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Are You Paying Your Employees to Cheat?
An Experimental Investigation

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ABSTRACT

We compare misrepresentations of performance that occur under a target-based compensation system with those that occur both under a linear piece-rate and a tournament-based bonus setting by means of a controlled laboratory experiment with salient financial incentives. A widely used anagram word-creation game was employed as the experimental task. Results show that whether one considers the number of over-claimed words, the number of work/pay periods in which such over-claims occur, or the number of participants who make an over-claim at least once, target-based compensation produced more cheating than the other two systems. In particular, as argued by Michael Jensen (2003), linear piece-rates produce significantly less cheating than a target-based scheme. Moreover, a tournament scheme based on relative performance also results in significantly less cheating than a target-based one. In addition, as first demonstrated by Schweitzer et al. (2004), cheating is more likely under a target-based scheme the closer a participant is to the target.

How people make decisions involving compliance with ethical guidelines and regulations in organizational and social life has been an important focus of research in such diverse fields as philosophy, psychology, accounting, economics, and management. Prior work has identified a number of important factors that affect such compliance decisions by individuals (see Ford & Richardson, 1994; Loe, Farell, & Mansfield, 2000, for comprehensive reviews). These determinants include gender (Ambrose & Schminke, 1999; Glover, Bumpus, Sharp, & Munchus, 2002), self-presentation concerns (Covey, Saladin, & Killen, 1989), stage of moral development (Trevino & Youngblood, 1990), and ethical framework or philosophy (Schminke, Ambrose, & Noel, 1997), as well as situational and contextual factors such as social norms (Donaldson & Dunfee, 1994), organizational culture (Chen, Sayers, & Williams, 1997), ethical training (Delaney & Sockell, 1992), the use of ethics codes (Trevino & Youngblood, 1990; Weaver, Trevino, & Cochran, 2000) and attitudes and behavior of friends and relatives (Schminke, Wells, Peyrefitte, & Sebor, 2002).

A large literature based on seminal work by Becker (1968) and Ehrlich (1973) exists in economics and law relating compliance with and enforcement of the law to economic costs and benefits. Much of this literature focuses on the relationship between enforcement mechanisms and crime. The application of this model to cheating within organizations has been dubbed the “rational cheater” model (Nagin, Rebitzer, Sanders & Taylor, 2002). In a fascinating field experiment, Nagin et al. (2002) find evidence that some employees of a telephone solicitation company respond to a reduction in monitoring with an increase in cheating, while others, perhaps motivated by conscience or guilt, do not. Rickman and Witt (2007) reach similar conclusions in their study of employee theft in the UK.

A considerable theoretical and empirical literature on tax evasion applies the “rational cheater” model to examine the relationship between the decision to evade and such enforcement mechanisms as the audit rate, audit selection methods, and the penalty

if caught (e.g., Alm, Cronshaw, & McKee, 1993; Alm, Jackson, & McKee, 1992a; 1992b; 1993; Alm, McClelland, & Schulze, 1992; 1999; Alm, McKee, & Beck, 1990; Alm, Sanchez, & de Juan, 1995; Beron, Tauchen, & Witte, 1992; Boylan & Sprinkle, 2001; Cadsby, Maynes, & Trivedi, 2006; Feld & Tyran, 2002; Moser, Evans, & Kim, 1995). At the same time, this literature also discusses other factors affecting incentives to evade or comply with one's tax obligations such as the tax rate or the use to which tax revenues are put. Alm and McKee (1998) provide an excellent review of this literature, and argue that many of the results from laboratory experiments on tax compliance are directly applicable to organizational compliance with regulations and compliance with regulations within organizations. For example, experimental work on the effects of different enforcement mechanisms can be applied to the use of analogous schemes by regulatory authorities and within organizations.

However, compliance within organizations does not depend solely on enforcement mechanisms. It also depends on the incentives created by an organization's compensation practices to act in accordance with or to disregard company regulations. The literature on tax evasion has considered how incentives created by different tax rates or tax systems may affect compliance, but this is not directly applicable to the analogous issue of how compensation systems may create incentives that tempt employees to cheat.

Production is not always easy to observe and pay is often based upon employee reports of hours worked or tasks accomplished. For example, lawyers, accountants and business consultants are often paid based on self-reported billable hours. Automobile and appliance service technicians charge customers based on their own diagnosis of the problem and of the resultant repairs. Similarly, physicians in many countries are paid based upon their own diagnosis of illness and the resultant treatment. Many executives are paid based on the financial performance of their organizations, which in turn can be manipulated by false or misleading reports. Nagin et al. (2002), as mentioned above,

discuss a case in which telephone canvassers soliciting money for non-profit organizations receive commissions based on their self-reports of contribution pledges.

Recently, in response to several business scandals associated with false sales reports to obtain rewards under goal-setting compensation systems (DeGeorge, Patel & Zeckhauser, 1999; Jensen, 2001), Michael Jensen (2003) has argued controversially that the use of production or sales targets in compensation formulas encourages people to lie or misrepresent their performance with serious consequences for firm productivity and profitability. Urging that such targets be replaced by linear pay-for-performance compensation systems in which people are rewarded in direct proportion to their productivity, he asserts:

“Everyone can benefit by bringing this game to an end, and I believe it starts by eliminating the use of targets in compensation systems, and in particular by eliminating the use of budgets as targets in compensation systems. Simply put this means creating linear pay-for-performance compensation systems” (Jensen, 2003, pg. 405).

However, it is not obvious that adopting a linear pay-for-performance (henceforth PFP) compensation system would really give people incentives to report their performance truthfully. As long as people are paid on the basis of performance, linearly or otherwise, they may still have an incentive to exaggerate their performance. Indeed, it is possible that a linear PFP system would encourage bigger lies about the number of items produced or sold within a budgetary period. If one is close to a target under a target-based system, one need claim only to have produced or sold a few more items to reach the target, thereby obtaining a large financial bonus. To obtain a similarly increased payoff under a linear piece-rate system, one might have to make far more exaggerated claims relative to actual performance. Such exaggerated claims could damage the sales and production planning processes, perhaps even more seriously than under a target-based system.

The purpose of this study is to compare the exaggerations and misrepresentations that occur under a target-based compensation system with those that occur both under a linear piece-rate setting and a tournament-based bonus setting by means of a controlled laboratory experiment with salient financial incentives. A recent study by Schweitzer, Ordóñez & Douma (2004) examined target-based production and compensation systems in the laboratory. They showed that a target-based system produces more lies about performance than simply paying people a lump sum and asking them to do their best. However, they neither compared target-based systems with the linear systems favored by Jensen (2003) nor compared them with another popular alternative, a tournament-based system.

Before replacing target-based with linear piece-rate systems, it is important to examine whether such linear systems will actually reduce misrepresentation. This is difficult to do in an actual business setting due to the hidden nature of misrepresentation and the many uncontrollable factors that might affect misrepresentation in the field. In contrast, a well-designed laboratory experiment allows us to observe directly the degree of misrepresentation under the three compensation systems— target-based, linear piece-rate, and tournament—while controlling for other confounding factors.

The next section outlines the theoretical background for the study, utilizing a simple model of the benefits and costs of cheating. This is followed by a section outlining the experimental methodology and another discussing the experimental results. A conclusion follows.

THEORY

A Simple Model

The Jensen hypothesis that target-based compensation encourages cheating and misrepresentation relative to linear PFP is based on an implicit assumption about the nature of guilt and its relationship to the amount of cheating. We illustrate this by constructing a simple model of cheating behavior. Like the models of Becker (1968) and

Ehrlich (1973), our model compares the benefits of cheating with the costs. In contrast to Becker and Ehrlich, the costs in our model are psychological costs based on the guilt experienced as a result of cheating rather than the expected costs of being caught and punished. Our model bears some resemblance to the one presented by Nagin et al. (2002). However, we focus more explicitly on the precise relationship between guilt and the amount of cheating and its interaction with the compensation system. In order to focus on this relationship, we do not include any system of monitoring, enforcement, or punishment in either our model or our experiment.

Suppose that an individual is working at a job that rewards each employee based on the number of self-reported units produced within a given time period. The allotted time has elapsed and the individual has managed to produce a certain amount. For the time being, assume that the actual amount produced is determined independently of any opportunity to misrepresent performance. The individual can either report truthfully or misrepresent his/her performance. Let c represent the number of over-claims (c stands for cheating) made by the individual in question. Over-claims may be beneficial in that under a PFP system, they may lead to higher pay. Higher pay in turn leads to higher utility or satisfaction. In particular, the utility of cheating is given by $U[P(c)]$, where P equals the monetary payoff resulting from any over-claimed words. The precise form of $P(c)$ is determined exogenously by the payment scheme. This will be the treatment variable in our experimental design. In all cases, $P(0) = 0$ since the payoff resulting from over-claims must be zero when no over-claims occur. For simplicity, we normalize $U[P(0)] = 0$. $U'[P(c)] > 0$ and $U''[P(c)] < 0$ by assumption.

$G(c)$ represents the psychological disutility resulting from the guilt associated with cheating. It is modeled as a function of the number of over-claimed words, c . $G(0) = 0$ since guilt arises only if cheating occurs. $G(c)$ is allowed to be discontinuous at 0. This allows for the possibility that for some people even a tiny amount of cheating results in a

large amount of guilt. However, it is assumed to be continuous elsewhere. $G'(c) \geq 0$, indicating that guilt does not decrease as the amount of cheating rises.

