Non-Compete Clauses, Employee Effort and Spin-off Entrepreneurship: A Laboratory Experiment

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We experimentally test the effect of enforceable non-compete clauses on working effort and spin-off entrepreneurship. An employee invests effort in the probability of a profitable innovation. After a successful innovation the employee may want to start her own spin-off firm and compete with her prior employer. In the baseline setup without non-compete clause, spin-offs result from failed negotiation about employee compensation even though they

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reduce the joint payoffs of both parties. In two treatments with non-compete clause the employer can prevent successful innovators from leaving the firm. We find no significantly negative effect of non-compete clauses on employee effort, even if compensation is low.

Keywords: non-compete clause, effort, spin-off entrepreneurship, reciprocity, fairness

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1. Introduction

Policy makers around the globe have attempted to emulate the success of Silicon Valley, the undisputed center of the global high-tech industry. Myriads of regional cluster initiatives are named alluding to Silicon Valley - examples range from New York City's Silicon Alley or London's Silicon Roundabout to Polymer Valley (Northeastern Ohio) or Telecom Valley (Sophia Antipolis, France). Efforts to set up new high-tech clusters often involve attempts to recreate Silicon Valley's legendary entrepreneurial culture. As many students of Silicon Valley have observed, the region is characterized by an extraordinarily fluid labor market, which not only features high levels of labor mobility across existing firms, but also exceptionally high rates of new firm formation. To spurn a similar extent of entrepreneurial dynamism, incubator and accelerator facilities for startup firms are set up, networking activities are supported, and access to venture capital is facilitated at many locations, often based on public policy efforts and substantial amounts of taxpayer money.

A crucial element of Silicon Valley's entrepreneurial history are new firms started by individuals who previously worked at incumbent firms in the same or closely related industries. This type of ”spin-off entrepreneurship” (also known as ”spin-out entrepreneurship”) was decisive for the formation and ascent of the Silicon Valley semiconductor industry, one of the Valley's signature industries. Spin-off entrepreneurship led to the formation of Fairchild Semiconductors, which in turn spawned the proverbial ”Fairchildren” - a large number of spin-offs including industry leaders such as Intel and AMD. Spin-offs are similarly prominent in other Silicon Valley industries such as computer disk drives (Christensen, 1993; Agarwal et al., 2004). Silicon Valley's spin-off juggernaut has not gone unnoticed, and it has been suggested that spin-offs are key to understanding the outstanding role the region plays in the contemporary high-tech industry (Klepper, 2010).

A recent strand of literature suggests that Silicon Valley's spin-off dynamics, as well as more generally its fluid labor market, may not just be a ”cultural” specificity, but may have developed in response to the unusual institutional setup of the Californian labor market. More specifically, Silicon Valley offers an exceptionally good environment for aspiring spin-off entrepreneurs, as Californian employers cannot prevent their employees from moving to competitors or to start their own firms in the same industry (Gilson, 1999). As opposed to most U.S. states or other industrialized countries, contractual provisions to this end - known as covenants not to compete or non-compete clauses (NCCs) for short - are not enforced in the state of California.

Non-compete clauses have been found to reduce labor mobility (Gilson, 1999) and to contribute to regional brain drain, as affected employees may be induced to relocate to jurisdictions where these clauses are not enforced (Marx et al., 2015). These findings suggest that policy makers may foster entrepreneurial activities by outlawing non-compete clauses or at least exempting entrepreneurial ventures from their enforcement. But would that be good policy? This question is difficult to answer in the light of field data. On the one hand, non-compete clauses may have desirable effects. A particularly relevant concern in this context is giving employers an incentive to invest in their employees' (non-firm specific) human capital (Garmaise, 2011). On the other hand, besides their chilling effects on labor mobility there may be further cost attached to enforceable NCCs.

Theoretical work (Kraekel and Sliwka, 2009) shows that non-compete clauses may have an adverse effect on employee effort. NCCs deprive an employee of the option to open her own business, or to offer her knowledge and talent to a new employer. This diminishes the scope for renegotiating her wage once her innovation efforts have led to success, which in turn reduces her incentive to exert innovation effort in the first place - or so the theoretical argument goes. The reduction in effort may be particularly pronounced if employees anticipate that their employer may not value their ideas as highly as they do themselves. In this situation, which has frequently been observed in the context of spin-off entrepreneurship (Klepper and Thompson, 2010), restrictions to employee mobility may be particularly problematic from a welfare perspective.

Effects of non-compete clauses on employee effort have largely been neglected in the empirical literature. Effort is not directly observable and thus difficult to study using field data. This suggests to close the gap by way of a laboratory experiment. In this paper, we translate the relationship between employer and employee into a simple sequential principal-agent game and compare behavior in a baseline design without NCC to two treatments with a NCC. In the experiment, the more effort the agent exerts the more likely a profitable innovation. If the innovation is successful, the agent may leave the firm to become a competitor; this is how we experimentally implement spin-off entrepreneurship. Instead, the agent may make a take-it-or-leave-it demand for a bonus that makes her willing to stay.

We use three variants of this basic setup differing only in the outside option of the bargaining stage. In the *Baseline* setup, if the employee leaves both players compete in the marketplace and each makes a small profit. In this case, joint profits are substantially lower than if the employee had stayed. This is due to competition, which in the experiment favors only the customers. In the two treatments with NCC, the agent receives a commonly known and predefined compensation and is prevented from competing in the market. In the *High* treatment, if renegotiations fail, her compensation is the same as the renegotiation outcome in the *Baseline* (under standard economics assumptions of commonly known opportunistic rationality). In the *Low* treatment, her compensation is considerably below this benchmark. By making sure that the two treatments and the *Baseline* only differ in one respect, namely the outside option at the renegotiation stage, we may directly compare behavior between these different settings.

In the setting of our experiment, self-interested rational agents would exert lower effort (relative to the *Baseline*) if an NCC with a *Low* level of compensation is imposed. Contrary to this theoretical prediction, we find no significant effect of the NCC on effort, irrespective of the level of compensation. We observe strong reciprocal patterns not only in wages and effort, but also in how renegotiation demands depend on received wages and invested effort.

From the perspective of the two players, enforcing non-compete clauses is efficiency enhancing because the spin-off event, and thus market competition, is prevented. In the experiment, if mandated compensation is low, employers win from the NCC and employees do not lose. If mandated compensation is high, employees win and employers do not lose.

In dealing with non-compete clauses, policy makers pondering a legal reform do not operate in a legal vacuum, but have to consider introducing or abolishing it. To see how a reform of NCCs affects experienced actors, we expose participants to a regime change. For the two supply-side parties, abolishing the enforcement of non-compete clauses clearly has adverse effects. Whether compensation is high or low, joint profit is reduced. Yet effort increases if the clause is no longer enforced and if compensation was low before. Newly introducing non-compete clauses has less pronounced effects. There is only a small positive effect on joint profit if compensation is high.

The remainder of the paper is organized as follows: Section 2 provides an overview of related literature. Section 3 introduces the design of the experimental game. In Section 4 we formulate hypotheses under alternative behavioral assumptions. We present the experimental setup in Section 5 and analyze the experimental data in Section 6. Section 7 concludes with a discussion of our main results and their relevance.

2. Non-Compete Clauses: Legal Provisions and Prior Literature

Legal orders in various jurisdictions treat non-compete clauses in very different ways (Ingram, 2002). In the U.S., some states simply prohibit them, such as California (Cal. Bus. & Prof. Code 16600), North Dakota (N.D. Cent. Code 9-08-06), and historically Michigan, where the respective provision (Mich. Comp. Laws 445.761) was repealed in 1985 as part of a broader antitrust reform (Marx et al., 2009). Most jurisdictions, however, apply a rule of reason and investigate how onerous the constraint is for the agent, and how legitimate the reasons are for the principal (for detail, see Ingram, 2002). Frequently, the ban on competition is limited in time, e.g. for two years, and in space, e.g. to the state where the firm is established. Similar to the U.S., the laws on non-compete clauses (NCCs) differ across European countries. Such covenants are illegal under European Community legislation if their duration exceeds five years, or if the contracting partner promises not to manufacture, purchase or sell goods or services after the termination of the contract (Art. 5 (1) Commission Regulation No. 330/2010, OJ L 102/1). Another example is a characteristic set of rules for non-compete clauses in German law, which inspired the setup of our model. In Germany, non-compete clauses are limited to a maximum of 2 years (§74 a I 3 HGB) and they are valid only if the principal pays at least half of the yearly salary (§74 II HGB).

