A General Test for Distortions in Performance Measures

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Abstract: An important lesson from the incentive literature is that performance measures are often distorted, eliciting dysfunctional and unintended responses, also known as gaming responses. The existence of these responses, however, is difficult to demonstrate in practice because this behavior is typically hidden from the researcher. We present a simple model showing that one can test for the existence of distortions by estimating how the association between a performance measure and the true goal of the organization changes after the measure is introduced: the measure is distorted if the association decreases. Using data from a public sector organization, we find evidence consistent with the existence of distortions. We draw implications for the selection of performance measures.

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1 Introduction

A central focus of the economics of the firm is the motivation of workers within organizations. The problem of identifying performance measures that impel workers to take appropriate actions is at the heart of the problem of incentive provision. In the real world, explicit performance measures are frequently distorted, eliciting dysfunctional and unintended responses, also known as gaming responses, which prove costly to organizations (see Holmstrom and Milgrom, 1991 and Baker, 1992, for example). Understanding when a performance measure is distorted, the extent of gaming responses and their nature, is essential to rule out poor incentive designs that could put the organization at risk and also more generally to improve the effectiveness of measurement systems. Gaming behavior, however, is difficult to identify because it is typically hidden from the researcher and in many cases, at least for some time, from the organization as well.

Despite this difficulty, a growing literature has documented the existence of distortions in several organizational contexts. For examples of distorted measures and the damage they cause in the private sector, see the specific references to performance pay and employee misconduct in Baker et al (1994). For an example from the public sector, see Jacob and Levitt (2003). Prendergast (1999) reviews the empirical evidence of distortion in performance measures. Following the seminal work of Healy (1985), this literature has circumvented the identification challenges by focusing on gaming responses where distortions can be unambiguously identified from the specifics of the contract (e.g. manipulating accounting figures). The main shortcoming of this approach is that by its very case-study nature, it can be applied only to a narrow set of gaming responses and it requires detailed information on the contracts and on agent behavior that is often difficult to observe. There is no general method to address the question of whether a performance measure is distorted.

We develop a new approach to identify whether a measure generates distortions. Our starting assumption is that different performance measures generate different sub optimal responses. Although performance measures share in common the feature that they attempt to communicate the

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true organizational goal, they are imperfect proxies and the sources of their imperfections likely differ. The strategies that optimally game a given measure may have little impact elsewhere. We propose to test for distortions by assessing how the performance outcomes change after the relative weight placed by the incentive system on a performance measure changes.[2] A natural application of the model, which matches our case study, occurs when a new performance measure is introduced.

We extend Baker's 2002 model to derive a simple test of distortions that only requires estimating how the association between a performance measure and the principal's objective (value of the organization) changes after a change in the incentive weights. The two statistics of association we use are the Pearson's correlation coefficient and the slope coefficient from a regression of organizational value on the performance measure. We consider these statistics because they have been commonly used in the literature, as we shortly argue, to evaluate performance measures. The term *association* in this paper will be used as shorthand for correlation and regression coefficient. We show that a measure is distorted if this association—either measured as a correlation or by regression—decreases after the relative weight placed on the performance measure increases. The intuition for this test is that after a measure receives more emphasis, the agent takes measure-specific actions that maximize the measure but that do not maximize the true goal. Not only will these actions decrease the covariance between the measure and organizational value, but they also increase the variability of the measure. These two channels imply a reduction in the association between the measure and the true goal.[3]

We test this hypothesis in the incentive system of a federal training organization created under the Job Training Partnership Act (JTPA), which, between 1982 and 2000, provided job training to the economically disadvantaged. There are several reasons for choosing this case study.

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[2] We call the performance measure the rule used to collect and aggregate the data generated by the agent's actions and the performance outcome the value generated when that rule is applied to specific data.

[3] Others have also argued that performance outcomes should change after a measure is activated. For example, Meyer and Gupta (1994) argued that the worthiness of a performance measure degrades after it is activated. The concept of

To start, JTPA used incentive-backed performance measurement, rewarding successful training agency managers with small budgetary increases. In addition, JTPA was the object of a large-scale experimental study that produced unusually precise and complete information on performance outcomes and organizational value. Another important reason for using this case study is that in the late 1980's the Department of Labor introduced new set of performance measures, designed to better reflect JTPA's stated objective to raise the earnings ability and lower the welfare dependency of the poor. For these new measures, we can observe performance outcomes as well as organizational impacts, before and after each measure's introduction. We find that the association between the new performance measures and the objective of the organization decreased after the introduction of these measures.

This paper contributes to two literatures. First, and as mentioned earlier, it contributes to the empirical literature that identifies the existence of distortions. A substantial fraction of the literature focuses on responses where the agent uses its discretion over the timing and reporting of performance outcomes to meet performance thresholds (Healy, 1985; Asch, 1990; Oyer, 1998; Jacob and Levitt, 2002; Oettinger, 2002; Burgess et al., 2002; and Courty and Marschke, 2004). In contrast, our approach offers a more general test that captures a wide class of distortive responses and that only requires computing changes in simple statistical measures of association before and after a change in relative incentive weights. The main advantages of our approach are that it is general and it relies on data that are often easy to collect.

This paper also contributes to the literature on the implementation of performance measurement. An important pre-occupation of the literature is the selection of performance measures (e.g., Gibbs et al, 2004). Researchers in this literature, and practitioners as well, evaluate the usefulness of performance measures based on how associated they are with the objective of the organization. Ittner and Larcker (1998), Banker, Potter, and Srinivasan (2000), and van Praag and

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degradation, however, does not suggest clear statistical predictions for how the measure and organizational value should change.

Cools (2001), for example, use regression methods to evaluate alternative performance measures for managerial compensation plans in the private sector. Much of the recent policy and public administration literature is also concerned with performance measurement, as interest in performance measurement and accountability in the public sector has waxed in recent years (e.g. Heckman, Heinrich, and Smith, 2002). Researchers in these literatures test the validity of performance measures by correlating them with "true" measures of the organization's objective. Measures that appear the most correlated with the objective are deemed most likely to be successful. These methods, however, lack a theoretical justification. By showing that a validation method based on correlation or regression analysis prior to the measure's introduction is flawed because it fails to capture the distortion margins that are not yet revealed, our model contributes to the practice of performance measurement validation and selection. Although one has to be cautious in using correlations or the results from regression analysis to select performance measures, one can use the change in association to test for the existence of distortions after a measure is introduced.

This paper is organized as follows. Section 2 presents a simple framework to test for distortions. Section 3 presents some preliminary evidence consistent with this framework and tests our predictions in the JTPA organization. Section 4 concludes.

2 Model

We adopt the multi-tasking principal agent paradigm (Holmstrom and Milgrom, 1991; Baker, 1992; Feltham and Xie, 1994; and Banker and Datar, 2001) and build upon our previous work (Courty and Marschke, 2003a).[4] In contrast to most of the principal agent literature, which concentrates on how the principal designs the incentive contract, we focus on the agent's effort decision because our goal is to investigate how the agent responds to different sets of performance

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[4] The theoretical literature on the design of incentives is reviewed in Gibbons (1997) and Prendergast (1999). More recently, Dixit (2002) reviews the incentive literature but focusing on those issues that are specific to the public sector.

measures and to derive conditions under which one can identify the existence of measurement distortion from these differences. Although our framework is fairly general, it does raise issues, many of which can be addressed satisfactorily without distracting from the main argument. Other aspects of the model will be more easily discussed after we have exposed our main result.

The empirical tests we derive presume that the researcher has multiple observations of agent performance, or performance outcomes, under different contracts. In addition, we assume that the performance outcomes have been generated in different production environments; that is, the marginal productivity of effort varies across observations. These assumptions hold in many environments where performance measurement has been used. To illustrate, consider an environment where performance is measured on a monthly basis (as for some salespersons who earn commissions based on sales volumes) and the marginal productivity of effort varies seasonally (perhaps due to fluctuations in consumer demand). Alternatively, consider an application to managerial compensation in which the researcher observes accounting performance in a sample of firms that use the same accounting measures. In this case it would be natural to treat each firm as a different production environment. As a final illustration, consider an agent who must complete a fixed number of heterogeneous projects. The researcher observes the agent's performance for each project. Since this corresponds to the interpretation that matches our case study, as we argue shortly, we will present the model in this context, but one should keep in mind that the model applies more broadly, encompassing all of the examples described above, as well as others.

The agent exerts effort on a set of projects $\alpha \in A$, which is assumed, for now, exogenously given. The index α captures the difference in production environments. The agent privately observes each project's type α after signing the contract but before making her effort decisions. In the context of our JTPA case study, for example, a project will be a group of enrollees that share the same demographic characteristics (e.g., race, gender, and educational level---we provide

evidence below that the marginal productivity of different training activities vary across demographic groups).

We propose a fairly general structure for the production environment that is designed to capture several of the problems associated with performance measurement that have received attention in the literature. We assume that the agent's actions on each project influences two performance measures and a measure representing the value of the organization. Following the literature (e.g. Holmstrom and Milgrom, 1991, and Baker, 1992) we assume linear production technologies. The performance outcome for project α on measure i=1,2 is

$$p_{i,\alpha}(e,g)=v_{0,\alpha}e_0+(v_{i,\alpha}+\eta_{i,\alpha})e_i+w_{i,\alpha}g_i,$$

where $(e,g)=(e_0,e_1,e_2,g_1,g_2)$ are the agent's productive efforts and gaming efforts (which the researcher does not necessarily observe). The agent makes choices (e,g) for each project type α. The decisions (e,g) correspond to the concept of state contingent action in Holmstrom and Milgrom (1991, p.30) and Baker (1992, p.601). We define gaming as an effort that is costly but does not enter—or, enters negatively—the principal's objective function. We assume that $v_{i,\alpha}\geq0$, $v_{i,\alpha}+\eta_{i,\alpha}\geq0$, $w_{i,\alpha}\geq0$ and $E\eta_{i,\alpha}=E\eta^3_{i,\alpha}=0$. Our specification ignores additive performance measurement noise. This assumption is not restrictive for the analysis, which focuses on distortion rather than on the optimal weighting of performance measures.[5]

We assume that the costs of e and g are the same across all projects and are respectively $\frac{1}{2}e_i^2$ and $\frac{1}{2}g_i^2$, for i=0,1,2. We explain later that our results generalize to the case where the cost functions depend on the projects' types. The value produced by the agent—or what the principal cares about in our case study—on project α is,[6]

$$V_\alpha(e,g)=v_{0,\alpha}e_0+v_{1,\alpha}e_1+v_{2,\alpha}e_2-(\xi_{1,\alpha}g_1+\xi_{2,\alpha}g_2).$$

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[5] In the standard principal agent model, measurement noise plays a role in the determination of the optimal contract, but it does not directly influence the agent's investment decisions.
[6] If the organization were a firm, V would be market value (Baker 1992). For non-market organizations, we mean for V to represent the contribution of the agent to the "social value" created by the organization.

with $\xi_{1,\alpha}$, $\xi_{2,\alpha} \geq 0$. e_0 captures the dimension of effort that is common to both performance measures and to the organizational objective. In addition, both performance measures imperfectly capture different dimensions of effort (e_1 and e_2) and both have a gaming dimension (g_1 and g_2).

We say that a performance measure is *distorted* if it either imperfectly captures the marginal productivity of effort, i.e. $\eta_{i,\alpha} \neq 0$ for some α, or if the agent can take gaming actions that increase the performance measure but not the objective, i.e. $w_{i,\alpha} > 0$ for some α.[7] The idea of random marginal productivity is that the agent may have incentives to exert good effort, but not in the correct quantities--e.g., if she were a teacher being evaluated by the results of standardized tests, she might spend too much time on tested skills (p. 733, Baker, 2002). Examples of gaming actions in the same context include a teacher providing her students with, or filling in, the answers to the test (Jacob and Levitt, 2002). In the context of our application to job training, Courty and Marschke (2004) present evidence of random marginal productivity in the form of distorted training investments, and of gaming in the form of accounting manipulations that sometimes waste resources and/or that have a negative impact on the objective ($\xi_{1,\alpha}$, $\xi_{2,\alpha} > 0$). We will revisit this work as well as other works in the next section. Although we introduce these dual channels of distortion for the sake of generality, our test will not be able to empirically distinguish the two. For generality, we consider the possibility that $\xi_{1,\alpha}$, $\xi_{2,\alpha} > 0$ but our main prediction also holds if $\xi_{1,\alpha} = \xi_{2,\alpha} = 0$.