In the target-based setting, $P(c)$ is discontinuous. In particular, $P(c) = 0$ if $c < c^*$ and $P(c) = B$ if $c \geq c^*$ where B equals a specified bonus paid if the target is achieved and c^* is the number of over-claims required to reach the prespecified target. Notice that a decision about whether to over-claim is only necessary if one has produced fewer than the number of prespecified units of output required to receive the bonus. Thus, c^* is the difference between the target and the amount actually produced when that difference is positive.

When faced with a decision about whether or not to cheat, an individual compares the benefits of the bonus with the cost of the guilt. If $U(B) > G(c^*)$, the benefits exceed the costs and the individual will over-claim c^* units. In contrast, if $U(B) < G(c^*)$, the costs exceed the benefits and the individual will not cheat. When $U(B) = G(c^*)$, the person is indifferent, and the decision may go either way. In any group of people, $G(c^*)$ is likely to differ between individuals since different people will generally have different guilt responses to a given number of over-claims. However, since $G'(c) \geq 0$ for each individual, a given person with an unknown guilt function is more likely to cheat by making c^* over-claims, the closer s/he is to the target, i.e. the smaller is c^* . This is because a smaller c^* implies less guilt. This prediction has already received empirical support in the work of Schweitzer et al. (2004). We reexamine this issue in our setting.

Hypothesis 1: Under a target-based compensation scheme, cheating is more likely to occur, the closer one is to the predetermined target.

In the linear piece-rate setting, $P(c) = k \cdot c$ is the payoff-from-overclaim function, where k is the payoff per unit of reported output. If $U(k \cdot c) < G(c)$ for all c , there will be no cheating. In contrast, if $U(k \cdot c) > G(c)$ for some c , cheating will occur at the level of c ,

\hat{c} , that maximizes $U(k \cdot c) - G(c)$, thus providing the greatest possible net gain.¹ In particular, there will be an interior maximum at \hat{c} where $kU'(k \cdot \hat{c}) = G'(\hat{c})$, i.e. where the marginal benefit from cheating just equals the marginal psychological cost of guilt if $k^2 \cdot U''(k \cdot \hat{c}) - G''(\hat{c}) < 0$, and it will be a global maximum if $k^2 \cdot U''(k \cdot c) - G''(c) < 0$ for all c associated with gains from cheating. If $U(k \cdot c) > G(c)$ for some c and $k^2 \cdot U''(k \cdot c) - G''(c) > 0$ for all such c , gains from cheating will rise without limit as cheating increases.

Notice that, in contrast to the target-based setting, cheating in the linear piece-rate setting can always lead to a higher payoff regardless of the amount actually produced.

Comparing Cheating under the Target-Based and Linear Piece-Rate Settings

Let t equal the prespecified target and q represent actual output. Then c^* , the number of over-claims necessary to receive the bonus, is equal to $t - q$ when $t > q$. When $t \leq q$, no financial gains can arise from cheating. Thus, $c^* \leq t$. When comparing target-based and linear piece-rate compensation schemes, we assume that $B = k \cdot t$ and therefore that $B \geq k \cdot c^*$ for all possible c^* . This simply means that the bonus under the target-based scheme is equal to the amount one would earn if one were to report exactly the targeted amount under the piece-rate scheme. The purpose of this assumption is to make the size of the compensation package under the two schemes equivalent for purposes of comparison. Nothing in the analysis that follows changes if $B > k \cdot t$.²

Proposition 1: If it is beneficial for a person to over-claim c^* units in the linear piece-rate setting, it will also be beneficial to over-claim c^* units in the target-based setting.

Proof: Over-claiming c^* units in the linear setting implies $U(k \cdot c^*) > G(c^*)$. Since $B \geq k \cdot c^*$, $U(B) \geq U(k \cdot c^*)$. Thus, $U(B) > G(c^*)$ and that individual will over-claim in the target-based setting as well.

¹ Since $G(c)$ may be discontinuous at $c = 0$, differentiation occurs over the range of $c > 0$. Recalling that $c < 0$ is not possible and that $U(k \cdot c) - G(c) = 0$ at $c = 0$, the assumption that $U(k \cdot c) > G(c)$ for some c guarantees that the maximum for $c > 0$ is also the global maximum.

² A firm with a given amount of money available for compensation would in fact set $B > k \cdot t$ when moving between schemes if the target were a challenging one such that average productivity were less than t under the two schemes.

Note that the converse need not be true. Over-claiming c^* units in the target-based setting implies $U(B) > G(c^*)$. However, since $U(B) \geq U(k \cdot c^*)$, one cannot say whether $U(k \cdot c^*)$ is greater or less than $G(c^*)$. Thus, one cannot say whether or not it would be beneficial to over-claim c^* units in the linear piece-rate case.

Intuitively, the financial incentives to over-claim c^* units of output are at least as high and generally higher in the target-based than in the linear case. This is perhaps the intuitive basis for Jensen's prediction that targets lead to substantially more cheating than linear pay systems. However, it is important to note that this general prediction is not implied by the theoretical model. Although the case of over-claiming c^* units in the linear case, but not in the target-based case is ruled out by our simple model, every other conceivable outcome is possible, depending upon the shapes and positions of the $U[P(c)]$ and $G(c)$ functions, which in turn depend on individual attitudes towards earnings and guilt. Thus, it is impossible based on the theoretical model alone to predict whether target-based or linear financial incentives will lead to more cheating.

Table 1 indicates nine possible cases. We discuss each in turn. In case a, there is no cheating in either the target-based or linear cases. This occurs when $U(B) < G(c^*)$ and $U(k \cdot c) < G(c)$ for all c . Figure 1 gives an example of such a case.

In case b, there is no cheating in the linear case, but cheating in the target-based case. This occurs when $U(B) > G(c^*)$, while $U(k \cdot c) < G(c)$ for all c . Figure 2 illustrates such a case.

In case c, there are positive but fewer over-claims in the linear case than in the target-based case. This case occurs when: (i) $U(B) > G(c^*)$, (ii) $U(k \cdot c) > G(c)$ for some c , (iii) $k \cdot U'(k \cdot \hat{c}) = G'(\hat{c})$ at $\hat{c} < c^*$, and (iv) $k^2 \cdot U''(k \cdot \hat{c}) - G''(\hat{c}) < 0$ at $\hat{c} < c^*$. An example is presented in Figure 3.

In case d, there are c^* over-claims in both the linear and target-based cases. It occurs under identical conditions to case c except that $\hat{c} = c^*$. An example is presented in Figure 4.

In case e, there are more over-claims in the linear case than in the target-based case. It occurs under identical conditions to case c except that $\hat{c} > c^*$. An example is presented in Figure 5.

In case f, there are unbounded over-claims in the linear case, and c^* over-claims in the target-based case. This requires $U(B) > G(c^*)$. In addition, either $k \cdot U'(k \cdot c) > G'(c)$ for all c , or if $k \cdot U'(k \cdot c) = G'(c)$ for any c , $k^2 \cdot U''(k \cdot c) - G''(c) > 0$ at that point. Examples representing the first of these conditions are presented in Figures 6 and 7 respectively. Notice that in the Figure 7 example, a person experiences the same amount of guilt when s/he cheats regardless of whether s/he cheats a little or a lot. An example representing the second condition is presented in Figure 8.

In case g, there is no cheating in the target-based case, but cheating at $\hat{c} < c^*$ in the linear case. This occurs when: (i) $U(B) < G(c^*)$, (ii) $U(k \cdot c) > G(c)$ for some c , (iii) $k^2 \cdot U'(k \cdot \hat{c}) = G'(\hat{c})$ at $\hat{c} < c^*$, and (iv) $k^2 \cdot U''(k \cdot \hat{c}) - G''(\hat{c}) < 0$ at $\hat{c} < c^*$. An example is presented in Figure 9.

In case h, there is no cheating in the target-based case, but cheating at $\hat{c} > c^*$ in the linear case. It occurs under identical conditions to case h except that $\hat{c} > c^*$. An example is presented in Figure 10.

Finally, in case i, there is no cheating in the target-based case, but unbounded over-claims in the linear case. This requires $U(B) < G(c^*)$. In addition, either $k \cdot U'(k \cdot c) > G'(c)$ for all c , or if $k \cdot U'(k \cdot c) = G'(c)$ for any c , $k^2 \cdot U''(k \cdot c) - G''(c) > 0$ at that point. Examples representing each of these conditions are presented in Figures 11 and 12 respectively.

Different people will generally possess different $U[P(c)]$ and $G(c)$ functions, representing different psychological reactions to changes in earnings and to different levels of cheating. At different times, $0 < c^* \leq t$ will also differ even for the same individual depending on his/her productivity in a given pay period. If most people belong to cases b and/or c, Jensen's hypothesis will hold and cheating will be greater under

target-based than under linear piece-rate compensation schemes. If most people belong to cases a and/or d, there will be little difference between the two. Finally, if most people belong to cases e, f, g, h, and/or i, Jensen's hypothesis will not hold and there will be more cheating under the linear than under the target-based scheme. Generally speaking, the Jensen cases involve relatively low amounts of guilt for small levels of cheating that increase rapidly at an increasing rate as the level of cheating rises. The opposite case, in which more cheating occurs under the linear scheme, can involve a relatively large amount of guilt for low levels of cheating and/or relatively slow increases in guilt as the level of cheating rises though these general descriptions are not exhaustive of the possibilities illustrated by the relevant figures.

Furthermore, the analysis above assumes that $G(c)$ is independent of the compensation scheme. However, this may not be the case. For example, it is possible that a scheme perceived as being unfair would mitigate guilt. For example, if a person works diligently and comes close to, but does not reach the target, s/he may react with anger at the perceived unfairness of the scheme, and use this perceived unfairness as a justification to cheat.³

Clearly, the validity of the Jensen prediction is ultimately an empirical issue, and an empirical examination of this issue is the primary focus of this paper.

