IN PURSUIT OF BALANCE: RANDOMIZATION IN PRACTICE IN DEVELOPMENT FIELD EXPERIMENTS[#]

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Abstract

We present new evidence on the randomization methods used in existing experiments, and new simulations comparing these methods. We find that many papers do not describe the randomization in detail, implying that better reporting is needed. Our simulations suggest that in samples of 300 plus, the different methods perform similarly. However, for very persistent outcome variables and in smaller samples pair-wise matching and stratification perform best and appear to dominate the re-randomization methods commonly used in practice. The simulations also point to specific recommendations for which variables to balance on and for which controls to include in the ex-post analysis.

Keywords: Randomized experiment; Program evaluation; Development.
JEL codes: C93, O12.

[#] We thank the leading researchers in development field experiments who participated in our short survey, as well as colleagues who have shared their experiences with implementing randomization. We thank Angus Deaton, Esther Duflo, David Evans, Xavier Gine, Guido Imbens, Ben Olken and seminar participants at the World Bank for helpful comments, We are also grateful to Radu Ban for sharing his pair-wise matching Stata code, Jishnu Das for the LEAPS data, and to Kathleen Beegle and Kristen Himelein for providing us with their constructed IFLS data.. All views are of course our own.

1. Introduction

Randomized experiments are increasingly used in development economics. Historically, many randomized experiments were large-scale government-implemented social experiments, such as Moving To Opportunity in the U.S. or *Progresa/Oportunidades* in Mexico. These experiments allowed for little involvement of researchers in the actual randomization. In contrast, in recent years many experiments have been directly implemented by researchers themselves, or in partnership with NGOs and the private sector. These small-scale experiments, with sample sizes often comprising 100 to 500 individuals, or 20 to 100 schools or health clinics, have greatly expanded the range of research questions that can be studied using experiments, and have provided important and credible evidence on a range of economic and policy issues. Nevertheless, this move towards smaller sample sizes means researchers increasingly face the question of not just whether to randomize, but how to do so. This paper provides the first comprehensive look at how researchers are actually carrying out randomizations in development field experiments, and then analyzes some of the consequences of these choices.

Simple randomization ensures the allocation of treatment to individuals or institutions is left purely to chance, and is thus not systematically biased by deliberate selection of individuals or institutions into the treatment. Randomization thus ensures that the treatment and control samples are, in expectation, similar in average, both in terms of observed and unobserved characteristics. Furthermore, it is often argued that the simplicity of experiments offers considerable advantage in making the results convincing to other social scientists and policymakers and that, in some instances, random assignment is the fairest and most transparent way of choosing the recipients of a new pilot program (Burtless, 1995).

However, it has long been recognized that while pure random assignment guarantees that the treatment and control groups will have identical characteristics on average, in any particular random allocation, the two groups will differ along some dimensions, with the probability that such differences are large falling with sample size.[1] Although ex-post adjustment can be made for such chance imbalances, this is less efficient than achieving ex-ante balance, and can not be used in cases where all individuals with a given characteristic are allocated to just the treatment group.

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[1] For example, Kernan et al. (1999) consider a binary variable that is present in 30 percent of the sample. They show that the chance that the two treatment group proportions will differ by more than 10 percent is 38% in an experiment with 50 individuals, 27% in an experiment with 100 individuals, 9% for an experiment with 200 individuals, and 2% for an experiment with 400 individuals.

The standard approach to avoiding imbalance on a few key variables is stratification (or blocking), originally proposed by R.A. Fisher. Under this approach, units are randomly assigned to treatment and control within strata defined by usually one or two observed baseline characteristics. However, in practice it is unlikely that one or two variables will explain a large share of the variation in the outcome of interest, leading to attempts to balance on multiple variables. One such method when baseline data are available is pair-wise matching (Greevy et al, 2004, Imai et al. 2007).

The methods of implementing randomization have historically been poorly reported in medical journals, leading to the formulation of the CONSORT guidelines which set out standards for the reporting of clinical trials (Schulz, 1996). The recent explosion of field experiments in development economics has not yet met these same standards, with many papers omitting key details of the method in which randomization is implemented. For this reason, we conducted a survey of leading researchers carrying out randomized experiments in developing countries. This reveals common use of methods to improve baseline balance, including several re-randomization methods not discussed in print. These are (i) carrying out an allocation to treatment and control, and then using a statistical threshold or ad hoc procedure to decide whether or not to redraw the allocation; and (ii) drawing 100 or 1000 allocations to treatment and control, and choosing the one amongst them which shows best balance on a set of observable variables.

This paper discusses the pros and cons of these different methods for striving towards balance on observables. Proponents of methods such as stratification, matching, and minimization claim that such methods can improve efficiency, increase power, and protect against type I errors (Kernan et al., 1999) and do not seem to have significant disadvantages, except in small samples (Imai et al. 2008, Greevy et al. 2004, Aickin, 2001)[2]. However, it is precisely in small samples that the choice of randomization method becomes important, since in large samples all methods will achieve balance. We simulate different randomization methods in four panel data sets. We then compare balance in outcome variables at baseline and at follow-up. The simulations show that when methods other than pure randomization are used, the degree of balance achieved on baseline variables is much greater than that achieved on the outcome variable (in the absence of treatment) in the follow-up period. The simulations show further that

[2] One other argument in favor of ex-ante balancing is that, if the treatment effect is heterogeneous and varies with observed covariates, ex-ante balancing increases the precision of subgroup analysis.

in samples of 300 observations or more, the choice of method is not very important for the degree of balance in many outcomes at follow-up. In small samples, and with very persistent outcomes, however, matching or stratification on relevant baseline variables achieves more balance in follow-up outcomes than does pure randomization.

We use our simulation results and theory to help answer many of the important practical questions facing researchers engaged in randomized experiments. The results allow us to provide guidance on how to conduct inference after stratification, matching or re-randomization. In practice it appears that many researchers ignore the method of randomization in inference. We show that this leads to hypothesis tests with incorrect size. On average, the standard errors are overly conservative when the method of randomization is not controlled for in the analysis, implying that researchers may not detect treatment effects that they would detect if the inference did take into account the randomization method. However, although this is the case on average, in a non-trivial proportion of draws, it will be the case that not controlling for the randomization method will result in larger standard errors than if the randomization method is controlled for. Thus it is possible that not controlling for the randomization method could lead the researcher to find a significant effect that is no longer significant when stratum or pair dummies are included. Moreover, we show further that stratifying, matching, or re-randomizing and then analyzing the data without controlling for the method of randomization results in lower power than if a pure random draw was used to allocate treatments, except in cases where the variables that balance is sought for have no predictive power for the future outcome of interest (in which case there is no need to seek balance on them anyway).

The paper also discusses the use and abuse of tests for baseline differences in means, the impact of balancing observables on achieving balance on unobservables, and the issue of how many (and which) variables to use for stratifying or matching. Finally, based on our simulation results and the previous econometric literature, this paper provides a list of actionable recommendations for researchers performing and reporting on randomized experiments.

This paper draws upon a large clinical trials literature, where many related issues have been under discussion for several decades, drawing out the lessons for development field experiments. It complements several recent papers in development on randomized experiments.[3]

[3] Summaries of recent experiments and advocacy of the policy case are found in Kremer (2003), Duflo and Kremer (2004), Duflo (2005) and Banerjee (2007).

The paper builds on the recent handbook chapter by Duflo, Glennerster and Kremer (2006), which aims to provide a "how to" of implementing experiments. Our focus differs, considering how the actual randomization is implemented in practice, and considering matching and re-randomization approaches. Finally, we contribute to the existing literature through new simulations which illustrate the performance of the different methods in a variety of situations experienced in practice.

Whilst our focus is on field experiments in development economics, to date the field with most active involvement of researchers in randomization, randomized experiments are also increasingly being used to investigate important policy questions in other fields (Levitt and List, 2008). In common with the development literature, the extant literature in these other fields has often not explained the precise mechanism used for randomizing. However, it does appear that re-randomization methods are also being employed in some of these studies. The ongoing New York public schools project being undertaken by the American Inequality Lab is one such high-profile example. The lessons of this paper will also be important in designing upcoming experiments in other fields of economics.

The remainder of the paper is set out as follows. Section 2 provides a stocktaking of how randomization is currently being implemented, drawing on a summary of papers and a survey of leading experts in development field experiments. Section 3 describes the data sets used in our simulations, and outlines in more detail the different methods of randomization. Section 4 then provides simulation evidence on the relative performance of the different methods, and on answers to key questions faced in practice. Section 5 concludes with our recommendations.

2. How is randomization being implemented?

2.1. Randomization as described in papers

We begin by reviewing a selection of research papers containing randomized experiments in development economics. Table 1 summarizes a selection of relatively small-scale randomized experiments with baseline data, often implemented via NGOs or as pilot studies. [4] For each study we list the unit at which randomization occurs. Typical sample sizes are 100 to 300 units, with the smallest sample size being 10 geographic areas used in Ashraf et al. (2006b).

[4] We do not include here experiments undertaken by the authors both for objectivity reasons, and because the final write-up of our papers has been influenced by the current paper.

The transparency in allocating a program to participants is likely to be greatest when assignment to treatment is done in public.[5] The column "done in public or private" therefore records whether the actual randomization was done publicly or privately. In between lies "semi-public", where perhaps the NGO and/or Government officials witness the randomization draw, but not the recipients of the program. Only 2 out of the 18 papers reviewed note whether it was public or not – in both cases public lotteries. The majority of the other randomizations we believe are private or at most "semi-public", but this is not stated explicitly in the papers.

Next we examine which methods are being used to reduce the likelihood of imbalance on observable covariates. Thirteen studies use stratification, two use matched pairs, and only three appear to use pure randomization. Ashraf et al. (2007) is the only documented example we have found of one of the methods that the next section shows to be in common use in our survey of experts. They note "at the time of randomization, we verified that observable characteristics were balanced across treatments, and, in a few cases, re-randomized when this was not the case".

Few papers provide the details of the method used, presumably because there has not been a discussion of the potential importance of these details in the economics literature. For example, stratification is common, but few studies actually give the number of strata used in the study. In practice there appears to be disagreement as to whether it is necessary to include strata dummies in the analysis after stratification – more than half the studies using stratification do not include strata dummies. Finally, all but one of the papers in Table 1 present a table for comparing treatment and control groups and test for imbalance. The number of variables used for checking imbalance ranges from 4 to 39.

2.2 Randomization in practice according to a survey of experts

The long lag between inception of a randomized experiment and its appearance in at least working paper form means the results above do not necessarily represent how the most recent randomized evaluations are being implemented. We therefore surveyed leading experts in

[5] Of course, privately drawn randomizations still have the virtue of being able to tell participants that the reason they were chosen or not chosen is random. However, it is our opinion that carrying out the randomization in a public or semi-public manner can make this more credible in the eyes of participants in many settings. This may particularly be the case when it is the Government doing the allocation (see Ferraz and Finan, 2008, p.5 who note "to ensure a fair and transparent process, representatives of the press, political parties and members of civil society are all invited to witness the lottery"). Nevertheless, public randomization may not be feasible or desirable in particular settings. We merely wish to urge researchers to consider whether the randomization can be easily publicly implemented in their setting, and to note in their papers how the randomization was done.

randomized evaluations on their experience and approach to implementation. A short online survey was sent to 35 selected researchers in December 2007. The list was selected from members of the Abdul Latif Jameel Poverty Action Lab, BREAD, and the World Bank who were known to have conducted randomized experiments. We had 25 of these experts answer the survey, with 7 out of the 10 individuals who did not respond having worked with those who did respond. The median researcher surveyed had participated in 5 randomized experiments, with a mean of 5.96.[6] 71 percent of the experiments had baseline data (including administrative data) that could be used at the time when randomization to treatment was done.

