Effort Momentum

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Abstract

This paper examines how past effort can impact subsequent effort, such as when effort is reduced following an interruption. I conducted 3 incentivized real-effort experiments in which both piece rates and leisure options were manipulated and find effort displays significant stickiness, even in the absence of switching costs. I demonstrate that this intertemporal evidence is indicative of effort "momentum", rather than on-the-job learning, reciprocity, or income targeting. Five minutes after incentives return to baseline, 45% of the effort increase or decrease persists. This finding is especially relevant for studies employing individual fixed effects and for organizations concerned with worker disruptions.

Keywords: intertemporal labor, effort allocation, momentum, interruptions

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1 Introduction

By some estimates, interruptions disrupt 1.5 to 2.1 hours per work day for over 56 million US "knowledge workers" (Gonzalez and Mark, 2004; Spira and Feintuch, 2005). Observational studies show that hospital workers are interrupted 5 times per hour (Weigl et al., 2014; Berg et al., 2013) while software developers and managers are interrupted 25 times per day (Gonzalez and Mark, 2004).[1] In similar studies, 15-23% of interrupted work is not resumed on the same day, a particular concern within the health services literature (Westbrook et al., 2010; Mark et al., 2005). However this evidence is difficult to interpret as the interruptions themselves may be necessary, e.g. within a hospital's emergency department. In other settings, disruptions from a manager may reduce principal-agent concerns through increased monitoring or communication as formulated by Coviello et al. (2014).

Thus, whether firms and government organizations might benefit from interruption-reducing policies depends on whether the productivity loss (or gain) persists over time. Even if the interrupting worker gets a boost in productivity, the interrupted worker may have difficulty returning to their work. With imperfect monitoring, interrupting workers may not internalize these externalities, potentially resulting in too many disruptions during work. Alternatively if workers are able to use periods of reduced effort as a "break", then the costs of interrupted work may be partly offset by increased productivity in the following time period.[2] In short, is knowledge-based work is best explained by a "fatigue" model or a "habit" model?

This paper addresses this more general question of whether a productivity loss persists over time, and if so, what might be done to recover this loss. This "loss of momentum" is often posited by the psychology literature,[3] media,[4] and consulting reports,[5] but has not been thoroughly examined within the economics literature. The closest literature on intertemporal labor supply tends to focus on longer time scales and hours worked rather than output (Camerer et al., 1997; Oettinger, 1999; Farber, 2005, 2008; Crawford and Meng, 2011; Chetty et al., 2011; Cai et al., 2016). Within this literature, the concurrent Cai et al. (2016) finds field evidence that when a machine breaks down for the day, workers

[1] While half of these interruptions are considered "internal", which may be more indicative of task juggling (Coviello et al., 2014), the remaining half of "external" interruptions are arguably the most time intensive. For example, the data in Gonzalez and Mark (2004) also shows that 1.5 hours per day were spent on unscheduled meetings such as workers stopping by or talking through cubicle walls.

[2] Similarly, if an employer actively monitors employees to boost worker effort in the short term, does this cause workers to be exhausted or does it have gains even after the employer leaves the area?

[3] See an extensive psychology literature on "flow", cf. Nakamura and Csikszentmihalyi (2002); Schaffer (2013), in which clear tasks with adequate challenge and objective goals enables a continuous work state.

[4] E.g. "*First, there is the diversion itself, taking your employees off task after they have assembled the resources and thinking necessary for that particular task. Then there is the restart—reassembling the resources, thoughts, and readiness. There is the loss of momentum caused by the initial distraction from the original purpose.*" (Brown (2015))

[5] Such as "*.. even a one-minute interruption can easily cost a knowledge worker 10 to 15 minutes of lost productivity due to the time needed to reestablish mental context and reenter the flow state.*" (Nielsen (2003))

are 2% less productive on the following day. Fehr and Goette (2007) employ a field experiment on bicycle riders and finds evidence consistent with a model in which past effort exhausts riders, making additional effort more costly. Under this model, an exogenous interruption in effort could actually boost future productivity as the worker has had a chance to physically recover.[6] However, the longer time scales and the physical nature of the tasks employed in previous studies make their findings difficult to apply to short scale disruptions among a broader class of knowledge workers.[7]

In this paper, I hypothesize and test a theory in which past effort has a direct effect on the marginal disutility from exerting effort in the present. This model has theoretical similarities to a model of habit preferences, but over effort rather than consumption. I refer to this theory as effort momentum to capture the basic insight of how (lack of) effort in one period persists in future time periods.[8]

To test for the presence of effort momentum over short time scales, I conducted a series of real-effort laboratory experiments with a total of 761 University of Pennsylvania students at the Wharton Behavioral Lab. This controlled setting allows me to observe workers' responses to both piece rates and leisure opportunities over multiple periods. The workers complete counting or slider tasks on a computer screen but have the option to engage in leisure by viewing YouTube videos at any time.[9] I manipulate (i) the piece rate for completed tasks and (ii) the leisure opportunities available (by varying subjects' access to their cell phones). Subjects are quizzed prior to every period to ensure incentives and leisure options are understood.

The laboratory setting for the experiment allows for accurate measurement of productivity. This accuracy is critical to inducing and detecting variation in effort over short time scales. In addition, while the tasks involved are somewhat artificial, exerting effort on a computer located in a cubicle closely resembles a relevant work environment for many knowledge workers (Gonzalez and Mark, 2004).[10] The laboratory also eliminates peer effect confounds that might be present in a field setting, such as fairness concerns over some workers being paid more. Moreover, the setting allowed me to replicate across multiple designs and tasks to ensure effort momentum is not limited to a single context.

[6] While consistent with an effort exhaustion model, the authors find that individual measures of loss aversion are predictive of the effort decrease, suggesting a model of loss-aversion may be more appropriate for that setting.

[7] There is a noted lack of consensus regarding what constitutes a "knowledge worker", but in line with the literature, I characterize knowledge work "as less tangible than manual work and using the worker's brain as the means of production" (Ramírez and Nembhard, 2004). This would roughly coincide with the 56 million "management, professional, and related occupations" from the 2014 Current Population Survey from the US.

[8] Yet it is worth noting that a separate strand of literature investigates the possibility of momentum in contests. Specifically, how winning one contest might impact the probability of winning another one (Page, 2009; Page and Gauriot, 2017). However, in such a game, momentum is no longer a function of only the individual's effort but also the opponent's, and is conceptually distinct.

[9] This is in line with recent work demonstrating the importance of outside leisure options for external validity of laboratory experiments (Corgnet et al., 2014; Charness et al., 2010; Eriksson et al., 2009; Kessler and Norton, 2015).

[10] However there remain notable concerns over external validity of laboratory experiments. For a detailed discussion, please see Charness and Kuhn (2011); Falk and Heckman (2009); Levitt and List (2007).

The laboratory environment also made it possible to accurately enforce available leisure opportunities, particularly cell phone access. This leisure variation is important for differentiating momentum from alternate theories such as reciprocity.

My results show significant evidence of effort stickiness. Workers treated with a higher (lower) piece rate exert more (less) effort in the treated period relative to control.[11] Even after financial incentives return to baseline, workers who previously received a higher piece rate continue to work harder than those who only received the baseline piece rate. This lingering effort differential is approximately half of the original effort increase induced by the heightened piece rate. By the same token, workers who receive a lower piece rate in one period continue to exert less effort in following periods relative to the control group. These findings indicate that effort allocation in one period may depend positively on recent work effort.

This evidence of effort stickiness could be a result of momentum, reciprocity, on-the-job learning, or potentially other interpretations. To identify the source of this effort stickiness, I structured the experimental design to provide additional comparisons informed by theoretical predictions. One key feature of this design is that some workers are randomly informed of future piece rate and leisure opportunities a full period in advance. Previous studies on intertemporal effort allocation feature either imperfectly anticipated shocks (Camerer et al., 1997; Oettinger, 1999; Pistaferri, 2003) or fully anticipated shocks (Lozano, 2011; Fehr and Goette, 2007); none (to my knowledge) intentionally manipulate the degree of anticipation of piece rate or leisure shocks.[12]

I am able to differentiate between reciprocity and effort momentum using this variation in anticipation. First, one would expect a reciprocating worker to reciprocate immediately upon receiving the news of an increased piece rate, not just the periods following the higher piece rate.[13] I find no such evidence.[14] Second, for those who enjoy the additional leisure opportunity (cellphone), one would expect them to reciprocate with higher effort in surrounding periods.[15] I also uncover no evidence of

[11] I find a positive elasticity of effort with respect to that period's piece rate of approximately 5% to 10%. In comparison to previous papers, this is a small but significant elasticity (Chetty et al., 2011). I also find a significant negative effect on effort when given access to cell phones in some specifications.

[12] As Fehr and Goette (2007) stress, the anticipation of wage changes is critical to interpretation of these elasticities. In studies with imperfect knowledge of future wage variation, workers with better predictions may react differently to wage changes and this prediction ability could also be correlated with work ability (see Pistaferri (2003)). Yet, informing workers about wage changes has the potential to trigger reciprocity toward the employer (Rabin, 1993; Fehr and Schmidt, 2006). This complicates the interpretation of previously measured intertemporal elasticities, as anticipation and reciprocation are linked in studies with anticipated wage changes. Furthermore, Gneezy and List (2006) suggest reciprocity may decline over time, potentially introducing an upward bias to wage elasticities measured over a short time period (if reciprocity is a large factor).

[13] Indeed, if one expects to find evidence of reciprocity either pre- or post-treatment, Gneezy and List (2006) suggests that the pre-treatment effects should be larger than post-treatment due to the declining effects of reciprocity over time.

[14] One specification is significant in experiment 3, but as seen in Robustness Table 6C this seems to be capturing period variation (as knowledge of future piece rates can only occur in period 1).

[15] Some recent evidence regarding the importance of non-monetary gifts suggests this might even trigger greater reciprocation than the financial rewards (Kosfeld and Neckermann (2011); Bradler et al. (2013); Kube et al. (2012)).

this, but rather find effort is significantly reduced following phone access for those affected, as consistent with effort momentum. Thus, effort stickiness seems unlikely to be driven by reciprocity in this setting.

To address concerns about on-the-job learning, the experiments feature extensive "training" periods. Analyzing the training period data suggests that subjects reach full competency with the tasks within the first 3 minutes (see Figures 3 and 4). After this time, average output is remarkably flat, as opposed to increasing output predicted by an on-the-job learning model.[16] This may not be surprising given the extreme simplicity of the tasks and is further confirmed by post-experimental surveys (see Section 3). In addition, one would expect the gains from on-the-job learning to persist. In an experiment with multiple post-treatment periods, however, post-treatment output continues to decline to baseline levels as predicted by momentum. Lastly, the magnitude of the effort stickiness implies implausibly large learning effects, addressed in more detail in Section 6.

Other potential explanations for effort stickiness, such as switching costs, neoclassical income effects, or income reference dependence, are outlined in the section on theoretical predictions and following the experimental results. In addition to mispredicting the observed comparative statics, these theories were also tested with one additional treatment involving the salience of income. I find this information salience had little-to-no effect on piece rate effects, further suggesting that effort momentum is the most parsimonious theory to explain the evidence at hand.

After addressing alternate theories, I employ an instrumental variable (IV) approach in which the previous period's piece rate and leisure options influence the previous period's effort.[17] The primary concern with this approach would be if a past period's piece rate could directly influence future effort (e.g. via a model of reciprocity). This intertemporal link is distinct from a model of effort momentum, in which the previous period's piece rate influences this period's optimal effort only through previous period's effort.

To summarize the main findings, I find that approximately 40-50% of effort changes persist for 5 minutes even after the incentives return to baseline levels. This increase continues to decline exponentially over multiple periods. Framed another way, after an interruption of effort, it takes about 15 minutes to return to 90% of pre-interruption effort levels. Structuring the findings using this momentum parameter also provides a way to transport findings to new populations or environments (Levitt

[16]Even if one allows for on-the-job learning to be combined with income effects in such a way to produce flat output, one would expect to see the efficiency increases result in increased leisure time. The evidence instead suggests that leisure time is also flat or even declining for the control group.

[17]Other methods were attempted to remove the known bias, but unfortunately those instruments (such as difference in lags) were identified as weak instruments by the Difference-in-Sargan and Difference-in-Hansen test. See Appendix Section 10.5.

and List, 2007; Falk and Heckman, 2009). For example, this estimate of 40-50% was replicated using a different “slider” task as discussed in Section 5.3.

To address whether this productivity loss can be prevented with knowledge, I treat some subjects with information about future piece rates and leisure opportunities. Analysis shows this advance information does not significantly impact productivity. This suggests the average subject follows a “naive” model of momentum as opposed to a more “sophisticated” model. These models are discussed in more detail in Section 2.

In addition to the broader intertemporal labor supply literature, this paper builds on an extensive literature that uses laboratory experiments to investigate labor economic theories (for a review see Charness and Kuhn, 2011). While many papers in this literature have intertemporal implications (e.g. Rabin, 1993; Dickinson, 1999; Gneezy and List, 2006; Levitt and List, 2007; Buser and Peter, 2012; Kube et al., 2012; Milkman et al., 2013; Kessler and Norton, 2015), I believe this is the first experimental study to vary incentives over short time periods specifically to investigate intertemporal spillovers.[18]

As I find evidence of effort momentum over short time periods, this paper also suggests not to estimate individual fixed effects with short time panels. This is discussed in more depth within Section 4, but follows from earlier work on the asymptotic bias from fixed effects in serially autocorrelated models, proven in Nickell (1981). Although this has been noted when estimating effects of training programs (Card and Sullivan, 1988), this study presents new evidence that the bias may be present in more general labor settings.

This research also provides new interpretations of existing labor studies. While pursuing other research topics, a few recent studies have uncovered intertemporal evidence consistent with momentum. In Cardella and Depew (2015), experimental subjects stuff fewer envelopes after being quantity constrained in the first period (compared to control). By itself, however, this could be evidence of on-the-job learning or reduced reciprocity due to constrained output. Bradler et al. (2015) experimentally varies payment structures in one period and also finds some persistence in effort after those incentives have been removed. For example, those who face a tournament structure exert greater effort for both creative and uncreative tasks, which significantly persists in the following period. Yet this effect was strongest among tournament winners, making it theoretically unclear whether there was a “joy of winning” effect as in Kräkel (2008) or whether tournament winners, who worked hardest, simply had the largest spillover effects. Despite these confounds, this suggests that effort momentum might fill a gap

[18] Following Corgnet et al. (2014), it is also among the first to experimentally vary leisure opportunities within the laboratory, providing additional evidence on the effect of leisure on effort (Chapela (2007); Connolly (2008); Lozano (2011); Ward (2012)).

between theory and empirics that has previously gone unreported.[19] Additional contributions to the literature can be found in the Appendix Section 10.6.

Even though the time periods are short, understanding intertemporal labor supply has important implications for labor markets and public policy. While competitive firms may have devised optimal monitoring to reduce interruptions in the workplace, public sector organizations may benefit from additional awareness of the full costs of work disruptions. In addition, if the intertemporal substitution elasticity is large and positive, one might interpret the lower pay of "flexible" positions as resulting from compensating differentials (Goldin, 2014) or a "Rat Race" equilibrium[20] (Akerlof, 1976; Landers et al., 1996). These outcomes might invite labor market policies to increase total surplus.[21] On the other hand, if this elasticity is small or negative, then the documented wage-flexibility tradeoff may be driven by firms' production and cost functions.[22] In this case, labor restrictions on hours could reduce firm efficiency.

The remainder of the paper is organized as follows. Section 2 derives straightforward comparative statics to distinguish the theories suggested above. Section 3 outlines the experiment designs. Section 4 discusses the specifics of the estimation strategy. Section 5 presents the results. Section 6 addresses additional alternate theories and Section 7 concludes.

2 Predictions

In this section, I derive predicted changes in labor supply to inform the experimental designs. I discuss three model classes below: (i) (neoclassical) time separable utility, (ii) effort momentum, and (iii) reciprocity. Additional model discussion may be found in Section 6 and the Appendix. I find straightforward comparative statics that can then be tested by the experimental design presented in Section 3.

[19] This literature review is unlikely to be comprehensive as previous studies may have suffered from bias driven by individual fixed effects or may have simply omitted reporting intertemporal spillovers.

[20] By a rat race equilibrium, I mean one in which workers work inefficient hours or effort to signal hard to observe qualities (such as ability) to employers. This was first proposed in a theoretical framework by Akerlof (1976), and there has been evidence to suggest this occurs in law firms (Landers et al. (1996)). As Arulampalam et al. (2007) point out, this rat race equilibrium could contribute to gender pay gaps, especially toward the top.

[21] For example, Goldin (2014) calls for "alterations in the labor market, in particular changing how jobs are structured and remunerated to enhance temporal flexibility" to reduce gender inequality in labor markets. Generally, if firms have imperfect information about worker productivity, workers may be afraid to express a desire for flexibility even though such a change would increase total surplus for the worker and firm. For example, if an hour's potential productivity is correlated with leisure opportunities, a worker's desire for flexibility could signal a desire to only work low productivity hours.

[22] For example, this could result either from per employee fixed costs (requisite search and training, benefits, or capital) or from increasing returns to hours worked (increasing worker knowledge flows, being available for clients).

2.1 Time Separable Utility

To serve as a starting point for predictions, I present a time separable model in which an individual maximizes lifetime utility

$$U_0 = \sum_{t=0}^{T} \delta^t u(c_t, e_t, \gamma_t)$$

where $\delta < 1$ represents the discount factor, $u(\cdot)$ represents the one-period utility function, c_t represents consumption, e_t is effort, and γ_t is a taste shifter that alters preferences for working in particular time periods. In my setting, γ_t can incorporate the varying leisure opportunities available, such as cell phone access. I further assume that the utility function is differentiable and $u_c > 0$, $u_e < 0$ and strictly concave in c_t and e_t. The lifetime budget constraint is given by

$$\sum_{t=0}^{T} p_t c_t (1+r)^{-t} \leq \sum_{t=0}^{T} (w_t e_t + y_t)(1+r)^{-t}$$

where p_t represents prices at time t, w_t the piece rate at time t for each unit of effort e_t, and y_t represents non-labor income. While the interest rate r is assumed to be constant for simplicity, generalizing to r_t does not impact the signs of the comparative statics.

As shown in Fehr and Goette (2007), along the optimal path, this model can be equivalently represented as an individual optimizing a static one period utility function that is linear in income. This can be written as:[23]

$$v(e_t, \gamma_t) = \lambda w_t e_t - g(e_t, \gamma_t)$$

where $g(e_t, \gamma_t)$ is strictly convex in e_t and captures the discounted disutility of effort. λ captures the marginal utility of life-time wealth. In this formulation, $\lambda w_t e_t$ represents the discounted utility from total income earned in period t.

Thus, as w_t increases, the optimal e_t^* will also increase. The effort exerted today is only influenced by past piece rates through the marginal utility of life-time wealth λ. In the literature on measuring temporary wage or piece rate shocks, this λ is assumed constant as the total impact on lifetime wealth is very small, implying small changes in λ (Fehr and Goette, 2007). Therefore, with no income effects, a single period's piece rate would have no impact on effort in surrounding periods.

If one allows for income effects, additional income would increase the attractiveness of leisure given

[23]Note that this formulation omits the price path $\{p_t\}$ and δ as these are not the objects of study. If these elements change, the corresponding g function would also change, but it could still be written in a similar format.

the concavity of consumption. As a result, allowing for income effects would reduce effort in periods surrounding a piece rate increase.[24]

Lastly, if one allows for leisure technology γ_t to increase the disutility of effort (e.g. harder to work when the World Cup is on), then increasing leisure technology would decrease optimal effort e_t^* in that period. As with piece rates, in the absence of income effects there are no predicted spillovers on the surrounding periods. If the additional pleasure had substantial income effects, then the worker would work harder in all other periods (say before or after the World Cup game). This partial spillover would occur as the reduced lifetime income would increase the marginal utility of lifetime income, λ, for all other periods.

2.2 Effort Momentum

Effort momentum is intended to capture the possibility for past period's effort to directly influence the disutility of future periods. For example, working hard may engage a flow-like state in which future effort is less costly.[25] Similarly, if effort is interrupted for a period ($e_t = 0$), the worker may face greater disutility to start working again.[26] This is comparable to a model of habit preferences, but as habits might imply long term preference shifts over consumption, I focus on 'momentum' to express a shorter time scale over effort.

To capture these ideas, I present a model in which an individual encounters lifetime utility:

$$U_M = \sum_{t=1}^{T} \delta^{t-1} u(c_t, e_t, e_{t-1}, \gamma_t)$$

where $\delta < 1$ represents the discount factor, $u(\cdot)$ represents the one-period utility function, c_t represents consumption, e_t is contemporaneous effort, and γ_t is a taste shifter that alters preferences for effort in particular time periods. In my setting, γ_t can incorporate varying leisure opportunities available and will be referred to as leisure technology.

I further assume that the utility function is twice-differentiable in its arguments with $u_1 \geq 0$, $u_2 \leq 0$, and has a positive cross partial $u_{23} \geq 0$. With these assumptions, consumption is enjoyable, effort is unenjoyable, and past effort decreases the marginal disutility of effort. I also assume that leisure

[24]Under the current experimental design, this would have very similar predictions to a model in which the worker has a daily income target.

[25]This is consistent with an extensive psychology literature on "flow", cf. Nakamura and Csikszentmihalyi (2002); Schaffer (2013), in which workers enter a state where the disutility of work is reduced as subjects report losing a sense of self.

[26]It is worth noting that these intertemporal effects do not necessarily have to be positive – an illustrative model proposed by Fehr and Goette (2007) includes a cost function in which greater effort today increases the marginal disutility of effort in the next period, perhaps due to stress or physical exertion.

technology makes effort more costly in utility terms ($u_{24} \leq 0$, $u_{34} \leq 0$), but also has no positive effect on consumption ($u_{14} \leq 0$).[27] Lastly, that effort does not make consumption less enjoyable ($u_{12} \geq 0$, $u_{13} \geq 0$). The lifetime budget constraint is given by

$$\sum_{t=1}^{T} p_t c_t (1+r)^{-t} \quad \leq \quad \sum_{t=1}^{T} (w_t e_t + y_t)(1+r)^{-t}$$

where p_t represents prices at time t, w_t the piece rate at time t for each unit of effort e_t, y_t represents non-labor income, and r is the interest rate from one period to the next.[28] I also assume there is no change in lifetime marginal utility of wealth λ is constant, as the total impact on lifetime wealth is very small. This is in line with other field and laboratory experiments in the labor economics literature (Fehr and Goette, 2007; Camerer et al., 1997).

2.2.1 Sophisticated Momentum

Sophisticated Momentum is the model as described above, in which the individual correctly realizes that today's effort will influence tomorrow's marginal disutility of effort.

Proposition 2.1 *Under the above assumptions, effort is monotonic non-decreasing in past, present, and future piece rates. Alternatively, effort is monotonic non-increasing in past, present, and future leisure technology expansions.*

Proof Application of supermodularity theorems. See Appendix Section 10.3. ■

The intuition for these comparative statics is straightforward. If a worker is aware that effort now will decrease the cost of effort in the next period, then both periods' optimal efforts will move together due to the spillover. For example, if the worker faces a higher piece rate next period, then next period's effort will become marginally more valuable. As work in the present reduces the costs of working next period, the marginal benefit of working in the present has also increased. By a similar argument, if the worker faces greater leisure opportunities next period, the benefits of working this period have also decreased as the value of the spillover from effort this period is reduced.

[27] Otherwise, increased leisure technology could boost desire for consumption to the extent that the individual works more to increase lifetime wealth.

[28] While the interest rate r is assumed to be constant for notation simplicity, this assumption does not impact the sign of the comparative statics.

2.2.2 Naive Momentum

Although the individual experiences the effects of momentum, it may be possible that either the individual does not realize this momentum will occur in the future, or otherwise uses an exogenous reference for future effort.[29] I call this model Naive Momentum. In this model, at period t, the individual maximizes a discounted stream of future utility:

$$U_t \quad = \quad u(c_t,\, e_t,\, e_{t-1},\, \gamma_t) + \sum_{j=t+1}^{T} \delta^j v(c_j,\, e_j,\, \gamma_j)$$

and will formulate plans of this period and future period's effort. For this model, I return to the assumption that $u(\cdot)$ is strictly concave in c_t and e_t. Note that the $v(\cdot)$ function above does not have e_{t-1} in its arguments. However, once the individual actually arrives at time $t+1$, he correctly incorporates previous period's effort into his lifetime utility:

$$U_{t+1} \quad = \quad u(c_{t+1},\, e_{t+1},\, e_t,\, \gamma_{t+1}) + \sum_{j=t+2}^{T} \delta^j v(c_j,\, e_j,\, \gamma_j)$$

This will cause the individual to revise his plans he made in time period t. The individual also faces the same budget constraint as before:

$$\sum_{t=0}^{T} p_t c_t (1+r)^{-t} \quad \leq \quad \sum_{t=0}^{T} (w_t e_t + y_t)(1+r)^{-t}$$

Proposition 2.2 *Under the assumptions above, there is an equivalent period utility function*

$$u = \lambda w_t e_t - g(e_t, e_{t-1}, \gamma_t)$$

This form demonstrates that effort is increasing in past and present piece rates, but future piece rates have no impact. By the same token, effort is decreasing in past and present leisure technology, but future leisure technology has no impact.

