Wexley, K. N., & Seymore, J. D. (1972). Effectiveness of pay incentives under variable ratio and continuous reinforcement schedules. *Journal of Applied Psychology, 56,* 19-23. doi: 10.1037/h0032137

Table 1

Experiment 1 Participant Demographic Information

Demographic	M_{05}		M_{10}		M_{20}	
Age (years)						
Min	20.0		20.0		21.0	
Max	72.0		66.0		63.0	
M	36.4		36.6		34.7	
SD	12.0		11.8		10.8	
	n	%	*n*	%	*n*	%
Gender						
Female	46	46.5	52	54.7	48	50.5
Male	53	53.5	43	45.3	47	49.5
Ethnicity						
Asian	3	3.0	4	4.2	6	6.3
Black/African American	9	9.1	4	4.2	5	5.3
Hispanic/Latino	2	2.0	4	4.2	3	3.2
Mixed	2	2.0	1	1.1	1	1.1
Other	0	0.0	0	0.0	2	2.1
White/Caucasian	83	83.8	82	86.3	78	82.1
Education						
Less than high school	1	1	2	2.1	1	1.1
High school/GED	13	13.1	8	8.4	12	12.6
Some college	18	18.2	22	23.2	33	34.7
2-year college degree	10	10.1	9	9.5	14	14.7
4-year college degree	47	47.5	39	41.1	29	30.5
Master’s degree	7	7.1	11	11.6	6	6.3
Doctoral degree	2	2.0	1	1.1	0	0.0
Professional degree (JD, MD)	1	1.0	3	3.2	0	0.0
Income						
Less than 30,000	37	37.4	22	23.2	31	32.6
30,000 – 39,999	18	18.2	13	13.7	17	17.9
40,000 – 49,999	6	6.1	12	12.6	11	11.6
50,000 – 59,999	9	9.1	18	18.9	10	10.5
60,000 – 69,999	7	7.1	5	5.3	7	7.4
70,000 – 79,999	5	5.1	4	4.2	4	4.2
80,000 – 89,999	5	5.1	6	6.3	4	4.2
90,000 – 99,999	1	1.0	3	3.2	2	2.1
100,000 or more	11	11.1	12	12.6	9	9.5

Disability						
ADHD	2	2.0	2	2.1	2	2.1
Learning disability	0	0	1	1.1	0	0
Physical disability	5	5.0	8	8.4	5	5.3
Other	1	1.0	1	1.1	3	3.2
Smoking status						
Yes – currently	19	19.2	16	16.8	22	23.2
Yes – previously	21	21.2	25	26.3	28	29.5
No – never	59	59.6	54	56.8	45	47.4

Table 2

Experiment 1 HWT and EWT Analog to Demand Indices and Model Fits

	Ratio Requirement					
	M_{05}		M_{10}		M_{20}	
Indices	HWT	EWT	HWT	EWT	HWT	EWT
α	.00029	.00032	.00019	.00023	.00017	.00018
EV	12.19	11.05	18.61	15.37	20.80	19.64
P_{max} Derived	9.95	9.02	15.18	12.54	16.97	16.03
P_{max} Empirical	128.00	8.00	10.00	8.00	20.00	20.00
O_{max} Derived	308.40	279.48	440.34	363.76	492.15	464.80
O_{max} Empirical	384.00	416.00	470.00	480.00	640.00	620.00
R^2	.87	.97	.95	.97	.98	.97
	Unit Price					
α	.0015	.0016	.0019	.0023	.0034	.0036
EV	2.36	2.21	1.86	1.54	1.04	0.98
P_{max} Derived	1.92	1.80	1.52	1.25	0.85	0.80
P_{max} Empirical	25.60	1.60	1.00	0.80	1.00	1.00
O_{max} Derived	55.78	52.29	44.03	36.38	24.61	23.24
O_{max} Empirical	76.80	83.20	47.00	48.00	32.00	31.00
R^2	.87	.97	.95	.97	.98	.97

Table 3

Experiment 2 Participant Demographic Information

Demographic	P_{90}		P_{50}		P_{10}	
Age (years)						
Min	19.0		19.0		22.0	
Max	64.0		65.0		72.0	
M	36.0		36.0		35.0	
SD	10.8		11.0		11.4	
	n	%	*n*	%	*n*	%
Gender						
Female	46	48.4	44	6.3	41	43.2
Male	49	51.6	51	53.7	54	56.8
Ethnicity						
Asian	5	5.3	8	8.4	8	8.4
Black/African American	6	6.3	2	2.1	3	3.2
Hispanic/Latino	3	3.2	3	3.2	3	3.2
Mixed	2	2.1	2	2.1	2	2.1
Other	0	0.0	0	0.0	2	2.1
White/Caucasian	79	83.2	80	84.2	77	81.1
Education						
Less than high school	0	0.0	2	2.1	0	0.0
High school/GED	10	10.5	15	15.8	5	5.3
Some college	29	30.5	28	29.5	25	26.3
2-year college degree	8	8.4	15	15.8	16	16.8
4-year college degree	35	36.8	27	28.4	38	40.0
Master's degree	9	9.5	7	7.4	10	10.5
Doctoral degree	2	2.1	0	0.0	1	1.1
Professional degree (JD, MD)	2	2.1	1	1.1	0	0.0
Income						
Less than 30,000	21	22.1	27	28.4	14	14.7
30,000 – 39,999	14	14.7	16	16.8	12	21.1
40,000 – 49,999	12	12.6	11	11.6	13	13.7
50,000 – 59,999	13	13.7	8	8.4	11	11.6
60,000 – 69,999	11	11.6	11	11.6	5	5.3
70,000 – 79,999	4	4.2	6	6.3	7	7.4
80,000 – 89,999	6	6.3	2	2.1	5	5.3
90,000 – 99,999	5	5.3	2	2.1	4	4.2
100,000 or more	9	9.5	12	12.6	16	16.8

Table 4

Experiment 2 Amazon Mechanical Turk Employment

Variable	P_{90}		P_{50}		P_{10}	
Average hours per week on mTurk						
Min	2.0		1.0		1.0	
Max	100.0		80.0		72.0	
M	15.6		19.2		15.2	
SD	14.0		14.2		12.0	
Average weekly mTurk income						
Min	1.5		5.0		5.0	
Max	1000.0		700.0		500.0	
M	79.9		114.4		79.3	
SD	109.8		103.3		78.8	
Hourly wage (hours/income)						
Min	.15		.75		1.0	
Max	25.0		50.0		40.0	
M	5.6		6.8		6.7	
SD	4.2		6.7		6.0	
	n	%	*n*	%	*n*	%
Reason for completing mTurk tasks						
Unemployed or employed part-time	10	10.5	12	12.6	13	13.7
Fruitful way to spend free time	59	62.1	46	48.4	52	54.7
Like to participate in research	33	34.7	28	29.5	28	29.5
Primary income	12	12.6	23	24.2	15	15.8
Secondary income	73	76.8	68	71.6	67	70.5
Tasks are fun	29	30.5	27	28.4	22	23.2
To kill time	17	17.9	15	15.8	21	22.1
Other	0	0.0	0	0.0	1	1.1

Table 5

Experiment 2 Employment and Incentive Information at Most Recent Paid Position

Variable	P_{90} *n*	P_{90} %	P_{50} *n*	P_{50} %	P_{10} *n*	P_{10} %
Job tenure						
No paid position in past 10 years	5	5.3	3	3.2	3	3.2
0 – 3 months	4	4.2	9	9.5	6	6.3
3 – 6 months	5	5.3	3	3.2	6	6.3
6 months – 1 year	10	10.5	8	8.4	7	7.4
1 – 3 years	20	21.1	20	21.1	27	28.4
3 – 5 years	17	17.9	21	22.1	16	16.8
5 – 10 years	22	23.2	17	17.9	17	17.9
10 – 20 years	6	6.3	9	9.5	11	11.6
20+ years	6	6.3	5	5.3	2	2.1
Promotions received						
0	40	44.4	43	46.7	40	43.5
1	22	24.4	29	31.5	27	29.3
2	14	15.6	11	12.0	14	15.2
3	5	5.6	7	7.6	8	8.7
4	4	4.4	1	1.1	1	1.1
5+	5	5.6	1	1.1	2	2.2
Occupation						
Architecture/engineering	1	1.1	2	2.2	7	7.6
Art and design	4	4.4	3	3.3	2	2.2
Building, grounds cleaning, and maintenance	2	2.2	0	0.0	0	0.0
Business and financial operations	9	10.0	5	5.4	2	2.2
Community and social service	1	1.1	1	1.1	1	1.1
Computer/mathematical	9	10.0	12	13.0	11	12.0
Construction/installation/repair	2	2.2	3	3.3	3	3.3
Education/training/library	6	6.7	7	7.6	9	9.8
Entertainer/performer	1	1.1	0	0.0	0	0.0
Farming/fishing/forestry	1	1.1	0	0.0	1	1.1
Food preparation/serving	8	8.9	12	13.0	6	6.5
Healthcare practitioner or technician	5	5.6	4	4.3	3	3.3

Healthcare support	3	3.3	4	4.3	7	7.6
Legal occupations	1	1.1	3	3.3	1	1.1
Life science	0	0.0	2	2.2	3	3.3
Management	3	3.3	5	5.4	5	5.4
Media and communications	3	3.3	1	1.1	2	2.2
Military and protective service	1	1.1	0	0.0	2	2.2
Office/administrative support	18	20.0	11	12.0	13	14.1
Personal care and service	0	0.0	1	1.1	0	0.0
Physical science	0	0.0	0	0.0	0	0.0
Production/manufacturing	1	1.1	2	2.2	0	0.0
Sales	11	12.2	11	12.0	12	13.0
Social science	0	0.0	0	0.0	0	0.0
Transportation	0	0.0	3	3.3	2	2.2
Reason for leaving last paid position						
Accepted a new job at a different company	41	45.6	28	30.4	26	28.3
Internal promotion	11	12.2	14	15.2	10	10.9
Decided not to work outside of the home	3	3.3	16	17.4	11	12.0
Laid off or otherwise terminated	13	14.4	9	9.8	14	15.2
Prefer not to answer	12	13.3	14	15.2	14	15.2
Other	10	11.1	11	12.0	17	18.5
Incentive availability						
Yes – monetary incentives	28	29.5	29	30.5	21	22.1
Yes – other gifts	24	25.3	24	25.3	26	27.4
No	51	53.7	49	51.6	52	54.7
Incentive frequency						
Daily	3	7.7	4	9.3	1	2.5
Weekly	7	17.9	2	4.7	12	30.0
Monthly	10	25.6	10	23.3	7	17.5
Quarterly	3	7.7	6	14.0	4	10.0
Twice yearly	14	35.9	15	34.9	11	27.5
Annually	0	0.0	3	7.0	1	2.5
Other	2	5.1	3	7.0	4	10.0

Table 6

Experiment 2 HWT and EWT Analog to Demand Indices and Model Fits

	P_{90}		**P_{50}**		**P_{10}**	
Indices	HWT	EWT	HWT	EWT	HWT	EWT
α	.00012	.00037	.00014	.0003	.00007	.00035
EV	29.46	9.56	25.25	11.79	50.51	10.10
P_{max} Derived	24.04	7.80	20.61	9.62	40.21	8.24
P_{max} Empirical	20.00	8.00	256.00	10.00	256.00	8.00
O_{max} Derived	697.21	226.12	597.61	278.88	1195.21	239.04
O_{max} Empirical	800.00	440.00	768.00	380.00	1280.00	440.00
R^2	.94	.95	.93	.92	.87	.93

Table 7

Experiment 3 Participant Demographic Information

Demographic	**D_{01}**		**D_{14}**		**D_{28}**	
Age (years)						
Min	19.0		19.0		18.0	
Max	75.0		67.0		64.0	
M	33.6		32.9		31.2	
SD	10.3		11.4		8.0	
	n	%	*n*	%	*n*	%
Gender						
Female	35	36.8	41	43.2	42	44.2
Male	60	63.2	54	56.8	53	55.8
Ethnicity						
Asian	6	6.3	14	14.7	7	7.4
Black/African American	7	7.4	8	8.4	10	10.5
Hispanic/Latino	6	6.3	2	2.1	6	6.3
Mixed	2	2.1	2	2.1	0	0.0
Other	1	1.1	0	0.0	1	1.1
White/Caucasian	73	76.8	69	72.6	71	74.7
Education						
Less than high school	0	0.0	0	0.0	0	0.0
High school/GED	15	15.8	18	18.9	11	11.6
Some college	26	27.4	31	32.6	26	27.4
2-year college degree	15	15.8	8	8.4	11	11.6
4-year college degree	34	35.8	29	30.5	39	41.1
Master’s degree	5	5.3	8	8.4	6	6.3
Doctoral degree	0	0.0	0	0.0	0	0.0
Professional degree (JD, MD)	0	0.0	1	1.1	2	2.1
Income						
Less than 30,000	27	28.4	27	28.4	24	25.3
30,000 – 39,999	17	17.9	15	15.8	21	22.1
40,000 – 49,999	11	11.6	8	8.4	12	12.6
50,000 – 59,999	14	14.7	14	14.7	7	7.4
60,000 – 69,999	6	6.3	4	4.2	6	6.3
70,000 – 79,999	3	3.2	7	7.4	6	6.3
80,000 – 89,999	3	3.2	4	4.2	6	6.3
90,000 – 99,999	8	8.4	5	5.3	4	4.2
100,000 or more	6	6.3	11	11.6	9	9.5

Table 8

Experiment 3 Amazon Mechanical Turk Employment

Variable	**D_{01}**		**D_{14}**		**D_{28}**	
Average hours per week on mTurk						
Min	2.0		1.0		2.0	
Max	70.0		80.0		100.0	
M	19.1		19.0		20.7	
SD	13.6		14.7		14.1	
Average weekly mTurk income						
Min	0.00		5.0		2.0	
Max	500.0		680.0		500.0	
M	83.1		92.4		119.3	
SD	76.8		97.5		98.3	
Hourly wage (hours/income)						
Min	.5		.7		.0	
Max	25.0		20.0		25.0	
M	6.5		5.8		5.8	
SD	4.7		4.3		5.4	
	n	%	*n*	%	*n*	%
Reason for completing mTurk tasks						
Unemployed or employed part-time	11	11.6	16	16.8	16	16.8
Fruitful way to spend free time	48	50.5	47	49.5	51	53.7
Like to participate in research	26	27.4	19	20.0	28	29.5
Primary income	22	23.2	21	22.1	25	26.3
Secondary income	61	64.2	62	65.3	52	54.7
Tasks are fun	29	30.5	21	22.1	26	27.4
To kill time	17	17.9	14	14.7	15	15.8
Other	2	2.1	1	1.1	2	2.1

Table 9

Experiment 3 Employment and Incentive Information at Most Recent Paid Position

Variable	D_{01} *n*	D_{01} %	D_{14} *n*	D_{14} %	D_{28} *n*	D_{28} %
Job tenure						
No paid position in past 10 years	1	1.1	2	2.1	2	2.1
0 – 3 months	4	4.2	4	4.2	6	6.3
3 – 6 months	6	6.3	4	4.2	6	6.3
6 months – 1 year	12	12.6	16	16.8	9	9.5
1 – 3 years	21	22.1	30	31.6	29	30.5
3 – 5 years	28	29.5	13	13.7	21	22.1
5 – 10 years	17	17.9	14	14.7	15	15.8
10 – 20 years	4	4.2	5	5.3	5	5.3
20+ years	2	2.1	7	7.4	2	2.1
Promotions received						
0	48	51.1	52	55.9	42	45.2
1	25	26.6	24	25.8	32	34.4
2	15	16.0	10	10.8	12	12.9
3	5	5.3	3	3.2	4	4.3
4	1	1.1	1	1.1	2	2.2
5+	0	0.0	3	3.2	1	1.1
Occupation						
Architecture/engineering	1	1.1	1	1.1	0	0.0
Art and design	2	2.1	3	3.2	2	2.2
Building, grounds cleaning, and maintenance	0	0.0	0	0.0	1	1.1
Business and financial operations	9	9.6	11	11.8	9	9.7
Community and social service	3	3.2	0	0.0	2	2.2
Computer/mathematical	7	7.4	8	8.6	10	10.8
Construction/installation/repair	4	4.3	6	6.5	1	1.1
Education/training/library	10	10.6	9	9.7	7	7.5
Entertainer/performer	1	1.1	1	1.1	0	0.0
Farming/fishing/forestry	0	0.0	1	1.1	2	2.2
Food preparation/serving	13	13.8	8	8.6	9	9.7
Healthcare practitioner or technician	3	3.2	9	9.7	3	3.2

Healthcare support	1	1.1	5	5.4	6	6.5
Legal occupations	0	0.0	0	0.0	2	2.2
Life science	1	1.1	1	1.1	1	1.1
Management	3	3.2	3	3.2	3	3.2
Media and communications	2	2.1	1	1.1	2	2.2
Military and protective service	2	2.1	1	1.1	0	0.0
Office/administrative support	13	13.8	7	7.5	11	11.8
Personal care and service	1	1.1	0	0.0	2	2.2
Physical science	1	1.1	0	0.0	0	0.0
Production/manufacturing	1	1.1	2	2.2	1	1.1
Sales	16	17.0	11	11.8	15	16.1
Social science	0	0.0	1	1.1	1	1.1
Transportation	0	0.0	4	4.3	3	3.2
Reason for leaving last paid position						
Accepted a new job at a different company	23	24.5	26	28.0	25	26.9
Internal promotion	19	20.2	9	9.7	18	19.4
Decided not to work outside of the home	9	9.6	13	14.0	10	10.8
Laid off or otherwise terminated	16	17.0	8	8.6	13	14.0
Prefer not to answer	13	13.8	18	19.4	18	19.4
Other	14	14.9	19	20.4	9	9.7
Incentive availability						
Yes – monetary incentives	20	21.1	25	26.3	33	34.7
Yes – other gifts	15	15.8	13	13.7	18	18.9
No	66	69.5	60	63.2	54	56.8
Incentive frequency						
Daily	1	3.6	2	6.1	2	5.1
Weekly	11	39.3	7	21.2	8	20.5
Monthly	8	28.6	4	12.1	14	35.9
Quarterly	2	7.1	4	12.1	3	7.7
Twice yearly	6	21.4	12	36.4	10	25.6
Annually	0	0.0	3	9.1	0	0.0
Other	0	0.0	1	3.0	2	5.1

Table 10

Experiment 3 HWT and EWT Analog to Demand Indices and Model Fits

	D_{01}		**D_{14}**		**D_{28}**	
Indices	HWT	EWT	HWT	EWT	HWT	EWT
α	.00011	.00023	.00014	.00032	.00011	.00032
EV	32.14	15.37	25.25	11.05	32.14	11.05
P_{max} Derived	26.23	12.54	20.61	9.02	26.23	9.02
P_{max} Empirical	256.00	8.00	10/20.00	8.00	256.00	8.00
O_{max} Derived	760.59	363.76	597.61	261.45	760.59	261.45
O_{max} Empirical	1024.00	496.00	600.00	416.00	1536.00	408.00
R^2	.90	.98	.96	.97	.74	.95

Table 11

Percentage of Accurate Responding at Each Ratio Requirement for All Conditions and Experiments

Ratio	M_{05}	M_{10}	M_{20}	P_{90}	P_{50}	P_{10}	D_{01}	D_{14}	D_{28}
PT				88.07	89.82	92.63	90.53	77.89	94.39
1	90.43	92.47	91.40	91.40	94.57	93.68	92.55	86.67	93.41
2	96.74	94.38	94.44	90.22	91.21	93.48	94.94	85.16	95.68
4	97.53	96.91	98.40	93.75	94.33	95.33	97.59	87.34	93.93
8	99.28	96.46	98.63	91.59	92.02	95.45	98.99	85.82	97.06
10	99.76	97.33	98.27	89.09	94.74	91.71	98.89	85.64	95.86
12	99.36	96.15	98.67	93.23	91.67	83.97	99.34	85.42	95.08
15	99.63	94.55	98.95	90.77	93.70	85.78	99.51	80.33	99.65
20	99.23	93.89	95.65	85.00	91.67	71.22	99.13	77.50	99.58
25	99.50	90.55	98.29	75.00	87.56	0.00	98.29	68.50	98.67
32	100.00	98.44	99.15	66.67	85.27	33.33	100.00	35.42	100.00
48	100.00	100.00	98.21	66.67	100.00	50.00	100.00	50.00	
64	100.00	100.00	96.35	100.00		50.00	99.22	100.00	
96	98.96	98.96	94.79			49.48	100.00	100.00	
128			98.44				100.00	100.00	
256			94.14				100.00	99.61	
Median	99.36	96.46	98.27	90.22	91.84	84.88	99.17	85.53	95.86

Note. Experiment 1 did not have the opportunity to provide inaccurate responses in the practice trial. PT = practice trial.

