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. In particular, we propose an approach for estimating the parameters of an "organizational design production function." Further, we identify consistent tests for two classes of hypotheses: first, that some sets of organizational design practices are mutually complementary; and second, that adoption patterns are consistent with static optimization of the organization's profit. We develop an economic model where multiple organizational design practices are endogenously determined. The model includes exogenous variation in the costs and returns to each of the individual practices, which is the source of the heterogeneity among organizations. In many empirical applications, some of these variables will be unobserved to the econometrician. The model is used to evaluate how different econometric strategies can be interpreted under alternative assumptions about the economic and statistical environment. Of particular interest are a set of results which demonstrate that, under plausible hypotheses about the joint distribution of the unobservables, different reduced-form approaches used in the existing literature to test for complementarity will be inconsistent. Further, a variety of different reduced-form tests may all yield qualitatively similar biases for a given set of hypotheses about the unobservables. 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

Firm choices about internal organization play a central role in determining labor demand, investment, and productivity. For example, human resource policies such as job design or training and promotion policies affect the composition of an organization's workforce as well as the wage premium for skilled labor. Further, an important issue for public policy is the extent to which the adoption of information technology by firms has affected the income distribution and productivity growth.

Most of the existing empirical work about labor demand and productivity either does not consider organizational design, or alternatively considers a single choice (such as training or computer technology) in isolation. However, a more recent theoretical and empirical literature suggests that there may be important interactions, particularly complementary interactions, between elements of organizational design (e.g., complementarity between the use of computers and the level of training).[1] This literature mainly studies cross-sectional variation in organizational design within an industry, and the data is usually collected using detailed surveys. One finding of the empirical studies, based on both quantitative and qualitative evidence, is that some sets of organizational design practices are positively correlated across firms within an industry, resulting in a "clustering" of observed practices.

If organizational design results from choices by organizations about the adoption of several interrelated practices, then a number of implications follow for both policy and empirical analysis. For policy, it becomes important to understand the nature of these interactions, since taxes and regulations which affect one practice may have unintended consequences for the adoption of other practices and productivity. For example, subsidies to training may affect a firm's choices about job guarantees or the adoption of computers. Further, there may be benefits to implementing coordinated changes in policies which affect distinct, but complementary, practices. Similarly, complementarity between a set of practices implies that the adoption of one practice has externalities for adoption decisions about other practices. Firms thus may make large changes in several organizational design practices in response to small changes in the economic or regulatory environment; and firms which decentralize the adoption of practices may wish to design incentives which ensure that these externalities are accounted for by the relevant decision-makers.

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[1] For examples of theoretical papers, see Milgrom and Roberts (1990, 1993), or Holmstrom and Milgrom (1994). In the empirical literature, see For example, see Ichniowski, Shaw, and Prennushi (1997), Brickley (1995), MacDuffie (1995); in the management literature, the *Academy of Management Journal* devoted a special issue in August, 1996 to "Human Resource Management and Organizational Performance." For more discussion of these

From the perspective of empirical analyses of the productivity of organizational design practices, however, interactions between choices complicate the ability to draw inferences. In order to analyze the implications of such interactions, we begin by developing a formal model. The model represents an "organizational design production function," with parameters which specify the interactions between choices, as well as exogenous variables which determine the costs and benefits of each practice. An important component of our analysis is the presence of exogenous variables which are observed by the firms but not the econometrician. These variables are the source of variation in firm practices that can not be explained by observables. They affect the marginal returns or costs of a given practice. 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. We argue that in most empirical analyses of organizational design, some of these factors will be unobserved. Building on existing tools from other applications (Heckman and Honore, 1990; Ichimara and Lee, 1991; Thompson, 1989), we then establish conditions under which the parameters of the production function and the joint distribution of unobservables are identified. For all of our analysis, we assume that the econometrician *does* observe some of the exogenous factors which affect the costs and benefits of each practice, a condition required for identification.

The paper uses this formal model for two main objectives. First, we analyze the conditions under which alternative econometric approaches (both reduced-form and structural) are able to provide a consistent test for complementarity between elements of organizational design. The second objective is to propose estimation strategies and hypothesis tests which allow us draw conclusions about the process which leads to adoption of practices, including the extent to which adoption is driven by unobservables, and the extent to which practice adoption appears to be consistent with optimal behavior. For example, an interesting hypothesis which can be tested using our approach is the extent to which the externalities across practices in the firm's "production function" are accounted for in the firm's practice adoption decisions.

In addressing our first objective, we first focus on the conditions under which a set of reduced-form procedures used in the existing literature can identify whether a given pair of practices are complementary. For example, we distinguish between two conditions: (TC) the practices are complements in the organizational design production function, and (TI) the practices are technologically independent. Previous empirical models have attempted to distinguish between

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and related papers see Section 2.

(TC) and (TI) by examining whether the adoption of different practices are correlated and whether the interaction effects among practices in a productivity equation using OLS or 2SLS are positive. In the context of our model, these procedures provide a consistent test for complementarity only under very restrictive conditions.

To see this, consider the following two assumptions about the joint statistical distribution of the unobserved returns to practice adoption: (UC) the unobserved returns to the practices are affiliated (a strong form of positive correlation)[2] and (UI) 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 (UC) leads to both (a) positive correlation in adoption among practices and (b) a positive estimate of the interaction effects resulting from an OLS or 2SLS productivity regression.

In general, under (UC), the interaction effects in productivity regressions will be *overstated* by reduced-form methods. Alternatively, if the environment is characterized by (UI) and (TC), reduced-form productivity regressions will tend to *understate* interaction effects. Under (TC), adopting a given practice (such as computer technology) leads to a less favorable selection of firms adopting complementary practices (such as higher skill requirements). While each of these biases are specific examples of selection biases (as analyzed by Heckman (1974) or Heckman and MaCurdy (1986)), our focus on the interactions between multiple organizational design practices yields novel interpretations of these biases. We further show that these biases confound the reduced-form analysis of the productivity of a single organizational design practice, so long as that practice is jointly determined with other practices whose returns are not fully observed; this provides an alternative interpretation of the standard single-practice selection bias.

Consider a simple example where this analysis might apply. Suppose that we observe that, conditional on observables, computer adoption and higher skill requirements are positively correlated in a cross-section of firms within an industry. Suppose further that we observe some exogenous adoption costs for both practices which vary across firms (such as cross-state variation in regulation). In a linear two-stage least squares regression of the productivity of production units on organizational practices and their interactions, suppose that we observed a positive interaction effect between computers and skill. While such evidence has been interpreted as support for the hypothesis of complementarity, this paper identifies alternative assumptions which are also consistent with these results. The above discussion further implies that even our

[2] That is, the random variables are positively correlated, and further every nondecreasing transformation of the random variables satisfies positive correlation.

interpretations about the direction of the bias from the reduced-form procedures hinge critically on our beliefs about the unobserved returns to practices. Thus, even in applications where there is insufficient data to identify the parameters of interest using a structural model, our framework can be used to more precisely interpret the results of commonly used procedures. Further, this logic may suggest the kinds of qualitative evidence which would be most useful in evaluating which of the different hypotheses is most likely to explain observed data.

We then discuss the advantages to using a structural approach 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. Accounting for the unobserved heterogeneity allows us to obtain consistent estimates of the parameters of the "organizational design production function."

By estimating a system of equations, we are able to impose and test the cross-equation restrictions implied by profit-maximizing behavior on the part of the firms. In particular, we can test whether firms tend to adopt practices in a way which accounts for the true interaction between choices, as estimated by the productivity equation. If we cannot reject the cross-equation restrictions implied by optimal adoption, we further argue that when studying problems about complementarity in organizational design, the system of equations approach can provide substantial efficiency gains. For example, when practices are complementary and unobserved returns are positively correlated, as is predicted by several of the theories we are interested in testing, the practices will be positively correlated in a cross-section of firms. Positive correlation between practices implies that, in small data sets, the parameters corresponding to some combinations of practices will be estimated imprecisely using the productivity approach, since they occur infrequently; however, under the hypothesis that firms adopt practices taking into account their productivity, the adoption equations will estimate the returns of one practice *relative* to other all other practices, thus providing information about the returns to practice combinations which are chosen infrequently.

Finally, we show that using our estimation approach, it is straightforward to test hypotheses about the nature and importance of the unobserved heterogeneity. In particular, we are interested in distinguishing what we call the *random practice model*, where there are unobserved returns to *individual* practices, but the interactions *between* practices are the same for all firms, from the *random systems model*, where unobserved variables might cause two practices to be substitutes for some firms and complements for others. Using an analogy to traditional productivity studies, the random practice model allows for prices of inputs to vary across firms (though in our context they may be unobserved); in contrast, the random systems model is analogous to allowing the elasticity of substitution between two inputs to vary across firms. We show that the random practice model is nested within the random systems model, and thus we can test the hypothesis that the random practice model applies in a particular context. This test is important for drawing the clear policy implications arising from the random practice model; in the random system model, economic interpretations and policy conclusions are more ambiguous and are sensitive to the properties of the distribution of the unobservables.

Our proposed estimation procedure is an application of existing methods which have been developed in the context of other applications, most notably in studies of productivity (Christensen, Jorgenson, Lau (1973), Evans and Heckman (1984)) and labor econometrics (Heckman, 1974; Heckman and MaCurdy (1986)); however, a number of special issues arise in the application to organizational design. In contrast to the classical productivity problem, the inputs to organizational design are not divisible goods purchased on perfectly competitive markets at observable prices; instead, many choices are discrete, and the "prices," in our case adoption costs and benefits, are only imperfectly observed. The fact that we cannot observe the total "input cost" for each practice implies that many of the standard techniques from the productivity literature are not applicable, in particular the approach based on dual cost functions. The presence of unobserved returns to adoption motivates our proposal to account for selectivity through the use of methods developed in labor econometrics. Indeed, the model is a specific applications of the general "switching regressions" model which can be used to model an agent choosing between several organizational forms. Our model decomposes each available organization form into its constituent "practices," each with its own costs and benefits varying across firms, some of which are observable. Therefore, our analysis considers the "practice," not the entire organizational form, as the unit of analysis, since both theory and policy speak to the practice as the unit of analysis.

Of course, implementing the structural approach we suggest will be computationally burdensome and will be sensitive to the specification of the problem. In order to highlight the tradeoffs between the different approaches we have discussed, we analyze a simple simulation model, demonstrating with Monte Carlo experiments that our approach avoids the bias of other approaches and can, in data sets of moderate size, disentangle the effects of complementarity from those of unobserved heterogeneity. Our experiments also lend some insight into the behavior of each of several alternative approaches in small samples and under different assumptions about the economic environment. Finally, our analysis suggests strategies for the collection of new data sets and for survey design, emphasizing the importance of instruments in the form of exogenous cost and benefits to different organizational design choices.

This paper proceeds as follows. Section 2 provides a more complete motivation of the problem, describing some examples of complementarity in organizational design and its implications for policy. Section 3 presents the formal economic model, while Section 4 presents general propositions about identification. Section 5 studies various econometric procedures which can be used to test for complementarity, and analyzes the conditions under which each test is valid. Section 6 analyzes tests for optimality. Section 7 analyzes approaches to testing alternative assumptions about unobserved heterogeneity; these approaches are further explored in Section 8, which presents the Monte Carlo experiments. Section 9 discusses issues for data collection and survey design. Section 10 concludes.

2. Motivation

As discussed in the Introduction, there are two classes of hypotheses which will receive particular emphasis in this paper: complementarity between organizational design practices, and the extent to which practice adoption is consistent with static optimization on the part of the organization. Before we turn to the formal economic and statistical analysis, we further motivate our focus on our emphasis on these hypotheses.

2.1. Interdependency and Complementarity in Organizational Design

There are a variety of reasons that elements of organizational design will interact in determining the organization's productivity. A number of recent theoretical analyses have sought to explain stylized facts about organizational designs using theories about interactions between variables, in particular, *complementary* interactions, where increasing one variable increasing the returns to the others. For example, Milgrom and Roberts' (1990) study of "modern manufacturing" develops and explores the theory of complementarity, analyzing the interrelationships between practices such as the adoption of flexible machinery, the ability to implement frequent product redesigns, investments in information gathering concerning customer preferences, low inventories, and quick delivery time.[3] As well, Holmstrom and Milgrom (1994) have studied complementarities in the provision of incentives and associated human resource policies when workers engage in multiple activities, such as effort to produce output as well as effort on process improvements. In their recent empirical study of the steel industry by Ichniowski, Shaw, and Prennushi (1997) (hereafter ISP) build on this logic to argue that several elements of what they call "high performance" human

[3] In related work, Athey and Schmutzler (1995) show that complementarities between demand-enhancing innovation and cost-reducing innovation can lead to complementarities between long-run organizational decisions such as product and process research and development and flexibility. Topkis (1995) analyzes general conditions under which complementarities arise between a firm's choices of quantity and investments.

resources practices (HPWP) are mutually complementary, such as individual and team-based incentives, extensive screening of new employees, worker training, employment security, job rotation and flexibility, and labor-management communication.[4] Further, several studies have advanced the hypothesis that information technology (IT) is complementary with some or all of the elements of HPWP (see Brynjolfsson and Hitt (1995)).