Finally, and to simplify, we assume that the marginal productivities $v_{i,\alpha}$, $\eta_{i,\alpha}$, $w_{i,\alpha}$, and $\xi_{i,\alpha}$ are orthogonal to one another. Although simplistic, this assumption is made for tractability and should be interpreted as a first order approximation of a more complex specification. Note that distortions are measure specific. The distortive actions that increase performance measure one leave

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[7] This concept of distortion is different from the concept of congruence or alignment sometimes used in the theory literature to mean $p_\alpha = V_\alpha$ for all α. In our framework, a non-distorted measure is still not aligned because it communicates only one of the two measure specific effort dimensions (e_1, e_2).

performance measure two unchanged and vice versa. This is reasonable as long as the two performance measures are unlikely to share the same weaknesses.[8]

The principal receives only two signals of performance. The performance outcome for measure i is the sum of performance outcomes over all projects

$$P_i = \sum_\alpha p_{i,\alpha}(e_\alpha, g_\alpha).$$

(We use the terminology performance outcome to refer to both the project level outcomes $p_{i,\alpha}$ and the aggregated outcome P_i and the context should clarify which concept we have in mind.) Assume for now that the weights on performance measure one and two are β_1 and β_2, respectively. The agent chooses productive and gaming efforts (e_α, g_α) to maximize

$$\sum_i \beta_i P_i - \tfrac{1}{2} \sum_\alpha (e_{0,\alpha}^2 + \sum_i (e_{i,\alpha}^2 + g_{i,\alpha}^2)).$$

To simplify the exposition, let $(e_\alpha(\beta_1,\beta_2), g_\alpha(\beta_1,\beta_2))$ denote the agent's optimal efforts for project α when the performance weights are β_1 and β_2 respectively, and similarly denote $p_{i,\alpha}(\beta_1,\beta_2)$ and $V_\alpha(\beta_1,\beta_2)$ the performance outcomes and realized value. The agent's effort response is

$$e_{0,\alpha}(\beta_1,\beta_2) = (\beta_1+\beta_2)v_{0,\alpha},$$

$$e_{i,\alpha}(\beta_1,\beta_2) = \beta_i(v_{i,\alpha}+\eta_{i,\alpha}), \text{ for } i=1,2, \quad (1)$$

$$g_{i,\alpha}(\beta_1,\beta_2) = \beta_i w_{i,\alpha}, \text{ for } i=1,2.$$

The agent's choice $(e_\alpha(\beta_1,\beta_2), g_\alpha(\beta_1,\beta_2))$ generates the performance outcome for project α and measure i

$$p_{i,\alpha}(\beta_1,\beta_2) = (\beta_1+\beta_2)v_{0,\alpha}^2 + \beta_i(v_{i,\alpha}+\eta_{i,\alpha})^2 + \beta_i w_{2,\alpha}^2 \qquad (2)$$

The value generated for project α is,

$$V_\alpha(\beta_1,\beta_2) = (\beta_1+\beta_2)v_{0,\alpha}^2 + \sum_i \beta_i\left(v_{i,\alpha}(v_{i,\alpha}+\eta_{i,\alpha}) - \xi_{i,\alpha}w_{i,\alpha}\right) \quad (2')$$

The realized performance outcome and value depend on the agent's efforts, which in turn depend on which measure is activated and on the performance weights.

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[8] We could have assumed that both measures also share a common dimension of distortion (either through a common random marginal productivity of effort $(v_{0,\alpha}+\eta_{0,\alpha})e_{0,\alpha}$ or through common gaming investment $w_{0,\alpha}g_{0,\alpha}$) but since this

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The central assumption of the model is that different measures are likely to display different distortionary weaknesses. As we will see, conditional on this assumption it is possible to identify the existence of (measure specific) distortions. The current model makes several simplifying assumptions about the cost and production technologies. Some of these assumptions are standard while others were made for ease of exposition. For example, we have ignored the possibility that the agent could select projects. We will return to this assumption as well as other aspects of the model after exposing the main result.

2-1 Change in the Relative Weight of a Performance Measure

Organizations often change the weights on the performance measures they use, and sometimes change the set of performance measures, replacing outdated ones, or augmenting old measures with new ones as they become available. Assume that the principal changes the weights on performance measure one and two from (β_1, β_2) to (β_1', β_2'). We have in mind that the principal could either introduce performance measure two in the second stage $(\beta_2' > \beta_2 = 0)$ or just increase the weight on performance measure two $(\beta_2' > \beta_2 > 0)$. For now, we assume that the agent (myopically) maximizes the performance award in each measurement regime. This assumption is valid if the agent's early actions do not influence the decision to change the performance weights, an assumption that holds in our case study, as we argue below.

Assume that the researcher can only observe $V_\alpha(\beta_1, \beta_2)$ and $p_{2,\alpha}(\beta_1, \beta_2)$ for $\alpha \in A$ before and after the change in performance weights and can compute population statistics (e.g. $EV_\alpha(\beta_1, \beta_2)$ and $Cov(V_\alpha(\beta_1, \beta_2), p_{2,\alpha}(\beta_1, \beta_2))$. We ask the following question: Is it possible to establish whether

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common effect can be picked up in our test only under strong functional form assumptions, we ignore it to keep the exposition simple.

performance measure two is distorted—that is, whether $\eta_{2,\alpha} > 0$ or $w_{2,\alpha} > 0$—based on simple

statistics involving $V_\alpha(\beta_1,\beta_2)$ and $p_{2,\alpha}(\beta_1,\beta_2)$?[9]

Expressions (2) suggests a prediction on how the mean of a performance measure should

change after its weight increases. If overall incentive weights do not decrease, $\beta_1'+\beta_2' \geq \beta_1+\beta_2$, the

mean performance outcome on measure two should increase when β_2 increases.

$$E\, p_{2,\alpha}(\beta_1',\beta_2') = (\beta_1'+\beta_2')Ev_{0,\alpha}^2 + \beta_2' E(v_{2,\alpha}+\eta_{2,\alpha})^2 + \beta_2'\ Ew_{2,\alpha}^2 > E\, p_{2,\alpha}(\beta_1,\beta_2)\,.$$

Similarly, the variance of measure two should also increase,

$$Var\, p_{2,\alpha}(\beta_1',\beta_2') = (\beta_1'+\beta_2')^2 Varv_{0,\alpha}^2 + \beta_2'^2\, Var(v_{2,\alpha}+\eta_{2,\alpha})^2 + \beta_2'^2\, Varw_{2,\alpha}^2 > Var\, p_{2,\alpha}(\beta_1,\beta_2)\,.$$

An increase in the mean or variance is consistent with distortion but it is also consistent with an

increase in measure specific productive effort (e_2, in this case). Stated formally, one would also

expect the mean and the variance of measure two to increase even if $\eta_{2,\alpha} = w_{2,\alpha} = 0$. This implies

that the evidence of a performance outcome increase or an increase in the variance of the outcome

is not sufficient to conclude that distortion takes place.

The covariance between $p_{2,\alpha}$ and V_α can provide a more conclusive test.

$$Cov(\,p_{2,\alpha}(\beta_1,\beta_2),V_\alpha(\beta_1,\beta_2)) = (\beta_1+\beta_2)^2 Varv_{0,\alpha}^2 + \beta_2^2 Cov((v_{2,\alpha}+\eta_{2,\alpha})^2,v_{2,\alpha}(v_{2,\alpha}+\eta_{2,\alpha}))$$
$$-\beta_2^2 Cov(\xi_{2,\alpha}w_{2,\alpha},w_{2,\alpha}^2)$$

The first term is clearly positive. As shown below, the second term is positive and the third one is

negative.

$$Cov((v_{2,\alpha}+\eta_{2,\alpha})^2,v_{2,\alpha}(v_{2,\alpha}+\eta_{2,\alpha})) = Varv_{2,\alpha}(v_{2,\alpha}+\eta_{2,\alpha})+E\eta_{2,\alpha}^2 v_{2,\alpha}(v_{2,\alpha}+\eta_{2,\alpha}) \geq 0$$
$$-Cov(\xi_{2,\alpha}w_{2,\alpha},w_{2,\alpha}^2) = -E\xi_{2,\alpha}E[(w_{2,\alpha}-Ew_{2,\alpha})^2(w_{2,\alpha}+Ew_{2,\alpha})] \leq 0\,.$$

Since the covariance decreases only if $w_{i,\alpha} \neq 0$ and $\xi_{i,\alpha} \neq 0$ for some α, finding that the covariance

decreases would constitute evidence of gaming. This test of distortion, however, can identify only

gaming responses—it cannot identify distortions due to random marginal productivity—and can be

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[9] Note that it could be possible in principle to identify the existence of distortions using information at the project level. We prefer to use population statistics to identify distortions because this is likely to generate more robust tests based on

used only in environments where gaming effort can reduce the organization's value, i.e. when $E\xi_{2,\alpha} > 0$. To derive more general predictions, one has to isolate distortive responses from the responses that have to do with measure specific productive effort. The following proposition uses the fact that $Var(p_{2,\alpha})$ and $Cov(p_{2,\alpha}, V_\alpha)$ respond differently to a change in the relative weight.

Proposition 1: (i) Measure two is distorted if and only if $Cov(p_{2,\alpha}, V_\alpha)/Var(p_{2,\alpha})$ decreases after an increase in β_2/β_1. (ii) If measure two is not distorted, then $Corr(p_{2,\alpha}, V_\alpha)$ increases when β_2/β_1 increases. (The proof is presented in the Appendix)

Claim (i) has a nice interpretation since $Cov(p_{2,\alpha}, V_\alpha)/Var(p_{2,\alpha})$ corresponds to the regression coefficient of V_α on $p_{2,\alpha}$. The intuition for this result is that if $Cov(p_{2,\alpha}, V_\alpha)/Var(p_{2,\alpha})$ decreases it has to be that $Var(p_{2,\alpha})$ increases more (in proportional terms) than $Cov(p_{2,\alpha}, V_\alpha)$ and this implies that some distortions have to have taken place. Claim (ii) implies that evidence of a change in correlation is not always as conclusive. Whether the correlation between measure two and the organization's objective increases or decreases depends on the relative impact of gaming and measure specific productive effort. Here it is worth considering two benchmark cases. Consider first the case where there are no distortion margins ($w_{i,\alpha}=\eta_{2,\alpha}=0$ for all α). In this case, the correlation increases because the agent exerts more measure specific productive effort. This increases the predictive power of the measure: the correlation between V_α and $p_{2,,\alpha}$ increases. Consider next the case where there is no measure specific effort ($v_{2,\alpha}=\eta_{2,\alpha}=0$ for all α) and assume $\xi_{1,\alpha}=\xi_{2,\alpha}=0$ to rule out the possibility that a decrease in the correlation is due to a decrease in the covariance. A sufficient condition for the correlation between measure two and value to decrease when β_2/β_1 increases is

$$Var\, w_{2,\alpha}^2 > \frac{\beta_1}{\beta_2}\, Var\, v_{1,\alpha}(v_{1,\alpha}+\eta_{1,\alpha}).^{10} \quad (3)$$

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aggregated information, and also because it relates to the previous literature on the use of association measures to validate performance measures.

[10] A proof is presented in the appendix.

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The intuition is that the increase in β_2/β_1 increases the noisiness of measure two and therefore decreases its predictive power.

