

# Group Lending, Matching Patterns, and the Mystery of Microcredit: Evidence from Thailand

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## Abstract

How has the microcredit movement managed to push financial frontiers? If borrowers vary in unobservable risk, then group-based, joint liability contracts price for risk more accurately than individual contracts, provided that borrowers match positively assortatively by project riskiness (Ghatak, 1999, 2000). This more accurate risk-pricing can attract safer borrowers and rouse an otherwise dormant credit market. We extend the theory to include correlated risk, and show that borrowers will seek to undo joint liability by matching to anti-diversify risk within groups. We test for positive assortative matching by project riskiness, and for intra-group diversification of risk, using data on Thai microcredit borrowing groups. We propose a new non-parametric methodology to test for homogeneous and positive assortative matching in a single dimension. Multidimensional matching analysis is also carried out using Fox's (2010a) matching maximum score estimator. Evidence is found for a) positive assortative matching by project riskiness and b) risk anti-diversification within groups, though not along occupational lines. This evidence supports the idea that group lending improves risk-pricing by embedding a discount for safe borrowers, and thus can plausibly explain part of the unprecedented rise in financial intermediation among the world's poor. However, the anti-diversification results point to a potential pitfall of voluntary group formation, and suggest strategies for lender intervention.

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# 1 Introduction

Recent impact studies have called into question the “miracle” of microcredit – i.e. transformative impacts of new formal credit access on well-being of poor households.<sup>1</sup> The question is not fully settled,<sup>2</sup> however, and there remains a strong prima facie case for some degree of net positive impacts from microcredit: the apparently large number of microcredit institutions lending sustainably to poor borrowers without needing subsidies<sup>3</sup> suggests that gains from trade are being realized.

Whatever microcredit’s net impact may be, there is little doubt about how widespread it is and how rapidly it has grown in recent years. Maes and Reed (2012) report that over two hundred million people have borrowed from nearly four thousand microfinance institutions throughout the world. Forty years ago, any prediction of this development would likely have been greeted with skepticism. As the 2006 Nobel Peace Prize Press Release puts it, “Loans to poor people without any financial security had appeared to be an impossible idea.”<sup>4</sup> This unprecedented expansion of microcredit gives rise to the following puzzle: how has this growth in intermediation and financial services among the world’s poor been possible? How have lenders managed to overcome the obstacles involved in lending to borrowers without using collateral?

The current paper is focused on this “mystery” of microcredit.<sup>5</sup> Specifically, it explores one candidate answer based on group lending and borrower matching, due to Ghatak (1999, 2000). The context is a standard adverse selection environment in which there is limited

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<sup>1</sup>See discussion in Banerjee et al. (2015), and the studies cited there.

<sup>2</sup>Some studies do find significant immediate impacts, e.g. Kaboski and Townsend (2011, 2012). Also, the studies cited can reject large impacts on the average villager, but typically cannot rule out large impacts on villagers who actually borrow (Banerjee et al., 2015). Thus the cost-benefit question remains unsettled, since the main costs are incurred on actual borrowers. Finally, longer-run impacts may be more dramatic, but remain largely unmeasured. Ahlin and Jiang (2008) explore the issue of long-run impact theoretically.

<sup>3</sup>For example, see Cull et al. (2009).

<sup>4</sup>This Prize was given to Muhammad Yunus and the Grameen Bank for pioneering efforts in microcredit.

<sup>5</sup>This paper is not the first to do so. A growing literature has explored innovative practices and contract forms associated with the microcredit movement that may underpin its unprecedented success in lending among the poor. See Armendariz and Morduch (2010), Ghatak and Guinnane (1999), and Morduch (1999) for introductions to the topic, and the next section for more elaboration.

liability and no collateral, and borrowers' projects have identical expected values but different degrees of risk. In this environment, a lender that cannot observe project risk offers all borrowers the same terms; its inability to price for risk results in cross-subsidization of riskier borrowers by safer borrowers and can cause a large portion of the potential market (safer households) to avoid borrowing. This market breakdown is the key inefficiency: good projects go unfunded due to the lender's inability to price for risk.

Ghatak adds to this context local information – borrowers know each other's risk, though the lender does not – and shows that group-based, joint liability lending contracts can harness this local information to improve the lender's ability to price for risk. The idea is as follows. First, given joint liability, borrowers match positively assortatively by riskiness. Second, given positive assortative matching (“PAM”), the lender can use joint liability contracts to screen or pool borrowers to increase efficiency. Consider the pooling case.<sup>6</sup> Even though contract terms are the same for all borrowers, an *implicit discount* is built in for safer borrowers: they have safer partners, due to PAM, and thus when they succeed the joint liability clause is less costly for them. That is, joint liability plus PAM helps to equalize the repayment burden across borrowers.<sup>7</sup> This can draw into the market safer borrowers who would have been inefficiently excluded under standard, individual loans.

The beauty of this result is that the lender is improving risk-pricing – and with it the efficiency and size of the market – by offering all borrowers the same contract, without learning their riskiness. This is appealing in practical terms. It implies that even a very passive or unsophisticated lender that offers a single, standardized group contract is giving implicit discounts to safe borrowers, and hence more accurately pricing for risk than if it used individual contracts. Thus, this is a theory that can help explain the popularity of group lending in microcredit – lenders that use it may be reversing partial market breakdown – as

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<sup>6</sup>Ahlin (2015b) shows that pooling works just as well as screening in this context, i.e. a single contract can achieve the same efficiency as any menu. What matters is not the lender's ability to screen borrowers, but its ability to improve risk-pricing through joint liability, with or without screening.

<sup>7</sup>Optimal joint liability plus PAM plus asymptotically large groups *fully* equalizes the repayment burden across borrowers, as long as project returns allow for affordability of typical bailout scenarios (Ahlin, 2015b).

well as the growth of credit markets among the poor as this contract form is discovered and diffused.

The lynchpin in this theory is that matching into microcredit borrowing groups is positive assortative by project risk; this is what provides the implicit discounts for safe borrowers.<sup>8</sup> To our knowledge, however, matching patterns of microcredit groups have yet to be empirically tested.<sup>9</sup> A main contribution of the current paper is to test directly for PAM by project risk among microcredit groups in Thailand.

The paper also extends the theory on matching for credit to consider *correlated* risk, asking whether borrowers will match with other borrowers exposed to similar, or different, risks. The result derived is that groups match homogeneously in both dimensions: they match with borrowers of similar riskiness (PAM), and among those, with partners exposed to the same type of risk. The intuition for the latter result is straightforward: groups anti-diversify in order to avoid facing liability for their partners. This points to a potentially negative consequence of voluntary group formation, since anti-diversification can limit the effectiveness of joint liability as a contracting tool.

To test empirically whether groups are homogeneous in both riskiness and type of risk exposure, the Townsend Thai dataset is used. It includes information on borrowing groups from the Bank for Agriculture and Agricultural Cooperatives (BAAC). The BAAC is the predominant rural lender in Thailand. It offers joint liability contracts to self-formed groups of borrowers with little or no collateral. Importantly, this unique dataset includes multiple groups from each of a number of villages – taking the village as the matching market, this allows matching patterns to be tested using a number of independent matching markets.

To assess homogeneity of matching along one dimension, we develop a new approach. For each village and variable, a variance decomposition, rank correlation, and/or chi-squared test

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<sup>8</sup>Random matching makes group lending no better than individual lending in this context (Ahlin, 2015b).

<sup>9</sup>The literature has recognized this as an important open question. For example, it is first on the microfinance mechanisms empirical research agenda Morduch (1999, p. 1586) lays out: “Is there evidence of assortative matching through group lending as postulated by Ghatak (1999)?” See next section for complementary and related work.

statistic is calculated to measure how homogeneously matched groups within the village are. These calculations are then scaled using a permutation test. Specifically, the result from the observed grouping is mapped into a village *homogeneity percentile* reflecting how homogeneous the village's observed grouping is relative to all possible groupings of the sampled village borrowers into groups of the observed sizes.

For any given variable, villages can be found at both ends of the spectrum, characterized by high and low homogeneity percentiles. Means and medians of the village homogeneity percentiles suggest predominant tendencies. We show that if matching is random with respect to the variable in question, then village homogeneity percentiles are drawn from a uniform distribution. Thus we can statistically compare the matching patterns observed in the data to random matching by comparing the distribution of observed village homogeneity percentiles to the uniform distribution. In short, this technique combines homogeneity metrics and permutation techniques at the village level with a statistical test against the benchmark uniform distribution across villages.

We find direct evidence for within-group homogeneity in project riskiness. That is, though far from maximally homogeneous matching (PAM), the data can reject random matching in the direction of homogeneity. We also find some evidence for anti-diversification within groups. While random matching based on agricultural occupation cannot be rejected, groups are mildly anti-diversified in terms of clustering of bad income years.

These results are extended in two ways. First, it has been shown that when groups have more than two members, homogeneous matching and PAM may no longer coincide (Ahlin, 2015a). Groups may appear more homogeneous than random by some metrics, but still be matching *negatively* assortatively. So, we replace the homogeneity metrics with a *complementarity* metric derived directly from the theoretical group payoff function, which induces PAM because it exhibits complementarity. This approach is more structural in taking a stance on functional form of the group payoff function, but it allows cleaner identification of complementarity vs. substitutability (along the lines of Fox, 2010b). Combined with the

permutation test described above, the complementarity metric can be transformed into a village complementarity percentile; comparison of village complementarity percentiles with the uniform distribution then shows whether observed matching patterns are dictated by a payoff function that features complementarity or substitutability, and hence induces PAM or NAM. Second, we carry out multidimensional matching analysis to check whether both dimensions of matching modeled here appear important for matching patterns. Fox's (2010a) matching maximum score estimator is used; it chooses parameter values that maximize the frequency with which observed groupings yield higher payoffs than feasible, unobserved groupings. The results of both extensions are supportive of earlier conclusions.

In sum, Ghatak's theory receives support from the data: within-group homogeneity of project risk is significantly greater than random matching would predict. Evidently, group lending is successfully embedding a non-negligible discount for safe borrowers because their equilibrium partners are safer; and this can partly explain how microcredit has successfully awakened previously dormant credit markets. However, results on anti-diversification caution of a potentially negative aspect of voluntary group formation, and suggest that lenders may benefit from increasing the incentives to match for diversification, if this can be done cleanly.

The paper does not decisively establish causal determinants of group formation. However, we argue that to assess whether group lending enables better risk-pricing by targeting discounts to safe borrowers, this is not necessary (section 5.3). Whether risk-homogeneity results from purposeful matching or as a byproduct of other constraints or objectives, it is by itself sufficient for the improvement in risk-pricing that enables group lending to revitalize markets.

In what follows, related literature is discussed in Section 2. The model setup and theoretical matching results are in Section 3. Data are described and variables defined in Section 4. Section 5 presents the methodology behind the nonparametric univariate tests (section 5.1), as well as the results (section 5.2) and a discussion of causality (section 5.3). Section 6 presents the multivariate estimation. Section 7 concludes. Proofs are in the appendix.

## 2 Relation to the Literature

This paper contributes to unraveling the “mystery” of microcredit – that is, how and why lending has exploded in these markets – by highlighting and finding empirical evidence for a plausible mechanism through which credit markets can be revived. It does not fully resolve the puzzle. For one, not all successful microlenders use group lending contracts. Also, the paper focuses on one mechanism, in an adverse selection environment, rather than testing across multiple mechanisms or environments. However, given that the puzzle’s solution is likely to be multi-faceted and to be reached in a step-wise manner, this paper makes the significant contribution of providing empirical backing to one key theory.

A number of other papers also shed light on this puzzle empirically or theoretically. Among other topics, they examine the innovations that gave rise to microcredit’s expansion,<sup>10</sup> the underlying credit market frictions,<sup>11</sup> and the types of contracts that work best.<sup>12</sup> Relative to this literature, this paper is the first to focus empirically on matching combined with group lending as a key mechanism for repairing credit markets, and to offer direct evidence on a specific mechanism that may help explain this mystery.

The substantive focus of the paper is an empirical assessment of matching patterns in microcredit groups. To our knowledge this has not been done before, though related and complementary work exists. Eeckhout and Munshi (2010) study commercial ROSCAs<sup>13</sup> in India and show that changes in group composition and characteristics, in response to new regulation capping interest rates, are in line with predictions of their matching model. We differ in focusing on microcredit rather than ROSCAs; ROSCAs tend to group together both borrowers and lenders, while microcredit groups are predominantly borrowers. We also focus on characterizing existing matching patterns rather than re-matching in response to changes

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<sup>10</sup>See for example Ghatak and Guinnane (1999), Armendariz and Morduch (2000), and Cull et al. (2009).