By characterizing the "organizational design production function," we will be able to make predictions about how the optimal organizational structure of firms changes in different economic environments. Such predictions might be especially useful when designing new government agencies or when a multinational opens a new division in a foreign country. In particular, if policies are complements, a number of qualitative predictions can be made which do not depend on particular values of parameters. For example, a change in the economic environment (such as a tax) which affects one practice will lead to mutually reinforcing changes in other practices, potentially leading to an empirical observation that sets of practices tend to be found in clusters.

However, the presence of interaction effects will also affect the design of public policy more directly. For example, we might wish to consider the effects of alternative policies on productivity growth, the demand for skilled versus unskilled labor, the bargaining power of production workers, or changes in the income distribution. In this context, we might consider policy instruments such as education, industry regulation, training subsidies, or anti-poverty programs. Each of these policies will directly affect the incentives of firms to change one or more organizational design practices, such as training policies, skill requirements, or the policies used to promote or discourage turnover. However, if there are interactions between human resource policies and technology adoption, any policy which affects one of these areas will indirectly affect the other.[5] Thus, if job security is important for a policy goal but difficult to affect directly using public policy instruments, the optimal public policy may instead target a different but

[4] Their arguments build from the theoretical results of Baker, Gibbons, and Murphy (1994), Kandel and Lazear (1992), Milgrom and Roberts (1990, 1993), and Holmstrom and Milgrom (1994).

[5] Further, understanding complementarities between government policies in a transition, and the role of complementary institutions such as a legal system, are critical guiding public policy in the former Soviet Union and Eastern Europe. When designing new firms in those developing economies, the complementarities between organizational design choices will be important. The theory of complementarities has also been fruitfully applied in theoretical and empirical studies of transition economies in the former communist states (Gates, Milgrom, and Roberts (1995), Johnson and Friedman (1995), Johnson (1996), McMillan (1995)). In a final example, Bagwell and Ramey (1994) show that complementarities between organizational practices and market decisions can have implications for anti-trust policy. In their study of the growing discount retailing industry, they analyze complementarities between advertising, large scale, low prices, and cost-reducing investments in practices such as inventory management systems and automated distribution systems. They show that despite the fact that complementarities between market share and cost-reducing investments will reinforce the market power of large

complementary practice, such as training programs, which might respond more directly to taxes and subsidies. Likewise, if two practices are both desirable from a policy perspective, but subsidies and taxes are expensive to implement, governments should recognize that bundling two policies may be more effective than implementing either policy in isolation.[6]

2.2. The Demand For Organizational Design Practices and Optimality

An additional area which may deserve special analysis in the context of organizational design is whether the adoption of organizational practices is consistent with a model of optimization by the affected organization. Economists and social scientists have a variety of theories about the determination of organizational design practices. 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 transactions costs and adjustment costs have been incorporated in the literature over time, but typically the assumption is that firms are doing as well as possible subject to constraints. Much of the theory of organizations over the last fifteen years has been devoted to articulating and exploring those constraints, many of which arise due to asymmetries in information and communication costs.

Thus, an interesting goal of empirical analyses of organizations is to provide evidence about the determination of organizational practices, and in particular to asses the relative importance of agency considerations and other transactions costs. For example, a large literature studies the agency problem of a company's managers, and one way those agency problems might manifest themselves would be in the demand for organizational design practices. Agency theories may take a variety of forms: managers may be slow to adopt new practices, even when they are optimal, or they may differentially adopt practices that allow them to receive more rents. A slightly more subtle theory involves decentralization of adoption decisions. For example, the agent responsible for computer adoption may be different than the agent responsible for general training and hiring practices in a firm. Likewise, division managers may choose practices for their own division. Decentralized decision-making coupled with agency problems may lead to scenarios where agents fail to fully account for the externalities between practices.

While current economic theory provides a variety of explanations for deviations of firm choices from the "best" set of practices from the perspective of one-period productivity, it still differs substantively from the approaches taken by some strands of the literature in sociology (specifically the field of organizational behavior) and management strategy about organizations.

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retailers, the availability of cost-reducing investments may lower prices.

[6] This point likely applies more generally to the design of anti-poverty and other social programs.

For example, organizational ecology (see Hannan and Freeman, 1984) 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, March's "garbage can" theory of organizations maintains that agents may be misinformed about the costs and benefits of different practices within their own organization. Although the methods of economists and sociologists may differ, we may wish to allow for the possibility of both non-economic factors as well as economic factors in empirical analyses of the adoption of organizational design practices.

The empirical model we develop is rich enough to allow for all of these possibilities. We further show that if performance data is available, we can formulate hypothesis tests about the extent to which practices are determined to maximize profits (this amounts to a test of cross-equation restrictions between adoption and productivity equations), as well as the extent to which practices appear to be systematically related to factors which affect their productivity in use. However, in some cases, performance data is not available, or we do not have instruments available which will allow us to identify parameters of the organizational production function. In such cases, our only recourse is to draw inferences about the production function from revealed preference. Of course, this requires us to maintain (untestable) hypotheses about the optimal adoption of practices.

3. The Model

In this section, we develop a model which is intended to capture the main components of a cross-sectional analysis of the adoption and productivity of organizational design practices. The model incorporates alternative assumptions about interaction effects between practices, the mechanism through which practice adoption decisions are determined, and the nature of the joint distribution over the unobserved returns to practices. Motivated by empirical studies of organizational design, the model is tailored to applications where there are organizations with similar objectives and options available operating in heterogeneous economic environments. For example, retail outlets for goods and services (such as banks, car dealerships, service organizations, or physician groups) are designed to accomplish similar goals, but operate in economic environments which differ in demographic characteristics, labor regulations, and technological infrastructure. An important element of our analysis is that, while many of these some of these differences across environments will be observed and acted upon by the organization, some of them will be unobserved to the econometrician.

In this paper, we restrict ourselves to a static analysis (i.e., a cross-section of organizations at a given point in time). This focus allows us to highlight both the assumptions required for identification as well as the difficulties which arise from reduced-form analysis in a cross-sectional setting. Of course, the framework may be less powerful when there are important issues associated with the precise timing of adoption (as might occur in a sector characterized by network externalities) or the diffusion of information about the productivity of different practices. However, when productivity is observed, the framework allows for a test of whether adoption follows a pattern consistent with the parameters of the organizational design production function, allowing us to test some of the assumptions required for inference using cross-sectional data. While the issues associated with the development of a dynamic model in the context of a panel dataset are extremely interesting, the myriad issues which arise in that context are beyond the scope of the current paper.

This section proceeds by developing an abstract model of the organization's production function and its "demand" for organizational design practices; we then introduce econometric assumptions about observability, and provide some interpretations for the model.

3.1. The Organizational Design Production Function

This section introduces the organizational design production function; the notation is summarized in Table 1. We consider a firm t where a vector of J practices, denoted $\mathbf{y}^t=(y_1^t,..,y_J^t)$, are endogenously determined; in Section 2.1, we gave many examples of these choices, such as technology adoption, training, and human resource practices. For simplicity, we will focus on the case where each of the practices y_j^t is a discrete choice from $\{0,1\}$. Thus, there are 2^J combinations of practices, or "systems," of organizational design practices.

The productivity of these practices varies (exogenously) across firms, in ways which may or may not be observed. We distinguish between two types of exogenous variation: *practice-specific* and *system-specific*. Practice-specific exogenous variables, denoted $\mathbf{X}^t=(X_1^t,..,X_J^t)$, affect the incremental gain in productivity from adopting a practice, while system-specific exogenous variables, denoted $\mathbf{Z}^t=(Z_1^t,..,Z_K^t)$, change the returns to the *joint* adoption of a group of practices (a "system" of practices, such as high IT investments and high training). We will wish to draw a distinction between these variables throughout the paper, so we maintain separate notation for them despite the fact that the practice-specific exogenous variables are just a special case of system-specific exogenous variables (as is further discussed in Section 4).

Productivity, denoted f^t, is determined as a function of these variables according to $f^t=f(\mathbf{y}^t,\mathbf{X}^t,\mathbf{Z}^t;\mathbf{M})$; for simplicity, we will treat the case where the function f is known up to some finite-dimensional parameter vector $\mathbf{M}$. Consider a specific example, which we will refer to throughout the paper. Let $\mathbf{y}^t \in \{0,1\}\times\{0,1\}$. The "systems" in this model are the practice combinations $\mathbf{y}^t \in \{(1,1),(1,0),(0,1),(0,0)\}$. The system-specific return to system $\mathbf{l}$ is $\theta_{\mathbf{l}} + \alpha_{\mathbf{l}} Z_{\mathbf{l}}^t$, 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 $y_j^t=1$.

Thus, we have the following functional form for productivity:

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

The parameter vector is $\mathbf{M}=(\boldsymbol{\theta},\boldsymbol{\alpha},\boldsymbol{\beta})$. We can also rewrite equation (1) in a way which makes it more closely analogous to a "switching regressions model" (Heckman and MaCurdy, 1986):

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

While there is no substantive difference between the two models, each highlights a qualitatively different way of characterizing the data. The switching regressions model takes the "system" as the unit of analysis while the productivity approach emphasizes that each system is composed of a set of practices each of which comes with a separate set of benefits and costs. With these basic elements in place, we can now turn to examine a model of the organization's choices about practice adoption.

3.2. Demand for Organizational Design Practices

The organization's "demand" for practices can be treated analogously to the demand for firm inputs in a standard production problem. Since $\mathbf{X}^t$ and $\mathbf{Z}^t$ affect the returns to each of the practices, we clearly wish to allow demand to depend on these variables. An important feature of the production function specified in (1) is that the exogenous variables $\mathbf{X}$ and $\mathbf{Z}$ interact with $\mathbf{y}$ in the production function. However, the only way to rationalize an observation that demand for practices varies with $\mathbf{X}$ with a theory of profit-maximization is to assume that the returns to $\mathbf{y}$ vary with $\mathbf{X}$ in the firm's objective function: by definition, an interaction effect. Since in many applications, some of these variables will be unobserved, such an assumption will have important consequences for our empirical approach.[7]

In addition to $\mathbf{X}$ and $\mathbf{Z}$, we allow for other factors which might affect adoption, such as regulations and fixed adoption costs. Thus, we introduce a final vector of *practice-specific* exogenous variables, $\mathbf{W}^t=(W_1^t,..,W_J^t)$, which affect the demand for but not the productivity of choices. The demand for a given practice is determined according to $y_j^t = D_j(\mathbf{X}^t,\mathbf{Z}^t,\mathbf{W}^t;\mathbf{\Lambda})$. In problems where f^t is observable, an important hypotheses to examine (and exploit in estimation, if it is upheld) is that demand is determined as the solution to the following problem:

$$\mathbf{y}^*(\mathbf{X}^t,\mathbf{Z}^t,\mathbf{W}^t;\boldsymbol{\delta},\mathbf{M}) = \arg\max_{\mathrm{y}} \pi(\mathbf{y}^t,\mathbf{X}^t,\mathbf{Z}^t;\mathbf{W}^t;\boldsymbol{\delta},\mathbf{M}) \equiv f(\mathbf{y}^t,\mathbf{X}^t,\mathbf{Z}^t;\mathbf{M}) + \textstyle\sum_j k^j(y_j^t,w_j^t;\delta_j) \quad (2)$$

where k^j is the net cost of adopting practice j, is separable from f, and is independent of the level of other choice. This last feature highlights another assumption which will facilitate analysis if it is upheld: all interactions between the practices are captured within the production function f. Relaxing this assumption compromises our ability to draw inferences about the interactions between the choices without relying critically on untestable assumptions about the firm's optimizing behavior, even if productivity is observed. If practices are chosen according to (2), the parameter vector $\mathbf{\Lambda}$ will be determined as a function of the parameters $\mathbf{M}$ and $\boldsymbol{\delta}$. The function π might represent the true economic profits of the firm, or any alternative objective function faced by the agent responsible for decision-making. If f^t is *not* observable, our interpretation of π and the demand functions will be much more important, since we will be trying to make inferences about the way in which practices affect productivity from the revealed preference of the firms.

