Free to Choose: Testing the Pure Motivation Effect of Autonomous Choice

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Abstract

We conduct an experimental test of the long-standing conjecture that autonomy increases motivation and task performance. Subjects face a menu consisting of two projects: risky and safe. The probability that the risky project succeeds depends on the subject's effort. In one treatment, subjects choose a project from the menu; in the other treatment, a project is assigned to them. Using a difference-in-differences approach which controls for selection into preferred projects, we show that autonomy (the act of choosing) can have a significant pure motivation effect on effort. Interesting patterns in the data, including how the pure motivation effect depends on properties of the (unchosen) safe project, are consistent with subjects experiencing a feeling of regret when they choose a risky project that fails.

1 Introduction

If workers differ in preferences and abilities, then letting them choose which project to work on may improve the match between workers and projects.

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This benefit of decentralization, called the *selection effect*, is perfectly consistent with standard economic theory. However, the gains from decentralization may go beyond the selection effect. Psychologists and organization theorists argue that autonomy generates a psychological state of personal responsibility which increases motivation and job performance. A worker will try harder to ensure that a project succeeds if the project was chosen by him rather than by someone else. This *pure motivation effect* implies that, controlling for project attributes and material incentives, autonomy will have a positive effect on effort.

For example, DeCharms (1968) argued that the key to motivation is to perceive oneself as being the cause of one's own actions. More recently, Cassar and Meier (2018) argued that autonomy adds meaning to a job and is directly related to workers' productivity and willingness to work. The link between autonomy and motivation is also a cornerstone of Deci and Ryan's self-determination theory. This link is postulated to be a psychological effect which is not caused by a concern about material incentives (promotion or salary increase) and so goes beyond standard economic theory.[1] Spector's (1986) meta analysis of empirical studies suggested that a high degree of autonomy is typically associated with high job performance, but this does not imply a causal effect (Renn and Vandenberg, 1995). Ryan and Deci's (2000) experiments did establish a causal effect: subjects who chose which puzzle to work on (the autonomy condition) were more motivated to solve the puzzle than subjects who were assigned a puzzle. However, this may not have been a pure motivation effect: a subject may prefer a particular puzzle because she finds it easier or more interesting, so autonomy might improve the match between subjects and puzzles (the selection effect).

Babcock, Bedard, Charness, Hartman and Royer (2015) recently found a pattern in their data that did indicate a pure motivation effect. Their

[1]Standard economic theory can explain why a worker might work hard to signal that he is competent, in order to obtain a future material reward such as a pay increase. But the pure motivation effect discussed by psychologists and organization theorists is due to an altered psychological state which does not require any such material incentives.

subjects could choose to work individually or in teams, yet selection was a non-issue as virtually all subjects chose to work individually. Their empirical strategy cannot be easily replicated in scenarios of varying degrees of autonomy, as would be required to test behavioral theories. Indeed, the absence of self-selection into different tasks indicates a lack of real autonomy. Real autonomy would involve several viable (but significantly different) options so that different agents are likely to make different choices. By using a difference-in-differences design, we identify a pure motivation effect even though subjects do self-select into different tasks. By examining the sensitivity of the pure motivation effect to variations in the incentive properties of different choice menus, we can test the predictions of a leading behavioral theory, namely, regret theory (Bell, 1982; Loomes and Sugden, 1982). Our simple decision theoretic setting also rules out any possible confounding effects of strategic considerations.

Our experiment is designed to identify the pure motivation effect while controlling for the selection effect. Subjects face a sequence of menus, each consisting of two projects: a safe project which always succeeds, and a risky project. The risky project can be attempted multiple times to increase the probability of success, incurring a cost for each attempt. The number of chosen attempts represents costly effort.[2] The basic question is whether a subject would supply more effort on a project when he himself has chosen it from a menu (as opposed to having been assigned this project from the same menu).

Our experiment has two treatments. In the *Chosen* treatment, subjects choose projects from menus; in the *Assigned* treatment subjects are randomly assigned projects from menus. Comparing average effort levels between these two treatments would provide an estimate of the sum of the pure motivation effect and the selection effect. The selection effect would be due to a better matching of subjects with projects (based on, say, individual risk at-

[2]An analogous real-world task would be deciding how many job applications to send out, knowing that each application has a given probability of success.

titudes) in the Chosen treatment. We control for the selection effect with a between-subject design, where each subject experiences only a single treatment variation.[3]

Each treatment has two parts: a Menu part (where a project is either chosen or assigned from a menu, depending on treatment) and an Isolation part (where each project is presented in isolation, not as part of any menu). The Isolation part does not differ between treatments. To estimate the pure motivation effect, we use difference-in-differences of effort choices.[4] The first differencing is between a subject's effort on a project in the Menu part and his effort on the same project in the Isolation part of the same treatment. Since this differencing contrasts two trials with the same subject and the same project, standard theory predicts a zero difference. Importantly, the differencing eliminates any selection effect, because a subject who is well matched with a particular project in the Menu part is equally well matched with the same project in the Isolation part. Thus we obtain, for each treatment, an average difference in effort levels between the two parts. Next, we contrast this average difference between the two treatments. A pure motivation effect would boost effort in the Menu part of the Chosen treatment, but not in the Assigned treatment.[5] In fact, since selection effects were controlled for by the treatment-differencing, any difference in the treatment-differences can be

[3]Alternatively, we could have used a within-subject design where each subject is exposed to both treatment variations. With such a design we would compare a subject's effort on a project when he chooses it from a menu, versus when the same project is assigned to him from the same menu. However, a design where the subject would encounter the same menu twice – once being asked to choose a project from the menu, and then being assigned a project from the same menu – would both be too artificial and too transparent; the subject might be alerted to the purpose of the experiment, and the sense of being manipulated could influence his behavior. He might wonder, for example, if the assignment of a project from a particular menu would in fact be dependent on the choice he previously made from the same menu.

[4]See Chetty, Looney and Kroft (2009) or Hennig-Schmidt, Sadrieh and Rockenbach (2010) for similar designs in field and lab settings.

[5]A pure motivation effect would also not impact the benchmark effort levels in the Isolation part.

attributed to the pure motivation effect.[6]

If a subject chooses a risky project, and the project fails, he may regret not having chosen the safe project. To avoid this negative feeling, he is motivated to work extra hard on the risky project. Therefore, regret theory provides a possible explanation for the pure motivation effect.[7] An interesting implication follows immediately: the pure motivation effect will be increasing in the value of the (foregone) safe option. In our experiment, the safe project yields $\$s$ for sure. The risky project yields \$0 in case of failure and $\$V$ in case of success, where $V > s$. The bigger is s, the more consequential is the choice of the risky project; the bigger is the regret if it fails, and the more effort will be provided to prevent a failure.[8] The pure motivation effect is also predicted to be stronger when the risky project is a highly risky "\$-bet" with a small probability of success and a large V (as opposed to a low risk/low return "p-bet"). These predictions agree with the intuition that there is more "real" autonomy when the choice menu is diverse and the choice is experienced as consequential and meaningful.

Consistent with these predictions, we found a significant pure motivation effect only when the choice is between a very risky (but potentially very profitable) project and a high-value safe option. In such cases, the pure motivation effect is both substantial and highly statistically significant; the

[6]A caveat is that the treatment variation in the Menu part of the experiment is assumed to have no asymmetric behavioral spillover (order) effects on choices in the Isolation part. In the experimental design we address this by inserting an unrelated survey between the two choice-relevant parts of the experiment. We also test for this assumption in the data and do not find any discrepancy.

[7]Early experimental support for regret theory was found in lottery choice data (e.g., Loomes *et al.*, 1991; Zeelenberg *et al.*, 1996; Zeelenberg, 1999). For a critique, see Starmer and Sugden (1993) or Humphrey (1995). More recently Bleichrodt et al. (2010), Camille et al. (2006) and Frydman and Camerer (2016) have offered neuroimaging evidence. For economic applications, see, for example, Filiz-Ozbay and Ozbay (2007), Engelbrecht-Wiggans and Katok (2008) or Shefrin and Statman (1984).

[8]All our menus have one risky and one safe project. We could have obtained more data on effort choices by only including risky projects, but then it would be harder to obtain unambiguous predictions from the regret hypothesis, potentially undermining the internal validity objective of this experiment.

strongest pure motivation effect we record (when project choice is most consequential) amounts to about 31% of the chosen effort. Overall, the effort on the risky project depends positively on the *forgone* return, s, and this pattern is stronger in menus where the risky project is highly risky.

