# Peer Effects and Entrepreneurship[•]

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## ABSTRACT

*Several recent studies in entrepreneurship have shown that the propensity for individuals to become entrepreneurs seems to vary considerably by the type of firm for which they work. Many of these studies have hypothesized that this may result from 'knowledge spillovers' within the firm or due to 'entrepreneurial exposure' to employees' colleagues. In this paper, we directly test the hypothesis that an individual's colleagues might impact their transition to entrepreneurship, by looking at how diversity in the prior career histories of an individual's peers relates to their own propensity to become an entrepreneur. We find that employees are more likely to become entrepreneurs if their co-workers have had prior self-employment experience or have had more diverse work experience in the past. Our findings suggest the presence of both 'knowledge-spillovers' and 'entrepreneurial exposure' as important mechanisms contributing to firm-level heterogeneity in entrepreneurial spawning, and highlight the importance of studying the social context of firms when looking at labor market outcomes.*


## 1 INTRODUCTION

In what ways do an individual's colleagues influence their own decision to become an entrepreneur? Although a well-established literature has documented the importance of both personal characteristics and institutional factors (such as credit constraints) in determining an individual's entry into entrepreneurship, remarkably little attention has been devoted to understanding the role that a person's work-environment might play in shaping entrepreneurial decisions. Understanding the ways in which the work environment shapes entrepreneurial outcomes is particularly important because the vast majority of start-ups are founded by individuals who were previously employed at another firm. Estimates from Burton, Sorensen and Beckman (2002) and Gompers, Lerner and Scharfstein (2005) suggest that over 90% of founding team members are employed at an established firm prior to founding their start-up. Given the significant

time spent by employees at the workplace, in close proximity with their colleagues, one might expect that the work-environment, and in particular their peers, would be an important factor influencing individuals' career decisions, including transitions to entrepreneurship.

Studying whether and how peers affect individual behavior has a long and rich history in the social sciences. For example, an extensive literature on neighborhoods has shown the profound effects that neighborhood peers can have on individuals (Mayer and Jencks (1989), Katz, Kling and Liebman (2001)). Related literatures on education and student outcomes have shown that peer quality and behavior are among the most important determinants of student outcomes (e.g., Sacerdote 2001), and that peers have a measurable impact on employee shirking and productivity (Ichino and Maggi 2000). Far less attention, however, has been devoted to the role that peers in the workplace might play in impacting employees' career choices and decisions such as entry into entrepreneurship.

Despite the paucity of empirical research, the role of peers in shaping entrepreneurial behavior has emerged as an important theme in recent work. Several recent papers have argued that the types of firms individuals work for shapes the likelihood of entry into entrepreneurship (Saxenian 2000, Gompers, Lerner and Scharfstein 2005, Sorensen 2005, Klepper 2001). Several of these papers allude to the possibility that the influence of firms may in part be due to peer effects. For example, Gompers, Lerner and Scharfstein (2005) argue that exposure to colleagues who have themselves been involved in startups is a specific type of peer effect that may play an important role in understanding why certain firms "spawn" more entrepreneurs than others. They examine the formation of venture capital-backed startups to see whether these firms tend to arise from large bureaucratic organizations or from young firms with an 'entrepreneurial' work-environment, where employees are exposed to new ideas and start-up experience through association with their colleagues. They find that although the startups in their sample are founded by entrepreneurs leaving both 'types' of organizations, they are more likely to arise from the younger, more 'entrepreneurial' firms (see also Sørensen 2005). They

argue that this suggests an important role for an employee's peers in determining transitions to entrepreneurship.

Similarly, a growing literature on regional differences in entrepreneurship rates has invoked peer effect mechanisms. Most prominently, Saxenian ((1994, 2000)) has hypothesized that the higher levels of entrepreneurship in regions such as Silicon Valley may be driven in part by the spillovers of new knowledge, entrepreneurial opportunities, and exposure to running new businesses that arise from exposure to workplace peers with diverse work experiences. She argues that the success of Silicon Valley in relation to Route 128 over the 1980s, was driven at least in part by the organizational structure, and work-environment of the firms in the respective regions. The firms in Silicon Valley were comprised of individuals who frequently moved between jobs, alternating between spells of entrepreneurship and employment, themselves gaining new knowledge and at the same time exposing their colleagues to new ideas, networks, and startup experience. These 'knowledge spillovers' or peer effects in Silicon Valley were, according to Saxenian, a key factor differentiating the firms in Silicon Valley from the large bureaucratic organizations in Route 128 that by virtue of their structure, did not encourage the mobility of employees between jobs, and hence did not allow for the types of 'spillovers' operating in Silicon Valley. Building on Saxenian's work, Fallick, Fleischman and Rebitzer (2003) look at the relationship between labor mobility and entrepreneurship at the regional level and find that Silicon Valley had both higher levels of inter-firm mobility and higher rates of entrepreneurship than other regions in the US. They argue that the higher rates of entrepreneurship in Silicon Valley are due at least in part to the greater knowledge spillovers in that region – arising from the greater inter-firm mobility of the employees.

