An Empirical Framework for Testing Theories About Complementarity in Organizational Design

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ABSTRACT: This paper studies alternative empirical strategies for estimating the effects of organizational design practices on performance, as well as the factors which determine organizational design, in a cross-section of firms. Our economic model is based on a firm where multiple organizational design practices are endogenously determined, and these organizational design practices affect output through an "organizational design production function." The econometric model includes unobserved exogenous variation in the costs and returns to each of the individual practices. The model is used to evaluate how different econometric strategies for testing theories about complementarity can be interpreted under alternative assumptions about the economic and statistical environment. We identify plausible hypotheses about the joint distribution of the unobservables under which several different approaches from the existing literature will yield biased and inconsistent estimates. We show that the sign of the bias depends on two factors: whether the organizational design practices are complements, and the correlation between the unobserved returns to each practice. We find several sets of conditions under which the sign of the bias can be determined, and we provide economic interpretations. Our analysis shows that for a particular set of hypotheses, a variety of different procedures may all yield qualitatively similar biases, presenting a challenge for the identification of complementarity. We then propose a structural approach, which is based on a system of simultaneous equations describing productivity and the demand for organizational design practices. As long as exogenous variables are observed which are uncorrelated with the unobserved returns to practices, the structural parameters are identified, yielding consistent tests for complementarity as well as the cross-equation restrictions implied by static optimization of the organization's profit function.

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1. Introduction

Until recently, empirical analyses of firms focused almost exclusively on labor demand, investment and productivity. Little consideration was given to internal organizational design choices (such as the adoption of a training or incentive program). Indeed, most empirical studies of factor demand or productivity either abstract away from organizational design or consider at most a single dimension. However, a recent theoretical and empirical literature emphasizes the potential importance of *interactions* between different elements of organizational design. A major finding of this literature is that organizational design practices are "clustered": the adoption of practices is correlated across firms, and some "sets" of practices consistently appear together. Economic theory suggests that such clustering might arise if the choices are complements. Recent empirical work builds directly upon this theoretical analysis and explicitly "tests" for complementarity among practices using a variety of approaches.[1] However, most of these studies have neither recognized nor accounted for the potential impact of unobserved variation in the costs and benefits of organizational design practices.

In this paper, we develop an econometric framework that can be used to provide a more complete evaluation of why practices appear together and how joint adoption affects firm-level productivity. We use this model to analyze the sources of bias that may be present in many of the econometric approaches used in the literature, and we formally analyze conditions under which the bias can be signed and interpreted. We further provide sufficient conditions for identification of the structural parameters of the "organizational design production function" and a consistent test for complementarity. Our analysis is tailored for cross-sectional applications where many firms face similar production technologies, make comparable choices about organizational design, but face different costs or benefits to adoption. For example, retail service outlets (such as a bank branch or a customer service center) are designed to accomplish similar goals, but operate in economic environments that differ in demographic characteristics, labor regulations, or technological infrastructure. These organizations make choices about practices such as on-the-job training, pre-employment screening and educational requirements,

[1] The early empirical work suggesting that practices may be clustered includes Anderson and Schmittlein (1984). Milgrom and Roberts (1990) and Holmstrom and Milgrom (1994) provide systematic theoretical treatments of complementarity. More recent work which is focused on "testing" for complementarity includes Arora and Gambardella (1990), Ichniowski, Shaw, and Prennushi (1997), Brickley (1995), and Brynjolfsson and Hitt (1998).

job guarantees, explicit incentives and bonuses, and the use of advanced telecommunications and information technology.

In this endeavor, we are motivated by the policy implications that follow if practices are interrelated in adoption and productivity. For example, if a training subsidy affects the adoption of training programs, it will also have indirect effects on the adoption and productivity of complementary practices, such as a commitment to job security. Consequently, optimal subsidies need to account for both direct and indirect effects on organizational design. Similarly, complementarity between a set of practices implies that the adoption of one practice has externalities for adoption decisions about other practices; thus, to explain cross-sectional variation in one practice, it may be necessary to identify exogenous variation in the returns to complementary practices.

Interactions between elements of organizational design present several distinct empirical challenges that do not normally arise in the context of productivity analysis. First, in contrast to analyses of traditional factor inputs such as capital or labor, the econometrician does not typically observe the relevant "input prices" that each firm faces in adopting organizational practices. For this reason, the tools of duality, which are exploited throughout the productivity literature, are not applicable, and it is thus necessary to confront the difficulties associated with direct estimation of the production structure. In particular, we must allow for the possibility that the costs and benefits to employing practices might vary across firms, and yet be unobservable.

A second challenge arises from the fact that some sets of practices are usually adopted in clusters (as theory would suggest, when practices are complements). The presence of clustering clearly implies that some combinations of practices will occur only infrequently, and so it may be difficult to precisely estimate the parameters describing the interactions between these parameters using a regression of productivity on practice combinations. We accommodate this potential difficulty by exploiting revealed preference: the fact that firms have chosen to adopt practices together is potentially informative about the joint returns to the practices. Formally, we can exploit a cross-equation restriction between the equations that describe practice adoption and the production function.

Our formal analysis begins by introducing a model of an "organizational design production function," with parameters that specify the interactions between choices, as well as exogenous variables that determine the costs and benefits of each practice. The firm chooses a set of "organizational design practices" taking into account the relevant costs and benefits of adoption. We focus on a model of a cross-section of firms facing stable production parameters, and thus abstract away from issues of the diffusion of organizational design practices and the dynamics of adoption decisions.

As discussed above, a central component of our analysis is the presence of exogenous variables that are observed by the firm but not the econometrician. These variables are the source of variation in firm practices that cannot be explained by observables but affect the marginal returns or costs of adoption.[2] Building on recent advances in the econometrics literature,[3] this paper establishes conditions under which the parameters of the production function and the joint distribution of unobservables are identified.[4] In the most general model, each *combination* of practices, or "system," (for example, the joint use of training programs, job security, and incentive pay) is subject to a random shock. We call this a Random Systems Model (RSM). The RSM model is a specific application of the general "switching regressions" model (Heckman and MaCurdy, 1986) of an agent choosing between several discrete choices; in our context, the discrete choices are systems of organizational design practices. In such a model, only the distribution of interactions among practices is identified; practices may be complements for some firms, and substitutes for others. Further, without additional assumptions, it is impossible to test whether the adoption of practices is consistent with static optimization on the part of the firm.

We then identify a testable restriction on the Random Systems Model, which we call the Random Practice Model (RPM), which allows for sharper empirical tests and policy predictions. The RPM essentially assumes that there is no unobserved variation in the *interactions* between practices. This is analogous to assuming a constant elasticity of substitution between inputs.

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[2] In problems of internal organizational design, these variables correspond to factors such as the talent and past experiences of managers and workers, the beliefs held within the firm about current and future market conditions, labor-management relations, the formal and informal processes for adopting changes in organizational design in a given firm, the influence exerted by various interest groups within the firm, and other adjustment costs.

[3] In particular, we draw from the literature about semi-parametric models of discrete choice and switching regressions; for example, see Heckman and Honore (1990), Ichimura and Lee (1991), or Thompson (1989).

[4] Of course, we must assume that the econometrician observes *some* exogenous elements affecting the costs or benefits of adoption, a condition required for identification.

Thus, the RPM incorporates an unobserved return to each individual practice, but that unobserved return does not depend on the adoption of other practices.

We use this formal structure in several ways. We first focus on the conditions under which a set of empirical procedures used in the existing literature provides a consistent test for complementarity between a given pair of practices. To do so, we distinguish between the two conditions: (TC) the practices are complements in the organizational design production function, and (TI) the practices are technologically independent. Previous empirical studies have attempted to distinguish between (TC) and (TI) in two main ways: first, by testing whether the practices are positively correlated, conditional on observables; and second, by using OLS or instrumental variables approaches to estimate the parameters of a productivity equation and test whether the interaction effects are positive.

Our analysis highlights how particular forms of unobserved heterogeneity bias the test statistic from these procedures in specific directions. Consider the following two alternative assumptions about the unobserved returns to practices: (a) the unobserved returns among practices are affiliated (a strong form of positive correlation) and (b) the unobserved returns are independent. Even when the choices do not interact in determining productivity (TI), the presence of positive correlation between the unobserved returns to the two different practices yields (i) positive correlation in adoption among practices and (ii) a positive estimate of the interaction effect in an OLS or 2SLS productivity regression. More generally, positive correlation in the unobservables results in a force for a positive bias in the estimate of interaction effects in a productivity regression.

In contrast, complementarity between practices (TC) results in a competing effect for the direction of the bias: (TC) creates a force towards *understating* interaction effects. Under (TC), adopting a given practice (such as a training program) leads to a less favorable selection of firms adopting complementary practices (such as higher skill requirements). If the unobserved returns to practices are independent and (TC) holds, the bias on the interaction effect will always be negative. While each of these biases are specific examples of selection biases (as analyzed by Heckman (1974) or Heckman and MaCurdy (1986)), the nature of selection biases which arise from statistical and technological interactions *between* choices has not received attention in the existing literature.

Our second use of the formal model is to analyze the properties of a structural estimator of the parameters with two main features: (i) it explicitly models the distribution of the unobserved heterogeneity, and (ii) it includes a system of equations, including an equation describing productivity and a set of equations describing the practice adoption decisions. There are several advantages to using such a structural approach in the context of this problem. First, by accounting for the unobserved heterogeneity, it is possible to obtain consistent estimates of the parameters of the organizational design production function as well as the covariance between the unobserved returns to different organizational design practices. Second, our model nests all prior models we are aware of, and so direct comparisons can be made between the implicit assumptions associated with previous approaches. Third, by specifying an internally consistent simultaneous equations system, we can impose the cross-equation restrictions on the interaction effects; as suggested above, since organizational design practices are often positively correlated in applications, the use of revealed preference can yield substantial efficiency gains. Finally, (if the Random Practice Model is appropriate) we can perform tests about the process that leads to the adoption of practices, including whether or not practice adoption appears to be consistent with optimal behavior.

The paper proceeds as follows. Section 2 presents the formal economic model. Section 3 analyzes the implicit assumptions and potential biases associated with prior approaches. Section 4 considers the identification and properties of a structural model. Section 5 discusses issues for data collection and survey design; a final section concludes.

2. The Model

In this section, we develop a model that can be used to analyze cross-sectional data on the adoption and productivity of organizational design practices. The model is general enough to incorporate alternative assumptions about the following elements: (i) the nature of interaction effects between practices in the "production function," (ii) the mechanism through which practice adoption decisions are determined, and (iii) the nature of the joint distribution of unobserved returns.