Non-compete clauses are widespread in jurisdictions where they are legal, and nearly ubiquitous in high-technology industries (Stuart and Sorenson, 2003). The literature has mainly been interested in their effect on labor mobility. Gilson (1999) first suggested that the non-enforceability of non-compete clauses enhanced labor mobility in California, which in turn may have contributed to Silicon Valley's emergence as the world's leading high-technology cluster. This conjecture has motivated subsequent studies exploring the potential trade-off between employers' incentives to invest in employees' human capital formation and positive externalities emanating from the mobility of highly skilled workers and its repercussions on industrial dynamics (Fallick et al., 2006; Franco and Mitchell, 2008). Empirical research based on field data provides substantial (albeit indirect) evidence indicating that non-compete clauses are effective constraints to employee mobility. Marx et al. (2009) use patent data to study how the legal change in Michigan affected the likelihood of inventors switching employers within the state. Relative to other U.S. states where non-compete clauses remained illegal, Michigan experienced a significantly lower increase in intra-state inventor mobility after the reform of 1985. This finding is corroborated by geographic patterns of citations to U.S. university patents (Belenzon and Schankerman, 2013). While U.S. state borders generally constrain knowledge flows (as measured by patent citations), the effect is significantly weaker for post-reform Michigan, suggesting a decrease in intra-state knowledge flows due to restricted employee mobility. Marx et al. (2015) furthermore show that post-reform Michigan suffered from an exodus of inventors to states without enforceable non-compete clauses.

Garmaise (2011) studies effects of non-compete clauses on top-level executives of publicly traded U.S. firms. He shows that increased enforceability of non-compete clauses is associated with decreased rates of mobility and with lower, more salary based compensation. This is interpreted as supporting a model in which both employers and employees can invest in non-contractible human capital, and where the enforceability of non-compete clauses shifts incentives to invest into human capital from employees to employers. Further results by Stuart and Sorenson (2003) for the U.S. biotechnology industry indicate that non-compete clauses also reduce rates of spin-off entrepreneurship. Samila and Sorenson (2011) likewise find that the regional availability of venture capital is more strongly associated with patent counts, new firm formation and employment growth in U.S. states where non-compete clauses are not enforceable. They attribute these results to spin-off entrepreneurship as well as positive effects of employee mobility on knowledge spillovers and the quality of employee-employer matching.

The substantial prior work on non-compete clauses notwithstanding, only a very small literature has explored the core issue of the present paper: the effect of NCCs on employee effort. In the model by Kraekel and Sliwka (2009), if an innovation occurs, an employer is always better off ex post with a non-compete clause. However, ex-ante incentives for the worker to exert effort are higher without a clause due to the prospect of a high retention offer in case of a successful innovation. Thus, whether or not the employer would like to impose a non-compete clause depends on the relative importance of effort. The empirical study of Marx et al. (2009) finds no evidence suggesting that the Michigan reform decreased overall patenting rates (which could be seen as a proxy of employee effort) of the state's inventors, but more direct individual-level results are lacking.

3. Model

In this section, we outline a simple principal-agent game, from which we will subsequently derive hypotheses about players' behavior. Suitably parameterized, the model also provides the foundation of our experiment. In the *Baseline* setup, a principal P and an agent A are exogenously matched and interact once. As is typical in principal-agent relationships, the initiative is taken by P who unilaterally chooses wage $w \in [0,W]$. Knowing w, agent A chooses effort $e \in [1,E]$, which remains private information. Nature decides whether a project is successful with probability $p = \sqrt{e}/\rho$, with $\rho > \sqrt{E}$. If the project is not successful, the game ends and payoffs are $V_P = \pi - w$ for P and $V_A = w - e$ for A, respectively. If the project is successful, A can ask for a retention offer $v \in [0, \Pi]$, with $\Pi > \pi$. P may only accept or reject. If P accepts, payoffs are $V_P = \Pi - w - v$ for P and $V_A = w + v - e$ for A. If P rejects, payoffs equal $V_P = \alpha\Pi - w$ and $V_A = \alpha\Pi + w - e$, with $\alpha < 1/2$. In the *Treatments*, if P rejects, payoffs equal $V_P = \Pi - w - k$ and $V_A = w + k - e$.

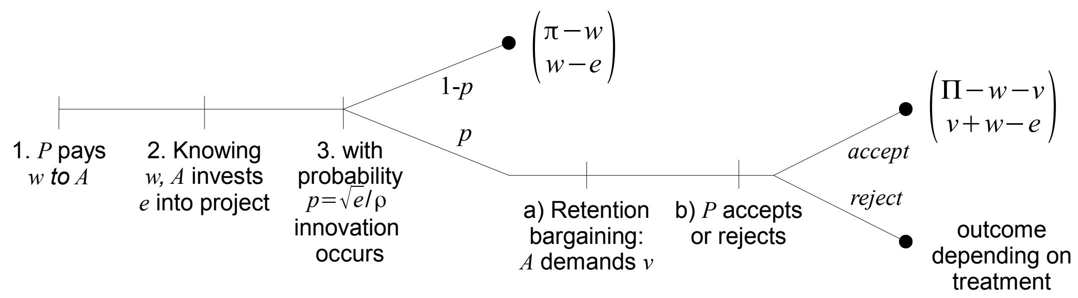

Figure 1: Timeline of the Principal-Agent Interaction

Note: Wage: $w \in [0,W]$ with $W > 0$; effort: $e \in [1, E]$ with $E > 1$; probability of success: $p = \sqrt{e}/\rho$, with $\rho > \sqrt{E}$; random success: Π with probability p and π (with $\Pi > \pi > 0$) with probability $1 - p$; retention offer $v \in [0, \Pi]$.

The model fits any activity that requires unobservable effort from an agent for reducing the uncertainty about the success of some activity that is profitable for the principal. One such activity is innovation, another one would be a potential extension of the customer base. If the project fails, the principal's payoff π is small. If the project is successful, the principal's payoff depends on the agent's choice. If the agent stays in the firm, the principal reaps monopoly profit Π. If, however, the agent leaves the firm and exploits the success for herself, both only earn $\alpha\Pi$. By $\alpha < 1/2$, this competitive profit is strictly below half the monopoly profit Π.[1] Consequently, from the perspective of principal and agent, it is efficient to keep the agent within the firm.[2] In the absence of a non-compete clause, the principal cannot enforce this outcome, though. The employee's inalienable right to leave the firm is captured by giving all the bargaining power to the agent once the project has proven successful. Without enforceable non-compete clauses, all that the principal can do to preempt ex-post conflict is to pay a wage upfront. While doing so does not change the opportunity structure, the agent might be less inclined to exploit a principal who has been generous in the first place.

The uneven distribution of bargaining power is maintained in the treatments with non-compete clauses. The agent may still make a take-it-or-leave-it offer if the project was a success. However, while the principal may only be sure to get the competitive profit in the *Baseline*, in the *Treatment* she is guaranteed the monopoly profit. In line with real-world legal provisions, we assume that the agent is compensated for the reduction in outside options brought about by the NCC. We distinguish between two alternative treatments that differ in the size of the compensation payment. Compensation payments are defined such that in the *High* treatment, a profit maximizing principal who expects the agent to have the same preferences is indifferent between the presence and the absence of the clause (see section 4 below). By contrast, in the *Low* treatment, according to the same assumptions, the principal strictly prefers an institutional environment with the clause. The opposite holds for the agent since the smaller compensation reduces her bargaining power. In the *High* treatment, $k = (1 - \alpha)\Pi$, while k is strictly smaller in treatment *Low*.

[1] As pointed out by an anonymous reviewer, our model does not reflect the riskiness of spin-off entrepreneurship, which could have been captured by letting A's α-share vary randomly. This has been avoided to limit the complexity of the experimental design.