Clearly, $\xi_{2,\alpha}>0$ introduces a force that moves the measure and organizational value in opposite directions therefore reducing the covariance between the two. Even in the absence of that force ($\xi_{2,\alpha}=0$), however, Proposition 1 still holds. In addition, Proposition 1 holds for any increase in β_2/β_1. In particular, it follows when a performance measure is introduced. This suggests a strategy for identifying distortions that does not necessitate the exact knowledge of the incentive weights, which are often difficult to obtain. The researcher only needs to know when a performance measure is activated.

2-2 Discussion

Agent's private information

An implicit assumption of the model is that the index α captures both the agent's private information on the production environment and also the unit of observation to compute population statistics. This assumes that the researcher and the agent observe the same set of projects. (The agent is still better informed since she observes $v_{i,\alpha}$, $\eta_{i,\alpha}$, and $w_{i,\alpha}$ while the researcher does not.) In practice, however, the agent will be able to condition her effort decisions on a richer information set than what the researcher observes. In the context of our application to a job training program, for example, we use the demographic characteristic of trainees to define the project population A. There are good reasons to believe that the marginal productivity of effort varies across demographic groups as we argue next. But it is also likely that the agent observes additional information on enrollees within a given demographic group. Assume the researcher does not observe α but observes a coarser information partition, with representative element $\gamma \subset A$. The researcher observes $E[p_{i,\alpha}(\beta_1,\beta_2)|\gamma]$ and $E[V_\alpha(\beta_1,\beta_2)|\gamma]$ where $E[.|\gamma]$ is the expectation conditional on information γ. Taking conditional expectation in expression (2)

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$$E[p_{i,\alpha}(\beta_1,\beta_2)\,|\,\gamma] = (\beta_1 + \beta_2)E[v_{0,\alpha}^2\,|\,\gamma] + \beta_i E[(v_{i,\alpha} + \eta_{i,\alpha})^2\,|\,\gamma] + \beta_i E[w_{2,\alpha}^2\,|\,\gamma]$$

and similarly for expression (2'). Obviously, $E[p_{i,\alpha}(\beta_1,\beta_2)|\gamma]$ varies less than $p_{i,\alpha}(\beta_1,\beta_2)$. Still, as long as distortion incentives vary within the coarser information set (Var $E[\eta^2_{i,\alpha}|\gamma] \neq 0$ and/or Var $E[w^2_{i,\alpha}|\gamma]\neq0$) the proof of Proposition 1 follows under additional, minor assumptions and it is possible to identify distortions although one is more likely to wrongly reject the hypothesis that there are no distortions.[11] To illustrate, consider the extreme case where the distributions of $\eta_{i,\alpha}$ and $w_{i,\alpha}$ are independent of γ.[12] The researcher observes multiple observations on performance outcomes but the production environment does not vary across observations. A correlation measure, for example, is degenerated (equal to 1) before and after the change in the weight independently of the existence of distortions.

Unobserved heterogeneity: Marginal cost

Our specification considers only one source of unobserved heterogeneity: variations in privately observed marginal productivities of effort. There could also be heterogeneity in costs that could be privately observed by the agent. One can show that Proposition 1 generalizes to unobserved heterogeneity in the marginal costs. Assume for example that the cost of effort $e_{0,\alpha}$ is $\tfrac{1}{2}c_{0,\alpha}e_{0,\alpha}^2$ and the other costs functions are similarly defined with random marginal costs $c_{e,i,\alpha}$ and $c_{g,i,\alpha}$. The expression for $p_{i,\alpha}$ and V_α in (2) and (2') can be written as

$$p_{i,\alpha}(\beta_1,\beta_2) = (\beta_1 + \beta_2)\tilde{v}_{0,\alpha}^2 + \beta_i(\tilde{v}_{i,\alpha} + \tilde{\eta}_{i,\alpha})^2 + \beta_i\tilde{w}_{2,\alpha}^2$$

$$V_\alpha(\beta_1,\beta_2) = (\beta_1 + \beta_2)\tilde{v}_{0,\alpha}^2 + \sum_i \beta_i\left(\tilde{v}_{i,\alpha}(\tilde{v}_{i,\alpha} + \tilde{\eta}_{i,\alpha}) - \tilde{\xi}_{i,\alpha}\tilde{w}_{i,\alpha}\right)$$

where

$$\tilde{v}_{0,\alpha} = \frac{v_{0,\alpha}}{\sqrt{c_{0,\alpha}}}, \quad \tilde{v}_{i,\alpha} = \frac{v_{i,\alpha}}{\sqrt{c_{e,i,\alpha}}}, \quad \tilde{\eta}_{i,\alpha} = \frac{\eta_{i,\alpha}}{\sqrt{c_{e,i,\alpha}}}, \quad \tilde{w}_{i,\alpha} = \frac{w_{i,\alpha}}{\sqrt{c_{g,i,\alpha}}}, \quad \tilde{\xi}_{i,\alpha} = \frac{\xi_{i,\alpha}}{\sqrt{c_{g,i,\alpha}}}$$

Proposition 1 follows.

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[11] Sufficient conditions for the proposition to hold are $E[E(\eta_{i,\alpha}|\gamma)E(\eta^2_{i,\alpha}|\gamma)] = 0$ (instead of $E\eta^3_{i,\alpha}=0$ before) and $Cov(E(w_{2,\alpha}\,|\,\gamma), E(w^2_{2,\alpha}\,|\,\gamma)) \geq 0$ (we had before $w_{2,\alpha}\geq0$ imply $Cov(w_{2,\alpha}, w^2_{2,\alpha}) \geq 0$.

Unobserved heterogeneity: Project selection

Another source of unobserved heterogeneity emerges if the agent can select the projects on which she exerts effort. Our assumption that the set of projects A was exogenously given previously ruled this kind of unobserved heterogeneity out. To illustrate the possibility of endogenous project selection, assume there is an additional additive term on the production technologies, $u_{i,\alpha}$, that is both measure and project dependent,

$$p_{i,\alpha}(e,g)=u_{i,\alpha}+v_{0,\alpha}e_0+(v_{i,\alpha}+\eta_{i,\alpha})e_i+w_{i,\alpha}g_i$$

$$V_\alpha(e,g)= u_{0,\alpha}+v_{0,\alpha}e_0+v_{1,\alpha}e_1+v_{2,\alpha}e_2-(\xi_{1,\alpha}g_1+\xi_{2,\alpha}g_2)$$

and assume that the agent can disregard some projects (e=g=0). If the weight on measure i increases, the agent will not select projects with large negative $u_{i,\alpha}$. As a result, the performance outcome should increase. In addition, the set of projects observed by the researcher will change. Whether project selection changes the predictions stated in Proposition 1 depends on the joint distribution of the random coefficients $u_{i,\alpha}$, $v_{i,\alpha}$, $\eta_{i,\alpha}$, $w_{i,\alpha}$, and $\xi_{i,\alpha}$. Consider first the benchmark case where $u_{i,\alpha}$, $v_{i,\alpha}$, $\eta_{i,\alpha}$, $w_{i,\alpha}$, and $\xi_{i,\alpha}$ are jointly independent. Then proposition 1 still holds even if the agent selects projects. Although the additive fixed effect influences the set of projects observed by the researcher, Proposition 1's predictions do not change because the selection rule is independent of the marginal productivities. The intuition is that even after having selected the most attractive projects, the agent still has an incentive to take advantage of the distortion margins that these projects offer. If such margins exist, the agent effort and gaming choices should be revealed in our test of distortion.

Under the independence assumption, selection and effort choices are independent issues that can be addressed separately. It should be recognized, however, that our test of distortion does not shed any light on the possibility that the change in the relative weights may increase the amount of inefficiencies that are due to perverse selection incentives. We acknowledge that such changes in

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[12] In this extreme example, γ is a random variable independent of α.

the selection rule may have important welfare impacts but this is not the focus of this paper. In the context of our case study, the selection issue has already received much attention in the literature (for example, the cream skimming literature is summarized in Heckman, Heinrich, and Smith, 2002).

If the projects fixed effects $u_{i,\alpha}$ are correlated with the marginal productivity of effort $(v_{0,\alpha}, v_{1,\alpha}, v_{2,\alpha}, \xi_{2\alpha})$ then Proposition 1 may not hold anymore. There is no a priori reason, however, why $u_{i,\alpha}$ should vary in systematic ways with $v_{i,\alpha}$, $\eta_{i,\alpha}$, $w_{i,\alpha}$, and $\xi_{i\alpha}$. In our application, for example, results from Heckman, Smith and Clements (1997) and Heckman, Heinrich, and Smith (2002) suggest no relation between the level of human capital $(u_{i,\alpha})$ and human capital impact $(v_{0,\alpha}, v_{1,\alpha}, v_{2,\alpha})$.[13] As a final comment, note that even in the case where the random coefficients are not jointly independent, Proposition 1 still holds for the subset of projects that are selected under both regimes (β_1, β_2) and (β_1', β_2').

Efficiency loss due to distortions

Define the efficiency loss due to distortions as the difference in the organizational value when $\eta_{i,\alpha} = w_{i,\alpha} = 0$ and when $\eta_{i,\alpha} \neq 0$, $w_{i,\alpha} \neq 0$, holding the incentive weights constant and assuming that the agent chooses the optimal effort responses (e_α, g_α) according to (1). Consider the simple case where gaming does not enter organizational value negatively, that is $\xi_{1,\alpha} = \xi_{2,\alpha} = 0$ and assume that measure one is not distorted. The expected loss due to the distortions in measure two is

$$-\frac{1}{2}\beta_2^2\left(E\eta_{2,\alpha}^2 + Ew_{2,\alpha}^2\right).$$

To estimate the efficiency loss of distortions, one needs to know the second moments of $\eta_{2,\alpha}$ and $w_{2,\alpha}$. Inspection of the expressions of $Ep_{2,\alpha}(\beta_1,\beta_2)$ and $EV_\alpha(\beta_1,\beta_2)$ reveals that the cost of distortions can be expressed as

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[13] Only for individuals within the lower 20th percentile of earnings levels did they find that higher earnings levels imply higher earnings impacts. But this suggests a positive correlation between $u_{i,\alpha}$ and $v_{0,\alpha}$, $v_{1,\alpha}$, $v_{2,\alpha}$, which imply that our test would underestimate distortions.

$$-\frac{1}{2}\beta_2^2\left(E\eta_{2,\alpha}^2 + Ew_{2,\alpha}^2\right) = -\frac{1}{2}\beta_2\left(\left(Ep_{2,\alpha}(\beta_1,\beta_2) - Ep_{2,\alpha}(\beta_1,0)\right) - \left(EV_\alpha(\beta_1,\beta_2) - EV_\alpha(\beta_1,0)\right)\right).$$

The usefulness of this result is limited to the extent that it rests on many functional form assumptions.

2-3 Summary

The model makes several predictions on how the first and second moments of a performance outcome should change after the performance weights change. These predictions rest on different scenarios on the changes in the weights and address different types of distortions. To recap, if the relative weight on measure two increases then:

(a) Measure two is distorted (i) if and only if $Cov(p_{2,\alpha},V_\alpha)/Var(p_{2,\alpha})$ decreases, and (ii) if the correlation between the performance outcome and organizational value decreases.

If the relative weight on measure two increases and the sum of the weights does not decrease then:

(b) The variance in performance outcome should increase.

(c) The average performance outcome should increase.

(d) The covariance between the performance outcome and organizational value could increase or decrease and the latter occurs only if gaming enters negatively organizational value ($E\xi_{2,\alpha} > 0$).

Predictions (b-c) should hold if the assumptions of the model are valid. In particular, finding evidence consistent with these predictions would provide support to the hypothesis that the agent responds to the incentive system. Prediction (a) offers a new way to test for the existence of distortions. Finding evidence that the correlation (or covariance divided by the variance of the measure) decreases is consistent with the hypothesis that the agent responds to incentives and with the hypothesis that some of these responses are distortive. Prediction (d) also suggests a test of distortions but it holds only for gaming actions that enter negatively organizational value (it

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excludes gaming actions that do not enter organizational value and distortions due to random marginal product). Finally, prediction (b-d) require information on absolute and relative weights while prediction (a) requires information on relative weights only. In particular, prediction (a) always holds if one considers the introduction of a new measure.