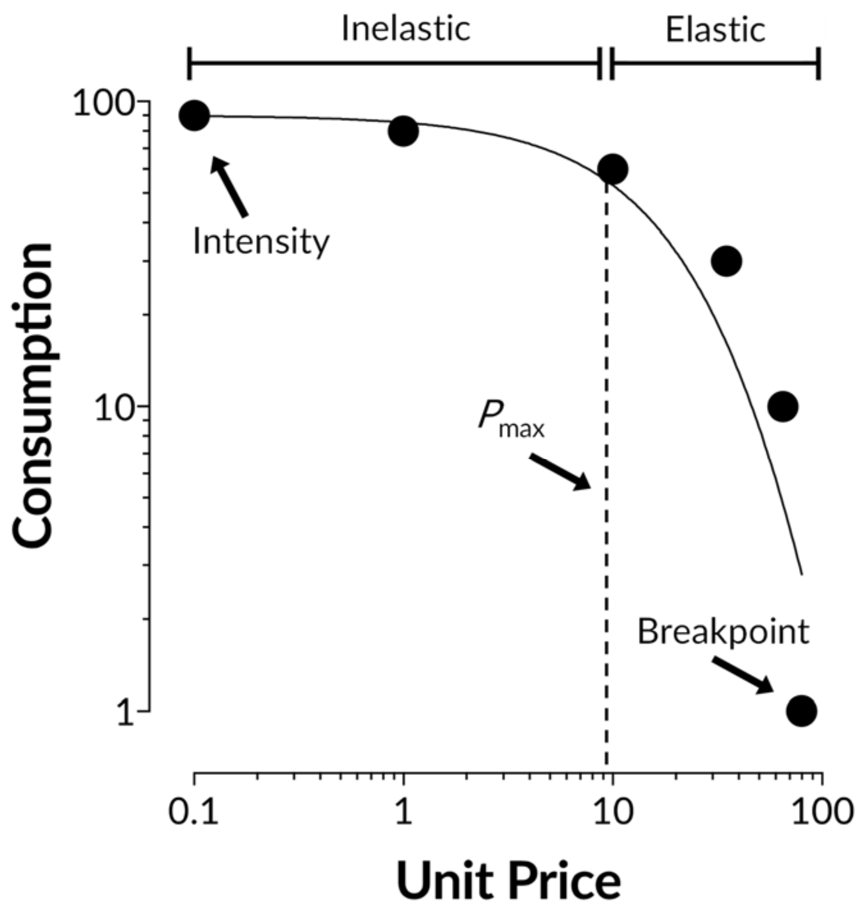


Figure 1. Prototypical demand curve.

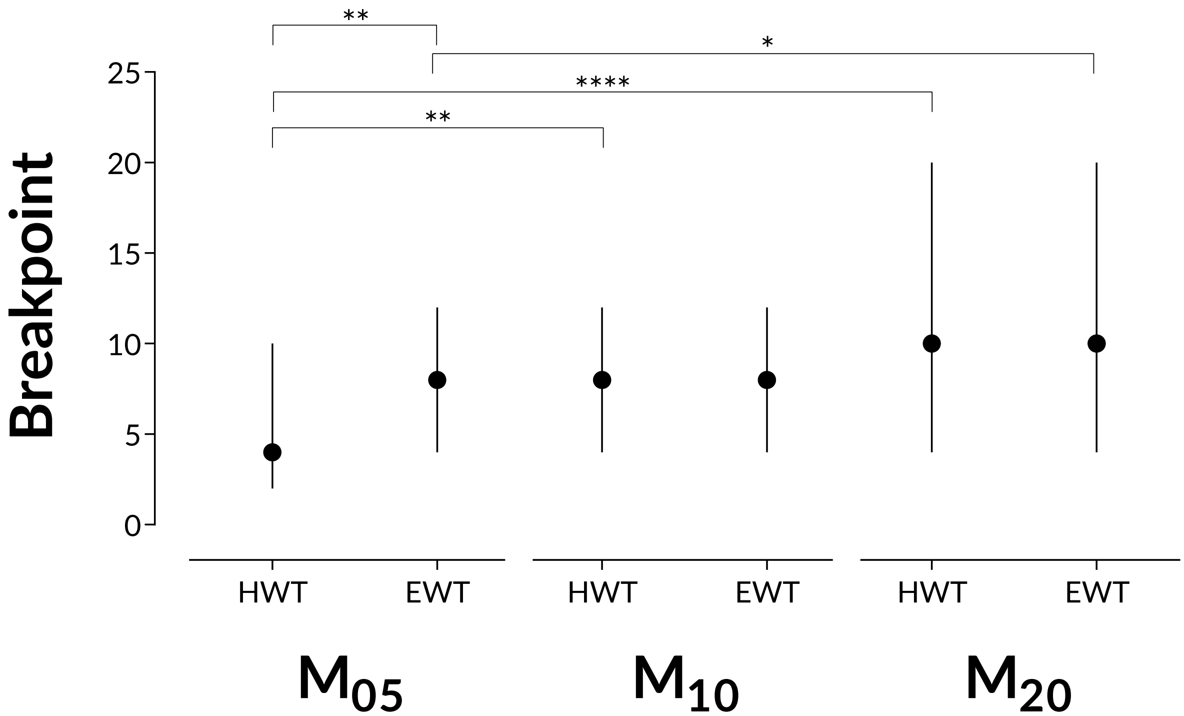


Figure 2. Median breakpoints in the HWT and EWT for the M_{05}, M_{10}, and M_{20} conditions. Error bars represent interquartile range. Statistically significant differences are denoted by * $p < .05$, ** $p < .01$, **** $p < .0001$.

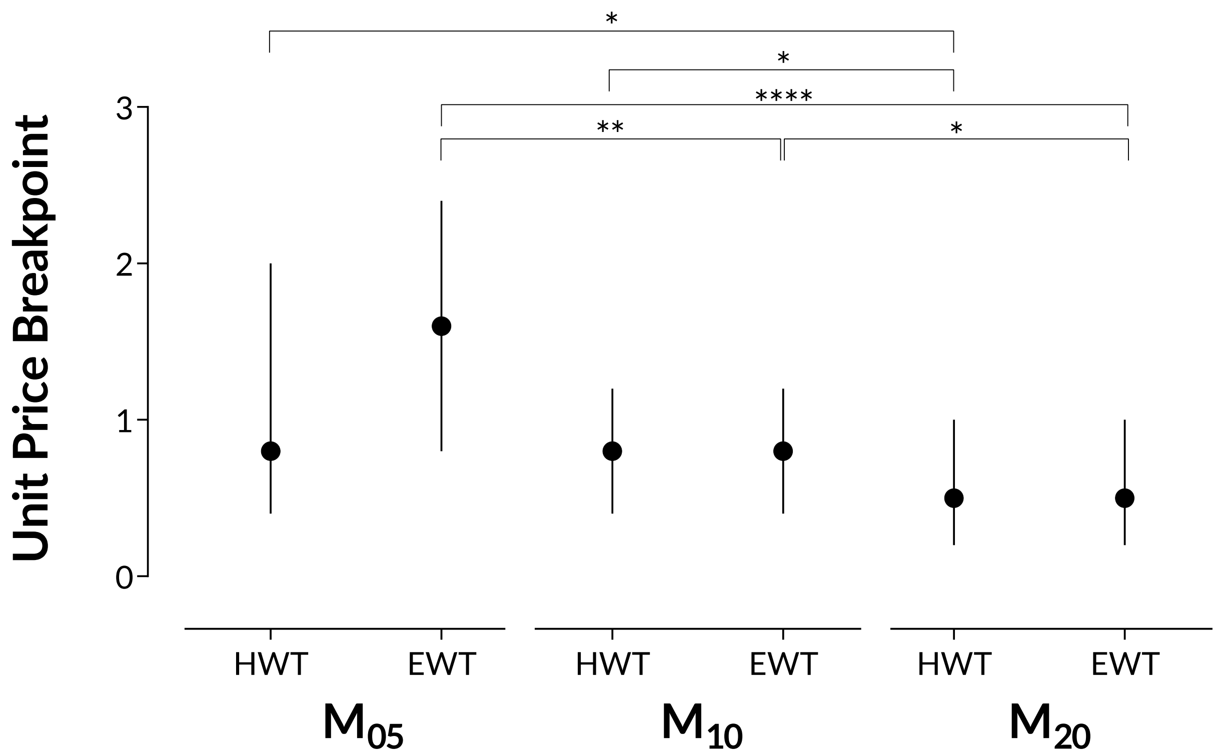


Figure 3. Median unit price breakpoints (work units per $0.01) in the HWT and EWT for the M_{05}, M_{10}, and M_{20} conditions. Error bars denote interquartile range. Statistically significant differences are denoted by * $p < .05$, ** $p < .01$, **** $p < .0001$.

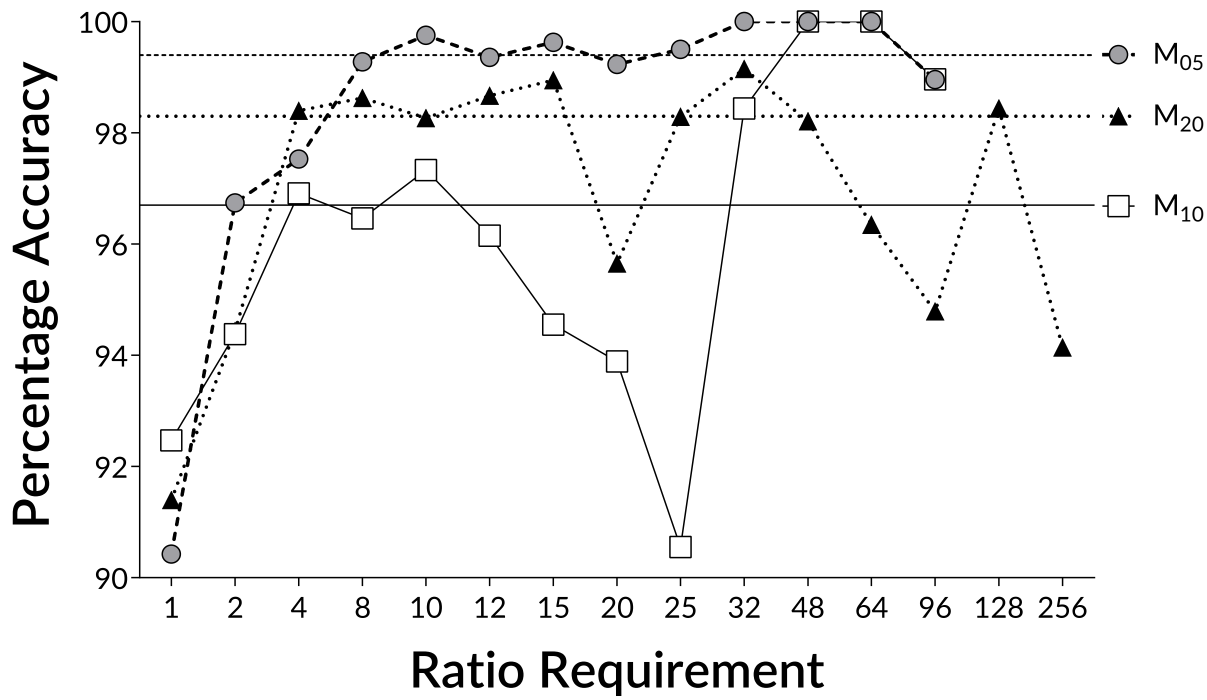


Figure 4. Experiment 1 percentage of accurately completed work units. Horizontal lines represent group medians across all ratio requirements and correspond to the data paths denoting the condition to which it belongs.

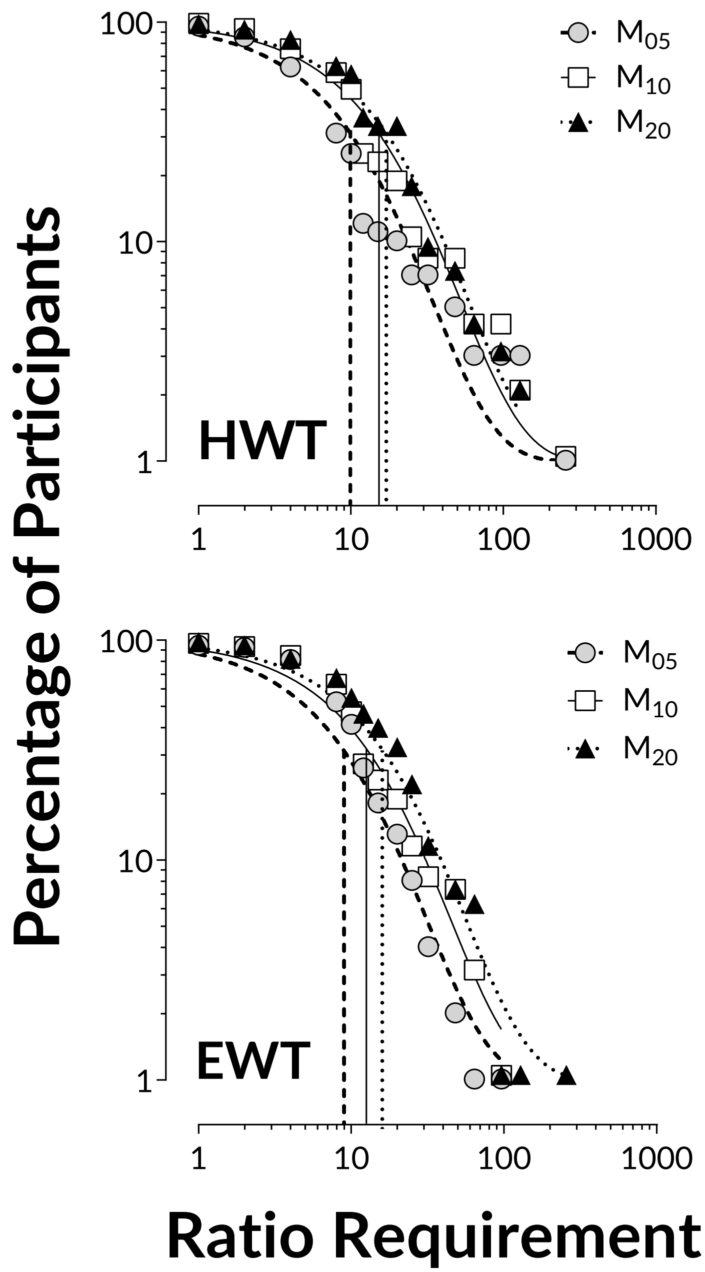


Figure 5. Analog to demand curves for the M_{05}, M_{10}, and M_{20} conditions in the HWT (top panel) and EWT (bottom panel). The y-axis depicts the percentage of participants in each incentive magnitude condition who indicated willingness to complete (HWT) or completed (EWT) the ratio requirement. The x-axis depicts the ratio requirement needed to earn the incentive. Vertical lines correspond to derived P_{max}.