[2] If the demand side is taken into consideration, this may of course be associated with a loss in social welfare.

4. Hypotheses

In developing hypotheses about the outcomes of the principal-agent interaction, we first assume rational participants who hold standard, opportunistic and risk neutral preferences, as well as common knowledge of these preferences. We look for the unique sequentially rational equilibrium (Kreps and Wilson, 1982). If the innovation is successful, P accepts if $v \leq (1-\alpha)\Pi$. Consequently A demands $v = (1-\alpha)\Pi$ and expects to earn

$$E[V_A] = \frac{\sqrt{e}}{\rho}(1-\alpha)\Pi + w - e \tag{1}$$

which she maximizes by investing (for relevant parameterization) optimal effort[3]

$$e^* = \frac{(1-\alpha)^2\Pi^2}{4\rho^2} \ . \tag{2}$$

As the initial wage is merely a gift, the principal sets optimal wage $w^* = 0$.

The game has multiple other equilibria. For instance, the bargaining stage after a successful innovation has a continuum of equilibria, each characterized by A demanding some v^+, with $\alpha\Pi < v^+ < (1-\alpha)\Pi$, and P accepting any $v \leq v^+$. There also exist equilibria with positive w.[4] Yet none of these equilibria are sequentially rational.

If there is a non-compete clause, the size of k affects P's willingness to accept v. She accepts any $v \leq k$, and rejects otherwise. This means that $(1-\alpha)\Pi$ in (1) is replaced by k and, thus, optimal effort changes to

$$e^* = \frac{k^2}{4\rho^2} \ . \tag{3}$$

[3]In the experiment, parametrization will guarantee $0 < e^* < E$.

[4] Suppose, for example, that A exerts optimal effort e^*, demands some $v > (1-\alpha)\Pi$ if $w < w^+$, and $v = (1-\alpha)\Pi$ if $w \geq w^+$. Then P pays w^+, accepts if $v \leq (1-\alpha)\Pi$, and rejects otherwise. This strategy profile constitutes an equilibrium as unilateral deviation leaves both with at most as much as before.

While higher k increases effort, it also reduces the profit of the principal in case of success. It is easy to show that the principal would see his expected return maximized for

$$k_P^* = \frac{\Pi - \pi}{2} \ . \tag{4}$$

The agent on the other hand always prefers a larger k whereas efficiency is maximized for $k = \Pi - \pi$.

The level of k is therefore critical for predictions. However, there are some natural limits that define our parametrization. Whenever k exceeds what the agent could at most demand in the absence of a NCC, then the principal would never invoke the NCC and rather pay the employee to stay in the firm. Thus a legal requirement of a very large k is equivalent to the absence of a NCC. On the other hand, a very small k is likely to be unconstitutional in societies that grant fundamental human rights to workers, and levels of mandated compensation are indeed substantial in most industrialized countries. Reflecting these considerations, in the experiment we will compare two very different levels of compensation k. In the treatment with a *High* compensation, the principal is indifferent between relying on the non-compete clause and A leaving the firm, i.e. $k = (1 - \alpha)\Pi$. Furthermore, A invests the same effort as in the *Baseline* without NCC. In the treatment with a *Low* statuary compensation on the other hand, the agent expects to earn about a third of what he could earn in the *Baseline*.

This setup leads to the following benchmark solutions:

Hypothesis 1 If all agents are rational, have opportunistic preferences and this is common knowledge, we expect

a) zero wages

b) less effort (and success) in treatment *Low*

c) successful renegotiation, irrespective of treatment

d) lower renegotiation demands v in treatment *Low* [5]

e) lower total profit in *Low*[5]

f) identical effort and outcomes in the *Baseline* and treatment *High*.

In view of the abundant evidence (e.g., on ultimatum experiments, see Güth and Kocher (2014), for a recent review), this benchmark prediction based on common(ly known) opportunism will likely be rejected. In addition, no differences in outcomes between the *Baseline* design and treatment *High* are predicted under these behavioral assumptions, which runs counter to abundant experimental evidence indicating the importance of other-regarding preferences. [6] We expect other-regarding concerns to differ between principal and agent. Whereas the former is more fairness concerned, the latter is more driven by reciprocity concerns.

A small chance of the principal holding non-opportunistic preferences suffices to introduce strategic uncertainty. In this case, the agent has to expect a reduction in payoff. In response, she will reduce effort, compared to the benchmark assuming preferences to be common knowledge.[7] By contrast, in the *High* treatment the agent expects the same payoff as the one predicted in the *Baseline*, this time, however, with certainty due to an exogenous rule. Thus, incentives to invest effort are lower in the *Baseline* than in treatment *High*.

Hypothesis 2 When principals hold social preferences, in comparison to the benchmark solutions,

a) agents make lower demands in the renegotiation in the *Baseline*

b) agents invest less effort in the *Baseline*

[5] Compared to the other settings.

[6] See experimental results on ultimatum bargaining as, for example, summarized in Camerer (2003).

[7] Note that if a selfish principal could ex ante restrict the agent's bargaining power, he would like to do so, as the increase in payoff in case of success outweighs the loss in effort.

c) principals reject high demands in the renegotiation stage

and, furthermore,

Hypothesis 3 Agents invest less effort in the *Baseline* than in treatment *High*.

In equilibrium, wage w is zero. However, in the spirit of an efficiency wage (Akerlof, 1984), principals might want to pay a positive wage. Two reasons hold irrespective of treatment. First, with any positive investment e, the agent makes a loss if the innovation fails. Loss aversion (see Tversky and Kahneman, 1992) could therefore deter agents from investing optimal effort. A positive wage (partly) insures the agent against the risk of loss. Second, principals could also hope that agents reciprocate a high wage by higher effort, as for example in gift exchange experiments (for an overview see Brandts and Charness 2004).

The gift exchange motive is independent of variations in treatment or k. The problem of loss aversion, however, is stronger in the *Baseline* and treatment *High* where optimal effort, and therefore possible losses, are higher than in *Low*. This suggests

Hypothesis 4 Due to reciprocity and (expected) loss aversion (of the agent),

a) principals pay positive wages

b) principals pay lower wages in the *Low* treatment

c) effort increases in the wage.

In treatment *Low* a third effect is possible. A fair minded principal may want to signal her willingness to concede more to the agent in case of a successful innovation in order to induce higher effort. Thus, by paying a high wage, she induces effort both via creating positive reciprocity and via creating an expectation that effort will pay off. While it is unclear whether this outweighs the previous negative effect from Hypothesis 4b, we alternatively expect

Hypothesis 5 Fair minded principals signal their intention to leave more to the agent in case of a successful innovation by paying a high w. Contradicting Hypothesis 4b this may lead to higher wages w in treatment *Low* than according to the benchmark.

Finally, it is possible that agents also show positive reciprocity in the renegotiation stage.

Hypothesis 6 Agents behave reciprocally in the renegotiation stage and demand less the higher the wage.

5. Design

The principal-agent interaction outlined in Section 3 (cf. Figure 1 above) was turned into an experimental game by choosing a suitable parameterization. Specifically, we set $\Pi = 18000, \pi = 4000, E = 6000, \rho = 100, \alpha = 1/3, k_{low} = 7000, k_{high} = 12000$, which results in the point predictions listed in Table 1. Parameters are chosen such that the principal's payoff in case the project fails is constant across treatments. With *High* compensation, principal and agent receive the same payoffs as in the *Baseline* in case the innovation occurs. Consequently, the profit maximizing degree of effort is also the same in both treatments. By contrast, with *Low* compensation, the principal is substantially better off if the project succeeds, compared with the remaining treatments. This translates into substantially lower effort of an agent who maximizes her expected payoff. The interaction is neutrally framed as participant "B" deciding about "investing", with effects also for the "payoff" of participant "B". The experiment was conducted in the lab of the Max Planck Institute of Economics in Jena, Germany.