2 Related Literature

A growing experimental literature documents the relationship between (self-)selection into a role, task or institution, and subsequent performance. For example, Sutter, Haigner and Kocher (2010) found that contributions to a public good were higher when the group unanimously voted for a reward or a punishment institution than when the same institution was imposed on them exogenously. Similarly, in a real-effort experiment, Mellizo, Carpenter and Matthews (2014) found that workers who voted on their compensation scheme provided higher effort than when the scheme was assigned to them. Herbst, Konrad and Morath (2012) found that in a group contest setting, alliances that were created endogenously by the individual members outperformed those that were put together externally. In contrast, Cooper and Sutter (2015) found that the increased internal conflict between team members eliminates any potential benefit from endogenous team formation. None of these studies attempted to identify a "pure" psychological effect of autonomy on performance, as opposed to a selection effect (e.g., the compensation scheme selected by the workers in a team may be correlated with their individual characteristics).

In Dal Bo, Foster and Putterman (2010), groups voted on whether to endogenously modify the (off diagonal) payoffs of a prisoner's dilemma and turn it into a stag hunt game. Their study controlled for selection by introducing a probability that the computer would override the group's decision and instead exogenously assign a game to the group. Among subjects who voted to modify the payoffs, a significantly larger proportion (82%) chose the cooperative strategy ("stag") in the endogenously modified game than in the exogenously modified game (58%). However, this difference cannot be assigned to a "pure" effect of autonomous choice in our sense, because these games involved strategic considerations. Voting behavior may signal information about preferences, beliefs or intentions. Such signaling motives may influence the play of the game differently depending on whether the game was selected by the majority vote or assigned randomly. In contrast, our experiment isolates a pure motivation effect in a simple individual choice problem, with no strategizing or signaling.

We already mentioned the independent work by Babcock, Bedard, Charness, Hartman and Royer (2015). They paid students to study at the university library. The subjects received \$2 per visit and a bonus of \$25 if their attendance reached or exceeded a target of 4 visits. In one of their treatments, each subject acted individually, i.e., they did not affect each others' earnings; in another treatment, each subject decided whether to act individually (the "individual option") or to condition his bonus payment on the target being reached jointly by himself and another subject (the "team option"). Although this design would potentially introduce both selection and strategic effects, these effects were minimized because the individual option was dominant (in terms of monetary payoffs) and was in fact chosen by almost all (97%) subjects. The finding was that subjects studied more when they chose to act individually (on average 2.33 library visits per subject) than when they did not have a choice (1.95 visits).

Their surprising finding is a pure motivation effect in our sense. We provide a somewhat different perspective of the pure motivation effect and a more nuanced analysis. We find that for autonomy to have a significant effect, the autonomous choice must be consequential and meaningful to the subject (as opposed to simply rejecting an obviously inferior option). Babcock, Bedard, Charness, Hartman and Royer (2015) used a single choice menu where one of the two alternatives (the team option) was obviously inferior in a monetary sense.[9] Making choices more meaningful in their set-up (i.e.,

[9]In the Concluding Discussion, we will try to interpret their finding in the light of

making the team a viable option) would likely introduce selection effects. In contrast, we implement a simple individual choice problem and vary the choice menu to get comparative statics results. This allows us to test implications of the regret hypothesis, such as how the pure motivation effect depends on the value of the forgone option.

Fehr, Herz and Wilkening (2013) and Bartling, Fehr and Herz (2014) found that decision rights have positive intrinsic value in principal-agent settings. The former argues that the principal may like to keep the decision rights in part to avoid the regret from a negative outcome in case the delegated decision fails. Neri and Rommeswinkel (2016) attribute the value of the decision rights primarily to the individual aversion to being interfered with by others. In contrast to these studies, our experiment focuses on the pure motivation effect of autonomous choice. There is no strategic interaction and selection effects are controlled for. Decision rights are exogenously determined: our subjects never choose whether to keep or give away decision rights. In the Chosen treatment the subject has decision-rights and *must* choose a project – a choice he may later regret. In the Assigned treatment the subject has no decision-rights and therefore *cannot* regret the project-choice.

The rest of the paper is organized as follows. In Sections 3 and 4 we present our experimental design, derive some behavioral predictions and outline our empirical strategy and procedures. Section 5 presents the main results and some auxiliary observations. Section 6 concludes.

3 Experimental Design and Predictions

3.1 Projects, Menus and Treatments

There are two kinds of projects, risky and safe. They generate income, referred to as "prizes." A safe project generates a prize s with certainty. A risky project can either succeed or fail; there is a prize V if it succeeds, and

regret theory.

no prize if it fails. By providing costly effort, the subject, in the role of a worker, determines the probability that the risky project succeeds. Effort is denoted e and must belong to the set $\{1, 2, 3, ..., 15\}$. The worker's cost function is linear in effort, $c \times e$.

The choice of effort is explained to the subject in an intuitive way. The subject is told that he can increase the probability of success by trying the risky project (running a lottery) multiple times (at least once, and at most 15 times), at a flat fee c per try. His effort e is simply the chosen number of tries. Each trial succeeds with probability p; the risky project succeeds if *at least one* of the e tries succeeds. Thus, a risky project can be characterized by a pair (p, V), where p is the probability of success *if the project is tried only once*, and V is the prize in case of success. More tries will increase the probability of success, at a decreasing rate. Importantly, the subject chooses e before any tries are resolved.[10] With probability $(1-p)^e$, all e tries fail and so the whole project fails. The risky project thus succeeds with probability $1 - (1-p)^e$. The expected monetary payoff is therefore

$$(1 - (1-p)^e)\, V - ce. \tag{1}$$

This presentation was designed to be easy for the subjects to grasp. From personal experience, they should be familiar with the idea that more tries will increase the probability of success. For example, the more job applications a student sends out, the greater will be the chances of landing a job.

Our Chosen treatment resembles decentralized decision making: the worker chooses a project from a menu. Our Assigned treatment instead resembles centralized decision making: the project is (randomly) assigned to the worker from the same menu. The pure motivation effect says that (controlling for project attributes) the worker is motivated to work harder in the former (decentralized) case. In Section 3.2 we show that aversion to anticipated regret

[10] The key point is that the subject chooses e before finding out if any try has succeeded. Thus, if the subject chooses five tries he must pay the cost of effort $5 \times c$ even if, say, the second try succeeds. This corresponds to sending out five job applications before knowing which, if any, will be successful.

implies the existence of a pure motivation effect. In Section 3.3 we outline our strategy of measuring the pure motivation effect while controlling for the selection effect (which is due to different matching of projects and workers).

3.2 A Simple Model of Regret Aversion

In the standard model of regret aversion (Loomes and Sugden, 1982) the agent has only two options. In each trial of our Chosen treatment, the subject has 16 options: either choose the safe project, or choose the risky project together with an effort level in the set $\{1, 2, 3, ..., 15\}$. It is not clear how to extend standard regret theory to this setting. To model regret in the simplest possible way, we make the following assumption: if the agent chooses the risky project and the risky project fails, then he experiences a feeling of regret which is proportional to the foregone sure payoff s. That is, he regrets not having chosen the safe project.[11] This regret occurs with probability $(1-p)^e$. Assuming risk-neutrality,[12] the decision maker's expected payoff from the risky project is

$$U(e; p, V, s) = (1 - (1-p)^e)V - ce - r(1-p)^e s \tag{2}$$

where $r \geq 0$ is a regret parameter. Setting $r = 0$ yields equation (1), which corresponds to the no-regret case. The partial derivative of (2) with respect

[11] Other plausible assumptions generate qualitatively similar predictions about behavior. For example, the subject might regret the effort he put into a failed project, thus feeling regret proportional to $s+ce$. It is easy to verify that the key theoretical result, Proposition 1, holds also in this case. Relatedly, "winner's regret" might be experienced when a risky project succeeds, as it might have succeeded with less effort. But winner's regret would (presumably) not depend on whether the risky project was chosen or assigned, so it does not bear on the issue of a pure motivation effect. In any case, there is no evidence that subjects anticipate winner's regret (see Filiz-Ozbay and Ozbay, 2007). Finally, a subject who succeeds after having chosen the risky project may "rejoice" that he made the right decision (Loomes and Sugden, 1982). Rejoicing would tend to strengthen the pure motivation effect, assuming it is felt more strongly when the risky project is chosen by the subject than when it is assigned. We present the simplest formulation for convenience.