The results of these papers imply that employees are more likely to become entrepreneurs after being exposed to certain types of peers, *even* when the employee was not necessarily entrepreneurially inclined in the first place. This notion put forward in these papers -- that 'exposure 'and 'spillovers' in young entrepreneurial firms has an impact on rates of entrepreneurship -- has broad intuitive appeal and is also supported by

numerous anecdotes[1]. The notion that peers may transmit entrepreneurially relevant knowledge is supported by research that has used patent citations to study the geographic localization of knowledge spillovers and argued that the mobility of skilled workers such as scientists, provides a medium by which economic knowledge is transmitted within a region (Zucker, Darby et al 1998; Almeida and Kogut 1999; Agarwal, McHale et al 2003). Similarly, the idea that peers with prior entrepreneurial experience may exert an influence exposing individuals to entrepreneurship is supported by research suggesting that entrepreneurial role models play an important role in influencing entrepreneurial entry (Sørensen 2006)

Yet prior research invoking peer effect arguments is largely speculative. Their inferences are based on *proxies* of peer effects such as firm age, firm size or patenting record, but is not able to directly test the relationship between diversity in an individuals' peer group, and their own entry into entrepreneurship. Directly testing for the association between an individuals' peers and their own propensity to become an entrepreneur is important for several reasons.

First, a big concern with prior work is that the effects they find may be due to omitted variables at the firm level. Young, venture capital backed, 'entrepreneurial' firms may 'spawn' more entrepreneurs (Gompers, Lerner and Scharfstein 2005), but this may be for many reasons that are unrelated to the effects of peers. For example, start-ups also give their employees operational experience in a range of different job functions – a factor that has been associated with increased rates of entrepreneurship (Lazear, 2002).

[1] The classic such example is that of Fairchild Semiconductor, epitomized in Gompers, Lerner and Scharfstein (2005) as "the Fairchild view of entrepreneurial spawning". Fairchild was founded in the late 1950s by 8 engineers who left Shockley Semiconductor to start their own firm. While Fairchild was a successful firm it its own right, at least 23 of the 67 firms that entered the semiconductor industry in the subsequent 20 years were founded by entrepreneurs who were previously employed at Fairchild (and were humorously referred to as "Fairchildren"). What is important about this anecdote is that Fairchild is believed to have spawned the number of firms it did because of the entrepreneurial environment that it created for its employees , much of which was believed to have come from the employees' peers.

Directly testing for an association between the prior labor market and entrepreneurial experiences of an individual's peers and their own entry into entrepreneurship will help to allay such a concern.

Secondly, directly looking at peer effects helps to understand whether this association is in fact genuine, or whether it is driven by selection at the individual, or firm level. This is not only of academic interest, but a key question of interest to policy makers. Consider the following three cases: In the first case, that of genuine peer effects, an employee exposed to peers who have had prior entrepreneurship experience develops important leads, gains key insights into recognizing opportunities and running a new business and thus is more likely to become an entrepreneur after being exposed to his peers than before the exposure. He in turn will expose others to these opportunities, leads and insights, thus creating a virtuous cycle of 'spillovers' or 'peer effects'.

In the second case, that of selection at the firm level, several individuals who are entrepreneurially inclined are all attracted to certain types of firms because of some firm attribute (e.g. firm culture, or a firm policy where the firm promotes entrepreneurship among its employees), but these individuals do not benefit from exposure to each other. If any of these individuals was to found a new firm, it would appear as if their peers had an effect on their decision to become an entrepreneur because of the type of peers that selected into the firm, when in fact the effect is driven purely by selection at the firm level.

In the third case, that of selection at the individual level, an individual who wants to become an entrepreneur goes to work for certain firms precisely because his peers might be able to help him gain the exposure and the insights into becoming a new entrepreneur. However, it is only the individuals who proactively seek out the benefits from their peers and select into specific firms who gain from peer effects. In this case too, if the individual in question were to become an entrepreneur, it would appear no different from the case of genuine peer effects, yet the mechanism is fundamentally different and is driven by selection at the individual level.

Policy makers are only interested in the first of these three cases – one in which they can alter a policy variable and hence directly spur entrepreneurship. In the second and third cases, there is no 'multiplier effect' from a policy reform. That is, individuals who were entrepreneurially inclined still become entrepreneurs, but those who were not are no more likely than before to become entrepreneurs. An example of this can be seen in the conclusions of Gompers, Lerner and Scharfstein (2005), where they write:

> "Our findings [...] suggest that entrepreneurial activity in a region has increasing returns[2] (Saxenian (1994))…Policies that have sought to foster entrepreneurial and venture capital activity by providing capital or investment incentives may not be enough. Instead regions may need to attract firms with existing pools of workers who have training and conditioning to become entrepreneurs."

The view that entrepreneurial activity in a region has increasing returns is based on the premise that peer effects are truly present within firms. The absence of such peer effects would mean that in fact, there may be no real *policy* benefit from attracting firms that have workers who are conditioned to become entrepreneurs. It would also shed light on the extent to which entry into entrepreneurship is in fact conditioned by environmental factors such as the workplace or broader institutional environment, versus being driven by inherent individual traits such as an appetite for risk or an 'entrepreneurial personality'.

