CERGE
Center for Economic Research and Graduate Education
Charles University Prague

The Impact of Financial Incentives on Task Performance: The Role of Cognitive Abilities and Intrinsic Motivation

Ondřej Rydval

DISSERTATION

Dissertation Committee: Andreas Ortmann (Chair), Jan Hanousek and Štěpán Jurajda, CERGE-EI, and Nathaniel T. Wilcox, University of Houston

Referees: Colin F. Camerer, California Institute of Technology, and Glenn W. Harrison, University of Central Florida

Prague, May 2007

TABLE OF CONTENTS

DISSERTATION ABSTRACT

Economists widely believe that, absent strategic considerations such as agency problems, financial incentives represent a dominant and effective stimulator of human productive activities. In production settings that are cognitively demanding, however, the effectiveness of financial incentives may be moderated by individual cognitive abilities and motivational characteristics. As a useful metaphor for the moderating channels, Camerer and Hogarth (1999) propose an informal capital-labor-production (KLP) framework, describing how financial incentives may interact in non-trivial ways with intrinsic motivation in stimulating cognitive effort (labor), and how the productivity of cognitive effort may in turn vary across individuals due to their different cognitive abilities (capital). Even if strong financial incentives induce high effort, both financial and cognitive resources may be wasted for individuals whose cognitive constraints inhibit performance improvements. This prediction, if warranted, calls for attention to individual cognitive abilities in designing efficient incentive schemes in firms, experimental settings and elsewhere.

This dissertation examines how financial incentives interact with intrinsic motivation and especially cognitive abilities in determining cognitive performance. In Rydval (2003), I present an initial review of the literature, theoretical issues and outstanding questions pertaining to the KLP framework. I document how the KLP framework has been addressed in economics, psychology and other fields, especially noting the lack of empirical evidence on the interaction between financial incentives and cognitive abilities. Building on the review, Chapter 1 of the dissertation illustrates that *general* cognitive abilities appear at least as important for performance in a psychometric test as does a sizeable variation in piece-rate financial incentives.

Chapter 2 focuses on the interaction between financial incentives and *task-specific*, as opposed to domain-general, forms of cognitive capital, the role of which is central to the KLP framework of Camerer and Hogarth (1999) and has long been studied in cognitive science and behavioral decision research. Using a task situated in an accounting setting, I show that both financial incentives and task-specific cognitive capital, and especially their interaction, matter for performance. In particular, the effect of task-specific cognitive

capital – proxied by accounting knowledge – on performance is stronger under performance-based financial incentives as compared to flat-rate incentives. The interaction effect arises because performance-based financial incentives lead to better performance only for individuals with more task-specific cognitive capital.

Chapters 1 and 2 chronologically precede Chapter 3. They both revisit previously collected experimental datasets that have certain deficiencies. Neither of the datasets offers sufficient information on individual characteristics that would permit accounting for potentially important sample composition differences. The dataset used in Chapter 1 further offers only an endogenous measure of general cognitive abilities with respect to performance in the psychometric test. The results presented in Chapters 1 and 2 should therefore be viewed mainly as illustrations motivating the much more thorough analysis in Chapter 3, which makes the core contribution of the dissertation.

In Chapter 3, I empirically test the key theoretical building block of the KLP framework, namely the *causal* effect of cognitive capital on performance. Drawing on contemporary cognitive psychology, I measure general cognitive capital by *working memory* – a robust predictor of general fluid intelligence and performance in tasks requiring controlled information processing. Since pre-existing task-specific cognitive capital (expertise) is vital for performance in many field cognitive tasks but is hard to measure, I intentionally minimize its potential relevance by designing an experiment where working memory arguably becomes the main component of cognitive capital, besides experience acquired endogenously through on-task learning.

Specifically, as a tool for identifying the causal effect of working memory, I design a time-series forecasting task that requires maintaining forecast-relevant information accessible in memory while simultaneously processing it, hence activating precisely the type of cognitive capital that working memory theoretically represents. To identify the causal effect of working memory on forecasting performance, two screens with forecast-relevant information are displayed either concurrently or sequentially. Since the sequential (concurrent) treatment features higher (lower) working memory load, working memory should be a stronger (weaker) determinant of forecasting performance, after controlling for other cognitive and personality (especially motivational) determinants of forecasting performance. This hypothesis is confirmed for individual differences in asymptotic forecasting performance.

I therefore show that the effectiveness of high-powered financial incentives as a stimulator of economic performance can be moderated by individual heterogeneity in cognitive capital in a causal fashion. The evidence also illustrates the need to attend to cognitive constraints, besides personality (preference-based) factors, when interpreting observed variance of behavior in cognitively demanding economic environments, be it in the laboratory or the field.

Establishing the causality of general cognitive capital such as working memory permits examining its substitutability with task-specific cognitive capital and in turn with cognitive effort, ultimately enhancing our understanding of cognitive production processes. Establishing the causality of cognitive capital is also a prerequisite for credibly addressing fundamental economic interactions underlying the KLP framework, such as how people perform under different incentive levels and schemes conditional on their cognitive capital; how they self-select based on their cognitive capital into incentive schemes varying in expected return to cognitive capital (and effort); whether people are willing to purchase "external" cognitive capital that would relax their cognitive constraints; and how cognitive capital affects the way people interact in strategic environments.

CHAPTER 1

How Financial Incentives and Cognitive Abilities Affect Task Performance in Laboratory Settings: An Illustration*

Abstract

Drawing on Gneezy and Rustichini (2000), we illustrate that subjects' cognitive abilities seem at least twice as important for their performance as do financial incentives they face. This result backs up the exhortation of Camerer and Hogarth (1999) to pay attention to the capital as well as the labor input of cognitive production.

Keywords: Financial incentives, Cognitive abilities, Experiments, Field experiments
JEL classification: C81; C91; C93; D83

* This chapter is co-authored with Andreas Ortmann and is published (with minor changes) under the same title in *Economics Letters*, 85(3), December 2004. An earlier version has also been circulated under the same title as CERGE-EI Working Paper No. 221 (February 2004). We thank Dirk Engelmann, Uri Gneezy, Robin Hogarth and Štěpán Jurajda for helpful comments.

1. Introduction

One of the distinguishing features of the practices of experimental economists is performance-dependent subject payments, or financial incentives (Hertwig and Ortmann, 2001). In fact, among economists financial incentives have become a strictly enforced convention based on the widely shared belief that decisions have to matter to those participating in experiments for the data to have meaning. This belief found its expression in Smith's nonsatiation and saliency tenets of proper experimentation (e.g., Smith, 1976, 1982) and, by and large, seems to be supported by empirical evidence produced by economists: In their survey article, Smith and Walker (1993) suggest that "increased financial rewards [may] shift the central tendency of the data toward the predictions of rational models…[and] in virtually all cases rewards reduce the variance of the data around the predicted outcome" (Smith and Walker, 1993, p. 245; see Hertwig and Ortmann, 2001, for a more differentiated assessment). Smith and Walker's conclusion is generally referred to as the labor theory of cognition.

Two recent papers have urged a reconsideration of this view. Gneezy and Rustichini (2000) demonstrated in a thought-provoking experiment that increasing financial incentives does not monotonically lead to more favorable evidence for economic theories. Rather, these authors' results suggest – as succinctly summarized by the title of their article – that the experimenter ought to pay enough, or not pay at all. In other words, while economists' belief in the effects of increasing financial incentives in experiments seems to be right on the money for reasonably high financial incentives, microscopic payments have – for reasons not yet agreed on – detrimental effects on subject behavior. Gneezy and Rustichini (2000) conjecture that their minimally paid subjects might have been insulted by the microscopic compensation offered to them and consequently performed worse than subjects who, apart from a flat participation fee, solved the tasks solely based on their intrinsic motivation.

Camerer and Hogarth (1999), importantly and in our view correctly, take a broader view and argue that the real problem lies in economists' focus on the labor aspect, and almost complete neglect of the capital aspect, of cognitive production in experiments. In their view, cognitive performance is affected not only by incentives and thus effort that subjects exert, but also by cognitive abilities that are fixed in the short run of the experiment and can be enhanced if learning is allowed. In turn, the authors argue, interpreting experimental results should be conditional not only on the particular financial incentives employed but also on cognitive abilities of the participants in the experiment.

And yet, while the arguments in Camerer and Hogarth (1999) are persuasive, they are informal. In fact, to the best of our knowledge there exists no persuasive empirical evidence produced by economists directly illuminating the *relative* importance of individual abilities and incentives in cognitive production,[1] although there are some precursors in related disciplines that are suggestive.[2] Here we provide such empirical evidence by drawing on the data in Gneezy and Rustichini (2000). Specifically, we show that the effects of financial incentives seem much less important, even under the best of circumstances, than those of cognitive abilities approximated by a measure of subjects' IQ.

---

[1] Palacios-Huerta (2003) examines the impact of tournament-type and increased incentives in the last 15 rounds of a repeated Monty Hall Three Door problem while controlling for subjects' GPA, learning, and several other treatment effects. The author finds that "more able individuals significantly respond to the size of incentives" (p. 247). However, this result is quite possibly due to the five sizeable cash prizes that induce aspects of a tournament-type competition rather than the increased incentives. The author also does not, and actually cannot because of the confounds introduced by the cash prizes, address the relative importance of individual abilities and incentives. In addition, it is not clear to what extent the impact of ability and incentives is confounded with social interactions allowed in the last 15 rounds. Eckel (1999) uses natural framing of lottery choices, which she argues is a substitute for increased incentives, and finds that individual performance is positively correlated with subjects' GPA score. She did not, however, run the obvious control treatment with increased incentives that would have completed her argument.

[2] In a unique example of interacting financial incentives and intrinsic ability, Awasthi and Pratt (1990) examined their relative impact in three accounting tasks. Contrary to what seems to hold for the data of Gneezy and Rustichini analyzed below, the authors find that incentive effects are significantly stronger for subjects with higher cognitive abilities (measured by the EFT test of perceptual differentiation, a very idiosyncratic measure of cognitive capital). However, the authors cannot do full justice to the estimation due to having only a binary performance measure. Therefore, they are not able to quantify the relative importance of individual ability and incentives.

2. The data

In Figure 1 below, we adapted individual-level data from Gneezy and Rustichini (2000), who examined the impact of financial incentives of different strength on performance in a psychometric (IQ) test. Importantly, the authors did not analyze their data at a disaggregate level. Such disaggregation provides us, however, with important quantitative insights about the separate effects of incentives and individual abilities on cognitive performance.

Gneezy and Rustichini randomly assigned 160 subjects to four treatments (no-pay, NIS0.1, NIS1 and NIS3; from here on "incentive treatments") and then examined the impact of financial incentives on average IQ score for each treatment.[3] They reported a non-monotonic impact of incentives on performance: average performance was highest and almost identical for the two high-incentive treatments (NIS1 and NIS3), but lowest for the NIS0.1 treatment. In fact, average performance in this treatment was statistically lower than performance in the no-pay treatment.

In Figure 1 we assume (as Gneezy and Rustichini did) that the subjects in the four incentive treatments were sampled from a common population, and we plot, for each of the four incentive treatments, individual IQ performance, in ascending IQ rank order. An individual's IQ performance, measured on the vertical axis, induces her or his IQ rank, indicated on the horizontal axis. Note that the individuals ranked "1" in each treatment scored worst, while those ranked "40"scored best. For each incentive treatment, the connected IQ-score observations yield a "performance curve" for that treatment. Whereas such a performance curve visually describes the *within-treatment* variation in performance, one can similarly inspect the *across-treatment* variation by making comparisons among the performance curves. Three observations are noteworthy:

[3] The subjects were also paid a flat participation fee of NIS60. At the time of the experiment, the exchange rate was 3.5 NIS (New Israeli Shekel) to $1. The fifty IQ-type questions, taken from a test normally used to screen university applicants, involved mainly reasoning and computation skills. The subjects were volunteer male and female undergraduate students at the University of Haifa from all fields of study with an average age of 23 years.

First of all, notice that the performance curves for the high-incentive treatments (NIS1 and NIS3) are virtually identical and slope considerably upwards, implying that there is a high within-treatment variation in performance but hardly any across-treatment one. Arguably, this is most likely due to a significant within-treatment variation in cognitive abilities. One could conceive that the large within-treatment performance variation is partly also effort-driven, but the variation in cognitive effort required to generate this result is unlikely; plus one would need to explain why the two performance curves seem almost identical despite the across-treatment incentive (and thus presumably effort) differential. Therefore, consistent with the interpretation of the IQ score as ability rank, it seems quite plausible that ability rather than incentive differentials determine individual performance differentials when incentives are high enough.

Next inspect the performance curves for the low-incentive treatments (no-pay and NIS0.1). Clearly, Gneezy and Rustichini (2000) were right in asserting that the NIS0.1 subjects overall were less motivated than the ones in the no-pay treatment. This is particularly apparent at the low-performance end where the gap between the performance curves for the low-incentive treatments widens (and, in addition, so does the gap between the performance curves of the two low-incentive treatments and the two high-inventive treatments). That the NIS0.1 subjects were less motivated than the ones in the no-pay treatment also seems confirmed by the performance curve for the NIS0.1 treatment lying below that for the no-pay treatment across the whole performance range. It is highly unlikely that this would be caused by across-treatment ability differentials, and thus across-treatment differences in motivation must have played the main role.[4]

Finally and most importantly, focus on the slope of all four performance curves and the distance between them. An eyeball test reveals that, leaving aside the motivational

[4] Clearly, effort is a function of motivation. But what we see at the low-performance end of the two low-incentive treatments is not only a lack of motivation to perform but an outright refusal to perform, and in any case a different form of "lack" of motivation. Gneezy (2004) proposes several possible explanations, such as participants feeling insulted or being at odds with their self-perception. That such reactions are much less prevalent in the no-pay treatment and entirely absent in the two high-incentive treatments (NIS1 and NIS3) seems to confirm the presence of a specific kind of motivational problem in the NIS0.1 treatment. It also seems to justify the current practice of most experimental economists of paying their subjects at least twice the minimum wage.

problems at the low-performance end, the within-treatment variation in performance is generally much greater than the variation across treatments. To give a meaningful comparison, consider the largest across-treatment performance differential at the median rank. This turns out to be 13 (i.e., 24 correct answers in the NIS0.1 treatment vs. 37 in the NIS1 treatment), which is equivalent to the performance differential associated with moving up from the first to the third quartile within the NIS1 treatment (28 vs. 41). Note, however, that within-treatment performance differentials can be much larger. For instance, in both of the high-incentive treatments (NIS1 & NIS3), the difference in performance for individuals ranked 1 and 40 is as large as 34.

Provided that the across-treatment performance variation can be assigned to incentive effects while the within-treatment performance variation to ability differentials, an important and powerful result follows: ability differentials among individuals seem to account for a much greater part of performance variation than incentive effects.

3. Discussion

As to the motivational complications in the first performance quartile, we do not know whether the "unmotivated" subjects at the low-performance end of the no-pay and NIS0.1 treatments have low abilities: it is possible, although in our view implausible, that these subjects are high-ability individuals.[5] But this makes the case for the relative importance of cognitive-capital effects even stronger: were it not for motivational problems of (possibly) high-ability individuals, cognitive-capital effects could be even larger.

Our illustration backs up the exhortation of Camerer and Hogarth (1999) to pay attention to both the labor and capital aspects of cognitive production in experiments. To the extent that our illustration is representative of the relative importance of cognitive capital effects, not controlling for cognitive abilities is an important shortcoming of almost all existing experimental studies examining incentive effects.

[5] In our view, low-ability participants have to exert more effort than high-ability ones, so we are unlikely to find the low-ability participants in the upper part of the performance curve. Reinforcing this argument, high-ability participants potentially face significantly higher reputational costs than their low-ability counterparts (Benabou and Tirole, 2003).

Our illustration makes a strong case for considering cognitive abilities, as well as motivational factors, as major determinants of cognitive performance. This was clearly a major deficiency of the labor theory of cognition, one which needs to be remedied by integrating the insights of Camerer and Hogarth (1999), and of Gneezy and Rustichini (2000), into that framework. For a very preliminary attempt at such a capital-labor framework, see Wilcox (1993) who proposes that to solve mental tasks, subjects employ algorithms of various sophistication and effort cost, yet he does not go into detail of what such algorithms consist of.

References

Awasthi, V., and J. Pratt (1990), "The effects of monetary incentives on effort and decision performance: The role of cognitive characteristics," *Accounting Review* 65, 797-811.

Benabou, R. and J. Tirole (2003), "Incentives and Prosocial Behavior," *American Economic Review* 96, 1652-1678.

Camerer, C. F., and R. Hogarth (1999), "The effects of financial incentives in experiments: A review and capital-labor-production framework," *Journal of Risk and Uncertainty* 19, 7-42.

Eckel, C. (1999), "Comment on 'The effects of financial incentives in experiments: A review and capital-labor-production framework'," *Journal of Risk and Uncertainty* 19, 47-48.

Gneezy, U. (2004), "The W effect of incentives," The University of Chicago Graduate School of Business manuscript.

Gneezy, U., and A. Rustichini (2000), "Pay enough or don't pay at all," *Quarterly Journal of Economics* 115, 791-811.

Hertwig, R., and A. Ortmann (2001), "Experimental practices in economics: A methodological challenge for psychologists?" *Behavioral and Brain Sciences* 24, 383-451.

Palacios-Huerta, I. (2003), "Learning to open Monty Hall's doors," *Experimental* Economics 6, 235-251.

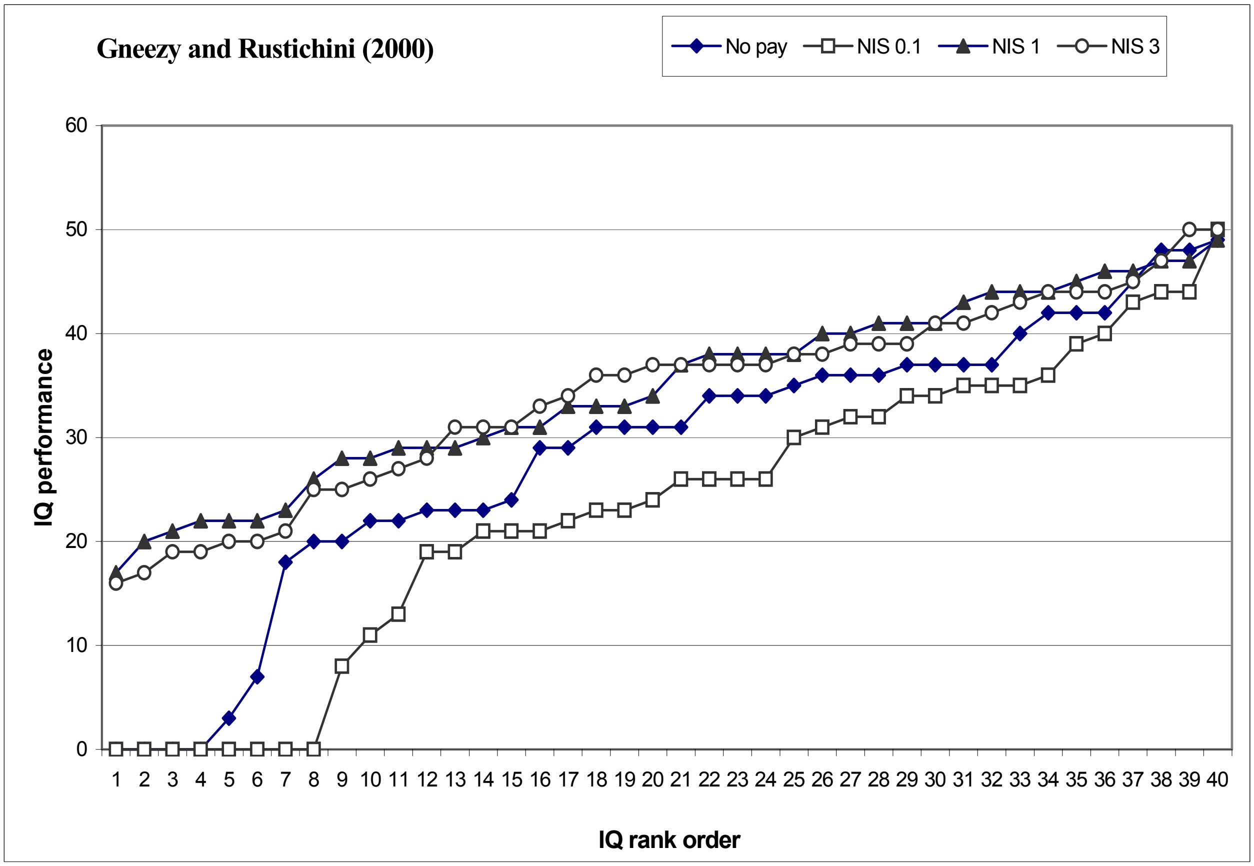
Smith, V. L. (1976), "Experimental economics: Induced value theory," *American Economic Review Proceedings* 66, 247-279.

Smith, V. L. (1982), "Microeconomic systems as an experimental science," *American Economic Review* 72, 923-955.

Smith, V. L., and J. Walker (1993), "Monetary rewards and decision cost in experimental economics," *Economic Inquiry* 31, 245-261.

Wilcox, N. (1993), "Lottery choice: Incentives, complexity, and decision time," *The Economic Journal* 103, 1397-1417.

Figure 1: Individual IQ performance, plotted in ascending IQ rank order, separately for each of the four incentive treatments.



CHAPTER 2

The Interaction between Financial Incentives and Task-specific Cognitive Capital: More Evidence in Support of Camerer and Hogarth (1999)[†]

Abstract

This paper extends existing evidence on the interaction between financial incentives and cognitive capital. I focus on the impact of task-specific cognitive capital, the role of which is central to the capital-labor-production framework of Camerer and Hogarth (1999) and has long been studied in cognitive science and behavioral decision research. Using a task situated in an accounting setting, I show that both financial incentives and task-specific cognitive capital, and especially their interaction, matter for performance. In particular, the effect of task-specific cognitive capital on performance is stronger under performance-based financial incentives as compared to flat-rate incentives. The interaction effect arises because performance-based financial incentives lead to better performance only for individuals with more task-specific cognitive capital. I draw implications for compensation practices in experiments as well as work settings.

Keywords: Financial incentives, Cognitive abilities, Experiments, Field experiments

JEL classification: C81; C91; C93; D83

[†] This chapter's earlier version has been circulated as CERGE-EI Working Paper No. 264 (June 2005) under the title "Capital and Labor Effects in a Recall Task: More Evidence in Support of Camerer and Hogarth (1999)." I thank Robert Libby and Marlys Gascho Lipe for providing the raw data, and Randall Filer, Jan Hanousek, Glenn Harrison, Robin Hogarth, Štěpán Jurajda, Jan Kmenta, Andreas Ortmann and Sarah Peck for helpful comments.

1. Introduction

In an attempt to portray how financial incentives induce performance in cognitively demanding tasks, Camerer and Hogarth (1999) propose a capital-labor-production framework. The authors informally describe how financial incentives may interact in non-trivial ways with intrinsic motivation in stimulating cognitive effort (labor), and how the productivity of cognitive effort may in turn vary across individuals due to their different cognitive abilities (capital).

To date, however, there is sparse empirical evidence on the relative importance of cognitive capital and effort as inputs of cognitive production. In fact, due to the complexity of measuring cognitive effort (see, e.g., Camerer and Hogarth, 1999), the existing empirical accounts of the capital-labor-production framework focus on the reduced-form interaction between financial incentives and cognitive capital. Awasthi and Pratt (1990) and Palacios-Huerta (2003) show that introducing and/or raising performance-based financial incentives yields a larger increase in judgmental performance for individuals with higher cognitive capital, as proxied by a perceptual differentiation test and schooling outcomes, respectively.[6] Rydval and Ortmann (2004) illustrate that cognitive capital appears at least as important for performance in an IQ test as does a sizeable variation in piece-rate financial incentives.

In this paper, I extend the above evidence by focusing on the interaction between financial incentives and more *task-specific*, as opposed to domain-general, forms of cognitive capital. The role of task-specific cognitive capital in cognitive production is central to the capital-labor-production framework of Camerer and Hogarth (1999) and has been extensively studied in cognitive science and behavioral decision research (see, e.g., Anderson, 2000, Libby and Luft, 1993, and Bonner and Sprinkle, 2002, for reviews).

---

[6] Awasthi and Pratt (1990) also illustrate that effort duration increases uniformly with the introduction of piece-rate financial incentives, quite regardless of their subjects' cognitive capital (i.e., perceptual differentiation test score).

Using a task situated in an accounting setting, I show that both financial incentives and task-specific cognitive capital, and especially their interaction, matter for performance. In particular, the effect of task-specific cognitive capital – proxied by accounting knowledge – on performance is stronger under performance-based financial incentives as compared to flat-rate incentives. The interaction effect arises because performance-based financial incentives lead to better performance only for individuals with more accounting knowledge. I draw implications for further research of the capital-labor-production framework and for compensation practices in experiments as well as work settings.

2. The task and experimental design

To illustrate the effect of task-specific cognitive capital and its interaction with financial incentives, I use data from an earlier experimental study by Libby and Lipe (1992). Their experiment is a computerized memory task in which subjects are asked to memorize and subsequently recall a list of accounting items – specifically, sentence-long expressions used by accountants in the internal control/audit system.[7]

Libby and Lipe study the effect of introducing performance-based financial incentives on recall performance. They randomly split the subjects – 117 accounting and auditing students – into three incentive treatments. In the flat-rate (FLAT) treatment, subjects know from the start that they earn \$2 regardless of their recall performance. In the encoding (ENC) and retrieval (RETR) treatments, subjects additionally earn \$0.1 per each correctly recalled accounting item and can also earn a \$5 prize for the best five performers. In ENC, this performance-based incentive scheme is announced prior to memorizing (i.e., prior to encoding) of the accounting items, whereas in RETR, the scheme is announced only *after* memorizing (i.e., prior to retrieval) of the accounting items.[8]

[7] See Table 1 in Libby and Lipe (1992) for details.
[8] The FLAT treatment featured the \$5 tournament-type prize as well but the prize was announced only after the experiment. Overall, subjects could earn \$2-11.80 in ENC and RETR and \$2-7 in FLAT. The recall task was followed by another task (which subjects did not know until after completing the recall task). See Libby and Lipe (1992) for further implementation details that appear innocuous with respect to the results presented here.

