Endogenous Matching and the Empirical Determinants of Contract Form

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Empirical work on contracts typically regresses contract choice on observed principal and agent characteristics. If (i) some of these characteristics are unobserved or partially observed *and* (ii) there are incentives whereby particular types of agents end up contracting with particular types of principals, estimated coefficients on the observed characteristics may be misleading. We address this *endogenous matching* problem using a data set on agricultural contracts between landlords and tenants in early Renaissance Tuscany. Controlling for endogenous

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matching has an impact on parameters of interest, and tenants' risk aversion appears to have influenced contract choice.

I. Introduction

Theoretical work on contract choice often starts with a principal with particular characteristics, an agent with particular characteristics, and characteristics of the task to be contracted on. One then proceeds to solve for the optimal contract form (e.g., the share of output to be given to the agent) as a function of these characteristics. Among other things, the implications of factors such as risk aversion, monitoring ability, moral hazard, and multiple tasks on optimal contracts and the second-best outcome have been examined (e.g., Mirrlees 1974; Stiglitz 1974; Grossman and Hart 1983; Holmstrom and Milgrom 1987, 1991, 1994).[1]

Empirical work on contract choice, often in an agrarian or franchising context, usually starts with such equations in mind and proceeds by regressing contract choice on observed principal, agent, and task characteristics. The point of such works is that knowing if, how, or how much certain characteristics affect contract choice can tell us something about which of these factors are important. This in turn can supply valuable information about the functioning of a microeconomy. For example, if risk sharing appears to be an important determinant of contract choice, one might make inferences about the state of insurance markets in an economy. Similarly, measuring the impact of potential capital constraints can shed light on the functioning of capital markets. Such knowledge can be beneficial for policy, particularly in the context of economic development.

Much of the empirical literature has focused on testing two possible determinants of contract choice. On one hand, risk-sharing models stress that, in the presence of a risk-averse agent who can shirk in performing the tasks assigned by the principal, share or royalty contracts offer insurance and, at the same time, provide incentives for the agent to be diligent. On the other hand, transaction cost models tend to ignore risk preferences and focus on enforcement costs and transaction-specific assets. Interestingly, there seems to be little empirical support of risk sharing as an important determinant of contract choice in either franchising or agriculture. Allen and Lueck (1995, p. 447) state that "accumulated evidence confronting risk sharing and transaction costs—covering such topics as franchising, gold mining, sharecropping, and

[1] Work on contract choice in an agrarian context includes Cheung (1969), Rao (1971), Newbery (1977), Hallagan (1978), Newbery and Stiglitz (1979), Braverman and Stiglitz (1982), Allen (1985), Eswaran and Kotwal (1985), Hayami and Otsuka (1993), Bell, Raha, and Srinivasan (1995), Luporini and Parigi (1996), and Lanjouw (1999).

timber—actually favors the transaction cost framework." Other empirical work has found support for moral hazard, capital constraints, and multitasking issues as important determinants of contractual arrangements.[2]

While the existing empirical literature on the determinants of contract choice is vast and interesting, there is a potential problem with much of the work mentioned above that deserves attention. A key dichotomy between the theoretical and empirical literatures is that in the theory literature, there is no measurement problem regarding principal, agent, and task characteristics. In contrast, in the empirical literature there clearly is. Many potentially relevant characteristics may be unobserved, partially observed, or observed with error by an econometrician. This observability problem is often acknowledged or mentioned in passing by the empirical literature, but the implications do not seem to have been fully discussed.

In contrast, we argue that if principals and the agents they contract with are "matched" with each other according to economic variables (and we argue that there are incentives for such *endogenous matching*), this observability problem is important and casts doubts on estimated coefficients in regressions of contract choice on observed characteristics.[3] More important, we suggest techniques for ameliorating these problems and show that these techniques influence estimates in a data set on agrarian contracts. Our ultimate goal is to test various theories of contract choice and to reassess the role of risk sharing.

To exemplify incentives for endogenous matching and its implications, consider Allen and Lueck (1992), which examines whether the inherent riskiness of a crop affects the type of contract used for that crop. Their hypothesis is that if risk effects are an important determinant of contract choice, then very risky crops will be more likely to be as-

[2] On agrarian contracts, see Shaban (1987), Allen and Lueck (1992, 1999), and Laffont and Matoussi (1995). On timber harvesting, see Leffler and Rucker (1991). On business franchising, see Mulherin (1986), Brickley and Dark (1987), Martin (1988), Lafontaine (1992), Lafontaine and Bhattacharyya (1995), Slade (1996), and Lafontaine and Slade (1997).

[3] There is a large theoretical literature on matching, but a very limited literature on the empirical implications of matching. Foster (1998) examines the consequences of matching in marriage markets. In their study of labor contracts, MacLeod and Parent (1999) use National Longitudinal Survey of Youth and Panel Study of Income Dynamics panel data to control for unobserved worker-specific attributes (which could cause matching problems) as fixed effects. They also address the problem of job-specific match quality using a selection equation. Dubois (2002) deals with a related endogeneity problem—the endogenous choice of what type of crop to grow on the land. Prendergast (2002) offers alternative explanations why the empirical literature has found little support for risk sharing.

sociated with share contracts than with fixed-rent contracts.[4] Empirically, they do not find this correlation and conclude that risk sharing is not an important determinant of contract choice.

Consider an alternative explanation of this empirical result in which risk sharing *is in fact* important (e.g., the Holmstrom-Milgrom [1987] model). Suppose that half the potential tenants in the economy are risk-neutral and the other half are risk-averse. Similarly, half the crops are very risky and half are somewhat less risky. For social welfare it would be best for the risk-neutral tenants to work on the very risky crops. If this "endogenous matching" equilibrium were exactly the outcome, the risky crops would be associated with fixed-rent contracts (because the tenants are risk-neutral, the optimal contract is fixed-rent), whereas the less risky crops, cultivated by risk-averse tenants, would be associated with share contracts. Note that this extreme example gives an empirical implication *exactly the reverse* of that argued by Allen and Lueck: fixed-rent contracts are found on the *risky* crops. The problem here is that while the "riskiness of crop" may be exogenous to the landlord who owns the land, it is endogenous, through principal-agent matching, to the type of tenant attracted to it.[5]

If a tenant's risk aversion were perfectly observed by the econometrician, the endogeneity problem would be solved by regressing contract choice on crop riskiness *and* risk aversion. However, economists rarely profess to exactly observe a tenant's risk aversion. Rather, they use a proxy or proxies for risk aversion such as wealth or property. We show that using proxies for risk aversion in such regressions *does not* solve the endogenous matching problem. With endogenous matching, the "crop variability" variable will still be correlated with the error term through the proxy error, that is, the unobserved component of risk aversion.