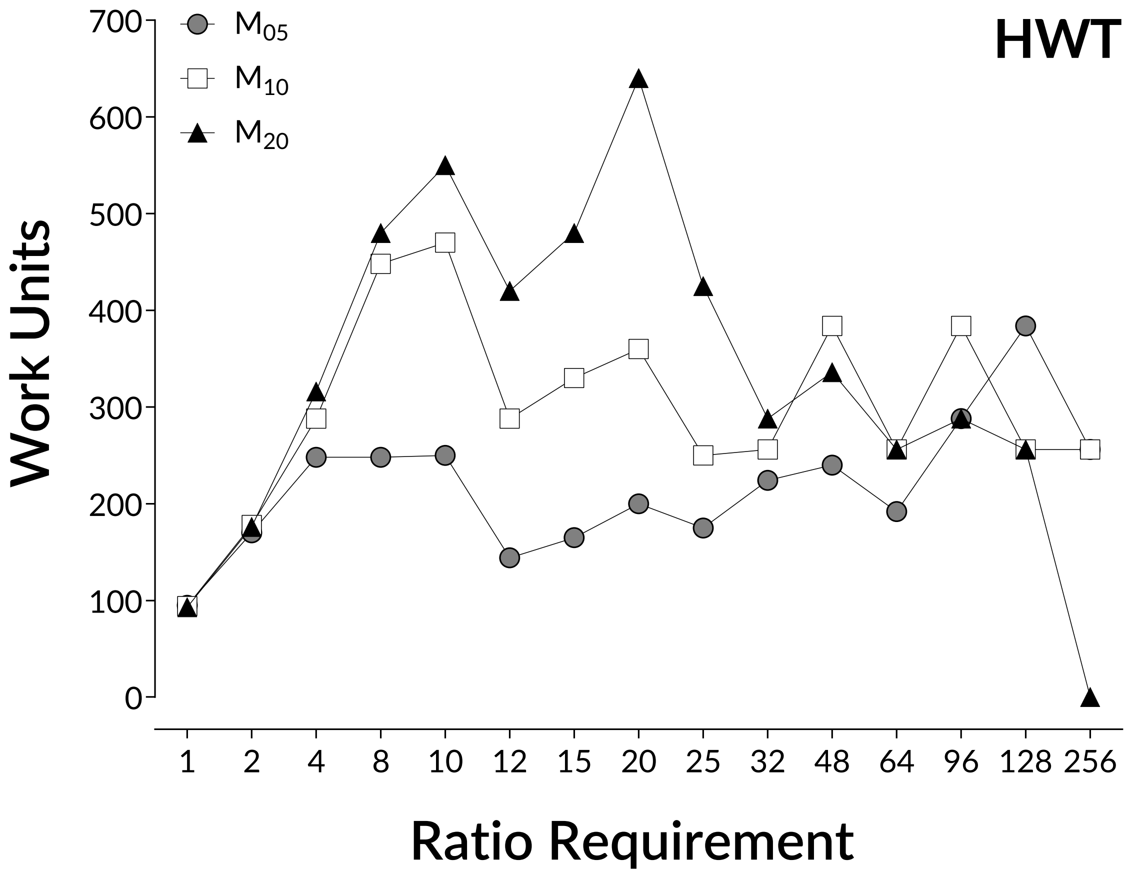


Figure 6. Work functions for the M_{05}, M_{10}, and M_{20} conditions in the HWT for Experiment 1.

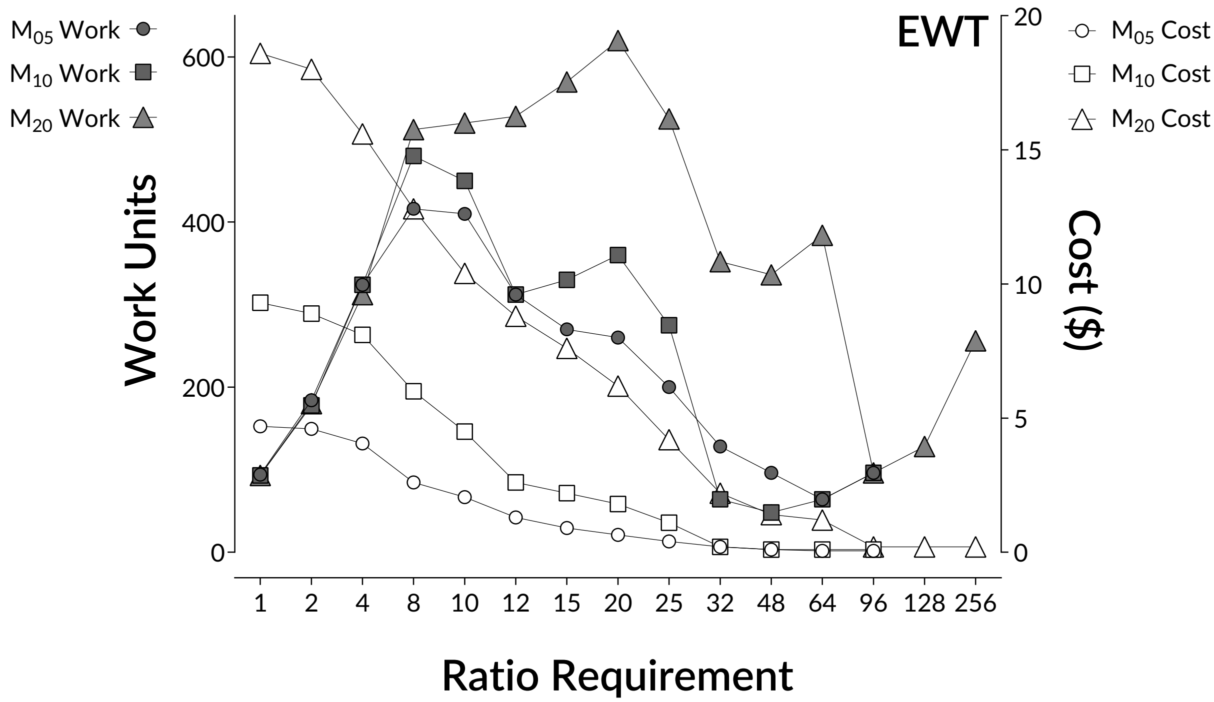


Figure 7. Cost-benefit analysis of the EWT for Experiment 1. The number of work units completed is depicted by the grey data points and scaled to the left y-axis. Cost in dollars per ratio requirement is depicted by the open data points and plotted on the right y-axes.

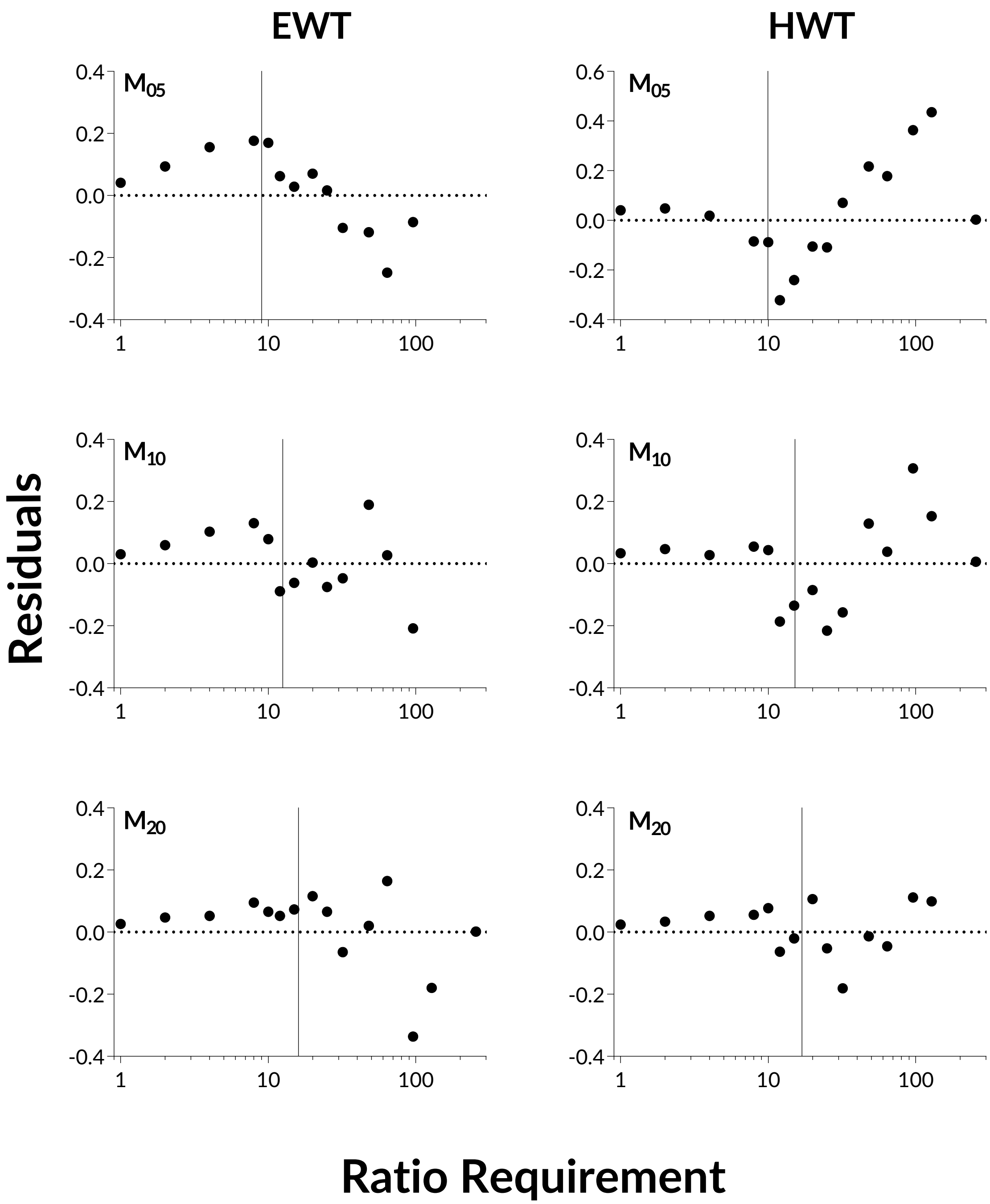


Figure 8. Residuals (distance from best-fit curve) from *AtD* curves in the EWT (left panel) and HWT (right panel) in Experiment 1. Residual plots for the M_{05}, M_{10}, and M_{20} conditions are displayed in the top, middle, and bottom panels, respectively. Vertical lines represent derived P_{max}. The horizontal line at zero denotes the best-fit curve of the *AtD* curve.

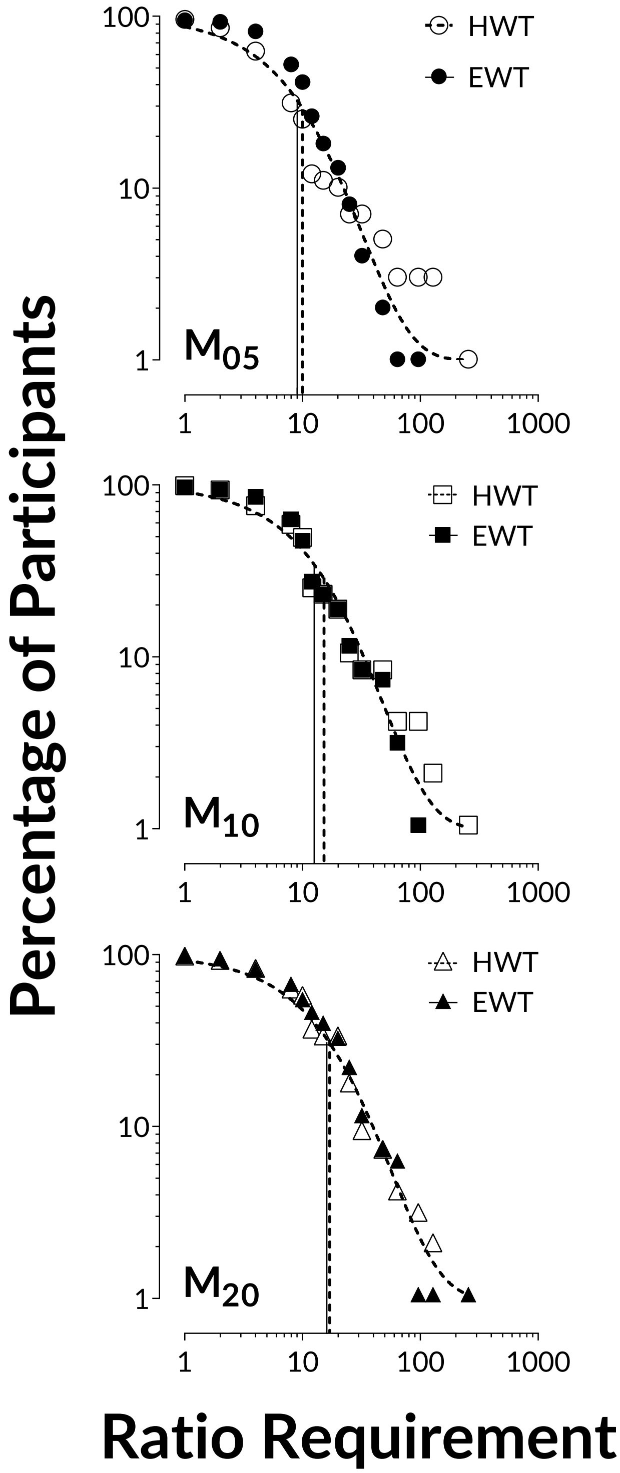


Figure 9. Experiment 1 analog to demand curves for the HWT and EWT separately. Vertical lines represent derived P_{max}.

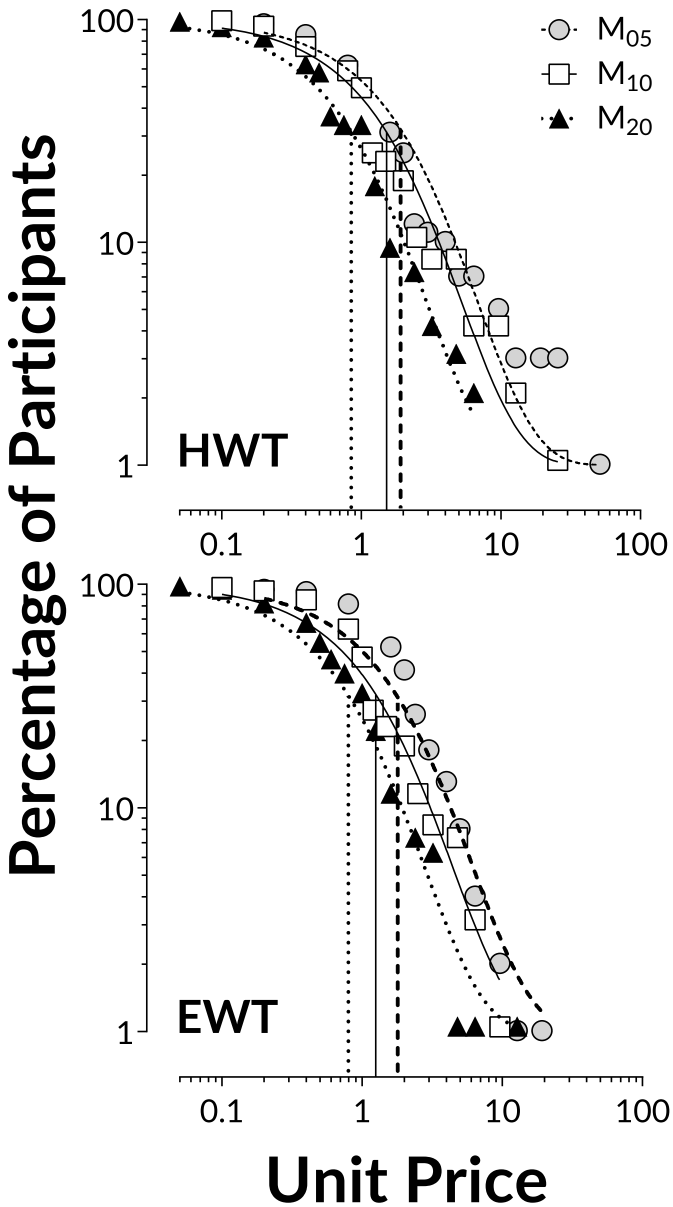


Figure 10. Analog to demand curves for the M05, M10, and M20 conditions in the HWT (top panel) and EWT (bottom panel). The y-axis depicts the percentage of participants in each incentive magnitude condition who indicated willingness to complete (HWT) or completed (EWT) the ratio requirement. The x-axis depicts unit price (ratio requirement/incentive amount). Vertical lines correspond to derived P_{max}.

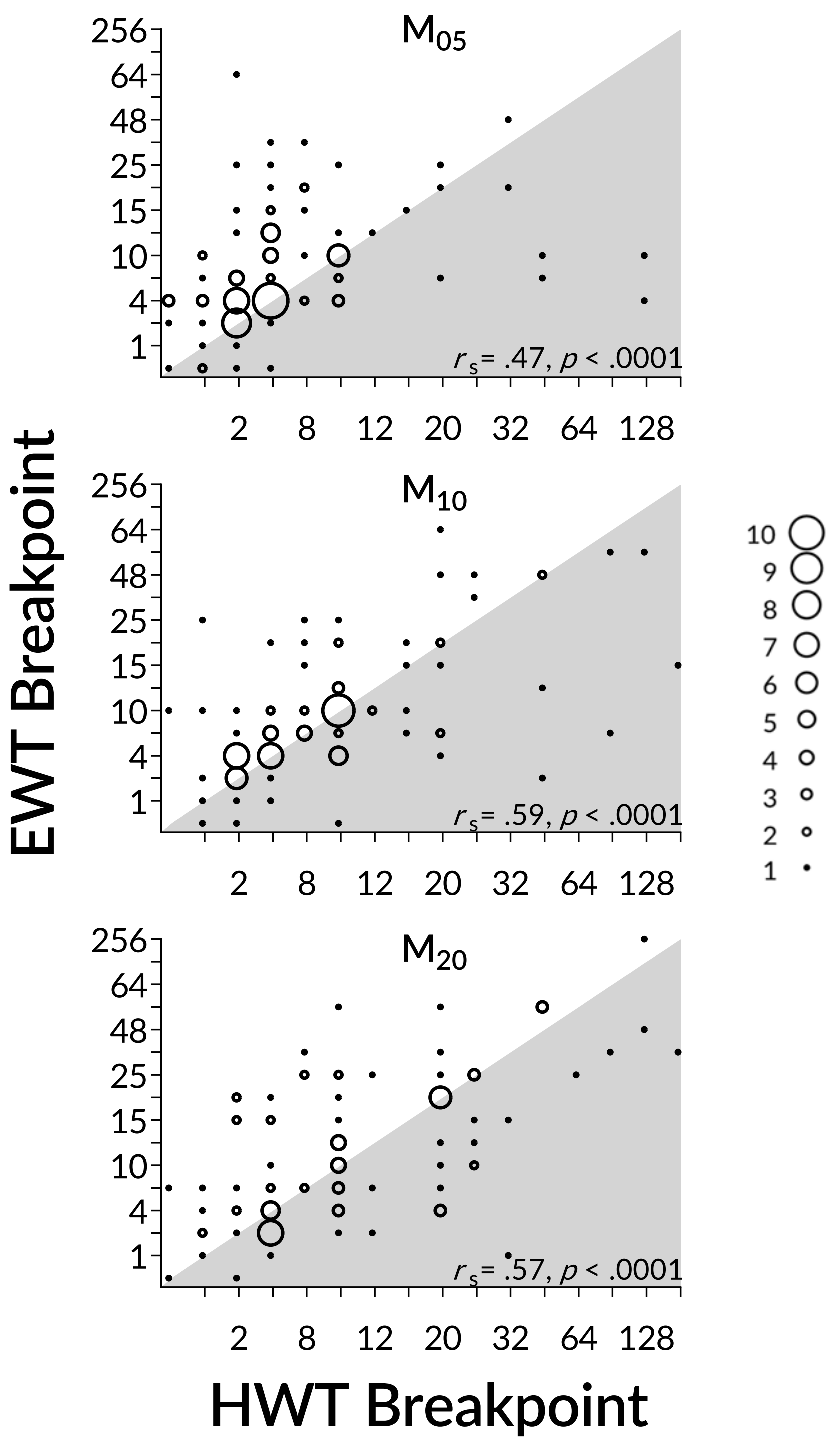


Figure 11. Predictive validity of the HWT in Experiment 1. The size of the data point symbols reflects the number of participants whose data are represented by that value.