Finally, if indeed peers genuinely impact an individual's entrepreneurial outcome, looking at the association between peer effects and entrepreneurship directly helps to understand the mechanism through which this association is being driven. We follow the prior literature in distinguishing between two distinct types of peer effects. The first type of peer effect, which we call 'exposure', stems from having "role models" in one's workplace. Working alongside other employees who have been entrepreneurs before helps an individual to learn critical business skills, and develop the confidence to start out

[2] Increasing returns implies that the benefit from exposing one person to a 'treatment' goes beyond that one person, so that the treatment is transmitted to others without a cost. This mechanism is called a 'spillover' and leads to increasing returns.

on their own. We distinguish between this type of peer effect and a second type that we call 'knowledge spillovers'. These stem from having learnt about specific business opportunities through discussions with one's peers. For example, in Saxenian's description of 'knowledge spillovers' in Silicon Valley, she writes that:

> These networks defy sectoral barriers: *individuals move easily from semiconductor to disk drive firms or from computer to network makers. They move from established firms to start-ups (or vice versa)*… And they continue to meet at trade shows, industry conferences, and the scores of seminars, talks and social activities organized by local business organizations and trade associations. In these forums, *relationships are easily formed and maintained, technical and market information is exchanged, business contacts are established, and new enterprises are conceived. This decentralized and fluid environment also promotes the diffusion of intangible technological capabilities and understandings*." (Saxenian 1990, pg 96-97; emphasis added)

The specific mechanism through which peer effects might drive entrepreneurial outcomes is important in that they help to untangle the types of spillovers that seem most relevant for entrepreneurial spawning. From a policy-maker's perspective it is critical to know whether it is "enough" to promote inter-firm labor mobility in a region, or whether it is also important to encourage movement between entrepreneurship and employment.

The fact that there are such few quantitative studies directly examining the relationship between peer effects and entrepreneurship is due in large part to the difficulty in gathering comprehensive data for such a study. Directly establishing a relationship between the attributes of an employee's peers and their own transition into entrepreneurship requires a dataset that can not only track individuals over time before some of them become entrepreneurs, but also gather information on co-workers of each individual in every firm they work in. Since transitions to entrepreneurship are a relatively rare event, the set of individual (and their peers) needed to be studied in order to run a robust empirical analysis would be massive. Collecting such data systematically has therefore been extremely hard.

We look directly at the association between variation in an employees' peer-group and their own propensity to become an entrepreneur, using a unique matched employer-employee panel dataset from Denmark. Our dataset has annual observations on all (legally) resident individuals in Denmark, allowing us to track individuals as they move between spells of employment, unemployment or self-employment for each of the seventeen years from 1980-1997. In addition, since we are able to match individuals to firms (and workplaces), we also know who a given individual's colleagues are, and what *their* prior career experiences have been. This allows us to therefore address the posited relationship between peer-effects and entrepreneurship directly, as well as to look in more detail at the specific mechanisms that might be at play.

We find that employees are more likely to become entrepreneurs if their co-workers have had prior self-employment experience or have had more diverse work experience in the past (measured in terms of inter-firm mobility). We perform a number of robustness checks for our results, in particular delving deeper into issue of selection at the individual and the firm level. Our results suggest that the presence of both 'knowledge-spillovers' and 'entrepreneurial exposure' are related to entrepreneurial decisions, and that they are important mechanisms contributing to firm-level heterogeneity in 'spawning' new firms.

The rest of the paper is structured as follows: In Section 2, we outline the data and our measures of entrepreneurship and peer effects. In Section 3, we outline our estimation procedure present our results. Finally, we provide a discussion of these results, and conclusions in Section 4.

# 2 DATA AND EMPIRICAL APPROACH

## *2.1 Description of Data*

This paper is based on data from the Integrated Database for Labor Market Research in Denmark, which is maintained by the Danish Government and is referred to by its Danish acronym, IDA. IDA has a number of features that makes it very attractive for this study. First, it is comprehensive: all people (legally) residing in Denmark in a given year are included in the registers. Individual characteristics are recorded in IDA on an annual basis, which means that IDA amounts to an annual census of the population of Denmark. The data is collected from government registers, which makes the quality of the dataset superior to those that are based on self reported surveys. Second, IDA is longitudinal, with annual observations starting in 1980. This makes it possible to collect panel data for individuals. The data for this study run from 1980-1997. Third, IDA covers a wide range of phenomena with respect to labor market status, so that it tracks the firm, industry, and region that an individual works in, and also tracks individuals as they move between jobs, and their transitions into and out of self employment (on an annual basis). Fourth, the design of IDA allows individuals to be linked according to a variety of relevant characteristics. For example, employees can be linked to their employers, a fact that allows one to study which other employees an individual came in contact with during their tenure with a given firm. Finally, the database also allows one to link an individual's labor market data with a range of other characteristics that serve as important controls in studies of entrepreneurship (such as their age, educational qualifications, annual income, wealth, marital status, number of children)[3].