The example above considered matching based on risk and risk aversion. There are many other stories that suggest matching between heterogeneous principals and agents. For example, principals with more ability to monitor or more ability to measure output (who might relatively prefer low-share or high-royalty contracts) might end up matching with agents with more risk aversion, more credit constraints, or a higher cost of effort (who might also relatively prefer low-share or wage contracts). We argue that *any* such matching can be a serious problem when one is relying on proxies for relevant variables, which is often the case

[4] Their maintained assumption is that the type of crop grown on a particular plot of land is fixed (e.g., because of climate or soil type) and is not a choice variable of the landlord.

[5] One can make similar arguments in the franchising context in which contract choice (e.g., royalty rate) is regressed against measures of riskiness. Again, if particular types of entrepreneurs (e.g., risk-neutral or risk-loving ones) are attracted to risky franchises, the results of such a regression are questionable.

in the contract choice literature. Put succinctly, the matching generates correlation between observable characteristics of one of the parties and proxy errors of the other party, potentially biasing many or all coefficients of interest.

This article addresses this problem and suggests potential solutions. These solutions involve consideration of a "matching equation" that describes how principals and agents are matched with one another. What we require is instruments that affect the matching equation but do not affect the contractual choice or proxy equations. We suggest and apply the use of geographical-based instruments that can affect the matching equation through, for example, differences in the exogenous distribution of land type across regions. The examination of this matching equation can also provide economically interesting information in its own right.

We apply our techniques to historical data on contracts between landlords and tenants in early Renaissance Tuscany. This is a data set similar to that used by Pudney, Galassi, and Mealli (1998) with the significant addition of variables measuring tenants' characteristics. These tenant variables are important because they enable us to both test for and econometrically control for endogenous matching. Comparing our results to those of a prior article (Ackerberg and Botticini 2000) in which we ignored matching, here (i) we find strong evidence of matching between principals and agents, and (ii) we find that econometrically controlling for the matching can have a fairly large impact on the magnitude and significance of contract choice coefficients. For example, without controlling for matching, we find little, if any, effect of a tenant's wealth on contract choice. *After* controlling for the biases due to endogenous matching, we find a stronger and significant effect of a tenant's wealth. Given that wealth proxies for risk aversion, this finding suggests that risk sharing is an important determinant of contract choice.[6] In addition to correcting for endogenous matching in our contract choice equations, we also make interesting economic inferences from our estimates of the direction of matching. This direction is consistent with multitasking issues (as in Holmstrom and Milgrom [1991]) being an important factor in contract choice.

These empirical results are important for at least a couple of reasons. First, given the lack of evidence of risk sharing in the prior empirical

[6] Limited liability is another conceivable interpretation of the correlation between low wealth and share contracts (see, e.g., Basu 1992; Sengupta 1997). These works rely on tenants' having to choose, in addition to effort, the riskiness of the production process. In essence, share contracts are used to prevent poor tenants from using production techniques that are too risky (e.g., high-yield and weather-sensitive seeds vs. low-yield and weather-insensitive seeds). In our data set, we do not believe that there was significant latitude for tenants to choose the level of riskiness of the production process.

literature, it suggests that prior work may suffer from biases because matching is ignored. Second, although early Renaissance Tuscany was a vibrant economy endowed with sophisticated economic institutions, its agricultural insurance and rural credit markets were either missing or imperfect. This is also the case in many contemporary developing countries. As a result, we think that our findings can also provide insights into issues currently debated in the context of developing countries, foremost the role of contractual arrangements in substituting for missing or imperfect insurance or capital markets.

This article is organized as follows. Section II illustrates the endogenous matching problem, Section III describes the sample, and Section IV presents the empirical results. Section V presents conclusions.

II. Identification

Consider a standard moral hazard model in which a principal and an agent are contracting over a task to be done. There is no hidden information; that is, both parties know the characteristics of their contracting partner. Suppose that theory gives us a contract choice equation describing the second-best contract y as a function of the characteristics of the principal/task (p) and agent (a):

$$y = \beta_0 + \beta_1 p + \beta_2 a + \epsilon. \tag{1}$$

Examples of p might be the inherent riskiness of the principal's crop or franchise, monitoring ability, risk aversion, or transactions costs. The variable a might measure the agent's risk aversion, productivity, or opportunity cost of effort. The variable y might indicate the share of output or revenue paid to the agent (e.g., royalty rate) or the discrete type of contract used (e.g., wage, share, or fixed-rent). While this equation might be nonlinear, we focus initially on the linear case since this keeps identification issues as transparent as possible. The variable ϵ is measurement error or optimization error in y, uncorrelated with p and a. This loses little generality since p and a contain all the characteristics relevant to contract choice, and we shall allow the possibility that elements of p or a are partially observed or unobserved by the econometrician.

In empirical work, characteristics in p or a are seldom perfectly observed. For example, an econometrician does not perfectly observe an agent's risk aversion. However, there are often proxies available for these variables (e.g., observed wealth may proxy for unobserved risk aversion). Past empirical work has proceeded by substituting these proxies into equation (1) for the true variables and using standard estimation techniques (e.g., ordinary least squares [OLS]). We argue that such a procedure is problematic when there is "matching" of heterogeneous principals and agents in a microeconomy, that is, when there are incentives for certain types of agents to match (contract) with certain types of principals.

In an economy with heterogeneous principals and agents, there are likely to be strong incentives for such matching. For example, suppose that agents differ in their level of risk aversion and principals differ in the level of riskiness of the task they are contracting out. One might expect the less risk-averse agents to "match" with the riskier principals/ tasks since these agents can more easily bear the risk. It is fairly easy to construct examples in which this is the only "matching" equilibrium of a simple economy.[7] There are other possible matching stories; for example, agents with high productivity levels might match with principals with low levels of monitoring ability since they both relatively prefer high-incentive contracts. Here we do not investigate exactly how this matching is occurring (see, e.g., Shimer and Smith [2000] or Legros and Newman [in press] for recent theoretical models of matching with contracting). We do address the implications of such matching for empirical work when relevant principal or agent characteristics are only partially observed.

Consider the simplest possible case in which p and a are scalars and p is perfectly observed by the econometrician. The agent characteristic a is not fully observed, but one observes a proxy w for it. Suppose that the proxy relationship is given by $a = \theta w + \eta$, with η mean independent of w.[8] Substituting this into the contract choice equation gives us

$$y = \beta_0 + \beta_1 p + \beta_2 \theta w + \beta_2 \eta + \epsilon. \qquad (2)$$

The goal of the empirical exercise is to estimate the coefficient β_1 and the product $\beta_2\theta$.

Again considering the simplest possible case, suppose that principals and agents match according to the following linear matching equation:

$$p = \gamma_0 + \gamma_1 a + \nu = \gamma_0 + \gamma_1 \theta w + \gamma_1 \eta + \nu, \qquad (3)$$

where ν is "matching error" (caused, e.g., by frictions in a search process). Note that without ν, estimating β_1 and $\beta_2\theta$ would be hopeless since p and w would be collinear.