3.3. Econometric Assumptions: Observables and Unobservables

The exogenous variables $\mathbf{X}^t$, $\mathbf{W}^t$, and $\mathbf{Z}^t$ are the sources of heterogeneity between firms: they account for the fact that different firms make different choices, even when the firms have the same production technology available to them. These variables describe differences in the firms' economic environment, such as the costs of factors of production, as well as features of the firm's location, market, and regulatory environment. In order to account for unexplained variation in adoption decisions by firms, it will be necessary to assume that some of the exogenous variables are not observed.

For each class of exogenous variables, we will in general allow for two components, observed (indicated in lowercase Roman typeface) and unobserved (lowercase Greek typeface). That is, we

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[7] McElroy (1986) uses this logic in a productivity study, where the errors in factor share equations are given interpretations as factors which affect the marginal returns to inputs such as capital and labor.

let $X_j^t=(x_j^t,\chi_j^t)$, $Z_j^t=(z_j^t,\zeta_j^t)$, and $W_j^t=(w_j^t,\omega_j^t)$.

3.4. Model Interpretation

Our formal analysis will consider several alternative assumptions about the existence and joint distribution of unobserved variables. The following subsection provides further interpretations of the observable and unobservable variables in this model. Consider first system-specific exogenous variables ($\mathbf{Z}=(\mathbf{z},\boldsymbol{\zeta})$). An example where system-specific exogenous variables will be present might occur if there is variation among firms in whether they have managers or consultants who are trained in implementing a management "method," such as HPWP, but are not particularly effective at using a particular practice is isolation. Further, the systems model is most appropriate if an endogenous variable has been left unobserved, where that endogenous variable interacts with other choices.

In contrast, the variables $\mathbf{W}=(\mathbf{w},\boldsymbol{\omega})$ and $\mathbf{X}=(\mathbf{x},\boldsymbol{\chi})$ are practice-specific variables. The variables $\mathbf{W}$ represent costs of adopting and implementing a practice, but which do not impact the productivity of practices once the practices are in place; in contrast, $\mathbf{X}$ affects the returns to each practice in observed productivity.

The most obvious example of a variable represented by $\mathbf{W}$ is the price paid to buy a new technology; other examples include state regulations or government subsidies for adopting a practice. However, since we do not assume that we know the firm's overall objective function π, we allow for very general interpretations of what the firm decision-maker(s) maximize, and thus general interpretations of what $\mathbf{W}$ represents. For example, π could incorporate the career concerns of the decision-maker, and $\mathbf{W}$ could include variables representing the extent to which a practice can enhance a manager's observable human capital.

On the other hand, many factors which affect the costs and benefits of adoption may be unobserved. Examples include costs of transition from previously existing technologies or HR practices, idiosyncratic talents of the firm's managers and workers, beliefs and information of the relevant agents about individual practices, and the private benefits of the organizational decision-makers from adopting practices (for example, practice adoption may affect human capital or career concerns).

In many contexts, the returns to using a given practice will vary across firms in a way which interacts with productivity ($\mathbf{X}$). For example, even within plants owned by the same firm, the productivity of practices may depend on the special features of a particular plant's production process and final product, such as its idiosyncrasies of physical layout, quality of existing equipment, complexity, observability of effort, and sensitivity of the production process to human error. The effectiveness of technology adoption and human resource policy changes may also depend on the extent to which managers responsible for overseeing the use of the practices have the right set of skills, experience, information, and knowledge to make the best use of the new practices. Some teams of workers may be easier to train than others. A past history of conflict between workers and managers may make promises of job security less credible. Many human resource practices may vary in productivity according to presence of union regulations which limit the extent to which firms can redefine the jobs of production workers. Again, some of these factors will be unobserved.

In many applications it will be plausible to expect that the unobserved variables $\boldsymbol{\chi}$ and $\boldsymbol{\omega}$ are not statistically independent. For example, optimization errors or beliefs about productivity may be correlated across practices, and further, some firms may find it less costly to implement complex technologies than others. Finally, several organizational design or technology variables may redistribute rents to the same group of people within a firm, and the internal political power of that constituency may vary from firm to firm. This correlation will play an important role in our analysis of tests about complementarity.

3.5. The Distinction Between Systems and Practices

Finally, we expand our discussion of the distinction between practice-specific and system-specific exogenous variables. Consider an analogy to the productivity literature. In the standard productivity study, we posit a flexible functional form for a firm's cost function, typically a functional form which is linear in parameters but nonlinear in factor prices. The factor prices vary across firms; but the production function is typically taken to be fixed. In particular, the parameters which determine the elasticity of substitution between factors such as capital and labor are assumed to be fixed across firms. Typically, a productivity study will focus on a suitably narrow industry such that these assumptions are at least somewhat plausible. It is unusual for such studies to allow for, or test, the hypothesis that the elasticity of substitution varies across firms.

Of course, in organizational design, the problem is more complex. As discussed in the introduction, we typically do not observe all of the "prices" and adoption costs of practices. The variables X_j and W_j in our model can be interpreted as analogous to prices for practice j: these are the variables which determine the costs and benefits to practice j. By allowing for some of these variables to be unobserved, we depart from the standard productivity model. We refer to a model with unobservables χ and ω, but no system-specific unobservables ζ, as the *random practice model*. In such a model, the question "Does y_i increase or decrease the returns to y_j?" has a fixed answer for the sample of firms under consideration.

However, in the context of organizational design, the specification of the production function is less clear than the classical productivity analysis. It is difficult to identify theoretically what the "factors" of production are, and more difficult to argue that all factors have been appropriately accounted for. Thus, the approaches we take to identification and estimation, as well as the interpretation of reduced-form models, allow for more general hypotheses. We refer to a model with system-specific unobservables ζ as the *random systems model*. This is analogous to allowing the elasticity of substitution between two inputs to vary across firms in the standard productivity analysis. In particular, some firms might see two practices to be substitutes, while the same two practices might be complements for other firms.

Equation (1′) can be used to the illustrate the extra restrictions imposed by the random practice model as opposed to the random systems model: if we wished to place restrictions on (1′) so that it satisfied the definition of a random practice model, we would require that for a given $\boldsymbol{\zeta}$, there exists a $\boldsymbol{\chi}$ such that $\zeta_{00}^{t} + \zeta_{11}^{t} = \zeta_{01}^{t} + \zeta_{10}^{t} = \chi_1^{0t} + \chi_2^{0t} + \chi_1^{1t} + \chi_2^{1t}$. 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.

However, the random systems model presents a number of conceptual and practical problems. If the interactions between practices vary across firms, one might argue that there is not much gained by taking the "practice" as the unit of analysis. Of course, a model with J 0-1 practices generates 2^J systems, so that the practice-based view is more parsimonious to describe; however, when the systems are the unit of analysis in the econometric approach, there is little additional content in naming systems according to the practices that describe them.

Another issue is that it is difficult to conduct policy analysis when a training subsidy might lead some firms to choose higher levels of information technology, but other firms to choose lower levels. In a random systems model, we will only be able to make statements about the average level of complementarity across firms, or more generally, we will be able to describe 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 all firms see two practices as complements, we can make qualitative predictions about the effect of a policy without relying on estimates of other features of the economic environment.

In the next section, we propose a test of the restriction to the random practice model. We further argue that in designing new studies of organizational design, a consideration should be to identify situations where firms are similar enough to satisfy the restrictions of the random practice model, since identification, estimation, and interpretations will be less demanding and more powerful in such contexts.

4. Identification and General Estimation Strategies

In this section, we consider general approaches to identification and estimation of our cross-sectional model of organizational design. We establish conditions under which we can identify and estimate the distribution over unobserved variables as well as parameters of the organizational design production and practice adoption equations. We further show that the random practice model is nested within the random systems model, and thus the random practice model is both identified and can be tested against the random systems model.

We will suppress **X** in our notation for the identification results, since such variables can be incorporated in **Z**; further, we suppress $\boldsymbol{\theta}$, since without further restrictions, $\boldsymbol{\theta}$ and $\boldsymbol{\zeta}$ will not be separately identified. With these simplifications, a general model with discrete choices is given as follows (where $\mathbf{1}^t_{l_1,..,l_J}$ is an indicator for a system where $y_j = l_j$).

$$f(\mathbf{y}^t;\mathbf{Z}^t,\boldsymbol{\alpha}) \equiv \sum_{l_1=0}^{1}\sum_{l_2=0}^{1}\mathbf{1}^t_{l_1,..,l_J}\cdot\left[\zeta^t_{l_1,..,l_J} + \boldsymbol{\alpha}_{l_1,..,l_J}\cdot\mathbf{z}^t_{l_1,..,l_J}\right] + \varepsilon^t \tag{3}$$

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

$$\pi(s^t_{l_1,..,l_J}) = \upsilon^t_{l_1,..,l_J} + \eta_{l_1,..,l_J}\cdot\mathbf{z}^t_{l_1,..,l_J} + \varphi_{l_1,..,l_J}\mathbf{v}^t_{l_1,..,l_J} \tag{4}$$

Note that we have introduced the variable **v** to simplify our identification results; we will interpret this variable in terms of **w** below.

Proposition 1 *Assume π is known up to a finite dimensional parameter vector $\boldsymbol{\Lambda}\equiv(\boldsymbol{\delta},\boldsymbol{\eta})$, $f(\mathbf{y}^t,\mathbf{Z}^t,\varepsilon^t;\boldsymbol{\alpha})$ is known up to a finite dimensional parameter vector $\boldsymbol{\alpha}$, and $\mathbf{y}\in\{0,1\}^J$. Suppose that $\mathbf{y}^t$, $\mathbf{z}^t$, and $\mathbf{v}^t$ are observed, while $(\boldsymbol{\zeta}^t,\varepsilon^t,\mathbf{v}^t)$ are unobserved with joint distribution denoted H. Finally, assume that **v** and **z** are independent of $(\boldsymbol{\zeta}^t,\varepsilon^t,\mathbf{v}^t)$, and that for all $(l_1,..,l_J)\in\{0,1\}^J$ there exists a subvector, $\tilde{\mathbf{v}}_{l_1,..,l_J}$ of $\mathbf{v}_{l_1,..,l_J}$ such that, (a) $\tilde{\varphi}_{l_1,..,l_J}\neq 0$, and (b)* $\Pr(\tilde{\mathbf{v}}_{l_1,..,l_J}\in E|\mathbf{v}\backslash\tilde{\mathbf{v}},\mathbf{z})>0$ for all $E\in\Re^K$.

(i) Consider models (3) and (4), where f is observed. Let H be a set of absolutely continuous probability distributions, and suppose that $(\boldsymbol{\alpha},\boldsymbol{\Lambda})\in\mathsf{M}$, a compact subset of $\Re^n$. Then $(\boldsymbol{\alpha},\boldsymbol{\Lambda},H)$ are identified in (M,H) up to the scale of $(\boldsymbol{\Lambda},\mathbf{v}^t)$ and location of $\mathbf{v}^t$.

(ii) Consider model (4) alone, where f is not observed. Consider a set of absolutely continuous probability distributions G, and suppose that $\boldsymbol{\Lambda}\in\mathsf{L}$, a compact subset of $\Re^n$. Then $(\boldsymbol{\Lambda},G)$ is identified in (L,G) up to the scale of $(\boldsymbol{\Lambda},\mathbf{v}^t)$ and the location of $\mathbf{v}^t$.

Proof: Apply the results of Thompson (1989), Proposition 5 and 6, for the discrete choice model given by (4). Heckman and Honore (1990), Theorems 9 and 10, establish identification of the joint distribution of ($\mathbf{v}^t$,$\boldsymbol{\zeta}^t$) in the context of the Roy model.

Consider the content of the identification results. First, notice that in part (i), we can identify the mean of $\boldsymbol{\zeta}^t$; thus, we can renormalize $\boldsymbol{\zeta}^t$ to have mean zero, and let the parameter $\boldsymbol{\theta}$ represent the average productivity of a system. Further, in part (ii), where productivity is unobserved, the parameters of the "utility" function are identified only up to scale; so, we can likewise let $\boldsymbol{\tau}$ represent the mean of $\mathbf{v}$. In this case we will be able to identify the *sign* but not the magnitude of any linear combination of $\boldsymbol{\tau}$, such as those combinations which represent average complementarity.