To investigate whether effects are stable over time, participants played the stage game eight announced times with varying partners. Roles were kept constant throughout and a stranger protocol was adopted; i.e., subjects were randomly rematched in each round. To ensure that observations are independent without inducing participants to

Table 1: Point Predictions Assuming Standard Preferences

	Baseline	*Low*	*High*
w	0	0	0
e^*	3,600	1,225	3,600
p	0.60	0.35	0.60
v	12,000	$\geq$7,000	$\geq$12,000
$V_{A\ success}$	8,400	5,775	8,400
$V_{A\ failure}$	-3,600	-1,225	-3,600
$E[V_A]$	3,600	1,225	3,600
$V_{P\ success}$	6,000	11,000	6,000
$V_{P\ failure}$	4,000	4,000	4,000
$E[V_P]$	5,200	6,450	5,200
$E[V_A] + E[V_P]$	8,800	7,675	8,800

second guess group composition (see, for example Charness, 2000; Montero et al., 2008), participants were assigned to matching groups of 16 (8 P and 8 A) but did not know that matching groups were of limited size. We collected data from eight matching groups for either treatment. In the treatments with non-compete clause, compensation k alternated between being *Low* or *High* from period to period to discourage participants from simply repeating prior choices without reassessing them in light of feedback information. First-round compensation was counterbalanced between matching groups.

In the interest of investigating the effects of a regime change, an unannounced restart followed after the end of period 8.[8] Participants who played the *Baseline* in the first phase were now assigned to an environment with an enforceable non-compete clause and changing size of compensation (alternating from period to period). Participants who were in a context with a non-compete clause now played the *Baseline*. This design was chosen to avoid confounding the institutional comparisons with the selection bias of between-subject comparisons and to control for order effects of institutional changes.

[8] Participants knew from the beginning that the experiment has several parts, but they did not know the design of future parts.

The second phase of the experiment also lasted eight announced periods. Roles and, unbeknownst to participants, matching groups were kept constant throughout.

The experiment was implemented in zTree (Fischbacher, 2007). Participants were invited with the software ORSEE (Greiner, 2004) and had to take a quiz to make sure they had understood the instructions. In eight sessions, we had a total of 256 student participants of various majors, 46.39% female, mean age 23.58. We had eight independent observations in the *Baseline* and in either treatment. Earnings from the experiment were translated into real money at an exchange rate of 1000 Taler = €1.40. We paid out one randomly chosen period from the first and the second part of the experiment. Participants on average earned €20.39 (26.44 USD), €22.46 for principals and €18.38 for agents. The experiment lasted on average around two and a half hours, including admission and payment.

6. Results

We first report treatment effects from the initial eight periods, before we take a closer look at the effects of changing legal regimes. From a policy perspective, comparing outcomes for participants who have not had experiences with the opposite regime is analogous to comparing jurisdictions that honor non-compete clauses with others where they are illegal.

6.1. Effort and Wages

Our study is motivated by the concern that non-compete clauses might stifle agent effort and thereby reduce innovation. The second to left barplot in Figure 2 shows a first, unexpected finding. Against theoretical expectations (Hypotheses 1b and 3), imposing a non-compete clause has no significant effect on effort. Effort in the *Baseline* does not differ significantly from that in either treatment, neither non-parametrically nor parametrically.[9] There is, however, some indication that effort in *High* is (marginally) significantly higher than in *Low* (one-sided signed rank test: $p = 0.0618$, mixed effects panel estimation: $p = 0.0716$).

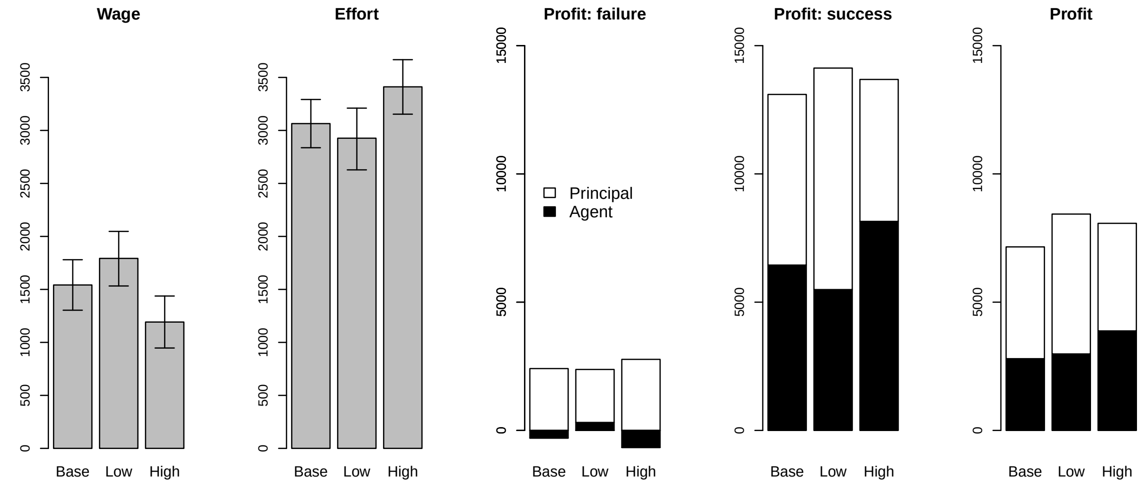

Figure 2: Main Effects, Inexperienced Participants
`Note:` Data from periods 1 to 8 only. 90% Confidence intervals based on 10,000 bootstrap resamples.

Result 1 Preventing agents from leaving the firm does not lead to significant increases or reductions of effort even if compensation paid to agents is low.

The result becomes even stronger if we compare outcomes with predictions from standard economic theory. Signrank tests over means of matching groups reject all point predictions. Effort is lower than predicted in the *Baseline* ($N = 8$, $p = .0499$) (confirming Hypothesis 4b) and in treatment *High* ($N = 8$, $p = .0499$), and it is higher than predicted in treatment *Low* ($N = 8$, $p = .0177$). Also, in all three settings, effort is significantly below the efficient effort of 4900 tokens ($p < .05$). If we express effort relative to the point prediction, they are at 83.06% in the *Baseline*, 87.37% in treatment *High*,

[9] Unless mentioned otherwise, all non-parametric tests in this paper are based on the distribution of averages over all periods per matching group. For parametric estimations, we control for the panel structure by including a random effect per individual nested in a random effect on the group level. Unless mentioned otherwise all tests are two-sided.

but at 227.30% in treatment *Low*. This relative deviation differs significantly between the *Baseline* and treatment *Low* (ranksum test, N = 16, p = .0008) and between treatments *Low* and *High* (signrank test, N = 8, p = .0117), but not between the *Baseline* and treatment *High* (ranksum test, N = 16, p = .4622). If there is an NCC and compensation is low, participants behave in a way that squarely rejects the effect predicted by standard economic theory.

One piece of explanation follows from the barplot of average wages in Figure 2. In the *Baseline* and in the two treatments, principals pay sizeable wages (signrank tests reject the null hypothesis that wages are zero for all three settings at N = 8, p = .0117[10]). Yet wages are higher in the *Low* treatment than in both the *Baseline* (Mann Whitney test, N = 16, $p = .0587$) and the *High* treatment (signrank test, N=8, $p = .0173$).[11] We thus reject Hypothesis 4b and confirm the competing Hypothesis 5: principals use wage to induce higher effort, especially when effort is particularly risky for the agent.

In line with Hypothesis 4c the wage level exerts a strongly positive effect on effort, which is also highly significant statistically (Table 2). Note also the significant and strong interaction between wage and treatment *Low*. In treatment *Low* effort on average increases by 0.46 for every unit increase in w, which is an entire 36% higher than the 0.29 in the *Baseline*. Thus, agents react stronger to changes in the wage if they are in a weaker position. However note that, conditional on wages, effort in treatment *Low* is significantly lower than in the *Baseline*. This shows the critical role of the wage. Through paying a substantial wage, principals counteract the detrimental effect of *Low* compensation on effort (see also Hypothesis 5).

Result 2 Principals pay positive wages. The higher the wage, the higher agents' effort.

[10] We have to test at the limit of the support, which makes the result less reliable. In addition, we therefore also report the highest wage for which the one-sample signrank test still rejects at conventional levels. Using this procedure, we learn that, in the *Baseline*, the wage is at least higher than 1600 (N = 8, p = .049). In the *Low* Treatment, the wage is at least higher than 1900 (N = 8, p = .0117), and in the *High* Treatment, the wage is at least higher than 1300 (N = 8, p = .036).