Table 1 replicates the main results of Libby and Lipe.[9] *Recall* performance (the number of accounting items recalled correctly) varies considerably across individuals as well as across the incentive treatments. As noted by the authors, *Recall* exhibits a significantly increasing trend from FLAT to RETR to ENC, with the averages being 9.80, 11.61 and 12.34 items, respectively. *Recall* is significantly higher on average in ENC compared to FLAT (at the 5% level using a *t*-test and a Wilcoxon rank-sum test) but there is no significant difference in *Recall* between RETR and ENC. Hence the differential timing of announcing the performance-based incentive scheme does not seem to affect *Recall* performance on average.

Table 1 also displays two proxies for effort duration: *Tmemo* denotes the time spent memorizing the accounting items, and *Trecall* denotes the time spent recalling the items. In ENC, announcing the performance-based incentive scheme prior to memorization leads to a significantly higher *Tmemo* compared to FLAT (at the 10% level using a *t*-test). In RETR, announcing the performance-based incentive scheme prior to recall leads to a significantly higher *Trecall* compared to FLAT (at the 5% and 10% level using a *t*-test and a Wilcoxon rank-sum test, respectively).

Table 1 further contains two proxies for task-specific cognitive capital, namely accounting knowledge: *Courses* denotes the number of accounting credit hours taken by subjects, and *Experience* denotes the number of months of their auditing job experience. Although neither *Courses* nor *Experience* vary significantly across treatments, they both vary across subjects, potentially reflecting subjects' differential familiarity with the accounting items encountered in the memory task. Hence I compare the impact of accounting knowledge and financial incentives on *Recall* performance, as detailed below.

3. The effect of accounting knowledge and financial incentives on performance

To first illustrate the size of the impact of accounting knowledge on *Recall* performance, I split subjects into two groups. The High-K group contains subjects with above-median

---

[9] Apart from one missing observation (subject), my dataset is identical to that analyzed in Libby and Lipe (1992).

accounting education (*Courses*>21) *or* above-median auditing job experience (*Experience*>0), and vice versa for the Low-K group. The rationale for the split is that more accounting education is likely to be important for *Recall* performance but having any positive amount of auditing job experience substitutes for it.[10]

Table 2 displays the capital-based differentials, i.e., the differentials attributable to differences in accounting knowledge between the High-K and Low-K groups. Focusing first on *Recall* performance, the largest capital-based *Recall* differential reported in Table 2 arises in RETR, on average almost 7 correctly recalled accounting items, which is highly significant and more than twice the size of the largest incentive-based *Recall* differential between FLAT and ENC reported in Table 1. The capital-based *Recall* differentials in FLAT and ENC are insignificant and smaller than in RETR but are still comparable in size to the incentive-based *Recall* differentials. The last column of Table 2 shows that the capital-based *Recall* differential in the pooled sample is on average slightly above 3 accounting items, which is highly significant and similar in size to the largest incentive-based *Recall* differential reported in Table 1.[11] These findings generally confirm those of Rydval and Ortmann (2004) who likewise find capital-based performance differentials to be at least as important as incentive-based performance differentials.

Table 2 further shows that, in contrast to the positive and significant capital-based *Recall* differentials, the corresponding effort differentials in *Tmemo* and *Trecall* are insignificant and go in either direction. As Libby and Lipe caution, *Tmemo* and *Trecall* are noisy measures of effort duration, let alone effort intensity: *Tmemo* can be confounded by individual differences in reading speed and *Trecall* by differences in computer literacy

[10] A practical reason for the split is that it yields relatively balanced sample sizes for the High-K and Low-K groups in each incentive treatment. The results presented in Table 2 are robust, in the statistical significance sense, to alternative High-K / Low-K splits, for example those based only on the median of *Courses* (e.g., *Courses*≥21 or *Courses*>21). Other splits based on the *Experience* variable are problematic in terms of balancing the sample sizes since only 30% of subjects have auditing job experience.

[11] Note that the pooled capital-based *Recall* differential is unlikely to be driven by incentive effects, simply because the High-K group in the pooled sample contains a decreasing percentage of FLAT to RETR to ENC subjects. Admittedly, the pooled capital-based *Recall* differential is driven by the exceptionally large differential in RETR.

(typing speed).[12] Nevertheless, Awasthi and Pratt (1990) similarly find that people with low and high cognitive capital do not differ in effort duration but do differ in judgmental performance.

Table 3 presents multivariate analysis that disentangles the impact of task-specific cognitive capital and financial incentives on *Recall* performance. In column (1), *Recall* is regressed on the proxies for accounting knowledge, *Courses* and *Experience*, while the treatment dummies, *RETR* and *ENC*, capture any remaining *Recall* differences in the incentive treatments with respect to FLAT. The estimates suggest that while *Recall* is significantly higher in both RETR and ENC compared to FLAT, confirming the incentive-based differentials reported in Table 1, *Recall* also improves with higher *Courses* (significantly) and *Experience* (insignificantly), confirming the capital-based differentials reported in Table 2.

Column (2) further includes interactions of *Courses* with the incentive treatments (*Courses x RETR* and *Courses x ENC*). As a consequence, the treatment dummies and accounting knowledge proxies become insignificant. In columns (3) and (4), however, omitting the insignificant treatment dummies from the estimation reveals a significant interaction between *Courses* and the incentive treatments. Namely, the impact of *Courses* on *Recall* is almost twice as high in RETR and ENC compared to FLAT.[13]

Combining the evidence from all three tables, the incentive-based differentials in *Recall* performance (reported in Table 1) seem primarily driven by the stronger impact of accounting knowledge on *Recall* under performance-based financial incentives (as reported

[12] Camerer and Hogarth (1999) discuss alternative measures of effort duration and effort intensity.

[13] Interactions of *Experience* with *ENC* and *RETR* as well as higher-order moments of *Courses* and *Experience* turn out individually and jointly highly insignificant and hence are not included in any of the estimations in Table 3. Libby and Lipe's (1992) dataset unfortunately does not contain any other observable characteristics such as demographics (with the exception of age which is nevertheless strongly correlated with *Courses*) that would permit controlling for sample composition differences. Libby and Lipe report a Pearson correlation of 0.44 between *Recall* and subjects' auditing course grade but the latter data is not available for further analysis. Observing the strong correlation, Libby and Lipe speculate that the impact of introducing performance-based financial incentives on *Recall* performance may depend on the decision maker's accounting knowledge base. The conjecture that "incentive-induced effort may interact with knowledge" is revisited in Libby and Luft (1993, p.443) but is not subject to closer empirical scrutiny.

in Tables 2 and 3). Table 2 further suggests that this result is primarily due to subjects with more accounting knowledge responding stronger to performance-based financial incentives. Specifically, *Recall* of the High-K groups is significantly higher at the 5% level in both RETR and ENC compared to FLAT (using a *t*-test and a Wilcoxon rank-sum test). By contrast, *Recall* of the Low-K groups is statistically indistinguishable among FLAT, RETR and ENC.[14]

4. Discussion

In line with Camerer and Hogarth's (1999) capital-labor-production framework, task-specific cognitive capital in the form of accounting knowledge, and especially its interaction with financial incentives, seem important determinants of *Recall* performance in the memory task. Specifically, the effect of accounting knowledge on *Recall* performance is stronger under performance-based financial incentives as compared to flat-rate incentives. The interaction effect seems to arise because performance-based financial incentives lead to better performance only for individuals with more accounting knowledge.

The above evidence of the positive interaction between incentives and task-specific cognitive capital bears close resemblance to the findings of Awasthi and Pratt (1990) and Palacios-Huerta (2003) who use more domain-general cognitive capital proxies. As in the case of Palacios-Huerta, however, it warrants further investigation to determine whether the positive interaction is predominantly due to the piece-rate or the tournament part of the performance-based incentive scheme.

To the extent that Libby and Lipe's (1992) dataset is relatively small and does not contain potentially important individual characteristics that would permit accounting for sample composition differences, the evidence presented above should be viewed as suggestive only. One would ideally account for the impact of other forms of cognitive capital related to the memory task, such as short-term and working memory (e.g., Kane et al., 2004).

[14] I should note, however, that the *difference* in the responsiveness of the Low-K and the High-K groups to announcing performance-based incentives is not significant (using a parametric *t*-test).

Subjects' responsiveness to financial incentives might also be influenced by their ex ante intrinsic motivation to engage in the memory task (e.g., Cacioppo et al., 1996). Another general concern is whether subjects better equipped with task-specific cognitive capital are in some sense "smarter" before they acquire it. Such endogeneity issues are implicitly discussed by LeDoux (2002) who argues that the process of cognitive capital development inevitably involves "nurturing nature," i.e., further developing inherited forms of cognitive capital.[15]

The interactions between financial incentives and cognitive, motivational and other personality characteristics underlie Camerer and Hogarth's (1999) capital-labor-production framework. Empirically disciplining the framework clearly requires not only identifying the relevant cognitive and motivational constructs but also thinking thoroughly about the structural relationships among them. Psychologists have argued that doing so may require attending not only to measurable, objective cognitive characteristics but also to their self-perceived, subjective counterparts (e.g., Bandura and Locke, 2003). Taking even one step further, economists have questioned whether decision makers can intentionally manipulate their cognitive self-perception and whether that self-perception can be influenced by performance-based incentives (e.g., Benabou and Tirole, 2002, 2003). These and other literatures should serve as a rich source of possible identifying restrictions.

Camerer and Hogarth's (1999) capital-labor-production framework deserves much further research, and its potential implications for compensation practices in experiments and work settings are wide-ranging (see, e.g., Bonner and Sprinkle, 2002). Consider, for example, the evidence discussed above suggesting that performance-based financial incentives tend to induce greater effort duration regardless of cognitive capital but lead to better performance only for individuals with more cognitive capital. As a consequence, efficiently using performance-based financial incentives may involve directing their impact predominantly at high-capital individuals in experimental subject pools or in company workforce, in order to maximize performance outcomes and minimize effort resource costs.

[15] See Plug and Vijverberg (2003) for an economic approach to the nature/nurture debate.

References

Anderson, J. R. (2000), *Cognitive Psychology and its Implications*, New York: Worth.

Awasthi, V., and J. Pratt (1990), "The effects of monetary incentives on effort and decision performance: The role of cognitive characteristics," *Accounting Review* 65, 797-811.

Bandura, A., and E. E. Locke (2003), "Negative self-efficacy and goal effects revisited," *Journal of Applied Psychology* 88, 87-99.

Benabou, R., and J. Tirole (2002), "Self-confidence and personal motivation," *Quarterly Journal of Economics* 117, 871-915.

Benabou, R., and J. Tirole (2003), "Intrinsic and extrinsic motivation," *Review of Economic Studies* 70, 489-520.

Bonner, S. E., and G. B. Sprinkle (2002), "The effects of monetary incentives on effort and task performance: Theories, evidence, and a framework for research," *Accounting, Organizations & Society* 27, 303-345.

Cacioppo, J. T., Petty, R. E., Feinstein, J. A., and W. B. G. Jarvis (1996), "Dispositional differences in cognitive motivation: The life and times of individuals varying in need for cognition," *Psychological Bulletin* 119, 197-253.

Camerer, C. F., and R. Hogarth (1999), "The effects of financial incentives in experiments: A review and capital-labor-production framework," *Journal of Risk and Uncertainty* 19, 7-42.

Cuzick, J. (1985) "A Wilcoxon-type test for trend," *Statistics in Medicine* 4, 87-90.

Kane, M. J., Hambrick, D. Z., Tuholski, S. W., Wilhelm, O., Payne, T. W., and R. W. Engle (2004), "The generality of working memory capacity: A latent variable approach to verbal and visuospatial memory span and reasoning," *Journal of Experimental Psychology*: *General* 133, 189-217.

LeDoux, J. E. (2002), *Synaptic Self*, New York: Viking.

Libby, R., and M. G. Lipe (1992), "Incentives, effort, and the cognitive processes involved in accounting-related judgments," *Journal of Accounting Research* 30, 249-273.

Libby, R., and J. Luft (1993), "Determinants of judgment performance in accounting settings: Ability, knowledge, motivation, and environment," *Accounting, Organizations & Society* 18, 425-450.

Palacios-Huerta, I. (2003), "Learning to open Monty Hall's doors," *Experimental Economics* 6, 235-251.

Plug, E., and W. Vijverberg (2003), "Schooling, family background, and adoption: Is it nature or is it nurture?" *Journal of Political Economy* 111, 611-641.

Rydval, O., and A. Ortmann (2004), "How financial incentives and cognitive abilities affect task performance in laboratory settings: An illustration," *Economics Letters* 85, 315-320.

Table 1: Summary statistics for the pooled sample and the incentive treatments

Treatment # subjects	POOLED 117	FLAT 41	RETR 38	ENC 38
Recall	11.21 (5.46) [1-23]	9.80^{t*r*} (4.17)	11.61 (5.57)	12.34^{c*} (6.33)
Tencoding (in seconds)	345.75 (198.97) [63-1349]	307.44^{t} (127.38)	345.74 (210.03)	387.11 (243.08)
Trecall (in seconds)	739.57 (366.53) [77-1675]	662.59 (313.99)	841.26^{t*r} (432.89)	720.95 (331.18)
Courses	20.29 (4.44) [6-30]	20.49 (3.96)	20.05 (5.18)	20.32 (4.24)
Experience	0.85 (1.41) [0-6]	0.78 (1.29)	0.87 (1.60)	0.89 (1.37)

Notes: The POOLED column displays the mean and beneath it the standard deviation (in parentheses) and the range (in brackets) for the pooled sample. The FLAT, RETR and ENC columns display the mean and beneath it the standard deviation (in parentheses) for the three incentive treatments. The t and t* superscripts denote a significant difference at the 10% and 5% level, respectively, between the relevant treatment and the treatment immediately to the left (except for FLAT which is compared to ENC), using a two-sided *t*-test accounting for unequal variances whenever appropriate. Analogously, the r and r* superscripts denote a significant difference at the 10% and 5% level, respectively, using a two-sided Wilcoxon rank-sum test. The c and c* superscripts denote a significantly increasing trend at the 10% and 5% level, respectively, from FLAT to RETR to ENC, as indicated by a non-parametric test for trend across ordered groups developed by Cuzick (1985). The latter two tests incorporate correction for ties.

Table 2: Summary statistics for the Low-K and High-K groups

Treatment	FLAT		RETR		ENC		POOLED	
# subjects	Low-K(15)	High-K(26)	Low-K(15)	High-K(23)	Low-K(16)	High-K(22)	Low-K(46)	High-K(71)
Recall	8.93	10.31	7.53	14.26^{t*r*}	11.13	13.23	9.24	12.49^{t*r*}
	(4.79)	(3.77)	(4.37)	(4.61)	(6.22)	(6.41)	(5.31)	(5.20)
Tmemo	342.67	287.12	344.20	346.74	417.38	365.09	369.15	330.59
	(146.81)	(112.77)	(302.43)	(126.42)	306.79)	(188.86)	(260.39)	(146.38)
Trecall	711.93	634.12	805.07	864.87	645.50	775.82	719.20	752.77
	(377.77)	(274.81)	501.54)	(391.93)	(329.87)	(328.68)	(404.15)	(342.32)

Notes: The FLAT, RETR and ENC incentive treatments and the POOLED sample are sub-divided into the Low-K and High-K groups as defined in Section 2 (number of subjects in parentheses). Each cell displays the mean and beneath it the standard deviations (in parentheses). The t and t* superscripts denote a significant difference at the 10% and 5% level, respectively, between the sub-divided High-K and Low-K groups, using a two-sided *t*-test accounting for unequal variances whenever appropriate. Analogously, the r and r* superscripts denote a significant difference at the 10% and 5% level, respectively, using a two-sided Wilcoxon rank-sum test adjusting for ties whenever appropriate.

Table 3: OLS regressions of *Recall* performance on incentive treatment dummies, accounting knowledge proxies and their interactions.

REGRESSOR	(1) Estimate (std. err.)	(2) Estimate (std. err.)	(3) Estimate (std. err.)	(4) Estimate (std. err.)
intercept	5.434** (2.295)	8.090** (3.816)	6.913*** (2.239)	7.000*** (2.247)
RETR	1.869* (1.089)	-4.329 (5.047)	—	—
ENC	2.546** (1.233)	2.710 (6.258)	—	—
Courses	0.205* (0.111)	0.076 (0.186)	0.139 (0.112)	0.126 (0.114)
Experience	0.232 (0.323)	0.193 (0.333)	—	0.226 (0.322)
Courses x RETR	—	0.306 (0.247)	0.105** (0.052)	0.103* (0.053)
Courses x ENC	—	-0.0089 (0.307)	0.120** (0.059)	0.119** (0.060)
R-squared	0.074	0.090	0.073	0.076
Joint significance	(**)	(**)	(**)	(**)

Notes: Subjects = 117 (41 in FLAT, 38 in RETR and 38 in ENC). Heteroskedasticity-robust standard errors in parentheses. *,**, and *** denote significance of estimates at the 10%, 5% and 1% level, respectively.

CHAPTER 3

Financial Incentives and Cognitive Abilities: Evidence from a Forecasting Task with Varying Cognitive Load[†]

Abstract

I examine how financial incentives interact with intrinsic motivation and especially cognitive abilities in explaining heterogeneity in performance. Using a forecasting task with varying cognitive load, I show that the effectiveness of high-powered financial incentives as a stimulator of economic performance can be moderated by cognitive abilities in a causal fashion. Identifying the causality of cognitive abilities is a prerequisite for studying their interaction with financial and intrinsic incentives in a unifying framework, with implications for the design of efficient incentive schemes.

Keywords: Financial incentives, Cognitive ability, Heterogeneity, Performance, Experiment

JEL classification: C81, C91, D83

[†] This chapter's earlier version has been circulated under the same title as CERGE-EI Discussion Paper No. 173 (January 2007). This research was supported by the Grant Agency of the Czech Republic (grant 402/05/1023) and by travel funds from the Hlavka Foundation and CERGE-EI. I am grateful to Randall Engle and Richard Heitz of the Attention and Working Memory Lab at the Georgia Institute of Technology for providing me with their "automated working memory span" tests, and to Brian MacWhinney of the Department of Psychology, Carnegie Mellon University, for providing me with his "audio digit span" test. I am greatly indebted to Andreas Ortmann and Nat Wilcox for providing me with invaluable advice throughout the project. I further thank Colin Camerer, Jan Hanousek, Glenn Harrison, Štěpán Jurajda and Jan Kmenta for helpful comments. All remaining errors are my own.

1. Introduction

Economists widely believe that, absent strategic considerations such as agency problems, financial incentives represent a dominant and effective stimulator of human productive activities (e.g., Gibbons, 1998; Prendergast, 1999). In production settings that are cognitively demanding, however, the effectiveness of financial incentives may be moderated by individual cognitive abilities and motivational characteristics. As a useful metaphor for the moderating channels, Camerer and Hogarth (1999) propose an informal capital-labor-production (KLP) framework, describing how financial incentives may interact in non-trivial ways with intrinsic motivation in stimulating cognitive effort (labor), and how the productivity of cognitive effort may in turn vary across individuals due to their different cognitive abilities (capital). Even if financial incentives induce high effort, both financial and cognitive resources may be wasted for individuals whose cognitive constraints inhibit performance improvements. This prediction, if warranted, calls for attention to individual cognitive abilities in designing efficient incentive schemes in firms, experimental settings and elsewhere.[16]

This paper provides an initial empirical test of the KLP framework. I identify the key theoretical building block of the KLP framework, namely the *causal* effect of cognitive capital on performance. Establishing the causality of cognitive capital is a prerequisite for credibly addressing fundamental economic interactions underlying the KLP framework, such as how people perform under different incentive levels and schemes conditional on their cognitive capital;[17] how they self-select based on their cognitive capital into incentive

[16] See Awasthi and Pratt (1990), Libby and Lipe (1992), and Libby and Luft (1993), among others, for earlier accounts of the KLP framework. Throughout the paper, I refer to cognitive abilities and cognitive capital interchangeably. One can think of individual cognitive capital, combined with the cognitive load of a particular cognitive task, as determining the extent to which individuals face cognitive constraints when executing the task.

[17] Economists, psychologists and researchers in other fields have paid considerable theoretical and empirical attention to the effect of financial incentives on (cognitive) performance, especially to their interaction with intrinsic motivation (see Bonner and Sprinkle, 2002, McDaniel and Rutström, 2001, and Rydval, 2003, for reviews). By contrast, we have much less evidence on the interaction of financial incentives with cognitive capital. In Awasthi and Pratt (1990) and Palacios-Huerta (2003), introducing and/or raising performance-contingent financial incentives yields a larger increase in judgmental performance for individuals with higher cognitive capital, as proxied by

schemes varying in expected return to cognitive capital (and effort);[18] whether people are willing to purchase "external" cognitive capital that would relax their cognitive constraints;[19] and how cognitive capital affects the way people interact in strategic environments.[20]

The notion of cognitive capital is of course not new to economists (e.g., Conlisk, 1980; Wilcox, 1993). Ballinger et al. (2005) provide a broad but pertinent theoretical perspective on cognitive capital, describing it as a vector of various (possibly interacting and time-variant) limits on cognition that can at any instance be "(perhaps imperfectly) measured by various tests of cognitive abilities." (p.3). Recent experimental evidence suggests that individual heterogeneity in cognitive capital can partly explain departures from rational saving behavior (Ballinger et al., 2005), deviations from normative game-theoretic solutions (Devetag and Warglien, 2003; Ortmann et al., 2006) and biases in risk and time preferences (Benjamin et al., 2006). Going a step further, I ask whether the effect of cognitive capital on economic behavior and performance is causal, and in turn whether the

---

a perceptual differentiation test and schooling outcomes, respectively. Rydval and Ortmann (2004) illustrate that cognitive abilities appear at least as important for performance in an IQ test as does a sizeable variation in piece-rate incentives. Contrasting the explanatory power of cognitive capital and personality characteristics under various incentive schemes – such as piece-rate, quota and tournament schemes – is likely a fruitful area of future research (e.g., Bonner et al., 2000).

[18] See Harrison et al. (2005), Lazear et al. (2006), and Vandegrift and Brown (2003) for examples of self-selection in experiments, and Bonner and Sprinkle (2002) for discussion and early evidence of self-selection based on cognitive abilities into incentive schemes.

[19] In a follow-up part of this project, I will interact financial incentives with the measures of cognitive capital identified here, by offering subjects to purchase a reduction of the cognitive load they face. See the Discussion section for more details.

[20] While I focus on the predictive power of cognitive capital in individual decision making, the methodological approach should be of interest in interactive decision making too. Economic strategic interactions vary in their cognitive load – for instance, differentially complex signaling games (e.g., Camerer, 2003, ch.8) – and hence are likely to activate different forms of cognitive capital relying to a varying extent on automated and controlled information processing (e.g., Stanovich and West, 2000; Feldman-Barrett et al., 2004). Detecting which forms of cognitive capital matter in particular strategic environments would help us understand the cognitive nature of the environments and to more accurately interpret the observed (variance in) behavior.

effectiveness of even strong financial incentives can be moderated by cognitive capital in a causal fashion.[21]

To impose basic theoretical structure on the KLP framework, one can broadly think of cognitive capital as a vector composed of general and task-specific cognitive capital. Drawing on contemporary cognitive psychology, I choose *general* cognitive capital to be represented by *working memory* – the ability to maintain relevant information accessible in memory when facing information interference and to allocate attention among competing uses while executing cognitively complex tasks. Working memory tests are strong and robust predictors of general “fluid intelligence” and performance in a broad range of cognitive tasks requiring controlled (as opposed to automated) information processing (e.g., Feldman-Barrett et al., 2004; Kane et al., 2004). Further, compared to alternative measures of general cognitive capital such as general fluid intelligence, working memory is more firmly established theoretically, neurobiologically and psychometrically (e.g., Engle and Kane, 2004).

Despite the wide-ranging predictive power of working memory in cognitive tasks studied by psychologists, working memory researchers themselves note almost complete lack of studies on the role of working memory in everyday information processing, especially in real-world problem-solving (“insight”) tasks requiring their solution to be gradually discovered (Hambrick and Engle, 2003).[22] Since many cognitively demanding, individual

[21] The causal effect of cognitive abilities has been extensively addressed in the field, for example in examining human capital determinants of schooling and labor market outcomes (e.g., Cawley et al., 2001; Heckman and Vytlacil, 2001; Heckman et al., 2006; Plug and Vijverberg, 2003). However, labor economists have generally been unable to pay attention to specific forms of cognitive capital, i.e., to the underlying cognitive capital constructs. Furthermore, studying the interaction between cognitive abilities and financial incentives is inherently difficult in the field since cognitive abilities tend to be *a priori* unobserved in field situations where their interaction with financial incentives is most relevant, for example in within-firm compensation settings (e.g., Prendergast, 1999). I demonstrate that identifying the causal effect of specific cognitive capital constructs and studying their interaction with financial incentives and other personality characteristics proves more transparent in experimental settings.

[22] As an exception, Welsh et al. (1999) report that working memory shares substantial variance with performance in the Tower of London puzzle, a variant of the Tower of Hanoi puzzle (e.g., McDaniel and Rutström, 2001). Hambrick and Engle (2003) further note that although working memory strongly predicts general fluid intelligence, we do not yet know through which channels.

decision making tasks in economics are "insight" tasks by their nature, I situate my test of the KLP framework in such a setting.

As a tool for identifying the causal effect of working memory, I design a time-series forecasting task that requires maintaining forecast-relevant information accessible in memory while simultaneously processing it. The task therefore "activates" precisely the type of cognitive capital that working memory theoretically represents and facilitates an accurate identification of the causal effect of working memory on forecasting performance.[23] The causality test relies on manipulating the task's working memory load: two screens with forecast-relevant information are presented either concurrently or sequentially. Since the sequential (concurrent) presentation treatment features higher (lower) working memory load, working memory should be a stronger (weaker) determinant of forecasting performance, after controlling for other potentially relevant cognitive, personality (especially motivational) and demographic determinants of forecasting performance. This causality hypothesis is confirmed for individual differences in asymptotic forecasting performance.