Hypothesis 2 (the Jensen Hypothesis): According to Jensen (2001, 2003), more cheating will occur under a target-based than under a linear piece-rate compensation scheme.

It is easy to extend the simple model discussed above to determine the amount of work effort and the number of over-claims simultaneously. In this case, people will choose effort and over-claim levels to equalize the marginal disutility of effort and guilt, setting both equal to the marginal utility of earnings when there is an interior solution in

³ We thank J. Atsu Amegashie for this observation.

the linear case. Thus, the availability of cheating opportunities may result in less effort since cheating can act as a substitute for effort. A similar result can occur in the target-based case if the guilt disutility associated with more over-claims is lower than the disutility of effort at the effort level that would have occurred in the absence of cheating opportunities.

Comparing Cheating under the Target-Based and Tournament Settings

The target-based compensation scheme examined in this study is binary in nature, rewarding reported performance if and only if the prespecified target is reported to have been reached. An analogous binary tournament rewards reported performance if and only if it is at or above a prespecified percentile relative to other participants. Suppose for comparability that this percentile is chosen so that if performance is honestly reported approximately the same percentage of participants will obtain the bonus as in the target-based scheme. Then the two schemes differ in two major respects.

First, the target-based scheme presents participants with a certain production target. Once a participant knows how much s/he has produced in a given pay period, s/he knows whether or not s/he has actually achieved the target, and if not, how many over-claims s/he would have to make to nonetheless receive the bonus. In contrast, in the tournament, a participant does not know either whether s/he has produced enough to obtain the bonus without cheating, or how many over-claims if any would be necessary to obtain it. This is because s/he knows neither how much other participants will actually produce, nor how much they will cheat. Thus, while the payoff from a given amount of cheating is certain under the target-based scheme, it is uncertain under the tournament scheme. For risk-averse and/or ambiguity-averse participants, this uncertainty means that the certainty equivalent outcome of a given number of over-claims is lower in the tournament than under the target-based scheme. Since there is ample literature showing that most people are risk averse (e.g. Holt and Laury, 2002), this would suggest that there would be less cheating in the tournament than in the target-based case.

Second, under the target-based scheme, cheating is at the expense of the firm's owners who must pay out more in bonuses. In a laboratory experiment, it is analogously at the expense of the experimenter's research grant. In contrast, in a tournament, cheating is at the expense of fellow participants since it causes bonuses to be given to one participant rather than to another. To the extent that participants feel more solidarity and less social distance between themselves and the other participants than between themselves and the owners of their firm or between themselves and the experimenter, they may be more willing to cheat under a target-based scheme than in a tournament. Together, these differences between the two schemes lead to hypothesis 3.

Hypothesis 3: There will be more cheating under the target-based compensation scheme than under the tournament compensation scheme.

EXPERIMENTAL DESIGN

Participants were recruited at a medium-sized Canadian university by means of email solicitation through the Bachelor of Commerce program listserv. Potential participants were told that they were participating in a study about workplace issues, and that they would be paid. They were not given any other details prior to the experiment. All 158 participants were undergraduates and majors in economics or other business subjects. There were 72 males and 86 females with an average age of 20.25 years and a standard deviation of 1.73 years. A widely used anagram word-creation game (e.g., Cadsby, Song, & Tapon, 2007; Schweitzer et al., 2004; Vance & Colella, 1990) was employed as the experimental task.

Upon arrival, participants were randomly divided into two groups of equal size. One group was instructed to stay in the room while the other group was taken to an adjacent room. The two groups followed exactly the same experimental instructions and went through the session in precisely the same way. The experimental instructions were read to the participants while they followed along on their own copies. Participants were provided with a prepared workbook containing the anagrams. Each anagram was

presented on a separate page of the workbook. Participants were not permitted to look ahead to future pages or to go back to previous pages. They were allowed to look at the next page only when instructed to do so by the experimenter. To ensure anonymity, participants wrote their assigned participant numbers, but not their names, on the cover of their workbooks.

The specific compensation scheme was the treatment variable of the study. The three experimental treatments were as follows. 1) *Linear Piece-rate*: Each participant was paid \$3 for word checking (explained below) and \$0.40 for each word they created in the game. 2) *Target-Based Bonus*: Each participant was paid \$3 for word checking and \$3.60 for each round if s/he created nine words or more in a round (there were seven rounds in the game). Nine words was the 85th percentile performance level in our pre-test data set. 3) *Tournament-Based Bonus*: Each participant was paid \$3 for word checking and \$3.60 for each round if his/her performance was at or above the 85th percentile, relative to other participants in the same session.⁴

After the instructions were read aloud to the participants and all questions from participants were individually answered, the session began with two practice rounds, which were designed to familiarize participants with the experimental procedure and task. After the two trial rounds, there were seven experimental rounds. After the last experimental round, participants completed a final trial round. Unknown to the participants, the final trial round contained a unique set of seven letters for each participant that we used to match participants' workbooks with their self-reported performance record. Thus, we could compare actual productivity to claimed productivity on the performance record for each subject after the completion of the experiment.⁵

⁴ Please contact one of the authors for the complete instructions used in each of the three experimental treatments..

⁵ We followed Schweitzer et al. (2004) in this regard. However, in contrast to Schweitzer et al. (2004), we made no specific claim in the instructions that we could not match up the self-reported performance record to participants' workbooks after the experiment.

After completing the word-creation game, participants answered a short demographic information questionnaire regarding their gender, age, and first language. Participants were then asked to submit their workbooks, which were then handed by the experimenter to another participant sitting in the other room. Each participant was then asked to check the work of another participant, whose workbook was given to him/her at random. We gave each participant a list of all known correct words and a scrabble dictionary in case any correct words were missing from the list to help with this task. The specific instructions were as follows: “At the end of the session, your performance will be checked by another person playing the same game. At the same time, you will be asked to check the words created by another participant. That participant will **not** be the same person checking your words.” The \$3 word-checking fee was designed to compensate participants for this cross-checking task.

After the cross-checking was completed, each workbook was returned to the participant to whom it belonged according to the participant number on the cover page. At that point, participants were instructed to check over their own workbook to make sure that all of the correct words had been marked “correct” and that all of the incorrect words had been marked “incorrect.” They were given permission to correct any mistake made by the rater. This system of cross-checking enabled us to distinguish between passive misrepresentation, in which a participant simply acceded to an error made by the rater, and active misrepresentation, in which a participant overruled the rater to make an incorrect claim. Participants were then asked to record the number of correct words they had created on the performance record sheet in the space provided next to the corresponding round number. At that point, participants were instructed to tear off the cover page of their workbooks containing their player number, deposit the cover page and the workbooks into two separate sealed boxes, and bring their performance record sheet to another room one by one to get paid according to the performance record plus the \$3 word-checking fee in cash.

RESULTS

We first examined whether demographic variables (age, gender, and first language) had any influence on our key dependent variables representing productivity and cheating behavior. We found no significant relationships between any of the demographic variables and the dependent variables. This is consistent with the finding reported in Schweitzer et al. (2004), who used a similar population and the same experimental task. As a result, we pooled the data from different demographic groups for the remainder of the analysis.

We then examined productivity under the three payment schemes. Productivity is defined as the number of correct words a participant created during the seven experimental rounds. These numbers are reported on the top line of Table 2a, and are compared between treatments on the top line of Table 2b. Productivity was virtually identical under the three PFP payment schemes. Thus, the remainder of our analysis focuses on over-reporting of productivity, i.e. cheating.

Hypothesis 1 predicts that under the target-based treatment, the probability of cheating behavior will be higher, the closer a person's actual output is to the prespecified target, i.e. the lower is c^* . We employed the following logit regression to test this hypothesis:

$$\ln [f/(1 - f)] = \beta_0 + \beta_1 \cdot c^* \quad (1)$$

where f is the probability of cheating for financial gain and c^* is the difference between the target and the actual number of words created. We used a random effects maximum-likelihood estimation procedure to deal with the lack of independence in the data due to the repeated measures from each participant over multiple rounds.⁶ Since this hypothesis applies only to target-based compensation, we used only the data from the target-based treatment. We ran the regression twice, using two different definitions of cheating for

⁶ We used the SAS NLMIXED Procedure and employed the Dual Quasi-Newton optimization technique.

financial gain. Under the first definition, a person was classified as having cheated for financial gain in a specific round if his/her actual productivity was less than the target level of nine words, but his/her reported productivity was greater than or equal to nine words. In this case, we utilized only the data from rounds in which a participant created fewer than nine words and hence had the opportunity to cheat for financial gain. The null hypothesis that $\beta_1 = 0$ was contrasted with the alternative specified by Hypothesis 1 that a smaller distance to the target, c^* , would be associated with a higher probability of cheating. The results of the logit regression reject the null hypothesis in the direction of the specified alternative ($\beta_1 = -1.486$, $p = 0.010$).

Recall that each participant's work was graded by another participant. We refer to this person as the rater. In some instances, the rater marked incorrect words as correct, and sometimes these rater errors, if left in place by the original participant, resulted in cheating for financial gain under the first definition above. However, these instances of relatively *passive* cheating were possibly due to carelessness, and even if intentional, may have produced less guilt than the more active and clearly intentional cheating that took place when a rater's correct assessment was modified by the participant, resulting in financial gain. Thus, we also employed a second definition in which a person was classified as having *actively* cheated for financial gain in a specific round if his/her actual productivity was less than the target level of nine words, the rater correctly indicated that he had created fewer than nine words, and yet his/her reported productivity was greater than or equal to nine words. In this case, we utilized only data from rounds in which a participant both created fewer than nine words and was graded as having produced fewer than nine words by the rater, and thus had the opportunity to cheat actively. Under this definition, the null hypothesis is also rejected in the direction of Hypothesis 1 ($\beta_1 = -1.900$, $p = 0.005$). These results are consistent both with our model of cheating behavior and with the evidence provided by Schweitzer et al. (2004).