<sup>11</sup>See for example Ahlin and Townsend (2007a, 2007b), who find evidence consistent with the adverse selection context studied here; and Karlan and Zinman (2009), who do not find strong evidence for adverse selection, but rather for moral hazard. The current paper studies the same geographic setting as Ahlin and Townsend (2007a, 2007b), raising our expectation that adverse selection may be an issue.

<sup>12</sup>See for example Gine and Karlan (2014) and Ahlin and Waters (forthcoming).

<sup>13</sup>“ROSCA” stands for rotating savings and credit associations.

in the environment. Although it is not their main focus, Gine et al. (2010) study group formation in a microcredit-inspired field laboratory game, and find evidence that participants with similar levels of risk aversion group together. A key difference is that we use data on practicing microcredit groups; this avoids the concern that a specific lab game may differ from practiced microcredit in important ways. There is also a literature on matching for risk-sharing, both in the lab (e.g. Attanasio et al., 2012 and Barr and Genicot, 2008) and using household data (e.g. Fafchamps and Gubert, 2007). While sharing some features in common with microcredit group formation, these settings lack key features of credit, so it is not clear that results are applicable to a microcredit context.<sup>14,15</sup>

The paper also proposes a new statistical test for homogeneous vs. heterogeneous matching, and matching based on complementarity vs. substitutability. This test applies to one-sided matching when data on matches in multiple markets is available. It shares in common with independent work by Fox (2010a) the idea of comparing observed and unobserved matches in multiple markets, but takes this in a new direction using permutation testing combined with a result linking the uniform distribution to random matching. Unlike Fox's estimator, however, this test is not equipped to estimate matching patterns with multi-dimensional characteristics. Permutation tests have been used elsewhere in studying matching, most commonly to conduct inference for OLS regressions with dyadic data.<sup>16</sup> For example, Gine et al. (2010) use permutation techniques from Krackhardt (1987) in the context of dyadic OLS regressions. However, the dyadic regression approach can give misleading results about the underlying matching pattern (Ahlin, 2015a), and this paper goes in a different direction.

Finally, the paper contributes to the theory of matching for microcredit by introducing a second dimension of heterogeneity of borrowers, the *type* of risk they are exposed to. This

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<sup>14</sup>Indeed, Schulhofer-Wohl (2006) finds equilibrium matching to be negative assortative in his model of matching to share risk, while the microcredit model of this paper finds positive assortative matching.

<sup>15</sup>An even more different, but interesting, setting in which matching has been analyzed is in the formation of Community-Based Organizations – e.g., Arcand and Fafchamps (2012) and Barr et al. (2015).

<sup>16</sup>An exception is Barr and Genicot (2008), who use permutation techniques of Krackhardt (1987) to statistically compare two network matrices based on fractions of identical entries.



is the first multi-dimensional matching analysis we know of in the microcredit context, and it uncovers a new result: that matching based on type of risk exposure may lead to anti-diversification, as borrowers form groups so as to undo joint liability. The novel implication is that voluntary matching need not work in favor of efficiency, at least not in all dimensions.

In sum, this paper advances the theoretical understanding of how microcredit groups form, and provides a first empirical characterization of matching patterns of existing groups. It also breaks new ground in analyzing one-sided matching (group formation), going beyond the dyadic regression techniques common in the literature to develop new techniques and use existing ones that can be argued to identify underlying matching patterns more reliably.

## 3 Theoretical Framework

### 3.1 Baseline model and results

The model here follows Ghatak (1999, 2000), which builds on work of Stiglitz and Weiss (1981). Risk-neutral agents are each endowed with no capital and one project. Each project requires one unit of capital and has expected value  $\bar{R}$ . Agents and their projects differ in riskiness, indexed by  $p \in \mathcal{P}$ , where  $\mathcal{P} = [\underline{p}, \bar{p}]$  and  $0 < \underline{p} < \bar{p} < 1$ . The project of an agent of type  $p$  yields gross returns of  $R_p$  (“succeeds”) with probability  $p$  and yields gross returns of 0 (“fails”) with probability  $1 - p$ . Thus  $p \cdot R_p = \bar{R}$ , for all  $p \in \mathcal{P}$ . The higher  $p$ , the lower the agent’s riskiness.

An agent’s riskiness is observable to other agents, but not to the outside lender. In this context, uncollateralized individual loan contracts can be inefficient. They bear an interest rate based on the average risk in a borrowing pool, a rate at which safer borrowers may find it unprofitable to borrow.<sup>17</sup> Thus, the lending market can (partially) collapse, excluding all but the riskier borrowers due to a failure to price for risk. Efficiency losses in this context

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<sup>17</sup>For evidence consistent with this behavior in the Thai context, see Ahlin and Townsend (2007b). For more detailed theoretical analysis see Ahlin (2015b, Section 5).

result from good projects left unfunded – the safer borrowers’ – and raising efficiency comes from attracting more borrowers.<sup>18</sup>

In this context, group lending can increase efficiency by improving risk-pricing, offering implicit discounts to safer borrowers. A lender requires potential borrowers to form groups of size two, each member of which is liable for the other. Without loss of generality (Ahlin, 2015b), a single, standardized contract is offered to all borrowers. In the contract, a borrower who fails pays the lender nothing, since loans are uncollateralized. A borrower who succeeds pays the lender gross interest rate  $r > 0$ . A borrower who succeeds and whose partner fails makes an additional liability payment  $c > 0$ . Thus, a borrower of type  $p_i$  who matches with a borrower of type  $p_j$  has expected payoff

$$\pi_{ij} = \bar{R} - rp_i - cp_i(1 - p_j), \quad (1)$$

assuming the borrowers’ returns are uncorrelated.

In order to compare to a standard individual loan contract, where the payoff is  $\bar{R} - p_i r$  and the interest rate does not vary by risk-type, one can rewrite the borrower’s payoff under the group lending contract (equation 1) as

$$\pi_{ij} = \bar{R} - p_i \tilde{r}_{ij},$$

where

$$\tilde{r}_{ij} \equiv r + c(1 - p_j). \quad (2)$$

Here  $\tilde{r}_{ij}$  is interpretable as the *implied* interest rate paid by borrower  $i$  when successful and matched with borrower  $j$ . Two components make up this implied interest rate: the direct interest rate  $r$ , and the expected bailout payment for the partner,  $c(1 - p_j)$ .

Because this second component depends on partner quality ( $p_j$ ), the question of how

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<sup>18</sup>Hence, the term “adverse selection” risks being somewhat misleading here: the goal is to include safe borrowers, not exclude risky, since all have equally good projects.

borrowers match becomes critical. Utility is transferable in this context, and side transfers between borrowers are allowed. Thus, following Ghatak (1999, 2000) and Legros and Newman (2002), the equilibrium includes a) an assignment of borrowing agents into two-member borrowing groups or non-borrowing, and b) payoffs of all borrowing agents such that two co-grouped agents' equilibrium payoffs sum to their total group payoff and such that no two agents can earn strictly higher payoffs by grouping together. It is well known that in such an equilibrium, no two groups can be rearranged to produce a higher sum of group payoffs – a fact that will be used later.

Note that

$$\frac{\partial^2(\pi_{ij} + \pi_{ji})}{\partial p_i \partial p_j} = 2c > 0. \quad (3)$$

That is, the group payoff function exhibits complementarity, and groups that are perfectly homogeneous in riskiness is the stable outcome when there is a continuum of agents, as Ghatak has shown. The intuition is that having a more reliable (safer) partner is worth more to safe borrowers, since a borrower is “on the hook” for his partner only if he succeeds.

Note that a borrower with a safer partner (higher  $p_j$ ) has a lower implied interest rate (equation 2), because his chance of owing a bailout payment when successful is lower. What homogeneous matching gives is that *safer borrowers* have *safer partners*, and thus, lower implied interest rates. With perfectly homogeneous matching,

$$\tilde{r}_{ij} = \tilde{r}_{ii} = r + c(1 - p_i) \quad \text{and} \quad \frac{\partial \tilde{r}_{ij}}{\partial p_i} = \frac{\partial \tilde{r}_{ii}}{\partial p_i} = -c < 0. \quad (4)$$

Safer borrowers have safer partners, and thus can expect fewer bailout payments when successful. Thus, safer borrowers face a lower *implied* interest rate under joint liability – just as they would under full information. In this way, group lending harnesses social information to vary the interest rate implicitly by riskiness, thus improving risk-pricing.

This is true even under an unsophisticated pooling strategy, where the lender simply offers all comers a standard joint liability contract. Whether the lender knows it or not, if

matching is homogeneous, the contract embeds discounts for safe borrowers and can draw more of them into the market. In this sense, unsophisticated group lending can be responsible for reviving a lending market, underpinning a substantial increase in intermediation.

### 3.2 Variations on the baseline model

Several plausible variations to the baseline model are discussed here, though doing so formally is beyond the scope of the paper.

Consider a finite population of borrowers rather than a continuum. Though perfectly homogeneous matching will generally not be possible, it can be shown that groups will be rank-ordered by riskiness in any equilibrium.<sup>19</sup> That is, the two riskiest borrowers will pair together, the next two riskiest will pair together, and so on. Given rank-ordered matching, group lending has qualitatively similar risk-pricing advantages over individual lending: safer borrowers have generally safer partners, so they face lower implied interest rates. Thus, the theory does not critically rely on a large number of borrowers or perfectly homogeneous matching.

Next, consider removing the assumption that borrowers know each other’s riskiness. If riskiness is uncorrelated with any characteristics that do drive matching, then matching would be *random* with respect to riskiness, instead of homogeneous. All borrowers would then face the same implied interest rate, in expectation, equivalent to matching with a borrower of average riskiness in the borrowing pool. With no variation in ex ante implied interest rate across borrowers, group lending would lose its risk-pricing advantage over individual lending in this context and could not draw additional borrowers into the market.<sup>20</sup> But, if borrowers matched into “homogeneous” groups based on non-risk characteristics that are themselves predictive of riskiness – e.g. due to proximity or friendship – one would still observe some

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<sup>19</sup>See Proposition 1, Ahlin (2015a).

<sup>20</sup>See Ahlin and Townsend (2002, section 5.4.7) and Ahlin (2015b, Lemma 3) for more formal analysis.

In a somewhat different context involving costly auditing, Armendariz and Gollier (2000) show how joint liability can raise efficiency even with random matching. The idea is that risky borrowers pay more under joint liability if audited when successful, since their projects have higher returns when successful.

degree of group homogeneity in riskiness.<sup>21</sup> Interestingly, group lending would still embed an implicit discount for safe borrowers, for the reasons discussed. To the extent that groups formed homogeneously in riskiness, group lending could still be a force for expanding the lending market.

Next, consider group size. For simplicity, the theory in this paper is for groups of fixed size two. Ahlin (2015b) generalizes the model to fixed group size larger than two, demonstrating the improvements to risk-pricing from larger groups and exploring the optimal group size that results from the tradeoff between improved risk-pricing and information deterioration. The same forces are at work – standard joint liability contracts induce homogeneous matching, which gives safe borrowers implicit discounts in their effective interest rates, and improves efficiency by drawing them into the market. Thus, with a more general treatment of group size, it remains homogeneous matching that is critical for the market-reviving effects of group lending.<sup>22</sup>

Summarizing so far, within-group homogeneity in riskiness obtains in a number of circumstances, allowing group lending to improve risk-pricing and facilitate more efficient lending.

Several authors have made the point that a different form of joint liability can reverse the matching pattern. Sadoulet (1999) and Guttman (2008) consider *dynamic* contracts where liability for one’s partner carries the threat of being denied future loans if both borrowers fail. In this context, the group payoff function can exhibit substitutability, which means PAM is no longer an equilibrium. The intuition is that having a more reliable partner is worth more to riskier borrowers: they more often need their partner to be successful in order to continue receiving loans.<sup>23</sup> Thus, joint liability per se does not necessarily lead to

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<sup>21</sup>One could similarly assume that borrowers do know each others’ riskiness but face custom-based or other constraints on matching, so that perfect homogeneity is not possible.

<sup>22</sup>Even if borrowers could choose their own group composition *and* size, if types are complements and the payoff function is sum-based (see Ahlin, 2015a), then any two equilibrium groups must still be rank-ordered. Otherwise, one could rearrange the borrowers within the two groups, holding group sizes fixed, and raise the payoffs of at least one group of borrowers – contradicting equilibrium.