To control for the effect of *task-specific* cognitive capital, I measure short-term memory which cognitive psychologists often regard as a task-specific cognitive capital counterpart of working memory (e.g., Engle et al., 1999). I find that both working memory and short-term memory have a causal effect on forecasting performance. Basic arithmetic abilities, another task-specific form of cognitive capital, predict forecasting performance but only in the less memory-intensive concurrent presentation treatment. Since other forms of task-specific cognitive capital such as prior forecasting expertise could be vital for performance but are hard to measure, I intentionally minimize their potential relevance by

[23] The channels behind the causal relationship might be numerous, both direct and indirect. For example, working memory might influence forecasting performance not only directly through affecting subjects' ability to effectively combine forecast-relevant information, but also indirectly through affecting their ability to develop efficient forecasting algorithms or strategies (e.g., Barrick and Spilker, 2003; Libby and Luft, 1993). Psychologists have further argued that not only the objective cognitive capital predispositions but also their self-perception and confidence in them (self-efficacy) may separately influence performance (e.g., Bandura and Locke, 2003). I discuss the alternative channels throughout the paper but do not explicitly address their relative importance.

implementation features detailed later. I further obtain a proxy for prior forecasting expertise but controlling for it leaves other results intact.

The KLP framework further warrants attention to motivational determinants of forecasting performance. I find that even under high-powered financial incentives, intrinsic motivation to some extent fosters forecasting performance. Also, individuals who win a large windfall financial bonus immediately prior to the forecasting task are able to forecast considerably better, everything else held constant. Exploring the predictive power of other personality characteristics, forecasting performance seems positively influenced by risk aversion and negatively by math anxiety. In sum, controlling for the alternative determinants of performance heterogeneity provides a clearer interpretation of the causality of working memory by confirming its robustness across alternative model specifications.

The next two sections introduce the forecasting task and experimental design and review the measured cognitive, personality and demographic covariates. The final two sections present the results and discuss their potential caveats, extensions and applications.

2. The forecasting task and experimental design

2.1 <u>The forecasting task</u>

The tool used for identifying the causal effect of working memory on economic performance is a time-series forecasting task. Subjects repeatedly forecast a deterministic seasonal process, Ω_t, of the following form:

$$\Omega_t = B_t + \Sigma_{s=1,2,3}\, \gamma_s D_{st} + \eta_t = B_t + \gamma_1 D_{1t} + \gamma_2 D_{2t} + \gamma_3 D_{3t} + \eta_t$$

$D_{1t}=1$ if $t=1,4,7,\ldots 100$; 0 otherwise

$D_{2t}=1$ if $t=2,5,8,\ldots 98$; 0 otherwise

$D_{3t}=1$ if $t=3,6,9,\ldots 99$; 0 otherwise

$\gamma_1 = 46,\ \gamma_2 = 34,\ \gamma_3 = 18$

$B_t \sim$ i.i.d. uniform $\{10, 20, 30, 40\}$

$\eta_t \sim$ i.i.d. uniform $\{-8, -4, 0, 4, 8\}$

Ω_t contains a state variable, B_t, a three-period seasonal pattern, $\Sigma_{s=1,2,3}\,\gamma_s D_{st}$, and an additive error term, η_t. In each period *t*, subjects forecast the value of Ω_{t+1} based on observing eight-period "history windows," ($B_t,\ldots,B_{t-7}$) and ($\Omega_t,\ldots,\Omega_{t-7}$), on their screen. Subjects also observe B_{t+1} to be able to forecast Ω_{t+1}. However, neither the length nor the parameters of the seasonal pattern are revealed to subjects. Hence discovering the seasonal pattern and combining it with the observed values of B_{t+1} is the key to accurately forecasting Ω_{t+1}. After each forecast, F_{t+1}, subjects receive feedback in terms of their current forecast error, Ω_{t+1}-F_{t+1}.[24]

The seasonal pattern, $\Sigma_{s=1,2,3}\,\gamma_s D_{st}$, and the B_t process both account for approximately equal shares of the total variance of Ω_t (namely 49% and 41%, respectively, with the remaining 10% attributable to the variance of η_t). As a consequence, the variability of B_t "masks" the seasonal pattern which cannot be inferred from past values of Ω_t alone. Subjects must instead attend to the differences between past values of Ω_t and B_t in order to infer the

[24] In fact, subjects are simply told by how much their forecast, F_{t+1}, is above or below Ω_{t+1}. Subjects are repeatedly reminded in the instructions that η_{t+1} is unpredictable, and they are guided through the implications of the presence of η_{t+1} for their interpretation of the observed "noisy" forecast errors, Ω_{t+1}-F_{t+1} (as opposed to the "true" forecast errors, Ω_{t+1}-F_{t+1}-η_{t+1}, the absolute value of which is used to measure forecasting performance). Judging from responses in a debriefing questionnaire (see Appendix 2), the instructions were successful in achieving subjects' understanding of the role and implications of η_t, something that people apparently have trouble comprehending in forecasting experiments where the implications of randomness are (often purposefully) not clarified (e.g., Dwyer et al., 1993; Hey, 1994; Maines and Hand, 1996; Stevens and Williams, 2004).
Providing only current-period forecast errors rather than a sequence of past forecast errors is meant to limit the possibility that subjects apply a simplifying feedback-tracking (exponential smoothing) forecasting heuristic often reported in the forecasting literature (e.g., Hey, 1994). I nevertheless note the potential caveat that due to subjects' varying desire to know more about their forecasting performance progress, not providing more extensive visual feedback might lead to subjects allocating differential amounts of their scarce memory resources to keeping track of how well they are doing, which might in turn dilute the power of the measured memory proxies in explaining forecasting performance *per se*. Arguably, however, providing current-period feedback is still better than providing none (e.g., Hey, 1994). Throughout the task, subjects are not provided with earnings feedback (beyond what they can infer from their forecast errors) in order to limit the potential impact of wealth accumulation on forecasting performance (e.g., Ham et al., 2005).

seasonal pattern.[25] Of course, the presence of η_t means that subjects can only extract past values of Ω_t-B_t = γ_s+η_t. Hence discovering the exact seasonal parameters, γ_s, is a gradual, memory-intensive signal extraction task.[26] The memory load does not cease entirely even after discovering the seasonal pattern since subjects continuously need to keep track of the revolving seasonal pattern and to combine it with B_{t+1} in order to form their forecasts of Ω_{t+1}.

The character of the forecasting task reflects a consensus among psychologists on the cue-discovery nature of human learning in probabilistic environments. Even in the presence of random error, people seem proficient at discovering which cues in their probabilistic environment are important (e.g., Dawes, 1979; Klayman 1984 and 1988), as opposed to learning the exact weights attached to a given set of cues, especially correlated ones (e.g.,

[25] In the paper instructions preceding the computerized forecasting experiment (see Appendix 1 for the English version of the instructions), subjects observe examples of seasonal patterns of various lengths and are advised to attend to the observed past values of Ω_t-B_t = γ_s+η_t to be able to gradually extract the seasonal parameters, γ_s. Furthermore, before proceeding to the forecasting task, subjects are required to complete a computerized training screen that tests their understanding of how Ω_t is collectively determined by its three components (see Appendix 3).
However, subjects are told neither how many nor which past values of Ω_t-B_t to attend to. The seemingly most efficient forecasting strategy would first focus on detecting the length of the seasonal pattern, perhaps by experimenting with various lengths, and then on accumulating season-specific information for each of the γ_s+η_t distributions, conditional on γ_s, to be able to extract the means of the distributions, γ_s. Nevertheless, a debriefing questionnaire (see Appendix 2) suggests that most subjects relied on less efficient (and likely more memory-intensive) forecasting strategies, attending to *successive* Ω_t-B_t values in an attempt to create a long enough "virtual" sequence of γ_s+η_t values that would allow them to gradually recognize the seasonal pattern. The debriefing questionnaire also offers suggestive evidence that subjects with higher working memory used more efficient forecasting strategies resembling the efficient strategy described above. This raises the possibility of an indirect "capital-strategy-performance" channel mentioned earlier but this paper does not address the relative importance of the channel.
[26] A sequence of pilots have indicated three key aspects of the cognitive complexity associated with extracting γ_s from γ_s+η_t: the number of values in the support of η_t; the degree of "overlap" of the γ_s+η_t distributions, conditional on γ_s (i.e., their degree of non-monotonicity and non-uniqueness relative to each other; see also the discussion of "type complexity" in Archibald and Wilcox, 2006); and the size of the "history window." Given the forecasting abilities in the student subject pool at hand, the present parameterization of γ_s and η_t has the convenient properties of bounding forecasting performance of a majority of subjects away from perfection throughout the task (and hence preserving financial incentives for learning) and generating sufficient potentially predictable between-subject variance in forecasting performance to be explained by individual cognitive, personality and demographic characteristics.

Hammond et al., 1980; Brehmer, 1980). These findings have been largely confirmed by the time-series forecasting and expectation formation experimental literatures: subjects are generally not very good intuitive forecasters when it comes to determining parameter values of stochastic time series with even simple autoregressive or moving-average components (e.g., Hey, 1994; Maines and Hand, 1996); by contrast, subjects are good at detecting recognizable patterns in even relatively complex real-world time series (e.g., Lawrence and O'Connor, 2005). Therefore, my subjects should generally be capable of discovering the deterministic seasonal pattern even in the presence of randomness, η_t, but I challenge them further by introducing the state variable, B_t, that raises the memory load.

The time-series forecasting literature further documents that when the nature of the forecasted process permits so – for example, when the time series contains correlated past values or a trending component or both – subjects tend to employ various "natural" simplifying heuristics of the Kahneman and Tversky (1984) kind. They almost invariably anchor their forecasts on the most recent past value of the forecasted process and adjust it either for a previous trend (extrapolation heuristic), or for a long-term average (averaging heuristic), or for their previous forecast error(s) (exponential smoothing heuristic). These simplifying heuristics make forecasting strategies appear boundedly rational and ultimately reduce the overall memory load of forecasting tasks (e.g., Harvey et al., 1994; Hey, 1994). To minimize the possibility that such simplifying heuristics (and their heterogeneity across subjects) dilute the memory load of my forecasting task, I choose a forecasting process that intentionally curbs the effectiveness of the heuristics and creates substantial opportunity cost to their use.[27]

[27] The ineffectiveness of the heuristics follows from the deterministic nature of the seasonal pattern, combined with the relatively high variance of B_t discussed earlier. Also contributing to the ineffectiveness of simplifying heuristics is the absence of a trending component in Ω_t. The relatively high opportunity cost of using a particular averaging heuristic, which I call a mechanical forecasting algorithm, is illustrated below in relation to the payoff function. The detailed task-property feedback in the instructions (see Appendix 1 and 3) is meant to further suppress the activation of simplifying heuristics and to instead encourage the use of memory-intensive, financially rewarding forecasting strategies described earlier.

2.2 The causality identification approach

To identify the impact of working memory on forecasting performance, the experimental design consists of two between-subject treatments that vary in their working memory load (and likely also in their short-term memory load).[28] The working memory load manipulation is achieved through temporal separation of the forecast-relevant information that subjects observe. In the treatment with higher working memory load, the two screens with the values of ($B_{t+1},\ldots,B_{t-7}$) and ($\Omega_t,\ldots,\Omega_{t-7}$), respectively, are in each period displayed sequentially – call this treatment T_{seq}. By contrast, in the treatment with lower working memory load, the two screens are displayed concurrently – call this treatment T_{con}.

To see the difference in the working (and short-term) memory load between T_{seq} and T_{con}, recall that in order to extract the seasonal pattern, subjects need to attend to the differences between past values of Ω_t and B_t. *Ceteris paribus*, doing so is unambiguously more memory-intensive in the sequential presentation treatment, T_{seq}, where subjects repeatedly need to memorize past B_t values of their choice from the ($B_{t+1},\ldots,B_{t-7}$) screen and then recall them and subtract them from the appropriate Ω_t values once the ($\Omega_t,\ldots,\Omega_{t-7}$) screen appears. By contrast, subjects in the concurrent presentation treatment, T_{con}, observe the ($B_{t+1},\ldots,B_{t-7}$) and ($\Omega_t,\ldots,\Omega_{t-7}$) screens parallel to each other and so can combine past B_t and Ω_t values visually. Hence T_{con} supplies "external memory" for the calculation of past values of Ω_t-B_t which relaxes the memory load of the calculation and leaves more memory resources for the actual extraction of the seasonal pattern. On the other hand, no such "external memory" is available in T_{seq} where past values of Ω_t-B_t must be calculated virtually, leaving less scarce memory resources for extracting the seasonal pattern.[29]

[28] The identification approach based on cognitive load manipulation has long been used by psychologists and especially working memory researchers in various modifications to study the causal effect of working memory on lower-order and higher-order cognitive processes (e.g., Baddeley and Hitch, 1974; Engle et al., 1999). Hambrick et al. (2005) provide an overview of the identification approach referred to as "microanalytic," as opposed to the "macroanalytic" approach that addresses the relationship between working memory and other cognitive constructs through latent variable modeling (e.g., Kane et al., 2004).

[29] In T_{con}, subjects observe the two parallel ($B_{t+1},\ldots,B_{t-7}$) and ($\Omega_t,\ldots,\Omega_{t-7}$) screens for 15 seconds. In T_{seq}, subjects observe the ($B_{t+1},\ldots,B_{t-7}$) screen for 10 seconds and subsequently the ($\Omega_t,\ldots,\Omega_{t-7}$)

I therefore tailor the design so that, as hypothesized, working memory *a priori* constitutes the central form of cognitive capital required to solve the forecasting task, especially in the more memory-intensive T_{seq} treatment. In fact, the cognitive load imposed in T_{seq} closely matches the aspects of cognition theoretically underlying the working memory construct, namely maintenance of relevant information in active memory, resolution of conflicting information and controlled allocation of attention (Engle and Kane, 2004). Put differently, forecasting in T_{seq} predominantly requires the use of System 2 (controlled processing) type of cognitive capital, of which working memory is a fundamental component. On the other hand, forecasting in T_{con} is likely to pose a much more reflexive, pattern-recognition exercise requiring mostly the use of System 1 (automated processing) type of cognitive capital (e.g., Feldman-Barrett et al., 2004; Stanovich and West, 2000).

The treatment variation in the working memory load permits identifying the causal effect of working memory on forecasting performance by testing the following hypothesis:

> Hypothesis: *Ceteris paribus*, since T_{seq} features higher working memory load compared to T_{con}, working memory has a stronger impact on forecasting performance in T_{seq} compared to T_{con}.

Ceteris paribus refers not only to the fact that, except for manipulating the working memory load, other features of the forecasting task remain intact.[30] It also means allowing for the possibility that, besides working memory, the forecasting task activates other forms

---

screen for 15 seconds. While this arrangement does not offer the same total time across treatments for observing the forecast-relevant information, it does offer the same "processing" time of 15 seconds for combining the forecast-relevant information, be it visually in T_{con} or virtually in T_{seq}. As regards the remaining screens, the feedback screen appears for 5 seconds in either treatment, and the two screens where subjects place their forecasts and bets (see below) are not time-constrained, allowing subjects to go along the forecasting task at their own pace. The working memory literature illustrates that sensible time constraints (and, more generally, individual differences in effort duration and intensity) are inconsequential for the relationship between working memory and cognitive performance. If anything, especially individuals with high working memory seem to take advantage of extra processing, coding or rehearsal time when time constraints are relaxed (Engle and Kane, 2004; Heitz et al., 2006).

[30] The manipulation of the memory load appears inconsequential as regards the surface features of the forecasting task, though it might alter the nature and effectiveness of forecasting strategies. Circumstantial evidence from a debriefing questionnaire (see Appendix 2) suggests that forecasting strategies were on average less efficient in the sequential presentation treatment.

of cognitive capital and that these also have a causal effect on performance. As detailed in the next section, I measure two additional forms of cognitive capital that are more task-specific in their nature compared to working memory, namely short-term memory and basic arithmetic skills. I also control for individual heterogeneity in personality (especially motivational) and demographic characteristics that may be relevant for forecasting performance and further might be correlated with cognitive characteristics.

The fact that subjects know the distribution of the components of Ω_t, combined with the detailed, example-oriented nature of the task instructions, make the forecasting task a logical rather than a statistical forward induction problem. This is meant to *a priori* minimize the influence of task-specific cognitive capital that accrues from prior forecasting expertise.[31] Another sense in which the impact of prior expertise is minimized is that forecasting performance is measured “asymptotically,” i.e., after learning in the forecasting task has ceased.[32] Prior expertise (or domain knowledge) effects, usually investigated as average treatment effects, have been frequently documented in the laboratory and the field.[33] Yet *individual differences* in prior expertise are hard to measure, and thus suppressing their potential importance seems desirable given my primary focus on the causal effect of general cognitive capital, namely working memory. It is nevertheless still possible that my measured cognitive, personality and demographic characteristics do not capture some aspects of prior expertise relevant for the forecasting task at hand, such as

[31] The detailed, example-oriented instructions are further meant to reduce the likelihood that subjects impute their own, possibly erroneous, forecasting context based on their past experience with solving “similar” forecasting problems (in the sense of Harrison and List, 2004). The Discussion section outlines a simple robustness check for this possibility, as part of a broader discussion of expertise effects.

[32] See later sections for details on measuring “asymptotic” forecasting performance. Evidence from cognitive psychology suggests that experience gained through on-task learning tends to be the most productive component of task-specific cognitive capital that often overrides the influence of prior expertise (e.g., Ericcson and Smith, 1991; Anderson, 2000).

[33] See, for example, Camerer and Hogarth (1999) and Libby and Luft (1993) for reviews. Rydval (2005) offers suggestive evidence on the interaction of prior expertise (accounting knowledge) and financial incentives in a memory recall task. Prior expertise is also likely to play a role in real-world forecasting settings. However, the experimental literature on forecasting company earnings provides inconclusive evidence on differences in forecasting performance of experienced and inexperienced forecasters, both in the lab and the field (e.g., Hunton and McEwen, 1997). See also Libby, Bloomfield, and Nelson (2002) for an overview of the company earnings forecasting literature, and the Discussion section for a further elaboration on expertise effects.

pattern recognition skills in the presence of randomness. I address this issue in the Results section and obtain a useful proxy for prior forecasting expertise.

2.3 The properties of forecasting sequences and the payoff function

Both T_{seq} and T_{con} feature the same set of Ω_t forecasting sequences. The sequences are "standardized" in terms of several theoretically relevant aspects of their forecasting complexity, henceforth "Ω_t-complexity," in order to retain basic control over how Ω_t-complexity varies across subjects.[34] Nevertheless, it is unlikely that the standardization would capture all empirically relevant aspects of Ω_t-complexity, and hence one should take into account the impact of the between-subject variance in Ω_t-complexity on forecasting performance, parametrically or otherwise.[35] In the multivariate analysis below, I adopt one

[34] As part of the standardization, only the η_t streams vary across subjects; the remaining components of Ω_t are identical across all subjects. Hence B_t is in fact not drawn entirely at random and is identical across subjects, consisting of a sequence of permutations on the support of B_t, {10,20,30,40}, that are selected and adjoined in such a way as to avoid repeating values and easily memorable sequences. Further, each B_t value is paired with each value of the seasonal pattern approximately equally often.

The η_t streams vary across subjects and their first 75 periods are generated randomly (after period 75, the η_t streams repeat a previous segment for reasons explained later). The 75-period η_t streams are to some extent standardized in terms of the complexity of extracting the seasonal parameters from past $\gamma_s+\eta_t$ realizations. The theoretically most important complexity characteristic is the frequency of events with which subjects encounter the full range of the $\gamma_s+\eta_t$ distributions, conditional on γ_s, for only after observing the range can a given seasonal parameter, γ_s, be determined with certainty. The arguably most salient aspect of this complexity characteristic is the frequency of events with which the range of a given $\gamma_s+\eta_t$ distribution, conditional on γ_s, can be *visually* inferred from successive seasonal realizations of Ω_t and B_t. To operationalize this complexity characteristic, all the 75-period η_t streams contain six such events (summed across seasons), six being approximately the sample mean of the frequency of the events for randomly generated 75-period η_t streams.

Another complexity characteristic common to all of the 75-period η_t streams is that their sample mean is approximately zero (i.e., the sample mean never significantly differs from zero based on a t-statistic at the 1% significance level). Also, the sampling variance of the 75-period η_t streams, measured in period 45, varies between 27 and 37, approximately the 10^{th} and 90^{th} percentiles, respectively, of the appropriate sampling variance distribution for randomly generated 75-period η_t streams. This condition is to ensure that the η_t streams are not too improbable in the early stages of the task where most learning occurs. I am greatly indebted to Nat Wilcox for guiding me through the design process of generating η_t streams with the desirable complexity characteristics.

[35] In a panel estimation not reported in this paper, I parameterize a broad set of Ω_t-complexity characteristics – variants of those listed in the previous footnote – that vary broadly between and within subjects throughout the forecasting task. I find that several of these characteristics weakly

possible solution to this issue based on removing the impact of Ω_t-complexity altogether. Specifically, provided that the effect of Ω_t-complexity on forecasting performance does not interact with the effect of cognitive, personality and other individual characteristics (including heterogeneity in forecasting strategies), the effect of Ω_t-complexity can be removed by comparing forecasting performance of the pairs of subjects facing identical Ω_t forecasting sequences across the two treatments.

As detailed below, I measure forecasting performance in terms of the "true" absolute forecast errors, *abs*(Ω_{t+1}-F_{t+1}-η_{t+1}). I focus on performance in a couple of distinct twelve-period segments of the 100-period forecasting task, namely in the EARLY segment (periods 21-32) and in the LATE segment (periods 84-95). For each subject, the EARLY and LATE segments of Ω_t (as well as the eight periods directly preceding them) are exactly matched in terms of all the Ω_t components, on a period-by-period basis. Each subject thus forecasts the same segment of his/her Ω_t sequence twice, first the EARLY segment and after a while the LATE segment, based on observing the same forecast-relevant information.[36] One advantage of this design feature is that a comparison of each subject's performance in the EARLY and LATE segments yields an unambiguous within-subject measure of learning in the forecasting task. As discussed below, another advantage is that the correlation between forecasting performance in the EARLY and LATE segments provides a useful indicator of the internal reliability of the chosen forecasting performance measures.

The payoff function in the forecasting task has the form of a betting scheme. At the very beginning of each period, i.e., prior to observing the screens with forecast-relevant information, subjects are asked to bet an amount x_t on their forecast, F_{t+1}. They can bet up

influence forecasting performance in early, learning stages of the forecasting task (for example, season-specific biases of the η_t streams seem to negatively affect performance) but much less so in later, asymptotic stages of the task.

[36] Reflecting findings from pilots, the EARLY segment is positioned sufficiently "late" in the Ω_t sequence to ensure task salience before measuring the EARLY segment's performance. The LATE segment is positioned just before the end of the 100-period forecasting task in order to avoid lapses of concentration in the last forecasting periods affecting the LATE segment's performance. See more detailed discussion in the Results section.

to M=100 ECU but at least x_{min}=50 ECU so that they always have sufficient financial incentives to forecast accurately. The payoff (in ECU) in period *t*, π_t, then depends on the "noisy" absolute forecast error, $abs(\Omega_{t+1}-F_{t+1})$, as well as on the amount bet, x_t:

$$\pi_t = x_t\theta g_t + (1-\theta)(M-x_t), \text{ where } x_{min} \leq x_t \leq M \text{ and } g_t = max\{c - abs(\Omega_{t+1}-F_{t+1}),0\}$$

M=100 ECU

x_{min}=50 ECU

c=20

θ=0.1

The return to betting, θg_t, is a negative linear function of the "noisy" absolute forecast error (as long as the forecast error does not exceed c whereby the return to betting becomes zero). On the other hand, every ECU not bet earns a riskless return of $(1-\theta)$. Clearly, betting $x_t>x_{min}$ is profitable only if $g_t>(1-\theta)/\theta$, i.e., only if $abs(\Omega_{t+1}-F_{t+1})<11$. The net gain from betting $x_t>x_{min}$ hence becomes positive only if subjects manage to reduce their "noisy" absolute forecast errors below 11 on average. As the (sample) mean of η_t is zero, the same simple rule also applies to the "true" absolute forecast error.[37]

The parameterization of the payoff function is conveniently linked with the parameterization of the Ω_t process. To see this, consider forecasting performance of a mechanical forecasting algorithm that, instead of focusing on extracting the seasonal pattern, forms its point forecast simply by adding B_{t+1} to the average of the three most

---

[37] To make the betting scheme conceptually transparent, the paper instructions explain in detail that not only forecasting accuracy pays, but also that the more accurately subjects forecast on average, the more profitable betting $x_t>x_{min}$ becomes on average. Recall that subjects are also guided through the implication of the presence of η_{t+1} for the interpretation of their "noisy" forecast errors, $F_{t+1}-\Omega_{t+1}$. One of the computerized training screens preceding the forecasting task tests subjects' understanding of the payoff function (see Appendix 3). A full payoff table is provided to subjects but they are reminded that it is far more important to understand the simple logic of how to bet profitably. The instructions also provide subjects with basic context for why they are required to bet on their forecasts in order to make it less likely that subjects provide their own, possibly misleading betting context (e.g., Harrison and List, 2004).

recent past values of Ω_t-B_t. When the mechanical forecasting algorithm is applied to the set of Ω_t forecasting sequences used in the experiment, its mean "noisy" absolute forecast error is approximately 11.3 on average (varying slightly across Ω_t sequences due to the variability of η_t streams described earlier), i.e., just outside the region of absolute forecast errors where betting $x_t>x_{min}$ is profitable. Hence to find betting $x_t>x_{min}$ profitable, subjects must perform better than the mechanical forecasting algorithm: they must attempt to discover the seasonal pattern. In turn, being able to reap the gains from betting should be a highly motivating factor for extracting the seasonal parameters, γ_s, as accurately as possible.[38]

3. The measured covariates and other implementation details

3.1 Working memory and other cognitive characteristics

In order to test the causal effect of **working memory** on forecasting performance, I measure working memory by a "working memory span" test, specifically by an automated (computerized) version of the "operation span" test (Turner and Engle, 1989). In a typical working memory span test, subjects are presented with sequences of to-be-remembered items interspersed with an "attention interference" task. Specifically, the automated operation span test requires subjects to remember sequences of briefly presented letters interspersed with solving simple mathematic equations.[39] At the end of each

[38] One reason I make subjects bet on their forecasts is to keep the relatively lengthy forecasting task intellectually stimulating throughout. Another reason is to extract a decision-relevant, incentive-compatible measure of confidence in forecasting abilities, and to analyze how the confidence evolves over time in relation to the evolution of forecasting performance. As mentioned earlier, psychologists have argued that confidence in one's cognitive capital or decision making abilities (self-efficacy) may have an indirect positive effect on performance beyond the direct effect of cognitive capital itself (e.g., Bandura and Locke, 2003). After removing the effect of personality characteristics (such as risk aversion) from the betting behavior, it will be possible to examine whether the "residual" measure of confidence in forecasting abilities indeed fosters forecasting performance beyond the direct effect of forecasting abilities themselves. Betting behavior is not analyzed in this paper since doing full justice to the analysis requires collecting more observations. See the Discussion section for more details.