Since an assumption of our model that all interaction effects between variables are incorporated in *f*, we now reinterpret the variable $\mathbf{v}$ in terms of $\mathbf{w}$. In particular, we can let $\varphi_{l_1,..,l_J}\mathbf{v}_{l_1,..,l_J} = \sum_{j=1}^{J}\delta_{j,l_j}w_j^{l_j,t}$, and the identification conditions of Proposition 1 can be reduced to restrictions on linear combinations of the *w*'s. Essentially, the conditions require that $\mathbf{w}$ provides sufficient variation to exhaust the space of unobservables, so that any region of the joint distribution of unobservables is potentially relevant for some realization of $\mathbf{w}$. Further, the conditions require that $\mathbf{w}$ is independent of the unobservables. Thus, existence of observed components, $\mathbf{w}$, which satisfy this condition will play a critical role in our analysis. Examples of variables which could potentially satisfy these conditions include the price of a computer system, the cost of implementing computer software and accounting procedures to manage bonus or incentive pay, or government subsidies for training at the state or local level. Further, notice that Proposition 1 requires that *both* $\mathbf{z}$ and $\mathbf{v}$ be independent of $\boldsymbol{\zeta}$. However, we can relax the assumption about $\mathbf{z}$, only at the cost of identifying the parameters $\boldsymbol{\alpha}$ and $\boldsymbol{\eta}$. The key assumption for identification of the distribution of unobservables is the conditions about $\mathbf{v}$.

A special case of the model described above is the random practice model, as discussed in Section 3.5. We can write the conditions for the random practice model as a restriction on the above model, as follows:

(RPM) Given $\boldsymbol{\zeta}$, there exists $\boldsymbol{\chi}$ such that for all $(l_1,..,l_J)\in\{0,1\}^J$, $\zeta^t_{l_1,..,l_J} = \sum_{j=1}^{J}\chi_j^{l_j,t}$.

Thus, identification of the random practice model follows from Proposition 1, and further this restriction can in principle be tested.

Corollary 1.1 *Restriction (RPM) is testable under the conditions of Proposition 1 (i).*

Likewise, the analogous restriction for part (ii) is testable.

Proof: Restriction (RPM) requires that the 2^J elements of $\boldsymbol{\zeta}$ can be constructed from $2 \cdot J$ random variables; it can equivalently be expressed as $\binom{J}{2}$ equality restrictions of the form $\zeta^t_{0,0,\mathbf{1}_{-ij}} + \zeta^t_{1,1,\mathbf{1}_{-ij}} = \zeta^t_{0,1,\mathbf{1}_{-ij}} + \zeta^t_{1,0,\mathbf{1}_{-ij}}$, one for each (i,j) pair such that $i \neq j$. This equality is invariant to the location and scale of $\boldsymbol{\zeta}$.

A variety of approaches are available to estimate the models analyzed in Proposition 1 and Corollary 1.1. The recent literature on semi-parametric methods suggests several estimators (see, for example, Thompson (1989), Ichimara and Lee (1991), Cosslett (1991)). Further, if a parametric form is assumed for the distribution over the unobservables, the models can be estimated using standard Generalized Method of Moments or Simulated Method of Moments estimators (see Hansen (1982), McFadden (1989), or McFadden and Ruud (1994)).

5. Testing Theories About Complementarity in Organizational Design

One of the main motivating questions for this paper is the question about interactions between choices. As discussed in Section 2.1, we will be particularly interested in the hypothesis that two choices are complements. The purpose of this section is to analyze the issues which arise in estimation when the true environment is characterized by both technological interactions between the practices, as well as statistical dependency between the unobserved returns to different practices. The formal model allows us to provide interpretations of the results of commonly used reduced-form procedures, thus motivating the use of structural approaches. Further, we show that systems of equations approaches can overcome potential difficulties in estimation in small samples, and further, we motivate a number of economically interesting hypothesis tests about cross-equation restrictions between demand equations and productivity equations as well as about the properties of the joint distribution of the unobservables.

To begin our analysis, consider a formal definition of complementarity

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:*

$$f(y_i^H, y_j^H, \cdot) - f(y_i^L, y_j^H, \cdot) \geq f(y_i^H, y_j^L, \cdot) - f(y_i^L, y_j^L, \cdot) \tag{5}$$

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

Note: *For a twice-continuously differentiable function $f(\mathbf{y})$, f is supermodular in* $\mathbf{y}$ *if and only if, for all $i \neq j$, $\frac{\partial^2}{\partial y_i \partial y_j} f(\mathbf{y}) \geq 0$ for all* $\mathbf{y}$.

If interaction effects are fixed across firms ($\mathbf{Z}$ is not present), (5) reduces to inequality restrictions on a series of “complementarity parameters”:

$$\kappa_{ij}(\mathbf{l}_{-ij};\boldsymbol{\theta}) \equiv \theta_{11,\mathbf{l}_{-ij}} - \theta_{10,\mathbf{l}_{-ij}} - \left[\theta_{01,\mathbf{l}_{-ij}} - \theta_{00,\mathbf{l}_{-ij}}\right] \geq 0 \text{ for all } \mathbf{l}_{-ij} \in \{0,1\}^{J\text{-}2} \tag{6}$$

In the case where there are two choices, this further simplifies to $\theta_{11} - \theta_{10} \geq \theta_{01} - \theta_{00}$. Intuitively, this just says that the returns to adopting practice 1 are higher when practice 2 has been adopted. In the two-choice model, there is a single complementarity parameter, $\kappa \equiv \theta_{11} - \theta_{01} - [\theta_{10} - \theta_{00}]$.

However, since $\mathbf{Z}$ affects the interactions between components of $\mathbf{y}$, two practices might be substitutes for some values of $\mathbf{Z}$ and substitutes for others. Choices i and j are complements for a given $\mathbf{Z}$ and $\boldsymbol{\alpha}$ if the following inequality holds:

$$\begin{aligned}\kappa_{ij}(\mathbf{l}_{-ij};\boldsymbol{\theta}) + &\left[\alpha_{11,\mathbf{l}_{-ij}} z_{11,\mathbf{l}_{-ij}} + \zeta_{11,\mathbf{l}_{-ij}} - \alpha_{10,\mathbf{l}_{-ij}} z_{10,\mathbf{l}_{-ij}} - \zeta_{10,\mathbf{l}_{-ij}}\right] \\ &- \left[\alpha_{01,\mathbf{l}_{-ij}} z_{01,\mathbf{l}_{-ij}} + \zeta_{01,\mathbf{l}_{-ij}} - \alpha_{00,\mathbf{l}_{-ij}} z_{00,\mathbf{l}_{-ij}} - \zeta_{00,\mathbf{l}_{-ij}}\right] \geq 0\end{aligned} \quad \text{for all } \mathbf{l}_{-ij} \in \{0,1\}^{J\text{-}2} \tag{7}$$

In this case, we may be interested in identifying the *average* complementarity conditional on $\mathbf{z}$. Since we have normalized the location of the distribution over $\boldsymbol{\zeta}$, this condition can be checked using the following parameters of average complementarity,

$$\lambda_{ij}(\mathbf{l}_{-ij};\mathbf{z},\boldsymbol{\alpha},\boldsymbol{\theta}) \equiv \kappa_{ij}(\mathbf{l}_{-ij};\boldsymbol{\theta}) + \left[\alpha_{11,\mathbf{l}_{-ij}} z_{11,\mathbf{l}_{-ij}} - \alpha_{10,\mathbf{l}_{-ij}} z_{10,\mathbf{l}_{-ij}}\right] - \left[\alpha_{01,\mathbf{l}_{-ij}} z_{01,\mathbf{l}_{-ij}} - \alpha_{00,\mathbf{l}_{-ij}} z_{00,\mathbf{l}_{-ij}}\right]$$

When analyzing the practice adoption equations, we can define complementarity parameters using $\boldsymbol{\tau}$ and $\boldsymbol{\eta}$ in place of $\boldsymbol{\theta}$ and $\boldsymbol{\alpha}$. Then the following result shows that the complementarity parameters of interest are identified.

Corollary 1.2. *Consider the hypotheses of Proposition 1 (i), and let* $\boldsymbol{\theta}$ *represent the mean of* $\boldsymbol{\zeta}$ *and let* $\boldsymbol{\tau}$ *be the mean of* $\boldsymbol{\nu}$. *Then the average complementarity parameters,* $\lambda_{ij}(\mathbf{l}_{-ij};\mathbf{z},\boldsymbol{\alpha},\boldsymbol{\theta})$, *are identified. Under the hypotheses of Proposition 1 (ii), the* signs *of the analogous average complementarity parameters* $\lambda_{ij}(\mathbf{l}_{-ij};\mathbf{z},\boldsymbol{\eta},\boldsymbol{\tau})$ *are identified.*

Notice further that under the assumptions of the random practice model, $\zeta_{11,\mathbf{l}_{-ij}} - \zeta_{10,\mathbf{l}_{-ij}} = \zeta_{01,\mathbf{l}_{-ij}} - \zeta_{00,\mathbf{l}_{-ij}}$, and so there is a single complementarity parameter for all firms, conditional on any observed $\mathbf{z}$. This highlights one of the main advantages of the random practice model for testing an interpretations.

Clearly, if consistent estimates of $\boldsymbol{\theta}$ can be obtained, the only remaining issue is one of hypothesis testing. Since the restrictions in (6) and (7) involve multiple inequalities, this issue is not entirely straightforward; we take this issue up in Section 5.4. However, many of the special issues which must be confronted in estimating $\boldsymbol{\theta}$, particularly using reduced-form approaches, rely on the way in which the practices vary with the observed and unobserved exogenous variables. Further, in some applications, productivity data is unavailable, so we will be interested in discovering testable consequences of complementarity for observed choices. Thus, we consider now the consequences of complementarity for the relationship between exogenous and endogenous variables. Consider the following assumptions, which we will refer to throughout the rest of the section:

(OPT) $\mathbf{y}^*$ is chosen to maximize π, as given in (2).

(ORD) π is supermodular in (y_j,X_j) and (y_j,W_j) for all j.

Condition (ORD) simply orders the variables X_j and W_j; this assumption is made so that we can speak unambiguously about the effect of *increasing* any of the exogenous variables. Using these conditions, we have the following simple comparative statics proposition (Milgrom and Roberts (1990); Topkis (1978)):

Proposition 2 *Assume OPT, ORD. If for a given* $\mathbf{Z}$, π *is supermodular in* $\mathbf{y}$, *then* $\mathbf{y}_*$ *is monotone nondecreasing in* (X_{j1},W_{j1}) *and monotone nonincreasing in* (X_{j0},W_{j0}). *Further,* $E[y_i|\mathbf{x},\mathbf{w},\mathbf{z}]$ *is nondecreasing in* $\mathbf{w}_j$.

This proposition states that, if all of the choice variables are mutually complementary, then, an increase in the gains from adopting any one practice will cause all practices will rise (weakly).[8] This is the prediction which relates the exogenous features of the firm's environment to firm choices in a systematic way: in response to an exogenous decrease in the marginal cost of one choice, *all* choices will weakly increase. Whenever this condition is satisfied, unambiguous predictions can be made about the qualitative response of a set of practices to a policy. This motivates many of the approaches based on adoption information only.

Finally, we wish to introduce a condition which we will refer to throughout the remainder of this section, a condition about exclusion restrictions:

(EXCL) There exists a vector $\mathbf{w}$ such that π is additively separable in $(y_i,\mathbf{w}_j)$ for all $i \neq j$.

In the results where we impose (EXCL), we assume for simplicity that *all* elements of $\mathbf{w}$ satisfy (EXCL).

[8] Topkis (1978), Milgrom and Roberts (1990), and Athey, Milgrom, and Roberts (1996) expand on the role of pairwise relationships between variables, arguing that this is the right notion of complementarity for models of additively separable profit functions.

5.1. Reduced Form Estimation and Biases

5.1.1. Previous Empirical Analyses of Complementarity in Organizational Design

Several approaches to testing theories about complementarity in organizational design have been used in the literature.[9] The first approach, analyzed theoretically in Holmstrom and Milgrom (1994), Arora and Gambardella (1990), and Arora (1995), is to test for correlation between the organizational design practices, conditional on observables. Several recent papers pursue this approach. For example, Brickley (1995) explicitly tests Holmstrom and Milgrom's (1994) predictions using data about franchising contracts. Other studies which incorporate similar analyses as evidence include ISP, Brynjolfsson and Hitt (1995), 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).

Another approach is based on exclusion restrictions. As shown by Holmstrom and Milgrom (1994) and Arora (1996), a simple reduced-form regression of each organizational variable on exogenous parameters can provide evidence which is consistent with a hypothesis of underlying complementarity between the choice variables. 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.[10] We discuss the strengths and weaknesses of this approach in Section 5.2.