The model is tailored to applications where there are organizations with similar objectives and options available operating in heterogeneous economic environments, at a given point in time.[5] To see a very specific example, consider a hypothetical telephone customer service center whose goal is maximize the number of customers served by each call-taker, subject to a quality constraint. The firm makes choices about two practices that may improve productivity: the level of worker training and the adoption of advanced computing equipment. Costs and benefits to adoption may vary in observable and unobservable ways. For example, worker expectations about their job tenure affect their return to making specific investments, thus influencing the firm's incentive to provide the training course. The benefits to computer technology may depend on the previous experience of managers and workers with similar technologies. In this context, we may wish to test whether computers and training are complementary. In order to conduct such a test, it will also be important that some factors that affect adoption, but not productivity in use, are observed. For example, some call centers might be in states where training is subsidized, and there might be variation in the pre-existing telecommunications infrastructure which affects the costs of adopting a system with caller identification or other advanced features.

We proceed by developing an abstract model of the organizational design production function and the firm's "demand" for organizational design practices; we then introduce econometric assumptions about observability and distinguish between different types of unobserved heterogeneity and the tradeoffs associated with different restrictions on the stochastic structure.

2.1. The Organizational Design Production Function

This section introduces the *organizational design production function* and the restrictions on that function implied by complementarity among practices. The notation is summarized in Table 1. We consider a firm t where a vector of J practices, denoted $\mathbf{y}^t = (y_1^t, ..., y_J^t)$, is endogenously determined. We focus on the case where each of the practices y_j^t is a discrete choice from {0,1}, resulting in 2^J distinct combinations or organizational "systems." A training program would be considered a practice, while a system is described by the choices the firm makes about the whole vector of practices. When there are two practices to be adopted,

[5] This focus allows us to highlight both the assumptions required for identification as well as the difficulties that arise from reduced-form analysis in a cross-sectional setting. The many interesting issues associated with panel

$\mathbf{y}^t \in \{0,1\} \times \{0,1\}$. The "systems" in this model are the practice combinations $\mathbf{y}^t \in \{(1,1),(1,0),(0,1),(0,0)\}$.

The productivity of different combinations of organizational design elements varies across firms from two sources: *practice-specific* and *system-specific*. First, system-specific exogenous variables, denoted $\mathbf{Z}^t = (Z_1^t, \ldots, Z_K^t)$, change the returns to the *joint* adoption of practice combinations. On the other hand, practice-specific exogenous variables, denoted $\mathbf{X}^t = (X_1^t, \ldots, X_J^t)$, affect the incremental gain in productivity from adopting a practice irrespective of what other elements are adopted.[6]

Productivity, denoted f^t, is determined as a function of these variables, $f^t = f(\mathbf{y}^t, \mathbf{X}^t, \mathbf{Z}^t; \mathbf{M})$, where $\mathbf{M}=(\theta,\alpha,\beta)$. The system-specific return to system $\mathbf{k} \in \{0,1\}^J$ is $\theta_{\mathbf{k}} + \alpha_{\mathbf{k}} Z_{\mathbf{k}}^t$, while the practice-specific payoff for practice j is $\beta_j^0 X_j^{0,t}$ if y_j^t=0, $\beta_j^1 X_j^{1,t}$ if y_j^t=1. For the two-choice example, then, the functional form for productivity is:

$$\begin{aligned} f(\mathbf{y}^t, \mathbf{X}^t, \mathbf{Z}^t; \mathbf{M}) = {} & (1-y_1^t)(1-y_2^t) \cdot \left[\theta_{00} + \alpha_{00} Z_{00}^t\right] + (1-y_1^t) y_2^t \left[\theta_{01} + \alpha_{01} Z_{01}^t\right] \\ & + y_1^t (1-y_2^t) \cdot \left[\theta_{10} + \alpha_{10} Z_{10}^t\right] + y_1^t y_2^t \cdot \left[\theta_{11} + \alpha_{11} Z_{11}^t\right] \\ & + (1-y_1^t) X_1^{0,t} \beta_1^0 + y_1^t X_1^{1,t} \beta_1^1 + (1-y_2^t) X_2^{0,t} \beta_2^0 + y_2^t X_2^{1,t} \beta_2^1 + \varepsilon^t \end{aligned} \tag{1}$$

Equation (1) highlights several key features of the model. First, the model allows for interactions between the practices: the productivity of one practice depends on the adoption decisions about the other practices. Second, the productivity of individual practices varies across firms, as formalized in the interaction between **X** and **y**. Third, complementarity among practices is a parametric restriction on the organizational design production function. Recall a formal definition for complementarity and supermodularity:

Definition *Two practices y_i and y_j are* **complements** *in the objective function f if the following inequality holds for all values of the other arguments of f:*

$$f(y_i^H, y_j^H, \cdot) - f(y_i^L, y_j^H, \cdot) \geq f(y_i^H, y_j^L, \cdot) - f(y_i^L, y_j^L, \cdot) \tag{2}$$

$f(\mathbf{y},\cdot)$ *is* **supermodular** *in* **y** *if y_i and y_j are complements for all $i \neq j$.*

When there are only two choices (1 and 2), then these choices are complements if

data and the dynamics of practice adoption are left for future research.

[6] We will draw a distinction between these two different sources of variation throughout the paper, so we maintain separate notation for them despite the fact that the practice-specific exogenous variables are just a

$$\kappa_{12} = ([\theta_{11} + \alpha_{11} Z_{11}^t] - [\theta_{01} + \alpha_{01} Z_{01}^t]) - ([\theta_{10} + \alpha_{10} Z_{10}^t] - [\theta_{00} + \alpha_{00} Z_{00}^t]) \geq 0 \tag{3}$$

It is straightforward to see that, in the presence of $\mathbf{Z}^t$, there is no single level of complementarity between practices across firms. However, in the absence of $\mathbf{Z}^t$, the interaction effects are fixed across firms and (3) reduces to a parametric (and linear) inequality restriction:

$$\kappa_{12} = (\theta_{11} - \theta_{01}) - (\theta_{01} - \theta_{00}) \geq 0 \tag{4}$$

Intuitively, (4) says that the returns to adopting practice 1 are higher when practice 2 has been adopted.[7] Finally, it is sometimes useful to rewrite (1) in a way that highlights its analogy to a switching regressions model (Heckman and MaCurdy, 1986):

$$f(\mathbf{y}^t, \mathbf{X}^t, \mathbf{Z}^t; \mathbf{M}) = \begin{cases} \theta_{00} + Z_{00}^t \alpha_{00} + X_1^{0,t} \beta_1^0 + X_2^{0,t} \beta_2^0 & \text{if } \mathbf{y}^t = (0,0) \\ \theta_{01} + Z_{01}^t \alpha_{01} + X_1^{0,t} \beta_1^0 + X_2^{1,t} \beta_2^1 & \text{if } \mathbf{y}^t = (0,1) \\ \theta_{10} + Z_{10}^t \alpha_{10} + X_1^{1,t} \beta_1^1 + X_2^{0,t} \beta_2^0 & \text{if } \mathbf{y}^t = (1,0) \\ \theta_{11} + Z_{11}^t \alpha_{11} + X_1^{1,t} \beta_1^1 + X_2^{1,t} \beta_2^1 & \text{if } \mathbf{y}^t = (1,1) \end{cases} \tag{1$'$}$$

The switching regressions model (1′) takes the "system" as the unit of analysis, while the production function model (1) emphasizes that each system is composed of a set of practices, each of which comes with a separate set of benefits and costs.

2.2. *Demand for Organizational Design Practices*

The mapping between the exogenous variables and firm's choices about practice adoption describes the firm's "demand" for these practices and so is, to some extent, analogous to a standard factor demand equation. Since $\mathbf{X}^t$ and $\mathbf{Z}^t$ affect the returns to each of the practices, demand clearly depends on these variables.[8] As well, there may be factors which affect adoption but *not* productivity (such as regulation or the presence of subsidies), and so we incorporate practice-specific exogenous variables, $\mathbf{W}^t = (W_1^t, \ldots, W_J^t)$, and system-specific variables, $\mathbf{U}^t = (U_1^t, \ldots, U_K^t)$. The demand for a given practice is thus determined according to

special case of system-specific exogenous variables (as is further discussed in Section 4.1).

[7]When there are multiple choices, there will be a distinct inequality corresponding to each combination of choices about the other practices.

[8] Since elements of $\mathbf{X}^t$ and $\mathbf{Z}^t$ may be unobserved, the relationship between demand shocks and productivity will have important consequences for our empirical approach. McElroy (1986) examines the properties of this relationship for the standard production function with continuous capital and labor inputs, and shows that the errors in the factor demands have the interpretation as shocks to the marginal returns to factor inputs.

$y_j^t = D_j(\mathbf{X}^t, \mathbf{Z}^t, \mathbf{W}^t, \mathbf{U}^t; \Lambda)$, and a policy of optimal adoption implies:

$$\mathbf{y}^* = \arg\max_{\mathbf{y}} \pi(\mathbf{y}^t, \mathbf{X}^t, \mathbf{Z}^t, \mathbf{W}^t, \mathbf{U}^t; \mathbf{M}, \mathbf{N}) \equiv f(\mathbf{y}^t, \mathbf{X}^t, \mathbf{Z}^t; \mathbf{M}) + g(\mathbf{y}^t, \mathbf{W}^t, \mathbf{U}^t; \mathbf{N}) \qquad \text{(OPT)}$$

(OPT) highlights two issues for our analysis. First, any estimation strategy that exploits (1) and (OPT) will include (potentially testable) cross-equation restrictions. In that case, the demand parameters Λ are determined as a function of the parameters of the production function and adoption costs, $\mathbf{M}$ and N. Second, π represents the objective function faced by the agent responsible for decision-making, which may be different than the overall economic profits of the firm.[9]

Now consider a condition under which we can speak unambiguously about the effect of *increasing* any of the practice-specific exogenous variables,

π is supermodular in (y_j, X_j^1), $(y_j, -X_j^0)$, (y_j, W_j^1), and $(y_j, -W_j^0)$ for all j and all $\mathbf{Z}$,$\mathbf{U}$. (ORD)

The restrictions on $\mathbf{X}$ in (ORD) are implicit in our definition of the model (1), and (ORD) simply extends these restrictions to the portion of profits not included in *f*. Under (ORD), we can apply results from Milgrom and Roberts (1990) and Topkis (1978) to state the conditions under which robust comparative statics predictions are available:

Proposition 1 *Assume OPT, ORD. For fixed* $\mathbf{Z}$ *and* $\mathbf{U}$, *if* π *is supermodular in* $\mathbf{y}$, *then* $\mathbf{y}^*$ *is monotone nondecreasing in* (W_j^1, X_j^1) *and monotone nonincreasing in* (W_j^0, X_j^0).