3   Empirical Application to a Government Job Training Program

We investigate predictions on (a-d) with a focus on (a). Before proceeding, we present background evidence to demonstrate the validity of our case study. First, we review the training program literature that has computed correlations as well as regression estimates to validate performance measures in similar environments as the one we consider. This provides a preliminary assessment of the model. Second, we argue that the assumptions of the model hold in our case study.

3.1 Literature Review and Preliminary Evidence

Job training programs that serve the economically disadvantaged have been an important part of the federal government's war on poverty at least since the Kennedy administration. In the 1970s several influential studies showing the ineffectiveness of federal job training efforts prompted Congress to reconsider how job training programs were constituted. Beginning with the Job Training Partnership Act (JTPA) of 1982 and continuing under the legislation that supplanted JTPA, the Workforce Investment Act (WIA) of 1998, the bureaucracy that runs the federal government's most important job training program for the poor has become highly decentralized. Since the early 1980's, training has been conducted by over 600 local job training agencies (the agent in the model) each enjoying substantial discretion over who they enroll and what types of training they provide. By allowing this discretion, Congress hoped that job training administrators would be free to use their expertise in training and their superior knowledge of "conditions on the ground" to provide better training. But in increasing administrators' discretion over their work,

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Congress anticipated that administrators would also have greater means to pursue private objectives. Therefore, in addition to allowing more freedom in decision-making, Congress sought to provide stronger incentives to promote programmatic objectives by linking financial incentives to measures of program outcomes. Thus, since JTPA's passage, training agency budgets have been partly contingent on their performance on explicitly defined measures. These measures were comprised of variants of program participants' employment and wage rates measured at or shortly after the participants' "graduation" from their training.

Performance measure validation literature

JTPA's stated goal was to promote increases in the employment and earnings of enrollees (JTPA, Section 106(a)). Numerous studies have attempted to test the ability of short-term outcome-based measures, such as those of JTPA, to capture long-term earnings and employment gains of enrollees.   These studies have been conducted using job training data from JTPA, but also from other job training programs that had not been subject to performance-based measurement.

Gay and Borus (1980), Friedlander (1988) and Zornitsky, et al. (1988) conducted their studies of the association of short-run outcomes and long-term employment and earnings gains based on data from job training programs that had no explicit performance measurement backed by financial incentives.[14] Gay and Borus regressed V (earnings impacts) on various measures of P. For those P that they constructed to look like JTPA's performance measures, they found no positive and significant estimates of the coefficient on P.   Friedlander used sample correlations of P and V to assess P's validity. He found that employment rates were correlated with measures of earnings impacts in most cases. Zornitsky et al. tested a wide range of JTPA-like performance measures and reported that enrollees who were likely to produce high scores on employment-based performance measures were also likely to generate high earnings impacts. They used statistical correlation to

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[14]   Each of these studies measure impact at the subgroup level (α in V represents a demographic subgroup) and performance outcome at the individual level (α in P represents an individual).

establish the positive association between various P measures and V. They also used R-squareds from regressions of V on P to rank different measures by their predictive power.

In their studies based on data generated from JTPA, Heckman, Heinrich, and Smith (2002) and Barnow (2000), however, found little evidence of a relationship between performance measures and earnings impacts. Heckman, Heinrich, and Smith found that the performance measures "are weakly and sometimes perversely, related to long-term impacts" (Heckman et al, p. 778). As we do in this paper, Heckman et al. related performance and measures of organizational value at the enrollee subgroup level. Also like us, they used regression analysis to test the association between P and V. Barnow's unit of analysis was the training agency. He used simple correlation and rank correlation tests to evaluate the association of P and V.

An important implication of our model is that the association between the performance measure and the objective of the organization is endogenous. That is, because placing incentives on performance measures cause agents to find low cost strategies that raise the performance measure and that do not also raise the objective of the organization, the association between the performance measure and the objective of the organization degrades after the measure's introduction. What is interesting about the above-cited studies for the purpose of our study is that only in programs where performance is uncompensated (Friedlander and Zornitsky et al) have researchers found statistically significant correlation between short-term performance measures and impacts.

Of course this observation is not definitive because we compare studies that are based on different programs and on different methodologies. Some of these studies construct their measures of job training success using data from social experiments, while others construct them by comparing the labor market outcomes of persons who obtained training to outcomes of persons from an artificially constructed control group. An analysis using a consistent methodology and data from a single program subject to exogenous variation in performance measures in an organizational environment that is in other ways unchanging would be more definitive. We describe such an analysis next.

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3.2 Application to JTPA

By meeting the numerical standards corresponding to a set of performance measures, training agencies were eligible to win a monetary award. In the early years of JTPA, performance measures were based on an enrollee's employment status at the date the enrollee officially graduated from the program. A training agency's performance outcome for a given fiscal year was computed as the *average* outcome over all enrollees graduated over the course of the year. One measure that was important during the early part of JTPA (and through the period of our analysis) was computed as the fraction of enrollees who were employed on the dates of their graduation. If the training agency's year-end employment rate at graduation exceeded the standard for that measure, the training agency would win an award.[15]

In the late 1980s, the U.S. Department of Labor (DOL) began to change the set of performance measures used to evaluate training agency performance. DOL formulated new measures that rewarded training agencies for employment rates measured not on the graduation date, but on the date ninety days following the graduation date. Other measures were based on an enrollee's employment history over the entire duration of the 90 days following graduation. Table 1 defines the new performance measures based on the ninety day follow-up period. By adding measures that captured labor market outcomes further removed from job training, DOL hoped to encourage training agencies to offer more substantive training that would produce longer-lasting impacts on enrollees' skills. DOL required states to implement these new measures, but gave states some leeway in how quickly they were added. Thus, different states made these transitions in different years.[16] We use this variation in the years of implementation as a natural experiment.

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[15] The award for the successful training agency averaged about seven percent of its budget. In some states, the highest awards amounted to about sixty percent of the training agency's budget. The reader who is interested in the details of the incentives confronting JTPA training agencies should see Courty and Marschke (2003b).

[16] See Courty and Marschke (2003b) for a description of the performance measures, incentive system, and the reasons for the changes in the performance measures in these years. Courty and Marschke also detail the timing of the performance measure changes by state.

Although the absolute weights for several measures have changed in our sample we focus on the addition of the follow-up measure for two reasons. First, this is the only set of measures for which we are sure that the relative weight increases. Since the weight on these measures was zero prior to activation, this weight relative to other measures has to increase after activation, and this statement holds independently of the changes on the weights of other measures. Second, most states have introduced the follow-up measures at some point in our sample while the change on the weights of other measures were typically more idiosyncratic.

Before proceeding, we review the key assumptions of the model and argue that they are likely to hold in our case study.

Exogeneity of changes in performance weights

We consider the introduction of the three performance measures presented in Table 1: the employment rate at follow-up, average weeks employed at follow-up, and average earnings at follow-up. An important assumption of the model is that the agent myopically maximizes the performance award in each period (ignoring the impact of first period productive effort and gaming choices on the second period performance weights). This is reasonable if the change in performance weights, or the introduction of new performance measures, as in our case study, is not influenced by the agent's behavior. (If this assumption is violated, then the agent may internalize the fact that her early actions may trigger a change in the performance measures. This may influence the agent's effort choice and also the association between the performance measure and organizational value.)[17] This assumption that each agent myopically maximizes her performance award is reasonable in our application because there are more than 600 independent and geographically separated agents and to our knowledge there is no evidence that the agents ever colluded. Because the principal applies the same set of measures to all agents, the decision of each

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[17] For example, assume the principal retains a measure only if the correlation between that measure and the true goal does not decrease after its initial introduction. If the agent knows this, she may try to keep the correlation constant by trying to maintain a balance between the increase in measure specific effort and distortions. If this is the case, our test may fail to identify distortions.

individual agent to game a measure has very little impact on the principal's decision to change the set of measures.[18] Another related concern is that some training agencies may have anticipated the introduction of the follow-up measures and may have prepared for this new regime. This, however, does not invalidate our test because these agencies had no reason to actually start investing in distortive effort until the new measures were actually implemented.

Variation in Production Environment and Examples of Distortion in JTPA

A fundamental assumption of the model is that the researcher has access to observations drawn from multiple production environments (index α in the model) and that the marginal return of distortions is, at least to some extent, measure specific. In our empirical work we define a project as a demographic group. The agency serves multiple demographic groups and we can measure the performance outcome and the value added for each demographic group before and after the introduction of the follow-up measures. (Note that this is not the case for individual trainees because we cannot compute value added at the individual level as we explain shortly.) There is a wide body of evidence showing that the marginal return of training varies across demographic groups, that is, $v_{i,\alpha}$ varies with α. See for example Orr, et al (1997), who show that the impacts of different kinds of training in JTPA vary by demographic group (see especially Table 4, p. 565), and also the validation literature reviewed earlier.

The existence of measure specific distortion margins ($\eta_{2,\alpha}\neq0$ and/or $w_{2,\alpha}>0$) is established in Courty and Marschke (2004). They document how training agencies delayed graduating unemployed enrollees, even after their training concluded, because unemployed graduates counted against the training agencies in the first decade of JTPA. In addition, they show that the training agency adapted this strategy toward the end of the year to respond to the threshold

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[18] The assumption that a single agent's actions have no or little influence on the principal's decision to change the set of measures is not inconsistent with the possibility that the behavior of all agents taken as a group could have such an influence. There is some evidence that this was indeed the case in our case study (Courty and Marschke, 2005).

effects in the incentive scheme.[19] Courty and Marschke found that by timing performance measurement in this way training agencies boosted their performance, and their awards, without providing higher-quality services, or providing services more efficiently. In addition, the authors found evidence that this kind of gaming behavior consumed program resources.

Survey evidence suggests that the follow up measures may have also displayed specific margins of distortions. Courty and Marschke (2005) offer evidence that these new measures induced case managers to begin monitoring enrollees between graduation and the end of the ninety day follow-up period.   To increase the chances that employment matches lasted until the thirteenth week after graduation, some training agencies reported that between graduation and follow-up they started to offer, after the introduction of the follow-up measures, additional services such as childcare, transportation, and clothing allowances. Case managers also actively pressed employers to retain the clients until the third month. If the client lost her job, case managers scheduled job-counselling appointments and offered placement services. After the ninety day follow up period, however, case managers apparently withdrew offers of placement or counselling services and severed contact with clients. These responses were clearly specific to the follow up measures and it seems reasonable that these responses (e.g. childcare allowance) varied across demographic groups. Although potentially productive, these responses qualify as random marginal productivity of effort ($\eta_{2,\alpha} > 0$) if they diverted resources from other activities that increased long term earning and employment more. Some of these responses may even qualify as gaming ($w_{i,\alpha} > 0$) if they were counter or un-productive.

3.3 Empirical Model

Define the observed outcomes as

[19] An agency usually arrived at the end of the fiscal year with an inventory of idle, unemployed enrollees on its books. The training agency would then have to decide which fiscal year to graduate the unemployed enrollees. If the agency finds itself either comfortably above or hopelessly below its standard, it could enhance its odds of winning an award in the next fiscal year without jeopardizing its award in the current year by graduating most or all of its inventory. If the

$$\tilde{p}_{i,\alpha}(\beta_1,\beta_2)=u_{i,\alpha}+e_\alpha(\beta_1,\beta_2)v_{i,\alpha}+g_\alpha(\beta_1,\beta_2)w_{i,\alpha}+\varepsilon_{i,\alpha}$$

$$\tilde{V}_\alpha(\beta_1,\beta_2)=u_{0,\alpha}+e_\alpha(\beta_1,\beta_2)v_{0,\alpha}-(\xi_{1,\alpha}g_1+\xi_{2,\alpha}g_2)+\varepsilon_{v,\alpha}$$

where $\varepsilon_{i,\alpha}$ and $\varepsilon_{v,\alpha}$ are zero mean random variables independent of $p_{i,\alpha}$ and V_α that capture

influences on performance and value-added that are unrelated to efforts. Predictions a-d still hold

for the observed outcomes under the assumption that the joint distribution of $(\varepsilon_{i,\alpha},\varepsilon_{v,\alpha})$ does not

change with the change in the performance weights.

