The First Deal:
The Division of Founder Equity in New Ventures

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January 2016[3]

Forthcoming in Management Science

Abstract: We examine the trade-off between efficiency and equality within the context of entrepreneurial founding teams. Using a formal theory where founders may have preferences over relative outcomes, we derive predictions about the antecedents and consequences of dividing equity equally amongst all founders. Using proprietary survey data we empirically test the predictions. Our central finding is that teams that split equity equally are less likely to raise funds from outside investors. The relationship appears not to be causal, but instead driven by selection effects across heterogeneous teams with varying degrees of inequality aversion.

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[3] We would like to thank David Hsu, Bill Kerr, Josh Lerner, Jason Snyder, Veikko Thiele and seminar participants at Arizona State University (W.P. Carey), Harvard University (HBS), Massachusetts Institute of Technology (Sloan), National Bureau of Economic Research, New York University (Stern), Stanford University, University of Arizona (Eller), University of British Columbia (Sauder), University of California Los Angeles (Anderson), University of Cape Town (GSB), University of Illinois at Urbana-Champaign, and the University of Toronto (Rotman) for their helpful comments. We thank Xiang Ao, Andrew Hobbs, Bill Holodnak, Aaron Lapat, Furqan Nazeeri, and Mike DiPierro for their valuable research support. Financial support from the Harvard Business School and SSHRC is gratefully acknowledged. Author names are in alphabetic order; both authors contributed equally to this paper. All errors are ours.

Section 1: Introduction

The trade-off between efficiency and equality is fundamental to society. One managerial application concerns members of a team deciding between equal allocations that may appeal to notions of fairness, versus unequal allocations that may help with efficiency. In this paper we consider this trade-off in the context of entrepreneurial founding teams. One of the first major decisions facing those teams is how to allocate their founder equity. In this paper we provide a theory and empirical evidence of how founding teams deal with this fundamental trade-off.

In a standard neoclassical framework, an equal split of founder equity should not really happen, because the probability that all founders in a team have identical characteristics is infinitesimally small. However, recent work on inequity-aversion argues that some (but not all) players have a preference for equal allocations; see Fehr and Schmidt (2006) and Dawes, Fowler et al. (2007). This literature has made robust progress on theoretical and experimental fronts, although direct empirical evidence remains scant.

In this paper we first develop a theory and then test it on data about founder agreements. The theory examines the role of "outcome-inequality aversion," and shows how this can explain equal splitting as an equilibrium outcome in asymmetric founding teams. We also look at the process by which founding teams reach an agreement, distinguishing between a scenario where founders acquire costly information about each other or not. We then use the model to generate predictions about the performance implications of optimal contract choices. The main predictions are that teams with higher outcome inequality aversion are less likely to engage in costly discovery, more likely to split the equity equally, and less likely to achieve their performance milestones.

To test our theory, we leverage an annual survey of North American technology startups. We use survey data for the period 2008-2013, resulting in a sample of 1367 companies, consisting of 3782 founders. We create empirical proxies for all the key parameters of the theory model. To measure variation in outcome inequality aversion we identify those teams that are composed of family members, arguing that outcome inequality aversion ought to be higher (on average) within family teams. The survey contains data on the original allocation of founder equity; we find that 32% of all teams split the founder equity equally, also known as the "1/n rule." We infer the discovery process by looking at the speed with which founding teams negotiate their agreements. A stark finding is that 42% of all teams decide on their equity split within a day or less. Finally we measure performance milestones by looking at whether companies manage to raise funds from outside investors in general, and also venture capitalists more specifically. The analysis includes a rich set of control variables.

Consistent with the predictions from theory, regressions reveal that family teams are more likely to negotiate quickly and more likely to agree to an equal split. We find a strong relationship between equal splitting and quick negotiations. In terms of performance, we find a negative and statistically significant relationship between equal splitting and our measures of fundraising success. The effect of family teams on performance is also negative.

We caution against jumping to the conclusion that equal splitting causes lower performance, as our theory suggests that it is an endogenous choice. Empirically we want to disentangle selection and treatment effects. We leverage our theory, which suggests two types of instrumental variables that satisfy the exclusion restriction. First, asymmetries in the resources provided by different founders increase the likelihood of equal splitting without affecting performance directly (empirically we control for the joint resources provided). Second, variation in the cost of discovering founder asymmetries (empirically we proxy these using the speed of negotiation) affects performance only through its effect on founder equity. Our theory makes an ambiguous prediction about the causal effect of equal splitting, showing how it may benefit in some circumstances but hurt in others. Empirically we find that the instrumented coefficient of equal splitting is invariably insignificant. Consistent with our theory, we attribute the negative correlation between equal splitting and performance to a selection effect, where those teams that select equal splits are more likely to have higher outcome inequality aversion and consequently lower expected performance. We provide numerous empirical tests to show the robustness of our main findings, and engage in an extensive discussion of our interpretation versus alternative explanations.

Our work is related to the work of Ruef, Aldrich et al. (2003) and Ruef (2010), which explores determinants of team formation and production, and provides descriptive data on equity arrangements within small businesses. However, this work does not focus on founder agreements and the associated performance implications. A related prior literature examines the role of founder experience on the evolution of founding teams. Beckman and Burton (2008) show how the prior career history of founders affects their subsequent firm evolution. Wasserman and Marx (2008) highlight the differing effects of prior relationships – e.g., prior social relationships vs. prior professional relationships – on founding-team stability. A large literature examines the relationship between team composition and firm performance. Beckman, Burton et al. (2007) argue that founding team diversity and team completeness is associated with higher rates of going public. Using a sample of 223 Swedish new ventures, Delmar and Shane (2006) find that the founding team's prior experience enhances survival and sales, though in a nonlinear fashion. Shane and Stuart (2002) use data on 134 MIT-related startups to show that founders' initial social capital makes it more likely that the startup will attract venture capital, will be less likely to fail, and increase the chances of the company's going public. Eesley, Hsu et al. (2013) demonstrate the importance of alignment between founding team composition and commercialization strategy. Åstebro and Serrano (2011) examine the benefits of starting a company with multiple founders rather than as a solo founder. Tamvada and Shrivastava (2011) examine the relationship between team size and team performance. Our research also shares some similarities with the literature on joint ventures. Hauswald and Hege (2006) examine ownership and control rights for joint ventures between established firms, and find a high incidence of equal share divisions. For related work on joint ventures, see Robinson and Stuart (2007), Dyer, Singh et al. (2008), and Gulati and Wang (2003). Finally, our formal theory model is related to the theory of optimal founder contracts in Hellmann and Thiele (forthcoming). The key innovation here is that we allow for outcome inequality aversion, and derive its implication for optimal contacts and performance.[4]

Section 2 discusses the theory model and derives predictions that guide our empirical analysis. Section 3 describes the data. Section 4 discusses the empirical results. Section 5 examines alternative interpretations. It is followed by a brief conclusion. The Appendix is available from the authors upon request.

Section 2: Theory

2.1 Model Assumptions

We now introduce a formal model of founder contracting that explicitly examines the efficiency-inequality trade-off. We chose the simplest possible model that simultaneously captures three essential elements. First, our model includes a distribution of exogenous team characteristics. We focus especially on differences in the degree of outcome inequality aversion. Second, our model allows for endogenous choices about founder contracts and the

[4] Beyond the literature on inequity aversion (Fehr and Schmidt 2006), a small economics literature discusses alternative reasons for compensating unequal agents equally (Encinosa, Gaynor et al. 2007; Bose, Pal et al. 2010). The problem of dividing shares is closely related to the "division of a pie" problem that has been studied extensively in game-theoretic literature (e.g., Rubinstein 1982; Binmore, Rubinstein et al. 1986).

discovery of information. Specifically, teams need to decide on how to split the founder equity, and whether or not to engage in a costly discovery process about each other. Third, our model generates predictions about the expected venture performance and its comparative statics.

In our model there are two founders, A and B.[5] There are three game stages. At the first contracting stage, the cofounders engage in costly discovery or not, and forge an agreement on founder equity. At the second stage there is an incentive problem where founders need to exert private effort to increase the likelihood of venture success. At the third stage the venture either succeeds in reaching a performance milestone, or else fails.

We allow for the possibility that cofounders can be either symmetric, so that both founders contribute equally, or asymmetric, so that one founder contributes more value than the other. We model founder differences in terms of project-specific skill differences. We assume that founder A can have one of two possible skill levels, high (denoted ϕ_H) or low (denoted ϕ_L with $\phi_L<\phi_H$). For tractability we assume that founder B always has low skills (ϕ_L). After discovery founders may be symmetric (i.e., $\phi_L^A= \phi_L^B$) or asymmetric (i.e., $\phi_H^A>\phi_L^B$). Prior to discovery, A has a probability q of being high skill. This is the simplest possible set up that allows for both symmetric and asymmetric teams. In section 2.6 we discuss this in greater detail.

We assume that at the time of founding, founders do not required any outside capital. However, the two founders may contribute some of their own resources, such as capital or intellectual property. We denote the total cost of providing these resources by R, and A's share of these costs by $(½+r)R$. The parameter r ($\in [-½,½]$) therefore describes potential founder resource asymmetries. Initially we assume that both founders contribute equally, i.e., r=0. However, where appropriate (such as for Propositions 4 and 7) we discuss how the model changes when we allow for a more general distribution of r.

The first decision at the contracting stage concerns skill discovery. More information allows cofounders to better adjust their equity stakes to their relative skills. For simplicity we assume that discovery costs are borne jointly by the two founders and incurred whenever it is jointly efficient to do so. We denote discovery costs by k and assume that they have some cumulative distribution function K(k) over $[0, \infty]$.[6]

The second contracting decision concerns the division of equity. By definition the cofounders' joint equity sums to 1 (i.e., 100%). It is convenient to denote the division of equity in terms of A's share premium σ, so that A gets an equity stake of ½ + σ and B of ½ - σ. Our focus will be on whether founders choose an equal split (σ=0) or an unequal split (σ≠0). Similar to Hellmann and Thiele (forthcoming) we assume that partners are wealth constrained and only bargain over σ. In the Appendix we show that in our model partners never want to renegotiate their original agreement. This result is specific to our model; in the model of Hellmann and Thiele (forthcoming) renegotiation occurs for some but not for other parameter regions. We discuss this further in section 5.

Moving on to the second stage, we use a standard moral hazard specification where each founder can provide effort e_i (i=A,B) at a private effort cost of $c(e_i)=½\psi(e_i)^2$. These efforts impact the probability of success, which is given by p =

[5] While our empirical analysis will allow for teams of more than two founders, for tractability the theory focuses on the case of two founders.
[6] There are multiple interpretations for k. It can represent direct costs of gathering information, or the opportunity costs of time lost in lengthy discussions. It can also represent "conflict avoidance," i.e., the discomfort of engaging in uncomfortable debates about the relative worth of cofounders. Note also that in this simple model discovery costs are exogenous and fixed. Hellmann and Thiele (forthcoming) derive endogenous costs of discovery that are generated by a threat of inefficient idea stealing.