Finally, authors such as ISP and Hwang and Weil (1996) use an approach which builds on the empirical productivity literature . For example, ISP's empirical study of complementarities between human resources practices in steel finishing lines follows the productivity literature in that it attempts to estimate the production function directly using a panel data set; they then interpret the parameters in terms of theories about complementarity. However, the data for these studies is gathered from surveys which were not designed to include variables which could be used as

[9] 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 (see Joreskog, K.G. and D. Sorbom (1995), *LISREL 8: Structural Equation Modeling with the SIMPLIS Command Language*, Chicago: Scientific Software International.), 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. While LISREL can accommodate correlation across equations, procedures available through LISREL do not allow for the estimation of the joint distribution of unobserved returns and costs to individual practice adoption (the focus of H2, below). Thus, the analyses will be subject to the same selectivity biases associated with ordinary least squares.

[10] For examples of this approach in other contexts, see Deolalikar and Evenson (1989) and Braga and Willmore (1991).

instruments; further, in ISP, the practices are correlated, so that not all combinations of practices are observed. Thus, ISP rely on constructed indices of the systems which cannot be used to disentangle interrelationships between the practices.

5.1.2. Evaluating Reduced-Form Approaches

In this section, we show that not only will unobserved heterogeneity in the returns to practices lead to bias in reduced form estimation procedures, but further, the direction of this bias depends on the qualitative properties of the joint distribution over unobserved returns. Further, for several economically plausible sets of assumptions about the unobservables, a variety of reduced-form approaches all deliver biases in the same direction. For example, when the unobserved returns to practices are positively correlated, then the conditional correlations between the practices will be positive, and further, the estimates of interaction effects from OLS and 2SLS will also be biased upward. This result has serious implications for our ability to draw inferences about interaction effects using reduced-form approaches.

For simplicity, in this section we focus on a two-choice model; we further maintain the assumption that there are no observed exogenous variables, **z**, which affect the returns to different systems. This allows us to avoid making statements about complementarity which are conditioned on **z**. However, our results generalize naturally when these assumptions are relaxed.

This section considers three main reduced-form procedures, in part motivated by the existing literature discussed above. The first procedure, which requires us to estimate the sign of $\text{cov}(y_1,y_2|\mathbf{x},\mathbf{w})$, is motivated by the fact that if y_1 and y_2 are complements, under the conditions of Proposition 1, an increase in ω_1 will lead to an increase in both y_1 and y_2, creating a force in favor of positive correlation between y_1 and y_2. This procedure has the advantage that it does not require information about productivity. If productivity data is available, we can further consider using OLS or 2SLS to directly estimate the complementarity parameter, $\kappa \equiv \theta_{11} - \theta_{01} - [\theta_{10} - \theta_{00}]$, from the following productivity equation:

$$f^t = \mathbf{1}_{00}^t\theta_{00} + \mathbf{1}_{01}^t\theta_{01} + \mathbf{1}_{10}^t\theta_{10} + \mathbf{1}_{11}^t\theta_{11} + \beta_{1t}x_{1t}y_{1t} + \beta_{2t}x_{2t}y_{2t} + \xi_t \qquad (8)$$

The 2SLS estimation will instrument for s_{ij} nonlinear functions of w_1 and w_2. For example, $w_1 \cdot w_2$, $(1-w_1)\cdot w_2$, $w_1\cdot(1-w_2)$, and $(1-w_1)\cdot(1-w_2)$ are potentially instruments under (EXCL). However, as we will argue below, they are only valid instruments under very strong assumptions about the presence of unobserved returns to practices. We let $\hat{\kappa}^{OLS}$ and $\hat{\kappa}^{2SLS}$ represent the estimates of κ derived from OLS and 2SLS, respectively.

We discuss reduced form procedures with reference to several maintained hypotheses. We will then introduce a series of propositions which relate the maintained hypotheses to the interpretation of reduced-form procedures.

Label	Description
M-WI	$\boldsymbol{\omega}^t$ is present, but $\boldsymbol{\omega}^t$ is an independent vector.
M-X0	$\boldsymbol{\chi}^t$ is not present.
M-XI	$\boldsymbol{\chi}^t$ is present, but $\boldsymbol{\chi}^t$ is an independent vector.
M-Z0	$\boldsymbol{\zeta}^t$ is not present.
M-ZI	$\boldsymbol{\zeta}^t$ is present, but it is an independent vector and the components are exchangable in the joint distribution. (Random systems model.)

We begin with a simple proposition which shows that, when the unobserved exogenous variables take a particularly simple form, reduced form testing procedures can be used to provide evidence about complementarity. The proposition considers the case where we are in a random practice model, so that all unobserved heterogeneity is practice-specific, and further the only unobserved heterogeneity comes through $\boldsymbol{\omega}$, which does not enter the production function directly. Finally the proposition requires that the components of $\boldsymbol{\omega}$ are statistically independent. 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 which relax the assumptions.

Proposition 3 *Assume OPT, ORD, M-Z0, M-X0. Then:*

(i) OLS provides a consistent estimate of κ.

(ii) If, in addition, M-WI holds, then $\mathrm{cov}(y_1,y_2|\mathbf{x},\mathbf{w})\geq 0$ *if and only if* $\kappa\geq 0$.

While the conditions required for this proposition are quite strong, it is worth considering economic environments where it might apply. We have already discussed the content of the assumption that the random practice model, rather than the random systems model, applies in Section 3.5. The additional assumption here is that all of the heterogeneity in the returns to practices enters into the firm's objective function *outside* of the productivity equation. The interpretation is that the production technology is uniform across firms, but the adoption or implementation costs differ across firms. This is more likely to be satisfied when the performance measure is narrow, such as the output of an assembly line (Ichniowski, Shaw, and Prennushi, 1997), and further differences in adoption decisions are either random or driven by adoption costs which do not affect the productivity of practices once in use. The assumption will be almost impossible to satisfy in applications such as corporate finance, where the productivity measure is one of the overall financial performance of the firm, so that all adoption costs affect performance.[11] In general, it may be possible to get information about many of the X variables through careful survey design; but it will in general be difficult to argue from a theoretical perspective that no relevant variables have been missed.

Now consider relaxing some of the assumptions of the latter proposition. Before continuing, we need an additional definition. A vector $\mathbf{x}$ of random variables is *affiliated* if, for all $i \neq j$ and all nondecreasing functions g_i and g_j, $\text{cov}(g_i(x_i), g_j(x_j)) \geq 0$. This implies in particular that conditional on any region $[a,b] \times [c,d]$, x_i and x_j are positively correlated. Now, suppose that $\boldsymbol{\omega}$ are affiliated and not independent. Then if $\kappa=0$, $\text{cov}(y_1, y_2|\mathbf{x},\mathbf{w})>0$, and further we might have $\text{cov}(y_1, y_2|\mathbf{x},\mathbf{w})<0$ even if $\kappa<0$ (y_1 and y_2 are substitutes). More generally, however, suppose that we allow for variables $\boldsymbol{\chi}^t$, which affect the marginal productivity of the practices and enter the observed portion of productivity. But let us maintain for the moment the assumption M-XI, which requires that the components of $\boldsymbol{\chi}^t$ be independent. In this case, we our results about the correlation tests remain unchanged; however, standard selection biases arise in the production function estimates.

Proposition 4 *Assume OPT, ORD, EXCL, M-Z0, M-XI. Then, if* $\kappa>0$ *(complementarity holds),* $\hat{\kappa}^{OLS}<\kappa$ *and* $\hat{\kappa}^{2SLS}<\kappa$*; further, it is possible that* $\hat{\kappa}^{OLS}<0$ *and* $\hat{\kappa}^{2SLS}<0$.

To interpret Proposition 4, let us further simplify the model so that no variables $\mathbf{x}$ enter the productivity equation; further, suppose that the unobserved returns to choosing $y_j=0$ are identically 0, and let χ_j be the unobserved return to adopting practice j. We can then compute the expected value of f^t conditional on $\mathbf{y}$, as follows:

$$\begin{aligned} E[f^t|\mathbf{y}] = I_{00}^t\theta_{00} + I_{01}^t \cdot \left[\theta_{01} + E\left[\chi_2|\mathbf{y}^* = (0,1)\right]\right] + I_{10}^t \cdot \left[\theta_{10} + E\left[\chi_1|\mathbf{x},\mathbf{y}^* = (1,0)\right]\right] \\ + I_{11}^t \cdot \left[\theta_{11} + E\left[\chi_2|\mathbf{y}^* = (0,1)\right] + E\left[\chi_1|\mathbf{y}^* = (1,0)\right]\right] \end{aligned} \tag{9}$$

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 $\boldsymbol{\chi}$). The fact that the bias terms are nonzero can be seen in Figures 1 and 2, which show that regions corresponding to each choice. However, in our application, our focus is not on any specific element of $\boldsymbol{\theta}$ but on the bias associated with κ, the complementarity coefficient. Thus, we are able

[11] ISP implicitly rely on such an assumption. For their estimates to be valid, one must maintain the assumption that their measure of productivity (uptime on steel finishing lines) is sufficiently narrow, and their controls

to calculate the expected value of $\hat{\kappa}^{OLS}$ as follows:

$$
\begin{aligned}
E[\hat{\kappa}^{OLS}] = \kappa + E\left[\chi_1 \middle| \mathbf{y}^* = (1,1)\right] - E\left[\chi_1 \middle| \mathbf{y}^* = (1,0)\right] \\
+E\left[\chi_2 \middle| \mathbf{y}^* = (1,1)\right] - E\left[\chi_2 \middle| \mathbf{y}^* = (0,1)\right]
\end{aligned}
\tag{10}
$$

The last four terms on the right-hand side of (10) will sum to zero only in the case when f is additively separable (i.e., no complementarities) *and* the components of $\boldsymbol{\chi}$ are uncorrelated. In general, the existence of interaction effects among the elements of **y** or correlation among elements of $\boldsymbol{\chi}$ will lead to a biased estimate of the test statistic κ. If $\boldsymbol{\chi}$ is independent and the choices are complements, then 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, with independent $\boldsymbol{\chi}$ and complementary choices, this bias will be negative: OLS will underestimate complementarity. This is a generalization of the standard single-choice selection bias.

Now consider assuming EXCL, and using 2SLS to estimate (8). Unfortunately, the particular nature of the endogeneity problem confounds our analysis. Consider again the two-choice example. 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 **y** is endogenously determined as a function of all of the exogenous variables, (**x**, **w**, $\boldsymbol{\chi}$, and $\boldsymbol{\omega}$), neither **x** nor **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. This will lead to biased estimates for the parameters of the productivity equation.

Now suppose that we allow for correlation in the components of $\boldsymbol{\chi}$. Then, our results about the sign of the bias from the previous analysis will be reversed: OLS and 2SLS will *overestimate* complementarity. Further, the correlation tests will also yield positive results even when the choices are substitutes.

Proposition 5 *Assume OPT, ORD, EXCL, M-Z0, M-WI. Assume further that* $\boldsymbol{\chi}$ *are affiliated and not independent. If* κ=0 (*technological independence),* $\hat{\kappa}^{OLS}>0$, $\hat{\kappa}^{2SLS}>0$, *and* $\mathrm{cov}(y_1,y_2|\mathbf{x},\mathbf{w})>0$; *further, the latter inequalities may still hold if* $\kappa<0$.

This proposition highlights a fundamental identification problem for testing complementarities with reduced form procedures: both the reduced for adoption approaches, as well as the reduced form productivity approaches, find complementarities (erroneously) under the same assumptions. The intuition is straightforward: practice 1 is adopted when its unobserved returns are high. But

---

sufficiently comprehensive, that variables such as $\boldsymbol{\chi}$ are of minimal importance.

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.

Several existing studies attempt to document complementarity by providing (independent) evidence in the spirit of the correlation tests and the productivity analysis.[12] Together, Propositions 4 and 5 show that any interpretation of the OLS or 2SLS estimates requires a maintained assumption about the unobservables: 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.

We are particularly interested in Proposition 5 because it describes a set of hypotheses which is plausible in many economic environments. As discussed in Section 3.4, when the benefits of adopting practices depend on the skills and talents of managers and workers, the productive benefits of practices like information technology, job security, and on-the-job training are likely to be correlated across firms.

While the effects due to $\boldsymbol{\chi}$ are perhaps most relevant for the applications which motivate this paper, we also wish to consider the random systems model. It turns out that analogous results are available for the random systems model: there exists a set of economically interpretable assumptions which lead to both positive correlation in the observed practices, as well as overestimates of the complementarity parameters in OLS and 2SLS regressions. If the systems (1,1) and (0,0) have unobserved returns which are *riskier* than (0,1) and (1,0), then (1,1) and (0,0) are more likely to be chosen, all else equal. This effect is further reinforced if the unobserved returns to (1,1) and (0,0) are negatively correlated across firms.