We evaluate the relation between organizational value and the new performance measures

described in Table 1 before and after their activation. We follow the methodology of Heckman,

Heinrich, and Smith (2002), who evaluate the JTPA's performance measures by regressing the

earnings and employment impacts of JTPA training (the V_α in our model) on the performance

measures (the $p_{i,\alpha}$), using the same data we use here. We also report results using both earnings and

employment impact, which are natural candidates for V_α since they correspond to the stated

objective of the organization and are typically used in the evaluation literature. We conduct

separate analyses for each of three performance measures of Table 1. The basic idea of our

analysis is that we construct estimates of performance outcomes and employment and earnings

impacts for various demographic subgroups of the sample. We then examine how the coefficient

estimates from regressions of subgroup impacts on subgroup outcomes (as well as other measures

of association) change with the activation of the corresponding measure.

Sample

In the sample period of our study, states were taking on additional performance measures;

they had for the most part not yet discarded the graduation-based measures.[20] We develop our

empirical measures of performance outcomes and programmatic impacts using data from the

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training agency found itself above but close to the standard, it could increase its award in the present year by
postponing graduation until the following year.
[20] Fourteen of the sixteen states added one or more of the follow-up measures described in Table 1 during the period we
study. Over the same period, two states dropped a cost standard that had rewarded training agencies for keeping costs

National JTPA Study (NJS), an experimental study of the effectiveness of JTPA commissioned by DOL and conducted between 1987 and 1989. Sixteen of the organization's roughly 640 job training agencies participated in the NJS.[21,22] The study was conducted using a classical experiment methodology according to which JTPA applicants were randomized into treatment and control groups. The control groups did not receive JTPA training services for at least 18 months after random assignment. 20,601 JTPA-eligible adults and youth participated in the study: 13,972 were randomized into the treatment group and 6,629 into the control group.

The empirical analysis in this study is based on the 13,338 adult NJS controls and treatments with valid data. The information contained in these data include participant-reported information on their education level, labor market history, family composition, welfare program participation and demographic characteristics, as well as labor market, training, and schooling activity for approximately 18 months after random assignment.[23] The data also contain information sufficient to construct measures of enrollee performance outcomes.

We first identify for each training agency in our data the program years for which the performance measure was in effect. The performance measures in place in each state and program year were obtained from documents on file in states' departments of labor (see Courty and Marschke, 2003b). We then assign each experimental participant to one of two subsamples based on whether their random assignment date occurred in a program year in which their training agency was evaluated by the performance measure under consideration: one subsample consisting of enrollees trained under regimes where the performance measure is activated, and the other consisting of enrollees trained under regimes where the performance measure is not or de-activated.

Computing V_α

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per employment at graduation low. We omit the cost measure from our analysis because we cannot produce training cost estimates at the enrolee level using our data.

[21] Note that we focus in this analysis on the adult side of JTPA, and ignore the smaller youth side.

[22] See Doolittle and Traeger (1990) for a description of the implementation of the National JTPA Study, and Bloom et al. (1997) for a detailed description of its results.

[23] For one quarter of the experimental participants, data were collected for an additional 18 months. This paper utilizes only the employment data for the first 18 months following random assignment.

To construct a measure of organizational value, one needs to observe earnings in two mutually exclusive states of the world: for a given participant one needs earnings after receiving training and earnings during the same period had the participant not received training. Of course, the economist only ever observes a person's earnings in one or the other state—she never observes the counterfactual. Selection and other issues imply that non-experimental estimators have to rely on strong assumptions (on these issues, see e.g., Lalonde, 1986, and Heckman, Smith, and Lalonde, 1999.) Many economists who evaluate social programs consider a social experiment the most reliable way to produce impact estimates. In a properly executed randomized trial, the persons receiving training are statistically equivalent in both observed and unobserved characteristics to the persons in the control group so that any difference in earnings and employment can reasonably be attributed to the training intervention. Thus to construct our estimates of earnings and employment impacts we exploit the experimental data of the NJS.

Although it is not possible to construct an experimental estimate of V_α at the individual level for the reasons mentioned above, it is possible to construct experimental estimates of V_α at the subgroup level. Following Heckman et al, we construct impact estimates for subgroups based on individual characteristics measured at the point of application. For each subsample, we construct 56 subgroups based on marital status, welfare/AFDC/Food Stamp receipt, race, age, gender, educational attainment, employment status at application, earnings in the year preceding application, and training agency. Thus, each individual appears in our sample several times. For each individual in a subgroup, we compute an earnings figure by aggregating his/her earnings over the 18 months following their random assignment.[24] In the absence of a drop out problem, consistent estimates of the subgroup earnings impact can be obtained from a simple comparison of the 18-month earnings of treatments and controls within the subgroup. Over one-third of the individuals in the treatment group drop out, however. We use a regression framework to estimate

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[24] Following Heckman, Heinrich, and Smith, we delete from our sample observations in the top one percentile of self-reported earnings to limit the influence of outliers.

the earnings impacts, employing a method suggested by Bloom (1984) to control for dropouts.[25]

We similarly compute employment impacts by comparing the number of months of employment reported by treatments and controls during the eighteen months following random assignment. Table 2A shows the estimated earnings and employment impacts for many of the subgroups we created. Table 2A shows that the impacts are often small relative to their standard errors. This is consistent with findings using these data reported elsewhere (Heckman, Heinrich, and Smith). This exercise produces for each of the three performance measures that we study, earnings and employment impacts for up to 112 subgroups: one set of up to 56 subgroups of enrollees trained in regimes where the performance measure is activated, and another set of up to 56 subgroups of enrollees trained in regimes where the performance measure is not activated. A regime may contain fewer than 56 subgroups if there are some subgroups that contain no individuals trained under the regime.

Computing $p_{l,\alpha}$

Because we compute earnings impacts by subgroup, we compute performance outcomes by subgroup as well. Participants supplied monthly wage and employment information for each job held in the 18-month period after random assignment. The NJS data file also contains the exact graduation dates from agency records. We constructed the enrollee-level follow-up date-based performance outcomes using the enrollee's reported employment hours and wage information from the calendar month containing the graduation date through the calendar month containing the follow-up date (which occurs ninety days after the graduation date). In computing the enrollee-level employment rate at follow-up outcome, we considered an enrollee employed at follow-up if he/she reported employment in the third calendar month following graduation. To be consistent with JTPA's definition of the measures, we constructed the earnings outcomes only for enrollees who were employed in the third month following graduation. We constructed the average weekly

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[25] For a comprehensive discussion of the Bloom assumption and of the problem of drop-outs in experimental evaluations more generally, see Heckman, Smith, and Taber (2002).

earnings at follow-up outcome by computing the average weekly earnings of all the enrollee's employment spells ongoing in the third month following the graduation month and then summing over all spells. We constructed the enrollee's average weeks worked outcome by aggregating her number of weeks of employment over the three month follow-up period. Then, for each of the performance measures, we computed the subgroup performance outcomes by averaging the individual performance outcomes within each subgroup. Table 2B describes the means of the three performance outcomes for selected subgroups in our sample.

3.4 Results

We regress subgroup estimated employment and earnings impacts on their performance outcomes, weighting the regression by the inverse of the Eicker-White standard errors from the impact estimations. In using a regression framework, we are following Heckman, Heinrich, and Smith, but also the performance measure validation literature in accounting (see, e.g., Ittner and Larcker and Banker, Potter, and Srinivasan). Note that this simple regression of V on P yields an estimate of $cov(P,V)/var(P)$. We thus test whether the coefficient on the performance outcome falls with the activation of the corresponding measure. We take a finding that the coefficient falls as evidence that activating a performance measure weakens its association with programmatic impacts and implies the existence of distortions. Because we have two impact measures and three outcome measures we have six equations, which we estimate jointly (using a seemingly unrelated regression framework).

Evidence of distortion: Change in regression coefficient (prediction a)

Table 3 shows the results of our estimation. The dependent variables are the estimated subgroup earnings and employment impacts. Each equation contains on the right hand side the subgroup outcome (either the employment rate at follow-up, average weeks worked at follow-up, or average weekly earnings at follow-up) and the outcome interacted with a dummy variable indicating whether the performance measure is activated. (Note that this model deviates from

Heckman, Heinrich, and Smith only by the inclusion of the activation dummy variable.) Each regression also contains an intercept and the activation dummy alone, whose coefficient estimates are omitted from the table.

First, note that all coefficient estimates on the performance outcomes are positive and significant. This suggests that the new performance outcomes do indeed predict impacts when the performance outcomes are not awarded. Next, note that five of the coefficient estimates on the interacted terms are negative. In addition, all six coefficients are *jointly* significant (the p value of the joint significance test is .0001). This finding alone is consistent with the model, which implies a reduction in the regression coefficient on the performance outcome with the measure's activation. Third, note that in three of the six cases—one case for each of the three performance measures— the coefficient estimate on the interacted term is negative and significant. The drop in the regression coefficient is consistent with the award triggering distortionary activities. The results suggest that each measure is distorted.

Evidence of response to incentive: Change in outcome variance (prediction b)

Our model predicts that the variance of the measure should rise with activation, both because activation elicits additional efforts and because activation may elicit distortionary behavior. Table 4 shows the sample variances of the performance outcomes with and without activation of the corresponding performance measures. The evidence provides some support for prediction b. In the case of the average weeks worked at follow-up and average weekly earnings at follow-up measures, the variances increase with activation. In the latter case, the change in variance is statistically significant by conventional significance criteria. The increase in the variance of the average weeks worked at follow-up outcome is marginally significant by conventional significance criteria.

Evidence of response to incentive: Change in outcome mean (prediction c)

The model predicts that activating a performance measure increases the performance outcome. To test this, we estimate econometric models of the determinants of subgroup outcomes,

which include a performance measure activation dummy as an independent variable.[26] Assume that

the subgroup follow-up outcome is determined as follows:

$$p_{f\alpha}=\lambda_{f0}+\lambda_{f1}d_{f\alpha}+\psi_{f\alpha}+X_{\alpha} \qquad (4)$$

where $p_{f\alpha}$ is the follow-up outcome for subgroup α. The subgroup outcome is presumed a function

of a constant (λ_{f0}), the influence of the incentive scheme in place during α's training ($\lambda_{f1}d_{f\alpha}$), and an

error term, $\psi_{f\alpha}$, with E ($\psi_{f\alpha}|d$) = 0. $d_{f\alpha}$ is an indicator variable, equal to one if the follow-up

measure is activated, and zero otherwise. The activation dummy is correlated with time, as states

adopted the follow-up measure later in the period of our study. X_{α} captures the influence of

unboservable non-incentive related factors on performance outcomes such as political and

economic conditions. Differences in X_{α} across incentive regimes, and in particular trends in labor

market conditions, may therefore bias the estimate of the activation effect λ_{f1} because

$$E(p_{f\alpha}|d_{f\alpha}=1)-E(p_{f\alpha}|d_{f\alpha}=0)=\lambda_{f1}+E(X_{\alpha} \mid d_{f\alpha}=1)-E(X_{\alpha}|d_{f\alpha}=0)\neq \lambda_{f1}.$$