The proposition states that if all of the choice variables are mutually complementary, an increase in the exogenous returns to one choice will lead to mutually reinforcing increases in all of the endogenously determined practices.[10] This proposition will play an important role in our analyses of alternative econometric approaches in Section 3.

2.3. *Observability Assumptions: The Random Systems Model & the Random Practice Model*

Exogenous variation in the model arises from differences in the economic environment faced by each firm, such as the costs of factors of production, as well as features of the firm's location, market, and regulatory environment. If we wish to incorporate the possibility that

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[9] This caveat is particularly important when *f* is *unobserved*. In that case, inferences about complementarity will be based solely on revealed preference, and so our assumptions about the adoption process will be critical.

[10] For additional discussion on the modeling issues associated with organizational design and complementarity,

firms choose practices to maximize profits in an application where the observables do not fully account for variation in practice adoption, it is necessary to accommodate the possibility that there are some exogenous costs and benefits to adoption which are observed by each firm but unobserved by the econometrician. This unobserved heterogeneity can be incorporated into the economic model by parsing each of the elements of exogenous variation ($\mathbf{X}^t$,$\mathbf{Z}^t$,$\mathbf{W}^t$,$\mathbf{U}^t$) into an observed component (indicated in lowercase Roman typeface) and an unobserved component (lowercase Greek typeface). That is, we let $X_j^t = (\mathbf{x}_j^t, \boldsymbol{\chi}_j^t)$, $Z_{\mathbf{k}}^t = (\mathbf{z}_{\mathbf{k}}^t, \boldsymbol{\zeta}_{\mathbf{k}}^t)$, $W_j^t = (\mathbf{w}_j^t, \boldsymbol{\omega}_j^t)$, and $U_{\mathbf{k}}^t = (\mathbf{u}_{\mathbf{k}}^t, \boldsymbol{\upsilon}_{\mathbf{k}}^t)$.

Our interpretation of the data will depend critically on our assumptions and findings concerning the nature of *unobserved* heterogeneity $(\boldsymbol{\zeta}, \boldsymbol{\upsilon}, \boldsymbol{\chi}, \boldsymbol{\omega})$. We distinguish between two different models of the overall source of heterogeneity: models with incorporate system-specific shocks, $(\boldsymbol{\zeta}, \boldsymbol{\upsilon})$ (which we call the Random Systems Model or RSM); and models which accommodate only the practice-specific shocks $(\boldsymbol{\omega}, \boldsymbol{\chi})$ (a Random Practice Model or RPM). The RPM is simply a restriction on the RSM; in the two-choice case described in (1) and (1′), it requires that for a given $\boldsymbol{\zeta}$, there exists a $\boldsymbol{\chi}$ such that $\zeta_{00}^t + \zeta_{11}^t = \zeta_{01}^t + \zeta_{10}^t = \chi_1^{0t} + \chi_2^{0t} + \chi_1^{1t} + \chi_2^{1t}$, and likewise for $\boldsymbol{\upsilon}$ and $\boldsymbol{\omega}$. That is, the unobserved return to any system can be composed into two parts, one for each practice, where these returns do not depend on choices about other practices.

Substantively, the main difference between these models is that the RPM imposes fixed unknown coefficients on all practice interaction effects. RPM might be an appropriate restriction when the differences across firms arise from regulatory or technological constraints associated with the exploitation of individual practices (such as work rule restrictions or the quality of equipment and facilities), while a RSM might be more appropriate when managers vary in their talent at exploiting combinations of practices. The RSM is required in the presence of an *unobserved* endogenous variable that interacts with more than one other choice, since its adoption will simultaneously change the value of adopting all other elements.

As discussed in the introduction, the RPM is analogous to a standard assumption in the productivity literature. Productivity models typically exploit duality, positing a "flexible"

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see Topkis (1978), Milgrom and Roberts (1990), and Athey, Milgrom, and Roberts (1996).

functional form for a firm's cost function which is linear in parameters but nonlinear in factor prices (Christensen, et al, 1973; McElroy, 1985; Jorgensen, 1995). In such models, it is typical to assume that the elasticity of substitution between factors (capital and labor) is fixed across firms. The RPM assumption is analogous: the RSM allows the elasticity of substitution between practices to vary across firms, while the RPM holds the interaction effect fixed.[11] Continuing the analogy, the variables X_j and W_j can be interpreted as analogous to the "prices" for practice j. In addition to the discreteness of our model, it is the fact that some of these prices are unobserved which restricts us from using the duality approach.

While the RPM thus seems to be a standard assumption, it should be emphasized that specifying the organizational design production function entails ambiguities which, while present in the traditional problem, are more difficult to ignore in the context of organizational design. Most importantly, it is difficult to identify from theory what the "factors" of production are, and more difficult to argue that all factors have been appropriately accounted for. Thus, we approach identification and estimation with the more general RSM model, and propose that the RPM assumption be tested before it is imposed.

3. The Impact of Unobserved Heterogeneity on Prior Tests for Complementarity

This section uses the formal model to examine the nature of the bias associated with several econometric procedures from the existing literature on complementarity in organizational design.[12] Our analysis highlights the precise assumptions that are required in order for these procedures to provide a consistent test statistic for complementarity. Section 3.1 reviews the two most common approaches used in the prior literature and shows that both are subject to bias due to unobserved heterogeneity; in Section 3.2, we argue that a third approach, based on

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[11] Consequently, under the RSM, we will only be able to make statements about the average level of complementarity across firms, or more generally, the distribution over complementarity parameters. In such cases, welfare computations will depend critically on the entire distribution of unobservables in the population; in contrast, if two practices are complements to all firms, we can make qualitative predictions about the effect of a policy without relying on estimates of other features of the economic environment.

[12] While our analysis will focus on procedures implemented within the economics literature, other social scientists (most notably sociologists and psychologists) have attempted to measure the effects of organizational design as well. The principal alternative statistical procedures used by other social scientists can be implemented with the software package, LISREL (Joreskog and Sorbom, 1995), which is used to estimate the parameters associated with systems of linear simultaneous equations, where firm practices may be discrete and measured with several indicator variables. LISREL does not explicitly account for the presence of unobserved heterogeneity

exclusion restrictions, cannot disentangle complementarity in the presence of more than two choice variables or under the RSM.

3.1 Unobserved Heterogeneity and Econometric Approaches to Complementarity

The first approach we examine is based on revealed preference (and thus implicitly makes an assumption of optimizing behavior). If y_i and y_j are complements, then, under the conditions of Proposition 1, a change in ω_i or χ_i will have a direct effect on the probability that y_i is adopted, which will in turn increase the probability that y_j is adopted. Thus, complementarity creates a force in favor of positive correlation (or "clustering") between y_i and y_j, even after controlling for observable, exogenous characteristics. This insight, analyzed theoretically in Holmstrom and Milgrom (1994), Arora and Gambardella (1990), and Arora (1996), motivates the use of the (CORR) approach:

> **(CORR)** Test whether the correlation among practices is positive, conditional on observables.

A substantial benefit to this approach is that it can be used even if only the adoption decisions are observed. Most of the recent empirical papers about complementarity use this approach,[13] at least as supporting evidence.

The second main approach has been to build on the empirical productivity literature. This approach relies on an OLS or 2SLS regression of a measure of productivity on a set of regressors, including interaction effects between different practices:

> **(PROD)** Estimate complementarity parameter from interaction effects in OLS or 2SLS estimation of the organizational design production function.

In the simple case of two practices, PROD requires the estimation of

$$f^t = \mathbf{1}_{00}^t\theta_{00} + \mathbf{1}_{01}^t\theta_{01} + \mathbf{1}_{10}^t\theta_{10} + \mathbf{1}_{11}^t\theta_{11} + \beta_{1t}x_{1t}y_{1t} + \beta_{2t}x_{2t}y_{2t} + \xi_t, \qquad (5)$$

where $\mathbf{1}_{ij}^t$ is an indicator variable for observing the practice combination (i,j), and the

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and so is subject to the same biases that are associated with the procedures reviewed in this section.

[13] Arora and Gambardella (1990) introduce a formal analysis of (CORR) as a test for complementarity. Brickley (1995) explicitly uses (CORR) as a test for the comparative static predictions of Holmstrom and Milgrom (1994) in the context of franchising contract provisions. Other recent studies which use this approach include Ichniowski, Shaw and Prennushi (1997), Brynjolfsson and Hitt (1998), Colombo and Mosconi (1995), Greenan et al (1993), Helper (1995), Helper and Levine (1993), Hwang and Weil (1996), Kelley, Harrison, and McGrath (1995), MacDuffie (1995), and Pil and MacDuffie (1996).

corresponding test statistic for complementarity is $\kappa \equiv \theta_{11} - \theta_{01} - [\theta_{10} - \theta_{00}]$. Throughout the remainder of our analysis, we let $\hat{\kappa}^{OLS}$ and $\hat{\kappa}^{2SLS}$ represent the estimates of κ derived from OLS and 2SLS in (5), respectively. Of course, 2SLS estimation requires instruments, and so its implementation requires an exclusion restriction, satisfied under the following assumption:

> There exists a vector **w** such that, for all *i*, $\pi(y_i, \mathbf{w}_i ;\cdot)\big|_{y_i=1} - \pi(y_i, \mathbf{w}_i ;\cdot)\big|_{y_i=0}$ varies with $\mathbf{w}_i$ but not with $\mathbf{w}_j$, for all $j \neq i$. (EXCL)

Of course, EXCL is not sufficient for the consistency of 2SLS; we will discuss conditions under which 2SLS is appropriate below. An OLS version of PROD has been implemented by several authors, most notably in Ichniowski, Shaw and Prennushi (1997, hereafter ISP), which presents a careful study of the impact on productivity of adopting different combinations of human resource practices in steel finishing lines. Like many such studies, ISP's data is gathered through detailed primary source surveys; unfortunately, the ISP data do *not* include potential instruments (and so 2SLS cannot be implemented).[14]

We discuss these two procedures with reference to different maintained hypotheses about the nature of unobserved heterogeneity and complementarity. One of the principal distinctions we will make is between cases where the practice-specific unobserved heterogeneity is composed of independent elements, versus *affiliated* elements. Affiliation is just a strong form of correlation: a vector **x** of random variables is *affiliated* if it has a joint density, $g(\mathbf{x})$, such that $\log(g(\mathbf{x}))$ is supermodular. Affiliation implies that for all $i \neq j$ and all nondecreasing functions g_i and g_j, $\text{cov}(g_i(x_i), g_j(x_j)) \geq 0$ (the latter property is called association), and further, this property holds *conditional* on any set of the form $\times_i [a_i, b_i]$. For simplicity, we will analyze the specific case in which the unobserved returns to choosing y_j=0 are identically 0, and so χ_j and ω_j can be defined as the unobserved returns and costs to adopting practice *j*.[15] Moreover, we will focus on a two-choice model, and we will abstract away from system-specific variation, $\mathbf{Z}^t$ and $\mathbf{U}^t$. The richness of the random systems model is not required to establish the weaknesses of (PROD) and

[14] ISP also mirror the more general literature in that their data is subject to extensive "clustering" so that many practice combinations are simply not observed (which preclude a direct estimate of the relevant parameters in (5)); their solution is to construct system "indexes" which aggregate over observed practice combinations.