[12] Regardless of risk preferences, standard expected utility theory predicts that there is no pure motivation effect. Since risk preferences are not critical to our results, we present the risk neutral case for simplicity.

to e is

$$U_e = -c - (V + rs)(1-p)^e \ln(1-p). \tag{3}$$

If we neglect integer constraints and consider an interior solution, the optimal effort level is given by $U_e = 0$. The cross-partial derivatives are

$$U_{eV} = -(1-p)^e \ln(1-p) > 0$$

and

$$U_{es} = -r(1-p)^e \ln(1-p) > 0.$$

These inequalities imply that U has strictly increasing differences in both (e, V) and (e, s). Monotone comparative statics (Milgrom and Shannon, 1994, Edlin and Shannon, 1998) implies that the optimal effort level is strictly increasing in both the prize V of the risky project and in the payoff s he could have obtained by choosing the safe project.[13] Intuitively, the more profitable is the forgone safe option, the more the subject will regret failing in the risky project (if chosen), and therefore the more effort will be expended. This is the key prediction, so we state it as a proposition:

> **Proposition 1** *In the Chosen treatment, $de/ds > 0$. That is, effort on the risky project will depend positively on the forgone income from the safe project.*

Proposition 1 has two important corollaries. The first is the existence of a pure motivation effect. To see this, suppose the subject is assigned the risky project in the Assigned treatment. If it fails, he cannot regret forgoing the safe payoff s, since this was not his choice. From the point of view of regret theory, failing in the Assigned treatment is like failing in a hypothetical (and trivial) Chosen treatment where the forgone option pays $s = 0$. But since

[13] It can be checked that we obtain exactly the same cross-partial derivatives if regret is proportional to $s + ce$, so the key theoretical prediction would still go through. However, the cross-partial U_{ep} cannot be signed without making assumptions on the remaining parameters. Thus, the effect of a change in p on effort is ambiguous.

in our Chosen treatments, the safe option always has a payoff $s > 0$, and $de/ds > 0$ by the proposition, regret theory predicts a strictly higher effort on the risky project in the Chosen treatment than in the Assigned treatment. The second corollary is that the pure motivation effect is increasing in s.

More generally, it seems intuitively plausible that the strength of the pure motivation effect should depend on how "meaningful" is the autonomous choice. In the Chosen treatment, the subject can choose a safe return of s or a risky project (p, V). If p is just slightly smaller than 1 and V is just slightly bigger than s, then the risky project is almost equivalent to the safe project: the diversity of the menu is very low. If the menu is made more diverse by increasing V and reducing p, then autonomy becomes more meaningful and we expect a larger pure motivation effect.

To be more precise, the implicit function theorem and equation (3) imply

$$\frac{de}{ds} = \frac{-U_{es}}{U_{ee}} = \frac{-r}{cr + (V + rs)\ln(1-p)}. \tag{4}$$

Proposition 1 implies that (4) is strictly positive. (Note that $U_{es} > 0$, and $U_{ee} < 0$ by the necessary second-order condition.) Proposition 2 will show that the expression in (4) increases if V is increased and p is reduced in such a way that pV does not become larger. Since effort becomes more sensitive to the safe payoff, the pure motivation effect increases (cf. the first corollary to Proposition 1). Thus, Proposition 2 implies a bigger pure motivation effect when the risky project is a "\$-bet" with a small p and a large V, rather than a low risk/low return "p-bet", at least as long as pV is larger in the latter case. This formalizes the intuition that autonomy will have a more powerful effect on effort when the choice menu becomes more diverse.

Proposition 2 *In the Chosen treatment, de/ds becomes larger if V is increased and p is reduced in such a way that pV does not increase.*

To prove Proposition 2, note that such a change in (p, V) can be accomplished in two steps. First, V is increased and p is reduced in such a way that

pV is held constant, $pV \equiv k$ (a mean-preserving spread). Second, p further reduced, holding V constant. We show that (4) increases in each step. For the first step, substituting $p = k/V$ into (4) we get

$$\frac{de}{ds} = \frac{-r}{cr + (V + rs)\ln\left(1 - \frac{k}{V}\right)} \tag{5}$$

which is increasing in V, because

$$\frac{\partial}{\partial V}\left((V + rs)\ln\left(1 - \frac{k}{V}\right)\right) = \ln(1-p) + \frac{p}{1-p}\left(1 + \frac{rs}{V}\right) > \frac{p}{1-p}\frac{rs}{V} > 0.$$

Here we used the fact that $\ln(1-p) > -p/(1-p)$. For the second step, simply note that (4) is decreasing in p. This completes the proof.

3.3 Controlling for the Selection Effect

To control for a possible selection effect in our between-subjects design, we use a difference-in-differences (DID) approach. Our experiment has two parts: a Menu part and an Isolation part (in that order), separated by a survey. In the Menu part, the subject sees a menu consisting of a safe and a risky project. In the *Chosen treatment*, the subject is asked to make a choice from the menu. If the subject chooses the risky project, then he goes on to select the number of tries, e. If he chooses the safe project, then he has no further decision to make: the safe project automatically uses a single try and succeeds. In the *Assigned treatment*, the subject is also shown the menu, but then one of the two projects is randomly assigned to him by the computer. If the assigned project was the risky one, he goes on to select the number of tries, e; if the project was safe, it automatically succeeds on a single try.

The Isolation part follows the Menu part and is identical between the two treatments. In this part, the same risky project from the Menu part is presented to the subject again, but this time, in isolation. The subjects only sees this one project and is asked to choose an effort level e. This establishes a benchmark effort which will depend on the subject's characteristics, such as risk-related preferences and beliefs, as well as properties of the project.[14]

In summary, our data consists of effort choices from subjects who have chosen the risky project in the Menu part of the Chosen treatment, and effort choices from subjects who were randomly assigned the risky project in the Menu part of the Assigned treatment. We also have a benchmark effort level for each subject and each risky project from the Isolation part.[15] For each subject and each risky project (either chosen or assigned, depending on treatment), we difference the two effort levels obtained in the two parts of the experiment, average the differences for each treatment, and obtain our estimate of the pure effect as the difference of the two averages between the treatments.

This identification strategy depends on what is known in the DID literature as the common trend assumption. In our case, it requires that the treatment variation in the Menu part did not affect behavior in the Isolation part. In order to reduce the risk of this happening, we inserted a personality survey between the two choice-relevant parts of the experiment. The survey took approximately 15 minutes to complete. The survey questions were short, easy to answer, and were unrelated to the objective of the experiment. Furthermore, after the data were collected, we tested for the validity of this assumption. If there were no spillovers between the two parts of the experiment, then there should be no significant differences in average benchmark effort levels between the two treatments. Giving a preview of the results, indeed we find no differences.

[14] In real-world scenarios, regardless of whether project choice is centralized or decentralized, there would always be a set of feasible projects to choose from (as in the Menu part). A situation where only one project is feasible (the Isolation part) may induce a different psychological state; it is a benchmark, but not a model of centralized decision-making. This is why we cannot estimate the pure motivation effect by simply comparing effort levels in the two parts of the Chosen treatment.

[15] There is no selection into projects in the Isolation part; it gives us benchmark effort levels for all subjects and all risky projects, regardless of whether they chose or were assigned the risky project in the Menu part.

3.4 Design of the Menus

One of our main objectives is to examine if the pure effect is stronger in menus that are, in some sense, more meaningful. Regret theory suggests a specific definition of "more meaningful": the higher the scope for regret, the stronger the predicted pure motivation effect. To test this prediction we need a variation in the types of choice menus.

Both the Menu and the Isolation parts involved a sequence of twelve rounds. In the Chosen treatment, each subject made twelve project decisions (choice of risky versus safe project), and for each chosen risky project he specified an effort. In the Assigned treatment, each subject chose an effort for each risky project that was assigned to him. In the Isolation part, each subject made twelve effort choices for twelve different risky projects.

Each project was presented to the subject as a box containing 20 balls. For a risky project, some of the balls would be red (representing success), others would be blue. The subject chose the number of tries, i.e., the number of random draws (with replacement) from the box. To simplify the presentation, from now on we will write a risky project in the form $r = (\rho, V)$, where $\rho = 20 \times p$ is the number of red (success) balls in the box. Thus, for example, the project $(5, 100)$ corresponds to a box with 5 red balls, i.e., the project succeeds with probability $p = 5/20 = 0.25$ on a single trial, and it pays $V = 100$ in case of success.