Proof Proof of the $g(\cdot)$ function equivalence and its convexity are provided in Appendix 9.1, but builds on work by Browning et al. (1985) and Fehr and Goette (2007). A brief proof for the comparative statics is provided below.

[29] One possible justification for this is the literature on Projection Bias, see Loewenstein et al. (2003); Conlin et al. (2007); Simonsohn (2010). Under such projection bias, a tired individual may incorrectly project that they will always be tired – but if he started working harder he may be surprised to find he isn't as tired as expected.

Consider the effect of an increase in w_{t+j}. In the first period, the first order condition states:

$$g_e(e_1^*, e_0, \gamma_1) = \lambda w_1$$

e_0 cannot be influenced by any $w_{t'}$ by construction, as time period 0 is before any information is received. γ_1 are not choice variables, they are only exogenously given. Thus when I take the derivative with respect to w_{t+j} to get:

$$g_{ee}\frac{de_1^*}{dw_{t+j}} = 0$$

Which, as $g_{ee} > 0$ gives us the effect in the first period of 0. In time period t, to complete the induction proof I assume $\frac{de_{t-1}^*}{dw_{t+j}} = 0$ and look to prove the same is true for $\frac{de_t^*}{dw_{t+j}}$. This follows from taking the total differential of the first order condition:

$$\begin{aligned} g_{ee}\frac{de_t^*}{dw_{t+j}} + g_{e2}\frac{de_{t-1}}{dw_{t+j}} &= 0 \\ \Rightarrow \frac{de_t^*}{dw_{t+j}} &= 0 \end{aligned}$$

Thus, by induction, optimal effort prior to a piece rate increase is unchanged when holding λ constant. This follows from the assumption of naivety that the individual does not anticipate future momentum. However, once the individual reaches the period with higher piece rates, an increase in the piece rate still elicits greater effort:

$$\frac{de_t^*}{dw_t} = \frac{\lambda}{g_{ee}} > 0$$

This follows from the convexity of g w.r.t. e_t^*. The same sign can be seen by looking at the total derivative with respect to past piece rates, w_{t-1}:

$$\frac{de_t^*}{dw_{t-1}} = -\frac{g_{e2}}{g_{ee}}\frac{de_{t-1}}{dw_{t-1}} > 0$$

As $g_{ee} > 0$, $\frac{de_{t-1}}{dw_{t-1}} > 0$ and $g_{e2} < 0$ (as $u_{23} > 0$). The proofs for leisure technology are the same as above with opposite signs (as leisure technology makes effort more costly, rather than less). ■

Although the above proposition gives us the required comparative statics of interest for naive momentum, considerably more can be said with an additional restriction on the period utility function. Without assuming a specific functional form, one can show that that the optimal effort will follow a linear time recursive structure.

Proposition 2.3 *Assuming further that* $u(c_t, e_t, e_{t-1}, \gamma_t) = q(c_t, e_t - \rho \cdot e_{t-1}, \gamma_t)$ *with* $|\rho| < 1$, *then optimal effort will follow a time recursive structure*

$$e_t^* = \rho \cdot e_{t-1} + z(w_t, \gamma_t)$$

with $z(\cdot)$ *increasing in* w_t *and decreasing in* γ_t.

Proof By a similar proof as above, the FOC will be

$$\begin{aligned} -q_e(c_t^*, e_t^* - \rho e_{t-1}, \gamma_t) &= \lambda w_t \\ q_c(c_t^*, e_t^* - \rho e_{t-1}, \gamma_t) &= \lambda p_t \end{aligned}$$

As q is strictly concave over the first argument, this allows for inverse of q_c:

$$c_t^* = q_c^{-1}(\lambda p_t, e_t^* - \rho e_{t-1}, \gamma_t)$$

Which can be inserted into the first FOC to give:

$$-q_e(q_c^{-1}(\lambda p_t, e_t^* - \rho e_{t-1}, \gamma_t), e_t^* - \rho e_{t-1}, \gamma_t) = \lambda w_t$$

Thus, a new utility function $\lambda w_t e_t - h(e_t^* - \rho e_{t-1}, \gamma_t)$. The convexity of $h(\cdot)$ gives us an inverse function for h_1:

$$e_t^* = \rho e_{t-1} + h_1^{-1}(\lambda w_t, \gamma_t)$$

As this is a special case of the first proposition (if $\rho > 0$), optimal effort e_t^* will still be an increasing function of w_t and decreasing in γ_t. In addition, past effort positively influences current effort and future piece rates or leisure technology does not influence current effort.■

It bears worth repeating that my theoretical work relies considerably upon previous work by others, particularly Browning et al. (1985) and Fehr and Goette (2007). However, to my knowledge, the resulting models of effort momentum are new to labor economics. Fehr and Goette (2007) provide a model where last period's effort enters into this period's utility function, they do not provide a general formulation and instead rely on an illustrative model. This makes sense given that paper's focus, but what may not be obvious is that their illustrative model inherently implies a "naive" model of effort momentum.[30]

2.3 Reciprocity

Consider instead a model in which changes in w_t and γ_t induce a desire to reciprocate. As formulated, this is similar to the time separable utility, but with an additional component of utility based on the piece rates and leisure offered across all time periods:

$$U_R = \sum_{t=0}^{T} \delta^t u(c_t,\, e_t,\, \gamma_t) + \alpha\left(\vec{w},\, \vec{\gamma}\right) \cdot \left(\sum_{t=0}^{T} \delta^t e_t\right)$$

In which u has the same properties as outlined previously and with $\alpha(\cdot)$ strictly increasing in its arguments. In this model, increases in future or past piece rates can increase the marginal utility of effort through the 'altruism' or 'fairness' function α, which takes all past and future piece rates and leisure opportunities, $\vec{w} = (w_1, w_2, ..., w_T)$ and $\vec{\gamma} = (\gamma_1, \gamma_2, ..., \gamma_T)$. This model is conceptually similar to ones found in Rabin (1993); Fehr and Schmidt (2006). Extending the work of Browning et al. (1985), if the full series of piece rates and leisure technologies were known, this utility function can be reformulated as a series of period utility functions:

$$v(e_t) = [\lambda w_t + \alpha(\vec{w},\, \vec{\gamma})] \cdot e_t - g(e_t,\, \gamma_t)$$

In a simple two period model for illustrative purposes, the individual receives additional marginal utility based on w_1 and w_2. For simplicity, I assume that this additional utility is linear in piece rate and effort, $\alpha\left(\vec{w},\, \vec{\gamma}\right) = \alpha_1(w_1 + w_2) + \alpha_2(\gamma_1 + \gamma_2)$. Thus the individual is maximizing:

$$\begin{aligned} U_R &= v(e_1) + v(e_2) \\ v(e_1) &\equiv (\lambda w_1 + \alpha_1 w_1 + \alpha_1 w_2 + \alpha_2 \gamma_1 + \alpha_2 \gamma_2) e_1 - g(e_1,\, \gamma_1) \\ v(e_2) &\equiv (\lambda w_2 + \alpha_1 w_1 + \alpha_1 w_2 + \alpha_2 \gamma_1 + \alpha_2 \gamma_2) e_2 - g(e_2,\, \gamma_2) \end{aligned}$$

In this setting, increasing the piece rate can increase reciprocity, even in surrounding periods. Under this simple model, if $\alpha_2 > 0$ and $e_1,\, e_2$ is an interior solution, then $\frac{\partial e_2}{\partial \gamma_1} > 0$. Likewise, if $\alpha_1 > 0$ and $e_1,\, e_2$ is an interior solution, then $\frac{\partial e_2}{\partial w_1} > 0$. Similar intuitions apply for future piece rates or leisure

[30] Because their $v(e_t,\, e_{t-1}) = \lambda e_t w - g(e_t(1 + \alpha e_{t-1})$ model only features contemporary piece rates rather than future ones, it implies that workers do not know that this period's effort will influence future disutility of effort. If this link was known to workers, they would want to increase effort prior to a piece rate to take full advantage of it.

technologies when informed in advance. For proofs, please see Appendix Section 10.3.

2.4 Summary

Owing to space limitations, several theories have been moved to a discussion following the results. To summarize the most relevant theories, I present the following table that outlines how output at time t will respond to piece rates and leisure technologies at different times (past, present, and future):

Predictions Summary Table

		Output at time t in response to increase in:					
		Piece Rate at time:			**Leisure Tech** at time:		
Models		$t-1$	t	$t+1$	$t-1$	t	$t+1$
Time Separable	No Income Effects	0	+	0	0	-	0
	Income Effects	-	+/-	-	+	-/+	+
Momentum	Naive	+	+	0	-	-	0
	Sophisticated	+	+	+	-	-	-
Reciprocity		+	+	+	+	-	+
On-the-job Learning		+	+	+	-	-	-
Income References	Period Target	0	-	0	0	-	0
	Total Target	-	+/-	-	+	-	+
	Previous Period	+	-	0	-	-	0
Experiment Results*		+	+	0	- or 0	- or 0	0

(*see Section 5 for details)

Please note that not all reference models give precise predictions for the sign of comparative statics, as shown in Brandon et al. (2014). Specifically, the table above assumes that the reference or target is strong enough to influences the intertemporal effort allocation. For example, an income target of $1000 would not be possible to achieve in this laboratory setting regardless of piece rate, and as a result would also not influence the intertemporal results – the individual would appear as following a neoclassical time-separable utility. While the intertemporal evidence is not explained by an income reference model, that does not preclude the possibility that people still have income reference points that influence behavior in general. Instead, it suggests that the effort momentum in this setting may be more prominent than the income reference effects. An additional experimental treatment and adaptive models of reference dependence are discussed in Section 6.

Also please note that although On-the-job Learning and Sophisticated Momentum have the same predictions for the 6 comparative statics above, there are additional tests to distinguish these two hypotheses. For example, if the gains are primarily driven by learning, one would expect either (a) increasing quantity over time or (b) increasing leisure engagement over time. Neither of these are found to occur. There are also reasons to believe that the magnitudes involved make learning a very unlikely possibility, see Section 6 for more details. In practice, as I find evidence for Naive Momentum, these comparisons are not as crucial.

3 Experiment Design Overview

In order to test these comparative statics, I investigate how effort responds to changes in (i) past (ii) contemporaneous and (iii) future piece rates and leisure opportunities. These manipulations were chosen rather than a complete "interruption" in work (forcing the subject to do 0 tasks) for three reasons. First, if I were to mechanically stop a worker from doing any work, the worker may suffer a 'switching cost' to get reacquainted with the task, but this may be task-specific rather than an actual decision on behalf of the subject. Second, it's not clear what the 'ideal' form an interruption would take, as staring at a blank screen for 5 minutes may not be the same as a coworker asking a question.[31] Lastly, these three changes correspond closely to the theory in Section 2, summarized in the table above.

These hypotheses were tested over two similar experiments (differences outlined below). In both experiments, subjects complete incentivized real-effort tasks in a laboratory setting. The tasks involve

[31] To some extent, one might think of the phone access treatment as analogous to many marketplace "interruptions". As discussed in Section 6, this may be why it appears the phone had may have had a more persistent impact for those affected.

counting images and are performed on a computer. This is similar to previous labor economics experiments studying effort in the laboratory, especially Abeler et al. (2011). Subjects count particular images from a matrix of 98 images, as can be seen in Figure 1.[32] This task was selected as it requires little to no training, but is menial and requires effort.[33] In post experiment surveys, subjects often mention the task is boring (see Appendix Figure 1), in line with findings presented in Abeler et al. (2011). Thus the primary measure of effort is the number of problems solved correctly – consistent with the experimental labor literature (Charness and Kuhn, 2011; Fehr and Goette, 2007).[34]

In line with Corgnet et al. (2014); Eriksson et al. (2009); Charness et al. (2010) and to mirror many labor contexts outside of the laboratory, I introduce a baseline leisure activity. Specifically, the participants were allowed to watch YouTube.com videos at any time instead of performing counting tasks (see bottom of Figure 1). To help make YouTube videos a potentially worthwhile leisure activity, a pair of headphones was attached to every computer. However, as the video was located below the counting problem, it was difficult to engage in both simultaneously. In the appendix, I confirm that YouTube videos were indeed a time substitute for effort.

As discussed in Section 2, many models of effort allocation allow for changes in either piece rates or leisure options to impact effort. To test these models, I experimentally varied the piece rate and leisure opportunities in specific periods. Although the piece rate varied in some periods, every period contributed to final earnings. This was done to focus on intertemporal substitution as opposed to regret or risk aversion. Paying in every period also allowed me to distinguish between potential "daily" income targeting and "period" income targeting models. Final payment also included a flat \$10 participation fee so long as they followed laboratory guidelines (e.g. no food, no talking).[35] However no payments were made until the end of the entire session.[36]

To vary the leisure opportunities, some subjects were randomly allowed access to their cell phones. The laboratory employed for this study, Wharton Behavioral Lab, ordinarily has a strict no phone policy to improve study compliance and concentration. This policy was put in place because partic-

[32]Abeler et al. (2011) has subjects counting zeroes in a string of 100 numbers. This exact task was not feasible in a web browser with a "search" feature, which makes the task trivial as one can merely search for 0. As a result, I ask the worker to count either heart or drop icons (randomized at the subject level). Only one subject tried bypass the task by searching the "source code" (after being asked not to) and is dropped from analysis.

[33]Gill and Prowse (2012) employ a task with sliders that also has attractive properties (further outlined in Gill and Prowse (2011)) – this task was employed in a replication experiment with very similar results, see Section 5.3. However, focusing on the task similar to Abeler et al. (2011) also allows for a closer comparison to their results, including testing for possibility of reference dependence.

[34]Though output and effort may not be perfectly correlated, changes in the production function are unlikely to explain evidence provided, as discussed in a Section 6.

[35]It was made clear and reiterated that they did not need to solve any problems to guarantee their \$10 participation fee. In practice, every participant adequately followed the laboratory guidelines and received the \$10 participation fee.

[36]Paying at the end was both a practical necessity given length of the periods and also mirrors the design of Fehr and Goette (2007).

ipants have a tendency to want to text, browse the web, and play games on their cell phones during the lab session. Thus, phone access has the potential to represent an increase in the marginal utility of leisure (γ_t from Section 2).[37] This is conceptually similar to experiments conducted in Corgnet et al. (2014) which allowed some users to browse the internet to expand possible leisure activities participants face.[38]

Prior to being allowed to start each period, the subjects had to correctly answer questions about the upcoming period's piece rate and cell access. These procedures were implemented to ensure subjects fully understood the incentives they faced.[39] In addition, counters at the bottom kept track of current earnings (as in Abeler et al., 2011) as well as visual indications for whether phone use was permitted.[40] In post experiment surveys, 98% of subjects report that the payments and leisure opportunities available were clear.

Although the experiments followed the general design above, I outline differences in the table below and elaborate in the following sections:

[37]The subjects of the experiment were University of Pennsylvania undergraduates. The second experiment surveyed cellphone access – only 8 out of 422 subjects (2%) did not bring a cellphone to the laboratory. Even though phone quality may vary or some subjects may not have a phone, this will not impact estimate validity if randomization was adequately done. However, this research will only be able to answer whether access to phones already owned by the subjects influence effort rather than the effect of access to a particular phone. This was done in part because introducing a new cell phone would lead to significant learning, additional experimental cost, and may not represent the same expansion of leisure opportunities as if the individual owned the phone (e.g. no contacts, no texts, etc.)

[38]When given access, the students could also use the internet on their phones, so internet access could be seen in some way as a lower bound of the potential leisure opportunity faced by allowing phone use. Other leisure technology expansions were considered, but deemed too difficult to adequately monitor under the current lab setup. With cell phones, lab assistants were able to quickly verify whether cell phone users were allowed to use the cell phone at that time.

[39]In one pilot study, rather than quiz the subject on the piece rate, the website merely didn't allow them to continue until 30 seconds have passed. This allows me to investigate potential salience effects from repeating the piece rate but in the post experiment analysis did not seem to make a difference.

[40]In addition to the subject's earnings for the current period, either (i) the total earnings or (ii) previous period earnings are displayed on the screen at all times. This treatment serves as a supplementary test for income targeting and is explained in more detail in Section 6.

Design Summary Table

	Experiment 1	Experiment 2	Experiment 3
Task	Image Counting	Image Counting	Sliders
Location	Wharton Behavioral Lab	Wharton Behavioral Lab	Wharton Behavioral Lab
Subjects	155 UPenn Undergraduates	422 UPenn Undergraduates	184 UPenn Undergraduates
# of Treatment Periods*	6	3	4
Treatment Period Duration	5 minutes	5 minutes	5 minutes
Duration of Pre-Treatment	5 minutes	15 minutes	10 minutes
Baseline Piece Rate	\$0.05	\$0.05	\$0.03 per 30 sliders
Treatment Piece Rates	\$0.15 or \$0.30	\$0.03 or \$0.08 or \$0.15	\$0.01 or \$0.09 per 30 sliders
Baseline Leisure Access	Youtube.com	Youtube.com	Youtube.com
Treatment Leisure Access	Cellphone Access	Cellphone Access	-
Advance Information	Periods 1, 3, 5	Randomly in Period 1	Randomly in Period 1
Instructions followed by	30 second timer and Quiz	30 second timer and Quiz	30 second timer and Quiz
Counters at Bottom	Period Earnings and Total Earnings	Period Earnings and either Total or Last Period Earnings	Period Earnings and either Total or Last Period Earnings
On Screen Timer	No	Yes	Yes
Image Counted	Hearts	Hearts or Drops (random)	-
Randomization	Subject Level by Computer	Subject Level by Computer	Subject Level by Computer

*Note: Number of treatment periods is the number of all periods after the pretreatment, i.e. periods in which individuals could differ in some way. In experiment 1, every subject experienced precisely 2 of the 6 rounds had a piece rate or leisure technology that was not baseline. In experiment 2, at most 1 period had a piece rate or leisure technology that was not baseline.

3.1 Experiment 1 Design

At the beginning of the session, the subject was given a series of instructions and an example problem. This was followed by one 5 minute "Pre-treatment" period to become acquainted with the task. This Pre-treatment period had the same incentives for all subjects and serves as a proxy of worker ability, as will be discussed in Section 4. For each solved problem in that period, the subject is informed she will earn \$0.05. In order to discourage random guessing, there was also a penalty for wrong answers – entering the incorrect answer three times for a single problem resulted in a deduction of \$0.20, akin to Abeler et al. (2011).[41]

The participants then completed six additional periods, each 5 minutes long, with three different possible treatments:

- **Control** – Subjects receive \$0.05 per completed problem for that period.
- **High Piece Rate** – Subjects receive a higher piece rate for that period, either \$0.15 or \$0.30.
- **High Leisure Technology** – Subjects receive the ability to access their cellphones for one period but still received \$0.05 per completed problem.

The control and piece rate treatments were calibrated using a small pilot study to allow for movement in either direction, as suggested by Charness and Kuhn (2011).

Regarding the randomization, these six treatment periods are broken up into three intervals of two periods each. Each interval consisted of either two periods of Control treatment; a Control treatment and a High Piece Rate treatment; or a Control treatment and High Leisure Technology treatment. Within each interval, the order of the treatments was randomized in order to test for anticipation effects. Each subject eventually receives all three treatment intervals, potentially allowing for both between and within subject analysis. This randomization was executed at the individual level by a pseudo-random number generator seeded by computer time (down to the millisecond).

To test for adequate randomization, I investigate whether pre-treatment indicators (such as gender, self-reported SAT scores, and pretreatment performance) predict the period at which the subjects faced the High Piece Rate or High Leisure treatments. As reported in Table 2A, none of these factors individually or together are predictive of the period that they receive the treatments.[42] As a result, I conclude that the treatment randomization was adequately done given the observable characteristics.

3.2 Experiment 2 Design

The second experiment simplifies the first by assigning each worker only a single primary treatment, over four periods rather than seven. The first "Pre-treatment" period lasted 15 minutes, was the same for all subjects and is used to generate proxies for worker ability (see Section 4). The following "treatment" periods were all 5 minutes. The first of these featured a baseline piece rate, but could (randomly) inform the subject about the next period piece rate and leisure opportunity. In the following

[41] On average the participants entered about 0.67 problems per period incorrectly, about 10% of total problems correctly solved per period.

[42] For regressing "High Piece Rate" treatment period # on pre-treatment variables, the F stat corresponds to a p-value of 0.29. For regressing the "High Leisure" treatment period # on pre-treatment variables, the F stat corresponds to a p-value of 0.71. Thus, for both treatments I fail to reject the hypothesis that all coefficients are zero and that none of the observable pre-treatment variables is significantly correlated with the period in which treatments occurred.

period, the subject receives either the baseline, a piece rate treatment, or a high leisure treatment. In the final period, the subject is returned to baseline piece rate and no access to the cell phone. As in experiment 1, randomization was executed at the individual level by a pseudo-random number generator seeded by computer time (down to the millisecond).

This experiment also expands on the first one in a number of ways. First, an additional treatment arm was included for piece rates, in which the piece rate is decreased from $0.05 to $0.03. and another treatment arm randomizing "total" vs "previous period" earnings shown. Second, by randomizing the information available for all subjects, the design eliminates concerns about "odd-period" x treatment interaction effects present in the first experiment.[43] Third, by keeping each individual to a single treatment, there may be less concern that interactions between multiple treatments confound effects. This also allowed for a longer training period to further reduce concerns about on-the-job learning. Fourth, a timer was added in accordance with Abeler et al. (2011) to minimize concerns about time uncertainty driving results. Lastly, additional variables, including specific timing and phone usage, were collected and a timer was added to the post experiment survey to improve information quality.

Due to the simpler structure of the experiment design, this experiment only allows for a between-subject analysis for the given treatments.

3.3 Experiment 3 Design

In addition to replicating findings of the first experiment with Experiment 2, I also ran an additional experiment with a different task to serve as an additional replication. In this setting, subjects face 30 "sliders" on a screen, as in Gill and Prowse (2011). This screen can be seen in Appendix Figure 5. The subjects are asked to move the slider to exactly 50% of the slider's length, with a numerical setting next to the slider indicating the current position. As in Gill and Prowse (2011), the exact position and length of sliders was randomized to make the task more difficult.

As the sliders take far less time than the counting problems, the piece rate was also substantially reduced – subjects received a baseline of $0.01 per 10 sliders. The "high piece rate" treatment was $0.03 per 10 sliders (three times baseline), while the "low piece rate" treatment was $0.0033 per 10 sliders (one third of baseline). Subjects were rounded to the nearest cent in the case they were unable to finish before time ran out. In addition, due to experiment budget constraints and the relatively "noisy" effect of cellphones in previous experiments, there was no phone access treatment. This affords greater

[43] In the first design, being "surprised" can only happen on treatment periods 1, 3, and 5 and "advance knowledge" can only occur for periods 2, 4, and 6. Although period fixed effects are included in most specifications, if odd-periods were interacting with treatments in some other way besides knowledge (e.g. piece rate increases are more effective in the final period), then estimates from experiment 1 could be a combination of those odd-period interaction effects and the effect of advance knowledge.

power in detecting effects through the financial incentives, but does make it harder to distinguish some theories previously ruled out in experiment 2.

In order to better understand the underlying model of momentum, the pre-treatment period was reduced 5 minutes and an additional 5 minute period following treatment was added. This was done to investigate whether the momentum continued to follow an AR(1) process in an additional period. As the design and results are similar to the main findings of the paper, I have omitted some results from the main text for brevity but additional details can be found in the Tables and Appendices.

4 Empirical Specifications

In Section 5 the results of the experiments will be addressed, but first three important notes on the empirical specification to help put these results in context:

1. First, the potential presence of momentum – where the previous period's effort could directly influence this period's effort – makes this a poor setting for individual fixed effects. Estimating these individual fixed effects will lead to bias in the estimate of the momentum.[44] Nickell (1981) proves this, but the intuition is that shocks will be partially absorbed into the fixed effect estimate rather than the coefficient estimate for the previous period's effort. This is worse when there are fewer periods as there are fewer shocks to properly distinguish the coefficient estimates.

For example, assume effort follows an AR(1) process (similar to Proposition 2.3) and there is a time-constant individual fixed component

$$e_{i,t} \quad = \quad \rho \cdot e_{i,t-1} + f_i + \beta x_{i,t} + \nu_{i,t}$$

where $e_{i,t}$ is the number of problems solved by individual i at time t, ρ captures the degree of "momentum" from the previous period, f_i is the individual ability or motivation component, $x_{i,t}$ include other shifters such as piece rate or leisure technology and $\nu_{i,t}$ is an error term. Under this model, estimating individual fixed effects will introduce an asymptotic downward bias to ρ, approximately equal to $-\frac{1+\rho}{T-1}$. In my setting with $T = 3$, even if ρ was 0.5, asymptotic estimates would become indistinguishable from 0 as $N \to \infty$. This remains an issue even though the piece rate and leisure technology are randomized. To be clear, this is not an issue of error terms correlated within an individual which could bias the standard errors[45] but rather a bias in the coefficient estimates themselves.

[44] Simply omitting the momentum term will not solve this issue in general but rather can bias other coefficients.

[45] Throughout the paper all standard errors are clustered at the subject level to reduce the influence of error terms correlated within an individual.