<sup>23</sup>Ahlin (2015b) also shows in a *static* setting that some joint liability contracts do not give rise to positive assortative matching in equilibrium, when group size is greater than two. As Ahlin (2015a) shows, potential equilibrium matching patterns can be quite diverse under substitutability when group size exceeds two.

homogeneous matching – the contract details matter.

However, these non-PAM matching results hold under contract forms that are not claimed to be optimal. To our knowledge the literature does not establish any efficiency properties of joint liability contracts that do not induce PAM, and hence offers no theoretical rationale for not observing PAM in joint liability lending. This does not rule out the existence of such a rationale; further, lenders may blunder in model/contract selection or operate under distorting political constraints – especially when heavily subsidized through the government budget, as our lender is. With less theoretical grounding, then, we will test empirically for matching that is more heterogeneous than random (or NAM), in addition to our main hypothesis of homogeneous matching (or PAM).

### 3.3 Matching over degree and type of risk

This section adds a second dimension of heterogeneity and points out a potential pitfall of relying on voluntary matching. We add to the baseline model the possibility for correlated risk. Given the agricultural setting of many micro-lenders, including the one in our data, this is a potentially important modification. However, it is little analyzed in the group lending literature, and to our knowledge not at all in the context of endogenous group formation.

Given two borrowers  $i$  and  $j$  with unconditional probabilities of success  $p_i$  and  $p_j$ , respectively, the joint output distribution can be written uniquely as:

	$j$ Succeeds ( $p_j$ )	$j$ Fails ( $1 - p_j$ )	
$i$ Succeeds ( $p_i$ )	$p_i p_j + \epsilon_{ij}$	$p_i(1 - p_j) - \epsilon_{ij}$	(5)
$i$ Fails ( $1 - p_i$ )	$(1 - p_i)p_j - \epsilon_{ij}$	$(1 - p_i)(1 - p_j) + \epsilon_{ij}$	

The case of  $\epsilon_{ij} \equiv 0$  is the case of independent returns considered by Ghatak. A positive (negative)  $\epsilon_{ij}$  gives positive (negative) correlation between borrower returns.

Correlation parameter  $\epsilon_{ij}$  may differ across pairs of borrowers  $\{i, j\}$ . We proceed by placing a simple structure on correlations which ensures that  $\epsilon_{ij} = \epsilon > 0$  for any two

borrowers facing the same types of risk, and  $\epsilon_{ij} = 0$  for all other pairings.

Assume there are two i.i.d. aggregate sources of uncertainty, or “shocks”,  $A$  and  $B$ . Each equals 1 or  $-1$  with equal probability. Every agent is assumed to be exposed to risk from either shock  $A$  or shock  $B$ , or neither ( $N$ ). Let  $s_i \in \mathcal{S} \equiv \{A, B, N\}$  denote agent  $i$ 's shock exposure-type. Shock exposure-type is known by all agents but not the lender.<sup>24</sup>

The probability of success of an agent with  $s_i = A$  and project risk parameter  $p_i$  equals  $p_i + \gamma A$ , for some  $\gamma > 0$ . That is, if there is a good shock ( $A = 1$ ), the agent's success probability is  $p_i + \gamma$ ; a bad shock ( $A = -1$ ) lowers the agent's success probability to  $p_i - \gamma$ . This agent's project outcome is independent of shock  $B$ . The success probability of an agent with  $s_i = B$  and project risk parameter  $p_i$  is exactly analogous:  $p_i + \gamma B$ , independent of  $A$ . The remaining agents, with  $s_i = N$ , succeed or fail independent of realizations of  $A$  and  $B$ .

With these assumptions, the  $\epsilon_{ij}$  of expression 5 varies across borrowers  $i$  and  $j$  in a straightforward way. Let  $\epsilon \equiv \gamma^2$  and  $\kappa_{i,j} = 1\{s_i = s_j = A \mid s_i = s_j = B\}$ . Then

$$\epsilon_{ij} = \kappa_{i,j}\epsilon .$$

In other words, returns are positively correlated for borrowers exposed to the same type of risk ( $\kappa_{i,j} = 1$ ), because probabilities of success are pushed in the same direction by the shock.<sup>25</sup> On the other hand, for borrowers not exposed to the same shock ( $\kappa_{i,j} = 0$ ),  $\epsilon_{ij} = 0$ , because the shocks each borrower is exposed to are independent.<sup>26</sup>

In summary, the correlation structure boils down to  $\epsilon_{ij} = \epsilon$  ( $\epsilon_{ij} = 0$ ) for pairs exposed (not exposed) to the same shock. The payoff of borrower  $i$  when matched with borrower  $j$  is now

$$\pi_{ij} = \bar{R} - rp_i - c[p_i(1 - p_j) - \epsilon\kappa_{i,j}] = \bar{R} - rp_i - cp_i(1 - p_j) + c\epsilon\kappa_{i,j} . \quad (6)$$

<sup>24</sup>In reality, the lender may have some clues, e.g. borrower occupation. One can interpret this assumption as applying to the unobserved aspects of risk exposure.

<sup>25</sup>With probability 1/2, the shock to which both are exposed is good and the probability of both succeeding is  $(p_i + \gamma)(p_j + \gamma)$ ; similarly, with probability 1/2 the probability of both succeeding is  $(p_i - \gamma)(p_j - \gamma)$ . The unconditional probability of both succeeding is thus  $p_i p_j + \gamma^2$ .

<sup>26</sup>Greater scope for diversification would be present if shocks  $A$  and  $B$  were negatively correlated, which could easily be incorporated without changing results.

The last term ( $c\epsilon$ ) represents a payoff boost from matching with a partner exposed to the same risk. This is because positive correlation of returns in the group lowers chances of having to bail out one's partner.

In this context, the following can be shown:

**Proposition 1.** *Assume a continuum of borrowers. In equilibrium, almost every group is homogeneous in both riskiness ( $p \in [\underline{p}, \bar{p}]$ ) and shock exposure-type ( $s \in \{A, B, N\}$ ).*

Thus, groups match homogeneously in riskiness ( $p_i$ ) and shock exposure-type ( $s_i$ ); they contain either all  $A$ -risk, all  $B$ -risk, or all  $N$ -risk borrowers. The intuition for shock exposure homogeneity is simple: borrowers choose to anti-diversify their groups so as to lower their chances of facing liability for their partners.

This result holds with a continuum of borrowers. In a finite population, unidimensionally-optimal matching along both dimensions simultaneously may not be feasible, in which case tradeoffs between the two dimensions of matching arise. Homogeneity in one dimension may be (partially) sacrificed to achieve it in the dimension with greater payoff salience. Nonetheless, payoff function complementarities will push toward homogeneity in both dimensions.<sup>27</sup>

Homogeneous borrower matching along the correlated risk dimension appears to work against efficient lending. In a finite population, it may divert borrowers from rank-ordered matching by riskiness – which is the basis for group lending's efficiency gains in this context. More importantly, correlated risk lowers the *effective* rate of joint liability. In the extreme case of perfect correlation, for example, the effective rate of joint liability is 0 regardless of how the bank sets  $c$ , since when one borrower fails, they both do. In general, the greater the correlation, the more irrelevant and blunted is any joint liability stipulation. This takes away from the lender a potentially valuable tool that can be used to increase lending efficiency, as shown in Section 3.1.<sup>28</sup> Thus, some dimensions of voluntary matching may not work in

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<sup>27</sup>As Fox (2010a,b) shows, with a sufficient amount of data on matches and borrower characteristics, and under a plausible assumption, complementarities in both dimensions can be identified, along with the relative strength of the two complementarities.

<sup>28</sup>See equation 4; the size of safe borrowers' implicit discount is proportional to the rate of joint liability.



favor of efficiency.

The point made in Section 3.2 again holds: other ways of implementing joint liability could lead to different matching patterns. For example, dynamic joint liability contracts involving the denial of future loans would reward formation of diversified groups; diversification would raise chances of partner bailouts that could extend the borrowing relationship. So, the empirical work will test for both diversification and anti-diversification, the latter being our focal hypothesis.

## 4 Data and Variable Descriptions

The empirical goal of the paper is to assess matching patterns of borrowing groups related to riskiness and types of risk exposure.

### 4.1 Data description and environment

The data come from the Townsend Thai survey effort. In May 1997, a cross section of 192 villages was surveyed, covering four provinces from two contrasting regions of Thailand, both with large agricultural sectors. In each village as many borrowing groups of the Bank for Agriculture and Agricultural Cooperatives (BAAC) as possible were interviewed, up to two. This baseline survey contains data on 262 groups, 200 of which are one of two groups representing their village. Unfortunately for the purposes of this study, the borrower-level data provided in this survey are minimal – they do not include risk variables – and they are all provided by the group’s official leader, not the individual borrowers.<sup>29</sup>

Hence, we turn to a resurvey, conducted in April and May 2000. The resurvey data were collected from a random subset of the same villages, stratified at the sub-district (tambon)

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The lender could in principle use a higher  $c$  to neutralize higher correlated risk. However, this is not possible since, as Gangopadhyay et al. (2005) argue,  $c$  is likely bounded above by  $r$  and is already optimally set at its upper bound when risk is uncorrelated.

<sup>29</sup>The concern is that when one person responds for each group member, measurement error can be highly correlated within the group. This would exaggerate differences across groups, and homogeneity of matching would be overestimated.

level. Included are data on 87 groups, 14 of which are the only groups in their village, 70 of which are one of two groups interviewed from the same village, and 3 of which are one of three groups interviewed from the same village.<sup>30</sup> Though observations are fewer, the resurvey data is preferable because individual group members respond to questions on their own behalf, up to five per group and on average 4.5; and because several resurvey questions were designed to measure income risk and correlatedness, the key variables in the theory. In total, we have 36 villages with multiple groups.

The BAAC is a government-operated development bank in Thailand. It was established in 1966 and is the primary formal financial institution serving rural households. It has estimated that it serves 4.88 million farm families, in a country that had just over sixty million inhabitants, about two thirds of which lived in rural areas. In the Townsend Thai baseline household survey covering the same villages, BAAC loans constitute 34.3% of the total number of loans, as compared with 3.4% for commercial banks, 12.8% for village-level financial institutions, and 39.4% for informal loans and reciprocal gifts (Kaboski and Townsend, 1998).

The BAAC allows smaller loans to be backed only with social collateral in the form of joint liability.<sup>31</sup> This kind of borrowing is widespread: of the nearly 3000 households in the baseline household survey, just over 20% had a group-guaranteed loan from the BAAC outstanding in the previous year. To borrow in this way, a borrower must belong to an official BAAC borrowing group and choose the group-guarantee option on the loan application. The group then faces explicit liability for the loan; that is, if a group member is delinquent on a loan, the BAAC may opt to follow up with the delinquent borrower *or* other group members in search of repayment. There can also be dynamic repercussions: some group members report delays or greater difficulties in getting future loans when a group member is in default.

Groups typically have between five and fifteen members; about 15% are larger. Typically,

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<sup>30</sup>This was apparently a mistake in implementation of the data collection methodology, which capped responses to two groups per village; we use the three-group village anyway.

<sup>31</sup>The cap on group loans at the time of the baseline survey was 50,000 Thai baht, about \$2000. The median group loan was closer to \$1000.

groups are born when borrowers propose a list of members to the BAAC, and the BAAC then approves some or all members. The BAAC seems to use its veto power sparingly: only about 12% of groups in the baseline survey report that the BAAC struck members from the list.<sup>32</sup> We know of no case where the BAAC adds members to a list or forms a group unilaterally. Thus, while the BAAC has some say in group formation, it appears that group formation is primarily at the discretion of the borrowers themselves.

## 4.2 Variable descriptions

The main empirical strategy involves comparing groups in the same village to determine whether within-group homogeneity is greater than random assignment of borrowers to groups would predict. To do so, measures of riskiness and of correlatedness are necessary. These are summarized in Table 1.

Our main measure of riskiness takes the theory (section 3.1) quite literally. Group members were asked what their income would be in the coming year if it were a good year ( $R_{Hi}$ ), what their income would be if it were a bad year ( $R_{Lo}$ ), and what they expected their income to be ( $\bar{R}$ ). Assuming that income can take only one of two values,  $R_{Hi}$  and  $R_{Lo}$ , and that  $\bar{R}$  represents the mean, the **probability of success**, or  $p$ , works out to be

$$p = \frac{\bar{R} - R_{Lo}}{R_{Hi} - R_{Lo}},$$

using the fact that  $p R_{Hi} + (1 - p) R_{Lo} = \bar{R}$ .<sup>33</sup> Another measure of risk, less directly related to the model, is the **coefficient of variation** of income.<sup>34</sup> Based on the same assumed income distribution, this works out to be

$$\frac{\sigma_R}{\bar{R}} = \sqrt{\frac{R_{Hi}}{\bar{R}} - 1} \sqrt{1 - \frac{R_{Lo}}{\bar{R}}},$$

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<sup>32</sup>This is in response to a free-form question about how the group's original members were determined.