However, if one assumes that X_{α} determines the outcome for the graduation-based measure in the

same way that it determines the outcome for the follow-up measure, then the omitted variable can

be differenced out. Assume that the graduation outcome is determined as follows:

$$p_{t\alpha}=\lambda_{t0} +\lambda_{t1}d_{f\alpha}+\psi_{t\alpha}+X_{\alpha} \qquad (5)$$

where $p_{t\alpha}$ is the graduation-based equivalent of $p_{f\alpha}$, $\lambda_{t1}d_{f\alpha}$ is the effect of activating the follow-up

measure on the termination $p_{t\alpha}$, and $E(\psi_{t\alpha}|d)$ = 0. The regression we run is

$$p'_{f\alpha}=\lambda'_{f0} +\lambda'_{f1}d_{f\alpha}+\psi'_{f\alpha} \qquad (6)$$

which is constructed by subtracting (5) from (4), and where $p'_{f\alpha} = (p_{f\alpha} – p_{t\alpha})$ and $\lambda'_{f1} = (\lambda_{f1} - \lambda_{t1})$,

$E(\psi'_{f\alpha}|d)$ = 0, and, because X_{α} differences out,

$$E(p'_{f\alpha}|d_{f\alpha} = 1)-E(p'_{f\alpha}|d_{f\alpha}=0)=\lambda'_{f1}=(\lambda_{f1}-\lambda_{t1}).$$

The theoretical model predicts $\lambda_{f1} > 0$ and $\lambda_{t1} < 0$, which imply $\lambda'_{f1} > 0$.[27]

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[26] We perform this analysis at the subgroup level to remain consistent with the rest of the paper.
[27] In JTPA, the activation of the follow-up measure was often accompanied by activations and deactivations of other measures so that the combined effect on the marginal return to effort exerted on $p_{t\alpha}$ is ambiguous. However, it is likely that any positive impact of the changes that accompanied the activation of the follow-up measure on the graduation

We estimate (6) for the activation of the employment rate at follow-up, where $p_{f\alpha}$ and $p_{t\alpha}$ are subgroup α's employment rate at follow-up and employment rate at graduation, respectively, and $d_{f\alpha}$ is an indicator of the employment rate at follow-up's activation.[28] For the average weekly earnings at follow-up, we construct a graduation date-based analogue to control for the unobserved heterogeneity (X_α) when we subtract (5) from (4). Thus for the average weekly earnings at follow-up test, we estimate (6), where $p_{f\alpha}$ and $p_{t\alpha}$ are subgroup α's average weekly earnings at follow-up and graduation, respectively, and $d_{f\alpha}$ is an indicator of the activation of the average weekly earnings at follow-up measure.[29]

Table 5 presents the results of the estimation of (6). The first two columns of the table show our estimates of λ_{f0} and λ_{f1} from a regression of the follow-up outcome on the activation dummy. The equations for the employment rate at follow-up and average weekly earning at follow-up outcomes were estimated jointly in a SUR framework. Note that the estimate of λ_{f1} in the employment rate at follow-up equation is negative, although statistically insignificant; the estimate of λ_{f1} in the average weekly earning at follow-up equation is positive. The second two columns present the results from an estimation which uses as its dependent variable the difference between the follow-up and graduation outcome. The estimates of λ'_{f1} are both positive as predicted but only statistically significant in the earnings equation.[30]

Evidence of distortion: Change in covariance (prediction d)

According to prediction (d) a drop in the covariance between V and P when P is activated is a sufficient indicator of gaming in P. A rising or constant covariance could occur if either there is no gaming in P or if the gaming behavior is small relative to the increase in measure specific effort.

outcome is dominated by the direct effect of the follow-up measure's activation on the follow-up outcome, and thus λ'_{f1} should be positive.

[28] The subgroup employment rate at follow-up outcome is constructed as above. The subgroup employment rate at graduation outcome is constructed directly from the employment status at graduation variable given in the NJS administrative data. These data provide performance outcome information only for the employment at graduation performance measure. Outcomes for all other performance measures must be estimated using enrollee's reporting of their employment information.

[29] There is no termination equivalent to the average weeks worked at follow-up. Thus, we do not conduct a test of prediction b for this measure.

To test for changes in the covariance with activation of the performance measure, we regress a transformed V on P. Following the analysis above, for each of the six performance outcome and impact combinations, we divide the sample into two subsamples, one containing the subgroups whose individuals' random assignment dates occur under an incentive regime where the performance measure is deactivated, and the other containing subgroups whose individuals' random assignment dates occur in a regime where the performance measure is activated. Then, within each subsample, we multiply V by the subgroup sum of the squared deviations of P from its mean, $\sum_{\alpha}(p_{\alpha} - \bar{p})^2$, pool the two subsamples and regress the transformed V on P, estimating a separate slope (and intercepts) for the activation and deactivation subsamples, using a SUR regression framework (as in Table 3). This transformation makes the estimate of the slope coefficient an unbiased estimate of (N-1)Cov(V,P) where N is the number of subgroups (because the $\sum_{\alpha}(p_{\alpha} - \bar{p})^2$ in the transformed V and the denominator of the slope coefficient estimate cancel out). For each performance outcome and impact pair, we simply compare the slope coefficient estimated from the subsample from the regime with the measure activated, to the slope coefficient from the subsample from the regime with the measure deactivated.[31] Because there are six impact-outcome combinations, we estimate six equations.

Table 6 reports the results from our estimation. The equations estimated are numbered I through VI. For example, in equation I the dependent variable is the subgroup 18 month earning impact (weighted by $\sum_{\alpha}(p_{\alpha} - \bar{p})^2$). The right hand side includes separate intercepts for the activation and deactivation subsamples, but these estimates are omitted from the table, as they are of no special interest. The right hand side of equation I includes the employment rate at follow-up outcome interacted with an activation and deactivation dummy. Below the pair of coefficient

estimates, the table reports the p value associated with the test of their equality. Thus we find that the covariance of earnings impact and the follow-up employment rate increases after this measure is activated and this increase is statistically significant. This suggests that measure specific productive effort, rather than gaming, must play a dominant role in the agent's response. This finding is consistent with the previous finding that there were no evidence of distortion in the employment rate at follow-up performance measure when one consider the earning impact (top left-hand of Table 3).

In the employment rate at follow-up equation (equation IV) where the dependent variable is the 18 month employment impact, we find no significant difference in the outcome coefficient estimate across the two regimes. Combined with the result in Table 3, the insignificant difference in covariance suggests that the employment impact distortion in the employment rate at follow-up measure (bottom left of Table 3) was probably driven by distortion due to random marginal productivity rather than gaming distortion.

For the other covariance tests involving employment impacts (equations V and VI), we find no statistically significant differences in the covariance. Recall that Table 3 reports no individually significant changes in the corresponding regression coefficient tests, as well. With the activation of the average weeks worked at follow-up and average earnings at follow measures, however, the covariances between these measures and earnings impact falls, and this change is statistically significant (the p values corresponding to the equality tests are both about .0001). Thus, we find that the results of the covariance and regression coefficient tests are consistent with each other and with the hypothesis that gaming plays a role: where activation produces a statistically significant decline in covariance, it also produces a statistically significant decline in regression coefficient.

Evidence of changes in performance measure ranking

Researchers and practitioners have used correlation methods to sort candidate performance measures and to rank them. Our theoretical model, however, shows that the correlation between outcomes and goals is endogenous and that using a correlation measure to identify good

performance measures can be misleading. To illustrate this point, assume the organization is considering adding a new performance measure to complement an existing one. Assume there are two candidate measures, measure 2 and 2' that are identical in all respects, but performance measure 2 is more correlated with the principal's objective than measure 2'. Selecting measure 2 on this criterion may be misleading. In fact, it may turn out that the correlation between organizational value-added and measure two drops after its introduction. It is even possible that measure 2' becomes more correlated with value, if the correlation is measured after the measure's introduction. Although our model does not provide a method to select performance measures, it suggests that one has to be cautious in using a correlation based selection criterion.

To illustrate this issue in our case study, we investigate whether the ranking of performance measures based on a correlation criteria changes before and after the introduction of a measure. Table 7 shows the explanatory power of each of the three performance measures in impact regressions by whether the measure is activated. The first line of the table shows the slope coefficient estimates and R-squareds of the regressions of earnings impacts on employment rate at follow-up, average weeks worked at follow-up, and average weekly earnings at follow-up outcomes, respectively, in training agency-years where the corresponding performance measure is not activated.[32] The second line shows the R-squared for these regressions using training agency-year data in which the performance measure is activated. The bottom two lines repeat the comparison for the employment impact measure.

Consider the results for the top half of the table which show the effect of activating performance measures on the earnings impact regressions.   Note first that the coefficient estimates

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[32] The R-squared is a measure of explanatory or predictive power of the performance measure because it shows the fraction of the variation in the impact that is explained by the variation in the performance outcome. Others in the literature have evaluated performance measures by a comparison of R-squareds; see Ittner and Larcker, pp. 14-15. The results shown in Table 7 differ from the earlier results of Table 3 because the earlier results are generated from a single SUR regression. The results of Table 3 are generated from twelve separate regressions: two dependent variables (the two impact measures) crossed with three independent variables (the three performance outcomes) for each of two subsets of the data (persons trained subject to the corresponding performance measure and persons trained absent the corresponding performance measure). We estimated the twelve models separately so as to observe how performance measure activation affected R-squareds.

are all significant and positive when the performance measure is not activated. When they are not activated, a ranking of the performance measures by R-squared places the employment rate at follow-up measure behind both the average weeks worked and average weekly earnings at follow-up measures. When the performance measure is activated, however, while the coefficient estimate for the employment rate at follow-up remains positive and significant, the coefficient estimates for the other measures fall—indeed they become insignificant—along with their R-squareds. Activating the performance measures, therefore, reverses their rankings: after activation, the employment rate at follow-up measure dominates the other two measures.

The bottom half of the table shows the effect of activating performance measures on the employment impact regressions. When the measures are not activated, only in the employment rate at follow-up regression is the slope coefficient estimate significant. When the measures are activated, all coefficient estimates are insignificant, but the R-squared of the employment rate at follow-up regression is higher than in the others. Thus, in the case of employment impacts, activation does not affect the ranking of performance measures.

4 Conclusions

An important lesson from the incentive literature is that performance measures are often distorted, eliciting dysfunctional and unintended responses, also known as gaming responses. These responses, however, are typically hidden from the researcher. This paper develops a simple multitasking model and derives tests for the existence of distortions in performance measures. The model shows that one can identify the existence of distortions by estimating how the association between a performance measure and the true goal of the organization changes with the activation of the measure.

Using data from the JTPA incentive system, we test the model's main prediction by estimating changes in regression coefficient in a regression of organizational value on the performance measure. To test for the existence of distortions, we focus on the introduction of the

follow-up measures, which corresponds to one of the most dramatic changes in the measurement system. For three follow-up measures, we test whether the regression coefficient estimate decreases after the introduction of the measure. We find evidence consistent with our hypothesis. We conclude that the new measures were distorted. These findings are corroborated by our previous work that used the specific rules of the performance measurement system to demonstrate the existence of distortions.

The paper also contributes to the literature on the implementation of performance measurement (Ittner and Larcker (1998), Banker, Potter, and Srinivasan (2000), Heckman, Heinrich, and Smith (2002)). Our model formally demonstrates the suspicion that the association between a performance measure and value-added is endogenous. Our evidence suggests that using a correlation measure to identify good performance measures can be misleading. A selection method for performance measures that is based on how well measures predict the true objective (using correlation or other methods), as is commonly used by practitioners, has important limitations. In fact, we show that the ranking of the performance measures according to how correlated they are with the principal's objective can depend on the set of performance measures used.

This point suggests that the construction of optimal measurement systems as well as the welfare assessment of the benefit of using performance measurement may be more complicated than presented in the literature. The standard model assumes that the principal knows the production environment and can compute the optimal combination of performance measures by balancing measurement noise and incentive provision (e.g. Banker and Datar 2001 and Baker 2002). Our paper shows that this method fails to take into account the distortion margins that are typically revealed only after the performance measure has been used. If the principal cannot observe the technology of distortion---arguably, a reasonable assumption---then the construction of optimal incentive systems cannot be modeled as a static decision problem. The principal has to take into account the fact that more information about the performance measure may become

available after the measure is introduced. This dynamic process suggests a new perspective on the construction of performance measurement systems.