Although this dataset is extremely comprehensive and has a number of features that make it ideal to address the question at hand, there may be some concerns about the external validity of dataset that is based on information from a relatively small country such as Denmark. Tables 1, 2 and 3 address these concerns by looking at how the rates of

[3] Since such detailed data raises issues about confidentiality, researchers are not allowed unfettered access to IDA, but must instead request particular extracts from the larger database. The analyses for this paper come from an extract that was created by identifying all individuals who were living in Denmark in 1994 and were between the ages of 15 and 74. This is a population of 3.9 million individuals. For all of these individuals, relevant information from IDA was collected for each year from 1980 until 1997. It should be noted that this design means that the extract only covers the entire population in 1994. In other years, the extract does not capture people who were not in the population in 1994, for example because they died or emigrated before 1994. This type of attrition is likely more serious for older cohorts, and so does not pose particular concerns for the current analyses.

entrepreneurship, the labor market characteristics and rates of entry across a number of industries compare to the US, and where available other European countries.

Table 1 is taken from Blanchflower's (2000) comparison of self employment in OECD countries. While self employment is not the only measure of entrepreneurship, and might differ considerably from the high-tech entrepreneurs in Silicon Valley who are shows that the self employment rate in Denmark is almost identical to that in the US and in Germany, and somewhat lower than in the UK.

Table 2 shows that Denmark has similar labor force participation as the US, UK and Germany. Although it has a somewhat higher unemployment rate and a higher fraction of its population in long term unemployment than the US, it is not atypical for a European country as can be seen in comparison to Germany and the UK. While the unemployment rate measures the stock of unemployment, a study by Albaek and Sorensen (1998) looks at the turnover of workers and jobs in Danish Manufacturing industries over the period 1980-1991. They find that the rate of job destruction in Denmark was almost identical to that in US manufacturing industries over a comparable period (11.5% vs. 11.1%), but that rate of job creation in Denmark was somewhat higher than in the US (12% as opposed to 8.4%). This was because there was a marked decline in US manufacturing over the period in question while this was not the case in Denmark.

Table 3 shows a comparison of entry rates of new firms across a range of manufacturing industries between the US and Denmark (US results taken from Dunn, Roberts and Samuelson, 1988). It shows that the entry rates are somewhat lower in Denmark than the US, but that the relative rates of entry between the different industries in similar in the two countries. These comparisons, taken together, provide confidence that the characteristics of Denmark's labor market and industry structure are sufficiently similar to the US and other large European countries to warrant comparison for studies of entrepreneurship, and that the results from this data may be applicable to contexts that go beyond Denmark.

As discussed below, we restrict the sample for the multivariate analyses to individuals who were between the ages of 16 and 40 in 1990; this age group is less likely to have suffered much non-random attrition (see footnote 3 above). However, the attrition from the sample does mean that there is some downward measurement bias associated with measures like a firm's number of employees as one moves away from 1994.

## *2.2 Construction of Sample*

The construction of the sample for analysis was guided by two major principles. First, since the dynamics of serial entrepreneurship are likely different from the initial transition into entrepreneurship, we excluded individuals with a prior history of entrepreneurial activity from the risk set. Second, the transition to entrepreneurship is a form of job turnover that depends on duration in the job. This suggests that employees should be observed from when they first become at risk of leaving a particular employer for entrepreneurship, in order to avoid the biases introduced by left censoring. These principles led us to impose a set of restrictions on the IDA data. Specifically, the sample is limited to those individuals who a) were employed in 1990; b) were newly hired by their employer in 1990 (i.e., zero firm tenure); c) had no prior self-employment experience between 1980 and 1990; d) were between the ages of 16 and 40 in 1990; e) were not employed in the primary sector (agriculture and extractive industries) or in industries dominated by the public sector; and f) whose employer was not a new employer in 1990. The decision to focus on people employed in 1990 reflected an attempt to balance the tradeoffs created by the left-censoring of all IDA data in 1980 and the right-censoring of observations in 1997. The left-censoring of IDA data in 1980 means that any prior labor force history is unknown for people in the labor market in 1980. By focusing on individuals in the labor force in 1990, we employ the IDA data from 1980 and 1990 to identify and exclude individuals with any self-employment history between 1980 and 1990. In combination with the age restriction, this should capture the vast majority of people with prior entrepreneurial experience. Similarly, the restriction to individuals newly hired in 1990 ensures that individuals are followed from

when they first become at risk of leaving their employer to enter entrepreneurship. We exclude individuals in the primary sector and in industries dominated by the public sector because the dynamics of entrepreneurial activity may be substantially different in these sectors. Finally, we exclude employees of newly founded firms because these individuals may be entrepreneurs. There are 282,911 individuals in the estimation sample.