Preliminary discussions with several leading researchers established that several methods involving multiple random draws were being used in practice to increase the likelihood of balance on observed characteristics. One such approach is to take a random draw of assignment to treatment, examine the difference in means for several key baseline characteristics, and then re-randomize if the difference looks too large. This decision as to what is too large could be done subjectively, or according to some statistical cutoff criteria. For example, one survey respondent noted that they "regressed variables like education on assignment to treatment, and then re-did the assignment if these coefficients were 'too big'".

The second approach takes many draws of assignment to treatment, and then chooses the one that gives best balance on a set of observable characteristics according to some algorithm or rule. For example, several researchers say they write a program to carry out 100 or 1000 randomizations, and then for each draw, regress individual variables against treatment. They then choose the draw with the minimum maximum t-statistic.[7] Some impose further criteria such as requiring the minimum maximum t-statistic for testing balance on observables to be below one. The number of variables used to check balance typically ranges from 5 to 20, and often includes the baseline levels of the main outcomes. The perceived advantage of this approach is to enable balance on more variables than possible with stratification, and to provide balance in means on continuous variables.

Researchers were asked whether they had ever used a particular method, and the method used in their most recent randomized experiment. All of the methods are often combined with some stratification, so we examine that separately. Table 2 reports the results. Most researchers

[6] This is after top-coding the number of experiments at 15.
[7] An alternative approach used by another researcher is to regress the treatment on a set of baseline covariates and choose the draw with the lowest R^2.

have at some point used simple randomization (probably with some stratification). However, we also see much more use of other methods than is apparent from the existing literature. 56 percent had used pair-wise matching. 32 percent of all researchers and 46 percent of the 5 or more experiments group have subjectively decided whether to re-randomize based on an initial test of balance. The multiple draws process described above has also been used by 24 percent of researchers and by 38 percent of the 5 or more experiment group.

More detailed questions were asked about the most recent randomization, in an effort to obtain some of the information not provided in Table 1. 23 of the 25 respondents provided information on these. Stratification was used in 14 out of the 15 experiments that were not employing a matched pair design. The number of *variables* used in forming strata was small: 6 used only one variable, typically geographic location; 4 used two variables (e.g. location and gender), and 4 used four variables. Of particular note is that it appears rare to stratify on baseline values of the outcome value of interest (e.g. test scores, savings levels, or incomes) with only 2 of these 14 experiments including a baseline outcome as a stratifying factor. While the number of stratifying variables is small, there is much greater variation in the *number of strata*: ranging from 3 to 200, with a mean (median) of 47 (18). Only one researcher said that stratification was controlled for when calculating standard errors for the treatment effect.

A notable feature of the survey responses was a much greater number of researchers randomizing within matched pairs than is apparent from the existing development literature. The vast majority of these matches were not done using optimal or greedy Mahalanobis matching, but were instead based on only a few variables and commonly done by hand. In most cases the researchers matched on discrete variables and their interactions only, and thus, in effect, the matching reduced to stratification.

One explanation for the difference in randomization approaches used by different researchers is that they reflect differences in context, with sample size, question of interest, and organization one is working with potentially placing constraints on the method which can be used for randomization. We therefore asked researchers for advice on how to evaluate the same hypothetical intervention designed to raise the incomes of day laborers.[8] The responses varied greatly across researchers, and include each of the methods given in Table 2. What is clear is that there appears to be no general agreement about how to go about randomizing in practice.

[8] See Appendix 1 for the exact question and the responses given.

3. Data, simulated methods, and variables for balancing

3.1 Data

To compare the performance of the different randomization methods in practice, we chose four panel data sets which allow us to examine a wide range of potential outcomes of interest, including microenterprise profits, labor income, school attendance, household expenditure, test scores, and child anthropometrics.

The first panel data set covers microenterprises in Sri Lanka and comes from de Mel et al. (2008). This data was collected as part of an actual randomized experiment, but we keep only data for firms that were in the control group during the first treatment round. The data set contains information on firms' profits, assets and many other firm and owner characteristics. The simulations we perform for this data set are meant to mimic a randomized experiment that administers a treatment aimed at increasing firms' profits, such as a business training program.

The second data set is a sub-sample of the Mexican employment survey (ENE). Our sub-sample includes heads of household between 20 and 65 years of age who were first interviewed in the second quarter of 2002 and who were re-interviewed in the following four quarters. We only keep individuals who were employed during the baseline survey and imagine a treatment that aims at increasing their income, such as a training program or a nutrition program.

The third data set comes from the Indonesian Family Live Survey (IFLS).[9] We use 1997 data as the baseline and 2000 data as the follow-up, and simulate two different interventions with the IFLS data. First, we keep only children aged 10-16 in 1997 that were in the 6th grade and in school. These children then receive a simulated treatment aimed at keeping them in school (in the actual data, about 26 percent have dropped out 3 years later). Second, we create a sample of households and simulate a treatment that increases household expenditure per capita.

The fourth data set comprises child and household data from the LEAPS project in Pakistan (Andrabi et al. 2008). We focus on children aged 8 to 12 at baseline and examine two child outcome variables: math test scores and height z-scores[10]. The simulated treatments increase test scores or z-scores of these children. There is a wide range of policy experiments

[9] See http://www.rand.org/labor/FLS/IFLS/.
[10] We also have performed all simulations with English test scores and weight z-scores. The results are very close to the results using math test scores and height z-scores and are available from the authors upon request.

that have targeted these types of outcomes, from providing text books or school meals to giving conditional cash transfers or nutritional supplements.

3.2 Simulated methods

For each data set, we draw three sub-samples of 30, 100, and 300 observations to investigate how the performance of different methods varies with sample size. All results are based on 10,000 bootstrap iterations which randomly split the sample into a treatment and a control group, according to five different methods. The first method is a single random draw, which we take as the benchmark for our comparison with the pros and cons of other methods.

3.2.1 Stratification

The second method is stratification. Stratified randomization is the most well-known, and as we have seen, commonly used method of preventing imbalance between treatment and control groups for the observed variables used in stratification. By eliminating particular sources of differences between groups, stratification (aka blocking) can increase the sensitivity of the experiment, allowing it to detect smaller treatment differences than would otherwise be possible (Box et al, 2005). The most often perceived disadvantage of stratification compared to some alternative methods is that only a small number of variables can be used in forming strata.[11]

In terms of which variables to stratify on, the econometric literature emphasizes variables which are strongly related to the outcome of interest, and variables for which subgroup analysis is desired. Statistical efficiency is greatest when the variables chosen are strongly related to the outcome of interest (Imai et al., 2008). Stratification is not able to remove all imbalance for continuous variables. For example, for two normal distributions with different means but the same variance, the means of the two distributions between any two fixed variables (i.e. within a stratum) will differ in the same direction as the overall mean (Altman, 1985). In the simulations, we always stratify on the baseline values of the outcome of interest and on one or two other variables, which either relate to the outcome of interest or constitute relevant subgroups for ex-post analysis.

[11] This is particularly true in small samples. For example, considering only binary or dichotomized characteristics, with 5 variables there are 2^5 = 32 strata, while 10 variables would give 2^10 = 1024 strata. In our samples of 30 observations, we stratify on 2 variables, forming 8 strata. In the samples of 100 and 300 observations, we also stratify on 3 variables (24 strata), and also on 4 variables (48 strata).

3.2.2 Pair-wise matching

As a third method, we simulate pair-wise matching. As opposed to stratification, matching provides a method to improve covariate balance for many variables at the same time. Greevy et al. (2004) describe the use of optimal multivariate matching. However, we chose to use the less computationally intensive "optimal greedy algorithm" laid out in King et al. (2007)[12]. In both cases pairs are formed so as to minimize the Mahalanobis distance between the values of all the selected covariates within pairs, and then one unit in each pair is randomly assigned to treatment and the other to control.

As with stratification, matching on covariates can increase balance on these covariates, and increase the efficiency and power of hypothesis tests. King et al. (2007) emphasize one additional advantage in the context of social science experiments when the matched pairs occur at the level of a community or village or school, which is that it provides partial protection against political interference or drop-out. If a unit drops out of the study or suffers interference, its pair unit can also be dropped from the study, while the set of remaining pairs will still be as balanced as the original data set. In contrast, in a pure randomized experiment, if even one unit drops out, it is no longer guaranteed that the treatment and control groups are balanced on average. However, the converse of this is that if units drop out at random, the matched pair design will throw out the corresponding pairs as well, leading to a reduction in power and smaller sample size than if an unmatched randomization was used.[13]

Note however that simply dropping the paired unit will only yield a consistent estimate of the average treatment effect for the full sample when the reason for attrition is unrelated to the size of the treatment effect. A special case of this occurring is when there is a constant treatment effect. If there are heterogeneous treatment effects, and drop-out is related to the size of the treatment effect, then one can only identify the average treatment effect for the subsample of units who remain in the sample when the treatment is randomly offered. Whether or not the average treatment effect for the subsample of units who remain in the sample is a quantity of

[12] The Stata code performing pair-wise Mahalanobis matching with an optimal greedy algorithm takes several days to run in the 300 observations sample. If there is little time in the field to perform the randomization this may thus not be an option. It is thus important to have ample time between receiving baseline data and having to perform the randomization to have the flexibility of using matching techniques if desired. Software packages other than Stata may be more suited for this algorithm and may speed up the process. We provide our Stata code in an online appendix to this paper.

[13] See Greevy et al. (2004) for discussion of methods to retain broken pairs.

interest will be up to the researcher to argue, and will depend on the level of attrition. It will understate the average treatment effect for the population of interest if those in the control group who had most to gain from the treatment drop out of the survey, either through disappointment or in order to take up an alternative to the treatment. It will overstate the average treatment effect for the population of interest if the individuals in the treatment group who do not benefit much (or perhaps even have a negative effect) of the treatment drop out.

3.2.3 Re-randomization methods

Since our survey revealed that several researchers are using re-randomization methods, we simulate two of these methods. The first, which we dub the "big stick" method by analogy with Soares and Wu (1983), requires a re-draw if a draw shows any statistical difference in means between treatment and control group at the 5 percent level or lower. The second method picks the draw with the minimum maximum t-stat out of 1000 draws.

We are not aware of any papers which formally set out the re-randomization methods used in practice in development, but there are analogs in the sequential allocation methods used in clinical trials (Soares and Wu, 1983; Taves, 1974; Pocock and Simon, 1975). The use of these related methods remains somewhat controversial in the medical field. Proponents emphasize the ability of such methods to improve balance on up to 10 to 20 covariates, with Treasure and MacRae (1998) suggesting that if randomization is the gold standard, minimization may be the platinum standard. In contrast, the European Committee for Proprietary Medicinal Products (CPMP, 2003) recommends that applicants avoid such methods and argues that minimization may result in more harm than good, bringing little statistical benefit in moderate sized trials.

Why might researchers wish to use these methods instead of stratification? In small samples, stratification is only possible on one or two variables. There may be many variables that the researcher would like to ensure are not "too unbalanced", without requiring exact balance on each. Re-randomization methods may be viewed as a compromise solution by the researchers, preventing extreme imbalance on many variables, without forcing close balance on each. Re-randomization may also offer a way of obtaining approximate balance on a set of relevant variables in a situation of multiple treatment groups of unequal sizes.