Appendix: Proofs of Claims made in section 2

Proposition 1: (i) Measure two is distorted if and only if $Cov(p_{2,\alpha},V_\alpha)/Var(p_{2,\alpha})$ decreases after an increase in β_2/β_1. (ii) If measure two is not distorted, then $Corr(p_{2,\alpha},V_\alpha)$ increases when β_2/β_1 increases.

Proof: (i) Let $\gamma = \beta_2/(\beta_1 + \beta_2)$

$$\frac{Cov(p_{2,\alpha}(\beta_1,\beta_2),V_\alpha(\beta_1,\beta_2))}{Var\, p_{2,\alpha}(\beta_1,\beta_2)} = \frac{Varv_{0,\alpha}^2 + \gamma^2\left(Cov((\eta_{2,\alpha}+v_{2,\alpha})^2,v_{2,\alpha}(\eta_{2,\alpha}+v_{2,\alpha})) - Cov(w_{2,\alpha}^2,\xi_{2,\alpha}w_{2,\alpha})\right)}{Varv_{0,\alpha}^2 + \gamma^2\left(Var(\eta_{2,\alpha}+v_{2,\alpha})^2 + Varw_{2,\alpha}^2\right)}$$

Let $S = sign\left(\dfrac{d}{d\gamma}\left[\dfrac{Cov(p_{2,\alpha}(\beta_1,\beta_2),V_\alpha(\beta_1,\beta_2))}{Var\, p_{2,\alpha}(\beta_1,\beta_2)}\right]\right)$

$S = sign\left(Cov((\eta_{2,\alpha}+v_{2,\alpha})^2,v_{2,\alpha}(\eta_{2,\alpha}+v_{2,\alpha})) - Cov(w_{2,\alpha}^2,\xi_{2,\alpha}w_{2,\alpha}) - \left(Var(\eta_{2,\alpha}+v_{2,\alpha})^2 + var\, w_{2,\alpha}^2\right)\right)$

$= sign\left(-Cov((\eta_{2,\alpha}+v_{2,\alpha})^2,\eta_{2,\alpha}(\eta_{2,\alpha}+v_{2,\alpha})) - Cov(w_{2,\alpha}^2,\xi_{2,\alpha}w_{2,\alpha}) - Varw_{2,\alpha}^2\right)$

But $Cov((\eta_{2,\alpha}+v_{2,\alpha})^2,\eta_{2,\alpha}(\eta_{2,\alpha}+v_{2,\alpha})) = var\,\eta_{2,\alpha}(\eta_{2,\alpha}+v_{2,\alpha}) + E\eta_{2,\alpha}^2 v_{2,\alpha}^2$ since $E\eta_{2,\alpha}^3 = 0$. We have

$$S = sign\left(-Var(\eta_{2,\alpha}(\eta_{2,\alpha}+v_{2,\alpha})) - E\eta_{2,\alpha}^2 v_{2,\alpha}^2 - Cov(w_{2,\alpha}^2,\xi_{2,\alpha}w_{2,\alpha}) - var\, w_{2,\alpha}^2\right).$$

If the measure is not distorted, S=0 and $Cov(p_{2,\alpha},V_\alpha)/Var(p_{2,\alpha})$ is independent of β_2/β_1. If the measure is distorted S<0 and $Cov(p_{2,\alpha},V_\alpha)/Var(p_{2,\alpha})$ decreases after an increase in β_2/β_1.

(ii) Assume $\eta_{2,\alpha}=w_{2,\alpha}=0$. The variance and covariance of $p_{2,\alpha}$ and V_α are

$Var\, p_{2,\alpha}(\beta_1,\beta_2) = (\beta_1 + \beta_2)^2 Varv_{0,\alpha}^2 + \beta_2^2 Varv_{2,\alpha}^2$

$Var\, V_\alpha(\beta_1,\beta_2) = (\beta_1+\beta_2)^2 Varv_{0,\alpha}^2 + \beta_1^2 Varv_{1,\alpha}(\eta_{1,\alpha}+v_{1,\alpha}) + \beta_2^2 Varv_{2,\alpha}^2$

$Cov(p_{2,\alpha}(\beta_1,\beta_2),V_\alpha(\beta_1,\beta_2)) = (\beta_1+\beta_2)^2 Varv_{0,\alpha}^2 + \beta_2^2 Varv_{2,\alpha}^2$

The correlation between p_2 and V is

$$Corr(p_{2,\alpha}(\beta_1,\beta_2),V_\alpha(\beta_1,\beta_2)) = \left[1 + \frac{\beta_1^2 Varv_{1,\alpha}(\eta_{1,\alpha}+v_{1,\alpha})}{(\beta_1+\beta_2)^2 Varv_{0,\alpha}^2 + \beta_2^2 Varv_{2,\alpha}^2}\right]^{-1/2}$$

$$Corr(p_{2,\alpha}(\beta_1,\beta_2),V_\alpha(\beta_1,\beta_2)) = \left[1 + \frac{Varv_{1,\alpha}(\eta_{1,\alpha}+v_{1,\alpha})}{\left(1+\dfrac{\beta_2}{\beta_1}\right)^2 Varv_{0,\alpha}^2 + \left(\dfrac{\beta_2}{\beta_1}\right)^2 Varv_{2,\alpha}^2}\right]^{-1/2}$$

The correlation increases with β_2/β_1. QED

Claim: Assume $v_{2,\alpha}=\eta_{2,\alpha}=\xi_{1,\alpha}=\xi_{2,\alpha}=0$ for all α. Condition (3) implies that the correlation decreases after an increase in β_2/β_1.

Proof: The correlation is

$$Corr(p_{2,\alpha},V_\alpha) = \frac{(\beta_1+\beta_2)^2 Varv_{0,\alpha}^2}{\left((\beta_1+\beta_2)^2 Varv_{0,\alpha}^2 + \beta_2^2 Varw_{2,\alpha}^2\right)^{1/2}\left((\beta_1+\beta_2)^2 Varv_{0,\alpha}^2 + \beta_1^2 Var\eta_{1,\alpha}(v_{1,\alpha}+\eta_{1,\alpha})\right)^{1/2}}$$

Write the correlation as $Corr(p_{2,\alpha},V_\alpha) = [(1+\gamma^2 K_1)(1+(1-\gamma)^2 K_2)]^{-1/2}$ where

$$K_1 = \frac{Varw_{2,\alpha}^2}{Varv_{0,\alpha}^2}, \quad K_2 = \frac{Varv_{1,\alpha}(v_{1,\alpha}+\eta_{1,\alpha})}{Varv_{0,\alpha}^2}$$

$$Sign\left(\frac{d}{d\,\beta_2/\beta_1}Corr(p_{2,\alpha},V_\alpha)\right) = -Sign\left(\frac{d}{d\gamma}(1+\gamma^2 K_1)(1+(1-\gamma)^2 K_2)\right)$$

$$\frac{d}{d\gamma}(1+\gamma^2 K_1)(1+(1-\gamma)^2 K_2) = 2(\gamma K_1 - (1-\gamma)K_2 + \gamma(1-\gamma)(1-2\gamma)K_1 K_2)$$

$$\frac{d}{d\gamma}(1+\gamma^2 K_1)(1+(1-\gamma)^2 K_2) = 2(1-\gamma)K_2\left(\frac{\gamma K_1}{(1-\gamma)K_2} - 1 + \gamma(1-2\gamma)K_1\right)$$

A sufficient condition for $dCorr(p_{2,\alpha},V_\alpha)/d\gamma < 0$ is $\dfrac{\gamma K_1}{(1-\gamma)K_2} \geq 1$ which is equivalent to

$K_1 \geq \dfrac{\beta_1}{\beta_2} K_2$. This condition is identical to (3). QED

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Table 1
Revised JTPA Performance Measures for JTPA's Adult Program

Performance Measure	Description
Employment Rate at Follow-up	Fraction of graduates who were employed at 13 weeks after graduation
Average Weekly Earnings at Follow-up	Average weekly wage of graduates who were employed 13 weeks after graduation
Average Weeks Worked by Follow-up	Average number of weeks worked by graduates in 13 weeks following graduation

Notes:
1. The date of graduation is the date the enrollee officially exits training. A graduate is an enrollee after he/she has officially exited training.
2. All measures are calculated over the year's *graduate* population. Therefore, the average follow-up weekly earnings for 1987 was calculated using earnings at follow-up for the graduates who graduated in 1987, even if their follow-up period extended into 1988. Likewise, persons who graduated in 1986 were not included in the 1987 measure, even if their follow-up period extended into 1987.

Experimental Impacts By Subgroup

Subgroup	18 Month Earnings Impacts ($)	18 Month Employment Impacts (months)
Receiving Food Stamps		
No	490.823	0.117
	(273.314)	(0.191)
Yes	269.646	0.298
	(281.878	(0.235)
Gender		
Male	46.236	0.099
	(335.312)	(0.219)
Female	574.451	0.240
	(225.046)	(0.200)
Highest grade completed		
< 10 yrs	508.109	0.458
	(466.333)	(0.371)
10-11 yrs	455.366	0.314
	(406.520)	(0.323)
12 yrs	528.377	0.104
	(314.432)	(0.237)
13-15 yrs	115.078	-0.312
	(597.620)	(0.399)
> 15 yrs	-394.491	0.735
	(1210.037)	(0.718)
Race		
White	513.505	0.070
	(261.964)	(0.192)
Black	373.860	0.362
	(358.973)	(0.282)
Hispanic	-330.982	0.212
	(629.107)	(0.458)
Other	85.816	1.008
	(1156.137)	(0.846)
Age		
22-29 yrs	582.795	0.436
	(303.606)	(0.220)
30-39 yrs	337.990	0.208
	(340.071)	(0.253)
40-49 yrs	167.248	-0.220
	(528.506)	(0.414)
50-54 yrs	-829.954	-1.722
	(1043.039)	(0.881)
> 54 yrs	588.204	0.197
	(765.385)	(0.758)
Employment status at time of application		
Currently employed	1037.772	0.191
	(483.793)	(0.324)
Last employed 0-2 months ago	196.212	0.009
	(470.613)	(0.318)
Last employed 3-5 months ago	-413.393	-0.086
	(562.806)	(0.382)
Last employed 6-8 months ago	607.643	0.440
	(746.237)	(0.544)
Last employed 9-11 months ago	1952.2044	0.805
	(949.928)	(0.695)
Last employed > 11 months ago	463.599	0.785
	(459.239)	(0.366)
Never employed	291.431	0.327
	(462.171)	(0.426)

Table 2A (continued)
Experimental Impacts By Subgroup

Subgroup	18 Months Earnings Impacts ($)	18 Months Earnings Impacts (months)
	Training Agency	
Corpus Christi, TX	-847.637	-0.295
	(731.918)	(0.531)
Cedar Rapids, IA	1057.610	-0.282
	(1295.772)	(0.997)
Coosa Valley, GA	1271.657	0.666
	(633.922)	(0.471)
Heartland, FL	1005.343	1.363
	(1270.066)	(0.942)
Fort Wayne, IN	-417.937	-0.644
	(505.544)	(0.369)
Jersey City, NJ	191.213	-0.277
	(985.257)	(0.729)
Jackson, MS	1974.543	1.633
	(747.497)	(0.557)
Larimer, CO	-12.400	0.490
	(874.319)	(0.628)
Decatur, IL	14.810	0.171
	(1171.069)	(0.783)
Northwest, MN	-2272.185	-2.287
	(1170.215)	(0.949)
Butte, MT	-913.574	-0.690
	(1098.605)	(0.801)
Omaha, NE	1185.606	1.335
	(662.263)	(0.576)
Marion County, OH	968.491	0.151
	(667.529)	(0.549)
Oakland, CA	-1034.381	-0.020
	(850.677)	(0.593)
Providence, RI	966.893	0.830
	(844.981)	(0.618)
Springfield, MO	378.675	-0.305
	(710.688)	(0.481)
	Earnings at time of application	
$0-$3,000	497.049	0.378
	(245.126)	(0.204)
$3,000-$6,000	487.801	0.075
	(521.988)	(0.347)
$6,000-$9,000	-439.713	-0.295
	(704.655)	(0.440)
$9,000-$12,000	842.508	-0.358
	(1091.196)	(0.635)
$12,000-$15,000	1569.974	0.636
	(1675.107)	(0.907)
>$15,000	-613.327	-0.211
	(2086.843)	(1.070)

Notes: Robust standard errors of the estimates reported in parentheses. The estimated impacts are corrected for treatment group drop-outs. The earnings and employment impacts are estimated from the 10746 adult experimental participants who report a valid earnings figure (zeros are included) in each of the 18 months after random assignment. The employment impacts are denominated in months of employment and the earnings impacts are denominated in dollars. Subgroups created using AFDC receipt, marital status, and family size excluded for space considerations.