However, this was a known issue when designing the experiment and a primary justification for the pre-treatment period. This pre-treatment period can then serve as a proxy for individual ability or motivation, taking the place of f_i. To minimize risk of overfitting the data, a non-parametric approach is employed – individuals are split into five quintiles based the number of problems solved in the pre-treatment period, then each quintile receives it's own binary indicator variable.[46] The pre-treatment period is therefore omitted from the dependent variable for all specifications. In line with Card and Sullivan (1988), I also present a random effects model with identical findings in the Appendix.

2. The second note is that, although many labor studies take logarithms of dependent and independent variables, my specifications are reported at the unit level of analysis. This is done for several reasons, first being that the theory of momentum in section 2.2 suggested a unit level of analysis of effort. Given the linearity of the task, one might think effort would be closely correlated with quantity, not log(quantity). Second, while not common, some individuals did opt to solve no problems in a given period, a common issue with log forms. Third, the unit level was my ex ante specification while designing the experiment and analysis, and interpretation of new p values would be problematic after analysis has already been completed.

On the other hand, a linear formulation with OLS may be considered problematic as effort shocks cannot be too negative if effort is bound at the lower end at 0. It's also hard to represent upward effort "caps" with a linear specification. That being said I employ a log(problems correct + 1) on log(piece rate) specification and find qualitatively very similar results. While these measurement issues are important, the hypotheses are primarily tested by the signs rather than the magnitudes.

3. Third, as explained in Section 3, experiment 1 had each subject being treated to all 3 possible treatments. This helped improve power of treatment effects given the smaller sample – but it runs the risk of multiple treatments interacting to confound estimates. For example, access to a cellphone following a high piece rate period could negate additional effort resulting from momentum or reciprocity. As a result, I also perform analysis on just the first treatment received (corresponding to the first 3 periods of treatment)[47] and find that it does influence the intertemporal results in some specifications. While the interaction of treatments may be interesting, this was not the primary goal of this research study. Given this and the above difficulties of within-individual analysis in this setting, the second experiment design was simplified so that each person received only one treatment. This also allowed

[46] Though in practice the results are virtually identical when using a linear and quadratic term for number of problems solved in pre-treatment.

[47] Restricting analysis to only the first treatment pair is equivalent to focusing on just periods 1 and 2; however as half of those subjects received treatment in period 2, the following period (for intertemporal analysis) would be period 3. Results change very little when limiting it to individuals who have only received baseline piece rate (0.05) and leisure (YouTube) in period 3.

for a longer "training" period to further ensure the results are not being driven by on-the-job learning.

5 Experiment Results

Within this section, experimental results are presented together, as they are overall very similar across the two experiments. I use these results, particularly the qualitative signs, to test predictions of different theories from Section 2. I begin with the contemporaneous (same period) effects and move on to intertemporal effects.

5.1 Contemporaneous Effects

The first question is whether the primary treatments impacted contemporaneous effort as predicted by most theories of intertemporal labor supply. Recall that the primary treatments (piece rate or leisure technology) were in place for only 1 period, so this is asking whether or not effort was influenced in that treated period. This is important because if there is no effect on effort in the treated period, it would be difficult to understand why they would affect earlier or later periods.[48]

Result 5.1 *In accordance with most intertemporal theories of effort allocation, an increase in the piece rate significantly raised effort in the effected period. Likewise, in some specifications, there was a significant decrease in effort when subjects are offered access to their cellphones. See Table 3 for details.*

In the first experiment, the average worker solves 0.20 to 0.45 more problems ($p < 0.01$) when faced with a 10 cent increase in the piece rate, as seen in Table 3. This treatment estimate corresponds roughly to an effort elasticity of 2% = (0.325/7.85) / (0.10/0.05). Thus, increasing the piece rate by 50% would increase average effort in this context by approximately 1%. This elasticity is small relative to previous findings in the literature, though still significant (Card 1991; Chetty et al. 2011; Fehr and Goette 2007). Compared to the existing literature, this low result may be best explained by an effort ceiling.[49] In other words, at a \$0.15 piece rate, subjects may have already been exerting close to their maximum potential effort. There is some evidence for this, as the \$0.15 and \$0.30 piece rates both elicited greater effort, did not significantly differ from one another. This in turn would push down the

[48]That is not to say that certain combinations of theories could not predict such a finding, e.g. if an subject had a period income target but also experienced reciprocity, the two effects might cancel out in the effected period but could influence outside periods. However, given the extensive literature on piece rates influencing effort in the given period, such a null finding would likely indicate the treatment or sample size is too small (Levitt and Neckermann (2014)).

[49]Though it may be difficult to compare as previous literature often focuses on hour or participation elasticity rather than effort.

average elasticity. To examine this possibility, experiment 2 features a \$0.08 piece rate (1.6x baseline) and a \$0.03 piece rate (0.6x baseline) instead of the \$0.30 piece rate (6x baseline). Alternatively, the low elasticity could be the result of multiple treatment interactions. As seen in Table 4, limiting the analysis to the first treatment increases the elasticity up to about 5%. Given these issues, I believe experiment 2 represents a better estimate for the contemporaneous elasticity.

In the second experiment, I find a larger effect of a higher piece rate on effort. As can be seen in Table 3, every 10 cents (200%) increase (decrease) in piece rate increases (decreases) the number of correct problems by 1.24 - 1.57 (17-22%). Thus, the estimated elasticity of effort with respect to contemporaneous piece rate is 9.3% for experiment 2, quite a bit higher than the 5% found in experiment 1. Although not my primary research question, the output was rather linear in the piece rate. That is, there did not seem to be a "kink" in the response of piece rate increases compared to piece rate decreases.[50] Experiment 3 provided qualitatively similar results but as the problems were quicker, the magnitude is very similar.

As discussed in Section 2, many intertemporal labor models also predict that increasing the marginal utility of leisure detracts from effort provision. One way to test this hypothesis is by increasing the leisure options available to the subject. To the extent that these leisurely options are complements with leisure time, one would expect an increase in leisure time and a corresponding decrease in total effort. In this experiment, the subjects had access to Youtube.com videos throughout the experiment, but during the "High Leisure Technology" treatment, were also given access to their cellphones. When faced with this phone access, experiment 1 subjects complete 0.37 to 0.43 fewer problems on average ($p < 0.05$), as seen in Table 3. This provides support for the hypothesis that leisure opportunities can reduce effort allocation.

However, once the sample is restricted to the first 3 periods to eliminate potential multiple-treatment interactions, this coefficient is no longer significant (though similar in magnitude), as seen in Table 4 specification 3. This suggests that cell phones were more effective at reducing effort later on in the session. This might have been driven by a treatment interaction effect, e.g. receiving cell phone access after a higher piece rate might have a stronger effect than receiving the cell phone access before the higher piece rate. Alternatively, cell phones might have been more tempting in general after more periods of work or this may simply result be the result of a smaller sample size reducing statistical power.

The simplified design of experiment 2 allows one to more cleanly identify the effect of cell phone

[50]This symmetric response may be the result of informing the subjects at the beginning of the experiment that piece rates may vary, or as discussed later, there seemed to be little evidence of reciprocity (including negative reciprocity) in response to piece rates.

usage and remove concerns about multiple-treatment interactions. In the second experiment, there was no significant decrease when cell phones were permitted. This matches the finding in the first experiment once restricted to the first three periods. When broken down by gender, cellphones may reduce effort in the contemporaneous period for males, as can be seen in Table 5.[51]

These contemporaneous estimates also serve to test the "period income" reference dependence model. In this model, the worker receives relatively greater disutility if she falls short of a particular income in a given period. For example, a subject might try to earn \$0.50 each period and then spent the rest of the time watching YouTube videos. Under this model one would usually[52] expect to see a reduction in effort when faced with a higher piece rate (as it has become easier to earn the target income for that period). Yet the findings indicate the opposite, with an increased piece rate inducing greater effort in the period it was enacted. This implies the contemporaneous evidence does not support a "period income" reference point.

5.2 Intertemporal Treatment Effects

In addition to a contemporaneous treatment effect, pre-and post-treatment effects are important to differentiate the theories outlined in Section 2. For example, if workers followed a neoclassical time separable utility function, then as total impact on income is small, one would not expect to see any reduction or increase in effort in the periods surrounding the high piece rate or high leisure treatments.[53] Instead, I find significant stickiness in effort:

Result 5.2 *In the period following an increase in the piece rate, effort was also significantly higher. This is consistent with models of Effort Momentum as well as Reciprocity. See Table 4 for details. However (randomized) advance knowledge of higher piecerates did not significantly influence effort. Of the models outlined in Section 2, these results are only consistent with a model of Naive Momentum. See Table 6 for details.*

In the second experiment, the intertemporal treatment effects are quite striking, presented in Table 4. An increase in the previous period's piece rate of \$0.10 (200%) significantly increases the effort in the following period by about 0.75 problems (10%). Thus, for experiment 2 the intertemporal elasticity

[51] However, these results must be taken with a grain of salt as the breakdown by gender was not specified in the pre-analysis plan, but was implemented based on annecdotal evidence of who was using the cell phones during the experiment.

[52] As discussed in Brandon et al. (2014), if the piece rate increase is large enough or if the target is too large, contemporaneous effort could still increase with piece rates akin to a neoclassical time separable utility model. But if the targets are not relevant enough to induce behavior differences, the model's predictive value is likewise reduced.

[53] Contrary to this prediction, the literature has found some evidence of effects in surrounding periods in the pursuit of other research (Cardella and Depew (2015); Bradler et al. (2015); Connolly (2008)).

is about half of the contemporaneous elasticity. By itself, this intertemporal effect could be due to reciprocity or momentum, as both predict higher effort following a higher piece rate.

Experiment 1 also has similar findings once restricted to the first three treatment periods, as can be seen in Table 4. This may be due to the fact that in the first experiment, every individual received all 3 treatment pairs. As discussed in Section 4, this suggests the presence of multiple treatment interactions that were not ex ante predicted. Therefore, to limit the analysis to the post-treatment effects of just the first treatment pair, I analyze only the first three treatment periods.[54] Upon doing so, estimates suggest that a 5 cent (100%) piece rate increase in the previous period increases effort by 0.5 - 0.66 correct problems (6 - 9%). By itself, this result could be indicative of either momentum or reciprocity, as shown in Section 2.

Also worth noting is that while cellphones do not effect effort on average, it does seem to reduce contemporaneous effort for men over both experiments (see Table 5). This effect also persists in experiment 2 and some specifications of experiment 1. Also worth noting is that while cellphone *access* does not significantly alter effort in future periods, that self-reported cellphone *usage* is correlated with decreased effort in future periods, even after controlling for worker ability with productivity proxies (see Appendix Table 5).[55] Therefore, the intertemporal evidence of leisure is broadly suggestive of momentum rather than reciprocity, as reciprocity would suggest a worker work harder after use or access to an increased leisure technology, not less hard.[56]

These findings are also not explained by a "total income" target model. In such a model, the worker receives relatively greater disutility for falling short of a particular income over multiple periods (in this case, the experimental session). If subjects in this experiment exhibited a total income reference point, subjects should reduce effort following a high piece rate period, as the subject was more likely to have hit their target in the preceding period. As shown in Table 4, the previous piece rate is instead positively correlated with effort in this period. An additional treatment to try and increase salience of total earnings also had no consistent impact (see Section 6 for details). Thus, I conclude there was no significant evidence of a total income target in this experiment.[57]

[54] Restricting analysis to only the first treatment pair is equivalent to focusing on the first 3 periods; however as half of those subjects received treatment in period 3, the following period would be period 4. Results change very little when limiting it to individuals who have only received baseline piece rate (0.05) and leisure (YouTube) in period 4.

[55] However, this finding has the potential for selection effects driving omitted variable bias, suggesting the coefficients should not be taken as causal estimates.

[56] As reciprocity seems more likely to trigger with non-monetary goods (Kube et al., 2012), one might expect cell phone access to be even more likely to generate reciprocity than increased piece rates. One possible caveat – if subjects engage in cellphone use but do not actually "enjoy" this ability to use cell phones, e.g. due to self-control problems, it may not necessarily engage in reciprocity. However, this does not seem to be the case as the subject has a number of other self-control methods for cell phones (turning phone off, pulling out battery, leaving at home) and otherwise might suffer a very similar self-control issue with YouTube.

[57] Although not detected in my setting, Abeler et al. (2011) have employed an elegant method to elicit loss aversion even in a laboratory setting by varying a random outside option.

Lastly, being informed of the upcoming piece rates one period in advance did not influence effort. In the results of experiment 1, presented in Table 6, there seems to be some borderline significant results when focusing on early panels, but not in the full panel. However, one weakness of experiment 1 was that knowledge of future piece rates was not randomized – in period 1, the subject learned of the piece rate of periods 1 and 2, in period 3, they learned of the piece rates of periods 3 and 4, etc. Thus, foreknowledge of the piece rate is confounded with period effects that may not be fully captured by period fixed effects.

In experiments 2 and 3, this confound was eliminated by randomized knowledge of the future piece rates. In these experiments, also presented in Table 6, knowledge of future piece rates had no significant effect on effort.[58] This null finding occurred despite having similar sized standard errors and a similar level of power to detect as the effect of past piece rates on effort. This evidence is suggestive of naive momentum rather than sophisticated momentum or reciprocity; both of these alternatives would predict effort increases upon learning about future piece rate increases.

Indeed, for both sophisticated momentum and reciprocity, one might expect the "pre-" piece rate effect to be larger than the "post-" piece rate effect. For reciprocity, work by Gneezy and List (2006) suggests that reciprocity decreases over time, thus the "post-" period having a larger effect is unlikely. For sophisticated momentum, extra effort in the "pre-" piece rate period would help take full advantage of the higher piece rates in the next period, whereas "post-" piece rate effects would be a result of previously expanded effort. Thus, the small and insignificant coefficient of future knowledge is strong evidence in favor of Naive Momentum.

5.3 Instrumental Variable Approach

If naive effort momentum best explains the data, Proposition 3 in Section 2 guides how one might estimate it – in particular using an AR(1) approach. As discussed in Section 4, assume the true model is of the following sort:

$$e_{i,t} = \rho \cdot e_{i,t-1} + f_i + \beta_1 w_{i,t} + \beta_2 \gamma_{i,t} + \nu_{i,t}$$

Where $e_{i,t}$ is the number of problems solved by individual i at time t, ρ captures the degree of "momentum" from the previous period, f_i is the individual ability or motivation component, $w_{i,t}$ is the piece rate at time period t and $\gamma_{i,t}$ is the leisure technology available at time t, and $\nu_{i,t}$ is an error term. This theory allows us to encapsulate the force of momentum in a single parameter, which

[58] One specification is significant in experiment 3, but as seen in Robustness Table 6C this seems to be capturing period variation (as knowledge of future piece rates can only occur in period 1).

is potentially broader in application and policy implications and allows for easier comparisons across tasks (Charness and Kuhn, 2011).

While one could design an OLS structure to estimate the above, as mentioned before, the presence of fixed effects may bias the parameter ρ. I can employ productivity proxies as in previous specifications, but there may be remaining omitted variable bias as the uncaptured component of f_i , which now resides in the error term, may be correlated with $e_{i,t-1}$.

From the design, natural instrumental variables for previous period's effort are available – specifically the previous period's piece rate and leisure technology. These variables are assigned randomly, but should influence the previous period's effort directly. To achieve asymptotic consistency, the instrumental variable w_{it-1} would need to satisfy the following:

$$\begin{aligned} Cov(w_{it-1}, e_{it-1}) &\neq 0 \quad (\text{"First stage"}) \\ Cov(w_{it-1}, \nu_{it}) &= 0 \quad (\text{"Exclusion Principle"}) \end{aligned}$$

While the "first stage" is strong as piece rates do impact contemporaneous effort (see Table 3), one might have reasonable doubts about the exclusion principle. In particular, suppose the true data generating process was a model of reciprocity, a process in which past piece rates directly influence current effort (rather than influencing effort through past effort). For example,

$$e_{it} = \rho e_{it-1} + \alpha_1 w_{it} + \alpha_2 w_{it-1} + \alpha_3 \gamma_{it} + \nu_{it}$$

If data from this data generating process was used to estimate an AR(1) model without w_{it-1} as a regressor, then $\alpha_2 w_{it-1}$ would remain in the error term. Since w_{it-1} will be correlated with e_{it-1}, this would result in omitted variable bias. In this case, it would overestimate the magnitude of ρ, as what is actually driven by reciprocity would be misinterpreted as momentum (w_{it-1} and e_{it-1} positively correlated).

Therefore, in order to believe the asymptotic consistency of an instrumental variable (IV) approach, one must be reasonably confident that the other models where previous piece rates enters directly (such as reciprocity or "total" income targeting) are not occuring. Although momentum most closely fits the comparative statics, additional discussion of alternate theories is provided below.

With this caveat in mind, I apply the instrumental variables (IV) approach using previous piece rate and phone access to predict previous period's effort. As presented in Table 7, the estimates find around 43-45% of the increased effort is retained in the following period, even once incentives revert to baseline.[59] I replicate this estimate of 43% using an alternate slider task in experiment 3 (see Table 7 and Section 5.3). Experiment 1 suffers from a weak instrument problem due to a smaller sample, but several specifications of experiment 1 are in line with this estimate of 45% (see Appendix Table 1).

6 Alternative Theories

While momentum seems to be the most parsimonious description of the contemporaneous and intertemporal results, careful consideration of other theories is warranted.

Reciprocity

To summarize the results above, the experiment design gives two tests to differentiate between momentum and reciprocity, both of which suggest momentum.

1. As discussed in Section 5.2, informing a subject of a future piece rate increase or leisure option did not immediately increase effort as predicted by a model of reciprocity. Instead, I find that individuals only increase effort once the higher piece rate is applied. See Table 6 for details.[60]

2. After cell phone access, subjects exert less effort, not more as predicted by reciprocity (see Section 2.3 for predictions). See Table 5 as well as Appendix Table 5 for results.

Another potential explanation is a lack of worker trust combined with reciprocity. If workers do not trust the promised piece rate increase, they may not reciprocate it initially, and instead wait until the piece rate is actually put in place. However, I believe two aspects make this explanation unlikely. First, 90% of subjects have previously completed 3 or more studies at the Wharton Behavioral Lab. As a dedicated experimental lab, Wharton Behavioral Lab has a reputation and incentives for upholding its promises to subjects. Secondly, if the worker did not trust promises of higher piece rates, it may be unclear why higher piece rates would incentivize them to work harder in the treated period, as there was no actual payment until the end of the experimental session.

[59]This is substantially higher than the estimate of 75% given by OLS without employing an IV strategy.

[60]In one specification in Table 6, future piecerates do seem to increase future effort, however, this appears to primarily be a period effect. Specifically in experiment 3, output declines over time (periods 1 and 4 are significantly different). Yet, future knowledge of piece rates only occurs in the first treatment period, and thus the positive effect in specification 2 is most likely a result of omitted variable bias. This is rectified by the period fixed effects in specification 3 but both specifications are reported for transparency.

On-the-job Learning or Training

One possible alternative explanation for the experimental findings is that workers who experienced additional problems were able to increase their productivity in post-treatment periods ("on-the-job learning"). If there is significant on-the-job learning, then increased effort in an early period could result in additional problems solved in later periods. If true, this could account for stickiness detected.

However on-the-job learning seems an unlikely explanation as there was no indication of increased productivity over time as one would predict – see figures 2A and 2B. In addition, one might expect the number of incorrect problems to fall over time with learning, but this does not happen. Also, the task itself (counting 100 images dozens of times) has a limited scope for learning – indeed, Figure 3 shows a rapid convergence of problems solved per minute even within the Pre-treatment period.

Furthermore, even if on-the-job learning were occurring, it seems unlikely to explain the post-treatment effects. In experiment 2, the increase in problems solved during the high piecerate treatment is small relative to the total number of problems solved by that point, approximately $4\% = \frac{1.5\,\text{problems}}{35\,\text{problems}}$. These additional 1.5 problems are then followed by a 10% increase in productivity in the next (baseline) period.

If these additional 1.5 problems helped increase productivity via an on-the-job learning model, one would expect *all* of the problems in period 2 to also increase future productivity. Specifically, even control individuals solved approximately 7 problems in a given 5 minute period. Thus, under a linear model, one would expect the control group productivity to also increase by roughly 45% in the following period $\left(7 \text{ problems} \cdot \frac{10\%}{1.5 \text{ problems}} \text{ increase in productivity}\right)$. This is inconsistent with the data, which demonstrates the control group quantity flat or even slightly declining. In addition, if this on-the-job learning was known to workers, then a future increase in piece rate would motivate additional effort in the preceding period so as to increase productivity. This was also not found in the data.

Income Targeting Models

To recap why a period income targeting model does not fit the data, one would expect an increased piece rate to decrease contemporaneous effort (if the period income target is a significant component of utility). In addition, without adaptive references, a period income target model would predict no intertemporal spillovers. For more details, see the discussion in the contemporaneous effects section above.

To address why a "total" income targeting model does not fit the data, note that an increase in piece rate should reduce effort in surrounding periods. A higher piece rate makes it easier to hit a fixed "total" income target. Thus, to the extent that income targets induce effort,[61] the worker would exert less effort in the lower piece rate periods compared to control.[62] Instead, the data displays an increase in effort. For more details, see the discussion in the intertemporal results section above.

However, another possibility is period-level income reference dependence with adaptive references. In other words, by earning more in the previous period, the individual increases the income target for the following period.[63] This model may be hard to distinguish empirically from momentum, but there are two related tests that suggest adaptive income references are not driving the results.

First, if references are an important component of the utility function, this model would predict subjects decrease effort when facing a higher piece rate. This would occur as it now takes less effort to hit the income reference of the previous period. Subjects would also increase effort when faced with a lower piece rate, as it requires more effort to match the income reference of the previous period. In the data, subjects increase effort when faced with higher piece rates and decrease effort when faced with lower piece rates. Furthermore under a model of loss aversion with last period's income, one would expect an asymmetry in the decrease or increase of the piece rate, but it is instead rather linear in the response.

Second, every subject had a counter to keep track of earnings from that period (see Figure 1). This was implemented to reduce subject uncertainty about earnings. In addition, experiment 2 subjects either had a "previous period earnings" or a "total earnings" counter located below the period earnings. This was randomly assigned at an individual level to potentially increase the salience of period- or total-income targeting. Specifically if an individual had been given the "previous period earnings" counter treatment and were driven by a period-level effort reference model, the post-treatment effects should have been stronger as they have more precise information about the effort and earnings exerted in the previous period. As can be seen in Table 8,[64] this information did not meaningfully change the impact of an increased piece rate – increasing (decreasing) the piece rate still results in more (less) effort regardless of the salience of the target.[65] Though it seems to have influenced phone use slightly

[61] If the target is too low or too high (e.g. \$0.10 or \$1000 for a laboratory study), then the subject will demonstrate behavior consistent with a neoclassical time separable model as the kink in utility will not be relevant to effort decisions.

[62] A neoclassical time separable model with income effects has similar predictions.

[63] Although not discussed extensively in this paper, a theory of adaptive income references presented in Brandon et al. (2014); Kőszegi and Rabin (2006, 2007, 2009) would also generally have an effect if information about piece rates is presented in advance. In addition, as shown by Brandon et al. (2014); Huffman and Goette (2006) workers who receive a higher lump sum early in the day should reduce their optimal effort afterward. This does not fit with the findings above, as workers treated to a higher piece rate worked harder even after incentives return to baseline.

[64] Unfortunately, while every subject did face a randomized counter, a small programming typo prevented the capture of this variable for the first day of Experiment 2. As it is unclear which counter day 1 subjects faced, they are dropped from Table 8.

[65] It is possible that due to the small piece rates of Experiment 3, some income targeting may play a role. However, the final results are quite similar qualitatively and quantitatively to Experiment 2. As Experiment 2 features the same real effort task (counting) that Abeler et al. (2011) employ, one might expect subjects in Experiment 2 to have shown stronger income targeting behavior.

in experiment 2, this is hard to construe as evidence consistent with income reference dependence.

Switching Costs

One might wonder if mechanical "switching costs" could drive this effort stickiness. Although the workers are allowed to continue working in all periods, such costs might appear as effort momentum if it is more costly to resume working after a reduction in effort. To address this theory, I also increase the piece rate and continue to observe persistence in effort after the piece rate returns to baseline. In addition, the experimental tasks are able to be stopped and resumed easily as the time investment in each problem is small, especially for the slider task in Experiment 3. Thus to the extent that interruptions incur additional switching costs in field settings, my estimates of effort momentum could represent a lower bound of the total effort loss.[66]

Subject Confusion

Another possibility is worker confusion regarding the piece rates. There are two reasons why this is unlikely to be driving results. First, before every period, the worker is presented a new instructions page which clearly outlines the piece rate in that period. This instructions page cannot be skipped for at least 30 seconds and workers must successfully type in the piece rate before they can continue. If the worker has information about future incentives, they are also quizzed on the future piece rate. Second, as mentioned above, in all experiments there was a counter that showed how much the subject had earned that period; thus even if they failed to understand the instructions, subjects would quickly see how much each problem was earning them. These seem to be reflected in post experiment surveys – for example, of the 422 workers in the second experiment, only 4 individuals answered that the compensation was "somewhat unclear" or "unclear".