The Menu part of the experiment included four risky projects: $(7, 90)$, $(5, 100)$, $(3, 130)$ and $(2, 160)$. The probability and prize parameters were chosen to make the projects comparable in terms of expected payoff, keeping the salience of incentives similar across projects and menus. Note that, comparing any two risky projects, the project with a higher V and lower p has a lower value of pV, so that Proposition 2 applies. Using the terminology of Grether and Plott (1979), it is helpful to refer to $(7, 90)$ and $(5, 100)$ as p-bets and to $(3, 130)$ and $(2, 160)$ as \$-bets. The p-bets had higher success probabilities (more red balls) but lower prizes. But even the p-bets would be likely to fail with too few draws. The optimal number of tries for a standard CRRA utility function with a 0.8 risk aversion parameter would be somewhere between 5 and 10 for all four projects.

Each of the four risky projects in the Menu part was paired with one of three different safe projects that increased in value (in terms of s) as shown in the top panel of table (1). Specifically, for each of the four risky projects, the highest value of s gave about the same expected utility for the safe and risky projects (i.e., rounded to the nearest nice integer) when evaluated using CRRA with the risk aversion parameter $k = 0.3$, which is about the midpoint of Holt and Laury's (2000) "slightly risk averse" category (see table (3) in Holt and Laury, 2000).[16] Similarly, the middle value of s was set to equalize the expected utilities for $k = 0.8$ (about the midpoint of the "very risk averse" category) and the lowest value equalized the expected utilities for $k = 2$ (well inside the "stay in bed" category).

Across our menus, our simple theory of regret aversion has the following implications: for each triple of menus with the same risky project, the pure effect should be increasing with the value of the safe alternative s; secondly, the pure effect should be stronger for menus including \$-bet risky projects than p-bet risky projects.

The four risky projects that were used in the Menu part were also included in the Isolation part. To make the two parts consistent in terms of the number of experimental tasks, we included eight additional projects that differed in values of the parameters p and V, i.e., as shown in the bottom panel of table (1). These projects incidentally give us exogenous variation on parameters p and V that can be used to check whether subjects properly responded to

[16]For the risky project the EU was

$$(1-(1-p)^{e^*})\frac{(75+V-5e^*)^{1-k}}{1-k}+(1-p)^{e^*}\frac{(75-5e^*)^{1-k}}{1-k},$$

where e^* is the optimal effort choice; for the safe project the EU was

$$\frac{(75+s-5)^{1-k}}{1-k}.$$

Table 1: Projects and menus

Menu part: $(20 \times p, V; s)$

Mn.	1	2	3	4	5	6
	(2,160; 50)	(2,160; 30)	(2,160; 0)	(3,130; 55)	(3,130; 45)	(3,130; 15)
Mn.	7	8	9	10	11	12
	(5,100; 55)	(5,100; 50)	(5,100; 40)	(7,90; 60)	(7,90; 55)	(7,90; 45)

Isolation part: $(20 \times p, V)$

Prj.	1	2	3	4	5	6
	(2,160)	(3,130)	(4,120)	(4,110)	(5,100)	(6,100)
Prj.	7	8	9	10	11	12
	(6,95)	(7,90)	(8,90)	(9,85)	(9,80)	(10,80)

incentives. The ordering of menus (in the Menu part) and projects (in the Isolation part) was randomized.

3.5 Some Procedural Details

As mentioned, we presented each project to the subjects using a box containing 20 balls, some blue and the others red. Red balls represented success.[17] The subject chose the number of costly random draws to be made (with replacement) from the box. He immediately incurred the cost of effort. On each try, one ball from the box was randomly highlighted.[18] If the ball was red then the project was deemed successful and the subject received the prize written on the face of the ball. If it was blue, then if the subject had more

[17]For a picture of the interface, please see the instructions in the Appendix.

[18]The random draw was simulated with a simple computer animation in which the balls in the box started flashing one by one in sequence form the top to the bottom in 1/3 of a second intervals. At some randomly determined termination time the flashing stopped and the last highlighted ball became the outcome of the draw.

tries available another ball was drawn; if all tries had been exhausted the project was deemed a failure which paid zero. For a safe project, all 20 balls would be be identical and yield the prize s; one ball would be automatically chosen on behalf of the subject. In each round the subject started with an endowment of $75 = 5 \times 15$ which eliminated the need for a bankruptcy rule.

Whenever a project (or a menu of projects) was displayed on the computer screen, to enhance the subject's attention and awareness he was asked to enter the following information (for each project): the number of red balls in the box, the prize if the project succeeded (which was written on each red ball), and the cost of a single try (which was always 5). If this was entered correctly, the information was displayed below the project for quick reference. Only after that was the subject prompted to choose effort. To reduce a possible outcome dependence between individual rounds, the evaluation of projects was postponed until the very end.

Before the experiment started, we made sure the subjects fully understood the task. They were first given a few minutes to study the instructions on their own. Then, the experimenter read the instructions aloud for everyone to hear. This was followed by a review, organized as a series of questions regarding the choices, incentives, and the structure of the experiment. Each question was first read aloud, and after a short pause (a few seconds) the correct answer was provided. The subjects then turned to their computer terminals and answered four comprehension questions on the computer screen. Everyone was required to answer the questions correctly before moving on.

Subsequently, subjects entered a practice stage in which they became familiar with the computer interface and experienced the whole process of effort choice and project evaluation in six practice rounds with two different projects. The project was $(10, 50)$ in the first three practice rounds and $(1, 200)$ in the next three rounds; in effect, these were boundary cases for all the projects in the actual experiment. In each practice round, the subject chose the number of tries and then watched the project being evaluated in real time. Practice rounds were not paid.

After all parts of the experiment were completed, subjects filled out a short questionnaire. Then, two rounds were randomly selected and evaluated for payment. All subjects saw their projects evaluated on their computer screens. Experiments were run at Rutgers University. The software was programmed in Visual Basic. 134 subjects participated: 65 in the Assigned treatment and 69 in the Chosen treatment. The experiment lasted about 75 minutes. The average earning was 21 dollars and 20 cents.

4 Results

We will first examine our assumption that the treatment variation in the Menu part did not contaminate the behavior in the Isolation part. Then we present our main results regarding the pure autonomy effect.

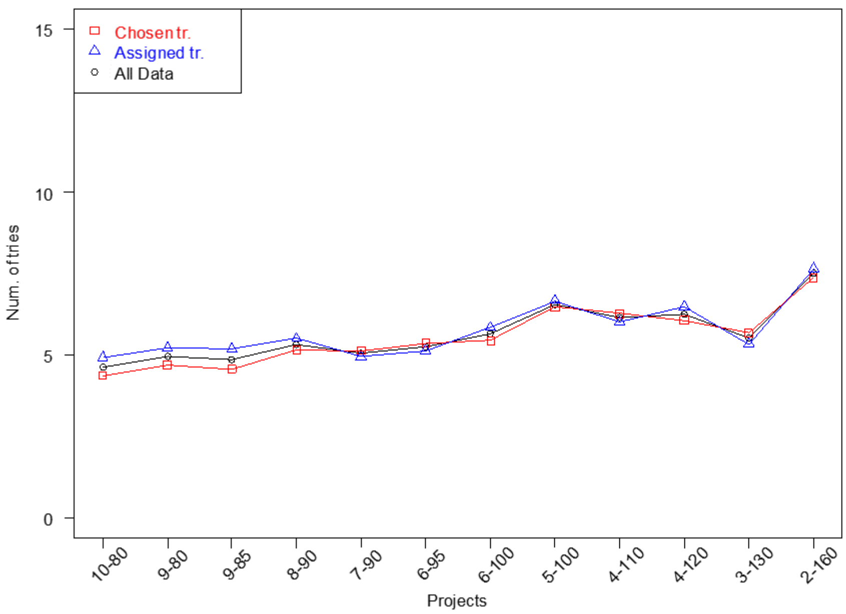
4.1 Isolation Part

Figure 1 shows the mean effort choices for the twelve risky projects in the Isolation part. The projects are in the order of increasing prize (and decreasing chance of success on a single try). Tables 6 and 7 in the Appendix provide descriptive statistics regarding average effort levels.