<sup>33</sup>The measure described here is used by Ahlin and Townsend (2007b) in their finding of direct evidence for adverse selection in this credit market.

<sup>34</sup>The coefficient of variation equals the standard deviation normalized by the mean.

**Table 1 — Summary Statistics**

<b>Variable</b>	Mean	Median	Std Dev	Min	Max	Obs
<u>Riskiness</u>						
Probability of Success (Future Income)	0.426	0.400	0.253	0.000	1.000	338
Coefficient of Variation (Future Income)	0.449	0.400	0.287	0.000	1.388	313
<u>Type of Risk Exposure</u>						
Revenue from _____ as Pct of Total Revenue:						
Rice Farming	30.6%	18.5%	31.1%	0.0%	100.0%	390
Corn Farming	4.0%	0.0%	13.4%	0.0%	83.3%	390
Vegetable Farming	1.0%	0.0%	7.1%	0.0%	76.9%	390
Orchard Farming	1.7%	0.0%	10.1%	0.0%	99.8%	390
Other Crop	10.9%	0.0%	20.3%	0.0%	94.3%	390
Raising Shrimp	2.8%	0.0%	14.4%	0.0%	100.0%	390
Raising Fish	0.0%	0.0%	0.1%	0.0%	1.4%	390
Raising Chicken or Ducks	0.2%	0.0%	1.5%	0.0%	20.9%	390
Raising Pigs, Cows or Buffalo	4.4%	0.0%	13.6%	0.0%	76.5%	390
Raising other Livestock	0.1%	0.0%	1.6%	0.0%	31.6%	390
(All Farming)	55.6%	58.8%	33.9%	0.0%	100.0%	390
(Wages, Pensions, Remittances)	27.2%	14.9%	29.3%	0.0%	100.0%	390
(All Business)	9.5%	0.0%	23.1%	0.0%	100.0%	390
Worst Year for Income	[65.6% last yr, 16.6% yr before, 17.4% same]					390

which is simply the percentage deviation from expected income, averaged (geometrically) over good and bad outcomes.

Correlatedness is proxied in two ways. First, we use information on **occupation**, and more specifically, fraction of revenue coming from various agricultural occupations. Each borrower reports the amount of revenue received in more than thirty categories. Ten of the categories are agriculture-related – “rice farming”, “corn farming”, “raising shrimp”, “raising chicken or ducks”, etc. Our measure of occupation is a *vector* with ten entries, each giving the fraction of total household revenue accounted for by one agricultural category.

This measure is motivated by the setup of section 3.3, which features household exposure to various shocks. We generalize from that section’s binary measure of occupation/shock-exposure because some degree of within-household occupational diversification is common in our data. Occupational similarity between two borrowers is then measured as the dot product of their respective vectors; this gives the probability that a random dollar drawn from each borrower’s revenue comes from the same agricultural occupation. It also is the measure of correlatedness directly suggested by Section 3.3’s model.<sup>35</sup> We also choose to focus specifically on agricultural revenue components because they arguably entail more exposure to common shocks than the other revenue categories.<sup>36</sup> Further, the BAAC explicitly targets farmers, so agricultural revenue makes up the majority of total revenue (see Table 1).

Second, we use timing of bad income years, **worst\_year**. Specifically, borrowers are asked which year of the past two was worse for household income: “one year ago”, “two years ago”, or “neither”. If borrowers are exposed to the same aggregate shocks, bad income years are more likely to coincide; thus coincidence of bad years can proxy anti-diversification.

One could certainly envision more informative measures of riskiness and correlatedness

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<sup>35</sup>The dot product would give the  $\kappa_{i,j}$  of Section 3.3 if we had only two revenue categories and the corresponding vector entries had to be 0 or 1. One can generalize the theory of Section 3.3 to allow for more than two occupations and fractional identification with these occupations, in such a way that the dot product remains the appropriate measure of correlatedness.

<sup>36</sup>Prevalent non-agricultural revenue categories include wages, remittances, business, and investment income. It is not clear that two households both with wage or small business income, for example, face the same degree of correlatedness as two households both engaged in rice or shrimp farming.

than the ones available from this dataset. However, the first-order effect from any noisiness in these measures should be to make non-random matching harder to detect; if the measures are pure noise, for example, the matching pattern will be indistinguishable from random matching. Thus, our main measurement-error concern is that we are likely to be underestimating existence of systematic matching patterns.

## 5 Univariate Methodology and Results

In this section, we examine matching patterns one characteristic at a time. Section 6 extends the analysis to consider matching along multiple characteristics.

### 5.1 Univariate Methodology

Section 5.1.1 proposes a way to combine permutation testing with standard metrics to assess homogeneity of matching in a given village (matching market). Section 5.1.2 shows how to use these village-level measures in a nonparametric statistical test across all villages for homogeneous/heterogeneous matching. Section 5.1.3 proposes alternative, model-driven metrics of matching that can be combined with this nonparametric test to assess whether matching is driven by complementarity (PAM) or substitutability (NAM).

#### 5.1.1 Measuring homogeneity/heterogeneity of matching

Consider data on variable  $X$  from two groups  $L$  and  $M$  in village  $v$ , of respective sample sizes  $l$  and  $m$ :  $L = (x_1, \dots, x_l)$  and  $M = (x_{l+1}, \dots, x_{l+m})$ .

First, assume  $X$  is an ordered variable, e.g. riskiness. One way to measure homogeneity of matching is to calculate a *variance decomposition* of  $X = (x_1, \dots, x_{l+m})$  into between-group and within-group components. The between-group variance component is maximized in a rank-ordered grouping, so a larger between-group component can be taken as greater evidence for homogeneous matching. To illustrate, consider a village with 2 groups of size

4, with success probabilities<sup>37</sup>  $X = (1, 2, 4, 5, 6, 7, 8, 9)$ . Compare the borrower grouping  $L = (2, 5, 6, 8)$  and  $M = (1, 4, 7, 9)$  with the grouping  $L' = (1, 2, 5, 6)$  and  $M' = (4, 7, 8, 9)$ . The first grouping has a between-group variance component of 0%, while the second grouping has a between-group component of 44%. The higher value reflects the more homogeneous matching of the second grouping – “close” to rank-ordering – while the lower value reflects the more mixed first grouping – equal means, and thus “far” from rank-ordering.

An alternative metric that uses only the data’s ordinality is a *rank correlation*. In particular, one can calculate Kendall’s tau<sub>b</sub><sup>38</sup> between the data  $X$  and a group index  $Y$ , where for example  $y_1 = \dots = y_l = 1$  and  $y_{l+1} = \dots = y_{l+m} = 2$ . Using the groupings of the previous example, Kendall’s tau<sub>b</sub> between  $(2, 5, 6, 8, 1, 4, 7, 9)$  and group index  $(1, 1, 1, 1, 2, 2, 2, 2)$  is 0%, and between  $(1, 2, 5, 6, 4, 7, 8, 9)$  and the same group index is 57%.<sup>39</sup> Again, the higher value reflects more homogeneous matching, and the lower value more mixing; Kendall’s tau<sub>b</sub> is maximized under rank-ordering.

An appropriate metric for non-ordered, categorical variables, e.g. occupation and worst\_year, is the *chi-squared* independence (or homogeneity) *test statistic*.<sup>40</sup> This statistic quantifies deviations from the grouping in which each group has the same proportion of responses in each category as the village population – thus it is maximized under group homogeneity, and minimized (at 0) under an equal distribution of types across groups. For example, letting  $A$  and  $B$  be two occupations (shocks), compare the following grouping:  $L = (A, A, B, B)$  and  $M = (A, A, B, B)$ , with an alternative:  $L' = (A, A, A, B)$  and  $M' = (A, B, B, B)$ . The chi-squared test statistic for the first grouping is 0 and for the second grouping is 2.

We thus have three homogeneity metrics, two for ordered variables and one for categorical variables. To move toward a statistical test for homogeneous matching, we use permutation

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<sup>37</sup>For brevity, all probabilities are multiplied by 10, the true data being (0.1, 0.2, 0.4, 0.5, 0.6, 0.7, 0.8, 0.9).

<sup>38</sup>Results using Spearman’s rho end up nearly identical, so we do not report them. Formulas for Kendall’s tau<sub>b</sub> and Spearman’s rho can be found in Gibbons and Chakraborti (2003, pp. 419-20, 422-3).

<sup>39</sup>The correlations would be the same but negative if the group indices were reversed, i.e. if we used group index vector  $(2, 2, 2, 2, 1, 1, 1, 1)$ . Since group index is arbitrary, we take the absolute value of the rank correlation (more generally, the maximum across all potential group indexings).

<sup>40</sup>The formula and discussion can be found in DeGroot (1986, pp. 536-7, 542-3).

testing to scale each metric.

Specifically, consider again observed data  $X = (x_1, \dots, x_{l+m})$  from two groups in village  $v$ ,  $L$  and  $M$ , of respective sizes  $l$  and  $m$ . We assume that the relevant matching market for group formation is the village – a reasonable assumption since villages are relatively small and geographically concentrated. Hence, we form all possible combinations of the  $l + m$  borrowers into two groups of respective sizes  $l$  and  $m$  and perform the same calculation – variance decomposition, rank correlation, and/or chi-squared statistic – on each one. The *observed* village grouping can then be assigned a “homogeneity percentile” based on where its calculated value falls relative to this universe of possibilities – or homogeneity percentile *range*, given ties and a finite population. That is, the permutation test scales the raw homogeneity score for each village into a value (or range) in  $[0, 1]$ , with higher numbers representing more homogeneous matching and lower numbers representing more heterogeneous matching. This permutation scaling is applied for each variable and each metric used.

To illustrate, consider again a village with 2 groups of size 4, with success probabilities  $X = (1, 2, 4, 5, 6, 7, 8, 9)$ . There are  $\binom{8}{4}/2 = 35$  groupings of these eight borrowers into two groups of size four. Compared to the grouping  $L = (2, 5, 6, 8)$  and  $M = (1, 4, 7, 9)$ , 32 groupings register higher between-group variance while 3 (including the grouping itself) register exactly the same (zero) between-group variance. Thus this grouping is somewhere between the 0th and 8.6th percentiles in terms of group homogeneity; its homogeneity percentile range is  $[0, 8.6]$ . The somewhat wide range reflects the ties and the relatively small number of groupings. Compared to the grouping  $L' = (1, 2, 5, 6)$  and  $M' = (4, 7, 8, 9)$ , 31 groupings have lower, 2 have the same, and 2 have higher between-group variance. This grouping’s homogeneity percentile range is thus  $[88.6, 94.3]$ . Similarly, applying this permutation test to the Kendall’s tau<sub>b</sub> rank correlation metric gives a slightly wider homogeneity percentile range to the first grouping,  $[0, 11.4]$ , and the same homogeneity percentile range to the second grouping,  $[88.6, 94.3]$ .

The same approach can be used with the chi-squared test statistic. There are 17 com-



binations with a larger chi-squared test statistic and 18 combinations tied with grouping  $L = (A, A, B, B)$  and  $M = (A, A, B, B)$ . This grouping's homogeneity percentile range is then  $[0, 51.4]$ . Compared to grouping  $L' = (A, A, A, B)$  and  $M' = (A, B, B, B)$ , 18 combinations have less, 1 combination has greater, and 16 combinations have the same chi-squared test statistic. This grouping's homogeneity percentile range is  $[51.4, 97.1]$ .

Thus for a given variable and homogeneity metric, each village is assigned a homogeneity percentile range. A higher homogeneity percentile range reflects more homogeneous matching, according to the metric used, while a lower homogeneity percentile reflects more heterogeneous matching. The critical role of the permutation test is to put into context the observed degree of matching homogeneity, relative to all possibilities given the amount of borrower heterogeneity that exists in the village.

### 5.1.2 A Nonparametric Test

We next combine villages in a single test (per variable and homogeneity metric) of the overall tendency to match homogeneously. Each village's homogeneity percentile is treated as a draw from the same distribution, and this distribution is compared using the Kolmogorov-Smirnov test to a benchmark distribution corresponding to a null hypothesis. An advantage of this approach is that it is non-parametric and requires no distributional assumptions.