[15] In this case, affiliation implies positive correlation among elements of χ or ω. This simple case is being imposed simply to ease the exposition for analyzing prior procedures; Section 4 considers the general model where unobserved returns impact the firm whether or not each practice is adopted or not.

(CORR), and the simple RPM allows us to evaluate several interesting economic and statistical environments. We define our main cases in Table 2.

TABLE 2	
Label	**Description**
Assumptions about complementarity	
TI	$\kappa = 0$
TC	$\kappa > 0$
Assumptions about practice-specific unobservables	
WI	ω^t is present, but ω^t is an independent vector.
WC	ω^t is present and strictly affiliated.
X0	χ^t is not present.
XI	χ^t is present, but χ^t is an independent vector.
XC	χ^t is present and strictly affiliated.

The goal of our formal analysis in this section is to derive sufficient conditions under which the biases in the procedures (PROD) and (CORR) can be signed and interpreted. One of our main results is to identify plausible economic assumptions under which productivity (PROD) and adoption (CORR) deliver biases in the *same* direction. For example, when the unobserved returns to practice adoption are affiliated, then the conditional correlation between the practices will be positive, and further, the estimates of interaction effects from OLS and 2SLS will also be biased upward. Thus, even if two distinct approaches are used to provide evidence about complementarity, one based on revealed preference (CORR) and the other on productivity (PROD), it is impossible to rule out alternative explanations. This finding further motivates the structural estimator developed in Section 4.

All of the propositions in this section will rely on the assumption that practices are optimally chosen (OPT). Without an assumption about practice adoption, it will be impossible to relate the primitives of the formal model to observable choices, a prerequisite to signing the biases from different procedures. Certainly, it would be unsatisfying for an economic analysis to *preclude* the hypothesis that choices respond to the economic environment; thus, our propositions should be interpreted as suggestive of the biases that could potentially arise if we allow for optimizing behavior on the part of the firms. Other behavioral assumptions would lead to different interpretations of the bias. It should be emphasized that despite its role in this section, the estimation procedure we propose in Section 4 does *not* require (OPT), and in Section 4.4 we discuss conditions under which (OPT) can be tested.

Finally, in order to compare the approaches (PROD) and (CORR), it will be useful to make the following assumption, which requires that there are no interactions between the practices outside the production function:

$$g(\mathbf{y},\mathbf{W},\mathbf{U};\mathrm{N})=\pi(\cdot)-f(\cdot) \text{ is additively separable in } \mathbf{y}. \tag{NI}$$

This assumption allows us to refer to a single measure of complementarity in our comparison of the revealed-preference approach (CORR) and the approach based on measurement of the production function (PROD).

We begin with a positive proposition that shows that, when the unobserved exogenous variables take a particularly simple form, each approach provides a consistent test for complementarity. Consider the case where (WI) holds. In this restriction on the RPM, there is an idiosyncratic shock to the cost of adopting each practice, which is independent across practices and is unobserved by the econometrician.

Proposition 2 *Assume OPT, ORD, EXCL, NI, and* $\mathbf{Z}$=$\mathbf{U}$=0. *Assume further X0 and WI (*$\boldsymbol{\chi}$=0, *and* $\boldsymbol{\omega}$ *is an independent random vector). Then:*
(PROD) $E(\hat{\kappa}^{OLS}) = \kappa$ *and* $E(\hat{\kappa}^{2SLS}) = \kappa$.
(CORR) $\mathrm{Corr}(y_1,y_2|\mathbf{x},\mathbf{w})\geq 0$ *if and only if TC is satisfied (*$\kappa\geq 0$*).*

This proposition is intended to highlight the strength of the assumptions required to draw inferences from reduced-form procedures, and further to serve as a point of comparison for subsequent propositions. However, it is also useful to consider scenarios under which the assumptions might be satisfied. Assumption (WI) corresponds to a scenario where there are random shocks to adoption or implementation costs that are unrelated across practices. Under X0, all of the unobserved heterogeneity enters outside the production function. This might occur if the production technology is similar across firms, the performance measure is a narrow one, and differences in adoption are driven by variation in the fixed cost of adopting practices. For example, ISP use a measure of assembly line productivity, and they provide qualitative evidence to support the hypothesis that their measure of performance is so narrow and their data so detailed that they have ruled out many variables which fit our definition of $\boldsymbol{\chi}$; the assumption they require is that the variation they observe in their data is a consequence of variables which would fit the definitions of $\mathbf{w}$ and $\boldsymbol{\omega}$ in our model, variables which do not directly affect the performance measure. Observe that such an assumption is almost impossible to satisfy in applications such as corporate finance, where the productivity measure is usually associated with a firm-level financial performance measure.

The next two propositions outline our results about the direction of biases when we introduce $\boldsymbol{\chi}$. We begin with a formal analysis of this bias. Assuming a two-choice model and the simple structure for $\boldsymbol{\chi}$ assumed above, we can compute the expected value of f^t conditional on $\mathbf{y}$, as follows:

$$
\begin{aligned}
E[f^t|\mathbf{y}] = \mathbf{1}_{00}^t\theta_{00} + \mathbf{1}_{01}^t \cdot \left[\theta_{01} + E\left[\chi_2 \middle| \mathbf{y}^* = (0,1)\right]\right] + \mathbf{1}_{10}^t \cdot \left[\theta_{10} + E\left[\chi_1 \middle| \mathbf{x}, \mathbf{y}^* = (1,0)\right]\right] \\
+ \mathbf{1}_{11}^t \cdot \left[\theta_{11} + E\left[\chi_2 \middle| \mathbf{y}^* = (0,1)\right] + E\left[\chi_1 \middle| \mathbf{y}^* = (1,0)\right]\right]
\end{aligned} \tag{6}
$$

Because each element of $\boldsymbol{\chi}$ only contributes to productivity (f) when the corresponding element of $\mathbf{y}$ is 1, $\hat{\theta}_{00}^{OLS}$ is unbiased, while $\hat{\theta}_{11}^{OLS}$ has two bias terms (one from each element of $\boldsymbol{\chi}$). Under (OPT), these bias terms are nonzero. Figures 1 and 2 illustrate the regions of the unobservables that correspond to the optimal choices of each practice; notice that the shapes of these regions, and thus the biases, depend on whether or not the practices are complements.

In our application, our focus is not on any specific element of θ, but on the bias associated with κ, the complementarity coefficient. Thus, we are able to calculate the expected value of $\hat{\kappa}^{OLS}$ as follows:

$$
\begin{aligned}
E[\hat{\kappa}^{OLS}] = \kappa + E\left[\chi_1 \middle| \mathbf{y}^* = (1,1)\right] - E\left[\chi_1 \middle| \mathbf{y}^* = (1,0)\right] \\
+ E\left[\chi_2 \middle| \mathbf{y}^* = (1,1)\right] - E\left[\chi_2 \middle| \mathbf{y}^* = (0,1)\right]
\end{aligned} \tag{7}
$$

The last four terms on the right-hand side of (7) will sum to zero only in the case when f is additively separable (i.e., TI) *and* the components of $\boldsymbol{\chi}$ are either uncorrelated (XI) or not present (X0). Moreover, the bias in the estimate $\hat{\kappa}^{OLS}$ cannot be fully eliminated even if (EXCL) is satisfied and 2SLS is implemented. Since the unexplained portion of the productivity equation is given by $\xi_t = \chi_{1t}y_{1t} + \chi_{2t}y_{2t} + \varepsilon_t$, and since $\mathbf{y}$ is endogenously determined as a function of all of the exogenous variables, ($\mathbf{x}$, $\mathbf{w}$, $\boldsymbol{\chi}$, and ω), neither $\mathbf{x}$ nor $\mathbf{w}$ will be valid instruments. That is, $E(\mathbf{w},\xi)\neq 0$ and $E(\mathbf{x},\xi)\neq 0$: the nature of the unobserved heterogeneity in our problem precludes us from assuming that the exogenous variables are independent from the disturbance.

It turns out that under several alternative economic assumptions, the bias in (7) can be signed. To begin, consider the assumption (XI), which requires that the components of $\boldsymbol{\chi}^t$ are independent.

Proposition 3 *Assume OPT, ORD, EXCL, NI, and* **Z=U**=0. *Assume further W0 and XI* (ω=0, *and* $\boldsymbol{\chi}$ *is an independent random vector), and TC* ($\kappa \geq 0$). *Then:*
(PROD) $E(\hat{\kappa}^{OLS}) < \kappa$ *and* $E(\hat{\kappa}^{2SLS}) < \kappa$.
(CORR) $\mathrm{corr}(y_1, y_2 | \mathbf{x}, \mathbf{w}) \geq 0$.

Note that the degree of bias in the productivity equation is not bounded below by zero; even if $\kappa > 0$, it might easily turn out that $E(\hat{\kappa}^{OLS})$ and $E(\hat{\kappa}^{2SLS})$ are both less than zero, yielding a test statistic which would (in expectation) reject complementarity when TC in fact holds. As well, while the CORR procedure is still valid, its power is reduced as the importance of unobserved heterogeneity increases. The intuition for the bias in (PROD) is that under XI and TC, when y_2=1, the returns to y_1 are higher. But in that case, a *lower* value of χ_1 will suffice to generate a choice of y_1=1 (see Figure 1). Thus, the bias from OLS and 2SLS is negative. This is a generalization of the standard single-choice selection bias. Finally, observe that even a finding that κ<0 but $\mathrm{corr}(y_1, y_2 | \mathbf{x}, \mathbf{w}) \geq 0$ has an ambiguous interpretation: it is also consistent with the hypothesis that the choices are substitutes, but ω are affiliated.

Alternatively, consider a case where TI is satisfied (practices are independent from one another in *f*) but the unobserved productivity gains from each practice are affiliated (XC). Such a form for unobserved heterogeneity is plausible in many economic environments; for example, it might arise when the benefits associated with adopting each practices depends on the skill of a single manager or group of workers. In this case, OLS and 2SLS as well as CORR will *overestimate* the complementarity parameter.