[39] Subjects in fact determine, in a true-false manner, whether the equations presented on the screen are solved correctly (e.g., "(9/3)-2=2?"). The computer initially measures subjects' individual speed of solving the equations and subsequently requires subjects to maintain the speed throughout the operation span test while also maintaining solution accuracy.

sequence, subjects are asked to recall as many letters as possible in the correct positions in the sequence. The operation span test score is based on the total number of correctly remembered letters, summed across numerous letter sequences of various lengths.[40]

As mentioned earlier, working memory constitutes theoretically and neurobiologically a well-defined general cognitive capital construct, and working memory span tests have strong internal reliability (e.g., Conway et al., 2005). Both theoretically and psychometrically, working memory appears superior to alternative, potentially broader tests of general cognitive abilities such as the “Beta III” test or the “Raven” test.[41] This is important given my focus on accurately identifying the causal effect of general cognitive capital. Put differently, in trying to understand the effect of general cognitive capital on economic performance, it seems more effective to start with exploring rather reductionistic general cognitive capital constructs such as working memory, preferring clarity of interpretation over breadth of measurement (e.g., Kane et al., 2004).

The above reasoning applies also to the second potentially relevant form of cognitive capital, namely **short-term memory**. I measure short-term memory by an automated (computerized) auditory “digit span” test, closely resembling the individually-administered Wechsler digit span test (e.g., Devetag and Warglien, 2003). Short-term memory span tests of the digit span variety require subjects to remember sequences of items of various lengths.[42] They are thought to reflect information storage capacity as well as information

[40] Alternative scoring procedures are described in Conway et al. (2005).

[41] The Beta III test is a set of “matrix reasoning,” “coding speed” and other nonverbal tasks (Kellogg and Morten, 1999); the Raven test and its variants are also “matrix reasoning” tests (Raven et al., 1998). These and similar nonverbal cognitive ability tests are thought to capture general “fluid intelligence” (e.g., Ackerman et al., 2002). In Ballinger et al. (2005), a sum of two analytical components of the Beta III test significantly predicts performance in their precautionary saving task, similar in predictive power to the operation span test.

[42] The auditory digit span test requires subjects to recall pseudo-random (not easily memorable) sequences of digits of various lengths immediately after hearing each sequence in the earphones. The test starts with a set of five three-digit sequences. If at least two of the five sequences are recalled entirely correctly, the sequence length increases to four digits (otherwise the sequence length decreases to two digits) and another set of five sequences follows. The same sequence-length rule applies throughout the whole test (except that the sequence length never decreases below one). Subjects complete eight sets of five sequences in total, thus being able to reach a maximum sequence length of ten digits, but most subjects reach much less than that. From several alternative digit span test scores, I use the one that is most directly comparable to my

coding and rehearsal skills that make the stored information better memorable (e.g., Engle et al., 1999). In the digit span test, for example, coding and rehearsing digits in short sub-sequences rather than memorizing them individually (i.e., “chunking” digits together) permits memorizing longer digit sequences overall. Such coding and rehearsal strategies are assumed to be eliminated from *working* memory span tests through the presence of an attention interference task, which in turn is the only differentiating design feature ensuring that the working and short-term memory span tests measure separate cognitive constructs.[43]

Being able to store, code and rehearse (“chunk”) forecast-relevant information might influence forecasting performance, for instance by affecting the number of past B_t values that subjects in the more memory-intensive T_{seq} treatment are able to memorize before the screen with past Ω_t values appears. Hence it seems well justified to pay attention to short-term memory, besides working memory, as a potentially relevant cognitive capital measure that might also have a causal effect on forecasting performance.[44] Nevertheless, short-term memory should not be regarded as a *general c*ognitive capital measure. It is a more *task-specific* cognitive capital measure, specific to the memory-intensive nature of the forecasting task. The working memory literature extensively documents that short-term memory is not as strongly related to general fluid intelligence and to performance in tasks requiring controlled information processing as is working memory.[45] In fact, the literature usually views working memory and short-term memory as comprising a functional working memory *system*, with working memory being the central component representing the ability to control attention and short-term memory being the supporting storage, coding

operation span test score described earlier, namely the total number of correctly remembered digits in the correct serial position summed across all sequences.

[43] In the working memory literature, short-term memory span tests are often referred to as “simple span” tests, precisely because the attention interference task is absent from them. Simple span tests usually have reasonable internal reliability (e.g., Kane et al., 2004).

[44] While cognitive psychology offers alternative short-term memory tests that do not allow “chunking,” such as the visual short-term memory test (e.g., Covan, 2001), I use the digit span test precisely because “chunking” skills might influence forecasting performance *and* are not captured by my working memory span test.

[45] This is particularly true if short-term memory is measured by verbal or numerical tests, such as the digit span, as opposed to spatial short-term memory span tests that seem to have more general predictive power (e.g., Kane et al., 2004).

and rehearsal component (e.g., Kane et al., 2004; Heitz et al., 2005).[46] As detailed below, I follow the practice common in the working memory literature and extract the "controlled attention" component from the working memory and short-term memory span test scores. This in turn allows me to provide a more accurate causality test for working memory (i.e., controlled attention) and to contrast it with the effect of short-term memory, further enhancing clarity of interpretation.[47]

As a last potentially relevant cognitive capital form,[48] even more task-specific in its nature, I measure basic **math** abilities under time pressure. I administer an "addition and subtraction" test in two parts, with 60 items and a two-minute time limit in each of them. The test sheets have alternating rows of 2-digit additions and subtractions, such as "25+29=__" or "96–24=__."[49] The addition and subtraction test belongs to the class of basic arithmetic skill tests provided by the "ETS Kit of Referenced Tests for Cognitive Factors" (Ekstrom et al., 1976). The tests are assumed to measure the ability to perform basic arithmetic operations with speed and accuracy but are not meant to capture mathematical reasoning or higher mathematical skills. The addition and subtraction test closely matches the basic arithmetic skills required in the forecasting task and hence can be regarded as a task-specific cognitive capital measure. While I have no strong priors as regards the relative impact of basic math skills on forecasting performance across

[46] One could perhaps view short-term memory as a clinically valid component of the system (i.e., a memory capacity benchmark in an idealized setting without attention interference), and working memory as an ecologically valid component (i.e., the ability to maintain and effectively allocate attention).

[47] As Conway et al. (2005) point out, this clarity is not achieved when using alternative "dynamic" short-term memory tests, such as the "*n*-back" task (e.g., Kirchner, 1958) that by their nature fall somewhere between the short-term and working memory span tests used here.

[48] One might argue for additionally including a measure of perceptual speed abilities as these apparently matter for basic encoding and comparison of items (such as numbers) under time pressure (e.g., Ackerman et al., 2002). Nevertheless, the working memory literature points out that complex perceptual speed tasks and working memory span tests share substantial variance and that the causality appears to run from working memory to perceptual abilities rather than vice versa (e.g., Heitz et al., 2005).

[49] Subjects are asked to calculate as many correct answers as possible but are also told that due to the strict time limit they are unlikely to be able to calculate all of them. The test and retest sheets are separated by a couple of unrelated tasks with a 15-20 minute gap between them. The math score is constructed as the total count of correct answers on both test parts. The test-retest reliability of the math score as measured by the Pearson correlation coefficient is 0.852.

treatments, the impact is likely to be overridden by the working and short-term memory constraints activated in the sequential presentation treatment.

3.2 Personality and demographic characteristics

Turning now to personality characteristics, my primary interest from the perspective of the KLP framework is clearly in individual heterogeneity in intrinsic motivation. Economists and especially psychologists have accumulated considerable theoretical and empirical work on the relationship between extrinsic motivation (ranging from performance-independent in-kind transfers to high-powered, performance-contingent financial incentives) and intrinsic motivation to perform well in a task (cognitive or physical, easy or demanding, interesting or mundane). The literature discusses a multitude of non-trivial channels through which intrinsic and extrinsic motivators might interact but provides inconclusive evidence for or against them. In certain task domains, high-powered financial incentives may "crowd-out" intrinsic motivation to exert effort and perform well (e.g., Deci et al., 1999).[50] Apparently, even non-salient financial incentives may have detrimental impact on intrinsic motivation and performance if people get discouraged by very low levels of performance-contingent pay (Gneezy and Rustichini, 2000; see also Rydval and Ortmann, 2004).

Not directly addressing any of the complex interactions, my goal here is much more basic. I include intrinsic motivation in the empirical model of forecasting performance in a reduced-form manner to account for the possibility that heterogeneity in subjects' intrinsic motivation to engage in the forecasting task affects their performance, especially in the more cognitively demanding T_{seq} treatment. I anticipate that, given the high-powered piece-rate financial incentives implemented in the forecasting task (see below), a direct effect of intrinsic motivation on forecasting performance is unlikely. However, intrinsic

[50] See Eisenberger and Cameron (1996) for an alternative interpretation of the (inconclusive) evidence behind the crowding-out hypothesis. McDaniel and Rutström (2001) and Ariely et al. (2005) find some empirical support for an alternative hypothesis referred to as the "distraction" hypothesis, embodied in the "Yerkes-Dodson law of optimal arousal" (Yerkes and Dodson, 1908), suggesting that high-powered incentives make people overly excited and lead to expending unwarrantedly high effort (i.e., not lower effort as predicted by the crowding out hypothesis) that subsequently turns out unproductive.

motivation might correlate with subjects' cognitive capital and thus not including it might confound the effect of cognitive and motivational characteristics on forecasting performance. Another reason for caution is that individual heterogeneity in intrinsic motivation might influence the *measured* cognitive characteristics.[51]

I measure intrinsic motivation by an item-response scale called "**need for cognition**," a well-established measure of the intrinsic motivation to engage in effortful, cognitively demanding tasks (e.g., Cacioppo et al., 1996). As with all other item-response personality scales discussed below, the need for cognition scale consists of a collection of statements. Subjects indicate their agreement or disagreement with each of the statements as follows: 1 = "entirely true," 2 = "mostly true," 3 = "mostly false" and 4 = "entirely false." Subjects are told that there are "neither good nor bad choices" and are asked to make choices most closely reflecting their attitudes and behavior. Since both positively and negatively worded statements are included, the choices are numerically recoded and each subject's score is the average of his/her recoded choices.[52]

As in the case of the need for cognition scale, the remaining personality scales are included in the empirical model of forecasting performance in a reduced-form fashion, as potential determinants of forecasting performance and potential correlates of the cognitive capital measures. Below I briefly introduce the personality scales and return to them when discussing the estimation results.

In particular, I use three of the four personality scales claimed by Whiteside and Lynam (2001) to capture various aspects of impulsive behavior: "**premeditation**" scale,

---

[51] Since subjects perform the cognitive tests for a flat fee rather than under performance-contingent financial incentives, intrinsic motivation might influence the cognitive test performance. I return to this issue in the Results section.

[52] Following Ballinger et al. (2005), I use a short version of the need for cognition scale of Cacioppo et al. (1984). The resulting shorter scale is more focused on eliciting intrinsic motivation attitudes and permits independently examining the predictive power of other personality scales described later. Subjects mark their choice for twelve statements such as "I would prefer complex to simple problems" or "I feel relief rather than satisfaction after completing a task that required a lot of mental effort" or "I really enjoy a task that involves coming up with new solutions to problems." The responses are recoded in such a way that a high overall score corresponds to high need for cognition. Ballinger et al. (2005) find virtually no impact of need for cognition on performance in their precautionary saving task.

"**sensation-seeking**" scale and "**perseverance**" scale (the fourth one being "urgency" scale).[53] Sensation-seeking attitudes have been found positively correlated with risk-taking behavior (e.g., Eckel and Wilson, 2004) and such attitudes might arguably be important for subjects' willingness to experiment with alternative forecasting strategies, for instance with alterative approaches to discovering the seasonal pattern and its length.[54] At the same time, sensation-seeking tends to be positively correlated with need for cognition (e.g., Crowley and Hoyer, 1989), so one ought to measure both to disentangle their impact. Premeditation attitudes might also be relevant for forming successful forecasting strategies, possibly complementing sensation-seeking.[55] Last, perseverance attitudes might matter because forecasting accurately throughout the lengthy forecasting task may require considerable mental determination, and especially because the key, "asymptotic" measure of forecasting performance is situated towards the end of the task.[56]

As a last scale in the item-response survey,[57] I use a "**math anxiety**" scale (e.g., Pajares and Urdan, 1996). Not only basic math skills but also anxiety to deal with numbers (under time pressure) could affect forecasting performance. Furthermore, similarly to intrinsic motivation, math anxiety may be a source of variance in the measured cognitive characteristics since the cognitive tests are number-intensive. The math anxiety scale is regarded as a measure of anxiety or feelings of tension that interfere with the manipulation

---

[53] The personality scales are discussed in more detail in Ballinger et al. (2005) where neither of them explains performance in their precautionary saving task.

[54] Subjects mark their choice for twelve statements such as "I sometimes like doing things that are a bit frightening" or "I generally seek new and exciting experiences and sensations" or "I'll try anything once." The responses are recoded in such a way that a high overall score corresponds to high sensation-seeking.

[55] Subjects mark their choice for eleven statements such as "My thinking is usually careful and purposeful" or "Before making up my mind, I consider all the advantages and disadvantages" or "I don't like to start a project until I know exactly how to proceed." The responses are recoded in such a way that a high overall score corresponds to high premeditation.

[56] Subjects mark their choice for ten statements such as "I finish what I start" or "Unfinished tasks really bother me" or "I am a productive person who always gets the job done." The responses are recoded in such a way that a high overall score corresponds to high perseverance.

[57] The five personality item-response scales are included in a single item-response survey and subjects encounter the various statements in a randomized order (identical across subjects). The item-response survey in fact includes an additional "judgmental confidence" scale to shed light on individual differences in betting behavior. I do not discuss the scale since the analysis of betting behavior is a focus of a separate study.

of numbers and the solving of math problems.[58] The math anxiety measure has been found correlated with mathematics achievement, aptitude and schooling grades (e.g., Pajares and Miller, 1994; Schwarzer et al., 1989), it has strong internal reliability (e.g., Betz, 1978), and it is closely related to other math-related psychological constructs such as math self-efficacy and math self-concept (e.g., Cooper and Robinson, 1991; Pajares and Miller, 1994).

In addition to the above personality scales, I also measure **risk** attitudes using a risk elicitation task in the multiple-price-list format (e.g., Holt and Laury, 2002).[59] Especially if sensation-seeking (and perhaps premeditation) attitudes turn out important for forecasting behavior, one may also want to have a direct measure of risk attitudes as usually measured by economists. While it is not immediately obvious how risk aversion could influence forecasting decisions *per se* (i.e., forecasts are not risky decisions in economic sense), risk attitudes could still play a role in the formation of forecasting strategies, as hypothesized above for sensation-seeking and premeditation attitudes.

Besides the cognitive and personality covariates, a questionnaire administered before the forecasting task was used to collect a set of demographic characteristics such as age, gender and university field of study. The questionnaire also collected proxies for family socioeconomic status that are later referred to as "**Carowner**" (a binary indicator for

[58] Subjects mark their choice for ten statements such as "When I am taking math tests, I usually feel nervous and uneasy" or "My mind goes blank and I am unable to think clearly when doing mathematics" or "Mathematics makes me feel uneasy and confused." Note that the responses are recoded in such a way that a high overall score corresponds to *low* math anxiety.

[59] I administer a risk elicitation battery with two identical booklets of six tables. Each table consists of an ordered list of risky choice pairs and subjects draw a horizontal line to indicate their willingness to switch from a fixed sure payoff to an increasingly attractive gamble. The average sure payoff across the six tables is 450 CZK (approximately PPP$35) but all choices are purely hypothetical. The test and retest booklets are separated by a couple of unrelated tasks with a 15-20 minute gap between them. The measure of risk attitudes is constructed as the summation of line locations in both test booklets. The test-retest reliability of the risk measure as indicated by the Pearson correlation coefficient is 0.936.

personal car ownership)[60] and “**Carshare**” (the number of functional cars per household member).[61]

Lastly, right after completing the collection of covariates (but before the forecasting task), subjects had a chance to win a substantial windfall financial bonus that could be regarded as a potentially interesting wealth proxy.[62] The substantial financial bonus, later referred to as “**Windfall**,” affected nine (out of 86) participants, eight earning 750CZK and one earning 1500CZK (approximately PPP$117). The multivariate analysis explores whether the bonus, though awarded completely exogenously with respect to the forecasting task, affects forecasting performance. However, I have no priors as to whether the bonus ought to foster or discourage *ex ante* intrinsic motivation to forecast well, and how the bonus interacts with the high-powered financial incentives implemented in the forecasting task itself.

3.3 Other implementation details

The experiment was conducted in seven experimental sessions, six in November 2005 and one in January 2006.[63] The subjects were full-time native Czech students (with a couple of exceptions permitted based on proficiency in Czech) from Prague universities and colleges, namely the University of Economics, the Czech Technical University, the Charles

[60] The questionnaire in fact also asked for a car price estimate but this information was not reported or was reported as a wide price range.

[61] Specifically, Carshare is the reported number of functional cars the household owned in the subject’s last year of high school divided by the reported number of household members in that year. Carshare varies across subjects in both its numerator and denominator and turns out only modestly correlated with Carowner (see Table 2a and Table 2b), so I use both of the wealth proxies in the multivariate analysis.

[62] In each experimental session, I conducted a short guessing game experiment from which 2-3 randomly selected subjects could earn as much as 1500CZK (approximately PPP$117), depending on their choice in the guessing game and the number of winners who split the amount. The chance of wining the bonus was pre-announced in the initial instructions. See Ortmann et al. (2006) for how subjects’ choices in the guessing game experiment are related to the cognitive, personality and demographic covariates discussed here.

[63] Due to concerns that subjects in successive experimental sessions might share information relevant for performing well in the forecasting task as well as in some of the cognitive tests, every attempt was made to ensure that successive sessions were overlapping or that subjects in non-overlapping sessions were recruited from different universities or university campuses. In retrospect, subjects’ behavior in the experiment – especially the lack of “perfect” performance in early stages of the forecasting task – suggests little or no degree of social learning.

University, and the Anglo-American College, with a majority of subjects recruited from the first two universities in approximately equal shares.[64]

Experimental sessions lasted approximately 4 hours on average (but no longer than 4.5 hours). The collection of covariates in the first part of each session usually lasted 1.5-2 hours and for logistic reasons was paced by the experimenter according to the slowest subject in a given session. For the completion, subjects earned a participation fee of 150 CZK (approximately PPP$12) and had a chance of earning the substantial financial bonus of 1500CZK (approximately PPP$117) discussed earlier. The order of covariate collection was the same across sessions, with the cognitive tests generally preceding the personality scales. The operation and digit span tests were conducted using E-prime (Schneider et al., 2002) while the remaining covariate collection was administered in a paper-and-pencil format.

After a 15-20 minute break, the forecasting task programmed and conducted in z-Tree (Fischbacher, 1999) lasted about two hours and was completed at each subject's individual pace. In the 92 forecasting periods (i.e., 100 periods *less* the first eight periods displaying the initial values of Ω_t and B_t), subjects could earn over 900CZK (approximately PPP$70). The average realized earnings across both treatments were 646CZK (approximately PPP$50). After finishing the forecasting task and completing the debriefing questionnaire (see Appendix 2), subjects were paid off privately in cash. All parts of the experiment were conducted anonymously (subjects were assigned a unique ID that they kept throughout the experiment).

A total of 95 subjects completed the whole experiment, five of whom did not meet an accuracy requirement of the working memory span test (their performance on the equation-

[64] The Czech Technical University is a relatively non-selective Prague university admitting technically-oriented students with heterogeneous educational background, while the Prague School of Economics is a relatively selective university admitting students with predominantly business-oriented background. However, the faculties within the two universities are rather heterogeneous in their admission requirements and curriculum content. Not reported in the Results section, I do not detect any differences in forecasting performance that might be related to subjects' university or faculty background, though the sample sizes entertained in the analysis are too small to draw any firm conclusions.

solving part of the test fell below a 85% speed/accuracy threshold normally required by working memory researchers), and four of whom did not follow the experimental instructions.[65] Excluding these nine subjects yields the final sample of 86 subjects, 43 in each treatment.

4. Results

4.1 Forecasting performance

As mentioned earlier, subject *i*'s forecasting performance in period *t* is measured in terms of his/her "true" absolute forecast error, $abs(\Omega_{i,t+1}\text{-}F_{i,t+1}\text{-}\eta_{i,t+1})$, henceforth simply "forecast error" unless otherwise noted. More specifically, let $M_{i,t}$ denote subject *i*'s twelve-period moving average of forecast errors up to period *t*. $M_{con,t}$ and $M_{seq,t}$ then denote the period-*t* averages of $M_{i,t}$ across subjects in the T_{con} and T_{seq} treatments, respectively.

Figure 1 displays the evolution of $M_{con,t}$ and $M_{seq,t}$ over time, illustrating that average forecasting performance is clearly better in the less memory-intensive T_{con} treatment throughout the whole task. At the same time, there is a considerable extent of learning on average in both treatments, especially in initial forecasting stages where the $M_{con,t}$ and $M_{seq,t}$ profiles are steeper compared to later stages. The evolution of average forecast errors can be judged relative to the performance benchmark provided by the above mentioned mechanical forecasting algorithm with the mean "true" forecast error of approximately 10.3 on average. Both $M_{con,t}$ and $M_{seq,t}$ gradually fall below that benchmark performance level, though especially $M_{seq,t}$ starts well above it. Put differently, the average subject in the more memory-intensive T_{seq} treatment takes around 40 forecasting periods to reach the $M_{seq,t}$=10.3 benchmark (i.e., in period 49) while the average subject in the less memory-intensive T_{con} treatment reaches the $M_{con,t}$=10.3 benchmark more than twice as fast (i.e., in

[65] For reasons related to the nature of the forecasting task, subjects were repeatedly reminded not to make any notes during the forecasting task itself. The four subjects who did not follow these instructions are excluded from the analysis below.

period 24).[66] This in turn suggests that subjects in T_{con} on average discover the seasonal pattern much earlier than subjects in T_{seq}.

Since the forthcoming analysis focuses on performance heterogeneity and what explains it, it is worth noting that both treatments generate plenty of potentially predictable between-subject variance in performance throughout the task. Figure 1 illustrates the substantial performance heterogeneity by displaying the 10^{th} and 90^{th} percentiles of $M_{i,t}$ for both treatments. The 90^{th} percentiles, ${}_{90}M_{con,t}$ and ${}_{90}M_{seq,t}$, suggest that the worst-performing subjects perform more or less similarly in both treatments. On the other hand, the parallel nature of the ${}_{10}M_{con,t}$ and ${}_{10}M_{seq,t}$ profiles suggests that the best forecasters generally perform slightly better in the less memory-intensive T_{con} treatment throughout the task. Note that despite the substantial performance heterogeneity, even the worst forecasters in either treatment show some learning progress on average, and even the best forecasters always have financial incentive to (and do) improve their forecasting performance. As an exception, the best forecasters in the less memory-intensive T_{con} treatment reach the performance ceiling towards the end of the task, which potentially reduces the extent of predictable between-subject variance in performance. This issue is addressed in the multivariate analysis below and turns out to be of minor importance.[67]

To look closer at the across-treatment differentials in forecasting performance as well as the extent of learning, I focus on performance in the perfectly matched twelve-period forecasting segments called EARLY (periods 21-32) and LATE (periods 84-95). Denote subject *i*'s performance in the EARLY and LATE segments as $M_{i,31} \equiv M_{i,EARLY}$ and

[66] Recall that subjects make their first forecast, F_9, in period 8 since the first eight periods of the task are reserved for displaying the initial values of B_t and Ω_t.

[67] An additional source of performance heterogeneity not apparent from Figure 1 is the seasonal nature of the forecasting task. In general, performance varies across the three forecasting seasons, with the "sandwich" seasonal parameter, $\gamma_2 = 34$, being associated with markedly lower and less variable forecast errors. Intuitively, the forecasting seasons represent within-subject treatments featuring various degrees of "overlap" of the $\gamma_s+\eta_t$ distributions, conditional on γ_s, which seems to matter for the relative ease of discovering the seasonal parameters, γ_s. While a more detailed seasonal performance analysis is possible (and available upon request), a potential caveat is that unobserved heterogeneity in subjects' forecasting strategies may imply different seasonal performance tradeoffs, in turn limiting interpretability of the results. In this paper, I adopt a more conservative approach by aggregating forecasting performance across seasons.

$M_{i,94} \equiv M_{i,LATE}$, respectively. The summary statistics for $M_{i,EARLY}$ and $M_{i,LATE}$ for each treatment are available in the first two rows of Table 1. The treatment averages for the EARLY segment, $M_{con,EARLY}$=8.81 and $M_{seq,EARLY}$=13.73, are significantly different from each other by a signed-ranks test based on comparing subjects facing identical Ω_t forecasting sequences in T_{con} and T_{seq} (p=0.0002). For the LATE segment, the treatment averages, $M_{con,LATE}$=5.13 and $M_{seq,LATE}$=6.56, do not differ from each other by an analogous signed-ranks test (p=0.2203). The extent of learning, unambiguously assessed by comparing $M_{i,EARLY}$ and $M_{i,LATE}$ by a signed-ranks test, is highly significant in both T_{con} (p=0.0000) and T_{seq} (p=0.0000). Finally, I compare the extent of learning, $M_{i,EARLY}$-$M_{i,LATE}$, across treatments (see the summary statistics in the third row of Table 1). A signed-ranks test of the learning measure, $M_{i,EARLY}$-$M_{i,LATE}$, for subjects with identical Ω_t forecasting sequences in T_{con} and T_{seq} suggests that learning is significantly stronger in the more memory-intensive T_{seq} treatment (p=0.0057). Based on the above observations, this result is mainly due to the much slower learning progress in T_{seq} compared to T_{con} in the early stages of the forecasting task.