## *2.3 Definition of entrepreneurship*

We rely on two types of data to measure transitions to entrepreneurship. The first source is the occupational classification scheme employed by Statistics Denmark. This classification scheme differentiates between a wide variety of labor force attachments, including employment with established firms (sub-divided into seven broad, hierarchical categories), unemployment, schooling, not in the labor force, and self-employment. Statistics Denmark employs two primary categories for self-employment. The first captures individuals who are unincorporated proprietors with employees; the second captures self-employed individuals with no employees. We treat entry into either of these two categories as transitions to entrepreneurship.

A shortcoming of relying on the occupational data is that it does not capture people who found incorporated ventures. Unfortunately, due to limitations in the government registers upon which IDA is based, the founders of incorporated ventures cannot be identified and linked to the other registers which provide the primary labor market data. This means that such transitions cannot be measured directly. Instead, we take advantage of the fact that incorporated ventures appear as new employers, with the founders of the incorporated ventures as employees. We therefore code individuals who are employees of newly founded firms as entrepreneurs, in addition to people who enter self-employment. Most of the new employers identified in the dataset are quite small, but there are a small number of large firms. It seems unreasonable to assume that all of the initial employees of a new firm with a large number of workers are entrepreneurs. We

assume that all employees of new firms with three or fewer employees are the founders of the new firm. For firms with more than three employees, we only consider individuals to be entrepreneurs if their occupational titles indicate that they are directors or top managers. In the analyses presented below, we pool all three of these types of entrepreneurial entry into a single transition.

Entry into entrepreneurship can be a response to poor employment prospects as well as a reaction to the presence of entrepreneurial opportunities. In order to limit the extent to which the observed transitions might be due to such push factors of various types, we treat transitions to entrepreneurship as censored if the individuals in question experienced a period of unemployment between their observed employment in one year and their subsequent self-employment the next year. Similarly, we censor transitions to entrepreneurship that occur simultaneously with the failure of the individual's employer.

## *2.4 Measures of Peer Effects*

For each of the focal individuals in the sample, we calculate peer-effect measures for each year between 1990 and 1997 (given that they are employed in that year). As discussed in Section 1 above, we follow the literature in distinguishing between two different types of peer effects for our analysis – 'entrepreneurial exposure', and 'knowledge spillovers'. In order to compute 'entrepreneurial exposure', we first identify every workplace that the focal individuals worked in for every year over the period 1990-1997. We then identify every other employee in that workplace in each year and calculate the number of years each of these colleagues had been entrepreneurs in the past five years. Our measure of "entrepreneurial exposure" thus changes in each year for every individual, and measures the average number of years (of the previous 5 years) the focal individual's peers were entrepreneurs. The maximum value this variable could take, therefore is five, and the minimum is zero

In order to create a measure of 'knowledge spillovers' we look at inter-firm labor mobility as a proxy of knowledge accumulation. A number of studies have noted that individuals exposed to a wide range of jobs and job functions accumulate diverse

industry knowledge and the ability to recognize entrepreneurial opportunities (Lazear 2002, Shane 2000) and have related this inter-firm labor mobility to the presence of knowledge spillovers. This mechanism has been invoked both in the entrepreneurship literature (Saxenian 1994, 2000, Fallick, Fleischman, Rebitzer 2003) and in the literature on patent citations (Almeida and Kogut 1999; Agarwal, McHale et al 2004). In order to compute a measure of the knowledge spillovers the focal individual is exposed to, we look at the average number of different firms their colleagues worked in over the prior 5 years. As with the other peer effects measure, the maximum value of this variable can take is five (if every colleague changed jobs in every year) and the minimum is zero.

# 3 RESULTS

We employ a discrete-time hazard rate modeling framework by estimating logit models where the dependent variable takes a value of 1 if the focal becomes an entrepreneur and is zero otherwise. The dependent variable is regressed on measures of peer effects outlined above, and a range of controls, including duration in job, the individual's own prior job mobility (note that their prior entrepreneurship is zero by construction of the sample), their prior labor force experience, their wealth, demographic characteristics and several observables of the firm. All models include industry fixed effects, to control for the possibility that certain industries may systematically differ in the nature of their 'peer- dynamics' and rates of entrepreneurship.

The first three models in Table 4 look at the presence of peer effects for the entire sample. In model 1, we look at the relationship between entrepreneurial exposure and transition to entrepreneurship, and in model 2 at the relationship between 'knowledge spillovers' and entrepreneurship. As can be seen from table 4, both types of peer-effects seem to be associated with a higher probability of entering entrepreneurship. In model 3, we enter both peer-effects together. Since we enter the two variables together, this allows us to examine the relationship between each of the peer effects and entrepreneurship after partialling out the effect of the other. Interestingly, both coefficients are extremely stable,

suggesting that our proxies for the two different types of peer-effects are indeed capturing different mechanisms through which individuals may benefit from peer-effects.

One potential criticism of our results is that the measure of peer-effects does not capture the "true" exposure that an individual has to their colleagues in large firms. In order to address this, we look at whether the results are affected by constraining the sample to include only those individuals who work in firms with less than 25 employees. These results are reported in model 4 of Table 4. As can be seen from model 4, the coefficients are extremely stable and equally significant, suggesting that this should not be a concern.