3.3 Variables for balancing

In practice researchers attempt to balance on variables they think are strongly correlated with the outcome of interest. The baseline level of the outcome variable is a special case of this kind of variable. We always include the baseline outcome among the variables to stratify, match or balance on[14]. In the matching and re-randomization methods, we also use six additional baseline variables that are thought to affect the outcome of interest. Stratification takes a subset of these six additional variables.[15]

Among these balancing variables, we tried to pick variables that are likely to be correlated with the outcome based on economic theory and existing data. There is, however, a caveat. Most experiments have impacts measured over periods of 6 months to 2 years. While our economic models and existing data sets can provide good information for deciding on a set of variables useful for explaining *current* levels, they are often much less useful in explaining *future* levels of the variable of interest. In practice, often we can not theoretically or empirically explain many short-run changes well with observed variables – and believe that these changes are the result of shocks. As a result, it may be the case that the covariates used to obtain balance on are not strong predictors of future values of the outcome of interest.

The set of outcomes we have chosen spans a range of the ability of the baseline variables to predict future outcomes. At one end is microenterprise profits in Sri Lanka, where baseline profits and six baseline individual and firm characteristics explain only 12.2 percent of the variation in profits six months later. Thus balancing on these common owner and firm characteristics will not control for very much of the variation in future realizations of the outcome of interest. School enrolment in the IFLS data is another example where baseline variables explain very little of future outcomes. For a sample of 300 students who were all in school at baseline, 7 baseline variables only explain 16.7 percent of the variation in school enrollment for the same students 3 years later. The explanatory power is better for labor income in the Mexican ENE data and household expenditure in the IFLS, with the baseline outcome and six baseline variables explaining 28-29 percent of the variation in the future outcome. The math test scores and height z-scores in the LEAPS data have the most variation explained by baseline characteristics, with 43.6 percent of the variation in follow-up test scores explained by the baseline test score and six baseline characteristics.

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[14] Note that this is rather an exception in practice, where researchers often do not balance on the baseline outcome.
[15] A list of the variables used for each dataset is in Appendix 2 (Table A2).

We expect to see more difference amongst randomization methods in terms of achieving balance on future outcomes for the variables that are either more persistent, or that have a larger share of their changes explained by baseline characteristics. We therefore expect to see least difference among methods for the Sri Lanka microenterprise profits data and Indonesian school enrolment data, and most difference for the LEAPS math test score and height z-score data.

More generally, we recommend balancing on the baseline outcome since our data show that this variable typically has the strongest correlation with the future outcome. In addition, we recommend balancing on dummies for geographic regions since they also tend to be quite correlated with our future outcomes. This may be because shocks may differ across regions. Moreover, in practice, implementation of treatment may vary across regions, which is another reason to balance on region. Finally, if baseline data is available for several periods, one could check which variables are strongly correlated with future outcomes and could also balance on these. If multiple rounds of baseline survey are not available, this data could come from an outside source, as long as it includes the same variables and was collected in a comparable environment.

When choosing balancing variables, researchers will often face a trade-off between balancing on additional variables and achieving better balance on already chosen variables. For example, with stratification, there is a trade-off between stratifying on another variable and breaking down an already chosen variable into finer categories. In this case, we recommend adding the new variable only if there is strong reason to believe that it would be correlated with follow-up outcomes or if subsample analysis in this dimension is envisioned. In our datasets, it tends to be the case that, except for the baseline outcome and geography, few variables are strongly correlated with the follow-up outcome. Moreover, note that simply adding the new variable and switching the randomization method to matching, which is not bound by the number of strata, does not necessarily solve the problem. The more variables are used for matching, the worse balance tends to be on any given variable.

4. Simulation results

Appendix 3 reports the full set of simulation results for all four data sets for 30, 100, and 300 observations. We summarize the results of these simulations in this section, organizing their discussion around several central questions that a researcher may have when performing a randomized assignment. We start by addressing the following core question:

4.1 Which methods do better in terms of achieving balance and avoiding extremes?

We first compare the relative performance of the different methods in achieving balance between the treatment and control groups in terms of baseline levels of the outcome variable. Table 3 shows the average difference in baseline means, the 95th percentile of the difference in means (a measure of the degree of imbalance possible at the extremes), and the percentage of simulations where a t-test for difference in means between the treatment and control has a p-value less than 0.10. We present these results for a sample size of 100, with results for the other sample sizes contained in Appendix 3. Figure 1 graphically summarizes the results for a selection of variables, plotting the densities of the differences in average outcome variables for all three sample sizes: 30, 100, and 300 observations.

Table 3 shows that the mean difference in baseline means is very close to zero for all methods – on average all methods lead to balance. However, Table 3 and Figure 1 also show that stratification, matching, and especially the minmax t-stat method have much less extreme differences in baseline outcomes, while the big stick method only results in narrow improvements in balance over a single random draw. For example, in the Mexican labor income data with a sample of 100, the 95th percentile of the difference in baseline mean income between the treatment and control groups is 0.384 standard deviations (s.d.) with a pure random draw, 0.332 s.d. under the big stick method, 0.304 s.d. when stratifying on 4 variables, 0.100 s.d. with pair-wise greedy matching, and 0.088 under the minmax t-stat method. The size of the difference in balance achieved with different methods shrinks as the sample size increases – asymptotically all methods will be balanced.

The key question then is to which extent achieving greater balance on baseline variables translates into better balance on future values of the outcome of interest in the absence of treatment. The lower graphs in Figures 1a though 1d show the distribution of difference in means between treatment and control at follow-up for each method, while Table 4 summarizes how the methods perform in obtaining balance in follow-up outcomes[16].

[16] The follow-up period is six months for the Sri Lankan microenterprise and Mexican labor income data, one year for the Pakistan test-score and child height data, and three years for the Indonesian schooling and expenditure data.

Panel A of Table 4 shows that on average, all randomization methods give balance on the follow-up variable, even with a sample size as small as 30. This is the key virtue of randomization. Figure 1 and Panel B show there are generally fewer differences across methods in terms of avoiding extreme imbalances than with the baseline data. This is particularly true of the Sri Lanka profit data and the Indonesian schooling data, for which baseline variables explained relatively little of future outcomes. With a sample size of 30, stratification and matching reduce extreme differences between treatment and control, but with samples of 100 or 300, there is very little difference between the various methods in terms of how well they balance the future outcome.

Baseline variables have more predictive power for the realizations at follow-up for the other outcomes we consider. The Mexican labor income and Indonesian expenditure data lie in an intermediate range of baseline predictive power, with the baseline outcomes plus six other variables explaining about 28 percent of the variation in follow-up outcomes. Figures 1b and 1c show that, in contrast to the Sri Lanka and IFLS schooling data, even with samples of 100 or 300 we find matching and stratification continue to perform better than a single random draw in reducing extreme imbalances. Table 4 shows that with a sample size of 300, the 95th percentile of the difference in means between treatment and control groups is 0.23 s.d. under a pure random draw for both expenditure and labor income. This difference falls to 0.20 s.d. for expenditure and 0.15 s.d. for labor income when pair-wise matching is used, and to 0.20 s.d. for both variables when stratifying or using the min-max re-randomization method.

Our other two outcomes variables, math test scores and height z-scores lie in the higher end of baseline predictive power, with the baseline outcome and six other variables predicting 43.6 percent and 35.3 percent of the variation in follow-up outcomes, respectively. Figure 1d illustrates that the choice of method makes more of a difference for these highly predictable follow-up outcomes than for the less predictable ones. Stratifying, matching, and the minmax t-state method consistently lead to narrower distributions in the differences at follow-up when test scores or height z-scores are the outcomes. Nevertheless, even with these more persistent variables, the gains from pursuing balance on baseline are relatively modest when the sample size is 300 – using pair-wise matching rather than a pure random draw reduces the 95th percentile of the difference in means from 0.23 to 0.17 in the case of math test scores.

4.2 What does balance on observables imply about balance on unobservables?

In general, what does balancing on observables do in terms of balancing unobservables? Aickin (2001) notes that methods which balance on observables can do no worse than pure randomization with regard to balancing unobserved variables.[17] We illustrate this point empirically in the Sri Lanka and ENE datasets by defining a separate group of variables from the data to be "unobservable" in the sense that we do not balance, stratify or match on them. The idea here is that, although we have these variables in these particular data sets, they may not be available in other data sets (such as measures of entrepreneurial ability). Moreover, these "unobservables" are meant to capture what balancing does to variables that are thought to have an effect on the outcome variable, but are truly unobservable. Table 3 indicates that the balance on these unobservables is pretty much the same across all methods.

Rosenbaum (2002, p. 21) notes that under pure randomization, if we look at a table of observed covariates and see balance "this gives us reason to hope and expect that other variables, not measured, are similarly balanced". This holds true for pure random draws, but will not be the case with methods which enhance balance on certain observed covariates. Presenting a table which shows only the variables used in matching or for re-randomization checks, and showing balance on these covariates, will thus overstate the degree of balance attained on other variables that are not closely correlated with those for which balance was pursued. For example, the 95th percentile of the difference in means in Table 3 gives a similar level of imbalance for the unobservables as the balanced outcome under a pure random draw, whereas under the other methods the unobservables have higher imbalance than the outcome variable.[18] We therefore recommend that if matching or re-randomization (or stratification on continuous variables) is used, researchers clearly separate these from other variables of interest when presenting a table to show balance.

4.3 To dummy or not to dummy?

[17] To see this, consider balancing on variable X, and the consequences of this for balance on an unobserved variable W. W can be written as the sum of the fitted value from regressing W on X, and the residual from this regression:

$$\begin{aligned} W &= P_X W + (I - P_X)W \\ P_X &= X(X'X)^{-1}X' \end{aligned} \qquad (1)$$

Balancing on X will therefore also balance the part of W which is correlated with X, $P_X W$. Since the remaining part of W, (I-PX)W is orthogonal to X, it will tend to balance at the same rate as under pure randomization.

[18] Note the imbalance on unobservables is similar to that of a single random draw, which concurs with the point that balancing on observables can do no worse than pure randomization when it comes to balancing unobservables.

We have seen that only a fraction of studies using stratification control for strata in the statistical analysis. Kernan et al. (1999) state that results should take account of stratification, by including strata as covariates in the analysis. Failure to do so results in overly conservative standard errors, which may lead a researcher to erroneously fail to reject the null hypothesis of no treatment effect. While the omission of balanced covariates will not change the point estimates of the effect in linear models, leaving out a balanced covariate can change the estimate of the treatment effect in non-linear models (Raab et al. 2000), so that analysis of binary outcomes makes this adjustment more important. The European Committee for Proprietary Medicinal Products (CPMP, 2003) also recommends that all stratification variables be included as covariates in the primary analysis, in order to "reflect the restriction on randomization implied by the stratification". Similarly, for pair-wise matching, dummies for each pair should be included in the treatment regression.

Furthermore, in practice, stratification is unlikely to achieve perfect balance for all of the variables used in stratification. Whenever there is an odd number of units within a stratum, there will be imbalance (Therneau, 1993). In addition, imbalance may arise from units having a baseline missing value on one of the variables used in forming strata. As a consequence, in practice, the point estimate of the treatment effect will also likely change if strata dummies are included compared to when they are not included.