Experimenter Demand

Lastly, one might question whether the instructions and instruction quizzes might have prompted some subjects to work harder not for monetary benefits but instead as a result of experimenter demand. To the extent that experimenter demand represents reciprocity, please see the section above, but perhaps a simpler model would be "Subjects do what they think you want them to do". Yet when given information about future piece rates being higher or lower, this does not alter their effort in the current period. And when the piece rate returns back to the baseline treatment, they also do not return

[66] One might expect cell phones to incur greater switching costs as there is a change in user focus. This might explain why I find larger estimates of effort stickiness for cell phones in some specifications.

their effort to the level they previously exerted. Furthermore, as seen in Table 5, most subjects were not impacted much by access to their cell phone, even though such an irregular treatment might be clear indication that the experimenter "wanted" them to use their cell phone instead of work. To further reduce experimenter demand, subjects were also informed and quizzed in advance that no problems would be required in order to receive their $10 for participating in the experiment. The recent De Quidt et al. (2017) also suggests that with incentivized real effort tasks, experimenter demand effects seem to be quite mild. However, future studies could employ their methodology to ensure it does not effect intertemporal effort allocation as well.

7 Conclusion

I investigated the intertemporal elasticity of labor supply with a series of incentivized real effort experiments and find effort levels persist even once incentives return to baseline. After testing predictions to distinguish theories, I find strong evidence of effort momentum over short time scales and estimate a 5-minute momentum parameter of 0.45 across multiple experiments and tasks. This suggests it takes 15 minutes after an interruption to return to 90% of prior productivity levels, in line with observational evidence on interrupted work (Mark et al., 2005). Providing information a full period in advance of does not seem to significantly influence this effort allocation – further suggesting a "naive" sort of momentum.

To put these findings in context, other research suggests US knowledge workers are interrupted somewhere between 12 and 40 times a day depending on work environment. Given the ubiquity of interruptions and the large number of US knowledge workers, it is perhaps not surprising that the resulting momentum loss is quite high. If the average knowledge worker suffers 15 interruptions per work day, this will result in about 1 hour of productivity loss due to momentum alone.[67] This works out to 200 hours per year per full-time worker. If each knowledge worker earns an average of $21 per hour, then 56 million workers[68] would lose $235 billion per year from momentum loss alone.[69] One important caveat is that if the reduced productivity results in greater leisure, this figure would also not account for any welfare gains from this leisure – however, workers tend to report interruptions as a major

[67]Momentum loss might also explain why subjective reports of time wasted due to interruptions (often as high as 40% of total work time) tend to be higher than the observed time loss (roughly 20% of total work time).

[68]From 2014 Current Population Survey, number of management, professional, and related workers. Most common examples include software developers, financial managers, accountants, lawyers, school teachers, registered nurses, and chief executives. This number also corresponds with the 77 million workers that reported using a computer at work in 2003 by the Bureau of Labor Statistics.

[69]Though this is just a rough estimate for a number of reasons. One might expect that the individuals who earn higher than average wages are less prone to interruptions or momentum loss. Alternatively, perhaps wages have already been lowered to account for interruption loss, underestimating the true value of productivity loss.

source of stress in the workplace, making welfare gains unlikely (Mark et al., 2008).[70] While there are serious concerns about generalizing evidence from students,[71] this back of the envelope calculation demonstrates the potential value of further research.

One weakness of this study is remaining uncertainty regarding the source of the momentum effects. For example, if effort momentum is a result of quickly decaying task-specific human capital (i.e. a "train of thought"), then switching tasks could be equally harmful as being interrupted. This would also be consistent with evidence that multitasking is less productive than sequential work, as found in Buser and Peter (2012). Alternatively, it may be that momentum has a physiological component, perhaps due to adrenaline or other neurobiological processes. Lastly, the momentum could also be related to effort reference dependence (as opposed to income reference dependence), though many reference dependence models would predict that receiving information in advance should change the effort allocation (Brandon et al., 2014; Kőszegi and Rabin, 2006, 2007, 2009). Distinguishing these theories could help provide additional suggestions on how to minimize momentum loss after an interruption, e.g. a cellphone reminding one to return to work after a phone call or doing 5 jumping jacks immediately after an interruption.

There's also some uncertainty to the extent to which workers are aware of these momentum effects. Although they do not seem to employ information to take advantage of momentum, there is still a chance workers are aware of it conceptually. As has occurred with some past studies (Price and Wolfers, 2010; Pope et al., 2013), increased awareness of the momentum effect may overturn or undo some of the effect. For example, if a worker knows they tend to work harder after working hard, they may slack off early and expect the work to "finish itself". Or as Mark et al. (2008) find, workers may work harder following an interruption to "catch up", though I find no evidence of this. One possibility to investigate the degree of self-awareness is to use costly commitment with a self-selected cut-off, akin to Kaur et al. (2010).

Another open question is whether these momentum effects would persist over longer time periods. Experiment 3 features multiple periods following the treatment suggests that effort continues to decay exponentially, suggesting that the effects of momentum would disappear within 20 minutes or so. That being said, even if short-lived, measuring momentum may have direct applications to the economics

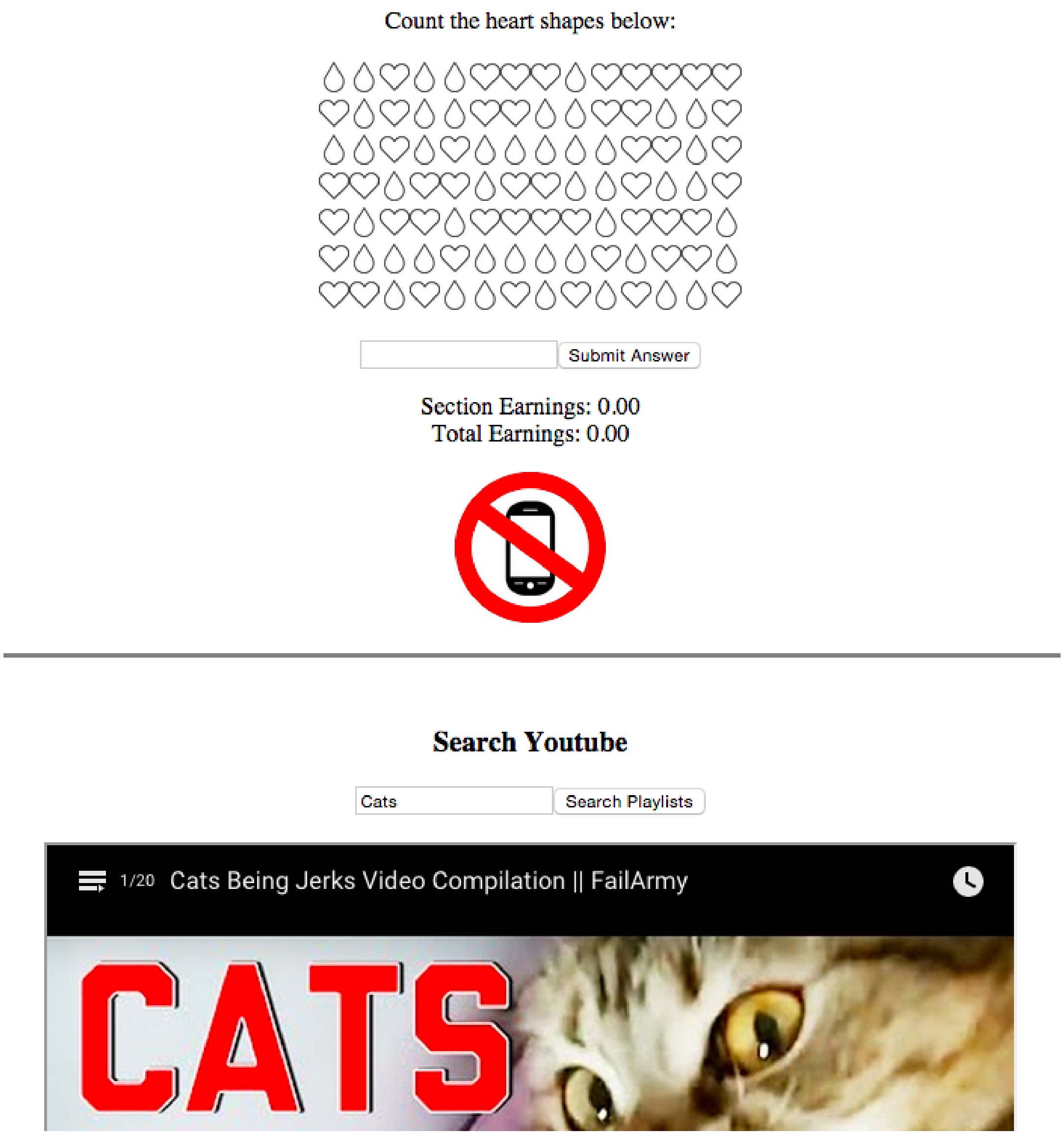
[70] This may not be surprising given that interruptions are unplanned, making such 'breaks' in effort unlikely to be ex ante optimal from the interrupted worker's perspective. Thus, even if the productivity loss following interruption increases utility through leisure, it may be used as a substitute for a more relaxing (planned) break. Thus, the utility from such leisure could be a net welfare loss as it disrupts the optimal on-the-job leisure schedule.

[71] For example, students may lack the workplace experience that could help reduce momentum loss. On the other hand, one might also expect college students to be better than average at avoiding momentum loss as they have passed college admissions. In addition, while the environment studied is similar to what many knowledge workers face, the tasks employed differ, raising additional concerns about external validity.

of task juggling and managing worker interruptions. As outlined in Section 5.3, approximately 45% of effort momentum persists after 5 minutes. Using this estimate as a starting point, 45% of productivity is lost in the first 5 minutes after an interruption, an additional 20% in the second 5 minutes, 9% in the third 5 minutes, 4% in the next 5 minutes and so on. In total, if an interruption causes me to lose 5 minutes of productivity, I lose an additional 4 minutes of productivity due to effort momentum loss spread out over the next 30 minutes. Put in other terms, total productivity loss from effort momentum is 80% of the original interruption loss. Given estimates of the number of interruptions knowledge workers face, that suggests up to an hour of productivity per work day could be lost due to effort momentum alone.

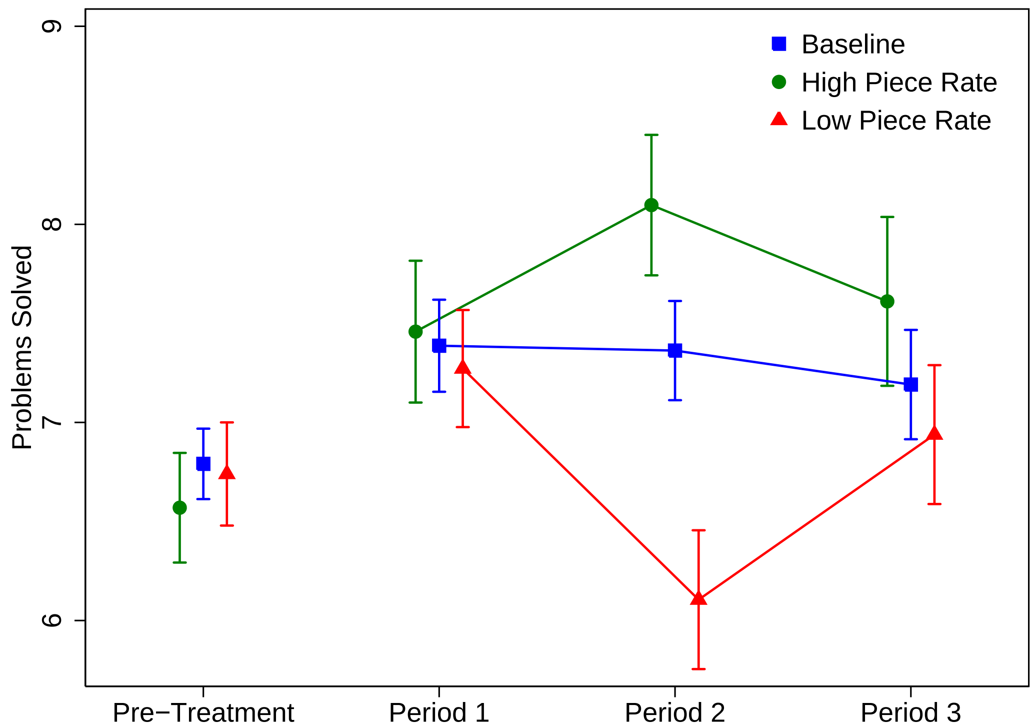
8 Figures

Figure 1: Example of Counting Problem Task – Experiment 1 and 2



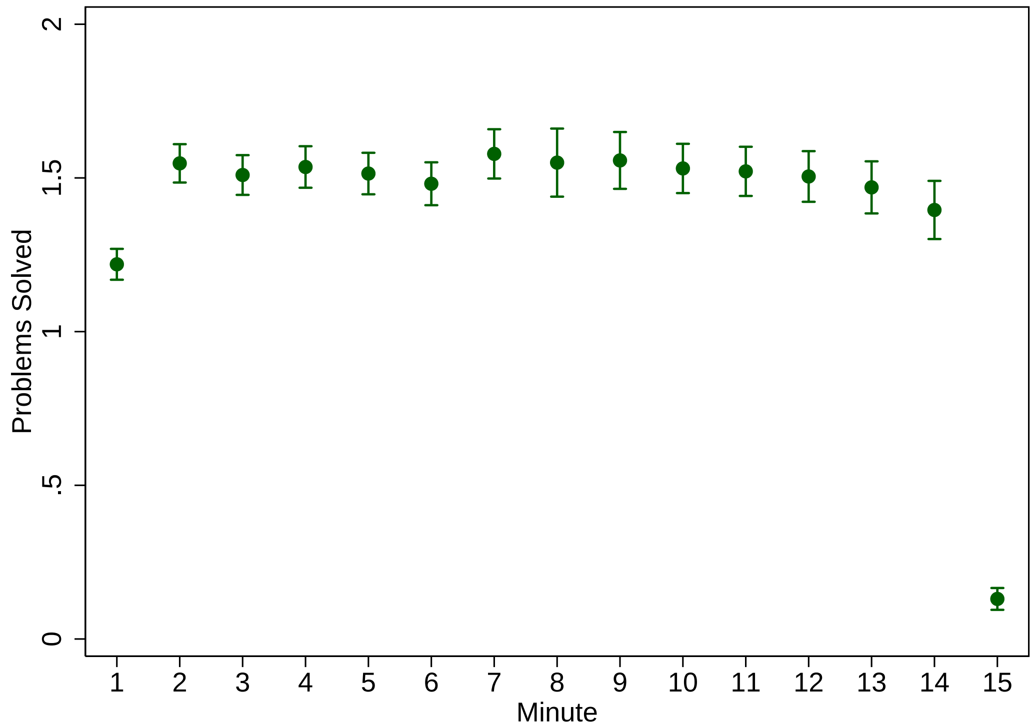
Notes: Figure demonstrates a typical counting task screen faced by subject. Whether subject was asked to count hearts or drops was randomized in experiment 2.

Figure 2: Solved Problems by Period – Experiment 2



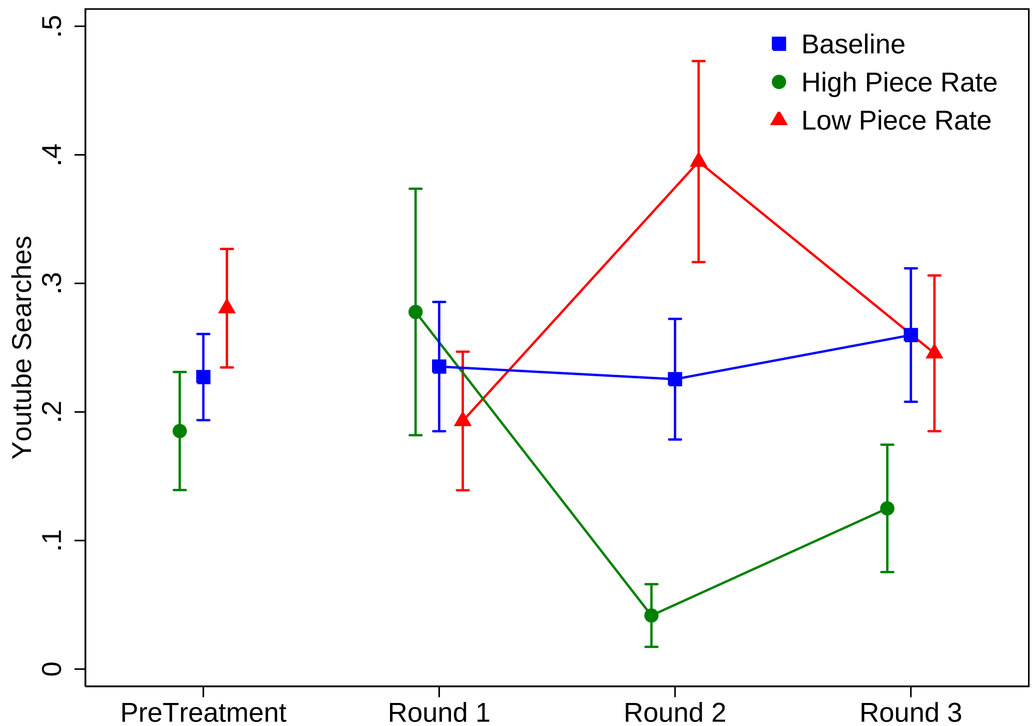
Notes: Bars represent standard errors, treating each period and treatment as a completely separate treatment group with no control variables. However, as can be seen by the regression tables, controlling for individual heterogeneity and including piece rate as a continuum of treatment rather than separate groups provides a more accurate representation. Vertical axis represents the number of problems solved by workers in a 5 minute period. Primary treatments were only in effect for period 2 (see experimental design in Section 3). The Pre-Treatment period is a training period to familiarize workers with the task. Pre-Treatment lasted 3 times the duration of the other periods and thus the problems solved in Pre-Treatment is divided by 3 to provide accurate comparison.

Figure 3: Solved Problems within Pre-Treatment Period – Experiment 2



Notes: Bars represent standard errors. This figure demonstrates the number of problems solved by minute of the pre-treatment period. As these counting problems take about 45 seconds, the final minute was lower due a mechanical effect (of being unable to finish a problem in time) and additional uncertainty of whether one is able to finish the problem in time (perhaps due to the timer reading "0 minutes left").

Figure 4: YouTube Searches by Period – Experiment 2



Notes: Bars represent standard errors. Vertical axis represents the number of YouTube searches performed by workers (baseline leisure option). Treatments were only in effect for period 2 (see experimental design in section 3). The Pre-Treatment period is a training period to familiarize workers with the task. Pre-Treatment lasted 3 times the duration of the other periods and thus the problems solved in Pre-Treatment is divided by 3 to provide accurate comparison.

9 Tables

Table 1. Summary Statistics

	Experiment 1				Experiment 2				Experiment 3			
	Mean	Standard dev	Min	Max	Mean	Standard dev	Min	Max	Mean	Standard dev	Min	Max
Individual Level Variables												
Female	0.72	0.45	0	1	0.71	0.45	0	1	0.48	0.50	0	1
Problems Solved in PreTreatment	6.59	2.75	0	14	20.2	7.60	0	67	103.1	36.3	2	197
Age	21.3	5.27	18	61	20.4	1.85	18	38	20.4	2.21	18	42
SAT Math Score	731	78	165	800	731	63	400	800	737.1	59.1	500	800
Total Payment	3.87	1.75	0	10.75	2.16	0.94	0	5.4	0.35	0.18	0.003	1.01
Computer Skill Test	2.01	0.08	2	3	2.01	0.10	2	3	2.01	0.08	2	3
Number of Previous Lab Studies	33.4	26.7	0	129	23.7	25.0	0	292	36.6	30.2	1	157
Period Level Variables												
Problems Solved	7.85	3.7	0	21	7.23	3.49	0	17	52.6	32.2	0	116
Problems Incorrect	0.06	0.29	0	4	0.08	0.30	0	3	N/A	N/A	N/A	N/A
Youtube Searches	0.16	0.59	0	6	0.24	0.67	0	5	0.58	1.24	0	10
Period Payment	0.60	0.65	−0.65	5.4	0.40	0.30	−0.1	2.1	0.06	0.05	0	0.34
High Piece Rate Indicator	0.17	0.37	0	1	0.08	0.28	0	1	0.08	0.27	0	1
Phone Access Indicator	0.17	0.37	0	1	0.09	0.29	0	1	N/A	N/A	N/A	N/A
Low Piece Rate Indicator	n.a.	n.a.	n.a.	n.a.	0.08	0.27	0	1	0.07	0.25	0	1
Number of Individuals	155				422				184			
Number of Treatment Periods	930				1266				736			

Notes: Computer Skill Test was a demographic variable collected by the Wharton Behavioral Lab prior to the experiment, however one with almost no variation. It is included as it was part of the ex ante specification, but excluding it does not change any results. SAT Math score is missing for individuals who either took the ACT, reported impossible numbers, or otherwise did not wish to share that information with researchers. Indicators for treatments are presented under the period level variables – as experiment 1 had no "low piece rate" treatment, it has no such indicator. Note that period payment could be negative as a result of incorrectly answering multiple times, but total payment is bound below by zero.

Table 2A. Randomization Check – Experiment 1

Dependent Variable	Period # for Piece Rate Treatment		Period # for Phone Treatment	
Female	−0.09	0.19	0.06	0.03
	(0.29)	(0.32)	(0.30)	(0.32)
SAT Math Score ('00s of points)		−0.002		−0.001
		(0.002)		(0.26)
PreTreatment Problems Solved		−0.047		0.011
		(0.058)		(0.048)
F-test	0.10	1.24	0.05	0.15
p value	0.75	0.30	0.83	0.93
Dependent Variable Mean	3.34	3.41	3.48	3.54
Number of Observations	930	738	930	738
Number of Individuals	155	123	155	123
Adj-R^2	0.001	0.018	0.001	0.005

Notes: Standard Errors (clustered at individual level) presented in parentheses above. As every subject in experiment 1 receives all treatments at some point, the dependent variable is the period in which they received the treatment in question. If randomization was done properly, the pre-treatment variables should not predict the period they received the treatment. Indeed, the F-stats are all large enough that I fail to reject the hypothesis that all coefficients are zero under $\alpha = 0.05$. Thus, I conclude the randomization was adequately done. SAT Math score is missing for 32 individuals who either took the ACT or otherwise did not wish to share that information with researchers.

Table 2B. Randomization Check – Experiment 2

Variable	Baseline	Piece Rate Decrease	Piece Rate Increase	Phone Access	
Female	0.68	0.71	0.74	0.71	$p < 0.76$
	(0.47)	(0.46)	(0.44)	(0.45)	(F-test = 0.39)
Age	20.26	20.13	20.73	20.39	$p < 0.11$
	(1.75)	(1.61)	(2.44)	(1.48)	(F-test = 2.05)
# Previous Studies at Lab	25.18	24.1	26.23	23.64	$p < 0.22$
	(23.9)	(20.5)	(34.5)	(18.7)	(F-test = 0.88)
Computer Skill Test	2.01	2.01	2.02	2.00	$p < 0.56$
	(0.01)	(0.09)	(0.14)	(no variation)	(F-test = 0.56)
Problems Solved in PreTreatment	20.57	20.23	19.54	20.23	$p < 0.80$
	(7.76)	(8.2)	(7.02)	(7.22)	(F-test = 0.33)
Number of Subjects Treated	103	114	104	101	

Notes: As every subject in experiment 2 receives (at most) one primary treatment, the subjects are split according to primary treatment. Means and standard deviations (in parentheses) are presented by primary treatment. If randomization was done properly, the pre-treatment variables should not differ significantly according to which treatment was received. Indeed, for all rows the F-stat corresponds to a p greater than 0.05 (fail to reject the hypothesis that all coefficients are less than zero under $\alpha = 0.05$).

Table 2C. Randomization Check – Experiment 3

Variable	Baseline	Piece Rate Decrease	Piece Rate Increase	
Female	0.45	0.55	0.45	$p < 0.46$
	(0.47)	(0.46)	(0.44)	(F-test = 0.78)
Age	20.42	20.63	20.24	$p < 0.64$
				(F-test = 0.45)
# Previous Studies at Lab	35	36.1	39.4	$p < 0.72$
				(F-test = 0.33)
Computer Skill Test	2.01	2	2	$p < 0.32$
	(0.01)	(0.09)	(0.14)	(F-test = 1)
Problems Solved in PreTreatment	102.6	98.4	109.4	$p < 0.22$
				(F-test = 1.52)
Number of Subjects Treated	73	60	51	

Notes: As every subject in experiment 3 receives (at most) one primary treatment, the subjects are split according to primary treatment. Means and standard deviations (in parentheses) are presented by primary treatment. If randomization was done properly, the pre-treatment variables should not differ significantly according to which treatment was received. Indeed, for all rows the F-stat corresponds to a p greater than 0.05 (fail to reject the hypothesis that all coefficients are less than zero under $\alpha = 0.05$).