In model 5, we constrain the sample to include only "knowledge-intensive" industries (in this case, pharmaceuticals, manufacture of radio equipment, manufacture of medical devices, and computer hardware and software industries). We might expect that the role of 'peer effects' might be much stronger in knowledge-intensive industries. Consistent with this view, the coefficients are much larger in this model than in the previous 4, providing some confidence that our peer-effects variables are capturing the concepts we are interested in measuring.[4]

The results from Table 4 suggest therefore that peer effects do appear to be an important mechanism impacting transitions to entrepreneurship. As discussed in Section 1 however, this basic specification is inadequate on several fronts. In particular, the estimates of peer effects may be biased by the presence of unobserved individual characteristics or unobserved firm characteristics related to the propensity to enter entrepreneurship.

A natural way to address the issue of an omitted variable at the firm-level is to use firm-level fixed effects. Firm fixed effects are attractive in that they help to identify whether the observed relationship between an employee's peers and their own transition to entrepreneurship might be driven by some unobserved firm-level characteristic (such

[4] A formal test of the significance of these differences in magnitude will be forthcoming.

as a policy promoting entrepreneurship, or firm culture). In addition, running firm fixed effects, while also controlling for the industry and year, is a particularly strong test for firm-level omitted variables, in that the effect of peers on the transition to entrepreneurship is identified off individuals employed in the same firm, but in different workplaces who become entrepreneurs in the same year. It is harder to believe that firm culture or policies vary systematically across workplaces in a way that also promotes entrepreneurship. The results for this analysis using firm fixed effects will be presented in Table 5[5]

An important limitation of using firm fixed effects is that it still does not preclude the results being driven by an omitted variable at the individual-level. For example, it is possible that individuals who have preference for working with certain types of colleagues (who have been entrepreneurs or who have been mobile between firms) are also more likely to become entrepreneurs themselves without the peers influencing their decision to become an entrepreneur at all. The most widespread method for addressing such bias due to individual unobserved heterogeneity is to include individual fixed effects in regression models. In the current context, a fixed-effects model would focus on the effect of within-career variation in the level of peer-effects on the individual's transition rate. As such, the fixed effects strategy asks whether individuals are more or less likely to enter entrepreneurship if they are working for a firm that has greater level of peer-effects than other firms during their career. Such a model can be estimated using conditional logistic regression as a fixed effects discrete time model (Allison and Christakis 2000). However, the analysis is limited to individuals who eventually entered into entrepreneurship because estimation of a fixed-effects model requires variation in the dependent variable within individuals; such variation is only present among individuals who transitioned to entrepreneurship.

While attractive, this analytic strategy has from a number of limitations. First, the nature of the conditional fixed-effects estimator in a hazard rate context limits the range of time-varying individual characteristics that can be controlled. In particular, the conditional

[5] Our robustness checks are still ongoing and we do not have completed results to present as of now

fixed effects estimator will lead to biased estimates of any variables that are correlated with time (Allison and Christakis 2000). This is a consequence of the fact that when studying a non-repeatable event, such as the first transition to entrepreneurship, the event necessarily occurs at the end of the observation period. Duration at risk is therefore a perfect predictor of the event, and any variable that is correlated with duration at risk will appear to be correlated with the hazard rate, even if the true correlation is zero. This fact rules out a wide range of variables plausibly related to the decision to enter entrepreneurship, including such factors as income and wealth, since they tend to increase with time.

Second, the sampling scheme necessitated by the conditional fixed effects estimator creates complications in estimating the effects of organizational characteristics. As noted earlier, individuals are only included in the sample if they transition to entrepreneurship at some time during the observation period. Because a large organization has more employees at risk of transitioning to entrepreneurship than a small organization, a transition to entrepreneurship from a large organization is more likely to be included in the sample than a transition from a small organization. In other words, transitions to entrepreneurship (and hence individual career histories) are sampled proportional to the size of the employing organization, even if organizational size is uncorrelated with the transition rate. This over-sampling of transitions from large firms will impart an upward bias to the estimates of the effects of organizational characteristics correlated with size. To account for this, we weight each individual's contribution to the likelihood function by the inverse probability of the organization's inclusion in the sample.

Third, within-person models rely on between-firm variation in levels of peer-effects. This between-firm variation may be correlated with other, unobserved firm characteristics that affect the rate of entrepreneurship (such as a particular corporate culture, or firm promotion policies). Unobserved organizational heterogeneity could therefore generate a spurious within-person correlation between peer effects and entrepreneurship. It is tempting to address this problem by including firm-level fixed effects in addition to individual fixed effects. However, firm fixed effects are not

identified when individual fixed effects are included and the event is non-repeatable, as is the case here. We therefore run a separate analysis with individual fixed effects but without firm fixed effects; these results will be presented in Table 6[6]