Since the Isolation part did not involve any treatment variation we do not expect significant differences in effort choices. Figure 1 shows the averages broken down by treatments (i.e., the line segments connected by squares and triangles). Visually, there seems to be no difference. Running a Wilcoxon signed-ranks test on the matched samples of means from the Chosen and the Assigned treatment yields a p-value $= 0.052$. Although, strictly speaking, the test rejects the equality of samples on the 5% level, this is clearly a borderline case. In 8 out of 12 cases the mean effort in the Chosen treatment is slightly lower than the mean effort in the Assigned treatment.

Is this something to be concerned about? We think not. The differences between the pairs of means are quite small, i.e., the maximum difference is

Figure 1: Isolation part: average effort



0.63 and the average difference is 0.36 of a single try.[19]

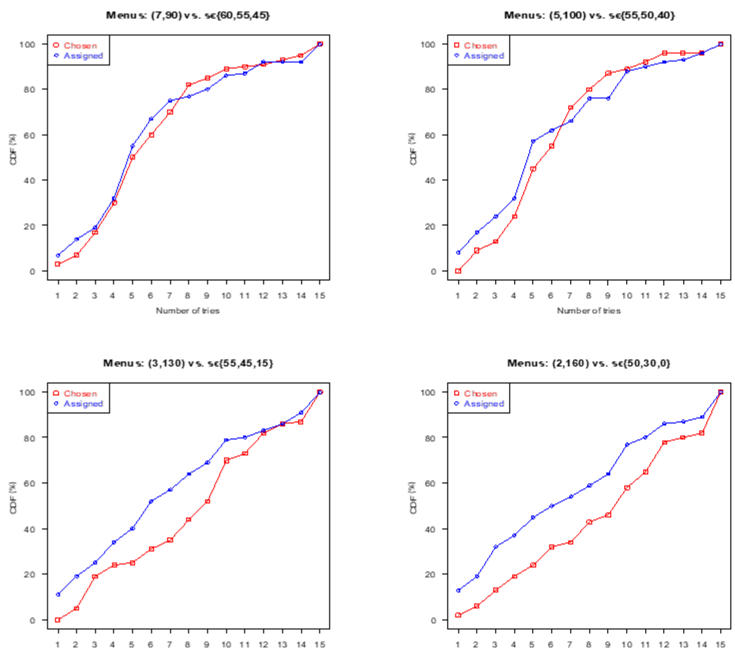
4.2 The Total Autonomy Effect

The Menu part was subject to the treatment variation (Chosen vs. Assigned). We focus in this subsection on the total effect of autonomy on effort. In the next subsection, we identify the components of the total effect: the pure motivation effect and the selection effect.

[19]If we restrict attention only to the four risky projects that were used in the Menu part of the experiment, the four differences in means between the Chosen and the Assigned treatment are 0.18, -0.18, 0.34, -0.27. The Wilcoxon test cannot reject the hypothesis that the two samples come from the same population (p-value $= 0.999$).

Our theoretical model predicts a smaller autonomy effect for less diverse menus. The diversity of our menus varies along two dimensions. First, a menu consisting of a safe project and a p-bet is less diverse than a menu consisting of a safe project and a \$-bet. Second, for any two menus including the same risky project, the menu with the lower-valued safe project is less diverse than the menu with the higher-valued safe project. We will take a look at both of these dimensions.

Figure 2: CDFs of effort in the Menu part



Note: In each panel the data is pooled across three menus with the same risky project and three different safe alternatives s.

Figure 2 plots the distribution of effort for the four risky projects. In each panel the data is pooled across the three menus pairing the same risky project with different safe projects. In the top two panels, corresponding to the p-bets, the distributions are very close together and crossing. An Epps-Singleton (ES) test indicates[20] no difference between the distributions (the p-values are 0.7 and 0.13 respectively). In contrast, the lower two panels, corresponding to the \$-bets, suggest first order stochastic dominance (FSD): the distributions for the Assigned treatment (circles) lie above those for the Chosen treatment (squares). The ES test is significant here: p-value $= 0.015$ for menus involving the project $(3, 130)$ and p-value $= 0.006$ for menus involving $(2, 160)$.

Table 2 shows differences in average efforts for all twelve menus. The first two rows confirm that there are no qualitatively significant autonomy effects for the p-bets. The bottom two rows suggest that, as predicted by our model of regret aversion, the driving force behind the autonomy effect on \$-bets is the (counter-factual) safe return s. As s grows, the autonomy effect becomes more pronounced.[21] To test whether the treatment differences are significantly different form zero, for each choice menu $((p, V), s)$ we run an OLS regression of effort (in the Menu part), e, on a treatment dummy Tr ($= 1$ if the treatment is Chosen), i.e. we estimate[22]

$$e = \alpha + \beta Tr + \varepsilon.$$

In summary, there are no significant autonomy effects for the p-bet projects

[20]The ES test is more powerful than other nonprametric alternatives (e.g., Kolmogorov-Smirnov test). More importantly, unlike other tests, it is designed to handle discrete data of the kind we have here.

[21]A Wilcoxon pairwise test applied to average efforts across menus rejects the equality of medians between the two treatments (p-value $= 0.004$).

[22]Our data is subject to some censoring: the effort (as measured by the number of tries) could not drop below 1 and could not exceed 15. Overall, 12.6% of all effort choices lie on one of the boundaries (7.3% at 1 and 5.3% at 15). Censored regression model, such as tobit, would be the appropriate estimation method. However, there are difficulties with identification as well as inference when tobit is used in difference-in-differences framework (see, e.g., Puhani, 2012). To avoid these complications we will stick with the ordinary least squares. However, in order to examine the impact of censoring on our OLS coefficients (reported in Table 2), we re-estimate the total autonomy effects using tobit. This is appropriate as the total effect can be obtained directly. Please see Table 8 in the Appendix. It can be seen that the results are very similar to the OLS estimates.

$(7, 90)$ and $(5, 100)$. For the \$-bet projects $(3, 130)$ and $(2, 160)$, the differences are distinctly larger and in a few cases also statistically significant. The strongest effects we obtain for menus including the riskiest \$-bet $(2, 160)$, and the highest-valued safe alternatives $s = 30$ or $s = 50$.

Table 2: Total autonomy effect

$p, V \setminus s$	0	15	30	40	45	50	55	60
7, 90					-0.06 (.87) [76]		0.11 (.91) [75]	0.08 (.65) [59]
5, 100				0.51 (.89) [60]		-0.36 (1.04) [46]	0.5 (.89) [61]	
3, 130		1.38 (1.06) [83]			1.7 (1.05) [64]		1.6 (1.08) [50]	
2, 160	1.13 (.97) [85]		2.77^{**} (1.1) [61]			3.0^{***} (1.09) [65]		

Note: Robust standard errors are included in the parentheses. For each case, the number of observations is displayed in brackets. For the break down of observations by treatment, pleasee see Table 7 included in the Appendix.

Result 1: *Total autonomy effect.*

(i) Overall, autonomy of choice generates a positive effect on effort.

(ii) The autonomy effect is qualitatively and statistically significant only when the menu pairs a high-risk high-return project (a \$-bet) with a very profitable (high s) safe project.

4.3 Identifying the Pure Motivation Effect

Result 1 suggests that choosing a project can have a significant impact on the subsequent performance, at least when the choice is highly conseqeuential (\$-bet vs. high-s safe option). However, is effort higher in the Chosen treatment because subjects self-select into tasks that they prefer (a selection effect),[23] or is it a purely psychological phenomenon (a pure motivation effect)?

[23]In theory, the sign of the selection effect is ambiguous. Highly risk averse subjects are less likely to choose the risky project (e.g., Holt and Laury, 2002), but if assigned a risky project they may choose a high effort level to insure against failure. Self-selection

We isolate the pure motivation effect by first differencing efforts between the two parts of the experiment, and then differencing between the treatments. Let $P \in \{M, I\}$ denote the part, where M is the Menu part and I the Isolation part. Let $T \in \{Ch, A\}$ denote the treatment, where Ch is the Chosen treatment and A the Assigned treatment. A menu consisting of a risky project r and a safe option with return s will be written (r, s).