In the analysis that follows, I mostly focus on forecasting performance as measured by $M_{i,LATE}$. One can think of $M_{i,LATE}$ as measuring subject i's "asymptotic" forecasting performance since, in a statistical sense, learning has ceased by the LATE segment in either treatment.[68] I further consider an alternative measure of forecasting performance that attempts to account for the fact that $M_{i,LATE}$ might be undesirably influenced by outliers, i.e., random "slip-ups" in forecasting performance arising from momentary distraction and other unwanted effects. In particular, I consider $M_{i,MEDLATE}$, the average of seasonal

[68] I test the "asymptoticity" by comparing $M_{i,LATE}$ with the performance in the immediately preceding twelve-period segment, $M_{i,82}$. While both treatments do show a small improvement in average forecast errors – namely from $M_{con,82}$=5.58 to $M_{con,LATE}$=5.13 and from $M_{seq,82}$=6.89 to $M_{seq,LATE}$=6.56, respectively, a signed-ranks test cannot reject equality of $M_{i,LATE}$ and $M_{i,82}$ in either T_{con} (p=0.1522) or T_{seq} (p=0.5139). On the other hand, a signed-ranks test rejects equality of $M_{i,82}$ and $M_{i,70}$ in both T_{con} (p=0.0182) and T_{seq} (p=0.0066), indicating significant learning between the two earlier twelve-period segments. These asymptoticity tests are not as efficient as the above learning tests based on comparing the perfectly matched EARLY and LATE segments.

medians of forecast errors in the LATE segment, as an arguably more robust alternative to $M_{i,LATE}$.[69] Analogously $M_{i,MEDEARLY}$ is considered as a robust alternative to $M_{i,EARLY}$.

4.2 Bivariate relationships

Tables 2a and 2b display Spearman correlations between forecasting performance and the measured covariates for T_{con} and T_{seq}, respectively.[70] Since lower forecast errors mean better forecasting performance, one generally expects negative correlations between the performance measures and cognitive covariates. First of all, note that the correlation between asymptotic forecasting performance, M_{LATE}, and Working memory is relatively strong at -0.345 (p=0.023) in the more memory-intensive T_{seq} treatment, especially compared to the negligible correlation of -0.022 (p=0.891) in the less memory-intensive T_{con} treatment. Hence in line with the causality hypothesis, working memory is more strongly associated with asymptotic forecasting performance when the working memory load is higher. The multivariate analysis below examines whether this conclusion is confirmed when other potential predictors of M_{LATE} are taken into account.

To that end, notice that M_{LATE} in T_{seq} is also relatively strongly correlated with Short-term memory at -0.269 (p=0.081). Furthermore, Short-term memory is in both treatments positively correlated with Working memory which in turn is positively correlated with Math. This shared variance is not surprising given that working and short-term memory are theoretically related cognitive constructs, and that the tests of Working memory, Short-

[69] Specifically, I use the four forecast errors per season to calculate season-specific median forecast errors separately for each season and then take an average of the medians to obtain $M_{i,MEDLATE}$. An analogous procedure is used for calculating $M_{i,MEDEARLY,}$ for which eliminating random slip-ups might be more important if their occurrence is more likely in the early forecasting stages. Admittedly, one might not want to partial out slipups from the performance measure if they are related to individual differences in working memory. However, $M_{i,MEDLATE}$ and $M_{i,MEDEARLY}$ might conveniently avoid the influence of "exogenous" distraction that possibly arises in the experimental lab and is entirely beyond subjects' control. The summary statistics for $M_{i,MEDLATE}$ and $M_{i,MEDEARLY}$ can be inspected in the fourth and fifth rows of Table 1, respectively. The statistical tests presented so far for $M_{i,LATE}$ and $M_{i,EARLY}$ yield qualitatively comparable results when applied to $M_{i,MEDLATE}$ and $M_{i,MEDEARLY}$. For example, learning between $M_{i,MEDEARLY}$ and $M_{i,MEDLATE}$ as judged by a signed-ranks test is statistically stronger in T_{seq} compared to T_{con} (p=0.0124).

[70] The summary statistics for the covariates are presented in Table 1. None of the covariates has a significantly different sample mean across treatments based on a t-test at the 10% significance level. The variances of the covariate distributions differ significantly across treatments in the case of Short-term memory, Carshare and Windfall based on an F-test at the 5% significance level.

term memory and Math share common surface features (they all involve dealing with numbers). To investigate the separate predictive power of working memory, psychologists often extract the underlying working memory ability (the ability to control attention) as the residual working memory variance that remains after removing its shared variance with short-term memory and other cognitive constructs (e.g., Engle et al., 1999). Following this practice, I extract WMresidual by partialling out Short-term memory, Math, Perseverance and Math anxiety from Working memory.[71] The correlation between M_{LATE} and WMresidual in T_{seq} is -0.353 (p=0.020), virtually identical to the correlation between M_{LATE} and Working memory. Hence the ability to control attention, as captured by WMresidual, has considerable predictive power for forecasting performance in T_{seq}, independent of the potential additional predictive power of Short-term memory, Math and other covariates.

Turning now to the correlations of M_{LATE} with personality and demographic covariates, less sensation-seeking and more premeditation attitudes seem partly beneficial for asymptotic performance in T_{con}, while, as expected, the two impulsiveness proxies also

---

[71] I regress Working memory on Short-term memory, Math, Perseverance and Math anxiety by OLS in the pooled sample (T_{con} and T_{seq}) and extract WMresidual as the regression residuals. There are theoretical reasons pertaining to the structure of the Working memory test for including Math, Math anxiety and Perseverance as covariates, and both Math and Perseverance indeed significantly explain some of the variance in Working memory, in addition to the explanatory power of Short-term memory. While it seems theoretically warranted to include Need for cognition as an additional covariate – given that Working memory is measured without performance-contingent financial incentives, Need for cognition turns out completely unrelated to Working memory, regardless of including Math anxiety and Perseverance which are both correlated with Need for cognition. Estimation details related to the extraction of WMresidual are available upon request.
The working memory literature offers several alternative approaches to extracting "controlled attention," the choice depending on the research goal. For instance, controlled attention variance can be extracted as the shared (as opposed to the residual) variance between working memory and short-term memory (e.g., Kane et al, 2004). Most approaches use latent-variable modeling to first extract the working memory and short-term memory variance from a battery of working and short-term memory tests, respectively, to remove the influence of test idiosyncrasies (i.e., surface features of the various tests). While I cannot use this approach due to the automated nature of the Working memory test (since conducting several automated working memory span tests in a sequence would alter their strategic nature), the WMresidual should be free of the surface features shared with the partialled out covariates, such as memorizing simple patterns (Short-term memory) and performing simple arithmetic operations (Math). Similar surface features in fact underlie the forecasting task itself and thus might influence the predictive power of Short-term memory and Math, but arguably *not* the predictive power of WMresidual.

correlate with each other. In T_{seq}, M_{LATE} is not correlated with any of the personality covariates, but subjects who receive the Windfall financial bonus seem to perform better. Similar bivariate relationships hold for the alternative asymptotic performance measure, $M_{MEDLATE}$. This is not surprising given that $M_{MEDLATE}$ almost perfectly correlates with M_{LATE} in either treatment. In fact, since the multivariate results are also closely similar for M_{LATE} and $M_{MEDLATE}$ in all important respects, I below report only the results for M_{LATE}.

Before doing so, I briefly look at the determinants of early forecasting performance and learning. In both T_{con} and T_{seq}, lack of sensation-seeking attitudes seems beneficial for early performance as measured by M_{EARLY} or $M_{MEDEARLY}$. In T_{con}, male forecasters seem to perform better than females. In T_{seq}, M_{EARLY} and $M_{MEDEARLY}$ correlate negatively with Age and positively with Carshare, the latter correlation suggesting a negative effect of family wealth on early performance. Clearly, however, all these demographic effects vanish when considering asymptotic performance.

The extent of learning, M_{EARLY}-M_{LATE}, seems partly positively related to Working memory and to being a female in T_{con}, and to Short-term memory and Windfall in T_{seq}.[72] However, it is especially noteworthy that, despite the considerable distance between their measurement, M_{EARLY} and M_{LATE} are strongly correlated with each other at 0.750 in T_{con} (p=0.000) and at 0.337 in T_{seq} (p=0.027). Especially the former correlation suggests strong internal reliability of the two performance measures, with implications for my causality test of the explanatory power of working memory: If, as hypothesized, working memory turns out to be a stronger predictor of M_{LATE} in T_{seq} compared to T_{con}, this is unlikely caused by lack of internal reliability of the M_{LATE} performance measure in T_{con} compared

[72] However, the interpretability of the correlations is likely limited, for M_{EARLY}-M_{LATE} is likely to be appropriate as a within-subject indicator of learning (i.e., whether $M_{i,EARLY}$>$M_{i,LATE}$) but less so as an indicator of *between*-subject variance in the extent of learning (i.e., how much subjects learn compared to each other). Intuitively, both $M_{i,EARLY}$ and $M_{i,LATE}$ vary greatly across subjects, and learning progress at different initial levels of forecast errors may be differentially difficult and might involve non-linearities related to the nature of discovering the seasonal pattern. To deal with these potential caveats, I examined various alternative learning measures based, for example, on proportional learning metrics or learning speed (duration) metrics, but none of the alternative measures seems related to the measured covariates in an economically meaningful way.

to T_{seq}. It is much more likely due to the causal effect of working memory on asymptotic forecasting performance.

4.3 Multivariate analysis

Now I turn to multivariate analysis appropriate for testing the causality of working memory. The causality hypothesis proposed that, holding short-term memory, basic math skills and other potentially relevant personality and demographic determinants of forecasting performance constant, working memory should be a stronger determinant of performance in the more memory-intensive T_{seq} treatment, compared to the less memory-intensive T_{con} treatment. I therefore estimate the impact of working memory (WMresidual) and other personality and demographic covariates on asymptotic forecasting performance, M_{LATE}, and test for the presence of an across-treatment differential in the impact of WMresidual.

Tables 3 and 4 present a sequence of empirical models, gradually expanding the set of covariates that are assumed relevant for asymptotic forecasting performance. Due to the different cognitive and possibly also personality (motivational) requirements of T_{con} and T_{seq}, each model *a priori* permits that not only working memory but also other included covariates might differ in their impact across treatments (to gain efficiency though, estimates are pooled across T_{con} and T_{seq} wherever justified by a two-tail t-test at the 10% significance level). As explained earlier, to eliminate the influence of Ω_t-complexity on M_{LATE}, I estimate the impact of working memory and other covariates on the *differences* in M_{LATE} calculated for the pairs of subject facing identical Ω_t forecasting sequences in T_{con} and T_{seq}. Furthermore, I take into account that M_{LATE} is top-bounded for a small minority of subjects and use an appropriate censored-type estimator.[73]

[73] The estimated model is ${}^{seq}M_{LATE} - {}^{con}M_{LATE} = \alpha + X^{seq}\beta^{seq} - X^{con}\beta^{con} + (\varepsilon^{seq} - \varepsilon^{con})$, where, assuming that variables are paired across treatments according to the identical Ω_t forecasting sequences, ${}^{seq}M_{LATE}$ and ${}^{con}M_{LATE}$ are the Nx1vectors of M_{LATE} in T_{seq} and T_{con}, respectively (N=43 is the number of subjects and unique forecasting sequences in each treatment), X^{seq} and X^{con} are the respective NxK matrices of covariates (the number of covariates, K, depending on the estimated model), β^{seq} and β^{con} are the respective Kx1 parameter vectors (assuming for simplicity of exposition that none of the parameters is pooled across treatments), ε^{seq} and ε^{con} are the respective

Model 1 in Table 3 presents the most bare-bone test of the causality hypothesis. It contains only the most theoretically relevant cognitive covariates, WMresidual and Short-term memory, implicitly assuming that Math and all the personality and demographic covariates are irrelevant for asymptotic forecasting performance. Confirming the previous correlation results, *Model 1* shows that working memory only affects performance in the more memory-intensive T_{seq} treatment while the effect is negligible in T_{con} and even has a wrong sign (recall that "helpful" covariates should have negatively signed coefficient estimates). A t-test presented beneath the WMresidual estimates indicates that the impact of WMresidual differs between T_{seq} and T_{con} at the 10% significance level, in line with the causality hypothesis. There is an even stronger across-treatment differential in the impact of short-term memory. Both working memory and short-term memory therefore independently contribute to explaining the variance in forecasting performance, yet only in the more memory-intensive T_{seq} treatment. On average, forecasting performance is better in T_{con} than in T_{seq}, as indicated by the significance of the intercept.

Model 2 includes two additional, theoretically relevant covariates: Math and Need for cognition. Math, a proxy for basic arithmetic abilities, turns out to influence forecasting performance only in the less memory-intensive T_{con} treatment. By contrast, both working memory and short-term memory again have predictive power only in the more memory-intensive T_{seq} treatment. As hypothesized, therefore, the higher memory load in T_{seq}

---

regression disturbances, and α is the intercept (the α estimate does not reflect the size of the average across-treatment differential in M_{LATE} since variables are not normalized). As mentioned earlier, the estimation model implicitly assumes that the effect of Ω_t-complexity on M_{LATE} interacts neither with the effect of the included cognitive and personality covariates nor with the heterogeneity in forecasting strategies.

I estimate *Model 1* through *Model 6* using a censored normal regression estimator that permits top-bounded performance to arise in either T_{con} or T_{seq}. In reality, there are five perfectly top-bounded subjects (with $M_{LATE}=0$) in T_{con} and two such subjects in T_{seq}, i.e., slightly below 10% of the total number of subjects in both treatments. Most of the seven subjects already have their performance almost perfectly or perfectly top-bounded for quite a while before the LATE segment, which justifies treating their performance as censored. In one case, both subjects in a given pair are top-bounded; I treat this as a "no censoring" case with no consequences for any of the reported results. The censored normal regression is a Tobit-type, asymptotic estimator that relies on the assumption of i.i.d. normal disturbances. While this assumption generally seems to be met, I compare the censored normal estimates to OLS estimates that, while potentially biased due to the minor censoring of M_{LATE},might be viewed as a useful robustness check (see *Model 7* in Table 4).

activates subjects' working and short-term memory constraints and identifies their causality. Relaxing the memory load in T_{con} makes these constraints irrelevant for forecasting performance and shifts explanatory power to Math, suggesting that T_{con} poses a number-intensive rather than a memory-intensive forecasting exercise. As for the other covariate added in *Model 2*, Need for cognition has the expected sign but is statistically insignificant. Nevertheless, including a measure of intrinsic motivation in the empirical model of forecasting performance seems theoretically justified, if not as a direct determinant of forecasting performance then as a potential co-determinant of the measured cognitive covariates.[74]

In *Model 3*, I initially attend to all the remaining personality and demographic covariates contained in Tables 2a and 2b but eventually include only those related to forecasting performance, namely Risk and Windfall.[75] *Model 3* confirms the strong explanatory power of working memory and short-term memory in T_{seq}, and conversely the impact of Math in T_{con}. Need for cognition now becomes (weakly) significant across treatments, suggesting that in addition to the high-powered financial incentives, subjects' intrinsic motivation fosters performance as well. In fact, the Windfall bonus appears to represent further extrinsic incentives, despite the bonus award scheme being entirely exogenous to the forecasting task.[76] Lastly, risk aversion attitudes seem beneficial for performance in both

[74] Recall that cognitive and other covariates were collected without using performance-contingent financial incentives, so individual differences in intrinsic motivation might be a source of variance in the measured values of the covariates. As discussed previously, however, I do not detect any influence of Need for cognition on Working memory (unlike Ballinger et al., 2005). This seems in line with evidence from the working memory literature suggesting that cognitive effort does not vary across the working memory distribution during working memory span tests (e.g., Heitz et al., 2006).

[75] The remaining personality and demographic covariates not listed in *Model 3* are individually as well as jointly highly insignificant at conventional significance levels. Including insignificant covariates in *Model 3* and other models considerably reduces the precision of the reported estimates, reflecting the relatively small sample size.

[76] It is possible that subjects who won the windfall bonus have higher cognitive abilities, as indicated by the positive correlation between Windfall and Math in either treatment. Nevertheless, in the models where Windfall is included (i.e., *Model 3, 6* and *7*), Windfall does not seem to interact with any of the cognitive, personality and demographic covariates.

treatments.[77] In sum, *Model 3* uncovers the influence of extrinsic and intrinsic incentives and risk attitudes on performance in either treatment but taking them into account does not harm the separate explanatory power of working memory and short-term memory in T_{seq}.

Next, I extend the empirical model by controlling for the influence of prior forecasting expertise not captured by the measured covariates. Tables 2a and 2b reveal that especially in T_{con}, M_{EARLY} and M_{LATE} correlate noticeably stronger with each other than either of them separately correlates with the measured covariates. Besides the implications for the internal reliability of M_{LATE} discussed above, this also suggests that both M_{EARLY} and M_{LATE} might be influenced by "unobserved forecasting ability" such as pattern recognition skills in the face of randomness. If such unobserved forecasting ability substantially contributes to explaining the variance in M_{LATE}, not including it among explanatory factors might bias the conclusions regarding the impact of the measured covariates. As a precaution against such a possibility, I create a proxy for unobserved forecasting ability and include it in the empirical model of M_{LATE}. Specifically, exploiting the design feature that M_{EARLY} and M_{LATE} are based on identical segments of the Ω_t forecasting sequence for each subject, I create a proxy, M_{EARLY}residual, by extracting the residual variance in M_{EARLY} that remains after removing the influence of theoretically and statistically relevant measured covariates.[78] In this way, the impact of M_{EARLY}residual on M_{LATE} will not reflect the impact

[77] This seems in line with the earlier reported bivariate results suggestive of a negative association between sensation-seeking and performance, especially in T_{con}. When Risk and Sensation-seeking are both included in *Model 3*, Sensation-seeking is less relevant compared to Risk, and only in T_{con}, while Risk is relevant in both treatments. Also note that Risk is strongly negatively correlated with Sensation-seeking at -0.277 (p=0.0099) and Sensation-seeking with Premeditation at -0.208 (p= 0.054) in the pooled sample (Risk is not as strongly correlated with Premeditation, only to some extent in T_{seq}). Hence when interpreting the positive impact of Risk on forecasting performance, one should bear in mind that a combination of risk aversion, sensation-seeking and premeditation attitudes might matter for performance, perhaps through influencing the development of successful forecasting strategies.

[78] I create M_{EARLY}residual by regressing M_{EARLY} on Working memory, Short-term memory, Math, Need for cognition and Premeditation. The first four covariates are included because they are theoretically relevant for forecasting performance and also statistically explain M_{LATE} in the models presented in Table 3. Only Working memory in fact turns out statistically relevant for M_{EARLY}, and Premeditation is the only other statistically relevant covariate. The estimation for M_{EARLY} is analogous to that for M_{LATE} except that the absence of top-bounded performance permits using OLS instead of censored normal regression. Furthermore, in order to retain the richest possible model of M_{EARLY}, I use Working memory instead of WMresidual and do not allow parameters to be pooled

of those measured covariates, so they should retain their independent influence on M_{LATE} if there exists any.

Apart from including M_{EARLY}residual, *Model 4* and *Model 5* in Table 4 are analogous to *Model 1* and *Model 2*, respectively. In fact, also the results in the two pairs of models are remarkably similar. The only novel insight from *Model 4* and *Model 5* is that M_{EARLY}residual is a strongly significant positive predictor of forecasting performance. The predictive power of working and short-term memory in T_{seq}, and basic arithmetic skills in T_{con}, remains essentially unchanged compared to the models without M_{EARLY}residual. It is noteworthy that the working memory across-treatment differential, and hence the support for its causality, now becomes slightly stronger in *Model 5* (p=0.0553) and reaches the 5% significance level in *Model 6* (p=0.0456).

The richest *Model 6* differs from its counterpart *Model 3* not only in the inclusion of M_{EARLY}residual but also in that lower math anxiety appears to improve performance in T_{con}. Lower math anxiety might help subjects deal with the arithmetic nature of the forecasting task – related to the positive impact of Math in T_{con}, but it might also be helpful for developing successful forecasting strategies – related to the positive impact of risk aversion in both treatments. As for the influence of extrinsic and intrinsic incentives on M_{LATE}, both Windfall and Need for cognition again exhibit a strong positive influence. I do not pool the impact of need for cognition across treatments, though warranted by the t-test (p=0.128), to illustrate that in this richest model, need for cognition seems more relevant for performance in the more memory-intensive T_{seq} treatment.

Finally, *Model 7* is exactly analogous to *Model 6* except that it is estimated by OLS. Since the degree of censoring of M_{LATE} is relatively minor, the OLS estimates might be viewed as a robustness check for the censored normal estimates. As expected, most of the OLS estimates in *Model 7* seem slightly biased towards zero compared to the censored normal estimates in *Model 6*. However, the precision of the estimates and hence the conclusions drawn from the two alternative estimations are essentially identical. The OLS results

across treatments. M_{EARLY}residual is extracted as the regression residuals. The estimation results are available upon request.

confirm the strong, independent contributions of working memory and short-term memory to explaining asymptotic forecasting performance in T_{seq}, and also confirm the presence of the across-treatment differential in the impact of working memory (p=0.064). The causality of WMresidual is not as statistically powerful as one might like but is considerably robust across the estimated models regardless of which classes of covariates are included.

5. Discussion and conclusion

This paper provides an initial test of the capital-labor-production (KLP) framework. I show that the effectiveness of high-powered financial incentives as a stimulator of economic performance can be moderated by cognitive capital in a causal fashion. Using a memory intensive time-series forecasting task, I identify the causal effect of both working and short-term memory on asymptotic forecasting performance. The effects are entirely independent of each other since my working memory measure shares no cognitive or surface features with short-term memory. The causal effect of working memory thus likely reflects individual heterogeneity in the ability to control attention, a strong predictor of performance in a wide range of tasks requiring controlled information processing (Engle and Kane, 2004). The present paper indicates that the ability to control attention may also affect decision quality in cognitively complex economic settings.

Exploring the role of motivational factors, I find that besides the strong financial incentives employed in the forecasting task, subjects' intrinsic motivation and a sizeable windfall financial bonus won prior to the forecasting task both positively foster forecasting performance. Given my auxiliary treatment of motivational factors (while focusing on the causality of cognitive capital), documenting their separate impact constitutes only an initial step in examining their interaction with cognitive capital, with implications for the design of efficient incentive schemes. Indeed, establishing the causality of particular cognitive capital measures is a prerequisite for examining their role in the multitude of structural relationships that the KLP framework potentially entails, such as the substitutability among various forms of cognitive capital and in turn their substitutability with cognitive effort.

Below I discuss some of the relationships and how one could start addressing them in the present forecasting setting.

One of the most economically relevant interactions in the KLP framework is the degree of substitutability between cognitive capital forms varying in task specificity.[79] I examined the predictive power of both general and specific forms of cognitive capital – working memory, short-term memory and basic math abilities – but I intentionally minimized the influence of task-specific cognitive capital in the form of prior expertise (or domain knowledge). Prior expertise is clearly vital for performance in many field cognitive tasks, and is central to the KLP framework of Camerer and Hogarth (1999), cognitive science literature (e.g., Anderson, 2000) and the "expertise paradigm" in behavioral decision research (e.g., Libby and Luft, 1993).[80] However, we still know relatively little about the interplay between prior expertise and more general forms of cognitive capital in economically relevant settings (e.g., Hambrick and Engle, 2003). As an initial step in that direction, Wittmann and Suess (1999) study performance determinants in a cognitively complex, simulated physical production task, finding that both prior expertise (domain knowledge) and working memory contribute to explaining variance in performance. Similarly, Ghosh and Whitecotton (1997) study performance determinants in a company earnings prediction task, finding that general cognitive capital, measured by a perceptual ability test, has a strong explanatory power that is overcome neither by prior expertise of professional financial analysts nor by provision of a forecast-relevant decision aid.

Arguably, however, only after establishing the causal effect of the relatively more general forms of cognitive capital can one credibly assess their substitutability with prior expertise. The forecasting task lends itself to examining that substitutability as it naturally extends to real-world settings. Imagine, for instance, a financially framed version of the forecasting

[79] In the following discussion, I abstract from "nature/nurture" issues related to the evolution of cognitive capital over time, such as whether various general and specific forms of cognitive capital are inherited or acquired and what determines their acquisition (e.g., Heckman et al., 2006; LeDoux, 2002; Plug and Vijverberg, 2003).

[80] As noted by Camerer and Hogarth (1999) and others, however, prior expertise seems only imperfectly transferable across even slightly different cognitive production settings. See also Kagel and Levin (1986) and the ensuing discussion on the relative productivity of prior expertise and experience acquired through on-task learning.

task where Ω_t is a financial variable such as a commodity price that follows my (simplistic) deterministic seasonal process and B_t is an economically relevant, perfectly predictable state variable linearly related to Ω_t. At a basic level, one could then use the above forecasting design (again with the sequential and concurrent presentation treatments) and challenge inexperienced forecasters (e.g., students) and experienced forecasters (e.g., commodity traders) with the framed and unframed versions of the task. The resulting 2x2x2 factorial design would shed further light on the above established causality of working and short-term memory and would permit gauging their substitutability with prior expertise.

Another key substitutability question pertains to the interaction between the various *a priori* acquired (or inherited) forms of cognitive capital discussed above and arguably the most task-specific form of cognitive capital, namely experience acquired endogenously through on-task learning. As mentioned earlier, evidence from cognitive psychology suggests that experience gained gradually through learning by doing (rather than learning by thinking) tends to be the most productive component of task-specific cognitive capital, overriding the productivity of prior expertise (e.g., Anderson, 2000; Ericcson and Smith, 1991; Reber, 1989). We nevertheless have limited evidence on the interaction between experience and general cognitive capital. Engle and Kane (2004) discuss suggestive evidence that various forms of on-task learning are inconsequential for the causal effect of working memory on performance in tasks requiring controlled attention. By measuring forecasting performance at its asymptotic stage in both treatments, I supply further evidence that the causal effect of working memory (and short-term memory) persists even after on-task learning has entirely ceased.