Proposition 6 *Assume OPT, ORD, EXCL, M-X0, M-WI. Assume further that $\boldsymbol{\zeta}$ is an independent vector, but that ζ_{11} and ζ_{00} are mean-preserving spreads of ζ_{10} and ζ_{01}. Then if $\kappa=0$ (technological independence), $\hat{\kappa}^{OLS}>0$, $\hat{\kappa}^{2SLS}>0$, and $\mathrm{cov}(y_1,y_2|\mathbf{x},\mathbf{w})>0$; further, the latter inequalities may still hold if $\kappa<0$.*

In interpreting this result, it is important to remember that in the random systems model, we must interpret our parameters as giving estimates of average complementarity, since the parameter

[12] ISP is one example; Arora and Gambardella (1990) propose testing for correlation between the residuals of OLS regressions of each choice on the exogenous variables which directly affect that choice. They argue that a finding of positive correlation is consistent with complementarity and apply this technique to study choices by incumbent pharmaceutical firms to invest in internal biotechnology capabilities and to invest in acquiring biotechnology from external sources. In another example, Brickley (1995) finds positive correlation between contract provisions which

of complementarity will vary across firms.

5.1.3. Implications for reduced from testing effects of a single choice.

Our above analysis also has implications for our interpretations of reduced form analyses of the effects of an individual choice on performance. Heckman (1997) analyzes selection biases in the evaluation of a single government program using instrumental variables; this section gives an alternative motivation for such biases. For example, if we wish to study the effects of training programs on productivity, but training interacts with information technology in the firm's production function, then our interpretations of reduced form models will necessarily depend on our assumptions about the interaction between the two practices as well as the correlation in the unobserved returns of the two practices. However, even if there are no unobserved returns to the practice of primary interest, failing to account for information technology in the analysis will affect interpretations.

Suppose for the moment that the study has succeeded in measuring all of the relevant features which affect the returns to y_1, so that χ_1=0, and that we are interested in measuring $\lambda_1 \equiv \theta_{11} - \theta_{01}$. Even in this case, however, and even if some exogenous factors which affect the returns to y_2 have been measured, the presence of unobserved returns to y_2 will lead to an estimate of the returns to y_1 conditional on y_2=1, as follows:

$$\hat{\lambda}^{OLS} = E[f^t|\mathbf{y}=(1,1)] - E[f^t|\mathbf{y}=(0,1)] = \theta_{11} - \theta_{01} + E\left[\chi_2|\mathbf{y}=(1,1)\right] - E\left[\chi_2|\mathbf{y}=(0,1)\right]$$

As above, we can also compute an analogous coefficient for a 2SLS estimation.

Proposition 7 *Suppose OPT, ORD, EXCL, and M-Z0.*

(i) Suppose either M-XI holds or χ_1=0. *Then if* κ>0, $\hat{\lambda}^{OLS}<\lambda$ *and* $\hat{\lambda}^{2SLS}<\lambda$, *while if* κ<0, $\hat{\lambda}^{OLS}>\lambda$ *and* $\hat{\lambda}^{2SLS}>\lambda$.

(ii) Alternatively, suppose $\boldsymbol{\chi}$ *is affiliated, not independent. Then if* κ=0, $\hat{\lambda}^{OLS}>\lambda$ *and* $\hat{\lambda}^{2SLS}>\lambda$.

This result highlights the benefits of understanding the interactions in an organization's production function, even when only one practice (such as a training program) is of immediate policy relevance. Even if we rule out unobserved variables which interact with the practice of interest, and even if we observe variables (such as training subsidies and computer subsidies) which are uncorrelated with the unobserved returns to all practices, our interpretation of the results of reduced-form productivity analyses will hinge on our hypotheses about the interaction between

provide incentives to franchisees in an empirical study of franchising versus company-owned stores.

training and computer technology, so long as we have not observed all of the factors which affect the returns to computers. Thus, we will not know whether increasing the magnitude of the training subsidy will affect computer adopters more or less than indicated by $\hat{\lambda}^{OLS}$ and $\hat{\lambda}^{2SLS}$.

5.2. Testing Net Interaction Effects Using Exclusion Restrictions

Another potential approach to drawing inferences about complementarity is motivated by Proposition 2. Under the random practice model, and if $\mathbf{z}$ is not present, y_j^* is nondecreasing in w_i, $i \neq j$, if and only if $\kappa \geq 0$. Since sums of nondecreasing functions are nondecreasing, this result will be preserved when we take expectations with respect to $\boldsymbol{\chi}$ and $\boldsymbol{\omega}$.

Proposition 8 *Assume (OPT), (ORD), and (EXCL). Suppose further that there are only 2 choices. Under M-Z0,* $E[y_1|\mathbf{x},\mathbf{w}]$ *is nondecreasing in* w_2 *if and only if* $\kappa \geq 0$.

However, under the random systems model, things are more complicated. The average effect of a change in w_i will depend on the relative likelihood of realizations of $\boldsymbol{\zeta}$ where the choices are complements, and there is no clear relationship between the effect of w_2 on $E[y_1|\mathbf{x},\mathbf{w}]$, and the average level of complementarity in the population.

Proposition 7 relies on relatively few assumptions. However, even when these assumptions are satisfied, using the exclusion restrictions approach, we are unable to disentangle the nature of interaction between any pair of variables when there are more than two endogenous variables (Arora, 1996). 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 $\mathbf{w}$, if the coefficient on an element of $\mathbf{w}$ is significantly negative, we would reject the hypothesis that *all* pairs are complements. Such a result could potentially be useful, but an incomplete test for any individual pair of practices.

To uncover the direct effect of one choice on another, one might consider an alternative regression of the following form:

$$y_1^t = \alpha_1^t + \lambda_{12} y_2^t + \cdots + \lambda_{1J} y_J^t + \beta_1 x_1^t + \delta_1 w_1^t + \varepsilon_j \tag{11}$$

Of course, such a specification will be prone to bias in the random practice model, since $(y_2,..,y_J)$ will be correlated with the error in this regression. Instrumental variables techniques may be employed, but since $\mathbf{y}_*$ is nonlinear, linear two stage least squares procedures will not provide direct estimates of the parameters of interest for testing complementarity. However, an appropriately specified nonlinear instrumental variables procedure can potentially uncover information about the pairwise interaction effects.

5.3. Systems of Equations Approaches

This section describes the way in which the two sets of moment conditions, those based on the productivity equation and those based on adoption equations, complement each other in order to yield precise estimates of the parameters of interest even when choice variables are highly correlated and when unobserved heterogeneity is important. In particular, this procedure can distinguish between alternative assumptions about technological and statistical interactions between variables in a wide range of data sets, subject to the availability of instruments. The feasibility and sensitivity of this procedure in small samples will then be addressed in Section 8.

Consider a more specific approach to estimating the model described in Section 4. Since in its most general form, our model is a special case of models already considered in the literature, we will discuss the special issues involved in semi-parametric estimation here. Instead, we will discuss an approach with a parameterized distribution over unobserved variables, such as the joint normal distribution,[13] and estimation using GMM or simulated method of moments.

If we impose the restrictions implied by (OPT) (which will be testable, as discussed in the next section), we can solve for the optimal choices $\mathbf{y}_*$, that is, those which solve (2), for a given vector of parameters and realization of (observed and unobserved) random variables. For simplicity, consider the random practice model. Then, we can construct the following moment conditions.

[13] We have several motivations for focusing on parameteric versus non-parametric approaches in our discussions. First, many of the applications which we might wish to consider have inherently limited samples -- limited by the number of firms within a narrowly defined industry, as well as by the time and expense of gathering detailed data about internal organization. Further, allowing for a distribution of unobservables with a variance-covariance matrix which is unrestricted provides the most parsimonious specification that still accomodates the alternative hypotheses about unobserved heterogeneity analyzed in this paper. Finally, a number of economically interesting hypothesis tests (described in Section 7) can be posed in terms of restrictions on the variance-covariance matrix of the unobservables. 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.

$$m^t(\boldsymbol{\theta},\boldsymbol{\gamma},\boldsymbol{\eta}) = \begin{pmatrix} \left[y_1^t - \int_{\boldsymbol{\chi}^t}\int_{\boldsymbol{\omega}^t} y_{1*}(\mathbf{x}^t,\mathbf{w}^t,\boldsymbol{\chi}^t,\boldsymbol{\omega}^t;\boldsymbol{\theta},\boldsymbol{\beta},\boldsymbol{\delta})dH(\boldsymbol{\chi}^t,\boldsymbol{\omega}^t) \right] \cdot \mathbf{v}_1^t \\ \vdots \\ \left[y_J^t - \int_{\boldsymbol{\chi}^t}\int_{\boldsymbol{\omega}^t} y_{J*}(\mathbf{x}^t,\mathbf{w}^t,\boldsymbol{\chi}^t,\boldsymbol{\omega}^t;\boldsymbol{\theta},\boldsymbol{\beta},\boldsymbol{\delta})dH(\boldsymbol{\chi}^t,\boldsymbol{\omega}^t) \right] \cdot \mathbf{v}_J^t \\ \left[f^t - E_{\boldsymbol{\chi}^t}\left[f(\mathbf{x}^t,\mathbf{y}^t,\boldsymbol{\chi}^t;\boldsymbol{\theta},\boldsymbol{\beta}) \middle| \boldsymbol{\gamma},\boldsymbol{\eta},\mathbf{y}_*^t(\mathbf{x}^t,\mathbf{w}^t,\boldsymbol{\chi}^t,\boldsymbol{\omega}^t;\boldsymbol{\theta},\boldsymbol{\beta},\boldsymbol{\delta}) = \mathbf{y}^t \right] \right] \cdot \mathbf{v}_{J+1}^t \end{pmatrix} \tag{12}$$

Under our identifying assumptions in Proposition 2 and (OPT), $E[m^t(\boldsymbol{\theta},\boldsymbol{\gamma},\boldsymbol{\eta})]=0$.

To interpret (12), 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.

There are several advantages to estimating the full system of demand and productivity equations jointly in the context of drawing inferences about complementarity. Of course, there are obvious efficiency gains. However, according to our above analysis, if f is supermodular and the exogenous variables are associated, then $\mathbf{y}$ will be positively correlated. This correlation may reduce the power associated with direct tests of complementarities through the estimation of f from a single productivity equation. For example, in the case where J = 2, we may not see many observations of (0,1) and (1,0). However, a direct test for supermodularity of f requires an estimate of f at those points in order to estimate $\kappa \equiv \theta_{00} + \theta_{11} - \theta_{01} - \theta_{10} \geq 0$. This is paradoxical: the stronger the evidence in support of supermodularity in terms of revealed preference, the more difficult it is to test supermodularity directly. In contrast, under the optimization hypothesis, we gain information about $\theta_{11} - \theta_{01}$ and $\theta_{11} - \theta_{10}$ simply by the fact that the firm *chose* to implement (1,1). Thus, by using the adoption equations as well as the productivity equation, we can potentially overcome the multicollinearity problems which arise inherently in problems with complementarity.

5.4. *Issues for Hypothesis Testing*

As suggested above, several issues arise in the specific forms of the hypothesis tests associated with each of the procedures described above. 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 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. In the first, one may want to take as the null hypothesis that the production function is in fact supermodular ($\kappa \geq 0$); alternatively, one may want to take the null to be the case of no complementarity ($\kappa \leq 0$). Of course, this latter hypothesis is the more conservative one; only a statistically significant positive coefficient on the test statistic (according to a one-tailed t-test in the case of two choice variables) will be evidence for complementarity.

However, the issues which arise in posing and implementing our hypothesis tests are not limited to our formulation of the null hypothesis. In particular, for the case of more than two choice variables, pairwise complementarity will imply multiple inequality restrictions. For example, in models with *J* discrete choices, each test for pairwise complementarity is composed of $2^{(J\text{-}2)}$ linear inequality restrictions. Each restriction imposes pairwise complementarity between the two choices of interest, for a given combination of the other J-2 choices. In the case of multiple inequality restrictions, specifying the appropriate critical value for a test of a certain size is more subtle than in the case of multiple equality restrictions. As developed by Gourieroux, Holly, and Manfort (1981), Kodde and Palm (1986), 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 restrictions are non-linear or the number of restrictions is greater than 2), there exists an inherent ambiguity in the specification of the distribution of the test statistic. In particular, this distribution will depend on which restrictions are slack and which are tight. In the case of nonlinear restrictions, there may be an infinite number of potential distributions.

Fortunately, the restrictions implied by our model are linear inequality restrictions, though the number of restrictions for each pairwise test will be greater than 2 when the number of discrete choice variables is greater than 3. In this case, Wolak suggests that the ambiguity associated with linear restrictions can be overcome, though one must be careful to calculate the test statistic under each of the alternative distributions. We hope to present evidence regarding the outcomes of test statistics under different alternatives in future work.