Finally, the use of a fixed-effects estimator only addresses the issue of *fixed* unobserved heterogeneity among individuals. It leaves open the possibility that people's preferences for entrepreneurship may vary in unobserved, time-varying ways that also impact the choice of employer prior to entrepreneurial entry. One might imagine, for example, that unobserved life events might increase an individual's entrepreneurial aspirations, and that as a result of these changes, the individual seeks out employment opportunities in more entrepreneurial settings. In order to address such "strategic" individual selection we examine the level of peer-effects for the focal individual in the firm prior to the one in which they are currently employed, and see whether there is evidence that is consistent with the idea that the pattern of movement between firms of peer-effects is different for individuals who enter entrepreneurship and those who don't. If nascent entrepreneurs strategically moved to smaller firms prior to entering entrepreneurship, one would expect to see that they were more likely than others to have joined a firm with 'high peer-effects' immediately prior to entrepreneurship. We assess the empirical support for this first by using log-linear models that cross-classify three variables: whether an individual's attachment to a firm ended with entry into entrepreneurship (0/1); the peer-effects present in the current employer; and the peer-effects present in the immediately prior employer. The results for this analysis will be presented in Table 7[7]

## 4 DISCUSSION AND CONCLUSIONS

Although the vast majority of start-ups are founded by individuals who have left established firms, the role that an individual's work environment plays in determining transitions to entrepreneurship has received little attention until recently. Two related

[6] Our robustness checks are still ongoing and we do not have completed results to present as of now
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streams of literature – one focused on regional variation in rates of entrepreneurship and another on understanding firm-level heterogeneity in entrepreneurial spawning – have hypothesized that variation in rates of entrepreneurship may be related to the workplace environment, and in particular due to 'peer-effects' in the workplace. However, these prior studies have been unable to directly relate peer effects to an individual's propensity to become an entrepreneur, and have therefore been unable to rule out the concern that these results may be driven by selection at the firm- or individual-level.

This study uses a unique matched employer-employee panel dataset with annual observations on all legal residents of Denmark to directly study the effect of variation in the career histories of an individual's peers on their own propensity to become an entrepreneur. We find that employees are more likely to become entrepreneurs if their co-workers have had prior self-employment experience or have had more diverse work experience in the past. Our findings therefore suggest the presence of both 'knowledge-spillovers' and 'entrepreneurial exposure' as important mechanisms contributing to firm-level heterogeneity in entrepreneurial spawning, and highlight the importance of studying the social context of firms when looking at labor market outcomes.

One of the main advantages of our dataset is that it allows us to go beyond prior work, in examining whether the observed relationships between 'peer-effects' and entrepreneurship may in fact be due to a spurious correlation – being driven by selection at the firm- or individual- level. Although we have not finished doing our robustness checks, initial results seem to indicate that selection may in fact play an important role in these results. If indeed this is the case, it will have important implications for both academics working in the field of entrepreneurship, and for policy makers focused on spurring entrepreneurship using industrial clusters. For scholars it will help establish a causal mechanism for the observed results that is different from the conventional wisdom, and also serve as a caution for those wishing to interpret results based on regional-and firm level constructs but using individual-level mechanisms. For policy makers, it will imply that while reforms aimed at driving labor mobility and attracting qualified 'entrepreneurial' firms to a region may have positive benefits, the *multiplier*

*effect* that is implicitly assumed may be overstated. At present, however, our results are still a work in progress, and should be read and interpreted as such.

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# Table 1

| | ***Self employment % non-agricultural employment*** | | ***Self Employment as % of all employment*** | |
|---|---|---|---|---|
| | *1986* | *1996* | *1986* | *1996* |
| Denmark | 7.7 | 7.2 | 11.6 | 9.5 |
| Germany | 7.7 | 8.3 | 11.5 | 10.6 |
| UK | 9.6 | 11.3 | 11.5 | 13.6 |
| USA | 7.1 | 6.8 | 8.9 | 8.4 |

Source: OECD Labor Force Statistics as reported in Blanchflower (2000), "Self Employment in OECD countries", *Journal of Labor Economics 7, 471-505*

# Table 2

## *Select Labor Market Indicators for 1990*

| | *Employment/population ratio 15-64* | *Labour force participation rate* | *Unemployment rate* | *<1 month* | *1-3 months* | *3-6 months* | *6mo -1 year* | *> 1 year* |
|---|---|---|---|---|---|---|---|---|
| Denmark | 77% | 68% | 8% | 7% | 10% | 29% | 23% | 29% |
| Germany | 66% | 57% | 5% | 7% | 13% | 18% | 18% | 44% |
| UK | 72% | 62% | 7% | 13% | 18% | 21% | 16% | 30% |
| US | 74% | 65% | 6% | 47% | 32% | 11% | 4% | 5% |

Source: OECD Statistics website

# TABLE 3

**Comparison of Entry Rates into Select Manufacturing Industries; Denmark and US**

| | *Denmark* | *US* |
|---|---|---|
| Food Processing | 22% | 24% |
| Tobacco | 3% | 21% |
| Textiles | 29% | 37% |
| Apparel | 49% | 40% |
| Leather | 16% | 29% |
| Lumber | 29% | 49% |
| Paper | 18% | 31% |
| Printing | 33% | 49% |
| Petroleum and Coal | 6% | 34% |
| Chemicals | 12% | 32% |
| Rubber and Plastics | 23% | 43% |
| Stone, Clay, Glass | 30% | 34% |
| Primary Metals | 15% | 32% |
| Fabricated Metals | 31% | 43% |
| Non-Electrical Machinery | 26% | 46% |
| Electrical Machinery | 28% | 46% |
| Transportation Equipment | 22% | 47% |
| Furniture | 33% | 47% |