$\phi_A e_A + \phi_B e_B$, so that success is a function of efforts and skills. The joint returns in case of success are normalized to 1. With (1-p) the project fails and generates no returns.

A key feature of our model is the presence of outcome inequality aversion (OIA henceforth). We discuss this assumption in great detail in section 2.7. The assumptions is that founders care not only about individual returns, but also about the comparison with their co-founders. There is a disutility of receiving a lower (higher) share than the other, which we can think of as "resentment" or "guilt." For simplicity we assume that both founders have the same OIA parameter, denoted by α (≥ 0).[7] However, different teams can have different OIA parameters. Specifically, we assume that there is a distribution $\Omega(\alpha)$ with $\alpha \in [0,\infty]$). Different teams may have different social structures and preferences that generate different degrees of OIA, consistent with the findings of Fehr and Schmidt (1999) who emphasize the heterogeneous nature of inequity aversion.

In case of success the founder utilities are denoted by $w_A = (½ + \sigma) - \alpha|\sigma|$ and $w_B = (½ - \sigma) - \alpha|\sigma|$, where the absolute value of σ measures the amount of outcome inequality, and α measures the extent of OIA. The expected utility at the effort stage is denoted by $v_i(j) = p(j)w_i - c_i$, which through p(j) depends on the skill realization j=[[[H,L]]]. At the initial stage the expected utilities are given by $u_i = q\, v_i(H)+(1-q)v_i(L)$.

The optimal equity split with discovery is found by maximizing $v(j) = v_A(j)+ v_B(j)$ for each realization of j. Without discovery the optimal equity split is found by maximizing $u = u_A + u_B$ without knowledge of the realization of j.

2.2: Optimal choices

In this section we derive optimal choices, looking at private efforts, the division of founder equity, and the discovery decision. Solving the model backward, we first derive the optimal private efforts. We find that $e_i = \phi_i w_i/\psi$, so that individual effort is determined by the final utility of success w_i (which includes the disutility from inequality) as well as the individual skill ϕ_i.

For the optimal allocation of founder equity, consider first the case with discovery. For each realization j the founders determine their optimal division of equity by maximizing v(j). For symmetric skills (j=L) equal splitting is always optimal, i.e., $\sigma^*=0$. For asymmetric skills (j=H), however, the optimal premium σ^* depends on skill differences as well as the degree of inequality aversion. In the absence of inequality aversion ($\alpha=0$) the optimal contract gives a positive equity premium to the higher skilled partner (i.e., $\sigma^*>0$). This creates efficiency gains by soliciting higher effort from the more skilled partner. For an intermediate region of $\alpha \in (0, \alpha_1)$ (the analytical expression of α_1 can be found in the appendix) we find that the optimal equity premium σ^* is decreasing in α. This is because efficiency gains are traded off against the team's dislike of unequal outcomes. For sufficiently high values of α (i.e., $\alpha > \alpha_1$), the optimal contract specifies a zero equity premium (i.e., $\sigma^*=0$). In this case the two partners mutually agree to an equal split, because their dislike of unequal outcomes trumps potential efficiency gains.

In the case of no discovery, the optimal premium is again affected by the fundamental trade-off between efficiency from asymmetric ownership versus equality from symmetric ownership, but it also takes into account the skill uncertainty. Consider again the optimal premium σ^* under j=H from above, but now allow for the possibility that the two partners may in fact be symmetric (j=L). This possibility makes a positive premium less attractive, because with symmetric skills it is suboptimal. In the appendix we formally show that the optimal premium without discovery

[7] In section 2.7 we discuss how to extend the model.

is always lower than the optimal premium with discovery of asymmetries (j=H). We also show that without discovery the critical value α_0, above which founders always chose equal splitting, is lower than with discovery, i.e., $\alpha_0 < \alpha_1$. This says that without discovery, founders agree more easily on an equal split. Based on the critical values α_0 and α_1 our model generates three parameter regions that we call the low OIA region ($\alpha<\alpha_0$), medium OIA region ($\alpha_0<\alpha<\alpha_1$) and high OIA region ($\alpha>\alpha_1$). In the appendix we formally derive the comparative statics; here we note that greater skill differences (ϕ_H/ϕ_L) increase both the low and medium OIA region, and higher values of q increase the low OIA region.

Figure 1 below summarizes the optimal equity splits for these three regions, broken down by whether partners are symmetric of not, and by whether discovery occurs or not.

Figure 1

Discovery	Partners	Low OIA	Medium OIA	High OIA
No	Symmetric	Unequal Splits	Equal Splits	Equal Splits
No	Asymmetric	Unequal Splits	Equal Splits	Equal Splits
Yes	Symmetric	Equal Splits	Equal Splits	Equal Splits
Yes	Asymmetric	Unequal Splits	Unequal Splits	Equal Splits

Consider finally the optimal discovery decision. The benefit of discovery is given by the differences in expected utilities between state-contingent versus non-contingent contracts. Discovery is optimal whenever the difference in joint utilities exceeds the discovery costs k. Having thus described the optimal effort, optimal contracting, and optimal discovery choices, we are now in a position to derive the main model predictions. In the main text we state all our propositions in an intuitive verbal manner. The Appendix contains formal statements as well as formal proofs.

2.3: Model Predictions about Optimal Contracts

In this section we derive properties of the optimal discovery decision and optimal equity premium. We denote the probability of discovery by μ, and the probability of equal splitting by λ.

Proposition 1: Teams with higher OIA are less likely to engage in discovery.

The key intuition for this result is that the benefit of discovery is lower for higher OIA teams. Indeed, in the high OIA region there is no benefit at all since equal splitting is always optimal, regardless of whether partners are asymmetric or not. The proof in the Appendix shows that a similar logic applies in the low and medium OIA regions. In these regions the net benefit of discovery is strictly positive, reflecting the benefits of contingent contracting. However, the net benefit shrinks with α, because the optimal share premium is decreasing in α. The appendix shows that μ is thus a decreasing function of α.

Proposition 2: Teams in the high OIA region are more likely to do an equal split than teams in the medium OIA region, which in turn are more likely than teams in the low OIA region.

Table 1 foreshadows Proposition 2 by showing that equal splitting occurs for sufficiently high values of α. The additional step in Proposition 2 is to account for endogenous discovery choices. Proposition 2 notes that the probability of equal splitting is always lowest in the low OIA region, higher in the medium OIA region, and highest in the high OIA region.[8]

Proposition 3: Teams that engage in discovery are less likely to do an equal split (provided that there are not too many low OIA types).

The key intuition here is that discovery allows cofounders to uncover their asymmetries, which leads to more unequal splitting, relative to the benchmark of no discovery. The intuition for the condition that there are not too many low OIA types stems from the fact that in this region teams never chose an equal split without discovery. The Appendix shows that this condition is very mild, and is not even needed for sufficiently high values of q.

So far we assumed that both founders make equal resource contributions, i.e., r=½. We now examine how unequal resource contributions affect the probability of equal splitting.

Proposition 4: The probability of equal splitting is lower with greater inequality of founder resource contributions.

This proposition is based on the simple insight that an optimal equal split is only possible if both partners provide similar resources. In the appendix we show how a sufficiently unequal distribution of resource contributions can violate the participation constraint of one of the founders. Specifically we show that there exists a lower and upper bound of r, so that equal splitting remains optimal within these bounds. However, outside of those bounds unequal splitting is necessary to satisfy the participation constraint of the partner that provides more resources.

2.4: Model Predictions about Performance Outcomes

We now turn to the performance predictions. We examine how exogenous team characteristics (OIA) and endogenous contracting choices (engaging in discovery and splitting the equity) affect performance. Our measure of performance is the probability of success p, which depends on the founder skills (ϕ) and equilibrium effort choices (e), which in turn depend on the incentives that arise from the division of equity (σ).

Proposition 5: Teams with higher OIA have lower expected performance.

This proposition exposes the fundamental trade-off between efficiency and inequality. Teams with higher OIA choose more equal contracts, but this comes at the price of lower expected performance. This result holds with and without discovery. The underlying mechanism is that higher OIA lowers the share premium of the higher-skilled founder. This reduces incentives and therefore leads to lower expected performance.

[8] In the Appendix we provide a more complete characterization of the probability of equal splitting λ, describing how it depends on α. We show that λ has discontinuous upward jumps at α0 and again at α1; that λ is increasing in the medium OIA region; but that it is decreasing in the low OIA region. The intuition for why λ jumps at the boundaries can be obtained from Figure 1, which shows that all teams that don't incur discovery costs switch to equal splitting at α0, and all teams that discover asymmetric skills switch at α1. The intuition why λ is decreasing within the low OIA region is that in that region, equal splitting only occurs when teams engage in discovery and find symmetric skills; yet we know from Proposition 1 that the probability of discovery μ is decreasing in α. The intuition for why λ is increasing within the medium OIA region is similar, since the probability of discovery continues to be decreasing in α; yet in this region this increases equal splitting since all teams that forgo discovery now choose an equal split.

Whereas Proposition 5 establishes a relationship between performance and exogenous preferences, Proposition 6 establishes the equilibrium relationship between performance and the endogenous choice between equal versus unequal splitting. To highlight the role of selection effects, we are careful to distinguish between the relationship conditional on α (Part 1) versus the unconditional relationship (Part 2).

Proposition 6, Part 1: For a given level of OIA, teams with equal splits have lower expected performance.

This proposition brings us back to the fundamental trade-off between equity and efficiency. In the low OIA region of Figure 1, equal splits are associated with symmetric teams who discovered their skills. Their performance is compared against teams with discovery and unequal splits, as well as teams who didn't engage in discovery. In the Appendix we show that adding up the performance across the different types of unequal splits always leads to a higher performance than under equal splits. A similar calculation applies to the medium OIA region, where equal splits come from symmetric teams with discovery as well as all teams without discovery, whereas unequal splits come from asymmetric teams with discovery.[9]

Proposition 6, Part 2: Accounting for endogenous selection of teams with different α, teams that agree on an equal split have lower expected performance (provided that there are not too many low OIA types).

Part 2 of Proposition 6 takes into account the self-selection across α. Proposition 2 provides the basis for understanding this, showing that teams with higher OIA are more likely to choose equal splits. And we know from Proposition 5 that teams with higher OIA have lower performance. Combining these insights we find that there is a negative selection effect associated with equal splitting, namely that teams with characteristics that lead to lower performance are more likely to choose equal splits. This effect reinforces the effects from Part 1 of Proposition 6. The condition that there not be too many types in the low OIA region stems from the fact that λ is locally decreasing in α in this region, as discussed in the context of Proposition 2.

2.5: Causal Effects of Equal Splitting

Our analysis so far focus on equilibrium relationships that include self-selection effects. Empirical work typically tries to separate out selection and treatment effect. We therefore ask whether our theory generates predictions about the causal effect of equal splitting. Recall that establishing causality requires random assignment to a treatment or control group. In our model there are two parameters that may generate such random assignment. Proposition 4 suggests that variation in the asymmetry parameter r can randomly allocate teams into equal or unequal splitting. Proposition 3 implies that variation in discovery costs also generates a random assignment into equal versus unequal splitting. Neither r nor k enter directly into the performance measure p, so they only affect performance indirectly through their effect on the equity premium σ. This insight suggests that both of these variables satisfy the exclusion restriction (see section 4.3).