Since many economic tasks are much more cognitively complex than my forecasting task and learning in them is a continuous process, one may further want to examine the interaction between *a priori* acquired (or inherited) cognitive capital and the on-task learning process itself.[81] To illustrate, one could examine the extent to which further

[81] As a possible approach mentioned earlier, a panel estimation not reported here reveals that several exogenously varied aspects of Ω_t-complexity weakly explain learning progress in early

learning has been inhibited in the forecasting task by the artificially imposed memory load (combined with the corresponding individual cognitive constraints) by further relaxing the memory load. To do that, one can extend the current forecasting design (call it Stage 1) for a number of forecasting periods with B_t=0 where the screen with (B_{t+1},…, B_{t-7}) values effectively disappears (call this Stage 2). Stage 2 then resembles an inductive reasoning task (with a random component) featuring only a minimum memory (and arithmetic) load in either treatment. We should therefore expect considerable degree of additional learning going on in Stage 2, *provided* that the major source of sub-optimal performance in Stage 1 was indeed the memory (and arithmetic) load, as opposed to other sources of sub-optimal performance such as poor pattern recognition skills and inability to deal with the random component.

Allowing enough periods in Stage 2 for additional learning to have ceased again, one can then decompose the effect of the cognitive load relaxation on the total between-subject variance of asymptotic performance into three separate components. The first component is the change in the total between-subject variance due to the additional learning opportunities between Stages 1 and 2. This "learning drift" component can be partialled out from the change in the total between-subject variance as the variance of the within-subject differences in asymptotic performance between Stages 1 and 2. The second component is the (likely) decrease in the total between-subject variance due to greater "cognitive control" and hence smaller within-subject forecast error variance (e.g., Hammond and Summers, 1972). This "cognitive control" component can be partialled out by allowing for a number of extra periods in both Stage 1 and 2 after asymptotic performance has been reached and treating each of the extra periods as performance re-tests. Finally, having partialled out the "learning drift" and "cognitive control" components, the remaining component of the (likely) decrease in the total between-subject variance is the change in the *predictable* between-subject variance in mean forecasting performance, conditional on what has been learned, attributable directly to the reduced

stages of the forecasting task. After enlarging the sample sizes in both treatments, the ultimate goal of this project is to exploit the exogenous variation in the Ω_t sequences – affecting not only Ω_t-complexity but also the clarity of forecasting feedback and hence confidence – to analyze the relationship between cognitive capital, learning progress, and betting behavior.

predictive power of working and short-term memory and basic arithmetic abilities between Stages 1 and 2.[82] Hence the decomposition sheds light on the relative importance of the three components of performance heterogeneity in Stage 1 compared to Stage 2, and also permits comparing the components between the concurrent and the sequential presentation treatments.

Leaving the confines of cognitive capital and getting to the heart of the KLP framework, one naturally turns to the issue of capital-effort substitutability in cognitive production. To that end, identifying the causality of cognitive capital is useful only to the extent that cognitive effort is observable. As with physical effort, one can think of cognitive effort as having two dimensions, duration and intensity, with especially effort intensity being difficult to define, let alone measure.[83] Evidence from the working memory literature is suggestive of a limited degree of capital-effort substitutability. In tasks where working memory is a strong predictor of performance, effort latencies (measured by response times, pupil dilation, fMRI "scans," etc.) do not vary across the working memory distribution in the sample, while effort latencies tend to increase relatively uniformly with higher financial incentives and higher task complexity (e.g., Heitz et al., 2006). Awasthi and Pratt (1990) provide further circumstantial evidence of limited capital-effort substitutability for cognitively constrained individuals. In their between-subject design, piece-rate (as compared to flat-rate) financial incentives yield an improvement in judgmental performance only for individuals with higher perceptual differentiation ability while effort duration increases uniformly regardless of the ability. As noted by Awasthi and Pratt (1990), Camerer and Hogarth (1999) and many others, such observations raise questions as to why cognitively constrained individuals might be inclined to exert sub-optimally high

[82] As a potential caveat of the variance decomposition, the asymptotic performance of a minority of subjects is already top-bounded in Stage 1 and the cognitive load reduction in Stage 2 would be likely the bring a further reduction of the total between-subject variance. For that reason, one would ideally want to increase the overall cognitive load of the forecasting task by raising the cognitive complexity of the forecasting task as discussed earlier.

[83] See Camerer and Hogarth (1999) for a discussion of various measures of cognitive effort.

levels of unproductive effort. Among potential reasons, cognitively constrained decision makers might only partly observe their cognitive capital and/or cognitive effort costs.[84]

These and other structural issues pertaining to the underlying cognitive "decision-making process" have recently received attention in neurobiology (e.g., Gold and Shadlen, 2001) and neuroeconomics (e.g., Camerer et al., 2005) but otherwise have remained empirically unexplored. The sparse empirical accounts of the KLP framework have instead focused on the reduced-form interaction between cognitive capital and financial incentive levels. Awasthi and Pratt (1990) and Palacios-Huerta (2003) both conclude that raising performance contingency of financial incentives yields a larger average improvement in judgmental performance for individuals with higher cognitive capital. While this positive interaction between financial incentives and cognitive capital appears economically interesting, for example from the point of view of within-firm wage structures, it is likely empirically tenuous. To the extent that cognitive effort is bounded from above and diminishing returns to cognitive capital eventually set in, the interaction relies on specific combinations of incentive variation, cognitive capital distribution in the sample and the shape of the cognitive production function.[85] This is not to question the validity of the above results *per se*, but rather to offer more applicable ways of investigating the

[84] My future analysis of the co-evolution of betting behavior and forecasting performance will address whether people with objectively lower forecasting abilities (as measured, for example, by working memory) demonstrate a higher degree of over-confidence in their forecasting abilities (as measured by the aggressiveness of their bets, after removing the effect of risk aversion and general judgmental confidence). The working memory literature suggests that people with lower working memory relying predominantly on automated processing might possess noisier estimates of their forecasting abilities compared to people with higher working memory relying mostly on controlled processing (e.g., Feldman-Barrett et al., 2004).

[85] A potentially more fruitful approach to interacting financial incentive levels and cognitive capital involves comparing the predictive power of cognitive and personality determinants of performance under performance-contingent as compared to flat-rate financial incentives (or under low- and high-powered performance-contingent incentives). In the forecasting task, one could for instance contrast the performance-contingent version of the sequential presentation treatment with its flat-rate counterpart (with the betting scheme removed from both versions). One could then compare whether intrinsic motivation is a stronger predictor of performance in the flat-rate version, and also whether the predictive power of working and short-term memory differs across the two versions (with the predictive power perhaps *a priori* favored in the flat-rate version because of the cognitive tests being performed under flat-rate incentives).

interaction between cognitive capital and financial incentives that might ultimately be of interest to designers of efficient incentive schemes.

In what follows, I again use the forecasting task as an illustration and the established causality of working memory as a prerequisite. One may, for instance, view the sequential presentation treatment as a cognitively demanding work setting and explore the welfare implications of implementing it under various incentive schemes – say, the presently used piece-rate scheme, a quota scheme, a tournament scheme and a flat-wage scheme. Due to their varying returns to cognitive capital and degree of competitiveness, the incentive schemes are likely to differ in how cognitive and personality characteristics moderate the effectiveness of financial incentives (e.g., Bonner et al., 2000; Bonner and Sprinkle, 2002). A steep piece-rate scheme or a tournament scheme is likely to be more suitable for less cognitively constrained (and less risk averse) employees, whereas more cognitively constrained but intrinsically motivated employees might perform better on average in a flat-wage scheme. Hence, given the low capital-effort substitutability discussed above, the utilization of both employers' financial and the employees' cognitive resources may be improved by *ex ante* assigning employees to incentives schemes that best correspond to their (observed) cognitive and personality characteristics. One may further like to explore how employees self-select based on their (observed) cognitive and personality characteristics into the various incentive schemes and the extent to which such endogenous sorting is efficient compared to the exogenous assignment.[86]

Finally, perhaps the most natural way of exploring the interaction between cognitive capital and financial incentives is to investigate people's willingness to pay for the relaxation of their cognitive constraints. In the forecasting setting, this can be achieved by implementing an additional treatment where subjects start forecasting in the more memory-intensive sequential presentation treatment but have the opportunity to pay for switching to the less memory-intensive concurrent presentation treatment. In any period, subjects can

[86] See Bonner and Sprinkle (2002) for a review of suggestive evidence. While the above discussion abstracts from the complexities of agency problems in real-world incentive scheme settings (e.g., Benabou and Tirole, 2003), observing individual cognitive and personality characteristics might still prove useful in designing more efficient incentive schemes.

therefore choose to purchase "external" memory and combine the forecast-relevant information visually. Figure 1 illustrates that switching to the concurrent presentation treatment does not guarantee perfect performance but it does improve performance and learning progress on average. Of course, subjects do not know this and their switching decisions will presumably reflect their expectation that the net (long-run) return to switching is positive. As with bets, switching behavior thus yields a decision-relevant and incentive-compatible indicator of subjects' estimates of their forecasting abilities, which can in turn be linked to their observed cognitive and personality characteristics, betting behavior and forecasting performance. One may further want to examine the effect of varying the price (or cost) of switching.

To conclude, the effect of financial incentives on human behavior has received widespread attention in the literature on the provision of incentives in organizations (e.g., Benabou and Tirole, 2003), experimental economics (e.g., Hertwig and Ortmann, 2001; Ariely et al., 2005) and neurobiology (e.g., Gold and Shadlen, 2001), as well as in newly emerging fields such as neuroeconomics (e.g., Camerer et al., 2005). Recent meta-studies and empirical surveys based on evidence from experimental economics and psychology have indicated that incentive effects depend in a complicated fashion on the nature of cognitive tasks.[87] Camerer and Hogarth (1999) argue that a complete explanation of incentive effects requires attending not only to how people balance financial incentives and cognitive effort costs (e.g., Conlisk, 1988; Smith and Walker, 1993; Wilcox, 1993) but also to how they combine cognitive effort with cognitive capital. I present initial evidence that the effectiveness of even strong financial incentives can be moderated by cognitive capital in a causal fashion. The evidence illustrates the need to attend to cognitive constraints, besides personality (preference-based) factors, when interpreting observed (variance of) behavior in cognitively demanding lab and field economic environments (Ballinger et al., 2005).

[87] E.g., Bonner et al. (2000); Camerer and Hogarth (1999); Hertwig and Ortmann (2001, 2003); Jenkins et al. (1998); Prendergast (1999).

References

Ackerman, P. L., Beier, M. E., and M. O. Boyle (2002), "Individual differences in working memory within a nomological network of cognitive and perceptual speed abilities," *Journal of Experimental Psychology*: *General* 131, 567-589.

Anderson, J. R. (2000), *Cognitive Psychology and its Implications*, New York: Worth.

Archibald, G., and N. Wilcox (2006), "Contingent processing in strategic reasoning: An experimental illustration," University of Houston manuscript.

Ariely, D., Gneezy, U., Loewenstein G., and N. Mazar (2005), "Large stakes and big mistakes," FRB of Boston Working Paper No. 05-11.

Awasthi, V., and J. Pratt (1990), "The effects of monetary incentives on effort and decision performance: The role of cognitive characteristics," *Accounting Review* 65, 797-811.

Baddeley, A. D., and G. Hitch (1974), "Working memory," in G. Bower (Ed.), *Recent Advances in Learning and Motivation*, Academic Press, New York.

Ballinger, P., Hudson, E., Karkoviata, L., and N. Wilcox (2005), "Saving performance and cognitive abilities," University of Houston manuscript.

Bandura, A., and E. E. Locke (2003), "Negative self-efficacy and goal effects revisited," *Journal of Applied Psychology* 88, 87-99.

Barrick, J. A., and B. C. Spilker (2003), "The relations between knowledge, search strategy, and performance in unaided and aided information search," *Organizational Behavior and Human Decision Processes* 90, 1-18.

Benabou, R., and J. Tirole (2003), "Intrinsic and extrinsic motivation," *Review of Economic Studies* 70, 489-520.

Benjamin, D. J., Brown, S. A., and J. M. Shapiro (2006), "Who is 'behavioral'? Cognitive ability and anomalous preferences," Dartmouth College manuscript.

Betz, N. E. (1978), "Prevalence, distribution, and correlates of math anxiety in college students," *Journal of Counseling Psychology* 25, 441-448.

Bonner, S. E., Hastie, R., Sprinkle, G. B., and S. M. Young (2000), "A Review of the effects of financial incentives on performance in laboratory tasks: Implications for management accounting," *Journal of Management Accounting Research* 12, 19-64.

Bonner, S. E., and G. B. Sprinkle (2002), "The effects of monetary incentives on effort and task performance: Theories, evidence, and a framework for research," *Accounting, Organizations & Society* 27, 303-345.

Brehmer, B. (1980), "In one word: Not from experience," *Acta Psychologica* 45, 223-241.

Cacioppo, J. T., Petty, R. E., and C. F. Kao (1984), "The efficient assessment of need for cognition," *Journal of Personality Assessment* 48, 306-307.

Cacioppo, J. T., Petty, R. E., Feinstein, J. A., and W. B. G. Jarvis (1996), "Dispositional differences in cognitive motivation: The life and times of individuals varying in need for cognition," *Psychological Bulletin* 119, 197-253.

Camerer, C. F., and R. Hogarth (1999), “The effects of financial incentives in experiments: A review and capital-labor-production framework,” *Journal of Risk and Uncertainty* 19, 7-42.

Camerer, C. F. (2003), *Behavioral Game Theory: Experiments in Strategic Interaction*, Princeton University Press.

Camerer, C. F., Loewenstein, G., and D. Prelec (2005), “Neuroeconomics: How neuroscience can inform economics,” *Journal of Economic Literature* 34, 9-64.

Cawley, J., Heckman, J. J., and E. J. Vytlacil (2001), “Three observations on wages and measured cognitive ability,” *Labour Economics* 8, 419-442.

Conlisk, J. (1980), “Costly optimization versus cheap imitators,” *Journal of Economic Behavior and Organization* 1, 275-293.

Conlisk, J. (1988), “Optimization Cost,” *Journal of Economic Behavior and Organization* 9, 213-228.

Conway, A. R. A., Kane, M. J., Bunting, M. F., Hambrick, D. Z., Wilhelm, O., and R. W. Engle (2005), “Working memory span tasks: A methodological review and user’s guide,” *Psychonomic Bulletin and Review* 12, 769-786.

Cooper, S., and D. Robinson (1991), “The relationship of mathematics self-efficacy beliefs to mathematics anxiety and performance,” *Measurement and Evaluation in Counseling and Development* 24, 4.

Cowan, N. (2001), “The magical number 4 in short-term memory: A reconsideration of mental storage capacity,” *Behavioral and Brain Sciences* 24, 87-185.

Crowley, A. E., and W. D. Hoyer (1989), “The relationship between need for cognition and other individual difference variables: A two-dimensional framework,” *Advances in Consumer Research* 16, 37-43.

Dawes, R. M. (1979), “The robust beauty of improper linear models in decision making,” *American Psychologist* 34, 371-582.

Deci, E., Koestner, R., and R. Ryan (1999), “A meta-analytic review of experiments examining the effects of extrinsic rewards on intrinsic motivation,” *Psychological Bulletin* 125, 627–668.

Devetag, G., and M. Warglien (2003), “Games and phone numbers: Do short term memory bounds affect strategic behavior?” *Journal of Economic Psychology* 24, 189-202.

Dwyer, G., Williams, A., Battalio, R., and T. Mason (1993), “Tests of rational expectations in a stark setting,” *The Economic Journal* 103, 586-601.

Eckel, C., and R. Wilson (2004), “Is trust a risky decision?” *Journal of Economic Behavior and Organization* 55, 447-465.

Eisenberger, R., and J. Cameron (1996), “Detrimental effects of reward: Reality or myth?” *American Psychologist* 51, 1153-1166.

Ekstrom, R. B., French, J. W., Harman, H., and D. Derman (1976), *Kit of Factor-Referenced Cognitive Tests* (rev. ed.), Princeton, NJ: Educational Testing Service.

Engle, R. W., and M. J. Kane (2004), "Executive attention, working memory capacity, and a two-factor theory of cognitive control," in B. Ross (Ed.), *The Psychology of Learning and Motivation*, Vol. 44, NY: Elsevier.

Engle, R. W., Tuholski, S. W., Laughlin, J. E., and A. R. A. Conway (1999), "Working memory, short-term memory, and general fluid intelligence: A latent variable approach," *Journal of Experimental Psychology*: *General* 128, 309–331.

Ericsson, K. A., and J. Smith (Eds.) (1991), *Toward a General Theory of Expertise: Prospects and Limits*, Cambridge UK: Cambridge University Press.

Feldman-Barrett, L., Tugade, M. M., and R. W. Engle (2004), "Individual differences in working memory capacity and dual-process theories of the mind," *Psychological Bulletin* 130, 553-573.

Fischbacher, U. (1999), "z-Tree: Zurich toolbox for readymade economic experiments – experimenter's manual," Working Paper No. 21, Institute for Empirical Research in Economics, University of Zurich.

Ghosh, D., and S. M. Whitecotton (1997), "Some determinants of analysts' forecast accuracy," *Behavioral Research in Accounting* 9 (Supplement), 50–68.

Gibbons, R. (1998), "Incentives in organizations," *Journal of Economic Perspectives* 12, 115-132.

Gneezy, U., and A. Rustichini (2000), "Pay enough or don't pay at all," *Quarterly Journal of Economics* 115, 791-811.

Gold, J. I., and M. N. Shadlen (2001), "Neural computations that underlie decisions about sensory stimuli," *Trends in Cognitive Sciences* 5, 10-16.

Ham, J. C., Kagel, J. H., and S. F. Lehrer (2005), "Randomization, endogeneity and laboratory experiments: The role of cash balances in private value auctions," *Journal of Econometrics* 125, 175-205.

Hambrick, D. Z., and R. W. Engle (2003), "The role of working memory in problem solving," in J. E. Davidson and R. J. Sternberg (Eds.), *The psychology of Problem Solving*, London: Cambridge Press.

Hambrick, D. Z., Kane, M. J., and R. W. Engle (2005), "The role of working memory in higher-level cognition: Domain-specific versus domain-general perspectives," in R. J. Sternberg and J. E. Pretz (Eds.), *Cognition and Intelligence: Identifying the Mechanisms of the Mind*, New York: Cambridge University Press.

Hammond, K. R., and D. A. Summers (1972), "Cognitive control," *Psychological Review* 79, 58-67.

Hammond, K. R., McClelland, G. H., and J. Mumpower (1980), *Human Judgment and Decision Making: Theories, Methods, and Procedures*, New York: Praeger.

Harrison, G. W., and J. A. List (2004), "Field experiments," *Journal of Economic Literature* 42, 1009-1055.

Harrison, G. W., Lau, M. I., and E. E. Rutström (2005), "Risk attitudes, randomization to treatment, and self-selection into experiments," UCF Economics Working Paper No. 05-01.

Harvey, N., Bolger, F., and A. McClelland (1994), "On the nature of expectations," *British Journal of Psychology* 85, 203-229.

Heckman, J. J., and E. Vytlacil (2001), "Identifying the role of cognitive ability in explaining the level of and change in the return to schooling," *Review of Economics and Statistics* 83, 1-12.

Heckmam, J. J., Stixrud, J., and S. Urzua (2006), "The effects of cognitive and noncognitive abilities on labor market outcomes and social behavior," NBER Working Paper No. W12006.

Heitz, R. P., Schrock, J. C., Payne, T. W., and R.W. Engle (2006), "The eyes have it: Individual differences in working memory capacity and mental effort," Georgia Institute of Technology manuscript.

Heitz, R. P., Unsworth, N., and R.W. Engle (2005), "Working memory capacity, attentional control, and fluid intelligence," in O. Wilhelm and R.W. Engle (Eds.), *Handbook of Understanding and Measuring Intelligence*, London: Sage Publications.

Hertwig, R., and A. Ortmann (2003), "Economists' and psychologists' experimental practices: How they differ, why they differ, and how they could converge," in I. Brocas and J. Carrillo (Eds.), *Economics and Psychology*, New York: OUP.

Hertwig, R., and A. Ortmann (2001), "Experimental practices in economics: A methodological challenge for psychologists?" *Behavioral and Brain Sciences* 24, 383-451.

Hey, J. D. (1994), "Expectations formation: Rational or adaptive or ...?" *Journal of Economic Behavior and Organization* 25, 329-349.

Holt, C. A., and S. K. Laury (2002), "Risk aversion and incentive effects," *American Economic Review* 92, 1644–55.

Hunton, J. E., and R. A. McEwen (1997), "An assessment of the relation between analysts' earnings forecast accuracy, motivational incentives and cognitive information search strategy," *The Accounting Review* 72, 497-515.

Jenkins, D., Jr., Mitra, A., Gupta, N., and J. Shaw (1998), "Are financial incentives related to performance? A meta-analytic review of empirical research," *Journal of Applied Psychology* 83, 777-787.

Kagel, J. H., and D. Levin (1986), "The winner's curse and public information in common value auctions," *American Economic Review* 76, 894-920.

Kahneman, D., and A. Tversky (1984), "Choices, values, and frames," *American Psychologist* 39, 341-350.

Kane, M. J., Hambrick, D. Z., Tuholski, S. W., Wilhelm, O., Payne, T. W., and R. W. Engle (2004), "The generality of working memory capacity: A latent variable approach to verbal and visuospatial memory span and reasoning," *Journal of Experimental Psychology*: *General* 133, 189-217.

Kellogg, C. E., and N. W. Morten (1999), *Beta III Manual*, San Antonio, TX: The Psychological Corporation.

Kirchner, W. K. (1958), "Age differences in short-term retention and rapidly changing information," *Journal of Experimental Psychology* 55, 352-358.

Klayman, J. (1984), "Learning from feedback in probabilistic environments," *Acta Psychologica* 56, 81-92.

Klayman, J. (1988), "Cue discovery in probabilistic environments: Uncertainty and experimentation," *Learning, Memory, and Cognition* 14, 317-330.

Lawrence, M., and M. O'Connor (2005), "Judgmental forecasting in the presence of loss functions," *International Journal of Forecasting* 21, 3– 14.

Lazear, E. P., Malmendier, U., and R.A. Weber (2006), "Sorting in experiments with application to social preferences," NBER Working Paper No. W12041.

LeDoux, J. E. (2002), *Synaptic Self*, New York: Viking.

Libby, R., Bloomfield, R., and Nelson, M. (2002), "Experimental research in financial accounting," *Accounting, Organizations & Society* 27, 775-810.

Libby, R., and M. G. Lipe (1992), "Incentives, effort, and the cognitive processes involved in accounting-related judgments," *Journal of Accounting Research* 30, 249-273.

Libby, R., and J. Luft (1993), "Determinants of judgment performance in accounting settings: Ability, knowledge, motivation, and environment," *Accounting, Organizations & Society* 18, 425-450.

Maines, L., and J. Hand (1996), "Individuals' perceptions and misperceptions of time series properties of quarterly earnings," *Accounting Review* 71, 317-336.

McDaniel, T, M., and E. E. Rutström (2001), "Decision making costs and problem solving performance," *Experimental Economics* 4, 145-161.

Ortmann, A., Ostatnicky, M., and O. Rydval (2006), "Three very simple games (and what it takes to solve them)," CERGE-EI manuscript.

Pajares, F., and T. Urdan (1996), "An exploratory factor analysis of the mathematics anxiety scale," *Measurement and Evaluation in Counseling and Development* 29, 35-47.

Pajares, F., and M. D. Miller (1994), "The role of self-efficacy and self-concept beliefs in mathematical problem-solving: A path analysis," *Journal of Educational Psychology* 86, 193- 203.

Palacios-Huerta, I. (2003), "Learning to open Monty Hall's doors," *Experimental Economics* 6, 235-251.

Plug, E., and W. Vijverberg (2003), "Schooling, family background, and adoption: Is it nature or is it nurture?" *Journal of Political Economy* 111, 611-641.

Prendergast, C. (1999), "The provision of incentives in firms," *Journal of Economic Literature* 37, 7-63.

Raven, J., Raven, J. C., and J. H. Court (1998), *Manual for Raven's Progressive Matrices and Vocabulary Scales*, San Antonio, TX: The Psychological Corporation.

Reber, A. S. (1989), "Implicit learning and tacit knowledge," *Journal of Experimental Psychology*: *General* 118, 219-235.

Rydval, O. (2003), "The impact of financial incentives on task performance: The role of cognitive abilities and intrinsic motivation," CERGE-EI Discussion Paper No. 112.

Rydval, O. (2005), "Capital and labor effects in a recall task: More evidence in support of Camerer and Hogarth (1999)," CERGE-EI Working Paper No. 264.

Rydval, O., and A. Ortmann (2004), "How financial incentives and cognitive abilities affect task performance in laboratory settings: An illustration," *Economics Letters* 85, 315-320.

Schneider, W., Eschman, A., and A. Zuccolotto (2002), *E-prime User's Guide*, Pittsburgh: Psychology Software Tools.

Schwarzer, R., Seipp, B., and C. Schwarzer (1989), "Mathematics performance and anxiety: A meta-analysis," in R. Schwarzer, H. M. Van Der Ploeg and C. D. Spielberger (Eds.), *Advances in Test Anxiety Research*, Vol. 6, 105-119, Berwyn, PA: Swets North America.

Smith, V. L., and J. Walker (1993), "Monetary rewards and decision cost in experimental economics," *Economic Inquiry* 31, 245-261.

Stanovich, K. E., and R. F. West (2000), "Individual differences in reasoning: Implications for the rationality debate?" *Behavioral and Brain Sciences* 23, 645-665.

Stevens, D., and A. Williams (2004), "Inefficiency in earnings forecasts: Experimental evidence of reactions to positive vs. negative information," *Experimental Economics* 7, 75-92.