[11] There is no significant difference in wages between the *Baseline* and *High* (p = .1415).

Table 2: Effect of Wage on Effort, Inexperienced Participants

effort	coef.	s.e.
cons	2137.46***	(248.61)
Low	-694.42**	(308.61)
High	185.51	(294.29)
wage	0.29***	(0.039)
Low×wage	0.17**	(0.068)
High×wage	0.02	(0.065)
N	1024	
p model	<0.001	

Note: Mixed effects linear ML estimation with random effect per subject nested in matching group effect. Data from periods 1 to 8 only. Regressions include control dummies for periods 2 to 8 (not reported). *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$

Wages are more effective in inducing effort if statutory compensation for success is *Low*.

We thus reject the first two statements of Hypothesis 1 (zero wages throughout, less effort in treatment Low), based on the assumption of agents holding standard preferences. If we look at our more behavioral expectations, we find support for the first statement of Hypothesis 4 (positive wages throughout) but reject the second (lower wages in *Low*). This latter rejection means we find support for the opposing Hypothesis 5: it appears that principals try to counter the low incentives to exert effort in treatment *Low* by paying higher wages. We argued that this works, as it signals the principal's intention to concede higher retention offers to the agent in case of success. Before we can make a concluding statement with respect to Hypothesis 5, however, we need to analyse the final bargaining stage.

6.2. Payoffs

We now turn to payoffs and their distribution. Figure 2 shows that payoffs are highest in treatment *Low*, followed by *High* and the *Baseline*. These differences are marginally significant at the 10% level. Table 3 summarizes the p-values from non-parametric tests.[12]

If we look at individual payoffs, the ranking becomes rather different. Principals earn considerably and significantly more in *Low* than in either *High* or the *Baseline*, where in turn they earn about the same. Agents on the other hand earn significantly more in *High* than in either *Low* or the *Baseline*, where in turn they earn about the same. As efforts did not differ significantly between the treatments and the *Baseline*, the reason for the poorer performance of the latter must result from more frequent conflict in the bargaining stage, which will be analyzed in more detail later.

Result 3 Payoffs are higher in the treatments with NCC. Principals earn most when compensation is *Low*, agents when it is *High*.

Table 3: Test Results for Comparisons of Payoffs

	Base vs. *Low*	*Base* vs. *High*	*Low* vs. *High*
Total Payoffs	*0.0754*	*0.0742*	*0.0929*
Agent	0.9164	**0.0460**	**0.0173**
Principal	**0.0157**	0.7527	**0.0117**

Note: Reported values are p-values from two-sided rank-sum tests (comparisons to *Baseline*) and signed-rank tests (*Low* vs. *High*).

Figure 2 shows profits conditioned on success, treatment and role. Due to the high wages, agents make only small losses in the *Baseline* and in treatment *High* in case the innovation fails. For a *Low* compensation, wages are high enough to outweigh the losses on average. If the project fails, agents earn significantly more than predicted, and principals earn considerably less (sign-rank tests compared to prediction, all $p \leq 0.0117$). This is yet another piece of evidence that through the wage, principals insure agents against the risk of project failure.

[12] Note that these results are in line with our results on effort which in combination with chance moves (success of innovation) determines efficiency.

If the innovation project is successful, in the *Baseline* agents earn significantly less than standard economic theory predicts, and principals earn significantly more. Effectively, the difference in earnings predicted by theory vanishes. If non-compete clauses are honored, agent earnings are not significantly different from theoretical predictions. Yet principals earn significantly less than predicted, irrespective of treatment (sign-rank tests compared to prediction, all $p \leq 0.0687$).

6.3. Retention Bargaining and Spin-off Entrepreneurship

Our experiment is deliberately designed such that renegotiations between purely self-interested players should always be successful. Contrary to predictions, we observe a substantial share of renegotiations that fail. Acceptance rates in the different treatments reflect that the consequences of rejecting differ between the treatments. Without the non-compete clause, failed renegotiation results in the agent starting her own firm and in reduced joint profits of both parties. In spite of this efficiency loss (from the perspective of the two players), 10.64% of the renegotiation demands are rejected in the *Baseline*. With 68.18% and 53.04% in the treatment with *Low* and *High* compensation, respectively, rejection rates are significantly higher (Mann Whitney, N = 16, p = .0008) when a non-compete clause limits the consequences of failed renegotiation.

Result 4 A non-negligible fraction of renegotiations fail. Renegotiation demands are more likely to be rejected in the treatments with NCC.

How does behavior unfold in the experiment, and what factors influence agents' renegotiation demands as well as principals' acceptance or rejection? The negotiation protocol gives agents ultimatum power, with outside options differing considerably between treatments. Only in the *Baseline*, agreement results in higher profits for the players and the outside option is rather fair. In treatment *Low* the outside option favors the principal, and vice versa in *High*. Figure 3 shows that in the *Baseline* agents make fairly cautious demands. Almost all of them are below the theoretical prediction of 12,000 tokens. By contrast, if there is a non-compete clause, the majority of agents demand more than the guaranteed compensation. Comparisons show that agents demand most in treatment *High*, and least in treatment *Low*.[13]

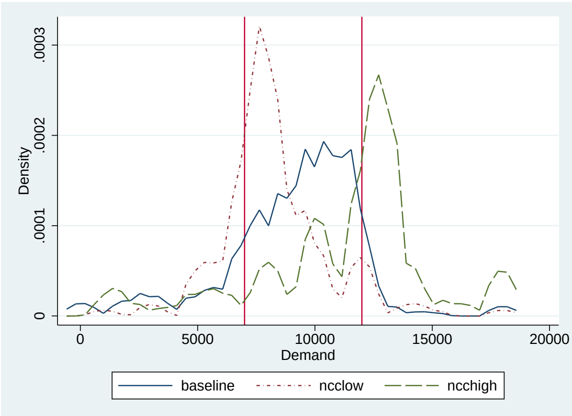

Figure 3: Demands
Note: Red line: theoretical prediction.
Epanechnikov Kernel, bandwidth=400

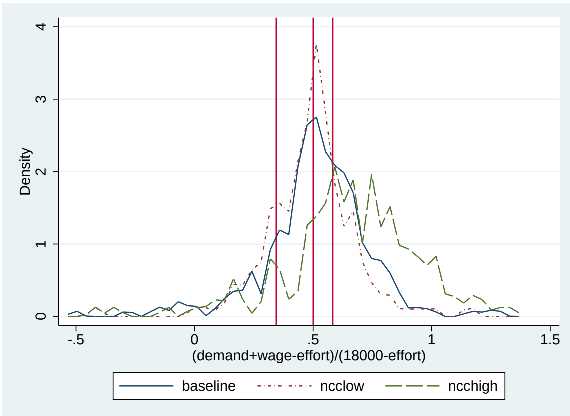

Figure 4: Demanded Share of Surplus
Note: Epanechnikov Kernel,
bandwidth=0.2

With respect to fairness concerns we need to look at how these demands compare to a benchmark. The most straightforward benchmark depends on the wage and invested effort. Figure 4 shows the distributions of the shares of the entire surplus agents demand overall, i.e. taking into account the wage they already received and the invested effort. Remember that effort is private information and agents could use that to their advantage. However, strikingly in the *Baseline* and in treatment *Low* where the outside option is either relatively fair or the agent receives less in conflict, demanded shares are centered on the equal split and do not differ significantly from it according to signrank tests. In treatment *High*, however, where the agent is ahead if the principal rejects, demanded shares are considerably above 0.5 (signrank test, p=0.0089) and significantly higher than in the other cases. We must also compare demanded shares with the benchmark

[13] All treatment comparisons are significant: *Baseline* vs. *Low*, Mann Whitney, $N = 16$, $p = .0567$; *Baseline* vs. *High*, $N = 16$, $p = .0016$; *Low* vs. *High*, signrank test, $N = 8$, $p = .0117$.

cases. In the *Baseline*, shares are significantly smaller than the benchmark (signrank test p=0.0179) and in the treatments they are significantly larger (signrank test *Low* p=0.0059; *High* p=0.0087).