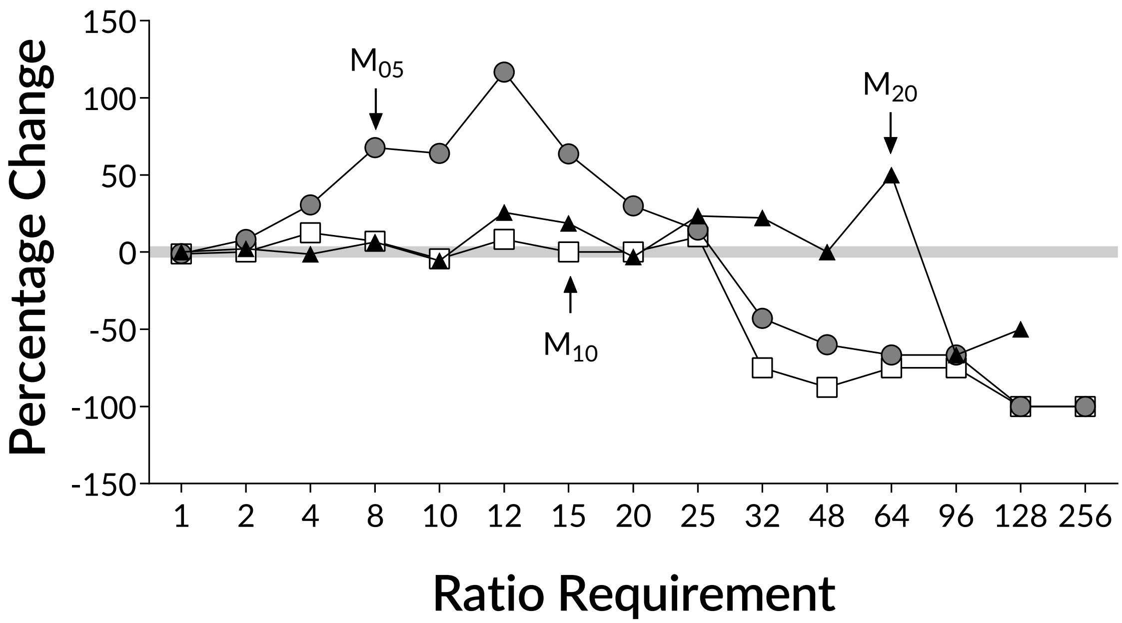


Figure 12. Experiment 1 percentage change in predicted work output and corresponding cost from the HWT to observed output and cost in the EWT. The gray horizontal line indicates a 0% change, or perfect aggregate correspondence. Data above the gray horizontal line indicate an increase in the amount of work completed and higher costs associated with the EWT than were predicted. Data points below the line indicate less work completed and a lower cost observed in the EWT than predicted by the HWT.

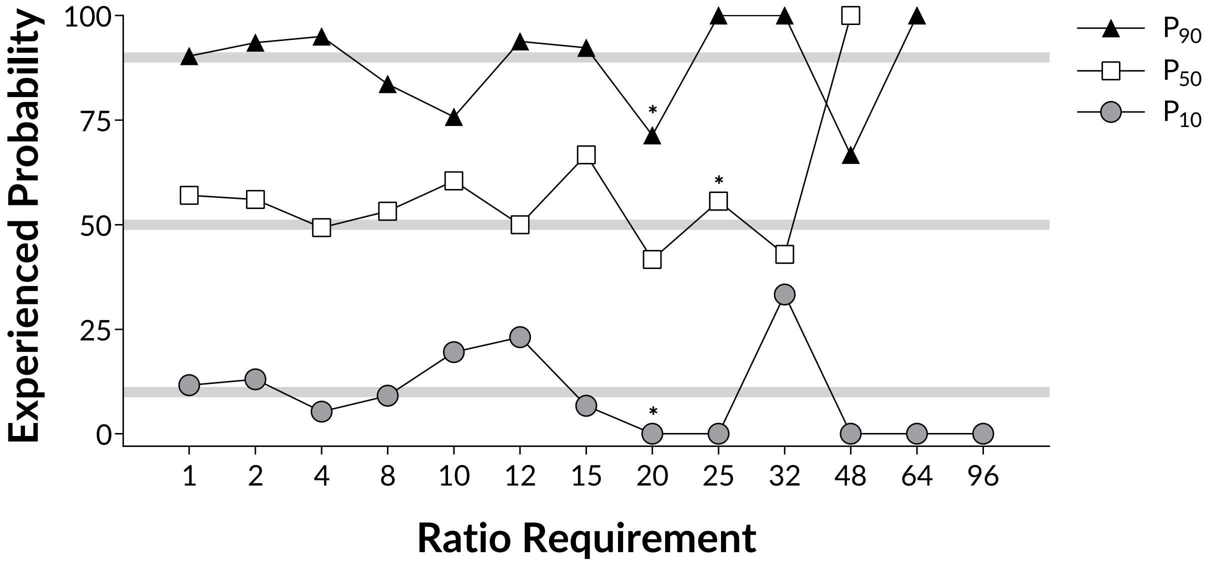


Figure 13. Experienced probabilities across ratio requirements for Experiment 2. Grey horizontal lines indicate the programmed probabilities for the P_{90}, P_{50}, and P_{10} conditions (from top to bottom), respectively. Asterisks denote the first ratio requirement at which fewer than ten participants continued responding.

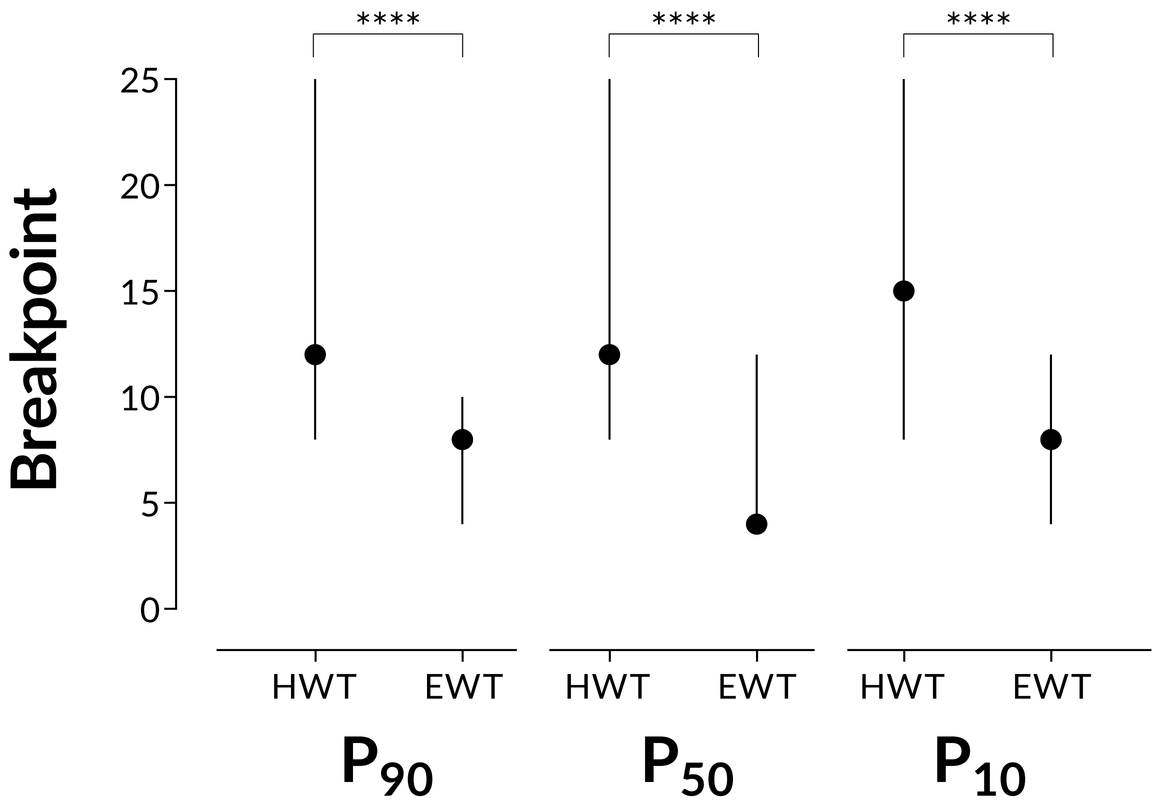


Figure 14. Median breakpoints in the P_{90}, P_{50}, and P_{10} conditions. Error bars denote interquartile range. Statistically significant differences are denoted by **** *p* < .0001.

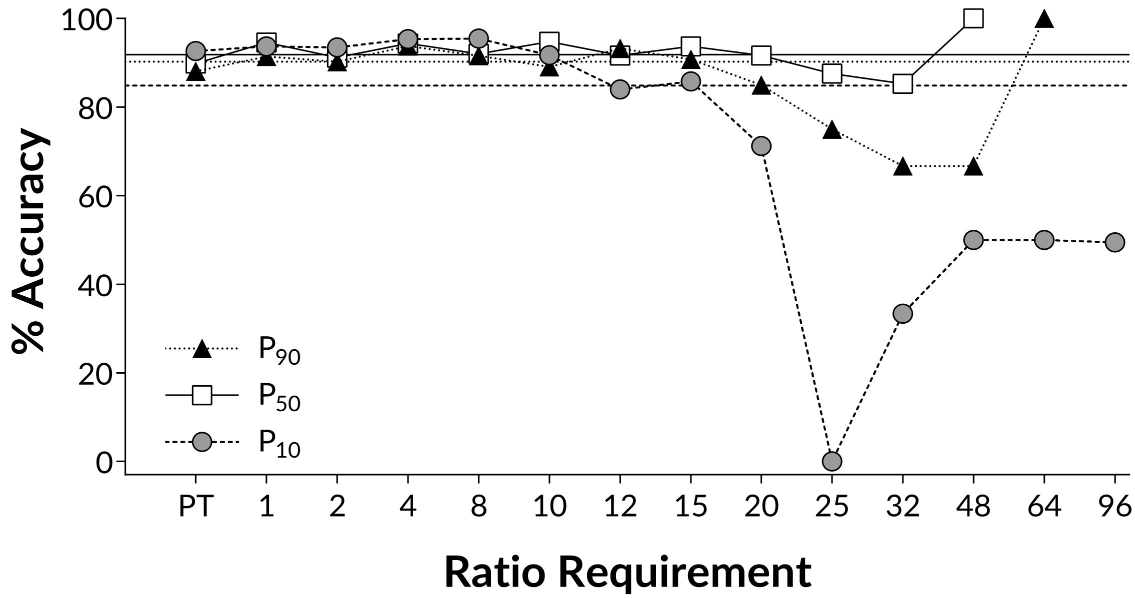


Figure 15. Experiment 2 percentage of accurately completed work units. Horizontal lines represent group medians across all ratio requirements and correspond to the data paths denoting the condition to which it belongs. PT = Practice trial.

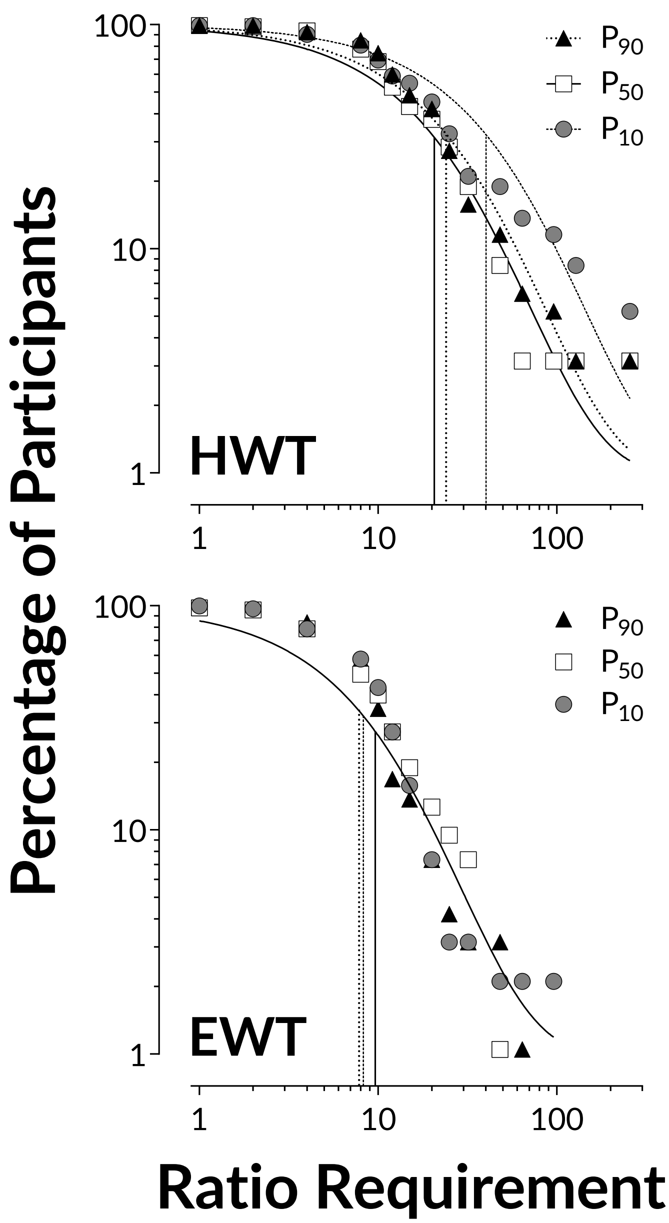


Figure 16. Experiment 2 analog to demand curves for the P_{90}, P_{50}, and P_{10} conditions in the HWT (top panel) and EWT (bottom panel). The y-axis depicts the percentage of participants in each incentive probability condition who indicated willingness to complete (HWT) or completed (EWT) the ratio requirement. The x-axis depicts the ratio requirement needed to earn the incentive. Vertical lines correspond to derived P_{max}.

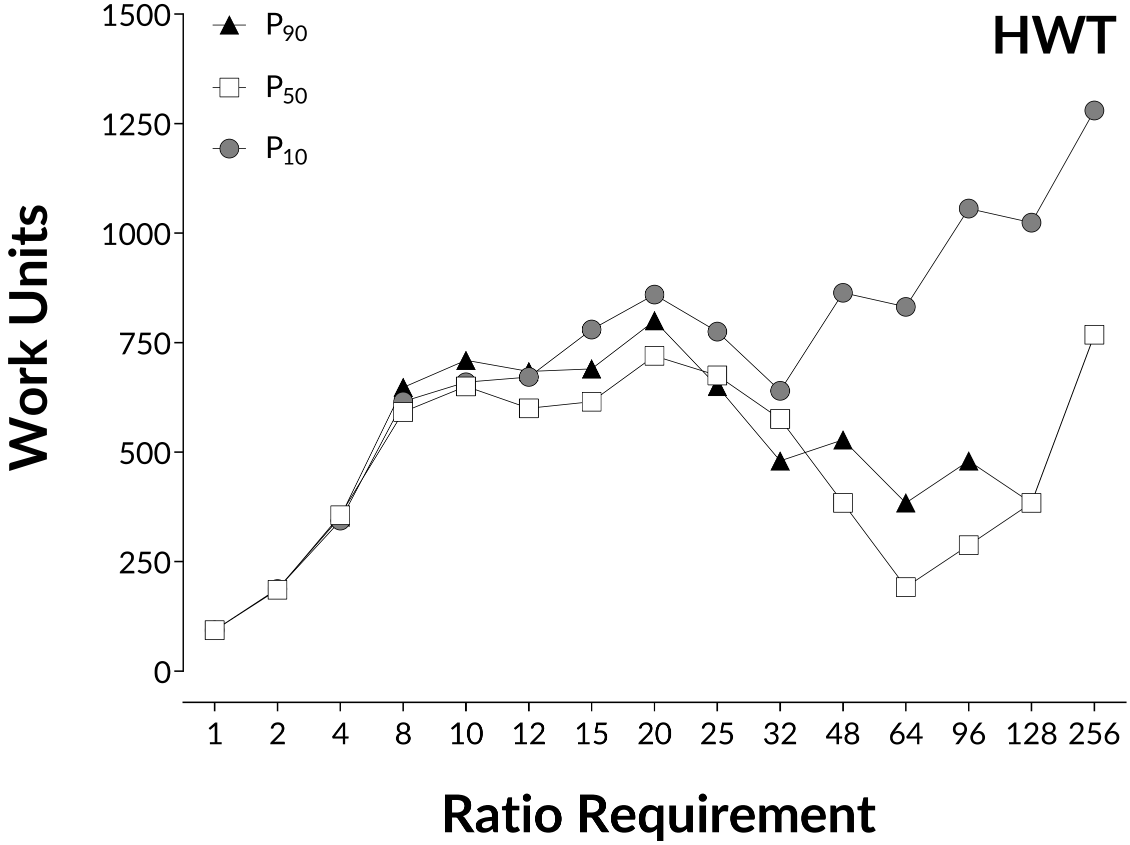


Figure 17. Work functions for the P_{90}, P_{50}, and P_{10} conditions in the HWT for Experiment 2.