6. Testing Theories About the Adoption of Organizational Design Practices

In the last section, we argued that there may be substantial efficiency gains to estimating complementarity parameters by jointly estimating the full set of moment conditions (12), imposing the restrictions implied by optimality. In this section, we describe additional advantages to estimating the full system, but allowing the parameters to vary across equations. In particular, additional tests can be performed by estimating the adoption equations independently of the productivity equation, allowing for tests of cross-equation restrictions implied by optimization of the organization's objective function.

In the context of our model of an organizational design production function composed of combinations of discrete choices, the set of testable restrictions is directly linked to the nature of the unobserved heterogeneity. If we allow for unobserved interaction effects in the adoption equations which differ from those in the productivity equations, we will never be able to distinguish whether an agent's patterns of adoption violate optimality, or are instead simply responding to unobservables. In contrast, in the random practice model, the interaction effects between practices are fixed for all organizations in terms of adoption and productivity. Thus, it is possible to identify differences in the complementarity parameter estimated in the productivity equation versus the complementarity parameter (appropriately scaled) estimated in the adoption equations. Further, we can test whether the firm over- or under- exploits the interactions between firms; as motivated in Section 2, decentralized decision-making can lead to decisions which fail to incorporate all of the externalities of practice adoption decisions.

In the case where the cross-equation restrictions *can* be rejected, there are several potential explanations for the lack the failure of the restrictions. The simplest explanation is that the net benefits associated with the interaction effects under examination are only partially captured in the productivity measure, f. This explanation would be particularly plausible when the organizational elements are quite general and the productivity measure is relatively "narrow;" one particular instance when this "under-inclusion" might take place is when the firm is maximizing a dynamic optimization problem but the analysis is performed on a single-cross-section. In some circumstances, however, it may be possible to provide evidence (perhaps qualitative) that the productivity measure captures an unbiased estimate of the level of productivity. In that case, the failure of the cross-equation restrictions can arise from several sources of economic interest, including the presence of agency in decision-making, the presence of costs in communicating information about the benefits to adoption to the relevant decisionmakers, and the possibility that the complexity of organizations leads to the adoption behavior predicted by some strands of sociology, behavior which is inconsistent with any model of optimization. For many studies of organizational design, gathering the additional data necesssary to distinguish between these hypotheses (such as by gathering data about the structure of decision-making authority or an observable measure of the costs of information transmission) seems to be a promising area for research.

We turn now a set of Monte Carlo experiments to evaluate the issues discussed in this section, including our focus on the properties of different estimators under various assumptions about the economic environment.

7. Testing Hypotheses About The Nature Of Unobserved Heterogeneity

THIS SECTION UNDER CONSTRUCTION!

8. Monte Carlo Experiments

THIS SECTION AWAITS REVISION

In this section, we present the results from some Monte Carlo exercises. These exercises are designed to illustrate several of the issues raised in our formal analysis, as well as to demonstrate feasibility and performance of our structural approach in small samples. In particular, we compare the statistical properties of different estimators in the presence of unobserved heterogeneity. Our analysis is meant to highlight the inherent tradeoff between accommodating the potential presence of unobserved heterogeneity, the assumptions required to justify our proposed econometric approach, and finally, the potentially heavy computational burden required to execute these procedures. These issues are examined in the context of relatively small Monte Carlo datasets (400 observations). Our focus datasets of this size is purposive; empirical studies of complementarity in organizational design will most likely require substantial primary data gathering efforts, and the nature of many problems limits potential sample size to at most a few thousand observations (such as the number of firms in a particular industry).

Our preliminary Monte Carlo analysis focuses on the two-choice model described by (1) and (2), where we are interested in the sign and magnitude of the "complementarity parameter" $\kappa \equiv \theta_{00} + \theta_{11} - \theta_{01} - \theta_{10}$. There are two hypotheses we would like to test in this model: $\kappa \geq 0$ and $\kappa \leq 0$. Thus, evidence in favor of complementarity can come in two forms: first, we *do not* reject the null of complementarity, $\kappa \geq 0$; and second, we *do* reject the null that the choices are substitutes, $\kappa \leq 0$. The latter is the more stringent test for a finding of complementarity.

We performed several Monte Carlo experiments. The experiments illustrate our theoretical discussions of the biases associated with the productivity approaches when OLS and standard instrumental variables are used. We conducted 1000 experiments, each with 400 observations, where for each iteration, we generated simulated data according to the data generating process described by our model and performed a battery of econometric procedures. We analyze the ability of the different procedures to identify the correct magnitude of the supermodularity coefficient, and the ability of the GMM estimator to distinguish complementarity from unobserved heterogeneity. We performed a series of estimation procedures and hypothesis tests. In particular, we estimated the conditional and unconditional correlation of the endogenous variables, and further we performed the OLS and instrumental variables procedures (Procedures 5 and 6).

Finally, we executed the system of equations GMM approach described in Procedure 8. However, in this version of the paper, we have available the results from our GMM procedures for only 50 of these experiments, so the interpretations of our results for those experiments should be appropriately qualified (although the GMM procedures turn out to be sufficiently precise so that even the small number of experiments conducted are informative).

Table 2 describes the parameter values and other features of the series of experiments. We performed a series of experiments where the choice variables did not interact (κ =0), and then we performed the same series of experiments for the case where the choices are complements (κ =.5). Our experiments included six different specifications for the patterns of correlation between the unobserved exogenous variables. For example, experiments A and B have no covariation in $\boldsymbol{\chi}$ and $\boldsymbol{\omega}$, while the next three are characterized by different combinations of positive correlations between these variables. The final experiment has negative correlation in $\boldsymbol{\chi}$ and $\boldsymbol{\omega}$.

Table 3 presents the results from these experiments, showing mean estimates of κ and their (empirical) standard deviation in each experiment for several different estimation procedures. The table also shows the frequency with which each combination of choices was observed in each experiment. Notice that in experiments characterized by either complementarity or positively correlated $\boldsymbol{\chi}$ and $\boldsymbol{\omega}$, we tend to see combinations (0,1) and (1,0) much less frequently than (1,1) and (0,0). However, in experiment F_.5, when the choices are complements but $\boldsymbol{\chi}$ and $\boldsymbol{\omega}$ are negatively correlated, we see that the effects of unobserved heterogeneity can overwhelm the effects of complementarity: the choices are highly negatively correlated.

To see the different effects of $\boldsymbol{\chi}$ (which affects f directly) and $\boldsymbol{\omega}$ in this context, it is useful to compare experiment A to B, and C to D. In experiments B and D, which do not have variables $\boldsymbol{\chi}$, we see that OLS and instrumental variables estimate the true complementarity coefficient, κ, extremely precisely. In contrast, when the roles of $\boldsymbol{\chi}$ and $\boldsymbol{\omega}$ are reversed in experiments A and C, the estimates of κ become much less precise, and further we see biases except in the case where $\boldsymbol{\chi}$ is independent and κ = 0 (case A_0). In experiment A_.5, the estimates are biased downward: due to selection biases, we underestimate the true degree of complementarity. In contrast, when $\boldsymbol{\chi}$ is positively correlated, we see large upward biases in our estimates of κ. Finally, experiment F_.5 shows that *negative* correlation in $\boldsymbol{\chi}$ and $\boldsymbol{\omega}$ can in principle lead to findings of strong "evidence" against complementarity even when the choices are in fact complements. All of these finding are consistent with the theoretical analysis in previous sections; they illustrate the striking effects of alternative assumptions about the economic environment.

Although we performed only 50 experiments with GMM, the results are quite suggestive. In each of the experiments, the GMM estimator was able to distinguish complementarity parameters and parameters of the distributions of $\boldsymbol{\chi}$ and $\boldsymbol{\omega}$. While the presence of the variables $\boldsymbol{\chi}$ decreases the precision of the estimator, GMM seems to exhibit neglible biases, and its precision is comparable to OLS (and much greater than instrumental variables). Of course, the stellar performance of the GMM estimator in our experiments will not be paralleled in data sets where our functional forms do not conform exactly to the data generating process.

Table 4 then states frequencies with which our hypothesis tests give evidence of complementarity. Consider experiments C-0 and E-0, where $\boldsymbol{\chi}$ is important and positively correlated, hypothesis tests based on any of the standard approaches give misleading results. In each of the 1000 experiments, we reject the null that the choices are negatively correlated, unconditionally or conditional on exogenous variables. Further, in each of the 1000 experiments, we cannot reject the null of complementarity using the OLS estimator, and further, in 97% of the experiments we were able to reject the null that the variables are *not* complements. Thus, our results again illustrate the biases predicted by theory. Instrumental variables is less precise and makes fewer errors of inference; however, it still rejects the null that the variables are *not* complements in 50% of the experiments. Likewise, in experiment A_.5, the selection bias leads OLS and IV to reject the null of *no* complementarity in only 6% and 38% of the experiments, respectively, despite the fact that the choices are in fact complementary.

GMM is more reliable than OLS and IV in terms of inferences. However, our hypothesis tests are performed using the asymptotic distributions and thus we see that they tend to overestimate the precision of GMM; in particular, we find that very small (in absolute magnitude) coefficients estimates are still found to be statistically different than zero. The asymptotic distribution of the test statistic does not appear to coincide with the small sample distribution. In future research we intend to address the issues of hypothesis testing in small samples more precisely.

Figures 3 and 4 illustrate the empirical distributions of the test statistics computed from two Monte Carlo experiments, C_0 and F_.5. We see that GMM is relatively precise, and can distinguish between hypotheses of unobserved heterogeneity and complementarity effectively relative to OLS.

We stress that, in each of the experiments, GMM imposes substantially greater computational burdens than OLS and the correlation test. It further raises a number of numerical issues in terms of computation of the integrals the relationship between that computation and the numerical optimization of the GMM objective function. We will discuss some of these issues further, as well as additional sensitivity analysis and multi-choice models, in subsequent versions of this paper.

9. Issues for Survey Design and Data Collection

Our results have several implications about the kinds of data which will be useful for testing theories about complementarities. We will now briefly summarize these implications. Our analysis of the adoption approach highlighted many of the difficulties which arise in designing tests when a measure of performance is unavailable. However, when conducting these tests, it will be useful to have as much information as possible about factors which influence the firms' adoption choices.

When implementing tests which make use of a performance measure, either in the single-equation or the system of equations approach, the distinction between $\mathbf{w}$ and $\mathbf{x}$ becomes more relevant. In particular, since instruments will be required, it is necessary to observe variables $\mathbf{w}$ 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 ($\mathbf{w}$) which represent costs to the organization that do not interact directly with productivity. 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 implication which must follow 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. If the unobserved heterogeneity is due to $\boldsymbol{\omega}$ and not $\boldsymbol{\chi}$, Procedure 5 (in the discrete choice case, a simple OLS regression) 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 more by central management's taste for management fads than by whether the firm's particular environment is well-suited for each of the individual practices, and taking into account the interactions between practices. For survey design, focusing interview and survey questions on the factors which enter the adoption process will be critical.

Thus, 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, it is possible to observe information about the costs and benefits to adoption, particularly costs and benefits which do not interact with productivity directly.

10. 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 which 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 produced misleading results when we allow for both complementarities between choice variables, as well as factors which affect the marginal costs and benefits of each individual choice are correlated across firms.

The empirical framework we propose, while subject to shortcomings, can in principle disentangle the different forces potentially at work behind positively correlated choice variables. Further, by estimating a system of equations which includes optimality conditions, our method is able to provide additional precision, making a test for complementarity possible, even in data sets where multicollinearity would make such a test impossible using single equation estimation. Our preliminary experiments show that it is computationally feasible to implement our framework, and that it performs better than other approaches in moderately sized data sets.

While in this paper we have developed a baseline framework, there are a number of interesting theoretical issues which remain to be explored. For example, we wish to study more carefully the issues associated with aggregating organizational design variables, and to explore the use of index functions. And, with multiple choice variables, there are issues to be addressed in about hypothesis testing with multiple inequality restrictions. The dynamics of the diffusion and adoption of organizational design practices also poses interesting challenges. Of course, the most important step still to follow is the implementation of these techniques in real-world data sets.