Hypothesis 2 predicts more cheating under the target-based scheme than under the

piece-rate scheme, while Hypothesis 3 predicts more cheating under the target-based scheme than under the tournament scheme. We compare cheating under all three schemes using several different measures of cheating. Table 2a reports measures that are based on numbers of over-claimed words. The first of these measures is simply the *gross number of over-claimed words*. This is defined as the sum over seven rounds of the difference between the number of words reported as being correct and the number of correct words actually created by a participant in each round, whenever this difference is positive, and zero otherwise. The *gross number of over-claimed words* was more than two and a half times as high in the target-based treatment than in either of the other two treatments. These differences are both significant according to both a simple t-test of differences in means and non-parametric Mann-Whitney test, lending support to both Hypothesis 2 and Hypothesis 3.

It is possible that over-claims are the result of carelessness rather than intentional cheating. In that case, as pointed out by Schweitzer et al. (2004), one might expect there to be as many under-claimed as over-claimed words. In fact, there were far fewer rounds in which the number of reported words was less than, as opposed to more than, the actual number of correct words created. These rounds were ignored by our gross measure. If under-claim rounds are entirely the result of carelessness, we might assume that a similar number of over-claim rounds are also the result of carelessness. We can adjust for such carelessness by subtracting the under-claims from the over-claims from the over-claim rounds. *Net over-claimed words* are thus the sum over seven rounds of the difference between the number of words reported as being correct and the number of correct words actually created by a participant in each round, regardless of whether this difference is positive or negative. This reduces slightly the apparent cheating from each of the three treatments. Nonetheless, net over-claims are still significantly higher in the target-based treatment than in either of the other treatments according to the t-tests. The Mann-Whitney test produces a similarly significant result for the target-based

versus the tournament treatment, while the difference between the target-based and piece-rate treatments is of marginal significance ($p = 0.059$).

Both of the measures reported above lump together instances in which over- or under-claims involve going along with the rater's assessment with instances in which a participant actively over-rules a rater's assessment. The former instances are more likely to be due to lack of vigilance. Even if the participant realizes the rater made an error, it is possible s/he feels less guilt at deferring to another's erroneous judgment than at actively overruling another's accurate judgment to his/her own benefit. The third measure recognizes this distinction by considering only active cheating, in which a participant overrules the rater's correct assessment of his/her actual output with a higher self-report. *Gross active over-claimed words* are defined as the sum over seven rounds of the difference between the number of words reported as being correct and the number of correct words actually created by a participant or incorrectly graded as correct by the rater, whenever this difference is positive, and zero otherwise. Once again, both Hypotheses 2 and 3 are strongly supported by the data, which show significantly more active over-claims in the target-based than in the other two treatments. This is also true for the fourth measure, *net active over-claimed words*, which is the sum over seven rounds of the difference between the number of words reported as being correct and the number of correct words actually created by a participant or incorrectly graded as correct by the rater, regardless of whether this difference was positive or negative.

Under the piece-rate scheme, an over-claim always has financial implications since pay increases directly with the number of reported words. Under the tournament scheme, over-claims do not always lead to increases in actual pay. However, they do always lead to a higher *ex ante* probability of being among the tournament winners and thus receiving a higher payoff. In contrast, an over-claim under the target-based scheme only has financial implications insofar as it results in the achievement of the predetermined target. Suppose the target is nine words. A participant makes seven words. S/he reports nine

words. The two over-claimed words result in increased pay. However, suppose this participant reports eight words. S/he overclaims by one word, but there are no financial implications. Similarly, if this participant reports 12 words, two of the over-claimed words have financial implications, but the other three do not. To our surprise, although the majority of over-claims in the target-based case did have financial implications, a considerable number did not. In particular, out of an average of 4.07 active overclaim words per participant in the target-based treatment, 3.04 had financial implications. The other 1.03 words, 25.3% of the total, did not.

In retrospect, there could be a number of reasons for such non-financial over-claims. First, the participant who makes seven words and decides to cheat in order to get the bonus might be reluctant to report precisely nine words, thinking this might provoke suspicion. S/he may feel it may look less suspicious to report a number of words somewhat in excess of the target. Second, a participant may strongly believe that a word is correct, so much so that s/he feels no need to check it against the word list or in the dictionary. Nonetheless, the word may not be in the dictionary, or may violate the rules of the game by, for example, being a proper noun. The participant erroneously counts it as a correct word, sometimes even in the face of a contrary decision by the rater regardless of whether or not there are financial consequences. Third, it is possible that a participant erroneously uses two forms of the same word, e.g. a singular and plural form. Although this is clearly not permitted by the instructions, the participant may not have realized this, and thus count both as correct words for reasons that have nothing to do with financial implications. The second and third reasons could also motivate over-claims in the piece-rate and tournament treatments; however, in those treatments, they do not stand out as being different from any of the other over-claims since in those treatments all over-claims may have financial implications.

Should over-claims from the target-based treatment that have no financial implications be counted as cheating when comparing cheating under the three treatments?

On balance, we think so. Although the first motivation applies only to the target-based case, the other two apply to all three cases. Thus, similarly motivated over-claims may occur under all three treatments. It is impossible to remove them from the piece-rate and tournament data. Therefore, in our view, it is best to include them in the target-based data as we have done above. However, it is nonetheless prudent to undertake an additional comparison that removes such non-financial over-claims to determine whether doing so affects the support we have obtained for Hypotheses 2 and 3. To do so, we need to take account of the fact that financial over-claims are only possible in a given round of the target-based treatment if a participant has created fewer than the nine words specified as the target. There are seven rounds in the experiment. However, if a participant makes nine or more words in three of those rounds, he only has the opportunity to make a financial over-claim during the other four rounds.

We thus define *gross active financial over-claimed words per eligible round* for the target-based treatment as follows: First, we calculate the number of active over-claim words that have financial implications for each participant. We then divide this number by the number of rounds in which that participant (i) made fewer than the target of nine words and (ii) was correctly deemed to have made fewer than nine words by the rater, i.e., the rounds in which the participant had the opportunity to actively cheat. The result is the gross number of active financial over-claimed words per eligible round for that particular participant. Table 2a reports the average of these numbers over all participants as 0.50 words per eligible round. For the piece-rate and tournament treatments, all active over-claims have financial implications, and all rounds are eligible rounds in which it is possible to cheat. Therefore, active financial over-claimed words are simply calculated as the number of gross active over-claimed words divided by the total number of seven rounds. Using t-tests, we find some support for Hypotheses 2 and 3 with p-values that are on the border of significance at $p = 0.057$ and $p = 0.056$ respectively (see Table 2b). The Mann-Whitney tests reject the null in favor of the two proposed Hypotheses 2 and 3

more decisively with significant p-values in both cases. Since incidents of active financial under-claims were negligible in all three treatments, with just one participant making such under-claims in just one round in each case, we do not repeat the analysis for net active financial under-claims.

So far, we have examined cheating only in terms of the number of over-claimed words under each payment scheme over the course of the experiment. This is comparable to comparing over-claims on units of output or sales over several pay periods. The experiment consisted of seven rounds or work/pay periods. Another way of comparing cheating is to consider the number of rounds in which each participant cheated by making at least one over-claim under each scheme. Table 3a reports the relevant statistics using definitions for over-claim rounds that are analogous to the definitions we used for over-claim words. Regardless of the definition used, the number of rounds per person in which over-claims occurred is more than twice as high under the target-based as under the linear piece-rate or tournament compensation schemes. Furthermore, the t-tests all reject the null hypotheses in favor of the two proposed Hypotheses 2 and 3 (see Table 3b). The Mann-Whitney tests give nearly identical qualitative results except for the marginal p-value of 0.081 for the piece-rate versus the target-based treatment using the net over-claim rounds measure. Thus, Hypotheses 2 and 3 both continue to receive strong support when cheating is compared based on the number of rounds in which it takes place rather than on the number of over-claims made over the course of the entire session.

A third perspective can be gained by comparing the probability of a person cheating at least once between treatments. Table 4a reports the number and percentage of people who cheated under each payment scheme for the different definitions of cheating. For the target-based scheme these numbers are between 41% and 54%, while for the piece-rate scheme, they are between 25% and 33% and for the tournament scheme, between 26% and 32%. We ran the following regression using the various definitions of cheating for each pair of treatments:

$$\ln [q/(1 - q)] = \beta_0 + \beta_1 \cdot T \quad (2)$$

where q is the probability of a person cheating at least once and T is a dummy variable representing the treatment or compensation scheme. The results are reported in Table 4b. They again provide support for both Hypotheses 2 and 3, significantly rejecting the null in the direction of these hypotheses for all but one of the cheating definitions. The exception is gross active financial over-claims for which the p -values were marginal, 0.079 for Hypothesis 2 and 0.104 for Hypothesis 3. Since the incidents of gross active financial over-claims and gross active over-claims are identical by definition under the piece-rate and tournament schemes, these marginal p -values are due to the fact that there were three fewer incidents of gross active financial over-claims than gross active over-claims under the target-based scheme. It should be remembered that in the target-based treatment, there were fewer opportunities available for active financial over-claims than in the other two treatments since such cheating was only possible in rounds in which a participant made fewer than nine words.

Although we did not put forward any hypothesis concerning the relative frequency of cheating between the piece-rate and tournament schemes, we nonetheless report comparisons between these two treatments for each definition of cheating in the each of the relevant tables. In no case was there a significant difference.