Table 3. Contemporaneous Piece Rate and Phone Access: Impact on Effort

$$Problems_{i,t} = \alpha \cdot Piece\,Rate_{i,t} + \beta \cdot Phone\,Access_{i,t} + \gamma X_i + \epsilon_{i,t}$$

Dependent Variable:	Experiment 1			Experiment 2			Experiment 3		
Problems Solved	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
Piece Rate	4.34***	4.62***	2.07*	12.5***	14.9***	16.7***	41.45**	62.34***	63.67***
	(1.17)	(1.15)	(1.09)	(3.39)	(2.67)	(2.98)	(16.84)	(14.21)	(16.26)
Phone Access	−0.38**	−0.37**	−0.46**	−0.05	−0.10	0.17	n/a	n/a	n/a
	(0.19)	(0.19)	(0.19)	(0.33)	(0.25)	(0.32)			
PreTreatment Quintiles		Yes	Yes		Yes	Yes		Yes	Yes
Period Fixed Effects			Yes			Yes			Yes
Session Fixed Effects			Yes			Yes			Yes
Individual Controls			Yes			Yes			Yes
Dependent Variable Mean	7.85	7.85	7.85	7.23	7.23	7.23	63	63	63
Number of Observations	930	930	930	1266	1266	1260	736	736	732
Number of Individuals	155	155	155	422	422	420	184	184	183
Adj-R^2	0.01	0.23	0.32	0.01	0.41	0.45	0.01	0.39	0.47

Notes: The dependent variable is the number of problems solved correctly in a single period. All specifications report results from OLS regressions and also include a constant term. PreTreatment Quintiles represent five binary variables to non-parametrically control for the number of problems subject solved in the pre-treatment training period. Individual Controls include sex, age, ethnicity bins, number of sessions done, and WBL computer diagnostic scores, except for 3 individuals who could not be matched to controls. Piece Rate is in dollars per problem for Experiment 1 and 2, and in cents per problem for Experiment 3. Standard errors are given in parentheses and clustered at the subject (individual) level. $* = p < 0.1$, $** = p < 0.05$, $*** = p < 0.01$.

Table 4. Previous Piece Rate and Phone Access: Impact on Effort

$$Problems_{i,t} \quad = \quad \alpha_1 \cdot Piece\,Rate_{i,t} + \alpha_2 \cdot Piece\,Rate_{i,t-1} + \gamma X_i + \epsilon_{i,t}$$

Dependent Variable:	Experiment 1			Experiment 2			Experiment 3		
Problems Solved	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
Piece Rate	5.00***	2.19*	5.43***	12.66***	15.09***	17.09***	41.87***	64.08***	64.90***
	(1.29)	(1.14)	(1.87)	(3.51)	(2.74)	(3.11)	(17.52)	(14.80)	(16.87)
Previous Period's Piece Rate	3.42*	0.45	5.91**	4.09	6.51**	7.87**	10.68	32.89**	27.22
	(1.74)	(1.36)	(2.78)	(4.03)	(3.11)	(3.40)	(20.16)	(16.47)	(18.01)
Phone Access	−0.26	−0.43**	−0.38	−0.03	−0.08	0.18	n.a.	n.a.	n.a.
	(0.21)	(0.20)	(0.30)	(0.36)	(0.28)	(0.33)			
Previous Period Phone Access	0.24	0.07	0.21	−0.03	−0.08	0.07	n.a.	n.a.	n.a.
	(0.26)	(0.23)	(0.34)	(0.41)	(0.33)	(0.36)			
PreTreatment Quintiles		Yes	Yes		Yes	Yes		Yes	Yes
Period Fixed Effects		Yes	Yes			Yes			Yes
Session Fixed Effects		Yes	Yes			Yes			Yes
Individual Controls		Yes	Yes			Yes			Yes
Periods 1 to 3 Only			Yes						
Dependent Variable Mean	7.85	7.85	7.79	7.23	7.23	7.23	63	63	63
Number of Observations	930	930	465	1266	1266	1260	736	736	736
Number of Individuals	155	155	155	422	422	420	184	184	184
Adj-R^2	0.01	0.32	0.39	0.01	0.41	0.45	0.01	0.39	0.47

Notes: The dependent variable is the number of problems solved correctly in a single period. All specifications report results from OLS regressions and also include a constant term. PreTreatment Quintiles represent five binary variables to non-parametrically control for the number of problems subject solved in the pre-treatment training period. "Periods 1 to 3" uses data of the first treatment period and following period to minimize treatment interactions. Individual Controls include sex, age, ethnicity bins, number of sessions done, and WBL computer diagnostic scores, except for 3 subjects who could not be matched to controls. Piece Rate is in dollars per problem for Experiment 1 and 2, and in cents per problem for Experiment 3. Standard errors are given in parentheses and clustered at the subject (individual) level.
$* = p < 0.1$, $** = p < 0.05$, $*** = p < 0.01$.

Table 5. Phone Access by Gender: Impact on Effort

$$Problems_{i,t} \;=\; \alpha_1 \cdot Phone_{it} \cdot Female_i + \alpha_2 \cdot Phone_{it-1} \cdot Female_i + \beta_1 \cdot Phone_{it} \cdot Male_i + \beta_2 \cdot Phone_{it-1} \cdot Male_i + \gamma X_i + \epsilon_{i,t}$$

Dependent Variable:	Experiment 1			Experiment 2		
Problems Solved	(1)	(2)	(3)	(1)	(2)	(3)
Phone Access * Female	−0.26	−0.29	−0.02	0.51	0.36	0.45
	(0.22)	(0.22)	(0.32)	(0.36)	(0.29)	(0.35)
Previous Period Phone * Female	0.35	0.29	−0.08	0.48	0.34	0.48
	(0.25)	(0.24)	(0.38)	(0.44)	(0.36)	(0.40)
Phone Access * Male	−1.10***	−1.11***	−2.02***	−1.68**	−1.59***	−1.64***
	(0.39)	(0.41)	(0.72)	(0.82)	(0.51)	(0.53)
Previous Period Phone * Male	−0.69	−0.65	0.18	−1.61*	−1.52**	−1.51**
	(0.57)	(0.55)	(0.71)	(0.88)	(0.61)	(0.61)
Male	−1.02*	−0.10	0.24	−0.45	−0.48	−0.53*
	(0.58)	(0.56)	(0.54)	(0.36)	(0.27)	(0.28)
Pre-Treatment Quintiles		Yes	Yes		Yes	Yes
Period Fixed Effects		Yes	Yes			Yes
Session Fixed Effects		Yes	Yes			Yes
Individual Controls		Yes	Yes			Yes
Periods 1 to 3 Only			Yes			
Dependent Variable Mean	7.85	7.85	7.79	7.23	7.23	7.23
Number of Observations	930	930	465	1263	1263	1260
Number of Individuals	155	155	155	421	421	420
Adj-R^2	0.03	0.33	0.39	0.02	0.42	0.44

Notes: The dependent variable is the number of problems solved correctly in a single period. All specifications report results from OLS regressions and also include a constant term. PreTreatment Quintiles represent five binary variables to non-parametrically control for the number of problems subject solved in the pre-treatment training period. "Periods 1 to 3" uses data of the first treatment period and following period to minimize treatment interactions. Individual Controls include sex, age, ethnicity bins, number of sessions done, and WBL computer diagnostic scores, except for 2 subjects who could not be matched to controls. Piece Rate is in dollars per problem. Standard errors are given in parentheses and clustered at the subject (individual) level. $* = p < 0.1$, $** = p < 0.05$, $*** = p < 0.01$.

Table 6. Next Period Piece Rate: Impact on Effort

$$Problems_{i,t} \quad = \quad \alpha_1 \cdot Piece\,Rate_{i,t} + \alpha_2 \cdot Piece\,Rate_{i,t+1} \cdot Knowledge_{i,t} + \alpha_3 \cdot Piece\,Rate_{i,t-1} + \gamma X_i + \epsilon_{i,t}$$

Dependent Variable:	Experiment 1			Experiment 2			Experiment 3		
Problems Solved	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
Piece Rate	5.33***	2.78**	6.46***	12.8***	15.5***	16.76***	42.31***	65.91***	65.29***
	(1.26)	(1.20)	(1.95)	(3.58)	(2.79)	(3.04)	(17.96)	(15.23)	(17.14)
Next Period Piece Rate	1.80	0.57	4.20*	1.09	3.23	2.63	6.86	24.59**	22.28
(if known)	(1.73)	(2.35)	(2.47)	(2.64)	(2.19)	(3.88)	(13.61)	(10.71)	(17.64)
Previous Period Piece Rate	3.64**	0.84	6.53**	4.21	6.88**	7.75**	11.11	34.72**	27.60
	(1.62)	(1.33)	(1.62)	(4.12)	(3.17)	(3.38)	(20.57)	(16.83)	(18.29)
Pre-Treatment Quintiles		Yes	Yes		Yes	Yes		Yes	Yes
Period Fixed Effects		Yes	Yes			Yes			Yes
Shown Next Period Binary		Yes	Yes			Yes			Yes
Session Fixed Effects		Yes	Yes			Yes			Yes
Individual Controls		Yes	Yes			Yes			Yes
Periods 1 to 3 Only			Yes						
Dependent Variable Mean	7.85	7.85	7.79	7.23	7.23	7.23	63	63	63
Number of Observations	930	930	465	1266	1266	1260	736	736	732
Number of Individuals	155	155	155	422	422	420	184	184	183
Adj-R^2	0.01	0.32	0.39	0.01	0.41	0.44	0.01	0.40	0.47

Notes: The dependent variable is the number of problems solved correctly in a single period. All specifications report results from OLS regressions and also include a constant term. PreTreatment Quintiles represent five binary variables to non-parametrically control for the number of problems subject solved in the pre-treatment training period. "Only Periods 1 to 3" uses data of the first treatment period and following period to minimize treatment interactions. Individual Controls include age, sex, ethnicity, computer skill test, and total # of experimental sessions done at the lab, except for 3 subjects who could not be matched to controls. Piece Rate is in dollars per problem for Experiment 1 and 2, and in cents per problem for Experiment 3. Standard errors are given in parentheses and clustered at the subject (individual) level. $* = p < 0.1$, $** = p < 0.05$, $*** = p < 0.01$.

Table 7. Previous Effort Instrumental Variable: Impact on Effort

$$
\begin{aligned}
Problems_{i,t-1} &= \alpha_1 \cdot Piece\,Rate_{i,t-1} + \beta_1 \cdot Phone_{i,t-1} + \gamma X_i + \epsilon_{i,t} \\
Problems_{i,t} &= \rho \cdot Problems_{i,t-1} + \alpha_2 \cdot Piece\,Rate_{i,t} + \beta_2 \cdot Phone_{i,t} + \zeta X_i + \nu_{i,t}
\end{aligned}
$$

Dependent Variable:	Experiment 2			Experiment 3		
Problems Solved	(1)	(2)	(3)	(1)	(2)	(5)
# Solved Previous Period	0.39	0.50***	0.43***	0.35	0.57***	0.42**
	(0.26)	(0.18)	(0.17)	(0.39)	(0.19)	(0.21)
Piece Rate	12.7***	13.9***	14.9***	41.73***	48.27***	57.51***
	(2.63)	(2.51)	(2.88)	(11.80)	(12.17)	(13.72)
Phone Access	0.04	0.07	0.23	n.a.	n.a.	n.a.
	(0.24)	(0.23)	(0.27)			
First Stage F Stat (IV)	6.1	14.5	16.2	5.3	18.8	15.1
PreTreatment Quintiles		Yes	Yes		Yes	Yes
Period Fixed Effects			Yes			Yes
Session Fixed Effects			Yes			Yes
Individual Controls			Yes			Yes
Dependent Variable Mean	7.22	7.22	7.22	63	63	63
Number of Observations	844	844	840	552	552	549
Number of Individuals	422	422	420	184	184	183
Adj-R^2	0.39	0.56	0.57	0.43	0.66	0.65

Notes: The dependent variable is the number of problems solved correctly in a single period. All specifications report results from linear Instrumental Variable regressions estimated by (iterative) GMM and also include a constant term. PreTreatment Quintiles represent five binary variables to non-parametrically control for the number of problems subject solved in the pre-treatment training period. Individual Controls include age, sex, ethnicity, computer skill test, and total # of experimental sessions done at the lab, except for 3 subjects who could not be matched to controls. Piece Rate is in dollars per problem for Experiment 2 and in cents per problem for Experiment 3. Similar results for experiment 1 can be found in the Appendix, but had insufficient F-stats across specifications. Standard errors are given in parentheses and clustered at the subject (individual) level. $* = p < 0.1$, $** = p < 0.05$, $*** = p < 0.01$.

Table 8. Period or Total Earnings Salience: Impact on Earnings

$$Earnings_{i,t} \quad = \quad \alpha_1 \cdot Piece\,Rate_{i,t} + \alpha_2 Piece\,Rate_{i,t} \cdot PS_i + \beta_1 \cdot Phone_{i,t} + \beta_2 Phone_{i,t} \cdot PS_i + \gamma Period\,Shown_i + \gamma X_i + \epsilon_{i,t}$$

Dependent Variable:	Experiment 2			Experiment 3		
Problems Solved	(1)	(2)	(3)	(1)	(2)	(3)
Piece Rate	9.32*	13.12***	15.68***	71.72***	87.39***	96.27***
	(5.27)	(3.83)	(3.88)	(23.04)	(21.83)	(23.55)
Piece Rate * Period Salience	2.41	−3.82	−1.00	−54.96	−44.82	−59.71**
	(6.93)	(5.21)	(5.13)	(33.60)	(27.81)	(27.42)
Phone Access	0.92*	0.62*	0.88**	n.a.	n.a.	n.a.
	(0.50)	(0.35)	(0.46)			
Phone Access * Period Salience	−2.03***	−1.20**	−1.15**	n.a.	n.a.	n.a.
	(0.73)	(0.53)	(0.54)			
Period Salience	-0.32	0.34	0.25	9.67	8.52*	10.22**
	(0.58)	(0.40)	(0.39)	(6.23)	(4.91)	(4.44)
Pre-Treatment Quintiles		Yes	Yes		Yes	Yes
Period Fixed Effects			Yes			Yes
Session Fixed Effects			Yes			Yes
Individual Controls			Yes			Yes
Dependent Variable Mean	7.28	7.28	7.28	63	63	63
Number of Observations	894	894	891	736	736	732
Number of Individuals	298	298	297	184	184	183
Adj-R^2	0.02	0.45	0.52	0.01	0.39	0.47

Notes: The dependent variable is the number of problems solved in a single period. All specifications report results from OLS regressions and also include a constant term. The subject is shown either the previous period's earnings (as indicated by "Period Salience") or shown total earnings up to that period at the bottom of the page. Experiments 2 and 3 were the only experiments that featured this variation. Unfortunately, while every subject in Experiment 2 did face a randomized period or total counter, a small programming typo prevented the capture of this variable for the first day, and those subjects were dropped. PreTreatment Quintiles represent five binary variables to non-parametrically control for the number of problems subject solved in the pre-treatment training period. Individual Controls include age, sex, ethnicity, computer skill test, and total # of experimental sessions done at the lab, except for 2 subjects who could not be matched to controls. Piece Rate is in dollars per problem for Experiment 2 and in cents per problem for Experiment 3. Standard errors given in parentheses and clustered at the subject (individual) level. $* = p < 0.1$, $** = p < 0.05$, $*** = p < 0.01$.

10 Online Appendix (For Online Publication)

10.1 Appendix Tables

Appendix Table 1. Previous Effort Instrumental Variable: Impact on Effort – Experiment 1

$$\begin{aligned} Problems_{i,t-1} &= \alpha_1 \cdot Piece\,Rate_{i,t-1} + \beta_1 \cdot Phone_{i,t-1} + \gamma X_i + \epsilon_{i,t} \\ Problems_{i,t} &= \rho \cdot Problems_{i,t-1} + \alpha_2 \cdot Piece\,Rate_{i,t} + \beta_2 \cdot Phone_{i,t} + \zeta X_i + \nu_{i,t} \end{aligned}$$

Dependent Variable:	Specification					
Problems Solved	(1)	(2)	(3)	(4)	(5)	(6)
Problems Previous Period	0.49**	0.48*	−0.02	0.41	0.71*	0.47
	(0.25)	(0.26)	(0.17)	(0.34)	(0.37)	(0.40)
Piece Rate	4.50***	4.51***	2.03	7.41***	7.02***	6.13***
	(1.31)	(1.29)	(2.88)	(2.41)	(2.54)	(2.30)
Phone Access	−0.42**	−0.40**	0.23	0.14	0.09	0.17
	(0.19)	(0.18)	(0.27)	(0.34)	(0.34)	(0.33)
First Stage F Stat (IV)	9.8	9.5	4.6	2.8	3.6	2.8
PreTreatment Quintiles		Yes	Yes		Yes	Yes
Period Fixed Effects		Yes	Yes		Yes	Yes
Session Fixed Effects			Yes			Yes
Individual Controls			Yes			Yes
Period 1 and 2 Only				Yes	Yes	Yes
Dependent Variable Mean	7.85	7.85	7.85	7.79	7.79	7.79
Number of Observations	930	930	930	465	465	465
Number of Individuals	155	155	155	155	155	155
Adj-R^2	0.01	0.31	0.37	0.02	0.66	0.66

Notes: The dependent variable is the number of problems solved correctly in a single period. All specifications report results from linear Instrumental Variable regressions estimated by (iterative) GMM and also include a constant term. PreTreatment Quintiles represent five binary variables to non-parametrically control for the number of problems subject solved in the pre-treatment training period. Individual Controls include age, sex, ethnicity, computer skill test, and total # of experimental sessions done at the lab. Standard errors are given in parentheses and clustered at the subject (individual) level. $* = p < 0.1$, $** = p < 0.05$, $*** = p < 0.01$.

Appendix Table 2. Previous Period Piece Rate and Phone Access: Random Effects – Experiment 2

$$Problems_{i,t} = \alpha_1 \cdot Piece\,Rate_{i,t} + \alpha_2 \cdot Piece\,Rate_{i,t-1} + \gamma X_i + \epsilon_{i,t}$$

Dependent Variable:	Specification				
Problems Solved	(1)	(2)	(3)	(4)	(5)
Piece Rate	11.05***	12.58***	14.95***	15.07***	15.00***
	(2.40)	(2.29)	(2.67)	(2.71)	(2.78)
Previous Period's Piece Rate	2.47	4.00	5.62**	5.73**	5.78**
	(2.68)	(2.55)	(2.80)	(2.83)	(2.89)
Phone Access	−0.05	−0.08	0.25	0.27	0.28
	(0.22)	(0.22)	(0.27)	(0.27)	(0.27)
Previous Period Phone Access	−0.05	−0.08	0.15	0.17	0.17
	(0.27)	(0.27)	(0.31)	(0.31)	(0.31)
Random Effects	Yes	Yes	Yes	Yes	Yes
PreTreatment Quintiles		Yes	Yes	Yes	Yes
Period Fixed Effects			Yes	Yes	Yes
Session Fixed Effects				Yes	Yes
Individual Controls					Yes
Number of Observations	1266	1266	1266	1266	1260
Number of Individuals	422	422	422	422	420
Adj-R^2	0.01	0.41	0.41	0.44	0.45

Notes: The dependent variable is the number of problems solved correctly in a single period. All specifications report results from OLS regressions and also include a constant term. PreTreatment Quintiles represent five binary variables to non-parametrically control for the number of problems subject solved in the pre-treatment training period. Individual Controls include age, sex, ethnicity, computer skill test, and total # of experimental sessions done at the lab. Standard errors given in parentheses and clustered at the subject (individual) level. $* = p < 0.1$, $** = p < 0.05$, $*** = p < 0.01$.

Appendix Table 3. Previous Period Piece Rate and Phone Access: Fixed Effects – Experiment 2

$$Problems_{i,t} \quad = \quad \alpha_1 \cdot Piece\,Rate_{i,t} + \alpha_2 \cdot Piece\,Rate_{i,t-1} + \gamma X_i + \epsilon_{i,t}$$

Dependent Variable:	Specification		
Problems Solved	(1)	(2)	(3)
Piece Rate	12.66***	10.46***	13.49***
	(3.51)	(2.54)	(3.00)
Previous Period's Piece Rate	4.09	1.89	4.16
	(4.03)	(2.63)	(2.94)
Phone Access	−0.03	−0.05	0.36
	(0.36)	(0.22)	(0.28)
Previous Period Phone Access	−0.03	−0.05	0.26
	(0.41)	(0.26)	(0.31)
Subject Fixed Effects		Yes	Yes
Period Fixed Effects			Yes
Dependent Variable Mean	7.23	7.23	7.23
Number of Observations	1266	1266	1266
Number of Individuals	422	422	422
Adj-R^2	0.01	0.72	0.72

Notes: In the presence of momentum, these estimates are **severely biased** downward. They are presented here only in the spirit of openness but are not intended to be taken as accurate estimates. The dependent variable is the number of problems solved correctly in a period. All specifications report results from OLS regressions and also include a constant term. PreTreatment Quintiles represent five binary variables to non-parametrically control for the number of problems subject solved in the pre-treatment training period. Note that session fixed effects and individual controls cannot be estimated with subject fixed effects as these variables do not vary within individual. Standard errors given in parentheses and clustered at the subject (individual) level. $* = p < 0.1$, $** = p < 0.05$, $*** = p < 0.01$.

Appendix Table 4. Previous Period Piece Rate and Phone Access: Impact on YouTube Searches – Experiment 2

$$Youtube_{i,t} \quad = \quad \alpha_1 \cdot Piece\,Rate_{i,t} + \alpha_2 \cdot Piece\,Rate_{i,t-1} + \gamma X_i + \epsilon_{i,t}$$

Dependent Variable:			Specification		
Youtube Searches	(1)	(2)	(3)	(4)	(5)
Piece Rate	−2.39***	−2.52***	−2.80***	−2.68***	−2.59***
(in cents)	(0.42)	(0.44)	(0.56)	(0.63)	(0.65)
Previous Period's Piece Rate	−1.01*	−1.15**	−0.94	−0.83	−0.72
	(0.56)	(0.55)	(0.58)	(0.66)	(0.68)
Phone Access	−0.02	−0.02	−0.06	−0.03	−0.04
	(0.07)	(0.07)	(0.08)	(0.08)	(0.08)
Previous Period Phone Access	0.09	0.09	0.12	0.14	0.14
	(0.09)	(0.08)	(0.09)	(0.09)	(0.09)
PreTreatment Quintiles		Yes	Yes	Yes	Yes
Period Fixed Effects			Yes	Yes	Yes
Session Fixed Effects				Yes	Yes
Individual Controls					Yes
Dependent Variable Mean	0.24	0.24	0.24	0.24	0.24
Number of Observations	1266	1266	1266	1266	1260
Number of Individuals	422	422	422	422	420
Adj-R^2	0.01	0.02	0.02	0.04	0.07

Notes: The dependent variable is the number of YouTube videos searched in a period. All specifications report results from OLS regressions and also include a constant term. PreTreatment Quintiles represent five binary variables to non-parametrically control for the number of problems subject solved in the pre-treatment training period. Individual Controls include age, sex, ethnicity, computer skill test, and total # of experimental sessions done at the lab, but could not be matched for 2 subjects. Standard errors given in parentheses and clustered at the subject (individual) level. $* = p < 0.1$, $** = p < 0.05$, $*** = p < 0.01$.

Appendix Table 5. Phone Usage: Relationship with Effort – Experiment 2

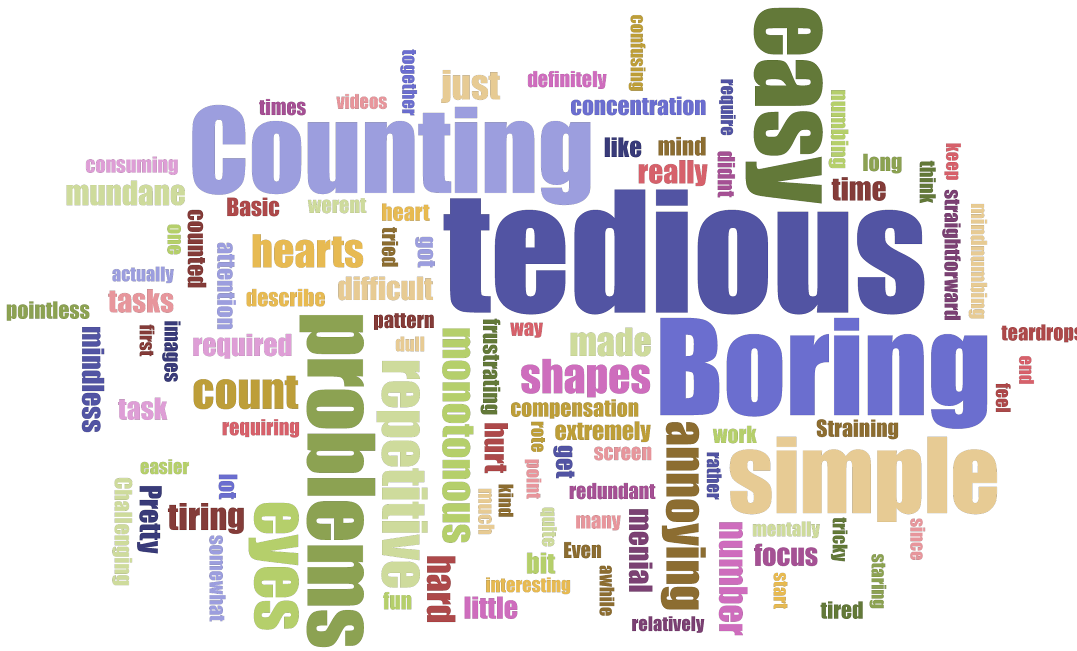
$$Problems_{i,t} \quad = \quad \alpha_1 \cdot Phone_{it} + \alpha_2 \cdot Phone_{it-1} + \beta_1 \cdot PieceRate_{it} + \beta_2 \cdot PieceRate_{it-1} + \gamma X_i + \epsilon_{i,t}$$

Dependent Variable:	Specification				
Problems Solved	(1)	(2)	(3)	(4)	(5)
Phone Usage	−3.81***	−2.19***	−2.06***	−2.11***	−1.93***
	(0.88)	(0.71)	(0.72)	(0.73)	(0.73)
Previous Period Phone Usage	−4.73***	−3.14***	−3.11***	−3.16***	−2.99***
	(0.92)	(0.65)	(0.67)	(0.69)	(0.70)
Piece Rate	12.41***	14.95***	15.99***	15.96***	16.13***
	(3.51)	(2.76)	(2.96)	(2.95)	(3.01)
Previous Period Piece Rate	3.95	6.50**	6.70**	6.67**	6.96**
	(3.93)	(3.05)	(3.23)	(3.24)	(3.29)
Pre-Treatment Quintiles		Yes	Yes	Yes	Yes
Period Fixed Effects			Yes	Yes	Yes
Session Fixed Effects				Yes	Yes
Individual Controls					Yes
Dependent Variable Mean	7.23	7.23	7.23	7.23	7.23
Number of Observations	1266	1266	1266	1266	1260
Number of Individuals	422	422	422	422	420
Adj-R^2	0.04	0.42	0.43	0.45	0.44

Notes: The dependent variable is the number of problems solved correctly in a period. Phone Usage is a self reported variable indicating use of the phone during period 3. As this is endogenously chosen, these regressions should not be taken as causal, as a subject who uses the phone may have unobservable differences. All specifications report results from OLS regressions and also include a constant term. PreTreatment Quintiles represent five binary variables to non-parametrically control for the number of problems subject solved in the pre-treatment training period. Individual Controls include age, ethnicity, computer skill test, and total # of experimental sessions done at the lab. Gender could not be matched for one subject, and the controls for an additional subject. Standard errors given in parentheses and clustered at the subject (individual) level. $* = p < 0.1$, $** = p < 0.05$, $*** = p < 0.01$.