Proposition 7: A random assignment to equal splitting has a *positive* effect on performance for symmetric teams, but it can have a *positive or negative* effect on performance for asymmetric teams.

The key finding is that the causal effect of unequal splitting is ambiguous, it can be positive or negative, depending on circumstances. The ambiguity applies to both types of random assignments (r and k), but let us focus here on r.

[9] Proposition 6 leverages the assumption that in symmetric teams both founders have low skills whereas in asymmetric teams one founder has high skills. Proposition 6 continues to hold even if symmetric and asymmetric teams have comparable skill distributions.

Consider a symmetric team (with or without discovery), then equal splitting is always optimal and achieves the highest performance. However, for sufficiently high or low realizations of r, satisfying the participation constraint requires an unequal split which results in lower performance. Contrast this to an asymmetric team with sufficiently high OIA. Without a binding participation constraint (lower values of r) the team is willing to compromise on performance to preserve outcome equality. However, a sufficiently high or low realization of r would force the team into an unequal equity split. If the higher (lower) skilled founders gets more equity, this results in higher (lower) performance. In the Appendix we show that a similar ambiguous prediction also arises for variation in k. Overall Proposition 7 cautions that the model does not suggest a systematic causal relationship between equal splitting and performance.

2.6: The role of skill asymmetries

The main objective of our theory is to explain the antecedents and consequences of equal splitting in founder teams. It would be easy to derive the optimality of equal splitting in a model with symmetric founders; it would merely follow from the assumption. However, in practice we would never expect two founders to be perfectly symmetrical; that is arguably a “measure zero” outcome. Our core challenge is therefore to explain equal splitting with asymmetric founders. For tractability, we use the simplest possible model where one of the two founders has uncertain skills. This model has some specific properties: after discovery there are two states, one symmetric, the other asymmetric; before discovery founders are asymmetric. Our modelling approach raises two questions. What would happen if founders were symmetric before discovery? And what if founders were never symmetric, neither before nor after discovery?

To answer the first question, the appendix sketches a more general model with two-sided uncertainty. Both founders can have high or low skills, with respective probabilities q_A and q_B. Unfortunately, moving from two to four states dramatically increases the complexity of the model. In the appendix we still derive an analytical expression for the optimal equity premium (without discovery), and the critical value α_0 above which equal splitting is always optimal. We show that α_0 is increasing in (q_A-q_B), implying that bigger skills differences increase the willingness to split the equity unequally. For the special case where founders are ex-ante symmetric (i.e., $q_A = q_B$) we find α_0= 0. In this case the low OIA region disappears, reflecting the fact that symmetric founders would always split the equity equally. We believe that this symmetric model is not suitable for empirical analysis, it represents a knife-edge case (for any small deviation from $q_A = q_B$ the low OIA region reappears), and in reality we would not expect founders to be perfectly symmetric.

This brings us to the second question of why we assume that after discovery one of the possible two states is symmetric, one could argue that should be a measure-zero event too. To justify our base model we show in the appendix that allowing for small asymmetries would not change the fundamental structure of the model. In fact we show that if there is a continuous distribution of ϕ_A over the interval $[\phi_L,\phi_H]$, then there exists ϕ_0 ($>\phi_L$) so that for all $\phi_A \in [\phi_L, \phi_0]$ the optimal premium is $\sigma^*=0$. This means that the skills distribution is divided into two parts. In the lower part, skills are sufficiently similar to make equal splitting optimal. In the upper part, skills are sufficiently different to warrant a positive premium. Our simple two-state model with one symmetric and one asymmetric state therefore catches the essence of this more general specification that has two main parameter regions, one low region where equal splitting is optimal and one high region where unequal splitting is optimal.

2.7: The assumption of outcome inequality aversion

Our theory is based on an assumption that founding teams have OIA. In this section we delve into what that means. It should be noted that if founders had purely individualistic preferences, even the smallest skill asymmetries should generate unequal splits. To explain the high incidence of equal splitting it seems reasonable to explorer a broader class of preference functions. Our assumption of OIA might be considered “behavioral” in the sense that it deviates from the standard neoclassical assumption of purely individualistic preferences. With OIA the individual utility function of one founder also depends on the returns obtained by the other founder. Such a utility function means the individual cares about social comparisons. For $\sigma>0$, the utility gain of making higher returns (call it greed) is partly clouded by the guilt of making more returns than the co-founder. However, for $\alpha<1$, greed remains stronger than guilt, so that receiving a higher stake is still preferable. For $\sigma<0$, the utility loss from making lower returns is compounded by resentment about making lower returns than the co-founder. We call this a “social” preference function, since it includes an element of social comparison.

We consider the assumption of social preferences very reasonable in the context of founding teams. Prior research has shown the importance of social preferences and concerns about fairness.[10] Such social preferences are not irrational, in the sense that they do not violate the axioms of rational choice theory (such as completeness or transitivity). They also differ across all teams, our model allows for a distribution $\Omega(\alpha)$. Such variation is consistent with the finding from the prior experimental literature that inequality aversion is not a homogenous trait (see Fehr and Schmidt 1999; Fehr and Schmidt 2006). Different team structures can also be associated with different values of α. For example, family teams are likely to have higher values of α.[11]

In our model inequality aversion pertains to final outcomes. An interesting alternative assumption would be that inequality aversion pertains to the ex-ante utilities. Let us call this “utility-inequality-aversion” (UIA henceforth), which we distinguish form OIA. The latter concerns the desire to receive the same returns in case of success, regardless of whether different efforts were made to get there. The former concerns a desire to achieve a balance of efforts and rewards ex-ante. Much of the prior fairness literature either has no ex-ante-ex-post dynamics, or assumes ex-ante symmetry. Under either of these circumstances, OIA and UIA are the same. However, our model is dynamic, and as discussed in section 2.6, our challenge is to explain why asymmetric founders chose symmetric contracts. A priori, both OIA and UIA seem to be reasonable assumptions. We therefore ask whether they generate similar predictions. In the Appendix we briefly outline the structure of the UIA model, and derive the following insight: In the UIA model, small asymmetries among the two founders immediately lead to asymmetric contracts. This is because an equal split never creates symmetric utilities for asymmetric founders. Founders therefore always want to deviate from an equal split, to compensate for the differences in utilities. This makes it intuitively clear why it the UIA has difficulty in explaining the main empirical patterns, especially the prevalence of equal splitting.

We may also ask if instead of a direct preference for equality, there may be some transaction costs that generate an indirect preference for equal splits. For example, one could argue that negotiating an unequal split involves higher transaction costs than simply agreeing on an equal split. In the appendix we briefly outline a model with such

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[10] See, for example, Adams (1965); Lazear (1989); Akerlof and Yellen (1990); and Skott (2005), as well as the large body of experimental work summarized by Fehr and Schmidt (2006).

[11] While the base model assumes that every team has a single parameter α, the appendix shows how to relax this assumption. First, different founders can have different OIA parameters. Second, individual founders can have different aversions to positive (‘guilt’) versus negative (‘resentment’) share premiums. We show that such founder teams continue to choose equal splits if at least one partner has sufficiently high OIA. This clarifies that OIA is an individual preference, not a team characteristic.

transaction costs. The model predicts a negative relationship between equal splitting and performance. This is because teams with higher value projects are more willing to incur the transaction costs. However, the model also generates an additional prediction that distinguishes it from our OIA model: it predicts a complete absence of "near-equal" splits. With transaction costs, all unequal splits must be sufficiently large to justify their transaction costs. Formally, the model predicts a range of low equity premiums $\sigma \in [0,\sigma^*]$ that are never chosen, because the transaction costs outweigh the benefits of negotiating such a small premium. This prediction is different from the OIA model, which predicts a continuous distribution of optimal premiums. In our data we observe a continuous distribution of equity premiums. Specifically we find that among the unequal splitters the kernel density of the maximum premium (defined as the highest premium within a team) remains clearly positive in the vicinity of the zero premium. This is inconsistent with the above prediction of a complete absence of near-equal splits, and casts doubt on the importance of transaction costs.

Section 3: Data and variables

3.1: Data Sources

Table 1 summarizes our empirical variables, their definitions, and their associated questions in our data-collection survey. Table 2 shows the descriptive statistics. Table 3 reports the pair-wise correlations between the main variables of interest.

The data come from the annual CompStudy survey of private North American ventures, for which one author is the lead investigator (see Wasserman 2003; Wasserman 2006; Wasserman 2012 for more details). The first CompStudy survey was conducted in 2000 on private information-technology ventures (broadly defined, including telecommunications). Two years later a parallel survey of life-sciences ventures was added, and since then, annual surveys of both industries have been conducted.[12] The list of target companies is generated by combining the list of private companies included in the VentureXpert database with the membership lists of local technology associations (e.g., the Massachusetts High-Technology Council). Invitations are sent to the CEOs and CFOs of those companies.[13] To encourage participation in the survey, participants are offered a free copy of a detailed "CompStudy Compensation Report" that is based on the survey results and made available only to participants. The report includes position-by-position breakdowns of salaries, bonuses, and equity holdings for the eleven most common C-level and VP-level positions in private ventures. The breakdowns provide compensation benchmarks by industry segment, location, company size and age, financing rounds, founder versus non-founder status, and other metrics collected in the survey. CompStudy's annual compensation reports have become a standard reference within the top management teams of private American ventures, and for their board members and investors.

The dataset used in this paper combines the Technology and Life Sciences surveys from 2008-2013. A major benefit of conducting annual surveys, and of collecting one's own data, is that each year the researcher can add new questions to tackle emerging research questions that aren't addressed by existing datasets (e.g., about the equity-split negotiation process) or are highly confidential (e.g., about the percentage of equity received by each founder). The 2008 CompStudy survey was the first to include detailed questions about each founding team, its prior work

[12] Details are reported in the Appendix of Wasserman (2012).
[13] The CEOs and CFOs of the ventures were targeted due to the sensitivity of the questions and the breadth of corporate knowledge required to complete the survey.

experience, and the equity split within the team. These questions were repeated in the subsequent surveys.[14] For repeat respondent we use only the most recent responses.[15]

Each year, survey response rates vary between 10%-20%, higher than the typical response rates for surveys of similar companies and targeting similar levels of executives (e.g., Graham and Harvey 2001). The surveys are conducted online so that fields can be validated as they are being entered. When possible, data are cross-checked with publicly-available information sources to validate the accuracy of the submissions, and the survey data are checked for representativeness against the VentureXpert population. Regarding geographic distributions and industry segments, the dataset is a representative sample of private high-potential ventures in the technology and life sciences industries within the United States. Regarding age of venture, the dataset contains slightly younger companies given that VentureXpert only includes ventures that have raised institutional capital while CompStudy also includes pre-funding ventures. For a more detailed comparison of the CompStudy and VentureXpert datasets, see Wasserman (2015).