To examine whether or not controlling for stratification matters in practice, Panels C and D of Table 4 compare the size of a hypothesis test for the difference in means of the follow-up outcome when no treatment has been given. Panel C shows the proportion of p-values under 0.10 when no stratum or pair dummies are included, and Panel D shows the proportion of p-values under 0.10 when these dummies are included. Recall that this is a test of a null hypothesis which we know to be true, so to have correct size, 10 percent of the p-values should be below 0.10. We see that this is the case for the pure random draw, whereas failure to control for the dummies leads the stratification and pair-wise matching tests to be too conservative on average.[19] For example, with a sample size of 30, less than 5 percent of the p-values are below 0.10 for all six outcomes when we don't include pair dummies with pair-wise matching. For the math test score, only 0.6 percent of the p-values under stratification and none of the p-values under pair-wise

[19] The child schooling in Indonesia is a binary outcome. The difference in means attending school can therefore be only a limited number of discrete differences, and this discreteness causes the test to not have the correct size even under a pure random draw when the sample is small.

matching are under 0.10. Even with a sample size of 300, less than 5 percent of the p-values are below 0.10 for the more persistent outcomes when stratification or matching is used but not accounted for by adding stratum or pair dummies. In contrast, Panel D shows that when we add stratum dummies or pair dummies, the hypothesis test has the correct size, with 10 percent of the p-values under 0.10, even in sample sizes as small as 30.

Thus, on average, it is overly conservative to not include the controls for stratum or pair in analysis. The resulting conservative standard errors imply that if researchers do not account for the method of randomization in analysis, they may not detect treatment effects that they would otherwise detect. However, although on average the p-values are lower when including these dummies, Table 5 shows that this is not necessarily the case in any particular random allocation to treatment and control. Including stratum dummies only lowers the p-value in 48 to 88 percent of the replications, depending on sample size and outcome variable. Thus in practice, researchers can not argue that ignoring stratum dummies will always result in larger standard errors than when these dummies are included. If researchers could commit to always ignoring the stratification during analysis, then this would be on average conservative. But since it is difficult to commit, if no standard for analysis exists, researchers may be tempted to try their analysis with and without stratum dummies and report the results that are more significant. We therefore recommend that the standard should be to control for the method of randomization in analysis.[20]

4.4 How should inference be done after re-randomizing?

While including strata or pair dummies in the ex-post analysis for the stratification and matching methods is quite straight-forward, the methods of inference are not as clear for re-randomization methods. In fact, the correct statistical methods for covariate-dependent randomization schemes such as minimization are still a conundrum in the statistics literature, leading some to argue that the only analysis that we can be completely confident about is a permutation test or re-randomization test. Randomization inference can be used for analysis of the method of re-randomizing when the first draw exceeds some statistical threshold (although it requires additional programming work). Using the rule which determines when re-randomization will take place, the researcher can map out the set of random draws which would be allowed by

[20] If authors believe they have a valid reason not to control for stratum dummies, they should explain this reasoning in their text, and also mention what the results would be if stratum dummies were included.

the threshold rule, throwing out those with excessive imbalance, and then carry out permutation tests on the remaining draws[21]. Such a method is not possible when ad hoc criteria are used to decide whether to redraw.

Optimal model-based inference is less clear under re-randomization, since allocation to treatment is data-dependent. To see this, consider the data generating processes:

$$Y_i = \alpha + \beta Treat_i + \varepsilon_i \qquad (2a)$$

$$Y_i = \alpha + \beta Treat_i + \gamma Z_i + u_i \qquad (2b)$$

Where $Treat_i$ is a dummy variable for treatment status, and Z_i are a set of covariates potentially correlated with the outcome Y_i. Under pure randomization, (2a) is used for analysis, assignment to treatment is in expectation uncorrelated with ε_i, and the standard error will depend on Var(ε_i). Suppose instead that re-randomization methods are used, which force the difference in means of the covariates in Z to be less than some specified threshold $\left|\overline{Z}_{TREAT} - \overline{Z}_{CONTROL}\right| < \delta$. If δ is invariant to sample size (e.g. difference in proportions less than 0.10), then this condition will occur almost surely as the sample size goes to infinity, and thus the conditioning will not affect the asymptotics. However, in practice δ is usually set by some statistical significance threshold. Then if (2a) is used for analysis (that is, the covariates are not controlled for), we will only have that ε_i is independent of $Treat_i$ conditional on $\left|\overline{Z}_{TREAT} - \overline{Z}_{CONTROL}\right| < \delta$. The correct standard error should therefore account for this conditioning, using Var(ε_i| $\left|\overline{Z}_{TREAT} - \overline{Z}_{CONTROL}\right| < \delta$).

In practice this will be difficult to do, so adapting the minimization inference recommendations of Scott et al. (2002), we recommend researchers instead include all the variables used to check balance as linear covariates in the regression.[22] Estimation of the treatment effect in (2b) will then be conditional on the variables used for checking balance. This entails a loss of degrees of freedom compared to not controlling for these covariates, but still requires fewer degrees of freedom than pair-wise matching. The simulation results in Table 4

[21] When multiple draws are used to select the allocation which gives best balance over a sequence of 100 or 1000 draws, there may be a concern that the resulting assignment to treatment is mostly deterministic. This will be the case in very small samples (under 12 units), but is not a concern for all but the smallest trials.

[22] If an interaction or quadratic term is used to check balance (which seems rare in practice), then this same term should also be included as a regressor. Note that in the special case of re-randomization method being used to seek balance on a set of binary variables X, Y and their interaction X*Y, for which it is possible to attain exact balance on these variables, then re-randomization inference with the X, Y and X*Y as controls would be equivalent to inference after stratification on these same variables with strata dummies used as controls.

suggest that this approach works in practice. Treating the big stick or minmax t-statistic methods as if they were pure random draws results in less than ten percent of replications having p-values under 0.10 (Panel C), whereas including the variables used for checking balance as linear controls results in the correct test size (Panel D). This correction is more important for the minmax method than the big stick method, since the minmax method achieves greater baseline balance.

4.5 How do the different methods compare in terms of power for detecting a given treatment effect?

To compare the power of the different methods we simulate a treatment effect by adding a constant to the follow-up outcome variable for the treatment group. We simulate constant treatments which add 1000 Rupees (25 percent of average baseline profits) to the Sri Lankan microenterprise profits; add 920 pesos (20 percent of average baseline income) to the Mexican labor income; add 0.4 (0.5 standard deviations) to log expenditure in Indonesia, and add 0.25 standard deviations to the Pakistan math test scores and child height z-scores. For the schooling treatment, we randomly set one in three schooling drop-outs to stay in school. These treatments are all relatively small in magnitude for the sample sizes used, so that we can see differences in power across methods, rather than have all methods give power close to one.

Table 6 then summarizes the power of a hypothesis test for detecting the treatment effect, taking as the t-test on the treatment coefficient in a linear regression of the outcome variable on a constant and a dummy variable for treatment status. We report the proportion of replications where this test would reject the null hypothesis of no effect at the 10 percent level. Panels A and C report results when the regression model does not include controls for the method of randomization, while Panels B and D report the power when stratum or pair dummies, or the variables used in checking balance for re-randomization methods are included. The results for the pure random sample in panels B and D include these same set of seven baseline controls, to enable comparison of ex-post controls for baseline characteristics to ex-ante balancing.

Table 6 shows that if we do not adjust for the method of randomization the different methods often perform similarly in terms of power. In cases where they differ, the methods which pursue balance tend to have less power than pure randomization. For example, with a sample size of 30, the power for both the height and math test-scores is approximately 0.17 under a single random draw, but can be as low as 0.016 for the math test score under pair-wise matching, and as low as 0.052 for the height z-score with the minmax method. As we have seen, the size of tests is too low for persistent variables when the method of randomization is not controlled for, which makes it difficult to detect a significant effect. This translates into low power in such cases.

Adding the strata and pair dummies or baseline variables used for re-randomizing increases power in almost all cases. Some of the increases in power can be sizeable – the power increases from 0.016 to 0.320 for the math test score with pair-wise matching when the pair dummies are added. This increase in power is another reason to take into account the method of randomization when conducting analysis.

Table 6 also allows us to see the gain in power from ex-ante balancing compared to ex-post balancing. The same set of variables used for forming the match and for the re-randomization methods were added as ex-post controls when estimating the treatment effect for the single random draw in panels B and D. When the variables are not very persistent, such as the microenterprise profits and child schooling, the power is very similar whether ex-ante or ex-post balancing is done. However, we do observe some improvements in power from matching compared to ex-post controls for some, but not all, of the more persistent outcome variables. The power increases from 0.584 to 0.761 for the Mexican labor income when ex-ante pair-wise matching on seven variables is done rather than a pure random draw followed by linear controls for these seven variables ex-post. However, there is no discernable change in power from balancing for child height, another persistent outcome variable.

4.6 Can we go too far in pursuing balance?

When using stratification, matching or re-randomization methods, one question is how many variables to balance on and whether balancing on too many variables could be counter-productive. The statistical and econometric literature is not very definitive with respect to how many variables to use in stratification.[23] We therefore investigate how changing the number of

[23] For example, Duflo et al. (2006) state that "if several binary variables are available for stratification, it is a good idea to use all of them, even if some of them may not end up having large explanatory power for the final outcome." In contrast, Kernan et al. (1999) argue that "fewer strata are better", and raise the possibility of unbalanced treatment assignment within strata due to small cell sizes, recommending that an appropriate number of strata is between *n/50* and *n/100.* Finally, Therneau (1993) shows in simulations with sample sizes of 100, that with a sufficient number of

strata affects balance and power in practice in our samples of 100 and 300 observations by simulating stratification with two, three and four stratifying variables, resulting in 8, 24, and 48 strata respectively. The results are shown in Table 7. Both the size of extreme imbalances and the power do not vary much with the number of strata for any of the six outcomes. In most cases there is neither much gain, nor much loss, from including more strata. However, we do note that for a sample size of 100, when strata dummies are included, power is always slightly lower when 4 stratifying variables (and 48 strata) are included than when 3 stratifying variables (and 24 strata) are used. For example, with the math test score, power falls from 0.464 to 0.399 when the number of strata is doubled.

A question related to the choice of how many variables to balance on is what happens when one balances on irrelevant covariates. Imbens et al. (2009) prove that stratification can do no worse than pure randomization in terms of expected squared error, even when there is little or no correlation with the variables being stratified on. However, although there is no cost to stratification in terms of the variance itself, there is a cost in terms of estimation of the variance. The estimator which takes account of the stratification itself has a larger variance, which comes from the degrees of freedom adjustment. Although one could instead use the estimator for the variance which ignores the stratification, this is overly conservative, and as we have seen, results in tests of low power.

Our personal viewpoint based on this is that there is thus a possible cost of overstratifying on irrelevant variables, in that the power of the experiment to detect a significant treatment effect can be diminished as a result of the degrees of freedom adjustment. To gauge how important this might be in practice, consider first a couple of examples, using the fact that controlling for k additional variables can at most increase the estimate of the variance by (n-k-2)/(n-2). For a sample size of 100, even 10 irrelevant covariates could at most increase standard errors by 5.5 percent, equivalent to a reduction in sample size from 100 to 90. With 200 or 400 as the sample size, balancing on 5 or 10 uncorrelated covariates will not increase standard errors by more than 3 percent. However, balancing on irrelevant variables will continue to have repercussions for standard errors if the number of variables balanced on increases at the same rate as the sample size. In pair-wise matching, the number of covariates used as controls in the treatment regression

factors used in stratifying (so that the number of strata reaches *n/2*), performance can actually be worse than using unstratified randomization.

is n/2. If the variables used to form matches do not have any role in explaining the outcome of interest, we see that the ratio of standard errors will approach $\sqrt{2}$, that is, can be 41 percent higher under pair-wise matching than pure randomization.