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Appendix

Proof of Proposition 5: Let $g_1(\chi_1)=\chi_1$, $g_2(\chi_2)=\mathbf{1}_{R_{11}}(\chi_1,\chi_2)$, and $a=\theta_{01}-\theta_{11}$, where R_{11} is the region where $\mathbf{y}^*=(1,1)$, that is, the region where $\chi_1 \geq a$ and $\chi_2 \geq \theta_{10}-\theta_{11}$. Define R_{10} and R_{01} similarly. Applying the definition of association, since g_1 and g_2 are nondecreasing functions, we have

$E\left[g_1(\chi_1)g_2(\chi_2)\middle|\chi_1 \geq a\right] \geq E\left[g_1(\chi_1)\middle|\chi_1 \geq a\right]E\left[g_2(\chi_2)\middle|\chi_1 \geq a\right]$. Simplifying,

$E\left[\chi_1 \cdot \mathbf{1}_{R_{11}}(\chi_1,\chi_2)\middle|\chi_1 \geq a\right] \geq E\left[\chi_1\middle|\chi_1 \geq a\right]\Pr\left((\chi_1,\chi_2)\in R_{11}\middle|\chi_1 \geq a\right)$, or

$E\left[\chi_1\middle|(\chi_1,\chi_2)\in R_{11}\right] \geq E\left[\chi_1\middle|\chi_1 \geq a\right]$, and applying the fact that $E\left[\chi_1\middle|\chi_1 \geq a\right] =$

$E\left[\chi_1\middle|R_{11}\right]\Pr(R_{11})+E\left[\chi_1\middle|R_{10}\right]\Pr(R_{10})$, we get $E\left[\chi_1\middle|\chi_1 \geq a\right] \geq E\left[\chi_1\middle|(\chi_1,\chi_2)\in R_{10}\right]$ as well.

This yields $E\left[\chi_1\middle|(\chi_1,\chi_2)\in R_{11}\right] \geq E\left[\chi_1\middle|(\chi_1,\chi_2)\in R_{10}\right]$. Similar arguments lead to the result that expression (12) is nonnegative, and thus $E[\hat{\kappa}^{OLS}] \geq 0$.

To see part (ii), simply refer to Figures 1 and 2. When the choices are indepedent and (H1) holds, R_{11} includes lower values of χ_1.

Proof of Proposition 6: Omitted from this draft.

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.
$\boldsymbol{\chi}=(\chi_1,..,\chi_J)$		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.
$\boldsymbol{\omega}=(\omega_1,..,\omega_J)$		Unobserved; exogenous.
$\mathbf{z}=(z_{0..0},..,z_{1..1})$	Vector of exogenous variables, where each component j affects the returns to a system $(l_1,..,l_J)$.	Observed; exogenous.
$\boldsymbol{\zeta}=(\zeta_{0..0},..,\zeta_{1..1})$		Unobserved; exogenous.
Parameters		
$\boldsymbol{\theta}$	Parameter of function f which determines supermodularity of f.	To be estimated.
$\boldsymbol{\tau}$	Parameter of adoption equations which determines the interaction between practices in adoption.	To be estimated.
$\boldsymbol{\alpha},\boldsymbol{\eta},\boldsymbol{\beta},\boldsymbol{\delta}$	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	A component of the firm's objective function which incorporates all interactions between choice variables.	Observed with (i.i.d.) error. Functional form assumed.
k^j	The portion of costs or benefits to choice j which does not directly affect f and does not involve any interactions between choices.	No measure observed. Functional form assumed.
H	The joint distribution over the unobservables.	

Table 2

Monte Carlo Experiments: The Two Choice Model

Parameters which are fixed across experiments:

Number of choices: 2

Number of observations: 400

Two *x* and two *w* for each choice, each i.i.d. N(0,1)

Parameters: coefficients on each *x*, *w*, (β and δ), each set equal to .5.

Variance of idiosyncratic productivity error, ε: .001.

Parameters which vary across experiments

Parameters of the Production Function Recall: $\kappa = \theta_{00} + \theta_{11} - \theta_{01} + \theta_{10}$	
$\theta_{00}=\theta_{10}=\theta_{01}=\theta_{11}=1$ (κ=0, No Complementarity)	$\theta_{00}=1, \theta_{10}=\theta_{01}=1.25,\ \theta_{11}=2$ (κ=.5, Complementarity)

	Distributional Assumptions for Unobserved Heterogeneity	
Label	Distribution of $\boldsymbol{\chi}$: Var-Cov($\boldsymbol{\chi}$)	Distribution of $\boldsymbol{\omega}$: Var-Cov($\boldsymbol{\omega}$)
A	Independent $\boldsymbol{\chi}$ $\begin{bmatrix} 2 & 0 \\ 0 & 2 \end{bmatrix}$	No $\boldsymbol{\omega}$ $\begin{bmatrix} 0 & 0 \\ 0 & 0 \end{bmatrix}$
B	No $\boldsymbol{\chi}$ $\begin{bmatrix} 0 & 0 \\ 0 & 0 \end{bmatrix}$	Independent $\boldsymbol{\omega}$ $\begin{bmatrix} 2 & 0 \\ 0 & 2 \end{bmatrix}$
C	Positively correlated $\boldsymbol{\chi}$ $\begin{bmatrix} 2 & 1.5 \\ 1.5 & 2 \end{bmatrix}$	No $\boldsymbol{\omega}$ $\begin{bmatrix} 0 & 0 \\ 0 & 0 \end{bmatrix}$
D	No $\boldsymbol{\chi}$ $\begin{bmatrix} 0 & 0 \\ 0 & 0 \end{bmatrix}$	Positively correlated $\boldsymbol{\omega}$ $\begin{bmatrix} 2 & 1.5 \\ 1.5 & 2 \end{bmatrix}$
E	Positively correlated $\boldsymbol{\chi}$ $\begin{bmatrix} 2 & 1.5 \\ 1.5 & 2 \end{bmatrix}$	Positively correlated $\boldsymbol{\omega}$ $\begin{bmatrix} 2 & 1.5 \\ 1.5 & 2 \end{bmatrix}$
F	Negatively correlated $\boldsymbol{\chi}$ $\begin{bmatrix} 2 & -1.5 \\ -1.5 & 2 \end{bmatrix}$	Negatively correlated $\boldsymbol{\omega}$ $\begin{bmatrix} 2 & -1.5 \\ -1.5 & 2 \end{bmatrix}$

Table 3

Mean and Sample Standard Deviation of Correlation Coefficients and Supermodularity Coefficients

	Experiments (Based on 1000 experiments, except GMM based on 50 experiments)	Frequencies of Combinations {(0,0),(0,1),(1,0),(1,1)}	Uncond-itional Correlation	Conditional Correlation	OLS estimate of κ	Instrumental Variables estimate of κ	GMM estimate of κ
Label	*No Complementarity*				True $\kappa = 0$		
A_0	Indep. χ, no ω	.250, .251, .250, .250	−0.001 (0.0505)	−0.001 (0.0488)	−0.0067 (0.205)	−0.011 (1.24)	−.0167 (.104)
B_0	No χ, indep. ω	.251, .249, .249, .251	.00437 (.0505)	.00482 (.0505)	.0000751 .00646	−.000307 (.0332)	−.001 (.025)
C_0	Pos. Corr. χ, no ω	.333, .167, .167, 0.333	0.332 (0.0483)	0.418 (0.0429)	1.55 (.227)	.774 (1.44)	−.093 (.202)
D_0	No χ, Pos. Corr. ω	.333, .166, .167, .334	.334 (.0479)	.420 (.0426)	−.000194 (.00682)	.000413 (.0326)	.021 (.098)
E_0	Pos. Corr. χ and ω	.353, .148, .148, .350	.408 (.0465)	.463 (.0418)	1.06 (.282)	.714 (186)	−.159 (.284)
F_0	Neg. Corr. χ and ω	.148, .352, .354, .147	−.411 (.0459)	−.466 (.0401	−1.1 (.192)	−.756 (1.4)	−.0011 (.048)
	Complementarity				True $\kappa = .5$		
A_.5	Indep. χ, no ω	.190, .185, .185, .441	0.211 (0.0492)	0.204 (0.0494)	0.117 (0.211)	0.0599 (1.53)	.481 (.106)
B_.5	No χ, indep. ω	.189, .186, .186, .439	.207 (.0518)	.202 (.0512)	.5 (.00681)	.5 (.0344)	.565 (0.051)
C_.5	Pos. Corr. χ, no ω	.271, .109, .110, .511	.536 (0.0438)	.586 (0.0383)	1.89 (.240)	.839 (1.96)	.538 (.225)
D_.5	No χ, Pos. Corr. ω	.270, .110, .110, .511	.533 (.0439)	.581 (.0393)	.500 (.00807)	.499 (.0336)	.527 (.050)
E_.5	Pos. Corr. χ and ω	.304, .104, .104, .488	.569 (.0417)	.602 (.0378)	1.47 (.307)	1.06 (2.39)	.490 (.2280
F_.5	Neg. Corr. χ and ω	.103, .294, .294, .309	−.229 (.0488)	−.293 (.0453)	−.905 (.181)	−.543 (1.40)	.501 (.098)

Notes: Sample Standard Deviations in parentheses.

Table 4

Hypothesis Tests at 95% Confidence Level: Evidence for Supermodularity

Frequency With Which We (1) *Can not* Reject Positive Parameter Value (2) *Can* Reject Negative Parameter Value

	Experiments (Based on 1000 experiments, except GMM based on 50 experiments)	Hypothesis	Unconditional Correlation	Conditional Correlation	OLS	Instrumental Variables	GMM
Label	*No Complementarity*				True $\kappa = 0$		
A_0	Indep. χ, no ω	(1) (2)	.942 .055	.958 .047	.953 .049	.639 .367	.8 .06
B_0	No χ, indep. ω	(1) (2)	.959 .062	.955 .062	.953 .062	.629 .385	.48 .4
C_0	Pos. Corr. χ, no ω	(1) (2)	1 1	1 1	1 1	.842 .571	.6 .06
D_0	No χ, Pos. Corr. ω	(1) (2)	1 1	1 1	.936 .038	.662 .352	.48 .5
E_0	Pos. Corr. χ and ω	(1) (2)	1 1	1 1	1 .971	.789 .529	.7 .22
F_0	Neg. Corr. χ and ω	(1) (2)	0 0	0 0	0 0	.405 .172	.54 .4
	Complementarity				True $\kappa = .5$		
A_.5	Indep. χ, no ω	(1) (2)	1 .995	1 .995	.994 .06	.652 .374	1 1
B_.5	No χ, indep. ω	(1) (2)	1 .991	1 .988	1 1	1 1	1 .98
C_.5	Pos. Corr. χ, no ω	(1) (2)	1 1	1 1	1 1	.801 .544	.98 .92
D_.5	No χ, Pos. Corr. ω	(1) (2)	1 1	1 1	1 1	1 1	1 .92
E_.5	Pos. Corr. χ and ω	(1) (2)	1 1	1 1	1 .991	.792 .555	.98 .92
F_.5	Neg. Corr. χ and ω	(1) (2)	.002 0	0 0	.002 0	.475 .225	1 1

Notes: Hypothesis tests were performed using the computed standard error for each experiment (based on asymptotic distributions).

FIGURE 1

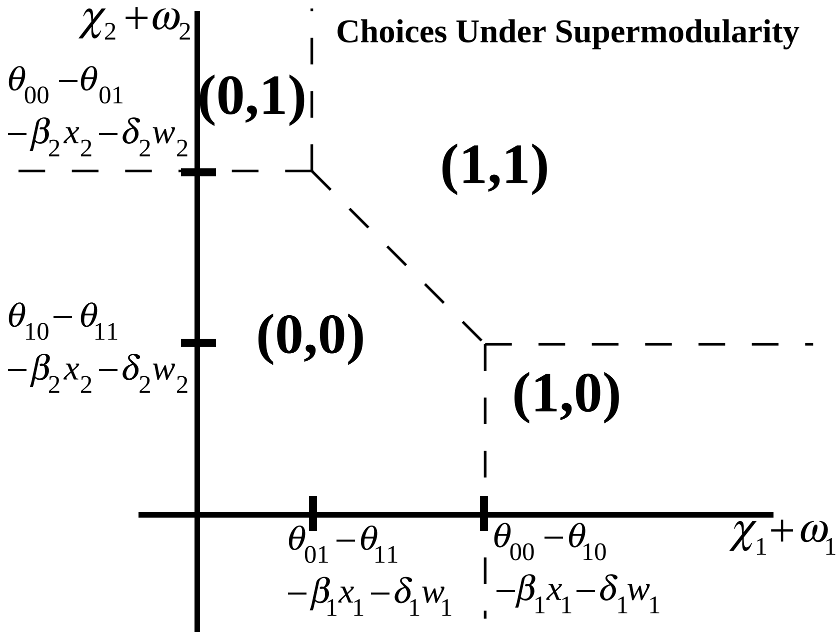

FIGURE 2