Our initial sample consists of 6037 respondent companies, but 4276 respondent provide no or incomplete information about founders and therefore have to be dropped for the regression analysis. Of the remaining 1761 companies with complete information, 394 have a single founder, and 1367 have founding teams.

We analyzed whether the companies with incomplete information are similar to the ones retained. We consider company age at the time of the survey. The average age is 7.73 years for all companies that reported age information, compared with 6.62 for those respondents with complete founder information that are actually used in the analysis. This suggests that, if anything, response biases force us to look at a sample of younger companies. Such companies are more likely to be representative of the true startup population, and less likely to suffer from recollection biases.[16]

Although our data should be less susceptible to survivorship bias than samples of public companies, or even than samples of venture capital-backed companies, we still recognize this possibility. The robustness checks mentioned above about younger versus older companies provide some reassurance that survivorship biases are unlikely to affect the main variables of interest.[17]

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[14] In order to test alternative explanations proposed by reviewers, supplementary questions were added to the 2013 CompStudy survey about connections between equity agreements and salary agreements, the influence of outside investors on the equity split, and renegotiation of the original equity agreement. This additional data is more limited, as it contains fewer observations and is drawn from a single cohort of respondents. Still, it generates some useful additional insight that we exploit in section 5.

[15] This gives us the most complete outcome data possible. We also compared the most recent submissions to the earliest ones to double-check the accuracy of the submissions.

[16] Recollection biases would be particularly important for founder agreement variables, namely Equal Splitting and Quick Negotiation. To look whether a longer recollection period biases responses one way or another, we divide our sample at the median company age, as measured at the time of the survey. Using t-tests for the difference of means between the older and younger companies, we find no significant differences across these two subsamples, both for Equal Splitting and for Quick Negotiation.

[17] Note also that our analysis starts at a point in time where teams have been formed, so that we cannot analyze the process of how teams are formed. Clearly the team-formation process is not random, and deserves its own research focus. However, our research question does not call for random assignment of founders to teams. Put differently, we are not interested in how random teams would negotiate hypothetical founder agreements, we are interested in how actual teams negotiate real agreements.

We also compared team sizes across different samples. The average team size of companies with team size information, excluding solo founders, is 2.86, compared to 2.77 for the companies used in the final sample. We can compare our survey data to comparable studies about startups. For example, Hsu, Roberts et al. (2007) gathered data on MIT startups and reported an average of 2.86 founders per team compared to our 2.77 founders per team. At first glance it appears that our percentage of serial entrepreneurs is relatively high; Gompers, Lerner et al. (2010) reported that approximately 10% of their entrepreneurs were serial entrepreneurs. However, their sample consists of venture capital-backed companies, and their definition of serial entrepreneur only counts experiences with previous venture capital-backed companies. Hsu, Roberts et al. (2007) report that 46% of their entrepreneurial teams had no prior startup experience. In our sample 40% of all teams have no founder with startup experience.

Our hypotheses focus specifically on founding teams, so our analysis focuses on the companies with founding teams. However, it is interesting to compare the sample of solo founders with the sample of founding teams. Using t-tests for the difference of means we find several significant differences.[18] Solo founders are less common in the clean tech industry, and in California. They also have more years of work experience, and more entrepreneurial and management experience. We also find that single founder companies are less likely to obtain outside investments, let alone venture capital.

3.2: Key Variables

To empirically test the theory developed in section 2 we need empirical proxies for the key variables of interest. Let us briefly review what variables are needed. In simplified terms, Proposition 1 predicts that higher OIA preferences lead to less discovery, whereas Proposition 2 says they lead to more equal splitting. Proposition 3 suggests that discovery itself is negatively related to equal splitting. Proposition 4 adds asymmetries of founder resources as a determinant of the equal splitting decision. Proposition 5 suggests a negative relationship of OIA and performance, Proposition 6 a negative relationship of equal splitting and performance. Proposition 7 finally uses random variation in discovery costs and founder resources asymmetries to identify causal effects. Overall we note that testing the theory requires empirical proxies for the following key variables: OIA preferences, discovery, equity splits, founder resources and performance. We now discuss how we measure each of these empirically.

To proxy OIA we note that such a social preference is not directly observable. However, we argue that OIA should be particularly strong within a family context, where differential rewards may be viewed as intrinsically unfair. In the realm of organization theory, equity theory highlights the tight connection between prior relationships and the economic arrangements adopted within teams (Adams 1965; Deutsch 1975; Leventhal 1976). Prior relationships affect the type of "logic" under which team operate: teams with tight prior social relationships (e.g., family members) operate under a social logic in which preserving personal relationships takes precedence over maximizing business success; other teams operate under a business logic in which maximizing business success takes precedence over preserving personal relationships.[19] Note that we do not argue that *all* family founding teams have higher OIA, we merely propose that *on average* we expect higher levels of OIA amongst family teams. We use a survey question that asks how many of the founders were related to each other. We create a dummy variable that takes the value 1 if all founders were related to each other, 0 otherwise. We do not count teams where only some but not all members are related to each other, where the effect on OIA is more complicated.

[18] Details available from the authors upon request.
[19] See also Almandoz Rios (2014) for a related argument about alternative institutional logics across founder teams.

Whether or not founding teams engage in costly discovery is not directly measurable, but our survey contains unique information about the time that founders spent negotiating the division of equity, categorized by: 1 day or less, 2 days to 2 weeks, 2 weeks to 2 months, 2 months to 6 months, and More than 6 months. We can think of the time spent negotiating as an exercise in evaluating relative skills and contributions within the founding team. Our analysis uses a dummy variable called "Quick negotiation" that takes the value 1 if the negotiations were concluded in 1 day or less, 0 otherwise. Somewhat surprisingly, 42% of all ventures concluded their negotiations in 1 day or less. We consider "Quick negotiation" as an empirical proxy for "no discovery," i.e., all teams that negotiated for more than a day are associated with the discovery regime.[20] In section 4.4 we report robustness checks on this.

Central to our theory is the decision whether to split founder equity equally or not. Our raw data include for each co-founder the specific percentage of equity received. We use a binary variable called "Equal Splitting" that takes the value 1 when there are no differences in equity stakes, 0 otherwise. 32% of all ventures chose an equal split.

Finally, to measure venture performance we use two simple survey-based metrics of external validation. We are interested in measuring performance milestones that are observable across a broad cross-section of companies, and that can reasonably be thought of as measures of interim progress. Building on the large literature on entrepreneurial finance (see Da Rin, Hellmann et al. 2012), we use fundraising from outside investors as a measure of interim performance. This can be interpreted in two complementary ways: first, raising outside is a milestone by itself; second, it can be viewed as a signal of investor confidence that the company has sufficient promise to achieve future performance milestones. Our survey provides information on the sources of funding from founding through the time of the survey. First, our analysis uses a dummy variable called "Outside Investments" that equals 1 if the company had obtained funding from outside investors (defined as angels, corporate investors or venture capitalists), 0 otherwise. 78% of our ventures had received funding from outside investors. Second, to provide a more selective metric of strong outside validation, we use a dummy variable called "Venture Capital" that takes the value 1 if the venture had obtained some venture capital at the time of the survey, 0 otherwise. 43% of all ventures had received venture capital.

3.3: Control Variables

We now explain our control variables. Several of our independent variables are constructed from data captured at the individual founder level. Our 1367 ventures comprise a total of 3782 founders (an average of 2.77 founders per venture). To aggregate founder variables to the venture level, we calculate two measures, consistent with organizational-demography studies that focus on both the average characteristics of a team as well as the differences across team members (e.g., Beckman, Burton et al. 2007). First, to consider average team characteristics, we calculate the average of the founder-level variables within the teams. Second, to measure team heterogeneity, we calculate the coefficient of variation of the founder variables within their ventures.

The prior experiences of founders are likely to affect founder agreements. Beckman (2006) argues that founders' early decisions, such as whether to adopt exploratory processes or exploitative processes, are shaped by their past work experiences. Regarding outcomes, greater heterogeneity regarding the founders' prior work experience is associated with a greater likelihood of raising venture capital (Beckman, Burton et al. 2007). Our individual-founder

[20] Our theoretical distinction between the no-discovery and discovery cases, and our empirical proxy of looking at fast versus slow negotiation, can also be related to the work of Kahneman (2013) on fast versus slow thinking, and the work of Rubinstein (2007) on instinctive versus cognitive reasoning .

variables include four measures of human capital: the founder's years of work experience prior to founding the current venture, whether the founder had prior founding experience ("serial entrepreneur"), whether the prior founding experiences resulted in a successful exit, and whether the founder had prior managerial experience.

A fifth individual-founder variable captures whether a founder generated the idea(s) on which the venture was founded ("idea person").[21] Generating ideas might command a premium stake. For instance, idea generation matters whenever teams want to reward founders for prior contributions. Prior idea creation may also be a sign of creativity; founders who came up with the original idea might be expected to also generate more future ideas.[22]

The sixth variable at the individual founder level captures the founders' financial contributions. For each founder, the survey asks whether the amount of capital contributed falls into one of five categories: $0k, $1k-$25k, $26k-$100k, $101k-$500k, and More than $500k. To be pragmatic, we use the midpoints when available, otherwise the lowest point, resulting in the following categories (in $ million): 0, 0.012, 0.063, 0.3 and 0.5. Ceteris paribus, we would expect founders who contribute more capital to also receive larger equity stakes. Note that we measure founder resources with the intellectual and financial contributions, i.e. the fifth and six variable discussed here. To estimate asymmetries amongst founders, we focus on the coefficient of variation.

The remainder of our control variables occurs not at the level of individual founders but at the company level. We control for the size of the founding team. We include controls for industry and geography. Industry dummies include: software, hardware, cleantech, pharmaceuticals, life sciences (excl. pharma) and other. The venture's location was categorized as follows: California, Massachusetts, Rest of US, and Canada. We include a set of year dummies based on the agreement date. We also include a linear and quadratic control term for the age of the company at the time of survey. For the starting date we use the earlier of the date that the company was founded or that the founder agreement was made. Finally we also include dummies for the year the company was surveyed.

We first examine founder agreements, considering both the negotiation process and the type of agreement. We begin with an empirical test of Proposition 1 that predicts less discovery by teams with higher OIA. The results are shown in column 1 of Table 4. We estimate a Probit model where the dependent variable is the dummy variable "Quick Negotiation," which we interpret as teams that do not incur discovery costs. The main independent variable is "Family Team," our proxy for OIA. Consistent with Proposition 1, Family Teams are more likely to do a quick negotiation. The coefficient is significant at the 1% level. The estimated marginal effect at the mean is a 25.8% increase in the probability of a quick negotiation. In terms of control variables, the most interesting finding is that larger teams are less likely to do a quick negotiation. This is an intuitive finding; we would expect larger teams to be more heterogeneous, and therefore have a greater benefit of discovery.