Given this matching, it is clear that simple OLS estimation of equation (2) is problematic. Because of matching, the principal's characteristic p will typically be correlated with the *unobserved* component of the agent's characteristic, η. As a result, p will be correlated with the econ-

[7] See the working paper version of this article (Ackerberg and Botticini 1999) for a numerical example in the context of the Holmstrom and Milgrom (1987) model.

[8] This equation represents the conditional expectation of a given w. Mean independence of η follows directly. The only assumption here is that the conditional expectation is linear. Again, this assumption is made in order to make identification issues more transparent.

ometric error term $(\beta_2\eta + \epsilon)$ in (2). This both directly biases the estimate of β_1 and indirectly biases the estimate of $\beta_2\theta$ (since the matching equation implies that p and w are correlated).

We suggest potential solutions to this endogenous matching problem. The first relies on instrumental variables. What is needed is an instrument z that influences matching but does not enter the contract choice or proxy equations. In other words, we want an instrument that exogenously shifts the type of principal an agent gets matched with. One might use characteristics of a tenant that appear to affect matching but can reasonably be excluded from entering the contract choice equation.

More interesting and general is the case in which observations come from different geographical or temporal markets. If the population distribution of observable principal or agent characteristics (e.g., the distribution of p or w) differs across these markets, the matching equation should differ across markets. Consider two geographically distinct markets, each with the same distribution of a but the second market with a higher mean of the p distribution. Then for a given a, an agent in the second market will tend to get matched with a principal with a higher p. While different means of these characteristic distributions generate differences in the intercept term of matching equations, different variances of these distributions will tend to generate different slope coefficients. If, for example, all jobs in the second market have the same level of riskiness, the slope coefficient in the second market will be zero.

If the matching equations vary across markets, we have two sets of potential instruments for the endogenous p: (i) market dummies and (ii) market dummies interacted with w. The question is whether these instruments can reasonably be excluded from the other equations in the system. First, consider the structural contract choice equation, which specifies optimal contractual form as a function of *all* the *fundamental* economic characteristics of the principal and agent. As there is neither a compelling argument to think nor a model to suggest that market of residence is one of these fundamental characteristics, these market variables can be excluded virtually by definition.

This is a bit misleading since some of the fundamental characteristics (p or a) might be "endogenous" to the market the actors are in. For instance, in a matching equilibrium, reservation utilities of principals and agents might be endogenously determined and vary across markets. These different reservation utilities could generate different transfer payments to the "same" agents across different markets. A problem arises if these differences in transfer payments indirectly affect the optimal contract choice y through income effects; that is, transfer payments affect tenants' "net" wealth, in turn affecting their risk aversion, in turn affecting the optimal contract choice. Many models do not have this income effect (e.g., in the Holmstrom and Milgrom moral hazard model, the agent's risk aversion is not affected by the transfer payment). This should also be the case in other constant absolute risk aversion models. Even in more general models, we feel that these income effects should be small and unlikely to cause significant problems.

Second, we need to assume that these market-based instruments do not enter the proxy equation. For this to be true, the markets must be as isolated as possible. Migration based on economic considerations is a problem if agents move to markets in which payoffs to their characteristics are high. While these assumptions may not always hold, they are on the same level as those in a standard proxy problem, that is, assumptions that the proxy errors are uncorrelated with other assumed *exogenous* variables in the model. Essentially, while we are allowing an agent to match with principals within a market endogenously, we are assuming that the market the agent is in is exogenous.[9]

A second econometric approach to addressing the endogenous matching problem is to examine covariances. The basic idea is that the observed correlation between p and w can inform us about the direction of the unobserved correlation between p and η. This can enable one to *sign* the biases in the contract choice equation. The primary caveat of this approach is the assumption that other unobservables in the model are uncorrelated with each other and the exogenous variables in the model; that is, η needs to be the only source of endogeneity. This covariance argument brings up an important point regarding the "first-stage" regression in the instrumental variables solution. Examination of this regression is of interest to determine whether one has significant instruments. But suppose that one finds no significant correlation between observed agent and principal characteristics. While this may rule out some potential instruments, it may also suggest that there may be no matching and thus no endogeneity problem. Clearly, a first step with this type of data is to examine correlations between principal and agent characteristics.

A third potential approach is to use "panel" data. If the same principal contracts with multiple agents (or the reverse) and unobserved characteristics are constant across these contracts, panel techniques can eliminate the endogeneity problem. In the empirical work, we use this strategy to accommodate potential observability problems with p. This

[9] These assumptions are also analogous to the "market instrument" assumption often used to solve the hedonic identification problem (see Epple 1987; Kahn and Lang 1988; Brandt and Hosios 1996). Note that if one has a large number of markets, it is safer to use characteristics (e.g., first and second moments) of the distributions of p as instruments instead of the market dummies. As with the market dummies, one can use both moments and moments interacted with w. This is safer because it relies on less restrictive assumptions. For example, the mean of η *can* vary across markets as long as this mean is uncorrelated with the moments of the p distribution.

addresses the potential "two-sided" matching problem in our data, where unobserved principal characteristics may be correlated with observed agent characteristics and unobserved agent characteristics may be correlated with observed principal characteristics.

III. Data

A. *The Sample*

We examine matching and its implications on contract choice equations in an interesting historical data set on agrarian contracts gathered from the Florentine *catasto* of 1427, a comprehensive census and property survey of Tuscany. The sample consists of landlords living in the Tuscan towns of Florence, Pescia, and San Gimignano and their tenants, who lived in many villages in the countryside. Landlords often contracted with more than one tenant: the data consist of 902 land plots/contracts owned by 128 landlords.[10] For a thorough description of the data, see Ackerberg and Botticini (2000). Our primary variables of interest are (i) the crop type grown on the land to be cultivated, (ii) the wealth of the tenant, and (iii) the type of contract used. The countrysides of these towns were isolated from each other economically, and migration rates were fairly low: 3 percent of peasant households living in the countryside of Florence declared to have emigrated from other Tuscan towns or from other places. The corresponding percentages for San Gimignano and Pescia were 6 percent and 13 percent, respectively. In light of the discussion in Section II, this separation is important since it justifies the choice of "towns" as potential instruments in the estimation.

The sample contains both share and fixed-rent contracts. Although the data do not often give information on exact shares, most were 50/50.[11] The crop type on a particular land plot was classified as "vines," "cereals," or "mixed," depending on whether vines (or other perennial

[10] To ease data collection, we oversampled wealthier landlords (who were more likely to lease their land plots out). We set our sampling criterion such that approximately 85 percent of plots under contract in each town were potentially in the sample. This large percentage should minimize selection problems, although our landlord fixed-effects specifications should also control for selection. These fixed-effects models should also somewhat control for a related selection problem arising from the fact that we did not include landlords who cultivated their own land in the sample.