Let $R((r, s), M, T)$ be the set of subjects such that, when facing menu (r, s) in the Menu part of treatment T, they either chose (for $T = Ch$) or were assigned (for $T = A$) project r. For $i \in R((r, s), M, T)$, let $e(i, (r, s), M, T)$ be subject i's effort in project r when r was chosen or assigned from menu (r, s) in the Menu part of treatment T. Let $\bar{e}((r, s), M, T)$ denote the average effort among those subjects who faced this particular risky project in the Menu part of treatment T. That is,

$$\bar{e}((r, s), M, T) = \frac{1}{\#R((r, s), M, T)} \sum_{i \in R((r,s),M,T)} e(i, (r, s), M, T).$$

Let $e(i, r, I, T)$ be subject i's choice of effort in project r in the Isolation part of treatment $T \in \{A, Ch\}$. Let $\bar{e}(r, I, T)$ denote the average of $e(i, r, I, T)$, taken over $R((r, s), M, T)$. That is,

$$\bar{e}(r, I, T) = \frac{1}{\#R((r, s), M, T)} \sum_{i \in R((r,s),M,T)} e(i, (r, s), M, T).$$

Note that the average effort in project r in the Isolation part of each treatment is being calculated for only those subjects who chose (or were assigned) project r over s in the respective treatment.

First-differencing between the two parts M and I within treatment T yields $\Delta((r, s), T) = \bar{e}((r, s), M, T) - \bar{e}(r, I, T)$. Thus, $\Delta((r, s), A)$ estimates

into the risky project may be also influenced by heterogeneous beliefs, or by psychological biases such as overconfidence (e.g., Benoît and Dubra, 2011; Burks *et al.*, 2013; Ben David *et al.* 2007), concern for personal image (Bénabou and Tirole, 2002, 2006), updating of (optimistic) subjective priors (van den Steen, 2004), anticipatory utility from positive expectations (Brunnermeier and Parker, 2005), or the illusion of control (Langer, 1975).

the increase in effort when a project is *assigned* from a menu, compared to the benchmark with no menu. Similarly, $\Delta((r,s),Ch)$ estimates the increase in effort when a project is *chosen* from a menu, compared to the benchmark.

Our estimate of the pure motivation effect for menu (r,s) is $\Delta(r,s) = \Delta((r,s),Ch) - \Delta((r,s),A)$. Note that this can be written as

$$\Delta(r,s) = \bar{e}((r,s),M,Ch) - \bar{e}((r,s),M,A) - (\bar{e}(r,I,Ch) - \bar{e}(r,I,A))$$

where the first term on the right-hand side is the total autonomy effect and the second term the selection effect. By differencing in this way, the selection effects are filtered out.

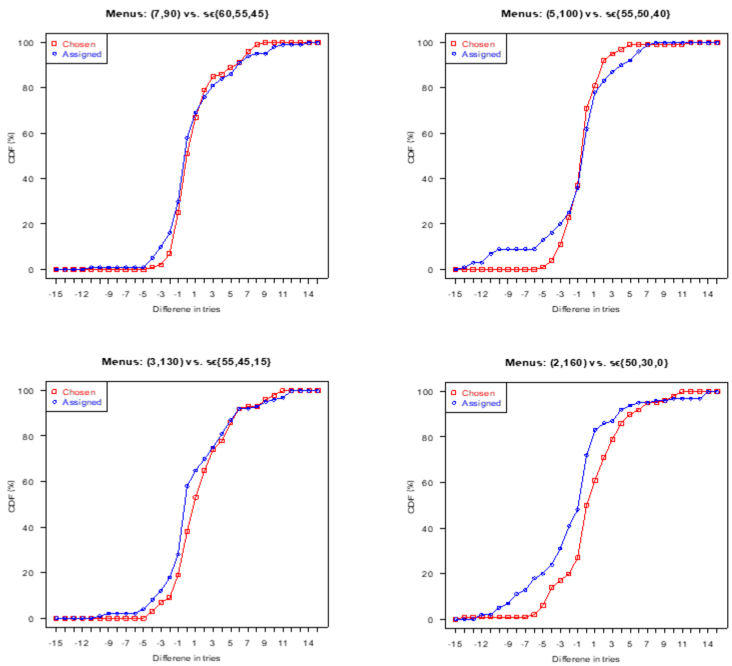
In Figure 3 we plot distributions of $\Delta((r,s),T)$ for each of the four types of menus corresponding to one of the risky projects. The pure motivation effect should appear in the data as the first order stochastic dominance relationship between the two distributions. There is a clear FSD relationship in the bottom-right panel corresponding to the project $(2,160)$. The ES test convincingly rejects the equality of distributions on 1% level (p-value = 0.000). The bottom-left panel (corresponding to the project $(3,130)$) also shows some hint of FSD relationship, but there the ES test does not come out significant (p-value = 0.14). The two menus that involve p-bets (in the top two panels) show no sign of FSD relationship.[24] This is consistent with the findings of the previous section regarding the total autonomy effect.

Table 3 gives a more detailed look at the pure effect by listing the average values of the Δ's for each of the twelve menus. As in the previous section, the values are increasing as we move from the less diverse to the more diverse menus, i.e., in the down-right direction.[25] To test if the Δ's are different from zero, we run OLS regressions analogous to those reported in Table 2, except

[24]For the top-left panel the ES p-value = 0.13 and for the top-right panel it is 0.002. In the latter case the significant p-value reflects differences in higher moments, such as, dispersion. The ES test is sensitive to any variation in the shapes of the distributions. The alternative hypothesis is that one distribution stochastically dominates the other. We do not have any ex-ante predictions for higher moments.

[25]Wilcoxon pairwise test applied to average deltas across menus rejects the equality of medians between the two treatments (p-value = 0.004).

Figure 3: CDFs of differences in effort ($\Delta(i,(r,s),T)$)



Note: In each panel the data is pooled across three menus with the same risky project and three different safe alternatives s.

with Δ as the dependent variable. The only significant treatment effect is on the menus involving the riskiest \$-bet $(2, 160)$ and the two most profitable safe projects with $s = 30$ and $s = 50$. Comparing the values in these two cells of Tables 2 and 3, we note that in both cases the pure motivation effect accounts for a large portion of the total autonomy effect (85% and 84%). The remaining portion is attributed to the selection effect.

The selection effect for each menu is summarized in Table 4. When the riskiest \$-bet is paired with the two most profitable safe projects (the two rightmost entries in the bottom row), there is about 15-16% of extra effort in the Chosen treatment that can be attributed to subject selection based

Table 3: Pure effect ($\Delta(r, s)$)

$\frac{p,}{V} \setminus s$	0	15	30	40	45	50	55	60
7, 90					0.06 (.72)		0.08 (.9)	0.08 (.53)
5, 100				−0.31 (.86)		0.84 (1.2)	1.02 (.77)	
3, 130		0.15 (.97)			0.98 (.83)		1.08 (.93)	
2, 160	1.44 (.98)		2.32** (1.13)			2.53** (1.08)		

Note: Robust standard errors are included in the parentheses. The Number of observations for each case is the same as in Table 2.

Table 4: Selection effect ($\bar{e}(r, I, Ch) - \bar{e}(r, I, A)$)

$\frac{p,}{V} \setminus s$	0	15	30	40	45	50	55	60
7.90					−0.12 (.57)		0.03 (.5)	0.0 (.61)
5, 100				0.82 (.93)		− 1.2 (1.28)	−0.52 (.93)	
3, 130		1.23 (.93)			0.72 (1.07)		0.52 (1.17)	
2, 160	−0.32 (1.05)		0.45 (1.24)			0.47 (1.20)		

Note: Estimates are based on the Isolation part choices of only the selected subjects – i.e., those who have chosen or got assigned the risky project in the Menu part. Robust standard errors are included in the parentheses. The number of observations for each case is the same as in Table 2.

on idiosyncratic tastes or beliefs. Thus, the selection effect turns out not to play a major role in our experiment. This is consistent with Dal Bo, Foster and Putterman (2010), who also find a rather low selection rate in their data (less than 10%).[26]

Result 2: *Pure motivation effect.*

(i) Overall, autonomy of choice has a pure motivation effect on effort.

[26] In our case, the selection effect is not significantly different from zero for any of the twelve menus.

(ii) The pure motivation effect is qualitatively and statistically significant only when the menu pairs a high-risk high-return project with a very profitable safe project.

(iii) The pure motivation effect can be substantial, increasing effort by as much as 31%.