Proposition 4 *Assume OPT, ORD, EXCL, NI, and* **Z=U**=0. *Assume further W0 and XC* (ω=0, *and* $\boldsymbol{\chi}$ *is strictly affiliated), and TI* (κ=0). *Then:*
(PROD) $E(\hat{\kappa}^{OLS}) > 0$ *and* $E(\hat{\kappa}^{2SLS}) > 0$.
(CORR) $\mathrm{corr}(y_1, y_2 | \mathbf{x}, \mathbf{w}) > 0$.

This proposition highlights a fundamental identification problem for testing complementarity: both CORR and PROD find complementarity (erroneously) under the same economically interpretable assumptions. Many features of an organization could induce affiliation between the costs and benefits of practices, including the corporate culture and the characteristics of the workforce. The intuition behind Proposition 4 is straightforward: practice

1 is adopted when its unobserved returns are high. But this tends to happen exactly when the unobserved returns to practice 2 are also high, since the unobserved returns are positively correlated, and thus the expected value of the unobserved returns to practice 2 are higher when practice 1 is adopted as well.

It is useful to note that several existing studies attempt to document complementarity by providing (independent) evidence in the spirit of CORR and PROD.[16] Together, Propositions 2-4 show that any interpretation of the OLS or 2SLS estimates require a maintained assumption about the unobservables. Under more general structures, the sign of the correlation between the unobserved exogenous variables can either lead to a finding that complementarities don't exist when they really do; or it can lead to a finding that they are present when the variables of interest are in fact independent.

Finally, our analysis has implications for the interpretations of the bias associated with analyzing the effects of an individual choice on performance when other *endogenous practices* are unobserved by the econometrician. For example, if we wish to study the effects of training programs on productivity, our interpretations of a regression of productivity on adoption will necessarily depend on whether there are other practices (such as the adoption of information technology) which both contribute to productivity, are potentially complementary with training, and are subject to unobserved heterogeneity in adoption. In our two-choice example, suppose that the econometrician observes all of the relevant features which affect the returns to y_1 (so that χ_1=0) and that we are interested in measuring $\theta_{11}-\theta_{01}$. Suppose further that we observe y_2.[17] The presence of unobserved returns to y_2 will lead to an OLS estimate of the returns to y_1 conditional on y_2=1, as follows:

$$\hat{\theta}_{11}^{OLS}-\hat{\theta}_{01}^{OLS}=E[f^t|\mathbf{y}=(1,1)]-E[f^t|\mathbf{y}=(0,1)]=\theta_{11}-\theta_{01}+E\big[\chi_2|\mathbf{y}=(1,1)\big]-E\big[\chi_2|\mathbf{y}=(0,1)\big] \quad (8)$$

An analogous computation can be offered for the 2SLS coefficient. Even in such a simple case, we can show that this parameter will be biased in the face of unobserved heterogeneity in the adoption of other practices:

Proposition 5 *Assume OPT, ORD, EXCL, NI, and* **Z**=**U**=0.

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[16] ISP, Brynjolfsson and Hitt (1998) both provide this sort of hybrid evidence.

[17] The analysis is similar if y_2 is unobserved, but we would need to consider the average returns to y_1.

(a) *Assume further W0 and XI (*ω=0*, and* **χ** *is an independent random vector), and TC (κ≥0). Then* $\hat{\theta}_{11}^{OLS} - \hat{\theta}_{01}^{OLS} < \theta_{11} - \theta_{01}$ *and* $\hat{\theta}_{11}^{2SLS} - \hat{\theta}_{01}^{2SLS} < \theta_{11} - \theta_{01}$.

(b) *Assume further W0 and XC (*ω=0*, and* **χ** *is strictly affiliated), and TI (κ=0).* Then $\hat{\theta}_{11}^{OLS} - \hat{\theta}_{01}^{OLS} > \theta_{11} - \theta_{01}$ *and* $\hat{\theta}_{11}^{2SLS} - \hat{\theta}_{01}^{2SLS} > \theta_{11} - \theta_{01}$.

Proposition 5 highlights why understanding the organizational design production function is important, even when only one practice is of immediate policy relevance. In particular, without considering the full set of organizational practices, one will routinely infer a biased estimate of the returns to individual practices.

3.2 Reduced-Form Procedure Using Exclusion Restrictions

A final approach in the prior literature is explicitly based on the satisfaction of (EXCL) for at least some of the choice variables. Consider the reduced-form regression

$$y_i^t = \alpha_i^t + \lambda_i x_i^t + \delta_i w_i^t + \lambda_j x_j^t + \delta_j w_j^t + \varepsilon_j \qquad (9)$$

As shown by Holmstrom and Milgrom (1994) and Arora (1996), Proposition 1 implies the following:

Proposition 6 *Assume OPT, ORD,EXCL, NI, and* **Z**=**U**=0. *Assume further TC (κ=0). Then* $E[y_i|\mathbf{x},\mathbf{w}]$ *is nondecreasing in* w_j.

Further, under (EXCL), in a two-choice model, only complementarity leads to a positive effect of w_i on y_j: a factor which has its sole direct effect on y_j will be uncorrelated with y_i unless y_j and y_j interact directly in the production function. This motivates:

(RED) Reduced-form tests exploiting exclusion restrictions.

Elements of this approach are present in many of the above-cited studies; for example, Brickley (1995) provides evidence about how several features of franchising contracts change with the degree to which the franchisee relies on repeat business.

Two issues arise in the use of RED, however. First, as discussed in Arora (1996), RED cannot disentangle the nature of interaction between any pair of variables when there are more than two endogenous variables. For example, consider the case in which there are three choice variables, and the following relationships hold: (y_2,y_3) are complements, (y_1,y_3) are complements, and (y_1,y_2) are substitutes. Under these conditions, an increase in w_1 might lead to an increase in all three choices, if the effects through the chain $y_1 \rightarrow y_3 \rightarrow y_2$ outweigh the effects through the chain $y_1 \rightarrow y_2$. Thus, the test based on exclusion restrictions cannot be used to test for complementarity between a particular pair of variables. However, under the assumption that the error is orthogonal to **w**, if the coefficient on an element of **w** is significantly negative, we would reject the hypothesis that *all* pairs are complements, thus providing a useful though incomplete test for an individual pair of practices.

Second, under the RSM, one potentially relevant experiment is whether the *average* level of complementarity is greater than 0. Unfortunately, the coefficients in a regression of the form of (9) are affected by the full joint distribution of ζ. As a result, there is no predetermined relationship between the significance of the exclusion restriction in an adoption equation, and the average level of complementarity in the population. Thus, while RED is not subject to the same inherent difficulties as CORR and PROD as a test for complementarity, it does not provide a general solution for testing complementarity or other properties of the organizational design production function when there are more than two choices.

4. A Structural Approach to Estimation and Testing in Organizational Design

4.1 Identification and the Nature of Unobserved Heterogeneity

This section considers sufficient conditions for the identification of the parameters describing complementarity. We further consider identification of the distributions of the unobserved exogenous variables. We discuss the testability of the (RPM) restriction as well as specific hypotheses about the distribution over unobservables analyzed in Section 3 (such as restrictions on the joint distribution of $\boldsymbol{\chi}$ and ω). Although our analysis in this section does not invoke assumptions about the functional form of the distribution over unobservables, the propositions we discuss cannot in general be sharpened by such functional form assumptions (even assuming a multivariate normal distribution).

For our analysis of identification, we suppress **X** (such variables can be incorporated in **Z**), and normalize $\theta = E[\zeta]$, since these two elements are not separately identified. A general model of productivity with discrete choices is given as follows (where $\mathbf{1}^t_{k_1,\ldots,k_J}$ is an indicator for a system where $y_j = k_j$).

$$f(\mathbf{y}^t;\mathbf{Z}^t,\alpha) \equiv \sum_{\mathbf{k}\in\{0,1\}^J} \mathbf{1}^t_{\mathbf{k}} \cdot \left[\zeta^t_{\mathbf{k}} + \alpha_{\mathbf{k}} \cdot \mathbf{z}^t_{\mathbf{k}}\right] \qquad (10)$$

Further, a discrete choice model for adoption can be built around the following specification for the decision-maker's utility from each system:

$$\pi^t_{\mathbf{k}} = \eta_{\mathbf{k}} \mathbf{z}^t_{\mathbf{k}} + \varphi_{\mathbf{k}} \mathbf{u}^t_{\mathbf{k}} + \zeta^t_{\mathbf{k}} + \upsilon^t_{\mathbf{k}} \tag{11}$$

We let $G(\boldsymbol{\zeta}+\boldsymbol{\upsilon})$ be the joint distribution of $\boldsymbol{\zeta}+\boldsymbol{\upsilon}$, and let $F(\boldsymbol{\zeta})$ be the joint distribution of $\boldsymbol{\zeta}$. An important assumption for identification is given as follows.

Assumption 1 *Assume that* $\mathbf{z}$ *and* $\mathbf{u}$ *are independent of* $(\boldsymbol{\zeta}^t,\boldsymbol{\nu}^t)$, *and that for all* $\mathbf{k}\in\{0,1\}^J$ *there exists a subvector,* $\tilde{\mathbf{u}}_{\mathbf{k}}$ *of* $\mathbf{u}_{\mathbf{k}}$ *such that, (a)* $\tilde{\varphi}_{\mathbf{k}} \neq 0$, *(b)* $\tilde{\mathbf{u}}_{\mathbf{k}}$ *is excluded from* $\mathbf{u}_{\mathbf{l}}$ *for all* $\mathbf{l}\neq\mathbf{k}$, *and (c)* $\Pr(\tilde{\mathbf{u}}_{\mathbf{k}} \in E \,|\, \mathbf{u}\backslash \tilde{\mathbf{u}}_{\mathbf{k}}, \mathbf{z})>0$ *for all* $E\in\Re^m$, *where m is the dimensionality of* $\tilde{\mathbf{u}}_{\mathbf{k}}$.

Essentially, Assumption 1 requires that any region of the space of instrumental variables is observed with positive probability. Several authors have studied discrete choice models *without* any observed productivity measure (see for example Thompson (1989)), establishing that under A1 and some additional technical assumptions, the joint distribution of $G(\boldsymbol{\nu}+\boldsymbol{\zeta})$ is identified up to location and scale.