[11] As is often the case with data on contracts, the preponderance of a few particular contracts does not completely coincide with the theoretical literature that predicts a wide range of shares (or nonlinear contracts). One argument is to appeal to institutional restrictions that limit the "optimal" choice to the optimum from a small set of commonly used contracts.

crops), cereals (or other annual crops), or both types of crops were grown.[12]

The distinction between cereals and vines is relevant for both the risk-sharing hypothesis and the multitasking model of Holmstrom and Milgrom (1991). First, while cereals were subject to weather variability, vines were much more sensitive (Galassi 2000). As such, all else equal, one might expect vines more likely to be associated with share contracts than with fixed-rent contracts. Second, vines had interesting multitasking features. A tenant's effort could be devoted to maximizing current production or to maintaining and improving the assets for future production. Peasant tenants could boost current production by pruning in a certain way or putting manure near the roots. However, this could damage the vines and result in less output in subsequent years (Fenoaltea 1984, pp. 647, 668; Hoffman 1984; Pudney et al. 1998). Thus owners of land plots with vines might be hesitant to sign contracts with strong incentives for current production (i.e., fixed-rent contracts).[13]

Both the multitasking and risk-sharing effects of vines suggest similar outcomes: fixed-rent contracts should be less likely with vines. As including the crop type alone is not a conclusive test of the risk-sharing hypothesis, we also consider the tenant's wealth as a potential proxy for his risk aversion (as, e.g., Laffont and Matoussi [1995]). If risk aversion is an important determinant of contract form and wealth is a valid proxy for risk aversion, wealthier tenants should be more likely to engage in fixed-rent contracts.

B. Summary Statistics

Table 1 presents summary statistics of the variables. The four cross tabulations in table 2 (for the aggregate and for the three towns individually) illustrate three interesting correlations that hold both in the aggregate and at the town level. First, land plots with vines (and other perennial crops) appear to be associated with share contracts, whereas land plots with cereals (and other annual crops) were most often leased out under fixed-rent contracts. Out of 902 observations, 482 land plots with vines or mixed crops were sharecropped, and 28 were leased out

[12] The nature of the land on a farm partially determined the crops to be planted (hills were better for vines and valleys for cereals). In addition, vines are long-lived plants, so it would not be easy to switch crops in the short term. Thus it is reasonable to think that the type of crop grown was exogenous to the land and landowner. However, since we end up modeling crop type as endogenous anyway (because of endogenous matching), this assumption is not completely necessary.

[13] Though we do not have any data on contract length for Tuscan agriculture, there is evidence that in nineteenth-century Sicily, perennial crops, such as vines, were more likely to be leased out with contracts longer than one year (Bandiera 1998). This also seems supportive of multitasking.

TABLE 1
Summary Statistics

Variable	Description	Mean	Standard Deviation
Rent	=1 if fixed-rent contract	.374	.484
Share	=1 if share contract	.626	.484
Vines	=1 if vines (and perennial crops) were grown	.102	.302
Cereals	=1 if cereals (and annual crops) were grown	.435	.496
Mixed	=1 if both types of crops were grown	.463	.499
Crop*	=1 if vines, .5 if mixed, and 0 if cereals	.334	.327
Tenant's wealth	Actual tenant's wealth in florins	82.51	132.2
Wealth*	Normalized ln(tenant's wealth)	0	1

Source.—State Archives of Florence, *catasto* 64, 67, 68, 72, 75, 76, 77, 78, 79, 80, 81, 233, 234, 235, 236, 258, 266, and 269.

Note.—The minimum and maximum values for all variables except tenant's wealth and wealth are zero and one, respectively. The minimum value of tenant's wealth is zero, and the maximum value is 1,552. The minimum value of wealth is −1.824, and the maximum value is 2.276. The number of observations is 902.

* These are the explanatory variables used in the regressions.

to fixed-rent tenants. As for land plots with cereals, 83 were sharecropped and 309 were leased out to fixed-rent tenants. Second, as one looks across contracts for a given crop type, the mean tenant's wealth is higher under fixed-rent contracts than under share contracts. Third, table 2 indicates correlation between tenants' wealth and the type of crop they were cultivating. Poorer tenants primarily cultivated land plots with vines only or farms with vines and cereals. Wealthier tenants mainly cultivated plots with cereals only. This last correlation is suggestive of matching between landlords and tenants: tenants with certain characteristics appear to be matched with specific crops.

The distribution of land types was very different across towns. In Pescia, 75 percent of land plots had cereals only, very few had both cereals and vines, and the rest (20 percent) were plots with vines only. In San Gimignano, 85 percent of land plots had both cereals and vines, 10 percent had cereals only, and 5 percent had vines only. Florence was somewhat in between, with 55 percent of the plots containing mixed crops, 38 percent cereals only, and 8 percent vines only. These differences in land distributions give exogenous variation in crop type that can help control for endogenous matching.

IV. Empirical Results

We present two sets of empirical results. The first are linear models similar to those discussed in Section II. While there is a considerable

TABLE 2

Distribution of Crop Type, Contract, and Tenant's Wealth

Crop Type	Contract: Share	Contract: Fixed-Rent	Contract: Total
	All Locations		
Vines	60.72	110.66	67.23
	(80)	(12)	(92)
Mixed	38.29	82.37	39.98
	(402)	(16)	(418)
Cereals	39.79	156.08	131.45
	(83)	(309)	392)
Total	41.69	150.96	82.51
	(565)	(337)	(902)
	Florence		
Vines	21.46	93.85	34.46
	(32)	(7)	(39)
Mixed	30.27	73.27	31.91
	(276)	(11)	(287)
Cereals	29.89	118.10	88.10
	(67)	(130)	(197)
Total	29.45	113.62	53.27
	(375)	(148)	(523)
	Pescia		
Vines	88.04	134.20	92.95
	(42)	(5)	(47)
Mixed	23.66	344.00	103.75
	(3)	(1)	(4)
Cereals	110.40	184.86	180.67
	(10)	(168)	(178)
Total	88.60	184.32	161.33
	(55)	(174)	(229)
	San Gimignano		
Vines	78.83	…	78.83
	(6)	(0)	(6)
Mixed	56.65	42.00	56.19
	(123)	(4)	(127)
Cereals	32.66	165.27	118.47
	(6)	(11)	(17)
Total	56.57	132.40	64.16
	(135)	(15)	(150)

Source.—See table 1.

Note.—The first element in each cell is the mean wealth of the tenants in that cell, and the second element (in parentheses) is the number of observations. "Vines" indicates that vines were planted on a land plot, "cereals" indicates that cereals were grown, and "mixed" indicates that both types of crops were grown.

amount of discreteness in our data (type of land and type of contract), the linear models make identification issues transparent. We then turn to nonlinear models, which are more appropriate given the data. Since sources of identification are less clear in these nonlinear models, we devote a subsection to addressing this issue.