In our simulations, we address the issue of balancing on irrelevant variables by stratifying and matching based on i.i.d. noise. The three two columns of Table 6 show the power of the stratified and matching estimators when pure noise is used. Once we control for stratum dummies, power is clearly less when irrelevant variables are used for stratifying or matching than when relevant variables are used. For example, the power with a sample size of 30 for household expenditure under pair-wise matching is 0.580 when relevant baseline variables are used to form the match compared to 0.356 when i.i.d. noise is used in the matching. Thus the choice of variables used in stratifying or matching does play an important role in determining power.

However, if we wish to compare the impact of matching or stratifying on irrelevant variables to a pure random draw, we should compare the power for a single random draw in panels A and C to the power for matching and stratifying on i.i.d. noise in panels B and D which contain controls for stratum or pair dummies. The power is very similar for all sample sizes. In practice, any given draw of i.i.d. noise is likely to have some small correlation with the outcome of interest, reducing the residual sum of squares when controlled for in a regression. It seems this small correlation is just enough to offset the fall in degrees of freedom, so that the worst-case scenarios discussed above don't come to pass.[24]. Hence in practice, it seems that stratifying on i.i.d. noise does not do any worse than a simple random draw in terms of power when sample sizes are not very small.

Finally, Table 6 shows that when stratification or matching is done purely on the basis of i.i.d. noise, treating the randomization as if it was a pure random draw does not lower power compared to the case when a single random draw is used. This is in contrast to the case when matching or stratification is done on variables with strong predictive power. Intuitively, when pure noise is used for stratification, it is as if a pure random draw was taken.[25]

[24] Note though that even our smallest sample size of 30 is larger than the cases Martin et al. (1993) study where a loss of power can occur.

[25] However, this does not mean that ex-post one can check whether the variables used for matching or stratification have predictive power for the future outcome, and if not, ignore the method of randomization. Ignoring the matching or stratification is only correct if the baseline variables are truly pure noise – if there is any signal in these stratifying or matching variables then ignoring the randomization method will result in incorrect size for hypothesis tests.

The simulation results for stratifying on i.i.d noise with 8 (48) strata and 30 (300) observations thus suggest that over-stratification is not a concern in practice when using a reasonable number of strata. In order to check what would happen in an extreme case, we also simulated stratification on i.i.d noise with 20 strata for 30 observations and 200 strata for 300 observations. In each case, one third of the strata include only one observation, reducing the number of observations that contribute to estimating the treatment effect. The results, included in the last column of Table 6, show that power is now quite a bit lower compared to a pure random draw. We thus conclude that, although in extreme cases it is possible to lose power due to over-stratification in practice it is unlikely that one would encounter this problem.

4.7 What is the meaning of the standard Table 1 (if any)?

Section 2 points out that most research papers containing randomized experiments feature a table (usually the first in the paper) that tests whether there are any statistically significant differences in the baseline means of a number of variables across treatment and control groups. The unanimous use of such tests is interesting in light of concern in the clinical trials literature about both the statistical basis for such tests, and their potential for abuse.[26] Altman (1985, p. 26) writes that when "treatment allocation was properly randomized, a difference of any sort between the two groups…will *necessarily* be due to chance…performing a significance test to compare baseline variables is to assess the probability of something having occurred by chance when we know that it did occur by chance. Such a procedure is clearly absurd." Altman (1985, p. 26) goes on to add that "statistical significance is immaterial when considering whether any imbalance between the groups may have affected the results". In particular, it is wrong to infer from the lack of statistical significance that the variable in question did not affect the outcome of the trial, since a small imbalance in a variable highly correlated with the outcome of interest can be far more important than a large and significant imbalance for a variable uncorrelated with the variable of interest.

A particular concern with the use of significance tests is that researchers may decide whether or not to control for a covariate in their treatment regression on the basis of whether it is significant. Permutt (1990) shows that the resulting test's true significance level is lower than the

[26] See also Imai, King and Stuart (2008) for discussion on this issue in social science field experiments, and for their suggestions as to what should constitute a proper check of balance.

nominal level, especially for variables which are more strongly correlated with the outcome of interest. He further shows adjusting on the basis of an initial significance test does worse than randomly choosing a covariate to adjust for. He reasons that the initial significance test tends to suppress covariate adjustment precisely where it would, on average, do some good – the cases where the adjustment would be enough to produce significance of the outcome, but where the difference in means falls short of significance. Instead greater power is achieved by always adjusting for a covariate that is highly correlated with the outcome of interest, regardless of its distribution between groups.

However, although controlling for covariates which are highly correlated with the outcome of interest will increase power and still yield consistent estimates, recent work by Freedman (2008), discussed further in Deaton (2008), shows that doing so will induce a finite-sample bias if the treatment effect is heterogeneous and correlated with the square of the covariate introduced. It therefore is of use to compare the point estimate with and without such controls. If the point estimate changes a lot when the covariate is added, then one can investigate further (using interaction models) whether the treatment effect varies with the covariate of interest.[27]

A final concern with the use of significant tests for imbalance is their potential for abuse. For example, Schulz and Grimes (2002) report that in the clinical trials literature, researchers who use hypothesis tests to compare baseline characteristics report fewer significant results than expected by chance. They suggest one plausible explanation is that some investigators may not report some variables with significant differences, believing that doing so would reduce the credibility of their reports. We have no evidence to suggest this is occurring in the development literature, and hope the profession can use this first table in a manner which doesn't lead to the temptation for such abuse. In particular, we urge referees and editors to view a lack of balance on one or two variables in a randomized experiment as simply the result of chance, not a reason per se to reject a paper.[28] And the criterion for robustness should be whether these variables are believed to be strongly correlated with the outcome of interest (authors can provide correlations

[27] Of course doing this requires a valid estimate of the standard errors. Consistent estimates are easily available, but the finite-sample properties of such estimators are not so clear. See Freedman (2008) and Deaton (2008) for further discussion.

[28] Unless there is a reason to suspect interference in the randomization, in which case a pattern of many variables showing systematic differences in means at high levels of significance may raise red flags. Another case where Table 1 could raise red flags is if there is attrition, and observations in the same strata or pair are not dropped from the analysis. In this case Table 1 could reveal whether observables are still balanced after attrition.

between baseline variables and the outcome as a guide), rather than whether the p-value for a difference in means is below 0.05 or not.

So how should we interpret such tables? The first question of interest in practice is, given that such a test shows a statistically significant difference in baseline means, does this make it more likely that there is also a statistically significant difference in follow-up means in the absence of treatment? The answer is yes, provided that the baseline data have predictive power for the follow-up outcomes (see Appendix 4).

The second question of interest is: If we observe statistical imbalance at baseline, but control for baseline variables in our analysis, are we more likely to observe imbalance at follow-up than if we had obtained a random draw which didn't show baseline imbalance? To examine this question, we take 10,000 simulations of a single random draw and divide them into two sets. The first set includes all draws that had a statistically significant difference at the 5 percent level in at least one of our 7 baseline variables. We call this the "unbalanced" set. The second set is the "balanced" set and includes all other draws. The top panels of Figure 2a and 2b show the distribution of the differences in means between treatment and control for baseline labor income and baseline math test scores are more tightly concentrated around zero in the balanced set than the unbalanced set.[29] The middle panels show that these differences are less pronounced, but still persist at follow-up, again showing that imbalance in baseline makes it more likely to have imbalance at follow-up. However, once we control for the 7 baseline variables, the distributions of a test of no treatment effect in the follow-up outcome (when no treatment was given) is identical regardless of whether or not there was baseline imbalance.

Intuitively, when randomization is used to allocate units into treatment and control groups, if we do find unbalanced baseline characteristics, once we control for them, the remaining unobservables are no more or less likely to be unbalanced than if we did not find unbalanced baseline characteristics. However, as recommended by Altman (1985), we should choose which baseline characteristics to control for not on the basis of statistical differences, but on the strength of their relationship to the outcome of interest.

5. Conclusions

[29] Appendix A3 presents the same figures for other outcome variables and sample sizes. They all show the same patterns as in Figure 9.

Our surveys of the recent literature and of the most experienced researchers implementing randomized experiments in developing countries finds that most researchers are not relying on pure randomization, but are doing something to pursue balance on observables. In addition to stratification, we find pair-wise matching and re-randomization methods to be used much more than is apparent from the existing development literature. The paper draws out implications from the existing statistical, clinical, and social science literature on the pros and cons of these various methods of seeking balance, and compares the performance of the different methods in simulations.

Our simulation results show the method of randomization matters more in small sample sizes, such as 30 or 100 observations, and matters more for relatively persistent outcome variables such as health and test scores than for less persistent outcome variables such as microenterprise profits or household expenditure. Overall we find pair-wise matching to perform best in achieving balance in small samples, provided that the variables used in forming pairs have good predictive power for the future outcomes. Stratification and re-randomization using a minmax method also lead to some improvements over a pure random draw, but in the majority of our simulations are dominated by pair-wise matching. With sample sizes of 300 we find that the method of randomization matters much less, although matching still leads to some improvement in balance for the persistent outcomes.

Our analysis of how randomization is being carried out in practice suggests several areas where the practice of randomization can be improved or better reported. This leads us to draw out the following recommendations:

1) *Better reporting of the method of random assignment is needed.* This should include a description of:
    a. Which randomization method was used and why?
    b. Which variables were used for balancing?
    c. For stratification, how many strata were used?
    d. For re-randomization, which cutoff rules were used?

   This is particularly important for experiments with small samples, where the randomization method makes more difference.
2) *Clearly describe how the randomization was carried out in practice*
    a. Who performed the randomization?

b. How was the randomization done (coin toss, random number generator, etc)?
c. Was the randomization carried out in public or private?

3) *Re-think the common use of Re-randomization.* Our simulations find pair-wise matching to generally perform as well, or better, than re-randomization in terms of balance and power, and like re-randomization, matching allows balance to be sought on more variables than possible under stratification. Adjusting for the method of randomization is statistically cleaner with matching or stratification than with re-randomization. If re-randomization is used, the authors should justify why re-randomization was preferred to the other methods of randomization.
4) *When deciding which variables to balance on, strongly consider the baseline outcome variable and geographic region dummies, in addition to variables desired for subgroup analysis*. In practice few existing studies stratify on baseline values of the outcome of interest, yet in all of our datasets, the baseline outcome variable is the one that is most strongly correlated with the future outcome. Justification for regional stratification comes from the fact that treatment implementation and shocks are likely to vary by region.
5) *Be aware that over-stratification can lead to a loss of power in extreme cases.* This is because using a large number of strata involves a downside in terms of loss in degrees of freedom when estimating standard errors, possibly more cases of missing observations, and odd-numbers within strata when stratification is used. We find a loss in power only in an extreme case where we stratify on i.i.d noise and have more strata than observations. In practice, researchers are unlikely to pursue balance to this extreme, meaning that over-stratification is unlikely to occur in practice. However, there is still a trade-off between stratifying or matching on more variables and achieving closer balance on a smaller number of variables.
6) *"As ye randomize, so shall ye analyze"* (Senn, 2004): Our simulations show that while on average failure to account for the method of randomization generally results in overly conservative standard errors, there are also a substantial number of draws in which standard errors which do not account for the method of randomization overstate the significance of the results. Moreover, failure to control for the method of randomization results in incorrect test size and low power. In general, we feel that it is important to follow a standard rule here to

avoid ex-post decision making of whether to control for the method of randomization or not. We recommend that the standard should be to control for the method of randomization.[30] Since the majority of inference in economics is model-based, rather than randomization inference, this means adding controls for all covariates used in seeking balance. That is, strata dummies should be included when analyzing the results of stratified randomization. Similarly, pair dummies should be included for matched randomization, or linear variables used for re-randomizations.