Result 5 Compared to equilibrium shares, agents demand less in the *Baseline* but more in the treatments with non-compete clause.

But how do final demands depend on the history of play? The regressions in Table 4 show that demands of agents in case of success decrease in the wage they received. As model (2) shows, this effect is the same in all treatments including treatment *Low* (see also the joint hypothesis tests in the footnote of the table). Agents are not more sensitive to the size of the wage if the compensation scheme favors the principal. In all treatments, however, demands decrease in wage by significantly less than one (Wald tests, $p < .001$).

Table 4: Explaining Renegotiation Demands, Inexperienced Subjects

demand	(1)	(2)
Low	-781.641**	-593.572
High	2152.843***	2101.854***
wage	-0.246***	-0.238**
Low×wage		-0.073
High×wage		0.027
cons	8021.256***	7987.925***
all models:	$N = 498$; $p < 0.001$	

Note: Mixed effects linear ML estimation with random effect per subject nested in matching group effect. Data from periods 1 to 8 only. Regressions include control dummies for periods 2 to 8 (not reported). *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. Wald tests: Model (2): wage+*Low*×wage=0 $p = 0.020$**; wage+*High*×wage=0 $p = 0.067$*.

Result 6 Demands decline equally strongly in the received wage in all three settings.

In Table 5 we analyze the acceptance rates of renegotiation demands. Model (1) confirms our non-parametric result that overall acceptance is significantly less likely

Table 5: Explaining Acceptance, Inexperienced Subjects

acceptance	(1)	(2)
Low	-3.133***	0.670
High	-2.444***	-0.847
demand		-0.002***
Low×demand		-0.001*
High×demand		-18e-6
cons	2.312***	17.792***
log*N*	498	
p model	<0.001	

Note: Mixed effects logit estimation with random effects on subjects nested in group effects. Model 2 includes dummies for periods 2 to 8 (not reported). Significance: ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

in the treatments with NCC.[14] As model (2) shows, the differences in the base rate of acceptance are due to differences in demands.[15] Acceptance decreases significantly in demands in all settings, but in treatment *Low* it decreases stronger than in the *Baseline*.[16] This contradicts our expectation from Hypothesis 5 that principals are more willing to concede a higher payoff for the agent in treatment *Low*.

Result 7 While principals pay higher wages in treatment *Low* (supporting hypothesis 5), they are not willing to concede more to the agent (contradicting it).

6.4. Regime Change

In regulatory practice, if the regulator is convinced that another regime is preferable over the current, introducing the purportedly better rule requires regime change. Individuals who have gained experience with the previous regime are exposed to new rules. We have shown above that, for inexperienced subjects, honoring non-compete clauses does

[14] Furthermore, it is significantly smaller in *Low* (Wald test: *Low*+*High*=0, $p < 0.0001$).

[15] In model (2) the base rate of acceptance is equal in the two treatments (Wald test: *Low*+*High*=0 $p = 0.9848$).

[16] Joint hypothesis test (Wald-test) model (2): demand+*Low*:demand=0 $p < 0.001$; demand+*High*:demand=0 $p < 0.001$; *Low*:demand + *High*:demand=0 $p = 0.195$.

not have significant effects on effort. Principals and agents unequivocally benefit. It depends on the size of the statutory compensation whether principals or agents make more profit.

So, do choices and outcomes change when changing the regime? As Figure 5 shows, treatment effects on effort are pronounced. Descriptively, introducing non-compete clauses has a clear positive effect on effort if compensation is *High*. This impression is already supported non-parametrically (signrank test, N = 8, p = .0251). Interestingly, parametrically we also find that effort significantly increases, in comparison to a regime with *Low* compensation, if non-compete clauses are no longer enforced (linear mixed effects, N = 1024, coef 354.440, p = .033).

Result 8 Introducing the enforcement of non-compete clauses increases effort if the compensation paid to agents is high. Abolishing the enforcement increases effort significantly if compensation was low.

Introducing non-compete clauses is less beneficial for the joint profit of both parties than one might have expected, given the clear positive effect on effort. Non-parametrically, we do not find a significant effect. Even parametrically, we only find a positive effect if compensation is *High*, and it is only weakly significant (linear mixed effects, N=1024, coef. 824.635, p = .070). By contrast, abolishing a non-compete regime clearly reduces joint profit, irrespective of the size of the compensation (linear mixed effects, N = 1024, *Low* 1381.783, p = .002; *High* 1021.025, p = 0.022).

Result 9 Abolishing the enforcement of non-compete clauses reduces the joint profit of principals and agents, irrespective of the size of compensation.

This result is due to the substantial number of disagreements in the *Baseline*.

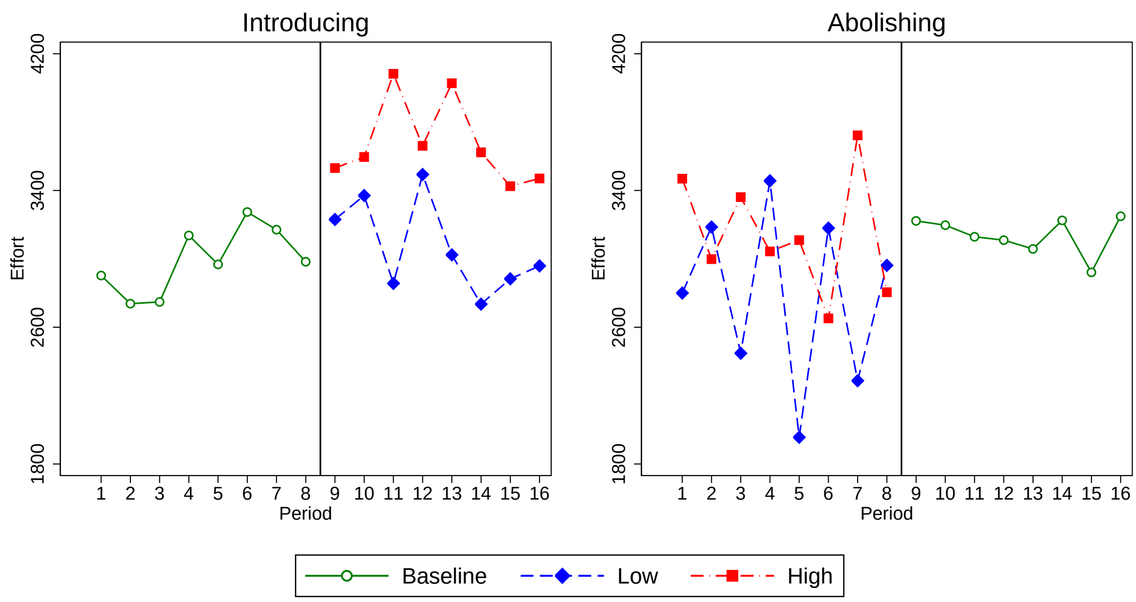

Figure 5: Effort, over time

7. Conclusion

A growing literature notwithstanding, the effects of non-compete clauses in labor contracts are still imperfectly understood. To provide additional evidence, in this paper we reported first experimental results from introducing non-compete clauses to a stylized principal-agent model with probabilistic returns to non-contractible effort. Our study was motivated by the question how non-compete clauses affect employee effort. As a non-compete clause prevents employees from starting a spin-off firm based on successful innovation, adverse effects on employee effort are to be expected. However, our experimental results do not suggest that such adverse effects are a substantial concern. We compared two treatments, one where statuary compensation for implementing a non-compete clause is *High* and one where it is *Low*, to a *Baseline* scenario without such a clause. Parameters were chosen such that, with commonly known opportunistic preferences, in the *High* treatment the principal would be indifferent between having or not having the right to exercise the clause. In the *Low* treatment, ex post the non-compete clause is very attractive for the principal. Contrary to theoretical predictions, effort did not differ significantly in all three settings, not even in the *Low* treatment, for which standard economic theory predicts a dramatic reduction of effort. In the experiment, inexperienced principals prevented this outcome by voluntarily paying a substantial wage to insure their agent against the risk of project failure, and to induce higher effort. Agents in turn reciprocated higher wages with more effort. In other words, employer and employee managed to overcome the lack of incentives inherent to the *Low* compensation by means of reciprocity in the spirit of gift exchange (see Fehr et al. 1993 for experimental evidence). Newly introducing non-compete clauses did not reduce employee effort, even if statutory compensation is low.