A. Linear Models

Naive Contract Choice Equations

Our first econometric take on the data is to estimate simple contract choice equations, ignoring the possibility of matching and subsequent endogeneity problems. Column 1 of table 3 presents results of a linear probability regression of contract choice (0 = share, 1 = fixed rent) on town dummies, tenant's wealth, and crop (0 if cereals, .5 if mixed, and 1 if vines). Our results are robust to alternative definitions of the crop variable.[14] The results confirm the casual evidence of table 2: moving from cereals to mixed crops to vines appears to decrease the probability of fixed-rent contracts, whereas increases in tenants' wealth raise the likelihood of fixed-rent contracts. The negative coefficient on crop might either (i) suggest that vines were riskier and thus more likely to be leased out under share contracts or (ii) support a multitasking argument that landlords were hesitant to use incentive-laden contracts on perennial crops. If wealth is a good proxy for risk aversion, the positive coefficient would seem to lend some support to the risk-sharing hypothesis. Foreshadowing our second set of estimates, column 3 repeats the analysis with a more appealing probit model of contract choice: again crop is very significant.

Columns 2 and 4 run the same models using only the data for Pescia and San Gimignano. There are a couple of reasons for this estimation strategy. Florence was considerably different from the other two towns. Not only was it much bigger in both size and population, but it was also the commercial center of Tuscany. Moreover, as shown by the estimates, there do appear to be differences, particularly in the coefficient on wealth. This coefficient is considerably smaller than the one in the regression with the full sample, *not* statistically significant in the probit model, and marginally significant in the linear probability model.[15] Thus, while the effect of crop on contract choice seems very strong

[14] For mixed crops, we also tried setting the crop variable equal to zero, .25, .75, or one (rather than .5). Results were very similar. The reason we make a priori restrictions on this variable and do not estimate coefficients on vines and mixed crops separately is that it adds an extra endogenous variable to the system later.

[15] The differences between the full-sample results and the Pescia–San Gimignano results become important later when we consider the potential instrumental variable identifying restriction that the wealth coefficient is equal across towns.

TABLE 3

LINEAR AND PROBIT RESULTS

DEPENDENT VARIABLE: CONTRACT CHOICE

	OLS		PROBIT		FIXED EFFECT	
PARAMETER	All Locations (1)	Pescia and San Gimignano (2)	All Locations (3)	Pescia and San Gimignano (4)	All Locations (5)	Pescia and San Gimignano (6)
β_0^P (constant)	.8662	.9252	1.2994	1.5220	…	…
	(.0226)	(.0195)	(.1342)	(.1577)		
	[.0000]	[.0000]	[.0000]	[.0000]		
β_0^S (constant)	.4849	.5011	−.1847	−.0770	…	…
	(.0305)	(.0338)	(.1845)	(.2066)		
	[.0000]	[.0000]	[.1583]	[.3545]		
β_0^F (constant)	.5811	…	.2877	…	…	…
	(.0230)		(.0897)			
	[.0000]		[.0006]			
β_1 (crop)	−.7909	−.8353	−2.9047	−2.9956	−.7363	−.8546
	(.0453)	(.0499)	(.1429)	(.2622)	(.0432)	(.0422)
	[.0000]	[.0000]	[.0000]	[.0000]	[.0000]	[.0000]
β_2 (wealth)	.0897	.0251	.4572	.1322	.0765	.0242
	(.0123)	(.0142)	(.0699)	(.1280)	(.0128)	(.0145)
	[.0000]	[.0403]	[.0000]	[.1507]	[.0000]	[.0447]
R^2	.5651	.7395	…	…	…	…

SOURCE.—See table 1.

NOTE.—The dependent variable is a dummy variable equal to zero if the contract chosen is a share contract and equal to one if the contract is a fixed-rent contract. Superscripts indicate town-specific coefficients (P=Pescia, S=San Gimignano, F=Florence). Bootstrapped standard errors are in parentheses, and bootstrapped p-values are in brackets. Col. 1 presents results of a linear probability regression of contract choice on town dummies, tenant's wealth, and crop (see table 1 for the definitions of wealth and crop). Col. 3 is a probit version of the regression in col. 1. Cols. 2 and 4 run the same models using only the data for Pescia and San Gimignano. Cols. 5 and 6 present fixed-effects specifications, which allow for unobserved variables that are constant for a given landlord (e.g., differences in monitoring or transactions costs) and might be correlated with crop or wealth. The number of observations is 902 in cols. 1, 3, and 5 and 379 in cols. 2, 4, and 6.

regardless of specification, support for the risk-sharing hypothesis through the wealth variable is mixed, particularly in Pescia and San Gimignano.

Columns 5 and 6 obtain similar results with linear fixed-effect models. These specifications allow for unobserved variables that are constant for a given landlord (e.g., differences in monitoring or transactions costs) and might be correlated with crop or wealth.

Matching Equations

We now consider the possibility of endogenous principal-agent matching. In our case, particular concern arises from the realization that tenant's wealth is probably not a perfect proxy for tenant's risk aversion. If tenants match with landlords on the basis of an unobserved component of risk aversion, the coefficients in table 3 on both crop and wealth will be biased. For the moment, assume that landlords have only one relevant characteristic—crop type grown on a land plot—and this crop type is perfectly observed by us as econometricians.

As suggested in Section II, a first step toward assessing potential matching problems is to examine correlations between observable principal and agent characteristics. Column 1 of table 4 addresses this question by regressing crop on town dummies and tenant's wealth. The results confirm the correlations in table 2 in that they show a very strong, significant, negative relation between the two variables; that is, it is the less wealthy tenants that appear to end up with vines.

At the very least, this regression suggests that there *is* matching between principals and agents. It does not tell us why there is such matching, although with more heroic assumptions one can draw more inferences from this negative relationship. On the one hand, if risk effects were very important and vines were considerably riskier than cereals, one might expect the *reverse* relationship, that is, less risk-averse tenants ending up on riskier crops. On the other hand, a multitasking story (with little or no difference in the riskiness of crops) might suggest that more risk-averse tenants end up with vines. The intuition is that, all else equal, both landlords owning vines and very risk-averse tenants relatively prefer share contracts. These two arguments suggest that under the hypothesis that matching can be caused *only* by risk issues or multitasking, the negative relation in the data might be more supportive of multitasking. Of course, this argument needs to assume away any other potential reasons for matching (e.g., less wealthy tenants like wine) or to assume that there are other economic reasons why less wealthy tenants would prefer to cultivate vines.

A second inference we can make from the estimated direction of matching is the direction of the biases in the contract choice equation.