One concern may be that family members already know each other well, and therefore do no need to engage in a discovery process with lengthy negotiations. Family members do know each other extremely well in the social setting, where social logics predominate and norms of equality are followed (Adams 1965; Deutsch 1975; Leventhal 1976). However, the professional realm exhibits a very different logic – a business logic – that prioritizes "equity" and matching ownership to contributions. The difference between the social and professional realms extends further. For instance, people exhibit different personalities in different settings (Ibarra 1999), making compatibility

[21] The information-technology survey asked about idea generation in general. The life-sciences survey asked about the generation of intellectual property, reflecting the importance of intellectual property in those industries.
[22] Wasserman (2012) provides a more detailed discussion of the role of idea founders.

in the social realm very different from compatibility in the professional realm. The divergence between the social and professional realms also extends to knowledge about each other's human capital. Family members often do not know each other's professional skills, let alone whether they have task-specific skills relevant to the specific venture idea. Thus, even family members have to engage in discovery of each other's founding personalities and relevant skills.[23]

The next step is to empirically test Propositions 2, 3 and 4, about the incidence of equal splitting. Column 2 of Table 4 examines Proposition 2, which predicts a positive relationship between OIA and equal splitting. The regression shows that Family Teams are more likely to do an equal split. The coefficient is significant at the 1% level and the estimated marginal effect at the mean is 20.6%. Column 3 of Table 4 examines Proposition 3, which predicts a negative relationship between discovery and equal splitting. Indeed we find that the coefficient for Quick Negotiation, which proxies the absence of discovery, is positive, with an estimated marginal effect of 12.0%, significant at the 1% level. In column 4 of Table 4 we jointly examine the effects of columns 2 and 3, and find that both results continue to hold at the 1% level, with only a modest decrease in coefficient size.

Proposition 4 predicts less equal splitting if founders have more unequal resource contributions. Our data contain two measures of founder resources: founder capital and ideas. In Table 4 we control not only for their mean value but also their dispersion, which is the relevant measure for Proposition 4. We find that greater dispersion of founder capital and ideas both have negative coefficients that are significant at the 1% level. This is consistent with the predictions of Proposition 4.

For the other control variables we find that more-experienced teams are less likely do an equal split, suggesting that it may be easier to recognize skills differences amongst experienced founders and/or that experienced founders are less averse to outcome inequality. We also find that larger teams are less likely to split the equity equally, which is again consistent with greater heterogeneity.

4.2 Performance Outcomes

We now turn to performance outcomes, focusing on Outside Investments and Venture Capital as our dependent variables. Proposition 5 shows that higher OIA is associated with lower performance. We test this in columns (i) and (iv) of Table 5. As predicted we find a negative effect of Family Teams on both outcome variables. The coefficient on Outside Investments (Venture Capital) is significant at 1% (5%), with an estimated marginal effect of -14.6% (-14.5%). In columns (iii) and (vi) we also control for equal splitting, and find very similar estimates.

Proposition 6 predicts a negative relationship between equal splitting and performance. Correspondingly we find in columns (ii) and (v) of Table 5 that the coefficient of equal splitting is negative and significant. For Outside Investments (Venture Capital), the coefficient is significant at 1% (10%), with an estimated marginal effect of -6.87% (-5.73%). In columns (iii) and (vi) we add Family Team as a control and find slightly lower coefficients (marginal effects of -6.25% and -4.95%), where Outside Investments is significant at 5% and Venture Capital marginally insignificant with a P-value of 12%.

[23] Note also that founders' knowledge of one another may already be implicit in some of the other control variables, such as the age and experience variables. The remaining effect of the "family team" coefficient thus captures the additional effect of family ties, which may therefore align more closely with the inequality aversion effect.

In terms of the control variables, we note that prior entrepreneurial success is associated with more investment. Managerial experience (as well as its coefficient of variation) has a negative coefficient; teams with managerial experience may be less likely to seek outside financing. Higher founder capital is associated with a lower probability of investments. This suggests a substitution effect between internal and external capital. The linear age coefficient is always positive, and the quadratic term always negative. The resulting non-linear function remains increasing up to 13.1 years for Outside Investment (19.6 years for Venture Capital), with over 91% (98%) of the sample companies falling into the increasing range. Note also that Table 5 excludes all instrumental variables, to which we turn next.

4.3 Instrumental Variables

The empirical results from Table 5 establish correlation, not causation. This is consistent with our theory, which explicitly accounts for endogenous selection effect, as discussed in Proposition 6. The question remains whether there are any causal effects of equal splitting. Ideally we would like to have a controlled experiment, but as in most empirical studies, we are confined to using variation in realized data. Thankfully our model provides guidance for a theoretically grounded identification strategy. Specifically, our model suggests two types of instrumental variables: Proposition 4 notes that greater inequality of founder resources decreases the likelihood of equal splitting, and Proposition 3 suggests that higher discovery costs reduce the likelihood of equal splitting.

In our data we identify three variables that lend themselves as instruments. Two variables measure the heterogeneity of founder resources: heterogeneity in founder capital, and heterogeneity in founder idea contributions. As for the third instrument, we note that discovery costs are not directly observable. However, we can leverage variation in the realized discovery decision, as measured by the Quick Negotiation variable. Our proposed three instruments are therefore COV Capital, COV Ideas and Quick Negotiation.

To assess the validity of these instruments we first consider the “rank condition,” i.e., we ask whether the instruments predict equal splitting. Theoretically speaking, we already noted that Proposition 4 makes such a prediction for the resource heterogeneity measures, and Proposition 3 makes such a prediction for the role of discovery. Empirically we note that Table 4 already finds significant effects for all three of these variables.

The “exclusion restriction” is typically more difficult to establish; it cannot be established on the basis of data. Most analyses limit themselves to a verbal defense of the exclusion restriction. However, we can go one step further and derive it from our theory. Our model shows that heterogeneity in founder resources, as reflected in high absolute values of r, lead to unequal splits. A key insight is that heterogeneity in founder resources has no direct effect on performance. Specifically, the only way that resource asymmetries r can influence performance p is indirectly through their effect on the equity premium σ. This is precisely the logic of an instrumental variable, which can only affect outcomes (i.e., performance) through its effect on the variable of interest (i.e., equal splitting). The same logit also applies to discovery costs k, which have no direct effect on performance p, but can have an indirect effect through the discovery decision d, which in turn affects the optimal equity premium σ. The theory therefore provides a clean rationale for the exclusion restriction.

How reasonable is the exclusion restriction beyond the model? We argue that the exclusion restriction remains persuasive. For the resource asymmetries we note that our regressions already control for the level of resources provided by founders, so that we are effectively holding constant the overall resources provided to the venture. Our heterogeneity measures only capture the distributional aspect of who provided these resources. The core intuition for the exclusion restriction is thus that the level of resources may well affect performance, but that the distribution of who provided those resources has no additional direct performance effect. For the Quick negotiations variable, fundraising success should not be directly affected by the speed at which founders agreed on their division for equity. This is a negotiation that typically occurs long before the time of fundraising, and the details of that negotiation would normally not be observable to investors. It seems reasonable to think that the *outcome* of the negotiation, namely the decision of equal versus unequal splits, might matter to investors, but the *process* of how that decision was reached is unlikely to matter directly. Naturally both types of instruments can still affect performance indirectly through their effect on the negotiation outcome, namely the allocation of founder shares.

Table 6 reports the results from linear two-stage instrumental variable regressions. The first stage regression, reported in column 1, again confirms that COV Capital, COV Idea and Quick Negotiation all have the expected sign and are all highly significant at the 1% level. For the second stage regression, columns 2 and 3 both report an insignificant coefficient for the instrumented Equal Splitting variable. Moreover, the coefficients have opposite signs. Thus there is no empirical support that equal splitting has a causal impact on performance, neither positive nor negative.

Empirical analysis often seeks to establish causal relationships, so should we be surprised by the lack of a causal relationship? Our theory helps to understand this empirical finding. In the model equal versus unequal splitting is not a "treatment," it is an endogenous decision. One should not expect random shocks that favor one decision over the other to have a systematic effect, because depending on circumstances each decision can be optimal or suboptimal. We should therefore expect random deviations from equal splitting to sometimes have positive and sometimes have negative performance effects, as explained in Proposition 7. We therefore consider the absence of a causal effect an important finding by itself. It also helps us interpret coefficients in the un-instrumented regressions of Table 5. If we think of the simple OLS coefficient as a combination of selection and treatment effects, then a finding of an insignificant treatment effect suggests that the correlation in the OLS model is driven by selection effects.[24]

4.4 Empirical Robustness

In this section we briefly examine the robustness of our main empirical analysis. One issue is who gets counted as a founder. For our main analysis we use the self-reported data. As a robustness check we examine whether our main results could be affected by some unusual founding teams. There are two types of gray areas in the data. First, some "founders" joined a long time after the founding date, making them look more like non-founding executives. Second, some founders received very low equity stakes, making them look more like employees. As a robustness check we removed from the sample all teams that had (i) any reported founders who joined more than two years after the first founder, or (ii) any reported founders with an equity stake that was less than one tenth of an equal stake. Omitting those ventures did not change the main insights of the analysis.

There is no perfect way of converting the ordinal measure of capital into a numerical measure. We considered several variations, such as varying the amount for the top-coded category, or using natural logarithms, and found

[24] We focus on the endogeneity of the Equal Splitting variable. One may also wonder about the endogeneity of family teams. To some extent this variable is clearly exogenous, because family ties are clearly predetermined. However, the team selection process may be different amongst family members. This effect could go either way, family founders could be more selective or more lax when deciding to come together for a new venture. While we have no data about the team formation process, our empirical analysis does control for many important founder team characteristics, thus controlling for some of the differences in the observable characteristics of family versus non-family teams.

that the results were very similar across all these permutations. We also considered entropy as an alternative measure to the coefficient of variation for all our categorical variables, but found no significant changes in our results. We focus on the dummy variable Quick Negotiation, but the survey also has a fine-grained set of categories. In unreported regressions we verified that replacing Quick Negotiation with the more fine-grained variable does not alter our main findings.[25] We also considered using a finer industry categorization, using 10 distinct industry segments, and again found no significant changes in our results.

For Table 6 we verified that the results are not driven by one particular instrument. In unreported regressions we find that dropping any one of the three instruments does not alter the finding that the instrumented Equal Splitting variable is always insignificant. We also reran the regressions from Table 6 dropping the Family Team control and found that this did not affect our findings.

Section 5: Alternative explanations

In this section we discuss alternative interpretations of our main empirical findings. We first examine alternative interpretation of the decision to split the equity equally. We then examine alternative explanations for the observed correlation between equal splitting and performance.

5.1 The meaning of equal splitting

In our analysis we associate equal splitting with equal outcomes, and thus link it to inequality aversion. In this section we critically examine alternative perspectives on the meaning of equal splitting. One concern is that there may be other ways in which founders can differentially compensate each other. In addition to receiving equity, founders receive salaries. An alternative hypothesis could therefore be that founders use differential salaries, not differential equity stakes, to create unequal allocations.