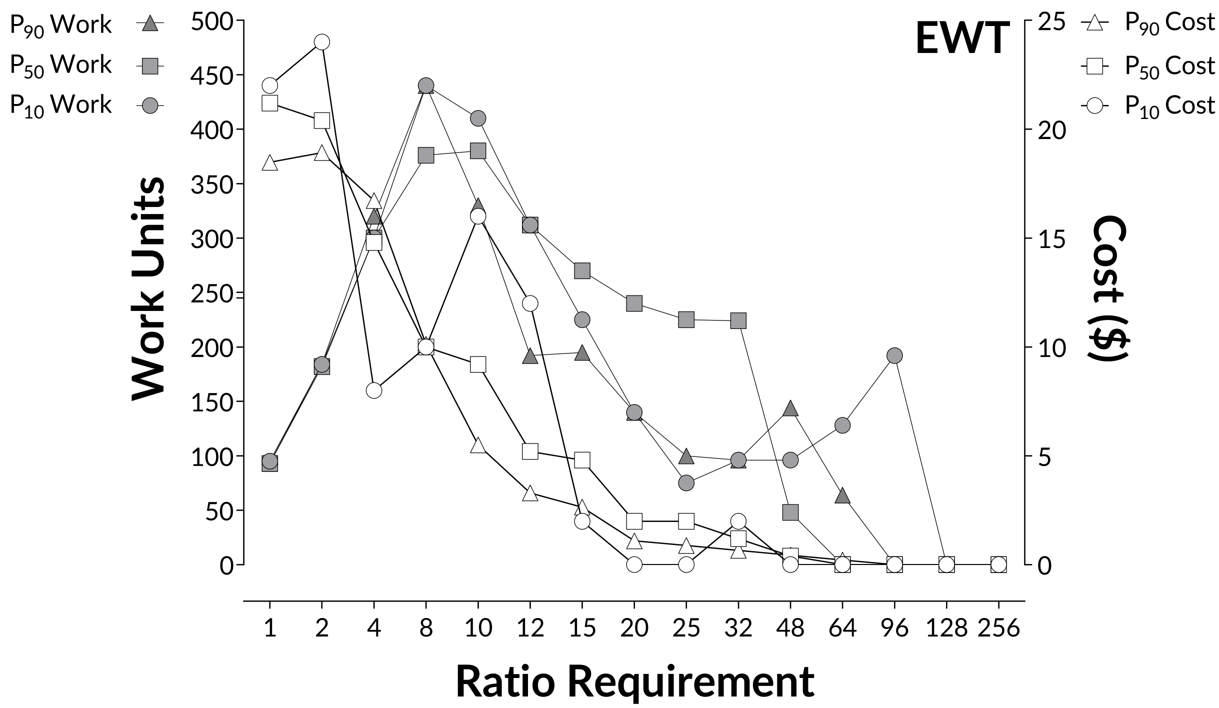


Figure 18. Cost-benefit analysis of the EWT in Experiment 2. Number of work units completed is denoted by the grey data points and scaled to the left y-axis. Cost in dollars per ratio requirement is depicted by the open data points and plotted on the right y-axes.

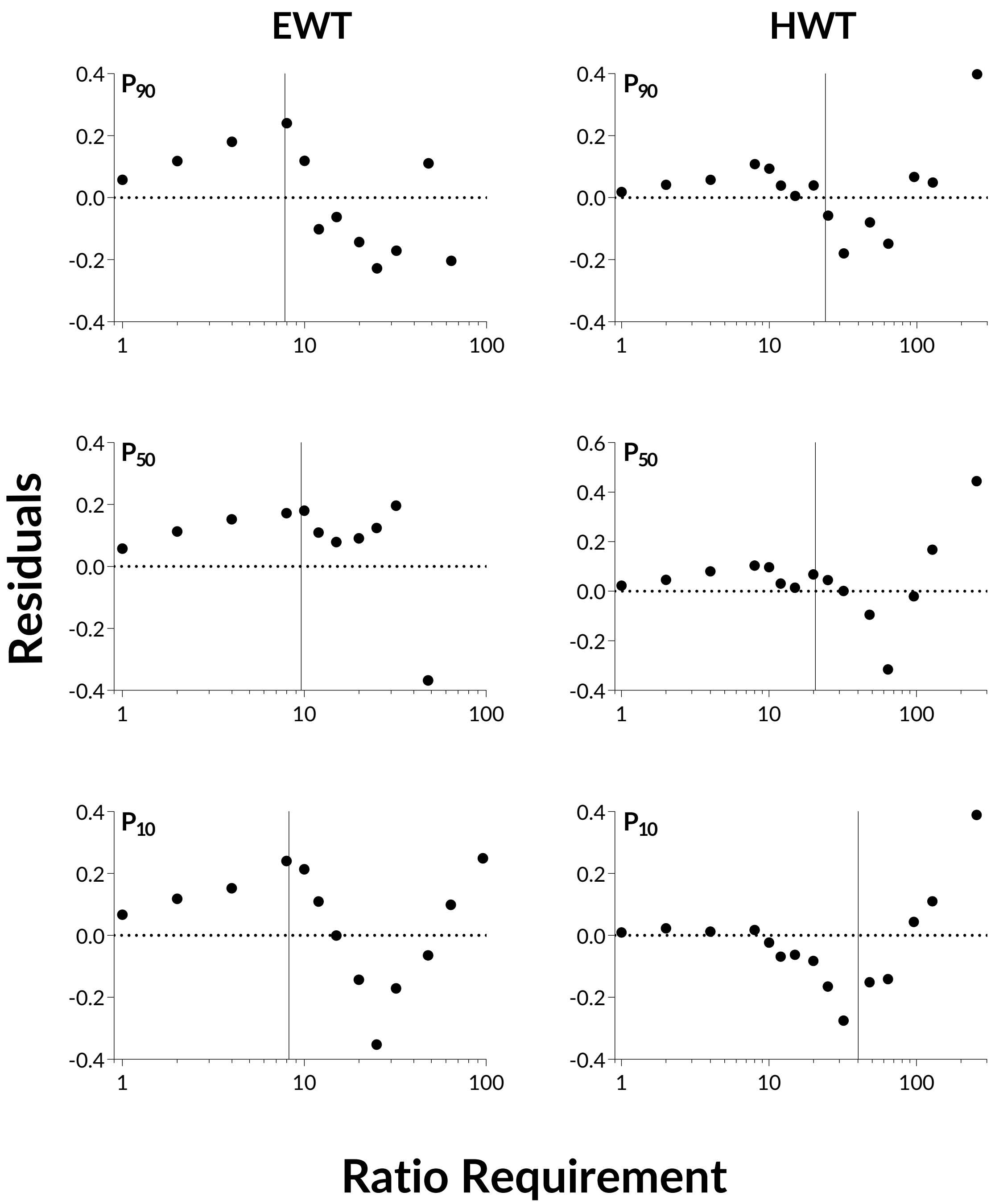


Figure 19. Residuals (distance from best-fit curve) from *AtD* curves in the EWT (left panel) and HWT (right panel) in Experiment 2. Residual plots for the P_{90}, P_{50}, and P_{10} conditions are displayed in the top, middle, and bottom panels, respectively. Vertical lines represent derived P_{max}. The horizontal line at zero denotes the best-fit curve of the *AtD* curve.

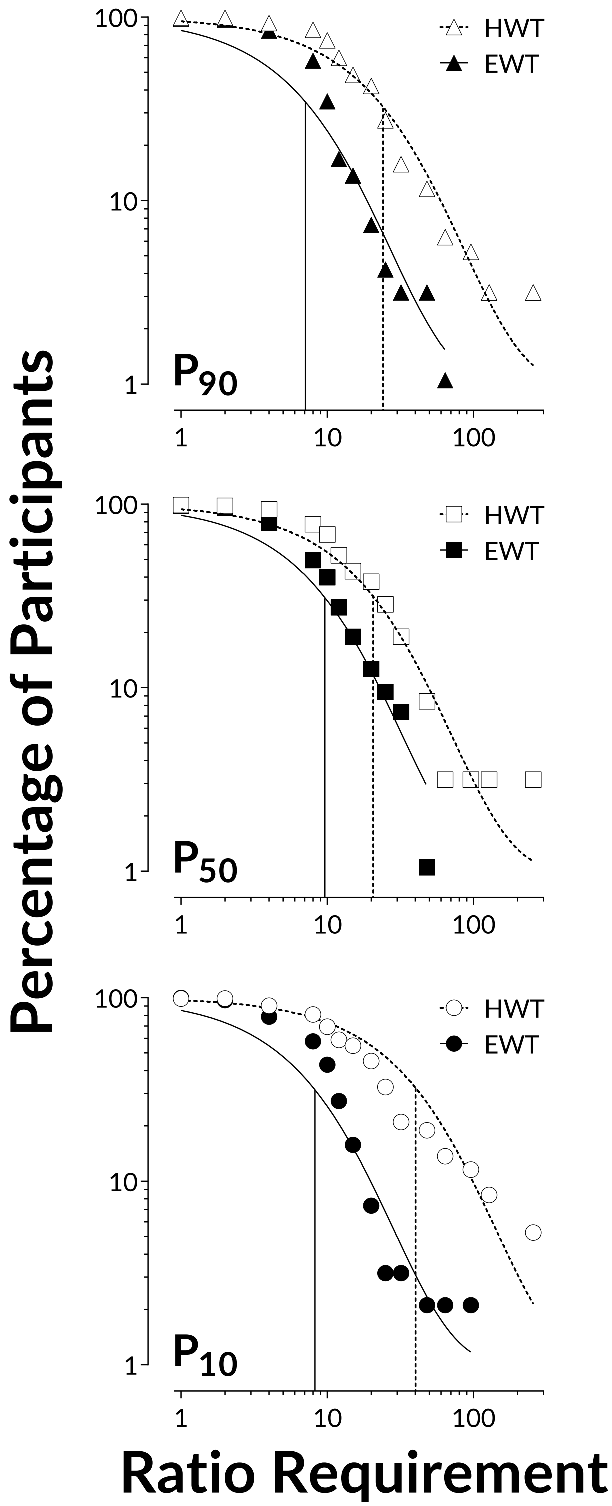


Figure 20. Experiment 2 analog to demand curves for the HWT and EWT separately. Vertical lines represent derived P_{max}.

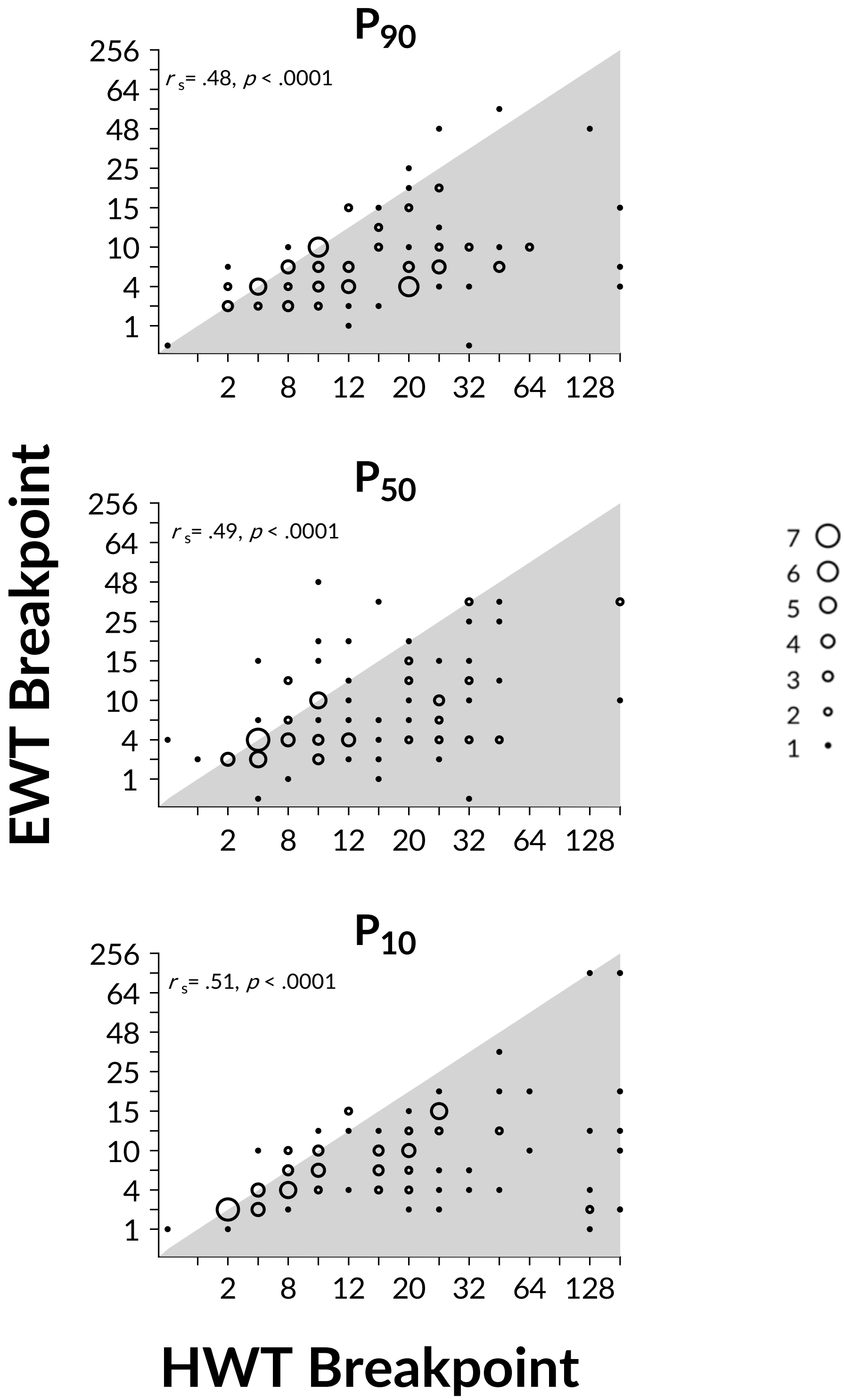


Figure 21. Experiment 2 predictive validity of the HWT. The size of the data point symbols reflects the number of participants whose data are represented by that value.

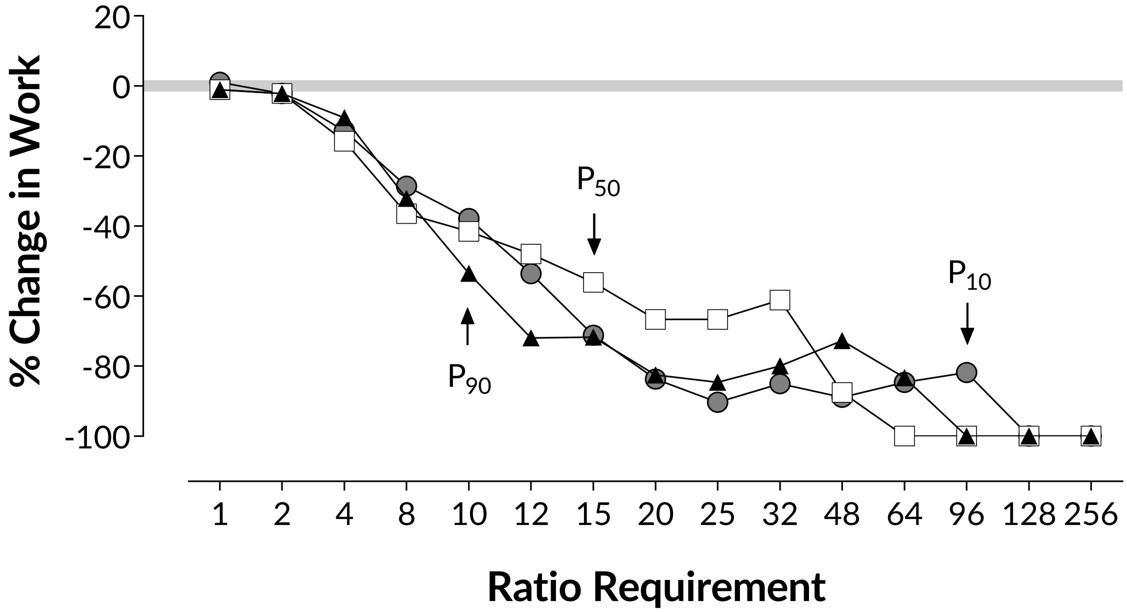


Figure 22. Percentage change in predicted work output from the HWT to observed output in the EWT for Experiment 2. The gray horizontal line indicates a 0% change, or perfect aggregate correspondence. Data points below the line indicate less work completed in the EWT than predicted by the HWT.

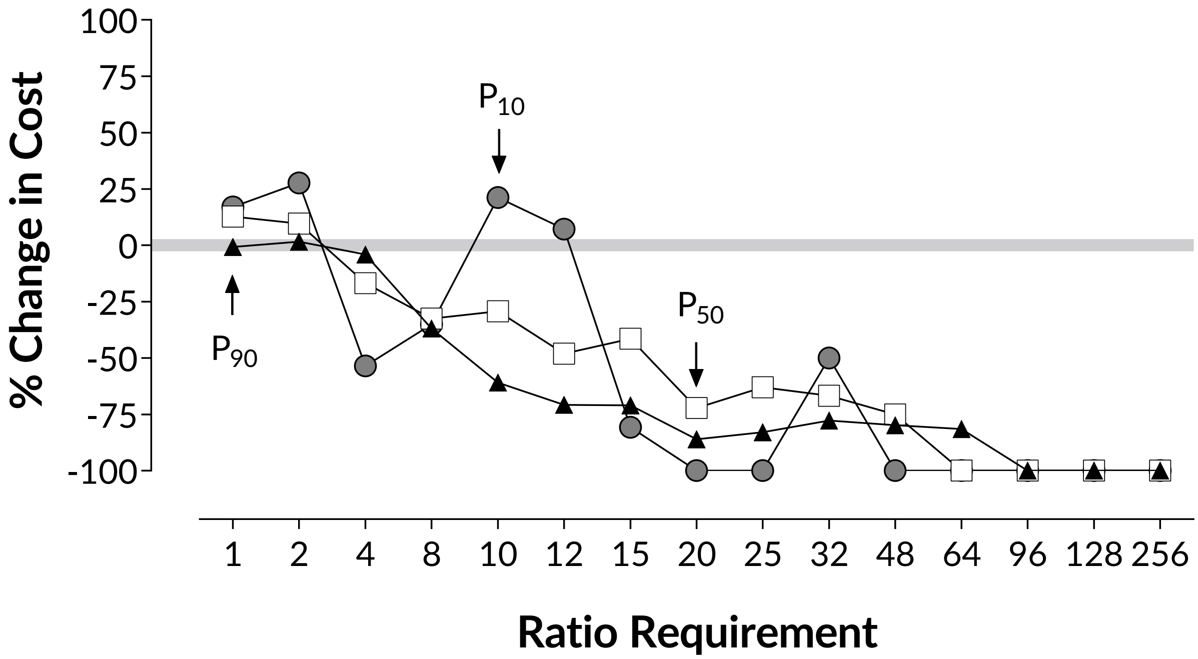


Figure 23. Percentage change in predicted cost from the HWT to observed cost in the EWT for Experiment 2. The gray horizontal line indicates a 0% change, or perfect aggregate correspondence. Data points below the line indicate the cost of incentive payments was less in the EWT than predicted by the HWT.