The null hypothesis of the test is that matching is random with respect to the given variable. The rationale is the same as the one underpinning use of the t-statistic in a linear regression; in both cases, the null hypothesis is that the variable has no explanatory power.

We claim that the distribution of village homogeneity percentiles that random matching generates is the uniform on  $[0, 1]$ . The idea is as follows. Consider the case of a large number of borrowers in a village, no two groupings of which result in a tie using the given homogeneity metric. If each of the  $N$ , say, possible groupings is equally likely, as it is under random matching, then each  $1/N$ th homogeneity percentile is equally likely to be realized by a given village. That is, a village's homogeneity percentile is drawn from the uniform

distribution – approximately, with the difference getting arbitrarily small as  $N$  increases.

With smaller numbers of borrowers and ties, villages are assigned non-negligibly wide homogeneity percentile *ranges*, not homogeneity percentiles. The approach is then to draw a village’s homogeneity percentile randomly from the village’s homogeneity percentile range via the uniform distribution.

To summarize, let a village’s homogeneity percentile range be calculated by the permutation methods described in the previous section; and let its homogeneity percentile (point estimate) be drawn at random from the uniform distribution over its homogeneity percentile range. Then the exact distribution of a village’s homogeneity percentile under random matching, regardless of the homogeneity metric, is the uniform distribution on  $[0, 1]$ .

**Proposition 2.** *Under random matching, a village’s homogeneity percentile  $z$  is drawn from the uniform distribution on  $[0, 1]$ .*

The test then constructs a sample CDF from the *observed* village homogeneity percentiles, and compares it using the Kolmogorov-Smirnov (KS) test to the uniform distribution. If the sample CDF stochastically dominates the uniform, this means villages’ homogeneity percentiles tend to be higher than random matching would give rise to and provides statistical evidence for homogeneous matching. On the other hand, if the sample CDF is stochastically dominated by the uniform, this means villages’ homogeneity percentiles tend to be lower than what random matching would produce, suggesting heterogeneous matching.

We can thus report p-values for these KS one-sided tests of stochastic dominance. Note that one such p-value involves a set of random choices: the random draws that select villages’ homogeneity percentiles from their homogeneity percentile ranges. Thus, even given the data, the p-value is a random variable. So, we repeat the test 1 million times under 1 million different sets of random draws, and report the average p-value across all draws.

### 5.1.3 The test with structural matching measures

The homogeneity metrics discussed so far – variance decomposition, rank correlation, and chi-squared statistic – have the advantage of being standard metrics that clearly quantify homogeneous matching. They also connect to the theory well in the sense that they are maximized under the matching pattern predicted by the theory. However, if the baseline theory held perfectly and there were no measurement error or matching on unobservables, *any* grouping other than the maximally homogeneous one would be evidence against the theory. On that basis, one could question whether matching that is more homogeneous than random but not perfectly homogeneous is really evidence in favor of the theory.

A typical approach to this issue is to assume that matching is occurring on unobservables as well;<sup>41</sup> this would allow “moderately” homogeneous matches to be both observable in equilibrium and compatible with maximal homogeneity as the unique outcome if matching were based only on observables. Two ways of implementing this approach empirically are to assume some structure on the unobservables (e.g. Choo and Siow, 2006), or to directly assume that matchings that produce higher observable surplus are more likely to be observed when matching is based both on observables and unobservables (Fox, 2010a,b).

Our approach thus far has been similar in spirit to the Fox approach, but with the implicit assumption that matchings that produce higher observable *homogeneity*, rather than higher observable *surplus*, are more likely to be observed. In this section, we follow more closely the Fox approach by focusing on match surplus. The advantage of using observable surplus rather than observable homogeneity is that this assumption is more closely aligned with the theory,<sup>42</sup> and in fact can be justified from model primitives in some settings (Fox, 2010a,b).<sup>43</sup> Ultimately, this method allows identification of complementarity vis a vis substitutability of

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<sup>41</sup>See Chiappori and Salanie (forthcoming) and its references.

<sup>42</sup>The theory predicts that any observed grouping must maximize the sum of group payoffs, i.e. total surplus, for if higher surplus were available in a different grouping, then there would exist at least one group in this alternative grouping whose members could all do better, contradicting equilibrium.

<sup>43</sup>Related, it has been shown that equilibrium *negative* assortative matching patterns can appear statistically *homogeneous*, even as other non-equilibrium matching patterns appear statistically heterogeneous (Ahlin, 2015a). This implies that the homogeneity spectrum is not identical to the group surplus spectrum.

the group payoff function (Fox, 2010b). And since complementarity is the basis for PAM, this uncovers the fundamental interactions driving the matching pattern.

To proceed, it is necessary to specify the form of the surplus function, i.e. the sum of group payoffs. This is taken directly from the theory. Consider first the baseline model with uncorrelated risk and observed groups  $L = \{i, j\}$  and  $M = \{i', j'\}$  in a village. Let group payoff functions be  $\Pi_L$  and  $\Pi_M$ , where  $\Pi_L = \pi_{ij} + \pi_{ji}$  and  $\Pi_M = \pi_{i'j'} + \pi_{j'i'}$ . Then  $\Pi_L + \Pi_M$  is the total surplus. Using equation 1,

$$\Pi_L + \Pi_M = 4\bar{R} - (r + c)(p_i + p_j + p_{i'} + p_{j'}) + c(p_i p_j + p_j p_i + p_{i'} p_{j'} + p_{j'} p_{i'}) .$$

Note that only the interaction terms (the last parenthetical) may differ across groupings of the four borrowers. Hence, given our ultimate purpose of comparing  $\Pi_L + \Pi_M$  against alternative groupings of the same set of borrowers, we can ignore all but these terms. Further, since  $c > 0$ , it can be ignored in these comparisons. Letting  $p_{-k}$  be the success probability of borrower  $k$ 's partner, these terms can be written

$$\sum_{k \in L} p_k p_{-k} + \sum_{k \in M} p_k p_{-k} . \quad (7)$$

Taking the theory to data is complicated by the fact that the borrowing groups in the data are not pairs, but typically involve 5-15 members. Further, we do not typically have the entire group's data, primarily because a maximum of five group members are sampled. Our strategy will be to proxy for  $p_{-k}$  in expression 7 using the average success probability of the other sampled group members.

Specifically, let group  $G$  be a set of grouped borrowers,  $S^G$  be the sampled subset of group  $G$ , and  $\bar{p}_{-k}^{S^G}$  be the average success probability in the sampled subset of group  $G$  excluding borrower  $k$ . The following is our sample estimate of the relevant part of the

surplus (expression 7):

$$\sum_{k \in \mathcal{S}^L} p_k \bar{p}_{-k}^{S^L} + \sum_{k \in \mathcal{S}^M} p_k \bar{p}_{-k}^{S^M}. \quad (8)$$

This estimate is simply the sum, over all sampled village borrowers, of the borrower's success probability multiplied by the average success probability of other same-group, sampled borrowers.<sup>44,45</sup> This can be directly calculated from the data using the success probability variable (see Section 4.2).

Consider also the contract under correlated risk. Let  $\kappa_{k,-k}$  be the indicator for whether borrower  $k$  shares the same risk exposure-type as his partner. Using payoff function 6 gives

$$\begin{aligned} \Pi_L + \Pi_M = & 4\bar{R} - (r + c)(p_i + p_j + p_{i'} + p_{j'}) \\ & + c \left( \sum_{k \in L} p_k p_{-k} + \sum_{k \in M} p_k p_{-k} \right) + c\epsilon \left( \sum_{k \in L} \kappa_{k,-k} + \sum_{k \in M} \kappa_{k,-k} \right). \end{aligned} \quad (9)$$

There are two types of interaction terms in this expression for surplus, involving riskiness and risk exposure-type (the first and second parenthesis on the second line, respectively). To test for (anti-)diversification using the univariate techniques of this section, only the interactions involving risk exposure-type are used.<sup>46</sup> Following the above techniques and defining  $\bar{\kappa}_{k,-k}^{S^G}$  as the average correlatedness dummy of borrower  $k$  in group  $G$  with other sampled group  $G$  members, our estimator for the part of surplus due to correlated risk is

$$\sum_{k \in \mathcal{S}^L} \bar{\kappa}_{k,-k}^{S^L} + \sum_{k \in \mathcal{S}^M} \bar{\kappa}_{k,-k}^{S^M}.$$

This estimate is simply the sum, over all sampled village borrowers, of the fraction of other

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<sup>44</sup>This form of the payoff function can be justified by the linear  $n$ -person group contract suggested in Ghatak (1999), in which each borrower who succeeds owes  $c$  per fellow unsuccessful borrower.

<sup>45</sup>To illustrate, sampled grouping  $L = (0.2, 0.5, 0.6, 0.8)$  and  $M = (0.1, 0.4, 0.7, 0.9)$  has sum of group payoffs of 2.02 – i.e.  $0.2 * 0.633 + 0.5 * 0.533 + \dots + 0.7 * 0.467 + 0.9 * 0.4$  – compared to 2.35 for more homogeneous grouping  $L' = (0.1, 0.2, 0.5, 0.6)$  and  $M' = (0.4, 0.7, 0.8, 0.9)$ .

<sup>46</sup>That is, we examine each separately in this univariate analysis, matching on riskiness (see equation 8) and (anti-)diversification (see below). Testing both together based on the entire payoff function, which requires dealing with unknown parameter  $\epsilon$ , is reserved for Section 6.

same-group, sampled borrowers exposed to the same risk.<sup>47</sup>

Comparing the observed grouping’s surplus with those of all unobserved groupings of the same borrowers into similarly-sized groups – i.e., the same permutation test as before – generates a “complementarity” percentile (range). We call this a complementarity percentile because it quantifies how well the observed grouping maximizes the (observable) surplus function, whose key feature is complementarity. A low complementarity percentile is evidence of substitutability of types in the surplus function; this is because the observed grouping comes closer to minimizing the surplus function based on complementarity, which is equivalent to maximizing the surplus function based on substitutability.<sup>48</sup>

The same logic as before gives that random matching would give rise to a uniform distribution of complementarity percentiles. Thus, the procedure is as before: use permutation tests to calculate complementarity percentile ranges for each village’s observed grouping, then use the KS test to compare the sample CDFs of village complementarity percentiles to the uniform distribution.

The remaining question in this approach is how to use the data to proxy for  $\kappa_{i,j}$ , the indicator for being exposed to the same risk. In the case of `worst_year`,  $\kappa_{i,j}$  is simply proxied by  $1\{\text{worst\_year}_i^G = \text{worst\_year}_j^G\}$ ; that is, iff the two borrowers give the same answer in identifying the worst year, they are considered exposed to the same risk. In the case of occupation, a vector with the fraction of total revenue coming from each of ten agricultural sectors,  $\kappa_{i,j}$  is measured as the dot product of the borrower’s vectors; see section 4.2 for explanation.

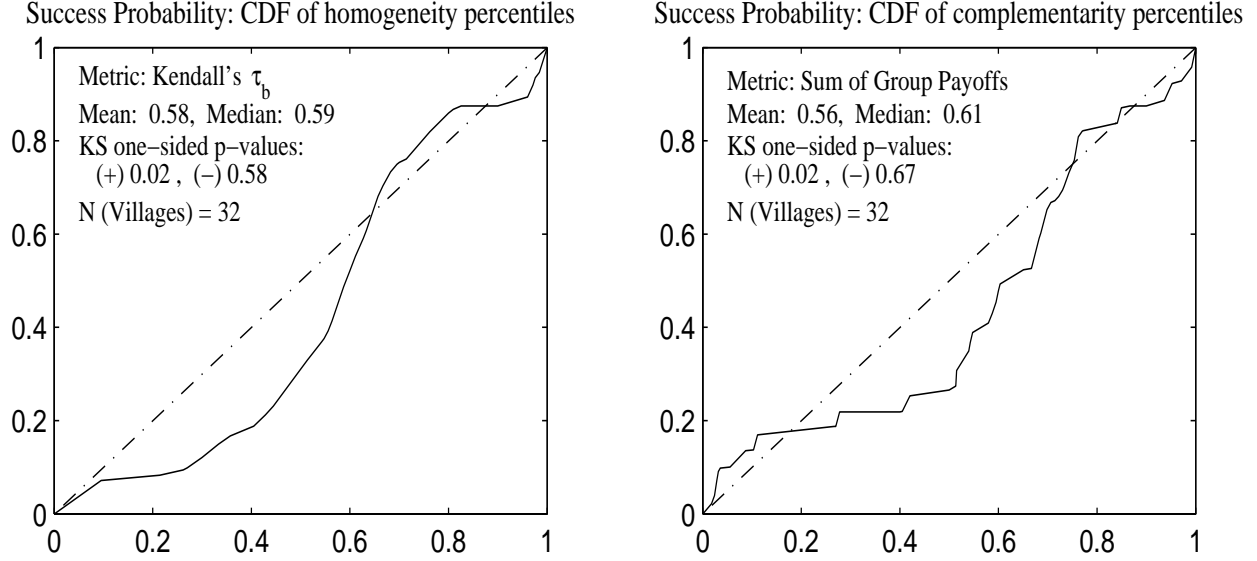


Figure 1: **Probability of success.** Dashed Lines: Uniform CDF. Solid Lines: Sample CDFs of villages' homogeneity percentiles based on rank correlation Kendall's  $\tau_b$  (left panel) and of villages' complementarity percentiles based on the matching surplus function (right panel).