TABLE 4
Matching Equation and Instrumental Variable Results
Dependent Variable: Crop Choice (Cols. 1, 2, 7) and Contract Choice (Cols. 3–6)

Parameter	Matching Equation: All Locations (1)	Linear First Stage: Pescia and San Gimignano (2)	Instrumental Variables: All Locations (3)	Instrumental Variables: Pescia and San Gimignano (4)	Fixed-Effects Instrumental Variables: All Locations (5)	Fixed-Effects Instrumental Variables: Pescia and San Gimignano (6)	Fixed-Effects First Stage: All Locations (7)
β_0^P (constant)	…	…	.8636	.8924	…	…	…
			(.1425)	(.0893)			
			[.0008]	[.0009]			
β_0^S (constant)	…	…	.4803	.4423	…	…	…
			(.2482)	(.1759)			
			[.0078]	[.0136]			
β_0^F (constant)	…	…	.5778	…	…	…	…
			(.1886)				
			[.0022]				
β_1 (crop)	…	…	−.7808	−.6971	−.4434	−.5181	…
			(.5596)	(.4053)	(.4111)	(.3014)	
			[.0246]	[.0362]	[.0612]	[.0730]	
β_2 (wealth)	…	…	.0904	.0344	.0954	.0530	…
			(.0367)	(.0277)	(.0276)	(.0229)	
			[.0272]	[.0450]	[.0018]	[.0048]	
γ_0^P (constant)	.2611	.3106	…	…	…	…	…
	(.0283)	(.0440)					
	[.0000]	[.0000]					
γ_0^S (constant)	.4494	.4603	…	…	…	…	…
	(.0168)	(.0171)					
	[.0000]	[.0000]					

γ_0^F (constant)	.3322	.3323	…	…	…	…	…
	(.0132)	(.0133)					
	[.0000]	[.0000]					
γ_1 (wealth)	−.0673	…	…	…	…	…	…
	(.0112)						
	[.0000]						
γ_1^P (wealth)	…	−.1381	…	…	…	…	−.1580
		(.0398)					(.0421)
		[.0012]					[.0000]
γ_1^S (wealth)	…	−.0144	…	…	…	…	−.0152
		(.0161)					(.0168)
		[.1808]					[.1832]
γ_1^F (wealth)	…	−.0670	…	…	…	…	−.0534
		(.0131)					(.0135)
		[.0000]					[.0001]
R^2	.0971	.1084	…	…	…	…	…

Source.—See table 1.

Note.—In cols. 1, 2, and 7, the dependent variable is crop (see table 1 for the definition of crop). In cols. 3–6, the dependent variable is a dummy variable equal to zero if the contract chosen is a share contract and equal to one if the contract is a fixed-rent contract. Superscripts indicate town-specific coefficients (P=Pescia, S=San Gimignano, F=Florence). Bootstrapped standard errors are in parentheses, and bootstrapped p-values are in brackets. Col. 1 estimates a matching equation by regressing crop on town dummies and tenant's wealth (see table 1 for the definition of wealth). Col. 2 assesses whether there are differences in the matching equation across towns by allowing regional differences in both the intercept and slope coefficients. Bootstrapped p-values for the significance of the differences of the γ_1 coefficients are .001 for $\gamma_1^P-\gamma_1^S$, .039 for $\gamma_1^P-\gamma_1^F$, and .0068 for $\gamma_1^S-\gamma_1^F$. Cols. 3 and 4 contain linear instrumental variable results for the full sample and the Pescia–San Gimignano sample, respectively. Cols. 5 and 6 present fixed-effects instrumental variable models that allow for two-sided matching to the extent that landlords' unobservables are constant across different tenants. This might be the case if landlords differed in unobserved monitoring or transactions costs or if they owned land plots nearby each other and similar in unobserved characteristics. Col. 7 is the first-stage regression corresponding to these fixed-effects instrumental variable models. Again, the γ_1 coefficients are significantly different from each other. The number of observations is 902 in cols. 1, 3, 5, and 7 and 379 in cols. 2, 4, and 6.

Since crop is negatively correlated with the observed component of risk aversion (wealth), it should also be negatively correlated with the unobserved component of risk aversion. This imparts negative bias on the crop coefficient. In addition, the coefficient on wealth will be biased toward zero (see Ackerberg and Botticini [1999] for details). Interestingly, these inferences are consistent with the OLS biases uncovered in the instrumental variable results below.

Linear Instrumental Variable Models

Given the apparent existence of matching and the resulting endogeneity of crop, Section II suggests the possibility of instrumenting using cross-region differences in the matching equation. The intuition is that significant such differences will provide instruments for the crop variable. With this method there is no need to make assumptions about the causes of matching.

For the validity of the instrumental variable approach, the coefficients on both crop type (crop) and risk aversion (wealth) in the contract choice equation need to be assumed identical across towns. The main intuition behind this exclusion restriction is theoretical (Sec. II). Theory predicts that contract choice should be a function of the fundamental characteristics of the principal and the agent, *not* of the market of residence. In other words, if one considers an agent with risk aversion a and a principal with crop type p, they agree on the same optimal contract *regardless* of the town they are living in.

While this assumption may be intuitive, it is not without potential problems. One might worry that there are fundamental differences across towns that affect the contracting environment. One example would be if crops behaved differently across towns (e.g., because of weather, soil, or different types of grapes). If this were the case, the dummy variable crop might have a different effect on contract choice across towns, invalidating our exclusion restriction. Fortunately, we have some evidence that this may not be the case. Tuscany has been divided into five topographical areas (Biagioli 1975, pp. 273–80): inland mountains, coastal mountains, inland hills, coastal hills, and valleys. The countrysides of Florence, Pescia, and San Gimignano all belong to the "inland hills" group. As for climatic conditions, the area in which the three towns are located is homogeneous with regard to temperature and rainfall (Almagia 1957, 1:411–26).

Another potential problem may arise from the possibility that there are fundamental differences in the economic environment in the towns. For example, Florence, with its size and status as a commercial center, might have had better insurance or capital markets than the two smaller towns. We consider samples both with and without Florence to address this issue: one might be more inclined to believe that our identification restriction holds across the two smaller, more similar, towns. We have no casual evidence of significant differences between San Gimignano and Pescia. In fact, the sizes of their populations were very similar (in the *catasto* of 1427, 532 and 576 households lived in Pescia and in San Gimignano, respectively), they had similar distances from Florence, and they had similar credit markets (Botticini 2000). However, it is hard to argue this point conclusively.[16]

Column 2 of table 4 assesses whether there are differences in the matching equation across towns by allowing regional differences in both the intercept and slope coefficients. Though there is a significant difference in constant terms, our allowance of either landlord fixed effects or regional dummies in the contract choice equation means that these differences are not helpful as instruments. More important for our purposes are differences in the matching function slope coefficients. The coefficients γ_1^S, γ_1^P, and γ_1^F represent the slope terms in San Gimignano, Pescia, and Florence, respectively. The estimates suggest that the relation between wealth and crop is negative in all three but strongest in San Gimignano, next strongest in Florence, and weakest in Pescia. The three terms are all significantly different from each other (bootstrapped p-values are all smaller than .05), suggesting that we have reasonably strong instruments.