Turner, M. L., and R. W. Engle (1989), "Is working memory capacity task-dependent?" *Journal of Memory and Language* 28, 127-154.

Vandegrift, D., and P. Brown (2003), "Task difficulty, incentive effects, and the selection of high-variance strategies: An experimental examination of tournament behavior," *Labour Economics* 10, 481-497.

Welsh, M.C., Satterlee-Cartmell, T., and M. Stine (1999), "Towers of Hanoi and London: Contribution of working memory and inhibition to performance," *Brain and Cognition* 41, 231-242.

Whiteside, S. P. and D. R. Lynam (2001), "The five factor model and impulsivity: Using a structural model of personality to understand impulsivity," *Personality and Individual Differences* 30, 669-689.

Wilcox, N. (1993), "Lottery choice: Incentives, complexity, and decision time," *The Economic Journal* 103, 1397-1417.

Wittmann, W. W., and H. M. Süß (1999), "Investigating the paths between working memory, intelligence, knowledge, and complex problem solving performances via Brunswik-symmetry," in P. L. Ackerman, P C. Kyllonen, and R. D. Roberts (Eds.), *Learning and Individual Differences. Process, Trait, and Content Determinants*, Washington, D.C.: APA-Books.

Yerkes, R. M., and J. D. Dodson (1908), "The relationship of strength stimulus to rapidity of habit-formation," *Journal of Comparative Neurology and Psychology* 18, 459-482.

Figure 1: Forecasting performance (12-period moving average) for the average and the 10th and 90th percentile subjects in both treatments.

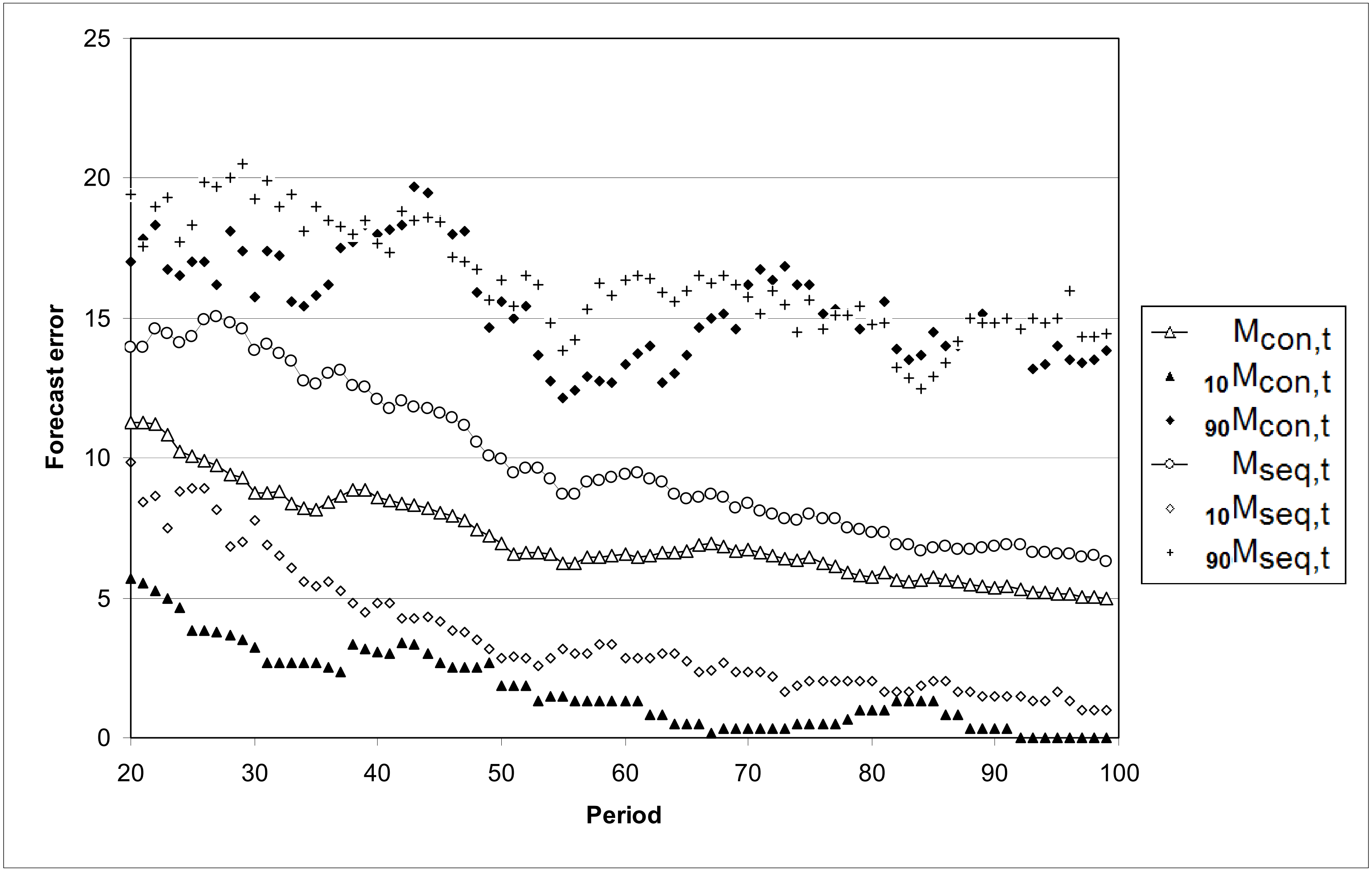


Table 1: Summary statistics for performance measures and covariates in both treatments.

Variable	Concurrent presentation treatment T_{con} (subjects=43)				Sequential presentation treatment T_{seq} (subjects=43)			
	Mean	St.Dev.	Min	Max	Mean	St.Dev.	Min	Max
M_{LATE}	5.13	4.85	0.00	14.67	6.56	5.21	0.00	19.67
M_{EARLY}	8.81	5.53	2.00	20.75	13.73	5.12	1.50	26.58
M_{EARLY}-M_{LATE}	3.68	4.03	-7.50	13.17	7.17	6.11	-6.17	23.92
$M_{MEDLATE}$	4.52	4.83	0.00	15.67	6.02	5.14	0.00	18.50
$M_{MEDEARLY}$	8.21	5.60	0.67	20.50	12.98	5.42	1.33	29.33
Working memory	64.09	9.64	30.00	74.00	63.47	10.38	20.00	75.00
Short-term memory	205.37	33.10	43.00	254.00	206.60	19.30	162.00	249.00
Math	61.23	16.88	34.00	99.00	55.81	17.85	21.00	110.00
Need for cognition	2.91	0.49	1.83	3.67	2.81	0.54	1.83	3.92
Perseverance	2.87	0.40	1.80	3.60	2.77	0.41	1.70	3.50
Risk	55.84	15.78	12.00	98.00	57.74	14.08	30.00	87.00
Sensation-seeking	2.82	0.65	1.25	3.83	2.91	0.74	1.33	4.00
Premeditation	2.92	0.48	1.55	3.82	2.88	0.37	1.91	3.64
Math anxiety	3.08	0.64	1.20	4.00	3.14	0.61	1.50	4.00
Age	22.93	2.76	19.00	35.00	22.16	2.10	19.00	27.00
Male	0.56	0.50	0.00	1.00	0.58	0.50	0.00	1.00
Carshare	0.32	0.23	0.00	1.00	0.40	0.45	0.00	3.00
Carowner	0.12	0.32	0.00	1.00	0.12	0.32	0.00	1.00
Windfall	0.09	0.29	0.00	1.00	0.14	0.41	0.00	2.00

Table 2a: Correlations between performance measures and covariates in the concurrent presentation treatment (T_{con}).
(Correlations are displayed in bold font with *p*-values underneath them.)

	M_{LATE}	M_{EARLY}	M_{EARLY}-M_{LATE}	$M_{MEDLATE}$	$M_{MEDEARLY}$	Working memory	Wmresidual	Short-term memory	Math	Need for cognition	Perseverance	Risk	Sensation-seeking	Premeditation	Math anxiety	Age	Male	Carshare	Carowner
M_{EARLY}	**0.750**																		
	0.000																		
M_{EARLY}-M_{LATE}	**-0.078**	**0.549**																	
	0.618	0.000																	
$M_{MEDLATE}$	**0.960**	**0.711**	**-0.116**																
	0.000	0.000	0.459																
$M_{MEDEARLY}$	**0.760**	**0.986**	**0.516**	**0.724**															
	0.000	0.000	0.000	0.000															
Working memory	**-0.022**	**0.071**	**0.263**	**-0.001**	**0.084**														
	0.891	0.650	0.088	0.997	0.592														
WMresidual	**0.179**	**0.206**	**0.134**	**0.174**	**0.128**	**0.749**													
	0.444	0.250	0.186	0.391	0.265	0.000													
Short-term memory	**-0.005**	**-0.131**	**-0.208**	**0.036**	**-0.110**	**0.372**	**-0.043**												
	0.974	0.403	0.182	0.821	0.483	0.014	0.786												
Math	**-0.119**	**-0.091**	**0.022**	**-0.111**	**-0.113**	**0.207**	**-0.140**	**0.199**											
	0.447	0.562	0.889	0.478	0.472	0.182	0.372	0.200											
Need for cognition	**0.037**	**0.148**	**0.211**	**0.036**	**0.145**	**-0.027**	**-0.157**	**0.010**	**0.142**										
	0.812	0.343	0.174	0.817	0.354	0.862	0.315	0.947	0.364										
Perseverance	**-0.121**	**-0.067**	**0.117**	**-0.072**	**-0.016**	**0.179**	**-0.077**	**-0.086**	**-0.006**	**0.318**									
	0.439	0.670	0.454	0.645	0.918	0.250	0.624	0.582	0.971	0.038									
Risk	**-0.045**	**-0.166**	**-0.224**	**-0.067**	**-0.155**	**-0.207**	**-0.136**	**0.025**	**0.028**	**-0.472**	**-0.302**								
	0.776	0.287	0.149	0.671	0.322	0.184	0.386	0.875	0.857	0.001	0.049								
Sensation-seeking	**0.286**	**0.279**	**0.027**	**0.217**	**0.298**	**0.154**	**0.066**	**0.126**	**0.031**	**0.201**	**-0.008**	**-0.437**							
	0.063	0.070	0.864	0.163	0.052	0.324	0.673	0.419	0.843	0.195	0.961	0.003							
Premeditation	**-0.302**	**-0.132**	**0.134**	**-0.251**	**-0.101**	**0.064**	**0.123**	**-0.282**	**0.094**	**0.107**	**0.206**	**0.102**	**-0.445**						
	0.049	0.400	0.393	0.104	0.521	0.683	0.431	0.067	0.547	0.496	0.186	0.515	0.003						
Math anxiety	**-0.130**	**-0.109**	**0.079**	**-0.116**	**-0.092**	**0.057**	**-0.034**	**0.167**	**0.312**	**0.510**	**0.196**	**-0.194**	**-0.008**	**0.070**					
	0.405	0.486	0.614	0.458	0.558	0.717	0.829	0.284	0.042	0.001	0.208	0.214	0.961	0.654					
Age	**-0.011**	**0.042**	**0.130**	**-0.044**	**0.060**	**-0.036**	**-0.028**	**-0.224**	**-0.296**	**0.168**	**0.059**	**0.005**	**-0.194**	**0.241**	**0.083**				
	0.942	0.790	0.408	0.779	0.702	0.819	0.858	0.148	0.054	0.281	0.709	0.976	0.212	0.119	0.597				
Male	**-0.147**	**-0.332**	**-0.287**	**-0.142**	**-0.308**	**0.006**	**-0.045**	**0.176**	**0.074**	**-0.053**	**0.051**	**-0.047**	**0.130**	**-0.076**	**0.019**	**0.021**			
	0.346	0.030	0.062	0.364	0.045	0.971	0.773	0.260	0.639	0.736	0.744	0.764	0.405	0.629	0.904	0.894			
Carshare	**-0.099**	**-0.158**	**-0.101**	**-0.089**	**-0.124**	**-0.002**	**-0.022**	**0.058**	**-0.048**	**-0.182**	**0.028**	**-0.155**	**-0.020**	**0.087**	**-0.215**	**-0.157**	**-0.050**		
	0.526	0.311	0.521	0.569	0.428	0.988	0.889	0.711	0.759	0.243	0.859	0.322	0.901	0.580	0.167	0.316	0.750		
Carowner	**-0.056**	**-0.225**	**-0.158**	**-0.053**	**-0.196**	**-0.117**	**-0.158**	**0.105**	**-0.161**	**-0.120**	**-0.097**	**0.237**	**-0.275**	**-0.288**	**0.196**	**0.083**	**0.031**	**-0.084**	
	0.723	0.147	0.312	0.737	0.208	0.454	0.312	0.501	0.303	0.443	0.536	0.126	0.074	0.061	0.207	0.598	0.846	0.595	
Windfall	**-0.029**	**0.013**	**-0.016**	**0.007**	**0.016**	**-0.068**	**-0.148**	**0.158**	**0.329**	**0.042**	**-0.084**	**0.016**	**-0.007**	**0.049**	**0.074**	**-0.085**	**0.285**	**-0.105**	**-0.116**
	0.853	0.935	0.918	0.967	0.918	0.665	0.342	0.311	0.031	0.789	0.591	0.918	0.967	0.757	0.636	0.589	0.064	0.502	0.458

Table 2b: Correlations between performance measures and covariates in the sequential presentation treatment (T_{seq}).
(Correlations are displayed in bold font with *p*-values underneath them.)

	M_{LATE}	M_{EARLY}	M_{EARLY}-M_{LATE}	$M_{MEDLATE}$	$M_{MEDEARLY}$	Working memory	Wmresidual	Short-term memory	Math	Need for cognition	Perseverance	Risk	Sensation-seeking	Premeditation	Math anxiety	Age	Male	Carshare	Carowner
M_{EARLY}	**0.337** 0.027																		
M_{EARLY}-M_{LATE}	**-0.473** 0.001	**0.588** 0.000																	
$M_{MEDLATE}$	**0.966** 0.000	**0.373** 0.014	**-0.416** 0.006																
$M_{MEDEARLY}$	**0.294** 0.055	**0.935** 0.000	**0.591** 0.000	**0.323** 0.034															
Working memory	**-0.345** 0.023	**-0.039** 0.807	**0.253** 0.102	**-0.303** 0.048	**0.021** 0.894														
WMresidual	**-0.353** 0.020	**-0.211** 0.174	**0.155** 0.322	**-0.359** 0.018	**-0.123** 0.434	**0.734** 0.000													
Short-term memory	**-0.269** 0.081	**0.079** 0.616	**0.314** 0.040	**-0.250** 0.105	**0.059** 0.706	**0.294** 0.056	**-0.064** 0.685												
Math	**-0.177** 0.256	**-0.109** 0.488	**0.011** 0.944	**-0.110** 0.482	**-0.126** 0.421	**0.350** 0.022	**0.066** 0.672	**-0.027** 0.864											
Need for cognition	**0.032** 0.841	**0.204** 0.190	**0.093** 0.555	**0.019** 0.905	**0.154** 0.326	**0.035** 0.823	**0.092** 0.558	**0.104** 0.507	**0.092** 0.556										
Perseverance	**0.054** 0.729	**0.144** 0.358	**-0.006** 0.968	**0.089** 0.571	**0.163** 0.297	**0.234** 0.131	**-0.149** 0.340	**0.045** 0.773	**0.068** 0.665	**0.059** 0.705									
Risk	**-0.141** 0.366	**0.031** 0.845	**0.126** 0.419	**-0.193** 0.216	**0.064** 0.682	**-0.291** 0.058	**-0.253** 0.101	**0.099** 0.528	**-0.201** 0.196	**-0.119** 0.448	**-0.187** 0.229								
Sensation-seeking	**0.085** 0.588	**0.247** 0.110	**0.161** 0.302	**0.082** 0.600	**0.279** 0.070	**0.153** 0.327	**0.148** 0.345	**0.031** 0.844	**-0.280** 0.069	**0.169** 0.280	**0.399** 0.008	**-0.158** 0.311							
Premeditation	**-0.102** 0.516	**-0.171** 0.272	**-0.083** 0.597	**0.021** 0.892	**-0.072** 0.645	**0.068** 0.664	**0.039** 0.802	**0.002** 0.990	**0.062** 0.692	**0.024** 0.878	**0.297** 0.054	**-0.287** 0.062	**0.018** 0.909						
Math anxiety	**0.018** 0.910	**0.009** 0.954	**0.015** 0.924	**0.029** 0.853	**0.032** 0.840	**0.016** 0.920	**0.113** 0.472	**0.048** 0.762	**0.330** 0.031	**0.485** 0.001	**0.071** 0.649	**-0.211** 0.176	**0.045** 0.774	**0.250** 0.106					
Age	**-0.002** 0.992	**-0.257** 0.096	**-0.240** 0.121	**-0.048** 0.759	**-0.354** 0.020	**-0.089** 0.570	**0.125** 0.423	**-0.063** 0.687	**-0.170** 0.277	**0.184** 0.238	**-0.200** 0.199	**0.076** 0.628	**-0.200** 0.199	**-0.178** 0.253	**0.073** 0.644				
Male	**-0.201** 0.195	**-0.105** 0.505	**0.004** 0.981	**-0.255** 0.099	**-0.082** 0.603	**0.128** 0.415	**0.160** 0.307	**-0.023** 0.885	**0.065** 0.681	**0.196** 0.208	**0.288** 0.061	**-0.044** 0.781	**0.204** 0.190	**0.015** 0.923	**-0.038** 0.809	**0.152** 0.331			
Carshare	**-0.002** 0.988	**0.401** 0.008	**0.265** 0.086	**0.051** 0.746	**0.309** 0.044	**0.186** 0.232	**0.063** 0.689	**0.139** 0.376	**0.070** 0.656	**0.258** 0.095	**0.285** 0.064	**-0.126** 0.420	**0.488** 0.001	**0.037** 0.816	**0.161** 0.304	**-0.137** 0.382	**-0.056** 0.720		
Carowner	**0.120** 0.444	**0.035** 0.823	**-0.064** 0.682	**0.214** 0.169	**-0.023** 0.882	**-0.003** 0.985	**0.053** 0.738	**-0.094** 0.551	**0.023** 0.882	**0.182** 0.244	**-0.053** 0.736	**-0.234** 0.131	**0.091** 0.562	**0.293** 0.056	**0.003** 0.985	**0.130** 0.405	**0.014** 0.931	**0.227** 0.143	
Windfall	**-0.352** 0.021	**0.186** 0.231	**0.401** 0.008	**-0.350** 0.021	**0.175** 0.263	**0.223** 0.151	**0.181** 0.246	**-0.037** 0.812	**0.330** 0.031	**-0.158** 0.313	**-0.273** 0.077	**0.060** 0.701	**-0.151** 0.335	**-0.159** 0.308	**0.028** 0.859	**-0.028** 0.860	**0.021** 0.896	**0.063** 0.691	**-0.131** 0.401

Table 3: Censored normal regressions of asymptotic forecasting performance (M_{LATE}) on cognitive, personality and demographic covariates for *Model 1 – Model 3*.

REGRESSOR	*Model 1*		*Model 2*		*Model 3*	
	Est. T_{con} (std. err.)	Est. T_{seq} (std. err.)	Est. T_{con} (std. err.)	Est. T_{seq} (std. err.)	Est. T_{con} (std. err.)	Est. T_{seq} (std. err.)
intercept	31.734** (13.233)		24.253* (13.414)		27.186** (11.156)	
WMresidual	0.023 (0.131)	-0.316** (0.122)	-0.0063 (0.126)	-0.352*** (0.118)	-0.060 (0.106)	-0.337*** (0.098)
	(*)		(*)		(*)	
Short-term memory	-0.030 (0.033)	-0.174*** (0.056)	-0.0080 (0.034)	-0.166*** (0.054)	-0.011 (0.028)	-0.182*** (0.045)
	(**)		(**)		(***)	
Math	—		-0.131* (0.066)	0.037 (0.059)	-0.130** (0.055)	0.041 (0.055)
			(*)		(**)	
Need for cognition	—		-0.455 (1.533)		-2.726* (1.371)	
Risk	—		—		-0.138*** (0.045)	
Windfall	—		—		-5.846*** (1.997)	
Log likelihood	-133.039		-131.068		-123.048	

Notes: Subjects = 86, 43 in T_{con} and 43 in T_{seq}. *,**, and *** indicate significance of estimates at the 10%, 5%, and 1% significance level, respectively. Analogously, (*),(**), and (***) indicate the significance of across-treatment differentials. In all models, the included regressors are jointly highly significant.

Table 4: Censored normal regressions of asymptotic forecasting performance (M_{LATE}) on cognitive, personality and demographic covariates and M_{EARLY}residual for *Model 4 – Model 6*. OLS regression in *Model 7*.

REGRESSOR	*Model 4* Est. T_{con} (std. err.)	*Model 4* Est. T_{seq} (std. err.)	*Model 5* Est. T_{con} (std. err.)	*Model 5* Est. T_{seq} (std. err.)	*Model 6* Est. T_{con} (std. err.)	*Model 6* Est. T_{seq} (std. err.)	*Model 7* Est. T_{con} (std. err.)	*Model 7* Est. T_{seq} (std. err.)
intercept	31.800** (11.778)		24.441** (11.815)		24.208** (9.731)		19.542** (8.954)	
WMresidual	0.018 (0.117)	-0.291** (0.108)	-0.011 (0.111)	-0.326*** (0.104)	-0.044 (0.083)	-0.290*** (0.075)	-0.060 (0.069)	-0.275*** (0.078)
	(*)		(*)		(**)		(*)	
Short-term memory	-0.030 (0.305)	-0.175*** (0.050)	-0.009 (0.030)	-0.167*** (0.048)	-0.0065 (0.023)	-0.186*** (0.035)	-0.011 (0.029)	-0.178*** (0.031)
	(**)		(***)		(***)		(***)	
Math	—		-0.130** (0.058)	0.034 (0.052)	-0.113** (0.044)	0.020 (0.042)	-0.113*** (0.040)	0.025 (0.075)
			(*)		(**)		(*)	
Need for cognition	—		-0.449 (1.350)		-0.746 (1.622)	-4.408** (1.637)	-1.102 (1.467)	-4.188** (1.593)
Risk	—		—		-0.135*** (0.036)		-0.112*** (0.039)	
Math anxiety	—		—		-4.031*** (1.220)	1.565 (1.415)	-3.817** (1.791)	1.667 (1.658)
					(***)		(**)	
Windfall	—		—		-5.168*** (1.590)		-4.854*** (1.624)	
M_{EARLY}residual	0.542*** (0.162)		0.533*** (0.152)		0.557*** (0.115)		0.500*** (0.137)	
Log likelihood	-127.945		-125.523		-111.758		R^2=0.745	

Notes: Subjects = 86, 43 in T_{con} and 43 in T_{seq}. *,**, and *** indicate significance of estimates at the 10%, 5%, and 1% significance level, respectively. Analogously, (*),(**), and (***) indicate the significance of across-treatment differentials. In all models, the included regressors are jointly highly significant. Heteroskedasticity-robust standard errors are computed for OLS estimates in *Model 7*.

APPENDIX 1: INSTRUCTIONS (SEQUENTIAL PRESENTATION TREATMENT)

The purpose of the experiment is to investigate how people make predictions. Hence we will ask you to make a number of predictions in the **prediction task** described below. Your payoff will depend on the accuracy of your predictions.

OVERVIEW OF THE PREDICTION TASK

In the prediction task you will repeatedly predict a number series that we call Omega. **You will predict the next-period value of Omega based on information displayed on your screen.** After each prediction, you will be informed on your screen how accurately you predicted the actual next-period value of Omega.

Your predictions will have no effect on Omega because Omega was generated before this experiment. **In each period, Omega is the sum of three independent components:**

Omega = Basic component + Cyclical component + Error

- The **Basic component** will in each period have one of the following values drawn at random: 10, 20, 30, or 40.
- The **Cyclical component** is a repeating sequence of several different numbers. You will try to discover the Cyclical component during the prediction task.
- The **Error** will in each period have one of the following values drawn at random (with equal chance of being drawn): -8, -4, 0, 4, 8.

You will predict the next-period value of Omega based on the following information displayed on your screen:

- In each period you will observe the values of **Omega** for the past 8 periods.
- In each period you will observe the values of the **Basic component** for the past 8 periods. You will also observe the next-period value of the Basic component, so that you can predict the next-period value of Omega.
- You will not observe the **Cyclical component** on your screen. However, we explain below how you can discover the Cyclical component by paying attention to the differences between the values of Omega and the Basic component.

- You will not observe the **Error** on your screen. Because the value of the Error in any period is unpredictable, you will usually not be able to predict Omega completely accurately. Nevertheless, the Error will affect Omega, and hence the accuracy of your prediction, by at most -8 or +8.

The following HELPBOX 1 and HELPBOX 2 explain the components of Omega in detail. After that we will explain how the prediction task runs.

HELPBOX 1: What is a Cyclical component?

A Cyclical component is a **fixed sequence of several different numbers repeating over periods**. There will be only **one Cyclical component** throughout the whole prediction task. The Cyclical component may consist of two or more numbers.

Here are three examples of a Cyclical component consisting of **two** numbers:

27, 44, 27, 44, 27, 44,...etc.

62, 40, 62, 40, 62, 40,...etc.

39, 75, 39, 75, 39, 75,...etc.

Here are three examples of a Cyclical component consisting of **three** numbers:

27, 44, 59, 27, 44, 59, 27, 44, 59,...etc.

62, 40, 17, 62, 40, 17, 62, 40, 17,...etc.

39, 75, 53, 39, 75, 53, 39, 75, 53,...etc.

Here are three examples of a Cyclical component consisting of **four** numbers:

27, 44, 59, 69, 27, 44, 59, 69, 27, 44, 59, 69,...etc.

62, 40, 17, 45, 62, 40, 17, 45, 62, 40, 17, 45,...etc.

39, 75, 53, 68, 39, 75, 53, 68, 39, 75, 53, 68,...etc.

Here are three examples of a Cyclical component consisting of **five** numbers:

27, 44, 59, 69, 30, 27, 44, 59, 69, 30, 27, 44, 59, 69, 30,...etc.