*Denmark for 1980-85 and 1986-1990*
*US for 1972-77 and 1977-82*

## TABLE 4

*Dependent Variable Takes a Value of 1 if Individual becomes an entreprenuer and 0 otherwise*

| | ***Full Sample*** | | | ***Firms with upto 25 employees*** | ***Firms in "Knowledge" Industries*** |
|---|---|---|---|---|---|
| Variable | *Model 1* | *Model 2* | *Model 3* | *Model 4* | *Model 5* |
| peer entrep in last 5 yrs | 0.286** | | 0.294** | 0.267** | 0.690** |
| | (0.05) | | (0.05) | (0.05) | (0.20) |
| peer job mobility in last 5 yrs | | 0.219** | 0.220** | 0.208** | 0.257* |
| | | (0.02) | (0.02) | (0.03) | (0.12) |
| own job mobility in last 5 yrs | 0.082** | 0.077** | 0.077** | 0.047* | 0.152* |
| | (0.01) | (0.01) | (0.01) | (0.02) | (0.07) |
| vocational degree | 0.044 | 0.045 | 0.044 | -0.112 | -0.08 |
| | (0.06) | (0.06) | (0.06) | (0.09) | (0.28) |
| academic degree | 0.152** | 0.138** | 0.140** | -0.04 | 0.381 |
| | (0.05) | (0.05) | (0.05) | (0.07) | (0.21) |
| university degree | -0.013 | 0.001 | 0.003 | 0.178 | -0.315 |
| | (0.10) | (0.10) | (0.10) | (0.15) | (0.43) |
| female | -0.788** | -0.791** | -0.790** | -0.761** | -0.911** |
| | (0.03) | (0.03) | (0.03) | (0.05) | (0.16) |
| danish citizen | -0.534** | -0.530** | -0.531** | -0.449** | -0.211 |
| | (0.07) | (0.07) | (0.07) | (0.10) | (0.29) |
| age | 0.130** | 0.127** | 0.125** | 0.151** | 0.077 |
| | (0.02) | (0.02) | (0.02) | (0.03) | (0.10) |
| age squared | -0.002** | -0.002** | -0.002** | -0.002** | -0.001 |
| | 0.00 | 0.00 | 0.00 | 0.00 | (0.00) |
| married | 0.084** | 0.086** | 0.086** | 0.110* | -0.006 |
| | (0.03) | (0.03) | (0.03) | (0.05) | (0.14) |
| kids | -0.017 | -0.015 | -0.015 | -0.018 | -0.017 |
| | (0.03) | (0.03) | (0.03) | (0.04) | (0.12) |
| laborforce experience | 0.320** | 0.321** | 0.323** | 0.305** | -0.005 |
| | (0.04) | (0.04) | (0.04) | (0.05) | (0.17) |
| log salary income | -0.081** | -0.083** | -0.083** | -0.081** | 0.054 |
| | (0.02) | (0.02) | (0.02) | (0.03) | (0.14) |
| log non salary income | 0.297** | 0.296** | 0.295** | 0.268** | 0.343** |
| | (0.04) | (0.04) | (0.04) | (0.08) | (0.11) |
| log debts | 0.027** | 0.026** | 0.026** | 0.026** | -0.003 |
| | (0.00) | (0.00) | (0.00) | (0.01) | (0.02) |
| log assets | 0.024** | 0.024** | 0.024** | 0.028** | 0.022 |
| | (0.01) | (0.01) | (0.01) | (0.01) | (0.02) |
| pse | 0.232** | 0.235** | 0.234** | 0.193** | 0.138 |
| | (0.03) | (0.03) | (0.03) | (0.04) | (0.13) |
| # establishments in firm | 0.048** | 0.056** | 0.053** | 10.718** | -0.122 |
| | (0.02) | (0.02) | (0.02) | (3.14) | (0.07) |
| Diversified Firm | -0.007 | 0.005 | 0.002 | 0.167 | -0.053 |
| | (0.06) | (0.06) | (0.06) | (0.18) | (0.31) |
| Firm Age 0-2 | 0.061 | -0.049 | -0.061 | -0.079 | -0.024 |
| | (0.05) | (0.05) | (0.05) | (0.06) | (0.25) |
| Firm Age 3-9 | 0.076* | 0.019 | 0.021 | 0.004 | 0.033 |
| | (0.04) | (0.04) | (0.04) | (0.04) | (0.15) |
| logfirmsize | -0.127** | -0.129** | -0.124** | -0.093** | -0.101* |
| | (0.01) | (0.01) | (0.01) | (0.02) | (0.04) |
| Industry Fixed Effects? | Yes | Yes | Yes | Yes | Yes |