Despite the strong support for the notion that the target-based scheme provides the greatest temptation to cheat for most participants, this is not necessarily the case for everyone. Since each participant was randomly assigned to just one treatment, we have no way of directly comparing the cheating behavior of particular individuals under the different payment schemes. However, it is interesting to note that the highest number of over-claims by one person occurred under the piece-rate scheme. In fact, that one person made 38 over-claimed words. Since 79 over-claimed words were made by all 52 participants in the piece-rate treatment, that one person was responsible for 48% of the cheating that occurred under the piece-rate scheme. Under the tournament scheme, the

biggest cheater was responsible for 38% of the over-claims, while under the target-based scheme, the comparable percentage was just 13%. Thus, we must cautiously note that while the target-based scheme resulted in far more cheating overall than either of the other two schemes, individuals nonetheless exist who are willing to cheat very substantially under the other two schemes.

CONCLUSIONS AND IMPLICATIONS

As Warren Buffett once remarked, “Managers that always promise to ‘make the numbers’ will at some point be tempted to *make up* the numbers” (Buffett, 2003). Our experiment provides strong support for this insight. Whether one considers the number of over-claimed words, the number of work/pay periods in which such over-claims occur, or the number of participants who make an over-claim at least once, financially-salient numerical targets produce more cheating than other compensation systems. In particular, as argued by Michael Jensen (2001, 2003), a linear piece-rate system produces significantly less cheating than one based on a prespecified target. Moreover, a tournament compensation system based on relative performance also results in significantly less cheating than a target-based scheme. In addition, as first demonstrated by Schweitzer et al. (2004), cheating is more likely under a target-based scheme the closer a participant is to the target.

In our experimental design, participants were instructed to report accurately the number of words that they had created. Furthermore, before making a decision about how much to report, each participant received anonymous feedback from another participant selected at random. Thus, in contrast to many experiments on tax evasion, participants were placed in a situation where cheating was really cheating rather than the acceptance of a perceived invitation to gamble (see Cadsby et al., 2006 for a discussion of this issue). In addition, we were able to separate instances of active cheating, in which a participant overruled the verdict of an anonymous rater, from instances where a participant simply went along with a beneficial error made by a rater. Both these active cheating data and

the total cheating data strongly support the notion that prespecified numerical targets lead to more cheating than the other compensation schemes.

In our design, there was no chance of being caught. The possibility of an audit was never mentioned throughout the experiment, and no such audit ever occurred. Of course, participants may have imagined that if they were to make outlandish claims, they might be challenged. Indeed if any participant under the piece-rate scheme had claimed to have made more words than our budget could tolerate, we would have been forced to depart from the experimental protocol and check his/her claim.⁷ As mentioned above, one participant made 38 over-claims in the piece-rate treatment. The belief that a ridiculously high claim might be challenged may have kept him/her from going further.

We purposely avoided an explicit audit system in order to examine the role played by the interplay of the utility of the financial rewards that may accrue from cheating with the psychological costs of guilt that may arise from such dishonest anti-social actions. If an audit and punishment system were in place, the costs of this system both to the employer and the employee would have to be considered in addition to the psychological costs of guilt. There is some evidence that the existence of such an audit system might replace in whole or in part the costs of guilt (Gneezy & Rustichini, 2000). The design of an audit system must be considered in conjunction with a particular compensation scheme. For example, an audit system that responds to large self-reports with a higher probability of audit, would likely constrain cheating under a piece-rate or tournament system much more effectively than under a target-based system, in which there is no reason to report selling much more than the prespecified target. Under a target-based system, a random audit of those claiming to have achieved the target would likely be more effective. This is a subject that warrants further study.

⁷ We budgeted for the possibility of large amounts of cheating. However, if somebody had brazenly claimed to have made a million words, we would have been unable to pay.

Another interesting area for further experimental research is to explore whether the existence of an opportunity to cheat affects effort and actual output. As mentioned above, a cheating opportunity might well be viewed as a substitute for effort when the marginal cost of cheating is less than the marginal cost of effort. The extent to which this would actually occur under the various compensation schemes is an empirical issue that requires study.

Michael Jensen's passionate appeal for an end to bonuses based on prespecified numerical targets and their replacement with linear compensation systems cannot be supported by economic theory alone. As we have demonstrated, economic theory is consistent with people cheating either more or less under a target-based than under a linear PFP system. However, Jensen's argument receives strong and unambiguous support from our experimental data. While it is important to take account of other characteristics of compensation systems in addition to their effects on cheating behavior, knowing that the choice of compensation scheme can have an important effect on cheating is clearly an important factor in the design of such systems. Jensen's appeal deserves serious attention.

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Table 1 Comparing Cheating under the Target-based and Linear Piece-rate Settings

Target-based	Linear Piece-Rate					
		no	$\hat{c} < c^*$	$\hat{c} = c^*$	$\hat{c} > c^*$	$\hat{c} \Rightarrow w$
	Yes	b	c	d	e	f
No	a	g	Not possible	h	i	

Table 2a: Cheating Measures Based on Over-Claimed Words per Person

Measures	Treatments		
	1. Piece-rate N=52	2. Target-based N=56	3. Tournament N=50
Productivity (i.e total number of correct words)	41.56	43.78	42.38
Gross over-claimed words	1.52	4.34	1.58
Net over-claimed words	1.42	4.02	1.48
Gross active over-claimed words	1.29	4.07	1.38
Net active over-claimed words	1.25	3.89	1.36
Gross active financial over-claimed words by eligible rounds	0.18	0.50	0.20

Table 2b: Treatment Effects on Cheating Measures Based on Over-Claimed Words per Person (p-values in parentheses)

Measures	T-tests of Differences in Means			Mann-Whitney Tests: z Approximation		
	1 – 2	2 – 3	1 – 3	1 vs. 2	2 vs. 3	1 vs. 3
Productivity	-0.910 (0.365)	0.539 (0.591)	-0.298 (0.766)	-1.53 (0.126)	-0.89 (0.375)	-0.50 (0.620)
Gross over-claimed words	-2.29 (0.024)	2.29 (0.024)	-0.06 (0.953)	-2.53 (0.011)	-2.59 (0.010)	-0.16 (0.872)
Net over-claimed words	-2.15 (0.034)	2.10 (0.038)	-0.06 (0.955)	-1.89 (0.059)	-2.15 (0.031)	-0.522 (0.602)
Gross active over-claimed words	-2.27 (0.026)	2.32 (0.022)	-0.09 (0.925)	-2.63 (0.009)	-2.43 (0.015)	-0.14 (0.889)
Net active over-claimed words	-2.20 (0.030)	2.19 (0.030)	-0.12 (0.908)	-2.32 (0.021)	-2.21 (0.027)	-0.03 (0.976)
Gross active financial over-claimed words by eligible rounds	-1.92 (0.057)	1.94 (0.056)	-0.09 (0.925)	-2.32 (0.020)	-2.10 (0.035)	-0.14 (0.889)

Table 3a: Cheating Measures Based on Over-Claim Rounds per Person

Measures	Treatments		
	1. Piece-rate N=52	2. Target-based N=56	3. Tournament N=50
Gross over-claim rounds	0.62	1.61	0.64
Net over-claim rounds	0.54	1.31	0.54
Gross active over-claim rounds	0.44	1.39	0.50
Net active over-claim rounds	0.42	1.29	0.48
Gross active financial over-claim rounds by eligible rounds	0.06	0.17	0.07

**Table 3b: Treatment Effects on Cheating Measures Based on Over-Claim Rounds per Person
(p-values in parentheses)**

Measures	T-tests of Differences in Means			Mann-Whitney Tests: z Approximation		
	1 – 2	2 – 3	1 – 3	1 vs. 2	2 vs. 3	1 vs. 3
Gross over-claim rounds	-3.09 (0.003)	2.94 (0.004)	-0.11 (0.912)	-2.53 (0.011)	2.55 (0.011)	-0.07 (0.948)
Net over-claim rounds	-2.32 (0.023)	2.20 (0.031)	-0.01 (0.995)	-1.75 (0.081)	1.95 (0.050)	-0.04 (0.675)
Gross active over-claim rounds	-3.12 (0.003)	2.88 (0.005)	-0.30 (0.766)	-2.62 (0.009)	2.44 (0.015)	-0.16 (0.871)
Net active over-claim rounds	-2.96 (0.004)	2.65 (0.010)	-0.30 (0.627)	-2.29 (0.022)	2.19 (0.028)	-0.00 (0.997)
Gross active financial over-claim rounds by eligible rounds	-2.91 (0.005)	2.62 (0.010)	-0.30 (0.766)	-2.46 (0.014)	2.18 (0.003)	-0.16 (0.871)

**Table 4a: Cheating Measures Based on Number of People who Over-Claimed
(Proportions in Parentheses)**

Measures	Treatments		
	1. Piece-rate N=52	2. Target-based N=56	3. Tournament N=50
Gross over-claim	17 (0.33)	30 (0.54)	16 (0.32)
Net over-claim	13 (0.25)	28 (0.50)	16 (0.32)
Gross active over-claim	13 (0.25)	26 (0.46)	13 (0.26)
Net active over-claim	13 (0.25)	26 (0.46)	13 (0.26)
Gross active financial over-claim	13 (0.25)	23 (0.41)	13 (0.26)

**Table 4b: Treatment Effects on Cheating Measures Based on Number of
People who Over-Claimed (p-values in parentheses)**