(iv) The pure motivation effect accounts for about 85% of the total autonomy effect.

Let us review the overall pure motivation effect estimated from the full data set, i.e., pooling data across all menus. In regression (1) of Table 9 included in the Appendix, we regress the differences in effort levels, $\Delta(i,(r,s),T)$, on the treatment (= 1 if Chosen tr.) and menu specific controls: the prize V, probability p and their interaction term. The coefficient on the treatment variable gives the overall estimate of the pure motivation effect. The effect (= 0.88) is substantially lower relative to our largest estimates from the most diverse menus, but it is significant (p-value = 0.032), which contrasts with our non-significant estimates from the least diverse menus. This is consistent with the idea that the motivational effect of autonomous choice depends on how meaningful or consequential is the choice.

Our theoretical model implies that the pure motivation effect should depend positively on s, the forgone income from the safe project. A glance at Table 3 reveals that the various Δ's indeed increase as we move along the last two rows in left-to-right direction. In order to test the null hypothesis that effort on the risky project is independent of s (as predicted by standard expected utility maximization), we perform a permutation resampling test on the menus involving project $(2,160)$ (which is where we found a significant pure motivation effect).

Let $\Delta(i,(r,s),T) = e(i,(r,s),M,T) - e(i,r,I,T)$. For a given risky project r and treatment T, let $\Sigma(r,T)$ be collection of all $\Delta(i,(r,s),T)$, as (i,s) ranges over all subjects and safe projects in the experiment. That is,

$$\Sigma(r,T) = \cup_s \cup_i \Delta(i,(r,s),T).$$

Under H_0, the Δ's in $\Sigma_{(r,s),T}$ are drawn from the same distribution which is independent of s. We permute the (i, s) indexes in $\Sigma(r, T)$ 100,000 times, each time calculating Δ for each of the three bins corresponding to low, medium, and high value of s. Next, we record the proportion of cases for which the differences in Δ's between the low and medium bin and between the medium and high bin are greater than the differences we observe in the data.[27] We find that under H_0 the likelihood of obtaining increasing differences that are more extreme than what we see in the data is less than 1% – i.e., the p-value $= 0.003$.[28]

> **Result 3:** *Shadow of the foregone option.*
>
> When the pure effect is observed, it is increasing in the value of the forgone safe alternative s.

4.4 Additional Observations

In this section we comment on a few additional interesting patterns in the data.

Expected Profits

Regret aversion distorts the incentives to maximize material payoffs. Compared to the material payoff maximizing effort level, regret averse agents are predicted to work excessively hard in the Chosen treatment (to avoid the psychological cost of failure). We therefore expected our subjects to record lower earnings (net of effort costs) in the Chosen treatment than in the Assigned treatment. Moreover, this difference should be proportional to the size of the measured pure motivation effect. To examine this, we calculated

[27] The actual differences between the high and medium bin and the medium and low bin are: $2.53 - 2.32 = 0.21$ and $2.32 - 1.44 = 0.88$.

[28] If we run the same test for the total effect reported in Table 2, we obtain p-value $= 0.002$.

the expected earnings in the Menu part, given the effort chosen in the risky project. That is, for project (p, V) we calculated

$$(1 - p/20)^e V - 5e.$$

The results are shown in table (5). In each cell, the top two numbers refer to expected earnings for the Chosen and the Assigned treatments; the bottom number is their difference.

Table 5: Average expected profits in the Menu pt.

$p, V \setminus s$	0	15	30	40	45	50	55	60
7, 90					57.3 53.5 3.8		51.9 53.8 −1.9	60.7 58.7 2
5, 100				67.4 66.9 0.5		67.8 65.5 2.3	65.7 66.1 −0.4	
3, 130		86.4 90.1 −3.7			87.9 93.9 −6		84.6 91.8 −7.2	
2, 160	118.5 122.7 −4.2		112 123.6 −11.6**			107.8 120.9 −13.1***		

Note: each entry is a triple where the top number refers to the expected profit in the Chosen tr. (top), Assigned tr. (middle) and their difference (bottom). The number of observations for each case is the same as in Table 2.

We see a familiar pattern. There are no obvious treatment differences in the top two rows (corresponding to the p-bets). The differences become increasingly negative and larger in magnitude as we move along the table in the down-right direction.[29] Statistically significant differences occur, as expected, when project $(2, 160)$ is matched with the two most profitable safe options, $s = 30$ and $s = 50$.

Demographic data

[29] Earnings in the two treatments do not come from the same distribution. This is confirmed by the Wilcoxon matched pairs test (the p-value $= 0.000$).

In the post-experiment questionnaire we collected information on demographic variables, including gender, major, number of semesters completed and number of months of work experience. Because the experiment was designed to be between rather than within subjects, we did not estimate the pure motivation effect on an individual level. But we can get at least some idea about the role of demographic variables by estimating the overall average treatment effect[30] conditional on individual demographic groups, i.e., see the regression (2) in Table 9 in the Appendix.

We regress the differences in effort levels, $\Delta(i,(r,s),T)$, on the treatment ($= 1$ if the subject was in the Chosen condition); menu specific controls, such as, prize, probability and their interaction term; demographic variables, such as, gender, major, number of completed semesters and number of months worked; and the demographic variables interacted with the treatment. The coefficients on the last set of interaction variables estimate the effects of individual characteristics on the pure effect. It can be seen from Table 9 that none of those coefficients are significant. It seems the pure effect is not affected by any specific demographic features.

Project choices

In Table 7 in the Appendix, the third row of the top panel gives percentages of subjects who chose the risky project in the Menu part. As expected, there is a decreasing trend for each risky project as the safe alternative increases in value. A surprising observation is that in the Chosen treatment, 23 subjects (33.3% of the total) chose the dominated $s = 0$ option over the risky project $(2, 160)$.[31] Perhaps they were near indifferent, perceiving the probability of success on a single try as negligible, and in the safe project they did not have to go through the trouble of choosing e. Or perhaps they failed to realize that the safe project was dominated. In any case, they would

[30] Not conditioning on individual menus.

[31] For the safe option, a single try ($e = 1$) was automatically imposed, which cost 5. Choosing $e = 1$ in $(2, 160)$ also cost 5, but then there is a possibility of winning 160. Therefore, choosing $(2, 160)$ and then $e = 1$ dominates the $s = 0$ option.

definitely be expected to choose the safe project also in the remaining two menus that paired $(2, 160)$ with a better safe option $s > 0$. Among the 23 subjects, 4 (or 17%) chose $(2, 160)$ over the $s = 30$ safe option, and 6 (or 26%) chose it over the $s = 50$ safe option. One subject chose $(2, 160)$ in both cases, so there where 9 ($= 4 + 6 - 1$) subjects who did not behave as expected. Perhaps this was due to confusion. As a precautionary measure, we excluded the 9 subjects from the analysis and reran all of our analysis with the restricted data set. All results remained qualitatively unchanged.

5 Concluding Discussion

In the real world, there are many possible reasons why autonomy might boost performance. One possibility is signaling: an employee who chooses a project will work hard to ensure its success in order to signal his value to the organization. Another possibility is the selection effect: a college student may study harder if he can freely choose his major, as opposed to his parents choosing it for him, because he will choose a major that suits him better. These effects are quite consistent with standard economic theory. In contrast, we identify an effect which is inconsistent with standard theory: the "pure motivation" effect. We find evidence for this effect in the simplest possible environment – a one-person decision problem – with full experimental control and meaningful economic trade-offs. Our results suggest that, holding worker and project characteristics constant, a worker will tend to supply more effort if the project was chosen by him rather than assigned to him.

To understand the pure motivation effect, a natural starting point is regret theory. The very idea of regret is closely linked to counterfactual reasoning and the feeling of personal responsibility: people experience regret only when they think the outcome would have been better had they chosen differently (Mellers *et al.*, 1999). For example, if a college student's parents chose his major then he will work less hard than if he had chosen the same major himself, because his parents share responsibility for the outcome. Our simple model of regret aversion implies that effort in the *chosen* project depends on the characteristics of the *forgone* project. The student who gave up a lucrative career for higher education is predicted to work harder than someone who had no such lucrative outside option. In our data, we find a significant pure motivation effect only when the menu pairs a high-risk high-return project with a very profitable safe project. In this situation, the project choice is highly consequential and the scope for regretting the choice is large, so the pure motivation effect is expected to be strong.[32]

Recall that the subjects in Babcock, Bedard, Charness, Hartman and Royer's (2015) experiment could choose to work individually or in two-person teams. In a team, both subjects had to meet the attendance target in order to get a bonus, so that the best way to earn money was to work individually. In this sense the choice was not very meaningful, yet it had a significant motivational effect. How can this be reconciled with regret theory? We speculate that perhaps some strategic aspect of the team spills over into the individual condition. For example, a subject might believe that if she chose the team, she would work extra hard in order not to let her partner down (perhaps due to guilt, see, e.g., Charness and Dufwenberg 2006). This belief in a high effort in the (non-chosen) team option might cause her to work hard individually in order to avoid feeling regret if the target is not met.