In order to account for selection bias in the estimation of a productivity model, we need to estimate elements of the distribution of $\boldsymbol{\zeta}$. On this point, Heckman and Honore (1990) show that under the extreme assumption that $\boldsymbol{\upsilon}\equiv\mathbf{0}$, the full joint distribution of $F(\boldsymbol{\zeta})$ is identified if f is observed. Unfortunately, in the context of organizational design, this is equivalent to assuming away the unobserved shocks to the costs of adoption that are not incorporated into the productivity measure, f. However, other authors (see Ichimura and Lee (1991), Heckman and Smith (1997)) show that in the presence of $\boldsymbol{\upsilon}$, only the marginal distribution of each $\zeta_{\mathbf{k}}$, given by $F_{\mathbf{k}}(\zeta_{\mathbf{k}})$, is identified. Intuitively, the systems are never observed together, and thus the covariance between the returns is impossible to recover. We apply these results to give conditions under which the parameters of the model, in particular a complementarity test statistic, are identified.[18]

Proposition 7 *(a) Assume that f is given by (10), and the choices* $\mathbf{y}$ *are determined by the discrete choice model described in (11). Suppose that* $\mathbf{y}^t$, $\mathbf{z}^t$, *and* $\mathbf{u}^t$ *are observed, while* $(\boldsymbol{\zeta}^t,\boldsymbol{\upsilon}^t)$ *are unobserved. Maintain Assumption 1.*

[18] See Matzkin (1990), (1992) and Das and Newey (1997) for identification theorems where functional form on the production function is not assumed. We will not consider such generalizations here. There are a variety of alternative approaches to estimating the models analyzed in Proposition 7, as will be discussed in Section 4.2.

(b) Let $\mathcal{F}$ and $\mathcal{G}$ be sets of absolutely continuous probability distributions, and suppose that $G(\boldsymbol{\zeta}^t + \boldsymbol{\upsilon}^t) \in \mathcal{G}$, and, for each $\mathbf{k} \in \{0,1\}^J$, $F_{\mathbf{k}}(\zeta^t_{\mathbf{k}}) \in \mathcal{F}$. Let $\mathbf{F} = (F_{0,\dots,0},\dots,F_{1,\dots,1})$.

(c) Suppose $(\eta,\varphi) \in \mathcal{L}$ and $\alpha \in \mathcal{M}$, where $\mathcal{L}$ and $\mathcal{M}$ are compact.

Then:

(i) If f is observed, then $(\alpha,\eta,\varphi,\mathbf{F},G)$ are identified in $(\mathcal{L},\mathcal{M},\mathcal{F}^{2^J},\mathcal{G})$ up to the scale of $(\eta,\varphi,\upsilon^t)$ and location of υ^t.

(ii) If f is unobserved, then (η,φ,G) is identified in $(\mathcal{L},\mathcal{G})$ up to the scale of $(\Lambda,\boldsymbol{\zeta}^t+\boldsymbol{\upsilon}^t)$ and the location of $\boldsymbol{\zeta}^t + \boldsymbol{\upsilon}^t$.

The most important consequence of Proposition 7 is to establish conditions under which we can identify the mean of $\boldsymbol{\zeta}^t$, which in turn implies that an estimate for "mean complementarity" is available. As well, a comparison of parts (i) and (ii) isolates the effect of observing productivity. When productivity is unobserved, the joint distribution of $\boldsymbol{\zeta}^t + \boldsymbol{\upsilon}^t$ is identified up to location and scale, while if productivity is observed, the marginal distributions of the components of $\boldsymbol{\zeta}^t$ are identified as well.

In addition to the identification of complementarity, we are interested in distinguishing between different forms of unobserved heterogeneity. When the restrictions of the random practice model (RPM) are satisfied, it will be possible to draw unambiguous policy conclusions about interaction effects, and further we will be able to interpret the selection biases of OLS analyses of productivity according to the propositions of Section 3. The next subsection will discuss further consequences of the RPM for drawing conclusions about optimal adoption of practices.

We begin by formally analyzing the restrictions on $G(\boldsymbol{\zeta}^t + \boldsymbol{\upsilon}^t)$ and $\mathbf{F}(\boldsymbol{\zeta}^t)$ imposed by the RPM. Recall that (RPM) requires that

$$\zeta^t_{\mathbf{k}} + \upsilon^t_{\mathbf{k}} = \sum_{j=1}^{J} \chi_j^{k_j,t} + \sum_{j=1}^{J} \omega_j^{k_j,t} \ \text{ and } \ \zeta^t_{\mathbf{k}} = \sum_{j=1}^{J} \chi_j^{k_j,t} \tag{12}$$

This restriction implicitly places restrictions on the joint distribution $G(\boldsymbol{\zeta}^t + \boldsymbol{\upsilon}^t)$. Recall first that the location of the latter distribution is not identified. Thus, we begin by choosing system (0,..,0) as the reference system, and observing that the distribution over $\zeta^t_{\mathbf{k}} - \zeta^t_{(0,\dots,0)} + \upsilon^t_{\mathbf{k}} - \upsilon^t_{(0,\dots,0)}$ is identified up to scale. The following is a consequence of (RPM):

$$\Delta\zeta^t_{\mathbf{k}} + \Delta\upsilon^t_{\mathbf{k}} \equiv \zeta^t_{\mathbf{k}} - \zeta^t_{(0,\dots,0)} + \upsilon^t_{\mathbf{k}} - \upsilon^t_{(0,\dots,0)} = \sum_{j=1}^{J} \mathbf{1}_{k_j=1} \cdot \Delta\chi^t_j + \sum_{j=1}^{J} \mathbf{1}_{k_j=1} \cdot \Delta\omega^t_j \tag{13}$$

where the incremental returns to a practice j are denoted using the notation $\Delta\chi_j^t = \chi_j^{1,t} - \chi_j^{0,t}$ and $\Delta\omega_j^t = \omega_j^{1,t} - \omega_j^{0,t}$. Thus, knowing $G(\boldsymbol{\zeta}^t + \boldsymbol{\upsilon}^t)$ only up to location can at best provide information about the incremental returns to each practice.

Equation (13) places implicit restrictions on the variance-covariance matrix of $\Delta\zeta_{\mathbf{k}}^t + \Delta\upsilon_{\mathbf{k}}^t$. Thus:

Proposition 8 *Suppose $G(\boldsymbol{\zeta}^t + \boldsymbol{\upsilon}^t)$ is known up to location and scale. Then RPM is testable.*

A straightforward restriction to test is simply

$$\mathrm{var}\begin{pmatrix} \Delta\upsilon_{11,\mathbf{k}_{-ij}}^t + \Delta\upsilon_{00,\mathbf{k}_{-ij}}^t - \Delta\upsilon_{01,\mathbf{k}_{-ij}}^t - \Delta\upsilon_{10,\mathbf{k}_{-ij}}^t \\ +\Delta\zeta_{11,\mathbf{k}_{-ij}}^t + \Delta\zeta_{00,\mathbf{k}_{-ij}}^t - \Delta\zeta_{01,\mathbf{k}_{-ij}}^t - \Delta\zeta_{10,\mathbf{k}_{-ij}}^t \end{pmatrix} = 0 \text{ for all } i,j,\ \mathbf{k}_{-ij}.$$

The restrictions imposed by (13) include both variance and covariance restrictions on the distribution of $\Delta\zeta_{\mathbf{k}}^t + \Delta\upsilon_{\mathbf{k}}^t$. However, if only the variances are known, (RPM) is *not* in fact a testable restriction. The hypothesis that $\zeta_{11,\mathbf{l}_{-jk}}^t + \zeta_{00,\mathbf{l}_{-jk}}^t - \zeta_{10,\mathbf{l}_{-jk}}^t + \zeta_{10,\mathbf{l}_{-jk}}^t = 0$ is not in general testable without knowledge of the covariances between the variables, even if we make a distributional assumption such as joint normality. In general, for distributions over $\boldsymbol{\zeta}^t$ in suitable families (such as normal distributions), it will often be possible to satisfy restrictions such as $\mathrm{var}\left(\zeta_{11,\mathbf{l}_{-jk}}^t + \zeta_{00,\mathbf{l}_{-jk}}^t - \zeta_{10,\mathbf{l}_{-jk}}^t + \zeta_{10,\mathbf{l}_{-jk}}^t\right) = 0$ for all $j \neq k$, simply by choosing the appropriate covariance restrictions on $\boldsymbol{\zeta}^t$.

In Section 3, we demonstrated that for the purposes of interpreting the results of several simple econometric procedures, affiliation of the incremental returns to practices is a leading "alternative" hypothesis to complementarity. Thus, we are also interested in conditions under which the distributions of $\Delta\chi_j^t = \chi_j^{1,t} - \chi_j^{0,t}$ and $\Delta\omega_j^t = \omega_j^{1,t} - \omega_j^{0,t}$ are identified. The following proposition indicates that some, but not all, elements of the joint distribution of these objects are identified.

Proposition 9. *Suppose that the vector of marginal distributions $\mathbf{F}(\boldsymbol{\zeta}^t)$ is such that (12) holds for some $\boldsymbol{\chi}^t$. Then:*

(i) For all $i \neq j$, $\mathrm{cov}(\Delta\chi_i^t, \Delta\chi_j^t)$ is uniquely identified from $\mathbf{F}(\boldsymbol{\zeta}^t)$.

(ii) Suppose, in addition, that $G(\boldsymbol{\zeta}^t + \boldsymbol{\upsilon}^t)$ is known up to location and scale, and that there

exists $\boldsymbol{\chi}^t$ and $\boldsymbol{\omega}^t$ such that (12) holds. Then the full joint distribution of $\{\Delta\chi_j^t + \Delta\omega_j^t\}$ is uniquely identified up to scale.

Proposition 9 indicates that the main objects of economic interest in the RPM are identified under the assumptions of Proposition 7. In particular, some of the driving forces behind positive correlation between practices (as analyzed in CORR) and a finding of positive interaction effects using OLS or 2SLS (as in PROD) can be identified and interpreted.

Proposition 9 (i) does not, however, give conditions for the identification of the full joint distribution of $\Delta\chi_j^t$. It can be shown, in fact, that the variance of each $\Delta\chi_j^t$ is *not* identified; only $\text{var}\left(\sum_{j=1}^{J}\chi_j^{k_j,t}\right)$ and $\sum_{j=1}^{J}\text{var}\left(\Delta\chi_j^{k_j,t}\right)$ are identified. On the other hand, all of the information needed to compute the selection bias *is* available.

4.2 An Structural Estimator for the Organizational Design Production Function

This section briefly outlines some of the issues associated with estimating the model described in Section 4.1. The approach we propose entails estimating a system of equations describing the productivity of organizational design and the adoption of organizational practices.

The recent literature on semi-parametric methods suggests several potential estimators for (10) and (11) (see, for example, Thompson (1989), Ichimura and Lee (1991), Cosslett (1991)). An alternative is to posit a parameterized distribution over the unobservables, which implies that estimation is feasible with GMM, or more generally, simulated method of moments (Hansen (1982), McFadden (1989), or McFadden and Ruud (1994)). Parametric approaches may be most appropriate in applications for several reasons. First, many of the applications that we might wish to consider have inherently limited samples. For example, there may be only a small number of firms within a narrowly defined industry, and the time and expense of gathering detailed data about internal organization may impose further constraints. Second, allowing for a distribution of unobservables with a variance-covariance matrix which is unrestricted (such as the multivariate normal) provides a parsimonious specification that still accommodates the main alternative hypotheses about unobserved heterogeneity analyzed in this paper. Thus, even if semi-parametric estimation is included in an analysis of organizational design, there may still be value to the parametric models for the purposes of summarizing the economically relevant properties of the distribution and testing hypotheses about them.