For the 2013 survey we added the following question: "When the founders split the equity, what was the agreement on founder salaries?" Out of 314 responses, 31% responded that no founders received any salaries, 22% responded that all founders received the same salary, and 47% responded that different founders received different salaries. Adding the companies with no founder salaries to the companies with equal positive salaries, we note that over half of the companies had equal salaries at founding. The correlation coefficient between Same Salaries (at founding) and Equal Splitting is 0.3372 (t=0.0000).[26] In Column 1 of Table 7 we use a regression model with Same Salaries (at founding) as the dependent variable. The independent variables are all those of column 4 of Table 4, plus equal splitting.[27] We find that the coefficient for Equal Splitting is positive and significant at the 1% level.

[25] Related to the question of negotiation speed is the degree of formality of founder agreements. Our survey includes a question about that; 51% of companies report that the agreement is formal. As expected, we find that formality is negatively correlated with Quick Negotiation, with a correlation coefficient of -0.1333 (t=0.0000). However, there is no significant correlation with Equal Splitting, the correlation coefficient being 0.0084 (t=0.7575).

[26] For the respondents with unequal founder salaries, the survey asked further detail. 35% reported that founders with higher founder stakes received higher salaries, 13% that founders with higher founder stakes received lower salaries, and 52% that there was no systematic pattern between ownership stakes and founder salaries.

[27] We also reran all of the models in Table 7 dropping Family Team and/or Quick Negotiation, but found that this had no material effects.

The CompStudy survey also collects salary information at the time of the survey. This allows us to see whether equal salary structures persist over time.[28] We find that 38.62% of companies have identical founder salaries at the time of the survey. The correlation between Same Salary (at founding) and Same Salary (at survey) is 0.2887 (t=0.0001). Column 2 of Table 7 finds that equal splitting is positively correlated to having the same salary at the time of the survey, the coefficients being again significant at the 1% level. All these results are not consistent with the alternative explanation that equal splits are typically undone with unequal salary arrangements.

Closely related to the argument about salary is the question of control: founding teams not only decide on equity, they also decide on roles. Of particular importance is who gets control. Our theory would suggest that teams that split the equity equally are also more likely to share control. An alternative hypothesis might be that equal splits are traded-off against unequal control arrangements.

Founding teams can chose amongst different organizational arrangements that either concentrate control in the hands of one founder, or distribute control more widely. While the details of the formal and informal power structure remain unobservable, the CompStudy survey contains data about the job titles of the individual founders. We consider the positions of CEO and Chair of the Board of Directors to be the two most important positions of power. We consider teams with exactly one founder in such a position of power to have a concentrated control structure. On the other hand, we associate teams that have either no, or multiple founders in positions of power with balanced control structures. We find that 21% of all teams have balanced control arrangements. The Balanced Control variable has a correlation of 0.0496 with Equal Splitting (significant at 10%). In column 3 of Table 7 we find that Equal Splitting has a positive coefficient, significant at 1%. This evidence is consistent with the notion that equal splitters are inequality averse and eager to maintain equality in multiple dimensions. This finding is inconsistent with the alternative hypothesis that equal splits are undone through unequal control arrangements.

Another challenge to our interpretation of the equal splitting decision is that renegotiation may undo the initial equity split. The concern here is that the initial equity decision may simply not matter very much. In our theory we find that there is no renegotiation in equilibrium, but this is due to simplifying assumptions. Indeed, in the related model by Hellmann and Thiele (forthcoming) renegotiation does occur in equilibrium. So the question remains how important renegotiation is, and whether there is a relationship with equal splitting. In our supplementary 2013 survey we asked companies whether “the team renegotiated the founders’ original equity-split agreement at some later point?” In 71% of all cases the equity split remained in place without renegotiation; in 11% of all cases the founder split was implicitly modified by granting some founders more stock options; and in 18% of all cases it was explicitly renegotiated because of changed circumstances. Stable Deals is a dummy variable that takes the value 1 if the deal stayed in place, and 0 if it was either explicitly renegotiated or implicitly changed by option grants. We find a small and highly insignificant correlation coefficient of 0.0243 (t=0.6915), suggesting that renegotiation is not systematically related to the decision to split the equity equally or not. We also reran (unreported) the regression model of Table 7 with Stable Deals as the dependent variable and find that the coefficient for Equal Splitting is highly insignificant. Finally we look at the relationship between renegotiation and outcomes. The correlation coefficient of Stable Deal with Outside Investments is 0.0601 (t=0.3257), and with VC it is 0.0544 (t=0.3741), suggesting no significant correlation. Overall the evidence does not support the alternative hypothesis that the original equity splits are irrelevant because they simply get renegotiated later.[29]

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[28] This part of the data has two limitations. Not all founders are still working for the company at the time of survey, and not all companies are willing to provide salary information. We obtained the salaries of all remaining founders for 668 companies, which is just under half of our respondents.

[29] A simplifying model assumption is that founders have no wealth for renegotiation. Empirically this is obviously not true, as we sometimes observe positive amounts of founder capital. However, what actually matters for renegotiation is not the amount of

5.2 The relationships between equal splitting and performance

We now turn to the interpretation of the observed correlation between equal splitting and outcomes. Our theory and interpretation is based on a selection logic. The most important alternative interpretation is a treatment logic. Section 4.3 investigates this with the use of instrumental variables, and finds no significant support for a causal channel. In this section we focus on other types of alternative interpretations. Our model explains equal splitting as the outcome of a self-selection process of rational agents with OIA. As discussed in section 2.7, our model is "behavioral" in the sense that founders have preferences over relative outcomes, but it is also "non-behavioral" in the sense that it continues to use standard assumptions of individual rational choice. We now consider alternative hypotheses that are based on more dramatic departures from rational choice, where teams are too spontaneous or too emotional when agreeing to an equal split, and later regret the mistake. We identify three main alternatives, based on "naivety," "premature contracting" and "inexperience." All three alternatives have clear "behavioral" overtones, but they all generate distinct empirical predictions.

One alternative hypothesis is that teams that chose equal splits are naïve, and don't understand the consequences of their actions. Such a naivety hypothesis predicts a negative correlation between equal splitting and performance, although the underlying reason would be a different selection mechanism: lower performance would be due to naivety itself. To distinguish the naivety and OIA hypotheses, we performed additional tests. The naivety hypothesis suggests dissatisfaction or regret concerning the original equity allocation. In our supplementary 2013 survey we asked two additional questions that shed light on this. First we asked, "At the time of the original founder agreement, what was the team's overall evaluation of their equity-split agreement?" Out of 289 respondents, 86% responded that all founders were satisfied with the deal; 13% responded that some founders were satisfied, others not; and only 1% responded that none of the founders were truly satisfied. Second, we asked respondents to use hindsight in answering the same question. In 66% of the responses all founders were satisfied with the deal with hindsight; in 32% of the cases, some were satisfied, others not; and in 2% of the cases none of the founders were satisfied with hindsight. We then consider the correlation between these responses and the decision to split the equity equally. Under the naivety hypothesis we should expect a negative correlation between equal splitting and deal satisfaction (defined as a dummy variable that takes the value 1 if all founders were satisfied). However, we find a positive correlation coefficient of 0.0945 (t=0.1088) for the first question, and 0.1919 (t=0.0012) for the second question. In unreported regressions we ran models equivalent to Table 7, finding that equal splitting has a positive but insignificant coefficient for the deal satisfaction dummy at the time of the agreement, and a positive and significant coefficient for the hindsight dummy. This evidence is inconsistent with the predictions from the naivety hypothesis.

Closely related to the naivety argument is the potential concern that equal splitting is merely the result of founders contracting prematurely. Under this alternative hypothesis founders may be rushing into an equal split at the beginning of the venture, regretting it later, and paying for it terms of lower performance. We empirically evaluate

founder capital deployed in the venture, but the amount of capital that founders hold outside of the venture – see Hellmann and Thiele (forthcoming) for an extensive discussion of this point. Unfortunately we cannot measure founders' wealth outside the venture, so we cannot empirically test the importance of founder wealth in renegotiations. The one thing we can do in our data is to correlate the founder capital deployed within the venture with the propensity to renegotiate. We find that there is no significant correlation between Avg. Capital and Renegotiation; the correlation coefficient is -0.0381 (t=0.5338). While descriptively interesting, we caution that this does not constitute a proper test for examining the effects of founder wealth on renegotiation.

these three claims. Our data allow us to distinguish teams that contract at the onset of the venture versus later in its development. We construct a dummy variable EARLY which takes the value 1 if negotiations happen at the onset of the venture. This variable concerns the *timing* of when the founders started the negotiation. This is not to be confused with the Quick Negotiation variable, which measures the *length* of the negotiation process. The correlation of EARLY with Equal Splitting is 0.0633, significant at 5%, but when we add the control variables of Table 4, the EARLY coefficient remains insignificant across all specifications. Next we correlate EARLY with our two measures of deal satisfaction, expecting to find a negative correlation. However, we find a positive correlation of 0.1549 (significant at 1%) with satisfaction at the time of deal, and 0.0628 (insignificant) with hindsight satisfaction. This does not support the premature contracting hypothesis either. Finally we consider the relationship between EARLY and performance. In unreported regressions we add EARLY to the model of Table 5, and find insignificant coefficients throughout. We further ask whether early contracting may attenuate the relationship between equal splitting and performance. We interact EARLY with the equal splitting variable, but find the interaction effect to be insignificant throughout. This evidence is not consistent with the alternative hypothesis of premature contracting.

We also ask whether lack of experience can explain the negative relationship between equal splitting and performance. Table 4 found a negative relationship between average team experience and equal splitting. The question is then whether inexperience may be driving the selection process that generates the negative correlation between equal splitting and performance. Table 5 already controls for average team experience, and finds it is to be insignificant throughout. Furthermore we ask whether the negative effect of equal splitting is attenuated by experience. We augmented the model of Table 5 with the interaction term Equal Splitting * Average Years of Experience. In unreported regressions we find that none of the interaction terms are significant. This suggest that while it is true that less-experienced teams are more likely to split equally, this effect is not responsible for the negative correlation between equal splitting and performance.

The above analysis of interaction effects discredits several alternative hypotheses. We briefly ask whether an analysis of interaction effects can also bolster our main hypothesis. In particular, we would expect the negative relationship between equal splitting and performance to be weaker for teams with higher OIA. In an unreported theory extension (proofs available upon request) we formally derive the prediction that the performance premium for unequal splitting is a decreasing function of the OIA parameter. In unreported regressions we take this prediction to the data by augmenting the models of Table 5 with the interaction term Equal Splitting * Family. Our theory predicts a positive coefficient. We find that the interaction term is positive and significant at 5% for Outside Investment, and positive but insignificant for Venture Capital. While not conclusive, this evidence is supportive of our OIA hypothesis.