## 5.2 Univariate Results

**Sorting by riskiness.** The **success probability**, or  $p$ , is the focus of our empirical tests for homogeneous matching by riskiness. The sample CDF of village homogeneity percentiles based on Kendall's  $\tau_b$  is graphed in the left panel of Figure 1.<sup>49</sup> Based on this rank correlation, the mean (median) village is more homogeneously matched than 58% (59%) of all possible combinations of borrowers into groups of the observed sizes. The random-matching benchmark, the uniform, is graphed as a dashed line. Using a one-sided KS test, we reject at the 5% level the hypothesis of heterogeneous matching, that is, that the true

<sup>47</sup>For example, compare grouping  $L = (A, A, B, B)$  and  $M = (A, A, B, B)$  with grouping  $L' = (A, A, A, B)$  and  $M' = (A, B, B, B)$ . The correlation-related payoffs sum to 2.67 in the first grouping and to 4 in the second, more anti-diversified grouping. In the first grouping, for example, 1/3 of each borrower's fellow group members are exposed to the same shock; summing 1/3 across 8 borrowers gives 2.67.

<sup>48</sup>With riskiness, note that if  $c < 0$ , types are substitutes in the surplus function and the equilibrium grouping would minimize the expression derived. With risk exposure-type, we are abusing complementarity/substitutability terminology a bit since the payoff function is not differentiable in borrower types; but the idea is similar in that low complementarity percentiles would imply higher payoffs to diversification.

<sup>49</sup>For this and all graphs, the reported p-values are averages over 1 million KS p-values based on random draws from each village's percentile ranges. The sample CDFs graphed are essentially averages over an infinite number of sample CDFs constructed based on these random draws; equivalently, they incorporate the percentile range of each village directly. Means and medians are computed using these sample CDFs.

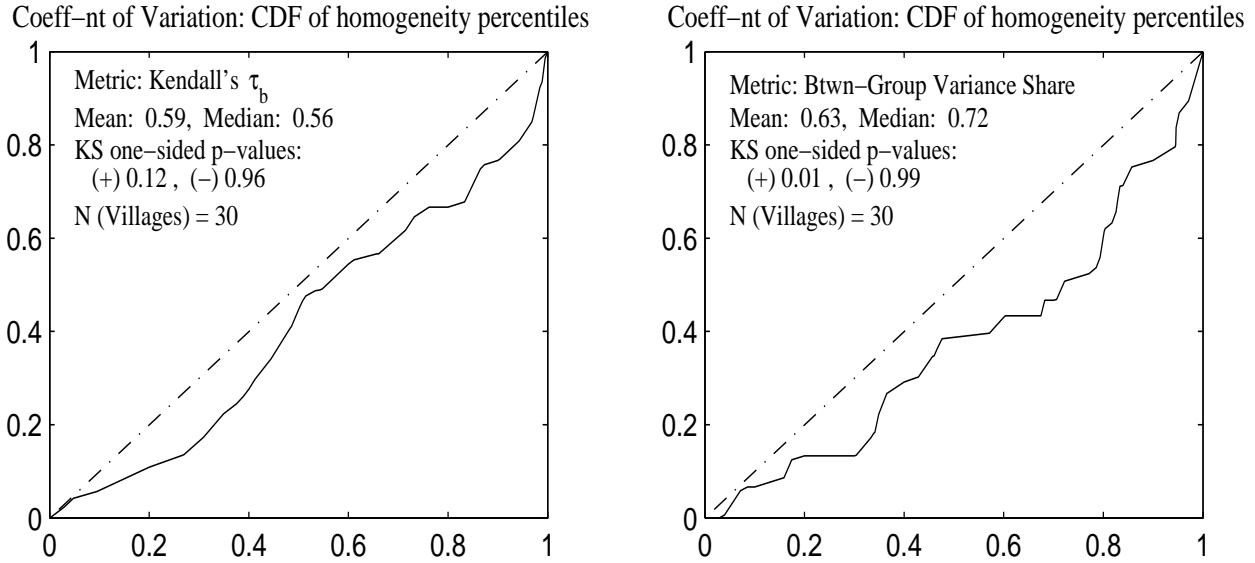


Figure 2: **Coefficient of Variation** for income (standard deviation/mean). Dashed Lines: Uniform CDF. Solid Lines: Sample CDFs of villages' homogeneity percentiles based on rank correlation Kendall's  $\tau_b$  (left panel) and based on variance decomposition (right panel).

distribution of village homogeneity percentiles is first-order stochastically dominated by the uniform.<sup>50</sup> These results point to matching by riskiness that, while not rank-ordered, is statistically distinguishable from random matching in the direction of homogeneity.

The sample CDF of village complementarity percentiles based on the surplus function is graphed in the right panel of Figure 1. The results are quite similar. The mean (median) complementarity percentile is 56% (61%) and substitutability is rejected at the 5% level.

A second measure of riskiness is the **coefficient of variation** of projected income. This measure is a proxy and cannot be substituted directly into the surplus function, making the structural approach infeasible. Hence, the sample CDFs of village homogeneity percentile ranges for the coefficient of variation based on Kendall's  $\tau_b$  and variance decomposition are graphed in Figure 2. Here, the variance decomposition gives strong evidence of homogeneous matching: the mean (median) village is more homogeneously matched than 63% (72%) of all possible borrower groupings, and heterogeneous matching is rejected at the 5% level.

<sup>50</sup>Results using the variance decomposition homogeneity metric are similar: mean (median) of 57% (62%), and KS one-sided (+) p-value of 0.01.



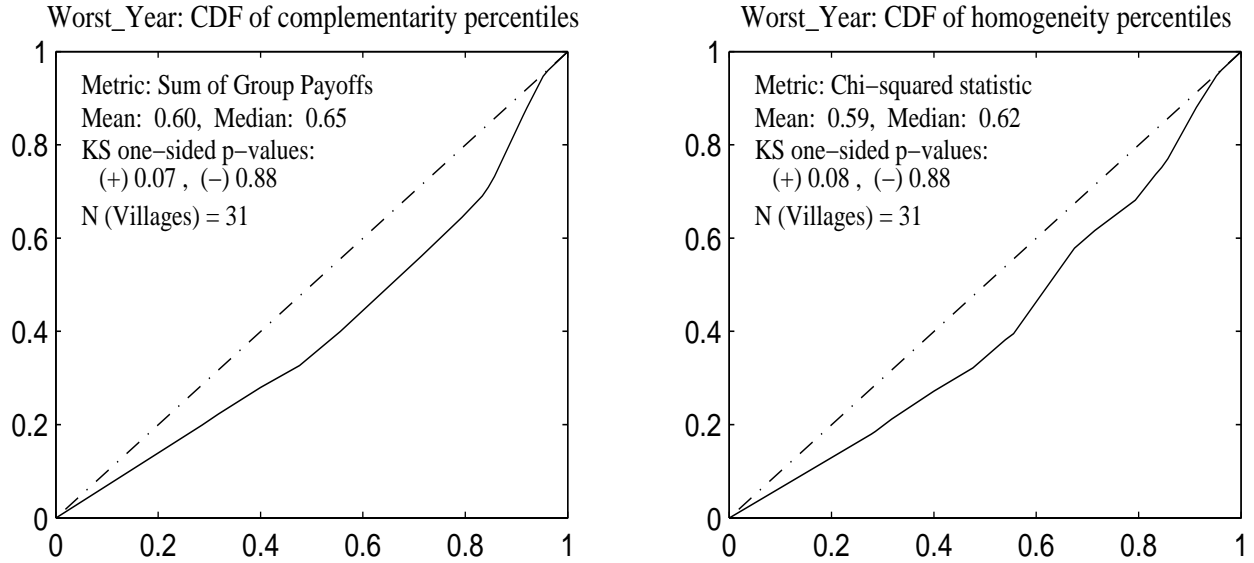


Figure 3: **Worst\_Year** for income. Dashed Lines: Uniform CDF. Solid Lines: Sample CDFs of villages' complementarity percentiles based on the matching surplus function (left panel) and of villages' homogeneity percentiles based on the chi-squared statistic (right panel).

The rank correlation gives a bit less evidence for homogeneous matching by coefficient of variation: the means and medians drop to 59% and 56%, respectively, and the KS tests come somewhat close but fail to reject heterogeneous matching at the 10% level.

Overall, the data give solid evidence for a non-negligible degree of homogeneous (and complementarity-based) matching by riskiness, and are typically able to reject heterogeneous (substitutability-based) matching. From the standpoint of the theory of section 3, this suggests that safe borrowers are indeed receiving lower implicit borrowing rates because they tend to have safer partners.

**Sorting by risk exposure-type.** We next examine diversification within groups. Since **worst\_year** is a categorical variable, Figure 3 reports results using the chi-squared test statistic and the matching surplus metric. Using the surplus metric, the average (median) village is more anti-diversified than 60% (65%) of possible groupings; using the chi-squared metric, the average (median) village is more anti-diversified than 59% (62%) of possible groupings. In both cases, diversification is rejected by the KS test at the 10% level.

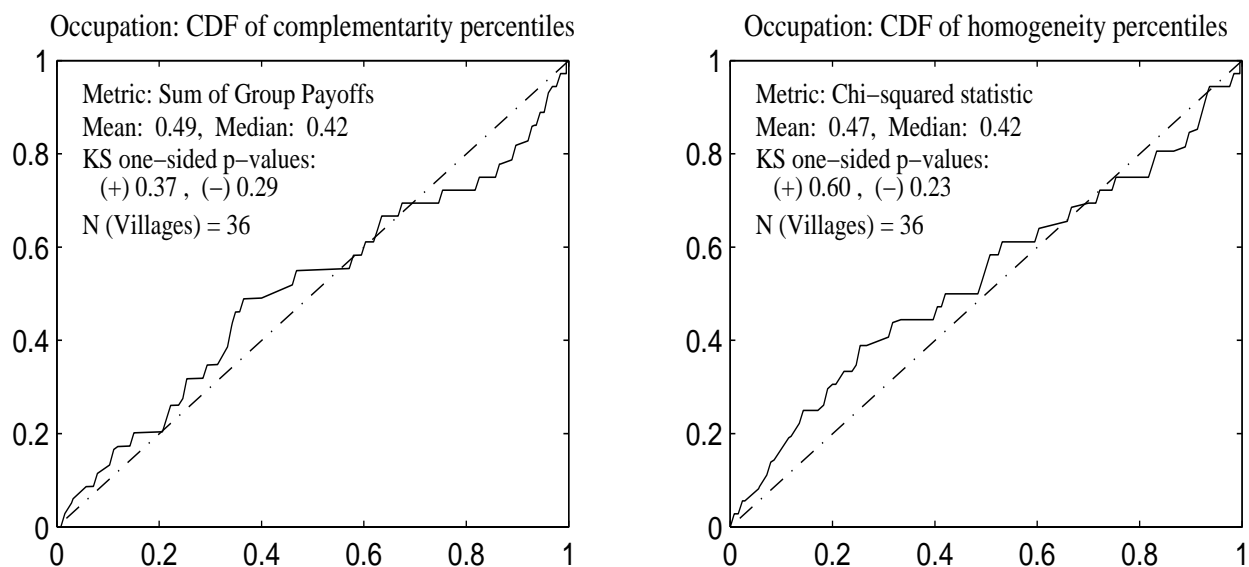


Figure 4: Agricultural **Occupation**. Dashed Lines: Uniform CDF. Solid Lines: Sample CDFs of villages' complementarity percentiles based on the matching surplus function (left panel) and of villages' homogeneity percentiles based on the chi-squared statistic (right panel).