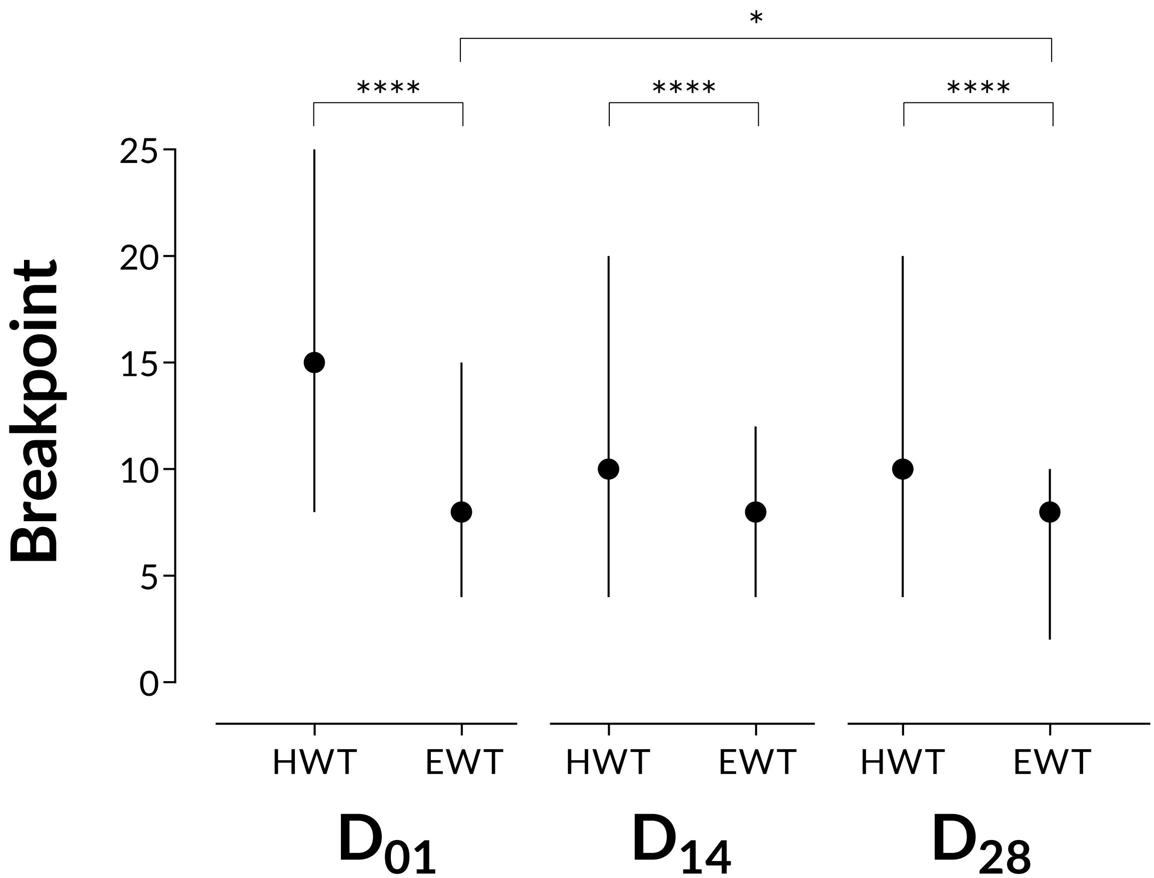


Figure 24. Median breakpoints in the D_{01}, D_{14}, and D_{28} conditions. Error bars denote interquartile range. Statistically significant differences are denoted by * $p < .05$, **** $p < .0001$.

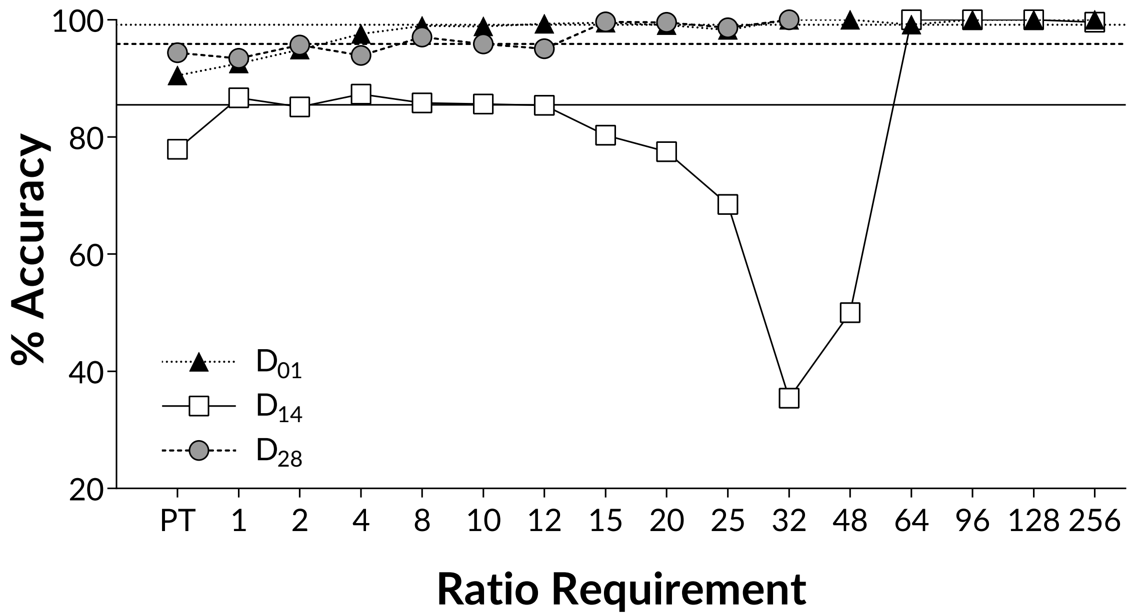


Figure 25. Experiment 3 percentage of accurately completed work units. Horizontal lines represent group medians across all ratio requirements and correspond to the data paths denoting the condition to which it belongs. PT = Practice trial.

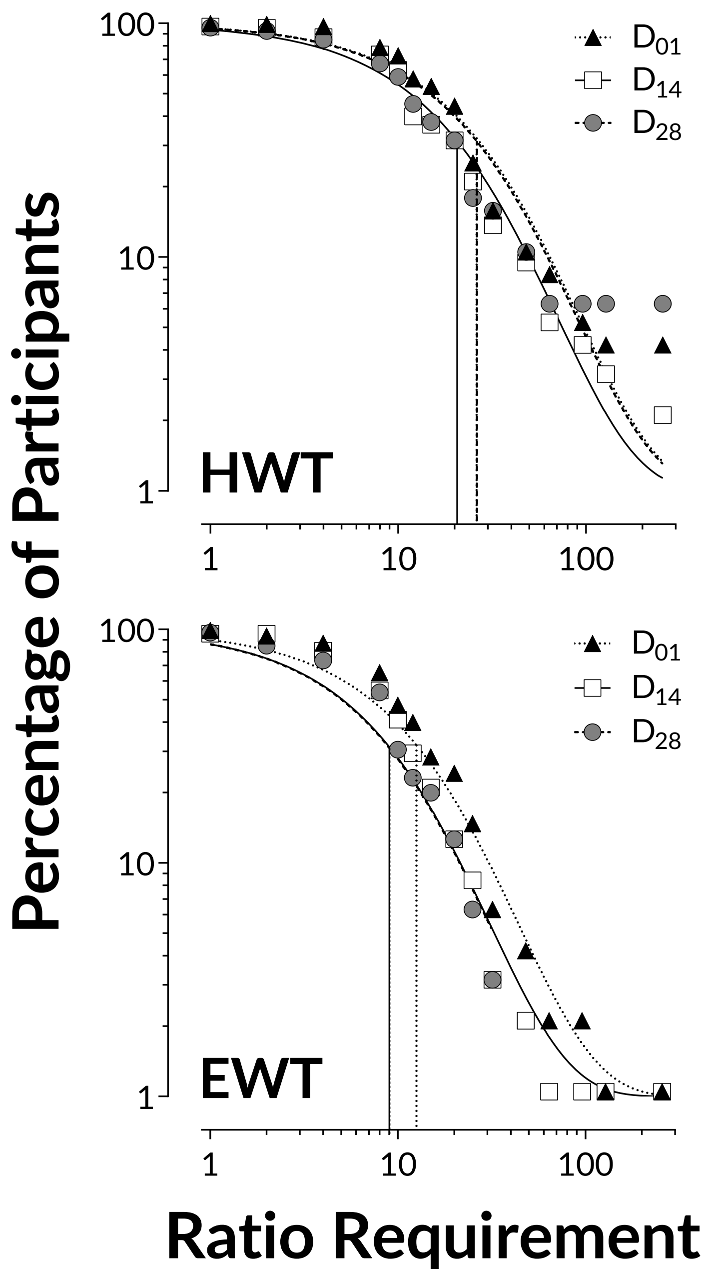


Figure 26. Analog to demand curves for the D_{01}, D_{14}, and D_{28} conditions in the HWT (top panel) and EWT (bottom panel). The y-axis depicts the percentage of participants in each incentive magnitude condition who indicated willingness to complete (HWT) or completed (EWT) the ratio requirement. The x-axis depicts the ratio requirement needed to earn the incentive. Vertical lines correspond to derived P_{max}.

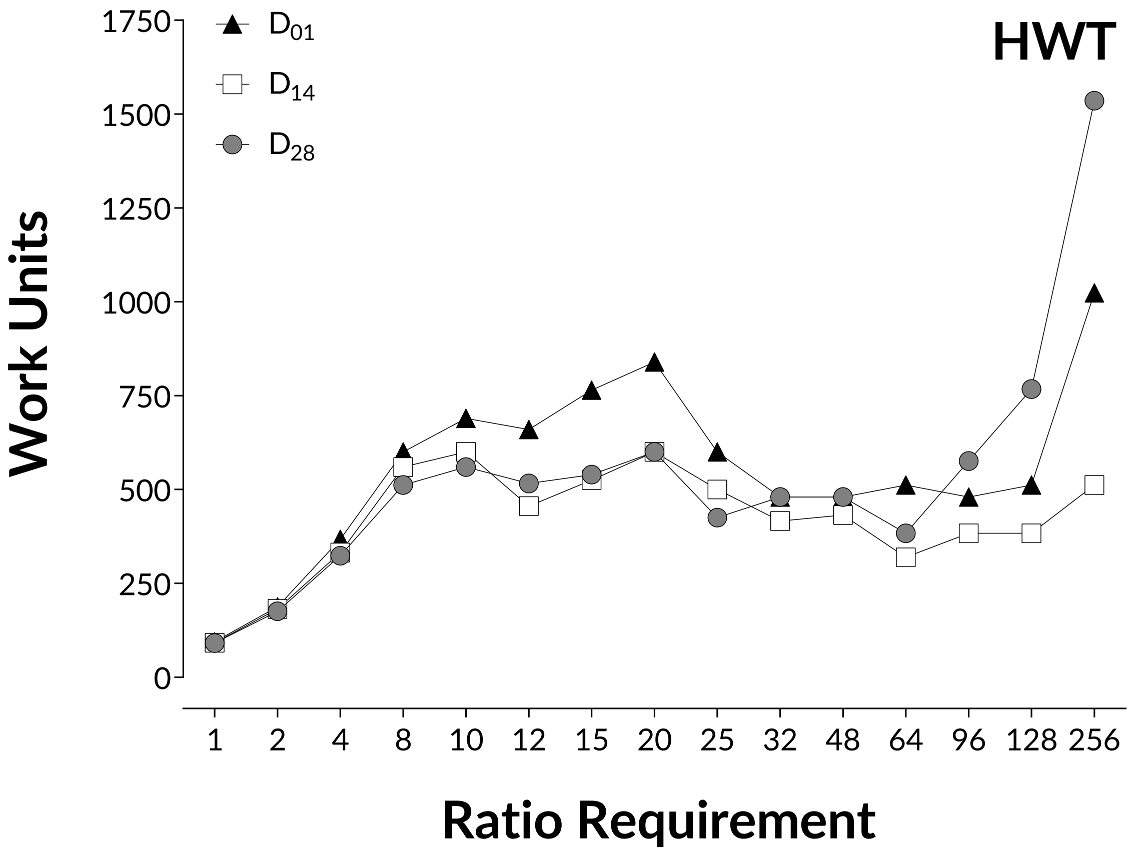


Figure 27. Work functions for the D_{01}, D_{14}, and D_{28} conditions in the HWT for Experiment 3.

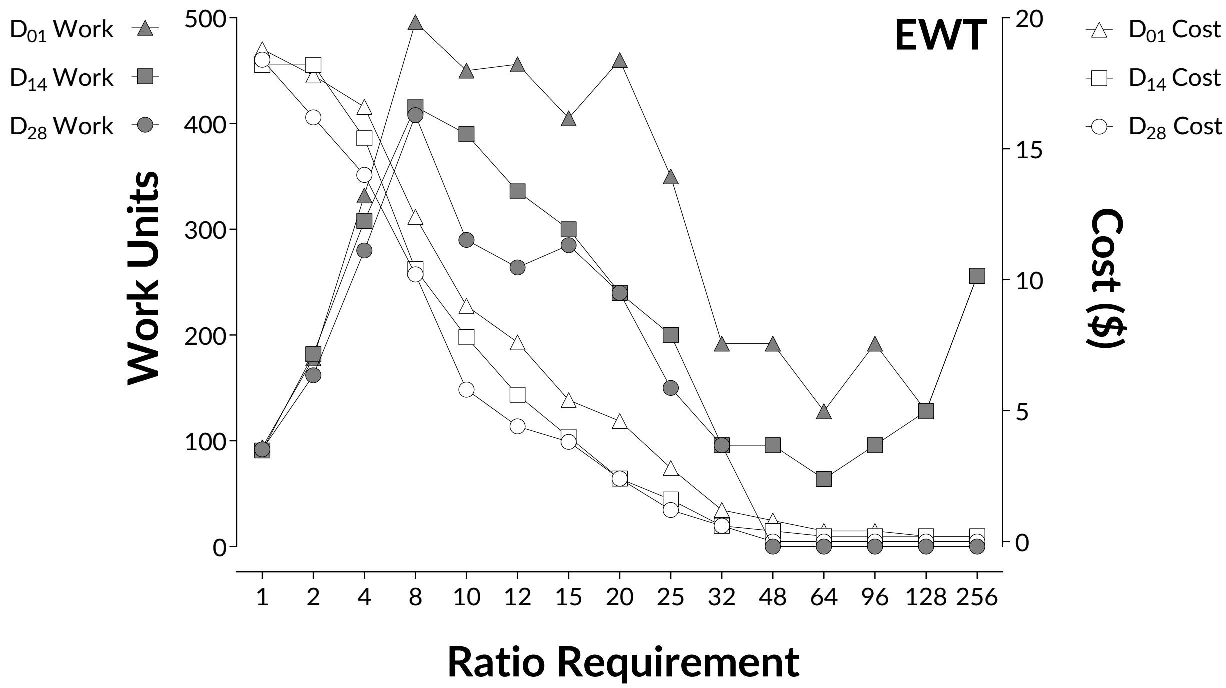


Figure 28. Cost-benefit analysis of the EWT in Experiment 3. Number of work units completed are denoted by the grey data points and scaled to the left y-axis. Cost in dollars per ratio requirement are depicted by the open data points and plotted on the right y-axes.

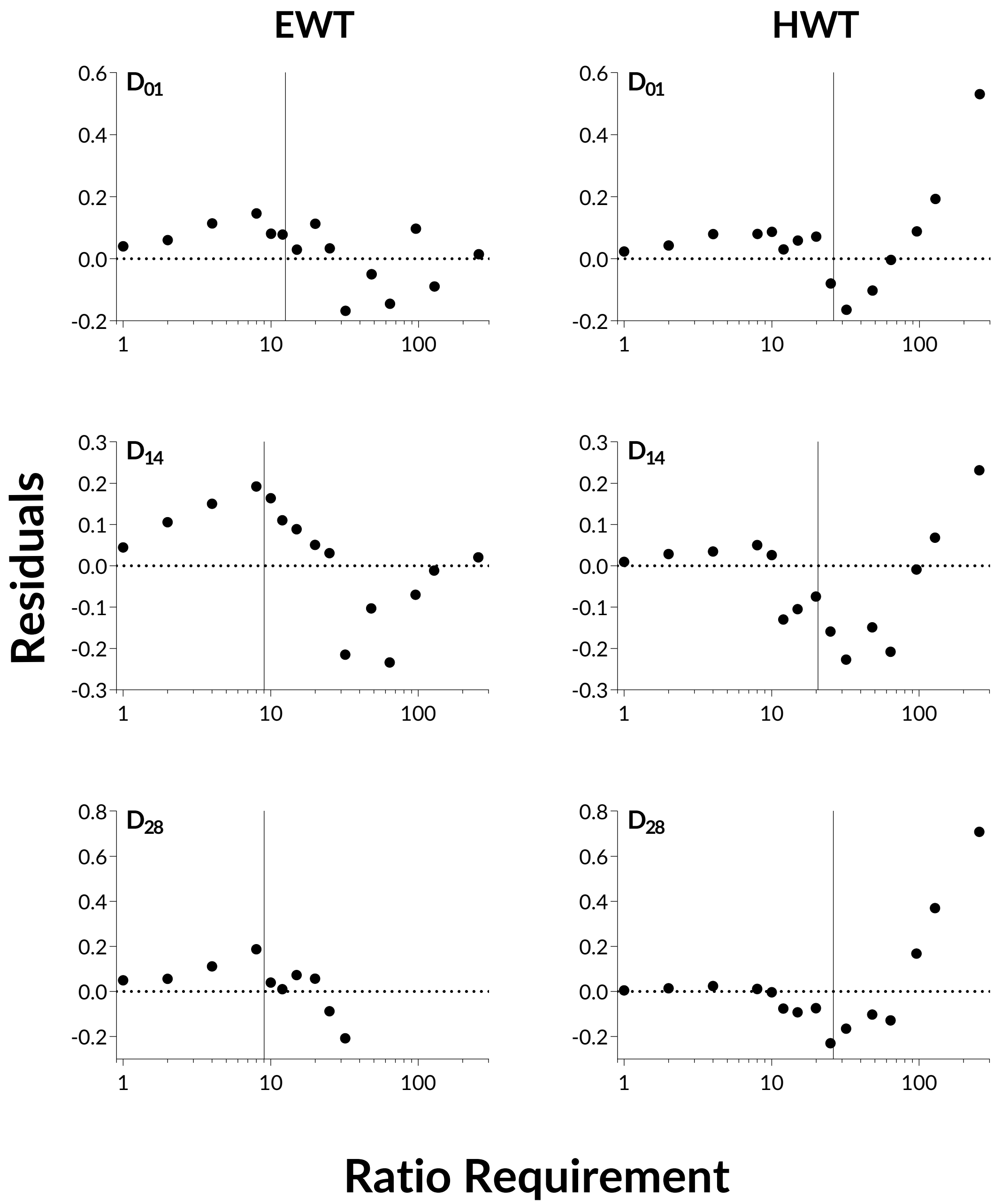


Figure 29. Residuals (distance from best-fit curve) from *AtD* curves in the EWT (left panel) and HWT (right panel) in Experiment 3. Residual plots for the D_{01}, D_{14}, and D_{28} conditions are displayed in the top, middle, and bottom panels, respectively. Vertical lines represent derived P_{max}. The horizontal line at zero denotes the best-fit curve of the *AtD* curve.