7) *In the ex-post analysis, do not automatically control for baseline variables that show a statistically significant difference in means.* The previous literature and our simulations suggest that it is a better rule to control for variables that are thought to influence follow-up outcomes, independent of whether their difference in means is statistically significantly or not. When there are several such variables and not all of them can be included in the analysis, correlations between baseline variables and follow-up data can be checked explicitly to pick the variables that are most strongly correlated with follow-up outcomes. One should still be cautious in the use of ex-post controls, given the potential for finite-sample bias if treatment heterogeneity is correlated with the square of these covariates.

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[30] If authors believe they have a valid reason not to control for stratum dummies, they should explain this reasoning in their text, and also mention what the results would be if stratum dummies were included.

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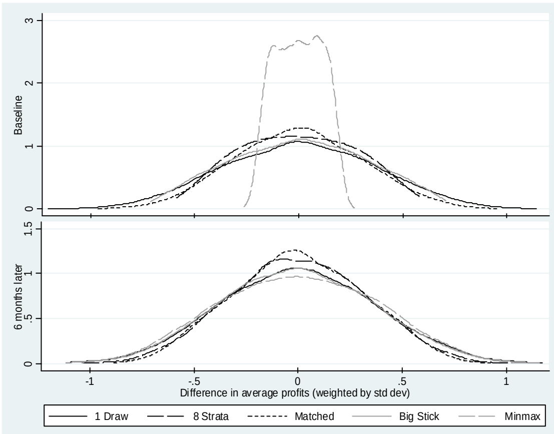
Figure 1: Distribution of Differences in Means between the Treatment and Control Groups and Baseline and Follow-up

1a: Sri Lanka Profits



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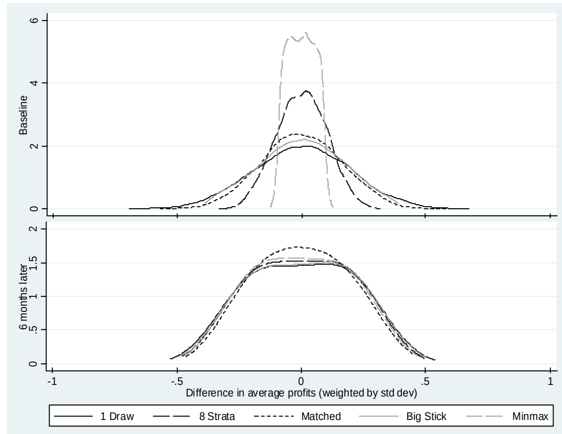

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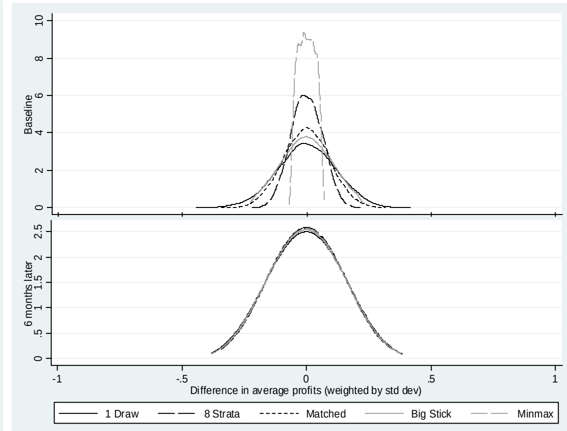
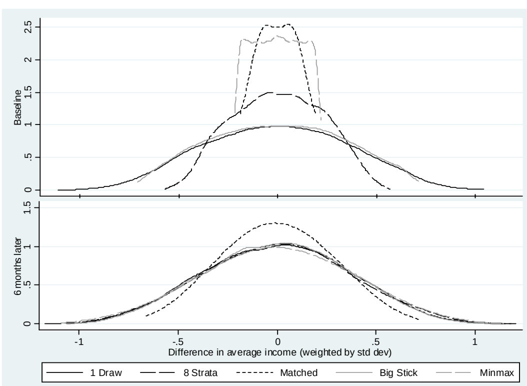


Sample Size 300

1b: Mexican ENE Labor Income



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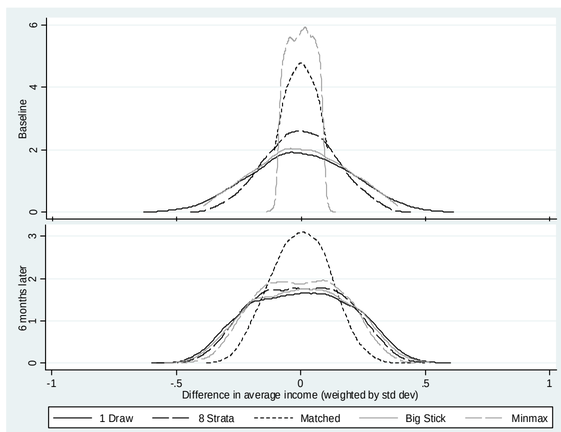

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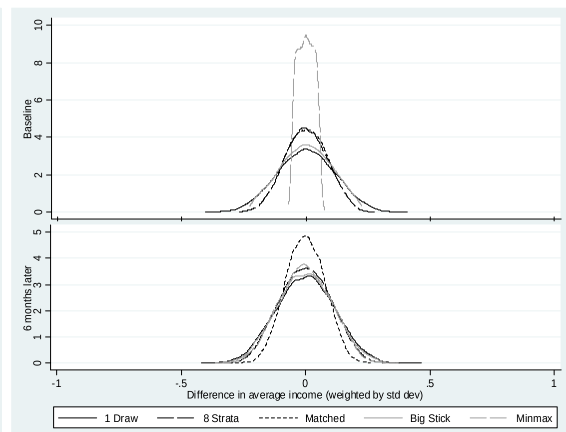
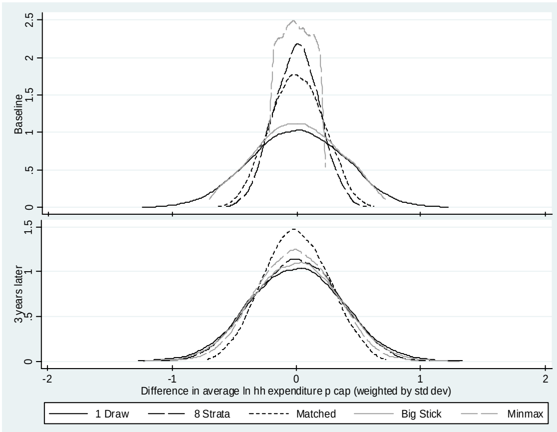


Sample Size 300

1c: IFLS Expenditure Data



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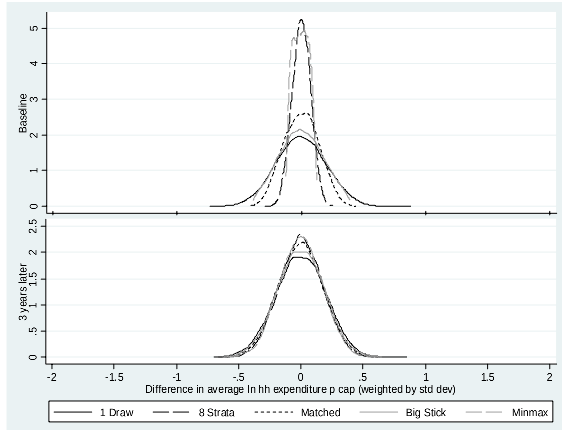

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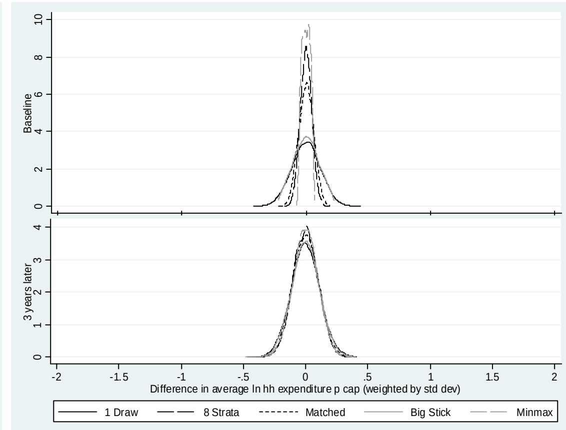
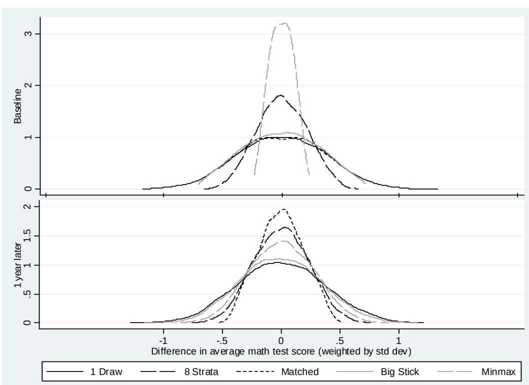


Sample Size 300

1d: LEAPS Math Test Score Data



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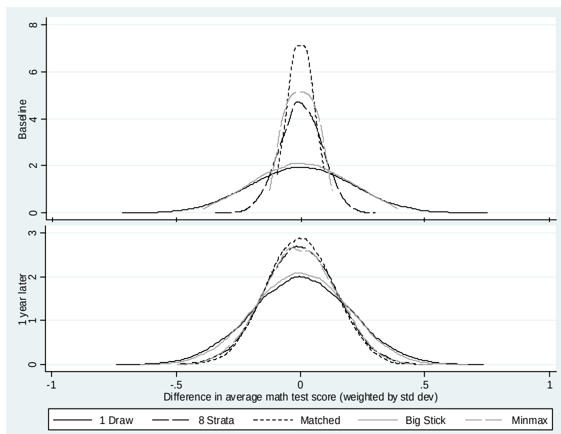

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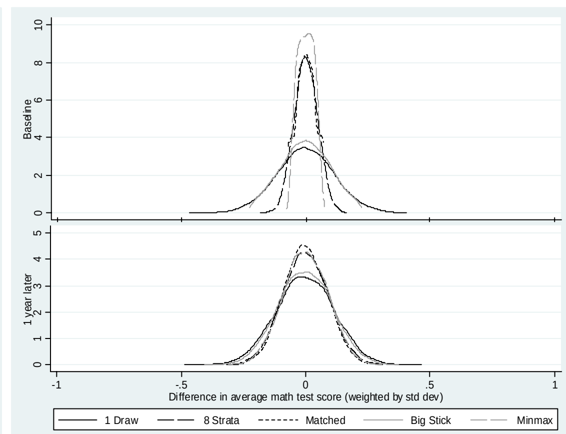
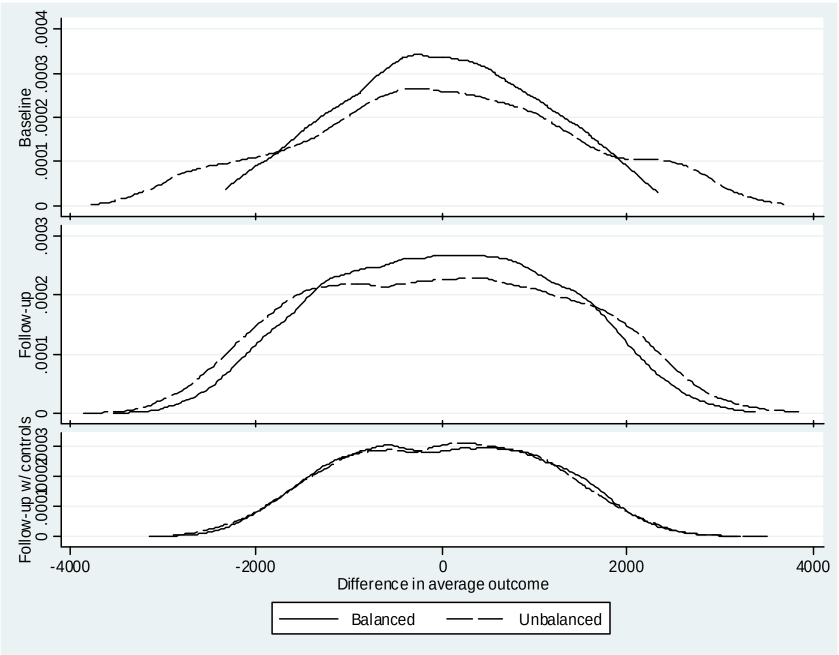


Sample Size 300

Figure 2: If we observe baseline imbalance, and control for baseline variables, is there any difference in follow-up balance?