Columns 3 and 4 contain linear instrumental variable results for the full sample and the Pescia–San Gimignano sample, respectively. Columns 5 and 6 are instrumental variable fixed-effects models. These fixed-effects models allow for two-sided matching to the extent that a landlord's unobservables are constant across different tenants. This might be the case if landlords differed in unobserved monitoring or transactions costs, or if they owned land plots nearby each other and similar in unobserved characteristics.

A comparison of the instrumental variable results in table 4 to the corresponding OLS results in table 3 shows a number of differences. In all cases the crop coefficient becomes smaller and considerably less significant. Similarly, in all cases the wealth coefficient increases in magnitude. These changes are most apparent in the *least restrictive* specification, the fixed-effects model with the Pescia–San Gimignano data (col.

[16] Recall that we also need the proxy relationship between wealth and risk aversion to have the same *slope* across towns; i.e., augmenting wealth by one florin has the same effect on risk aversion regardless of the town the tenant lives in. Given our inclusion of either landlord fixed effects or region dummy constants in the contract choice equation, it is not essential that the proxy relationship have the same mean across markets. Any differences will be picked up by the fixed effects or the constants. It is also not necessary for the unobserved component of risk aversion to have the same variance across markets.

6).[17] When these estimates are compared to their OLS analogue (col. 6 of table 3), the estimated coefficient on wealth more than doubles (from 0.0242 to 0.0530) and moves from borderline significance (one-sided p-value of .0447) to clear significance (p-value .0048). Note that the differences between the OLS and instrumental variable results are indicative of negative correlation between the errors in the contract choice and matching equations. Given the signs of the γ_1's ($-$) and β_2 ($+$), this negative correlation is consistent with the entry of an unobserved component of risk aversion in both of these error terms.

In summary, the linear models suggest that (i) there is matching and (ii) controlling for this matching *does* make a difference. Particularly in the Pescia–San Gimignano sample, the OLS estimates overestimate the effect of crop and underestimate the effect of wealth, in both value and statistical significance.

B. Nonlinear Models

Full Information Maximum Likelihood Models

The linear instrumental variable models ignore a considerable amount of discreteness in the model. The contract variable is one of two discrete types, and crop type (crop) has three discrete types. To control for endogenous matching accommodating these nonlinearities, we need to consider the full system of equations. Assume that contract choice is given by the probit

$$y_i = I(\beta_0^k + \beta_1 c_i + \beta_2 w_i + \epsilon_i^1 > 0), \tag{4}$$

and the matching equation is given by the ordered probit

$$c_i(w_i,\ k,\ \epsilon_i^2) = \begin{cases} 1 & \text{if } \gamma_1^k w_i + \epsilon_i^2 > \bar{C}^k \\ .5 & \text{if } \underline{C}^k < \gamma_1^k w_i + \epsilon_i^2 < \bar{C}^k \\ 0 & \text{if } \underline{C}^k > \gamma_1^k w_i + \epsilon_i^2, \end{cases} \tag{5}$$

where y_i is the contract choice dummy, c_i is the crop type variable, w_i is the tenant's wealth, and k indexes the three (or two) different towns.

A few remarks are in order. First, both the slopes (γ_1^k) and cutoffs ($\bar{C}^k$ and $\underline{C}^k$) in the ordered probit matching equation are allowed to depend on town k. Because of the discrete nature of the processes, the variances of the assumed normal unobservables ϵ_i^1 and ϵ_i^2 need to be

[17] This is the least restrictive specification since it does not assume that the contracting environment is the same in San Gimignano and Pescia as it is in Florence; it also allows for unobserved landlord characteristics. With the subsample, there are no overidentifying restrictions. With the full sample, we were able to use overidentifying restrictions to reject the hypothesis that Florence is the same as the other two towns (p-value .001), but we were unable to reject the hypothesis that San Gimignano and Pescia have the same slope coefficients (p-values .171 and .339).

TABLE 5
FIML Results: Pescia and San Gimignano Sample
Dependent Variable: Contract Choice (Cols. 1, 3, 4) and Crop Choice (Col. 2)

Parameter	Probit (1)	Ordered Probit First Stage (2)	Full Model (3)	No Functional Form Identification (4)
β_0^P (constant)	1.5220	…	1.0229	.1291
	(.1481)		(.2965)	(.5749)
β_0^S (constant)	−.0770	…	−.8043	−1.1213
	(.2313)		(.4046)	(.4559)
β_1 (crop)	−2.9956	…	−1.2274	−2.3640
	(.2789)		(.9902)	(.9564)
β_2 (wealth)	.1322	…	.2305	.3186
	(.1356)		(.1164)	(.1115)
θ (nonlinear wealth)	…	…	…	3.0660
				1.7632
γ_1^P (wealth)	…	−.4511	−.4256	−.4281
		(.1127)	(.1123)	(.1177)
γ_1^S (wealth)	…	−.0929	−.0956	−.0984
		(.1395)	(.1525)	(.1585)
$\bar{C}^P$ (upper cutoff)	…	.7088	.7149	.7242
		(.1184)	(.1195)	(.1164)
$\bar{C}^S$ (upper cutoff)	…	1.8052	1.8073	1.7854
		(.1494)	(.1503)	(.1538)
$\underline{C}^P$ (lower cutoff)	…	.6451	.6516	.6604
		(.1159)	(.1178)	(.1177)
$\underline{C}^S$ (lower cutoff)	…	−1.1646	−1.1669	−1.1752
		(.1418)	(.1399)	(.1545)
Corr$(\epsilon^1\epsilon^2)$	…	…	−.6587	−.8611
			(.3577)	(.5905)
Log likelihood	−90.54	−205.55	−294.31	−293.31

Source.—See table 1.

Note.—In cols. 1, 3, and 4, the dependent variable is a dummy variable equal to zero if the contract chosen is a share contract and equal to one if the contract is a fixed-rent contract. In col. 2, the dependent variable is crop (see table 1 for the definitions of crop and wealth). Superscripts indicate town-specific coefficients (P=Pescia, S=San Gimignano, F=Florence). In all columns, asymptotic standard errors are in parentheses. Col. 1 reestimates the naive probit contract choice equation ignoring the endogeneity of crop. Col. 2 presents results of the ordered probit matching equation estimated independently of the contract choice equation. Col. 3 estimates the contract choice and the matching equations simultaneously, allowing for correlation between the error terms. Col. 4 also estimates the contract choice and the matching equations simultaneously but corrects for nonlinear identification (as explained at the end of Sec. IV). The number of observations is 379 in all columns.

normalized to one, but we estimate a correlation coefficient between the two unobservables. This correlation is particularly important to accommodate because, at the very least, both ϵ_i^1 and ϵ_i^2 include the unobserved component of tenant's risk aversion. Second, the "ordered" variable c_i, *not* the latent variable determining c_i, enters the contract choice equation. This corresponds with our interpretation that it is the discrete move from vines to cereals that has an effect on contract choice.