The pure motivation effect and the selection effect constitute the *instrumental* value of autonomy. But the fact that regret is a negative feeling suggests that autonomy may have negative *intrinsic* value: if a project is assigned to an agent then he may be better off, because the responsibility for a failure will not rest on his shoulders. Recall that our subjects were on average financially better off (i.e., the material benefit net of the cost of effort was higher) in the Assigned treatment than in the Chosen treatment. In addition, they may suffer the psychological cost of regret in the latter case. On the other hand, a successful autonomous agent may experience

[32]If the choices involve tasks such as solving puzzles or sorting documents, autonomy might have even more powerful motivational effects. This is left for future research.

the positive feeling of *rejoicing*, "the extra pleasure associated with knowing that, as matters have turned out, he has taken the best decision"(Loomes and Sugden, 1982, p. 808). Our simple theoretical model was meant to show how regret aversion can produce a pure motivation effect, and to obtain some testable predictions. Adding rejoicing to the model would not provide any new insights into this effect; it would simply reinforce the effect by increasing the payoff to success in the Chosen treatment. It might help us understand the intrinsic value of autonomy. However, our experiment was not designed to identify the intrinsic value of autonomy.

Sen (1999) argued that autonomy ("being one's own boss") is intrinsically valuable. But one could not easily modify our experimental design to find out whether people like autonomy, or would rather be told what to do. For example, suppose we ask a person to choose which treatment (Chosen or Assigned) to participate in. Asking this question gives him *de facto* autonomy, and we still would not know whether he likes this autonomy or not. He might decide to participate in the Chosen treatment because he fears that he will regret choosing the Assigned treatment; yet he might wish that he had been told to participate in the Assigned treatment (with no possibility of regret). We cannot uncover how a person feels about autonomy by asking him to choose whether or not to be autonomous, because the very choice imposes autonomy by fiat.

Finally we mention some other possible explanations for why effort might depend on the "pure act of choice". The first possibility is overconfidence (Owens, Grossman and Fackler, 2014) or the "illusion of control" (Langer, 1975): for whatever reason, subjects think they are more likely to make the right choice than other people would be. This would allow the "pure act of choosing" to influence effort, but the direction could be positive or negative. If a subject thinks that the very fact that he chose a project makes it very likely to succeed, then he may put very little effort into it – the essence of overconfidence. Moreover, the overconfidence or illusion of control hypotheses cannot tell us why or how effort would depend on the characteristics of a forgone project. In contrast, our simple model of regret aversion makes quite definite predictions, which are largely supported by our data.

A second possible explanation is reference dependence theory (Kőszegi and Rabin, 2006). This theory allows the possibility of multiple "personal equilibria", where the worker's effort choice is consistent with his beliefs. Perhaps in the Chosen treatment a high-effort personal equilibrium is selected, while in the Assigned treatment a low-effort equilibrium is selected. However, this selection principle seems ad hoc. Even if one could justify it, it could not rationalize other patterns in the data, such as the pure motivation effect increasing in the value of the safe option.

A third possible explanation is self-signalling theory (Bodner and Prelec, 2003, and Bénabou and Tirole, 2006). This theory posits a doer-self who takes actions, and an observer-self who learns about the doer. But it is not clear what the observer would want to learn in our experimental set-up: the doer's risk aversion, or perhaps his ability to make "good choices" and become successful? Since the appropriate formulation is not clear, we do not pursue this theory further. We simply note that if, for some reason, the observer rewards choices in proportion to the difference between the actual and the forgone payoff, the doer might behave as if he were regret averse.

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6 Appendix: additional tables

Table 6: Average effort in the Isolation part – all subjects

p, V	10, 80	9, 80	9, 85	8, 90	7, 90	6, 95	6, 100	5, 100	4, 110	4, 120	3, 130	2, 160
All	4.64	4.96	4.87	5.33	5.04	5.25	5.64	6.57	6.15	6.27	5.51	7.51
Ch	4.38	4.7	4.57	5.16	5.13	5.36	5.45	6.48	6.28	6.05	5.68	7.38
A	4.92	5.23	5.2	5.51	4.95	5.12	5.85	6.66	6.02	6.49	5.34	7.65
Df	-.54	-.53	-.63	-.35	.18	.24	-.4	-.18	.26	-.43	.34	-.27

Note: All – pooled data (134 obs.); Ch – Chosen treatment (69 obs.); A – Assigned treatment (65 obs.); Df – difference between the Chosen and the Assigned tr.

Table 7: Average efforts in the Menu and Isolation parts – selected subjects

Chosen treatment

p, V; s	7, 90; 45	7, 90; 55	7, 90; 60	5, 100; 40	5, 100; 50	5, 100; 55	3, 130; 15	3, 130; 45	3, 130; 55	2, 160; 0	2, 160; 30	2, 160; 50
M	6.24	6.97	5.63	6.41	6.24	6.72	8.67	8.38	9.05	8.15	9.58	10.43
I	5.11	5.31	5.0	6.5	6.48	6.45	6.6	6.92	6.68	7.91	8.58	8.5
#	37	39	24	32	21	22	48	24	19	46	24	30

Assigned treatment

p, V; s	7, 90; 45	7, 90; 55	7, 90; 60	5, 100; 40	5, 100; 50	5, 100; 55	3, 130; 15	3, 130; 45	3, 130; 55	2, 160; 0	2, 160; 30	2, 160; 50
M	6.31	6.86	5.63	5.89	6.6	6.23	7.29	6.68	7.45	7.03	6.81	7.43
I	5.23	5.28	5.0	5.68	7.68	6.97	5.37	6.2	6.16	8.23	8.14	8.03
#	39	36	35	28	25	39	35	40	31	39	37	35

Note: M – Menu part; I – Isolation part (limited to selected subjects – i.e., those who were chose or were assigned r in the Menu part); # – number of observations

Table 8: Tobit: total autonomy effect ($\bar{e}((r,s),M,Ch) - \bar{e}((r,s),M,A)$)

$p,V\setminus s$	0	15	30	40	45	50	55	60
7, 90					0.09 (1.01)		−0.05 (1.06)	0.15 (0.67)
5, 100				0.65 (1.01)		−0.29 (1.1)	0.65 (.96)	
3, 130		1.93 (1.39)			2.15^{*} (1.23)		1.74 (1.26)	
2, 160	1.39 (1.2)		3.35^{**} (1.38)			4.05^{***} (1.5)		

Note: Bootstrapped standard errors are in parentheses. The number of observations for each regression is the same as in Table 2.

Table 9: Difference-in-differences analysis of the pure effect

	(1)		(2)	
Constant	-33.2^{***}	(9.266)	-32.49^{***}	(8.717)
Prize	−0.005	(0.018)	−0.002	(0.018)
Probability	-205.2^{***}	(53.14)	-202.1^{***}	(51.61)
Prize*Probability	3.371	(0.87)	3.345	(0.845)
Chosen Tr.	0.879^{**}	(0.41)	0.284	(1.636)
Female			−0.077	(0.502)
Major: science			−0.328	(0.965)
Major: econ/bus			−0.016	(0.517)
No. of semesters			−0.29	(0.15)
Months worked			0.028^{***}	(0.008)
Female x Tr.			−0.261	(0.79)
Major: science x Tr.			1.181	(1.13)
Major: econ/bus x Tr.			0.184	(0.855)
No. of semesters x Tr.			0.12	(0.199)
Months worked x Tr.			−0.007	(0.017)
Observations	785		785	
R^2	0.049		0.079	

Note: the dependent variable is the difference between the effort levels in the Menu and Isolation parts $\Delta(i,(r,s),T)$; standard errors were clustered by subject and are included in the parentheses; the data includes the full sample.