Regarding **occupational** diversification, results using the chi-squared and the matching surplus metrics are graphed in Figure 4. Interestingly, they suggest that matching is not too different from random based on agricultural occupation. The means and medians are in the 40%'s, and the KS p-values are lower in the test against anti-diversification; however, neither diversification nor anti-diversification can be rejected at better than a 20% significance level.

Interestingly, the results for `worst_year` suggest that borrowers have incomes that are somewhat anti-diversified along group lines, while the results for occupation suggest that this anti-diversification does not take the form of (agricultural-)occupational homogeneity. A potential interpretation is that the lender encourages diversification within groups along observable dimensions, including by agricultural occupation, but that the borrowers are able to achieve some anti-diversification by exploiting other, unobserved traits. This suggests that anti-diversification is occurring, and partially undoing the risk-pricing improvements, but perhaps not to the degree it would be if matching were more homogeneous by occupation.

### 5.3 Discussion of Univariate Results

The univariate tests suggest that group composition is homogeneous along both riskiness and risk exposure-type dimensions – not perfectly homogeneous, but more so than under random matching. Of course, the evidence is not proof of causality running from riskiness or exposure-type to matching behavior. For example, it may be that friends or relatives group together, friends or relatives that are alike in certain regards, including along risk dimensions. Or, perhaps monitoring is easier within a group of similarly-occupied individuals, who by nature of their occupation face similar amounts and types of risk (though the lack of observed occupational homogeneity casts doubt on this particular story).

However, if the goal is to assess whether the Ghatak model is an empirically plausible explanation of group lending’s popularity and ability to revive credit markets, these simple univariate results are in some ways preferable to alternatives. The reason is that the safe-borrower discount embedded in lending to homogeneous groups exists *regardless of how* groups end up homogeneous by riskiness. Borrowers may have consciously considered the risk of their partners in forming groups, or they may have simply formed groups with friends or relatives who happened to have similar risk characteristics; either way, safe borrowers end up with safer partners. Given this homogeneous matching by riskiness, the joint liability stipulation is less onerous for safe borrowers, and they get an implicit discount in their borrowing rate. It is this discount that allows group lending to draw more borrowers into the market. The point is that, in this framework, matching that is homogeneous by riskiness – by whatever mechanism – is all that is needed for group lending to offer an improvement in contracting.

Thus, testing directly the degree of risk homogeneity is arguably the most appropriate approach to testing the main idea of the Ghatak model. Conversely, rejecting Ghatak’s main idea based on causally identifying, e.g., kinship and not riskiness as the key matching determinant would appear to be misguided, if the evidence pointed to risk-homogeneous groups (as it does here). Similarly, rejecting homogeneous risk-matching based on a zero

coefficient in a multivariate regression does not necessarily reject the main idea of the Ghatak model if the coefficient is positive in a univariate regression.

A similar argument can be made about the extended model that incorporates correlated risk. If there is unconditional evidence for anti-diversification of risk, then that is enough to raise the concern that some of the contractually stipulated joint liability is being undone – whether or not the anti-diversification is a conscious choice on the part of borrowers.

Thus, we believe the results of this section are directly informative about the ability and limitations of the theory to explain the rise of group lending and microcredit.

One shortcoming of these results is that they cannot differentiate between homogeneous matching and group conformity, in which groups gravitate toward similar risk choices *because* they are grouped together. Ideal to distinguish these two stories would be risk data that pre-dates group formation, which we unfortunately lack. This issue must be left for future work.<sup>51,52</sup>

## 6 Multivariate Methodology and Results

The univariate results are consistent with both dimensions of risk – riskiness and type of risk exposure – being important for matching. One might wonder, though, if one dimension of homogeneity is driving the other. Hence, we turn next to a multivariate approach that allows both dimensions of risk simultaneously to affect payoffs and matching behavior.

We use the matching maximum score estimator of Fox (2010a) to estimate parameters of the model surplus function that determine whether it displays complementarity or substitutability, along both dimensions.<sup>53</sup> The estimator works by choosing parameters that most

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<sup>51</sup>Barr et al. (2015) analyze matching into community-based organizations (CBOs) using pre-match data.

<sup>52</sup>One might also wonder whether the appearance of more correlated risk within groups than random matching would predict is a mechanical consequence of the joint liability contract, which can make one borrower’s bad year a bad year for others who are liable. This is unlikely because the question used asks about worst year for income, which is likely to be interpreted by the respondents as not including transfers. Indeed, when asked for the reason for the bad year, about 85% of responses are agricultural shock-related – prices, weather, or pests. This is by design; the following question asks about how the household responded to the bad income year, and here is where many of the responses have to do with transfers.

<sup>53</sup>A reduced-form estimation that included more controls than we use could also be interesting. However,

frequently give observed agent groupings higher joint surplus (sum of payoffs) than feasible, unobserved agent groupings. It has been shown consistent in an environment with many matching markets (as in our setting), assuming that groupings that give higher observable surplus are more likely to be observed.

Consider observed groups  $L$  and  $M$  in village  $v$ . Let  $\tilde{L}$  and  $\tilde{M}$  denote an alternative arrangement of the borrowers from  $L$  and  $M$  into two groups of the original sizes. As in section 5.1.1, we assume that borrowers can match with any others in their village; thus  $\tilde{L}$  and  $\tilde{M}$  represent a feasible, unobserved grouping. If  $\Pi_G(\phi)$  gives the sum of payoffs of any group  $G$  as a function of parameters  $\phi$ , theory predicts

$$\Pi_L(\phi) + \Pi_M(\phi) \geq \Pi_{\tilde{L}}(\phi) + \Pi_{\tilde{M}}(\phi). \quad (10)$$

The matching maximum score estimator chooses parameters  $\phi$  that maximize the score, i.e. the number of inequalities of the form 10 that are true, where each inequality corresponds to a different unobserved grouping  $\tilde{L}, \tilde{M}$ .

Our set of unobserved groupings, and thus inequalities, comes from all  $k$ -for- $k$  borrower swaps across two groups in the same village.<sup>54</sup> For example, if we have data on five borrowers in each of two groups in the same village, there are  $5 \times 5 = 25$  one-for-one swaps,  $10 \times 10 = 100$  two-for-two swaps, and so on.

Consider the model's expression for the surplus  $\Pi_L + \Pi_M$  from section 3.3, reproduced from equation 9 here:

$$\begin{aligned} \Pi_L + \Pi_M = & 4\bar{R} - (r + c)(p_i + p_j + p_{i'} + p_{j'}) \\ & + c \left( \sum_{k \in L} p_k p_{-k} + \sum_{k \in M} p_k p_{-k} \right) + c\epsilon \left( \sum_{k \in L} \kappa_{k,-k} + \sum_{k \in M} \kappa_{k,-k} \right). \end{aligned}$$

Note that all terms in the group payoff function that do not involve interactions between

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this is not as attractive in part because our dataset lacks data on social networks.

<sup>54</sup>If the larger group in a village has sample size  $m$  and the smaller group has sample size  $n$ ,  $k$  is capped at  $\min\{n, m - 1\}$ .

borrower characteristics drop out of inequality 10, since they appear identically on both sides;<sup>55</sup> hence, we can ignore the non-interaction terms.

We proceed as in section 5.1.3. Since groups contain more than 2 members and since our data contain a subset of each group (up to 5 members), we use a sample analog expression for the payoff function. Again, let  $G$  be defined as a set of grouped borrowers,  $\mathcal{S}^G$  as the sampled subset of group  $G$ ,  $k$  as a sampled group- $G$  borrower,  $\bar{p}_{-k}^{\mathcal{S}^G}$  as the average success probability in the sampled subset of group  $G$  excluding borrower  $k$ , and  $\bar{\kappa}_{k,-k}^{\mathcal{S}^G}$  as the average correlatedness dummy of borrower  $k$  with other sampled group- $G$  borrowers. Then, the sample analog to the (relevant part of the) payoff function is

$$\Pi_L + \Pi_M = c \left( \sum_{k \in \mathcal{S}^L} p_k \bar{p}_{-k}^{\mathcal{S}^L} + \sum_{k \in \mathcal{S}^M} p_k \bar{p}_{-k}^{\mathcal{S}^M} \right) + c\epsilon \left( \sum_{k \in \mathcal{S}^L} \bar{\kappa}_{k,-k}^{\mathcal{S}^L} + \sum_{k \in \mathcal{S}^M} \bar{\kappa}_{k,-k}^{\mathcal{S}^M} \right). \quad (11)$$

It is this expression that we use for group payoffs in inequality 10.<sup>56</sup>

Given data on borrower probabilities of success ( $p_i^G$ 's) and correlatedness ( $\kappa_{i,j}$ 's), the parameters  $c$  and  $\tilde{\beta} \equiv c\epsilon$  can be estimated, but only up to scale, since multiplication by any positive scalar would preserve the inequality. Note that  $\epsilon$  would be identified as  $\tilde{\beta}/c$ . This approach, however, requires data that can capture the existence of correlation ( $\kappa_{i,j}$ ) as distinct from the intensiveness ( $\epsilon$ ) of correlation. That is, to identify  $\epsilon$ ,  $\kappa_{i,j}$  should reflect the similarity of shocks to which borrowers are exposed, but not the degree of exposure to those shocks. Our measures of correlatedness (coincidence of bad years, and occupation) probably cannot be assumed to distinguish extensiveness from intensiveness of correlatedness.

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<sup>55</sup>Thus coefficients on non-interaction payoff function terms (e.g.  $\bar{R}$ ,  $r$ ) cannot be estimated.

<sup>56</sup>That is, interaction terms involving *sampled* borrowers are used to estimate the *group* payoff function. Similarly, the counterfactual groups are formed via  $k$ -for- $k$  borrower swaps across the sampled subsets of the groups. Using samples rather than entire groups represents a departure from Fox's analysis justifying the estimator. While a full analysis is beyond the scope of this paper, two points can be made. First, it seems clear that a modification of the key assumption used by Fox would lead to the same results on identification. The modified assumption would assume that groupings of randomly sampled group borrowers that produce greater observable surplus are more likely to be observed. Second, this modified assumption appears not to be much stronger than Fox's original assumption in the context of i.i.d. assignment-specific error terms, a context that justifies the original assumption; we conjecture that the sampling-modified assumption is justified in the same context.

Rather than attempt to identify  $\epsilon$  separately from  $\kappa_{i,j}$ , we focus on the overall correlation between borrowers  $i$  and  $j$ , call it  $C_{i,j} \equiv \epsilon\kappa_{i,j}$ .  $C_{i,j}$  is proxied in different ways, depending on the variable used (see section 5.1.3). When `worst_year` is used,  $C_{i,j} = \phi_{\text{worst\_year}} 1\{\text{worst\_year}_i = \text{worst\_year}_j\}$ . When `occupation` is used,  $C_{i,j} = \phi_{\text{occupation}}(\vec{oc}_i \cdot \vec{oc}_j)$  (i.e. the dot product of the occupational vectors of borrowers  $i$  and  $j$ ). The  $\phi$  parameters are assumed strictly positive. Thus, correlatedness is proxied by similarity in bad income years and/or (agricultural) occupations.

Incorporating  $C_{i,j}$  – for concreteness, proxied here using `worst_year` – and notation similar to the above into the sampled groups’ payoff function 11 gives

$$\begin{aligned} \Pi_L + \Pi_M = & \beta_1 \left( \sum_{k \in S^L} p_k \bar{p}_{-k}^{S^L} + \sum_{k \in S^M} p_k \bar{p}_{-k}^{S^M} \right) \\ & + \beta_2 \left( \sum_{k \in S^L} \overline{1\{\text{worst\_year}_k = \text{worst\_year}_{-k}\}} + \sum_{k \in S^M} \overline{1\{\text{worst\_year}_k = \text{worst\_year}_{-k}\}} \right), \end{aligned} \quad (12)$$

where  $\beta_1 = c$ ,  $\beta_2 = c\phi_{\text{worst\_year}}$ , and the terms in the second-line sums represent the fraction of other, same-group sampled borrowers naming the same `worst_year` as borrower  $k$ . Parameter  $c$  can thus be identified in sign but not magnitude; hence,  $\beta_1$  is normalized to +1 or –1 in estimation.