10.2 Appendix Figures

Appendix Figure 1. Word Cloud for Survey – "Opinion of Task"



Notes: Top 100 words from responses to a post experiment survey question asking "What is your opinion of the task?" Size scaled linearly with count.

Source: Jasondavies.com word cloud generator.

Appendix Figure 2. Quiz for Introduction Instructions – Experiment 2

Please answer the following questions about the experiment today.

How many problem sections are there today?

- ○ 2 problem sections.
- ◉ 4 problem sections.
- ○ 6 problem sections.

I can earn extra compensation at different rates for each section. These rates:

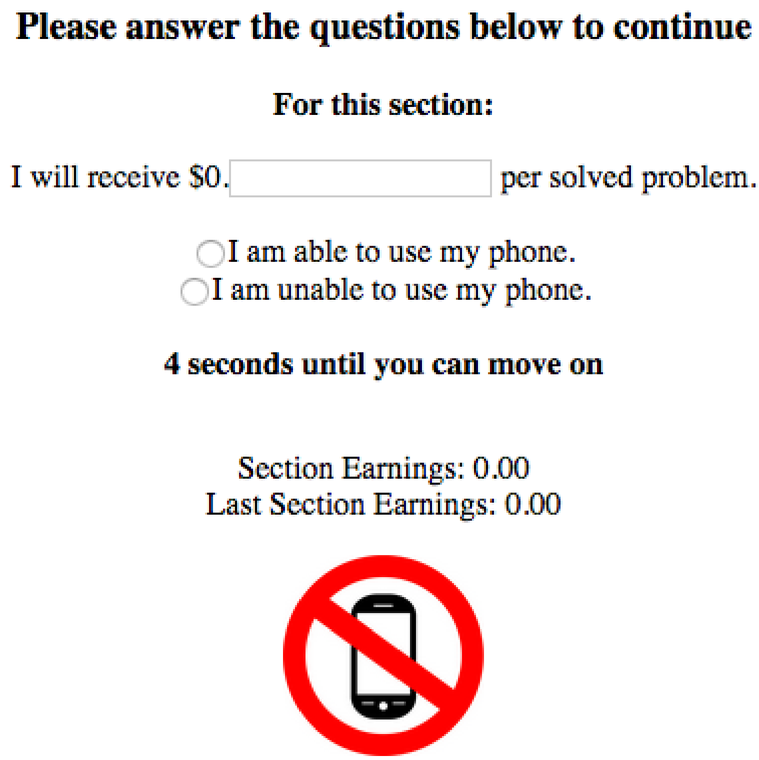
- ◉ Are random and do not depend on how many problems I solved in the previous section(s).
- ○ Depend on how many problems I solved in the previous section(s).

In order to get the $10 participation compensation, I need to:

- ○ Do at least a few problems.
- ◉ Do not have to do any problems.

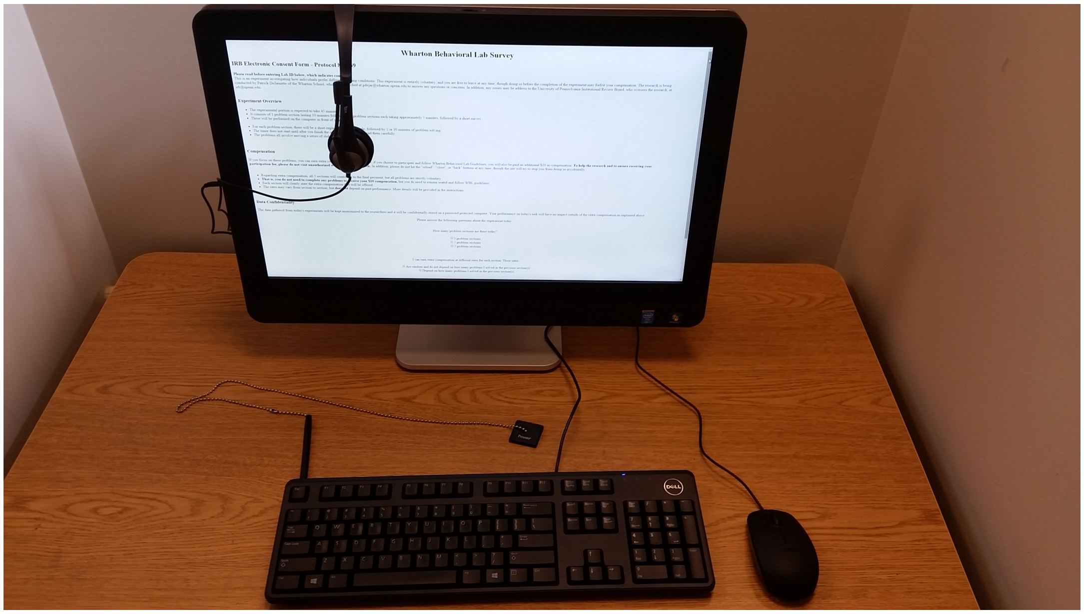
Notes: Every participant in experiment 2 had to answer the above questions after reading experiment instructions. Subjects had to answer all three questions correctly to proceed. If the subject entered the wrong answers, the browser would alert them to this and ask for them to review the instructions again.

Appendix Figure 3. Quiz for Instructions Prior to Each Period – Experiment 2



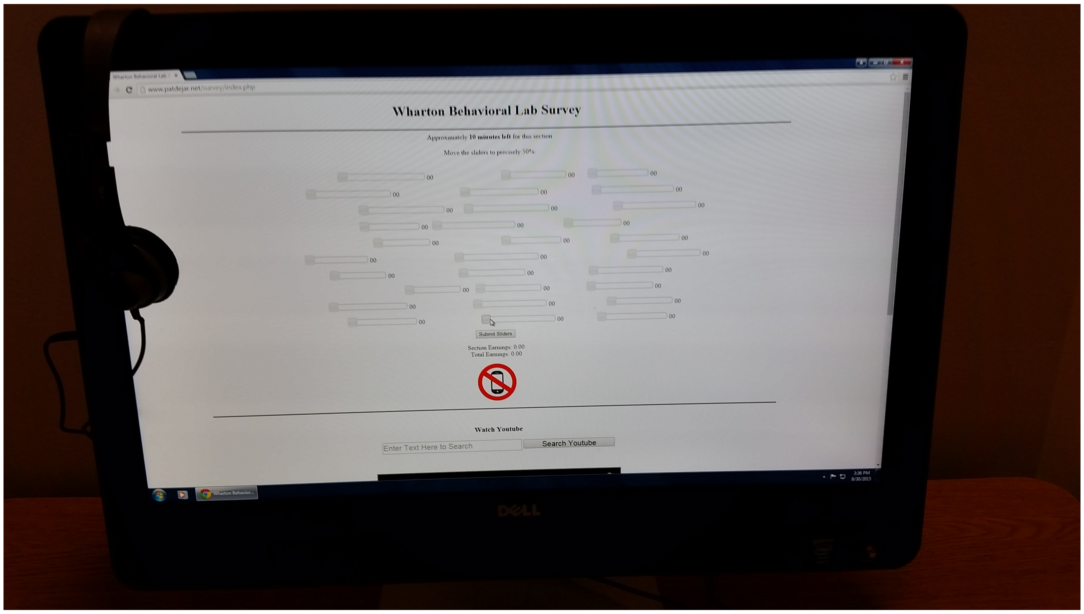
Notes: Every participant had to answer the following questions prior to every period (including Pre-Treatment). If the subject had information about future periods, they were also quizzed on the piece rate and phone access for future periods. If the subject entered the wrong answers, the browser would alert them to this and ask for them to review the instructions again.

Appendix Figure 4. Cubicle Environment



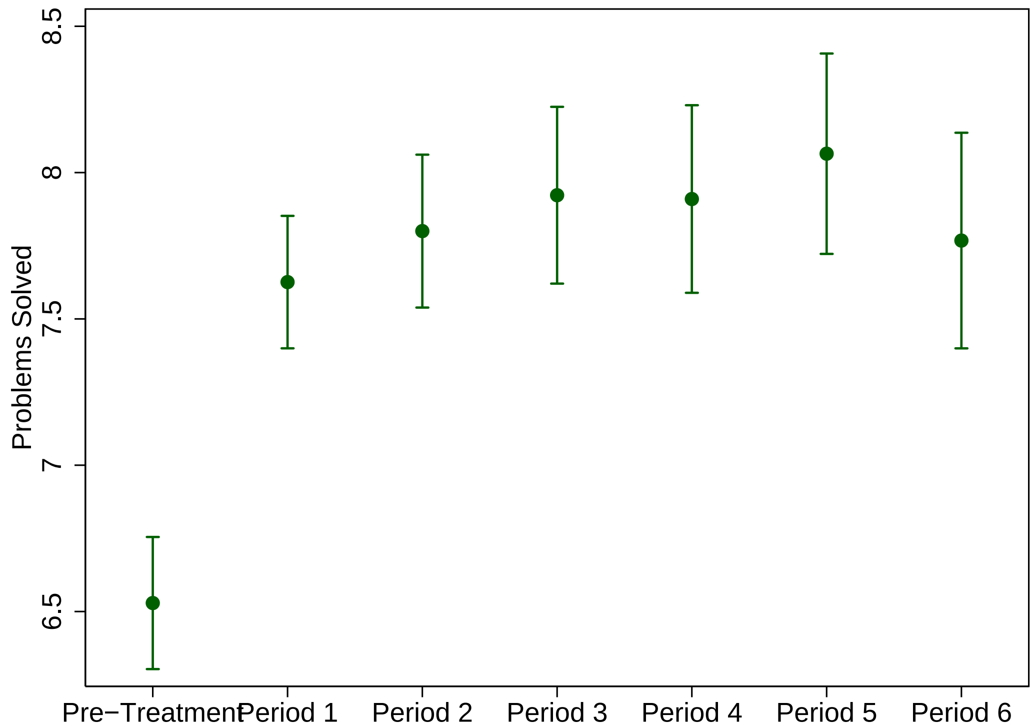
Notes: Every participant had access to an identical computer with headphones as pictured above. Screen brightness was uniformly set at 95% to ensure consistency across cubicles. It was not possible to see other subjects from within the cubicle. Google Chrome was employed as the browser during the task, with the window maximized (full screen mode). All instructions were written, but research assistants were on site to answer any additional questions.

Appendix Figure 5. Slider Task Example



Notes: Slider Task employed as a replication of momentum effects. As can seen above, it would be difficult to view YouTube and move sliders at the same time on the monitor and resolution employed. Furthermore, the website code blocked any attempt to "zoom" in or out.

Appendix Figure 6: Solved Problems by Period – Experiment 1



Notes: Bars represent standard errors. Three treatment pairs were applied at varying periods (see experimental design in section 3). The Pre-Treatment period is a training period to familiarize workers with the task.

10.3 Proofs

10.3.1 Proof of Proposition 2.1: Sophisticated Momentum

This proof will be using methods of supermodularity discussed in Milgrom and Shannon (1995).[72] For simplicity, I retain the assumption about the utility function being twice differentiable over c_t, e_t, e_{t-1}, however this assumption could be weakened as long as the utility function maintains increasing differences.

MS Theorem 6a: I begin by applying Theorem 6 of Milgrom and Shannon (1995), which first states that a twice differentiable function $f : \mathbb{R}^n \times \mathbb{R}^m \to \mathbb{R}$ has increasing differences in (x, z) if and only if $\frac{\partial f^2}{\partial x_i \partial z_j} \geq 0$ for all $i = 1, ..., n$ and $j = 1, ..., m$.

MS Theorem 6a Conditions: In this case, $x_1 \equiv c_1$, $x_2 \equiv c_2$,..., $x_T \equiv c_T$, and $x_{T+1} \equiv e_1$, $x_{T+2} \equiv e_2$, ..., $x_{2T} \equiv e_T$. y are the non-choice state variables, $z_1 \equiv w_1$, $z_2 \equiv w_2$, ..., $z_T \equiv w_T$, and $z_{T+1} \equiv -\gamma_1$, $z_{T+1} \equiv -\gamma_2$, ...$z_{2T} \equiv -\gamma_T$, and f is the full Lagrangian (treating y_t, r, and p_t as fixed):

$$f(x, z) = \sum_{t=1}^{T} \delta^{t-1} u(c_t, e_t, e_{t-1}, \gamma_t) + \lambda \left(\sum_{t=1}^{T} (w_t e_t + y_t)(1+r)^{-t} - \sum_{t=1}^{T} p_t c_t (1+r)^{-t} \right)$$

With variables redefined this way, we can check the conditions. For consumption and wage, it is clear that $\frac{\partial f^2}{\partial c_i \partial w_j} = 0$ as w_j does not enter the utility function and the budget constraint is linear with no term containing both c_i and w_j. For consumption and leisure technology, $\frac{\partial f^2}{\partial c_i \partial \gamma_j} = 0$ if $i \neq j$. However, without an assumption on $\frac{\partial f^2}{\partial c_t \partial \gamma_t}$, it is possible that increased leisure technology increases the utility of consumption so much that effort rises in every period (including time period t) to satisfy the greater demand for consumption goods. However, while leisure and consumption might have complementarities (and hence effort and consumption exhibit substitutability), leisure technology itself is constructed to not influence the utility from consumption.[73]

For effort and wage, $\frac{\partial f^2}{\partial e_i \partial w_j} = 0$ if $i \neq j$ as wage and effort only appear together in the same time period. In this case $\frac{\partial f^2}{\partial e_t \partial w_t} = \frac{\partial f^2}{\partial e_t \partial w_t} = \lambda(1+r)^{-t}$, which is ≥ 0 as consumption c_t is enjoyable and $r > -1$.

For effort and leisure technology, $\frac{\partial f^2}{\partial e_i \partial \gamma_j} = 0$ if $i > j$ or $i + 1 < j$ as e_t only appears in two $u(\cdot)$ functions, at time t and time $t+1$. Thus, the only cases we need to check are on $\frac{\partial f^2}{\partial e_i \partial(-\gamma_i)}$ and $\frac{\partial f^2}{\partial e_i \partial(-\gamma_{i+1})}$, which are equivalent to $-\delta^{i-1} \frac{\partial u^2}{\partial e_i \partial \gamma_i}$ and $-\delta^{i} \frac{\partial u^2}{\partial e_i \partial \gamma_{i+1}}$, respectively. From the assumptions above, $\frac{\partial u^2}{\partial e_i \partial \gamma_i} \leq 0$, as leisure technology makes marginal contemporaneous effort more costly in utility terms, and $\frac{\partial u^2}{\partial e_i \partial \gamma_{i+1}} = 0$, as leisure technology does not carry over across periods. Thus, both $\frac{\partial f^2}{\partial e_i \partial(-\gamma_i)}$ and $\frac{\partial f^2}{\partial e_i \partial(-\gamma_{i+1})}$ are ≥ 0. And in conclusion, f has increasing differences in (x, z).

MS Theorem 6b: I apply the second half of Theorem 6 of Milgrom and Shannon (1995), which states that a twice differentiable function $f : \mathbb{R}^n \times \mathbb{R}^m \to \mathbb{R}$ is supermodular in x if and only if $\frac{\partial f^2}{\partial x_i \partial x_j} \geq 0$ for all $i \neq j$ in $1, ..., n$.

MS Theorem 6b Conditions: As before, $x_1 \equiv c_1$, $x_2 \equiv c_2$,..., $x_T \equiv c_T$, and $x_{T+1} \equiv e_1$, $x_{T+2} \equiv e_2$, ..., $x_{2T} \equiv e_T$, and f is the full Lagrangian:

$$f(x, z) = \sum_{t=1}^{T} \delta^{t-1} u(c_t, e_t, e_{t-1}, \gamma_t) + \lambda \left(\sum_{t=1}^{T} (w_t e_t + y_t)(1+r)^{-t} - \sum_{t=1}^{T} p_t c_t (1+r)^{-t} \right)$$

[72] An earlier draft of the proof uses the (Dini) Multivariate Implicit Function Theorem, but supermodularity allows for fewer restrictions and also removes the need for matrix manipulation in calculating determinants.

[73] E.g. Popcorn is enjoyable while watching the new Star Wars movie; the new Star Wars movie does not make popcorn itself taste better on December 18th if you did not get to watch it.

Note that $\frac{\partial f^2}{\partial c_i \partial c_j} = 0$ for $i \neq j$ as there is no overlap in the additively separable terms. Likewise $\frac{\partial f^2}{\partial e_i \partial e_j} = 0$ if $i > j$ or $i+1 < j$ as only last period's effort influences this period's effort.[74] Thus, for time period t we are concerned with two terms: first, $\frac{\partial f^2}{\partial e_t \partial e_{t-1}} = \delta^{t-1} \frac{\partial u^2}{\partial e_t \partial e_{t-1}} \geq 0$, as we have positive momentum (last period effort makes this period's effort marginally less costly in terms of utility). Second, $\frac{\partial f^2}{\partial e_{t+1} \partial e_t} = \delta^t \frac{\partial u^2}{\partial e_{t+1} \partial e_t} \geq 0$ for the same reasons.

The only remaining terms of interest are the cross-partials between consumption and effort. Similar to above, $\frac{\partial f^2}{\partial e_i \partial c_j} = 0$ if $i > j$ or $i+1 < j$ as the overlap only occurs for a c_t and e_t or c_t and e_{t-1}, as per the utility function. First, $\frac{\partial f^2}{\partial c_t \partial e_t} = \delta^{t-1} \frac{\partial u^2}{\partial c_t \partial e_t} \geq 0$, as assumed in the set up. This implies that consumption and effort are not complements. By a similar assumption $\frac{\partial f^2}{\partial c_t \partial e_{t-1}} = \delta^{t-1} \frac{\partial u^2}{\partial c_t \partial e_{t-1}} \geq 0$ as last period's effort should have no negative effect on this period's consumption. These assumptions, while not trivial, ensure the utility function is reasonably well behaved – otherwise if last period's effort greatly reduced the demand for consumption, it could theoretically reduce this period's effort as well as the demand for consumption has decreased so dramatically.[75] Thus, in conclusion, the conditions are satisfied, and f is supermodular in x.

MS Theorem 4: Using these results from Theorem 6, apply Theorem 4 of Milgrom and Shannon (1995) to achieve the main result. The theorem states that if $f : X \times Z \to \mathbb{R}$, where X is a lattice, T is a partially ordered set, and $S \subset X$, then $argmax_{x \in S} f(x, z)$ is a monotone nondecreasing function in (z, S) if and only if f is quasisupermodular in x and satisfies the single crossing property in $(x; z)$.

MS Theorem 4 Conditions: First note that $\mathbb{R}^n$ with component-wise order forms a lattice as for $\forall x,\ y \in \mathbb{R}^n$, $x \wedge y$ and $x \vee y$ are both in $\mathbb{R}^n$. By the same token, $\mathbb{R}^m$ with component-wise ordering is a partially ordered set. Thus, using the lagrangian function above as f, with x and z defined as above, we have already established supermodularity in x, which implies quasisupermodularity in x. In addition, as f has increasing differences, it satisfies the single crossing property. Thus it follows that $\vec{c}^*$, $\vec{e}^* \in argmax_{c,e \in \mathbb{R}^{2T}_+} f(c, e, z)$ are monotone decreasing functions over $(z,\ \mathbb{R}^n_+)$ – but recall that z_{T+1} was defined as negative γ_1 , z_{T+2} as negative γ_2, and so on. Thus, consumption and effort are monotonically non-decreasing over piece rate vector $\vec{w}$ and monotonically non-increasing over leisure technology vector $\vec{\gamma}$.[76]

Alternate Proofs

It's possible to relax the assumptions of the model. The differentiability of u is unnecessary as long as the conditions of increasing differences / single crossing condition and quasi-supermodularity are satisfied. However, I felt the assumptions above are more familiar with readers compared to assumptions of increasing differences. In addition, the assumption that only last period enters the utility function is unnecessary.

It may also be worth mentioning that there are other ways to achieve a similar result. An earlier draft included a proof using the Multivariate Implicit Function Theorem and also assumed second order conditions and positive determinant Jacobian matrices to get the stronger result of effort strictly increasing in piece rates (or strictly decreasing in leisure technology). However, the matrix notation was cumbersome relative to the above proof.

[74] Though this assumption of only last period's effort influencing this period is not necessary for this proof. For example, if all previous periods' efforts entered the utility function as a discounted sum with non-negative weights, as long as effort is positive with respect to that sum (positive effort momentum), the result would be the same.

[75] For example, if after working a long day, the individual no longer cared for consumption. Under this example, the individual might call in sick, even though effort would have been easier due to effort momentum. In this odd model of behavior, an increase in piece rate last period could decrease effort in the next period.

[76] To be clear, as I am using component-wise ordering, increasing just one element of the piece rate vector or one element of the leisure technology vector causes the vector to be ordered as higher than the unaltered vector.

10.3.2 Proof of Proposition 2.2: Naive Momentum g(.) function

Under the FOC for e_t, c_t:

$$\begin{aligned} -u_e(c_t^*, e_t^*, e_{t-1}, \gamma_t) &= \lambda w_t \\ u_c(c_t^*, e_t^*, e_{t-1}, \gamma_t) &= \lambda p_t \end{aligned}$$

As u is strictly concave over the first argument, this allows for inverse of u_c:

$$c_t^* = u_c^{-1}\left(\lambda p_t,\, e_t^*,\, e_{t-1},\, \gamma_t\right)$$

Which can be inserted into the first FOC to give:

$$-u_e(u_c^{-1}\left(\lambda p_t,\, e_t^*,\, e_{t-1},\, \gamma_t\right), e_t^*,\, e_{t-1}, \gamma_t) = \lambda w_t$$

The e_t^* which solves this first order condition is equivalent to the e_t^{**} which would maximize (by construction):

$$e_t^{**} = argmax_{e_t}\ \lambda w_t e_t + \int_0^{e_t} u_e(u_c^{-1}\left(\lambda p_t,\, x,\, e_{t-1},\, \gamma_t\right), x,\, e_{t-1}, \gamma_t) dx$$

This objective function can be rewritten as $U_t^{'} = \lambda w_t e_t - g\left(e_t,\, e_{t-1},\, \gamma_t\right)$. It remains to be shown that this $g(e_t,\, e_{t-1},\, \gamma_t)$ is convex in e_t, which in this case is equivalent to having a negative second derivative:

$$\begin{aligned} \frac{dg}{de_t} &= - \frac{\partial u(u_c^{-1}\left(\lambda p_t,\, e_t^*,\, e_{t-1},\, \gamma_t\right), e_t^*,\, e_{t-1}, \gamma_t)}{\partial e_t} \\ \frac{d^2 g}{de_t^2} &= -\frac{\partial^2 u}{\partial e_t^2} - \frac{\partial^2 u}{\partial e_t \partial c_t}\frac{dc_t}{de_t} \end{aligned}$$

Note $\frac{de_{t-1}}{de_t} = \frac{d\gamma_t}{de_t} = 0$ as e_{t-1} and γ_t are not choice variables at time t. The total differential of the first order condition for c_t yields:

$$\begin{aligned} u_{cc} dc_t + u_{ce} de_t &= 0 \\ \Rightarrow \frac{dc_t}{de_t} &= -\frac{u_{ce}}{u_{cc}} \end{aligned}$$

Thus for the second derivative:

$$\begin{aligned} \frac{d^2 g}{de_t^2} &= -u_{ee} + u_{ec}\frac{u_{ce}}{u_{cc}} \\ &= -\frac{1}{u_{cc}}\left(u_{ee}u_{cc} - u_{ec}u_{ce}\right) \\ &> 0 \end{aligned}$$

As $u_{cc} < 0$ and $u_{ee}u_{cc} - u_{ec}u_{ce} > 0$ from strict concavity of $u(\cdot)$ in both e_t and c_t. Given this derivation, one can derive how past, present, and future piece rate and leisure technology influence effort as outlined in Section 2.