For these full information maximum likelihood (FIML) models, we focus on the Pescia–San Gimignano data set, both because it relies on fewer identifying restrictions and because this is where controlling for matching makes the largest difference. Column 1 of table 5 reestimates the naive probit contract choice equation ignoring the endogeneity of crop: β_1 is very significant, but β_2 is not. Column 2 presents results of the ordered probit matching equation estimated independently of the contract choice equation. Both the upper and lower cutoffs and the coefficient on wealth in the ordered probit differ significantly across towns. This suggests that we do have potential instruments for the crop variable c_i.

Column 3 estimates the contract choice and the matching equations simultaneously, allowing for correlation between the error terms. The pattern is very similar to that in the linear models, if not a bit more dramatic. The estimate of β_1 more than halves and becomes insignificant, whereas the estimate of β_2 almost doubles and becomes significant. The correlation term is negative and significant, consistent with an unobserved component of risk aversion. Again, while the naive estimates indicate that the effect of crop is the strongest statistical relation in the data, controlling for endogenous matching suggests that the wealth effect predominates.

Identification Caveats and Robustness

The intuition behind the FIML estimates in column 3 of table 5 is the same as in the linear models. We want cross-region differences in the matching equation to identify the contract choice equation. Interestingly, there are additional, perhaps unwanted, sources of identification in the full model. They arise from the inherent nonlinearities in the structural matching equation. In a linear model with no instruments, it is impossible to distinguish the direct effect of w_i on the contract variable $y_i^* = \beta_0^k + \beta_1 c_i + \beta_2 w_i + \epsilon_i^1$ versus the indirect effect of w_i through c_i. In contrast, in the *nonlinear* matching model above, the direct effect of w_i on y_i^* is still assumed linear, but the indirect effect, that is, $E[\beta_1 c_i \mid w_i]$, is nonlinear through the ordered probit structure. Hence, the system of equations (4) and (5) will generally be "identified" even without standard instruments. However, we would not describe this as "good" identification since it is not robust to alternative functional forms for w_i in the contract choice equation. As such, we want to be careful in assessing what is telling us what about β_1 and β_2.

One way to separate what the "good" instruments are telling us from what the "bad" ones are would be to allow w_i to directly enter the contract choice equation nonparametrically. As this would be taxing on our data, we take an alternative approach. We include w_i in the contract choice equation in a very particular nonlinear way, specifically, one that imitates w_i's nonlinear effect through c_i. Consider the function

$$OP(w_i, \epsilon_i^2) = \begin{cases} 1 & \text{if } E[\gamma_1^k]w_i + \epsilon_i^2 > E\bar{C}^k \\ .5 & \text{if } E\underline{C}^k < E[\gamma_1^k]w_i + \epsilon_i^2 < E\bar{C}^k \\ 0 & \text{if } E\underline{C}^k > E[\gamma_1^k]w_i + \epsilon_i^2, \end{cases}$$

where the E are expected values of the town dummy instruments (e.g., $E\gamma_1^k$ is the population-weighted average of the two [or three] γ_1^k's). Note that $OP(w_i, \epsilon_i^2)$ is a nonlinear function of w that is very similar to $c_i(w_i, k, \epsilon_i^2)$ and *exactly equal* to it if the town coefficients are all the same. Thus the contract choice equation

$$y_i = I(\beta_0^k + \beta_1 c_i(w_i, k, \epsilon_i^2) + \beta_2 w_i + \theta OP(w_i, \epsilon_i^2) + \epsilon_i^1 > 0)$$

will *not be identified* if the matching equation does not depend on region.

Column 4 presents estimates of this model. The coefficient on crop is now significant, and θ is positive and significant. Note that the overall effect of wealth on contract choice in this model is a combination of the linear term β_2, which is positive, and the nonlinear term θ, which ends up being negative since wealth has a negative effect on $OP(w_i, \epsilon_i^2)$. The overall effect ends up positive and very significant (significance was computed by bootstrapping). We conclude that the insignificant coefficient on crop in column 3 may be misleading through contamination with some nonrobust sources of identification. When we allow for endogenous matching and utilize only good sources of identification, both crop and wealth appear statistically significant.

V. Conclusions

We have the potential to learn a great deal about markets and economies by studying the determinants of contractual arrangements between parties. For example, empirical evidence on these arrangements can shed light on the functioning of insurance markets, capital markets, or the significance of moral hazard. There has been a great deal of recent empirical work looking at such issues in a wide range of contexts: from historical data, to agriculture in developing countries, to franchising and agriculture in the contemporary United States.

This article introduces issues that suggest care in these endeavors. We focus on the empirical implications of potential matching of heterogeneous principals and agents. We argue that there are economic incentives for such matching and show that when there are econometrically unobserved or partially observed characteristics, this matching is problematic for estimates of optimal contract choice equations.

We then suggest potential solutions for these problems. These solutions revolve around consideration of a matching equation describing how principals and agents match. At the very least, the solutions suggest that a very relevant correlation to examine is the one *between* the characteristics of the contracting parties. Not only can the examination of these "matching equations" solve problems in contract choice equations, but it can also provide interesting economic insights.

Consideration of these matching equations also points toward future work. If one is willing to make assumptions on the process through which principals and agents match with each other, one can start with basic economic primitives (e.g., utility and production functions) and build a fully specified model of both matching and contract choice. Such a model would more fully exploit the interesting information contained in the way principals and agents match.

In the empirical work, we apply our techniques to a historical data set on agricultural contracts between landlords and tenants in three towns and the villages in their respective countryside in early Renaissance Tuscany. We look for potential evidence of risk sharing and of multitasking issues arising from the perennial nature of some of the crops. We find a number of interesting empirical results. First, there is very strong evidence that particular types of tenants matched with particular types of landlords. Second, naive estimates ignoring this matching can give misleading results. In our least restrictive specifications, controlling for endogenous matching more than doubles one of the parameters of interest and calls into doubt the very strong significance of the other. Finally, we end up with some interesting economic conclusions. While most of the literature has not found significant evidence of risk sharing, we do find evidence for risk sharing between landlords and tenants in early Renaissance Tuscany. We also find some evidence that multitasking issues played a role in contractual choice on perennial crops.

These findings are interesting both from a historical point of view and in the context of developing countries. Historians have debated why share contracts were predominant agrarian arrangements in medieval Tuscany, early modern France and Spain, and the postbellum U.S. South (Reid 1973; Alston and Higgs 1982; Hoffman 1984; Epstein 1994; Emigh 1997; Carmona and Simpson 1999). At least for medieval Tuscany, we can argue that both risk-sharing considerations and multitasking features of medieval viticulture had an impact on the spread of share contracts. In the absence of insurance markets, share contracts seem to have provided an insurance mechanism for tenants while at the same time giving incentives not to overproduce and damage the perennial crops. Given that some contemporary developing countries have some similar attributes (e.g., imperfect insurance or capital markets), the findings of this article have the potential to provide interesting insights into current policy debates on the role of agrarian arrangements in these countries.

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