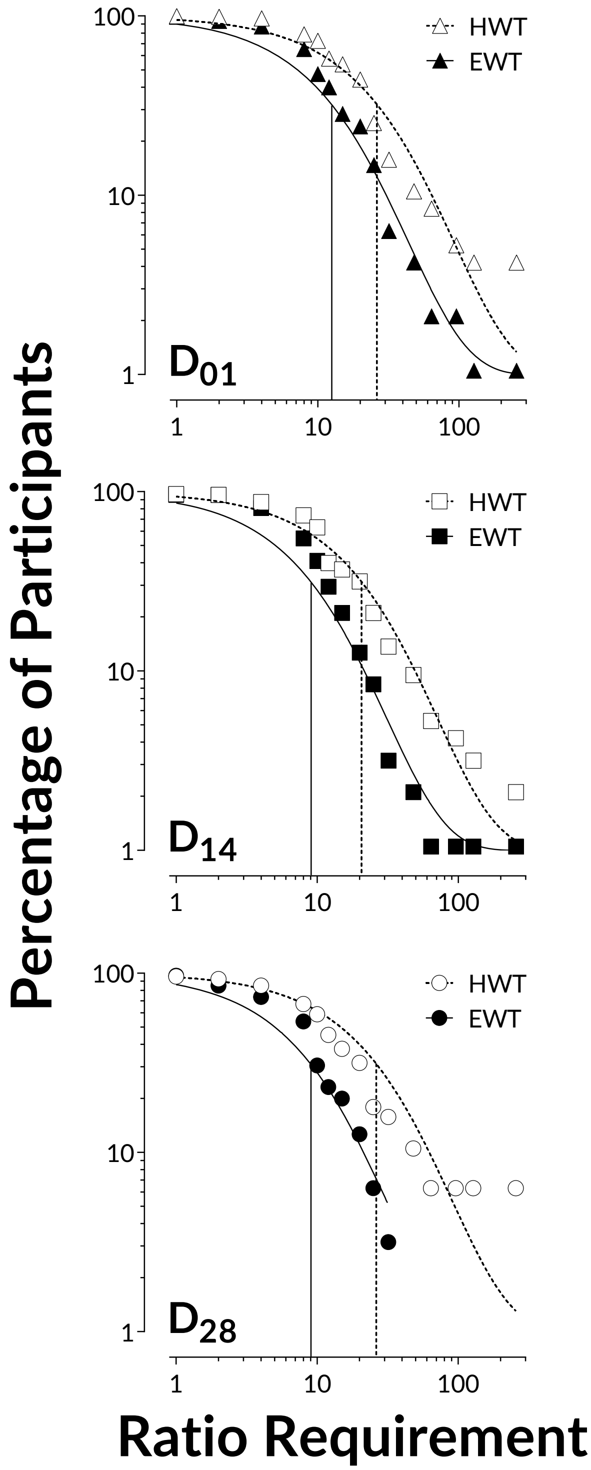


Figure 30. Experiment 3 analog to demand curves for the HWT and EWT separately. Vertical lines represent derived $P_{\max}$.

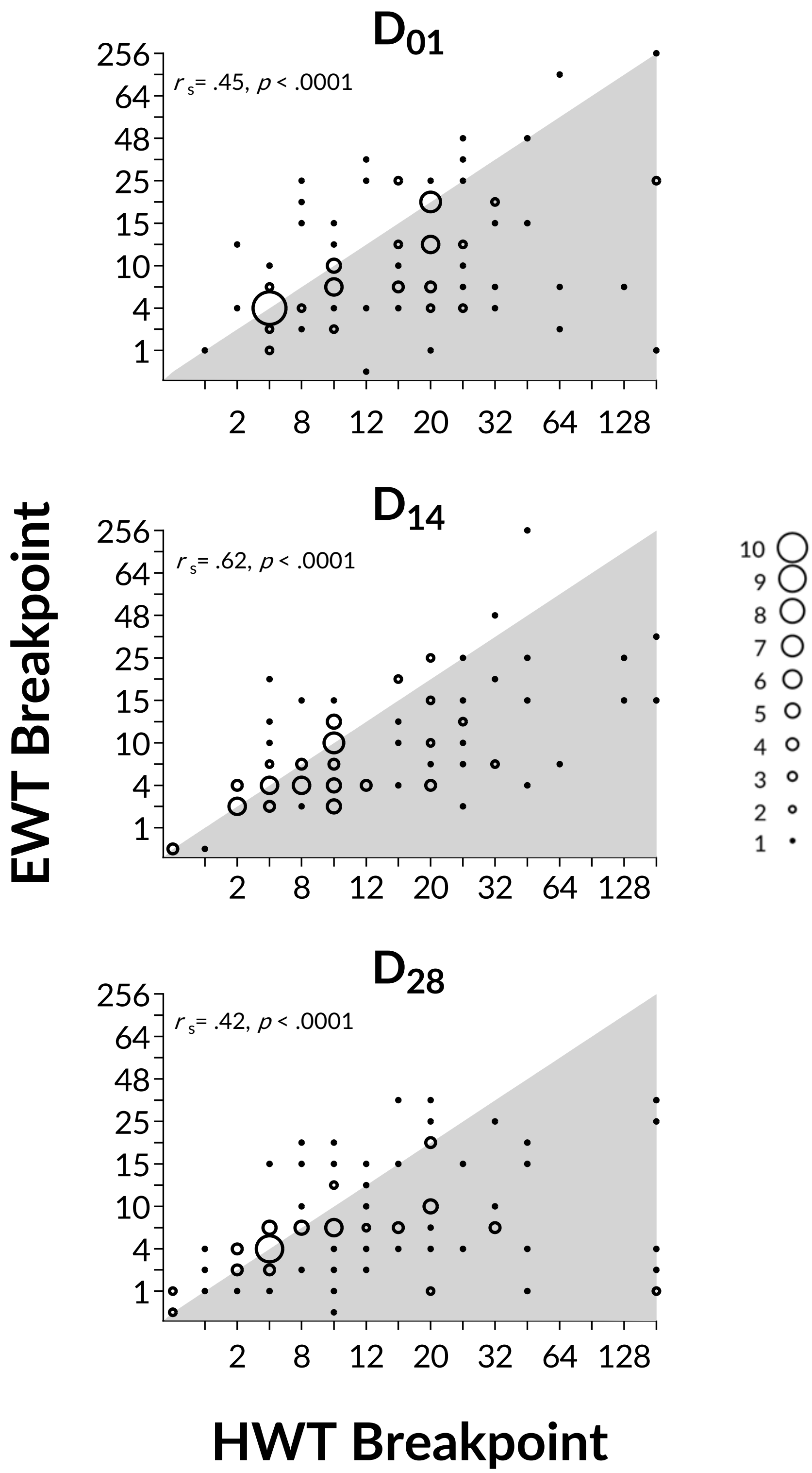


Figure 31. Predictive validity of the HWT in Experiment 3. The size of the data point symbols reflects the number of participants whose data are represented by that value.

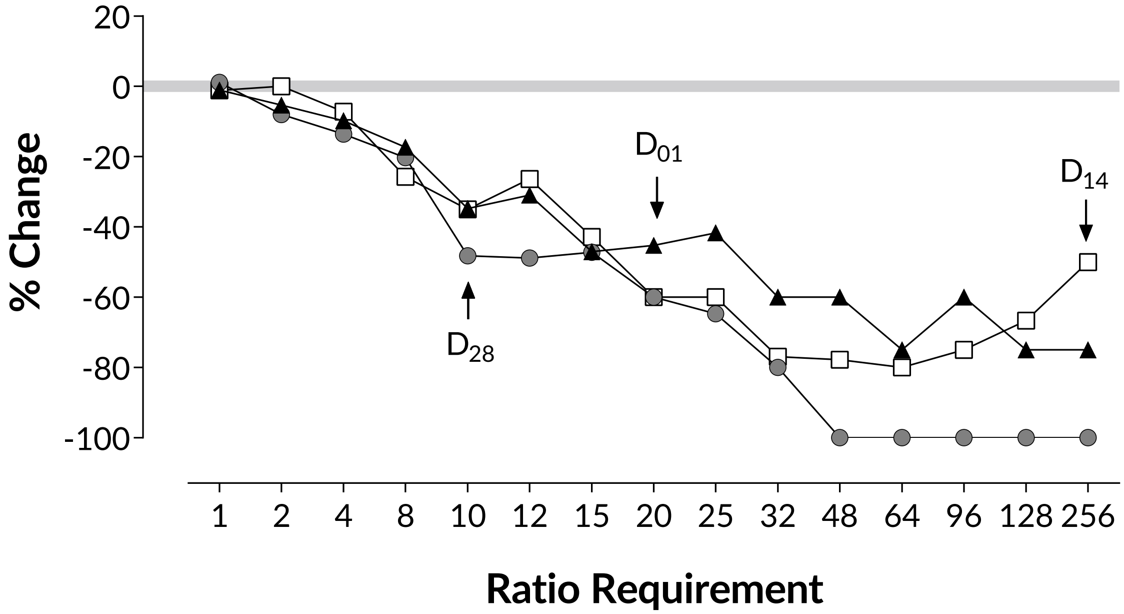
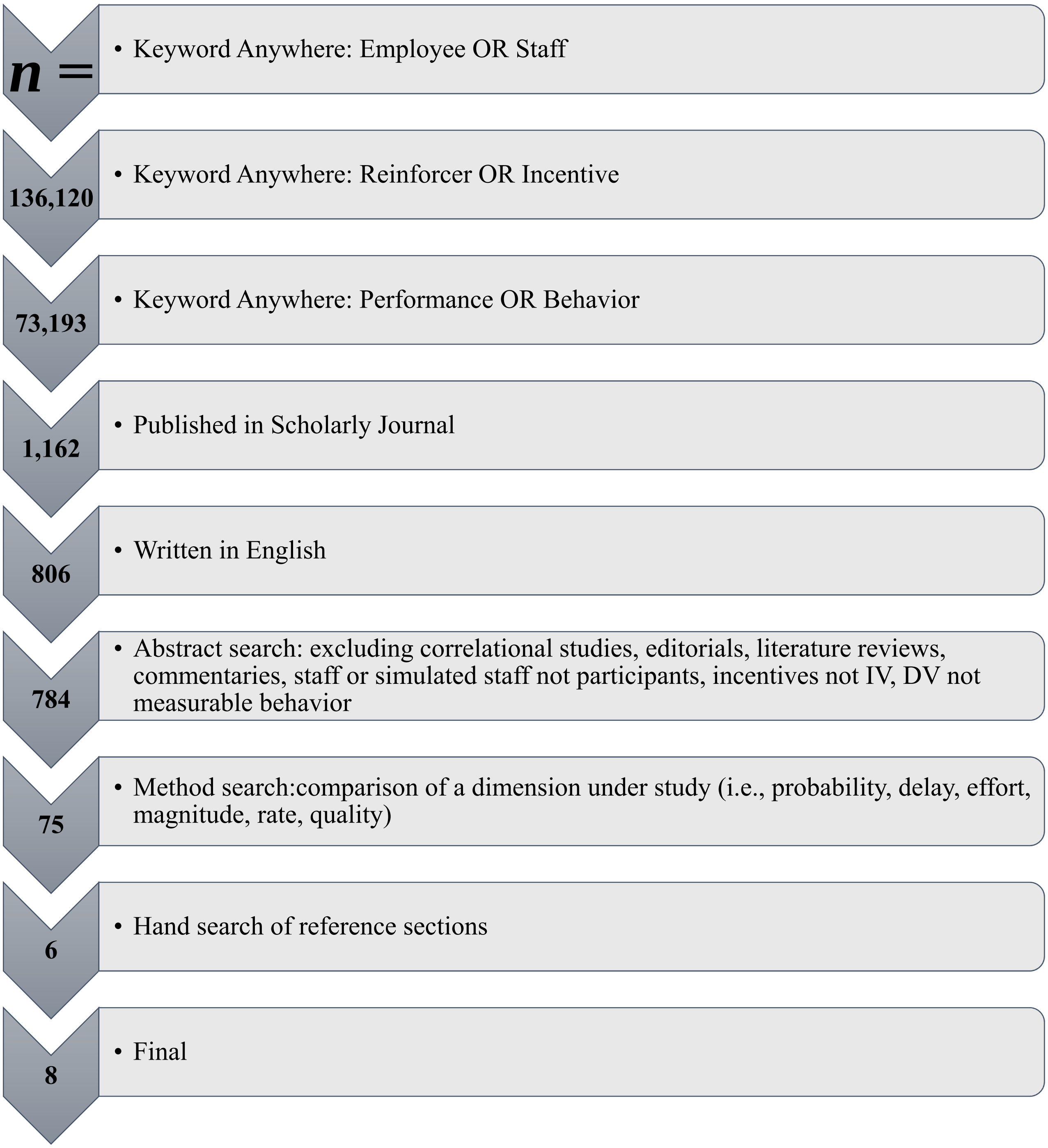


Figure 32. Percentage change in predicted work output and corresponding cost from the HWT to observed output and cost in the EWT for Experiment 3. The gray horizontal line indicates a 0% change, or perfect aggregate correspondence. Data points below the line indicate less work completed and a lower cost observed in the EWT than predicted by the HWT.

Appendix A

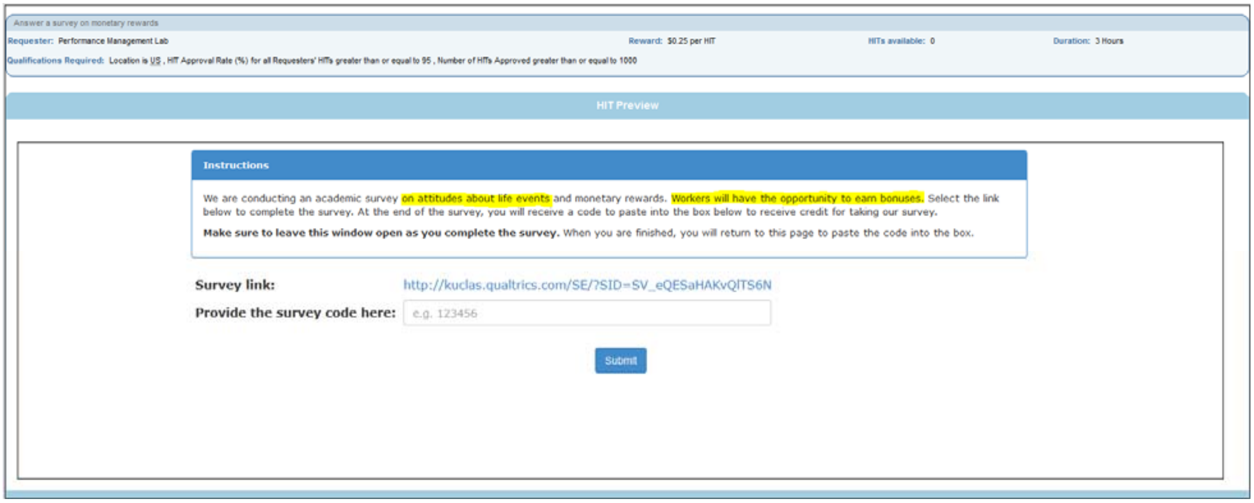
Literature search procedures, conducted on November 5, 2015.



Appendix B

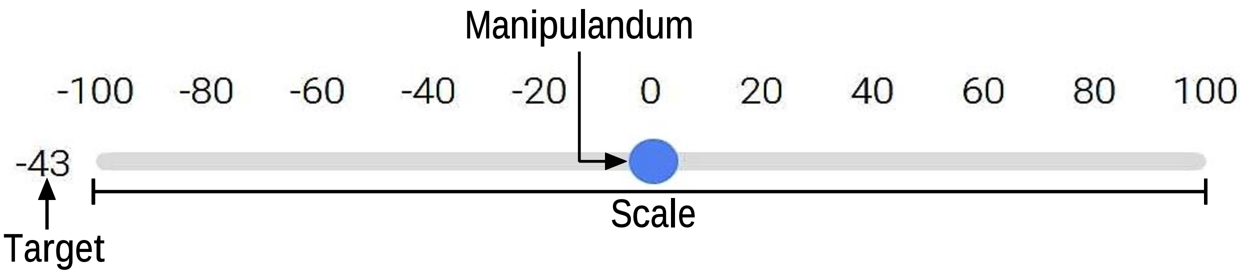
Experiment 1 Amazon Mechanical Turk HIT.

Highlighted content indicates information removed in Experiments 2 and 3.



Appendix C

Sample work unit.



Appendix D

Monetary choice questionnaire.

For each question below, please select the option that you would prefer.

Which would you rather have:	
$54 tonight	$55 in 117 days
$55 tonight	$75 in 61 days
$19 tonight	$25 in 53 days
$31 tonight	$85 in 7 days
$14 tonight	$25 in 19 days
$47 tonight	$50 in 160 days
$15 tonight	$35 in 13 days
$25 tonight	$60 in 14 days
$78 tonight	$80 in 162 days
$40 tonight	$55 in 62 days
$11 tonight	$30 in 7 days
$67 tonight	$75 in 119 days
$34 tonight	$35 in 186 days
$27 tonight	$50 in 21 days
$69 tonight	$85 in 91 days
$49 tonight	$60 in 89 days
$80 tonight	$85 in 157 days
$24 tonight	$35 in 29 days
$33 tonight	$80 in 14 days
$28 tonight	$30 in 179 days
$34 tonight	$50 in 30 days
$25 tonight	$30 in 80 days
$41 tonight	$75 in 20 days
$54 tonight	$60 in 111 days
$54 tonight	$80 in 30 days
$22 tonight	$25 in 136 days
$20 tonight	$55 in 7 days

Appendix E

Behavioral inhibition system/behavioral activation system scale.