In our theory we do not consider direct contact between founders and investors at the time of the equity splits. In practice it may be that some teams are already in contact with investors at the time of allocation founder equity. To empirically ascertain the possibility that equity splits are shaped by investors, our supplementary 2013 survey asks how much the founders' initial equity split was influenced by the outside investors who provided the initial financing. Out of 289 responses, 77% of companies reported no influence, 8% reported a little influence, 9% a moderate amount, and 6% a lot of influence. The vast majority of founders therefore reach agreements on their own. We also create a simple dummy variable about whether there was any investor influence or not, and find that this dummy is negatively correlated with Equal Splitting.[30] This is consistent with our main argument that more investor-oriented teams are more willing to split the equity unequally.[31]

Section 6: Conclusion

Relatively little is known about the arrangements among founders within entrepreneurial firms. This paper provides initial insights into the importance of this first deal, in the hopes of providing a foundation for future research on these early arrangements. Future empirical research could delve into several other important pieces of the puzzle that we could not tackle here. Naturally one always want to improve measurement, such as by incorporating richer founder characteristics, more company controls, and more fine-grained measure of company performance. Of particular interest would be more direct measures of founder preferences, especially concerning inequality aversion. Because our data come from founding teams that had already formed, we cannot observe the prior decisions about which founders were included or excluded from the team, nor can we assess whether the core founder tried to attract cofounders but failed. Another promising line of research concerns the flexibility of founder agreements, such as the use of vesting terms for individual founders that are based on their continued involvement and/or their successful completion of pre-specified milestones. Finally, social and cultural norms are likely to affect the process and outcome of founder negotiations. Another avenue for future research might be to go beyond technology and life-sciences startups in North America, and examine how the process differs across different industries and countries.

Our analysis generates some subtle managerial implications. The most important insight concerns the fundamental trade-off between efficiency and equality in founder negotiations. Our analysis suggests that if a founding team has a choice between an equal or unequal split, the choice of an equal split is typically associated with lower expected performance. While there may be valid reasons for choosing the equal split, our analysis suggests that there is an efficiency-equality trade-off to consider.

[30] The correlation coefficient is -0.1277 (t=0.0299). Furthermore, the correlations of this dummy with Family, Quick Negotiation, Outside Investment and VC are -0.0719 (t=0.2229), -0.1980 (t=0.0007), 0.2272 (t=0.0001) and 0.2726 (t=0.0000).

[31] A related question of interest is whether companies that don't raise any venture capital do so because they can't get it, or because they don't even try. In our supplementary 2013 survey we ask how much companies tried to raise capital from venture capitalists. Of the 60 companies without venture capital that responded, 63% never tried, 10% tried a little, 22% tried a moderate amount and 5% tried a lot. We find no significant correlation between trying to raise venture capital and equal splitting, with a correlation coefficient of 0.0559 (t=0.6717). This finding is of descriptive interest, but from a theory perspective it matters less how much effort is spent on raising venture capital. This is because fundraising efforts are endogenous to their expectations of being able to raise any funding in the first place.

Table 1. Variable Definitions

Variable	*Description*	*Survey Question*
Key Variables (discussed in section 3.2)		
Family Team	Dummy variable for whether all founders are related to each other, or not.	Calculated from "How many of the founders were related to each other?" and "What was the size of the founding team?"
Quick Negotiation	Dummy variable for whether founders took more less than one day to negotiate their founder stakes	Calculated from "How much time did the founders spend negotiating the initial equity split?"
Equal Splitting	Dummy variable for whether all founders received (did not receive) the same amount of equity	Calculated from "% of company's equity received at time of initial equity split"
Outside Investment	Whether the startup had obtained financing from outside investors	(For each round of financing) Whether angel investors, VCs, or corporate investors participated in the round
Venture Capital	Whether the startup had obtained financing from venture capitalists	(For each round of financing) "Venture capitalist(s) participated in this round"
Control Variables (discussed in section 3.3)		
Years of Exp	Number of years of prior work experience before founding the venture	"Years of work experience *before* founding this company"
Entre Exp	Whether the founder had prior founding experience; dummy variable	"Previously founded another company?"
Entre Success	For serial entrepreneurs, degree of success in the prior startup	"The previously founded company: (A.) Successfully went public, (B.) Was acquired by another company, (C.) Is still operating as an independent company, (D.) Is no longer in business?"
Mana Exp	Whether the person had prior managerial experience before this company	"Prior managerial experience?"
Ideas	Whether the founder came up with the idea on which the venture was based; dummy variable	"Founder whose idea it was to begin this venture"
Capital	Amount of initial founding capital provided by the founder, categorized by: $0k, $1k - $25k, $26k - $100k, $101k - $500k, More than $500k	"Amount of founding capital contributed by this founder"
Team size	Number of people in the founding team	"Number of people who founded your company"
Age	Continuous variables capturing the time elapsed between first recorded date and date of the survey	The first recorded date is calculated as the lower of two dates: "Month/Year your company was founded" and "When did the founders split the equity?"
Industry dummies	The venture's industry segment: Software, Hardware, Cleantech, Pharmaceuticals, Life sciences (excl. Pharma) and Other	"Please select one primary as well as a secondary business segment if applicable"
Geography dummies	Where the venture was located: California, Massachusetts, Rest of US, and Canada	Calculated from: "State in which your company is headquartered"
Agreement year dummies	Dummy variables capturing the year in which the founding team split the equity	Calculated from: "When did the founders split the equity?"
Survey year dummies	Dummy variables capturing the year in which the respondent answer they survey	
Auxiliary Variables (discussed in section 5)		
Same Salary	Dummy variable whether founders received the same salary, measured once at founding, and once at the time of survey.	Calculated from "Annual cash salary"
Balanced Control	Dummy variable takes value 0 if exactly one founder holds the CEO and/or Chair position, 1 if either no or multiple founders hold such positions	Calculated from list of executive team and board of directors

Table 2. Descriptive Statistics

	Variable	Mean	Std. Dev.	Min	Max
(1)	Family Team	0.0571	0.2321	0.0000	1.0000
(2)	Quick Negotiation	0.4173	0.4933	0.0000	1.0000
(3)	Equal Splitting	0.3155	0.4649	0.0000	1.0000
(4)	Outside Investment	0.7818	0.4131	0.0000	1.0000
(5)	Venture Capital	0.4297	0.4952	0.0000	1.0000
(6)	Team Size	2.7665	1.1395	2.0000	12.0000
(7)	Age	6.6240	4.7498	0.0834	37.7500
(8)	Avg Years of Exp	16.3660	8.2070	0.0000	50.0000
(9)	Avg Entre Exp	0.3707	0.3702	0.0000	1.0000
(10)	Ave Entre Success	0.1786	0.2879	0.0000	1.0000
(11)	Avg Mana Exp	0.6428	0.3940	0.0000	1.0000
(12)	Avg Ideas	0.4994	0.3433	0.0000	1.0000
(13)	Avg Capital	0.0693	0.1085	0.0000	0.5000
(14)	COV Years of Exp	0.3165	0.3453	0.0000	2.6458
(15)	COV Entre Exp	0.6165	0.7674	0.0000	3.4641
(16)	COV Entre Success	0.4372	0.7286	0.0000	3.4641
(17)	COV Mana Exp	0.4250	0.6524	0.0000	2.6458
(18)	COV Ideas	0.8592	0.7874	0.0000	3.1623
(19)	COV Capital	0.5055	0.6686	0.0000	2.4495
(20)	Software	0.3770	0.4848	0.0000	1.0000
(21)	Hardware	0.2350	0.4241	0.0000	1.0000
(22)	Cleantech	0.0820	0.2745	0.0000	1.0000
(23)	Pharmaceutical	0.1032	0.3044	0.0000	1.0000
(24)	Life Sciences	0.1186	0.3234	0.0000	1.0000
(25)	California	0.3184	0.4660	0.0000	1.0000
(26)	Massachusetts	0.1816	0.3856	0.0000	1.0000
(27)	Canada	0.0146	0.1202	0.0000	1.0000
(28)	Same Salary (at founding)	0.5318	0.4998	0.0000	1.0000
(29)	Same Salary (at survey)	0.3862	0.4872	0.0000	1.0000
(30)	Balanced Control	0.2070	0.4053	0.0000	1.0000

Table 3. Pairwise correlations

All variables are defined in Table 1. Variable numbers can be found in Table 1. Standard errors are reported underneath the correlation coefficients.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
(2)	0.137											
	0.0000											
(3)	0.1249	0.1787										
	0.0000	0.0000										
(4)	-0.0993	-0.0701	-0.0497									
	0.0002	0.0095	0.066									
(5)	-0.0863	-0.0228	-0.0489	0.4587								
	0.0014	0.3997	0.0705	0.0000								
(6)	-0.1435	-0.2016	-0.2785	-0.0008	0.0639							
	0.0000	0.0000	0.0000	0.9752	0.0182							
(7)	0.1257	-0.0104	0.0308	-0.0052	-0.0227	-0.0259						
	0.0000	0.7007	0.255	0.8487	0.4007	0.3386						
(8)	-0.0262	0.0034	-0.09	0.0211	0.0249	0.0603	-0.0694					
	0.3328	0.8994	0.0009	0.4366	0.3574	0.0259	0.0102					
(9)	0.0379	0.0444	-0.0219	-0.0115	-0.0204	-0.0936	-0.1641	0.2579				
	0.1619	0.1007	0.4184	0.6707	0.4508	0.0005	0.0000	0.0000				
(10)	-0.0376	0.0192	-0.021	0.0354	0.0212	-0.0257	-0.0783	0.2469	0.6			
	0.1646	0.4776	0.4386	0.1908	0.4343	0.3421	0.0038	0.0000	0.0000			
(11)	-0.0438	0.0281	-0.0169	-0.0516	-0.0574	-0.0571	-0.1189	0.331	0.2569	0.2796		
	0.1055	0.2986	0.5313	0.0564	0.0338	0.0348	0.0000	0.0000	0.0000	0.0000		
(12)	-0.024	0.0578	0.1521	0.0325	0.0131	-0.2141	0.0079	0.024	0.1526	0.1532	0.2274	
	0.3751	0.0327	0.0000	0.2296	0.6279	0.0000	0.7713	0.3746	0.0000	0.0000	0.0000	
(13)	0.0849	-0.0167	0.0312	-0.1399	-0.1689	-0.0828	0.1141	0.1944	0.1419	0.2133	0.182	0.0946
	0.0017	0.5379	0.2497	0.0000	0.0000	0.0022	0.0000	0.0000	0.0000	0.0000	0.0000	0.0005

Table 4. Determinants of Quick Negotiation and Equal Splitting

The table reports the coefficient estimates of Probit regressions and their associated robust standard errors. ***, ** and * indicate statistical significance at confidence levels of 99%, 95% and 90% respectively. Controls include industry, geography, agreement year and survey year dummies, as well as a constant term. All variables are defined in Table 1.