62, 40, 17, 45, 71, 62, 40, 17, 45, 71, 62, 40, 17, 45, 71,...etc.

39, 75, 53, 68, 25, 39, 75, 53, 68, 25, 39, 75, 53, 68, 25,...etc.

The Cyclical component in the prediction task will be similar to the examples above, but we will not tell you how many numbers and which numbers it contains. We only tell you that **there will be only one Cyclical component throughout the whole prediction task**.

Discovering and correctly using the Cyclical component (see HELPBOX 2), together with observing and correctly using the Basic component, will enable you to predict Omega more accurately.

HELPBOX 2: How to discover the Cyclical component?

The four tables below illustrate the importance of discovering the correct Cyclical component for predicting Omega. Each of the four tables contains a different Cyclical component: we chose four different Cyclical components from HELPBOX 1. By contrast, all four tables contain the same values of the Basic component and the Error.

Period	Basic component	Cyclical component	Error	Omega
1	40	27	4	71
2	20	44	-8	56
3	30	27	8	65
4	10	44	0	54
5	20	27	4	51
6	40	44	0	84
7	30	27	-4	53
8	10	44	8	62
9	20	27	8	55
10	10	44	0	54
11	40	27	-4	63
12	10	44	-8	46
13	30	27	-8	49
14	20	44	4	68
15	10	27	-4	33
16	30	44	-8	66
17	20	27	4	51
18	30	44	8	82
19	10	27	0	37
20	40	44	-4	80

Period	Basic component	Cyclical component	Error	Omega
1	40	62	4	106
2	20	40	-8	52
3	30	17	8	55
4	10	62	0	72
5	20	40	4	64
6	40	17	0	57
7	30	62	-4	88
8	10	40	8	58
9	20	17	8	45
10	10	62	0	72
11	40	40	-4	76
12	10	17	-8	19
13	30	62	-8	84
14	20	40	4	64
15	10	17	-4	23
16	30	62	-8	84
17	20	40	4	64
18	30	17	8	55
19	10	62	0	72
20	40	40	-4	76

Period	Basic component	Cyclical component	Error	Omega
1	40	39	4	83
2	20	75	-8	87
3	30	53	8	91
4	10	68	0	78
5	20	39	4	63
6	40	75	0	115
7	30	53	-4	79
8	10	68	8	86
9	20	39	8	67
10	10	75	0	85
11	40	53	-4	89
12	10	68	-8	70
13	30	39	-8	61
14	20	75	4	99
15	10	53	-4	59
16	30	68	-8	90
17	20	39	4	63
18	30	75	8	113
19	10	53	0	63
20	40	68	-4	104

Period	Basic component	Cyclical component	Error	Omega
1	40	27	4	71
2	20	44	-8	56
3	30	59	8	97
4	10	69	0	79
5	20	30	4	54
6	40	27	0	67
7	30	44	-4	70
8	10	59	8	77
9	20	69	8	97
10	10	30	0	40
11	40	27	-4	63
12	10	44	-8	46
13	30	59	-8	81
14	20	69	4	93
15	10	30	-4	36
16	30	27	-8	49
17	20	44	4	68
18	30	59	8	97
19	10	69	0	79
20	40	30	-4	66

You can observe in the tables that in each period, Omega is indeed the sum of the three independent components: **Omega = Basic component + Cyclical component + Error**. You can further see that although the four tables contain exactly the same values of the Basic component and the Error, the different Cyclical components lead to considerably different values of Omega across the four tables. That is why **discovering the correct Cyclical component is important for predicting Omega**.

You can discover the Cyclical component by paying attention to the **differences between the values of Omega and the Basic component**. You will in each period observe the values of Omega and the Basic component for the past 8 periods, so you will be able to calculate the differences "**Omega – Basic component**". These differences will not usually tell you the exact values of the Cyclical component since **Omega – Basic component = Cyclical component + Error**. Nevertheless, paying attention to the differences **Omega – Basic component** will enable you to gradually discover how many numbers and which numbers the Cyclical component contains.

Discovering and correctly using the Cyclical component, together with observing and correctly using the Basic component, will enable you to predict Omega more accurately.

HOW THE PREDICTION TASK RUNS IN EACH PERIOD

The following example will show you how the prediction task runs in each period. The example uses values from the first table in HELPBOX 2. Looking back at the table, imagine you are currently in period 15 and hence **you are predicting the value of Omega for the next period 16**. The prediction task has the following five steps in each period:

1. On the first screen, you will be asked to **bet on your prediction of Omega**. Betting is explained in the next section below.
2. After entering your bet, you will observe for 10 seconds a screen with the values of the **Basic component** for the past 8 periods, and you will also observe the value of the Basic component for the next period:

Current period 15 of 100	Time remaining 10

	Basic component
period 8	10
period 9	20
period 10	10
period 11	40
period 12	10
period 13	30
period 14	20
Current period 15	10
Next period 16	30

3. Then you will observe for 15 seconds a screen with the values of **Omega** for the past 8 periods:

Current period 15 of 100	Time remaining 15

	Omega
period 8	62
period 9	55
period 10	54
period 11	63
period 12	46
period 13	49
period 14	68
Current period 15	33
Next period 16	?

4. On the next screen, you will be asked "**What is your prediction of Omega for the next period?**"
5. After entering your prediction, the last screen will show you the difference between your prediction of Omega and the actual next-period value of Omega.

The forecasting task will in each period have the five steps described above. In each period, **the values of the Basic component and Omega displayed in steps 2 and 3 will be updated**. Hence in the example above, the updated screen with the values of Omega would in period 16 look as follows:

Current period 16 of 100	Time remaining 15

	Omega
period 9	55
period 10	54
period 11	63
period 12	46
period 13	49
period 14	68
period 15	33
Current period 16	66
Next period 17	?

The prediction task will have **92 periods**: you will make the first prediction of Omega in period 8 and the last one in period 99. The screens in the forecasting task are displayed only until the **time remaining** for a given screen has elapsed. An exception are the screens on which you will be entering your bets and your predictions of Omega – these two screens are not time constrained and the timer on them is only informative.

Note: As you already know, the Error – one of the components of Omega – is unpredictable and can take any of the values -8, -4, 0, 4, or 8. In the above example, the value of the Error in period 16 is -8, which leads to the value of Omega of 66. However, the value of the Error in period 16 could equally likely be -4, 0, 4, or 8, which would have led to the value of Omega of 70, 74, 78, or 82, respectively. Since **the next-period value of the Error is unpredictable**, you will usually not be able to predict Omega completely accurately. Nevertheless, you do know that the Error will affect Omega, and hence the accuracy of your prediction, by at most -8 or +8.

YOUR PAYOFF IN THE PREDICTION TASK

Your payoff in the prediction task will be denominated in ECU (Experimental Currency Unit) and will be converted to CZK at the end of the experiment (see below). Your payoff will depend on the accuracy of your prediction. The accuracy of your prediction will be measured in items of **your prediction error**, which is the difference between your prediction of Omega and the actual next-period value of Omega. **The lower your prediction error, the higher your payoff in ECU**. You will observe your prediction error on the last screen in each period (in step 5).

Your payoff will also depend on how many ECU you bet on your prediction. Specifically, on the first screen in each period, you will be asked "**Would you like to bet more than 50 ECU on your prediction in the current period? Please enter a bet between 50 and 100 ECU.**" It will generally be profitable for you to bet more ECU the lower your prediction error is. The following HELPBOX 3 explains why.

HELPBOX 3: How to bet on your prediction?

On the first screen in each period, we will ask you to bet an amount between 50 and 100 ECU on your prediction in the current period. Your payoff will depend on the number of ECU you bet and the number of remaining ECU you do not bet according to the following formula:

- **Every ECU you bet earns you [20 – your prediction error] ECU.** (If your prediction error is 20 or more, however, every ECU you bet earns you nothing.)
- **Every remaining ECU you do not bet always earns you 9 ECU.**

Suppose, for example, that you bet 70 ECU and your prediction error is **10**.
The 70 ECU you bet earns you 70 x [20 – **10**] = 700 ECU.
The remaining (100 – 70) ECU you do not bet earns you (100 – 70) x 9 = 270 ECU.
Thus your total payoff in this example is 700 + 270 = 970 ECU.

You can see from the above formula that if your prediction error is **11**, every ECU you bet earns you [20 – **11**] = 9 ECU, which is what every ECU you do not bet earns you as well. Therefore, **betting more than 50 ECU is profitable only if your prediction error is on average below 11**. The following payoff table closer illustrates this basic betting rule:

		Your prediction error			
		14	**11**	**10**	**5**
Your bet in ECU	**50**	750	900	950	1200
	70	690	900	**970**	1320
	100	600	900	1000	1500

The payoff table shows what your payoff would be if you bet 50, 70 or 100 ECU and your prediction error were on average 14, 11, 10 or 5. The above example, where we assumed your bet is 70 ECU and your prediction error is 10, is included in the payoff table (the resulting payoff of **970** ECU is in bold). The remaining payoffs in the payoff table are calculated in identical manner.

Looking at table column by column, you can see that betting more than 50 ECU is indeed profitable only if your average prediction error is below 11, as in the last two columns. By contrast, if your average prediction error is above 11, as in the first column, it is most profitable to bet the lowest possible amount of 50 ECU. You can further see that as your average prediction error improves from 10 to 5, betting more than 50 ECU becomes even more profitable: when your prediction error is 10, you can earn 950 to 1000 ECU, whereas when your prediction error is 5, you can earn 1200 to 1500 ECU. Hence it is **profitable for you to bet more ECU the lower your prediction error is**.

Especially in the initial periods of the prediction task, it may be hard for you to judge whether your average prediction error is above or below 11. During the prediction task, however, you should learn how to better judge your average prediction error and that will help you to make profitable betting decisions.

The reason we are asking you to bet is so that we can see how your ability to correctly judge your average prediction error develops during the prediction task. If you wonder why we are "forcing" you to bet at least 50 ECU, this is because we always want you to benefit from improving your prediction accuracy. Of course, the more ECU you bet, the more you can potentially benefit from improving your prediction accuracy.

For your betting, it is most important that you understand the basic betting rule: **you can earn more money not only by predicting accurately, but also by making profitable betting decisions – that is, by betting more than 50 ECU only if your prediction error is on average below 11**. Nevertheless, if you wish to have a detailed payoff table to look at, we have provided a complete payoff table for you at the end of these Instructions. You can read the complete payoff table in exactly the same way as the simpler (less detailed) payoff table in HELPBOX 3.

In the prediction task, you will make 92 bets and 92 predictions. Your total payoff in ECU will be the sum of your payoffs in the 92 periods. This means that you can earn over 180 000 ECU. Your total payoff will be converted to CZK at the rate of 200 ECU = 1 CZK, which means that you can earn over 900 CZK. You will be paid off in cash immediately after the experiment.

FINAL COMMENTS ON THE PREDICTION TASK

As you go along the prediction task, please bear in mind that predicting Omega is not easy. Discovering and correctly using the Cyclical component, together with observing and correctly using the Basic component, will enable you to predict Omega more accurately. You should be able to gradually learn how to make better predictions. Bear in mind, however, that since the next-period value of the Error is unpredictable, you will usually not be able to predict Omega completely accurately.

If you wish to ask any questions, please raise your hand. The experimenter will come to you and answer your question privately.

If you are ready to start the prediction task, please raise your hand holding the paper instructions. The experimenter will come to you and launch the prediction task.

Once the prediction task is running, you will first go through a couple of training screens which give you an opportunity to check that you have correctly understood the instructions.

Please do not make notes of any kind during the prediction task.

THE COMPLETE PAYOFF TABLE

The following complete payoff table shows how your payoff in ECU depends on "Your bet in ECU" and on "Your prediction error". You can read this complete payoff table in exactly the same way as the simpler (less detailed) payoff table in HELPBOX 3.

	Your prediction error																				
Your bet in ECU	>19	19	18	17	16	15	14	13	12	11	10	9	8	7	6	5	4	3	2	1	0
50	450	500	550	600	650	700	750	800	850	900	950	1000	1050	1100	1150	1200	1250	1300	1350	1400	1450
51	441	492	543	594	645	696	747	798	849	900	951	1002	1053	1104	1155	1206	1257	1308	1359	1410	1461
52	432	484	536	588	640	692	744	796	848	900	952	1004	1056	1108	1160	1212	1264	1316	1368	1420	1472
53	423	476	529	582	635	688	741	794	847	900	953	1006	1059	1112	1165	1218	1271	1324	1377	1430	1483
54	414	468	522	576	630	684	738	792	846	900	954	1008	1062	1116	1170	1224	1278	1332	1386	1440	1494
55	405	460	515	570	625	680	735	790	845	900	955	1010	1065	1120	1175	1230	1285	1340	1395	1450	1505
56	396	452	508	564	620	676	732	788	844	900	956	1012	1068	1124	1180	1236	1292	1348	1404	1460	1516
57	387	444	501	558	615	672	729	786	843	900	957	1014	1071	1128	1185	1242	1299	1356	1413	1470	1527
58	378	436	494	552	610	668	726	784	842	900	958	1016	1074	1132	1190	1248	1306	1364	1422	1480	1538
59	369	428	487	546	605	664	723	782	841	900	959	1018	1077	1136	1195	1254	1313	1372	1431	1490	1549
60	360	420	480	540	600	660	720	780	840	900	960	1020	1080	1140	1200	1260	1320	1380	1440	1500	1560
61	351	412	473	534	595	656	717	778	839	900	961	1022	1083	1144	1205	1266	1327	1388	1449	1510	1571
62	342	404	466	528	590	652	714	776	838	900	962	1024	1086	1148	1210	1272	1334	1396	1458	1520	1582
63	333	396	459	522	585	648	711	774	837	900	963	1026	1089	1152	1215	1278	1341	1404	1467	1530	1593
64	324	388	452	516	580	644	708	772	836	900	964	1028	1092	1156	1220	1284	1348	1412	1476	1540	1604
65	315	380	445	510	575	640	705	770	835	900	965	1030	1095	1160	1225	1290	1355	1420	1485	1550	1615
66	306	372	438	504	570	636	702	768	834	900	966	1032	1098	1164	1230	1296	1362	1428	1494	1560	1626
67	297	364	431	498	565	632	699	766	833	900	967	1034	1101	1168	1235	1302	1369	1436	1503	1570	1637
68	288	356	424	492	560	628	696	764	832	900	968	1036	1104	1172	1240	1308	1376	1444	1512	1580	1648
69	279	348	417	486	555	624	693	762	831	900	969	1038	1107	1176	1245	1314	1383	1452	1521	1590	1659
70	270	340	410	480	550	620	690	760	830	900	970	1040	1110	1180	1250	1320	1390	1460	1530	1600	1670
71	261	332	403	474	545	616	687	758	829	900	971	1042	1113	1184	1255	1326	1397	1468	1539	1610	1681
72	252	324	396	468	540	612	684	756	828	900	972	1044	1116	1188	1260	1332	1404	1476	1548	1620	1692
73	243	316	389	462	535	608	681	754	827	900	973	1046	1119	1192	1265	1338	1411	1484	1557	1630	1703
74	234	308	382	456	530	604	678	752	826	900	974	1048	1122	1196	1270	1344	1418	1492	1566	1640	1714
75	225	300	375	450	525	600	675	750	825	900	975	1050	1125	1200	1275	1350	1425	1500	1575	1650	1725
76	216	292	368	444	520	596	672	748	824	900	976	1052	1128	1204	1280	1356	1432	1508	1584	1660	1736
77	207	284	361	438	515	592	669	746	823	900	977	1054	1131	1208	1285	1362	1439	1516	1593	1670	1747
78	198	276	354	432	510	588	666	744	822	900	978	1056	1134	1212	1290	1368	1446	1524	1602	1680	1758
79	189	268	347	426	505	584	663	742	821	900	979	1058	1137	1216	1295	1374	1453	1532	1611	1690	1769
80	180	260	340	420	500	580	660	740	820	900	980	1060	1140	1220	1300	1380	1460	1540	1620	1700	1780
81	171	252	333	414	495	576	657	738	819	900	981	1062	1143	1224	1305	1386	1467	1548	1629	1710	1791
82	162	244	326	408	490	572	654	736	818	900	982	1064	1146	1228	1310	1392	1474	1556	1638	1720	1802
83	153	236	319	402	485	568	651	734	817	900	983	1066	1149	1232	1315	1398	1481	1564	1647	1730	1813
84	144	228	312	396	480	564	648	732	816	900	984	1068	1152	1236	1320	1404	1488	1572	1656	1740	1824
85	135	220	305	390	475	560	645	730	815	900	985	1070	1155	1240	1325	1410	1495	1580	1665	1750	1835
86	126	212	298	384	470	556	642	728	814	900	986	1072	1158	1244	1330	1416	1502	1588	1674	1760	1846
87	117	204	291	378	465	552	639	726	813	900	987	1074	1161	1248	1335	1422	1509	1596	1683	1770	1857
88	108	196	284	372	460	548	636	724	812	900	988	1076	1164	1252	1340	1428	1516	1604	1692	1780	1868
89	99	188	277	366	455	544	633	722	811	900	989	1078	1167	1256	1345	1434	1523	1612	1701	1790	1879
90	90	180	270	360	450	540	630	720	810	900	990	1080	1170	1260	1350	1440	1530	1620	1710	1800	1890
91	81	172	263	354	445	536	627	718	809	900	991	1082	1173	1264	1355	1446	1537	1628	1719	1810	1901
92	72	164	256	348	440	532	624	716	808	900	992	1084	1176	1268	1360	1452	1544	1636	1728	1820	1912
93	63	156	249	342	435	528	621	714	807	900	993	1086	1179	1272	1365	1458	1551	1644	1737	1830	1923
94	54	148	242	336	430	524	618	712	806	900	994	1088	1182	1276	1370	1464	1558	1652	1746	1840	1934
95	45	140	235	330	425	520	615	710	805	900	995	1090	1185	1280	1375	1470	1565	1660	1755	1850	1945
96	36	132	228	324	420	516	612	708	804	900	996	1092	1188	1284	1380	1476	1572	1668	1764	1860	1956
97	27	124	221	318	415	512	609	706	803	900	997	1094	1191	1288	1385	1482	1579	1676	1773	1870	1967
98	18	116	214	312	410	508	606	704	802	900	998	1096	1194	1292	1390	1488	1586	1684	1782	1880	1978
99	9	108	207	306	405	504	603	702	801	900	999	1098	1197	1296	1395	1494	1593	1692	1791	1890	1989
100	0	100	200	300	400	500	600	700	800	900	1000	1100	1200	1300	1400	1500	1600	1700	1800	1900	2000

APPENDIX 2: EXCERPT FROM THE DEBRIEFING QUESTIONNAIRE

1. Please write down how many and which numbers the Cyclical component consisted of. If you did not discover the exact values, please write down approximate values or possible alternatives:

2. Imagine that you were asked to help future participants in the prediction task. What would be your most important piece of advice? What should the future participants concentrate on when solving the prediction task? Imagine that the future participants will face a different Cyclical component, so it would not help them if you told them the values of the Cyclical component. Instead, try to describe them a few key steps necessary to accurately forecast Omega.

Please select answers which best describe <u>your</u> behavior in the forecasting experiment.

3. Which values of the Basic component and Omega did you pay attention to during the experiment? Please select <u>1 answer</u> best describing your behavior.

(A) I paid attention to all displayed values of the Basic component and Omega.
(B) I paid attention only to the most recent displayed values of the Basic component and Omega in the last period.
(C) I paid attention only to the most recent displayed values of the Basic component and Omega for the past several periods.
(D) I paid attention to different values of the Basic component and Omega.
If your chose (D), please specify which values you paid attention to:

4. Which of the following statements best describes your way of discovering the Cyclical component? Please select 1 answer best describing your behavior.

(A) I paid attention to differences “Omega – Basic component” in several consecutive periods, so that I could discover how many and which numbers the Cyclical component consists of.

(B) I paid attention to differences “Omega – Basic component” several periods apart (i.e. in non-consecutive periods), so that I could discover how many and which numbers the Cyclical component consists of.

(C) I used a different way of discovering the Cyclical component.

(D) I did not pay attention to discovering the Cyclical component.

If you chose (C) or (D), please specify your answer:

5. How did the presence of the Error influence the way you were predicting Omega? Please select 2 answers best describing how you dealt with the presence of the Error.

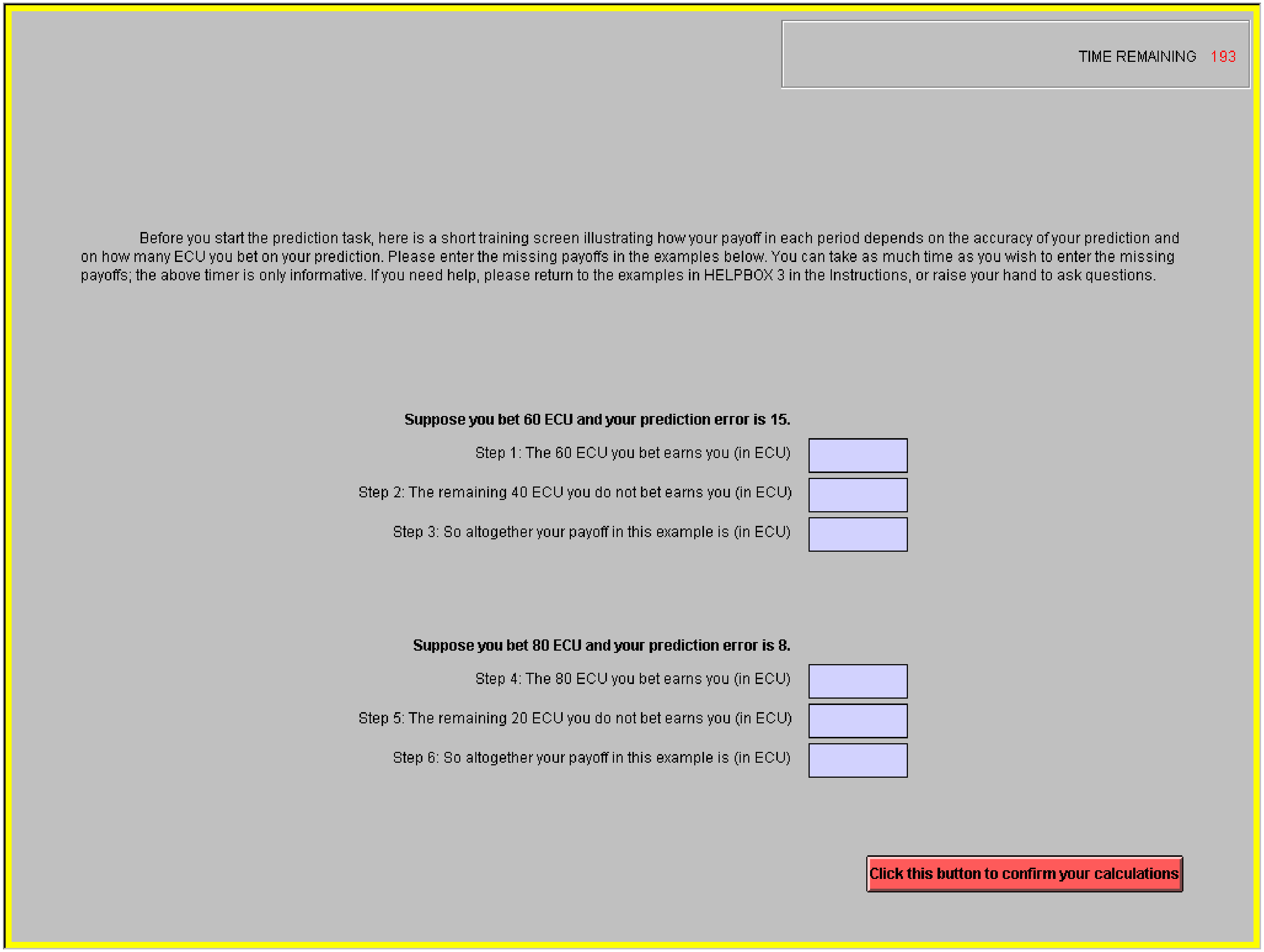
(A) Because the Error was affecting Omega and hence the accuracy of my predictions, I tried to predict the value of the Error in the next period.

(B) Especially the large values of the Error (+8 and -8) allowed me to discover more precisely the values of the Cyclical component.

(C) Even though the Error was affecting Omega and hence the accuracy of my predictions, I tried to predict Omega as if the value of the error in the next period were zero.

(D) Especially the smaller values of the Error (+4, 0, and -4) allowed me to discover more precisely the values of the Cyclical component.

APPENDIX 3: TRAINING SCREENS COMPLETED BY SUBJECTS BEFORE THE FORECASTING TASK



TIME REMAINING 193

Before you start the prediction task, here is a short training screen illustrating how your payoff in each period depends on the accuracy of your prediction and on how many ECU you bet on your prediction. Please enter the missing payoffs in the examples below. You can take as much time as you wish to enter the missing payoffs; the above timer is only informative. If you need help, please return to the examples in HELPBOX 3 in the Instructions, or raise your hand to ask questions.

Suppose you bet 60 ECU and your prediction error is 15.

Step 1: The 60 ECU you bet earns you (in ECU)

Step 2: The remaining 40 ECU you do not bet earns you (in ECU)

Step 3: So altogether your payoff in this example is (in ECU)

Suppose you bet 80 ECU and your prediction error is 8.

Step 4: The 80 ECU you bet earns you (in ECU)

Step 5: The remaining 20 ECU you do not bet earns you (in ECU)

Step 6: So altogether your payoff in this example is (in ECU)

Click this button to confirm your calculations

TIME REMAINING 263

Here is another short training screen illustrating how **Omega = Basic component + Cyclical component + Error**. Please enter the missing values in the table below (enter the missing values of the Cyclical component first). You can take as much time as you wish to enter the missing values; the above timer is only informative. If you need help, please return to the examples in HELPBOX 1 and HELPBOX 2 in the Instructions, or raise your hand to ask questions.

	Basic component	Cyclical component	Error	Omega
period 1	40	38	0	78
period 2	10			75
period 3	30		0	68
period 4	20			97
period 5	10	38	-6	
period 6	40		0	111
period 7	20		6	64
period 8	30		0	101
period 9	40		-6	72
period 10			6	107

Click this button to confirm your calculations