Please respond to the following statements by indicating the degree to which you either Agree or Disagree.

A person's family is the most important thing in life.[e]			
Strongly Disagree	Disagree	Agree	Strongly Agree
Even if something bad is about to happen to me, I rarely experience fear or nervousness.[a,f]			
Strongly Disagree	Disagree	Agree	Strongly Agree
I go out of my way to get things I want.[b]			
Strongly Disagree	Disagree	Agree	Strongly Agree
When I'm doing well at something I love to keep at it.[d]			
Strongly Disagree	Disagree	Agree	Strongly Agree
I'm always willing to try something new if I think it will be fun.[c]			
Strongly Disagree	Disagree	Agree	Strongly Agree
How I dress is important to me.[e]			
Strongly Disagree	Disagree	Agree	Strongly Agree
When I get something I want, I feel excited and energized.[d]			
Strongly Disagree	Disagree	Agree	Strongly Agree
Criticism or scolding hurts me quite a bit.[a]			
Strongly Disagree	Disagree	Agree	Strongly Agree
When I want something I usually go all-out to get it.[b]			
Strongly Disagree	Disagree	Agree	Strongly Agree
I will often do things for no other reason than that they might be fun.[c]			
Strongly Disagree	Disagree	Agree	Strongly Agree
It's hard for me to find time to do things such as get a haircut.[e]			
Strongly Disagree	Disagree	Agree	Strongly Agree
If I see a chance to get something I want I move on it right away.[b]			
Strongly Disagree	Disagree	Agree	Strongly Agree

I feel pretty worried or upset when I think or know somebody is angry at me.[a]			
Strongly Disagree	Disagree	Agree	Strongly Agree
When I see an opportunity for something I like I get excited right away.[d]			
Strongly Disagree	Disagree	Agree	Strongly Agree
I often act on the spur of the moment.[c]			
Strongly Disagree	Disagree	Agree	Strongly Agree
If I think something unpleasant is going to happen I usually get pretty "worked up."[a]			
Strongly Disagree	Disagree	Agree	Strongly Agree
I often wonder why people act the way they do.[e]			
Strongly Disagree	Disagree	Agree	Strongly Agree
When good things happen to me, it affects me strongly.[d]			
Strongly Disagree	Disagree	Agree	Strongly Agree
I feel worried when I think I have done poorly at something important.[a]			
Strongly Disagree	Disagree	Agree	Strongly Agree
I crave excitement and new sensations.[c]			
Strongly Disagree	Disagree	Agree	Strongly Agree
When I go after something I use a "no holds barred" approach.[b]			
Strongly Disagree	Disagree	Agree	Strongly Agree
I have very few fears compared to my friends.[a,f]			
Strongly Disagree	Disagree	Agree	Strongly Agree
It would excite me to win a contest.[d]			
Strongly Disagree	Disagree	Agree	Strongly Agree
I worry about making mistakes.[a]			
Strongly Disagree	Disagree	Agree	Strongly Agree

[a]Behavioral inhibition scale.
[b]Behavioral activation scale – drive.
[c]Behavioral activation scale – fun seeking.
[d]Behavioral activation scale – reward responsiveness.
[e]Filler question.
[f]Reverse scored item

Appendix F

Experiment 1 demographic questionnaire.

What is your current age?

What is your gender?
- ❍ Male
- ❍ Female

Race/ethnic background:
- ❍ White/Caucasian
- ❍ Black/African American
- ❍ Hispanic/Latino
- ❍ Asian
- ❍ Native American
- ❍ Pacific Islander
- ❍ Mixed
- ❍ Other

In what state do you currently reside?
[drop down list]

What is the highest level of education you have completed?
- ❍ Less than High School
- ❍ High School / GED
- ❍ Some College
- ❍ 2-year College Degree
- ❍ 4-year College Degree
- ❍ Masters Degree
- ❍ Doctoral Degree
- ❍ Professional Degree (JD, MD)

What is your combined annual household income?
- ❍ Less than 30,000
- ❍ 30,000 – 39,999
- ❍ 40,000 – 49,999
- ❍ 50,000 – 59,999
- ❍ 60,000 – 69,999
- ❍ 70,000 – 79,999
- ❍ 80,000 – 89,999
- ❍ 90,000 – 99,999

- ❍ 100,000 or more

Do you have a DOCUMENTED disability?

- ❍ Yes
- ❍ No

If yes, please specify:

- ❍ Learning disability
- ❍ Attention deficit/hyperactivity disorder
- ❍ Physical disability
- ❍ Other

Are you currently, or have you ever been a smoker?

- ❍ Yes - Currently
- ❍ Yes - Previously
- ❍ No - Never

Appendix G

Experiment 1 practice trial, HWT, and EWT instructions and ratio wording.

INSTRUCTIONS AT START OF EACH SECTION:

PRACTICE TRIAL

In the following section you will be asked whether or not you would be willing to complete a given number of "slider questions" to earn a 5/10/20 cent bonus if given the opportunity. To continue please complete the practice slider question according to the instructions below:

Please move the slider to match the number indicated to the left. Once the numbers match, press the next button to continue

HWT

For each question below, please indicate whether you would complete the number of slider questions indicated to receive the bonus.

Please answer honestly, thoughtfully, and as if you were actually in this situation.

EWT

Next, you will have the opportunity to complete slider questions to earn **real** bonuses paid after you complete the study.

Each section will begin with a statement indicating the amount of the bonus and the number of questions that must be completed to earn the bonus. You will then be asked if you would like to complete the section. If you select "no" the survey will end.

If you would no longer like to continue after beginning a section, you may exit the survey by closing your browser or returning to the mTurk website. If you exit the survey before finishing the section you will **not** receive the bonus for the section. Thank you for taking the time to complete the survey.

- **If you wish to receive bonuses**, you must wait to submit your unique access code until you are finished. Be sure to write the code down.
- **If you no longer wish to continue**, copy your unique access code found below and submit it through the Amazon Mechanical Turk survey page.

WORDING FOR EACH RATIO:

HWT

Would you complete 1 slider question in exchange for a 5/10/20 cent bonus?

EWT

You can earn a bonus of 5/10/20 cents by completing 1 question in this section. Would you like to continue?

Appendix H

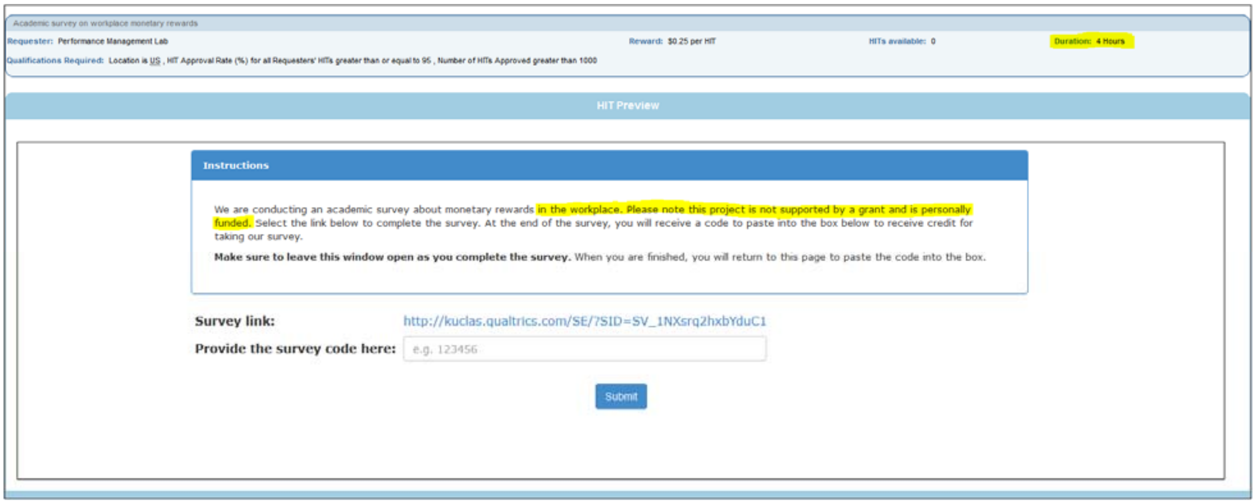
Scoring for the Monetary Choice Questionnaire.

Response Option			
Smaller-sooner	**Larger-later**	***k***	**Magnitude**
$34 tonight	$35 in 186 days	0.00016	Small
$54 tonight	$55 in 117 days	0.00016	Medium
$78 tonight	$80 in 162 days	0.00016	Large
$28 tonight	$30 in 179 days	0.0004	Small
$47 tonight	$50 in 160 days	0.0004	Medium
$80 tonight	$85 in 157 days	0.0004	Large
$22 tonight	$25 in 136 days	0.001	Small
$54 tonight	$60 in 111 days	0.001	Medium
$67 tonight	$75 in 119 days	0.001	Large
$25 tonight	$30 in 80 days	0.0025	Small
$49 tonight	$60 in 89 days	0.0025	Medium
$69 tonight	$85 in 91 days	0.0025	Large
$19 tonight	$25 in 53 days	0.006	Small
$40 tonight	$55 in 62 days	0.006	Medium
$55 tonight	$75 in 61 days	0.006	Large
$24 tonight	$35 in 29 days	0.016	Small
$34 tonight	$50 in 30 days	0.016	Medium
$54 tonight	$80 in 30 days	0.016	Large
$14 tonight	$25 in 19 days	0.041	Small
$27 tonight	$50 in 21 days	0.041	Medium
$41 tonight	$75 in 20 days	0.041	Large
$15 tonight	$35 in 13 days	0.1	Small
$25 tonight	$60 in 14 days	0.1	Medium
$33 tonight	$80 in 14 days	0.1	Large
$11 tonight	$30 in 7 days	0.25	Small
$20 tonight	$55 in 7 days	0.25	Medium
$31 tonight	$85 in 7 days	0.25	Large

Appendix I

Experiments 2 and 3 Amazon Mechanical Turk HIT.

Highlighted content denotes modifications from Experiment 1.



Appendix J

Experiments 2 and 3 practice trial, HWT, and EWT instructions and ratio wording.

INSTRUCTIONS AT START OF EACH SECTION:

PRACTICE TRIAL

The task in this HIT involves matching numbers on a slider scale. Below you have three opportunities to practice before you begin. Please move the slider to match the number indicated on the left of the slider scale. Then press the "next" button to continue.

HWT

For each question below, please indicate whether you would complete the indicated number of slider questions to receive the bonus if given the opportunity.

There are no right or wrong responses. Please answer honestly, thoughtfully, and as if you were actually in this situation.

EWT

Next, you will have the opportunity to complete slider questions to earn **real** bonuses paid after you complete the study.

Each section will begin with a statement indicating the amount of the bonus and the number of slider questions that must be completed to earn the bonus. You will then be asked if you would like to complete the section.

If you exit the survey before finishing the section, you will not receive the bonus for that section.

WORDING FOR EACH RATIO:

HWT

Experiment 2 - Probability: Would you complete 1 slider question in exchange for a 9 in 10 (90%) / 5 in 10 (50%) / 1 in 10 (10%) chance of receiving a 22 cent / 40 cent / 2 dollar bonus paid in 1 day?

Experiment 3 - Delay: Would you complete 1 slider question in exchange for a 20 cent bonus paid for certain in 1/14/28 day(s)?

EWT

Experiment 2 - Probability: Would you like to complete 1 slider question in exchange for a 9 in 10 (90%) / 5 in 10 (50%) / 1 in 10 (10%) chance of earning a 22 cent / 40 cent / 2 dollar bonus paid in 1 day?

Experiment 3 - Delay: Would you like to complete 1 slider question in exchange for a 20 cent bonus paid for certain in 1/14/28 day(s)?

Appendix K

Experiments 2 and 3 demographic, employment, and incentive survey.

What is your current age?

What is your gender?
- ❍ Male
- ❍ Female

Race/ethnic background:
- ❍ White/Caucasian
- ❍ Black/African American
- ❍ Hispanic/Latino
- ❍ Asian
- ❍ Native American
- ❍ Pacific Islander
- ❍ Mixed
- ❍ Other

In what state do you currently reside?
[drop down list]

What is the highest level of education you have completed?

- ❍ Less than High School
- ❍ High School / GED
- ❍ Some College
- ❍ 2-year College Degree
- ❍ 4-year College Degree
- ❍ Master's Degree
- ❍ Doctoral Degree
- ❍ Professional Degree (JD, MD)

What is your combined annual household income?

- ❍ Less than 30,000
- ❍ 30,000 – 39,999
- ❍ 40,000 – 49,999
- ❍ 50,000 – 59,999
- ❍ 60,000 – 69,999
- ❍ 70,000 – 79,999
- ❍ 80,000 – 89,999
- ❍ 90,000 – 99,999
- ❍ 100,000 or more

Approximately how many hours do you spend on Mechanical Turk a week?

[ ]

Why do you complete tasks in Mechanical Turk? Please check **all that apply**:

- ❑ Fruitful way to spend free time and get some cash (e.g., instead of watching TV)
- ❑ For "primary" income purposes (e.g., gas, bills, groceries, credit cards)
- ❑ For "secondary" income purposes, pocket change (e.g., for hobbies, gadgets, going out)
- ❑ To kill time
- ❑ I find the tasks to be fun
- ❑ I am currently unemployed, or have only a part time job
- ❑ I like to participate in research
- ❑ Other: ____________________

Approximately how much money do you earn a week on Mechanical Turk?

[ ]

How long were you employed at your most recent paid position outside of Mechanical Turk in the past 10 years?

- I have not had a paid position in the past 10 years
- 0-3 months
- 3-6 months
- 6 months - 1 year
- 1 - 3 years
- 3 - 5 years
- 5 - 10 years
- 10 - 20 years
- 20+ years

How many promotions did you receive at your most recent paid position?

- 0
- 1
- 2
- 3
- 4
- 5+

Which occupational category **best** describes your employment?

- ❍ Architecture/Engineering (e.g. architect, landscape architect, surveyor, cartographer, engineer, drafter)
- ❍ Art and Design (e.g. fine artist, animator, graphic/floral/interior designer, multimedia artist, set/exhibit designer, art director)
- ❍ Building and Grounds Cleaning and Maintenance (e.g. landscaper, tree-trimmer, building cleaner, janitor, pest control)
- ❍ Business and Financial Operations (e.g. financial specialist, budget analyst, event planner, agent, buyer, claims adjuster, real estate assessor, human resources specialist, accountant)
- ❍ Community and Social Service (e.g. mental health counselor, social worker, guidance counselor, clergy, health educator, probation officer)
- ❍ Computer/Mathematical (e.g. computer programmer, network/database administrator, mathematician, statistician, software/web developer, user support)
- ❍ Construction/Installation/Repair (e.g. mason, carpenter, electrician, pipefitter, building inspector, equipment repair, electronics installer, mechanic)
- ❍ Education/Training/Library (e.g. teacher, adult educator, teaching assistant, librarian, curator, archivist)
- ❍ Entertainer/Performer (e.g. actor, producer, director, musician, dancer, athlete, coach)
- ❍ Farming/Fishing/Forestry Worker (e.g. farm/greenhouse/fishing/forestry worker, agricultural inspector)
- ❍ Food Preparation/Serving (e.g. cook, bartender, food server, caterer, dishwasher, host)
- ❍ Healthcare Practitioner or Technician (e.g. physician, nurse, veterinarian, physical/occupational/recreational therapist, nutritionist, EMT, laboratory technician)
- ❍ Healthcare Support (e.g. medical/dental/veterinary assistant, massage therapist, home health aide)
- ❍ Legal Occupations (e.g. lawyer, legal assistant, paralegal, title examiner)
- ❍ Life Science (e.g. biologist, ecologist, zoologist, biochemist, conservation/plant/soil scientist, forester)
- ❍ Management (e.g. managers of: operations, marketing, public relations, human resources, advertising, finance, hotels, restaurants, etc.)
- ❍ Media and Communications (e.g. writer, editor, reporter, announcer, interpreter, media equipment technician, photographer, film/video/TV operator, public relations specialist)
- ❍ Military and Protective Service (e.g. military officer, infantry, police officer, firefighter, security guard, lifeguard, ski patrol, animal control, game warden)
- ❍ Office/Administrative Support (e.g. financial/billing/file/mail clerk, bookkeeper, teller, receptionist, administrative assistant, data entry processor, library assistant, legal secretary)
- ❍ Personal Care and Service (e.g. hairstylist, fitness trainer, usher, childcare worker/nanny, recreation worker, travel/wilderness/river raft/kayak guide, nonfarm animal caretaker/trainer)
- ❍ Physical Science (e.g. physicist, chemist, astronomer, hydrologist, geoscientist)
- ❍ Production/Manufacturing (e.g. assembler, machinist, textile worker, woodworker, plant operator, photo processor, welder, printing worker, baker, butcher)

- ❍ Sales (e.g. retail management, cashier, sales/advertising representative, travel agent, real estate broker, telemarketer)
- ❍ Social Science (e.g. clinical/counseling/school psychologist, economist, survey researcher, anthropologist, sociologist, historian, political scientist, regional planner)
- ❍ Transportation (e.g. truck/bus/taxi/ambulance driver, material mover, sailor, pilot, flight attendant, railway worker)

What was the reason for leaving your last paid position?
- ❍ I accepted a new job at a different company
- ❍ I was promoted in the same company
- ❍ I decided not to work outside of the home
- ❍ I was laid off or otherwise terminated (seasonal work, employer downsizing, dismissed)
- ❍ I prefer not to answer
- ❍ Other (please specify):

Excluding an annual pay increase or holiday bonus, did you receive incentives or rewards in the form of money or other gifts (preferred parking spaces, time off, lunch, water bottles, etc) at your most recent paid position? **Mark all that apply.**
- ❍ Yes, I received monetary incentives
- ❍ Yes, I received other gifts
- ❍ No

How often were these incentives delivered?
- ❍ Daily
- ❍ Weekly
- ❍ Monthly
- ❍ Quarterly
- ❍ Twice yearly
- ❍ Annually
- ❍ Other (please specify) ____________________