10.3.3 Reciprocity Comparative Statics

Claim: If $\alpha_2 > 0$ and e_1, e_2 is an interior solution, then $\frac{\partial e_{t+1}}{\partial \gamma_t} > 0$

Proof:

$$U = (\lambda w_1 + \alpha_1 w_1 + \alpha_1 w_2 + \alpha_2 \gamma_1 + \alpha_2 \gamma_2)(e_1 + e_2) - g(\gamma_1 e_1) - g(\gamma_2 e_2)$$

First order condition:

$$\begin{aligned}
(\lambda w_1 + \alpha_1 w_1 + \alpha_1 w_2 + \alpha_2 \gamma_1 + \alpha_2 \gamma_2) &= \gamma_1 g'(\gamma_1 e_1) \\
(\lambda w_2 + \alpha_1 w_1 + \alpha_1 w_2 + \alpha_2 \gamma_1 + \alpha_2 \gamma_2) &= \gamma_2 g'(\gamma_2 e_2)
\end{aligned}$$

Multivariate implicit function theorem (Dini) gives us:

$$\begin{aligned}
\frac{\partial e_2}{\partial \gamma_1} &= -\frac{det \begin{bmatrix} \frac{\partial F_1}{\partial e_1} & \frac{\partial F_1}{\partial \gamma_1} \\ \frac{\partial F_2}{\partial e_1} & \frac{\partial F_2}{\partial \gamma_1} \end{bmatrix}}{det \begin{bmatrix} \frac{\partial F_1}{\partial e_1} & \frac{\partial F_1}{\partial e_2} \\ \frac{\partial F_2}{\partial e_1} & \frac{\partial F_2}{\partial e_2} \end{bmatrix}} \\
&= -\frac{det \begin{bmatrix} -\gamma_1^2 g''(\gamma_1 e_1) & \alpha_2 - g'(\gamma_1 e_1) - \gamma_1 e_1 g''(\gamma_1 e_1) \\ 0 & \alpha_2 \end{bmatrix}}{det \begin{bmatrix} -\gamma_1^2 g''(\gamma_1 e_1) & 0 \\ 0 & -\gamma_2^2 g''(\gamma_2 e_2) \end{bmatrix}} \\
&= -\frac{-\alpha_2 \gamma_1^2 g''(\gamma_1 e_1)}{\gamma_1^2 \gamma_2^2 g''(\gamma_1 e_1) g''(\gamma_2 e_2)} \\
&= \frac{\alpha_2}{\gamma_2^2 g''(\gamma_2 e_2)} > 0
\end{aligned}$$

Claim: If $\alpha_1 > 0$ and e_1, e_2 is an interior solution, then $\frac{\partial e_{t+1}}{\partial w_t} > 0$:

Proof: By Multivariate Implicit Function Theorem using the above FOC,

$$\begin{aligned}
\frac{\partial e_2}{\partial w_1} &= -\frac{det \begin{bmatrix} \frac{\partial F_1}{\partial e_1} & \frac{\partial F_1}{\partial w_1} \\ \frac{\partial F_2}{\partial e_1} & \frac{\partial F_2}{\partial w_1} \end{bmatrix}}{det \begin{bmatrix} \frac{\partial F_1}{\partial e_1} & \frac{\partial F_1}{\partial e_2} \\ \frac{\partial F_2}{\partial e_1} & \frac{\partial F_2}{\partial e_2} \end{bmatrix}} \\
&= -\frac{det \begin{bmatrix} -\gamma_1^2 g''(\gamma_1 e_1) & \lambda + \alpha_1 \\ 0 & \alpha_1 \end{bmatrix}}{det \begin{bmatrix} -\gamma_1^2 g''(\gamma_1 e_1) & 0 \\ 0 & -\gamma_2^2 g''(\gamma_2 e_2) \end{bmatrix}} \\
&= -\frac{-\alpha_1 \gamma_1^2 g''(\gamma_1 e_1)}{\gamma_1^2 \gamma_2^2 g''(\gamma_1 e_1) g''(\gamma_2 e_2)} \\
&= \frac{\alpha_1}{\gamma_2^2 g''(\gamma_2 e_2)} > 0
\end{aligned}$$

10.4 Secondary Outcome Treatment Effects

In addition to correct problems solved as a metric for effort, I also collected the number of YouTube videos searched as a proxy for leisure time.[77] As predicted, subjects in the first experiment search 0.2 fewer YouTube videos when the piece rate is increased (see Appendix Table 4). In some specifications, this reduction in leisure persists into the next period as well. This is consistent with the model in which YouTube videos are a leisurely activity, and when faced with a higher piece rate, the worker exerts more time and effort working (and less on leisure).

To further validate that the phone access was actually a leisure activity, subjects in the first experiment searched about 0.12 to 0.15 fewer searches ($p < 0.05$) when given access to cell phones. This is consistent with a model in which phone access and YouTube videos are both leisure activities that compete for attention. Anecdotally, YouTube and the cellphone reside on different screens and may require focus to provide relaxation, making them difficult to serve as leisure complements.

10.5 Other Estimation Strategies

In this appendix section, I discuss GMM strategies from Arellano and Bond (1991) and Blundell and Bond (1998). If the results presented above are driven by naive effort momentum, the theory provides additional moment conditions that could be employed to remove the asymptotic bias discussed in Nickell (1981). These methods were employed using the xtabond2 command in Stata on Experiment 2 and 3, which feature the cleanest experimental designs (Roodman, 2006).

In both experiments, the estimated coefficient was reduced relative to OLS and the IV estimates, but still significantly different from zero. However, based on later tests, it appears the assumptions for the moment conditions were not met. In particular, piece rate and lagged piece rate are strong exogenous instruments for effort, as they were randomized at the individual level. Yet in both experiments, the Difference–in-Hansen[78] tests for exogeniety (for the other instruments) reject the null hypothesis that all instruments employed are exogenous in the levels. In addition, for experiment 3, the Arellano-Bond test for AR(2) in first differences is also rejected.[79] Based on additional Monte Carlo studies, with substantial serial correlation of the error terms, these estimates may also perform poorly.

In a related strand of econometric literature, Andrews (1993) outlines a method for adjusting the well known bias of using OLS to estimate an AR(1) for three cases: (i) without an intercept, (ii) with an intercept, and (iii) with an intercept and time trend. Unfortunately, the original Andrews (1993) paper does not allow for individual fixed effects or individual exogenous variables x_{it}. Estimating the model without fixed effects or individual-level covariates would likely bias the ρ parameter upwards through omitted variable bias, as individuals have time-constant heterogeneity in their effort allocation.[80] Thus the Andrews (1993) MUE estimator would still be biased in this setting as it would be too low, unable to account for the additional omitted variable bias.

However, there has been considerable work extending the original MUE estimator to panel data. One direction can be found in work on Panel Exactly Median-Unbiased Estimators (PEMU) by Cermeno (1999) and Phillips and

[77]Ideally I would prefer total length of YouTube videos played, but this information could be gathered from the web browser. In addition, playing a video does not necessarily imply that the subject is actually watching a video, and length of the video may be imperfect measures if the subjects skip portions. Thus, both would be noisy estimates of the total leisure time.

[78]Or when using non-robust standard errors, the Difference-in-Sargan tests also reject the null.

[79]It is expected for the Arellano-Bond test for AR(1) in first differences to be rejected, but the AR(2) also being rejected indicates that perhaps some of the individual heterogeneity remains. In other words, the tests indicate that the AR(1) assumptions allowing one to use lagged differences as instruments are unlikely to be true. It is also worth noting that as Experiment 2 too few time periods, this AR(2) test cannot be performed with the data from this experiment.

[80]For example, if there were two types of workers, lazy and hard working, then even if the true ρ was 0 in the model above, because the individual heterogeneity is not being addressed, you would detect a very large $\hat{\rho}$.

Sul (2003). However, an important assumption of this early work is that the error terms are homoskedastic and i.i.d. normal.[81] In addition, these works do not allow for other exogenous regressors x_{it} aside from individual and time fixed effects. Under these assumptions, the mapping between $\hat{\rho}^{LSDV}$ from Least Squares Dummy Variable (LSDV) and the median unbiased $\hat{\rho}^{MU}$ does not depend on the individual fixed effects and can be obtained by Monte Carlo simulations.

Carree (2002) extends Andrews (1993) by allowing exogenous variables x_{it} to be included as well as individual fixed effects. This addition may be important as I have exogenous treatment variables (piece rates and leisure options) that could influence effort. As Carree (2002) proves, the Least Squares Dummy Variable estimator of ρ will still be biased downward, as in the original Nickell (1981) paper. One additional benefit of the Carree (2002) paper is that it provides closed form solutions for $T = 2$ and $T = 3$, which one of my experiments satisfies.[82] This enabled me to provide you some results below, but does not provide closed forms for the standard errors (which would be estimated by Monte Carlo simulations).

To reiterate, I applied the Carree (2002) results using the piece rate as the exogenous variable and number of problems solved (per 5 minutes) as the outcome variable, and differencing out the running sum to remove individual fixed effects:

$$\begin{aligned}
e_{it} &= \rho \cdot e_{it-1} + \mu_i + \beta x_{it} + \epsilon_{it} \\
\Rightarrow e_{it} - \bar{e}_{it} &= \rho \cdot (e_{it-1} - \bar{e}_{i,t-1}) + \beta \cdot (x_{it} - \bar{x}_{it}) + (\epsilon_{it} - \bar{\epsilon}_{it}) \\
\tilde{e}_{it} &= \rho \cdot \tilde{e}_{it-1} + \beta \cdot \tilde{x}_{it-1} + \tilde{\epsilon}_{it}
\end{aligned}$$

As in Nickell (1981) and proven in Carree (2002), the ρ estimated from the OLS of this specification is still biased downward. However, Carree provides a median unbiased estimator when $T = 3$ (as in my case). Specifically, the first step estimator would be:

$$\begin{aligned}
\hat{\rho}^{MUE} &= \frac{9\hat{\rho}^{OLS} + 2\hat{g}}{9 - \hat{g}} \qquad \text{(from equation 12b)} \\
\hat{g} &\equiv \frac{\hat{\sigma}^2_{\tilde{\epsilon}}}{(1 - \text{corr}_{\tilde{x},\tilde{y}_{t-1}}) \cdot \hat{\sigma}^2_{\tilde{y}_{t-1}}} \qquad \text{(from equation 10)}
\end{aligned}$$

When I constructed the above OLS regression differencing out running means using Experiment 2 Data, I received a $\hat{\rho}^{OLS} = 0.1052$. Once I use this method for correcting the bias, I receive a $\hat{\rho}^{MUE} = 0.4610$ which is very close to the Instrumental Variable estimates I find in the paper of 0.43 to 0.45. I provide more details below on how this estimate was constructed:

[81] These assumptions are relaxed in Phillips and Sul (2003) in an estimator called Panel Feasible Generalized Least Squares Median-Unbiased Estimators (PFGLSMUE). A less important difference is that they assume the AR(1) component is actually embedded in a latent variable:

$$\begin{aligned}
e_{it} &= \mu_i + e^*_{it} + \epsilon_{it} \\
e^*_{it} &= \rho e^*_{it-1} + \nu_{it}
\end{aligned}$$

However, this can still be mapped to my original model above by scaling $\mu^*_i = \mu_i/(1-\rho)$

[82] Although the paper only gets "nearly" unbiased asymptotic estimators for $T > 3$, for $T = 2$ and $T = 3$ the estimator is asymptotically exactly unbiased. However, the usual issues with sample size remains.

Term	Estimate	Origin
$\hat{\sigma}^2_{\tilde{\epsilon}}$	2.686	Estimated from residuals of OLS of differenced equation above
$\hat{\sigma}^2_{\tilde{y}_{t-1}}$	2.066	Estimated from the data of $\tilde{y}_{i,t-1}$
$\text{corr}_{\tilde{x},\tilde{y}_{t-1}}$	5.23×10^{-7}	Estimated from the data of $\tilde{x}_{i,t}$, $\tilde{y}_{i,t-1}$. Note this should be 0 (see details below).
$\hat{g}$	1.301	Transformation of above statistics using closed form solution
$\hat{\rho}^{OLS}$	0.1052	Estimated coefficient from OLS of differenced equation above (biased downward)
$\hat{\rho}^{MUE}$	0.4610	$= (9\hat{\rho}^{OLS} + 2\hat{g})/(9 - \hat{g})$ from Carree (2002), $T = 3$ case, equation 12b
$\hat{\rho}^{IV}$	0.43 to 0.45	From IV strategy, see Table 5

Note that $\text{corr}_{\tilde{x},\tilde{y}_{t-1}}$ is essentially 0. This is not a mistake or a sign of a weak regressor, but rather a sign that treatment was properly randomized. Because it represents the correlation between $\tilde{x}$ at time t and $\tilde{y}$ at time t-1, this is saying that last period's effort difference does not predict the piece rate treatment in the next period. This is to be expected as treatment was randomized, so last period's effort should not predict next period's piece rate treatment.

As Carree (2002) mentions, "An exogenous variable which is very highly correlated with the lagged endogenous variable and which provides little additional explanatory power will lead to worse bias." An exogenous variable x which is highly correlated with the lagged endogenous variable would result in a large $\text{corr}_{\tilde{x},\tilde{y}_{t-1}}$. As can be seen from the above equations, a large correlation would *increase* $\hat{g}$, increasing the bias. However a predictive x also helps lower $\sigma^2_{\tilde{\epsilon}}$, which reduces the bias. Thus, there is a potential trade off for including exogenous covariates. In my case, piece rate helps predict effort – the $\hat{\beta}$ from the above regression was 11.99 with a t-value of 5.49, very much in line with the Instrumental Variable results from the main specification – and does not correlate with past effort, so it helps reduce bias in both directions.

However, there are three issues to address with this estimation. First, although Carree (2002) mentions that the model tended to converge over multiple steps, this was not the case with my data or monte carlo studies I conducted using this estimator. Secondly, as with PEMU models, this model's bias correction relies on homoskedastic and i.i.d. normal errors. Third, as with other MUE bias correction, the estimator is only median unbiased asymptotically, as $N \to \infty$. While Monte Carlo results have explored the small sample properties of these estimators in some cases, this is still a potential concern.

10.6 Additional Literature Contributions

The experimental designs employed are also able to address whether higher *piece rates* induce reciprocity. While the role of reciprocity in labor markets is an area of active research (for review see Kessler, 2013; Levitt and Neckermann, 2014), especially for higher *wages,* I believe this is the first paper to test for reciprocity in *piece rates.* The answer is ex ante unclear because while a higher piece rate expands the budget set, the worker must still exert effort to receive the benefits. Most previous studies testing for reciprocity in labor markets employ flat hourly wage variation in a reputation free environment (Kube et al., 2012; Fehr et al., 2008; Englmaier and Leider, 2010, 2012; Kessler, 2013; Gneezy and List, 2006; Charness, 2004). As workers have arguably no financial incentive to work harder, evidence of greater effort is taken as evidence of reciprocity. Recent work such as Kube et al. (2012); Bradler and Neckermann (2015) suggests workers may reciprocate based on their impressions of employer intentions, rather than the actual "gift".[83] As outlined above, I find no evidence that workers engage in reciprocity from higher piece rates.

[83] However earlier work from Charness (2004) suggests that exogenously determined wages elicit almost as much reciprocation as employer designated wages.

I also address whether salience of information induces workers to engage in income targeting. In a real-effort laboratory experiment, Abeler et al. (2011) find workers exert more effort when facing a chance of a higher fixed payment. Pope and Schweitzer (2011) finds evidence of loss aversion in a high stakes labor market (professional sports). In these settings, the reference point is at least partially induced by the environment (i.e. the magnitude of the outside option in Abeler et al. (2011) and golf par score in Pope and Schweitzer (2011)), but there remains some uncertainty whether information about own performance can alter endogenously chosen reference points. To investigate this possibility, I vary whether the worker sees her total earnings or her past period earnings and find it has little to no effect on effort allocation.

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11 Additional Robustness Checks (For Online Publication)

Table 2A. Randomization Check – Experiment 1

Dependent Variable	Period # for Piece Rate Treatment		Period # for Phone Treatment	
Female	−0.09	0.19	0.06	0.03
	(0.29)	(0.32)	(0.30)	(0.32)
SAT Math Score ('00s of points)		−0.002		−0.001
		(0.002)		(0.26)
PreTreatment Problems Solved		−0.047		0.011
		(0.058)		(0.048)
F-test	0.10	1.24	0.05	0.15
p value	0.75	0.30	0.83	0.93
Dependent Variable Mean	3.34	3.41	3.48	3.54
Number of Observations	930	738	930	738
Number of Individuals	155	123	155	123
Adj-R^2	0.001	0.018	0.001	0.005

Notes: Standard Errors (clustered at individual level) presented in parentheses above. As every subject in experiment 1 receives all treatments at some point, the dependent variable is the period in which they received the treatment in question. If randomization was done properly, the pre-treatment variables should not predict the period they received the treatment. Indeed, the F-stats are all large enough that I fail to reject the hypothesis that all coefficients are zero under $\alpha = 0.05$. Thus, I conclude the randomization was adequately done. SAT Math score is missing for 32 individuals who either took the ACT or otherwise did not wish to share that information with researchers.

Table 2B. Randomization Check – Experiment 2

Variable	Baseline	Piece Rate Decrease	Piece Rate Increase	Phone Access	
Female	0.68	0.71	0.74	0.71	$p < 0.76$
	(0.47)	(0.46)	(0.44)	(0.45)	(F-test = 0.39)
Age	20.26	20.13	20.73	20.39	$p < 0.11$
	(1.75)	(1.61)	(2.44)	(1.48)	(F-test = 2.05)
# Previous Studies at Lab	25.18	24.1	26.23	23.64	$p < 0.22$
	(23.9)	(20.5)	(34.5)	(18.7)	(F-test = 0.88)
Computer Skill Test	2.01	2.01	2.02	2.00	$p < 0.56$
	(0.01)	(0.09)	(0.14)	(no variation)	(F-test = 0.56)
Problems Solved in PreTreatment	20.57	20.23	19.54	20.23	$p < 0.80$
	(7.76)	(8.2)	(7.02)	(7.22)	(F-test = 0.33)
Number of Subjects Treated	103	114	104	101	

Notes: As every subject in experiment 2 receives (at most) one primary treatment, the subjects are split according to primary treatment. Means and standard deviations (in parentheses) are presented by primary treatment. If randomization was done properly, the pre-treatment variables should not differ significantly according to which treatment was received. Indeed, for all rows the F-stat corresponds to a p greater than 0.05 (fail to reject the hypothesis that all coefficients are less than zero under $\alpha = 0.05$).

Table 2C. Randomization Check – Experiment 3

Variable	Baseline	Piece Rate Decrease	Piece Rate Increase	
Female	0.45	0.55	0.45	$p < 0.46$
	(0.47)	(0.46)	(0.44)	(F-test = 0.78)
Age	20.42	20.63	20.24	$p < 0.64$
				(F-test = 0.45)
# Previous Studies at Lab	35	36.1	39.4	$p < 0.72$
				(F-test = 0.33)
Computer Skill Test	2.01	2	2	$p < 0.32$
	(0.01)	(0.09)	(0.14)	(F-test = 1)
Problems Solved in PreTreatment	102.6	98.4	109.4	$p < 0.22$
				(F-test = 1.52)
Number of Subjects Treated	73	60	51	

Notes: As every subject in experiment 3 receives (at most) one primary treatment, the subjects are split according to primary treatment. Means and standard deviations (in parentheses) are presented by primary treatment. If randomization was done properly, the pre-treatment variables should not differ significantly according to which treatment was received. Indeed, for all rows the F-stat corresponds to a p greater than 0.05 (fail to reject the hypothesis that all coefficients are less than zero under $\alpha = 0.05$).

Table 3A. Contemporaneous Piece Rate and Phone Access: Impact on Effort – Experiment 1

$$Problems_{i,t} \quad = \quad \alpha \cdot Piece\ Rate_{i,t} + \beta \cdot Phone\ Access_{i,t} + \gamma X_i + \epsilon_{i,t}$$

Dependent Variable:			Specification		
Problems Solved	(1)	(2)	(3)	(4)	(5)
Piece Rate	4.34***	4.62***	4.62***	2.07*	2.07*
(in cents per problem)	(1.17)	(1.15)	(1.14)	(1.09)	(1.09)
Phone Access	−0.38**	−0.37**	−0.39**	−0.46**	−0.46**
	(0.19)	(0.19)	(0.19)	(0.19)	(0.19)
PreTreatment Quintiles		Yes	Yes	Yes	Yes
Period Fixed Effects			Yes	Yes	Yes
Session Fixed Effects				Yes	Yes
Individual Controls					Yes
Dependent Variable Mean	7.85	7.85	7.85	7.85	7.85
Number of Observations	930	930	930	930	930
Number of Individuals	155	155	155	155	155
Adj-R^2	0.01	0.23	0.23	0.31	0.32

Notes: The dependent variable is the number of problems solved correctly in a single period. All specifications report results from OLS regressions and also include a constant term. PreTreatment Quintiles represent five binary variables to non-parametrically control for the number of problems subject solved in the pre-treatment training period. Individual Controls include sex, age, ethnicity bins, number of sessions done, and WBL computer diagnostic scores. Standard errors are given in parentheses and clustered at the subject (individual) level. $* = p < 0.1$, $** = p < 0.05$, $*** = p < 0.01$.

Table 3B. Contemporaneous Piece Rate and Phone Access: Impact on Effort – Experiment 2

$$Problems_{i,t} \quad = \quad \alpha \cdot Piece\,Rate_{i,t} + \beta \cdot Phone\,Access_{i,t} + \gamma X_i + \epsilon_{i,t}$$

Dependent Variable:	Specification				
Problems Solved	(1)	(2)	(3)	(4)	(5)
Piece Rate	12.5***	14.9***	16.6***	16.7***	16.7***
(in cents per problem)	(3.39)	(2.67)	(3.02)	(2.98)	(2.98)
Phone Access	−0.05	−0.10	0.12	0.15	0.17
	(0.33)	(0.25)	(0.32)	(0.32)	(0.32)
PreTreatment Quintiles		Yes	Yes	Yes	Yes
Period Fixed Effects			Yes	Yes	Yes
Session Fixed Effects				Yes	Yes
Individual Controls					Yes
Dependent Variable Mean	7.23	7.23	7.23	7.23	7.23
Number of Observations	1266	1266	1266	1266	1260
Number of Individuals	422	422	422	422	420
Adj-R^2	0.01	0.41	0.41	0.43	0.45

Notes: The dependent variable is the number of problems solved correctly in a period. All specifications report results from OLS regressions and also include a constant term. PreTreatment Quintiles represent five binary variables to non-parametrically control for the number of problems subject solved in the pre-treatment training period. Individual Controls include sex, age, ethnicity bins, number of sessions done, and WBL computer diagnostic scores, but could not be matched for 2 subjects. Standard errors given in parentheses and clustered at the subject (individual) level. $* = p < 0.1$, $** = p < 0.05$, $*** = p < 0.01$.

Table 3C. Contemporaneous Piece Rate: Impact on Effort – Experiment 3

$$Problems_{i,t} \quad = \quad \alpha \cdot Piece\,Rate_{i,t} + \gamma X_i + \epsilon_{i,t}$$

Dependent Variable:	Specification				
Problems Solved	(1)	(2)	(3)	(4)	(5)
Piece Rate	41.45**	62.34***	73.12***	66.26***	63.67***
(cents per 100 problems)	(16.84)	(14.21)	(16.36)	(16.35)	(16.26)
PreTreatment Quintiles		Yes	Yes	Yes	Yes
Period Fixed Effects			Yes	Yes	Yes
Session Fixed Effects				Yes	Yes
Individual Controls					Yes
Dependent Variable Mean	63	63	63	63	63
Number of Observations	736	736	736	736	732
Number of Individuals	184	184	184	184	183
Adj-R^2	0.01	0.39	0.39	0.45	0.47

Notes: The dependent variable is the number of problems solved correctly in a period. All specifications report results from OLS regressions and also include a constant term. PreTreatment Quintiles represent five binary variables to non-parametrically control for the number of problems subject solved in the pre-treatment training period. Individual Controls include sex, age, ethnicity bins, number of sessions done, and WBL computer diagnostic scores, but could not be matched for 2 subjects. Standard errors given in parentheses and clustered at the subject (individual) level. $* = p < 0.1$, $** = p < 0.05$, $*** = p < 0.01$.