Given distributions F and G, we can specify the following set of moment equations, where we have imposed (OPT) to illustrate the cross-equation restrictions (and where $\mathbf{v}_j$ is a vector of appropriately specified instruments):

$$m^t(\mathbf{y}^t,\mathbf{z}^t,\mathbf{u}^t;\boldsymbol{\alpha},F(\cdot),G(\cdot)) = \begin{pmatrix} \left[y_1^t - \int_{\zeta^t+\upsilon^t} y_{1*}(\mathbf{z}^t,\mathbf{u}^t,\zeta^t+\upsilon^t;\boldsymbol{\alpha})\,dG(\zeta^t+\upsilon^t)\right]\cdot \mathbf{v}_1^t \\ \vdots \\ \left[y_J^t - \int_{\zeta^t+\upsilon^t} y_{J*}(\mathbf{z}^t,\mathbf{u}^t,\zeta^t+\upsilon^t;\boldsymbol{\alpha})\,dG(\zeta^t+\upsilon^t)\right]\cdot \mathbf{v}_J^t \\ \left[f^t - E_{\zeta^t}\left[f(\mathbf{y}^t,\mathbf{z}^t,\zeta^t;\boldsymbol{\alpha})\middle|\mathbf{y}_*^t(\mathbf{z}^t,\mathbf{u}^t,\zeta^t+\upsilon^t;\boldsymbol{\alpha})=\mathbf{y}^t\right]\right]\cdot \mathbf{v}_{J+1}^t \end{pmatrix} \quad (14)$$

Under the assumptions in Proposition 7 and (OPT), $E[\,m^t(\mathbf{y}^t,\mathbf{z}^t,\mathbf{u}^t;\boldsymbol{\alpha},F(\cdot),G(\cdot))\,]=0$.[19] When (RPM) is imposed, we can derive the parameters describing complementarity from $E[\boldsymbol{\zeta}]=\theta$. Perhaps the most important efficiency advantage of (14) arises from the fact that organizational design practice data are often "clustered" (which we showed in Section 3 could be a consequence of complementarity). The cross-equation restrictions in (14) allow us to estimate the distribution of incremental returns to a system, $\Delta\zeta_{\mathbf{k}}^t$, from the adoption equations.

Under (OPT), (NI), and the (RPM) restriction, complementarity can be tested even if some combinations of practices are never observed, based entirely on the estimates of the incremental returns to each practice from the adoption equations. For example, in the simple two-choice example, there are two estimable parameters, θ_{00}–θ_{01} and θ_{10}–θ_{11}, and four systems. Since one of the systems is normalized to determine location, we are left with three parameters $E[\,\Delta\zeta_{01}^t+\Delta\upsilon_{01}^t]$, $E[\,\Delta\zeta_{10}^t+\Delta\upsilon_{10}^t]$, and $E[\,\Delta\zeta_{11}^t+\Delta\upsilon_{11}^t]$. Thus, θ_{00}–θ_{01} and θ_{10}–θ_{11} are over-identified.

4.3 Issues for Hypothesis Testing

Several additional econometric issues arise in testing hypotheses about complementarity. First, testing for complementarity implies that one is interested in imposing one-sided inequality restrictions (e.g., $\kappa \geq 0$) under the null hypothesis. In a simple 2-choice variable model, only

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[19] To interpret (14), note that the last equation is simply the difference between observed productivity and the expected value of productivity given observables. We will refer to this equation as the "productivity equation." In contrast, we call the first J equations the "adoption equations." For each choice, the moment condition is equal to the difference between the observed value of the choice, and the expected value of the choice given the observables and the parameters.

one inequality constraint is implied by the model ($\kappa \equiv \theta_{00} + \theta_{11} - \theta_{01} - \theta_{10}$), and so the test will be a simple one-tailed *t*-test. However, even in this case, one needs to distinguish between two alternative null hypotheses: $\kappa \geq 0$ and $\kappa \leq 0$. Of course, the latter hypothesis is the more conservative one; only a statistically significant positive coefficient on the test statistic according to a one-tailed *t*-test will be evidence in favor of complementarity.

When there are more than two choices, more subtle issues arise. In particular, pairwise complementarity will imply multiple inequality restrictions: if there are *J* practices, each restriction imposes pairwise complementarity between the two practices of interest, for a given combination of the other *J*-2 choices. Thus each test for pairwise complementarity is composed of $2^{(J-2)}$ linear inequality restrictions. In the case of multiple inequality restrictions, specifying the appropriate critical value for a test of a certain size requires choosing a test statistic consistent with the potential presence of multiple slack restrictions. As developed by Gourieroux, Holly, and Manfort (1981), Kodde and Palm (1986), and Wolak (1989, 1991), the appropriate test statistic under the null hypothesis will be distributed according to a weighted sum of chi-squared distributions. Moreover, Wolak (1991) shows that when the model is nonlinear and the number of linear restrictions is greater than 2, there exists an inherent ambiguity in the specification of the distribution of this test statistic, and so the test must be performed under every combination of potential combination of "tight" and "slack" restrictions (with a separate critical value for each as suggested above). An alternative solution would be to restrict the production function to be composed only of pairwise interaction effects; in this case, a test for complementarity between a pair of practices will always be composed of a single linear inequality restriction.

4.4 Testing Theories about the Adoption of Organizational Design Practices

While most of our analysis has been centered on testing of complementarity, the model suggests a set of natural cross-equation restrictions regarding adoption behavior. Hypotheses about the nature of the adoption process are particularly salient since both economists and other social scientists often disagree about the nature of this process and its consequences for policy. At one extreme, neoclassical economics assumes that production decisions are chosen to maximize firm profits taking as given a vector of observable input prices. A variety of theories of transaction costs and adjustment costs have been incorporated into the literature over time, but the assumption in the economics literature is still that firms are doing as well as possible subject to constraints.

While there is substantial heterogeneity among economic theories, all of these theories differ sharply from the approach taken by some strands of the literature in sociology and organizational behavior. For example, organizational ecology (see Nelson and Winter (1982) or Hannan and Freeman (1989)) posits that firms change only slowly and not necessarily systematically; rather, a process of "selection" eliminates firms which are poorly adapted to the current environment. More generally, much of the organizational behavior literature takes the view that organizations should not be thought of as rational decision-makers. For example, the "garbage can" theory of organizations maintains that agents may be systematically misinformed about the costs and benefits of different practices within their own organization (Cohen, March, and Olsen, 1972).

Although the methods of economists and sociologists may differ, the empirical model that we develop is rich enough to allow for all of these possibilities. Let us briefly consider the conditions under which testable hypotheses can be formulated. We are particularly interested in whether the adoption patterns are consistent with the interaction effects estimated in the productivity equations.

In the context of our model, the set of testable restrictions about optimality is directly linked to the nature of the unobserved heterogeneity. Allowing for unobserved returns to practices outside the production function (in our model, ω) necessarily prevents us from drawing conclusions about the propensity to adopt an individual practice: an organization might simply have low unobserved returns to that practice. However, in the random practice model (RPM), there should be a single complementarity parameter for all firms. Thus, it is possible to identify differences in the (unique) complementarity parameter estimated in the productivity equation versus the (appropriately scaled) complementarity parameter estimated in the adoption equations.[20] In particular, if (NI) holds (and all of the productive interaction effects are captured in the production function), we can test whether the firm over- or under- exploits the interactions between practices. For example, decentralized decision-making can lead to decisions that fail to

[20] Observe that the scaling of the complementarity parameter from the adoption equation can be normalized with

incorporate all of the externalities of practice adoption decisions. A finding of complementarity in the production function, but a negligible interaction effect in the adoption equations, would support the hypothesis that organizations fail to account for interaction effects.

Unfortunately, if the (RPM) model is not supported in an application, it will be more difficult to test theories about adoption. By allowing for unobserved interaction effects in the adoption equations that differ from those in the productivity equations (in our model, the variables υ), we will never be able to distinguish whether an agent's patterns of adoption violate optimality, or are instead simply responding to unobservables. At best, we will be able to compare the distributions of interaction effects from the adoption and productivity equations.

Even if (RPM) fails, we can still draw some inferences about the adoption process if there are variables (such as $\mathbf{x}$ in our model) which theory suggests should affect productivity in use, but not adoption. In such cases, we can test the cross-equation restrictions on the coefficient of $\mathbf{x}$ in the adoption and productivity equations.

5. Issues for Data Gathering and Survey Design

Our results have several implications about the kinds of data that will be useful for testing theories about complementarities. We will now briefly summarize these implications. First, our analysis of Section 3 highlighted the central role of unobserved returns to different practices, and their statistical interrelationships, on the biases associated with a variety of simple econometric procedures. Thus, even if systematic quantitative evidence is impossible to gather, qualitative information about the factors which affect the adoption and productivity of practices, and their covariation, can play a central role in distinguishing between alternative theories and interpreting the results of procedures such as (CORR) and (PROD). Further, such qualitative evidence can be compared against the estimated distributions of unobservables in the approaches discussed in Section 4.

Of course, in order to satisfy the assumptions (such as EXCL or Assumption 1) required to implement many of the procedures discussed in this paper, some quantitative information about exogenous factors which affect the adoption process will be required. When implementing tests that make use of a performance measure, either in the single-equation or the system of equations

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reference to the estimated incremental returns to an individual practice.

approach, the distinction between **w** and **x** becomes more relevant. In particular, since instruments will be required, it is necessary to observe variables **w** which affect the individual adoption decisions, but do not directly affect productivity. This consideration is important in choosing the productivity measure in an application. In particular, to manage the problems created by the unobserved heterogeneity, it is useful to find the narrowest possible measure of productivity which still incorporates all of the interactions between endogenous variables. This makes it easier to find instruments (**w**) which represent costs to the organization that do not interact directly with productivity. If the performance measure is suitably narrow such that the unobserved heterogeneity is due to ω and not χ, OLS will yield unbiased results.