Measures	Logit Regression Treatment Coefficients		
	1 – 2	2 – 3	1 – 3
Gross over-claim	0.87 (0.030)	-0.90 (0.027)	-0.02 (0.940)
Net over-claim	1.10 (0.008)	-1.05 (0.013)	0.03 (0.908)
Gross active over-claim	0.96 (0.022)	-0.90 (0.031)	0.03 (0.908)
Net active over-claim	0.96 (0.022)	-0.90 (0.031)	0.03 (0.908)
Gross active financial over-claim	0.74 (0.079)	-0.69 (0.104)	0.03 (0.908)

Figure 1

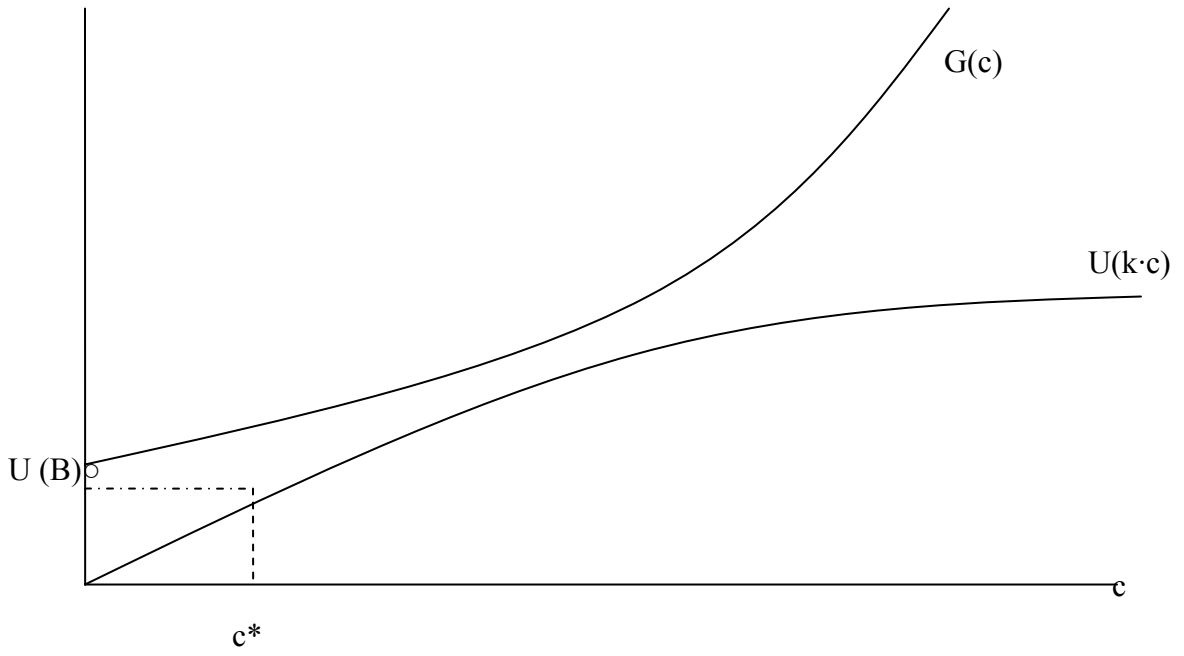


Figure 2

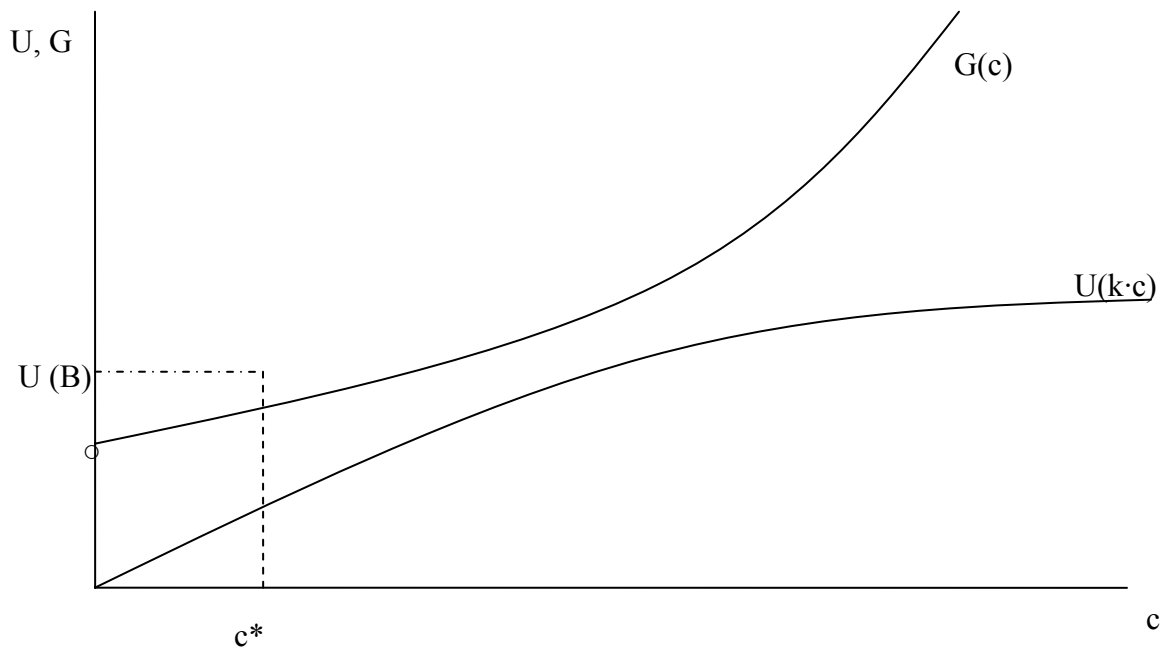


Figure 3

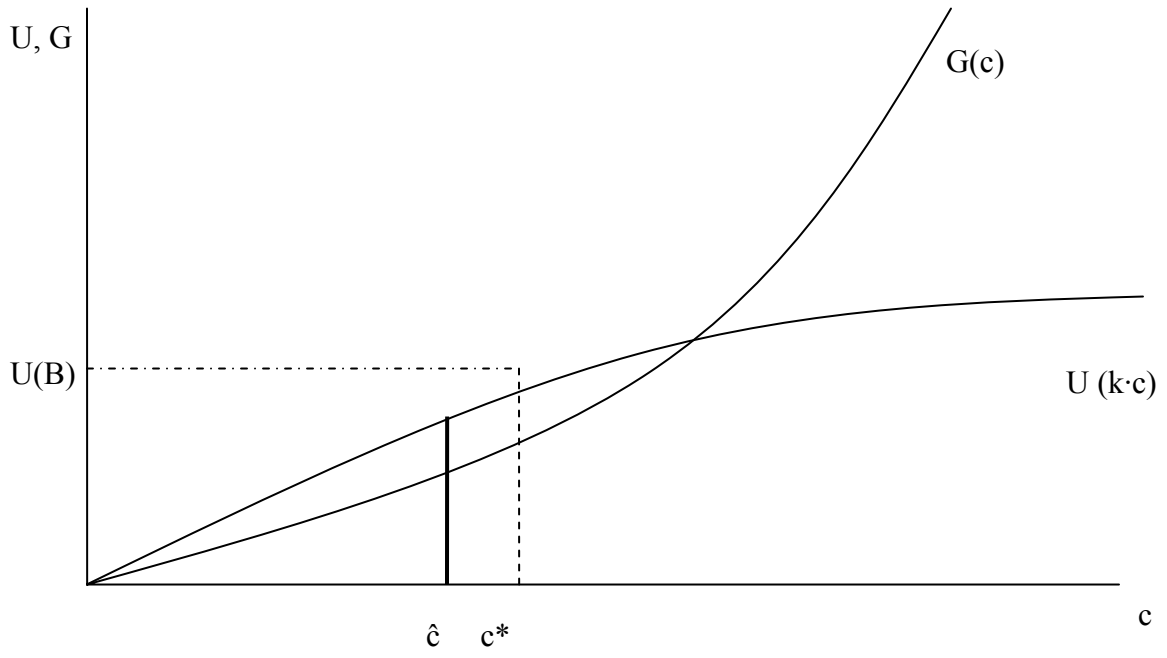


Figure 4

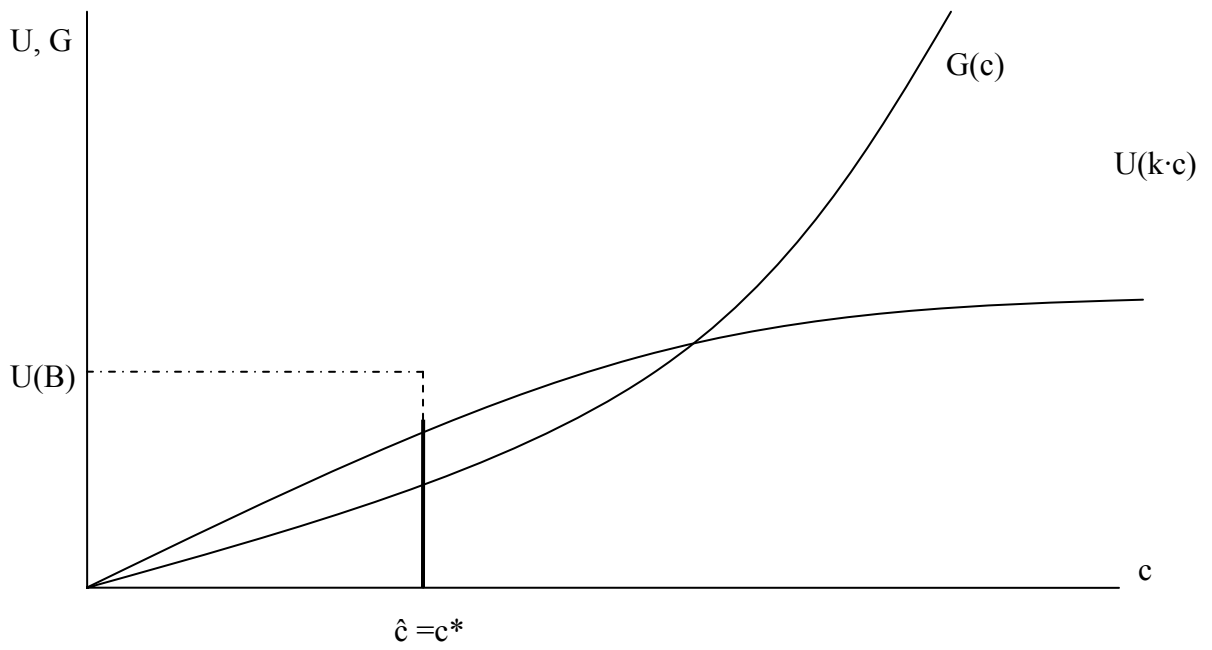


Figure 5

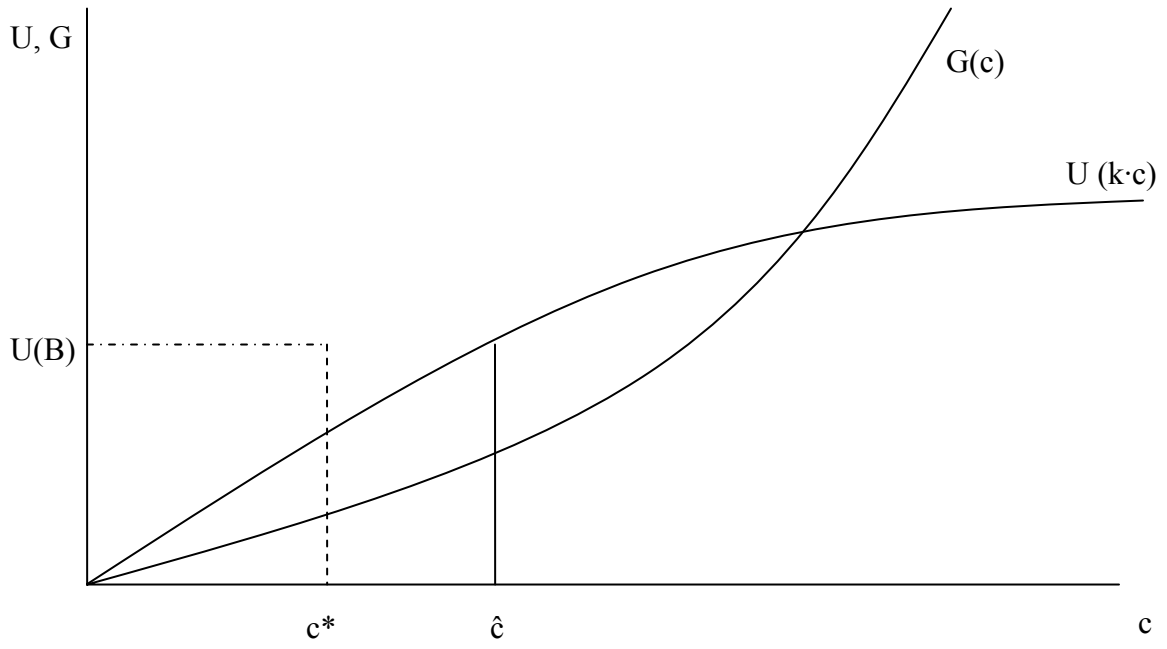