2a: ENE Labor Income Data 100 Observations



2b: LEAPS Math Test Score Data 300 Observations

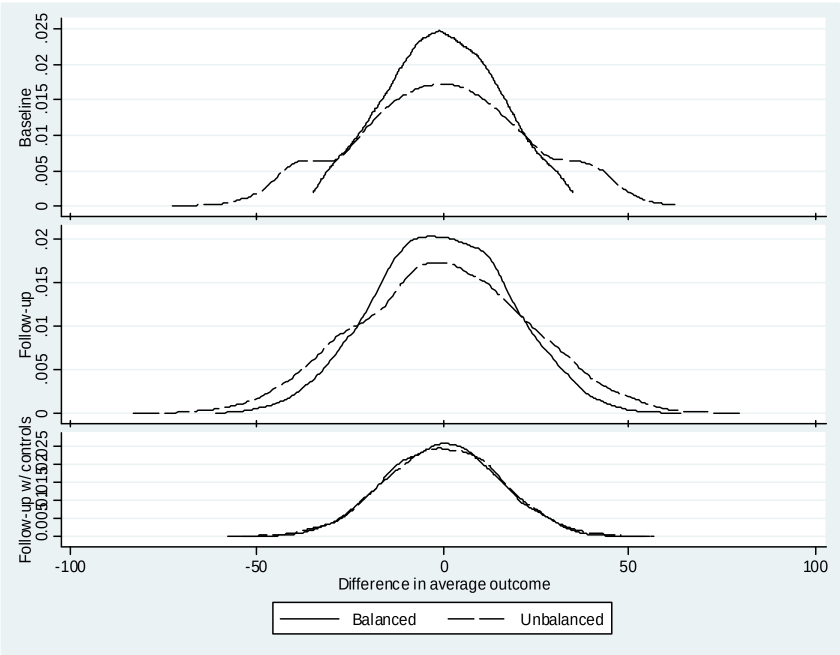


Table 1: Summary of Selected Randomized Experiments in Developing Countries

Paper	Randomization Unit	Sample Size	Number Treated	Public or Private	Stratification Used?	Matched pairs?	Number of Strata	Strata or pair dummies used?	Table for assessing balance?	# variables used to check balance	Test of significance for balance?
Published/forthcoming Papers											
Ashraf et al. (2006a)	Microfinance clients	1777	710	n.a.	No	No			Yes	12	Yes
Ashraf et al. (2006b)	Barangay (area)	10	5	n.a.	No	Yes		Yes	Yes	12	Yes
Banerjee et al. (2007)	School	98	49/49	n.a.	Yes	No	n.a.	No	Yes	4	Yes
	School	111	55/56	n.a.	Yes	No	n.a.	No	Yes	4	Yes
	School	67	32/35	n.a.	Yes	No	n.a.	No	Yes	4	Yes
Bertrand et al. (2007)	Men wanting a Driver's license	822	268/264	Public	Yes (C).	No	23	Yes	Yes	22	Yes
Bobonis et al. (2006)	Preschool cluster	155	59/51/45	n.a.	No	No			Yes	24	Yes
Field and Pande (2008)	Microfinance group	100	38/30	Public	No	No			No (A)		
Glewwe et al. (2004)	School	178	89	n.a.	Yes	No	n.a.	No	Yes	8	Yes
Miguel and Kremer (2004)	School	75	25*3	n.a.	Yes	No	n.a.	No	Yes	21	Yes
Olken (2007a)	Village	608	202/199	n.a.	Yes	No	156	Yes	Yes	10	Yes
	Subdistrict	156	n.a.	n.a.	Yes	No	50	Yes	Yes	10	Yes
Working Papers											
Ashraf et al. (2007)	Household	1260	6 groups	n.a.	Yes	No	5	Yes	Yes	14	Yes
Björkman and Svensson (2007)	Community	50	25	n.a.	Yes	No	n.a.		Yes	39	Yes
Duflo et al. (2007)	School	113	57	n.a.	Yes	No	n.a.	No (E)	Yes	15	Yes
Dupas (2006)	School	328	71	n.a.	Yes	No	n.a.	No	Yes	17	Yes
Glewwe et al. (2006)	Township	25	12	n.a.	No	Yes			Yes	4	Yes
He et al. (2007)	School division	194	97	n.a.	Yes	No	n.a.	No	Yes	22	Yes
Karlan and Valdivia (2006)	Microfinance group	239	104/84	n.a.	Yes	No	n.a.	No (D)	Yes	14	Yes
Kremer et al. (2006)	Spring	200	50/50/100	n.a.	Yes	No	n.a.	No	Yes	28	Yes
Olken (2007b)	Village	48	17	n.a.	Yes	No	2	Yes	Yes	8	Yes

Notes:
n.a. denotes information not available in the paper.
A: Paper says check was done on a number of variables and is available upon request.
C: It appears randomization was done within recruitment session, but the paper was not clear on this.
D: Dummies for location are included, but not for credit officer which was the other stratifying variable.
E. Dummies for district are included, but not for the number of households in the area which were also used for stratifying within district.

Table 2: Survey Evidence on Randomization Methods Used by Leading Researchers

	% WHO HAVE EVER USED			% Using Method in Most Recent Experiment
	Unweighted	Weighted	5+ experiment Group	
Single Random Assignment to Treatment (possibly with stratification)	80	84	92	39.1
Subjectively deciding whether to redraw	32	52	46	4.3
Using a statistical rule to decide whether to redraw	12	15	15	0.0
Carrying out many random assignments, and choosing best balance	24	45	38	17.4
Explicitly matching pairs of observations on baseline characteristics	56	52	54	39.1
Number of Researchers	25	25	13	23

Notes:
Methods described in more detail in the paper.
Weighted results weight by the number of experiments the researcher has participated in
5+ experiment group refers to researchers who have carried out 5 or more randomized experiments

Table 3: How do the different methods compare in terms of Baseline Balance?
Simulation Results for 100 Observation Sample Size

	Single Random Draw	Stratified on 2 variables	Stratified on 4 variables	Pairwise Greedy Matching	Big Stick Rule	Draw with minmax t-stat out of 1000 draws
Panel A: Average difference in BASELINE between treatment and control means (in std. dev.)						
Microenterprise profits (Sri Lanka)	0.001	0.000	-0.001	-0.003	0.001	0.000
Household expenditure (Indonesia)	-0.002	0.001	-0.001	-0.002	-0.001	-0.002
Labor income (Mexico)	0.000	0.000	0.000	0.000	-0.001	0.000
Height z-score (Pakistan)	0.001	0.001	0.000	0.000	-0.001	0.000
Math test score (Pakistan)	0.003	0.000	-0.001	0.000	0.002	0.000
Baseline unobservables (Sri Lanka)	0.000	0.000	0.000	0.000	0.000	0.001
Baseline unobservables (Mexico)	0.000	0.000	0.000	0.000	0.000	0.000
Panel B: 95th percentile of difference in BASELINE between treatment and control means (in std. dev.)						
Microenterprise profits (Sri Lanka)	0.386	0.195	0.241	0.313	0.324	0.091
Household expenditure (Indonesia)	0.390	0.145	0.191	0.268	0.328	0.107
Labor income (Mexico)	0.384	0.280	0.304	0.100	0.332	0.088
Height z-score (Pakistan)	0.395	0.160	0.206	0.102	0.319	0.089
Math test score (Pakistan)	0.392	0.164	0.237	0.074	0.328	0.106
Baseline unobservables (Sri Lanka)	0.434	0.417	0.414	0.434	0.434	0.434
Baseline unobservables (Mexico)	0.457	0.448	0.439	0.457	0.457	0.427
Panel C: Proportion of p-values <0.1 for testing difference in BASELINE means						
Microenterprise profits (Sri Lanka)	0.097	0.000	0.005	0.037	0.045	0.000
Household expenditure (Indonesia)	0.102	0.000	0.000	0.013	0.049	0.000
Labor income (Mexico)	0.100	0.015	0.029	0.000	0.053	0.000
Height z-score (Pakistan)	0.100	0.000	0.001	0.000	0.038	0.000
Math test score (Pakistan)	0.100	0.000	0.006	0.000	0.048	0.000
Baseline unobservables (Sri Lanka)	0.101	0.096	0.095	0.082	0.098	0.091
Baseline unobservables (Mexico)	0.108	0.095	0.093	0.103	0.102	0.090

Notes:
Statistics are based on 10,000 simulations of each method. Details on methods and variables are in Table A2.

Table 4: How do the different methods compare in terms of Balance on Future Outcomes?