To the extent that observed choice variables are positively correlated and unobserved heterogeneity is important, and thus we rely on the adoption equations to estimate the relevant parameters, it is crucial to understand the nature of the adoption process in organizations. Thus, our approach will be most powerful in applications where adoption is relatively systematic and can be at least partially explained by observables (whether or not adoption is "profit-maximizing" in a strict sense). If the adoption process is too noisy, very little will be learned from estimating adoption equations. For this reason, applications which will be difficult to analyze include those where there is rapid diffusion of organizational practices, but we only observe a cross-section. In particular, difficulties will arise if firms are adopting sets of practices together, without fully understanding their interactions, and if the choice of which firms adopt is determined by factors such as central management's taste for management fads. For survey design, focusing interview and survey questions on the factors that enter the adoption process will be critical.

Of course, if adoption is completely unrelated to productivity (as predicted by some of the theories from sociology described above), it may be possible to treat the determination of organizational design as a "natural experiment." Unfortunately, this hypothesis will be untestable in the absence of observable exogenous variables that drive adoption.

Our analysis also indicates that when the restrictions of the "random practice model" are satisfied, there will a single parameter of complementarity between any two practices for all firms, and we can test and exploit cross-equation restrictions between adoption and productivity equations. For this reason, we believe that the framework is most suitable applied with a narrow industry with well-defined practice adoption choices that are the same for all firms in an industry.

In summary, our proposed method can be most fruitfully applied in scenarios where there is a precise and suitably narrow productivity measure available, the adoption process is systematic, firms face the same basic production technology, and it is possible to observe information about the costs and benefits to adoption, particularly costs and benefits which do not interact with productivity directly. Potential examples include customer service organizations or service industries, such as banks or retail outlets, where many outlets serve a variety of customer types and are located in a variety of labor market and regulatory environments.

6. Conclusions

Understanding the sources of inter-firm heterogeneity, and the nature and importance of complementarities between practices, is important for public policy and business policy. This paper highlights many of the difficulties that arise in trying to disentangle different hypotheses about the causes of positive correlation between organizational choice variables. In particular, we show that the approaches which have been most commonly used in the literature can yield misleading results when we allow for complementarities between choice variables as well as unobserved factors which affect the marginal costs and benefits of each individual choice.

The empirical framework we propose is tailored to disentangle the different forces behind the observation of "clustered" organizational design practices. Using one of the simultaneous equations systems proposed in this paper (the random practice model or the random systems model), we can, in principle, distinguish between two competing assumptions about the nature of unobserved heterogeneity, test for the cross-equation restrictions associated with static optimization, and, most importantly, provide a consistent test statistic for complementarity. As well, this system of equations approach can provide substantial increases in precision, particularly important when the sample sizes associated with many applications are small and there is a tendency towards clustering among the dependent variables.

In this paper, we have developed a baseline framework, several interesting theoretical issues that remain to be explored. For example, we wish to study more carefully the issues associated with aggregating organizational design variables, in particular the use of "indices" to describe the adoption of a set of organizational design practices (as in ISP). Further, this paper has not addressed the issues associated with the dynamics of the diffusion and adoption of organizational design practices. Of course, the most important next step is the implementation of these techniques in real-world data sets, which we hope to pursue in future work.

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Appendix

Proof of Proposition 3: (PROD) Abstract away from **x** and **w**. It is clear from Figure 1 that $E\left[\chi_1 \middle| (\chi_1,\chi_2)\in R_{11}\right] \le E\left[\chi_1 \middle| \chi_1 \ge \theta_{00}-\theta_{10}\right] = E\left[\chi_1 \middle| (\chi_1,\chi_2)\in R_{10}\right]$ since the random variables are independent and since $\kappa \ge \mathbf{0}$ (notice that R_{11} includes regions which are strictly less than R_{10}) The same logic implies that $E\left[\chi_2 \middle| (\chi_1,\chi_2)\in R_{11}\right] \le E\left[\chi_2 \middle| (\chi_1,\chi_2)\in R_{01}\right]$, yielding the desired result.

(CORR) By Proposition 1, $\mathbf{y}^*(\boldsymbol{\chi}|\mathbf{x},\mathbf{w})$ is nondecreasing, and $\boldsymbol{\chi}$ is independent. The result is then immediate.

Proof of Proposition 4: (PROD) When κ=0, $\theta_{00}-\theta_{10}=\theta_{01}-\theta_{11}$. Then $R_{11}=\{(\chi_1,\chi_2) | (\chi_1,\chi_2)\ge(\theta_{00}-\theta_{10},\theta_{00}-\theta_{01})\}$, while $R_{10}=\{(\chi_1,\chi_2) | \chi_1\ge\theta_{00}-\theta_{10},\ \chi_2\le\theta_{00}-\theta_{01}\}$. We now apply a theorem from Athey (1998), which states that if a set A is larger than B in the "strong set order," and the random variables are affiliated, then $E\left[g(\chi_1,\chi_2) \middle| (\chi_1,\chi_2)\in A\right] \ge E\left[g(\chi_1,\chi_2) \middle| (\chi_1,\chi_2)\in B\right]$ for any nondecreasing function g. Since $g(\chi_1,\chi_2)=\chi_1$ is nondecreasing, and R_{11} is larger than R_{10} in the strong set order, the theorem applies to yield the result that $E\left[\chi_1 \middle| (\chi_1,\chi_2)\in R_{11}\right] \ge E\left[\chi_1 \middle| (\chi_1,\chi_2)\in R_{10}\right]$. Similar arguments applied to χ_2 lead to the result that $E[\hat{\kappa}^{OLS}]\ge 0$.

(CORR) The result follows because nondecreasing functions of affiliated random variables are affiliated.

Proof of Proposition 5: Part (a) follows from the proof of Proposition 3, while part (b) follows from the proof of Proposition 4.

Proof of Proposition 9: (i) Let $\psi(\mathbf{k}) \equiv \mathrm{var}\left(\sum_{j=1}^{J}\chi_j^{k_j,t}\right)$, which is given from the marginals $\mathbf{F}(\boldsymbol{\zeta})$. Expanding, we have $\psi(\mathbf{k}) = \sum_{j=1}^{J}\mathrm{var}(\chi_j^{k_j,t}) + 2\sum_{i=1}^{J}\sum_{j=1}^{i-1}\mathrm{cov}(\chi_i^{k_i,t},\chi_j^{k_j,t})$. But we know that $\mathrm{cov}(\Delta\chi_j^t,\Delta\chi_k^t) = \mathrm{cov}(\chi_j^{1,t},\chi_k^{1,t}) + \mathrm{cov}(\chi_j^{0,t},\chi_k^{0,t}) - \mathrm{cov}(\chi_j^{1,t},\chi_k^{0,t}) - \mathrm{cov}(\chi_j^{0,t},\chi_k^{1,t})$. Then, it is straightforward to show that
$2\,\mathrm{cov}(\Delta\chi_i^t,\Delta\chi_j^t) = \psi(0,0;\mathbf{k}_{-ij}) + \psi(1,1;\mathbf{k}_{-ij}) - \psi(0,1;\mathbf{k}_{-ij}) - \psi(1,0;\mathbf{k}_{-ij})$.

(ii) By assumption, the variance-covariance matrix of $\Delta\zeta_{\mathbf{k}}^t + \Delta\upsilon_{\mathbf{k}}^t$ is identified. Applying (13) to $\mathbf{k}$=(..0,1,0,..) the variance-covariance matrix of $\{\Delta\chi_j^t + \Delta\omega_j^t\}$ is identified.

Table 1: Table of Notation

Notation	Description	Observed/Unobserved Endogenous/Exogenous
Variables		
$\mathbf{y}=(y_1,..,y_J)$	Vector of J discrete choices made by the firm.	Observed; endogenous.
$\mathbf{x}=(x_1,..,x_J)$	Vector of exogenous variables which affects observable performance (f)	Observed; exogenous.
$\chi=(\chi_1,..,\chi_J)$		Unobserved; exogenous.
$\mathbf{w}=(w_1,..,w_J)$	Vector of exogenous variables which does not affect performance, where each component j affects the costs and benefits of the corresponding component of $\mathbf{y}$	Observed; exogenous.
$\omega=(\omega_1,..,\omega_J)$		Unobserved; exogenous.
$\mathbf{z}=(z_{0..0},..,z_{1..1})$	Vector of system-specific exogenous variables which affect productivity	Observed; exogenous.
$\zeta=(\zeta_{0..0},..,\zeta_{1..1})$		Unobserved; exogenous.
$\mathbf{u}=(u_{0..0},..,u_{1..1})$	Vector of system-specific exogenous variables which *do not* affect productivity	Observed; exogenous.
$\upsilon=(\upsilon_{0..0},..,\upsilon_{1..1})$		Unobserved; exogenous.
Parameters		
θ	Parameter of function f which determines supermodularity of f.	To be estimated.
α,η,β,δ	Parameters which affect the returns to exogenous variables.	To be estimated.
Functions		
π	The firm's overall objective function.	No measure observed. Functional form assumed.
f	The firm's performance or output.	Observed with (i.i.d.) error. Functional form assumed.
g	The portion of the firm objective which does not directly affect f.	No measure observed. Functional form assumed.
F	The joint distribution over the unobservables ζ.	Marginals to be estimated.
G	The joint distribution over the unobservables $\zeta+\upsilon$.	To be estimated.

FIGURE 1

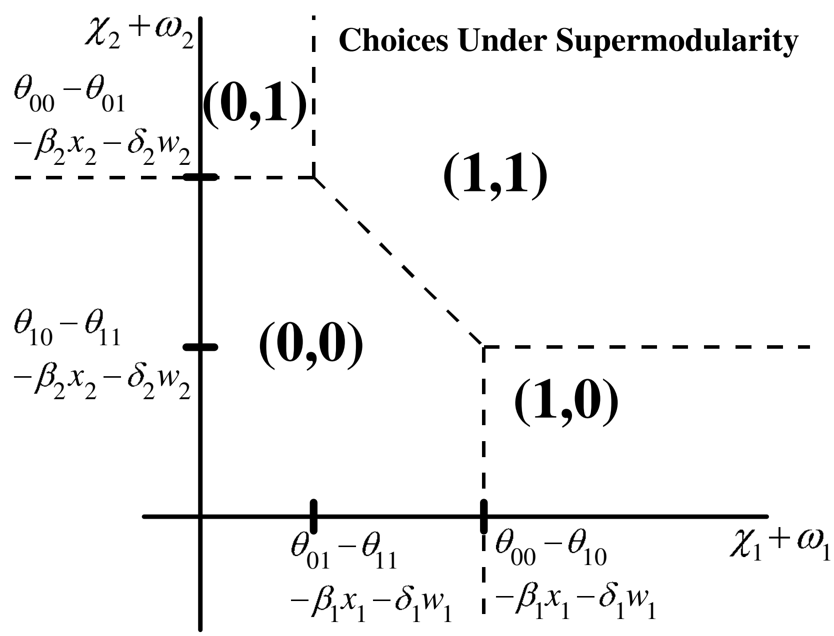

FIGURE 2