Our findings accordingly indicate that unless the statuary compensation is extremely low, non-compete clauses are unlikely to have a substantially adverse effect on employee effort. Even in the stylized experimental setting, both sides of the labor market found alternative ways to create incentives. This result is likely to be even stronger in the real world where, unlike our simplified interaction, other contractual agreements can be used to enhance agent effort.

Although competition reduces the joint profit of (former) employer and employee, in the experiment spin-off entrepreneurship results from failed renegotiation after successful innovations. Many agents do not attempt to extract as much of the innovation surplus as standard economic theory would suggest. Nonetheless a substantial share of their demands is rejected, with high demands being more likely to fail. This breakdown of cooperation in the stylized setting of the experiment resonates with empirical evidence suggesting that spin-offs are often induced by strategic disagreements between employers and employees, including disagreement about compensation schemes (Klepper, 2010; Thompson and Chen, 2011).

It is in the nature of lab experiments to abstract from many elements characterizing real-life interaction between employers and employees. The multi-dimensional nature of employee behavior encompassing, e.g., decisions about investing in employer-specific human capital as well reactions to variations in compensation schemes, is reduced to a one-dimensional effort choice to keep the experimental setup sufficiently simple for the participants. In order not to confound the effect of the non-compete clause itself with effects resulting from the endogenous choice to include the clause in the employment contract or to invoke it if the employee plans to leave the company, in the experiment we impose the clause exogenously. As a consequence, our design brackets the question whether waiving the clause despite its legality creates trust. Our design also excludes direct investments by employers: in the experiment only agents invest into the success of the project. The only option for the principal is to pay a high wage to induce effort. If agents are sensitive to reciprocity, a substantial investment by the principal in the relationship might give agents an additional reason to exert higher effort. Yet note that, as our experiment shows, reciprocity in investment is not a condition for agents to become active. In addition, interaction in the experiment is anonymous and ad hoc, and participants are rematched every period, which renders our setup a worst-case scenario for the formation of mutual trust and reputation, e.g. of being intrinsically motivated. Employees are merely assigned to their employer. There is no labor market and hence no potentially adverse effect of non-compete clauses on labor mobility and embodied knowledge transfer (Gilson, 1999). Effort is not laborious, but simply a costly investment. If the project is successful, this simply means that total profit is higher. There is no creation of knowledge, and no personal attachment of workers to the knowledge they have created. Nor are fundamentally new products developed, which might lead to disagreements about product strategy, and possibly to welfare losses if the new product cannot be marketed through an entrepreneurial spin-off but is shelved (Klepper and

Thompson, 2010).

In future work, at least some of these aspects might be explored via additional treatments. Yet we believe that our experiment still captures the most essential aspects of non-compete clauses in the context of spin-off entrepreneurship. While we did not find adverse effects of non-compete clauses on employee effort, their enforcement substantially reduced the intensity of competition even in the stylized experiment. Such a reduction in competition may be all the more problematic in the real world where competition tends to increase product variety and to spurn innovation efforts. Our experimental study may thus help policy makers in their quest to design institutions conducive to high-tech entrepreneurship, which in turns holds the promise of innovation, job creation and growth as epitomized by the example of Silicon Valley.

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A. Translation of Instructions

The following is a translation of the German instructions for the sessions in which participants first faced the baseline. Whenever the instructions in the other sessions differed, we indicate this by putting the differing text in parentheses and preceding it by "**N-B**:".

Instructions

Welcome! Please stop any communication with other participants and switch off your mobile phone. In the following experiment you can earn money, which is why you should read these instructions very carefully. If anything is unclear, please raise your hand and wait for a supervisor to come to your cubicle. The instructions are identical for all participants. You will remain anonymous throughout this experiment. This means that no participant will be informed about your identity.

There are two different roles. Half of the participants decide in role A, the other half in role B. Roles are randomly assigned at the beginning and you keep your role throughout the entire experiment. In total, the experiment consists of two sections. Each section in turn consists of eight rounds. For each section, you will receive new instructions. How you behave in a round or section has no effect whatsoever on the proceedings in any of the following rounds or sections.

During the experiment, all sums of money will be denominated in *Taler.* You can earn money in every round - but it is also possible to lose money. How much money you earn depends on your decisions, on the decisions made by the participant assigned to you, and on random draws. However, in each section only the money earned in one single round is paid to you: At the end of the experiment a random draw determines which round per section is relevant for the calculation of the payoffs of all participants (there is one independent draw per section). For every participant, the total payoff is then the sum of amounts reached in these two rounds. The payoff you earn in *Taler* is converted into €at the end of the experiment and paid to you in cash at the exchange rate of 1000 Taler = €1.40.

For showing up on time you receive an additional payment of €2.50. At the beginning of each section you will furthermore be asked to answer a control questionnaire. For answering these correctly you receive an additional €2.00 each.

Should you incur losses during the experiment, these will be subtracted from your other payments. If your losses can not be balance against your fixed payments, you have two options: Either you work the remaining difference off (counting the frequency of a letter in a text), or you pay it out of your own expenses.

Part 1

In each of the following eight rounds, you will interact with another participant in the other role who will be randomly assigned to you. In this section you will interact with no one more than once.

In each round, participants A and B successively make the following decisions.

1. A determines a fixed payment f for B, which can lie anywhere between 0 and 10,000. B receives this fixed payment for sure, regardless of how the round progresses.

2. B is informed about the size of payment f and determines how much to invest in a project which can earn a high profit. This investment e, which can lie anywhere between 1 and 6,000, costs B exactly the amount e and is subtracted from B's payoff.

3. Investment e determines the probability of the project being successful. With probability $w = \sqrt{e}/100$ it will be successful; with residual probability $1 - w = 1 - \sqrt{e}/100$ it will not be successful: The more B invests, the more likely will the project be successful (see Figure 1). Only B knows how much he invested. Participant A is never informed about it.

A random draw therefore determines with probability w, whether the project is successful.

- If the project is not successful, the round ends and
  A receives: $4{,}000 - f$
  B receives: $f - e$

- If the project is successful, B has the opportunity to demand a further payment v from A.

  a) B determines his demand v (any amount between 0 and 18,000).

  b) A is told how much B is demanding and decides whether to accept or reject. The round ends with the following payments:

    * If participant A accepts participant B's demand v, the round ends and A receives: $18{,}000 - f - v$ B receives: $f + v - e$

    * If participant A rejects participant B's demand v, the total payoff from the successful project is reduced, and the round ends and
      A receives: $6{,}000 - f$ B receives: $6{,}000 + f - e$

(**N-B**: *If participant A rejects participant B's demand v, the round ends and*

A receives: $18{,}000 - K - f$

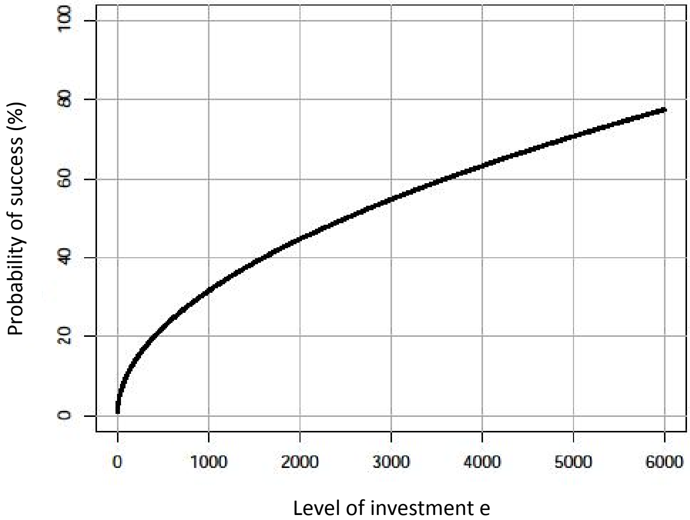
B receives: $K + f - e$)

(**N-B**: *The sum K is determined at the beginning of a round, and both participants (A and B) are informed about it. This sum can be either 7,000 or 12,000 Taler.*)

The diagram in Figure 2 illustrates how a round proceeds.