	Sample Size of 30					*Sample Size of 300*					
	Single Random Draw	Stratified on 2 variables	Pairwise Greedy Matching	Big Stick Rule	Draw with minmax t-stat	Single Random Draw	Stratified on 2 variables	Stratified on 4 variables	Pairwise Greedy Matching	Big Stick Rule	Draw with minmax t-stat
Panel A: Average difference in FOLLOW-UP between treatment and control means (in std. dev.)											
Microenterprise profits (Sri Lanka)	0.001	0.000	0.001	-0.003	0.002	0.000	0.001	0.001	0.000	0.000	0.000
Child schooling (Indonesia)	-0.005	-0.010	-0.002	0.004	-0.006	0.002	0.003	-0.001	0.000	-0.002	-0.002
Household expenditure (Indonesia)	0.000	0.002	0.002	0.000	-0.006	-0.001	-0.001	0.000	-0.001	-0.001	-0.001
Labor income (Mexico)	-0.003	0.000	0.000	0.000	0.001	0.001	0.000	0.001	-0.001	0.001	-0.002
Height z-score (Pakistan)	0.007	0.001	0.004	-0.003	0.001	-0.001	0.000	0.000	0.000	0.002	0.000
Math test score (Pakistan)	0.001	0.002	0.001	-0.003	0.005	-0.001	0.000	0.000	-0.001	-0.001	0.001
Panel B: 95th percentile of difference in FOLLOW-UP between treatment and control means (in std. dev.)											
Microenterprise profits (Sri Lanka)	0.713	0.627	0.556	0.705	0.708	0.220	0.210	0.209	0.211	0.216	0.224
Child schooling (Indonesia)	0.834	0.745	0.556	0.556	0.556	0.213	0.219	0.212	0.227	0.227	0.196
Household expenditure (Indonesia)	0.721	0.643	0.496	0.677	0.590	0.226	0.194	0.196	0.200	0.219	0.198
Labor income (Mexico)	0.703	0.713	0.503	0.688	0.704	0.227	0.196	0.198	0.149	0.213	0.195
Height z-score (Pakistan)	0.710	0.620	0.557	0.620	0.443	0.222	0.186	0.189	0.189	0.212	0.225
Math test score (Pakistan)	0.717	0.448	0.350	0.648	0.525	0.227	0.180	0.184	0.167	0.209	0.175
Panel C: Proportion of p-values <0.1 for testing difference in FOLLOW-UP means with inference as if pure randomization was used (e.g. no adjustment for strata or match dummies)											
Microenterprise profits (Sri Lanka)	0.105	0.059	0.027	0.101	0.109	0.100	0.080	0.080	0.085	0.092	0.103
Child schooling (Indonesia)	0.052	0.113	0.033	0.041	0.010	0.121	0.087	0.082	0.098	0.111	0.096
Household expenditure (Indonesia)	0.102	0.069	0.014	0.083	0.046	0.101	0.056	0.052	0.064	0.092	0.059
Labor income (Mexico)	0.101	0.106	0.007	0.093	0.103	0.100	0.056	0.062	0.011	0.087	0.028
Height z-score (Pakistan)	0.097	0.056	0.030	0.059	0.007	0.097	0.044	0.049	0.049	0.081	0.097
Math test score (Pakistan)	0.101	0.006	0.000	0.072	0.022	0.101	0.038	0.042	0.028	0.076	0.032
Panel D: Proportion of p-values <0.1 for testing difference in FOLLOW-UP means with inference which takes account of randomization method (i.e. controls for stratum, pair, or re-randomizing variables)											
Microenterprise profits (Sri Lanka)	0.103	0.091	0.098	0.103	0.122	0.098	0.103	0.133	0.103	0.102	0.101
Child schooling (Indonesia)	0.103	0.117	0.033	0.098	0.108	0.098	0.102	0.104	0.098	0.104	0.104
Household expenditure (Indonesia)	0.102	0.098	0.097	0.101	0.094	0.099	0.100	0.099	0.101	0.105	0.100
Labor income (Mexico)	0.102	0.109	0.107	0.102	0.117	0.100	0.095	0.101	0.104	0.100	0.112
Height z-score (Pakistan)	0.100	0.097	0.101	0.100	0.103	0.094	0.097	0.097	0.098	0.095	0.102
Math test score (Pakistan)	0.099	0.102	0.103	0.098	0.098	0.101	0.097	0.099	0.097	0.100	0.102

Notes:
Panels A and B coefficients are for specifications without controls for stratum or pair dummies.
Statistics are based on 10,000 simulations of each method. Details on methods and variables are in Table A2.

Table 5: Is it always conservative to ignore the method of randomization?
Proportion of replications where controlling for stratum or pair dummies lowers the p-value on a test of difference in means between treatment and control groups

	Stratified on 2 variables	Stratified on 4 variables	Pairwise Greedy Matching	Big Stick Rule	Draw with minmax t-stat
Panel A: Sample Size 30					
Microenterprise profits (Sri Lanka)	0.690	.	1.000	0.493	0.555
Child schooling (Indonesia)	0.373	.	0.686	0.567	0.854
Household expenditure (Indonesia)	0.622	.	1.000	0.523	0.657
Labor income (Mexico)	0.477	.	1.000	0.496	0.532
Height z-score (Pakistan)	0.579	.	1.000	0.537	0.825
Math test score (Pakistan)	0.684	.	1.000	0.522	0.740
Panel B: Sample Size 300					
Microenterprise profits (Sri Lanka)	0.668	0.731	1.000	0.526	0.689
Child schooling (Indonesia)	0.705	0.634	1.000	0.506	0.674
Household expenditure (Indonesia)	0.869	0.733	1.000	0.522	0.738
Labor income (Mexico)	0.874	0.712	1.000	0.525	0.725
Height z-score (Pakistan)	0.860	0.655	1.000	0.522	0.754
Math test score (Pakistan)	0.882	0.735	1.000	0.533	0.776

Notes:
Statistics are based on 10,000 simulations of each method. Details on methods and variables are in Table A2.

Table 6: How do the different methods compare in terms of Power in detecting a given treatment effect?

	Sample Size of 30							
	Single Random Draw	Stratified on 2 variables	Pairwise Greedy Matching	Big Stick Rule	Draw with minmax t-stat	Stratified on i.i.d noise	Matching on i.i.d. noise	20 strata i.i.d. noise
Panel A: Proportion of p-values<0.10 when no adjustment is made for method of randomization								
Microenterprise profits (Sri Lanka)	0.144	0.106	0.095	0.139	0.154	0.132	0.086	0.111
Child schooling (Indonesia)	0.123	0.146	0.111	0.115	0.066	0.116	0.144	0.119
Household expenditure (Indonesia)	0.390	0.382	0.342	0.382	0.360	0.388	0.387	0.391
Labor income (Mexico)	0.181	0.177	0.098	0.178	0.184	0.177	0.203	0.208
Height z-score (Pakistan)	0.174	0.134	0.133	0.134	0.052	0.195	0.194	0.193
Math test score (Pakistan)	0.167	0.051	0.016	0.139	0.087	0.154	0.131	0.193
Panel B: Proportion of p-values<0.10 when adjustment is made for randomization method (and for the single random draw controls for the seven baseline variables are added to the regression)								
Microenterprise profits (Sri Lanka)	0.130	0.135	0.153	0.131	0.167	0.144	0.164	0.109
Child schooling (Indonesia)	0.109	0.131	0.121	0.112	0.095	0.118	0.144	0.149
Household expenditure (Indonesia)	0.409	0.424	0.580	0.419	0.461	0.387	0.356	0.280
Labor income (Mexico)	0.164	0.172	0.242	0.167	0.196	0.165	0.173	0.125
Height z-score (Pakistan)	0.246	0.201	0.206	0.251	0.281	0.161	0.157	0.142
Math test score (Pakistan)	0.183	0.313	0.320	0.187	0.217	0.159	0.170	0.129

	Sample Size of 300								
	Single Random Draw	Stratified on 2 variables	Stratified on 4 variables	Pairwise Greedy Matching	Big Stick Rule	Draw with minmax t-stat	Stratified on i.i.d noise	Matching on i.i.d. noise	200 strata i.i.d. noise
Panel C: Proportion of p-values<0.10 when no adjustment is made for method of randomization									
Microenterprise profits (Sri Lanka)	0.288	0.274	0.278	0.267	0.280	0.280	0.285	0.279	0.278
Child schooling (Indonesia)	0.606	0.585	0.562	0.607	0.597	0.600	0.560	0.610	0.555
Household expenditure (Indonesia)	0.999	0.999	1.000	1.000	0.999	1.000	0.999	0.999	0.999
Labor income (Mexico)	0.494	0.486	0.480	0.475	0.489	0.474	0.479	0.484	0.489
Height z-score (Pakistan)	0.728	0.757	0.756	0.766	0.743	0.767	0.727	0.728	0.739
Math test score (Pakistan)	0.615	0.654	0.650	0.655	0.619	0.657	0.631	0.624	0.620
Panel D: Proportion of p-values<0.10 when adjustment is made for randomization method (and for the single random draw controls for the seven baseline variables are added to the regression)									
Microenterprise profits (Sri Lanka)	0.301	0.305	0.343	0.290	0.302	0.309	0.283	0.338	0.295
Child schooling (Indonesia)	0.608	0.596	0.589	0.602	0.600	0.595	0.458	0.607	0.403
Household expenditure (Indonesia)	1.000	1.000	1.000	1.000	1.000	1.000	0.999	0.998	0.994
Labor income (Mexico)	0.584	0.561	0.541	0.761	0.584	0.582	0.501	0.602	0.408
Height z-score (Pakistan)	0.863	0.849	0.854	0.853	0.867	0.866	0.741	0.721	0.642
Math test score (Pakistan)	0.812	0.792	0.781	0.829	0.816	0.826	0.630	0.603	0.460

Notes:
Statistics are based on 10,000 simulations of each method. Details on methods and variables are in Table A2.
Stratifications on i.i.d noise are for 8 (48) strata in the sample of 30 (300) observations.
Simulated treatment effects are as follows
Microenterprise profits: A 1,000 Sri Lankan Rupee increase in profits (about 25% of average baseline profits)
Child schooling: One in three randomly selected children in the treatment group who would have dropped out don't
Household expenditure: An increase of 0.4 in ln household expenditure per capita, which corresponds to about one half a standard deviation or moving a household from the 25th to the 50th percentile.
Labor income: A 920 Peso increase in income (about 20% of average baseline income)
Height z-score: An increase of one quarter of a standard deviation in the z-score, where the z-score is defined as standard deviations from mean US height for age
Math test score: An increase of one quarter of a standard deviation in the test score

Table 7: How does stratification vary with the number of strata?
Simulation results

	Sample Size 100			Sample Size 300		
	Stratified on 2 variables (8 strata)	Stratified on 3 variables (24 strata)	Stratified on 4 variables (48 strata)	Stratified on 2 variables (8 strata)	Stratified on 3 variables (24 strata)	Stratified on 4 variables (48 strata)
Panel A: Imbalance - 95th percentile of difference in follow-up means						
Microenterprise profits (Sri Lanka)	0.322	0.338	0.338	0.210	0.213	0.209
Child schooling (Indonesia)	0.399	0.346	0.369	0.219	0.211	0.212
Household expenditure (Indonesia)	0.337	0.335	0.343	0.194	0.193	0.191
Labor income (Mexico)	0.335	0.327	0.344	0.196	0.196	0.198
Height z-score (Pakistan)	0.297	0.299	0.310	0.186	0.191	0.189
Math test score (Pakistan)	0.285	0.298	0.316	0.180	0.181	0.184
Panel B: Power: Proportion of p-values<0.10 when no strata dummies included						
Microenterprise profits (Sri Lanka)	0.129	0.138	0.144	0.274	0.281	0.278
Child schooling (Indonesia)	0.303	0.267	0.273	0.585	0.574	0.562
Household expenditure (Indonesia)	0.852	0.850	0.845	0.999	1.000	1.000
Labor income (Mexico)	0.170	0.161	0.180	0.486	0.486	0.480
Height z-score (Pakistan)	0.286	0.295	0.297	0.757	0.757	0.756
Math test score (Pakistan)	0.236	0.245	0.254	0.654	0.649	0.650
Panel C: Power: Proportion of p-values<0.10 when strata dummies included						
Microenterprise profits (Sri Lanka)	0.186	0.273	0.242	0.305	0.327	0.343
Child schooling (Indonesia)	0.278	0.301	0.255	0.596	0.596	0.589
Household expenditure (Indonesia)	0.904	0.914	0.876	1.000	1.000	1.000
Labor income (Mexico)	0.204	0.212	0.199	0.561	0.551	0.541
Height z-score (Pakistan)	0.487	0.463	0.457	0.849	0.843	0.854
Math test score (Pakistan)	0.464	0.464	0.399	0.792	0.790	0.781

Notes:
Statistics are based on 10,000 simulations of each method. Details on methods and variables are in Table A2.