Figure 6

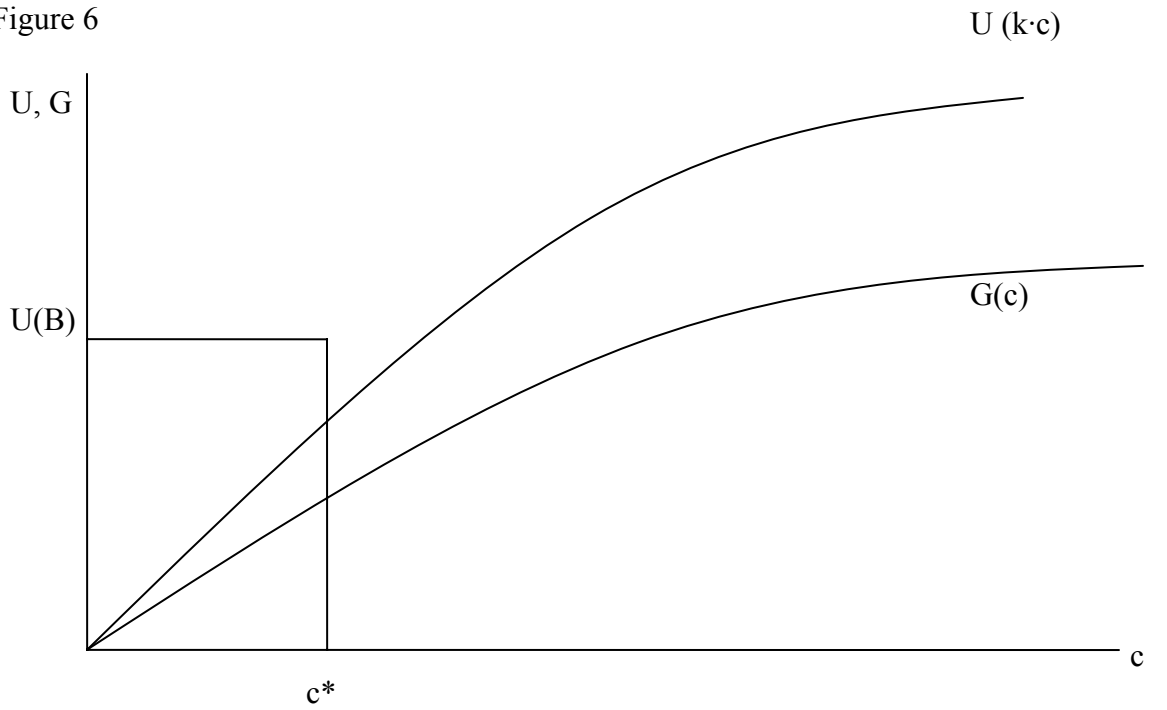


Figure 7

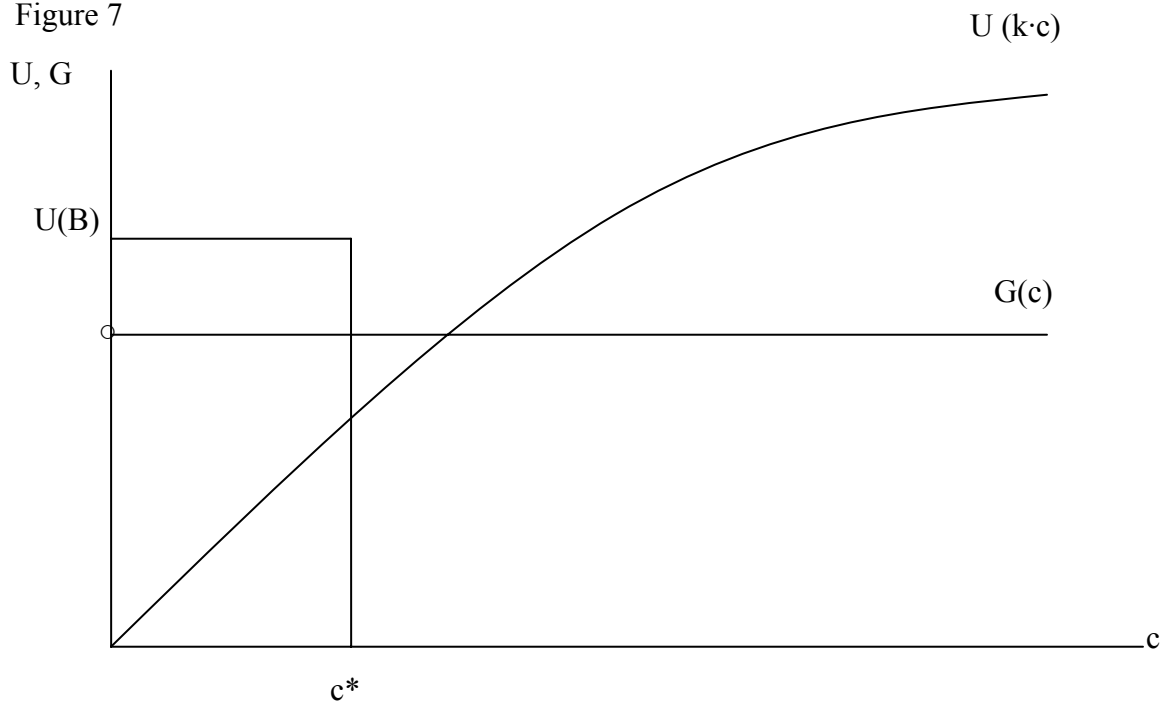


Figure 8

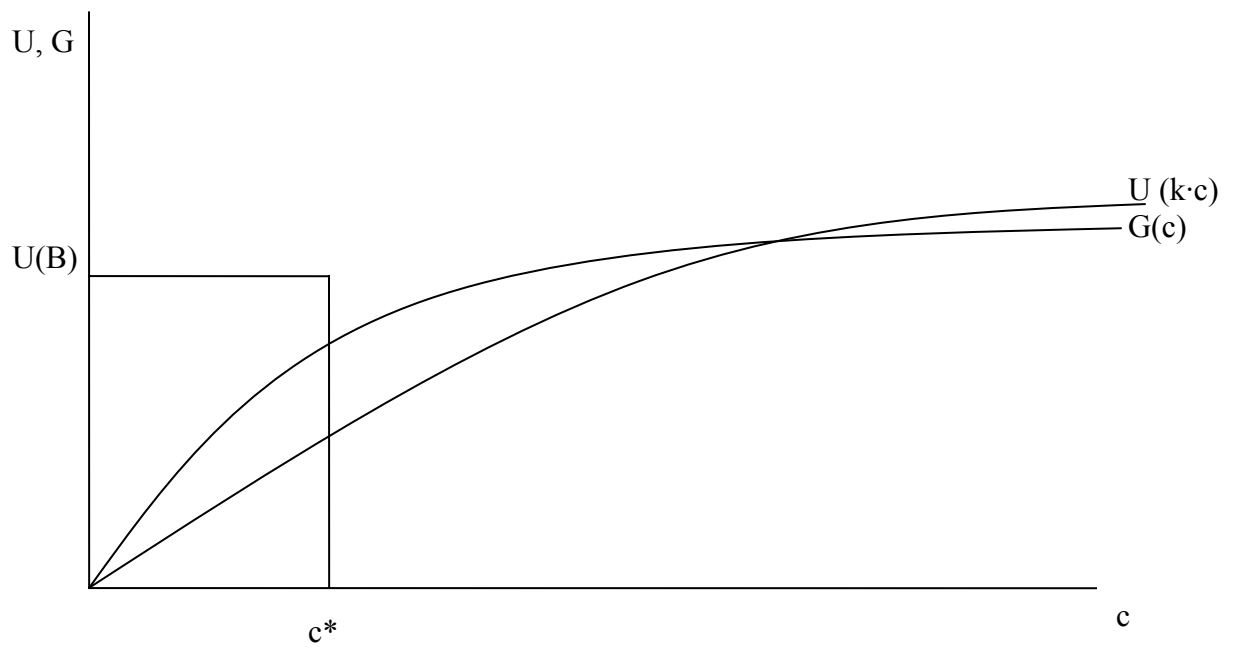


Figure 9

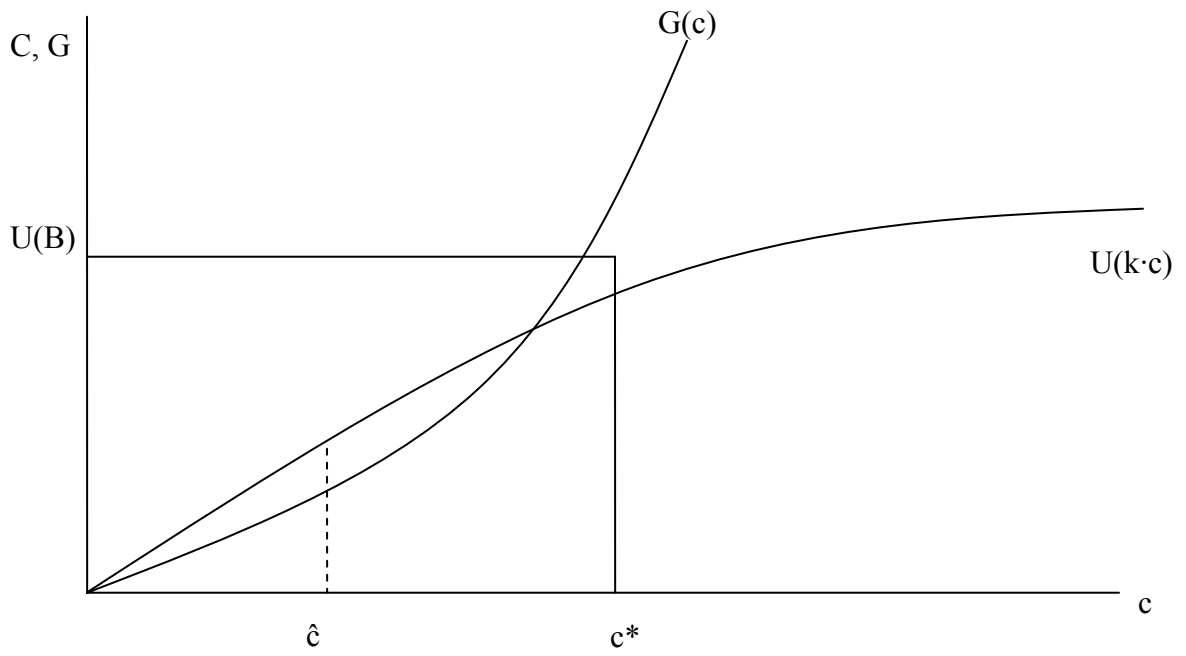


Figure 10

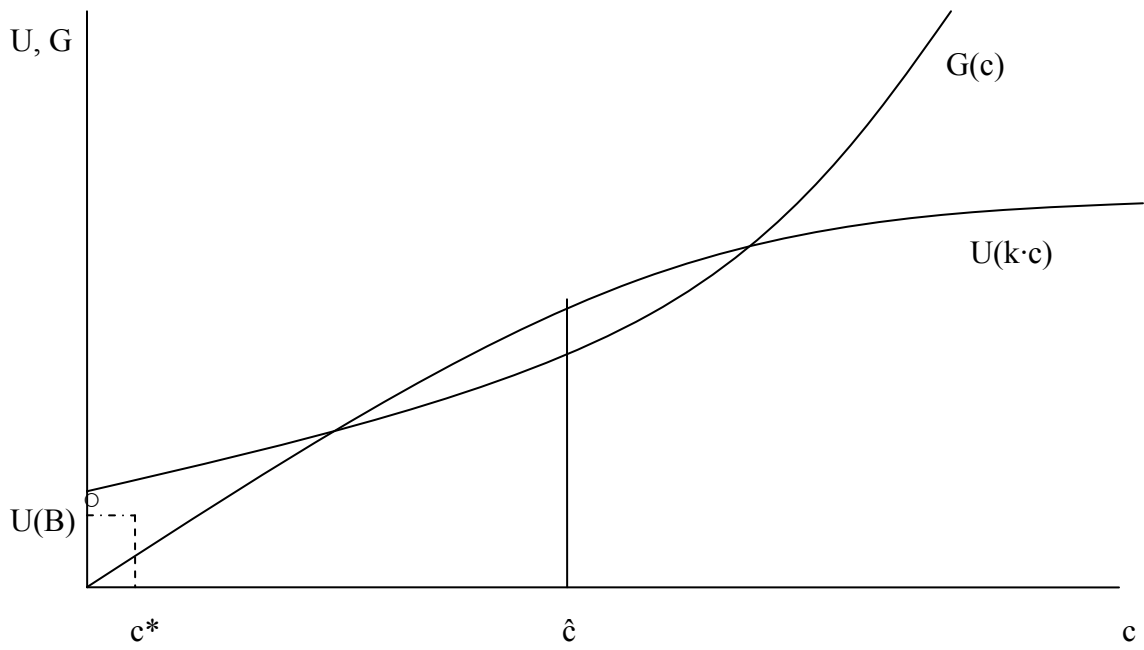


Figure 11

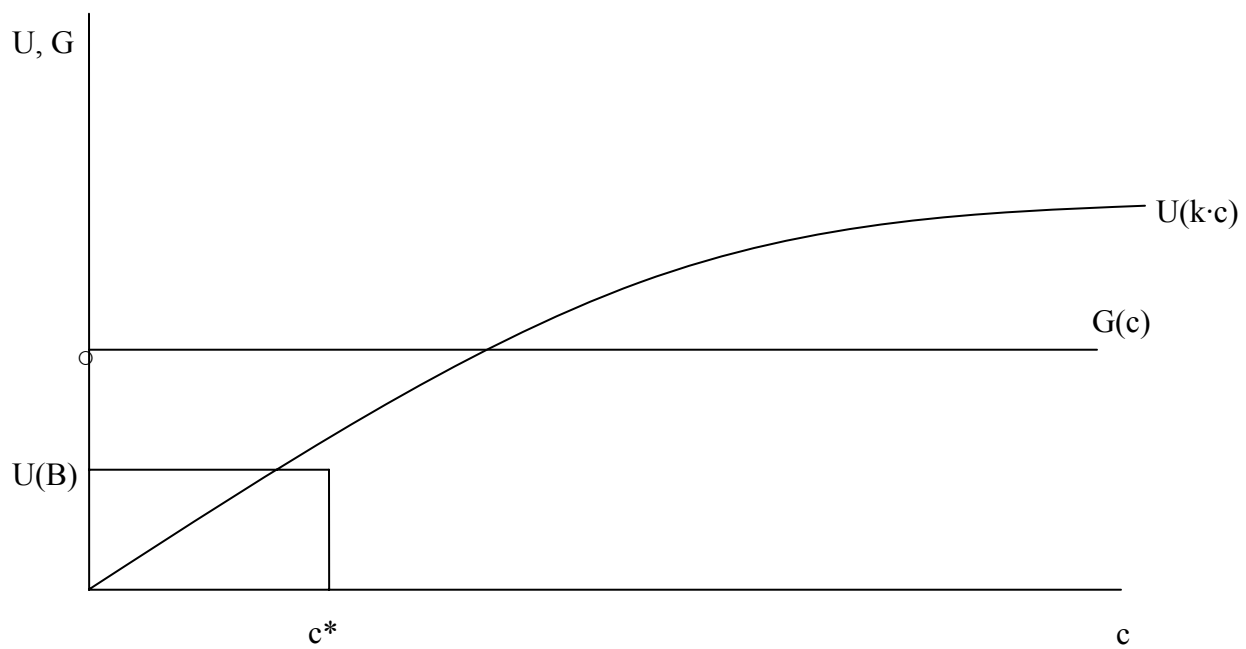


Figure 12