The main test of the Ghatak (1999) theory and our extension is whether all  $\beta$ ’s are positive. The model assumes that  $c > 0$ , which underlies complementarity of types in the payoff function and hence drives positive assortative matching. A positive estimate of  $\beta_1$  is thus direct evidence for this complementarity, while a negative estimate suggests matching based on substitutability of types. Regarding  $\beta_2 (= c\phi_{\text{worst\_year}})$ , since  $\phi_{\text{worst\_year}}$  (and  $\phi_{\text{occupation}}$ ) is restricted to be positive and since  $c > 0$  is assumed, the model requires a positive estimate for  $\beta_2$ . A negative estimate would contradict the model, giving evidence that matching is more consistent with payoffs valuing diversification rather than anti-diversification.

Probabilities of success  $p_k$  are measured as discussed in section 4. Correlatedness is

proxied using `worst_year`, `occupation`, or both, as described. If there are  $V$  villages indexed by  $v$ , and each village  $v$  has two (sampled) groups,  $L_v$  and  $M_v$ , the estimator comes from

$$\max_{\beta_1 \in \{-1, 1\}, \beta_2, \beta_3} \sum_{v=1}^V \sum_{\tilde{L}_v, \tilde{M}_v} 1\{\Pi_{L_v} + \Pi_{M_v} > \Pi_{\tilde{L}_v} + \Pi_{\tilde{M}_v}\},$$

where the alternate groupings  $\tilde{L}_v$  and  $\tilde{M}_v$  come from all  $k$ -for- $k$  borrower swaps, as discussed above, and there are three parameters when two proxies for correlatedness are included.

We also estimate based on a slightly different objective function, where the score is the sum of all villages' *fractions* of correct inequalities rather than *numbers* of correct inequalities. This weights each village equally in its contribution to the estimation and provides a more similar basis of comparison with the univariate KS results, where each village counts as a single draw from a distribution.

Maximization is carried out using the genetic algorithm routine in Matlab. Results from six estimations that alternately use the two objective functions combined with three sets of proxies for correlated risk are reported in Table 2. The point estimates are based on the 32 villages with sufficient data, and the corresponding 3620 total inequalities. Inference is carried out by subsampling.<sup>57</sup>

We find that the estimated coefficient on probabilities of success is consistently positive. Thus, even when controlling for correlated risk measures, including occupational similarity, riskiness has explanatory power for group formation consistent with complementarity. This supports the model, since complementarity is the basis for homogeneous matching and hence group lending's improved risk-pricing.

The correlated risk results are also similar to the univariate results, if a bit weaker

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<sup>57</sup>Fox (2010a) notes that the bootstrap is proved inconsistent by Abrevaya and Huang (2005) for a class of estimators that converge at rate  $\sqrt[3]{n}$ , which almost certainly includes the matching maximum score estimator. Thus, for each estimation, we create 200 subsamples containing 24 villages' data, by randomly sampling without replacement from the 32 villages. Estimation is carried out for each subsample. Operating under the assumption of  $\sqrt[3]{n}$ -convergence, one can apply the distribution of  $(\frac{24}{32})^{1/3} (\hat{\beta}_{24,i} - \hat{\beta}_{32})$  to  $(\hat{\beta}_{32} - \beta_0)$  to construct confidence intervals, where  $i \in \{1, \dots, 200\}$  corresponds to the subsamples,  $\hat{\beta}_{24,i}$  are the subsample estimates,  $\hat{\beta}_{32}$  is the full-sample estimate, and  $\beta_0$  is the true parameter. See Politis et al. (1999, 2.2).



**Table 2 — Matching Maximum Score Estimation**

Variable		Number	Share	Number	Share	Number	Share
<b>Success Probability</b>	Est.	+1	+1	+1	+1	+1	+1
	p-val.			Superconsistent			
<b>Worst_Year</b>	Est.	0.40	0.32*			0.32	0.33
	p-val.	0.18	0.06			0.27	0.31
<b>Occupation</b>	Est.			-0.062	-0.0029	-0.051	-0.025
	p-val.			0.46	0.47	0.34	0.51
Number of Inequalities		3620		3620		3620	
Number of Villages			32		32		32
Maximized Objective Fn.		2348	19.3	2205	18.3	2382	19.8
Percent Correct		65%	60%	61%	57%	66%	62%

Note: Each column corresponds to a different estimation; differences arise from the objective function used (noted atop each column) and the proxies for correlated risk. P-values are from one-sided tests for a negative (positive) true parameter if the point estimate is positive (negative). They are constructed using subsampling methods on 200 subsamples, each containing 24 distinct villages. Significance at the 10% level is denoted by \*.

statistically. The estimates for occupation are slightly negative, but not statistically different from zero; this closely parallels the univariate results, which were discussed in section 5.2. Worst\_year has more explanatory power than occupation (see last two rows of Table 2) and has consistently positive estimates in a fairly tight range (0.32 – 0.40). A negative coefficient can be rejected at the 10% level in one case and the 20%-level in another; when occupation is included, significance levels drop somewhat.

We interpret these results as supportive of the univariate results, and thus of both aspects of the theory. They suggest that borrower surplus is higher with both greater homogeneity in riskiness and greater anti-diversification. The main exception is that occupational anti-diversification does not appear to be driving matching; as noted earlier, this could be because the lender encourages diversification along observable dimensions.

## 7 Conclusion

In the context of joint liability lending and unobserved risk, theory suggests that borrowers will match homogeneously by riskiness; this embeds an implicit discount for safe borrowers

and can draw them into the market, increasing intermediation and improving efficiency. We develop tests of this hypothesis about matching behavior, and find supportive evidence from Thai microcredit groups: groups are more homogeneous in riskiness than random matching would predict. Thus, given joint liability, safe borrowers are effectively paying lower interest rates.

This is direct evidence on a mechanism through which the microcredit innovation of group lending has contributed to reviving credit markets among poor households around the world. It adds to our understanding of how innovations in lending have been able to extend finance to the world's poor and why group lending has been such a popular lending mechanism in the microcredit movement.

However, theory also suggests that borrowers may match to anti-diversify risk and thereby to minimize potential liability for fellow group members. While the first kind of matching works in favor of efficiency, the second may work against it by limiting the lender's ability to use group lending effectively. The data here suggest that mild anti-diversification is indeed occurring, though not via agricultural occupation.

From a policy standpoint these results show that voluntary matching by borrowers may also have its downside. Matching to anti-diversify can work against the lender's interests and, in equilibrium, the borrowers'. Applying the results narrowly, lenders may want to intervene to promote risk diversification within groups – for example, requiring occupational diversity – provided their intervention will not undermine positive assortative matching by riskiness. More generally, lenders may wish to step in with respect to group composition on certain dimensions while leaving other dimensions to the borrowers' discretion.

The paper leaves some open questions for future work to address. First, the risk and correlation measures used here could be improved upon. Future work with income histories and/or more detailed elicitation of future income distributions could perhaps push the analysis further, including in a more quantitative direction. Second, it would be ideal for matching tests to use measures of risk that pre-date group formation, to distinguish match-

ing behavior from within-group conformity that occurs after group formation. Third, richer datasets that include data on social networks, physical distances, etc., could potentially be used to identify whether risk-homogeneity and anti-diversification are purposeful or are by-products of other matching considerations. They could also help quantify and pinpoint matching frictions in these environments.

## Appendix

**Proof of Proposition 1.** Consider an equilibrium assignment. There are six sets into which all equilibrium groups can be partitioned: AA, BB, NN, AB, AN, BN, where the set names denote the pair of risk exposure-types of all groups within the partition.

The cross-partial of group payoff functions with respect to  $p_i$  and  $p_j$  is still given by equation 3. Thus the baseline result of homogeneous matching in almost every group holds in any set of groups within which correlatedness is fixed for all possible pairings of borrowers within the set – in particular, within AA, BB, and NN.

It remains to show that the sets AB, AN, and BN have zero measure in equilibrium. Consider AB, for example. Riskiness complementarity implies rank-ordering within risk exposure-type. That is, if  $(i, j)$  and  $(i', j')$  are equilibrium groups and borrowers  $i, i'$  ( $j, j'$ ) are  $A$ -risk ( $B$ -risk), then one of the following pairs of statements must hold:  $p_i \geq p_{i'}$  and  $p_j \geq p_{j'}$ , or  $p_{i'} \geq p_i$  and  $p_{j'} \geq p_j$ . Otherwise, the grouping  $(i, j')$  and  $(i', j)$  would raise surplus by increasing payoffs from riskiness complementarity without altering the nature of the exposure-type matching.

Given this fact and if set AB has positive measure, then for any  $\delta > 0$ , there must exist two groups  $(i, j)$  and  $(i', j')$  with  $|p_i - p_{i'}| < \delta$  and  $|p_j - p_{j'}| < \delta$ . Fix  $\delta = \sqrt{\epsilon/4}$  and two such groups. We will show that with riskiness levels so close, the gains from anti-diversification (matching A with A, B with B) outweigh any losses from decreased similarity in riskiness.

Without loss of generality, let  $(i, j)$  be the safer group, i.e.  $p_i \geq p_{i'}$  and  $p_j \geq p_{j'}$ . Using equation 6, the sum of both groups' payoffs can be written

$$4\bar{R} - (r + q)(p_i + p_j + p_{i'} + p_{j'}) + 2q(p_i p_j + p_{i'} p_{j'}) ,$$

since no borrowers are exposed to the same shocks. An  $(i, i')$  and  $(j, j')$  grouping would

instead pay

$$4\bar{R} - (r + q)(p_i + p_j + p_{i'} + p_{j'}) + 2q(p_i p_{i'} + p_j p_{j'}) + 4q\epsilon ;$$

the last term capturing the gains from anti-diversification. Now if  $p_{j'} \geq p_i$  or  $p_{i'} \geq p_j$ , then the new grouping is rank-ordered by riskiness, so  $p_i p_{i'} + p_j p_{j'} \geq p_i p_j + p_{i'} p_{j'}$  and surplus has increased. If instead  $p_{j'} < p_i$  and  $p_{i'} < p_j$ , then all four riskiness levels  $(p_i, p_j, p_{i'}, p_{j'})$  are within  $2\delta$  of each other, which caps the difference between  $p_i p_{i'} + p_j p_{j'}$  and  $p_i p_j + p_{i'} p_{j'}$  at  $4\delta^2 = \epsilon$ .<sup>58</sup> In this case too, surplus has increased. Since this alternate grouping raises surplus, the matching must not be an equilibrium; we thus contradict the hypothesis that AB has positive measure. By a similar argument, AN and BN cannot have positive measure.

**Proof of Proposition 2.** Let there be  $N$  groupings and  $K \leq N$  unique values that arise when the given sorting metric is applied to the  $N$  groupings, with values  $v_1 < v_2 < \dots < v_K$ . (Ties involve  $K < N$ .) Let  $n_i$  be the number of combinations that give rise to value  $v_i$  and  $N_i$  be the number of combinations that give rise to any value  $v \leq v_i$ , with  $N_0 \equiv 0$ ; then  $N_i = \sum_{k=1}^i n_k$  and  $N_K = N$ . If sorting is random, each of the  $N$  combinations of borrowers is equally likely to obtain. With probability  $\pi_i \equiv n_i/N$  the realized combination will result in value  $v_i$ , leading to calculated sorting percentile range  $[\frac{N_{i-1}}{N}, \frac{N_i}{N}]$ .

We show next that the CDF of sorting percentiles is uniform, i.e.  $F(z) = z$ . Fix  $z \in [0, 1]$ . There exists some  $i \in \{1, 2, \dots, K\}$  such that  $z \in [\frac{N_{i-1}}{N}, \frac{N_i}{N}]$ . Then the probability that a village's sorting percentile is less than  $z$ , i.e.  $F(z)$ , is the probability that its grouping leads to any value strictly less than  $v_i$  plus the probability that its grouping leads to value  $v_i$  and its sorting percentile picked from the uniform on  $[\frac{N_{i-1}}{N}, \frac{N_i}{N}]$  is below  $z$ :

$$F(z) = \sum_{k=1}^{i-1} \pi_k + \pi_i \int_{\frac{N_{i-1}}{N}}^z \frac{1}{\frac{N_i - N_{i-1}}{N}} dz = \sum_{k=1}^{i-1} \frac{n_k}{N} + \frac{n_i}{N} \frac{N}{n_i} (z - \frac{N_{i-1}}{N}) = z,$$

where the definitions of the  $\pi_i$ 's and the  $N_i$ 's have been used in the simplification.

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<sup>58</sup>For more detailed derivation, see Ahlin (2009).

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