Table 4A. Previous Period Piece Rate and Phone Access: Impact on Effort – Experiment 1

$$Problems_{i,t} \quad = \quad \alpha_1 \cdot Piece\,Rate_{i,t} + \alpha_2 \cdot Piece\,Rate_{i,t-1} + \gamma X_i + \epsilon_{i,t}$$

Dependent Variable:	Specification					
Problems Solved	(1)	(2)	(3)	(4)	(5)	(6)
Piece Rate	5.00***	5.34***	2.19*	8.06***	7.74***	5.43***
(cents per problem)	(1.29)	(1.24)	(1.14)	(2.48)	(1.94)	(1.87)
Previous Period's Piece Rate	3.42*	3.86**	0.45	7.99**	8.04***	5.91**
(cents per problem)	(1.74)	(1.56)	(1.36)	(3.59)	(2.93)	(2.78)
Phone Access	−0.26	−0.25	−0.43**	−0.29	−0.30	−0.38
	(0.21)	(0.21)	(0.20)	(0.36)	(0.32)	(0.30)
Previous Period Phone Access	0.24	0.28	0.07	0.22	0.18	0.21
	(0.26)	(0.24)	(0.23)	(0.41)	(0.35)	(0.34)
PreTreatment Quintiles		Yes	Yes		Yes	Yes
Period Fixed Effects		Yes	Yes		Yes	Yes
Session Fixed Effects			Yes			Yes
Individual Controls			Yes			Yes
Periods 1 to 3 Only				Yes	Yes	Yes
Dependent Variable Mean	7.85	7.85	7.85	7.79	7.79	7.79
Number of Observations	930	930	930	465	465	465
Number of Individuals	155	155	155	155	155	155
Adj-R^2	0.01	0.24	0.32	0.04	0.34	0.39

Notes: The dependent variable is the number of problems solved correctly in a single period. All specifications report results from OLS regressions and also include a constant term. PreTreatment Quintiles represent five binary variables to non-parametrically control for the number of problems subject solved in the pre-treatment training period. "Periods 1 to 3" uses data of the first treatment period and following period to minimize treatment interactions. Individual Controls include sex, age, ethnicity bins, number of sessions done, and WBL computer diagnostic scores. Standard errors are given in parentheses and clustered at the subject (individual) level. $* = p < 0.1$, $** = p < 0.05$, $*** = p < 0.01$.

Table 4B. Previous Period Piece Rate and Phone Access: Impact on Effort – Experiment 2

$$Problems_{i,t} \quad = \quad \alpha_1 \cdot Piece\,Rate_{i,t} + \alpha_2 \cdot Piece\,Rate_{i,t-1} + \gamma X_i + \epsilon_{i,t}$$

Dependent Variable:			Specification		
Problems Solved	(1)	(2)	(3)	(4)	(5)
Piece Rate	12.66***	15.09***	16.57***	16.97***	17.09***
(cents per problem)	(3.51)	(2.74)	(3.03)	(3.05)	(3.11)
Previous Period's Piece Rate	4.09	6.51**	7.24**	7.64**	7.87**
(cents per problem)	(4.03)	(3.11)	(3.32)	(3.35)	(3.40)
Phone Access	−0.03	−0.08	0.12	0.16	0.18
	(0.36)	(0.28)	(0.32)	(0.32)	(0.33)
Previous Period Phone Access	−0.03	−0.08	0.02	0.06	0.07
	(0.41)	(0.33)	(0.36)	(0.36)	(0.36)
PreTreatment Quintiles		Yes	Yes	Yes	Yes
Period Fixed Effects			Yes	Yes	Yes
Session Fixed Effects				Yes	Yes
Individual Controls					Yes
Dependent Variable Mean	7.23	7.23	7.23	7.23	7.23
Number of Observations	1266	1266	1266	1266	1260
Number of Individuals	422	422	422	422	420
Adj-R^2	0.01	0.41	0.41	0.44	0.45

Notes: The dependent variable is the number of problems solved correctly in a period. All specifications report results from OLS regressions and also include a constant term. PreTreatment Quintiles represent five binary variables to non-parametrically control for the number of problems subject solved in the pre-treatment training period. Individual Controls include age, sex, ethnicity, computer skill test, and total # of experimental sessions done at the lab, but could not be matched for 2 subjects. Standard errors given in parentheses and clustered at the subject (individual) level. $* = p < 0.1$, $** = p < 0.05$, $*** = p < 0.01$.

Table 4C. Previous Period Piece Rate and Phone Access: Impact on Effort – Experiment 3

$$Problems_{i,t} \quad = \quad \alpha_1 \cdot Piece\,Rate_{i,t} + \alpha_2 \cdot Piece\,Rate_{i,t-1} + \gamma X_i + \epsilon_{i,t}$$

Dependent Variable:	Specification				
Problems Solved	(1)	(2)	(3)	(4)	(5)
Piece Rate	41.87***	64.08***	73.63***	67.16***	64.90***
(cents per 100 problems)	(17.52)	(14.80)	(16.54)	(16.79)	(16.87)
Previous Period's Piece Rate	10.68	32.89**	34.91*	28.45	27.22
(cents per 100 problems)	(20.16)	(16.47)	(18.09)	(18.29)	(18.01)
PreTreatment Quintiles		Yes	Yes	Yes	Yes
Period Fixed Effects			Yes	Yes	Yes
Session Fixed Effects				Yes	Yes
Individual Controls					Yes
Dependent Variable Mean	63	63	63	63	63
Number of Observations	736	736	736	736	736
Number of Individuals	184	184	184	184	184
Adj-R^2	0.01	0.39	0.40	0.45	0.47

Notes: The dependent variable is the number of problems solved correctly in a period. All specifications report results from OLS regressions and also include a constant term. PreTreatment Quintiles represent five binary variables to non-parametrically control for the number of problems subject solved in the pre-treatment training period. Individual Controls include age, sex, ethnicity, computer skill test, and total # of experimental sessions done at the lab, but could not be matched for 2 subjects. Standard errors given in parentheses and clustered at the subject (individual) level. $* = p < 0.1$, $** = p < 0.05$, $*** = p < 0.01$.

Table 5A. Phone Access by Gender: Impact on Effort – Experiment 1

$$Problems_{i,t} = \alpha_1 \cdot Phone_{it} \cdot Female_i + \alpha_2 \cdot Phone_{it-1} \cdot Female_i + \beta_1 \cdot Phone_{it} \cdot Male_i + \beta_2 \cdot Phone_{it-1} \cdot Male_i + \gamma X_i + \epsilon_{i,t}$$

Dependent Variable:	Specification					
Problems Solved	(1)	(2)	(3)	(4)	(5)	(6)
Phone Access * Female	−0.26	−0.28	−0.29	0.08	−0.18	−0.02
	(0.22)	(0.22)	(0.22)	(0.41)	(0.35)	(0.32)
Previous Period Phone * Female	0.35	0.32	0.29	0.12	−0.39	−0.08
	(0.25)	(0.24)	(0.24)	(0.45)	(0.40)	(0.38)
Phone Access * Male	−1.10***	−1.10***	−1.11***	−2.33***	−1.78**	−2.02***
	(0.39)	(0.41)	(0.41)	(0.77)	(0.73)	(0.72)
Previous Period Phone * Male	−0.69	−0.59	−0.65	−0.42	0.42	0.18
	(0.57)	(0.55)	(0.55)	(1.05)	(0.69)	(0.71)
Male	−1.02*	−0.68	−0.10	−0.36	−0.28	0.24
	(0.58)	(0.58)	(0.56)	(0.61)	(0.50)	(0.54)
Pre-Treatment Quintiles		Yes	Yes		Yes	Yes
Period Fixed Effects		Yes	Yes		Yes	Yes
Session Fixed Effects			Yes			Yes
Individual Controls			Yes			Yes
Periods 1 to 3 Only				Yes	Yes	Yes
Dependent Variable Mean	7.85	7.85	7.85	7.79	7.79	7.79
Number of Observations	930	930	930	465	465	465
Number of Individuals	155	155	155	155	155	155
Adj-R^2	0.03	0.24	0.33	0.03	0.32	0.39

Notes: The dependent variable is the number of problems solved correctly in a single period. All specifications report results from OLS regressions and also include a constant term. PreTreatment Quintiles represent five binary variables to non-parametrically control for the number of problems subject solved in the pre-treatment training period. "Periods 1 to 3" uses data of the first treatment period and following period to minimize treatment interactions. Individual Controls include sex, age, ethnicity bins, number of sessions done, and WBL computer diagnostic scores. Standard errors are given in parentheses and clustered at the subject (individual) level. $* = p < 0.1$, $** = p < 0.05$, $*** = p < 0.01$.

Table 5B. Phone Access by Gender: Impact on Effort – Experiment 2

$$Problems_{i,t} \quad = \quad \alpha_1 \cdot Phone_{it} \cdot Female_i + \alpha_2 \cdot Phone_{it-1} \cdot Female_i + \beta_1 \cdot Phone_{it} \cdot Male_i + \beta_2 \cdot Phone_{it-1} \cdot Male_i + \gamma X_i + \epsilon_{i,t}$$

Dependent Variable:	Specification				
Problems Solved	(1)	(2)	(3)	(4)	(5)
Phone Access * Female	0.51	0.36	0.39	0.44	0.45
	(0.36)	(0.29)	(0.32)	(0.34)	(0.35)
Previous Period Phone * Female	0.48	0.34	0.42	0.47	0.48
	(0.44)	(0.36)	(0.39)	(0.40)	(0.40)
Phone Access * Male	−1.68**	−1.59***	−1.57***	−1.61***	−1.64***
	(0.82)	(0.51)	(0.54)	(0.51)	(0.53)
Previous Period Phone * Male	−1.61*	−1.52**	−1.43**	−1.48**	−1.51**
	(0.88)	(0.61)	(0.63)	(0.61)	(0.61)
Male	−0.45	−0.48	−0.48	−0.51*	−0.53*
	(0.36)	(0.27)	(0.27)	(0.28)	(0.28)
Pre-Treatment Quintiles		Yes	Yes	Yes	Yes
Period Fixed Effects			Yes	Yes	Yes
Session Fixed Effects				Yes	Yes
Individual Controls					Yes
Dependent Variable Mean	7.23	7.23	7.23	7.23	7.23
Number of Observations	1263	1263	1263	1263	1260
Number of Individuals	421	421	421	421	420
Adj-R^2	0.02	0.42	0.42	0.44	0.44

Notes: The dependent variable is the number of problems solved correctly in a period. All specifications report results from OLS regressions and also include a constant term. PreTreatment Quintiles represent five binary variables to non-parametrically control for the number of problems subject solved in the pre-treatment training period. Individual Controls include age, ethnicity, computer skill test, and total # of experimental sessions done at the lab. Gender could not be matched for one subject, and the controls for an additional subject. Standard errors given in parentheses and clustered at the subject (individual) level. $* = p < 0.1$, $** = p < 0.05$, $*** = p < 0.01$.

Table 6A. Next Period Piece Rate: Impact on Effort – Experiment 1

$$Problems_{i,t} \quad = \quad \alpha_1 \cdot Piece\,Rate_{i,t} + \alpha_2 \cdot Piece\,Rate_{i,t+1} \cdot Knowledge_{i,t} + \alpha_3 \cdot Piece\,Rate_{i,t-1} + \gamma X_i + \epsilon_{i,t}$$

Dependent Variable:	Specification					
Problems Solved	(1)	(2)	(3)	(4)	(5)	(6)
Piece Rate	5.33***	5.75***	2.78**	8.74***	8.62***	6.46***
(cents per problem)	(1.26)	(1.23)	(1.20)	(2.58)	(1.98)	(1.95)
Next Period Piece Rate	1.80	2.65	0.57	3.63*	5.01**	4.20*
(if known)	(1.73)	(2.23)	(2.35)	(2.01)	(2.14)	(2.47)
Previous Period Piece Rate	3.64**	3.90***	0.84	8.75**	8.49***	6.53**
(cents per problem)	(1.62)	(1.45)	(1.33)	(3.73)	(2.97)	(1.62)
Pre-Treatment Quintiles		Yes	Yes		Yes	Yes
Period Fixed Effects		Yes	Yes		Yes	Yes
Shown Next Period Binary		Yes	Yes		Yes	Yes
Session Fixed Effects			Yes			Yes
Individual Controls			Yes			Yes
Periods 1 to 3 Only				Yes	Yes	Yes
Dependent Variable Mean	7.85	7.85	7.85	7.79	7.79	7.79
Number of Observations	930	930	930	465	465	465
Number of Individuals	155	155	155	155	155	155
Adj-R^2	0.01	0.23	0.32	0.04	0.34	0.39

Notes: The dependent variable is the number of problems solved correctly in a single period. All specifications report results from OLS regressions and also include a constant term. PreTreatment Quintiles represent five binary variables to non-parametrically control for the number of problems subject solved in the pre-treatment training period. "Only Periods 1 to 3" uses data of the first treatment period and following period to minimize treatment interactions. Individual Controls include age, sex, ethnicity, computer skill test, and total # of experimental sessions done at the lab. Standard errors are given in parentheses and clustered at the subject (individual) level. $* = p < 0.1$, $** = p < 0.05$, $*** = p < 0.01$.

Table 6B. Next Period Piece Rate: Impact on Effort – Experiment 2

$$Problems_{i,t} \quad = \quad \alpha_1 \cdot Piece\,Rate_{i,t} + \alpha_2 \cdot Piece\,Rate_{i,t+1} \cdot Knowledge_{i,t} + \alpha_3 \cdot Piece\,Rate_{i,t-1} + \gamma X_i + \epsilon_{i,t}$$

Dependent Variable:	Specification				
Problems Solved	(1)	(2)	(3)	(4)	(5)
Piece Rate	12.8***	15.5***	16.3***	16.65***	16.76***
(cents per problem)	(3.58)	(2.79)	(2.92)	(2.96)	(3.04)
Next Period Piece Rate	1.09	3.23	0.73	1.59	2.63
(if known)	(2.64)	(2.19)	(2.62)	(3.92)	(3.88)
Previous Piece Rate	4.21	6.88**	7.20**	7.52**	7.75**
(cents per problem)	(4.12)	(3.17)	(3.27)	(3.31)	(3.38)
Pre-Treatment Quintiles		Yes	Yes	Yes	Yes
Period Fixed Effects			Yes	Yes	Yes
Shown Next Period Binary				Yes	Yes
Session Fixed Effects				Yes	Yes
Individual Controls					Yes
Dependent Variable Mean	7.23	7.23	7.23	7.23	7.23
Number of Observations	1266	1266	1266	1266	1260
Number of Individuals	422	422	422	422	420
Adj-R^2	0.01	0.41	0.41	0.44	0.44

Notes: The dependent variable is the number of problems solved correctly in a period. All specifications report results from OLS regressions and also include a constant term. PreTreatment Quintiles represent five binary variables to non-parametrically control for the number of problems subject solved in the pre-treatment training period. Individual Controls include age, sex, ethnicity, computer skill test, and total # of experimental sessions done at the lab, but could not be matched for 2 subjects. Standard errors given in parentheses and clustered at the subject (individual) level. $* = p < 0.1$, $** = p < 0.05$, $*** = p < 0.01$.

Table 6C. Next Period Piece Rate: Impact on Effort – Experiment 3

$$Problems_{i,t} \quad = \quad \alpha_1 \cdot Piece\,Rate_{i,t} + \alpha_2 \cdot Piece\,Rate_{i,t+1} \cdot Knowledge_{i,t} + \alpha_3 \cdot Piece\,Rate_{i,t-1} + \gamma X_i + \epsilon_{i,t}$$

Dependent Variable:	Specification				
Problems Solved	(1)	(2)	(3)	(4)	(5)
Piece Rate	42.31***	65.91***	73.73***	67.34***	65.29***
(cents per 100 problems)	(17.96)	(15.23)	(16.58)	(16.96)	(17.14)
Next Period Piece Rate	6.86	24.59**	10.08	22.59	22.28
(if known)	(13.61)	(10.71)	(12.32)	(17.39)	(17.64)
Previous Piece Rate	11.11	34.72**	35.02*	28.64	27.60
(cents per 100 problems)	(20.57)	(16.83)	(18.14)	(18.44)	(18.29)
Pre-Treatment Quintiles		Yes	Yes	Yes	Yes
Period Fixed Effects			Yes	Yes	Yes
Shown Next Period Binary				Yes	Yes
Session Fixed Effects				Yes	Yes
Individual Controls					Yes
Dependent Variable Mean	63	63	63	63	63
Number of Observations	736	736	736	736	732
Number of Individuals	184	184	184	184	183
Adj-R^2	0.01	0.40	0.40	0.45	0.47

Notes: The dependent variable is the number of problems solved correctly in a period. All specifications report results from OLS regressions and also include a constant term. PreTreatment Quintiles represent five binary variables to non-parametrically control for the number of problems subject solved in the pre-treatment training period. Individual Controls include age, sex, ethnicity, computer skill test, and total # of experimental sessions done at the lab, but could not be matched for 2 subjects. Standard errors given in parentheses and clustered at the subject (individual) level. $* = p < 0.1$, $** = p < 0.05$, $*** = p < 0.01$.

Table 7A. Previous Effort Instrumental Variable: Impact on Effort – Experiment 2

$$
\begin{aligned}
Problems_{i,t-1} &= \alpha_1 \cdot Piece\,Rate_{i,t-1} + \beta_1 \cdot Phone_{i,t-1} + \gamma X_i + \epsilon_{i,t} \\
Problems_{i,t} &= \rho \cdot Problems_{i,t-1} + \alpha_2 \cdot Piece\,Rate_{i,t} + \beta_2 \cdot Phone_{i,t} + \zeta X_i + \nu_{i,t}
\end{aligned}
$$

Dependent Variable:			Specification		
Problems Solved	(1)	(2)	(3)	(4)	(5)
Problems Previous Period	0.39	0.50***	0.45***	0.44***	0.43***
	(0.26)	(0.18)	(0.17)	(0.17)	(0.17)
Piece Rate	12.7***	13.9***	15.3***	15.2***	14.9***
(cents per problem)	(2.63)	(2.51)	(2.73)	(2.81)	(2.88)
Phone Access	0.04	0.07	0.24	0.25	0.23
	(0.24)	(0.23)	(0.27)	(0.27)	(0.27)
First Stage F Stat (IV)	6.1	14.5	15.5	16.5	16.2
PreTreatment Quintiles		Yes	Yes	Yes	Yes
Period Fixed Effects			Yes	Yes	Yes
Session Fixed Effects				Yes	Yes
Individual Controls					Yes
Dependent Variable Mean	7.22	7.22	7.22	7.22	7.22
Number of Observations	844	844	844	844	840
Number of Individuals	422	422	422	422	420
Adj-R^2	0.39	0.56	0.57	0.57	0.57

Notes: The dependent variable is the number of problems solved correctly in a single period. All specifications report results from linear Instrumental Variable regressions estimated by (iterative) GMM and also include a constant term. PreTreatment Quintiles represent five binary variables to non-parametrically control for the number of problems subject solved in the pre-treatment training period. Individual Controls include age, sex, ethnicity, computer skill test, and total # of experimental sessions done at the lab, but could not be matched for 2 subjects. Standard errors are given in parentheses and clustered at the subject (individual) level. $* = p < 0.1$, $** = p < 0.05$, $*** = p < 0.01$.

Table 7B. Previous Effort Instrumental Variable: Impact on Effort – Experiment 3

$$\begin{aligned} Problems_{i,t-1} &= \alpha_1 \cdot Piece\,Rate_{i,t-1} + \gamma X_i + \epsilon_{i,t} \\ Problems_{i,t} &= \rho \cdot Problems_{i,t-1} + \alpha_2 \cdot Piece\,Rate_{i,t} + \zeta X_i + \nu_{i,t} \end{aligned}$$

Dependent Variable:			Specification		
Problems Solved	(1)	(2)	(3)	(4)	(5)
Problems Previous Period	0.35	0.57***	0.46**	0.42**	0.42**
	(0.39)	(0.19)	(0.18)	(0.21)	(0.21)
Piece Rate	41.73***	48.27***	60.26***	58.21***	57.51***
(cents per 100 problems)	(11.80)	(12.17)	(13.39)	(13.51)	(13.72)
First Stage F Stat (IV)	5.3	18.8	20.2	16.6	15.1
PreTreatment Quintiles		Yes	Yes	Yes	Yes
Period Fixed Effects			Yes	Yes	Yes
Session Fixed Effects				Yes	Yes
Individual Controls					Yes
Dependent Variable Mean	63	63	63	63	63
Number of Observations	552	552	552	552	549
Number of Individuals	184	184	184	184	183
Adj-R^2	0.43	0.66	0.63	0.64	0.65

Notes: The dependent variable is the number of problems solved correctly in a single period. All specifications report results from linear Instrumental Variable regressions estimated by (iterative) GMM and also include a constant term, except for specification 5 which could not converge and limited information maximum likelihood was employed. PreTreatment Quintiles represent five binary variables to non-parametrically control for the number of problems subject solved in the pre-treatment training period. Individual Controls include age, sex, ethnicity, computer skill test, and total # of experimental sessions done at the lab, but could not be matched for 2 subjects. Standard errors are given in parentheses and clustered at the subject (individual) level. $* = p < 0.1$, $** = p < 0.05$, $*** = p < 0.01$.

Table 8A. Period or Total Earnings Salience: Impact on Earnings – Experiment 2

$$Earnings_{i,t} \quad = \quad \alpha_1 \cdot Piece\,Rate_{i,t} + \alpha_2 Piece\,Rate_{i,t} \cdot PS_i + \beta_1 \cdot Phone_{i,t} + \beta_2 Phone_{i,t} \cdot PS_i + \gamma Period\,Shown_i + \gamma X_i + \epsilon_{i,t}$$

Dependent Variable:			Specification		
Problems Solved	(1)	(2)	(3)	(4)	(5)
Piece Rate	9.32*	13.12***	17.05***	15.84***	15.68***
	(5.27)	(3.83)	(4.17)	(3.87)	(3.88)
Piece Rate * Period Salience	2.41	−3.82	-2.86	−1.01	−1.00
	(6.93)	(5.21)	(5.33)	(5.11)	(5.13)
Phone Access	0.92*	0.62*	0.94**	0.92**	0.88**
	(0.50)	(0.35)	(0.41)	(0.46)	(0.46)
Phone Access * Period Salience	−2.03***	−1.20**	−1.21**	−1.23**	−1.15**
	(0.73)	(0.53)	(0.54)	(0.56)	(0.54)
Period Salience	-0.32	0.34	0.40	0.22	0.25
	(0.58)	(0.40)	(0.39)	(0.39)	(0.39)
Pre-Treatment Quintiles		Yes	Yes	Yes	Yes
Period Fixed Effects			Yes	Yes	Yes
Session Fixed Effects				Yes	Yes
Individual Controls					Yes
Dependent Variable Mean	7.28	7.28	7.28	7.28	7.28
Number of Observations	894	894	894	894	891
Number of Individuals	298	298	298	298	297
Adj-R^2	0.02	0.45	0.48	0.51	0.52

Notes: The dependent variable is the number of problems solved in a single period. All specifications report results from OLS regressions and also include a constant term. The subject is shown either the previous period's earnings (as indicated by "Period Salience") or shown total earnings up to that period at the bottom of the page. Experiments 2 and 3 were the only experiments that featured this variation. Unfortunately, while every subject in Experiment 2 did face a randomized period or total counter, a small programming typo prevented the capture of this variable for the first day. As it is unclear which counter subjects faced on the first day, they are dropped from analysis above. PreTreatment Quintiles represent five binary variables to non-parametrically control for the number of problems subject solved in the pre-treatment training period. Individual Controls include age, sex, ethnicity, computer skill test, and total # of experimental sessions done at the lab, but could not be matched for one subject. Standard errors given in parentheses and clustered at the subject (individual) level. $* = p < 0.1$, $** = p < 0.05$, $*** = p < 0.01$.

Table 8B. Period or Total Earnings Salience: Impact on Earnings – Experiment 3

$$Earnings_{i,t} \quad = \quad \alpha_1 \cdot Piece\, Rate_{i,t} + \alpha_2 Piece\, Rate_{i,t} \cdot PS_{i,t} + \gamma Period\, Shown_i + \gamma X_i + \epsilon_{i,t}$$

Dependent Variable:	Specification				
Problems Solved	(1)	(2)	(3)	(4)	(5)
Piece Rate	7171.9***	8738.8***	10220.7***	9816.4***	9627.3***
(in cents per problem)	(2304.0)	(2183.2)	(2386.0)	(2369.4)	(2354.9)
Piece Rate * Period Salience	−5495.7	−4481.6	−5040.7*	−5791.4**	−5970.8**
(interaction)	(3360.0)	(2780.6)	(2784.2)	(2665.0)	(2741.5)
Period Salience	9.67	8.52*	9.25*	9.27**	10.22**
(binary variable)	(6.23)	(4.91)	(4.91)	(4.47)	(4.44)
Pre-Treatment Quintiles		Yes	Yes	Yes	Yes
Period Fixed Effects			Yes	Yes	Yes
Session Fixed Effects				Yes	Yes
Individual Controls					Yes
Dependent Variable Mean	63	63	63	63	63
Number of Observations	736	736	736	736	732
Number of Individuals	184	184	184	184	183
Adj-R^2	0.01	0.39	0.40	0.45	0.47

Notes: The dependent variable is the number of problems solved in a single period. All specifications report results from OLS regressions and also include a constant term. The subject is shown either the previous period's earnings (as indicated by "Period Salience") or shown total earnings up to that period at the bottom of the page. Experiments 2 and 3 were the only experiments that featured this variation. PreTreatment Quintiles represent five binary variables to non-parametrically control for the number of problems subject solved in the pre-treatment training period. Individual Controls include age, sex, ethnicity, computer skill test, and total # of experimental sessions done at the lab, but could not be matched for one subject. Standard errors given in parentheses and clustered at the subject (individual) level.
$* = p < 0.1$, $** = p < 0.05$, $*** = p < 0.01$.