You will now receive a questionnaire with control questions which shall ensure that all participants have understood the instructions. Should you have any questions you may contact a supervisor at any time. You will receive €2.00 for answering the questionnaire.

Figure 1: Probability w



Part 2

In each of the following eight rounds, you will interact with another participant in the other role, who will be randomly assigned to you. In this section you will interact with no one more than once.

In each round, both participants make the same decisions as in the first part. The only difference is that the payoffs in case of success and in case of A rejecting demand v change:

Thus, as before, if the project is successful, B can demand a further payment v from A:

a) B determines his demand v (any sum between 0 and 18,000).

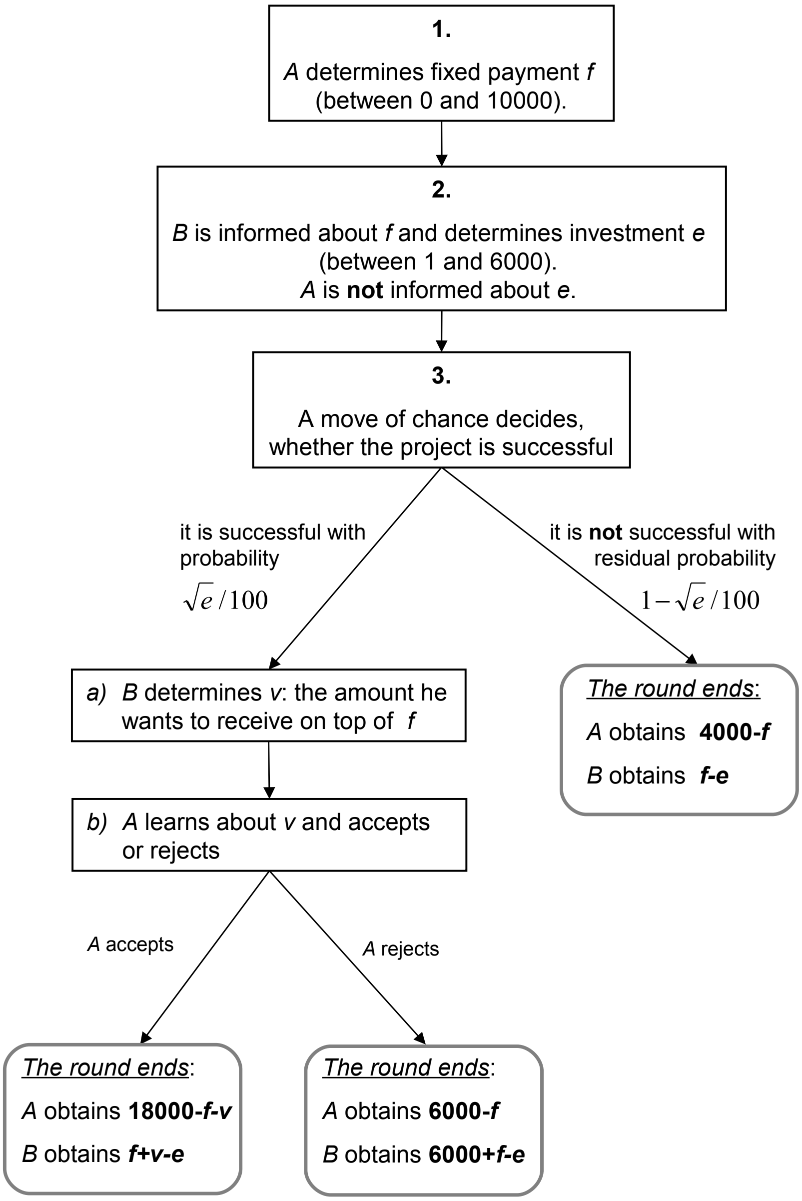
b) A is told how much B is demanding and decides whether to accept or reject. The round ends with the following payments:

   - If participant A accepts participant B's demand v, the round ends and
     A receives: $18{,}000 - f - v$
     B receives: $f + v - e$
   - If participant A rejects participant B's demand v, then
     A receives: $18{,}000 - K - f$
     B receives: $K + f - e$

   (**N-B**: *If participant A rejects participant B's demand v, the total payoff from the successful project is reduced, and the round ends and*
   A receives: 6.000-f
   B receives: 6.000+f-e.)

The sum K is determined at the beginning of a round, and both participants (A and B) are informed about it. This sum can be either 7,000 or 12,000 Taler. (**N-B**: *paragraph deleted.*)

Figure 2: Timeline of a round

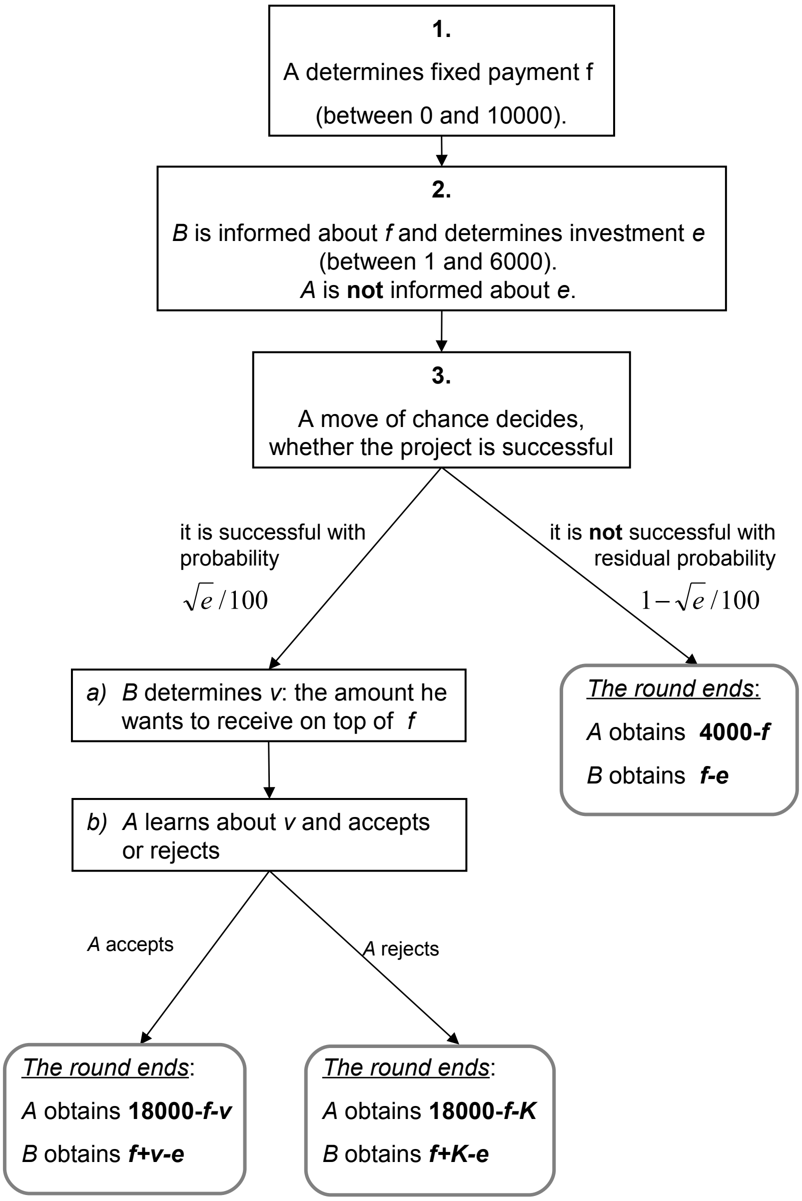


(**N-B**: *see Figure 3 below.*)

The diagram in Figure 2 illustrates how a round proceeds.

You will now receive a questionnaire with control questions which shall ensure that all participants have understood the instructions. Should you have any questions you may contact a supervisor at any time. You will receive €2.00 for answering the questionnaire.

Figure 3: Timeline of a round



(**N-B**: *see Figure 2.*)