Table 2B
Mean Performance Outcomes By Subgroup

Subgroup	Employment Rate at Follow-up	Average Weeks Worked at Follow-up (weeks)	Average Weekly Earnings at Follow-up ($)
Receiving Food Stamps			
No	0.574	8.868	230.247
	(0.495)	(5.657)	(119.805)
Yes	0.496	7.113	207.264
	(0.500)	(6.079)	(118.138)
Gender			
Male	0.531	8.393	8.393
	(0.499)	(5.820)	(5.820)
Female	0.520	7.918	7.918
	(0.500)	(5.960)	(5.960)
Highest grade completed			
< 10 yrs	0.469	7.388	207.738
	(0.499)	(6.024)	(114.193)
10-11 yrs	0.484	7.419	214.441
	(0.500)	(6.038)	(104.217)
12 yrs	0.545	8.414	219.978
	(0.498)	(5.822)	(121.682)
13-15 yrs	0.572	8.883	242.324
	(0.495)	(5.659)	(135.507)
> 15 yrs	0.692	9.764	250.349
	(0.463)	(5.239)	(127.915)
Race			
White	0.566	8.601	221.835
	(0.499)	(5.788)	(122.813)
Black	0.467	7.267	227.086
	(0.499)	(6.031)	(123.865)
Hispanic	0.468	7.801	207.418
	(0.499)	(5.966)	(100.502)
Other	0.525	8.186	237.655
	(0.500)	(5.784)	(110.805)
Age			
22-29 yrs	0.525	8.194	220.764
	(0.499)	(5.898)	(105.620)
30-39 yrs	0.525	8.165	228.466
	(0.499)	(5.858)	(124.000)
40-49 yrs	0.522	7.939	223.282
	(0.500)	(5.969)	(144.536)
50-54 yrs	0.500	7..558	219.755
	(0.501)	(6.053)	(172.393)
> 54 yrs	0.553	8.068	161.767
	(0.498)	(6.028)	(102.369)
Employment status at time of application			
Currently employed	0.666	10.120	221.082
	(0.472)	(5.054)	(111.279)
Last employed 0-2 months ago	0.594	8.949	234.982
	(0.491)	(5.587)	(143.728)
Last employed 3-5 months ago	0.552	8.658	231.269
	(0.498)	(5.660)	(113.235)
Last employed 6-8 months ago	0.522	8.168	229.595
	(0.500)	(5.856)	(108.832)
Last employed 9-11 months ago	0.525	8.098	217.886
	(0.500)	(5.922)	(101.381)
Last employed > 11 months ago	0.435	6.721	209.313
	(0.496)	(6.108)	(125.440)
Never employed	0.392	6.302	190.362
	(0.489)	(6.189)	(102.424)

Table 2B
Mean Performance Outcomes By Subgroup

Subgroup	Employment Rate at Follow-up	Average Weeks Worked at Follow-up (weeks)	Average Weekly Earnings at Follow-up ($)
	Training Agency		
Corpus Christi, TX	0.499	8.500	193.378
	(0.501)	(5.780)	(111.705)
Cedar Rapids, IA	0.559	8.479	235.631
	(0.498)	(5.911)	(179.038)
Coosa Valley, GA	0.581	8.168	232.552
	(0.494)	(5.847)	(134.171)
Heartland, FL	0.504	7.779	207.493
	(0.502)	(5.502)	(82.616)
Fort Wayne, IN	0.658	9.873	216.703
	(0.475)	(5.213)	(99.946)
Jersey City, NJ	0.411	6.244	268.118
	(0.493)	(6.062)	(112.040)
Jackson, MS	0.578	8.291	210.692
	(0.494)	(5.737)	(134.768)
Larimer, CO	0.509	8.553	218.458
	(0.500)	(5.855)	(122.377)
Decatur, IL	0.643	9.562	247.448
	(0.480)	(5.237)	(149.839)
Northwest, MN	0.571	8.736	222.843
	(0.497)	(5.938)	(90.900)
Butte, MT	0.509	7.923	234.589
	(0.501)	(5.969)	(178.290)
Omaha, NE	0.508	7.279	197.955
	(0.500)	(6.025)	(91.602)
Marion County, OH	0.411	6.451	197.563
	(0.492)	(6.190)	(107.089)
Oakland, CA	0.393	7.120	271.745
	(0.489)	(6.121)	(125.767)
Providence, RI	0.369	5.977	249.088
	(0.483)	(6.137)	(96.094)
Springfield, MO	0.702	10.250	209.348
	(0.458)	(4.871)	(98.995)
	Earnings at time of application		
$0-$3,000	0.467	7.286	210.294
	(0.499)	(6.042)	(123.507)
$3,000-$6,000	0.601	9.163	223.564
	(0.490)	(5.503)	(114.387)
$6,000-$9,000	0.665	9.966	240.109
	(0.473)	(5.108)	(112.070)
$9,000-$12,000	0.642	10.130	265.643
	(0.480)	(4.968)	(113.000)
$12,000-$15,000	0.609	9.711	293.612
	(0.490)	(5.497)	(98.025)
>$15,000	0.694	10.078	345.931
	(0.463)	(5.233)	(192.342)

Notes: Standard deviations are reported in parentheses. The performance outcome means are reported from 10746 adult experimental participants who report a valid earnings figure in each of the 18 months after random assignment. Subgroups created using AFDC receipt, marital status, and family size excluded for space considerations.

Table 3

Outcome-Impact (SUR) Regressions

Coefficient	Employment Rate at Follow-up	Average Weeks Worked at Follow-up	Average Weekly Earnings at Follow-up
Dependent Variable = 18 Month Earnings Impact			
Performance outcome	1478.014	67.570	2.548
	(6.16)	(6.44)	(6.38)
Performance outcome	924.906	-80.582	-3.708
X Activation Dummy	(0.99)	(-2.22)	(-2.05)
Dependent Variable = 18 Month Employment Impact			
Performance Outcome	1.009	0.031	0.001
	(5.71)	(4.16)	(4.11)
Performance Outcome	-1.616	-0.040	-0.002
X Activation Dummy	(-2.31)	(-1.72)	(-1.51)
Degrees of Freedom		534	
R^2		0.3947	

Notes: T statistics in parentheses. Activation dummy coded as one if the relevant performance measure in effect, as zero otherwise. Each equation includes a constant and an activation dummy, whose coefficient estimates are omitted. Regressions are weighted by the inverse of the Eicker-White standard errors from the impact estimations. Earnings are trimmed in construction of impact estimates.

Table 4
Test of Performance Measure Activation on Variance of Performance Outcome

Performance Measure Activated	Sample Variance of		
	Employment Rate at Follow-Up	Average Weeks Worked at Follow-up	Average Weekly Earnings at Follow-up
No	0.250	34.287	13958.75
Yes	0.249	36.233	16318.43
F Test of equal variances (p value)*	0.505	0.105	0.003

*F test: $H_o : \sigma^2_{Yes} = \sigma^2_{No}$, $H_a : \sigma^2_{Yes} > \sigma^2_{No}$

Table 5
Outcome (SUR) Regressions

| Coefficient | Regression I | | Regression II | |
	(1) Employment Rate at Follow-up	(2) Average Weekly Earnings at Follow-up	(3) Employment Rate at Follow-up – Employment Rate at Termination	(4) Average Weekly Earnings at Follow-up – Average Weekly Earnings at Termination
	Dependent Variable			
Constant	0.547	227.576	-0.083	3.324
	(45.32)	(41.34)	(-6.89)	(1.66)
Activation dummy	-0.013	11.526	0.020	10.927
	(-0.75)	(1.45)	(1.18)	(3.77)
Degrees of Freedom	188		188	
R^2	0.020		0.038	

Notes: T statistics in parentheses. Activation dummy coded as one if the relevant performance measure in effect, as zero otherwise. The activation dummy for columns (1) and (3) are for the employment rate at follow-up measure. The activation dummy for the regressions (2) and (4) are for the average weekly earnings at follow-up measure.

Table 6
Regressions for Covariance Tests

	Dependent Variable = Weighted* 18 Month Earnings Impact		
	I	II	III
	Employment Rate	Average Weeks Worked	Average Weekly Earnings
Coefficient	at Follow-up	at Follow-up	at Follow-up
Performance outcome	0.04	0.139	3.486
X (1 – Activation Dummy)	(1.73)	(7.92)	(7.13)
Performance outcome	0.010	0.009	0.188
X (Activation Dummy)	(5.42)	(0.31)	(0.26)
P value of test of equivalence of above coefficients	0.0392	0.0001	0.0001

	Dependent Variable = Weighted* 18 Month Employment Impact		
	IV	V	VI
	Employment Rate	Average Weeks Worked	Average Weekly Earnings
Coefficient	at Follow-up	at Follow-up	at Follow-up
Performance outcome	5.071	47.752	1163.968
X (1 – Activation Dummy)	(1.39)	(3.11)	(2.89)
Performance outcome	2.860	11.231	344.213
X (Activation Dummy)	(1.06)	(0.53)	(0.67)
P value of test of equivalence of above coefficients	0.5909	0.1235	0.1652
Degrees of Freedom		468	
R^2		0.4945	

Notes: T statistics in parentheses. The six equations are estimated in a single SUR framework. Each equation pools the activation and deactivation subsamples and allows separate intercepts for the two subsamples, but only the estimates of the coefficients on the (interacted) performance outcomes are reported. For each equation, we dropped any subgroup that was void of individuals from one or both of the incentive regimes. This assured that the number of subgroups (N) was identical for the two subsamples and that the test of regression coefficient equality was also a test of the equality of Cov(V,P) (see text). Regressions are weighted by the inverse of the Eicker-White standard errors from the impact estimations. Earnings are trimmed in construction of impact estimates.
*The dependent variable is weighted by the sum of squared deviations of the performance outcome about its mean.

Table 7

Table 7
R^2's and P Values and from Least Square Regressions of Impacts on Outcomes
By Whether Performance Measure Activated*

Dependent Variable	Performance Measure Activated	Employment Rate at Follow-Up				Average Weeks Worked at Follow-up				Average Weekly Earnings at Follow-up			
		Obs.	Coef. Est.	P Value	R^2	Obs.	Coef. Est.	P Value	R^2	Obs.	Coef. Est.	P Value	R^2
Earnings Impact	No	48	1167.20	0.0008	0.215	48	72.40	<0.0001	0.370	48	2.84	<0.0001	0.383
	Yes	43	1039.18	0.0003	0.270	43	-16.51	0.6756	0.004	43	-0.47	0.7336	0.003
Employment Impact	No	48	0.56	0.0204	0.109	48	0.01	0.3632	0.018	48	0.00	0.1761	0.039
	Yes	43	0.11	0.6244	0.006	43	0.00	0.9613	0.000	43	0.00	0.8498	0.001

* This table describes the slope coefficient estimates and R^2's of 12 regressions of impacts on performance outcomes. For each of the six outcome-impact combinations, we perform two regressions: one for individuals trained in regimes with the corresponding performance measure activated and one for individuals without the performance measure activated. Regressions are weighted by the inverse of the Eicker-White standard errors from the impact estimations. Earnings are trimmed in construction of impact estimates.