	DV: Quick Negotiation	DV: Equal Splitting		
Independent Variables				
Family Team	0.661***	0.558***		0.480***
	(0.164)	(0.164)		(0.165)
Quick Negotiation			0.360***	0.334***
			(0.0787)	(0.0795)
Avg Years of Exp	0.00408	-0.0120**	-0.0126**	-0.0127**
	(0.00517)	(0.00572)	(0.00574)	(0.00575)
Avg Entre Exp	-0.0247	-0.199	-0.151	-0.196
	(0.126)	(0.136)	(0.136)	(0.137)
Avg Entre Success	0.0823	0.120	0.0687	0.112
	(0.174)	(0.187)	(0.187)	(0.188)
Avg Mana Exp	-0.0175	-0.132	-0.123	-0.126
	(0.110)	(0.118)	(0.117)	(0.117)
Avg Ideas	0.107	0.249	0.221	0.229
	(0.149)	(0.155)	(0.156)	(0.155)
Avg Capital	-0.527	0.349	0.529	0.427
	(0.349)	(0.368)	(0.368)	(0.373)
COV Years of Exp	0.00523	-0.269**	-0.263*	-0.274**
	(0.115)	(0.134)	(0.136)	(0.136)
COV Entre Exp	-0.0328	-0.0242	-0.0234	-0.0187
	(0.0563)	(0.0624)	(0.0633)	(0.0634)
COV Entre Success	0.0544	-0.0427	-0.0474	-0.0461
	(0.0670)	(0.0801)	(0.0807)	(0.0806)
COV Mana Exp	-0.0830	-0.0597	-0.0416	-0.0527
	(0.0625)	(0.0693)	(0.0700)	(0.0702)
COV Ideas	0.0223	-0.277***	-0.273***	-0.283***
	(0.0645)	(0.0695)	(0.0696)	(0.0696)
COV Capital	-0.0855	-0.430***	-0.430***	-0.430***
	(0.0574)	(0.0686)	(0.0687)	(0.0687)
Team Size	-0.233***	-0.348***	-0.342***	-0.324***
	(0.0426)	(0.0600)	(0.0602)	(0.0601)
Age	-0.0976**	0.0247	0.0295	0.0363
	(0.0419)	(0.0438)	(0.0446)	(0.0442)
Age-Squared	0.00280**	-0.00125	-0.00131	-0.00157
	(0.00117)	(0.00121)	(0.00128)	(0.00124)
Controls	YES	YES	YES	YES
Number of Observations	1367	1367	1367	1367
Pseudo R-square	0.0663	0.1724	0.1774	0.1825
Chi-square	111.06	210.88	221.19	229.90

Table 5: Determinants of Outside Investments and Venture Capital

The table reports the coefficient estimates of Probit regressions and their associated robust standard errors. ***, ** and * indicate statistical significance at confidence levels of 99%, 95% and 90% respectively. Controls include industry, geography, agreement year and survey year dummies, as well as a constant term. All variables are defined in Table 1.

	DV: Outside Investments			**DV: Venture Capital**		
Independent Variables						
Family Team	-0.451***		-0.413**	-0.391**		-0.362**
	(0.166)		(0.170)	(0.171)		(0.171)
Equal Splitting		-0.237***	-0.217**		-0.147*	-0.127
		(0.0901)	(0.0913)		(0.0817)	(0.0822)
Avg Years of Exp	0.00896	0.00778	0.00796	0.00724	0.00660	0.00661
	(0.00585)	(0.00589)	(0.00587)	(0.00524)	(0.00526)	(0.00527)
Avg Entre Exp	-0.107	-0.159	-0.123	-0.130	-0.169	-0.140
	(0.139)	(0.139)	(0.140)	(0.127)	(0.126)	(0.127)
Avg Entre Success	0.415**	0.457**	0.423**	0.333*	0.355**	0.335*
	(0.200)	(0.199)	(0.200)	(0.181)	(0.179)	(0.181)
Avg Mana Exp	-0.363***	-0.368***	-0.370***	-0.266**	-0.270**	-0.270**
	(0.130)	(0.131)	(0.131)	(0.110)	(0.111)	(0.111)
Avg Ideas	0.100	0.166	0.152	0.261**	0.297**	0.289**
	(0.140)	(0.141)	(0.142)	(0.124)	(0.126)	(0.126)
Avg Capital	-1.922***	-1.979***	-1.920***	-2.306***	-2.361***	-2.304***
	(0.372)	(0.369)	(0.369)	(0.396)	(0.395)	(0.396)
COV Years of Exp	-0.104	-0.138	-0.132	-0.226**	-0.251**	-0.242**
	(0.123)	(0.124)	(0.124)	(0.114)	(0.115)	(0.116)
COV Entre Exp	-0.00744	-0.00559	-0.0108	0.0183	0.0170	0.0149
	(0.0627)	(0.0628)	(0.0630)	(0.0567)	(0.0567)	(0.0567)
COV Entre Success	-0.0272	-0.0335	-0.0333	0.0233	0.0236	0.0223
	(0.0750)	(0.0751)	(0.0752)	(0.0672)	(0.0673)	(0.0673)
COV Mana Exp	-0.157**	-0.176**	-0.166**	-0.138**	-0.148**	-0.144**
	(0.0693)	(0.0699)	(0.0696)	(0.0631)	(0.0631)	(0.0632)
Team Size	-0.0271	-0.0304	-0.0420	0.0645*	0.0624*	0.0550
	(0.0393)	(0.0398)	(0.0398)	(0.0347)	(0.0350)	(0.0352)
Age	0.122**	0.126**	0.124**	0.0910**	0.0949**	0.0914**
	(0.0506)	(0.0500)	(0.0503)	(0.0414)	(0.0412)	(0.0413)
Age-Squared	-0.00466***	-0.00481***	-0.00474***	-0.00232**	-0.00248**	-0.00234**
	(0.00150)	(0.00147)	(0.00150)	(0.00114)	(0.00113)	(0.00114)
Controls	YES	YES	YES	YES	YES	YES
Number of Observations	1367	1367	1367	1367	1367	1367
Pseudo R-square	0.0863	0.0819	0.0903	0.0788	0.0775	0.0801
Chi-square	111.16	107.96	115.72	135.48	135.62	137.90

Table 6: Instrumental Variable Regressions

The table reports the coefficient estimates of linear two-stage instrumental variable regressions and their associated robust standard errors. ***, ** and * indicate statistical significance at confidence levels of 99%, 95% and 90% respectively. Controls include industry, geography, agreement year and survey year dummies, as well as a constant term. All variables are defined in Table 1.

	Dependent Variables		
	Equal Splitting	**Outside Investments**	**Venture Capital**
Independent Variables	First stage	Second stage	
Quick Negotiation	0.106***		
	(0.0245)		
COV Ideas	-0.086***		
	(0.0209)		
COV Capital	-0.111***		
	(0.017)		
Equal Splitting		-0.0574	0.160
		(0.104)	(0.127)
Family Team	0.180***	-0.124**	-0.165***
	(0.0575)	(0.0605)	(0.0595)
Avg Years of Exp	-0.00418**	0.00220	0.00350*
	(0.00173)	(0.00165)	(0.00200)
Avg Entre Exp	-0.0547	-0.0303	-0.0365
	(0.0423)	(0.0407)	(0.0466)
Avg Entre Success	0.0161	0.104**	0.110*
	(0.0585)	(0.0527)	(0.0653)
Avg Mana Exp	-0.0426	-0.0981***	-0.0913**
	(0.0380)	(0.0329)	(0.0414)
Avg Ideas	0.0833*	0.0400	0.0579
	(0.0500)	(0.0415)	(0.0521)
Avg Capital	0.150	-0.559***	-0.807***
	(0.121)	(0.118)	(0.124)
COV Years of Exp	-0.0946**	-0.0389	-0.0596
	(0.0390)	(0.0371)	(0.0443)
COV Entre Exp	-0.0105	-0.00337	0.00830
	(0.0175)	(0.0179)	(0.0208)
COV Entre Success	-0.00687	-0.00447	0.0122
	(0.0203)	(0.0204)	(0.0246)
COV Mana Exp	-0.0165	-0.0452**	-0.0452*
	(0.0208)	(0.0195)	(0.0238)
Team Size	-0.0579***	-0.0118	0.0361**
	(0.0113)	(0.0137)	(0.0157)
Age	0.0119	0.0357***	0.0289**
	(0.0141)	(0.0135)	(0.0145)
Age-Squared	-0.000535	-0.00140***	-0.000630*
	(0.000397)	(0.000371)	(0.000352)
Controls	YES	YES	YES
Number of Observations	1367	1367	1367
Centered R-squared	NA	0.0962	0.0700
F-value	NA	4.34	4.65

Table 7. Equal Splitting and Equal Outcomes

The table reports the coefficient estimates of Probit regressions and their associated robust standard errors. ***, ** and * indicate statistical significance at confidence levels of 99%, 95% and 90% respectively. Controls include industry, geography, agreement year and survey year dummies, as well as a constant term. All variables are defined in Table 1.

	DV: Same Salary (at founding)	**DV: Same Salary (at time of survey)**	**DV: Balanced Control**
Independent Variables			
Equal Splitting	0.848***	0.368***	0.252***
	(0.197)	(0.131)	(0.0908)
Family Team	-0.163	-0.126	-0.258
	(0.439)	(0.278)	(0.193)
Quick Negotiation	0.530***	0.338***	-0.262***
	(0.172)	(0.114)	(0.0828)
Avg Years of Exp	-0.0266**	-0.0235***	0.00495
	(0.0120)	(0.00862)	(0.00609)
Avg Entre Exp	-0.208	0.250	0.203
	(0.294)	(0.199)	(0.140)
Avg Entre Success	0.352	-0.804***	0.00177
	(0.370)	(0.268)	(0.194)
Avg Mana Exp	0.159	-0.167	-0.116
	(0.306)	(0.201)	(0.125)
Avg Ideas	2.027	0.129	0.0301
	(1.659)	(0.279)	(0.156)
Avg Capital	0.292	1.255**	0.545
	(0.816)	(0.528)	(0.382)
COV Years of Exp	-0.443*	-0.252	0.104
	(0.257)	(0.170)	(0.125)
COV Entre Exp	-0.0859	-0.102	-0.0756
	(0.131)	(0.0857)	(0.0605)
COV Entre Success	-0.122	0.00341	-0.0325
	(0.144)	(0.104)	(0.0733)
COV Mana Exp	-0.0482	0.123	-0.108
	(0.157)	(0.100)	(0.0714)
COV Ideas	0.753	0.00366	-0.0967
	(0.605)	(0.113)	(0.0692)
COV Capital	-0.124	-0.151*	-0.00638
	(0.135)	(0.0914)	(0.0639)
Team Size	0.137	-0.0750	0.113***
	(0.0912)	(0.0618)	(0.0390)
Age	0.00398	-0.0616	0.0458
	(0.113)	(0.0654)	(0.0463)
Age-Squared	-0.00256	0.00136	-0.000701
	(0.00370)	(0.00167)	(0.00128)
Controls	YES	YES	YES
Number of Observations	314	664	1367
Pseudo R-square	0.2017	0.1782	0.0470
Chi-square	85.29	148.46	67.23

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